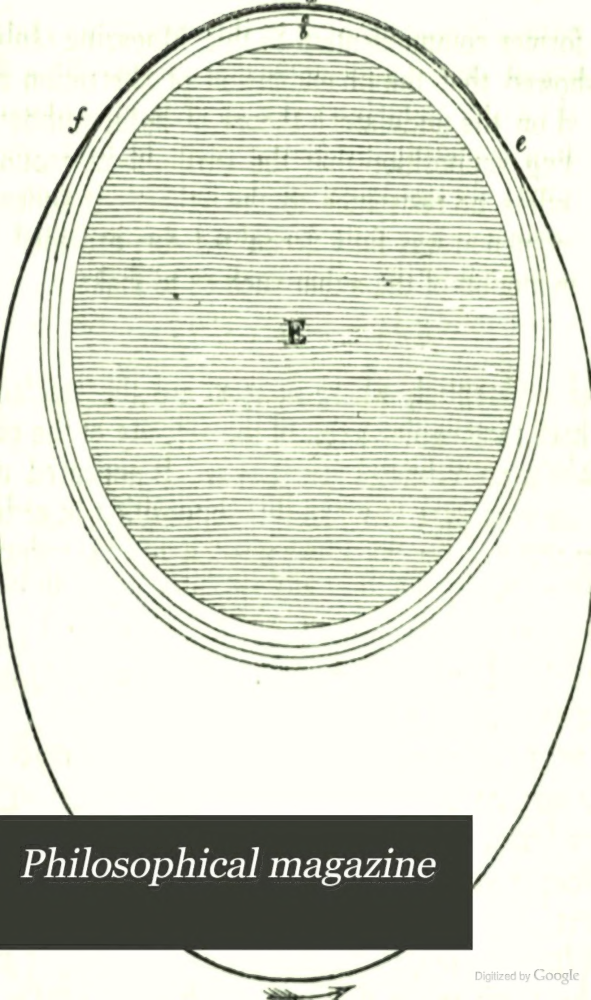

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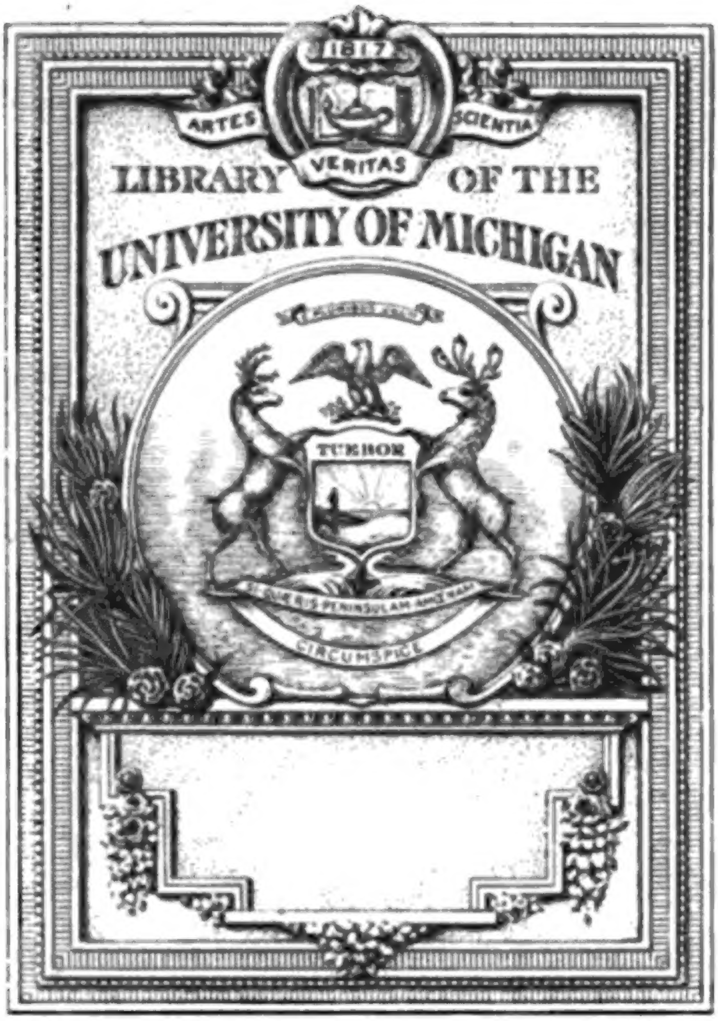
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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

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RICHARD TAYLOR, F.L.S. G.S. Astr.S. Nat.H.Mosc. &c.
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SIR ROBERT KANE, M.D. M.R.I.A.

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

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

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NUMBER CLXXXVIII.—MAY.

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NUMBER CXC.—SUPPLEMENT TO VOL. XXVIII.

PLATES.

Errata and Addenda.

Page 190, Note **, between "3." and "p." add vol. xxvii.
... 391, for *Salis deliquio* read *Solis*.
... ... , for *Bontschii* read *Bartschii*.
... 393, for *Saginensibus* read *Saganensibus*.

Fig. 1.

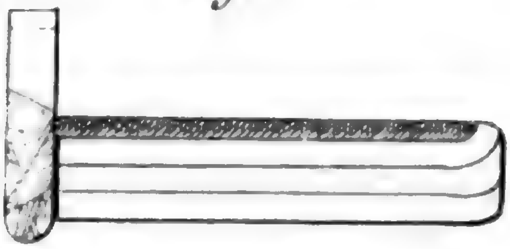


Fig. 3.

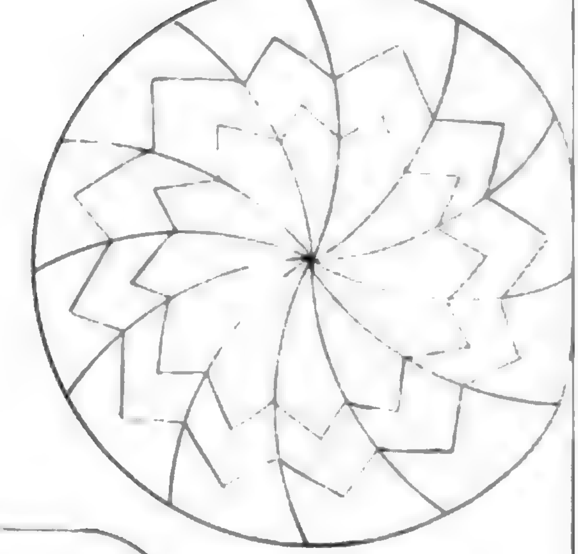


Fig. 2.

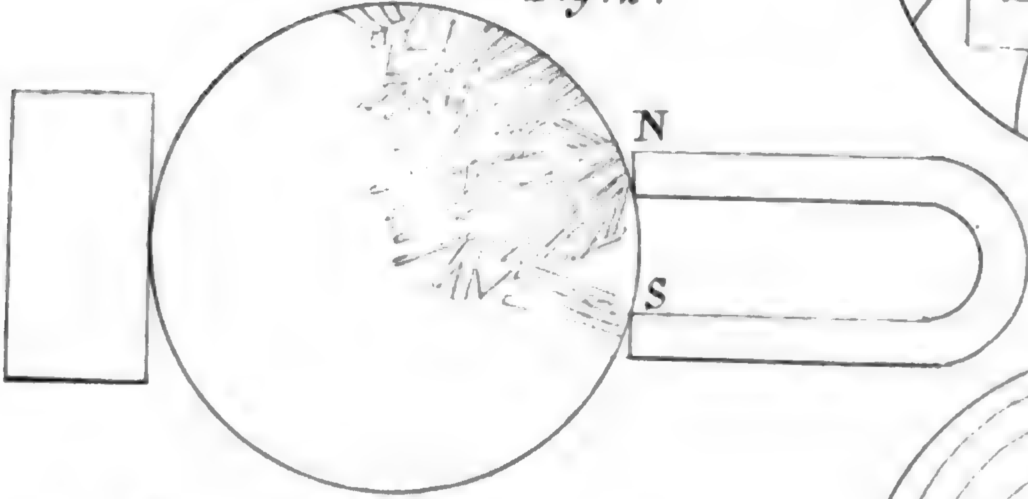
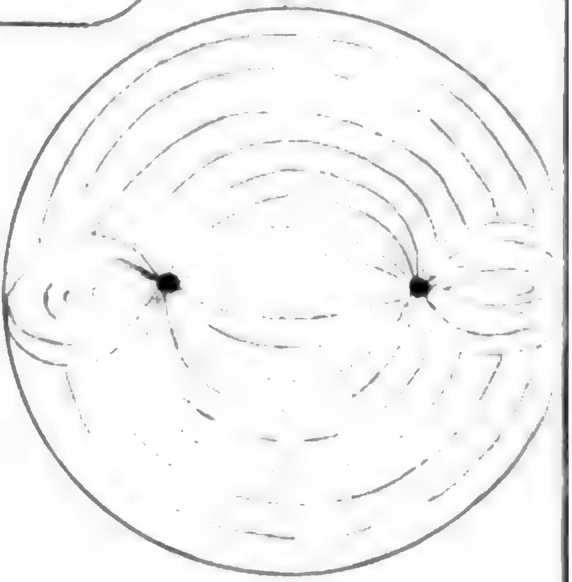


Fig. 4.



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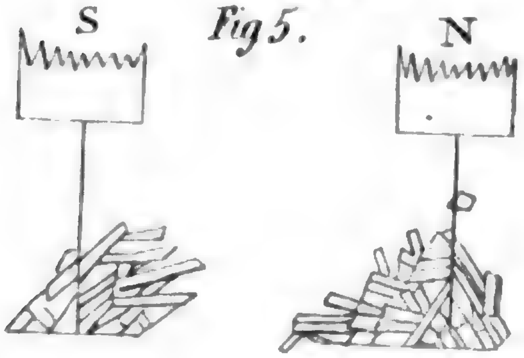

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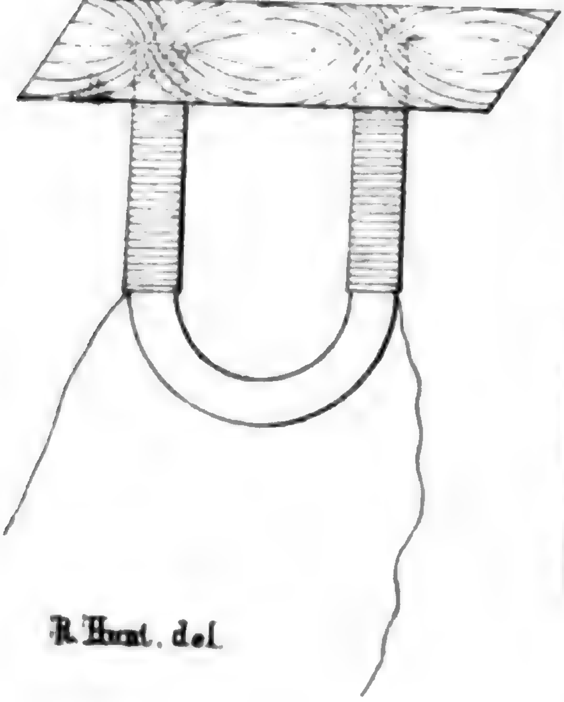


Fig. 8.

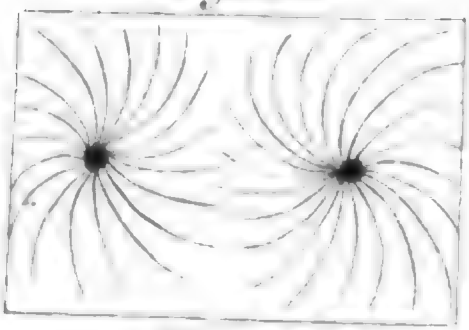


Fig. 7.

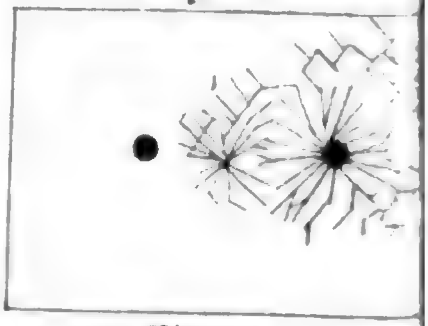


Fig. 10.

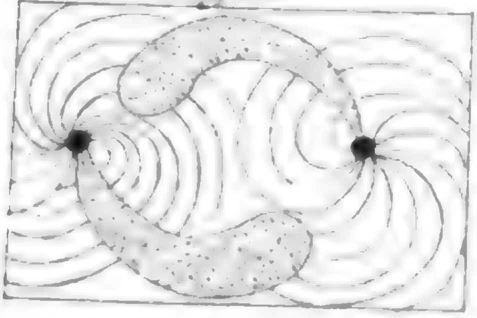
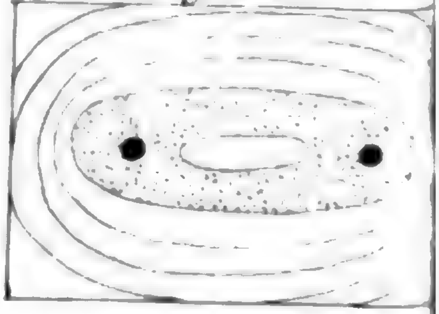


Fig. 9.



R. Hunt. del.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1846.

I. *The Influence of Magnetism on Molecular Arrangement.*

By ROBERT HUNT, *Keeper of Mining Records, Museum of Œconomic Geology.*

[With a Plate.]

To Richard Phillips, Esq., F.R.S.

DEAR SIR,

HAVING been engaged some time since in investigating the influences of bodies on each other in the dark, the results of which investigations were published under the title of "Thermography," I then observed many peculiar effects which led me to believe that magnetic electricity had some influence in determining the arrangements of molecules. From that time until a few days since, the subject has rested with me without any further research. Having however put the subject to the test of experimental examination, I am induced, the results being of great interest, to transmit to you an account of my experiments. In doing this, I shall, for the present, confine myself strictly to a description of the arrangements used and the results obtained, reserving any theoretical views for some future period, when by a greater number and variety of experiments it appears probable some general law of action may be satisfactorily deduced.

1. I placed a concentrated solution of nitrate of silver in a test-tube, against the poles of a permanent horse-shoe magnet, having another tube containing a similar solution not in contact with it. The crystallization commenced first in the tube connected with the magnet, immediately at the point opposite the upper surface of the metal (Plate II. fig. 1); a large tabular crystal shot off from this point towards the bottom of the glass, dividing the lower portion of the fluid in two parts. Other crystals sprung off from different points above and be-

low this crystalline plate, but all of them arranged themselves at angles inclining towards the magnet; no crystallization taking place in the upper stratum of the fluid. In the other tube, crystals formed irregularly throughout the fluid, but in no part were the crystals so dense as in the tube which I suppose to be under the influence of magnetism.

2. With a view of determining if the cooling influence of the metal had anything to do with the crystalline arrangement, portions of the same solution of nitrate of silver were put into glass capsules. One of these was placed against the poles of the magnet, and the other in contact with a mass of brass of the same weight. In the first, crystallization commenced opposite the north pole of the magnet, and proceeded slowly in regular lines to crystallize over every part; all these lines have a tendency towards the poles of the magnet. In the capsule in contact with the brass, crystallization commenced at a point furthest from the metal, and even when the fluid had become quite cold, nearly one quarter of it, which was nearest the mass of metal, remained quite free from any crystalline formation.

3. To exhibit this in a more striking manner, a capsule was placed between the mass of brass and the magnet, in contact with each, as shown in fig. 2; the solution of nitrate of silver in this case, not being so concentrated as that previously used, the arrangement was allowed to remain at rest for some hours. It was then found that crystallization had taken place only over one portion of the fluid, and that immediately in connexion with the north pole of the magnet, except three long crystals which sprung from the fluid opposite the south pole, and were directed towards those springing from the north pole. This experiment was repeated four times, and, except when the solution was so concentrated as to crystallize almost immediately, the same result was obtained.

4. The phænomenon of molecular disposition under magnetic influence is pleasingly seen by a modification of the arrangement described. The two glass capsules with their solutions are placed on a plate of glass blackened on its under surface, one glass being put in contact with the brass and the other with the magnet. Their images are to be observed in the black mirror on which they rest, the light falling upon them at an angle of about 25°. As the fluids cool, the circulating currents coloured by their refracting powers are seen in the mirror. In the image of the capsule in contact with the brass, no regularity of circulatory movement is observable; but in that under magnetic influence, a series of perfectly regular curved lines proceed from the circumference to the

centre; and these are crossed by small streamers springing laterally from these primary curves, presenting an appearance similar to that shown in fig. 3. These curves are constantly varying in position, but they uniformly preserve the utmost regularity.

5. The magnet was suspended from a tripod, and two steel needles attached to its poles; these needles were made to dip into a solution of nitrate of silver in a watch-glass. As the pellicle formed over the surface, it arranged itself in a series of curved lines, as represented in fig. 4, which are strikingly similar to those produced by sprinkling iron-filing on stretched paper placed over a magnet. That these curves are due to magnetic influence there can be no doubt, as no such effect could be produced by any cooling influence, independent of magnetic excitation.

6. A similar arrangement was allowed to remain in action for twelve hours. At the end of this time crystallization had taken place in every part of the fluid, but there was an evident tendency to a curvilinear arrangement of the crystals. Around the wire depending from the north pole of the magnet, some revived silver had made its appearance: no such change was discovered at the south pole.

7. Wires similarly suspended were dipped into a solution of sulphate of iron. Crystallization commenced around the wire at the north pole, but after a few hours crystals had formed around both of the wires, but in the greatest quantity around the north pole wire. On removing them from the solution, the crystals were found to present an arrangement similar to that represented in fig. 5, showing obviously a tendency to arrange themselves along lines of magnetic direction.

8. A solution of protonitrate of mercury was placed under similar circumstances; crystallization commenced at the wire suspended from the north pole, and proceeded rapidly to a line midway between the two wires; one-half of the fluid being crystallized and the other remaining fluid. At length a few crystals formed around the wire hanging from the south pole, which all took a direction towards the opposite arrangement of crystals.

9. With a more dilute solution, the crystallization of the nitrate of mercury took place only around the wire at the north pole, and immediately at the central point between the two wires, from which small needle-shaped crystals radiated towards either pole.

10. A plate of glass, with an edge of clay, forming a shallow trough, was placed *upon* the poles of an electro-magnet,

capable of supporting fifty pounds when connected with a single galvanic pair excited by water acidulated with sulphuric acid. On pouring a warm and tolerably strong solution of nitrate of mercury into the trough, there was immediately formed over the surface a series of beautifully regular curves from pole to pole, as shown in fig. 6, which also represents the arrangement.

11. A similar glass trough, filled with a moderately strong solution of the nitrate of mercury, was supported on the poles of the same electro-magnet, connected with a small battery of a more permanent, but less powerful arrangement, and all was allowed to remain at rest until crystallization had taken place. The result was similar to that already described (9.), but much more strikingly shown. The order of arrangement taken by the crystals is shown in Plate II. fig. 7.

12. A plate of copper with an edging of wax was placed on the electro-magnet in the same manner as the glass plate; over it a very weak solution of nitrate of silver was quickly poured; the plate immediately blackened from the decomposition of the silver salt by the copper. In about a minute the finely divided silver arranged itself into curves, as represented in fig. 8, which were after a few minutes again destroyed. By using a sheet of chemically-pure copper, obtained by electrotype deposit, I found a permanent impression of these curves could be obtained, owing to the oxidation of the copper along the spaces, which the finely divided silver, when distributed in curve-lines, did not cover.

13. A plate of hard copper, such as is used by engravers, was placed in precisely the same circumstances, and covered with a tolerably strong solution of nitrate of silver. It was left in contact with the electro-magnet for a night. On washing off the deposit of silver which covered it, it was found that the acid of the silver salt had bitten deeply into the plate over an oval space around the poles, leaving a small space between them quite bright. The copper over this etched space was covered with an immense number of minute holes; and beyond this the oxidation of the surface had proceeded in curved lines, as represented in fig. 9. We thus have permanent evidence of the influence of magnetic force in determining chemical action.

14. Into one of the glass troughs before named, placed on the electro-magnet, a weak solution of nitrate of silver was poured, and into this an equally weak solution of sulphate of iron. In about five minutes precipitation of silver commenced; this precipitate arranged itself over the glass in curves proceeding from and around the poles in the same manner as it

distributed itself over the copper plate. In a short time, precipitation increasing, two curious curved spaces were formed by the fine deposit, proceeding from one pole towards the other in opposite directions, increasing in width as they proceeded, until they were abruptly checked at a little distance from the poles towards which they were directed; these spaces being very distinct from the first formed curved lines. Fig. 10 represents this very interesting arrangement.

These experiments are sufficient to show that magnetism exerts a powerful influence on molecular arrangements, and that it regulates the direction of crystalline formations. I hope to be enabled to pursue this interesting inquiry still further; it has most important bearings on many of the great phænomena of nature, and I am therefore anxious thus early in my inquiry to call attention to the singular and conclusive results which I have obtained.

I have the pleasure of remaining,
Dear Sir, yours truly,
ROBERT HUNT.

6 Craig's Court, Dec. 10, 1845.

II. *On certain Pseudomorphous Crystals of Quartz.*

By ROBERT WERE FOX, *Esq.*[*]

I SUBMIT to the Society's notice some specimens of quartz, with pseudomorphous octahedral crystals of the same substance, which appear to me to possess a sort of historical interest, or at least to indicate that a succession of changes must have occurred in the condition of the mineral vein from which they were taken. They were found by S. Peters (dealer in minerals) in one of the heaps of vein stones, at the Consolidated Mines, and I understand were broken from a copper vein in "*killas*," at the depth of about 160 fathoms below the surface. He observed that many of the crystals contained water, and he secured some of it for me, by carefully breaking some of them. This he did mostly in my presence, and we had considerable difficulty in collecting even very small portions of the liquid in different phials. Two of these portions were nearly tasteless, or saline in a very slight degree, as far as I could judge from a single drop of each. In both common salt was detected, and nothing else in one of the portions; but the other, when evaporated, left minute needle-formed crystals, which I was prevented by an accident from examining. The third portion of water was much more in

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[*] Read at a meeting of the Cornwall Polytechnic Society, on the 8th of October, 1845, and communicated by the Author.

quantity than both the others—nearly a tea-spoonful, and obtained from only one crystal. It was very acrid to the taste, and gave very copious precipitates when tested by muriate of barytes and hydrocyanate of potash, showing the presence of much sulphuric acid and iron. Oxalate of ammonia and nitrate of silver, indicated, moreover, the presence of lime and muriatic acid. The saline matter in this water (mostly sulphate of iron) was equal to one-tenth of its weight; and if it contained any common salt, of which I am not positive, the proportion was very small indeed. Litmus paper showed an excess of acid, the nature of which was not ascertained.

Many of the pseudomorphous crystals are more than an inch in diameter, and are partly or entirely filled with crystalline quartz, whilst others are empty, or partly filled with more or less numerous fragments of disintegrated fluor. I counted nearly a hundred of such fragments taken from one of the crystals or cavities, exclusive of many other very small pieces. All the fragments are corroded, and indicate, by their rounded edges and indented surfaces, the action of a solvent which penetrated most readily between the *planes of cleavage**. Besides this disintegrated fluor, perfect octahedrons of fluor occur in the same specimens; but they were rather more imbedded in the quartz and more protected from injury than the others. Water was found alone in some of the pseudomorphous crystals or cavities, and in others it was found with fragments of fluor, or with crystalline quartz.

The most perfect pseudomorphous octahedrons occur within large cavities of quartz. Some of the latter are more than two inches in diameter, having the same form, and their sides generally parallel to those of the former.

The quartz specimens to which the crystals are attached, present, when broken, the appearance of fortification agate, having lines parallel to their structure of transparent and milk-white quartz, differing in thickness; these seem to indicate that the siliceous matter had been deposited at intervals of greater or less duration, or at least under different circumstances. After a time an entire change of conditions apparently occurred in the vein, and octahedral crystals of fluor were formed on the quartz; then silex was deposited either in a compact form, or in minute crystals, and coated the crystals of fluor; afterwards fluor again appeared, forming octahedrons over the others, and mostly with sides and angles parallel to them. These processes appear from some of the

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* When crystals of alum were kept for a time in water, the planes of cleavage were first acted on, and fragments were separated from the crystals resembling those of the disintegrated fluor.

crystals to have been again repeated: then came a coat of silex over the fluor, or judging from the lines, many coats of it, forming a thick crust, having a surface of small quartz crystals. Some specimens were found at the same time with one or more layers of quartz between two or more portions of fluor, which tend to confirm these views.

I think it may be inferred, from the well-defined and smooth impressions which the octahedrons of fluor have left in the quartz, and the general parallelism of the sides and angles of the outer cavities to those of the smaller pseudomorphous crystals inclosed in them*, that the inner and outer crystals of fluor were perfect and uninjured until after the whole series of them were coated with quartz. At some subsequent period then it would appear that other changes occurred in the vein, and that the solution or destruction of the fluor commenced. Some of the cavities which were found to contain water only, as well as those which contained water together with disintegrated fluor, have the appearance of having been so hermetically sealed, that it is difficult to understand how the liquid solvent could have obtained access to the fluor and abstracted it from its case. It cannot be supposed that the pressure of the column of water above it, although equal to more than half a ton on some of the larger crystals, could alone have produced the effects; for not only must the solvent have been continually admitted through the crusts of the quartz, but the salts resulting from the solution of the fluor must, at the same time, have passed through them in the opposite direction,—a sort of *endosmose* and *exosmose* must have existed, as I conceive, to produce the phænomena; whilst in other instances, the thick envelopes of quartz were impervious and protected the fluor from injury. The salts resulting from the solution of the fluor must have been soluble, although this condition seems to present some difficulties under the circumstances of the case; and doubtless the destruction of the fluor was very slowly effected in many instances, and in others it was begun, but never completed. The differences in the saline contents of the water obtained from some of the crystals is another circumstance of some interest, indicating the existence of different conditions in the vein when the water was last admitted into the respective crystals.

The phænomena exhibited by these minerals cannot, I con-

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* How are such coincidences to be accounted for? Are we to assume that *polarising forces* have determined the arrangement? In many instances the layers of quartz which were *interposed* between the crystals are very thin, imperfect, and pervious to water; but in others they are not so, and some of the inner crystals now contain water.

ceive, be accounted for but by supposing the water existing in the fissures of the earth to have been changed by circulation from time to time, and to have been charged with different ingredients at different periods.

I have on former occasions alluded to various causes which would produce circulation in the subterranean waters, such as the opening or closing of any portions of fissures; the ascent of warm and the descent of cooler currents of water, in consequence of the differences in their specific gravities; or in some instances by the pressure of the sea-water acting on the fresh*. Nearly two years ago I stated in this room my views in reference to the operation of this latter cause on land springs, and at the same time I attempted to show the possibility, not to say probability, of steam existing in fissures *below* the water at a very great depth. I may perhaps be permitted to refer again to this subject, because it appears to me to be one of some interest. I then took it for granted that the temperature of the earth increases in some proportion to the increase of depth below its surface, and that if the ratio be taken at 1° Fahr. for every forty-eight feet, as found in our deep mines, and if Le Roche's data for calculating the elastic force and density of steam be adopted, the forces of steam and of water pressure would balance each other at rather more than nine miles deep, each being equal to the pressure of more than 1400 atmospheres. The density of the steam would there be about one-fourth that of water at 60° Fahr., and its temperature above 1050° Fahr. But the temperature may probably not increase so rapidly as this at great depths, and the equilibrium in the pressures of the column of water and of steam may occur much further below the surface, where the density of steam under an augmented pressure of water would, of course, be still greater. However this may be, it would seem that, under any probable circumstances in regard to the ratio of increase in the earth's temperature, the increase in the pressure of the lengthened column of water would not keep pace with the rapidly increasing tendency of the water in descending into more heated parts of the earth to expand into steam, the elasticity of which at very high temperatures, when confined and in contact with water, is greatly augmented by very small increments of sensible heat.

No water could long remain unchanged into steam below the line of division between them, and there the steam would

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* Columns of *sea and spring water*, about five feet high, balanced against each other in a U-shaped tube, more than a year ago, still remain unmixed, showing nearly the same difference of level as at first (exceeding an inch).

be denser than at any deeper station, for it would be continually diminishing in density in descending further, from the augmentation of the temperature of the earth, because the *expanding* influence of the increasing heat would much exceed the *condensing* influence of the extended column of steam, added to that of the nearly constant column of water.

The line of demarcation between the water and steam would, doubtless, conform in some degree to the inequalities of the surface. It may be difficult at first to conceive the steam capable of supporting the water, or rather of existing permanently under it; but this difficulty will, I think, be obviated by the consideration, that the points of contact may be, for the most part, in very narrow fissures, or mere cracks in the rocks; and that the water being greatly heated, may be much less than *four times* the density of the steam in immediate contact with it. A continual struggle would, no doubt, exist between the water and steam under such circumstances, so that in many places they would alternately encroach beyond the line of demarcation; but as the checks on both would increase in proportion to the extent of their encroachments from the diminution of the temperature above and its augmentation below, such encroachments would probably not be very extensive, or of long duration under ordinary circumstances. Suppose a temporary encroachment of the water on the limits of the steam to occur at one point, the steam would probably encroach on the water at another at the same time, and then, reactions taking place, the effects would be reversed. Thus, assuming what indeed would appear to follow from *admitted data as necessary consequences*, steam would not only exist below the water, but such oscillations would tend to give motion and activity to the water in the neighbouring fissures, causing it to circulate in the earth more or less freely and extensively according to circumstances. In volcanic districts, where the heat may be great at comparatively small depths, analogous phænomena sometimes occur at the surface, which are probably caused by the action and reaction of steam and water. Amongst these may be included the intermitting Geyser springs in Iceland, as well as some of the mud volcanoes found in Sicily, and in Asia, and America.

It seems probable that earthquakes may be produced by the action of highly elastic vapour rapidly generated at great depths, in consequence perhaps of copious and sudden influxes of water into intensely heated parts of the earth; and their lines of direction are doubtless influenced by those of the fissures or veins of the districts in which they occur. But these are phænomena of comparatively rare occurrence, and it is no wonder that they should be so, when we consider how vastly greater must be the force required to uplift the rocky crust of the earth and wrench it asunder, than that which will support a column of water equal to the thickness of that crust.

Since the foregoing paper was read, I have rather hastily examined some other portions of water taken from different pseudomorphous crystals. One of those portions contained muriatic and sulphuric acids, iron, a trace of lime, and of common salt. Acid was a little in excess, and some peroxide of iron was left in the cavity from which the water was taken. In another the same acids were detected and some iron. In the third portion there seemed to be nothing besides a little common salt. In many of the octahedral cavities, oxide of iron was found, and sometimes iron pyrites or copper pyrites adhering to the sides; these were apparently deposited from some of the water which had entered the crystals in some instances, but in others they were evidently imbedded in the fluor, and, adhering to the deposit of quartz, were not dissolved with the former.

Earthy carbonate of iron occurs in some cavities mixed with very minute crystals of quartz; and I have one pseudomorphous quartz crystal which is filled with fragments of fluor, intermixed with translucent fragments of carbonate of iron and earthy carbonate of iron, all curiously cemented together into one mass; the iron ore being rather in excess.

I have also some hollow pseudomorphous crystals of quartz formed originally on carbonate of iron, which appear to be water-tight, and yet the latter substance has, like the fluor, been abstracted.

III. *On the General Expression for the Sum of an Infinite Geometrical Series. By* J. R. YOUNG, *Professor of Mathematics in Belfast College*[*].

THE general expression for the sum of the infinite series

$$1-x+x^2-x^3+x^4-\&c.$$

is

$$S=\frac{1}{1+x}-\frac{x^{\infty''}}{1+x},$$

which reduces to $\frac{1}{1+x}$ when x is a proper fraction, either po-

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[*] Communicated by the Author.

sitive or negative, on account of the evanescence of $x^{\infty''}$. It is usual to consider the infinite exponent in this expression as invariable throughout all the changes of x within the limits 0 and 1; although it is known that for any fixed exponent short of infinite, however great it may be, the expression into which it enters becomes more and more considerable as x advances from 0 towards 1; and notwithstanding the additional fact, that when this exponent is actually infinite, the expression referred to becomes ultimately equal to $\frac{1}{e}$.

But it is evident—due weight being given to the circumstances here mentioned—that this assumption, as to the invariability of the infinite exponent, is unwarrantable and erroneous; and that the exponent must follow some law of variation exactly fitted to counteract and neutralize the tendency which, as x approaches to 1, the expression $x^{\infty''}$ would otherwise have to depart from zero, and ultimately to become $\frac{1}{e}$. If x, at any stage of its approach to 1, be generally represented by $1 - \frac{1}{k}$, then the law of variation alluded to will be expressed by $\infty'' = k\,\infty'$: that is, the exponent must vary as k. For it is a remarkable fact that, commencing with the exponent 4 and proceeding onwards to infinity, we shall invariably have

$$\left(\frac{3}{4}\right)^{4} = \cdot 3\ldots,\ \left(\frac{4}{5}\right)^{5} = \cdot 3\ldots,\ \left(\frac{5}{6}\right)^{6} = \cdot 3\ldots,\ \left(\frac{6}{7}\right)^{7} = \cdot 3\ldots,$$

$$\left(\frac{16801}{16802}\right)^{16802} = \cdot 3\ldots, \ldots \left(\frac{25684}{25685}\right)^{25685} = \cdot 3\ldots, \ldots \left(1-\frac{1}{\infty}\right)^{\infty} = \cdot 3\ldots$$

And since $(\cdot 3\ \ldots)^{\infty'}$ is necessarily zero, and no power short of infinite can give zero, it follows that in order that $\left(1-\frac{1}{k}\right)^{\infty''}$ may be uniformly zero, and that all tendency to depart from zero may be counteracted, ∞'' must be $k\,\infty'$; so that the strictly accurate form for S is

$$S = \frac{1}{1+\left(1-\frac{1}{k}\right)} - \frac{\left(1-\frac{1}{k}\right)^{k\infty'}}{1+\left(1-\frac{1}{k}\right)},$$

which is equal to $\frac{1}{2}$ when k is infinite. And in this manner is the formula, employed in my paper (p. 363, last vol.), established.

In the same way that it has now been proved that $\left(1-\frac{1}{k}\right)$ is always equal to ·3 ..., whatever be k, above 3, may it be further shown that $\left(1+\frac{1}{k}\right)^k$ is always equal to 2· ...; and thence that $\left(1+\frac{1}{k}\right)^{k\infty'}$ is necessarily infinite when k is: so that it is indisputably true that the extreme of the *convergent* cases of the above series S, usually written in the form

$$1-1+1-1+1-1+\&c.$$

is $\frac{1}{2}$, and that the extreme of the *divergent* cases, usually written in the same form, is really infinite, as stated in my former paper; which last conclusion could never have been anticipated from the theory hitherto prevalent. The views now developed are only the continuation and completion of those exhibited in my paper on Series submitted to the British Association in June 1845. If I have been anticipated in any of these views, which are doubtless calculated to produce a reform in the existing theory, I hope to be informed of the circumstance through the medium of this Journal. I have only further to add, that when an expression for the convergent cases of a series is found—as it often may be by aid of the differential theorem—then the general equivalent of the series may afterwards be ascertained by developing this expression sufficiently far to unfold to us the general form of the remainder. The expression for the convergent cases of the general series, discussed at page 439 of the last volume, may in this manner be determined; and the development of this expression by common division, as there proposed, furnishes the formula by which that expression must be corrected, in order that the algebraical equivalent of the series may be exhibited in its utmost generality.

Belfast, November 21, 1845.

IV. *On the Conversion of Cane-sugar into a substance isomeric with Cellulose and Inuline.* *By* THOMAS TILLEY, *Esq., Ph.D., and* DOUGLAS MACLAGAN, *M.D., F.R.S. Edin.*[*]

WHEN the juice of beetroot undergoes fermentation at temperatures varying from 30° to 40° C., the cane-sugar which it contains is at first converted into sugar of grapes, and after some time into mannite, lactic acid and a

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[*] Communicated by the Chemical Society; having been read April 21, 1845.

gummy substance, having a composition identical with that of gum-arabic. This is remarkable, inasmuch as it affords an instance of what may be called a retrograde chemical action, the sugar being converted into dextrine,—a change similar to that which occurs in fruits when they lose their sweetness, and assume that condition commonly called "sleepy." The conversion of cellulose into dextrine and sugar seems to be a process of continual occurrence and great importance in the vegetable œconomy, but we are not aware of any example of the reverse of this action, except those instances mentioned above; in the former of which sugar is converted by fermentation into a body having all the properties and composition of gum; in the latter, the sugar being changed into cellulose*. We therefore consider the observation we are about to describe to be possessed of some interest, as affording another case of a similar retrograde action. It has been observed that the effervescing drinks known as lemonade, gingerade, &c., made by forcing carbonic acid gas into solutions of sugar variously flavoured with tartaric acid and essential oils, in certain cases lose their fluidity, and assume a thick, slimy consistence. When the bottles containing these thickened liquids are opened, the expansion of the carbonic acid expels their contents with difficulty, owing to their extreme tenacity. Instances of this change are of continual occurrence, all the manufacturers of whom we have inquired having observed it when the bottles had been kept for some time. Various opinions have been expressed by them as to the cause of the conversion, but it seems to occur invariably when the liquor is kept long enough. We are indebted to Mr. Baildon of this city (Edinburgh) for an opportunity of examining a sample of gingerade, in which this thickening had occurred. This liquid is made by sweetening an infusion of ginger-roots with cane-sugar, and flavouring it with oil of lemons and tartaric acid; this is then placed in bottles, and carbonic acid forced by pressure into the fluid. Another manufacturer uses the following ingredients in the preparation of effervescing lemonade:—2 ounces of honey, 4 pounds of sugar, 2 ounces of citric acid, 2 drachms of oil of lemons and 1½ ounce of bicarbonate of soda. According to the opinion of this manufacturer, the change occurs chiefly in winter, when the liquid is exposed to cold, and he thinks that the addition of a double proportion of honey tends to prevent it. To separate the substance to which the viscidity was owing, the contents of a bottle were digested with six or seven parts of alcohol, under the action of which the gummy matter consolidated, and when dried became so hard

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* See Mulder's *All. Phys. Chem.*, p. 243 *et seq.*

as to be pulverizable. After being powdered, it was again digested and washed with alcohol until nothing more was dissolved. When dried at 100° C. it had the appearance of a semi-transparent horny substance, and was sufficiently elastic to render pulverization difficult. The alcohol contained in solution a quantity of sugar of a brownish colour, quite uncrystallizable, and rendered sour by the presence of the acid used in the manufacture.

The gummy substance treated with cold water slowly reassumes its original appearance. When treated with a large quantity of boiling water it forms a mucilage, which filters with difficulty. Iodine produces no effect on the solution. Subjected to Trommer's test for dextrine, sugar and gum, this did not indicate the presence of any of these substances. With nitric acid it produces oxalic acid. It gives a precipitate with diacetate of lead. It contains, after having been washed with alcohol, a small quantity of ashes, amounting to 1·37 per cent. It was analysed in the usual manner.

I. 0·746 of substance gave, with oxide of copper and chlorate of potash, 4·070 HO and 1·1735 CO_2 = 0·04727 H and 0·32448 C.

II. 0·1525 of substance gave 0·092 HO and 0·232 CO_2 = 0·010222 H and 0·06474 C.

These numbers, allowance being made of the ashes, give the following proportions:—

	I.	II.	Atoms.	Calculated.
Carbon . .	43·80	43·31	24	43·71
Hydrogen .	6·14	6·80	21	6·25
Oxygen . .	50·06	49·89	21	50·04

From this it would appear that this gummy substance is isomeric with cellulose and inuline*.

This substance, which has a composition similar to cellulose and inuline, is evidently formed from the cane-sugar in the lemonade, as all its other constituents exist in too small quantity to admit the idea of their having been its origin.

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Cellulose, Payen. Endine.		From turnip. Fromberg.	
Carbon .	43·40	Carbon .	43·95
Hydrogen	6·12	Hydrogen	6·13
Oxygen .	50·38[a]	Oxygen .	49·66[b]
Inuline. Parnell.		From Dahlia root. Payen.	
Carbon .	43·95	Carbon .	44·19
Hydrogen	6·30	Hydrogen	6·17
Oxygen .	49·75[c]	Oxygen .	49·64[d]

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[a] *Ann. des Sc. Nat.*, 1840, p. 73. Bot.

[b] Mulder, *Op. cit.*, p. 203.

[c] Phil. Mag., vol. xvii. p. 126.

[d] *Op. cit.*, p. 91.

2 atoms sugar	C 24	H 22	O 22
1 ... water		H	O
1 ... gummy substance	C 24	H 21	O 21

This substance is formed then from 2 atoms of sugar by the abstraction of 1 atom of water.

As a solution of the gummy substance gave a compound with lead, we endeavoured to obtain by its aid its atomic weight. 0·260 of the precipitate gave of lead and oxide of lead quantities equal to 0·316 oxide of lead, which, when allowance is made for ashes, is equal to 55·8 per cent. of oxide of lead. We had not enough of the salt to enable us to make the combustion, but have calculated the formula from the quantity of lead.

		Atoms.	Calculated.
Carbon .	19·31	24	1834·4 = 18·7
Hydrogen	2·76	21	260·0 = 2·7
Oxygen .	22·11	21	2100·0 = 21·4
PbO . .	55·80 found	4	5578·0 = 57·1

From 55·8 per cent. oxide of lead the atomic weight found is 4400·0. The calculated one is 4198·4.

We had imagined that this curious change in sugar might have been the effect of organization, but our friend Mr. John Goodsir was kind enough to examine the substance, and informed us that he could discover no trace of organization.

V. *Remarks on* Professor Challis's *Theoretical Explanation of the Aberration of Light. By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge**.

THERE are a few points connected with Prof. Challis's paper on the Aberration of Light, published in the number of this Magazine for November 1845, respecting which I wish to offer a few remarks.

In the first place I perfectly agree with Prof. Challis, that the explanation of aberration is really independent of the manner in which light may pass through the eye; but I cannot agree with him that it is necessary to suppose that we see a star in its true place, and that it is the wire of the telescope with which it is observed that is affected by aberration. The following mode of viewing the subject, due to Boscovich, will perhaps put the matter in a clearer point of view.

If we wish to determine the real or apparent direction of an object, we may, theoretically speaking, adopt the following plan:—Let two small circular holes be so adjusted that the

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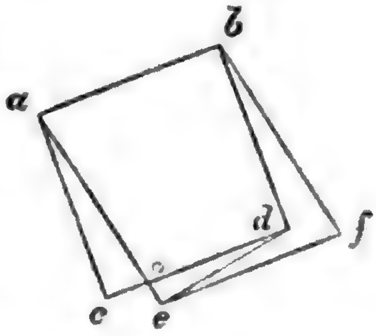
* Communicated by the Author.

light from the object which passes through the centre of the one shall also pass through the centre of the other. The line joining the centres of the holes will then determine the direction of the object. Now this is, in principle, just what is done in the case of an astronomical instrument, only, the fixed points are replaced by the optical centre of the object-glass of the telescope with which the object is viewed, and by the wire to which it is referred. When the image of a star is bisected by the wire, we define the apparent direction of the star to be that of the line joining the optical centre of the object-glass with the bisecting wire. Whether it is the wire or the star which is seen out of its true place, is a question with which we have no concern. The answer which we shall be disposed to give to it depends on the theory of aberration which we adopt. According to the theory of aberration which I explained in the July number of this Magazine, the answer would of course be, that it is the wire which is seen in its true place.



The principal thing, however, to which I object in Prof. Challis's paper, is the reasoning by which he establishes his equation (5.). In the figure, $a\,b$ is a very small portion of a wave of light, which in the small time t would be propagated to $c\,d$ if the æther from a to b were moving with the velocity of the æther at b, while, in consequence of the difference in the velocity of the æther at a and b, the disturbance at a is propagated to e. Now Prof. Challis takes $c\,a\,e$ for the angle through which the normal to the wave's front is displaced as the wave passes from $a\,b$ to $e\,d$. But $a\,e$ is only the direction in space along which the disturbance at a is propagated, a direction which has no immediate relation to the normal to the wave, inasmuch as it differs from it by an angle which is of the order of the aberration, the very order of quantities that we are considering. In fact, according to the reasoning in my paper, to which Prof. Challis does not appear to object, I found that the law of aberration does not result from supposing the waves of light to be carried by the moving æther, so long as its motion is taken arbitrary; and in order to explain aberration, I was *compelled* to suppose $u\,dx+v\,dy+w\,dz$ to be an exact differential, at least when the square of the aberration is neglected.

It is evidently immaterial whether we make the construction that Prof. Challis has given, or suppose $e\,f$ to be the position into which the wave $a\,b$ would come at the end of the time t, in consequence of the velocity of propagation combined with the velocity of the æther at a, and suppose that f is brought to d in consequence of the difference of velocity of the æther at a and b. It is easy to show that df is equal and parallel to ce; so that, according to *this* construction, the normal to the wave ought to be displaced by the motion of the æther through the angle fbd from fb to db, which is just the contrary direction to that given by Prof. Challis's construction.

Prof. Challis seems to think that the undulatory theory of light cannot be maintained unless it can be shown that the law of aberration ought to be the actual law, *whatever* may be the motion of the æther. But it is surely sufficient to show that a conceivable kind of motion exists which would lead to the observed law of aberration, provided we have no reason for regarding that sort of motion as improbable. Now even were I to allow that $u\,dx + v\,dy + w\,dz$ cannot, in the case of ordinary fluids, be an exact differential unless the motion is rectilinear, that would not be a fatal objection. For the equations of motion of fluids commonly employed are formed on the hypothesis that the mutual action of two elements of the fluid is normal to the surface which separates them, whereas one of the most remarkable properties of the æther with which we are acquainted, is the great tangential force which it is capable of exerting, in consequence of which the transversal vibrations which constitute light are propagated with such an immense velocity.

VI. *On the Solubility of Oxide of Lead in Pure Water.*
By Lieut.-Col. PHILIP YORKE*.

IN the Philosophical Magazine for August 1834, I published a paper on the action of water and air on lead. Some of the principal results contained in it were confirmed by Bonsdorff in two papers; he found that 7000 parts of pure water free from access of carbonic acid dissolved one of oxide of lead; my experiments gave $\frac{1}{12,000}$th to $\frac{1}{10,000}$th. Since that time two papers have appeared on the same subject, one by Dr. Christison†, and one by Mr. R. Phillips, Jun.‡ The last-named chemist considers that the oxide of lead is not dissolved, but merely mechanically suspended in the water, because the liquid is deprived of the lead by passing it through

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* Communicated by the Chemical Society; having been read May 17, 1845.

† Transactions of the Royal Society of Edinburgh.

‡ Chemical Gazette for Jan. 1, 1845.

a paper filter. It is to this opinion that I propose to direct attention in the present notice.

The fact that the aqueous solution of oxide of lead would not pass through a filter was noticed by me in the paper already referred to; but as the action of tests on the liquid was just what one observes with solutions; as no time allowed for subsidence made any difference in these appearances; as the liquid deposited crystals of oxide of lead not only on the lead but on other bodies; as when decomposed by the voltaic battery it gave metallic lead at the negative pole, and peroxide at the positive; I did not consider that the stoppage of the oxide of lead by the filter was any proof of its not being dissolved. There still, however, remains this question to be answered,—In what way does the paper act in retaining the oxide? and I think that the following experiments afford an answer to the question.

I placed some clean rods of lead in bottles of distilled water loosely stoppered; in this way, after removing the rods of lead, I obtained a clear liquid, which, when tested by sulphuretted hydrogen, gave a deep brown colour. On passing this liquid through a double filter, which had been previously washed with hot distilled water, it appeared to be very nearly deprived of lead: when two or three fluid ounces had passed through, the filters were removed, washed, then immersed in a solution of sulphuretted hydrogen, again washed and dried. Some torn fragments of the filters were then mounted in Canada balsam for examination by the microscope. On examination with powers of from 150 to 400, the fibres of the flax composing the paper were seen to be browned, and in many instances it could be distinctly observed that the colouring substance occupied the interior of the tubular fibre. Now, it is stated by Mr. Crum, in the Philosophical Magazine for April 1844, that cotton wool possesses the power of abstracting the oxide of lead from its solution in lime-water, and that this property is made available in the processes for dyeing cotton with the chromates. I found that on filtering a solution of oxide of lead in lime water through a triple filter, that whereas the original solution gave a deep black when tested by sulphuretted hydrogen, the filtered liquid gave but a pale brown; and it required that the unfiltered liquid should be diluted with thirty times its volume of water to produce the same test as the filtered.

I then tried the effect of mere immersion of the paper in the aqueous solutions before used. A bit of filtering-paper ten inches by two inches was boiled in distilled water and then put into an ounce phial filled with the aqueous solution; after remaining six hours the liquid was poured off and tested: it gave a pale brown, and it required that the liquid which had not been in contact with the paper should be diluted with ten times its volume of water to produce the same tint. This experiment was repeated with a stronger solution of oxide of lead in water, the water was poured off at the end of four hours; it then gave a pale brown, and it required that the original liquid should be diluted with four times its bulk of water to produce the same tint. A fresh portion of the same solution was then poured on the same paper and left for a night; then, on testing, the liquid gave a brown tint, barely perceptible, and it required that the original liquid should be diluted with from fifteen to twenty times its volume of water to produce the same.

From these experiments it is clear that the effect in question is dependent on a power possessed by the paper in common with several other porous bodies and organised fibres, of separating certain substances from their solutions, a power sufficiently well known, though little understood*. In considering this view of the subject in the present instance, there is a circumstance of some practical importance which it would appear ought to follow, viz. that after the fibres of the paper had been saturated with the oxide of lead, then this substance should pass through in solution. To ascertain whether this was the case I made the following experiments.

I obtained a strong aqueous solution of oxide of lead by immersing slips of clean lead in about three quarts of distilled water, contained in a two-necked bottle, through which oxygen gas was passed and maintained in contact with, under a slight pressure. In this manner I procured a solution which when quite clear yielded $\frac{1}{7300}$th of ignited oxide of lead. A filter of paper rather less than $\frac{1}{200}$th of an inch thick and four inches in diameter was prepared and washed; then, by fitting into one of the two necks of the bottle a siphon with equal legs, so as to resemble Gay-Lussac's apparatus for washing filters (except that I used a contrivance to prevent the necessity of the air supplied to the bottle from bubbling through the solution), I was enabled to allow the filtration to go on with considerable regularity for many hours. The first portion of liquid which passed through gave a pale brown when tested; when nine fluid ounces had passed through the effect was the same as at first, and a portion (*a*) was reserved for future comparison. When forty fluid ounces had passed through, the

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* The effective filter mentioned by Dr. Clark is formed of well-washed sand, and has been in use during twelve months without any apparent diminution of power.

liquid, which was quite clear, gave a much darker tint with the test than any which had previously been obtained in the experiment. It gave a tint about equal to that given with the unfiltered liquid when diluted with its own volume of water; while it (*i. e.* the last filtered portion) required to be diluted with twice its volume of water to produce the same tint as that given by the reserved filtered portion (*a*). The liquid now passed through the filter very slowly; it was tested again, when eight more fluid ounces had passed through, with the same result as before, except that the tint was a trifle darker.

This experiment sufficiently shows that the effect contemplated does occur, and that it would be unsafe to trust to the action of a filter to separate oxide of lead from water for an unlimited time.

VII. *Equations for the Determination of the Motion of a Disturbed Planet by means of* M. Hansen's *Altered Time.* *By the* Rev. BRICE BRONWIN*.

THE theory of M. Hansen on Lunar and Planetary Perturbations, owing to the two times τ and t which it contains, is attended with many difficulties, and is very perplexing. His results I think are in an advantageous form; but perhaps they might be obtained more easily by the equations given in this paper, which are referred to the plane of the orbit as if it were a fixed plane, because I have proved that so referred they are true. [See this Magazine for November 1844, and also the Cambridge Mathematical Journal, No. 24.]

The equation

$$\frac{h^2}{\mu r}=1+e\cos(v-\pi)=1+e\cos\pi\cos v+e\sin\pi\sin v$$

is true for the disturbed orbit; h, e, and π having their known variable values depending on the disturbing force. If h_0, e_0, and π_0 be the values of these quantities when the disturbing force is made to vanish, then

$$e\cos\pi=e_0\cos\pi_0+\int(\cos\pi\,de-e\sin\pi\,d\pi),$$

$$e\sin\pi=e_0\sin\pi_0+\int(\sin\pi\,de+e\cos\pi\,d\pi),$$

$$h^2=h_0^2+2\int h\,dh.$$

These values substituted in the above equation give

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* Communicated by the Author.

$$\frac{h_0^2}{\mu r} = 1 + e_0 \cos(\upsilon - \pi_0) - \frac{2}{\mu r} \int h\,dh$$
$$+ \cos\upsilon \int (\cos\pi\,de - e\sin\pi\,d\pi) + \sin\upsilon \int (\sin\pi\,de + e\cos\pi\,d\pi).$$

Let the constant quantities λ and ϱ be the same functions of a constant time τ which υ and r are of t; then putting the former in place of the latter, we may put them under the sign of integration, changing τ into t after the integrations are performed. This will change the last equation into

$$\frac{h_0^2}{\mu r} = 1 + e_0 \cos(\upsilon - \pi_0) - \frac{2}{\mu} \int \frac{h\,dh}{\varrho} + \int \cos(\lambda - \pi)\,de$$
$$+ \int e \sin(\lambda - \pi)\,d\pi.$$

But
$$\int h\,dh = -\int \frac{d\mathrm{R}}{d\upsilon} h\,dt,$$

$$de = -\frac{hdt}{\mu}\sin(\upsilon - \pi)\frac{d\mathrm{R}}{dr} - \left(\frac{2hdt}{\mu r}\cos(\upsilon - \pi) + \frac{dr}{\mu}\sin(\upsilon - \pi)\right)\frac{d\mathrm{R}}{d\upsilon},$$

$$d\pi = \frac{hdt}{\mu e}\cos(\upsilon - \pi)\frac{d\mathrm{R}}{dr} - \left(\frac{2hdt}{\mu re}\sin(\upsilon - \pi) - \frac{dr}{\mu e}\cos(\upsilon - \pi)\right)\frac{d\mathrm{R}}{d\upsilon}.$$

The coefficients of $\frac{d\mathrm{R}}{d\upsilon}$ are put under the above form for convenience. Substituting these values, we find

$$\frac{1}{r} = \frac{\mu}{h_0^2} + \frac{\mu e_0}{h_0^2}\cos(\upsilon - \pi_0) + \frac{1}{h_0^2}\int \frac{d\mathrm{R}}{dr}\sin(\lambda - \upsilon)\,h\,dt$$
$$+ \frac{1}{h_0^2}\int \frac{d\mathrm{R}}{d\upsilon}\left\{\sin(\lambda - \upsilon)\,dr - \frac{2hdt}{r}\cos(\lambda - \upsilon) + \frac{2hdt}{\varrho}\right\}.$$

To abridge, let this be written

$$\frac{1}{r} = \frac{\mu}{h_0^2} + \frac{\mu e_0}{h_0^2}\cos(\upsilon - \pi_0) + \mathrm{P}.$$

But if βt be the progression of the apse,

$$\cos(\upsilon - \pi_0) = \cos(\upsilon - \beta t - \pi_0 + \beta t) = \cos(\upsilon - \beta t - \pi_0)$$
$$- \beta t \sin(\upsilon - \beta t - \pi_0),$$

neglecting higher powers of βt. Therefore

$$\frac{1}{r} = \frac{\mu}{h_0^2}\{1 + e_0\cos(\upsilon - \beta t - \pi_0) - e_0\beta t\sin(\upsilon - \beta t - \pi_0)\} + \mathrm{P}.$$

Terms similar to the above, containing t in their coefficients, will arise from the development of P; and β must be so determined as to drive them out, which will be easily done. We may always neglect terms involving the higher powers of t.

Let r_1 and v_1 be the same functions of the new time ζ, and the constants h_1, e_1, ϖ_0, ϵ_0 which r and v are, when there is no disturbing force, of the time t and the constants h_0, e_0, ϖ_0, and ϵ_0. Also let $v = v_1 + \beta t$. We shall have

$$\frac{1}{r_1} = \frac{\mu}{h_1^2}\{1 + e_1 \cos(v_1 - \varpi_0)\}.$$

We shall not with M. Hansen find the log of r, and therefore shall make $\frac{1}{r} = \frac{1}{r_1} + p$. Substituting this value, we shall easily find

$$p = \mu\left(\frac{1}{h_0^2} - \frac{1}{h_1^2}\right) + \mu\left(\frac{e_0}{h_0^2} - \frac{e_1}{h_1^2}\right)\cos(v_1 - \varpi_0)$$
$$- \frac{e_0 \beta t}{h_0^2}\sin(v_1 - \varpi_0) + \mathrm{P}.$$

Whence p is of the order of the disturbing force, and it has the advantage of requiring only one integration.

In virtue of the supposed equation

$$r_1^2\, d v_1 = h_1\, d\zeta, \text{ or } \frac{d v_1}{d\zeta} = \frac{h_1}{r_1^2},$$

we have

$$\frac{dv}{dt} = \frac{d v_1}{dt} + \beta = \frac{d v_1}{d\zeta}\frac{d\zeta}{dt} + \beta = \frac{h_1}{r_1^2}\frac{d\zeta}{dt} + \beta.$$

This value, substituted in the known equation

$$\frac{dv}{dt} = \frac{1}{r^2}\left(h_0 - \int \frac{d\mathrm{R}}{dv}\, dt\right)$$

gives

$$\frac{d\zeta}{dt} = (1 + r_1 p)^2\left(\frac{h_0}{h_1} - \frac{1}{h_1}\int\frac{d\mathrm{R}}{dv}\, dt\right) - \frac{\beta r_1^2}{h_1}.$$

Of the four quantities h_0, h_1, e_0, e_1, two are to be found in terms of the others, which will be arbitraries of the theory; and the mode of determining them will be obvious after the development is effected.

Putting ϕ for the latitude, i for the inclination, ϑ and θ for the longitude of the node on the plane of the orbit and on the fixed plane, we have

$$\vartheta = \int \cos i\, d\theta,\ \sin\phi = \sin i \sin(v - \vartheta)$$
$$= \sin i(\cos\vartheta \sin v - \sin\vartheta\cos v),$$

$$\sin i \cos\vartheta = \sin i_0 \cos\vartheta_0 + \int(\cos i \cos\vartheta\, di - \sin i \sin\vartheta\, d\vartheta),$$

$$\sin i \sin \vartheta = \sin i_0 \sin \vartheta_0 + \int (\cos i \sin \vartheta \, di + \sin i \cos \vartheta \, d\vartheta).$$

Substituting these values, and changing υ into λ, and putting it under the sign of integration, we obtain

$$\sin \phi = \sin i_0 \sin (\upsilon - \vartheta_0) + \int \sin (\lambda - \vartheta) \cos i \, d i$$
$$- \int \cos (\lambda - \vartheta) \sin i \, d \vartheta.$$

But $$d i = \frac{d t}{h \sin i} \, \frac{d \mathrm{R}}{d \theta}, \quad d \vartheta = - \frac{\cos i \, d t}{h \sin i} \, \frac{d \mathrm{R}}{d i}.$$

These values being put in the above, it will become

$$\sin \phi = \sin i_0 \sin (\upsilon - \vartheta_0) + \int \frac{\cos i \, d t}{h \sin i} \, \frac{d \mathrm{R}}{d \theta} \sin (\lambda - \vartheta)$$
$$+ \int \frac{\cos i \, d t}{h} \, \frac{d \mathrm{R}}{d i} \cos (\lambda - \vartheta).$$

To abridge, we may write this

$$\sin \phi = \sin i_0 \sin (\upsilon - \vartheta_0) + \mathrm{Q},$$

or $$\sin \phi = \sin i_0 \sin (\upsilon + \gamma t - \vartheta_0) - \gamma t \sin i \cos (\upsilon + \gamma t - \vartheta_0) + \mathrm{Q}.$$

Here γt is the regression of the node, and γ is to be so determined as to take away from Q the terms having t in their coefficients.

If in the equations

$$\sin \phi = \sin i \sin (\upsilon - \vartheta),$$
$$\frac{d \sin \phi}{d t} = \sin i \cos (\upsilon - \vartheta) \frac{d \upsilon}{d t},$$

we change ϕ, υ, i, and ϑ into $\phi_0 + \delta \phi$, $\upsilon_0 + \delta \upsilon$, $i_0 + \delta i$, and $\vartheta_0 + \delta \vartheta$, $\delta \phi$, &c. being the parts depending on the disturbing force; and if we expand, taking account of the first power only of $\delta \phi$, &c., we shall find equations of the form

$$\delta i = \mathrm{A} \, \delta \phi + \mathrm{B} \, \delta \upsilon,$$
$$\delta \vartheta = \mathrm{C} \delta \phi + \mathrm{D} \delta \upsilon.$$

From these we may correct the values of i and ϑ or θ by means of the corrections of ϕ and υ due to the disturbing force, and in doing so we may take account of higher powers of $\delta \phi$, &c.

I think the development of the preceding equations would be attended with much less difficulty and perplexity than the development of M. Hansen's. I have not noticed the reduction to a fixed plane, but must refer for that to the Number of this Magazine for February 1844, where I have given equations particularly adapted to the lunar theory, and leading to results expressed in terms of the true elliptic longitude.

I see Mr. Cayley has amended his paper of November 1844. If he would amend it a little further, it would not be amiss. He has now made p a prime number instead of any odd one: θ is made of the second instead of the general form. In the expression of $\phi_{,} x$, or rather $\phi\, x'$, he should have had the transformed function $x' = \frac{x}{\beta}$, not x, β a function of θ. Moreover,

$$\omega' = \frac{1}{p\beta}(\alpha\omega + \beta\upsilon), \quad \upsilon' = \frac{1}{p\beta}\alpha'\omega + \beta'\upsilon).$$

Some other amendments are greatly wanted. When x has a determinate value, x' should have one also, since β is a known function of θ. And if we know what value to assign to x, we should have the value of x', which is the complete function. I have no room to enlarge, and as Mr. Cayley has done nothing which invalidates what I have done, it is unnecessary.

Gunthwaite Hall, Dec. 12, 1845.

VIII. *On some Points in the Meteorology of Bombay.*

By Lieut.-Colonel SABINE, *R.A., F.R.S.*[*]

[With a Plate.]

IN a communication which I had the honour to make to the Section at the York meeting of the British Association, on the subject of the meteorological observations made at Toronto in Canada in the years 1840 to 1842[†], I noticed some of the advantages which were likely to result to the science of meteorology, from the resolution of the barometric pressure into its two constituents of aqueous and of gaseous pressure. It was shown that when the constituents of the barometric pressure at Toronto were thus disengaged from each other and presented separately, their annual and diurnal variations exhibited a very striking and instructive accordance with the annual and diurnal variations of the temperature. The characteristic features of the several variations when projected in curves were seen to be the same, consisting in all cases of a single progression, having one ascending and one descending branch; the epochs of maxima and minima of the pressures being the same, or very nearly the same, with those of the maxima and minima of temperature; and the correspondence in other respects being such as to manifest the existence of a very intimate connexion between the periodical variations of the temperature, and those of the elastic forces of the air and vapour. The curve of gaseous pressure was inverse in respect to the other two; that is to say, as the temperature increased the elastic force of the vapour increased also, but that of the air diminished, and *vice versâ*; and this was the case both in the annual and the diurnal variations.

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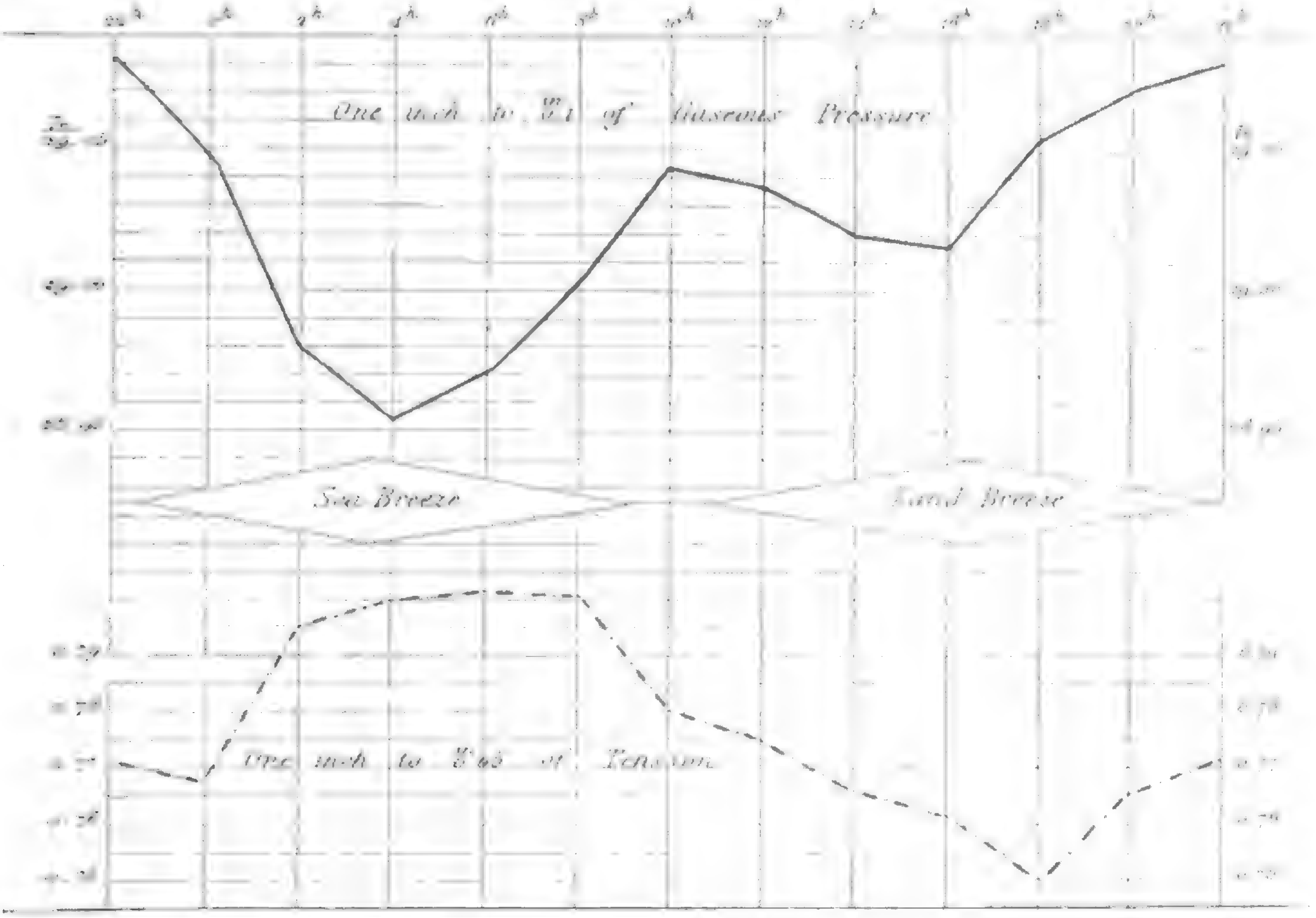
[*] Communicated to the Mathematical and Physical Section of the British Association for the Advancement of Science, at the Cambridge Meeting, 1845.

[†] See Phil. Mag., vol. xxvi. p. 94.

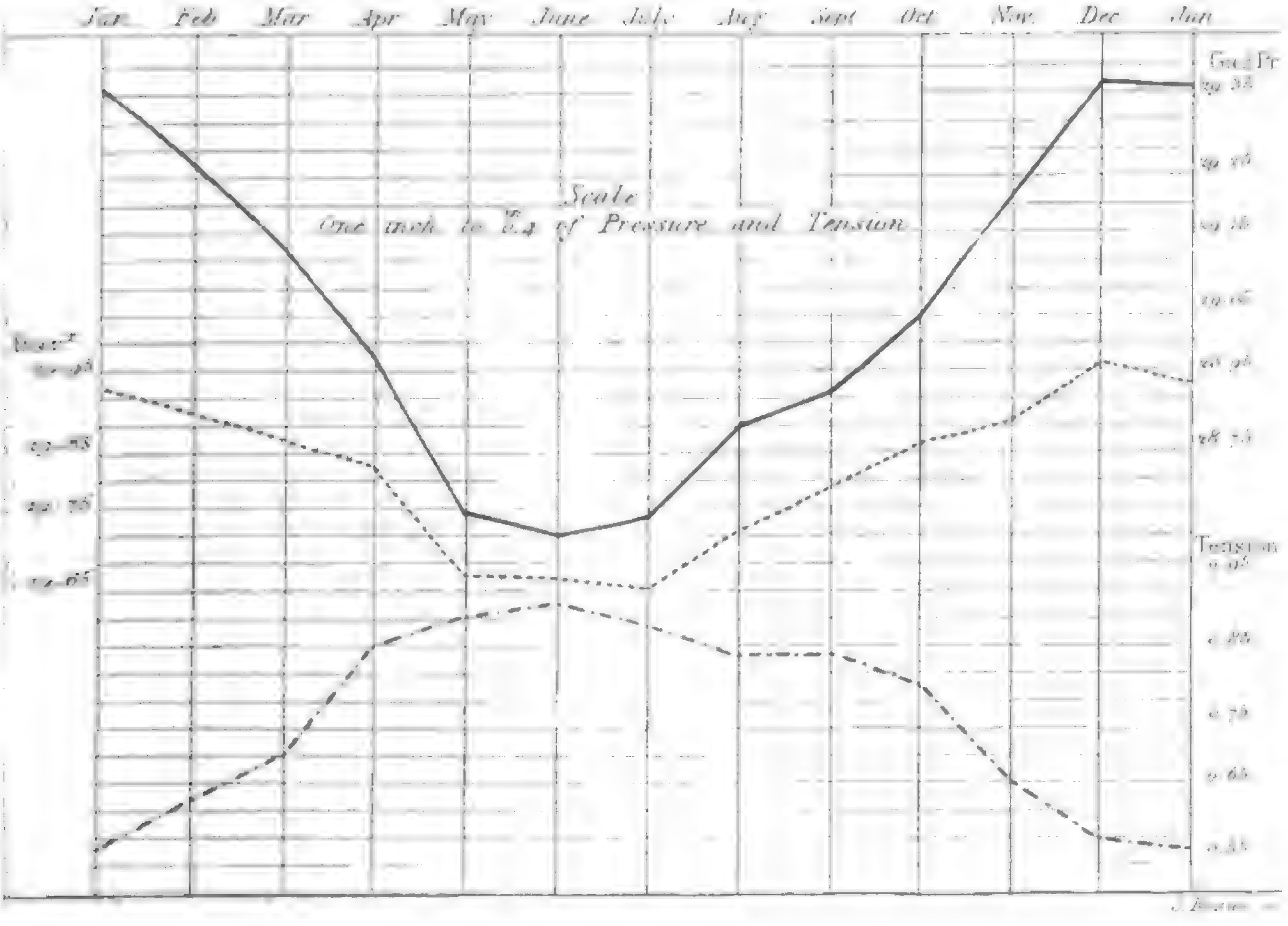
BOMBAY 1843. *Diurnal Variations.*

Gaseous Pressure ——— *Tension of Vapour* -.-.-.-



Annual Variations

Gaseous Pressure ——— *Barom.c Pressure* .... *Tension of Vapour* -.-.-.



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Such being the facts, I endeavoured to show, in the case of the diurnal variations, that the correspondence of the phænomena of the temperature and gaseous pressure might be explained, in accordance with principles which had been long and universally admitted in the interpretation of other meteorological phænomena, by the suppositions,—of an extension in height and consequent overflow in the higher regions of the atmosphere of the column of air over the place of observation, during the hours of the day when the surface of the earth was gaining heat by radiation,—and of a contraction of the column during the hours of diminishing temperature, and consequent reception of the overflow from other portions of the atmosphere, which in their turn had become heated and elongated.

According to this explanation there should exist, during the hours of the day when the temperature is increasing,—1st, an *ascending current* of air at the place of observation, of which the strength should be measured by the amount of the increments of temperature corresponding to given intervals of time; and 2nd, a *lateral influx of air at the lower parts of the column*, of proportionate velocity, constituting a *diurnal variation in the force of the wind* at the place of observation, which should also correspond with the variations of the temperature in the epochs of its maximum and minimum, and intermediate gradation of strength. The anemometrical observations at Toronto were shown to be in agreement with the view which had been then taken, confirming the existence of a diurnal variation in the force of the wind, corresponding in all respects with the variation of the temperature.

Admitting the explanation thus offered to be satisfactory in regard to the diurnal variations, it was obvious that the correspondence of the annual variations of the temperature and pressures might receive an analogous explanation.

A comparison of the results of the observations at Toronto with those of the observations of M. Kreil at Prague in Bohemia, (published in the *Mag. und Met. Beob, zu Prag*, and in the *Jahrbuch für Prag*. 1843,) showed that the characteristic features of the periodical variations at Toronto were not peculiar to that locality, but might rather be considered as belonging to stations situated in the temperate zone and in the interior of a continent. The annual and diurnal variations at Prague were also single progressions, and the same correspondence was observable between the variations of the temperature and of the gaseous pressure.

The publication of the volume of magnetical and meteorological observations made at Greenwich in 1842, which took place shortly after the meeting of the Association at York, enabled me to add a postscript to the printed statement of my communication in the annual volume of the Association Reports, showing the correspondence of the results at Greenwich with the relations which had been found to exist in the periodical march of the phænomena at Toronto and at Prague.

From the concurrence of these three stations, it was obvious that a considerable insight had been obtained into the laws which regulate the periodical variations in the temperate zone, and into the sequence of natural causes and effects, in accordance with which the annual and diurnal fluctuations of the elastic forces of air and vapour at the surface of the earth depend on the variations of temperature: and from these premises it was inferred, that the normal state of the diurnal variations of the pressures of the air and vapour and of the force of the wind, in the temperate zone, might be regarded as that of a single progression with one maximum and one minimum, the epochs of which should nearly coincide with those of the maximum and minimum of temperature*.

That exceptions should be found to this state of things in particular localities in the temperate zone was far from being impro-

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* Since this communication was read at Cambridge I have received from M. Dove a copy of a paper read to the Academy of Berlin, entitled 'Ueber die periodischen Aenderungen des Druckes der Atmosphäre im Innern der Continente,' in which the remarkable facts are stated, that at Catherinenbourg and Nertchinsk (on the mean of several years), and at Barnaoul (in the years 1838 and 1840), the mean diurnal *barometric* curve itself exhibits but one maximum and one minimum in the twenty-four hours; the maximum coinciding nearly with the coldest, and the minimum with the hottest hours of the day. At these stations therefore, and in the years referred to, the forenoon maximum disappeared, and the barometric curve assimilated in character to the curve of the dry air in other places in the temperate zone. These stations are situated far in the interior of the greatest extent of dry land on the surface of our globe, and at a very great distance from an expanse of water, from whence vapour can be supplied. The diminished pressure of the dry air produced by the ascending current and overflow as the temperature of the day increases, is not therefore compensated by an increased elasticity of vapour, and the curve of the diurnal variation of the barometer approximates to the form assumed when the elasticities of the vapour at the several hours of observation are abstracted. This assimilation in character of the barometric and (inferred) gaseous curves, which is thus found to take place in cases where, from natural causes, the influence of the vapour is greatly lessened, appears a confirmation of the propriety of separating the effects of the elastic forces of the dry air and vapour in their action on the barometer.

M. Dove considers that the single progression of the diurnal barometric curve, which takes place at the three Asiatic stations referred to in this note, is characteristic of a true continental climate. It is, without doubt, characteristic of an extreme climate, and as such is highly instructive. There appears reason to doubt whether an extreme climate of corresponding character exist at all in the temperate latitudes of the continent of America.

If, however, we examine the record of the observations made hourly in the year 1842 at Catherinenbourg, Barnaoul and Nertchinsk, in the 'Annuaire Magnétique et Météorologique de Russie,' we find that at Catherinenbourg in that year the barometer exhibits a double progression, but that the morning maximum, which occurs at the observation hour of $8^h\ 22^m$ A.M., exceeds the antecedent minimum only by a quantity less than 0·003 in. At Barnaoul there is also a double progression in the barometric mean in that year, the morning maximum being still small, and taking place between the observation hours of $9^h\ 54^m$ and $10^h\ 54^m$ A.M. At Nertchinsk also there is a morning maximum occurring at the observation hour of $9^h\ 17^m$ A.M. In all the three cases the double progression shown by the barometer disappears wholly in the curve of the dry air, which curve exhibits at these three stations, as well as at Toronto, Prague and Greenwich, but one maximum and one minimum in the twenty-four hours. At the three stations of extreme dryness cited by M. Dove, therefore the vapour was still sufficient to impart, in the year 1842 at least, a double progression to the diurnal variation of the barometer; but the hour of the morning maximum was earlier than where the increase of vapour, as the day advances, is greater.

bable; it could not be expected that the influences of temperature should always be so simple and direct as they appeared to be at Toronto; and a more complex aspect of the phænomena might particularly be looked for, where a juxtaposition should exist of columns of air resting on surfaces differently affected by heat (as those of land and sea), and possessing different retaining and radiating properties. In such localities *within the tropics*, the well-known regular occurrence of land and sea breezes for many months of the year made it obvious that a double progression in the diurnal variation of the force of the wind must exist, and rendered it highly probable that a double progression of the gaseous pressure would also be found. It was therefore with great pleasure that I received, through the kindness of Dr. Buist, a copy of the monthly abstracts of the two-hourly meteorological observations, made under that gentleman's superintendence at the observatory at Bombay in the year 1843; accompanied by a copy of his meteorological report for that year, possessing a particular value, in the full account which it gives of the periodical variations of the wind, and in the means which it thereby affords of explaining the diurnal variation of the gaseous pressure. This pressure presents at Bombay an aspect at first sight more complex than at the three above-named stations in the temperate zone, but I believe it to be equally traceable to variations of the temperature, and to furnish a probable type of the variations at intertropical stations similarly circumstanced in regard to the vicinity of the sea.

The observatory at Bombay is situated on the island of Colabah, in N. lat. 18° 54′ and E. long. 72° 50′ at an elevation of thirty-five feet above the level of the sea. In the copy of the observations received from Dr. Buist, the monthly abstracts are given separately for each month, of the standard thermometer,—of the wet thermometer, and of its depression below the dry,—and of the barometer, In Table I. I have brought in one view the thermometrical and barometrical means at every second hour, and the mean tension of the vapour and mean gaseous pressure at the same hours. The tension of the vapour at the several observation hours has been computed from the *monthly means*, at the same hours, of the wet thermometer and of its depression below the dry thermometer. The values are consequently somewhat less than they would have been, had the tension been computed from each individual observation of the wet and dry thermometers, and had the mean of the tensions thus obtained been taken as the value corresponding to the hour. The difference is however so small, that for the present purpose it may be regarded as quite insignificant. It would not amount in a single instance to the hundredth part of an inch; and as in every instance the difference would be in the same direction, the *relative* values, which are those with which we are at present concerned, would be scarcely sensibly affected. The pressures of the dry air (or the gaseous pressures) are obtained by deducting the tension of the vapour from the whole barometric pressure.

Table I.

Bombay, 1843.—Mean Temperature, Mean Barometric Pressure, Mean Tension of Vapour, and Mean Gaseous Pressure at every second hour.

Hours of Mean Bombay Time. Astronomical Reckoning.	Temperature.	Barometer.	Tension of Vapour.	Gaseous Pressure.
	°	in.	in.	in.
18	78·4	29·805	0·750	29·055
20	79·6	29·840	0·766	29·074
22	81·8	29·852	0·771	29·081
0	83·2	29·817	0·768	29·049
2	84·1	29·776	0·795	28·981
4	83·9	29·755	0·800	28·955
6	82·3	29·774	0·802	28·972
8	81·2	29·806	0·801	29·005
10	80·3	29·825	0·780	29·045
12	79·8	29·809	0·775	29·034
14	79·4	29·786	0·766	29·020
16	78·9	29·778	0·761	29·017
Mean of the year ...	81·1	29·802	0·780	29·022

The sun is vertical at Bombay twice in the year, viz. in the middle of May and towards the end of July. The rainy season sets in about the commencement of June (in 1843 on the 2nd of June), and terminates in August, but with heavy showers of no long duration continuing into September. During the rainy season, and in the month of May which immediately precedes it, the sky is most commonly covered with clouds, by which the heating of the earth by day, and its cooling at night by radiation, are impeded, and the range of the diurnal variation of the temperature is greatly lessened in comparison with what takes place at other times in the year. The strength of the land and the sea breezes in those months is also comparatively feeble, and on many days the alternation of land and sea breeze is wholly wanting. During the months of November, December, January and February, the diurnal range of the temperature is more than twice as great as in the rainy season, and the land and sea breezes prevail with the greatest regularity and force.

In addition to the monthly tables, we may therefore advantageously collect in one view, for purposes of contrast, the means of the months of May, June, July and August, as the season when the sky is generally clouded,—and of the months of November, December, January and February, as the season of opposite character, when the range of the diurnal temperature is greatest, and the land and sea breezes alternate regularly, and blow with considerable strength. These seasons are contrasted in Table II.

If we direct our attention to the diurnal variations, commencing with those of the temperature, we find them exhibiting a single progression, having a minimum at 18^h and a maximum at 2^h; the average difference between the temperature at 18^h and 2^h being 7°·77 in the clear season, 3°·71 in the clouded season, and 5°·7 on the mean of the whole year.

When however we direct our attention to the gaseous pressure, we perceive, very distinctly marked, the characters of a double progression, having one maximum at 10^h and another at 22^h; one minimum at 4^h and another at 16^h. The double progression is exhibited both in the clouded and in the clear seasons, with a slight difference only in the hours of maxima; the principal maximum in the cloudy season being at 20^h instead of 22^h, and the inferior maximum in the clear season being at 12^h instead of 10^h. The range of the diurnal variation, like that of the temperature, is more than twice as great in the clear as in the clouded season, marking distinctly the connexion subsisting between the phænomena of the temperature and of the gaseous pressure.

TABLE II.

Bombay, 1843.—Comparison of the Temperature and of the Gaseous Pressure in the months of May, June, July and August, when the sky is usually covered with clouds; and in November, December, January and February, when the sky is usually clear.

Hours of Mean Time at Bombay. Astronomical Reckoning.	Temperature.		Gaseous Pressure.	
	November, December, January and February.	May, June, July and August.	November, December, January and February.	May, June, July and August.
	°	°	in.	in.
18	74·1	81·9	29·344	28·782
20	75·3	83·1	29·368	28·806
22	78·1	84·3	29·391	28·798
0	80·8	85·1	29·353	28·782
2	81·9	85·6	29·230	28·746
4	81·7	85·4	29·195	28·724
6	79·6	84·3	29·199	28·740
8	78·4	83·4	29·248	28·754
10	76·9	83·0	29·308	28·800
12	76·2	82·7	29·316	28·775
14	75·7	82·6	29·295	28·754
16	74·9	82·2	29·285	28·753
Means	77·8	83·7	29·298	28·763

If we now turn our attention to the phænomena of the direction and force of the wind, we find by Dr. Buist's report, that for 200 days in the year there is a regular alternation of land and sea breezes. The land breeze springs up usually about 10^h, or between 10^h and 14^h, blows strongest and freshest towards daybreak, and gradually declines until about 22^h, at which time the direction of the aërial currents changes, and there is generally a lull of an hour or an hour and a half's duration. The sea breeze then sets in, the ripple on the surface of the water indicating its commencement being first observed close in shore, and extending itself gradually out to sea. The sea breeze is freshest from 2^h to 4^h, and progressively declines in the evening hours.

The diurnal variation in the force of the wind during these 200 days is therefore obviously a double progression, having two maxima and two minima; one maximum at or near the hottest, and the other at or near the coldest hour of the day,—being the hours when the difference of temperature is greatest between the columns of air which rest respectively on the surfaces of land and sea; and two minima coinciding with the hours, when the surface temperature over the land and over the sea approaches nearly to an equality.

In the remaining portion of the year the diurnal range of the temperature is most frequently insufficient to produce that alternation in the direction of the wind, which prevails uninterruptedly during the larger portion. There appears however to have been only one month, viz. July, in the year 1843, in which there were not some days in which the alternation of land and sea breezes was perceptible. The causes which produce the alternation are not therefore wholly inoperative, though the effects are comparatively feeble during the clouded weather which accompanies the south-west monsoon*.

If we now view together the diurnal variations of the wind and gaseous pressure, as shown in Plate I., we find a minimum of pressure coinciding with the greatest strength of the sea breeze; a second minimum of pressure coinciding with the greatest strength of the land breeze; and a maximum of pressure at each of the periods when a change takes place in the direction of the aërial currents; or, otherwise stated, we find a decrease of pressure coincident with the increase of strength both of the land and sea breezes, and an increase of pressure coincident with their decline in strength.

The facts thus stated appear to me to admit of the following explanation:—the diminution of pressure which precedes the minimum at 4^h is occasioned by the rarefaction and ascent of the column during the heat of the day, and its consequent overflow in the higher regions of the atmosphere, which is but partially counterbalanced in the forenoon by the influx of the sea breeze at the lower part of the column. Shortly after the hottest hour is passed, the overflow above and the supply below become equal in amount, and the diminution of pressure ceases. As the temperature falls towards evening, the column progressively contracts, when the influx from the sea breeze more than counterbalances the overflow, and the pressure again increases until a temporary equilibrium is restored, when the sea breeze ceases and the pressure is stationary.

As the night advances, the air over the land becomes colder than over the sea; the length of the column over the land contracts, and the air in its lower part becomes denser than in that over the sea: an interchange then commences of an opposite character to that which prevailed during the day. The outward flow is now from the *lower* part of the column, or from the land towards the sea,

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* There are no data in Dr. Buist's report from which the diurnal variation in the force of the wind may be judged of in the days during the south-west monsoon, when no alternation takes place in its direction. It would seem probable that on such days the variation should be a single progression, weakest towards daybreak, and strongest about the hottest hour of the day.

causing the pressure to diminish over the land; it continues to do so until towards daybreak, when the strength of the land breeze is greatest, because the air over the land is then coldest in comparison with that over the sea. As the sun gains in altitude and the temperature of the day advances, the land heats rapidly; the density of the air over the land and sea returns towards an equality; the land breeze declines in strength, and the drain from the lower part of the column ceases to counterbalance the overflow which the land column is at the same time receiving in the higher regions; the pressure consequently having attained a second minimum at or near the hour of the greatest disproportion of temperature, again increases until the temperature and height of the column over the sea and land are the same, and the pressure again becomes stationary. But now the rarefaction of the column over the land continuing, its increase in height above the less heated column with which it is in juxtaposition, and its consequent overflow, occasion the pressure to decrease until the minimum at 4 o'clock is reached.

We have thus therefore at Bombay a *double progression of the diurnal variation of the gaseous pressure*; the principal minimum occurring at 4 o'clock in the afternoon, occasioned by an overflow from the column in the higher regions of the atmosphere; and the second minimum occurring at 18^h, occasioned by an efflux from the lower part of the column. The first minimum is similar to that which has been shown to take place at Toronto, Prague and Greenwich, and is similarly explained: the second minimum, which does not take place at the three above-named stations, is owing to the juxtaposition of the columns of air over the sea and land, which differ in temperature, and therefore in density and height, in consequence of their resting respectively on surfaces which are differently affected by heat.

Plate I. shows the curve of the gaseous pressure, and the curve of the elastic force of the vapour; and between them is placed a diagram illustrating the hours of prevalence and of the greatest strength of the land and sea breezes. At Toronto and at Greenwich the diurnal curve of the vapour is a single progression, having its maximum at or near the hottest hour of the day, and its minimum at or near the coldest hour. We perceive in the Plate which represents the phænomena at Bombay, the modification which takes place in consequence of the supply of vapour brought in by the sea breeze continuing until a late hour in the evening, and prolonging the period during which the tension is at or near its maximum. The minimum occurs as usual at or near the hour of the coldest temperature.

If, then, the explanation which I have offered to the Section, of the physical causes which produce the diurnal variation of the gaseous pressure at Bombay, be correct, the diurnal variation of the barometric pressure occurring there is also explained, since it is simply the combination of the two elastic forces of the air and of the vapour.

The solution of the problem of the diurnal variation of the barometer is therefore obtained by the resolution of the barometric pressure into its constituent pressures of vapour and air; since the physical causes of the diurnal variation of the component pressures have been respectively traced to the variations of temperature produced in the twenty-four hours by the earth's revolution on its axis, and to the different properties possessed by the material bodies at the surface of the globe in respect to the reception, conveyance, and radiation of heat.

Annual variation.—We now proceed to the annual variations, which are shown in the subjoined table.

TABLE III.

1843.	Temperature.	Vapour Pressure.	Gaseous Pressure.	Barometer.	Humidity.	Monthly Means greater (+) or less (—) than the Annual Means.		
						Temperature.	Vapour Pressure.	Gaseous Pressure.
January ...	76°·4	0°·578	29°·352	29°·930	67°	−4°·7	−0°·202	+0°·329
February ...	77·7	0·648	29·246	29·894	71	−3·4	−0·132	+0·223
March	79·7	0·710	29·128	29·838	74	−1·4	−0·070	+0·105
April	84·2	0·853	28·961	29·814	76	+3·1	+0·073	−0·062
May	85·9	0·921	28·743	29·664	78	+4·8	+0·141	−0·280
June.........	85·4	0·935	28·718	29·653	80	+4·3	+0·155	−0·305
July	82·1	0·896	28·737	29·633	85	+1·0	+0·116	−0·286
August	81·2	0·859	28·869	29·728	84	+0·1	+0·079	−0·154
September..	81·1	0·859	28·920	29·779	84	0·0	+0·079	−0·103
October ...	82·2	0·819	29·026	29·845	78	+1·1	+0·039	+0·003
November ..	80·5	0·675	29·213	29·888	67	−0·6	−0·105	+0·190
December ..	76·6	0·592	29·368	29·960	67	−4·5	−0·188	+0·345
	81·1	0·780	29·023	29·803	76			

We here perceive that the leading features of the phænomena are clearly analogous to those which have been seen to present themselves at Toronto, Prague and Greenwich; viz. a correspondence of the maximum of vapour pressure and minimum of gaseous pressure with the maximum of temperature,—and of the minimum of vapour pressure and maximum of gaseous pressure with the minimum of temperature; and a progressive march of the three variations from the minimum to the maximum, and back to the minimum again. The epochs, or turning-points of the respective phænomena, are not in every case strictly identical; but their connexion, which is the subject immediately before us, is most obvious.

We have thus a further illustration of the universality of the principle of the dependence of the regular periodical variations, annual as well as diurnal, of the pressures of the dry air and of the vapour, on those of the temperature*.

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* In the tropics and in the temperate zone the heat of summer produces and accompanies a low gaseous pressure, and the cold of winter a high gaseous pressure. When we consider how large a portion of the northern hemisphere is occupied by land, which becoming greatly heated in summer rarefies the superincumbent atmosphere, causing it to overtop the adjacent less heated masses, and to overflow them, we should be led to expect that in parts of the Arctic Circle situ-

The humidity exhibits also a single progression; but may perhaps be rather characterized as evidencing a very dry season from November to February, and a very humid one from June to Septem-

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ated to the north of the great continents, the gaseous pressure should be *increased* in summer, and that the curve of annual variation should become the converse of what it is in the lower latitudes. It appears from the meteorological observations made in 1843 by Messrs. Grewe and Cole, and presented to the British Association at the York meeting by Dr. Lee, that such is the case at Alten, near the north cape of Europe. The barometer and thermometer were observed three times a day, from October 1842 to December 1843 inclusive. The hours of observation were 9 A.M., 3 P.M. and 9 P.M. No hygrometric observations were made, but we are able to infer the approximate tension of the vapour from the record of the thermometer. The quarterly means of the barometer and thermometer in 1843 are as follows; the barometer being reduced to the level of the sea, and corrected for gravity:—

	Barometer.	Thermometer.
	in.	°
December, January, February......	29·375	24 F.
March, April, May	29·948	27·7
June, July, August	29·905	52·4
September, October, November ...	29·716	34·2
Mean of the year	29·736	34·6

Assuming the humidity in each quarter of the year to be 75, or the vapour to be in each case three-fourths of that required for saturation at the respective temperatures, we should have the following gaseous pressures:—

	in.
December, January, February	29·257
March, April, May	29·804
June, July, August	29·616
September, October, November, December ...	29·566
	29·561

It appears therefore that in the six summer months the mean barometric pressure exceeded that of the winter months by 0·381 inch; and the mean gaseous pressure of summer exceeded that of winter by about 0·3 inch. As in this case the curve of the gaseous pressure, as well as that of the aqueous vapour, accords in character with the curve of temperature, i. e. ascends with ascending temperature, and descends with descending temperature,—the barometric annual range is greater than the gaseous annual range, which is contrary to what takes place in the temperate and equatorial zones. It is not improbable that in the Antarctic Circle the phænomenon which we have just noticed as taking place in the Arctic Circle, viz. the summer increase of the gaseous pressure,—may not be found in the same degree, if at all; for the two hemispheres present a remarkable contrast in their respective proportions of sea and land, and the rarefaction of the atmosphere over the middle latitudes of the southern hemisphere during its summer must be greatly less than in the same latitudes of the northern hemisphere in the corresponding season. The barometrical observations made by Sir James Ross in summer in the Antarctic Circle accord with this inference; since after correcting them for the shortening of the column of mercury by the increased force of gravity in the high latitudes, and abstracting the small tension of vapour corresponding to the temperature, the mean gaseous pressure deduced from them, though nearly equal to the mean gaseous pressure of the year at Bombay, does not exceed it; whereas at Alten it is only in the winter months that the gaseous pressure descends so low as to approximate to the usual mean gaseous pressure of the tropical regions.

It is much to be desired that the zealous observers at Alten should observe the ber, the latter season being that of the rains. The average degree of humidity in the year is very slightly lower than either at Toronto or at Greenwich, but is still closely approaching to a value expressing the presence of three-fourths of the quantity of vapour required for saturation.

The mean gaseous pressure in 1843, derived from the two-hourly observations, appears to have been (29·023 + 0·025, an index correction which Dr. Buist gives as that of the barometer with which the observations were made =) 29·048 English inches; or, measured by the height of a mercurial column in the latitude of 45°, 28·988. The height above the sea is thirty-five feet, and the latitude 19° N.

The mean height of the barometer in the year 1843, derived from observations at every second hour, appears to have been (29·803 + 0·025 =) 29·828, or, with the correction applied for gravity, 29·768, the elevation being thirty-five feet above the sea. This is less than what is generally received as the average height of the barometer in the same latitude. From the careful comparison described in Dr. Buist's report of the standard barometer with several other barometers, there seems great reason to believe that the mean height shown by it must be a very near approximation at least to the true mean atmospheric pressure in the year 1843 at Bombay.

The mean height of the barometer in the four clouded months of May, June, July and August, is 29·667; and in the four clear months of November, December, January and February, 29·921. The mean vapour pressure in the same seasons is respectively 0·904 and 0·623, and the gaseous pressure consequently 28·763 and 29·298. There is therefore between the two seasons a difference of 0·535 in. of gaseous pressure, and of 5°·84 of temperature; the lowest pressure corresponding to the highest temperature, and *vice versâ*. If we may allow ourselves to make a rough proportion drawn from a single case, we may estimate a decrement of 0·1 in. of pressure to an increment of 1° F. The highest temperature and lowest pressure are accompanied for nearly the whole of the period by the southern monsoon; the lowest temperature and the highest pressure are accompanied by the northern monsoon.

The curves of the annual variation of the gaseous, barometric, and vapour pressures, which are represented in Plate I., show how much of the influence produced on the gaseous pressure, by the alternation of the overflow in the high regions of the atmosphere as either side of the equator becomes heated in its turn, is masked in the barometric curve by the combination in the latter of the vapour pressure, the variations of which take place throughout the year

wet thermometer at the same time as the barometer; the register would also be rendered much more complete by the addition of another observation-hour, about 6 A.M., which might not perhaps be inconvenient. The atmospheric pressure and the tension of vapour at or near the coldest hour of the twenty-four, are important data in meteorological discussions.

in the opposite direction to those of the gaseous pressure. From this cause the range of the barometric curve during the year is only 0·327 inch, whilst that of the gaseous pressure is 0·650 inch.

The analogy of the annual and diurnal variations, considered in respect to the explanation which has been attempted of the latter, is too obvious to be dwelt upon. The decreased gaseous pressure in the hot season is occasioned by the rarefaction of the air over the land whilst the sun is in the northern signs, and its consequent overflow in the higher regions, producing a return current in the lower strata; and the increased pressure in the cold season is occasioned by the cooling and condensation of the air, whilst the sun is on the south side of the equinoctial, and its consequent reception of the overflow in the upper strata from the regions which are then more powerfully warmed, and which is but partially counteracted by the opposite current in the lower strata.

In concluding this communication, I beg respectfully to submit to the consideration of the eminent meteorologists here present, that it is very important towards the progress of this science, that the propriety (in such discussions as the present) of separating the effect of the two elastic forces which are considered to unite in forming the barometric pressure, should be speedily admitted or disproved. The very remarkable fact recently brought to our notice by Sir James Ross, as one of the results of his memorable voyage, that the mean height of the barometer is full an inch less in the latitude of 75° S. than in the tropics, and that it diminishes progressively from the tropics to the high latitudes, presses the consideration of this point upon our notice; for it is either explained wholly or in greater part by the diminution of the vapour constituent in the higher latitudes, which diminution appears nearly to correspond throughout to the decrease of barometric pressure observed by Sir James Ross; or it is a fact unexplained, and I believe hitherto unattempted to be explained, on any other hypothesis, and of so startling a character as to call for immediate attention.

If, by deducting the tension of the vapour from the barometric pressure, we do indeed obtain a true measure of the pressure of the gaseous portion of the atmosphere, the variations of the mean annual gaseous pressure, which will thus be obtained in different parts of the globe,—and the differences of pressure in different seasons at individual stations,—may be expected to throw a much clearer light than we have hitherto possessed on those great aërial currents, which owe their origin to variations of temperature proceeding partly from the different angles of inclination at which the sun's rays are received, and partly from the nature and configuration of the material bodies at the surface of the earth: and a field of research appears to be thus opened by which our knowledge of both the persistent and the periodical disturbances of the equilibrium of the atmosphere may be greatly extended.

IX. *On some New Species of Animal Concretions.*
By THOMAS TAYLOR, *Surgeon.*

To Richard Taylor, Esq.

DEAR SIR,

AS the Catalogue of the Calculi belonging to the Royal College of Surgeons has now been published some months, and there consequently remains no further necessity for silence, I purpose in the following paper to redeem the promise I formerly made, of describing some of the more remarkable of the concretions which have been discovered during the examination of that very large collection; and also to detail the experimental proofs on which the assertions as to their composition were founded in the short notice which you did me the favour of inserting in this Journal in May 1844.

I do this the more willingly, as it was considered advisable to omit the details of the analyses in the Catalogue. Moreover, the Catalogue having but a limited circulation, many of the new facts that have been elicited would not otherwise be generally known. I shall, however, confine myself in this paper to the notice only of such concretions as are entirely new, or whose composition has been either imperfectly or incorrectly described. For the historical account of the successive steps by which our present knowledge of these bodies has been obtained, and for the description of the more common species of calculi, I must refer to the Catalogue itself.

Urinary Calculus from the Iguana, consisting of Urate of Potass.

Small and unimportant quantities of urate of potass may occasionally be detected in human urinary calculi, but no instance of this salt constituting an entire calculus has hitherto been described. There are three specimens of this description in the College collection, which resemble each other in every respect save in size. Two of them were described in the MS. Catalogue of Sir Hans Sloane's collection as "Piedra de Yguana," and there is little doubt but that they were taken from the urinary bladder of some of the large Iguanas or tree lizards of South America. The other concretion had no history, but had been described as "a mixed calculus in which uric acid predominates." Although much larger, it was so similar in composition and general appearance to the others, that there does not appear any reason to doubt its having a similar origin. In their external characters these concretions resembled calculi composed of the mixed phosphates, being made up of concentric layers of a dirty white colour with a shade of pink. They were of an ovoid figure, but all of them were remarkable for being flattened in a peculiar manner on one side.

When heated before the blowpipe they consumed like a uric acid calculus, but left a fusible salt which spread over the platina foil, and tinged the outer flame violet. When heated in a test-tube, carbonate and hydrocyanate of ammonia with a little empyreumatic oil and water were given off. The carbonaceous residue, when treated with water, gave a solution which had a strong alkaline reaction, effervesced with acids, and emitted a slight odour of prussic acid. Tartaric acid and chloride of platina produced in the solution precipitates indicative of the presence of potass.

Water digested upon the powdered calculus afforded a solution which deposited small scales of suburate of potass upon being evaporated; the liquor gave no precipitate with a salt of lime, consequently no soluble oxalate was present.

When digested with caustic potass, ammonia was freely evolved, and the whole dissolved with the exception of a little flocculent matter. The alkaline solution, when mixed with muriatic acid, gave a copious precipitate of uric acid. It was therefore evident that these concretions consisted chiefly of urate of potass mixed with urate of ammonia.

The relative proportion of their constituents was estimated in the following manner:—

19·10 grs., when submitted to a current of dry air at 212° Fahr., lost 0·32 water, = 1·67 per cent. The dried powder was digested in boiling acetic acid, which decomposed the urate of potass and left a residue which weighed 15·02 grs., = 78·64 per cent. This residue was found to consist of uric acid, containing a small trace of oxalate of lime.

The acetic solution being evaporated to dryness was boiled with proof spirit; the whole dissolved with the exception of some light yellow flocks of animal matter, which amounted to 0·52 gr., = 2·73 per cent.

The spirituous solution being evaporated to dryness, the earthy and alkaline acetates it had contained were decomposed by the addition of muriatic acid; the mixture was evaporated to dryness and heated red-hot in a platina crucible. The residue dissolved entirely in water, with the exception of 0·06 gr. of phosphate of lime. The aqueous solution was mixed with carbonate of ammonia, a precipitate fell, which when dried and ignited was equivalent to 0·36 gr. of pure lime. It contained however a trace of phosphate of lime.

The solution from which the above precipitates had been separated was evaporated to dryness and the residue heated red-hot. It was redissolved in water, and mixed with alcohol and chloride of platina. Chloride of platinum and potassium was thrown down, which when washed with alcohol and carefully dried weighed 10·32 grs., = 1·99 PO.

The quantity of ammonia was estimated by boiling the powdered calculus in a solution of potass, transmitting the ammonia evolved through diluted muriatic acid, and precipitating it in the ordinary manner by chloride of platina. 11·40 grs. yielded 4·57 of chloride of platina and ammonia, = 3·10 per cent. I do not, however, place much confidence in this mode of estimating the ammonia. The result of the analysis of this calculus, calculated in 100 parts, is therefore as follows: it is compared with an analysis of the calculus which had no history, in which the quantity of potass is rather greater:—

Uric acid mixed with a trace of oxalate of lime	78·64	78·36
Potass	10·42	13·19
Ammonia	3·10	3·09
Lime	1·89	1·49
Magnesia	0·00	0·29
Phosphate of lime	0·32	0·02
Animal matter	2·73	0·43
Water	1·67	1·80
Sulphate of soda with chloride of sodium		traces
	98·77	98·67

By another analysis, in which the quantity of potass was alone estimated by calcining the calculus until nothing but carbonate of potass was left, and precipitating the dissolved salt by chloride of platina, 10·72 per cent. of potass was found in the first, and 13·07 in the second calculus.

The potass in these concretions is probably derived from the leaves and other vegetable matter on which the Iguana partly subsists, while the carnivorous or insectivorous habits of the reptile are indicated by the large quantity of uric acid they contain.

Urinary Calculus from the Sturgeon, consisting of Diphosphate of Lime. Beluga stones.

These calculi are found by the fishermen of the Caspian Sea and of the Volga in a species of Sturgeon (*Acipenser Huso*, Linn.). The statements of different authors as to the situation of the stone in the fish, are very conflicting, some describing it as occurring in the air-bladder, others in the head and stomach. In Schrober's *Memorabilia Russico-Asiatica*, as quoted by Klaproth, it is said to be most frequently found in a small pouch communicating with the pancreatic duct; his description is however confused and anatomically incorrect. The subjoined extracts from the works of Pallas[*] leave no doubt as to these concretions being taken from the

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[*] "Les pêcheurs rencontrent assez souvent dans les gros biélougas, la pierre dont j'ai parlé, qui est encore un problème. Ils la vendent à un prix assez modique, de doux à trois roubles. Tous les pêcheurs à qui j'en ai parlé, m'ont assuré qu'on la trouve dans le gros boyau, qui leur sert à se vider et à jeter leurs œufs. On rencontre quelquefois des pierres dans les gros esturgeons ordinaires; elles sont semblables à celles des biélougas. On en trouve aussi dans les gros barbeaux, mais elles sont d'une espèce différente. Les pierres de biélouga sont ovales, unies, et quelques-unes grumelés assez grossièrement; d'autres sont triangulaires et toutes plates. Cette variété, dans la forme et la place qu'elles occupent, prouve que c'est une vraie pierre, et non une arête. Elles ont toutes la couleur et la texture de l'arête. Lorsqu'on les brise, on trouve dans leur substance des rayons luisans spathiques qui tendent de la circonférence au centre; outre la texture écailleuse qu'on distingue à la première superficie, il se détache de l'intérieur de quelques-unes de ces pierres un noyau; il a la même substance que la pierre, mais une autre forme; il ne se trouve pas toujours au centre. J'en ai vu plusieurs qui pesoient jusqu'à trois onces; je les croyois plus pesantes d'après leur grosseur. On peut en raper avec la lame d'un couteau, mais avec peine. J'ai essayé d'en mettre dans des acides et je n'y ai apperçu aucune marque d'effervescence. En Russie, on se sert de cette pierre comme remède domestique, dans les accouchemens laborieux, pour les maladies de l'urètre et celles des enfans; il est très en vogue, et l'on a grand tort. On en fait prendre dans de l'eau à très-petite dose. On attribue les mêmes vertus, et nombre d'autres, à la pierre qu'on rencontre quelquefois dans la vessie urinaire des sangliers, qu'on appelle Kabannoï Kamen, pierre de sanglier; elle est beaucoup plus chère que celle du biélouga."—*Voyages de Pallas*, tom. i. p. 683.

"On fend le cartilage du dos pour en retirer les nerfs; on les lave et étend sur des perches pour les faire sécher.

"C'est en partageant ce cartilage dans toute sa longueur que l'on trouve quelquefois dans les plus gros ichthyocolles cette pierre si vantée. On ne l'apperçoit que lorsque le couteau s'arrête au moment où il la touche. Cette pierre est renfermée dans la chair rouge glanduleuse, qui est adhérente à la partie postérieure de l'épine du dos, et elle tient lieu de rognons. Elle est dans une petite peau particulière, qui remplit l'intérieur de cette espèce de glande. Je rapporte ici ce que M. Sokolof a pu apprendre de plus certain sur sa vraie position, des pêcheurs les plus instruits, qui assuroient en avoir trouvé quelques-unes. A' l'extérieur, elle est un peu molle et humide lorsqu'elle est fraîchement tirée, mais elle durcit aussitôt qu'elle est à l'air. C'est dans les pêches qui se font près d'Astrakan qu'on la rencontre le plus souvent. Elle n'est jamais plus grosse qu'un œuf de poule. Elle est ovale et assez plate un peu concave; où elle a l'angle qui adhère au cartilage un peu courbé."—*Voyages de Pallas*, 1789, vol. ii. p. 486.

"In visceribus uropœis Husonum maximorum et ætate provectiorum sæpius reperitur *Calculus* ovalis, depressus, hinc concavus, solidus, albus, intus Zeolithi fere instar a centro radiatus, nitidusque, cujus chemica analysis adhuc deest. Hunc plebs Rossica, et honoratiores etiam, pro magno medicamento uragogo et partum promovente æstimant atque infantibus propinant, unde a piscatoribus pretio non exiguo redimuntur, *Calculi Husonis* (Bjelushie Kamen) nomine."—*Zoographia Rosso-Asiatica*, vol. iii. p. 87.

dilated ureter or from the common cloacal termination of the gut of the fish.

These concretions have generally a flattened oval figure, their centre being often depressed or slightly concave. They vary considerably in size, but are usually about that of a hen's egg. Their surface is unequal but quite smooth, and of a yellowish-white colour. When broken they present a highly crystalline structure, consisting of fine plates or needles radiating from the centre to the circumference, but which are made up of very thin concentric layers adhering firmly together. Fragments of these calculi are translucent, and their interior is of a pure white colour. They are exceedingly scarce, and are highly esteemed for their supposed medicinal virtues. Dr. Cook informs us that the powder is highly commended as a diuretic and lithontriptic, and that the common people in the neighbourhood of the Volga take from ten to sixty grains, scraped fine in a little water, three or four times a day when the case is dangerous.

The composition of these calculi was first determined by Klaproth, but the earliest description of them is to be found in the Philosophical Transactions for 1748.

The specimen analysed by Klaproth had been received from Prof. Pallas. It weighed above seven ounces troy, and consisted of albumen 1, water 24, phosphate of lime 71·50, sulphate of lime 0·50.

17·13 grs. of one of the specimens in this collection, previously calcined, gave by solution in dilute muriatic acid and precipitation by oxalate of ammonia, 13·87 grs. of carbonate of lime, which is = 17·54 of the diphosphate of lime; 100 grs. of the same calculus gave—

		By calculation.
Water	26·33	25·60 = 5 atoms.
Organic matter . .	0·40	1·13
Diphosphate of lime .	73·27	73·27 = 1 atom.
	100·00	100·00

The Beluga stones therefore consist of an atom of diphosphate of lime combined with 5 atoms of water. The water is necessarily over-estimated in the analysis, on account of the organic matter being partially soluble in the diluted acid.

By another and more rigid analysis, I found the calculus to consist of 32·21 CaO, 40·33 PO_5, and 26·33 HO, which would give 72·54 per cent. of diphosphate of lime. This calculus has also been analysed by Prof. Wöhler, who ascertained that four of the five atoms of water are driven off at 392° Fahr., while the last atom is expelled by a red heat.

The phosphoric acid is therefore in the tribasic state, and the formula of the salt will be $PO_5\,2(CaO), HO, + 4aq$. I think it right to state that the analysis of this concretion had been printed some months previous to the publication of Prof. Wöhler's paper, and its analysis was made in 1843.

Intestinal Concretions.

The composition of the different kinds of intestinal concretions has been very little studied by chemists; a circumstance the more remarkable, as they present many points of interest both to the chemist and physiologist. The only description of these bodies to be found in the systematic works on chemistry, is almost wholly derived from the paper of Messrs. Fourcroy and Vauquelin, published in the 4th volume of the *Annales du Muséum National.*

By these chemists intestinal concretions were divided into the following species:—1, Calculi consisting of superphosphate of lime; 2, of phosphate of magnesia; 3, of phosphate of magnesia and ammonia; 4, of a biliary matter analogous to the colouring matter of the bile; 5, resinous concretions; 6, fungous concretions; and lastly, hair-balls. Their description of these bodies is however exceedingly slight and imperfect, and much inferior in accuracy to their previous researches on urinary calculi. In no instance did they determine the relative proportion of the constituents of the earthy concretions, and under the head of resinous concretions two essentially distinct species were included.

In the Catalogue I have endeavoured to supply these deficiencies by submitting most of these calculi to quantitative analysis, and by the addition of some new species. The following list includes all the intestinal concretions with which I am at present acquainted:—1, Calculi consisting of animal hairs; 2, of vegetable hairs; 3, of ellagic acid—the *oriental bezoar*; 4, of resino-bezoardic acid—the *occidental bezoar*; 5, of phosphate of magnesia and ammonia; 6, of diphosphate of magnesia; 7, of diphosphate of lime; 8, of oxalate of lime; 9, of ambergris.

The Ellagic Acid Calculus.—The Oriental Bezoar.

The composition of this species of calculus was described in a report to the Museum Committee in 1841, and in May 1843 I was permitted to insert a short notice as to its composition in the London and Edinburgh Philosophical Magazine. Since that period the ellagic acid calculus has been examined by MM. Merklein and Wöhler, who have confirmed my statements as to its nature*. The result of a careful comparison of the chemical characters of these concretions with those of ellagic acid prepared from the gall-nut† so fully established their identity, that I did not think it necessary to corroborate my statement by an ultimate analysis, especially as from the limited quantities of the calculus on which I could operate, and the facility with which ellagic acid becomes oxidized when dissolved in an alkali, I did not feel certain that I could ensure that perfect purity without which an organic analysis is wholly valueless. Its ultimate analysis has however been made by MM. Merklein and Wöhler, who have found it to agree with the analysis of ellagic acid by M. Pelouze, *minus* one atom of hydrogen.

The following description of the ellagic acid calculus, its properties and history, I shall quote verbatim from the College Catalogue published in July 1845, as it conveys in a condensed form the results of a long and troublesome series of experiments.

"Ellagic acid calculi are generally of an ovoid figure; their outer surface is smooth, polished, and of a deep olive or greenish brown colour; internally they are brown; they are made up of thin concentric layers, which in some cases adhere so slightly together, as to cause the calculus to fall to pieces on attempting to divide it with a saw. When any of the outer layers of these calculi are removed, the exposed surface readily acquires a high polish by slight friction, and when cut or scraped they assume a waxy lustre. These calculi invariably contain some foreign body as their nucleus, which is generally a small twig or seed.

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* *Ann. der Chemie und Pharm.*, August 1845. If by the following passage, "Aus dieser Zusammensetzung und den oben angegebenen Eigenschaften der Bezoarsäure folgt ferner der merkwürdige Umstand, dass diese Substanz, wie bereits von Th. Taylor vermuthet wurde, in der That nichts Anderes ist als Ellagsäure oder die Säure, die zuerst von Chevreul aus den Galläpfeln dargestellt und von Braconnot näher untersucht worden ist. Um nicht den geringsten Zweifel hierüber zu lassen, haben wir selbst Ellagsäure aus Galläpfeln dargestellt und ihre Eigenschaften mit denen der Bezoarsäure verglichen; sie zeigten sich vollkommen identisch," MM. Merklein and Wöhler intend to imply that some doubt existed in my mind as to the composition of this concretion, I beg to state that such was not the case; and it is difficult to conceive on what grounds they could form such an opinion, as in the notice alluded to I simply stated the fact in the most concise and positive terms that could be made use of.

† For the opportunity of doing this on rather a large scale, I am indebted to my friend Mr. T. Morson, who kindly placed at my disposal a large quantity of the residue left in the preparation of gallic acid, and it gives me much pleasure to have an opportunity of acknowledging this and similar favours.

"The chemical characters of the constituent of these calculi agree so exactly with those of ellagic acid procured from the infusion of gall-nuts, as to leave no doubt of their being composed principally of that substance. When heated they do not fuse, but emit a slight balsamic odour and partially sublime; if more highly heated they catch fire, burn with a low flame, give off the smell of burning wood, and leave behind a carbonaceous ash. If the powder of the calculus be heated in a glass tube a yellow sublimate is produced, which condenses in the form of long spear-shaped crystals of a yellow colour, with a shade of green. These crystals do not differ in their chemical habitudes from the powder of the calculus, and they are identical in shape and appearance with those procured from the ellagic acid of the gall-nut when similarly treated. When the calculus is reduced to powder and diffused through water, several days elapse before the whole of the powder is deposited, and the water remains opalescent even for weeks. It is also difficult to separate the powder by filtration, as the liquid passes turbid for some time.

"Ellagic acid calculi easily dissolve, with the exception of a few flocks, in a cold solution of caustic potass or soda. The solution is of a deep brownish red colour, with a shade of green; when the ellagic acid is, however, freed from some extractive or colouring matter with which it is generally mixed in the calculus, the solution is of so deep a yellow as to appear red when viewed in bulk. Muriatic acid throws down from the potass solution a greenish, buff-coloured powder, while the supernatant liquor is of a light red colour. If the precipitate be examined by the microscope, it is seen to consist of small thread-like particles, generally blunt, but sometimes tapering at their extremities, and which are occasionally twisted or curved, especially if the solution from which they were thrown down was hot: they are not transparent, and can scarcely be termed crystals.

"When the potass solution is exposed to the air, oxygen and carbonic acid are absorbed, the solution becomes much darker coloured, and a silky greenish yellow precipitate is deposited, consisting of ellagate of potass. This precipitate appears under the microscope as thin rectangular plates, frequently arranged in stellate groups. If a current of carbonic acid is passed through the solution, a buff-coloured precipitate of ellagate of potass is thrown down, while the supernatant liquid remains of a dark reddish colour.

"Ellagic acid calculi are very sparingly soluble in solution of ammonia; the liquid acquires a yellow colour, which on exposure to the air becomes brown and turbid. The small quantity of ellagic acid dissolved is precipitated by an acid.

"Concentrated sulphuric acid readily dissolves these calculi when assisted by a gentle heat. The solution is of a greenish brown colour, and is precipitated by dilution with water. The precipitate has the form of minute prisms arranged in stellate groups; the extremities of some of the prisms are blunt, others are pointed.

"When mixed with nitric acid, the ellagic acid calculus dissolves. If the acid be strong or slightly warmed, effervescence takes place, nitrous fumes are given off, and a solution is produced of a beautiful pink-red colour, similar to that produced by the action of nitric acid upon uric acid. The red colour quickly disappears upon standing; on being heated, a deep yellow solution remains, from which crystals of oxalic acid may be obtained by evaporation. Ammonia added to the solution causes it to assume a red colour, but does not render it turbid.

"The ellagic acid is best obtained from these calculi by dissolving the powdered calculus in a weak solution of caustic potass, and transmitting through it a current of carbonic acid. The precipitate which falls is to be digested in diluted muriatic acid, by which the potass is removed, and tolerably pure ellagic acid remains. During the whole of the operation great care must be taken to prevent the contact of atmospheric air; for when dissolved in alkaline liquids, ellagic is quickly converted into a species of ulmic acid. It is not improbable that catechuic acid is sometimes present in these calculi.

"This species of intestinal concretion appears to have been first examined by Fourcroy and Vauquelin, and is included in their class of *resinous* Bezoars*. It was shortly afterwards examined by Berthollet, and subsequently by other chemists, all of whom failed in deciding upon its true nature; even so recently as 1843 this calculus was described by M. Lippowitz as consisting of a peculiar organic acid, for which he proposed the name of Bezoaric acid†.

"The concretions analysed by Berthollet, and of the properties of which he has given a very accurate account, had been

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* "La seconde variété d'une couleur brune ou violacée, sans saveur amère, presque insoluble dans l'alcohol, entièrement soluble dans les alcalis, donnant dans cette dernière dissolution une liqueur qui devient rouge purpurine, lorsqu'elle s'épaissit et se sèche à l'air: fournissant à la distillation un sublimé concret, jaune, d'une saveur et d'une couleur de suie, insoluble dans l'eau et dans l'alcohol."—*Annales du Muséum National*, tom. iv. 334.

† Simon's *Beiträge zur Phys. und Pathol. Chemie und Mikroskopie*, B. i. 463.

presented to the Emperor Napoleon by the Shah of Persia. They were of a greenish brown colour externally, and brown within; they had an oval figure, and their surface was highly polished; they were formed of irregular concentric layers, and in the centre of all of them was some vegetable matter; their sp. gr. = 1·463. They were regarded by Berthollet as consisting of the woody fibre (*lignin*) of the food of the animal, and he conjectures that they must have been taken from the stomach, on account of the little alteration which the vegetable matters that formed their nucleus* had undergone.

"The constituent of the ellagic acid calculus is likewise described by John under the name of *Bezoarstoff*†; and Leopold Gmelin thinks it probable that the calculi examined by John were identical in composition with those analysed by Berthollet‡, and that they consisted of a species of ulmin arising from the decomposition of woody fibre or lignin.

"From the descriptions which Tavernier, Kæmpfer, and other Oriental travellers have given of the Oriental Bezoar, corroborated by the analyses of Fourcroy and Berthollet, there is no doubt that it is identical with the ellagic acid concretion above described. The signs by which a true Oriental Bezoar might be distinguished were, according to Tavernier, by steeping it in hot water, and observing whether the liquid became coloured, or the stone lost in weight. If either of these occurred, the stone was to be regarded as fictitious: but the best test was to apply a red-hot iron wire to the calculus, when, if it melted and permitted the iron to enter, it was certainly fictitious. Another test consisted in smearing a piece of paper with chalk, and rubbing the calculus over it. The genuine stone always left a greenish mark. All these criteria would be fulfilled by the ellagic acid calculus, but by none of the other species§.

"This species of concretion was the most valued of the Bezoars, and is denominated by Kæmpfer the 'verus et pretiosus *Pasahr*,' from which word, by a corruption of sound, he believes the word Bezoar to have been derived.

"With regard to the origin of this concretion, we have the fullest and most satisfactory evidence. W. Methold, Fryer, Tavernier and Kæmpfer all agree that it is taken most frequently from the alimentary canal of a species of wild goat termed *Pasen* by the Persians, which inhabits the mountainous ridges in Persia, particularly in the province of Chorasaan or

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* *Mémoires de la Société d'Arcueil*, tom. ii. p. 448.

† *Chem. Schr.* iii. 38.

‡ *Handbuch der Chemie*, B. ii. S. 828, 1488.

§ In the Sloanian MS. Catalogue all the ellagic acid calculi were termed East Indian or Oriental Bezoars.

Chorasmia. Tavernier states that they likewise come from a province of the kingdom of Golconda. The account as to the exact situation of the stone is however not so clear. Most writers indicate the maw or stomach: Kæmpfer says it is found in the pylorus, 'sive productior quarti, quem vocant ventriculi fundus*,' and that the natives are in the habit of ascertaining how many stones are contained in the stomach by feeling through the parietes of the abdomen, the value of the animal being considerably enhanced by their presence. When recently taken from the animal, they are said to be somewhat soft, or of the consistence of a hard-boiled egg, and that in order to preserve them it was customary to place them in the mouth, and retain them there until they acquired greater hardness.

"The Oriental Bezoar was not however confined to the wild goats, or to the ruminant tribes, as the *Pedra Bugia* or Ape stone also consists of ellagic acid. These concretions were held in higher esteem than those from the Goat, and were generally included, for the sake of preserving them, in a small cavity scooped out of two portions of a very light wood, which were held together by hoops wove from the twigs of the Rotang cane. There is in the Museum a specimen preserved in this manner. Kæmpfer informs us that they were found in a species of ape termed Antar by the Mongols, which he believes to be the *Babianum cynocephalum*. The composition of these concretions renders their origin no longer a matter of uncertainty, and confirms, in a very remarkable manner, the statements of Tavernier and Kæmpfer, that they are derived from the juices of the plants on which the animals fed."

MM. Merklein and Wöhler have proposed that the word ellagic should be changed for that of bezoaric acid, partly because the German word "Gall," when reversed, is not capable of being converted into ellagic, and partly from its want of euphony. If we were however, for the sake of euphony, to reject all the inharmonious appellations which the industry of modern chemists has introduced into the science, we might alter half the names at present in use; besides, as the ellagic acid was first procured from the gall-nut by Chevreul and subsequently named by Braconnot, I think it a matter of courtesy to adhere to its French derivation. The term bezoaric is also peculiarly improper, inasmuch as it would imply that the entire class of bezoars consisted of this peculiar acid.

I remain, dear Sir,

Yours most truly,

New Bridge Street, Nov. 20, 1845. THOMAS TAYLOR.

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[*] *Op. cit.* p. 400.

X. *Inquiries in the Elements of Phonetics*[*].

By C. B. Cayley, *Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SCHEME of Consonants.—Liquid Aspirates.—I start with that scheme which has the authority of Messrs. Latham and Whewell (waiving some undecided points), to which I would make a few additions. I denote, for convenience, the aspirates (so called perhaps fitly, as the breathing is more heard in them) universally by adding to the corresponding non-aspirates a small *h*, which thus becomes a symbol from a letter, and I make *J*, suitably to its form and origin, stand for German *J*, English *Y*.

Sharp.	Flat.	Liquid.
P Ph	B Bh	M . . W
T Th	D Dh	N
K Kh	G Gh	L . . J
S Sh	Z Zh	R . . (place uncertain)

I observe that the flat consonants appear formed from the sharp by an effort (to make their sound stronger and more continuous), which also, being repeated, converts the flat into the liquid. This effort has not prevented, in the flat order, the introduction of the second force of aspiration as in the sharp: hence there is some reason to conjecture that the liquids might be susceptible of this force. In short, the symmetry of the system requires an order of liquid aspirates, which might be written *Mh*, *Nh*, *Lh*, *Rh*. May we not identify *Mh* with French *m* or *n* nasal, *Nh* with English *Ng*, *Lh* with Welsh *Ll*, *Rh* with Arabic Ghain?

Concerning the latter two, my chief difficulty is to know whether they are simple or compound sounds; but

The Welsh *Ll* is stated in Davis's Grammar to be the aspirate of *L*, pronounced by pressing the tongue against the teeth on both sides with a forcible emission of breath.

The Arabic Ghain is described by De Sacy as a sound resembling *R* and *G*, "comme l'R graissayé des Provencaux." We often hear imperfect pronunciations of *R* which might give the idea of this secondary liquid.

But I shall chiefly insist on *Mh* or *m* nasal, because it coincides with *F* and *V*, the aspirates as *M* with *B*, *N* with *D*, &c. Compare the pronunciation of *combattre*, *entendre*, *enfant*, *envahir*; likewise it has the same resemblance to the *dental N*, which *Ph*, *Bh* being formed by the teeth in part have to *Th* (whence the confusion made by children in those sounds, and

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[*] From letters addressed to Mr. Latham.

the Russian corruptions Feodor, Marpha, &c. for Theodore, Martha), so that all the labial aspirates would approximate to dentals, and on this principle might it be that the liquid aspirate *Nh* approaches the next order of gutturals.

Should this paper be admitted, I shall hereafter consider some objections that have been communicated to me, and proceed to an attempted analysis of vowel sounds.

C. B. CAYLEY.

XI. *On Fresnel's Theory of Double Refraction.*
By ARCHIBALD SMITH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN an article on Fresnel's Theory of Double Refraction, in the Supplement to the December Number of the Philosophical Magazine, Mr. Moon has quoted part of an article of mine in the first volume of the Cambridge Mathematical Journal, in which the following passage occurs:—"Let the particle receive a small displacement, the projections of which on the co-ordinate axes are δx, δy, δz. Then supposing the displacement to be very small, *the force of restitution may be taken as proportional to it, so that* we have," &c. I am not surprised that Mr. Moon should remark on this passage, "what is meant by the mysterious principle 'supposing the displacement to be very small, the force of restitution may be taken as proportional to it,' I profess myself unable to understand."

The clause in italics, which was added to my manuscript when it was sent to the press, to remove, I believe, what was thought an abruptness in the reasoning, is certainly incorrect when applied to a doubly refracting medium. What was intended to be expressed, no doubt, was, that in the case supposed terms involving powers of δx, δy, δz higher than the first might be neglected. But this expression is only equivalent to the other in the case of a singly refracting medium. I may mention that in the middle of page 7 of the article in the Cambridge Mathematical Journal, the word "rays" was, by a mistake, substituted for "waves." These mistakes are corrected in the second edition of the first volume of the Cambridge Mathematical Journal which is now printing. As they have been noticed in your Journal, I shall feel much obliged by your inserting this explanation when you can afford space for it.

Your obedient Servant,

25 Old Square, Lincoln's Inn, Dec. 20. ARCHIBALD SMITH.

XII. *On the Origin of the constituent and adventitious Minerals of Trap and the allied Rocks.* By James D. Dana*.

THE minerals of trap and the allied rocks may be arranged in two groups:—

1. Those essential to the constitution of the rock, or intimately disseminated through its texture.

2. Those which constitute nodules or occupy seams or cavities in these rocks.

Of the first group, are the several felspars, with augite, hornblende, epidote, chrysolite, leucite, specular, magnetic and titanic iron; and occasionally Hauyne, sodalite, sphene, mica, quartz, garnet and pyrites. Of the second group are quartz, either crystallized or chalcedonic, the zeolites or hydrous silicates, Heulandite, Laumonite, stilbite, epistilbite, natrolite, scolecite, mesole, Thomsonite, Phillipsite, Brewsterite, harmotome, analcime, chabazite, dysclasite, pectolite, apophyllite, prehnite, datholite, together with spathic iron, calc-spar and chlorite. Native copper and native silver might be added to both groups, yet they belong more properly to the latter. To the same also might be added sulphur, and the various salts that are known to proceed from decompositions about active volcanoes, including the crystallizations of alum, gypsum, strontian, &c.; but these more properly form still a *third* group, and being well understood, will not come under consideration in the remarks which follow.

We observe with regard to the minerals of the first group, that they are all anhydrous, that is, contain no water. In this respect, the essential constituents of trap and basalt are like those of granite and syenite. But in the second group, consisting of the minerals occurring in cavities or seams, all contain water except pectolite, quartz, calc-spar and spathic iron; and the last three are known to be always deposited in an *anhydrous* state from aqueous solutions.

We proceed to give a few brief hints with regard to the first group, intending only to glance at this branch of the subject, and then take up more at length the group of adventitious minerals.

Essential constituents of modern Plutonic rocks.—It is obvious that modern igneous rocks, although in some cases derived from the original material of the globe, have proceeded to a great extent from a simple fusion of rocks previously existing, and especially of the older igneous rocks. In accordance with this view, we may with reason infer that the tra-

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* Read before the Association of American Geologists and Naturalists, May, 1845, and communicated by the Author.

chytes and porphyries, which consist essentially of felspar, have proceeded, in many instances at least, from felspathic granites; the basalts and trap from syenites, hornblende or augitic rocks.

A theory proposed by Von Buch supposes that the felspathic rocks, as they are of less specific gravity, are from the earliest eruptions, or the more superficial fusings, while the heavier basalt has come from greater depths. Darwin thus accounts for the granites of the surface being intersected by basaltic dykes; the latter having originated from a deeper source, where their constituents took their place at some former period from their superior gravity. It virtually places hornblende rocks below felspathic granites in the interior structure of our globe. The hypothesis is ingenious and demands consideration; but it may not be time to give it our full confidence.

But supposing these more modern rocks to have been derived from the more ancient granitic—what has become of the quartz and mica which occur so abundantly in the latter, while they are so uncommon in the former? By what changes have they disappeared?

In the fusion produced by internal fires, the elements are free to move and enter into any combinations that may be favoured by their affinities. If silica, alumina, magnesia, lime, iron, the alkalies, potash and soda, were fused together—and these are the actual constituents of basalt—what result might we expect? From known facts, we should conclude that the silica would combine with the different bases, and these simple silicates would unite into more complex compounds. The silicates of alumina and the alkalies or lime, form thus one set of compounds, the felspars; the silicates of magnesia and the isomorphous bases, iron and lime, another set, to which belong augite, hornblende and chrysolite; and if much iron is present, we might have with the lime and alumina, the mineral epidote. The experiments of Berthier, Mitscherlich and Rose, and the facts observed amongst furnace slags, confirm what is here stated.

But not to go back to a resolution of the fused minerals into their elements, we may consider for a moment what changes the minerals themselves might more directly undergo in the process of fusion.

Much of the mica in granite differs from felspar in containing half the amount of silica in proportion to the bases, the bases in each being alumina and potash or soda. The change then in the conversion of the mica into felspar would require an addition of silica, which might be derived from the free quartz of granite. Other varieties of mica contain magnesia, which would go towards the formation of some mineral of the magnesian series. It is possible that trachytes and porphyry have thus been made from granite; but trap rocks could not have been so derived, as they contain from 10 to 25 per cent. less of silica.

Again, hornblende and augite are so nearly related, that they have been considered by Rose the same mineral, the different circumstances attending the cooling giving rise to the few peculiarities presented. There can be no difficulty therefore in deriving augite by fusion from hornblende rocks. This moreover has been actually confirmed by experiment.

Augite, by giving up half of its silica, and receiving additional magnesia in place of its lime, is reduced to chrysolite*. The Gehlenite, nepheline, anorthite and meionite of Vesuvius, contain, like scapolite, only 40 to 45 per cent. of silica and a large proportion of lime, and it is no improbable supposition, judging from the small amount of silica, and from the lime present, that scapolite rock, or rather limestones containing scapolite, may have contributed in part towards the lavas of that region. The ejections of unaltered granular limestones, and many mineral species pertaining to such beds, strongly support this view; and it is no less sustained by the fact, that in the Vesuvian basalts, Labradorite, which includes lime instead of the alkalies, replaces common felspar. The original felspar seems to have given way to leucite and Labradorite †.

An important source of new combinations is found in the sea-water which gains access to the fires of volcanoes. The decomposition which takes place eliminates muriatic acid, so often detected among volcanic vapours; but the soda and other fixed constituents remain, to enter into combination with some of the ingredients in fusion. Is not this one source of the soda forming the soda felspar, or albite, and of the muriatic acid and soda in sodalite? Phosphates have been long known to occur occasionally in volcanic rocks, and lately phosphoric acid has been proved to be generally common in small quantities. Sea-water is also a very probable source of this ingredient, as has been shown by late analyses of the same by Dr. Jackson.

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* The formula of augite is $\dot{R}^3\dddot{Si}^2$; that of chrysolite, $\dot{R}^3\dddot{Si}$.

† Using $\overline{R}$ for the bases and Si for silica, the formula of leucite is $\overline{R}\,Si^{\frac{4}{3}}$; that of common felspar, $\overline{R}\,Si^2$; that of Labradorite, $\overline{R}\,Si$. From this, it appears that felspar may be reduced to leucite by giving up one-third of its silica, the bases being the same in the two; and with this excess and other silica combining with the lime at hand, Labradorite might be formed.

These few hints are barely sufficient to indicate something of the interest that attaches to this field of investigation, which the future developments of science will probably open fully to view. We do not attempt to explain why in these modern fusings, mica should not have remained mica, and the quartz still free uncombined quartz. The facts prove some peculiarity of condition attending the formation of the granitic rocks. Of this condition we know nothing certain, and can only suggest the common supposition of a higher heat and slower cooling, attending a greater pressure and different electrical conditions, and the same circumstances may have existed during the granites of different ages.

With these brief suggestions I pass to the second division of the subject before us.

2. *Minerals occupying cavities and seams in amygdaloidal trap or basalt.*—These minerals have been attributed to a variety of sources, and even at the present time there are various opinions respecting their origin. According to some writers, they result from the process of segregation; that is, a separation of part of the material containing rock during its cooling by the segregating powers of crystallization; and in illustration of the process we are pointed to the many segregations of felspar, quartz and mica, in granite and other rocks, the siliceous nodules in many sandstones, the pearlstones in trachytes and obsidian. Others have thought them foreign pebbles, enclosed at the time the rock was formed. Again, they are described as proceeding from the vapours which permeated the rock while still liquid, and which condensed as the rock cooled, in cavities produced by the vapours. By a few it is urged, admitting that the cavities are inflations by vapours like those of common lava, that they may have been filled either at the time the rock cooled or at some subsequent time, either by crystallization from vapours, or from infiltrating fluids, but more generally the latter.

Of these views we believe the last to accord best with the facts. Macculloch, in his System of Geology—a work which anticipated many of the geological principles that have since become popular—dwells at length on this subject, and supports the opinion here adopted with various facts and arguments. Lyell also admits the same principles. A review of the facts will enable us to judge of its correctness.

1. In the first place, the cavities occupied by the nodules are in every respect similar to the common inflations or air-bubbles in lava. These cavities are open and unoccupied in common lava, and may be no less frequently so in the ejections under water; and should they not be expected to fill in

some instances by infiltration? They are the very places where an infiltrating fluid would deposit its sediment, or collect and crystallize, if capable of crystallization; and such infiltrating fluids are known to permeate all rocks, even the most solid, and especially if beneath a body of water. It is evident, therefore, that we are supporting no strange or improbable hypothesis. On some volcanic shores one variety of the process may be seen in action. The cavities of a lava may be detected in the process of being filled with lime from the sea-water washing over dead shells or coral sand, and at times a perfect amygdaloid is formed. But the positions and characters of the minerals themselves establish clearly the view we support.

2. The mineral in these cavities sometimes only fills their lower half, as if deposited from a solution; and again, it incrusts the upper half or roof, as if solidified on infiltrating through. In the large geodes of chalcedony, stalactites depend from above like those of lime from the roof of caverns, and, as Macculloch states, the stalactite is often found to correspond to an inferior stalagmite,the fluid silica having dripped to the bottom and there become solid; moreover, the superior pendent stalactite is sometimes found united with the stalagmite below. The same results are here observed as with lime stalactites in caverns, and often a similar laminated or banded structure, the result of deposition in successive layers. Such results can proceed only from a slow and quiet process,—a gradual infiltration of a solution from above into a ready-formed cavity; they can no more be supposed to arise from ascending vapours, or gaseous emanations from below, than the stalactite in the limestone cavern.

Another fact is often observed. A geode of quartz crystals, sometimes amethystine, in which every crystal is neatly and regularly formed, is found with the surface coated over with an incrustation of chalcedony, the part above hanging in small stalactites; and this chalcedonic coat sometimes scarcely adheres to the crystals it covers; or is even loose, and may be easily separated. There can scarcely be a doubt of a subsequent infiltration in a case of this nature.

We might rest our argument here, since the fact being ascertained with regard to quartz, it is necessarily established as a general principle with reference to the zeolites and other amygdaloidal minerals; for quartz or chalcedony, when present in these cavities, is, with rare exceptions, the *lower* or *outer mineral*. We find zeolites implanted on quartz, but very seldom quartz on zeolites. I have met with no instance of the latter, while the former is the usual mode of occurrence.

Any deduction, therefore, respecting quartz, holds equally for the associated minerals.

How a cavity coated with a deposit of chalcedony can still be afterwards filled up with other minerals, has been deemed a mystery in science, but the possibility of it is now not doubted. Even flint and agate, as Macculloch states, are known to give passage to oil and sulphuric acid; and much more will this take place in the moist rocks before the agate has been hardened by exposure to the air. Silica remains in a gelatinous state for a long period after deposition, and in this condition is readily permeable by solutions. It is not necessary that the fluid which has acted the part of a solvent and filled the cavity, should yield place to another portion of fluid; for the process of crystallization having commenced, a new portion of the material is constantly drawn in to the same fluid, and the necessary chemical changes are also promoted by the inductive influence of the changes in progress—the catalytic action as it is called—one of the most efficient, and at the same time one of the most universal agencies in nature.

Other evidence with reference to amygdaloidal minerals is presented by the zeolites themselves.

3. The zeolites occupy veins or seams as well as cavities. Often the seams were opened by the contraction of the cooling rock, and at other times they were of more recent origin. In either case the minerals filling these seams must be subsequent in formation to the origin of the rock itself, and could not have proceeded from vapours attending the eruption. These seams sometimes open upward and can be seen to have no connexion with the parts below, the rock in this portion being solid. Origin from above or from either side, is the only supposition in such cases.

Messrs. Jackson and Alger, in their valuable memoir on the geology of Nova Scotia, mention the occurrence of crystals of analcime attached to the extremity of a filament of copper, the copper having been the nucleus about which the solution crystallized, and state that their formation must have been subsequent to the formation of the rock.

4. Zeolites, moreover, have been found forming stalactites in basaltic caverns, as was observed by the writer in some of the Pacific islands: and Dr. Thomson has described and analysed one (Antrimolite) from Antrim in Ireland near the Giant's Causeway.

These facts favour throughout the view we urge, that the amygdaloidal minerals have in general resulted from infiltration, and were not necessarily formed simultaneously with the erupted rock.

5. We remark further, that no lavas have ever been shown to contain at the time of ejection any of the zeolitic minerals. The zeolites of Vesuvius are known to occur only in the older lavas, and afford no evidence against our position. The cavities in lavas, as far as observed, are empty as they come from the volcanic fires, with the exception of those containing sparingly some metallic ores which are condensed within them. Considering the fusibility of the zeolites and their easy destruction by heat and by volcanic gases, sulphureous and muriatic, we should *à priori* say that they could not be formed under such circumstances.

6. Besides, as we have stated, none of the proper constituents of trap or basalt—or the minerals disseminated through these rocks,—contain water. They are all anhydrous. The minerals formed accidentally in furnaces are anhydrous. The constituents of granite, syenite and porphyry, are all anhydrous. It is only those minerals which are found in geodes or seams that contain water. Of equal importance is the fact, that none of the essential constituents of these rocks have ever been found in these geodes or cavities along with the zeolites, as might have been the case had they been formed together, by segregation or otherwise. Neither felspar, although so abundant, nor augite, nor chrysolite, have been found filling, like zeolites, or with them, the cavities of amygdaloid. There is then a wide distinction between the anhydrous constituents of these rocks, and the hydrous zeolitic minerals.

A few zeolites have been found in granite or gneiss, but they are so disseminated that they can be shown to be of more modern origin than the rock, and to have resulted from some decompositions of true granite minerals. They differ entirely in their mode of distribution from the felspar, garnet, &c. of granite. Along with the decomposing felspar it is not unusual to find stilbite in the cavities formed by the decomposition.

Zeolites also have been found disseminated through the texture of basalt, clinkstone, &c., like the felspar, augite, &c. But the proportion varies widely, and in some parts of the same bed they are found to be wanting: so that we have sufficient reason for classing these disseminated zeolites with those in the cavities, as formed or introduced by infiltration.

7. Bearing upon this subject, it should be observed, that the constituents of amygdaloidal minerals are, in general, those of the containing rock. Silica, potash, soda, alumina, are found in the felspars; lime, magnesia and iron, in augite or hornblende; iron and magnesia in chrysolite. These are all

the constituents needed, except a little baryta for one species. The felspar decomposes readily and gives up its ingredients, its potash or soda, silica and alumina; the same is true of augite and chrysolite, which afford magnesia, lime, silica and iron. With water to infiltrate, we should therefore have all the necessary ingredients at hand for the required compounds. The fact already stated, that zeolites have been found as stalactites in caverns, seems to prove that they *do* actually result from decompositions and recompositions, such as have been supposed. Thus we have all the conditions at hand necessary for producing, by infiltration, the zeolites and the chlorite nodules of these rocks; the alumina, alkalies and lime, contribute, along with a portion of the silica, to the zeolites, and the magnesia, iron, and another portion of the silica, to the chlorite *, often as abundant as the former. The amygdaloidal nodules frequently have a green coating, which further indicates the probable truth of these views; for it appears evidently to be a precipitate from the solution before a crystallization of the zeolites took place—a settling, perhaps, of the insoluble impurities taken up by the filtrating fluid in its passage through the rock, or of the formed chlorite, less soluble than the zeolites. Occasionally, when the rock contains copper, these nodules have an earthy coating of green carbonate of copper—the carbonate having proceeded, apparently, from the native copper of the rock, by the same process as explained.

The hypothesis of filtration seems, then, to be at least the principal source of these minerals. In some instances the filtrating fluid may have derived its ingredients from distant sources. The salts of sea-water may act an important part in these changes. Silica is dissolved on a grand scale during submarine eruptions, as we have elsewhere urged, and is thence distributed to the rocks around. Lime, also, is taken up in a similar manner. But the rock itself has often afforded the ingredients for the forming minerals, during the passage of the filtrating fluid through it. By the same means, the adjoining walls of a seam or dyke—which received the drainings from the rock of the dyke—are often penetrated by zeolitic minerals.

It may be thought that I am giving undue influence to a favourite theory, and in the minds of some, these conclusions may be set down among mere speculations in science. But

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* Chlorite consists of the same elements as augite or hornblende, except that the lime is excluded and water added. They are silica, alumina, magnesia, oxide of iron, with 12 per cent. of water.

the circumstances attending submarine igneous action, I am persuaded, are not generally apprehended. What is the condition of the deep bed of an ocean? Even at a depth of three miles, the waters press upon the bottom with a force equivalent to a million of pounds to the square foot; and with such a forcing power above, can we set limits to the depth to which these sea-waters—magnesia and soda solutions—will penetrate? Will not every cavern, every pore, far down, be filled under such an enormous pressure? Let a fissure open by an earthquake effort, and can we conceive of the tremendous violence with which the ocean will rush into the opened fissure? Let lava ascend, can we have an adequate idea of the effect of this conflict of fire and water? The rock rises, blown up with cavities like amygdaloid, and will a long interval elapse before every air-cell will be occupied from the incumbent water? Suppose an Hawaii to be situated beneath the waves, pouring forth its torrents of liquid rock;—this island contains about five thousand square miles, which is less than the probable extent of many a region of submarine eruption;—suppose, I say, the fires were opened and active over an area of some thousands of square miles—are there no effects to be discovered of this action? There is no geologist that pretends to deny the premises—the fact of such submarine eruptions, the ocean's pressure, the effect of fire in heating water, and in giving it increased solvent power; and why should they not reason upon the admitted facts, and study out the necessary consequences? Surely, if there have been effects, we might expect to see some of them manifested in the cavities of the ejected rocks, which were opened at the time to receive the waters and any depositions they might be fitted under the circumstances to make.

We are led by these considerations to another point in connexion with this subject,—the probable condition under which the different amygdaloidal minerals have been formed. Have they all proceeded from heated solutions, or all from cold solutions? or can we distinguish some which are indubitably of one or the other mode of formation?

Bearing on these questions, we notice such facts as are afforded by the condition and relative positions of the minerals in geodes. And I would here acknowledge my obligations to the valuable memoir, before alluded to, by Messrs. Jackson and Alger. The paucity of information on this subject to be found in the various accounts of similar rocks by other writers is surprising. Even where special pains have been taken to describe the mineral species, the relative positions of the minerals are very seldom noted. It has been altogether too common among geologists to treat mineral information with a degree of neglect almost amounting to contempt, although, as facts will probably hereafter show, they lie at the basis of an important branch of geological science.

But to proceed with the subject before us. We find that *Quartz* or *chalcedony*, and *datholite*, very seldom overlie other mineral species in geodes or amygdaloidal cavities, while the latter often overlie them*.

Prehnite is usually lowermost with reference to all the species except the two just mentioned. Occasionally it is found upon analcime, as at the Kilpatrick hills.

Analcime is commonly situated below all, except quartz, datholite and prehnite.

Of the remaining species, chabazite, stilbite, harmotome, Heulandite, scolecite, mesole, Laumonite and apophyllite, it is more difficult to distinguish an order of arrangement. My investigations only enable me to state that chabazite is usually covered by the rest (when associated with them), yet it is sometimes superimposed on stilbite; and apophyllite is almost uniformly above all with which it may be associated; calc-spar is at different times above and below. We thus arrive at the following as the usual order of superposition:—

1. Quartz.
2. Datholite.
3. Prehnite.
4. Analcime.
5. Chabazite, harmotome.
6. Stilbite, Heulandite, scolecite, natrolite, mesole, Laumonite, apophyllite.

It is a reasonable inference that the species which covers the bottom of a cavity was first deposited, and, as a general rule, that the others above were formed, either simultaneously, or in succession upon the lowermost, as their order may indicate. Each is usually perfect in its most delicate crystallizations, so that we cannot suppose that the minerals first deposited often underwent change after their deposition, though instances of this may no doubt be detected.

It is also evident, that if there were any species formed previous to the complete cooling of the rock, or if any require for their formation an elevated temperature, they are those first deposited—the first in the above series. A few considerations will place this, if possible, in a clearer light.

Quartz, as we have stated in a preceding page, and fully remarked upon elsewhere, enters largely into solution during

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* The writer has observed stilbite, apophyllite, calc-spar and prehnite overlying datholite, and various species over prehnite.

submarine eruptions. This solution has been shown, by actual experiment, to be a necessary consequence of such action. This fact corresponds most completely with the above deductions. Quartz usually forms the first lining of the geode or amygdaloidal cavity, when it is found at all, and, moreover, it is the most abundant of all amygdaloidal minerals.

Quartz may also proceed from decompositions of the rock in the cold, and incrustations of this kind are known to occur; but such an explanation does not account for its generally preceding all other species in filling cavities and seams in trap rocks, and is insufficient to produce the large deposits of silica, sometimes amounting to many tons in a single geode.

It should not be understood that the quartz is supposed to be derived always from the same heated waters that attended the formation of the containing rock; for later eruptions in the same region might, at a subsequent period, produce a like result: yet, as its place in the series proves it to be the earliest in formation, it has probably been generally deposited from the water heated during the eruption of the rock. Leaving quartz, we pass to the other minerals.

It is a striking fact that the minerals next to quartz in the table given—*datholite*, *prehnite* and *analcime*—contain less water than either of the following species. While the others include from 10 to 20 per cent., the first, datholite, has but 5 per cent., prehnite about 4¼ per cent., and analcime 8 per cent.* This fact certainly leans towards the view of their having originated at a somewhat more elevated temperature than the other species—the same conclusion that is drawn from their lower position in geodes.

The fact, also, that prehnite has been found forming pseudomorphs, bears the same way; for heat would be necessary, in all probability, to aid in removing the original mineral. The vast extent of some prehnite veins—occasionally, as Dr. Jackson has observed, three or four feet wide—refers to an origin like that of the quartz in similar rocks. Indeed, there seems little doubt that prehnite is often derived from that

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* The following table shows the per-centage of water, and gives at the same time a general view of the composition of the zeolites:—

Silica, boracic acid, lime.—Datholite (5 Aq.).

Silica, alumina, lime.—Prehnite (4¼ Aq.). Heulandite (14 Aq.). Scolecite (13¼ Aq.). Epistilbite (14 Aq.). Stilbite (17 Aq.). Laumonite (17 Aq.).

Silica, alumina, lime and potash, or soda.—Mesole (12 Aq.). Thomsonite (13 Aq.). Phillipsite (17 Aq.). Chabazite (21 Aq.).

Silica, alumina, and either soda, baryta or strontia.—Analcime (8 Aq.). Natrolite (9¼ Aq.). Harmotome (15 Aq.). Brewsterite (13 Aq.).

Silica, lime and potash.—Apophyllite (16 Aq.).

Silica, lime.—Dysclasite (16¼ Aq.).

portion of the silica in solution which entered into combinations at the time with the alumina and lime which the siliceous waters contained; and probably the lime as well as silica was derived in part from an external source. The pseudomorphs prove that prehnite may have been the result also of subsequent eruptions, at the same time that they show the probable necessity of heat for its formation.

Datholite is a compound of silica, lime and boracic acid, with about 5 per cent. of water. Besides the small per-centage of water, and its being, next to quartz, the lowermost mineral in geodes, we find an additional fact, alone almost decisive with regard to its origin, in its containing boracic acid. Boracic acid is often evolved about volcanoes or in volcanic regions. The hot lagoons of Tuscany, and the volcano of Lipari, are the most noted examples.

Although boracic acid has never been detected in sea-water, there can be little doubt of its occurring in it. The usual modes of analysis by evaporation would dissipate it, and of course it could not thus be detected except with special care and by operating on a large quantity of water. Borate of soda (boracite) is found only in beds of salt and gypsum,—both sea-water products. Moreover, borate of lime has been lately found on the dry plains in the northern part of Chili, along with common salt, iodic salts, gypsum and other marine salts; and all are so distributed over the arid country, that the region has been lately described as having been beyond doubt once the bed of the sea. These facts render it altogether probable that sea-water which gains access to volcanic fires is the source of the boracic acid in volcanic regions*.

If this be its origin, the necessity of heat and pressure must be admitted, in order to produce the chemical combinations in datholite. Its elements are not those of the felspar or other trap minerals, like the zeolites superimposed on it; but they have come from an extraneous source, and none is more probable than the sea waters, which were heated at the submarine eruption, and permeated the bed of molten rock shortly after ejection. Thus placed in circumstances of pressure and confinement, along with silica in solution, the volatile boracic acid might enter into the combination presented in datholite.

An interesting fact bearing upon the history of datholite

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* The only other known source is the mineral tourmaline, quite an improbable one in the case before us. It is possible that tourmaline may have received its boracic acid from the sea during granitic eruptions, and the occurrence of this mineral in the vicinity of trap dykes is explained in the same manner.

was observed by Dr. Jackson at Keweena Point, Lake Superior. The datholite is often found there in veins with native copper, and is associated in some places with a curious slag of boro-silicate of iron and copper. Sometimes the crystals of datholite, as well as the prehnite and calc-spar, contain scales or filaments of native copper. These very important observations seem to establish the same origin for the three minerals, for Dr. Jackson states that they appear to be contemporaneous; and if calc-spar has been deposited from a solution, the same holds true of the others. They have all been formed subsequent to the copper filaments of the cavities, for they were deposited around them; yet may have been the next to form during the cooling of the rock. The boro-silicate of iron and copper has resulted from the same causes.

Analcime approaches the zeolites in composition, but like the prehnite and datholite it contains less water, and is very different in its crystallization. We have less evidence as to the heat necessary for its formation; yet it was probably formed at a somewhat elevated temperature.

With regard to the other amygdaloidal minerals, we are in still greater doubt as to the necessity of heat. We cannot at present fully appreciate the efficiency of chemical agents in a nascent state acting slowly without heat through long periods. Many of them may require heat, and some may be the last depositions from the filtrating waters after they have nearly or quite attained their reduced temperature. But the formation of zeolitic stalactites in caverns favours the view that some at least may form at the ordinary temperature by the slow decomposition of the containing rock after it had emerged from the waves *. Kersten has lately described a modern stellated zeolite forming incrustations on the pump-wells of the Himmelsfahrt mine near Freyberg. It consisted of silica, oxides of iron and manganese and water. Further examination will probably bring more of these modern products to light †.

The formation of particular minerals in certain regions depends of course upon the supply of the necessary ingredients. Where the supply of lime has been large, we should expect to find some of the minerals, prehnite, Heulandite, Laumonite, stilbite, scolecite, dysclasite, chabazite, for carbonate of lime decomposes the silicates of potash and soda. Instances of

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* *Annales des Mines*, ii. (4th Ser.) 465, 1842.

† Carbonate of iron seems never to form from water at the surface, its solutions depositing a hydrated peroxide of iron instead of the carbonate; it may therefore require a submerged condition of the rock, although not necessarily a raised temperature.

this association of the lime zeolites with a large supply of lime in the vicinity are common. When there is little or no lime, or only the results proceeding from the decomposing rock, the other zeolites are formed—the hydrous silicates of alumina and potash or soda, occasionally with some lime. But if a salt of baryta or strontia is present, the decomposition of the silicates of the alkalies takes place as by the lime, and the mineral harmotome or Brewsterite is produced.

In the above explanations we have scarcely appealed to one source of amygdaloidal minerals admitted in the outset—their proceeding from vapours rising with the erupted rock; for it seems to be of but limited influence. Besides the arguments already brought forward, we state that the vapours which rise at the moment of eruption are insufficient. They inflate the rock or blow up the cavities; but the little vapour required to open the cavities most assuredly could not afford by condensation the mineral matter necessary to fill them,—to produce stalactites, stalagmite and successive layers of minerals. The vapours then, if the source, must have continued to rise for some time afterward. But is it possible that vapours should rise up through the solid rock? Such does not happen in the case of recent volcanoes; for fissures are first opened and then the vapours escape. And could it happen with the water above pressing down into the rock with the force of an ocean even a mile deep?

There may be instances of this mode of formation; but that it should be the usual mode is irreconcilable with the many facts stated. The form and condition of quartz or chalcedony in geodes, as well as the vast amount of this mineral in some cases,—the relative positions of the zeolites, and their occurrence as incrustations on rocks, or as fillings of cavities or seams, and never in disseminated crystals through the texture of the rock,—the green coating of the nodules, which is sometimes a carbonate of copper when there is native copper in the rock to undergo alteration,—the correspondence between the elements of the minerals and the composition of the including rock, and at the same time their contrast in being hydrous while the constituents of the latter are anhydrous,—and the known formation of zeolites in caverns,—these various facts appear to establish infiltration as the principal means by which amygdaloidal minerals have been produced.

XIII. *Reflections on the Resolution of Algebraic Equations of the Fifth Degree.* *By* G. B. JERRARD.

[Continued from vol. xxvi. p. 574.]

47. THE remarks in No. 44. related to a difficulty which must arise if we can, as seems to have been proved, succeed in tracing the equation for W to a class of equations of the sixth degree, the solution of which can be effected. I have lately reconsidered the subject of that number, and the exact nature of the difficulty in question will, I think, appear from what follows.

Since every symmetric function of the quantities V_I, V_K, V_L will be such as to remain unchanged whilst one of the roots, x_1, continues fixed, and x_2, x_3, x_4, x_5 are permuted in every possible way among themselves, it might easily be shown that the equation for V,

$$V^{15} + C_1 V^{14} + \ldots + C_{15} = 0,$$

will admit of being resolved into five factors of the form

$$V^3 + \frac{1}{5} C_1 V^2 + r_2(x_\alpha) V + r_3(x_\alpha) = 0,$$

obtained by writing 1, 2, 3, 4, 5 successively for α: r_2 and r_3 being expressive of rational functions, and such that $r_n(x_\alpha)$ shall essentially involve x_α. In this equation, therefore, we cannot generally write $r_n(0)$ instead of $r_n(x_\alpha)$.

But the equation for V will evidently lead to twenty-five expressions for the five roots x_1, x_2, .. x_5, obtainable from a system of functions of x_α;

$$\Psi_1(x_\alpha), \;\; \Psi_2(x_\alpha), \;\; \ldots \; \Psi_5(x_\alpha)^*.$$

The question therefore suggests itself: Is it permitted, since the number of distinct values of x cannot exceed 5, to suppose that

$$\Psi_m(x_\alpha) = \Psi_n(0),$$

or that the five roots, x_1, x_2, .. x_5, without considering in what order they will arise, may be expressed by

$$\Psi_1(0), \;\; \Psi_2(0), \;\; \ldots \; \Psi_5(0);$$

and thus to avoid the conclusion that the equation of the third degree, at which we shall arrive, will, in the ordinary meaning of the term, be simultaneous with $V^{15} + C_1 V^{14} + \ldots + C_{15} = 0$?

In fine, if we can effect the resolution of algebraic equations of the fifth degree, it must be possible to withdraw the terms involving x_α from $\Psi(x_\alpha)$ considered throughout its extent, although we retain those which involve x_α in $r(x_\alpha)$.

London, December 13, 1845.

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* See (31.).

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 27, 1845. "EXPERIMENTAL Researches in Electricity." By Michael Faraday Esq., D.C.L., F.R.S., &c. Nineteenth Series. Section 25: On the Magnetization of Light, and the Illumination of Magnetic Lines of Force.

For a long time past the author had felt a strong persuasion, derived from philosophical considerations, that among the several powers of nature which in their various forms of operation on matter produce different classes of effects, there exists an intimate relation; that they are connected by a common origin, have a reciprocal dependence on one another, and are capable, under certain conditions, of being converted the one into the other. Already have electricity and magnetism afforded evidence of this mutual convertibility; and in extending his views to a wider sphere, the author became convinced that these powers must have relations with light also. Until lately his endeavours to detect these relations were unsuccessful; but at length, on instituting a more searching interrogation of nature, he arrived at the discovery recorded in the present paper, namely, that a ray of light may be electrified and magnetized; and that lines of magnetic force may be rendered luminous.

The fundamental experiment revealing this new and important fact, which establishes a link of connexion between two great departments of nature, is the following. A ray of light issuing from an Argand lamp is first polarized in the horizontal plane by reflexion from a glass mirror, and then made to pass, for a certain space, through glass composed of silicated borate of lead, on its emergence from which it is viewed through a Nichol's eye-piece, capable of revolving on a horizontal axis, so as to intercept the ray, or allow it to be transmitted, alternately, in the different phases of its revolution. The glass through which the ray passes, and which the author terms the *dimagnetic*, is placed between the two poles of a powerful electro-magnet, arranged in such a position as that the line of magnetic forces resulting from their combined action shall coincide with, or differ but little from the course of the ray in its passage through the glass. It was then found that if the eye-piece had been so turned as to render the ray invisible to the observer looking through the eye-piece before the electric current had been established, it becomes visible whenever, by the completion of the circuit, the magnetic force is in operation; but instantly becomes again invisible on the cessation of that force by the interruption of the circuit. Further investigation showed that the magnetic action causes the plane of polarization of the polarized ray to rotate, for the ray is again rendered visible by turning the eye-piece to a certain extent; and that the direction of the rotation impressed upon the ray, when the magnetic influence is issuing from the south pole, and proceeding in the same direction as the polarized ray, is right-handed, or similar to that of the motion of the hands of a watch, as estimated by an observer at the eye-piece. The direction in which the rotation takes place will, of course, be reversed by reversing either the course of the ray or the poles of the magnet. Hence it follows that the polarized ray is made to rotate in the same direction as the currents of positive electricity are circulating, both in the helices composing the electro-magnet, and also in the same direction as the hypothetical currents, which, according to Ampère's theory, circulate in the substance of a steel magnet. The rotatory action was found to be always directly proportional to the intensity of the magnetic force, but not to that of the electric current; and also to be proportional to the length of that portion of the ray which receives the influence. The interposition of substances which occasion no disturbance of the magnetic forces, produce no change in these effects. Magnets consisting only of electric helices act with less power than when armed with iron, and in which magnetic action is consequently more strongly developed.

The author pursues the inquiry by varying in a great number of ways the circumstances in which this newly-discovered influence is exerted; and finds that the modifications thus introduced in the results are all explicable by reference to the general law above stated. Thus the effect is produced, though in a less degree, when the polarized ray is subjected to the action of an ordinary magnet, instead of one that derives its power from a voltaic current; and it is also weaker when a single pole only is employed. It is, on the other hand, increased by the addition of a hollow cylinder of iron, placed within the helix, the polarized ray traversing its axis being then acted upon with great energy. Helices act with equal power in any part of the cylindric space which they enclose. The heavy glass used in these experiments was found to possess in itself, no specific magneto-inductive action.

Different media differ extremely in the degree in which they are capable of exerting the rotatory power over a polarized ray of light. It is a power which has no apparent relation to the other physical properties, whether chemical or mechanical, of these bodies. Yet, however it may differ in its degree, it is always the same in kind; the rotation it effects is invariably in one direction, dependent, however, on the directions of the ray and of the magnetic force. In this respect it differs essentially from the rotatory power naturally possessed by many bodies, such as quartz, sugar, oil of turpentine, &c., which exhibit the phenomena of circular polarization; for in some of these the rotation takes place to the right, and in others to the left. When, therefore, such substances are employed as dimagnetics, the natural and the superinduced powers tend to produce either the same or opposite rotations; and the resulting effects are modified according as they are cumulative in the former case, and differential in the latter.

In the concluding section of the paper, the author enters into general considerations on the nature of the newly-discovered influence of electricity and magnetism over light, and remarks that all these powers possess in common a duality of character which constitutes them a peculiar class, and affords an opening which before was wanting for the appliance of these powers to the investigation of this and other radiant agencies. The phenomena thus brought to light confirm the views entertained by the author relative to the constitution of matter as being spheres of power, for the operation of which the conception of a solid nucleus is not necessary; and leads to the presumption that the influence of magnetism on bodies which exhibit no magnetic properties consists in producing in them a state of electric tension tending to a current; while on iron, nickel, and other bodies susceptible of magnetism, currents are actually established by the same influence.

The author states that he is still engaged in the prosecution of these inquiries.

"On the Action of the Rays of the Spectrum on Vegetable Juices:" being an Extract from a Letter by Mrs. M. Somerville to Sir John F. W. Herschel, Bart., dated Rome, September 20, 1845. Communicated by Sir John F. W. Herschel, Bart., F.R.S.

In the experiments of which the results are here recorded, the solar spectrum was condensed by a lens of flint glass of seven inches and a half focus, maintained in the same part of the screen by keeping a pin-hole or pencil-mark constantly at the corner of the red rays, which were sharply defined by being viewed through blue spectacles; and the apparatus was covered with black cloth in order to exclude extraneous light. Thick white letter-paper, moistened with the liquid to be examined, was exposed wet to the spectrum, as it was found that the action of the coloured light was thus rendered more immediate and more intense, than when the surface of the paper was dry.

The action of the spectrum at the junction of the lavender with the violet rays was found in some cases to be different from what it is with either of these colours separately, indicating a break in the continuity of action, and suggesting the idea of a secondary spectrum. In many instances the yellow and green rays exert a powerful influence on vegetable substances, an influence apparently unconnected with heat; for the darkening is generally least under the red rays and immediately below them, where the calorific rays are most abundant. The action, in a great number of cases, produces insulated spots in different parts of the spectrum, but more especially in the region of the rays of mean refrangibility, in which neither the calorific nor the chemical powers are the greatest. The point of maximum intensity is sometimes altered by the addition of acids, alkalies, or diluted alcohol. But altogether, as the author states, the action of the different parts of the spectrum seems to be very capricious, the changes of colour produced being exceedingly irregular and unaccountable.

XV. *Intelligence and Miscellaneous Articles.*

ACTION OF NITRIC ACID ON WAX.

WHEN wax is boiled in nitric acid, the same phænomena, according to M. Gerhardt, result as when the acid is made to act upon stearic acid or other fatty bodies; much nitrous vapour is disengaged; but the action is not so vivid, as when olive oil, for example, is treated by the acid.

About 4300 grains of wax were boiled with rather less than two pints of common nitric acid for about two hours, and the mixture, allowed to cool, became a solid mass; this was perfectly dissolved by carbonate of soda, with the production of slight effervescence. On cooling the whole became one mass; the wax was unctuous and of an apricot colour. After twenty-four hours' ebullition, the greater part of the wax was dissolved in the nitric acid; an oily substance, having the smell of rancid butter, floated on the solution; this was entirely dissolved by potash: this oil was acid, and could not be distilled without decomposing, and possessed all the properties attributed by M. Laurent to azoleic or œnanthylic acid. The formation of this acid has been observed to occur, as is well known, during the oxidizement of stearic and oleic acids, and other fatty bodies.

Wax was afterwards boiled with twice its weight of nitric acid, during several days, until all the oily matter disappeared; the first crystalline grains which deposited by the cooling of the solution, were pimelic acid, as shown by analysis, which gave carbon 52, hydrogen 7·8, indicating as its composite, carbon 52·5, hydrogen 7·5, oxygen 40 in 100 parts, or $C^7 H^6 O^4$.

The mother-water yielded a considerable quantity of adipic acid, but which appeared to be mixed with lipic acid. The last portions of the mother-water yielded no crystals, but were rendered turbid by the addition of water, and deposited fresh portions of oily azoleic acid. Lastly, when the wax was treated with nitric acid, till red vapours ceased to be produced, fine crystals of succinic acid were obtained. The formation of this body has been already shown by Mr. Ronalds.—*Ann. de Ch. et de Phys.*, Oct. 1845.

DRY DISTILLATION OF WAX.

M. C. Gerhardt states, that when wax is submitted to dry distillation, there condenses in the receiver a solid, white granular matter, floating in an oily liquid, and during the whole time of the operation a mixture of carbonic acid and bicarburetted hydrogen gases is evolved. The condensed portions consist of a fatty acid, a solid carburetted hydrogen, and several liquid carburetted hydrogens; the products become more and more impure as the operation approaches its termination, and sometimes, when the last remains of the wax are carbonized, a small quantity of a reddish solid matter is obtained. If the products be separately received at different times, it is found that the fatty acid passes first, and afterwards the solid carburetted hydrogen; the liquid carburetted hydrogens are among the last products. When the distillation is rapidly performed, there remains little else than a coaly residue.

The first portions of the distillation saponify almost entirely, except a few particles of solid carburetted hydrogen. The soap yields, by the action of hydrochloric acid, a perfectly white fatty acid; when crystallized once or twice from æther slightly alcoholized, this acid melts at 140° Fahr., and becomes a radiated mass on cooling; this acid, after being fused, yielded by analysis,—

Carbon	75·1
Hydrogen	12·8
Oxygen	12·1
	100·

which gives as the formula $C^{17} H^{17} O^{2}$, and the substance produced was therefore margaric acid. The solid carburetted hydrogen which accompanies the above substance is paraffin, as shown by the experiments of M. Ettling.—*Ann. de Ch. et de Phys.*, Nov. 1845.

ANALYSIS OF PHOSPHATE OF ALUMINA. BY M. A. DELESSE.

M. Danhauser discovered at Bernay, near Epernay, a white substance, considerably resembling alumina dried on a filter; it invested a gangue coloured by the oxides of iron and manganese, and appeared to belong to the plastic clay formation. Several collections in Paris contain specimens of it, but that examined by M. Delesse contained phosphoric acid.

In the closed tube this substance blackens and yields much water, containing bituminous matter; it is acid, reddens litmus paper, and appears also to corrode glass slightly, which may indicate the presence of a little hydrofluoric acid. In the outer flame of the blowpipe, the black colour produced by the carbon of the organic matter disappears and the substance becomes white; it is infusible. With the salt of phosphorus it readily dissolves, and a very transparent bead is formed; with carbonate of soda this substance swells, but does not dissolve; with nitrate of cobalt it yields a fine blue colour.

When not calcined this substance dissolves entirely and with the greatest facility in acids; it also dissolves, but with difficulty, in potash. After calcination, it is scarcely and with difficulty acted upon by acids.

It will be observed that the substance possesses all the properties of pure alumina, and, as already observed, it has the appearance of it; the presence of phosphoric acid was, however, ascertained by the process of Vauquelin and Thenard; it also contains a little lime, which is undoubtedly in the state of carbonate, for when acted upon by acids there is a disengagement of gas.

After several hours' drying, so as to expel the hygrometric moisture, the loss amounted to about 10 per cent; and by analysis the substance yielded,—

Phosphate of alumina	46
Water and organic matter......	49
Carbonate of lime and loss	5
	100

M. Delesse states that he did not possess a sufficient quantity of the mineral to determine the quantity of phosphoric acid; but it is evident that it must form a distinct species from wavellite, which contains 20 to 30 per cent. of water, while the phosphate of Bernon contains 49 per cent. and an organic substance. Vauquelin has also described, in the 21st vol. of the *Annales de Chimie et de Physique*, an hydrated phosphate of alumina, from the Isle of Bourbon, the composition of which is also different from that of wavellite, and likewise contains ammonia.—*Annales des Mines*, 1844.

A NEW PLANET.

The Astronomer Royal has forwarded to the *Times* newspaper the following letter from Prof. Encke of Berlin, relating the discovery of a new planet. Mr. Hind had previously communicated an extract of a letter from Prof. Schumacher, announcing the fact of Mr. Hencke's new planet, accompanied with a statement on the part of Mr. Hind, that he could not find any star answering the description of the supposed new one.

"Berlin, Dec. 15th.

"On the 13th of December, Mr. Hencke, of Driessen, gave notice that he had found a star of the ninth magnitude, in a place where before there was none. He gave its position by reference to the star-map of the Berlin Academy, 4th hour (which particular map was very carefully drawn by Prof. Knorr), from which its place appears to have been: Dec. 8.—At 8 hours; right ascension in arc, 65° 25′; declination north, 12° 41′.

"Yesterday, Dec. 14, we sought for it with our refractor, and found, by comparison with the star-map of the Berlin Academy (which alone, on account of the fulness of its details, could have enabled us to discover it), a star of the ninth magnitude, not marked in the map, whose place was: Dec. 14.—At 6 hours 28 min. mean time, right ascension in arc, 64° 4′ 53″·2. At 12 hours 43 min. mean time, right ascension in arc, 64° 1′ 10″·3.

"We then determined the following places with the wire micrometer, each place being the mean of five observations. At 13 hours 34 min. 55·6 sec. mean time, right ascension in time, 4 hours 16 min. 2·44 sec.; declination north, 12° 39′ 54″·2. At 13 hours 42 min. 36·5 sec., right ascension in time, 4 hours 16 min. 2·08 sec.; declination north, 12° 39′ 53″·1. At 14 hours 33 min. 27·1 sec., right ascension in time, 4 hours 16 min. 0·2 sec.; declination north, 12° 39′ 52″·1. Or, taking the mean, at 13 hours 56 min. 59·7 sec. mean time; right ascension in arc, 64° 0′ 23″·6; declination north, 12° 39′ 53″·1.

"The motion is retrograde, and its daily amount, as determined from the observations, eight hours apart, is—in right ascension, 14′ 21″·2 of arc; in declination it is quite insignificant.

"Mr. Hencke's place of December 8th agrees very nearly with this.

"The star is probably a new planet near its opposition. Vesta is pretty near it, and is also in opposition.

"On account of the difficulty of following it, I have thought it best to send you the news directly; and I beg you to make it known in England, that a sufficient number of observations may soon be collected. Excuse the shortness of this letter, which is written in great haste. Yours, &c. "ENCKE."

Professor Airy says, there appears to be no reasonable doubt that the object to which the foregoing relates is a new planet.

Mr. Hind has since observed the new star: At 0 h. 20 min. 15 sec., sidereal time, on Wednesday evening, the right ascension of the new planet was 4 hs. 8 min. 17·58 sec., and the declination 12° 45′ 32″·6, north. He was enabled to establish its motion in R.A. from the observations made at Mr. Bishop's Observatory, Regent's Park, on that evening. The planet has the appearance of a star of the ninth or tenth magnitude.—*Literary Gazette*, Dec. 27.

The following letter from the Astronomer Royal has since been published in the *Times* newspaper of the 29th inst.:—

Royal Observatory, Greenwich, Dec. 27.

Sir,—I have this day received from Prof. Schumacher a letter relating to the new planet, of which I request you to publish the following extract.

I am, Sir, your obedient Servant,
G. B. AIRY.

(Extract of a letter from Professor Schumacher.)

"Mr. Encke obtained an observation on the 20th of December, and this has enabled him to give an approximate sketch of the orbit of the new planet. I send you the elements:—

"Epoch of mean longitude, 1846, Jan. 0, at 0 hour, 89° 32′ 12″·1; longitude of perihelion, 214° 53′ 7″·0; longitude of ascending node, 119° 44′ 37″·5; inclination, 7° 42′ 8″·4; eccentricity, 0·207993; logarithm of semiaxis major, 0·42144; daily mean motion in longitude, 827″·65; periodic time, 1565 days.

"The discoverer has left the determination of the name to Mr. Encke, and Mr. Encke calls it 'Astræa.'

"Yours, &c., H. C. SCHUMACHER.

"Altona, Dec. 23."

NOTICE OF AN AURORA BOREALIS SEEN AT MANCHESTER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I will thank you to record in your Philosophical Magazine, &c. an aurora borealis which was seen at this place on Wednesday evening, the 3rd instant. It was first seen as a luminous arch about six o'clock; at half-past six the arch was complete throughout, from the eastern to the western horizon, with a span of upwards of 100°. The arch of light was perfectly steady and of an unusual breadth, much broader indeed than any I have before noticed. The altitude of the arch also was unusually great. *α Ursæ Majoris*, then near the meridian beneath the pole, was within the lower margin of the luminous band, whose highest point was about the meridian of *α Draconis*. From the upper edge of the western limb sprang several extensive streamers; but the light of the moon (then about four days old) prevented their being very brilliant. Shortly after seven, heavy clouds completely covered the aurora, showing their distance to be less than that of the luminous meteor which they obscured, and which, by their long continuance, they finally closed.

We had some very heavy hail showers during the day, and a perfect gale of wind the previous night. The same day a terrific thunderstorm visited a great part of Wales, killing several head of cattle and doing other serious damage to property.

I am, Gentlemen,
Your obedient Servant,
Manchester, December 19, 1845. W. STURGEON.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1845.

Chiswick.—November 1. Slight haze: very fine. 2. Slight fog: overcast. 3. Frosty: fine: clear and frosty. 4. Frosty, with dense fog: clear and frosty at night. 5. Frosty and foggy: very fine: overcast. 6. Very fine: rain. 7. Clear and fine: cloudy: rain. 8. Cloudy. 9. Very fine: slight rain. 10. Very fine: heavy clouds. 11. Hazy: rain. 12. Very fine. 13. Hazy: very fine. 14. Foggy throughout. 15. Foggy: fine. 16. Densely clouded: rain. 17. Fine: rain. 18. Cloudy: clear. 19. Boisterous, with rain: showery: very clear at night. 20. Fine. 21. Overcast: heavy rain. 22. Fine: clear and cold. 23. Sharp frost: fine. 24. Very fine: foggy at night. 25. Uniformly overcast: slight rain: foggy. 26. Densely overcast. 28. Cloudy. 29. Heavy rain. 30. Cloudless: overcast at night.—Mean temperature of the month 1°·43 above the average.

Boston.—Nov. 1. Fine. 2, 3. Cloudy. 4—7. Fine. 8. Cloudy: rain early A.M. 9. Fine. 10. Foggy. 11. Fine: rain P.M. 12. Cloudy. 13. Fine. 14, 15. Cloudy. 16. Cloudy: rain early A.M. 17. Cloudy: rain early A.M.: rain P.M. 18. Cloudy: rain early A.M. 19. Stormy: rain A.M. 20—23. Fine. 24. Fine: snow and rain early A.M. 25—28. Cloudy. 29. Cloudy: rain P.M. 30. Fine.

Sandwick Manse, Orkney.—Nov. 1. Bright: cloudy. 2. Fine: cloudy. 3. Fine: frost: cloudy. 4. Bright: clear. 5. Clear. 6. Damp: cloudy. 7. Damp: hazy. 8. Drizzle: cloudy. 9. Cloudy: damp. 10. Damp. 11. Cloudy: fog in valleys. 12. Frosty: fog: clear. 13. Fine. 14. Fine: frost: fine. 15. Fine: cloudy. 16. Fine: rain. 17. Fine: showers. 18. Cloudy. 19. Rain: cloudy. 20. Showers. 21. Showers: sleet. 22. Cloudy: showers. 23. Cloudy: snow-showers. 24. Cloudy: snow: rain. 25. Showers: rain. 26. Showers: thunder and showers. 27. Showers: hail: showers. 28. Cloudy: showers. 29. Cloudy: showers: sleet. 30. Sleet-showers: snow on hills.

Applegarth Manse, Dumfries-shire.—Nov. 1. Fair and fine. 2. Fair and chilly. 3. Fair, but dull: frost A.M. 4. Frost, hoar: clear and cold. 5. Frost: dull. 6. Fair and fine: fresh. 7—10. Rain early A.M. 11. Fair and fine. 12—13. Hoar-frost: fine. 14. Raw and cloudy. 15. Rain P.M. 16. Heavy rain P.M. 17. Fine: dry. 18, 19. Heavy showers. 20. Fine A.M.: rain P.M. 21. Showers. 22. Frost. 23. Frost: a few drops of rain. 24. Frost: cloudy P.M. 25. Wet. 26—28. Very heavy rain. 29. Showers. 30. Heavy rain P.M.

Mean temperature of the month	42°·7
Mean temperature of Nov. 1844	43 ·6
Mean temperature of Nov. for twenty-three years.	40 ·2

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Nov.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
1.	30·177	30·132	29·80	30·08	30·09	30·04	30·22	55	32	38	53	41	47½	44½	e.	calm	n.	n.	……	……	……	·05
2.	30·273	30·186	29·80	30·15	30·20	30·32	30·30	56	26	46·5	49½	41	44½	44½	e.	calm	e.	se.	……	……	……	……
3.	30·317	30·288	29·91	30·21	30·19	30·22	30·10	54	20	47	47	33	40	44	e.	calm	se.	sse.	……	……	……	……
4.	30·268	29·965	29·90	30·04	29·82	29·90	29·75	42	22	38	45½	28½	47½	45	ne.	calm	e.	sse.	……	……	……	……
5.	29·835	29·722	29·54	29·63	29·51	29·64	29·53	58	42	39	48	30½	45	45	s.	calm	ese.	se.	……	……	……	……
☽ 6.	29·634	29·313	29·32	29·40	29·20	29·46	29·42	62	47	50	56½	43	50	50	se.	s.	se.	se.	·05	……	……	·15
7.	29·539	29·411	29·10	29·22	29·26	29·20	29·39	57	49	51	53½	50	50	48	s.	sw.	se.	se.	·06	……	0·26	……
8.	29·451	29·335	29·10	29·29	29·38	29·45	29·49	60	40	52	53	49½	49½	49	s.	calm	se.	s.	·01	·09	……	……
9.	29·516	29·478	29·16	29·36	29·39	29·57	29·50	58	38	47	54	44	47	49	s.	calm	e.	se.	·03	……	……	·09
10.	29·387	29·340	29·00	29·30	29·20	29·43	29·40	54	35	48	51	45	48	47	s.	calm	e.	……	·04	……	……	·10
11.	29·255	29·185	28·97	29·28	29·31	29·49	29·52	49	42	47	50	40	47	40	e.	calm	ne.	……	·13	……	……	·14
12.	29·662	29·460	29·00	29·40	29·56	29·60	29·69	52	26	49	49½	36	38	44	sw.	calm	e.	nw.	·01	·11	……	……
13.	29·935	29·725	29·40	29·72	29·82	29·88	29·94	50	32	38	46½	30½	39½	40½	n.	calm	e.	s.	……	……	……	·11
○ 14.	30·028	29·970	29·66	29·88	29·80	29·93	29·83	45	30	43	41	30½	41	39	sw.	calm	ne.	s.	……	……	0·28	……
15.	29·918	29·719	29·54	29·63	29·50	29·63	29·33	53	41	41·5	48	31½	43	46½	se.	calm	e.	s.	·06	……	……	……
16.	29·624	29·263	29·21	29·30	28·89	29·39	29·17	53	39	51	51½	45	43½	43	s.	calm	ese.	ne.	·11	·09	……	……
17.	29·362	29·283	28·90	29·04	29·23	29·15	29·16	55	39	45	49	43½	45	43½	w.	calm	wnw.	nw.	·17	·06	……	·33
18.	29·439	29·155	28·79	28·90	29·00	29·12	29·03	57	43	48	49	37	43	43	sw.	w.	ssw.	……	·03	·32	……	·10
19.	29·123	29·077	28·60	28·60	28·52	28·50	28·31	58	46	56	51	46½	47	46	sw.	sw.	ssw.	se.	·09	……	……	·23
20.	29·428	29·143	28·63	28·77	28·98	28·27	28·72	54	35	47	47½	42	47	46	sw.	w.	ssw.	w.	·33	·18	……	·25
21.	29·485	29·382	29·06	29·20	29·30	28·97	29·18	54	32	38	46	38½	46½	42	sw.	calm	w.	w.	·36	……	1·30	·05
☾ 22.	29·680	29·555	29·20	29·42	29·55	29·44	29·64	46	23	35	41½	28½	41½	39½	w.	calm	ssw.	n.	……	……	……	·32
23.	29·981	29·836	29·49	29·76	29·74	29·73	29·88	43	27	36	40	26½	39	31½	w.	calm	wnw.	n.	……	……	……	·03
24.	30·144	29·995	29·68	29·94	29·81	29·94	29·50	43	23	34	37½	28	36	42	n.	calm	nw.sw	s.	……	·04	……	·20
25.	30·021	29·937	29·61	29·60	29·51	29·32	29·11	50	35	38	48½	36	44	46	sw.	w.	sw.	s.	·06	……	……	·42
26.	29·846	29·814	29·33	29·30	29·31	29·48	28·98	54	49	52	53½	47	44	45½	sw.	calm	wsw.	w.	……	……	……	·57
27.	29·957	29·857	29·49	29·50	29·45	29·23	29·38	55	47	54	51	46	46	45	sw.	w.	sw.	w.	……	……	……	·48
28.	29·580	29·549	29·24	29·34	29·15	29·41	29·20	53	41	49	51	47½	39	46	s.	calm	ssw.	ssw.	·04	……	……	……
● 29.	29·733	29·702	29·33	29·28	29·49	29·03	29·30	52	34	46	51	43½	42	41	sw.	calm	w-sw.	w.	·51	……	……	·08
30.	29·936	29·885	29·45	29·55	29·33	29·41	29·07	52	37	37	46	36	41	42	w.	calm	sw.	ssw.	·02	·24	1·45	·31
Mean.	29·744	29·622	29·30	29·469	29·449	29·471	29·434	52·80	35·73	44·7	48·6	36·8	44·06	43·93					2·11	1·13	3·29	4·01

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XVI. *On the Application of the Photographic Camera to Meteorological Registration. By* HENRY COLLEN, *Esq.**

[With a Plate.]

IN April 1844, Mr. Ronalds applied to me for the purpose of obtaining some photographic representations of figures, forming "a sort of pictorial register of atmospheric electricity" upon glass plates coated with Canada balsam, which figures had been executed at the Kew Observatory by means of his electrograph, described in the Fourteenth Report of the British Association. The desired result was quickly obtained by the usual photogenic process, and also by the camera; the latter being found however, as was to be expected, the greatly superior mode. Several other impressions were afterwards made from figures on coated metallic plates, some of which were shown attached to Mr. Ronalds's report to the meeting at York. The sharpness and delicacy of the positive impressions thus obtained gave rise to some experiments, made by us conjointly, for the purpose of applying the photographic camera to the registration of Volta's electrometer, the thermometer, and the siphon barometer. The projection of shadows on photographic paper, which, by the way, had been already proposed and tried by several persons, was at once objected to by Mr. Ronalds, whose knowledge of the delicacy required in observing and registering the various instruments at the Observatory, made him fully aware of the necessity of obtaining as perfect definition as the best optical arrangement would produce; an excellent compound lens, made and kindly lent to us by Mr. Ross, was therefore used, and has been employed on each of the instruments, *i. e.* the electro-

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* Communicated by the Author.

meter, the barometer, and the thermometer, and a series of experimental observations permanently registered at Kew.

The accompanying figure (Plate III. fig. 1) is part of a day's registration of the effect of atmospheric electricity on Volta's electrometer; the gradual decline of daylight is shown, and also the continuation of the registration, by artificial light; without the use of the latter, it is obvious that the application of photography to these purposes would be very incomplete, if not wholly useless; and it may perhaps, in some cases, be advisable to make its use constant.

The various intensities of light from a *clouded* sky frequently give rise (of course) to variations in depth of tint on the paper, which thus becomes an approximation to Sir John Herschel's actinograph; and it may be here worth while to remark, that sometimes, when with *such a sky* these intensities of action on the paper are *augmented*, the electricity of serene weather manifests a tendency to *increase* also; this fact may be compared with the almost invariable tendency of the sun's light and heat, in a *clear* sky, to *diminish* the tension of those electrometers which receive their charges by absorption.

The calotype process is that which is used, being, of all those upon paper, the most sensitive, which quality is highly essential during the use of artificial light; it is very advantageously employed for these purposes, instead of the Daguerreotype, on account of its cheapness, and also on account of the facility with which representations can be obtained of any required length.

In the apparatus at present constructed, the paper is moved by a clock at the rate of one inch per hour, and is cut into pieces nine inches long; but for constant use they should be twelve inches long, so that by the introduction of two pieces during twenty-four hours, a continuous register of the effects would be preserved without further attention than the application of the artificial light (if not used constantly) at the decline of daylight; at present an Argand lamp is used, which, of course, requires some attention, but where available, a common gas-light would be greatly preferable; this however is not the case at the Kew Observatory, and for this reason only, the experiments have not been continued during the night.

The construction of the apparatus is very simple, although many tedious experiments have been made to produce the result; it consists essentially of the following arrangement:—The instrument to be registered is placed so as to be between the light and a lens of considerable aperture, with very short focus, and flat field of sufficient extent for the purpose; and the paper is placed so as to be in the exact focus for obtaining an image of the same size as, or larger than, the object. When the electrometer is the instrument to be registered, the figures of the extreme ends only of the straws are allowed to fall upon the paper, an opake diaphragm pierced with a slit, the curve of which is part of a circle of which the length of the straws is the radius, being placed very near the paper.

In the registration of the thermometer or barometer, the difficulty arising from the refraction of light by the glass tube was proposed to be met in two ways, the first of which, the one adopted, consists in the use of a diaphragm with a straight slit, which can be opened from, or contracted towards, its exact centre by a very simple arrangement, and is placed in front of the mercury, *i. e.* on the side next to the light, so as to regulate the quantity admitted; this regulation has also the effect of preserving the necessary sharpness of figure, which too much light tends to injure.

The second method proposed, which has not yet been tried, consists in the employment of a piece of glass tube, the bore of which is a trifle larger than the outside of the tube of the instrument; this, having two opposite surfaces ground flat and polished, and being long enough to include the range of variation, is cemented on to the tube of the instrument with Canada balsam, and would render it easy (by making all but a central slit opake) to get rid of the partial illumination of the column of mercury on the side which is required, for a good impression on the paper, to be quite dark.

The surface of the mercury in the barometer sustains a blackened pith-ball of the same diameter as the bore of the tube, but freely sliding therein; it is proposed however to make a float of platinum foil with a sharp edge, which will probably be found to be more advantageous.

The thermometer used is mercurial, with a *broad* flat bore.

The wet-bulb, hair hygrometer, &c., as well as every other instrument which by its action affords a distinct sign, may obviously be registered in the same manner.

Several minor points of difficulty remain still to be overcome, but it is hoped that enough has been done to justify the expectation that the photographic camera may become a really useful and convenient instrument in the hands of the exact meteorologist.

The electrical experiments were made by means of a small conductor, insulated for the occasion; Mr. Ronalds not feeling either authorised, or disposed, to interrupt the course of observations carried on by means of the ordinary high conductor, until the proposed mode of registration is quite matured.

XVII. *On* Fresnel's *Theory of the Aberration of Light.* *By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge*[*].

THE theory of the aberration of light, and of the absence of any influence of the motion of the earth on the laws of refraction, &c., given by Fresnel in the ninth volume of the *Annales de Chimie*, p. 57, is really very remarkable. If we suppose the diminished velocity of propagation of light within refracting media to arise solely from the greater density of the æther within them, the elastic force being the same as without, the density which it is necessary to suppose the æther within a medium of refractive index μ to have is μ^2, the density in vacuum being taken for unity. Fresnel supposes that the earth passes through the æther without disturbing it, the æther penetrating the earth quite freely. He supposes that a refracting medium moving with the earth carries with it a quantity of æther, of density μ^2-1, which constitutes the excess of density of the æther within it over the density of the æther in vacuum. He supposes that light is propagated through this æther, of which part is moving with the earth, and part is at rest in space, as it would be if the whole were moving with the velocity of the centre of gravity of any portion of it, that is, with a velocity $\left(1-\frac{1}{\mu^2}\right)v$, v being the velocity of the earth. It may be observed however that the result would be the same if we supposed the whole of the æther within the earth to move together, the æther entering the earth in front, and being immediately condensed, and issuing from it behind, where it is immediately rarefied, undergoing likewise sudden condensation or rarefaction in passing from one refracting medium to another. On this supposition, the evident condition that a mass v of the æther must pass in a unit of time across a plane of area unity, drawn anywhere within the earth in a direction perpendicular to that of the earth's motion, gives $\left(1-\frac{1}{\mu^2}\right)v$ for the velocity of the æther within a refracting medium. As this idea is rather simpler than Fresnel's, I shall adopt it in considering his theory. Also, instead of considering the earth as in motion and the æther outside it as at rest, it will be simpler to conceive a velocity equal and opposite to that of the earth impressed both on the earth and on the æther. On this supposition the earth will be at rest; the æther outside it will be moving with a velocity v, and the æther in a refracting medium with a velocity

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[*] Communicated by the Author.

$\frac{v}{\mu^2}$, in a direction contrary to that of the earth's real motion. On account of the smallness of the coefficient of aberration, we may also neglect the square of the ratio of the earth's velocity to that of light; and if we resolve the earth's velocity in different directions, we may consider the effect of each resolved part separately.

In the ninth volume of the *Comptes Rendus* of the Academy of Sciences, p. 774, there is a short notice of a memoir by M. Babinet, giving an account of an experiment which seemed to present a difficulty in its explanation. M. Babinet found that when two pieces of glass of equal thickness were placed across two streams of light which interfered and exhibited fringes, in such a manner that one piece was traversed by the light in the direction of the earth's motion, and the other in the contrary direction, the fringes were not in the least displaced. This result, as M. Babinet asserts, is contrary to the theory of aberration contained in a memoir read by him before the Academy in 1829, as well as to the other received theories on the subject. I have not been able to meet with this memoir, but it is easy to show that the result of M. Babinet's experiment is in perfect accordance with Fresnel's theory.

Let T be the thickness of one of the glass plates, V the velocity of propagation of light in vacuum, supposing the æther at rest. Then $\frac{V}{\mu}$ would be the velocity with which light would traverse the glass if the æther were at rest; but the æther moving with a velocity $\frac{v}{\mu^2}$, the light traverses the glass with a velocity $\frac{V}{\mu} \pm \frac{v}{\mu^2}$, and therefore in a time

$$T \div \left(\frac{V}{\mu} \pm \frac{v}{\mu^2}\right) = \frac{\mu T}{V}\left(1 \mp \frac{v}{\mu V}\right).$$

But if the glass were away, the light, travelling with a velocity $V \pm v$, would pass over the space T in the time

$$T \div (V \pm v) = \frac{T}{V}\left(1 \mp \frac{v}{V}\right).$$

Hence the retardation, expressed in time, $= (\mu - 1)\frac{T}{V}$, the same as if the earth were at rest. But in this case no effect would be produced on the fringes, and therefore none will be produced in the actual case.

I shall now show that, according to Fresnel's theory, the laws of reflexion and refraction in singly refracting media are uninfluenced by the motion of the earth. The method which I employ will, I hope, be found simpler than Fresnel's; besides it applies easily to the most general case. Fresnel has not given the calculation for reflexion, but has merely stated the result; and with respect to refraction, he has only considered the case in which the course of the light within the refracting medium is in the direction of the earth's motion. This might still leave some doubt on the mind, as to whether the result would be the same in the most general case.

If the æther were at rest, the direction of light would be that of a normal to the surfaces of the waves. When the motion of the æther is considered, it is most convenient to define the direction of light to be that of the line along which the *same portion* of a wave moves relatively to the earth. For this is in all cases the direction which is ultimately observed with a telescope furnished with cross wires. Hence, if A is any point in a wave of light, and if we draw A B normal to the wave, and proportional to V or $\frac{V}{\mu}$, according as the light is passing through vacuum or through a refracting medium, and if we draw B C in the direction of the motion of the æther, and proportional to v or $\frac{v}{\mu^2}$, and join A C, this line will give the direction of the ray. Of course, we might equally have drawn A D equal and parallel to B C and in the opposite direction, when D B would have given the direction of the ray.

Let a plane P be drawn perpendicular to the reflecting or refracting surface and to the waves of incident light, which in this investigation may be supposed plane. Let the velocity v of the æther in vacuum be resolved into p perpendicular to the plane P, and q in that plane; then the resolved parts of the velocity $\frac{v}{\mu^2}$ of the æther within a refracting medium will be $\frac{p}{\mu^2}$, $\frac{q}{\mu^2}$. Let us first consider the effect of the velocity p.

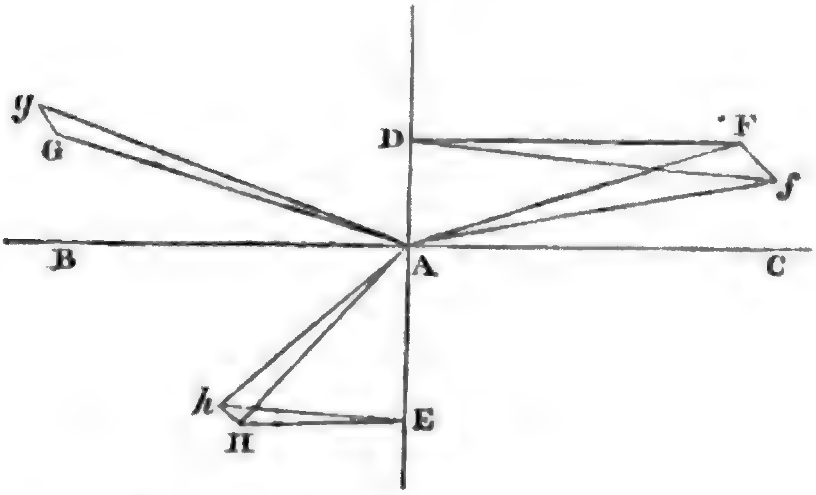
It is easy to see that, as far as regards this resolved part of the velocity of the æther, the directions of the refracted and reflected *waves* will be the same as if the æther were at rest. Let BAC (fig. 1) be the intersection of the refracting surface and the plane P; D A E a normal to the refracting surface; A F, A G, A H normals to the incident, reflected and refracted waves. Hence A F, A G, A H will be in the plane P, and

$$\angle \mathrm{GAD} = \mathrm{FAD}, \quad \mu \sin \mathrm{HAE} = \sin \mathrm{FAD}.$$

Take $$\mathrm{AG} = \mathrm{AF}, \quad \mathrm{AH} = \frac{1}{\mu}\,\mathrm{AF}.$$

Draw G g, H h perpendicular to the plane P, and in the direction of the resolved part p of the velocity of the æther, and

Fig. 1.



F f in the opposite direction; and take

$$\mathrm{F}f : \mathrm{H}h : \mathrm{F\,A} :: p : \frac{p}{\mu^2} : \mathrm{V}, \text{ and } \mathrm{G}g = \mathrm{F}f,$$

and join A with f, g and h. Then fA, Ag, Ah will be the directions of the incident, reflected and refracted rays. Draw F D, H E perpendicular to D E, and join fD, hE. Then fD F, hE H will be the inclinations of the planes fA D, h A E to the plane P. Now

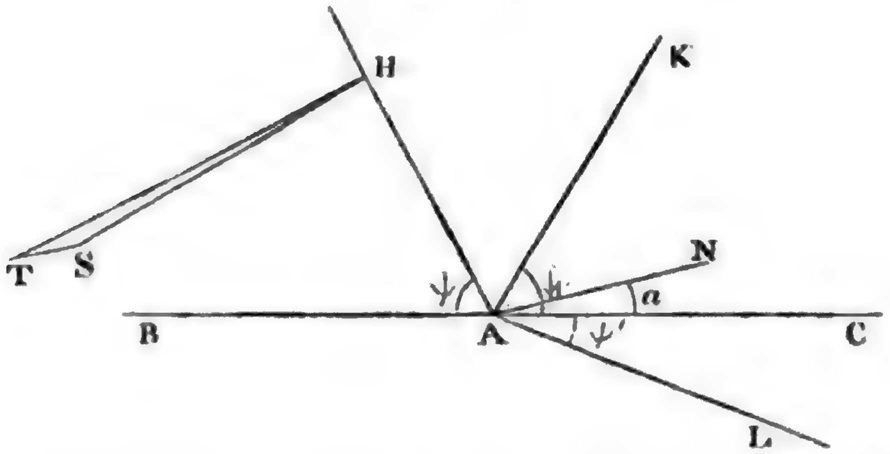
$$\tan \mathrm{F\,D}f = \frac{p}{\mathrm{V} \sin \mathrm{F\,A\,D}}, \quad \tan \mathrm{H\,E}h = \frac{\mu^{-2} p}{\mu^{-1} \mathrm{V} \sin \mathrm{H\,A\,E}},$$

and $\sin \mathrm{F\,A\,D} = \mu \sin \mathrm{H\,A\,E}$; therefore $\tan \mathrm{F\,D}f = \tan \mathrm{H\,E}h$, and therefore the refracted ray Ah lies in the plane of incidence fA D. It is easy to see that the same is true of the reflected ray Ag. Also $\angle\, g\mathrm{A\,D} = f\mathrm{A\,D}$; and the angles fA D, h A E are sensibly equal to F A D, H A E respectively, and we therefore have without sensible error, $\sin f\mathrm{A\,D} = \mu \sin h\mathrm{A\,E}$. Hence the laws of reflexion and refraction are not sensibly affected by the velocity p.

Let us now consider the effect of the velocity q. As far as depends on this velocity, the incident, reflected and refracted rays will all be in the plane P. Let A H, A K, A L be the intersections of the plane P with the incident, reflected and refracted waves. Let ψ, $\psi_{\prime}$, ψ' be the inclinations of these waves to the refracting surface; let N A be the direction of the resolved part q of the velocity of the æther, and let the angle N A C $= \alpha$.

The resolved part of q in a direction perpendicular to A H is $q \sin(\psi + \alpha)$. Hence the wave A H travels with the velocity $\mathrm{V} + q \sin(\psi + \alpha)$; and consequently the line of its intersection with the refracting surface travels along A B with the

Fig. 2.



velocity $\operatorname{cosec}\psi\,\{V+q\sin(\psi+\alpha)\}$. Observing that $\frac{q}{\mu^2}$ is the velocity of the æther within the refracting medium, and $\frac{V}{\mu}$ the velocity of propagation of light, we shall find in a similar manner that the lines of intersection of the refracting surface with the reflected and refracted waves travel along A B with velocities

$$\operatorname{cosec}\psi_,\,\{V+q\sin(\psi_,-\alpha)\},\quad \operatorname{cosec}\psi'\left\{\frac{V}{\mu}+\frac{q}{\mu^2}\sin(\psi'+\alpha)\right\}$$

But since the incident, reflected and refracted waves intersect the refracting surface in the same line, we must have

$$\left.\begin{aligned}\sin\psi_,\{V+q\sin(\psi+\alpha)\}&=\sin\psi\{V+q\sin(\psi_,-\alpha)\},\\ \mu\sin\psi'\{V+q\sin(\psi+\alpha)\}&=\sin\psi\left\{V+\frac{q}{\mu}\sin(\psi'+\alpha)\right\}.\end{aligned}\right\}\quad (A)$$

Draw H S perpendicular to A H, S T parallel to N A, take S T : H S :: q : V, and join H T. Then H T is the direction of the incident ray; and denoting the angles of incidence, reflexion and refraction by φ, $\varphi_,$, φ', we have

$$\varphi-\psi=\text{S H T}=\frac{\text{S T}\sin\text{S}}{\text{S H}}=\frac{1}{V}\times\text{ resolved part of } q \text{ along A H}$$

$$=\frac{q}{V}\cos(\psi+\alpha).\quad\text{Similarly,}$$

$$\varphi_,-\psi_,=\frac{q}{V}\cos(\psi_,-\alpha),\quad \varphi'-\psi'=\frac{q}{\mu V}\cos(\psi'+\alpha):$$

whence
$$\sin\psi=\sin\varphi-\frac{q}{V}\cos\varphi\cos(\varphi+\alpha),$$

$$\sin\psi_,=\sin\varphi_,-\frac{q}{V}\cos\varphi_,\cos(\varphi_1-\alpha),$$

$$\sin\psi' = \sin\phi' - \frac{q}{\mu V}\cos\phi'\cos(\phi' + \alpha).$$

On substituting these values in equations (A), and observing that in the terms multiplied by q we may put $\phi_{,} = \phi$, $\mu\sin\phi' = \sin\phi$, the small terms destroy each other, and we have $\sin\phi_{,} = \sin\phi$, $\mu\sin\phi' = \sin\phi$. Hence the laws of reflexion and refraction at the surface of a refracting medium will not be affected by the motion of the æther.

In the preceding investigation it has been supposed that the refraction is out of vacuum into a refracting medium. But the result is the same in the general case of refraction out of one medium into another, and reflexion at the common surface. For all the preceding reasoning applies to this case if we merely substitute $\frac{p}{\mu'^2}$, $\frac{q}{\mu'^2}$ for p, q, $\frac{V}{\mu'}$ for V, and $\frac{\mu}{\mu'}$ for μ, μ' being the refractive index of the first medium. Of course refraction out of a medium into vacuum is included as a particular case.

It follows from the theory just explained, that the light coming from any star will behave in all cases of reflexion and ordinary refraction precisely as it would if the star were situated in the place which it appears to occupy in consequence of aberration, and the earth were at rest. It is, of course, immaterial whether the star is observed with an ordinary telescope, or with a telescope having its tube filled with fluid. It follows also that terrestrial objects are referred to their true places. All these results would follow immediately from the theory of aberration which I proposed in the July number of this Magazine; nor have I been able to obtain any result, admitting of being compared with experiment, which would be different according to which theory we adopted. This affords a curious instance of two totally different theories running parallel to each other in the explanation of phænomena. I do not suppose that many would be disposed to maintain Fresnel's theory, when it is shown that it may be dispensed with, inasmuch as we would not be disposed to believe, without good evidence, that the æther moved quite freely through the solid mass of the earth. Still it would have been satisfactory, if it had been possible, to have put the two theories to the test of some decisive experiment.

XVIII. *Observations on the Development and Growth of the Epidermis. By* Erasmus Wilson, *F.R.S., Lecturer on Anatomy and Physiology in the Middlesex Hospital*[*].

IT is the commonly received doctrine at the present day, that the cells of the epidermis and of epithelium in general, originate out of materials furnished by the liquor sanguinis or plasma of the blood. In order that this purpose may be effected, the liquor sanguinis is conveyed by endosmosis through the walls of the capillary vessels and through the peripheral boundary of the surface, the "basement membrane" of Bowman. Having reached the exterior plane of the latter, the changes commence which result in the development of granules in the previously fluid liquor sanguinis, or rather, perhaps, in the aggregation of the molecules of the organisable material or blastema, which was previously held in intimate suspension or solution by the liquor sanguinis. Out of the body an action of this kind would be termed coagulation, and where inorganic matter is concerned, crystallization. The process to which I am now referring, though taking place within the body, is analogous to these phænomena, with the difference of being controlled and directed by the power of life, of being, in point of fact, a vital coagulation or crystallization. Indeed, coagulation, although occurring out of the body, and sometimes after the lapse of a considerable period, may be regarded as the last act of vital existence, or as a vestige of the atmosphere of life with which the coagulating fluid was previously charged in abundance.

As regards the tissue under consideration, there is every ground for belief, that the organisable material or blastema of the liquor sanguinis is appropriated by the epidermis the very instant it reaches the exterior plane of the "basement membrane;" some portion of it, and the greater part of the serum of the liquor sanguinis, being taken up by the newly-formed cells to be transmitted in succession to more superficial ranges of cells, and the remaining portion being converted on the spot into the primitive granules of the tissue. This belief is supported by the fact of the absence of any fluid stratum between the epidermis and the dermis, and by the close connexion known to subsist between those two membranes. It is well known that to separate the epidermis from the dermis, until the former is so thoroughly saturated with fluid by maceration as to have acquired a considerable addition to its dimensions in all directions, or until decomposition has com-

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[*] Read before the Royal Society, June 19, 1845, and communicated by the Author.

menced, is next to impossible; and in the living state of the body, separation never takes place until the mutual connexion between the layers has been destroyed by the effusion of fluid. The microscope gives additional weight to this evidence. I have observed that the cells of the deep surface of the epidermis are in immediate contact with the boundary limit of the dermis, and that moreover it is frequently difficult to determine the exact line between them. I have also made the following experiment:—I cut very thin vertical slices of the skin, at daily periods, from the moment of death until decomposition had become established, and submitted them to the action of the compressor on the field of the microscope, but in every instance, while fresh, the two tissues yielded to the pressure in equal proportion without any separation occurring. As soon, however, as decomposition had commenced, separation was produced, and in the early stages took place with difficulty. This experiment proves that the firm adhesion subsisting between the epidermis and dermis is not alone due to the numerous inflexions of the former into the latter, which take place at the sudoriferous tubes, hair tubes, and sebaceous ducts, although these inflexions must co-operate powerfully in the result.

Being desirous of examining the under surface of the epidermis with the higher powers of the microscope, and failing in all my attempts to effect this object by taking the entire thickness of the epidermis or scraping, I awaited the first indication of its separation from the dermis, and then removing it carefully made a thin slice parallel with the surface which I wished to examine. This plan succeeded beyond my expectations; for not only did I obtain parts so diaphanous as to enable me to see the surface distinctly, but the septa between the depressions for the papillæ of the dermis afforded natural laminæ of such transparency as permitted their structure to be well examined.

When the under surface of the epidermis was exposed to view, I found it to be composed of four kinds of elements, arranged in such a manner as to constitute an irregular mosaic plane. These elements are,—1, *granules*, measuring about $\frac{1}{20000}$th of an inch in diameter; 2, *aggregated granules*, measuring about $\frac{1}{10000}$th; 3, *nucleated granules*, measuring $\frac{1}{6000}$th to $\frac{1}{4000}$th; 4, *cells*, measuring $\frac{1}{3000}$th to $\frac{1}{2300}$th of an inch.

1. The granules, which I may distinguish by the name of *primitive granules*, are globular in form, homogeneous, solid, brightly illumined by transmitted light when the centre is under the focus of the microscope, but dark when viewed upon the surface, the darkness being increased whenever they are

congregated in clusters. These granules I conceive to be the first organic shape of the blastema of the liquor sanguinis.

2. The *aggregated granules*, measuring about $\frac{1}{10000}$th of an inch in diameter, are minute masses, composed of four, five or six of the preceding, or as many as can be aggregated without leaving an unoccupied space in the centre of the mass. With an imperfect focus, these granules have the appearance of possessing a transparent globular nucleus, but this appearance ceases when the focus is perfect, and then the component granules are quite obvious, and the centre becomes a dark point, namely, the shadow caused by the meeting of the primitive granules.

3. The *nucleated granules*, measuring between $\frac{1}{6000}$th and $\frac{1}{4000}$th of an inch in diameter, are in point of construction an "aggregated granule" with a single tier of "aggregated granules" arranged around it, so as to give the entire mass a circular or oval form. The central aggregated granule has now become a nucleus, and at the same time has undergone other changes which indicate its longer existence. For example, the primitive granules composing it are denser than they were originally, and they are separated from each other by a very distinct interstitial space filled with a transparent and homogeneous matter. Sometimes this interstitial substance presses the granules asunder equally on all sides, constituting a circular nucleus; but more frequently two opposite granules are more widely separated than the rest, and the nucleus receives an elongated form. The interstitial substance is most conspicuous at the line of junction of the nucleus with the secondary tier of "aggregated granules," and in this situation gives a defined character to the nucleus. Close observation and a perfect focus render it quite obvious that the peripheral tier of granules are in reality aggregated, they are lighter than the shaded granules of the nucleus, and apparently softer in texture.

The nucleated granules are more or less flattened in form, and present a flat surface of contact with the dermis. It is the latter circumstance that gives the facility of determining their mode of construction.

4. The cells of the deep stratum of the epidermis, measuring $\frac{1}{3000}$th to $\frac{1}{2500}$th of an inch in their long diameter, are the most striking feature of this layer, and may be said to be its chief constituent. They originate, as is evident from their structure, in the nucleated granules previously described, and consist of a transparent layer added to the exterior of the former; or, if I might be permitted to describe them as they appear in their tesselated position, they are constituted by the

addition of a transparent border to the last-described nucleated granule. The periphery of this transparent border is bounded by a dark interstitial substance, which gives the border a defined outline; and in the latter situation I imagine a cell-membrane to exist. I am not satisfied, however, that this is the case; and the difficulty of isolating these cells, and their roughness of outline when separated, serve to prove that if a membrane be really present, it must be exceedingly thin and easily torn. Assuming therefore, from analogy rather than from demonstrative evidence, that there exists a boundary membrane to the bodies I am now describing, I have termed them "cells;" the cavity of the cell I apprehend to be the "transparent border," the "nucleated granule" is the *nucleus* of the cell, the "aggregated granule" of the latter the *nucleolus*, and the entire body a "nucleolo-nucleated cell."

Before quitting the structure of the "nucleolo-nucleated cell," or primitive cell of the epidermis, there is a point of much interest to be mentioned with regard to it, which is, that the "transparent border" just described is itself a tier of "aggregated granules." The nucleolus therefore is an "aggregated granule," the nucleus a tier (taking its flat surface) of "aggregated granules" surrounding the former, and the cell a tier of "aggregated granules" enclosing the whole.

To return to the mosaic-like plane of the under surface of the epidermis, the largest of the pieces composing this plane are the nucleolo-nucleated cells. These are placed without order, some being closely pressed together, others being separated by moderate intervals, and here and there some separated by interspaces equal to the breadth of the cells. The interspaces, or intercellular spaces, are occupied by the "nucleated granules," "aggregated granules," and "primitive granules," irregularly set in a homogeneous interstitial substance, which fills up all vacuities. The granules and interstitial substance modify the light transmitted through them variously at different foci of the microscope; sometimes the granules look dark while the interstitial substance is light, and sometimes the reverse is the case.

Such is the structure of the mosaic-like plane of the under surface of the epidermis, and so far, my observations, having reference to facts, are demonstrable and admit of being spoken to positively. The interpretation of the facts I would willingly leave to others, but feel that I am called upon to state any opinion, founded on the above observations, that I may have formed of the signification of these appearances. In the first place, then, I must acknowledge myself wholly divided between a belief in the formation of the "aggregated granule" by the *aggregation of primitive granules*, the idea which prompted me to give them that name, and the formation of the aggregated granule by the cleavage of a primitive granule. If this question related merely to the formation of the "primary aggregated granule," it would be unimportant, but it has a more extended application. The outermost layer of the nucleus is composed, as I have shown, of "aggregated granules," and so also is that layer which alone forms the space in the nucleolo-nucleated cells. To them the hypothesis of cleavage of a simple granule would be most suitable, and this theory would explain better than any other, changes which remain to be described in the further growth of the epidermic cell. In the second place, the relation of cell and nucleus is a question on which I feel considerable doubt. The process of development appears to consist in the successive production of granules, one layer of granules succeeding another, so that if the organisable principle exist in each separate granule, the organisable force may be supposed to be more and more weakened in successive formations, until the moment arrives when it ceases entirely. Is that which I have described as a nucleolo-nucleated cell really a cell or still a nucleus? The only solution of the question that occurs to me is, determining the presence of a cell-membrane, in which I have not satisfactorily succeeded.

Admitting the nucleolo-nucleated bodies now described to be cells in their earliest state of formation, their size is $\frac{1}{3000}$th to $\frac{1}{2300}$th of an inch in the long diameter, and that of their nucleus from $\frac{1}{6000}$th to $\frac{1}{4300}$th of an inch. In the stratum immediately above the deepest layer, I find cells measuring $\frac{1}{2000}$th of an inch with nuclei of $\frac{1}{4300}$th. Above these, cells measuring $\frac{1}{1800}$th, with nuclei varying from $\frac{1}{4000}$th to $\frac{1}{3000}$th, and above the latter cells measuring $\frac{1}{1300}$th, with nuclei of $\frac{1}{2300}$th. In following the layers of the epidermis upwards to the surface, cells may be observed possessing every intermediate degree of size between the last-mentioned cell, namely $\frac{1}{1300}$th and $\frac{1}{600}$th, which is the measurement of the scales which constitute the uppermost stratum of the epidermis. It must not be supposed, however, that the growth of the epidermic cells reaches its maximum only at the surface: I have found cells of that magnitude in the deeper strata; and there is every indication of the growth of these cells being completed in the stratum immediately above the mosaic-like layer.

Young cells are remarkable for the large size of the nucleus as compared with the entire bulk of the cell; and it is quite evident also that the nuclei, up to a certain point, grow with the cells, their mode of growth appearing to be, the separation of the original granules by the deposition between them of interstitial matter; and in addition, as I believe, by cleavage of the latter and the consequent multiplication of the granules. In cells measuring $\frac{1}{2000}$th and $\frac{1}{1800}$th of an inch, I found the granular character of the nucleus to be very manifest. Besides growth, it is apparent that other changes are taking place in the nucleus; imbibition and assimilation of organisable material must necessarily be in action in order to accomplish the formation of interstitial matter; but in addition to this the central granules undergo another change, by which they are altered in character and become distinguished from the rest when submitted to chemical experiment. For example, when dilute acetic acid is added to the cells measuring $\frac{1}{2000}$th of an inch and less, the entire nucleus is rendered transparent and less discernible than before; but when cells of a somewhat larger size, and consequently longer growth, are submitted to the same process, the nucleus is rendered much more distinct than it was previously. But the body which is made so conspicuous in this latter experiment is not the entire nucleus, but simply the central and older granules of the nucleus; the younger granules retain the character of those of the young cells, they are made more transparent than they were before, and have faded from sight. I may mention also, that the nucleus brought into view by the acetic acid is more or less irregular in form, and has the appearance of being constituted by the fusion of the original granules. How much of this appearance may be real and how much the effect of the acid, I do not pretend to say, and I set no value on the experiment beyond the demonstration of the mere fact which it is made to illustrate.

I now turn to the growth of the cells. I have remarked, in an earlier paragraph, that the formation of the young cell appears to be due to the development of a stratum of "aggregated granules" externally to the nucleated mass, which I have regarded as the cell-nucleus. Now nothing is more certain than that the growth of the cell is due to a successive repetition of this process, the growth of the cell-membrane being consentaneous with the development and growth of "aggregated granules" within it. In cells of $\frac{1}{1800}$th to $\frac{1}{1500}$th of an inch, the "aggregated granules" of the periphery are not easily discernible; but in cells measuring $\frac{1}{1000}$th, and thence upwards to the complete size of the epidermic cell, the fact is quite evident, and is apparent even in the cell-scale. Indeed a cell, at the full period of growth, is a kind of cell-microcosm, containing in its interior secondary cells, tertiary cells, nucleolo-nucleated cells, nucleated granules, aggregated granules, and primitive granules.

It will be observed that this hypothesis of cell-growth differs from that of Schwann. The theory of Schwann always appeared to me to be incompetent to the explanation of the growth of the large scale of epidermis and epithelium in a tissue manifestly subjected to considerable pressure. I sought in vain for the watch-glass cells, elliptical cells, and globular cells in the epidermis; but my search has been rewarded by the discovery of the above-described beautiful process of formation and growth. It will be seen that, according to this view of the growth of the epidermic cells, they never possess anything approaching to a globular shape, that the scales are not flattened spheres, but on the contrary always possessed a flattened form, and have increased by a peripheral growth. This mode of growth again is made manifest by the observation of a vertical section of the epidermis. The most careful examination can distinguish no difference between the size of the deeper and the superficial strata of cells; they have all the same average thickness, all the same average length,—an appearance easily explained when we regard them as parent-cells containing secondary and tertiary cells of the same average size as the cells of earlier formation. It is true that the complete size of a cell is very quickly attained, and that its growth, taking place in the deepest stratum of the epidermis, could not be expected to produce any difference of character in the middle and superficial strata; but this is not mentioned, as far as I know, by Schwann.

The process of growth here described explains also the fact of the disappearance of the nucleus in the scales of epidermis. The outermost granules of the nucleus have become the nuclei or nucleoli of secondary cells, and have consequently been moved away from their original position in the performance of their office of centres of growth to secondary cells. The original nucleus, therefore, is not lost, but merely robbed of some of its component granules, which may be discovered in many parts of the epidermic scale, instead of being concentrated in a single mass. In these scales, and particularly in epithelial scales, the central and dense part of the original nucleus is generally perceptible; in the latter it constitutes the scale nucleus; and in the epidermic scale there is always some one little mass larger than the rest, particularly if the scale have been for some time immersed in fluid, as when it is examined in the serum of a blister. In an epidermic cell, measuring $\frac{1}{600}$th of an inch in long diameter, I found several secondary cells measuring $\frac{1}{1300}$th, others measuring $\frac{1}{3000}$th; and in the interstices, primitive granules, aggregated granules, and nucleated cells.

My observations, it will be seen, have been chiefly directed to the epidermis, and I am prevented at present from carrying them further, but I have no doubt that the epithelium will be found to be identical in the phænomena of development and growth with the epidermis. I have observed the same structure in the epithelium of the mouth and fauces, and also in that of the bladder and vagina. Incomplete epithelial cells, measuring $\frac{1}{750}$th and $\frac{1}{700}$th of an inch from the fauces, presented a very remarkable appearance; they had a rounded lobulated border, evidently composed of a row of secondary cells and a depressed centre, as though the action were subsiding in the latter, while it was progressing in the circumference.

Another illustration of the structure now described, I found in the cells of melanosis, and in the pigmentary cells of the choroid membrane of the eyeball. I am induced to believe that the same structure will be discovered more extensively than at present can be anticipated. The corpuscles of melanosis, according to my observations, are parent-cells, having an average admeasurement of $\frac{1}{1000}$th of an inch, containing secondary cells and nucleated and aggregated granules, as well as separate primitive granules. The "aggregated granules" measured from $\frac{1}{11000}$th to $\frac{1}{7000}$th of an inch, and the primitive granules about $\frac{1}{20000}$th.

There is another feature in the history of development of the epidermic cell, which I regard as peculiarly interesting. This relates to an organic change taking place in the assimilative powers of the primitive granules, by which the latter are altered in their colour, in short, are converted into "pigment granules." Pigment granules appear to differ in no respect from the "primitive granules," excepting in that of colour, and perhaps also in chemical composition. They have the same globular form, the same size, and occupy the same position in the cell, being always accumulated around the nucleus, and dispersed less numerously through the rest of the cell. The nucleus of the cell in the epidermis of the Negro appears to consist wholly of pigment granules, while in the European there is a greater or less admixture of coloured and uncoloured granules. The central granules are generally lighter in tint than the rest, and give the idea of a colourless nucleolus, while those around the circumference are deeper coloured. Besides a difference in the depth of colour of the separate granules entering into the composition of a single cell, there is also much difference in the aggregate of the granules composing particular cells. For example, intermingled with cells of a dark hue, there are others less deeply tinted, which give the tissue in which they are found a mottled appearance. This fact is well-illustrated in the hair, and also in the nails, in which latter it is no uncommon thing to find an isolated streak produced by the accumulation of a number of cells containing coloured granules, in the midst of colourless cells.

When pigment granules are examined separately, they offer very little indication of the depth of colour which is produced by their accumulation. I have observed some to have the hue of amber, while others scarcely exceeded the most delicate fawn. The depth of colour of the deep stratum of the epidermis in the Negro, is evidently due to the composition of that layer of these granules, while the grayness of the superficial layers of the same tissue results not merely from the desiccation of these granules, but also from the fact of those subsequently produced being less strongly coloured, and also from the addition of a colourless cell-membrane.

The epidermic scale of the Negro has a mottled appearance, from the numerous secondary nuclei and their attendant coloured granules which are scattered through its texture.

P.S. Since my communication of the above paper to the Royal Society, I have confirmed its truth by further observations, and have ascertained that the same principle of growth is applicable to the formation of mucus and pus-corpuscles.

December 1845.

XIX. *On the Aberration of Light, in Reply to* Mr. Stokes. *By the* Rev. J. Challis, *M.A., Plumian Professor of Astronomy in the University of Cambridge*[*].

THE remarks Mr. Stokes has made on my Explanation of the Aberration of Light, since they have little reference to the more important parts of the communication, require from me but brief notice.

I agree with all Mr. Stokes has said about the direction of vision through a telescope, but cannot perceive what it has to do with aberration. In selecting the wire of an astronomical telescope for the terrestrial object to which the direction of the celestial object is referred, I had not the least reference to vision through a telescope. It would have answered my pur-

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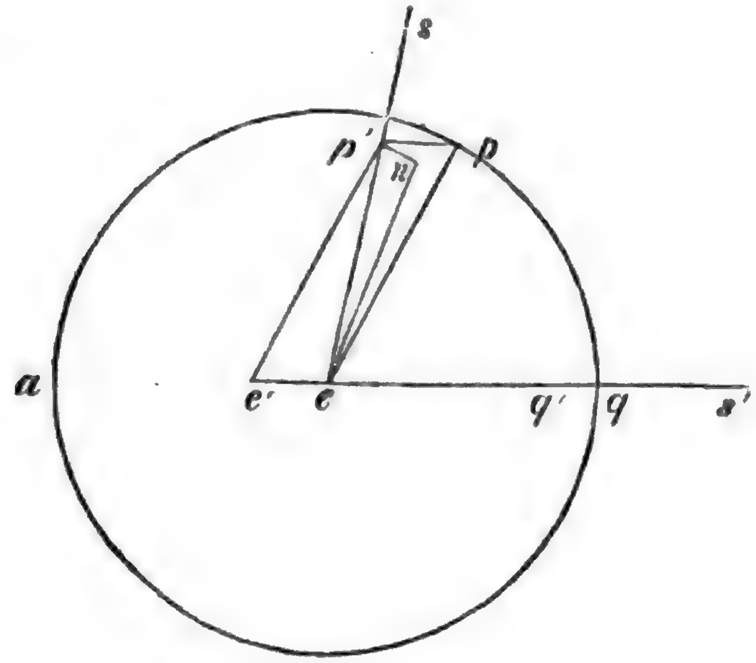
[*] Communicated by the Author.

pose equally well if I had fixed upon a division of the graduation of a circle used for astronomical observation. The following method of viewing the subject will put this in a clear light:—

Let apq be a circle of given radius, the centre of which coincides with the place of the spectator's eye, and partakes of the earth's motion. Let the plane of the circle pass through $e'eq$, the line in which the eye moves, and through $sp'e$, the line in which light comes to the eye from a star. To find the point of the circle on which the light impinges by which the star is seen, take $e'e$ equal to the space passed over by the eye while light travels over the radius of the circle; take $e'p'$ equal to the radius of the circle; draw ep parallel to $e'p'$, and join $p'p$. Then p is the point required; for $p'p$ is evidently equal and parallel to $e'e$. Consequently p is the point of the circle which is seen in coincidence with the star s. If the star were at s' in the direction of the line $e'eq$ produced, the point q on that line would be seen in coincidence with it. Now suppose the circle to be graduated, and to be used by an astronomer to measure the angle ses'; then most certainly he would read off the angle peq instead of the angle $p'eq$, because to turn a given point of the circle from apparent coincidence with one star to apparent coincidence with the other, the circle would be moved through the angle peq. The difference between the two angles, viz. sep, is equal to the amount of aberration determined by astronomical observation. The phænomenon is thus entirely accounted for.



It follows as a corollary from this reasoning, that the direction $sp'e$ of the progression of light from a celestial object, just before it enters the eye, is the *true* direction of the object, atmospheric refraction not being considered.

It seems probable also that this is the direction in which the object is *seen*; if so, the point p is seen out of its true place. This, however, is not an essential consideration.

I think it important to remark, that the foregoing explanation of aberration rests on no hypothesis whatever, being a strict deduction from ascertained facts, without reference to any theory of light. The cause assigned for aberration is, therefore, a *vera causa*, which consequently excludes every explanation of a hypothetical kind, such, for instance, as that which Mr. Stokes proposed in the July number of this Magazine.

Aberration being explained in this manner, it is interesting to inquire whether a proposed theory of light be consistent with this explanation. The object of such an inquiry would be to test the truth of the proposed theory. The only condition the theory is required to fulfil, in addition to that of temporaneous transmission, is, that the light from an object traverse, just before it enters the eye, a straight line directed to the true position of the object.

The above condition is satisfied on the theory of emission, because according to that theory light passes from the object to the eye in a straight line. In the undulatory theory, the direction of transmission of light is the direction of transmission through space of a given point of a wave in a given phase of vibration. Where the æther is undisturbed, this direction is normal to the front of the wave. Where the æther is in motion, it is the direction resulting from the composition of the motion of propagation of the wave with the motion of translation of the æther. It is easy, therefore, to determine, for a given motion of translation of the æther, the angle which the normal to the front of the wave makes with the direction of transmission of light. In the figure, let $p' n$ (not necessarily equal nor parallel to $e' e$) represent the motion of the æther, $p' e$ representing the velocity of light; then $e n$ is the direction of the normal to the wave that enters the eye at e. If the normal underwent no angular deviation the whole distance from the object to the eye, $e n$ would also be the direction of the object, and consequently aberration on this theory would not be accounted for. I gave Mr. Stokes the credit of having first shown that the normal is shifted through a certain angle as the wave is propagated through the æther set in motion by the earth, and by reasoning as he has done, and supposing certain analytical conditions, which I shall speak of presently, to be satisfied, the deviation is found to be from p' towards n, exactly through the angle $p' e n$. Consequently $e p'$ is the direction of the object, and the required condition is satisfied by the undulatory theory of light.

I admit the correctness of Mr. Stokes's strictures on that part of my communication to which he principally objects. Mr. Stokes's own reasoning in the July number, or the following, may be substituted for the part objected to. The point a is carried with the velocity $V-w$, and the point b with the velocity $V-w'$, in the direction of the axis of z. As w is less than w', a is carried further than b in the small time δt, by $(V-w)\,\delta t-(V-w')\,\delta t$, that is, by $(w'-w)\,\delta t$, or $\frac{dw}{dx}\,\delta x\,\delta t$. Dividing by δx, the interval between a and b, the angular displacement of the front of the wave in the plane of $z\,x$ is $\frac{dw}{dx}\,\delta t$, which is equal to $\frac{dw}{dx}\cdot\frac{\delta z}{V}$, since $V=\frac{\delta z}{\delta t}$ very nearly. To integrate this expression, it is necessary to assume that $\frac{dw}{dx}=\frac{du}{dz}$. So considering the motion in the plane $z\,y$, the integration requires that $\frac{dw}{dy}=\frac{dv}{dz}$. These conditions, which are alluded to above, I agreed with Mr. Stokes that it was necessary the motion of the æther should satisfy. I went a step further, and endeavoured to show that they do not restrict the motion. The reasoning for this purpose was based on hydrodynamical equations, in which the squares of the velocities were neglected. This may generally be done when the motion is small. But obviously all cases of motion for which $\frac{du}{dt}$, $\frac{dv}{dt}$, and $\frac{dw}{dt}$ vanish are to be excepted, and the instance before us may be one of this class; for the motion must be nearly symmetrical about the line in which the earth's centre moves, and if the earth's centre be taken for origin of co-ordinates, the velocity must be very approximately a function of co-ordinates independent of the time. On this account I doubt the applicability of those equations, and in the present state of our knowledge of the subject, it seems the best course simply to suppose the motion of the æther to be such as to satisfy the two conditions $\frac{dw}{dx}=\frac{du}{dz}$ and $\frac{dw}{dy}=\frac{dv}{dz}$. There is nothing improbable in the supposition: it saves the undulatory theory; but I must protest against its being considered necessary for the explanation of the aberration of light.

Cambridge Observatory, January 8, 1846.

XX. *Observations on certain Molecular Actions of Crystalline Particles, &c; and on the Cause of the Fixation of Mercurial Vapours in the Daguerreotype Process.* By AUGUSTUS WALLER, *M.D.*[*]

[With a Plate.]

WHEN a piece of glass is covered with a solution containing the double phosphate of ammonia and magnesia, and traces are made upon it by any hard body, it is known that they become visible shortly afterwards by the salt being precipitated upon them. Berzelius, who mentions this test in his Elements of Chemistry, states that Wollaston proposed to make use of this fact as a test of the presence of magnesia in solution, which has since been frequently adopted. According to Berzelius, "the cause of this property is of a mechanical nature, probably from the glass being covered with microscopic crystals, the facets of which take a different position on the traces, for some reason which is not easily explained." More recently, Prof. Liebig has alluded to this subject in his Vegetable Physiology, § 157. These effects are referred by him to a state of unstable equilibrium of the various particles which compose the liquid, which is destroyed whenever a dynamical action is created sufficiently powerful to overcome the feeble attractions, or the inertia of the molecules in solution. He ascribes to the same cause the sudden solidification of water, which had remained liquid when below the freezing-point, upon being agitated; the precipitation of a mixture of potash and tartaric acid; also the detonation of fulminating powder from the contact of any solid body. Neither of these eminent observers mentions having submitted these traces to microscopic examination, although that is the only manner to test the hypothesis advanced by Berzelius.

On the present occasion it is my intention to describe some observations I have made, in order to elucidate the influence of molecular action on the precipitation of saline bodies, similar to that observed in the double phosphate, and to show that a similar influence is exerted over bodies in a gaseous state and in a state of vapour, and afterwards to point out some phænomena hitherto unexplained, such as the fixation of the mercurial vapours in the Daguerreotype for instance, which evidently depend upon a like cause.

In order to obtain the double phosphate, I have generally used a solution containing about ten grains of phosphate of soda with about three of carbonate of ammonia in an ounce and a half of water. I have preferred this mixture, because the ingredients are more easily procured, and are less acted

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[*] Communicated by the Author.

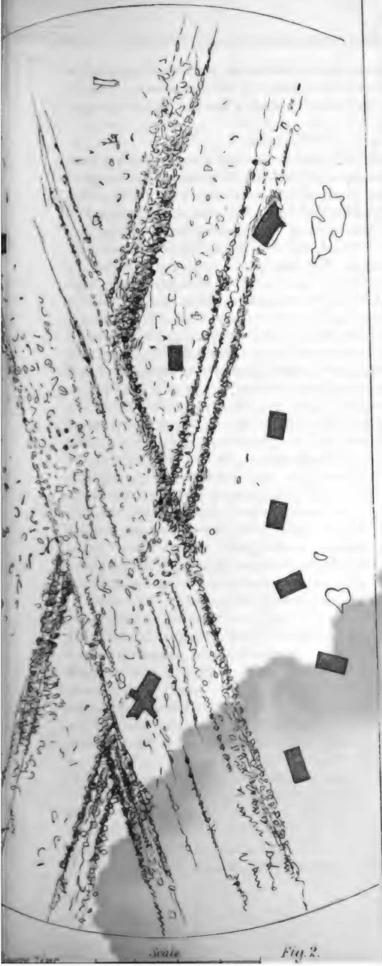
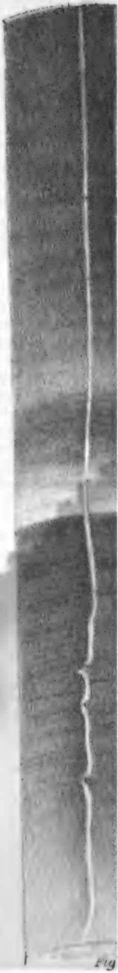


Fig. 2.



Fig

upon by the atmosphere than the phosphate of ammonia. The magnesian solution was generally a few grains of sulphate of magnesia to the same quantity of water as above.

A small quantity of the first mixture is poured on a piece of glass, and to this are added a few drops of the magnesia in solution; if it be allowed to remain undisturbed, in a few minutes the surface of the liquid becomes covered with a thin film, and on the glass appear minute shining crystals; but if before these crystals have time to form, any solid substance, as a glass rod or an empty pen, for instance, is passed over the glass through the liquid, the course it follows becomes visible shortly after. The images which are thus formed are double, and may be termed the upper and lower images.

I will first describe the upper images:—They appear on the surface of the liquid itself, when the film would otherwise have been formed. They are seen immediately after the passage of the pen through the liquid, whereas the lower ones only become apparent a few moments after. Being formed on a moveable surface, they are not perfect representations of the traces that have been made, and are changed and distorted by any movement of the liquid. When the solution of the salt is weak, they frequently disappear a few moments after their formation and are redissolved in the liquid; when the liquid is more concentrated, they likewise disappear, owing to the formation of the film on the surface. The production of these images appears to be independent of the chemical nature of the body used for tracing. They may be obtained independently of the lower ones, by drawing a thread gently over the surface of the liquid, without its coming in contact with the surface of the glass.

The lower images are formed on the surface of the glass, under the upper ones. A few seconds after the tracing has been made upon the glass, they begin to appear, and gradually become more distinct. The space of time which elapses before their appearance depends upon the strength of the solution. When it is strong they appear quickly, and when weak they take several minutes before they are visible.

To cause the formation of any images, the tracing must always be made after the mixture of the two solutions; under no other circumstances have I been able to create them. Thus, when the tracing is made on a perfectly dry glass, or on one slightly wet, and then immediately covered with the solution, no images will be created. This is likewise the case when we make traces in either the magnesian or the phosphate solution before their mixture together.

The passage of any solid substance in the proper solution on glass will cause the formation of a deposit. Wood, glass, slate, and other similar substances, all have equal power in this respect, but metallic substances are less active. Other polished surfaces may be used instead of the glass plate, and I have formed these images on quartz and agate with the same effect.

The difference of crystalline texture exerts no influence, but the images seem to be with more difficulty produced on polished silver and copper than on a vitreous surface.

A very slight degree of friction will excite the formation of an image, although a moderate degree of pressure is more favourable.

Electricity exerts no influence in the formation of these images. In one experiment, in order to diminish the friction, I adapted two fine wires of a spiral form to a battery sufficiently strong to decompose water freely. These wires were moved through the solution in various directions, and the marks of the passage of the two poles became equally apparent without any difference on either side; and when afterwards disconnected from the battery and used in a similar manner, they produced the same effects.

It is remarkable with what fidelity the traces of lines become visible in this manner. Letters thus formed by a pen, are much more faithfully rendered than when written on paper with ink, and lines may be formed which are scarcely visible to the naked eye. Microscopic inspection shows this extreme exactness to a much greater degree than could have been anticipated; for we see a simple line become as it were decomposed into a number of parallel lines, which represent the point of contact between the two solids (see Plate III. fig. 2). These lines are composed of very minute and confused crystals, of an irregular appearance and joined together. Their diameter varies from 0·02 of a millimetre to about double that size. Between these parallel lines are frequently seen others still more minute. The other crystals which become deposited by the common crystalline powers over the untouched parts of the glass, are much larger than either of these. When the point of intersection of two lines is examined under the microscope, we perceive the appearance represented. While crystalline masses are in process of formation, it is impossible to prevent the deposition of crystals on other parts of the glass; but if while these are fresh they are subjected to a sharp current of water, the irregular crystals are mostly carried away, while the images are left almost intact. It is therefore evident that the same power which causes this deposit, renders them more adherent to the surface of the glass than the other crystals. Another method of demonstrating the difference of their adherence, is by allowing the solution to dry on the glass, when by brushing it slightly with the feather of a pen, most of the irregular crystals are taken off and the images remain.

Other substances capable of forming a like deposit.—Chloride of platinum and nitrate of potash, mixed together, form a double chloride, with which images can be obtained with as much ease as with the double phosphate. The only difference is, that the double chloride precipitates in the shape of octahedrons, &c. Solutions of tartaric acid and nitrate of potash deposit crystals of bitartrate of potash, which are capable of forming upper and lower images with nearly as much facility as the double phosphate. The lower images formed by the bitartrate differ in one respect from those by the phosphate, for shortly after their formation they appear to lose their adhesion to the glass, and the slightest agitation of the liquid causes them to be detached; and if a sentence has been written, the curious appearance is presented of fragments of words and letters floating about in confusion. Under the microscope also they differ, fewer parallel lines are perceived, and the crystals are larger and unequal in size. Liquor potassæ added to a solution of tartaric acid will form images exactly similar to those just mentioned. Caustic soda and tartaric acid produce the same result, but the solution must be much more concentrated.

Images formed by gaseous bodies.—These traces are formed in the same manner as those which are crystalline, by passing a solid body over a piece of glass covered with a liquid containing a gas in solution, when they are immediately perceived by the bubbles which are deposited. On account of the specific gravity of the gas, these images are not very durable, for after a short time the gas which composes them rises to the surface. As a general rule, the ingredients, whose combination causes the formation of the gas, should be added together gently, and so diluted that whatever gas is formed they remain dissolved in the liquid. I have been surprised to find how much gas may be in this way made to remain in solution; and as most of them appear capable of being dissolved in this unstable manner, traces may be obtained from them all; and I have ascertained by experiment, that such is the case with carbonic, acetic and hydrochloric acids.

To obtain carbonic acid, I have generally used the subcarbonate of soda and tartaric acid. Acetate of ammonia was employed to liberate acetic acid, and hydrochloric acid was obtained from common salt and sulphuric acid. A mixture capable of forming traces has the property of disengaging its gas in bubbles, whenever it is brought in contact with any dry surface; as for instance, when a mixture of this sort formed on a slip of glass is caused to spread over a part of the surface which has not previously been wetted, bubbles of gas are immediately evolved on that spot, although none are perceived elsewhere. This effect is also produced with champagne, seltzer and other effervescing waters, which however have not the property of forming gaseous traces. Any surface, whether metallic or non-metallic, will be found to effect the separation of the gas from the liquid; and I have not perceived that there was any difference from the surface being perfectly polished or rough.

The immersion of a piece of bread in champagne to renew the effervescence, is merely an example of the contact of a fresh surface with the gas; in a short time it ceases to have this effect, but if a fresh piece is used, the effervescence is renewed as before. The difference of effect between this and a piece of metal arises solely from the superior extent of surface presented by the cavities of the bread. The disengagement of steam from boiling water by platinum foil or any other solid substance, is likewise of the same nature. After a very short time this effect ceases, unless renewed by a fresh surface. The most natural explanation of these phænomena, is to refer them to some molecular action of the solid on the gas, probably of a mechanical nature, which lasts a very short time, when the solid acquires a "droit de domicile" in the liquid, and becomes perfectly inert. M. Legrand, who has made most correct experiments on the point of ebullition of saline solutions, remarks, that platinum possesses no power in equalizing ebullition after a few moments, when, according to him, all the air has been expelled from its surface; but on the contrary, zinc and iron will act as long as they are present in the liquid, which he attributes to their power of decomposing water.

Previously to showing the existence of the same action in bodies in a state of vapour or of fume, I will make a short digression with respect to the constitution of vapours in general.

The term vapour is commonly applied to bodies in three different conditions,—1st, that of temporary gas diffused in the atmosphere; 2nd, that of liquid particles mechanically suspended there; 3rd, that of solid particles suspended in like manner. To the two latter, to speak more correctly, may be applied the term of fumes. The first correspond to solution in a liquid, and the other two to that of suspension in the same. As examples of the first, we have the vapours of water while in an invisible state, and those of bromine, &c. Of the second, water as in mists, fogs, &c.; and of the third, the vapours of arsenic and of corrosive sublimate. Bodies in either of these conditions possess the faculty of assuming a definite crystalline form on becoming solid. The properties of the gaseous vapours are so well known, that it is unnecessary to dwell upon them here.

The second class, or the liquid globular vapours or fumes, which, as we have said, cause those accumulations known under the name of fogs, clouds, or mists, are those which I intend at present to examine, as they comprehend the theory of the fixation of the mercurial vapours in the Daguerreotype. It was formerly believed that vapour or mist was composed of minute spherules or globules of liquid water, and in Newton's works we find evidence that such was his opinion. According to another view, first advanced I believe by De Saussure, these vapours were composed of vesicles or very minute bubbles, exactly resembling, on a small scale, the common soap-bubble. This opinion has received the assent of Fresnel and Berzelius, and at present obtains general credence. The proofs on which it is considered to be founded, are principally the observations of De Saussure, who asserts that on high mountains, or in the clouds, he has been able to detect these air-vesicles with the naked eye, and has seen them burst as they came in contact with each other. Berzelius recommends the examination of the vapour of water over a dark surface, such as that of ink, with a lens of a short focus. He says, that vesicles may be detected in this manner, varying in size from $\frac{1}{1500}$ to $\frac{1}{2780}$th of an inch, which occasionally burst as they touch each other. The suspension of clouds is also used as an argument in favour of the vesicular theory, as it is contended that liquid spherules would descend to the ground by their specific gravity in such situations. Fresnel indeed compares the globules to small balloons, which dilate or contract, according to the temperature of the air they contain.

A few days' stay at the convent of St. Bernard gave me an opportunity of repeating the observations on the clouds, as mentioned by De Saussure, which may be also made in this season on our London fogs. Globules of various sizes in these circumstances are frequently discerned by the naked eye floating in all directions. I have endeavoured to ascertain their vesicular structure, but have been unable to do so from direct observations. It is frequently a most difficult point, in microscopic investigation, to decide upon the existence of a thin transparent membrane. It is still more so to pronounce upon the vesicular or spherular structure of globules in constant agitation; and I believe that if minute spherules and vesicles could be mixed together, we do not possess any means at present of distinguishing them.

I have never been able to detect that appearance of bursting of the globules mentioned by De Saussure, but sometimes, when the agitation of the air is slight, two of the larger globules may be seen floating towards each other, and afterwards disappear suddenly, which may be explained, if we admit that it is caused by the union of the two spherules into one, which is too heavy to remain any longer in suspension, and whose rapid deposition conceals it from the sight.

There may be urged as objections to the vesicular theory, that if the pellicle become extremely thin, the vesicle would no longer be perceived any more than the apex of an air-bubble before bursting, or the central black spot of a system of Newton's coloured rings. It will be seen below that the globules of vapour possess the power of depositing themselves in a crystalline form, which requires a tranquil deposition of particles, such as could scarcely be deemed possible, if the air contained in each had to escape at the moment of its crystallization.

I have endeavoured to fix the globules of water on glass and other substances, so as to be enabled to submit them to microscopic inspection, but from their volatile nature and other causes have not succeeded. However, it is easy to do so with almost any other volatile substance; and I have examined several in this way without detecting the slightest appearance of a vesicular structure. Mercury is deposited under the form of globular particles, with a metallic lustre whose diameter is $\frac{1}{300}$th of a millimetre, in which I have never detected any internal cavity by the most careful examination[*]. Flour

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[*] In order that others who may wish to verify these results may operate in the same conditions as myself, it is proper to state that the mercurial vapours were disengaged in a box, such as is used in the Daguerreotype process; and after the mercury had been raised to a temperature of about 90° centigrade, it was allowed to cool. Three experiments were made in this manner: in the two first the glass plate was placed four inches above the mercury, in the other it was eight inches distant. The appearance of the globules was the same in each case; if any difference existed in their size, those of the last experiment were rather larger. In another experiment, where a common Daguerreotype plate was substituted for one of glass, the appearance of the globules was in all respects the same. From the manner in which they are deposited, they appear to exert an influence over each other, as they are frequently found in groups of three or four, or more. Mr. Ross has stated on the part of Mr. Solly (Microscopical Society, December 1843), that these globules are deposited in hexagonal groups; but with preconceived ideas no doubt it would be very easy to form such shapes, as it would be to form triangles or any other simple geometrical figure, particularly when the illusions inseparable from catoptric microscopy

of sulphur is found to consist of solid globules, several of which adhere together; when acted upon by a gentle solvent, their external portion is dissolved, and there remains a regular octahedron. An interesting experiment may be made on the fumes of sal-ammoniac, which appear whenever muriatic acid and ammonia are brought together. Two small phials, each containing one of these substances, are covered by an inverted tumbler: above the surface of the acid are seen at a short distance the fumes of the salt, which at the end of a few hours are found to have condensed into a thin snowy pellicle, completely obturating the mouth of the bottle. This partition is so delicate, that the slightest agitation will cause it to fall into the liquid.

In all these cases it is found that the fumes possess the power of remaining suspended a much greater length of time than would be expected from the difference of their specific gravity with that of air, which is also the case with the fumes of other substances, and smoke in particular. This can only be accounted for by the continual state of agitation of the air, even within an enclosed space, and by the elasticity of the solid and liquid particles. In the case of solid particles this can be readily admitted, but with regard to liquid globules, there is probably some action similar to that which takes place on the impinging of solid elastic balls, which after becoming flattened rebound in virtue of their tendency to recover their original shape.

The causes which act in fixing different vapours and fumes are the same as those which determine the precipitation of solid particles in solution, such as for instance, sharp points of any kind, minute filaments, and more especially the existence of a crystalline particle to act as a nucleus. Non-conducting substances, as woollen cloth, the nap of a hat, the web of the spider, &c., are covered with aqueous globules when no rain has fallen, and when polished surfaces near present no such deposition.

Having now shown the existence of a crystalline power in vapours, we shall proceed to prove the influence of a force which disturbs this equilibrium in the same manner as in the saline solutions above mentioned. The friction of a solid body on glass will leave traces which are invisible until breathed upon.

are added to those of physiology. This tendency of the mind, of which a good account has been given by Müller in his Elements of Physiology, is so strong, that where groups of globules are concerned, I would always advise their being mapped down under the microscopic camera lucida, and put by for some time for future inspection. I shall have occasion to advert to this subject more fully hereafter.

Many bodies possess this property, but the mineral steatite, or soap-stone, produces the effect better than any other I know. A considerable degree of friction may be used over the traces thus produced by steatite, without affecting the appearance of the traces when breathed upon repeatedly. The glass may even be heated considerably without affecting them. By examining with the microscope the parts that have been traced upon by steatite, we are unable, any more than with the naked eye, to detect any material cause for the deposition of vapours in these places, as it probably depends upon the transparency of the mineral, which being so attenuated is unable to affect the rays of light. When the traces have been brought out by breathing upon them, they must be covered with another piece of glass, which impedes the evaporation of the water and allows them to be submitted to the microscope. The parts untouched by the steatite present the appearances that have been already mentioned. On the lines created by the mineral, the drops of water are differently disposed, their long diameters being parallel to the direction of the lines. These minute drops very much resemble the globules of gas deposited from a liquid, the only difference between the two consisting in the deviation from the globular form in the liquid traces, which evidently arises from the power which the water possesses of wetting glass.

It is evident, therefore, that the secondary cause of these images is a difference in the position of the minute drops of water, reflecting the light differently from the other drops, which are irregularly disposed on the other parts of the glass.

There exists another method of fixing vapours, which has been long known, and to which I believe attention was first directed by Prof. Draper. It consists in merely placing a body on a plain surface, such as that of a metallic speculum, or even of glass; after a short time it is found that simple contact, such as this, has caused some molecular action, as the spot occupied by the object will become apparent by breathing on it in the same way as with the images of steatite. This observation is the more interesting, as it serves as a connecting link between the effects of mechanical power and those caused by other agents.

The experiments of Mr. Hunt have shown the influence of heat in causing the fixation of vapours.

An image of this sort formed on glass by the breath, when examined under the microscope, presents exactly the same appearance as those formed by steatite. The same difficulty is experienced in bringing out, by mercurial vapours, the thermographic images on glass, as is found with the traces of steatite, which possess but in a very slight degree the power of fixing mercurial vapours. It appears therefore that the power which water has of wetting glass, causes it to have a greater tendency to deposit than mercury, which does not wet glass. The cause of the production of thermographic images is evidently similar to that which causes the deposition of a solid body from a solution.

The fixation of the mercurial vapours in the Daguerreotype process, which has excited so much interest, and for which so many theories have been advanced, is but another example of the force which causes the deposition of solid and gaseous particles from a liquid, and which produces so many other effects. In this case the chemical rays of light act in the same manner as mechanical action and caloric in causing a certain molecular disturbance. By the discoveries of Moser, it is shown that these rays possess the power of acting upon almost any body, in such a manner as to render it capable of fixing the particles of various vapours. Thus simple minerals, glass, &c. may be made to fix the mercurial vapour.

It appears however that silver, gold, copper, &c., which form amalgams, or in other words, are capable of being wetted by mercury, possess this property in a greater degree than any other bodies which are incapable of being wetted by it; in the same way as we have seen that glass has the greatest power to fix the vapour of water. Admitting the truth of this theory of the Daguerreotype process, we are naturally led to inquire whether the same agent may not likewise cause the fixation of particles in a state of solution or of vapour, in the same manner as by simple mechanical action. After several unsatisfactory attempts, I finally succeeded in clearly proving this fact. The solution which shows the influence of light the most evidently, is that of the neutral chloride of gold. A few grains of this salt dissolved in an ounce of water, when exposed to the light, deposits minute crystals of a metallic appearance on that side of the glass nearest the light.

The action of light in causing the deposition of gaseous vapours may be shown by placing some iodine in a bottle closed with a glass stopper. After being exposed to the sunshine for several hours, minute black crystals will appear on the side nearest the light, which will change their position according to the side of the glass exposed. Another substance which shows this action still better, is camphor, a piece of which, merely covered with a glass shade, will give rise to a crystalline deposit, after an hour or two of exposure to light, and which presents the same phænomena as that of iodine. By a prolonged exposure these crystals become very abundant, and are very beautiful*. I have applied this property to the construction of an instrument for measuring the chemical rays of light. As the details respecting this would be foreign to our present subject, I will defer them to another occasion, and confine myself now to prove that these phænomena are independent of the deposits caused by radiation.

1st. The crystals are formed on the side exposed to the action of direct or diffused light.

2nd. They are not formed during the night, when the radiation from the earth is sufficient to cause the deposition of water.

3rd. Green glass, which retards photographic action, likewise impedes this deposit.

In an experiment which is now going on, a bottle of pale green common glass is exposed to the north, while another of white glass is placed in a southern aspect. The first became covered with minute crystals, in size averaging about a millimetre, which have remained stationary for a week; the second is covered with arborescent ramifications, which are daily increasing.

Several familiar, but hitherto unexplained phænomena, may in my opinion be easily accounted for by these molecular actions.

The formation of hail I consider to be an instance of an action precisely similar to that which causes the deposition of the solids of gaseous and liquid particles. If we admit the influence of this force on the globular vapours of water, it is not at all improbable that certain conditions may arise in nature when these vapours may be much more liable to this influence than we find them in our imperfect experiments. We have seen that a solution of sulphate of soda or water in a pure state may be brought by the abstraction of caloric to such a condition of unstable equilibrium, that the slightest perturbating cause will immediately reduce them to a solid form.

If we admit that the globules which form the clouds are capable of being placed in a similar condition, we have sufficient data to explain all the phænomena that occur in the production of hail. Any nucleus formed within a cloud in this state, would create around it a deposition of all the neighbouring particles; and the size of the hail-stones would be dependent upon the thickness of the cloud it had to traverse. In the storm at Ordenburg, in 1825, mentioned by Dr. Eversman, pyrites was found in the centre, and had acted like a nucleus

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* I am informed by a friend, that this action of camphor was mentioned twenty years since by Dr. Hope in his lectures, but I am not aware of anything having been published upon the subject.

round which the crystallization had taken place. Where the centre is not formed by a foreign body of this sort, it has frequently been mentioned that it consisted of an opake nucleus of a spongy nature, like congealed snow, which may be easily accounted for. The succession of concentric layers would be caused by the passage of the particles through strata of liquid globules not all at the same temperature; and the radiated structure indicates a gradual increase of crystalline action proceeding from the centre. The temperature of the hail-stones, which has generally been found below the freezing-point, is a further corroboration of this view.

The formation of butter is likewise in all probability another instance of molecular action of the same nature. It is well known that after the cream has been agitated for a certain length of time, the globules suddenly coalesce, and by their union butter is produced. The sudden appearance of this product is the more remarkable, as it takes place at different temperatures, although more quickly at some than others, and not gradually, as might have been expected, which precludes the idea of its being owing to any caloric developed by friction. The most minute observations have been unable to show any material alteration in the appearance of the fatty globules at the moment before the butter is formed. Little doubt can be entertained of its being caused by some molecular action, or engendered in the globules by the continued agitation they have undergone.

Some of the most permanent gases likewise exhibit phænomena closely allied to the above, by their action on platinum and other metals. According to Dulong and Thenard, platinum foil newly beaten has the property of acting at the common temperature, on a mixture of hydrogen and oxygen; but after a few minutes' exposure to the air, it entirely loses that power, which may however be restored to it in a stronger degree than before by heating it in a covered crucible. If it be kept in a covered vessel, so as to exclude the air, it will retain the power without decrease for four-and-twenty hours.

Platinum filings, made with an ordinary sized file, have the same property immediately after their formation, and which they retain for above an hour. It has also been observed, that a hollow ball of platinum has the power of condensing and absorbing different gases, which are generally disengaged at a temperature below the boiling-point (Pouillet, *Elémens de Physique*, § 131). The action of the gases on platinum in all the above cases greatly resembles that of carbonic acid on glass, except that not merely simple lines, but the whole surface of the metal exerts its influence, and that the gases themselves are invisible.

XXI. *Note to* Mr. Hennessy's *Paper on the Connexion between the Rotation of the Earth and the Geological Changes of its Surface**.

THE values of I_1 and K must be altered, as some incorrect assumptions were made in obtaining them. This alteration will produce no changes in the general conclusions which have been arrived at.

The method for obtaining the moment of inertia of a solid of revolution contained in equation (3.), appears to have been inapplicable to the case of the internal spheroid of the earth from the nature of the expression for ρ. The expression for the earth's moment of inertia, which is used for obtaining the theoretical coefficients of precession and nutation, is, however, adapted to our purpose. In this case we have†,

$$I_1=\frac{8\,A}{3\pi^3}\left(\frac{6p}{5}\right)^4\left\{\left(5\pi-\left(\frac{5}{6}\right)^3\pi^3\right)\cos\frac{5}{6}\pi+\left(\frac{25}{12}\pi^2-6\right)\sin\frac{5}{6}\pi\right\},$$

$$K=\frac{10368}{1875}\left(\frac{\alpha-D}{\beta-D}\right)^4\left\{\left(5\pi-\frac{125}{216}\pi^3\right)\cot\frac{5}{6}\pi+\frac{25}{12}\pi^2-6\right\}.$$

When this value of K is substituted in (19.), that resulting for P will evidently be less than what has been already found, and it will give an amount of denudation of the earth's surface still more within the limits of geological observations than that which has been previously obtained.

H. HENNESSY.

XXII. *Letter to* Henry Lord Brougham, *F.R.S., &c., containing Remarks on certain Statements in his Lives of* Black, Watt *and* Cavendish. *By the* Rev. WILLIAM VERNON HARCOURT, *F.R.S. &c.*

MY DEAR LORD,

IN a volume of biography which you have lately published, I perceive that you have reprinted your contribution to M. Arago's historical notice of Watt, in which the distinguished author attempted to transfer to the subject of his *éloge* the credit of a celebrated chemical discovery, hitherto by the common consent of chemists attributed to Cavendish.

Your personal challenge to myself would not have moved me to enter again on a question which I scarcely think open to dispute since the publication of the fac-similes of Cavendish's original notes of that discovery, in the Transactions of the British Association for the Advancement of Science, had I not

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* Phil. Mag. vol. xxvii. p. 376, November 1845.

† Airy's Tracts, Precession and Nutation, Art. 43.

observed, as it seems to me, other mistakes in this volume, on points of scientific history, which, venial as they are in one who cannot be supposed to have devoted much of his valuable time to these *umbratile studies*, are yet such as ought not to pass without some notice.

I must begin, however, my criticisms on your historical chemistry, by repeating the grounds on which I deemed it needful to controvert the statements of M. Arago respecting the discovery of the composition of water. "The *éloge* of Watt, delivered before the French Academy by one of its secretaries, and subjoined to the *Annuaire* for 1839, had just been published. It was blemished by statements which reflect unjustly on the character of one whose memory is cherished among us as a bright example of the union of modesty with science, of the purest love of truth with the highest faculties for its discovery, and the most eminent success in its attainment. Perceiving these statements to be founded in error, I took the earliest opportunity of rectifying them, at the meeting of the British Association which followed within two or three weeks after I became acquainted with them, rejoiced that I had it in my power, from the position in which, as President of that body, I had then the honour to be placed, to make the correction of the error as formal and public as its promulgation had been; and persuaded that M. Arago, as soon as he should be fully possessed of the facts, would consider it a duty which he owes both to the Academy and himself, to retract the suspicions which he had expressed*."

Those who feel that a sense of justice is a material part of the character of illustrious men and illustrious bodies, are still "waiting," not "till your fellow champion," as you express it, "shall seal your adversary's doom," but till he makes the *amende honorable* by withdrawing, in explicit terms, imputations which since the lithographing of the Cavendish MSS. he must know to be unfounded. I am not content, my dear Lord, that you should either for your "colleague" or yourself, half retract and half retain those doubts which I perceive that you have *republished* in one part of your volume, whilst you *disclaim* them in another.

"I cannot easily suppose," you say, "that M. Arago ever intended, and I know that I never myself intended, to insinuate in the slightest degree a suspicion of Mr. Cavendish having borrowed from Mr. Watt." Certainly, as regards yourself at least, no declaration can be more explicit than this. But what then, give me leave to ask, is the significance of the following words in your *now republished* appendix to M. Arago's

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* Report of the British Association for 1839, p. 22.

éloge? "*Whether or not Mr. Cavendish had heard of Mr. Watt's theory previous to drawing his conclusions, appears more doubtful: the supposition that he* had *so heard rests on the improbability of Sir C. Blagden and many others knowing what Mr. Watt had done and not communicating it to Mr. Cavendish, and on the omission of any assertion in Mr. Cavendish's paper, even in the part written by Sir C. Blagden with the view of claiming priority as against M. Lavoisier, that Mr. Cavendish had drawn his conclusion before April* 1783. Mr. Watt's theory was well known among the members of the Society some months before Mr. Cavendish's statement appears to have been reduced into writing, and eight months before it was presented to the Society. That the first letter of April 1783 was for some time,—two months as appears from the papers of Mr. Watt,—in the hands of Sir Joseph Banks and other members of the Society during the preceding spring, is certain from the statements in the note to p. 330; *and that Sir C. Blagden, the Secretary, should not have seen it seems impossible,* for Sir Joseph Banks must have delivered it to him at the time when it was intended to be read at one of the Society's meetings (Phil. Trans. p. 330, note); and as the letter itself remains among the Society's records in the same volume with the paper into which the greater part of it was introduced, it must have been in the custody of Sir C. Blagden. *It is equally difficult to suppose that the person who wrote the remarkable passage already referred to respecting Mr. Cavendish's conclusions having been communicated to M. Lavoisier, should not have mentioned to Mr. Cavendish that Mr. Watt had drawn the same conclusion in the spring of* 1783, *that is, in April at the latest*; for the conclusions are identical, with the single difference that Mr. Cavendish calls dephlogisticated air water deprived of its phlogiston, and Mr. Watt says that water is composed of dephlogisticated air and phlogiston."—(Life of Watt, pp. 396–398.)

To what does all this argument tend?—Would it lead any one to guess that you mean to acquit Cavendish of plagiarism, or that "you have yourself," as you elsewhere affirm, "always been convinced that Mr. Watt had, *unknown to Cavendish*, anticipated his great discovery?" Allowing a certain interval of time and place, I should not wonder at your having forgotten or laid aside your doubts whether Cavendish, with the connivance of Blagden, had not purloined the *conclusions* of Watt; but I have never before known an instance of so deliberate a disavowal of a suspicion contemporaneous and in juxtaposition with its no less deliberate reiteration.

Your *reprinting now* these old doubts is the more unaccountable, not only because they consist so ill with your profession of belief in the good faith of Cavendish, and are indeed a mere trifling after that point has been satisfactorily established, but because I have corrected the *particular* error out of which this tissue of suspicions was spun; and you are now apprised that the Secretary of the Royal Society at that time was *Mr. Maty*, and not, as you persist in taking for granted, Cavendish's friend *Dr. Blagden*, who did not enter on the office till *May* 1784. "So that," as I told you in the Appendix to my address to the British Association, "he is not liable to the suspicion intimated by Lord Brougham, of having shown Watt's letter to Cavendish, nor to the reproach which M. Arago casts upon him, of not speaking the whole truth respecting the precise date at which Watt's opinions were made known in London."

The confidence which you place, with so much simplicity, in the innocence of M. Arago's "*intentions*," contrasts strangely with the disposition you have shown to suspect Cavendish and Blagden: for M. Arago does not, like yourself, "just hint a fault," but retorts in good set terms on the English philosophers the imputation which Blagden had cast on Lavoisier, "That he had told the truth, but not the whole truth." "This is a heavy charge," says your illustrious colleague; "let us see whether *all who took part in this affair* are not liable to the same reproach;"—and then in a style of pointed irony, into the spirit of which I should have thought you apt enough to enter, he proceeds to fix the charge on all the parties concerned. I believe I have given no more than the plain meaning of these clever sarcasms when I said, "The Secretary of the Academy has not confined himself to taking from Cavendish the honour of this discovery, but has in fact imputed to him the claiming a discovery which he borrowed from another; of inducing the Secretary of the Royal Society to aid in the fraud, and even causing the very Printers of the Transactions to antedate the presentation copies of his paper."

The real truth is, that M. Arago having, when in England, heard but one side of the story, was persuaded of the *insincerity* of Cavendish. If he is now disabused of this persuasion, I hope he will choose another method of withdrawing what he wrote under such an impression than that which you have framed for him in the following protest. "As a strange notion seems to pervade this paper that every thing depends on the character of Cavendish, it may be as well to repeat the following disclaimer, already very distinctly made, of all intention to cast the slightest doubt upon that great man's perfect good faith in the whole affair, I never having supposed that he borrowed from Mr. Watt, though M. Arago, Professor Robison and Sir H. Davy, as well as myself, have always thought that Mr. Watt had, unknown to him, anticipated his great discovery."

Of the deceased philosophers, whose names are here pressed into this service, I shall presently have occasion to speak; but let me first venture to answer for M. Arago, that if *he* has "read the fac-similes" of Cavendish's notes, you will not find him at the same loss as yourself to discover the *inferences* of the experimental philosopher in *the steps of his investigation*; he will not join you in propounding, "that in all Cavendish's diaries and notes of his experiments, not an intimation occurs of the composition of water having been *inferred* by him earlier than Mr. Watt's paper of spring 1783."

Those celebrated experiments of 1781, which pass with chemists for a model of a well-combined train of analytical and synthetical research, you imagine to have been without object or *inference*, till an imperfect attempt to repeat them had the good luck to be reasoned upon by Watt in 1783. You appear to think that the manner in which the great facts of experimental philosophy are ascertained is by one man's stumbling on the *proofs*, and another some time after hitting on the *conclusion*. If it be so, I believe that you would have been as capable of interpreting such experiments, once made, as James Watt himself; and could you have been at hand when Cavendish, in July 1781, completed the discovery of those facts which prove the composition of water, he need not have waited so long to learn what to *infer* from them: I doubt not but that you would at once have drawn the *inference* for him, established the *theory*, and become for ever memorable as the true discoverer; you would, in your own amended phrase, "*unknown to him, have anticipated his great discovery*."

But I own I do not suspect your "colleague" of these peculiar views. Once satisfied that Cavendish spoke truth when he said that all the experiments on this subject published in his paper were made by him in the summer of 1781, he will no longer doubt to whom the discovery of this important fact is due; once convinced that the experiments were communicated to Priestley, and that the attempt to repeat them was made in consequence of that communication; once aware that the repetition was abortive because made with a *wrong* gas, that neither the phlogiston nor the inflammable air of Priestley and Watt were convertible terms for *hydrogen*, and that their notions of the change of water into air and air into water had no reference to that particular gas, but first to *nitrogen*, and afterwards to a mixture of gases, the chief of which was *carbonic oxide*—M. Arago will keep you "*waiting*" long before he rejoins you in the advocacy of any part of the supposed claims of your client, or thanks you for classing him with yourself as still cherishing the conviction that "Mr. Watt had, unknown to Cavendish, anticipated his great discovery."

That which renders the self-devotion of this knight-errantry complete, is the singular fact that you are fighting for Watt *against himself*. I had formerly come to the conclusion that he never thought of claiming the discovery in the sense which you suppose, nor in any other respect than as regards the theory of the extrication of heat and light from the combining gases; and a circumstance has lately been pointed out to me by a friend, which establishes this conclusion.

The edition of Robison's Mechanical Philosophy, published by Sir D. Brewster, was revised by Watt himself. In that revision we find him by no means indifferent to his own just fame. Writing to the Editor he says, "I have carefully perused my late excellent friend Dr. Robison's articles, 'Steam and Steam-Engines,' in the *Encyclopædia Britannica*, and have made remarks upon them in such places, where either from the want of proper information, or from too great a reliance on the powers of his extraordinary memory at a period when it probably had been weakened by a long state of acute pain, and by the remedies to which he was obliged to have recourse, he had been led into mistakes in regard to facts, and also in some places where his deductions have appeared to me to be erroneous. Dr. R. qualifies me as 'the pupil and intimate friend of Dr. Black:' he afterwards, in his dedication to me of Dr. Black's Lectures upon Chemistry, goes the length of supposing me to have professed to owe my improvements upon the steam-engine to the instructions and information I had received from that gentleman, which was certainly a misapprehension; as though I always felt and acknowledged my obligations to him for the information I had received from his conversation, and particularly for the knowledge of the doctrine of latent heat, I never did nor could consider my improvements as originating in those communications. He is also mistaken in his assertion, p. 8 of the preface to the above work, that I had attended two courses of the Doctor's lectures; for unfortunately for me, the necessary avocations of my business prevented me from attending his or any other lectures at College." Mr. Watt then quotes from these lectures a passage in which Black is made to say, "My own fortunate observation of what happens in the formation and condensation of steam, had suggested to my friend Mr. Watt his improvements in the steam-engine," and remarks, "it is very painful to me to controvert any assertion or opinion of my revered friend; yet in the present case I find it necessary to say that he appears to have fallen into an error*."

But in revising the article on Steam, and making remarks on those places in which Dr. Robison had been led into mistakes, Watt makes no remark on the following very decisive passage:—"We know that in vital or atmospheric air there is not only a prodigious quantity of fire which is not in the vapour of water, but that it also contains light, or the cause of light, in a combined state. This is fully evinced *by the great discovery of Mr. Cavendish of the composition of water*: there we are taught that water, and consequently its vapour, consists of air from which the light and greatest part of the fire have been separated; and the subsequent discoveries of the celebrated Lavoisier show that almost all the condensable gases with which we are acquainted, consist either of airs which have lost much of their fire, and perhaps light too, or of matters in which we have no evidence of light and fire being combined in this manner."

Thus you see, that jealous as Watt appears of any undue share in his own discoveries being attributed even to his "revered friend" Dr. Black, he allows "the great discovery of the composition of water" to be assigned to Cavendish without reclaiming the least participation in it for himself.

These extracts entirely relieve his memory from any suspicion of his having been a party to the erroneous statements contained either in the article 'Water' in the first edition of the *Encyclopædia Britannica*, to which you have referred, or in the posthumous lectures of Black. Nor do I hold Black responsible for the fabulous history of this discovery given in the latter work. It is well known that that unambitious man left behind him no MSS. of any account, and that the Lectures published under his name were chiefly composed out of the reminiscences of the able but incorrect Editor. Robison, on historical points, was a very inaccurate writer; and to his inaccuracy I attribute the extraordinary string of errors on this subject which I have formerly pointed out.

It is from the latter work that you seem to have taken your

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* I conceive Watt to mean that the *facts* known to him respecting the condensation of steam, independent of Black's *theoretical explanation* of them, were the foundation of his improvements; and I am bound therefore, on his own showing, to allow that M. Arago has done right in not placing the merit of Watt in the study and application of *abstract philosophical principles*, so much as in ingenuity of mechanical contrivance and the happy adaptation of well-observed facts.

supposed facts; and you have in consequence entirely misstated the nature of Cavendish's experiments. Where, allow me to ask, do you find in his paper, or his notes, any such matter as this? "He then weighed accurately the air of both kinds, which he exposed to the stream of electricity; and he afterwards weighed the liquid formed by the combustion: he found that the two weights corresponded with great accuracy" (Life of Cavendish, p. 433): and again, "Water equal to the weight of the two gases taken together remained as the produce of the combustion." *Cavendish made no such experiments*; as you will find whenever you take the trouble to read either the documents themselves*, or my account of them†. I have already stated that *this* method of determining the composition of water, which is attended with great practical difficulties, was tried indeed at a later time by the French philosophers with such accuracy as it admits of, but that Cavendish, with his usual sagacity, had taken an easier and more certain road: having mastered beyond any of his cotemporaries the analysis of gases, and possessed himself of their specific properties, he was enabled to substitute the method of volume for that of weight; he found that about two volumes of hydrogen and one of oxygen, when burnt together, entirely disappeared without loss of weight, and that *pure water* was the result. To draw from these premises the obvious conclusion, there was no need to weigh or compare the weight of the airs, and the water that lined the glass after combustion; and he did *not* compare it. Lavoisier followed in his steps: and should you ever read his papers, you will find that *he* too in the first instance contented himself with *deducing* the equality of the weights as a *corollary* from experiments of the same kind as those of Cavendish.

Had you happened to consult the *second* edition of the *Encyclopædia Britannica* as well as the *first*, you would have found it purged both of these, and some other of Robison's historical mistakes. You would have found all that you have referred to *omitted*; and in the article 'Chemistry,' compiled under the revision of friends and connections of Watt, the following account substituted in its place. "In the year 1781, Mr. Cavendish proved that water is not a simple element, but that it is composed of pure or vital air, and inflammable air." "In the mean time the French chemists were not idle; the celebrated Lavoisier, in conjunction with some of his philosophical friends, confirmed by the most decisive experiments the truth of *Mr. Cavendish's discovery of the composition of water*, which

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* Phil. Trans. vol. lxxiv. Experiments on Air, by H. Cavendish, Esq. Report of the British Association for 1839, autograph notes of experiments.

† Report of the British Association for 1839, pp. 35, 36.

was now received and adopted by almost every chemist." A detailed account is then given of Cavendish's experiments; and it is added, "*these experiments were made in* 1781, *and they are undoubtedly conclusive of the composition of water.* It would appear that Mr. Watt entertained the same ideas on this subject. When he was informed by Dr. Priestley of the result of *these* experiments, he observed, Let us consider what obviously happens in the deflagration of oxygen and hydrogen gases," &c. "Thus it appears that Mr. Watt had *a just view of the composition of water, and of the nature of the process by which its component parts pass to a liquid state from that of an elastic fluid.*"

In this account the ideas entertained by Mr. Watt obtain more notice perhaps than would have been accorded to them by an indifferent historian; but the statement of the discovery is correct, as is also that of the view which Watt took of the subject, if we confine the assertion of the justness of his ideas to his apprehension of the relation of Cavendish's discovery to certain *theories of light and heat*; for of the *material base* of water he had certainly no just conception when he wrote the letter which is quoted above. I have shown that his views in 1783 and 1784 were founded on several suppositions:—1st, that Priestley had converted water into *atmospheric air*; 2nd, that he had obtained a weight of water equal to the weight of a mixture of oxygen with *the gases extricated by heat from moist charcoal*; 3rd, that he had shown good reason to believe that *carbon*, combined in a certain proportion with oxygen, constitutes water. All these suppositions agreed perfectly with the opinions which Watt really expressed, that water was formed of *dephlogisticated air* and *phlogiston*; but no one of them is consistent with the opinions attributed to him by an erroneous translation of his words, that water is formed by the combination of oxygen with *hydrogen* gas*.

From your mention of Sir H. Davy's sentiments without a quotation, I suppose that he, like Dr. Henry, has been among the number of those on whose attention this untenable claim has been privately pressed; all I know of Davy's opinion on the subject is from his *published works*, in which he has spoken, like other chemists, of the composition of water and of nitric acid as "*the two grand discoveries of Mr. Cavendish.*" But in referring to the name of this much-honoured and regretted friend, I must take the opportunity of noticing what I think a serious error in your impressions respecting one point in his personal character. You begin your sketch of his life with these words: "Sir H. Davy being now removed beyond the reach of

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* See Report of the British Association for 1839, pp. 24, 25.

such feelings, as he ought always to have been above their influence, that may be said without offence of which he so disliked the mention; he had the honour of raising himself to the highest place among the chemical philosophers of the age, emerging by his merit alone from an obscure condition." A simple anecdote may suffice to set his feelings on this subject in a more favourable light. When Davy was exhibiting to myself and three others the discoveries which he had then recently made relative to his safety-lamp, and when those present, among whom were the Hanoverian minister and the late Lord Lonsdale, were highly admiring the beauty of his experiments, with still higher admiration I heard him reply, "Yes, I have some reason to be proud of them, for my experiments on flame were first made *with a tallow candle in an apothecary's shop*."

In these slight sketches which you have given us of the history of men eminent in science, there is one other scientific subject besides the discovery of the composition of water, on which you appear to have bestowed some consideration, namely,—the first discoveries of the gases. Here Cavendish is still out of favour with you. You pluck another feather from his wing; and having made a present of the discovery of water to Mr. Watt, dispense that of hydrogen gas to Dr. Black.

"The nature of hydrogen," you say, "was perfectly known to him, and both its qualities of being inflammable, and of being so much lighter than atmospheric air; for as early as 1766 he invented the air-balloon, showing a party of his friends the ascent of a bladder filled with inflammable air: Mr. Cavendish only more precisely ascertained its specific gravity, and showed, what Black could not have been ignorant of, that it is the same from whatever substance it is obtained [*]."

You ought to have recollected, when again contravening the received opinion of chemists [†], your own remarks on the supposed omission of Cavendish to state exactly the time when he had communicated to Priestley his experiments on the composition of water. "Dans une addition de Blagden faite avec le consentement de Cavendish, on donne aux expériences de ce dernier le date de l'été de 1781. On cite une communication de [à] Priestley, *sans en préciser l'époque*, sans parler de conclusions, sans même dire quand ces conclusions se pré-

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[*] Life of Black, p. 383.

[†] The *received* account of the discovery of hydrogen is this:—"Its combustible quality is described in the works of Boyle and Hales, of Boerhaave and Stahl; but it was not till the year 1766 that its properties were particularly ascertained, and the difference between it and atmospheric air pointed out by Mr. Cavendish."—Encycl. Brit., Art. Chemistry, 1810.

sentaient à l'esprit de Cavendish. Ceci doit être regardé comme une très grosse omission (*a most material omission**)." Nothing indeed can be more unfounded than this animadversion. In the passage to which you refer, the words of Cavendish are these:—"All the foregoing experiments on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made by me in the summer of 1781, and mentioned by me to Dr. Priestley, *who in consequence of it made some experiments of the same kind, as he relates in a paper printed in a preceding volume of the Transactions.*" Now you need only have referred to the volume of the Transactions which Cavendish quotes, to have found the "*epoch*" which you wanted. Priestley's paper was printed in March 1783; and therefore Cavendish's communication of his "conclusive" experiments was *anterior* to Watt's speculations in April, as well as to Lavoisier's experiments in June of the same year.

But though this "*most material*," or in M. Arago's translation, this "*grosse*" omission turns out to be *none*, you ought, I repeat it, to have remembered your own demand for preciseness of dates, when you ascribed to Black a prior knowledge of the distinguishing properties of hydrogen gas. In proof that Black knew before Cavendish that this gas is "*so much lighter than atmospheric air*," you allege, that "as early as 1766 he invented the air-balloon, showing a party of his friends the ascent of a bladder filled with inflammable air: Mr. Cavendish only more precisely ascertained its specific gravity."

As early as 1766?—Are you not aware that Cavendish's paper on factitious airs was published in this year? Is it not a "*most material omission*" that you have forgotten to "*préciser l'époque*" of Black's experiment with the balloon, so as to show whether it was before or after the publication of Cavendish's paper? Professor Leslie tells the story of the balloon somewhat differently from you. "The late most ingenious and accurate Mr. Cavendish, in 1766, found, by a most nice observation, this fluid to be at least seven times lighter than atmospheric air. It *therefore* occurred to Dr. Black of Edinbro', that a very thin bag filled with hydrogen gas would rise to the ceiling of a room. He provided accordingly the allantois of a calf, with a view of showing at a public lecture such a curious experiment before his numerous auditors; but owing to some unforeseen accident or imperfection it chanced to fail, and that celebrated Professor, whose infirm state of health and indolent temper more than once allowed the finest dis-

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* Historical note, Life of Watt, p. 383.

coveries when almost within his reach to escape his penetration, did not attempt to repeat the exhibition, or seek to pursue the project any further."

If you are dissatisfied with Leslie's version of your anecdote, let me refer you to other authorities. In one of those articles of the *Encyclopædia Britannica* which are stated to have been composed or revised by Professor Miller, Dr. Muirhead, and Sir David Brewster, the circumstance is thus narrated:—"In the year 1766 Mr. Henry Cavendish ascertained the weight and other properties of this gas, determining it to be at least seven times lighter than atmospheric air. *Soon after which* it occurred to Dr. Black that perhaps a thin bag filled with hydrogen gas might be buoyed up by the common atmosphere."

I hope I have now illustrated sufficiently the value of the canon of criticism which you have laid down for these delicate inquiries,—that nothing is so necessary as to "*préciser l'époque.*"

"Cavendish," you say, "only more precisely ascertained the specific gravity of inflammable air; and showed, *what Black could not be ignorant of, that it is the same from whatever substance it is obtained.*" Now, in the first place, *inflammable air* is *not* the same from whatever substance it is obtained. This was the error into which Priestley fell when he attempted to repeat the experiments by which Cavendish had discovered the composition of water; this was the error under which Watt laboured till after the publication of Cavendish's paper in 1784, and which nullified the researches of the one and the speculations of the other. But supposing you to mean "that inflammable air is the same, *whether obtained from zinc or iron*," why do you say that Black could not be ignorant of *that*? How do you think he was to know it? How did Cavendish know it? He tells you that he learnt it by having ascertained by experiment that the specific gravity of the gas from either material was the same. Had Black ascertained this? Had he any test whatever by which he could know that these gases were the same?

But Cavendish "*only* more precisely ascertained the *specific gravity* of inflammable air." If any person conversant in the history of pneumatic discoveries were to be asked to enumerate the most important of the early advances in that branch of science, he would certainly name—1st, the discovery of the weight of the air by Galileo; 2nd, the discoveries of its law of compression, and of the factitious gases, by Boyle; 3rd, the theory of the fixation of gases by chemical attraction, propounded by Newton; 4th, the discovery of specific and elective affinities in one of those gases, by Black; 5th, the discovery of the difference of specific gravity in several gases, by Cavendish.

You do not distribute their honours to any of these great discoverers with a severe attention to matter of fact; but I must do you the justice to own that you preserve a principle of equity in your adjudications. You omit, it is true, to dwell upon, or even to mention, the main point of novelty in the researches of Black; but then you give to Black the discoveries of Boyle and Cavendish, and make it up to Cavendish by allowing him a slice of the merit which belongs to Galileo.

For Cavendish you say, "He carried his mathematical habits into the laboratory; and not satisfied with showing the other qualities which make it clear that these two aëriform substances are each *sui generis*, and the same from whatever substances, by whatever processes they are obtained,—not satisfied with the mere fact that one of them is heavier, and the other much lighter than atmospheric air," (a previous acquaintance with all which facts you have taken care to ascribe to Dr. Black) "he inquired into the precise numerical relation of their specific gravities with one another and with common air, and *first showed an example of weighing permanently elastic fluids*: unless indeed Torricelli may be said before him to have shown the relative weight of a column of air and a column of mercury, or the common pump to have long ago compared in this respect air with water. It is however sufficiently clear that neither of these experiments gave the relative measure of one air with another; nor indeed could they be said to compare common air with either mercury or water, although they certainly showed the relative specific gravity of the two bodies, taking air for the middle term or common measure of their weights."

What a strange qualification of a still stranger assertion! If instead of this confusion of specific gravities with equiponderant columns, ending with the grave suggestion, that "the relative specific gravities of water and mercury" might have been taken by the intermediation of "air," you had said that philosophers have attempted, from the relative heights of the barometer at different elevations, to calculate the mean specific gravity of the atmosphere*, there would have been mean-

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* The following quotation will show the nature of these calculations (Dan. Bernoulli Joh. Fil. Hydrodynamica, *Argentorati*, 1738. Sect. 10. 16. p. 209):—"Patet exinde quid censendum sit de illa methodo qua in Anglia aliquando usos esse recenset D. Du Hamel, in Hist. Acad. Sc. Paris. ad indagandam rationem inter gravitates specificas aëris et mercurii: observata nimirum altitudine mercurii in loco humiliori, tum etiam in altiori, gravitates specificas in aëre et mercurio statuerunt, ut erat differentia

ing at least in the qualification; but then what an assertion to hazard! considering the great number of experiments extant for the *direct* determination of the weight of air compared with that of water, first instituted by Galileo, and then repeated successively by Descartes, Mersenne, Boyle, Hook, Newton, Cotes, and lastly by Hawksbee, whose determination was assumed and quoted by Cavendish himself for the purpose of comparing the specific gravity of common air with those of the factitious gases,—it is a strong instance of the kind of equity for which I have given you credit, that you should have allotted to Cavendish the merit of having "*first showed an example of weighing permanently elastic fluids.*" Even Descartes allowed that Galileo's "method of weighing

altitudinum mercurii in barometro ad altitudinem inter locos observationum interceptam; etiamsi aër ejusdem densitatis ponatur ab imo observationis loco ad alterum usque, non licet tamen inde judicare de ejus gravitate specifica ratione mercurii. Quicquid ab experimento colligere licet hoc solum est:—

"Consideremus scilicet integram crustam aëream terram ambientem atque inter ambo observationis loca interceptam, et erit pondus istius crustæ ad superficiem terræ ut pondus columnæ mercurialis qualis in barometro descendit ad basin ejus; manifesta hæc sunt ex eo quod summa basium A et B sustinent quidem summam ponderum quæ habent columnæ aëreæ A C et B D, neque tamen quævis basis premitur suæ columnæ pondere seorsim, et quod idem resectis columnis A *g* et B *h* intelligi debet de columnis *g* C et *h* D, diaphragmatis in *g* et *h* positis, incumbentibus. Igitur experimentum non tam gravitatem specificam aëris in quo factum est indicat, quam omnis aëris terræ proximi gravitatem specificam mediam determinat; prior admodum variabilis est, altera procul dubio constanter eadem fere permanet.

"Faciamus computum *gravitatis specificæ istius mediæ* aëris omnis qui terram ambit. Multis vero experimentis, quæ in diversis locis parum supra mare elevatis sumpta fuerunt, id constat, elevationi 66 pedum proxime descensum respondere unius lineæ in barometro. Sequitur inde quod aëris gravitas specifica media ratione mercurii sit, ut altitudo unius lineæ ad altitudinem 66 pedum, *i. e.* ut ut 1 ad 9504, ergo posita gravitate specifica mercurii = 1, erit gravitas specifica *media* aëris = 0·00105. Notabile est profecto tantam esse hanc gravitatem mediam aëris: certus enim sum vel maxime sævienti hic locorum frigore aëris gravitatem specificam vixdum tantam esse quantam nunc exhibuimus pro statu medio omnis aëris terram ambientis; at sub æquatore multo erit minor, et omnibus recte perpensis non crediderim *gravitatem mediam* aëris qui inter utramque latitudinem 60 gr. continetur, ultra 0·000090 excurrere; quo posito erit *gravitas media* aëris ab utroque polo ad 30 gradus terram cingentis, quod spatium paullo plus quam octavam totius terræ superficiei efficit partem, = 0·000210, quæ dupla est aëris hic locorum densissimi: sub ipso autem polo, præsertim antarctico, admodum gravior erit aër, et fortasse aqua vix decies levior, cum est frigidissimus atque densissimus.

"32. Et quia aëris mediocriter densi gravitas specifica est ad gravitatem specificam merc. ut 1 ad 11000, ipsaque altitudo media merc. in barometro pro locis parum a superficie maris elevatis sit 2⅓ ped. Paris. erit altitudo aëris homogenii mediocriter densi 25666 pedum."

the air was not amiss*;" and the experiments of the great Italian philosopher, which laid the original foundation of all our knowledge of elastic fluids, ought not to have been entirely forgotten by any one who appreciates duly those capital discoveries by which the ideas of men are fixed and a new order of facts is ascertained.

To Black, on the other hand, with like even-handed justice, you ascribe a knowledge of the lightness of hydrogen and the heaviness of carbonic gas, which you have no ground for suspecting him to have possessed. Experiments, indeed, had been made with a view of ascertaining such points, and your assertion, that "Cavendish first set the example of weighing permanently elastic gases," is so far from the truth, that the factitious gases themselves had been weighed both by Hawksbee and Hales. Hales weighed the "air of tartar," which consists of a mixture of carburetted hydrogen and carbonic gases in a bladder, and then filling it with common air compared the weights †; Hawksbee ascertained accurately the specific gravity of air that had passed through tubes filled with iron wires, and heated red in the fire, which consisted partly of carbonic acid and partly of nitrogen ‡. But these mixed

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* "Sa façon de peser l'air *n'est pas mauvaise*, si tant est que la pesanteur en soit si notable qu'on la puisse apercevoir par ce moyen; mais j'en doute." (*Œuvres de Descartes*, tom. vii. p. 440.) Thus Descartes wrote to Mersenne in 1638. In 1642 he repeated the experiment himself by a method far less susceptible of accuracy, and obtained a result much further from the truth, which satisfied him however, "que la poids de l'air est sensible en cette façon." (*Œuvres*, tom. viii. p. 567.) Dr. Whewell has taken notice (History of Mechanics, p. 66) that "in a letter of the date of 1631 he (Descartes) explains the suspension of mercury in a tube closed at the top by the pressure of the column of air reaching to the clouds." In this letter the atmosphere is compared to a pack of wool, the filaments of which are all heavy, and press on each other from the clouds to the earth, being only kept apart by the æther which plays between them, "ce qui fait un grand pesanteur"—expressions which at first sight might lead to the idea that he had anticipated the theory of the elevation of the barometric column; but it is evident from many subsequent letters of Descartes, that he had no correct conception of the statical pressure of fluids, and was therefore incapable of reasoning justly on this subject. The tube in which the mercury was suspended in the case in question, was *a straight tube without a bason*: he tried to account for the phænomenon of its suspension on his principle of *circular movement in a plenum*, by supposing that the mercury, before it could quit the tube, must effect the circle of motions required to bring down from the sky a current of æther to supply the vacuum left at the top of the tube by the descent of the quicksilver; and presuming the column of air which it had to lift to be as heavy as itself, he concluded that no such circular motion in the chain of matter could take place. It is possible however that this representation of the atmosphere as a heavy column may have conduced to suggest the more correct views of the subject afterwards adopted.

† Veg. Status, p. 185.

‡ Phil. Trans., No. 328, p. 199.

gases approached too nearly to common air in that respect to enable the experimenters to establish a distinction. An attempt too had been made by Greenwood, a Professor of Mathematics at Cambridge in New England, to ascertain the specific gravity of the deleterious air in a well, which was doubtless chiefly carbonic gas; but the method employed by him was not sufficiently delicate to show a difference of density. Such was the state of knowledge, or rather ignorance, on this subject previous to the experiments of Cavendish. We have not the least reason to believe that any one had observed the different weights of the different kinds of air. Dr. Mayow* indeed about a century before had supposed his "*nitro-igneous aura*," to the combinations of which he ascribed the phænomena of acidification, combustion and vitality, to be *heavier* than the residual air from which it is separated in those processes; and this opinion, which proved to be correct, he entertained so distinctly, as to represent the specific lightness of the vitiated air, after it had served its purpose of sustaining life, as a provision of nature for freeing us from a noxious atmosphere. But he had no better ground for entertaining such an opinion than his observation of the movements of animals which he had confined in a close vessel, and which appeared in his experiments to seek for a less suffocating air in the lower part of the receiver, whilst they avoided the upper.

Such loose surmises as these detract nothing from the great experimental discovery of Cavendish, the importance of which cannot be better expressed than in the words of an eminent chemist and chemical historian†: "It can scarcely be said that pneumatic chemistry was properly begun till Mr. Cavendish published his valuable paper on Carbonic Acid and Hydrogen Gas, in the year 1766." On the fruits of this discovery, in the hands of its author and of all succeeding chemists, and its consequences to the study of gaseous substances and their combinations, I need not dwell. It is enough to remark, that the ascertainment of *this physical* difference in the gases was the first *conclusive* proof of *a plurality* of elastic fluids.

Another point of no small consequence to pneumatic chemistry was first made out in this paper. From the earliest discovery of factitious airs, it had been observed that a considerable portion of several of these disappeared after they had been generated, though there had been no change of temperature or pressure. The usual statement of this phænomenon

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* *De parte aëria igneaque Spir. Nitri.*

† Dr. J. Thomson's Biographical Account of Priestley, Ann. Phil., vol. i. p. 91.

was, that the elasticity of the air had been *destroyed*. Dr. Hales, dissatisfied with so loose an explanation, accounted for the loss, which in the case of nitrous acid *he* first observed, after the following manner:—"When fresh air is let into the receiver, whose included air is impregnated with the fumes arising from the mixture of compound *aquafortis*, or spirit of nitre, and Whitstable pyrites, mentioned in the following experiment, then the air in the receiver turns very red and turbid, and much air is absorbed after several repeated admissions. When fresh air is thus admitted into the glasses full of sulphureous, though clear, air, a good many particles of the fresh air must needs be reduced by the sulphureous ones from an elastic to a fixed state, as in the effervescences of other liquors. Therefore the rising of the water in the glass vessel does not seem to be wholly owing to the rebating of the air's elasticity in some degree, but rather to the reduction of it from an elastic to a fixed state, which is further probable from hence, viz. that the whole quantity of air admitted at several times is equal, or nearly equal, to the quantity of sulphureous air A. Z., so that *both airs are at the same time contained within the space A. Z.*"

In this important observation, which was subsequently turned to such good account by Priestley and Cavendish, Hales gave the true theory of the loss of volume which occurs by the admission of common air to nitrous gas; but the variable, and apparently capricious, loss of elasticity which he remarked in other gases he could not explain. "Though a good part of the air," he says, "which rises from *fluids* seems to have existed in an elastic state in those fluids, yet the air which arises from *solid* bodies, either by the force of fire or effervescence, does not seem to arise only from the interstices of those bodies, but principally from the most fixed parts of them. For since the airs which are raised by the same acid spirit from a vast variety of substances have very different degrees of permanency, as was shown in Exp. 10, No. 3, 4, 5, 6, and in Exp. 11, No. 6, 7, 8, 9, 10 of experiments on stones, hence it is probable that these airs do not arise from latent interstices of the dissolved stones, &c., but from the solid fixed particles of them; and since the whole of some of these newly-generated airs does in a few days lose its elasticity, it should seem hence probable, that whatever air arises from the spirit in the effervescence is not permanently elastic, or else that in the rotation of some stones it is thrown off into a more permanently elastic state than from others."

The cause of this loss of volume was first explained in Cavendish's paper: he proved by experiment, that carbonic acid is condensed over water, but not over mercury. *You* indeed tell us that Black "*found this gas incondensable*," but he has nowhere told us as much himself; and you might with more safety have presumed the contrary; the true statement being, that he and his predecessors had found it *condensable*, and that Cavendish found the conditions under which it is *not condensed*.

In the same spirit of liberality you take "*the capital discovery*," that the air of the atmosphere is not the only air permanently elastic, from its ancient owners, to appropriate it to Black, and expend much learned pains in setting forth the originality and importance of the "*doctrine*" which you ascribe to him. "The great step," you say, "was now made, that the air of the atmosphere is not the only permanently elastic body, but that others exist, having perfectly different qualities from atmospheric air, and capable of losing their elasticity by entering into chemical union with solid and with liquid substances, from which, being afterwards separated, they regain the elastic or aëriform state."....."In order to estimate the importance of this discovery, and at the same time to show how entirely it attends the whole face of chemical science, and how completely the doctrine was original, we must now examine the state of science which philosophers had previously attained to. It has often been remarked, that no great discovery was ever made at once, except perhaps that of logarithms: all have been preceded by steps which conducted the discoverer's predecessors nearly, though not quite, to the same point. Some may perhaps think that Black's discovery of fixed air affords no second exception to this rule; for it is said that Van Helmont, who flourished at the end of the sixteenth and beginning of the seventeenth century, had observed its evolution during fermentation, and gave it the name of *gas sylvestre*, spirit from wood, remarking that it caused the phænomena of the Grotto del Cane near Naples; but though he, as well as others, had observed an aëriform substance to be evolved in fermentation and in effervescence, there is no reason for affirming that they considered it as differing from atmospheric air, except by having absorbed or become mixed with various exhalations or impurities. Accordingly a century later than Van Helmont, Hales, who made more experiments upon air than any of the old chemists, adopts the commonly received opinion, that all elastic fluids were only different combinations of the atmospheric air with various exhalations or impurities: and this was the universal opinion upon the subject, both of philosophers and the vulgar."....."It is now fit that we see in what manner the subject was treated by scientific men at the period immediately preceding Black's discoveries. The article 'Air,' in the French *Encyclopédie*, was published in 1751, and written by D'Alembert himself. It is, as might be expected, able, clear and elaborate. He assumes the substance of the atmosphere to be alone entitled to the name of air, and to be the foundation of all other permanently elastic *bodies*. When D'Alembert wrote this article, he gave the doctrine then universally received, that all the other kinds of air were only impure, and that this fluid alone was permanently elastic, all other vapours being only like steam, temporarily aëriform. Once the truth was made known, that there are other gases in nature, only careful observation was required to find them out*."

After all this, should I venture to affirm that you have postdated our knowledge of permanently elastic gases, other than the atmosphere, by about a hundred years, were I to suggest that in this case also the old story is the true one, and that Priestley has correctly recorded the real historical fact when he said, "Mr. Boyle, I believe, was the first who discovered that what we call fixed air, and also inflammable air, are really elastic fluids capable of being exhibited in a state unmixed with common air," were I to add that the existence of various elastic fluids was generally recognised by the philosophers of Europe, and particularly by those whom you have quoted as instances to the contrary, during the century which preceded Black's essay, as distinctly, and more distinctly, than by Black himself,—I know not what you would think of me. Nevertheless, since this is a passage in the history of science which deserves to be told with a strict regard to dry matter of fact, I must beg you to listen with patience to an account of it certainly very different from your own.

It was in December 1659 that Boyle published his "New Physico-mechanical Experiments," among which is to be found a description of two of those gases separable from fixed bodies, which he subsequently denominated *factitious airs*. The high interest which may be justly attached to all the circumstances of discoveries so important as this, induces me to give the details of it in the words of the author.

"Contenting myself," he says, "to have mentioned our author's (Kircher's) experiment as a plausible, though not demonstrative, proof that water may be transmuted into air, we will pass on to mention, in the third place, another experiment which we tried in order to the same inquiry. We took a clear glass bubble, capable of containing by guess about three ounces of water, with a neck somewhat long and wide of

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* Life of Black, pp. 331-36.

a cylindrical form: this we filled with oil of vitriol and fair water, of each almost a like quantity, and casting in half a dozen small nails we stopped the mouth of the glass, which was top-full of liquor, with a flat piece of *dia palma* provided for the purpose, that, accommodating itself to the surface of the water, the air might be exquisitely excluded; and speedily inverting the phial, we put the neck of it into a small wide-mouthed glass that stood ready with more of the same liquor to receive it. As soon as the neck had reached the bottom of the liquor it was dipped into, there appeared at the upper part, which was before the bottom of the phial, a bubble of about the bigness of a pea, which seemed rather to consist of small and recent bubbles produced by the action of the dissolving liquor upon the iron, than any parcel of the external air that might be suspected to have got in upon the inversion of the glass, especially since we gave time to those little particles of air which were carried down with the nails into the liquor to fly up again. But whence the first bubble was produced is not so material to our experiment, in regard it was so small; for soon after we perceived the bubbles produced by the action of the menstruum upon the metal, ascending copiously to the bubble named, and breaking, did soon exceedingly increase it, and by degrees depress the water lower and lower, till at length the substance contained in these bubbles possessed the whole cavity of the glass phial, and almost of its neck too, reaching much lower in the neck than the surface of the ambient liquor wherewith the open-mouthed glass was by this means almost replenished. And because it might be suspected that the depression of the liquor might proceed from the agitation whereinto the exhaling and imprisoned steams were put by that heat which is wont to result from the action of corrosive salt upon metals, we suffered both the phial and the open-mouthed glass to remain as they were in a window for three or four days and nights together; but looking upon them several times during that while, as well as at the expiration of it, the whole cavity of the glass bubble and most of its neck seemed to be possessed by air, since by its spring it was able for so long to hinder the expelled and ambient liquor from regaining its former place. And it was remarkable that just before we took the glass bubble out of the other glass, upon the application of a warm hand to the convex part of the bubble, the imprisoned substance readily dilated itself like air, and broke through the liquor in divers bubbles succeeding one another.

"Having also another time tried the like experiment with a small phial and with nails dissolved in *aquafortis*, we found nothing incongruous to what we have now delivered. And this circumstance was observed, that the newly-generated steams did not only possess almost all the whole cavity of the glass, but divers times without the assistance of heat of my hand did break away in large bubbles through the ambient liquor into the open air: so that the experiments with corrosive liquors seemed manifestly to prove, though not that air may be generated out of water, yet that in general air may be generated anew.

"Lastly, to the foregoing arguments from experience, we might easily subjoin the authority of Aristotle and of his followers the schools, who are known to have taught that air and water, being symbolizing elements in the quality of moisture, are *easily transmutable into each other**; but we shall rather to the foregoing argument add *this*, drawn from reason—that if, as Leucippus, Democritus, Epicurus and others, followed by divers modern naturalists, have taught, the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the small parts they consist of (all the qualities that make them differ being deducible from thence), there appears no reason why the minute parts of water, and other bodies, may not be so agitated and connected as to deserve the name of air; for if we allow the Cartesian hypothesis, according to which the air may consist of any terrene or aqueous corpuscles, provided they be kept swimming in the interfluent celestial matter, it is obvious that air may be as often generated, as terrestrial particles, minute enough to be carried up and down by the celestial matter, ascend into the atmosphere. And if we will have the air to be a congeries of little slender springs, it seems not impossible, though it be difficult, that the small parts of divers bodies may, by a lucky concourse of causes, be so connected as to constitute such little springs, since water in the plants it nourisheth is usually contrived into springy bodies, and even the bare altered position and connexion of the parts of a body may suffice to give it a spring that it had not before, as may be seen in a thin and flexible plate of silver, into which, by some strokes of a hammer, you may give a spring; and by only heating it red-hot, you may make it again as flexible as before.

"These, my Lord, are some of the considerations at present occurring to my thoughts, by which it may be made probable that air may be generated anew."

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* This I presume is the *hypothesis*, *doctrine*, or *theory* which Cavendish was suspected of deriving from Watt.

In a subsequent part of the same treatise, Boyle adds an account of another discovery of a similar kind. "I took," he says (exp. 42), "whole pieces of red coral, and cast them into as much spirit of vinegar as sufficed to swim about an inch over them: these substances I made use of that the ebullition upon the solution might not be too great, and that the operation might last the longer." It gave but few bubbles, till the receiver under which it was placed was exhausted; "then the menstruum appeared to boil in the glass like a seething-pot. To avoid suspicion, that these proceeded not from the action of the *menstruum* upon the coral, but from the sudden emersion of those many little parcels of air that are wont to be dispersed in liquors, we conveyed over distilled vinegar alone into the *receiver*, and kept it awhile there to free it from the bubbles, which were but very small, before ever we put the coral into it. The former experiment was another time tried in another small receiver with coral grossly powdered, and the success was much alike."

Of the two gases thus first obtained and separated, he observed some time afterwards that the one was inflammable*,

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* "Having provided a saline spirit, which by the uncommon way of preparation was made exceeding sharp and piercing, we put into a phial, capable of containing three or four ounces of water, a convenient quantity of filings of steel, which were not such as are commonly sold in shops to chemists and apothecaries, those being usually not free enough from rust, but such as I had awhile before caused to be purposely filed off from a piece of good steel. This metalline powder being moistened in the phial with a little of the menstruum, was afterwards drenched with more, whereupon the mixture grew very hot, and belched up copious and very stinking fumes, which, whether they consisted altogether of the volatile sulphur of the Mars, or of metalline steams participating of a sulphureous nature, and joined with the saline exhalations of the menstruum, is not necessary here to be discussed. But whencesoever this stinking smoke proceeded, so inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a bluish and somewhat greenish flame at the mouth of the phial for a good while together; and that though with little light, yet with more strength than one would easily suspect. This flaming phial therefore was conveyed to a *receiver*, which he who managed the pump affirmed that about six exsuctions would exhaust. And the receiver being well cemented on, upon the first suck the flame suddenly appeared four or five times as great as before, which I ascribed to this, that upon withdrawing of the air, and consequently the weakening of its pressure, great store of bubbles were produced in the menstruum, which breaking, could not but supply the neck of the phial with store of inflammable steams, which as we thought took not fire without some noise. Upon the second exsuction of the air, the flame blazed out as before, and so it likewise did upon the third exsuction; but after that it went out, nor could we rekindle any fire by hastily removing the receiver: only we found that there remained such a disposition in the smoke to inflammability, that holding a lighted candle to it a flame was quickly rekindled."—*New Experiments touching the Relation between Flame and Air*, 1671.

and the other liable, in part, to lose its elasticity*; he extended his experiments on the generation of "factitious airs" to a variety of materials, multiplying them to such an extent that one of Cotes's hydrostatical lectures is filled with the repetition of them; he remarked the condensability of muriatic acid gas†, and the orange colour of nitrous acid gas ‡; and extricated from red lead, by the burning-glass, the gas§, which Priestley afterwards having obtained by the same method, was led by reasoning from the manner in which red lead is

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* "*Experiment* 8.—A mercurial gauge having been put into a conical glass whose bottom was covered with beaten coral, some spirit of vinegar was poured in, and then the glass stopper closing the neck exactly: on the working of the menstruum on the coral, store of bubbles were for a good while produced, which successively broke in the cavity of the vessel; and their accession compressed the confined air in the closed leg of the gauge three divisions, which I guessed to amount to about the third part of the extent it had before; but some hours after the compression made by this newly-generated air grew manifestly fainter, and the imprisoned gauge-air drove down the mercury again, till it was depressed within one division of its first station; so that in this operation there seemed to have been a double compressive power exercised, the one transient by the brisk agitation of vapours, the other durable from the aërial or springy particles either produced or extricated by the action of the spirit of vinegar on the coral." —*Phil. Trans.* 1675.

† "May 26, 1676.—Sal-ammoniac was put into a receiver with a sufficient quantity of oil of vitriol. Then the air being exhausted, the salt was put into the oil, whereupon a great ebullition presently followed, and the mercury in the gauge showed a good quantity of air to be generated; but this by the same gauge soon after appeared to be destroyed again. The experiment was repeated, and both the production and destruction were slower than before. It was confirmed by these trials that some artificial airs may be destroyed, but why this destruction happens sometimes sooner and sometimes slower, may perhaps seem worthy of further inquiry."—*2nd cont. Phys. Mech. Expts.* 1676.

‡ "We put an ounce of such strong spirit of nitre as is above mentioned into a moderately large bolt head, furnished with a proportionable stem, over the orifice of which we strongly tied the neck of a thin bladder, out of which most part of the air had been expressed, and into which we had conveyed a small phial with a little highly rectified spirit of wine. Then this phial, that was before closed with a cork, being unstopped without taking off the bladder, a small quantity, by guess not a spoonful, of the alcohol of wine was made to run down into the spirit of nitre, where it presently produced a great commotion, and blew up the bladder as far as it would well stretch, filling also the stem and cavity of the glass with very red fumes, which presently after forced their way into the open air, in which they continued for a good while to ascend in the form of an orange-coloured smoke."—*New Experiments about Explosions*, 1672.

§ "September 4, 1678.—I exposed one ounce of minium in an open glass to the sunbeams, concentrated by a burning-glass, and found that it had lost three-fourths of a grain of its weight, though much of the minium had not been touched by the solar rays. May 29.—Repeated the same experiment, in a light glass phial sealed hermetically and exactly weighed, manufactured, to identify with the oxygen of the atmosphere[*], just as Mayow identified with it the gas from saltpetre.

In giving to the gases which he discovered the title of "factitious *airs*," Boyle did not confound them with *common air*. The extracts which I have given sufficiently show that he used the word *air* generically, in the sense which he assigns to it in the following passage:—"If I were to allow acids to be one principle, it should be only in some such metaphysical sense as that wherein *air* is said to be one body, though it consist of the associated effluviums of a multitude of corpuscles of very different natures that agree in very little, save in their being minute enough to concur in the composition of a fluid aggregate consisting of flying parts[†]."

It would indeed be a great mistake in the history of science, to suppose that the notion of the air being a simple element prevailed among philosophers down to the days of Black. From the time of that remarkable revolution in the scientific mind of Europe which attended the revival of the mechanico-corpuscular philosophy, when the phænomena of nature were accounted for no longer by forms and qualities, but by the sizes and motions, the cohesions and disjunctions of the particles of bodies, the atmosphere came at once to be conceived of as a miscellaneous aggregate of the molecules of a variety of heavy substances thrown into an elastic state, or floating in an active medium of a still finer and more divided consistence.

"Tout corps invisible et impalpable," says Descartes, "se nomme *air*, à savoir en sa plus ample signification[‡]." "By air," says Dr. Wallis, "I find Mr. Hobbs would sometimes have us understand a pure æther, 'aërem ab omni terræ aquæque effluviis purum, qualis putatur esse æther,' to which I suppose answers the *materia subtilis* of Descartes, and M. Hugens's 'more subtile matter' than air: on the other hand, M. Hugens here by air seems to understand that feculent matter arising from those the earth's and water's effluvia, which

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and the loss of weight came to $\frac{1}{33}$rd part of a grain. May 30.—I endeavoured to burn the same minium again, but such plenty of air was produced, that the glass broke into a hundred pieces."

[*] "At the same time that I got the air above-mentioned from mercurius calcinatus and the red precipitate, I had got the same kind from red lead or minium. In this process that part of the minium on which the focus of the lens had fallen turned yellow. The experiment with red lead confirmed me more in my suspicion, that the merc. calcinatus must have got the property of yielding this kind of air from the atmosphere, the process by which that preparation and this of red lead is made being similar."—*Priestley's Experiments on Air*, vol. ii. p. 111.

[†] Reflections on the Hypothesis of Alkali and Acidum, ch. iv. 1676.

[‡] *Œuvres*, tom. vii. p. 237.

are intermingled with this subtle matter. *We* mean by *air* the aggregate of both these, or whatever else makes up that heterogeneous fluid wherein we breathe, commonly called air, the purer part of which is Mr. Hobbs's air, and the feculent of it is M. Hugens's air*."

It is curious to trace the fortunes of this *materia subtilis*, from the naked condition in which it was first ushered into notice, to the figure which it now makes in the speculations of science.

Descartes was undoubtedly the first who formed the idea of a liquid medium grosser than heat, but more subtle than air, extending from the heavenly bodies to the earth, filling the aërial interstices with a continuous series of molecular globules, pervading the pores of glass, diamond, and the densest substances, without interruption, and propagating, by communication of impulses from one molecule to another, the movement, or rather the *pressure without locomotion*, simple and compound, which he considered as constituting light† and colours.

This was a grand conception, for which the philosophy of optics is under an obligation to the inventor greater perhaps than has been confessed. But the range of Descartes's views in physics was too limited to admit of his turning such a conception to its full account. He seems to have had no idea of intermittent or elastic forces, and did not even endow either

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* Extract of Letters from Dr. J. Wallis to the publisher, 1672, Phil. Trans. No. 91.

† Dr. Whewell takes Descartes's "hypothesis concerning light" to have been, "that it consists of small particles *emitted* by the luminous body," and considers this as "the first form of the *emission* theory" (Phys. Optics, ch. x.); and so the theory of the Dioptrics seems to have been understood by some of Descartes's cotemporaries; but he explains himself otherwise in his letters. "Je vous prie de considérer que ces petits globes dont j'ai parlé ne sont point des corps qui exhalent et qui s'écoulent des astres jusques à nous; mais que ce sont des parcelles imperceptibles de cette matière que V. R. appelle elle-même céleste, qui occupent tous les intervalles que les parties des corps transparents laissent entre elles, et qui ne sont autrement appuyées les unes sur les autres que le vin de cette cuve que j'ai pris pour exemple en la page 6 de ma Dioptrique, où l'on peut voir que le vin qui est en C tend vers B, et qu'il n'empêche point pour cela que celui qui est en E ne tend vers A, et que chacune de ces parties tend à descendre vers plusieurs divers endroits, quoiqu'elle ne se puisse mouvoir que vers un seul en même temps. Or j'ai souvent averti que par la lumière je n'entendois pas tant le mouvement, que cette inclination ou propension que ces petits corps ont à se mouvoir; et que ce que je disois du mouvement, pour être plus aisément entendu, se devoit rapporter à cette propension; d'où il est manifeste que, selon moi, l'on ne doit entendre autre chose par les couleurs que les différentes variétés qui arrivent en ces propensions." (*Œuvres*, tom. vii. p. 193). "J'admire que vous alléguiez les pages 4 et 5 afin de prouver que

his filaments of air, or his æthereal globules interposed between them, with attractive or repulsive powers.

The genius of Hook, so comprehensive of clear physical notions, soon lent to this luminiferous æther the mechanical attribute which it needed, and added the notion of *vibratory pulses*,—a notion which was instantly reduced by Newton to the form most competent to account for the phænomena*, and on which Huygens founded, and Young with his illustrious coadjutors have gone far to finish, the mathematical fabric of the undulatory theory of light, as it is now commonly received.

So necessary indeed to any account of the phænomena of light and colours did the admission of such a medium appear, that Wallis, who not only rejected the use which Huygens and others proposed to make of it in explaining the extraordinary height at which mercury, purged of air, may be suspended in a tube, but denied it the properties of elasticity and weight, nevertheless did not scruple to say, "That there is in our air a body more subtle than the fumes and vapours mixed with it in our lower region *is very certain*: but whether that subtle body be, as Dr. Garden seems to suppose, much heavier than our common air, I much doubt, and rather think it is not, not having hitherto had any cogent experiment either to prove it heavy or elastic; but it may, for aught I know, be void as well of weight as spring, and what is found of either in our common air may be attributed to the other mixtures in it†."

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le mouvement des corps lumineux ne peut passer jusques à nos yeux, qu'il n'y passe quelque chose de matériel qui sorte de ces corps; car je ne fais en ces deux pages qu'expliquer la comparaison d'un aveugle, laquelle j'ai principalement apportée pour faire voir en quelle sorte *le mouvement peut passer sans le mobile*; et je ne crois pas que vous pensiez lorsque cet aveugle touche son chien de son bâton qu'il faille que ce chien passe tous le long de son bâton jusque à sa main, afin qu'il en sent les mouvements. Mais afin que je vous reponds *in formâ*, quand vous dites que le mouvement n'est jamais sans le mobile, *distinguo*; car il ne peut véritablement être sans quelque corps, mais il peut bien être transmis d'un corps en un autre, et ainsi passer des corps lumineux vers nos yeux par l'entremise d'un tiers, à savoir, comme je dis en la page 4, par l'entremise de l'air et des autres corps transparents, ou comme j'explique plus distinctement en la page 6, par l'entremise d'une matière fort subtile qui remplit les pores de ces corps et s'étend depuis les astres jusques à nous" (p. 240).

* Phil. Trans., No. 88, p. 5088. An. 1672. "The most free and natural application of this hypothesis I take to be this: That the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the æther" &c.

† Phil. Trans. No. 171, p. 1002.

[To be continued.]

XXIII. *On a Proposition relating to the Theory of Equations. By* JAMES COCKLE, *M.A., of Trinity College, Cambridge; of the Middle Temple, Special Pleader*[*].

1. LET x be the root of the general equation of the nth degree, and

$$y = \Lambda' x^{\lambda'} + \Lambda'' x^{\lambda''} + \Lambda''' x^{\lambda'''} + \Lambda^{\text{iv}} x^{\lambda^{\text{iv}}}; \quad . \quad . \quad . \qquad \text{(a.)}$$

also let ${}_m\mathrm{Y}$ be composed of symmetric functions of, and be homogeneous and of the mth degree with respect to y; then, if $n > 2$, ${}_2\mathrm{Y}$ may be reduced to the form

$$(a'_1 \Lambda' + a''_1 \Lambda'' + b')^2 + (a''_2 \Lambda'' + b'')^2, \quad . \quad . \quad . \qquad \text{(b.)}$$

where b' and b'' are not both zero.

2. For, let

$$\Lambda''' x_n^{\lambda'''} + \Lambda^{\text{iv}} x_n^{\lambda^{\text{iv}}} = l' x_n^{\lambda'} + l'' x_n^{\lambda''}, \quad . \quad . \quad . \qquad \text{(c.)}$$

then if $$y_r = (\Lambda' + l') x_r^{\lambda'} + (\Lambda'' + l'') x_r^{\lambda''} + \mathrm{l}_r, \quad . \quad . \qquad \text{(d.)}$$

$$\mathrm{l}_n = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(e.)}$$

Now ${}_2\mathrm{Y}$ is to be reduced, by means of (d.), to the form (b.), independently of Λ, or, what is the same thing, of $\Lambda + l$; but[†]

$${}_2\mathrm{Y} = \text{(b.)} + [\mathrm{l}_1 \dots \mathrm{l}_{n-1}]^2, \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(f.)}$$

$[\quad]^m$ denoting a homogeneous function of the enclosed quantities of the mth degree. And, if $n - 1 > 1$,

$$[\mathrm{l}_1 \dots \mathrm{l}_{n-1}]^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(g.)}$$

may be satisfied without making the l's zero.

3. Following a notation similar to that used in my last paper[‡], let (p, q) represent the coefficient of $\Lambda^{(p)} \Lambda^{(q)}$ in the development of

$$t p_2 - s p_1^2 = {}_2\mathrm{Y} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(h.)}$$

p_2 and p_1 being respectively the coefficients of the third and second terms of the transformed equation in y; then, if (h.) be reducible to the form (b.), we have

$$\dots + \dots + b' \pm \sqrt{-1} \, . \, b'' = 0; \quad . \quad . \quad . \qquad \text{(i.)}$$

and both the values of the above expression can only vanish when $b' = 0 = b''$. Substitute for b' and b'', equate each expression to zero, and eliminate $\dfrac{\Lambda'''}{\Lambda^{\text{iv}}}$ between the two; then we have

$$(1, 3)(2, 4) - (1, 4)(2, 3) = 0, \quad . \quad . \quad . \quad . \quad . \qquad \text{(j.)}$$

where, for instance,

$$(1, 3) = t \Sigma (x_1^{\lambda'} x_2^{\lambda'''}) - 2 s \Sigma (x_1^{\lambda'}) \, . \, \Sigma (x_1^{\lambda'''}); \quad . \quad . \qquad \text{(k.)}$$

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[*] Communicated by the Author.

[†] For the process, see par. 3 of the place which I have before cited, at the first line of p. 126 of vol. xxvii. of the Phil. Mag. S. 3.

[‡] Phil. Mag. S. 3. vol. xxvii. p. 292.

so that, on developing, we shall have on writing $\lambda' . \lambda''$ for $\Sigma(x^{\lambda'}) . \Sigma(x^{\lambda''})$, &c.,

$$\begin{aligned}0 = (t-2s) \times \{&\lambda' . \lambda^{\text{iv}} . (\lambda'' + \lambda''') + \lambda'' . \lambda''' . (\lambda' + \lambda^{\text{iv}}) \\ &- \lambda' . \lambda''' . (\lambda'' + \lambda^{\text{iv}}) - \lambda'' . \lambda^{\text{iv}} . (\lambda' + \lambda''')\} \\ + t\{(\lambda' + \lambda''') &. (\lambda'' + \lambda^{\text{iv}}) - (\lambda' + \lambda^{\text{iv}}) . (\lambda'' + \lambda''')\}. \quad . \quad (\text{l.})\end{aligned}$$

Let $t = 2n$, and $s = n - 1$, then, if $n < 3$, the last equation is identically true, but not in any other case. The method of the two first paragraphs, consequently, detects every case of failure; the last-mentioned instance of which is connected with the fact that, implicitly at least, every expression of the form (a.) contains in its right-hand side a term free from x which, with the above values of t and s, vanishes from ${}_2\text{Y}$. These values are those which occur in exterminating the 2nd, 3rd, and rth terms of an equation.

4. If, in the case of $n=2$, $t=4$, and $s=1$, we reject in (g.) the solution $\text{l}_1 = 0$, we are conducted to

$$(x_2^{\lambda'} - x_1^{\lambda'})^2 (x_2^{\lambda''} - x_1^{\lambda''}) = 0, \quad . \quad . \quad . \quad . \quad (\text{m.})$$

having multiplied by the coefficient of Λ'^2 before commencing our operations. This agrees with what we have inferred from (l.).

5. It seems to follow from this, that biquadratics can be reduced to a binomial, and equations of the fifth degree to a trinomial form, by an expression for y consisting of four terms, determinable by one linear*, one quadratic, and one cubic equation.

6. At p. 384 of the 26th vol. of this work, I have only alluded to the equation (3.), which, for cubics, conducts to the reducing equation

$$\xi^2 + \xi \Sigma \left(\frac{\varphi'_1}{\varphi_1}\right) + \frac{\varphi'_1 \varphi'_2}{\varphi_1 \varphi_2} = 0; \quad . \quad . \quad . \quad . \quad (3.)'$$

and to a similar one for biquadratics; but if we discuss the equation

$$\varphi\{(\Lambda x^{\lambda} + \text{M} x^{\mu})^{-1}\} = 0, . \quad . \quad . \quad . \quad . \quad (3.)''$$

it will be found that, though in appearance more complicated, it is in reality simpler than the former, inasmuch as the case of $\lambda = 0$ is not excluded; and if $\lambda = 0$ and $\mu = 1$, we have the form actually taken by the reducing equation in my solution of a perfect cubic at p. 248 of vol. ii. of the Cambridge Mathematical Journal.

Devereux Court, Temple Bar,
December 29, 1845.

JAMES COCKLE, Jun.

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* The 'base' equations are linear, as will be seen on referring to my definition at note † of p. 126 of this (27th) vol. If the roots of the trinomial equation of the fifth degree are given by the expression $b\,\psi(a)$, then ψ is contained in the solutions of the functional equation $\psi^2(a) - a = 0$.

XXIV. *On* Fresnel's *Theory of Double Refraction.* *By* R. MOON, *M.A., Fellow of Queen's College Cambridge, and of the Cambridge Philosophical Society.*

[Continued from vol. xxvii. p. 559.]

AFTER proving, as he imagines, the existence of the three axes of elasticity, Fresnel enters into the most elaborate calculations as to the motion of the originally disturbed particle, and then proceeds to discuss the laws according to which the disturbance is transmitted from it to the rest of the medium. His labours in this respect are perfectly futile. The motion of the original particle, which is of the most simple character, is altogether different from what he supposes; and as to the laws according to which the disturbance is communicated from it to the rest of the medium, *no disturbance whatever can be propagated.*

As to the first point, Sir John Herschel proceeds:—"Suppose now any molecule set in vibration," in a plane passing through the centre of the surface of elasticity, "then at any period of its motion it will not be urged directly to its point of rest; but obliquely so that it will not describe a straight line, but will circulate in a curve more or less complicated; its motion however will always be resolvable into two vibratory rectilinear ones at right angles to each other, one parallel to the greatest, and the other to the least diameter of the section," which diameters it is shown, and this incontestably, are at right angles to each other. "Each of these vibratory motions will, by the laws of motion, be performed independently of the other; and therefore the motion propagated through the crystal will affect every molecule of it in the same way as if two separate and independent vibrations (at right angles, as above) were propagated through it with different velocities."

It is perfectly true that "the motion of the particle will always be resolvable into two vibratory rectilinear ones at right angles to each other, one parallel to the greatest, and the other to the least diameter of the section." But it is not true, as Fresnel quietly assumes, that the motion will be the same as if two separate disturbances were communicated, one in the direction of the greatest, and the other of the least diameter of the section. The distinction between the two cases is very palpable. We may resolve the actual force on the particle into two, one parallel to the greatest and the other to the least diameter of the section; and so the motions of the particle parallel to those lines may be determined; but these motions respectively are not the same as if we calculate the effect of a disturbance communicated in the direction of the greatest, and then of another communicated in the direction of the least diameter. In the latter case, if u and v be the co-ordinates of the particle in the plane of the section, respectively parallel and perpendicular to the greatest diameter, the equation of motion parallel to that line is

$$\frac{d^2 u}{d t^2} = \mathrm{A} u. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

In the former, if $\alpha \beta \gamma$ be the inclinations of the greatest diameter to the axes of elasticity, we have

$$\frac{d^2 u}{d t^2} = a^2 x \cos \alpha + b^2 y \cos \beta + c^2 z \cos \gamma; \quad . \quad . \quad (2.)$$

but if r be the radius vector of the particle,

$$u = r \cos \theta = x \cos \alpha + y \cos \beta + z \cos \gamma,$$

from which it is obvious that equations (1.) and (2.) can never be identical. This single circumstance would alone be sufficient to condemn the whole theory; I mention it however chiefly to show the gross fallacies which have been unhesitatingly received into it.

As to the second point which I have asserted, that *no disturbance whatever will be propagated* from the originally disturbed particle, a circumstance which if true must scatter the whole theory to the winds, I must say I approach the discussion of it with considerable pain, when I reflect that a result so immediately and incontrovertibly flowing from Fresnel's assumptions should so long have been overlooked or disregarded; and this when the theory has for years been subjected to the scrutiny of the ablest philosophers of this and of other countries.

Assuming Fresnel's proof of the axes of elasticity to be genuine, we get the following equations for determining the motion of the disturbed particle:—

$$\frac{d^2 x}{d t^2} = - a^2 x,$$

$$\frac{d^2 y}{d t^2} = - b^2 y,$$

$$\frac{d^2 z}{d t^2} = - c^2 z,$$

from which we obtain

$$x = \mathrm{A}\cos(at + \mathrm{B}),$$
$$y = \mathrm{A}_{,}\cos(bt + \mathrm{B}_{,}),$$
$$z = \mathrm{A}_{,,}\cos(ct + \mathrm{B}_{,,}),$$

where $\mathrm{A}\,\mathrm{A}_{,}\,\mathrm{A}_{,,}$. $\mathrm{B}\,\mathrm{B}_{,}\,\mathrm{B}_{,,}$ are constants to be determined from the initial circumstances of the motion of the particle. From these equations, coupled with the fact which Fresnel assumes in his demonstration of the axes of elasticity, viz. that the change of position of the surrounding particles from the state of rest does not affect the forces upon the disturbed particle, we gather, that (1) without some special interposition of providence directed to each individual particle, *it would never move at all, whatever might be the state, whether of rest or motion, of the other particles* around it; and (2) that *once in motion, it would vibrate for ever without the least reference to or influence upon the other particles.* In my former paper, I said that Fresnel was driven to make an assumption as to the velocity of propagation, which rested only on the analogy of a case most widely differing from that under consideration. I now show that it is futile to talk of the velocity of propagation, when on his own showing *no wave whatever can be propagated.*

I purpose, in a future paper, to consider Fresnel's expressions for the intensity of the reflected and refracted rays when polarized light is incident on a surface.

Liverpool, December 3, 1845.

XXV. *Reply to some Remarks contained in* Prof. Young's *recent paper "On the Evaluation of the Sums of Neutral Series." By* R. Moon, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society*[*].

IN a paper published in this Journal some months ago, upon the symbols sin ∞ and cos ∞, I entered upon the discussion of the value of the series $1-1+1-1+$&c. continued to infinity, which I then showed to be 1 or 0 indifferently, in opposition to the commonly received opinion, which would make it equal to $\frac{1}{2}$. Prof. Young appears to be partly of my opinion in this respect, but seems to think I have made a mistake in supposing this to hold in all cases; for he appears to be of the opinion, that when the above series is considered as the limit of the converging series $1-x+x^2+$&c., where x

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[*] Communicated by the Author.

is less than 1, it is still equal to $\frac{1}{2}$. I am not sorry to have the opportunity afforded me of expressing more fully my views upon this point.

It is somewhat difficult to conceive how, by considering the series $1-1+1-$ &c. as the limit of another series, or by considering it in any other point of view, we can make its value different from what it is. *If it be* the limit of the series $1-x+x^2-$&c., where x is less than 1, and if moreover the limit of this last is $\frac{1}{2}$, it follows incontrovertibly that $1-1+1-$&c. must in all cases $=\frac{1}{2}$. The mistake here arises from calling $1-1+1-$&c. the limit of $1-x+x^2-$&c., where x is less than 1. It is no such thing. It is indeed the value which that series assumes when the limiting value is given to the variable; but it does not thence follow, nor is it the fact, that the one series is the limit of the other. We might expect the case to be otherwise, but it is not. Prof. Young himself admits, that without exception

$$1-x+x^2+\text{\&c.}\,(-x)^n=\frac{1}{1+x}+\frac{(-x)^{n+1}}{1+x}; \quad . \quad . \quad (a.)$$

and this holding always will hold in the limit when $x=1$, which gives us, when n is infinite,

$$1-1+1-\text{\&c.}=\frac{1}{2}\pm\frac{1}{2}=1 \text{ or } 0 \text{ indifferently.}$$

From the same original equation we likewise deduce this other, that when x is not greater than 1,

$$1-x+x^2-\text{\&c.}=\frac{1}{1+x},$$

except in the limit: whence it follows that the series $1-x+x^2-$&c., where x is not greater than 1, approaches $\frac{1}{2}$ as its limit. Now this is not more incontrovertible than that $1-1+1-$&c. is equal to 1 or 0, from which it is evident that the series $1-x+x^2-$&c., where x is less than 1, does *not* approximate to the series $1-1+1-$&c. as its limit; for the limit of a quantity or ratio is that quantity or ratio to which it continually approximates, and from which, although it never actually reaches it, its difference can be made less than any assignable quantity. It is perfectly true then that the limit of the series $1-x+x^2-$&c., where x is less than 1, or of the series $1-(1-x)+(1-x)^2-(1-x)^3+$&c., where x is greater than 0, is $\frac{1}{2}$, but let no one thence attempt to draw the inference that $1-1+1-\&c.=\frac{1}{2}$; for $1-1+1-\&c.$ is not the limit of either of the series, it is simply the form they respectively assume when the variable has its limiting value, which is a very different thing, as we have seen.

The broad fact which, although as clear as the sun at noon day, so many seem to hesitate to admit, is, that when x is very small, *if it be an actual magnitude,*

$$1-(1-x)-(1-x)^2+\ \&c.$$

differs very little from $\frac{1}{2}$, but that when x vanishes, it assumes two values, 1 and 0. There is in this case no middle term between entity and non-entity. The idea is simple and the fact certain.

Prof. Young holds it to be an axiom, that "the value which suffices for all cases except the extreme case, will suffice for that too," or uses words to that effect. This is a most unwarrantable and false assumption. Take the case of the same series,

$$1-x+x^2-x^3+\&c.,$$

where x is greater than 1. The value in this case, as is easily seen from equation (a.), is $\pm\infty$ indifferently; and this holding always, except in the extreme case, when $x=1$, it would follow, on Prof. Young's principle, that it holds in that too, and therefore that $1-1+1-\&c.=\pm\infty$; and he has before supposed it to be equal to $\frac{1}{2}$, which is absurd.

We may hence see the absurdity of any attempt to prove that $1-1+1-\&c.$, considered as the limit of $1-x+x^2-\&c.$, where x is less than 1, is equal to $\frac{1}{2}$; for by this nothing else can be meant than to prove that the one series *is* the limit of the other, which is contrary to the fact.

Prof. Young's attempt in this respect depends on the assumption that $\left(1-\frac{1}{\infty}\right)^{-\infty}=e$, the base of the Napierian system, which is untrue, and which at any rate I challenge him to prove. Does he consider that $(1-0)^{-\frac{1}{0}}=e$?

Liverpool, November 10, 1845.

Postscript.

I have just read Prof. Young's second paper, and without entering into all the windings of his argument, I shall proceed to animadvert upon such parts of it as refer to my own.

After some preliminary matter the Professor proceeds, "Let us now examine the series

$$\frac{1}{2} + \mathrm{A}\cos\theta + \mathrm{A}^2\cos 2\theta + \&c. + \mathrm{A}^n\cos n\theta,$$

so intimately connected with Fourier's integral, and which has already been the subject of consideration in Mr. Moon's paper before adverted to. This series, as there shown, or much more simply, by common division, arises from the development of the fraction

$$\frac{1-\mathrm{A}^2}{2(1-2\mathrm{A}\cos\theta+\mathrm{A}^2)}; \quad . \quad . \quad . \quad . \quad . \quad [1.]$$

so that, taking account of the *remainder* of the division, the general equivalent of the series is this fraction *minus*

$$\mathrm{A}^{n+1}\frac{\cos(n+1)\theta - \mathrm{A}\cos n\theta}{1-2\mathrm{A}\cos\theta+\mathrm{A}^2}. \quad . \quad . \quad . \quad [2.]$$

Now confining our attention to the continuous values of A, it is obvious, upon the principles laid down in the former part of this paper, that in the extreme case of $\mathrm{A} = 1$ and $n = \infty$, the fraction [2.] vanishes; and [1.] alone correctly represents the sum of the series in the limiting case."

What is meant by "confining our attention to the continuous values of A?" Can we draw any conclusion from the equation

$$\frac{1}{2} + \mathrm{A}\cos\theta + \mathrm{A}^2\cos 2\theta + \&c. + \mathrm{A}^n\cos n\theta,$$

$$= \frac{1-\mathrm{A}^2}{2(1-2\mathrm{A}\cos\theta+\mathrm{A}^2)} - \mathrm{A}^{n+1}\frac{\cos(n+1)\theta - \mathrm{A}\cos n\theta}{1-2\mathrm{A}\cos\theta+\mathrm{A}^2}$$

other than the following, viz. that so long as A is positive, and differs from 1 by an actual magnitude, in the limit when $n = \infty$,

$$\frac{1}{2} + \mathrm{A}\cos\theta + \mathrm{A}^2\cos 2\theta + \&c. \textit{ in inf.}$$

$$= \frac{1-\mathrm{A}^2}{2(1-2\mathrm{A}\cos\theta+\mathrm{A}^2)};$$

and that when A ceases to differ from 1 by an actual magnitude, the same series

$$= \left\{\frac{1-\mathrm{A}^2}{2(1-2\mathrm{A}\cos\theta+\mathrm{A}^2)}\right\}_{\mathrm{A}=1} - \left\{\frac{\cos(n+1)\theta - \cos n\theta}{2(1-\cos\theta)}\right\}_{n=\infty}?$$

Prof. Young appears to be haunted by the ghost of an argument, and it is somewhat difficult for others whose rest is not scared by the same phantom, to tell what he is driving at; but it appears to me that his difficulties, whatever they may be, arise from an inaccurate mode of expression which has crept into use, and of which he has not perceived the impropriety. Thus later on, we find him saying, "The real error, so frequently committed in analysis, consists in confounding

$$\frac{1}{2} + \cos\theta + \cos 2\theta + \&c. \textit{ in inf.}$$

with the limit of

$$\frac{1}{2} + A\cos\theta + A^2\cos 2\theta + \&c. \textit{ in inf.}$$

and calling [1.], when $A = 1$, the sum of the former." Now I would observe that the expression, the limit of the series

$$\frac{1}{2} + A\cos\theta + A^2\cos^2\theta,$$

is a relative term, and in this case means the value to which the series tends, when the difference between A and 1 gradually diminishes; and from which value (or rather quantity) it can be made to differ by a quantity less than any that can be assigned. But there is no propriety in the expression, "the limit of the series when $A = 1$." The phrase should be, *the particular value of* the series when $A = 1$. It is perfectly true that so long as the difference between A and 1 is an *actual magnitude* (which phrase I use advisedly, as the abuse of the expressions *finite quantities* and *indefinitely small quantities* has led many people to believe that there is after all no essential difference between entity and non-entity), the series by diminishing that difference can be made to differ from

$$\left\{\frac{1 - A^2}{2(1 - 2A\cos\theta + A^2)}\right\}_{A=1}, \textit{ i. e.} \text{ from } \frac{1}{2\sin^2\frac{\theta}{2}}$$

by a quantity less than any that can be assigned; but it does not thence follow, nor is it the fact, that when $A = 1$ the series becomes $= \dfrac{1}{2\sin^2\frac{\theta}{2}}$; for from the same evidence as that by which we are led to the conclusion that when $A < 1$ limit,

$$\frac{1}{2} + A\cos\theta + A^2\cos 2\theta + \&c. \textit{ in inf.} = \frac{1}{2\sin^2\frac{\theta}{2}},$$

we likewise deduce the fact that

$$\frac{1}{2} + \cos\theta + \cos 2\theta + \&c. \textit{ in inf.}$$

$$= \frac{1}{2\sin^2\frac{\theta}{2}} - \left\{\frac{\cos(n+1)\theta - \cos n\theta}{2(1-\cos\theta)}\right\} n = \infty.$$

Prof. Young indeed "considers it an axiom, that what holds for all but the extreme case will hold for that too," but I must beg to submit that this is a matter of fact and not of opinion; and the fallacy of the principle in the present case I have sufficiently shown in the former part of this paper. With all due deference therefore to Prof. Young, I shall reassert, that Mr. De Morgan *is* in error in affirming (1.) to be the limit of the proposed series "*when* A = 1." Omit the words "when A = 1," and I admit the proposition. Insert those words, and the fact expressed is untrue, if it be not wholly unmeaning.

Prof. Young says again, "It is easily proved that

$$\int_0^\infty e^{-ax}\cos x\,dx = \frac{a}{1+a^2}, \qquad \int_0^\infty e^{-ax}\sin x\,dx = \frac{1}{1+a^2}, \quad (\alpha.)$$

from which it certainly follows, though the inference is denied by Mr. Moon, that in the limit, when $a = 0$, the true values of these integrals are 0 and 1." I do not deny the inference, that the limits of the integrals when a is diminished indefinitely, so long as it continues an actual magnitude, are 0 and 1; but I do deny that the limits of the integrals are to be found by putting $a = 1$ in the left-hand members of the two equations (α.), that is, I deny that

$$\int_0^\infty \cos x\,dx \qquad \text{and} \int_0^\infty \sin x\,dx$$

are the limits of

$$\int_0^\infty e^{-ax}\cos x\,dx, \qquad \int_0^\infty e^{-ax}\sin x\,dx,$$

which is all I care to establish.

December 10, 1845.

Postscript 2.

I have read Prof. Young's third paper. Had my reply to his first paper been inserted in proper course, it is probable that he would have saved himself the trouble of writing, and the public of reading his last two papers. The staple of what I have to advance in opposition to this last is contained in my two previous notices. A few words, however, are still called for.

The Professor begins, "The general expression for the sum of the infinite series

$$1-x+x^2-x^3+x^4-\&c.$$

is $$S=\frac{1}{1+x}-\frac{x^{\infty''}}{1+x}."$$

What may be meant by the recondite symbol ∞'', I do not profess to understand. But it appears to me that

$$S=\frac{1}{1+x}-\frac{(-x)^{\infty}}{1+x};$$

and I am induced to think that Prof. Young himself will come to be of the same opinion when he again undertakes to examine the subject.

It is unnecessary for me to reply to the argument of the present paper, which appears to rest upon one of Prof. Young's previous fallacies which I have elsewhere exposed. I shall merely advert to the conclusion at which he ultimately arrives, "that it is indisputably true that the extreme of the *convergent* cases of the above series S, usually written in the form

$$1-1+1-1+\&c.$$

is $\frac{1}{2}$, and that the extreme of the *divergent* cases, usually written in the same form, is really infinite, as stated in his former paper."

I am afraid that Prof. Young will be apt to mystify both himself and his readers by talking about "the extreme of the divergent cases" and "the extreme of the convergent cases." If these "extremes" are usually written under the above form, I can only say that such usage is "extremely" improper. But let Prof. Young define the terms he uses. What is meant by "the extreme of the *convergent* series" for example? Is it the value of the series

$$1-(1-x)+(1-x)^2+(1-x)^3-\&c. \quad . \quad . \quad . \quad . \qquad (\alpha.)$$

when $x=0$? If so, I beg to assure him that the extreme value of the convergent series is *not* $\frac{1}{2}$, but 1 or 0 indifferently. The fact is, it is absurd to talk of "extreme values" in these cases. If x be made ever so small, there will always be some smaller value which might be assigned to it; so that it is impossible to assign an extreme value to x so long as it is an existing magnitude, and the moment it ceases to be such the series ceases to be equal (or rather to approximate) to $\frac{1}{2}$. Twist and turn it as he may, Prof. Young will never be able to prove the series

$$1 - x + x^2 - x^3 + \&c. = \frac{1}{2}.$$

It may be made to differ from that quantity by a quantity less than any that can be assigned, but it is *never* actually to it—least of all is it so when $x=0$, in which case it assumes a totally different value.

It was not for nothing that Newton devised his method of limits, and to the present case it applies with peculiar clearness and beauty. But no longer to fight with shadows, I shall take up a definite position, and shall leave it to Professor Young to drive me from it if he can. I assert, then, that the series

$$1-(1-x)+(1-x)^2-(1-x)^3+\&c.,$$

so long as x lies between 0 and 1, and differs from each of them by an *actual* magnitude (I do not say a sensible magnitude, for the present is not a question of degree), approaches to $\frac{1}{2}$ as its limit when x is made to diminish; that when $x=0$, the absolute value of the series is 1 or 0 indifferently; that when x is less than 0, it becomes $\pm\infty$ indifferently; and I defy any man now living, or as the lawyers say, who may hereafter come *in esse*, to prove anything else, be it more or less concerning it.

Prof. Young considers that the conclusion, that "the extreme of the *divergent* cases is really infinite," could "never have been anticipated from the theory hitherto prevalent." Protesting as I do against the use of the term "extreme of the divergent cases," I may say that long before Prof. Young either said or wrote a word upon the subject, I had shown that *all the divergent cases* have the value $\pm\infty$ indifferently.

Again, Prof. Young says, "if he has been anticipated in any of these views, which are doubtless calculated to produce a reform in the existing theory, he hopes to be informed of the circumstance through the medium of this Journal." I beg to assure him therefore, through the pages of this Journal, that *all his* views which are not erroneous (though what proportion that may be I confess myself unable to state, as I do not understand very clearly what they are) *have been anticipated* in my paper dated March 17, 1845, and published in the number of this Journal for June in the past year.

I now await Prof. Young's answer, trusting that I may not be under the necessity of replying to any more of his papers till he has had an opportunity of reading some of mine.

January 3, 1846.

XXVI. *Remarks on a Paper by* Mr. Moon *on* Fresnel's *Theory of Double Refraction**. *By* JESUITICUS.

THE hypothesis on which Fresnel's theory of double refraction is based, is the following:—

"That the displacement of a molecule of the vibrating medium in a crystallized body is resisted by different elastic forces according to the different directions in which the displacement takes place."

This is not a mere speculative hypothesis, but is based on experiment. It is found that glass, possessing only the power of single or ordinary refraction, may be made by the application of heat, or by mechanical pressure, to possess that of double refraction.

It is further supposed that the medium is symmetrical with respect to three rectangular axes in space, but, in general, not symmetrical with respect to any other axis through the same origin. These axes are called the axes of elasticity. It is then proved, that if any particle of the æther be suddenly displaced, *the other particles remaining quiescent*, the force of restitution developed by such disturbance will not in general be in the direction of the displacement, but only when such displacement is in the direction of the aforesaid axes of elasticity. The elegant demonstration of Smith, quoted by Mr. Moon, is by Mr. Moon's own showing fully adequate to establish the theorem as I have enunciated it, which is doubtless the sense in which Fresnel (the illustrious Fresnel, "whose name is enrolled amongst those which pass not away,") doubtlessly conceived it.

Any one who understands the subject must at once acknowledge that any theory of light must be, to a considerable extent, imaginative; and that theory which can explain the greatest number of facts ought to claim the attention of the philosopher more than any other. It is to this that the undulatory theory owes its great celebrity, and of all parts of the undulatory theory, that of double refraction is the most extraordinary. It ought to be regarded as a stupendous monument of human ingenuity. It must not be forgotten how admirably the properties of uniaxal crystals follow from the general investigation of the biaxal class; but above all, how from this same investigation, conical and cylindrical refraction were discovered by Sir William Hamilton. Such an unexpected refinement as this, which probably would never have been recognised by the mere experimentalist, undirected by the skill

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* Phil. Mag. S. 3. No. 183. vol. xxvii.

of so great an analyst, is surely no slight recommendation of the theory.

Mr. Moon subsequently gives a quotation from Airy's Tracts, concerning which he is by no means sparing in arrogant and supercilious criticism. But is it likely that Airy would make such a fool of himself as Mr. Moon earnestly endeavours to represent? It must be remembered, that at the time Airy's Tracts were published, very little of the undulatory theory was studied or known in Cambridge.

It was the part of this philosopher, therefore, to put everything as much as possible in the clearest and most simple point of view. That there are, and will perhaps long continue to be, difficulties in the undulatory theory, none of its supporters will deny. None of those difficulties are *shirked* or glossed over in the Tract of Airy; he plainly acknowledges each as it arrives. He no doubt himself considered the part quoted by Mr. Moon more as an illustration than anything else. Those who wish to see the matter treated with all the analytical generality of which it is capable, are referred to a tract on the Reflexion and Refraction of Light at the Surface of two contiguous Media, by the late famous George Green, in the Cambridge Philosophical Transactions.

I have one word more with Mr. Moon. He says, that on substituting for u,

$$u - \frac{du}{dx}h + \frac{d^2u}{dx^2}\frac{h^2}{1\,.\,2}, \text{ \&c.},$$

and for u',

$$u + \frac{du}{dx}h + \frac{d^2u}{dx^2}\frac{h^2}{1\,.\,2}, \text{ \&c.},$$

that h is considered small with respect to u.

Does Mr. Moon know anything of analysis? He was eighth wrangler in 1838, and therefore he ought to know something. His knowledge, however, has served him miserably on this occasion. The substitutions, stopping at h^2, merely require that h should be small in comparison with the *length of a wave*, not with respect to u.

JESUITICUS.

XXVII. *Observations on the subject of the Preceding Communications. By the* EDITORS.

ON the subject of the foregoing letter, the Editors are induced to subjoin a few remarks, as besides the attacks on Fresnel, they have also received from Mr. Moon one more paper, containing strictures on the writings of another distinguished mathematician, besides the reply to Mr. Young which they now publish.

In the admission of mathematical articles, the Editors are obliged to consult both quantity and character, as follows:—

It is not in their power to admit any very great quantity of *pure* mathematics. The majority of the readers of the Magazine are more interested in other sciences, and the Magazine would soon cease to exist if it were more than sparingly supplied with articles on lofty mathematical subjects.

As to the character of their mathematical articles, the Editors are placed in a peculiar position. They do not themselves profess to be so conversant with the higher mathematics as to rely entirely on their own judgement. In the articles which they insert, they must be guided by opinions. If they occasionally insert an article in which the general opinions of mathematicians are controverted, it is because they feel that mathematicians themselves would occasionally like to see the manner in which dissent from generally received principles manifests itself; and because they know that such occasional insertion will not, in the eyes of those same mathematicians, make them, the Editors, appear to be assuming a side in controversies of the merits of which they are not sufficient judges.

But if the Editors were to lend their Magazine to an extensive system of attack upon any usual results and methods of mathematics, either pure or mixed, they feel that they could not escape the charge of presumption. Whatever might be thought of an occasional paper, they feel sure that a series of such papers would cause them to be considered, and justly considered, as expressing an opinion on matters in which their knowledge is but limited. They would make just the same answer to a proposition for as extensive a system of defence to be inserted in their Magazine. They would suggest to both assailants and defendants to carry their communications to quarters in which they will find more competent judges. The pages of the Philosophical Transactions, of the Memoirs of the Royal Irish Academy, of the Cambridge Philosophical Society, of the Cambridge Mathematical Journal, &c., are much fitter vehicles for extensive mathematical discussion than those of the Philosophical Magazine.

As to one point, however, the Editors feel that a responsibility rests upon themselves, namely, as to the tone and temper in which controversial communications are expressed. They are persuaded that the differing opinions of men of science upon difficult subjects may be fully conveyed without any deviation from the respect and courtesy with which public discussion ought to be conducted:—and they feel regret when anything which is objectionable in this respect obtains a place in their pages.

XXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

"EXPERIMENTAL Researches in Electricity." By Michael Faraday, Esq., D.C.L., F.R.S. &c. Twentieth Series. Section 26th. "On New Magnetic Actions; and on the Magnetic Condition of all Matter."

The following is the order in which the several divisions of the subject treated of in this section of the author's researches in electricity succeed one another:—1. Apparatus required. 2. Action of magnets on heavy glass. 3. Action of magnets on other substances acting magnetically on light. 4. Action of magnets on the metals generally. 5. Action of magnets on the magnetic metals and their compounds. 6. Action of magnets on air and gases. 7. General considerations.

In giving an account of the contents of this paper, any attempt to follow the track of the author in the precise order in which he relates the consecutive steps of his progress in this new path of discovery, would fail of accomplishing its object: for, by adhering to such a course, it would scarcely be possible to comprise within the requisite limits of an abstract the substance of a memoir extending, as the present one does, to so great a length, and of which so large a portion is occupied with minute and circumstantial details of experiments; or to succeed in conveying any clear and distinct idea of the extraordinary law of nature brought to light by the author, and of the important conclusions which he has deduced.

One of the simplest forms of experiment in which the operation of this newly-discovered law of magnetic action is manifested, is the following:—A bar of glass, composed of silicated borate of lead, two inches in length, and half an inch in width and in thickness, is suspended at its centre by a long thread, formed of several fibres of silk cocoon, so as to turn freely, by the slightest force, in a horizontal plane, and is secured from the agitation of currents of air by being enclosed in a glass jar. The two poles of a powerful electro-magnet are placed one on each side of the glass bar, so that the centre of the bar shall be in the line connecting the poles, which is the line of magnetic force. If, previous to the establishment of the magnetic action, the position of the bar be such that its axis is inclined at half a right angle to that line, then, on completing the circuit of the battery so as to bring the magnetic power into operation, the bar will turn so as to take a position at right angles to the same line; and, if disturbed, will return to that position. A bar of bismuth, substituted for the glass bar, exhibits the same phenomenon, but in a still more marked manner. It is well known that a bar of iron, placed in the same circumstances, takes a position coincident with the direction of the magnetic forces; and therefore at right angles with the position taken by the bar of bismuth subjected to the same influence. These two directions are termed by the author *axial* and *equatorial*; the former being that taken by the iron, the latter that taken by the bismuth.

Thus it appears that different bodies are acted upon by the magnetic forces in two different and opposite modes; and they may accordingly be arranged in two classes; the one, of which iron is the type, constituting those usually denominated *magnetics*; the other, of which bismuth may be taken as the type, obeying a contrary law, and therefore coming under the generic appellation of *diamagnetics*. The author has examined a vast variety of substances, both simple and compound, and in a solid, liquid, or gaseous form, with a view to ascertain their respective places and relative order with reference to this classification. The number of simple bodies which belong to the class of magnetics is extremely limited, consisting only of iron, which possesses the magnetic property in an eminent degree, nickel, cobalt, manganese, chromium, cerium, titanium, palladium, platinum and osmium. All other bodies, when either solid or liquid, are diamagnetic; that is, obey the same law, with regard to magnetic action, as bismuth, but with various degrees of intensity: arsenic is one of those that give the feeblest indications of possessing this property. The following exhibit it in increasing degrees, according to the order in which they are here enumerated; namely, ether, alcohol, gold, water, mercury, flint glass, tin, lead, zinc, antimony, phosphorus, bismuth. On the other hand, no gaseous body of any kind, or in any state of rarefaction or condensation, affords the slightest trace of being affected by magnetic forces. Gases may therefore be considered as occupying the neutral point in the magnetic scale, intermediate between magnetic and diamagnetic bodies.

The magnetic properties of compound bodies depend on those of their elements; and the bodies are rendered either magnetic or diamagnetic according to the predominance of one or other of these conditions among their constituent parts. Thus iron is found to retain its magnetic power when it has entered into combination with other bodies of the diamagnetic class; the two forces acting in opposition to one another, and the resulting effect being only that due to the difference in their power. Hence the oxides and the salts of iron are still in a certain degree magnetic, and the latter even when they are held in solution by water; but the water may be present in such a proportion as that neither shall prevail; and the solution, as far as respects its magnetic properties, will then be exactly neutralized. These saline solutions, prepared of various degrees of strength, also afford a convenient method of comparing the relative degrees of force, both magnetic and diamagnetic, of different bodies, whether solid or fluid, but more especially the latter, as they admit of the body under examination being suspended in another liquid, when its position of equilibrium will indicate which of the two substances has the strongest magnetic power.

In one respect, indeed, the diamagnetic action presents a remarkable contrast with the magnetic; and the difference is not merely one of degree, but of kind. The magnetism of iron and other magnetics characterized by polarity; that of diamagnetics is devoid of any trace of polarity; the particles of two bodies of the latter class, when jointly under the influence of the magnetic forces, manifest towards each other no action whatever, either of attraction or repulsion. It has long been known that the magnetism of iron is impaired by heat; and it has been generally believed that a certain degree of heat destroys it entirely. The author finds, however, that this opinion is not correct; for he shows that, by applying more powerful tests than those which had been formerly confided in, iron, nickel and cobalt, however high their temperature may be raised, still retain a certain amount of magnetic power, of the same character as that which they ordinarily possess. From the different temperatures at which the magnetic metals appear to lose their peculiar power, it had formerly been surmised by the author that all the metals would probably be found to possess the same character of magnetism, if their temperature could be lowered sufficiently; but the results of the present investigation have convinced him that this is not the case, for bismuth, tin, &c. are in a condition very different from that of heated iron, nickel or cobalt.

The magnetic phenomena presented by copper and a few other metals are of a peculiar character, differing exceedingly from those exhibited by either iron or bismuth, in consequence of their being complicated with other agencies, arising from the gradual acquisition and loss of magnetic power by the iron core of the electro-magnet, the great conducting power of copper for electric currents, and its susceptibility of being acted upon by induced currents of magneto-electricity, as described by the author in the first and second series of these researches. The resulting phenomena are to all appearance exceedingly singular and anomalous, and would seem to be explicable only on the principles referred to by the author.

Pursuing his inductive inquiries with a view to discover the primary law of magnetic action from which the general phenomena result, the author noticed the modifications produced by different forms given to the bodies subjected to experiment. In order that these bodies may set either axially or equatorially, it is necessary that their section, with reference to the plane of revolution, be of an elongated shape: when in the form of a cube or sphere they have no disposition to turn in any direction: but the whole mass, if magnetic, is attracted towards either magnetic pole; if diamagnetic, is repelled from them. Substances divided into minute fragments, or reduced to a fine powder, obey the same law as the aggregate masses, moving in lines, which may be termed *diamagnetic curves*, in contradistinction to the ordinary magnetic curves, which they everywhere intersect at right angles. These movements may be beautifully seen by sprinkling bismuth in very fine powder on paper, and tapping on the paper while subjected to the action of a magnet.

The whole of these facts, when carefully considered, are resolvable, by induction, into the general and simple law, that while every particle of a magnetic body is attracted, every particle of a diamagnetic body is repelled, by either pole of a magnet. These forces continue to be exerted as long as the magnetic power is sustained, and immediately cease on the cessation of that power. Thus do these two modes of action stand in the same general antithetical relation to one another as the positive and negative conditions of electricity, the northern and southern polarities of ordinary magnetism, or the lines of electric and of magnetic force in magneto-electricity. Of these phenomena, the diamagnetic are the most important, from their extending largely, and in a new direction, that character of duality which the magnetic force was already known, in a certain degree, to possess. All matter, indeed, appears to be subject to the magnetic force as universally as it is to the gravitating, the electric, the cohesive and the chemical forces. Small as the magnetic force appears to be in the limited field of our experiments, yet when estimated by its dynamic effects on masses of matter, it is found to be vastly more energetic than even the mighty power of gravitation, which binds together the whole universe: and there can be no doubt that it acts a most important part in nature, and conduces to some great purpose of utility to the system of the earth and of its inhabitants.

Towards the conclusion of the paper, the author enters on theoretical considerations suggested to him by the facts thus brought to light. An explanation of all the motions and other dynamic phenomena consequent on the action of magnets on diamagnetic bodies might, he thinks, be offered on the supposition that magnetic induction causes in them a state the reverse of that which it produces in magnetic matter: that is, if a particle of each kind of matter were placed in the magnetic field, both would become magnetic, and each would have its axis parallel to the resultant of magnetic force passing through it; but the particle of magnetic matter would have its north and south poles opposite to, or facing the contrary poles of the inducing magnet; whereas, with the diamagnetic particles, the reverse would obtain; and hence there would result, in the one substance, approximation; in the other, recession. On Ampère's theory, this view would be equivalent to the supposition that, as currents are induced in iron and magnetics, parallel to those existing in the inducing magnet or battery wire, so, in bismuth and other diamagnetics, the currents induced are in the contrary direction. As far as experiment yet bears upon such a notion, the inductive effects on masses of magnetic and diamagnetic metals are the same.

XXIX. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF A SUBSTANCE OCCURRING WITH DISTHENE.

BY M. A. DELESSE.

In most mineralogical collections the disthene of Pontivy occurs, crystallized in large prisms of a sky-blue colour; they are often nearly 4 inches long and about $\frac{4}{10}$ths of an inch wide; the spaces occurring between the crystals are filled with a white lamellar, pearly substance, which is sometimes so intermixed with the cleavable faces of the prisms of disthene, that it is difficult to determine the limits of the two minerals.

This substance differs from any hitherto described, and possesses the following characters: it has the form of small crystalline laminæ, usually radiating from a centre; it is sometimes separated from disthene by a thin stratum of yellowish oxide of iron, evidently resulting either from the recent decomposition of disthene, or of the rock in which it occurs. In fragments, the substance is yellowish-white, and translucent; is cut by the knife, and formed by the agglomeration of a multitude of small laminæ, indicating a radiated structure; these laminæ are perfectly transparent, but have no crystalline form.

The cohesion of this substance is slight, but still it is difficult to reduce it to a fine powder. When pulverized it has the appearance of small scales of a shining silvery-white colour, with a pearly lustre; it is soft to the touch, but not unctuous like talc; it is harder than talc, for it scratches it; but it is not so hard as fluor spar; its density is 2·792; after drying at 212° and heated in a tube, it yields water; when dried over sulphuric acid *in vacuo*, it loses only a few thousandths of its weight, and retains its water, which is consequently in a state of combination; heated on platina it swells and becomes milk-white; when more strongly heated, it agglutinates and then fuses, but with difficulty, into a white enamel; it is phosphorescent and emits a brilliant light; with nitrate of cobalt it becomes of a pure blue colour, when strongly heated; with borax it dissolves readily and perfectly, with a slight colour proceeding from iron; with the salt of phosphorus it yields a colourless crystalline bead; the solution is quite complete, no silica skeleton remaining; with carbonate of soda effervescence ensues; alumina is left unacted upon, even when excess of the carbonate is employed.

Neither hydrochloric acid nor aqua regia acts upon this substance, but when finely levigated and boiled with concentrated sulphuric acid, it is completely decomposed; the silica remains in the granular state and retains the form of the scales. After calcination, it is not acted upon by the acid.

The qualitative analysis of this substance shows that it contains silica, alumina, a little iron and manganese, the last two not appearing to be in a state of combination, potash and water; soda was not found to be present, which it is proper to state, for usually the two alkalies occur together. As the mineral possesses some of the characters of mica, fluorine was sought for but not found.

In determining the quantity of water, it was found requisite to heat it pretty strongly to separate the whole of it; when only a part of it was expelled, it was found that on placing it in water for some days and then drying it by exposure to the air, it regained exactly as much water as it had lost; when however it is strongly heated and loses all the water, it does not regain it by immersion.

Analysed by means of nitrate of barytes, this substance yielded—

Silica......	45·48
Alumina....	38·20
Potash	11·20
Water	5·24
	100·12

with traces of iron and manganese.

We may therefore consider it as composed of—

Twelve eqs. of	silica	16 × 12 =	192	44·75
Nine ...	alumina....	18 × 9 =	162	37·76
One ...	potash	=	48	11·20
Three ...	water......	9 × 3 =	27	6·29
			429	100·

Ann. de Ch. et de Phys., Octobre 1845.

HYDRATED SILICATE OF MAGNESIA. BY M. A. DELESSE.

This substance is arranged at the Ecole des Mines with the mineral species which M. Breithaupt has named *Kerolite*, and it appeared to M. Delesse to require examination. It comes from Germany, its locality however is unknown; but it has evidently occurred in serpentine.

Its colour is yellowish-white, it is opaline and slightly transparent; its fracture resembles wax, and it is greasy to the touch; it is occasionally spotted with milk-white spots, which appear to be a different substance. Its specific gravity is 2·335; when slightly heated in a glass tube it becomes black and loses water; when strongly heated it becomes of a dead-white colour, and loses its transparency. The black colour appears to be owing to bitumen, for it disappears when the substance is strongly heated in a closed tube; this property belongs also to kerolite, mestaxite, saponite, &c.

When put into water after calcination it emits a great number of bubbles of gas, becomes hard, and is with difficulty acted upon by acids; whereas before heating it is scratched by calcspar and easily acted upon; it is completely infusible; with the salt of phosphorus it gives a skeleton of silica.

A qualitative analysis showed that this substance contains only water, silica, magnesia, a little alumina and traces of iron, which appear to be in the state of peroxide, and disseminated in small veins throughout the mass.

By analysis this mineral gave—

Silica	53·5
Magnesia	28·6
Alumina and a trace of oxide of iron	00·9
Water........................	16·4
	99·4

Annales des Mines, 1844.

ANALYSIS OF THE ELIE PYROPE OR GARNET.

BY PROF. CONNELL.

This mineral, which is known to amateur collectors under the name of Elie ruby, is found on the sea shore at Elie, in the county of Fife, proceeding from the debris of trap-rocks. It has been long known to Scottish mineralogists, and has been regarded as one of the varieties of precious garnet, and is occasionally called pyrope. It is not crystallized, but occurs in angular grains, which evidently have not come from any distance. Its other leading external characters, including transparency and colour, agree with those of precious garnet and pyrope, the colour approaching the deeper tint of the latter; its specific gravity is 3·661.

Twenty grains of this mineral in very fine powder, were fused with four times their weight of carbonate of potash; the mass was treated with muriatic acid, and no smell of chlorine observed. Silica was separated by the usual method; the precipitate obtained by ammonia was dissolved in muriatic acid, the solution boiled with excess of potash which took up the alumina, and the matter left by this alkali was dissolved in muriatic acid; to this solution tartaric acid and ammonia in excess were added, and a current of sulphuretted hydrogen, passed into it, threw down sulphuret of iron with a little sulphuret of manganese. The filtered liquid was evaporated to dryness, and the residue incinerated was pure white; it was carefully examined for yttria, which Dr. Apjohn, some few years ago, announced that he discovered in pyrope. This white matter was dissolved in muriatic acid, and muriate of ammonia and excess of ammonia were added; a gelatinous precipitate fell, which by ignition acquired a greenish-yellow tint, and magnesia was left in solution. The ignited precipitate was again dissolved in muriatic acid, and yielded a gelatinous precipitate by treatment with ammonia and its muriate; this was dissolved to a great extent by potash, leaving a substance which was principally oxide of iron, but gave a permanent fine, though pale emerald-green colour to salt of phosphorus, and therefore contained a trace of oxide of chromium. It was determined by a separate experiment that the iron contained in the mineral was entirely in the state of peroxide.

One hundred parts of this substance were found to consist of

Silica	42·80
Alumina	28·65
Peroxide of iron	9·31
Protoxide of manganese	0·25
Lime	4·78
Magnesia	10·67
Oxide of chromium, trace	
	96·46

The deficiency Prof. Connell conceives to be probably owing to some magnesia which might have escaped precipitation by the carbonate of potash.

Prof. Connell remarks, that even if the oxide of iron in this mineral were held to be protoxide (instead of peroxide, as he found it), there would be quite as much difficulty in bringing the result under the garnet formula as there is in bringing the leading analyses of Bohemian pyrope under it. This circumstance, as well as the general conformity between the above result and the analyses of pyrope, comprising those of Klaproth, Wachtmeister and Von Kobell, particularly as respects the considerable quantity of magnesia and the comparatively small quantity of oxide of iron, notwithstanding the discrepancy as to the state of oxidation of the latter, tend to show a close connexion between the Elie mineral and pyrope. The occurrence of oxide of chromium in both minerals, and their specific gravity, which is 3·661 for the Elie mineral, 3·78 for pyrope, while that of precious garnet exceeds 4·, lead to a similar view of this connection.—*Jameson's Journal*, Oct. 1845.

ANALYSIS OF METEORIC IRON FROM BURLINGTON, OSTEGO COUNTY, NEW YORK. BY MR. C. H. ROCKWELL.

In the year 1819 two or three masses of native iron, as it appeared to be, were procured from the farmer who first turned it over with his plough, in a field near the north line of the town of Burlington, Ostego County, New York. These consisted of remnants of an entire mass originally supposed to weigh between one and two hundred pounds, and found several years before. It had been in the forge of a country blacksmith, and the whole heated in order to enable him to cut off portions for the manufacture of such articles as the farmer most needed.

The mass was divided by broad laminæ, crossing each other at an angle of 60° and 120°, cutting up the surface into triangular and rhombohedral figures. It broke with a hackly fracture, and only with the greatest difficulty on the thinnest edges.

Two deep and broad sutures marked its two most regular opposite faces, made by the wedge or chisel by the smith, who severed it from the adjoining portion. It bore the marks of having been intensely heated in the forge, and numerous microscopic crystals, of a black colour and brilliant lustre, covered some parts of its surface; they resembled phosphate of iron, but were too small to be detached.

The specific gravity was 7·501; it dissolved quickly and completely in nitric acid, with the application of a gentle heat. The solution, treated with nitrate of silver, gave no cloudiness, showing the absence of chlorine; it yielded by the usual process for separating iron from nickel,

Iron............	92·291
Nickel..........	8·146
	100·437

No trace of any other substances could be detected.—*Silliman's Journal*, vol. xlvi.

PREPARATION OF CHLORO-ACETIC ACID.

M. Malaguti recommends the following process for the preparation of chloro-acetic acid readily and in large quantity:—Let chlorine act upon sulphuric æther, by which sesquichloride of carbon is obtained, and then the water which is suffered to remain in the bottles with the rough product is merely a solution of chloro-acetic and hydrochloric acids; or perchloric æther is prepared, and by distilling it and causing the product of the distillation to mix with water, a solution of chloro-acetic and hydrochloric acids is obtained. In both cases it is sufficient to employ a vacuum with sulphuric acid and potash to obtain chloro-acetic acid in a state of great purity. This process possesses the advantage of also obtaining as a secondary product, a considerable quantity of sesquichloride of carbon.—*Ann. de Ch. et de Phys.*, Jan. 1846.

COMPOSITION OF PHOSPHATE OF AMMONIA AND MAGNESIA.
BY M. FRESENIUS.

The erroneous statements contained in chemical treatises with respect to the ammoniaco-magnesian phosphate have given inaccurate results as to the proportions of magnesia indicated by this salt, and they have also prevented its being employed in estimating the quantity of phosphoric acid. M. Fresenius has discovered that the double salt in question is absolutely insoluble in free ammonia, so that it may be employed in quantitative analysis.

He ascertained the solubility of this salt in water, solution of ammonia, solution of hydrochlorate of ammonia, and in a mixed solution of ammonia and hydrochlorate. He found it dissolved by 15293 parts of water at the usual temperature, and requires a mean quantity of 44330 parts of ammoniacal water for solution, so that one part of magnesia, in the form of this salt, requires 120760 parts of water, and one part of phosphoric acid 70000. According to these statements this salt may be for a long time washed with ammoniacal water before dissolving a very minute fraction of a grain either of magnesia or phosphoric acid.

As to solution of hydrochlorate of ammonia, one part of the double salt is dissolved by 7548 parts of it, and by 15627 parts of a mixed solution of ammonia and hydrochlorate; sal-ammoniac therefore slightly increases the solubility of the salt; still however this solubility is so slight as to be inappreciable in estimating its quantity.

M. Fresenius has also performed some comparative experiments to ascertain if the double phosphate would answer for analyses.

He analysed a determinate quantity of very pure sulphate of magnesia; by calculation the magnesia was estimated at 34·01 per cent.; experiment gave 34·0 and 34·02 per cent.; the phosphoric acid of phosphate of magnesia was calculated at 19·90 per cent., while experiment gave 19·87.—*Journ. de Pharm. et de Ch.*, Dec. 1845.

COMPOSITION OF COMMON PHOSPHATE OF SODA.

M. Fresenius states that the undermentioned chemists found this salt to consist of

	Berzelius.	Malaguti.	Graham.	Clark.
Phosphoric acid	20·33	18·80	37·1	37·48
Soda	17·67	16·71		
Water	62·00	64·25	62·9	62·52
	100·00	99·76	100·00	100·00

M. Fresenius found 19·87 of phosphoric acid and 62·67 of water;

his results therefore agree with those of Berzelius, Graham and Clark. —*Journ. de Pharm. et de Ch.*, Dec. 1845.

ON SEVERAL NEW SERIES OF DOUBLE OXALATES. BY M. REES HEECE.

These salts were discovered in investigating the action of alkaline and earthy bases on the oxalates of the sesquioxides.

It is well known that the salts of lime produce but a slight precipitation of oxalate of lime in a moderately concentrated solution of the oxalates of the sesquioxides of iron and chromium, &c., and none in a very dilute solution of oxalate of chromium and potash, a salt discovered by Prof. Gregory, and in which there are 3 equiv. of oxalic acid combined with the alkaline base. A concentrated solution of the same salts gives rise to an abundant precipitate, which has been considered as oxalate of lime, but in which I found a considerable proportion of chromium. These were the facts which led me to pursue this inquiry.

The combination by means of which I have prepared the double salts which are the objects of this memoir, is an oxalate of chromium and ammonia, having the same formula as the salt of Mr. Gregory; but it is preferable to this on account of its great solubility.

A concentrated solution of this salt, mixed with its volume of chloride of strontium, barium or calcium, yields voluminous precipitates, which, separated from the mother-leys and recrystallized, have the following composition:—

Oxalate of chrome and barytes (A)	$3C^2O^3 + Cr^2O^3 + 3(C^2O^3BaO) + 12HO$.
Oxalate of chrome and barytes (B)	$3C^2O^3 + Cr^2O^3 + 2(C^2O^3BaO) + 18HO$.
Oxalate of chrome and strontian ...	$3C^2O^3 + Cr^2O^3 + 3(C^2O^3SrO) + 18HO$.
Oxalate of chrome and lime	$2(3C^2O^3 + Cr^2O^3) + 3(C^2O^3CaO) + 36HO$.

If oxide of iron is substituted for the oxide of chromium, we obtain the corresponding salts with an iron base, and which are represented by the following formulæ:—

Oxalate of iron and barytes	$3C^2O^3 + Fe^2O^3 + 3(C^2O^3BaO) + 7HO$.
Oxalate of iron and barytes	$3C^2O^3 + Fe^2O^3 + 3(C^2O^3BaO) + 12HO$.
Oxalate of iron and strontian	$3C^2O^3 + Fe^2O^3 + 3(C^2O^3SrO) + 18HO$.

The oxalate of iron and of lime does not crystallize.

If alumina is substituted for the oxide of chromium, we obtain similar salts, which are represented by—

Oxalate of alumina and barytes.........	$3C^2O^3 + Al^2O^3 + 3(C^2O^3BaO) + 10HO$.
Oxalate of alumina and barytes.........	$3C^2O^3 + Al^2O^3 + 3(C^2O^3BaO) + 30HO$.
Oxalate of alumina and strontian	$3C^2O^3 + Al^2O^3 + 2(C^2O^3SrO) + 18HO$.

The oxalate of alumina and lime cannot be isolated in a state of purity, on account of its insolubility.

These salts crystallize in small silky needles; those of the oxide of chromium are of a dark violet colour, those of iron of a greenish-yellow, and those of alumina of a brilliant white. They are soluble in about 30 times their weight of boiling water (excepting the salts of lime and oxide of chromium, of alumina and strontia, which are decomposed by water); they are scarcely soluble in cold. All the alkalies decompose them by precipitating the sesquioxide and earthy oxalate. The salts of chromium however behave differently towards ammonia, which does not throw down the oxide of chromium, even when the barytes has been separated by sulphuric acid.

The iron salts are decomposed by the solar rays, with an abundant disengagement of carbonic acid, even when the crystals are dry.

I shall conclude this summary of my investigations by drawing attention to the importance of these salts in analysis; the more so as it was with this view I undertook them.

I think that I have succeeded in explaining the fact, long since known, of the solubility of the oxalate of lime in solutions of the sesquioxides. Iron, aluminum and chromium are always separated from their ores as sesquioxides; lime and strontia, in the state of oxalates; but we know that lime cannot be separated from the solution of the sesquioxide; this circumstance is owing to the formation of a double salt, of which the oxalate of lime forms a part. It is therefore necessary to precipitate the sesquioxide by ammonia, which leaves the lime free in the solution, and it is therefore difficult to prevent its being thrown down by the carbonic acid of the atmosphere, and thus affecting the weight of the sesquioxide.

It is especially in the case of alumina and oxide of chromium, that the error may be the greatest. This difficulty is easily avoided by the following process:—I will select as an instance a mineral containing iron and lime. It is dissolved in hydrochloric acid; then a suitable quantity of oxalic acid added, which, if the liquor is diluted, will not produce any precipitate; I now add some oxalate of ammonia in excess, which will precipitate the whole of the lime, which is separated by filtration, oxide of iron remaining in solution entirely free from lime; this is precipitated, in the ordinary manner, by ammonia.—*Comptes Rendus*, Nov. 17, 1845.

REACTION FOR THE DISCOVERY OF SULPHUROUS ACID.
BY M. HEINTZ.

The substance to be examined, dissolved in water or hydrochloric acid, is to be heated with a solution of protochloride of tin in dilute hydrochloric acid to ebullition. If the liquid contains much sulphurous acid, sulphuret of tin is precipitated; but if the quantity be small, no precipitation occurs; the liquid becomes yellow and exhales the odour of sulphuretted hydrogen. It is then requisite only to add a few drops of solution of sulphate of copper to obtain an immediate precipitate of the brown sulphuret of this metal.

This method of detecting sulphurous acid, it will be observed, is merely a modification of that proposed more than fifty years ago by Pelletier, and since recommended by M. Gerard.

It is preferable to the process of MM. Fordos and Gelis, which is based on the formation of sulphuretted hydrogen by the contact of metallic zinc and sulphurous gas, inasmuch as it does not require the use of an apparatus to disengage the gas.—*Journ. de Pharm. et de Ch.*, Janvier 1846.

ANALYSIS OF THE MOLARES OF A FOSSIL RHINOCEROS.

M. E. I. Meyer, by employing the process of Wöhler, found that besides phosphate and a little carbonate of lime, these teeth contained 2·10 per cent. of fluor.

EXPERIMENTS ON THE YOLK OF EGGS. BY M. GOBLEY.

The author remarks, that a German chemist of the name of John was the first who carefully examined the yolk of the egg, the chemists who preceded him regarding it merely as consisting of water, albumen, oil, gelatine and colouring matter. John concluded from his experiments, published in 1811, that the yolk of egg was composed of water, a sweet yellow oil, traces of free acid, which he presumed to be the phosphoric, a small quantity of reddish-brown matter, soluble in æther and in alcohol, gelatin, much of a modified albuminous substance, and sulphur.

In 1825, Prout found the yolk to be composed of 54 water, 17 albumen, and 29 oil; and that it contained besides sulphur, phosphorus, the chlorides of sodium and potassium, the carbonates of potash and soda, lime and magnesia, partly in the state of carbonates.

Chevreul was of opinion that the orange colouring matter of the yolk was due to the combination of two colouring principles, one yellow, approximating that of the bile, and the other red, resembling that of the blood.

Lastly, in 1829, M. Lecanu discovered in the oil of the egg, a fat, crystallizable, unsaponifiable matter, which he considered to be cholestrine.

Such, says M. Gobley, was the state of our knowledge respecting the yolk of the egg when he began his experiments; he states that the substances which he obtained from the yolk are,—

1. Water.
2. Albuminous matter or vitelline.
3. Oleine.
4. Margarine.
5. Cholesterine.
6. Margaric acid.
7. Oleic acid.
8. A peculiar acid containing phosphorus, which is in fact phosphoglyceric acid.
9. Lactic acid and extract of meat.
10. Various salts, as chloride of sodium, chloride of potassium, hydrochlorate of ammonia, sulphate of potash, phosphate of lime, and phosphate of magnesia.
11. Yellow and red colouring matter.
12. Azotized organic matter, which does not appear to be albumen.

The oleic, margaric, and phosphoglyceric acids appear, in the author's opinion, to be combined with ammonia.

In the opinion of Berzelius, the yolk of egg contains some volatile fatty acids, on account of the facility with which the yolk becomes rancid; M. Gobley has not been able to discover them, nor any gelatine, and sulphur was met with only in the albuminous matter.—*Journ de Pharm. et de Ch.*, Janvier 1846.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1845.

Chiswick.—December 1. Rain: cloudy: clear. 2. Clear and fine: heavy rain. 3. Overcast: showery: clear. 4. Clear: fine: heavy rain. 5—7. Clear: frosty. 8. Sharp frost: overcast: drizzly. 9. Fine. 10. Clear. 11. Cloudy: clear and windy at night. 12. Overcast: fine: clear. 13. Frosty and foggy: cloudy. 14. Foggy: hazy: drizzly. 15. Rain: fine. 16. Fine. 17. Overcast: slight drizzle. 18. Foggy: rain. 19. Densely and uniformly overcast: rain. 20. Clear: dark clouds, with rainbow. 21. Boisterous and densely clouded: clear and frosty at night. 22. Densely overcast: sleet: showery: very boisterous at night. 23. Cloudy and boisterous at night. 24. Cloudless, with bright sun. 25. Hazy: thick fog at night. 26. Cloudy. 27. Clear: fine: overcast. 28. Boisterous, with rain: clear. 29. Frosty: overcast. 30. Overcast: clear. 31. Very fine: heavy rain and boisterous at night.—Mean temperature of the month 0°·4 above the average.

Boston.—Dec. 1. Cloudy: rain early A.M. 2. Fine: rain P.M. 3. Fine. 4. Fine: rain P.M. 5—7. Fine. 8. Fine: rain P.M. 9. Cloudy: stormy P.M. 10. Fine. 11. Stormy: stormy night. 12. Cloudy: rain early A.M. 13, 14. Fine. 15. Stormy. 16. Cloudy. 17. Cloudy: rain P.M. 18. Rain: rain early A.M.: rain all day. 19. Cloudy. 20. Cloudy: rain early A.M. 21. Windy: rain early A.M. 22. Windy and showery. 23. Stormy. 24. Fine. 25. Rain: rain early A.M. 26. Cloudy: rain P.M. 27. Fine. 28. Rain: rain early A.M. 29. Fine: rain P.M. 30. Windy: stormy P.M. 31. Fine.

Sandwick Manse, Orkney.—Dec. 1. Showers: sleet-showers. 2. Showers: sleet: clear: aurora borealis very brilliant. 3. Fine: clear: aurora borealis very brilliant. 4. Showers: hail: cloudy. 5, 6. Rain: cloudy. 7. Clear frost: clear. 8. Bright: cloudy. 9. Showers. 10. Cloudy: rain. 11. Showers. 12. Cloudy. 13. Cloudy: showers. 14. Rain. 15. Sleet-showers: rain. 16. Sleet-showers: showers. 17. Frost: cloudy: clear frost. 18. Frost: cloudy: snow-showers. 19. Showers: clear frost. 20. Frost: cloudy: sleet-showers. 21. Frost: bright: cloudy: thaw. 22. Showers. 23. Showers: clear. 24. Cloudy: showers. 25. Showers: cloudy. 26. Showers. 27. Snow-showers: sleet-showers. 28. Snow: frost. 29. Rain. 30. Showers: clear frost. 31. Cloudy: rain.

Applegarth Manse, Dumfries-shire.—Dec. 1, 2. Showers. 3. Showers of snow. 4. Frost: rain P.M. 5. Very heavy rain. 6. Showers. 7. Fair and fine: slight frost. 8. Frost: rain P.M. 9. Fine A.M.: rain P.M. 10. Fair, but damp. 11. Fair and clear: frost. 12. Frost. 13. Frost, hard. 14. Very wet P.M.: frost A.M. 15, 16. Heavy showers. 17. Fine A.M.: shower P.M. 18. Fine A.M.: frost P.M. 19. Frost A.M.: rain P.M. 20. Frost A.M. 21. Frost: clear. 22. Heavy showers. 23. Slight frost. 24. Frost A.M.: shower P.M. 25. Fine. 26. Heavy rain all day. 27. Heavy showers. 28. Fair and fine. 29. Heavy rain: frost. 30. Heavy rain. 31. Frost A.M.: rain P.M.

Mean temperature of the month *	39°·5
Mean temperature of Dec. 1844	33 ·8
Mean temperature of Dec. for 23 years	38 ·3
Mean rain in Dec. for 18 years	3 inches.

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* It would be worth while for the meteorological correspondents to note the particulars here stated in their reports.

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

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Dec.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	29·977	29·749	29·28	29·30	29·50	29·00	29·10	50	39	47	49½	36	40	42	w.	w.	sw.	wsw.	·02	·02	......	·25
2.	29·952	29·501	29·52	29·52	29·15	29·30	29·10	52	36	41	42	37½	39½	36	sw.	calm	sw.	sw.	·30	......	......	·25
3.	29·570	29·422	29·00	29·11	29·30	29·04	29·20	53	30	36	41	30	35	36	w.	calm	sw.	w.	·04	·06	......	·03
4.	29·769	29·541	29·38	29·48	29·16	29·34	29·14	45	35	35	41½	29½	37	33	w.	calm	sw.	se.	·39	......	......	·13
5.	29·514	29·470	29·00	29·01	29·06	28·83	28·95	51	30	43·5	45½	40	38½	36	w.	w.	w.	s.	......	·16	......	·32
☽ 6.	29·495	29·454	29·10	29·09	29·23	29·14	29·38	47	28	35	42½	37½	36	37	sw.	calm	w.	n.	·01	......	......	·25
7.	29·990	29·634	29·34	29·51	29·78	29·60	29·76	47	19	34	40	32½	38	39	nw.	calm	nw.	wnw.	......	......	0·55	·08
8.	30·156	30·014	29·80	29·78	29·70	29·73	29·50	50	41	31	44	29	38	42½	sw.	calm	sw.	w.	·06	......	......	......
9.	30·171	30·109	29·72	29·80	29·92	29·53	29·89	48	29	43	46	40	42	42½	w.	calm	wnw.	nnw.	......	·04	......	·23
10.	30·310	30·256	29·88	30·17	29·90	30·16	29·69	53	29	37	45½	36	40½	39	nw.	calm	w.	se.	......	......	......	......
11.	30·027	29·879	29·44	29·66	30·02	29·63	30·15	51	34	50	49½	43½	39	40½	nw.	w.	w.	n.	......	......	......	·58
12.	30·428	30·236	29·88	30·25	30·34	30·40	30·38	43	23	37	41½	35	38	40	n.	w.	n.	nw.	......	·02	......	·08
○ 13.	30·447	30·360	30·09	30·30	30·22	30·30	30·19	36	25	29	33½	24	39	42	sw.	calm	n.	sw.	......	......	......	·06
14.	30·311	29·908	30·00	30·02	29·51	29·70	29·24	44	35	28·5	47½	24	47	44	sw.	calm	wsw.	w.	·02	......	0·24	·47
15.	29·836	29·749	29·29	29·48	29·49	29·13	29·14	51	39	47	48	40½	40	37	w.	w.	w.	nw.	·02	......	......	·92
16.	29·773	29·753	29·25	29·40	29·52	29·30	29·36	51	42	46·5	45½	40	34	30	w.	w.	wnw.	n.	......	......	......	·23
17.	29·821	29·556	29·49	29·62	29·46	29·60	29·60	49	40	40	39	33½	35	32	sw.	calm	n.	s.	·05	......	......	·03
18.	29·417	29·254	29·00	29·45	29·45	29·63	29·35	49	35	43	38	33	29	35	sw.	calm	n.	s.	·33	·33	......	......
19.	29·358	28·937	28·92	28·74	28·53	28·65	28·53	49	36	37	43½	29	35	31½	sw.	calm	w.	se.	·17	·40	......	·57
20.	29·019	28·785	28·35	28·56	29·02	28·83	29·36	47	32	39	40	35	37	35	sw.	calm	nw.	n.	·03	·27	......	·08
☾ 21.	29·793	29·421	29·05	29·55	29·68	29·72	29·51	39	28	38	40	31½	33	35	nw.	nw.	n.	se.	·10	·15	0·72	·14
22.	29·618	28·983	29·29	29·00	28·80	28·84	28·60	46	34	33	44½	31½	40	36	sw.	nw.	wnw.	nw.	·20	......	......	·51
23.	29·719	29·081	28·77	29·22	29·78	29·48	29·90	44	31	38	43½	36	40½	39½	n.	nw.	wnw.	n.	·01	·24	......	·19
24.	30·211	30·042	29·78	30·00	29·88	29·81	29·52	43	24	34	42½	28½	43	47	ne.	calm	wnw.	ssw.	......	......	......	......
25.	30·264	30·176	29·72	29·91	30·00	29·82	29·89	48	34	39	47	40½	43	42	w.	calm	sw.	sw.	·01	·04	......	·25
26.	30·076	29·876	29·64	29·52	29·31	29·15	29·08	50	37	43	49	37	47	40	sw.	calm	sw.	nw.	......	......	......	·28
27.	30·100	29·788	29·60	29·63	29·07	29·34	29·02	52	41	38·5	39½	34½	36	34	w.	nw.	w.	ne.	·17	·14	......	·40
● 28.	29·805	29·471	29·62	29·32	29·63	29·30	29·60	52	22	46	51½	37	36½	33½	sw.	calm	e.	nnw.	·16	·21	......	·19
29.	30·013	29·700	29·60	29·62	29·22	29·23	28·97	52	40	33	50½	29½	43	40	s.	calm	sw.	s.	·06	·05	......	·20
30.	29·968	29·644	29·19	29·12	29·70	28·82	29·46	54	25	52·5	51	49	41	37	w.	w.	sw.	w.	·02	·19	1·08	·49
31.	30·106	29·465	29·70	29·73	29·11	29·52	28·96	52	40	38	45½	34	41	38	sw.	w.	sw.	s.	·42	......	......	·20
Mean.	29·903	29·651	29·40	29·512	29·500	29·415	29·403	48·32	32·51	39·0	44·1	34·6	38·75	37·80					2·61	2·32	2·59	7·41

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XXX. *On the Determination of the Temperature and Conducting Power of Solid Bodies.* By CHR. LANGBERG *of Christiania*[*].

NOTWITHSTANDING the important progress which has been made in the mathematical theory of the phænomena of heat by the analytical researches of Fourier, Poisson and others, it is not to be denied that the influence which these have exerted upon the extension of our physical knowledge of the phænomena is very limited, and that only a few of the results obtained from mathematical theory have been demonstrated and proved by experiment. The reason of this is in a great measure owing to the want of accurate modes of ascertaining changes of temperature in solid bodies, without militating against too many of the conditions required by the mathematical theory.

Thus we are taught by mathematical analysis that one of the most important elements in the theory of heat, namely the conducting power of solids, can be ascertained by placing in connexion with a constant source of heat the end of a long, thin, homogeneous, cylindrical or prismatical rod, composed of the substance to be examined, and observing the temperature of this rod at different distances from the heated end; the difference between the observed temperatures on the rod and that of the surrounding air decreases in geometrical progression when the points of observation are at equal distances from each other.

For establishing these laws experiments have been instituted by Biot[†], and more lately by Despretz[‡]; those of the latter

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[*] Being an abstract from Poggendorff's *Annalen*, 1845, No. 9; communicated by Dr. Ronalds.

[†] *Traité de Physique*, tom. iv. p. 670.

[‡] *Annales de Chimie et de Physique*, tom. xxxvi. p. 422. *Traité Elémentaire de Physique*, p. 210.

however, as far as they have been made public, appear to me to prove exactly the converse of what they are intended to do, as the temperatures observed decrease in a much quicker ratio than the geometrical progression would require, and the difference between the calculated and observed values is too constant to admit of the supposition that it is solely due to errors of observation. One source of the difference may probably arise from the Newtonian law of cooling having been made the basis upon which to found the mathematical deduction of the law in question. According to Newton's law, a heated body cools with a rapidity proportional to the degree of temperature to which it has been raised above that of the air surrounding it, and applies with correctness only to very slight differences of temperature. [In the experiments alluded to above, this difference amounts to 60° or 70° C.] Again, it is presumed that the power of conduction remains unchanged at different temperatures, which is certainly not probable; and the theory further requires that the heated rod should be infinitely thin, or at least so thin that its temperature in every part of a normal section should be exactly the same.

Despretz used in his experiments prismatical rods, the square section of which was 21 millimetres in breadth; holes were bored in these at 10 millimetres distance from each other, 6 millimetres in diameter, and 14 millimetres deep. When the rod had been brought into a horizontal position, these holes were filled with mercury, and in each the bulb of a thermometer was placed, the temperature of which, when it had become stationary, was considered as that of the section of the rod passing through the middle of the hole. As the breadth of the holes amounted to nearly one-third of the whole breadth of the rod, there is reason to fear that these large and frequent interruptions in the continuity of the rod might cause a considerable obstruction to the progress and distribution of the heat. The results show that the method employed in the experiments fulfilled very imperfectly the conditions required by the theory, and it remains therefore still uncertain whether the variations from the theoretical law which were observed are to be attributed to an inaccurate method of observation, or to an error in theory.

The importance of the law, forming as it does the basis of the mathematical theory of the phænomena of heat, as well as from its application for determining the conducting power of solid bodies, appeared to me sufficiently great, and induced me to seek a mode of observation not subject to the objections which I have raised above. The first requisite was therefore a mode of ascertaining correctly such slight differences between the temperature of the rod and the surrounding air as would come with accuracy within the scope of the Newtonian law of cooling, and this must be done with rods of indefinitely small diameter, having their continuity unimpaired by any holes bored into their substance. The thermo-electrical battery appeared to me to be an instrument admirably adapted for this purpose; and arranged in the manner which I shall presently describe, I hope that it will be found a much more accurate measure for observing uncombined heat in solid bodies than any other means that have yet been applied to that purpose.

The experiments were performed in the laboratory of Professor Magnus, who was kind enough to lend me the necessary apparatus, and to whose friendly guidance the successful result of the experiments is chiefly to be attributed.

I found by several preliminary experiments, that the same divergence in the needle of the multiplier was always attained when the end of a thermo-electrical battery, consisting of but few alternations, was connected in a similar manner with a body of constant temperature, and pressed against it with the same amount of force. It was always 2 to 2½ minutes before the needle of the multiplier became stationary; the connection might then be continued for an indefinite time without perceptibly affecting the position of the needle. In order to secure perfectly uniform contact, which is hardly possible with a battery composed of many alternations, I had one constructed of only two elements, bismuth and antimony, having therefore but one soldered point of contact at each end. The ends were filed off presenting facets, so that each presented a rectangular surface of 1·7 millimetre in length and 0·7 millimetre in breadth. The whole length of the bars was 36·3 millimetres; each bar was very thin, being 1·7 millimetre in width and 1·0 millimetre thick.

On a strong horizontal board, on which divisions had been marked, three uprights were erected, bearing each a forked arm in which were fixed two perpendicular glass rods drawn out to a point and placed opposite to each other, between which the metallic bars to be examined were clamped parallel to the horizontal divided board, and at about 24 centimetres above it; a fourth upright at the end of the board served to fix securely, with the help of a screw, the cool end of the rod during the experiment. To obtain for a length of time a uniform source of heat, hot water was used: the end of the rod to be heated, passed through a cork into the water by an opening made in the boiling vessel below the surface of the water.

By means of two double polished brass screens, through holes in the centres of which the rod passed, the battery and that part of the rod to be examined were effectually protected from the radiant heat of the boiling vessel. The support of the battery was screwed to a sledge which could be moved along the edge of the horizontal divided board in a direction parallel with the metallic rod, by which means the relative distances of the points to be examined on the rod and their temperatures could be easily ascertained. In order that the battery should press with equal force each time against the rod, a spiral spring was so placed in the support of the battery as to force it upwards against the under side of the rod; or it could be brought into contact with the rod in a direction at right angles to it.

The observations were made in the following manner:—When the rod had attained a constant temperature, which seldom occurred until about 2½ to 3 hours after the commencement of the experiment, the sledge bearing the battery was so placed that the upper end of the battery bore perpendicularly upon the under side of the rod. The spiral spring was then allowed to force the end against that part of the rod the temperature of which was to be examined: the needle of the multiplier diverged immediately. I waited generally about two minutes to allow the needle to come to rest, and having noted the divergence, removed the battery. After each observation I allowed four minutes to elapse before the battery was again placed in contact with the rod, partly that the needle of the multiplier might return to 0°, and partly that the equilibrium of temperature in the rod, which might possibly have been disturbed by its contact with the battery, might again be restored. This latter precaution was however needless, as observations made upon the same part of the rod immediately the one after the other, were found to give the same deviations in the needle as when a space of time was allowed to elapse between each observation.

Thus far we have given very nearly the author's own words; but as our space will not allow us to follow him through each experiment, we shall here briefly add some of the precautions which were taken to avoid error, and then give the results to which the experiments have led.

To ascertain whether the battery itself, after being for some time in contact with the rod, might by becoming warm no longer indicate with correctness the difference of temperature between the rod and the surrounding air, it was left in connexion with the heated rod for three quarters of an hour, but during the whole of that time the divergence of the needle of the multiplier scarcely changed. The circumstance that the needle of the multiplier seldom returned to precisely the point from which it had been deflected, sometimes becoming stationary a little to the right, sometimes to the left of its original position, the cause of which deviation could not be traced, would give rise to a slight error, perhaps 0·1 to 0·2 of a degree, upon the scale of the multiplier, which had been divided in the manner recommended by Melloni.

The divisions upon this scale not corresponding exactly with the temperatures they are intended to indicate, might also lead to a slight error; and although the observations were always made with the aid of a magnifier, still it is possible that from 0·1 to 0·2 of a degree should escape observation.

These three sources of error, which arose chiefly from the author's having used an imperfect instrument, added to another more important one, the effect, namely, of currents of air in the room cooling one part of the rod under examination more than another, the author calculates will not amount to more than 0·4° C. in the final result of any experiment. In fact it never actually amounted to so much.

The metals used in the experiments were copper, steel, tin and lead; they were all drawn out into cylindrical wires or thin rods, and their length was such that even in the middle of the rod no effect of the heating body was perceptible. The copper alone, being the best conductor, was slightly affected through its whole length.

The object of the experiments was not so much to determine the conducting power of the metals employed, as to submit the analytical law to the test of experiment; the metallic surface of the rods was therefore not protected, and remained unimpaired in the three first metals; the lead wire however soon became covered with a layer of oxide, which increased in thickness every time it was heated.

The results at which the author arrived were principally the following:—

The law of Biot,—that in a very thin, long metallic rod, one end of which is kept at a constant temperature above that of the surrounding air, after equilibrium of temperature has been established, the excess of temperature in any part of the rod above that of the surrounding air, decreases in geometrical progression as the part examined is removed by equal distances from the heated end—is not generally confirmed by the author's experiments, and is only true for most of the metals in the case of a very small excess of temperature. Among the metals examined copper was the only one for which the law held good, at least when the excess of temperature amounted to 30° C.

With tin, the law no longer applied when the excess was

4° C.; with steel when it amounted to 2° or 3°; and lastly, with lead, when 1° of difference existed, it was not accurate.

2. The reason for this want of accordance between the observation and the mathematical law is, that in establishing the latter the outward and inward power of conduction of bodies was considered as independent of the temperature. If these be considered as functions of the temperature, a proximate formula for the distribution of heat in the rod may be established, which would very nearly agree with my observations.

3. The conducting powers of bodies established by former philosophers with the aid of Biot's law are consequently incorrect, and can only be considered as partial approximations to the truth.

4. The constant coefficient for the conducting power with a difference of temperature equal 0, is therefore only to be determined in this manner; either by using Biot's law and observing the distribution of heat in the rod with very small differences of temperature, or more correctly, by ascertaining its value, as was done in the author's experiments by means of Poisson's formula.

5. That the method employed in the observations is accurate, and that the thermo-electrical battery will become in the hands of natural philosophers a more correct means of ascertaining the temperature of the surfaces of bodies than any other, and may be used in cases where the common thermometers cannot be employed.

XXXI. *On the Causes of the Semi-diurnal Fluctuations of the Barometer*. *By* THOMAS HOPKINS, *Esq.*[*]

THAT the non-condensable gases and the aqueous vapour of the atmosphere, when it is at rest, press on the mercury of the barometer independently of each other, and constitute the general atmospheric pressure, is evident from their known laws of diffusion and independent existence while diffused through each other.

But that the facts and reasonings, commonly adduced, resting on those circumstances, together with the daily alterations of thermometic temperature, account for the two risings and the two fallings of the barometer, as is contended by some parties, cannot be admitted.

In certain parts, such as Canada, of which an account has been recently given by Colonel Sabine, the influence of the causes named may be sufficient to account for a considerable

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[*] Read to the Literary and Philosophical Society of Manchester, Dec. 30, 1845, and communicated by the Author.

portion of the semi-diurnal movements of the barometer which occur in that country; but these causes are not sufficient to produce the diurnal fluctuations in other places, such as Bombay, Calcutta and La Guayra. And there can be little doubt that the real causes, whatever they may be, which give rise to the double undulations in these tropical parts, produce them in places where they are less extensive, although the operation of the causes in the latter places may be weaker and more difficult to trace.

I have shown in my "Atmospheric Changes" that there is no reason to believe that the daily warming of the atmospheric gases by the direct influence of the sun produces any appreciable alteration in their pressure on the mercury of the barometer, as the effect of that warming on the whole column in the locality is so small, as to prevent much disturbance of atmospheric pressure; yet great influence has been attributed to solar heating near the surface in producing the semi-diurnal fluctuations that take place.

Colonel Sabine, in his Report on the Meteorology of Toronto at the meeting of the British Association in 1844, gives an explanation of the daily oscillations. He says, "As the temperature of the day increases, the earth becomes warmed and imparts heat to the air in contact with it, and causes it to ascend. The column of air over the place of observation thus warmed rises, and a portion of it diffuses itself in the higher regions of the atmosphere, where the temperature at the surface is less. Hence the statical pressure of the column is diminished. On the other hand, as the temperature falls, the column contracts, and receives in its turn a portion of air which passes over in the higher regions from spaces where a higher temperature prevails; and thus the statical pressure is augmented."

In the Athenæum of July 5, 1845, the Colonel is represented as having said at the then recent meeting of the British Association, that in Dr. Buist's Meteorological Report from Bombay, "the explanations thereby afforded of the diurnal variations of the *gaseous* pressure at Bombay, which, although at first sight more complex than at the stations of Toronto, Prague or Greenwich, he conceives to be equally traceable to variations of temperatures." Colonel Sabine therefore, after having examined Dr. Buist's meteorological registers, retains the opinion that the semi-diurnal alterations of the *gaseous* pressure are produced by alterations of temperature, as that temperature is shown by the thermometer.

As I propose to examine this theory and to compare it with another, it will be convenient to designate the two by distinct names. I shall therefore call the Colonel's "*the temperature theory*," and the other, "*the condensation theory*." Both of these rest on alterations of temperature; but the former depends on the temperature found by thermometric measurement near the earth's surface, and the latter on the temperature which must be produced by condensation of vapour in a higher part of the atmosphere, of which we have no direct measure.

The semi-diurnal fluctuations of the barometer are the greatest within the tropics; and as details of those at Bombay have not yet been published, we will proceed to examine accounts furnished by Kaemtz in his valuable work on Meteorology. In page 248 of that work we have the following tables of the hourly heights of the barometer:—

TABLE I.

Mean height of the barometer expressed in millimetres for all hours, and in different places.

Places	Gt. Ocean.	Cumana.	La Guayra.	Calcutta.	Padua.	Halle.	Abo.	Petersburg.
Latitude ..	0° 0′	10° 23′ N.	10° 36′ N.	22° 35′ N.	45° 24′ N.	54° 29′ N.	60° 57′ N.	59° 66′ N.
Observers .	Horner.	Humboldt.	Boussingault.	Balfour.	Ciminello.	Kaemtz.	Hallstroem.	Kupffer.
Noon	752·35	756·57	759·41	759·61	757·02	753·29	759·31	759·47
1	751·87	755·99	758·91	759·22	756·85	753·11	759·29	
2	751·55	755·47	758·41	758·39	756·67	752·99	759·27	759·38
3	751·15	755·14	758·12	758·12	756·54	752·89	759·25	
4	751·02	754·96	758·05	757·91	756·47	752·84	759·25	759·32
5	751·31	755·14	758·10	757·93	756·46	752·86	759·27	
6	751·71	755·41	758·40	758·01	756·50	752·91	759·29	759·31
7	751·93	755·81	758·90	758·02	756·63	753·02	759·34	
8	752·35	756·21	759·19	758·54	756·79	753·14	759·39	759·32
9	752·74	756·59	759·69	759·24	756·92	753·24	759·44	
10	752·85	756·87	759·93	759·33	757·02	753·31	759·47	759·36
11	752·86	757·15	759·98	759·09	757·02	753·29	759·47	
Midnight	752·47	756·86	759·64	758.80	757·01	753·23	759·41	759·35
13	752·20	756·53	759·34	758·62	756·90	753·14	759·33	
14	751·77	756·21	759·05	758·57	756·84	753·05	759·24	759·32
15	751·63	755·89	758·81	758·49	756·78	752·99	759·14	
16	751·32	755·66	758·68	758·47	756·74	752·99	759·07	759·32
17	751·65	755·79	758·85	758·44	756·75	753·34	759·03	
18	751·95	756·18	759·32	758·68	756·79	753·12	759·04	759·39
19	752·84	758·58	759·94	759·16	756·89	753·24	759·08	
20	752·95	756·98	760·50	759·88	757·01	753·37	759·15	759·49
21	753·16	757·31	759·63	760·11	757·08	753·44	759·21	
22	753·15	757·32	760·50	759·19	757·14	753·46	759·29	759·51
23	752·80	757·01	759·99	759·09	757·07	753·40	759·32	

From an examination of this table, it will be seen that the fluctuations are the greatest within the tropics, and they diminish, though not invariably, with the increase of latitude.

The first column exhibits the fluctuations at the equator in the great ocean. The range extends beyond two millimetres, and the descent from 9 in the morning till 4 in the afternoon gives the whole extent of the range.

The two next columns show the alterations at Cumana and La Guayra, both above 10° north latitude, and the ranges are nearly equal to that at the equator.

The fourth column shows the changes at Calcutta to be nearly as great as in the preceding places, but both this and the La Guayra columns exhibit singular irregularities in the earlier parts of the mid-day descents.

In the Padua column, 45° north, the fluctuation is much reduced in the extent of its range, but retains the same general character.

In Halle, in latitude 54°, the alterations do not differ materially from those at Padua.

The changes are very small in Abo and Petersburg, and in the former place the second rise attains a greater height than the first.

To these it is desirable that we should add the following table (p. 170) of the height of the dry- and wet-bulb thermometers, and the difference between the two,—with the dew-point and the height of the barometer at Plymouth for three years, as furnished by Mr. S. Harris, and published in the Ninth Report of the British Association (p. 167).

In all these places the temperature shows only a single fluctuation, such as is seen in the table of the thermometer at Plymouth, namely one rise generally from about 5 A.M. to 1 or 2 P.M., and one fall from that time until 5 the following morning. Now, if the temperature of the atmosphere, as marked by the thermometer, caused the diurnal fluctuations in the way supposed, we ought to have in all these places one undulation in the twenty-four hours instead of two,—the rise of temperature causing a decline of the barometer during the hotter part of the day, and the fall of temperature producing a rise of the barometer in the colder part. Yet Colonel Sabine himself says that at Bombay, where there is only one rise and one fall of temperature, there are two risings and two fallings of the barometer! And these movements of the barometer take place not only when that instrument is taken as the measure of the whole pressure of the atmosphere, but also when the vapour pressure is deducted, and the mercury of the barometer is taken as the measure of the gaseous pressure alone. These facts are opposed to, and are irreconcileable with, the temperature theory.

TABLE II.

Table of the heights of the dry- and wet-bulb thermometers, and the difference between the two, together with the dew-point and height of the barometer at Plymouth for three years.

Hour.	Thermometer.	Wet-bulb thermometer.	Difference.	Dew-point.	Barometer.
1 A.M.	47·52	46·20	1·32	45·00	29·8017
2	47·33	46·03	1·30	44·75	29·7993
3	47·11	45·92	1·19	44·75	29·7944
4	47·00	45·66	1·34	44·25	29·7928
5	46·98	45·77	1·21	44·75	29·7928
6	47·41	46·01	1·40	44·50	29·7960
7	48·44	46·83	1·61	45·25	29·8002
8	49·68	47·51	2·17	45·00	29·8032
9	51·30	48·50	2·80	45·26	29·8048
10	52·84	49·45	3·39	46·25	29·8061
11	53·00	50·02	3·88	46·75	29·8045
12	54·51	50·40	4·14	46·75	29·8002
1 P.M.	55·83	50·55	4·28	46·75	29·7957
2	54·77	50·44	4·33	46·75	29·7922
3	54·25	50·24	4·01	46·75	29·7908
4	53·45	49·80	3·65	46·75	29·7895
5	52·27	49·06	3·21	46·25	29·7938
6	51·24	48·46	2·78	46·00	29·7970
7	50·28	47·90	2·38	45·75	29·8019
8	49·44	47·51	1·93	45·75	29·8061
9	48·83	47·17	1·66	45·60	29·8094
10	48·48	46·93	1·55	45·60	29·8099
11	48·10	46·66	1·44	45·00	29·8092
12	47·80	46·43	1·37	45·00	29·8065
Mean ...	50·32	47·89	2·43	45·60	29·7999

As however the aqueous vapour of the atmosphere presses on the mercury of the barometer separately and independently, it has been attempted to be shown that the variable pressure of the vapour arising from difference in the quantity in the atmosphere at different periods of the day, combined with change of the gaseous pressure resulting from alteration of surface temperature, and that the two causes acting together produced the double undulation of the barometer; to this view therefore we will direct our attention.

The temperature near the surface of the earth at Plymouth, as well as at the other places, rises from about 5 in the morning till about 2 in the afternoon; and when the wet bulb, as well as the dry thermometer, is used, as it was at Plymouth, it is seen that the temperature of the latter rises more than that of the former, or of the dew-point, and evaporation must consequently become progressively more active;

there must therefore be successively more water evaporated and thrown into the atmosphere to be added to its weight. And according to the temperature theory, this water, now converted into vapour, must, up to say 10 o'clock, press with sufficient force on the mercury to counteract the lightening influence of the rising temperature, as during that time the barometer rises.

From 10 until 1 o'clock, as the temperature rises still higher, as compared with the wet-bulb thermometer and the dew-point, evaporation must go on increasing, and the increase of vapour pressure ought to continue; but it appears from the table not to do so, as the mercury of the barometer falls instead of continuing to rise; we have therefore to try to ascertain what can be the cause of this fall, while additional vapour is passing into the atmosphere.

Those who advance the temperature theory, say that the fall of the barometer is caused by the increasing temperature of the atmosphere produced by the action of the sun on the surface of the earth, and the air near to it; and they must maintain that this increase is sufficient, not only to lighten the atmosphere enough to cause the fall of the barometer, but also in addition to counteract the influence of the increased vapour pressure. Now at Plymouth the temperature rises from 5 to 10 A.M. nearly 6°, and may be supposed to lighten the atmosphere to a certain extent; at the same time evaporation throws vapour into the atmosphere. We are, however, required to suppose that the vapour produces so much greater effect by pressing on the mercury, than the heating of the atmosphere does in reducing atmospheric pressure, that the whole pressure becomes greater and the mercury rises. But after 10 o'clock the temperature continues to rise, but in a smaller degree, say nearly 3°, and vapour must be more abundantly thrown into the air, as is shown by the extent to which the wet-bulb thermometer is kept down; yet the barometer, instead of continuing to rise, suddenly turns and falls, and continues falling from 10 to 1 o'clock, the time of the highest temperature! So that according to this theory, from 5 to 10 o'clock, the sun heats the air nearly 6° and produces some vapour; and the two influences acting together cause the barometer to *rise*, but from 10 to 1 the sun heats the air about 3°, and must throw much additional vapour into the atmosphere; and then these two influences still acting together cause the barometer to fall! This is attributing opposite effects to the same causes, and must be presumed to be erroneous.

But let us examine the valuable Plymouth tables a little more minutely. The first column gives the temperature as shown by the ordinary thermometer; the second, the temperature of the wet-bulb thermometer, as kept down by the cooling influence of evaporation; and the third gives the difference between the two first. Now as this difference arises from the extent of the evaporation, the numbers of the difference may be taken to express the force and amount of evaporation, and to indicate the additional vapour that is discharged into the atmosphere. This force or amount at 5 o'clock in the morning is 1°·21, from which time it increases to 3°·39 at 10 o'clock. So that during this time, five hours, the increase in the force of evaporation is 2°·18; and this in the temperature theory must be held to be sufficient to overcome the lightening effect of a rise of 5°·86 of temperature, and *also* to raise the mercury of the barometer to the full extent of the morning rise! After this time, from 10 to 1 o'clock, the temperature rises further from 52°·84 to 55°·83 or 2°·99; and during the same period the force of evaporation increases 1°·89, that is, from 3°·39 to 4°·28. Thus we are required to believe, that from 5 to 10 in the morning, 2°·18 of evaporation overcame the lightening influence of 5°·86 of temperature, and in addition raised the mercury of the barometer; and from 10 to 1 in the day, 1°·89 of evaporation not only failed to overcome the lightening effect of 2°·99 of temperature, but allowed this relatively small amount of temperature to produce the further result of a fall of the mercury of the barometer. Or put in a tabular form, say that from

5 to 10 o'clock, 5°·86 of temperature and 2°·18 of evaporation caused a rise.
10 to 1 o'clock, 2°·99 of temperature and 1°·89 of evaporation caused a fall!

That is, where temperature, the influence which lightens the atmosphere, is relatively great and should cause a fall, the mercury of the barometer rises; and where the influence of temperature is relatively small and should cause the vapour to produce a rise, the mercury falls! This must be erroneous.

In the same place, at Plymouth, from 1 o'clock until 4 P.M., as may be seen in the table, the temperature falls; and as far as that temperature acted the atmosphere would of course become heavier. At the same time evaporation shows vapour is passing into the atmosphere; it ought therefore to follow that the barometer should rise, and considerably too, through the operation at the same time of both the causes which are supposed to contribute to the production of a rise. But the barometer does not rise; on the contrary, it falls, and continues falling until 4 o'clock. These facts and reasonings prove that neither the daily variations of surface temperature, nor the different amounts of vapour pressure, nor both taken together, are adequate to the production of the fall of the barometer from 10 to 4 o'clock in the day.

And if we proceed with our inquiries into the next period of six hours, that is, from 4 to 10 P.M., we meet with facts that do not harmonize with the temperature theory. During the whole of this time, it is true the temperature falls and the barometer rises: but the vapour pressure must have diminished according to the temperature theory, as the dew-point, the measure of vapour pressure, falls; and the lowering of the dew-point after 4 o'clock showed that vapour was then condensing in the lower part of the atmosphere. So that here it becomes necessary to suppose that the atmosphere cools enough, not only to raise the barometer to the full extent of its daily range, but also to counteract the reduction which takes place at the same time in the vapour pressure. Again, from 10 at night, although the atmosphere continued to cool, the barometer did not continue to rise, but once more fell, which fall is attributed to a diminution of vapour pressure. Thus from 4 to 10 in the afternoon and evening, cooling the atmosphere is represented as more powerful than reduction of vapour pressure; and from 10 in the evening to 4 in the morning, reduction of vapour pressure is supposed to be more powerful than cooling the atmosphere. The two forces, we are required to believe, do not merely neutralize each other, but each in its turn exercises a paramount influence, and for the time determines an absolute rise or a fall of the barometer; and this we are called upon to admit without any satisfactory or even plausible evidence being adduced to prove it.

What has been here advanced applies with the greatest force to the semi-diurnal fluctuations in atmospheric pressure which take place within the tropics. Aqueous vapour exists in the atmosphere in larger proportions in that part of the world than it does in higher latitudes; and it is to the daily condensation of that vapour in the atmosphere, and its subsequent evaporation there, that we are really to attribute the great deviation of the movements of atmospheric pressure from the daily march of temperature. If no vapour existed in the atmosphere, the alteration of pressure would be very little, and it would be the reverse of temperature. As the atmosphere became warmer, the pressure would be less; as it became colder, the pressure would be more. And the hourly variation in the quantities of vapour actually found in the atmosphere which arises from alteration of surface temperature, only introduces another element of pressure into the inquiry, which is simple in its character,—the vapour increasing or diminishing with an increase or diminution of temperature.

If the two were equal while acting in opposite directions, they would balance each other. But the separate action of these two causes cannot produce such a double undulation of the mercury of the barometer as that which occurs daily in the tropical regions and at Plymouth.

The double undulation which takes place may be thus accounted for. When the sun acts with force on the surface of the earth in the morning, it heats that surface, and the air near it; increases evaporation of moisture from wet surfaces, and sends forth vapour, which presses on the mercury of the barometer and causes it to rise. The lower part of the atmosphere being heated also rises at the same time, probably in separate vertical streams, until it reaches a height where its expansion and consequent cooling is sufficient to condense a part of the vapour which it contains. A cloud is then formed, and the heat which has been evolved in the condensation of the vapour makes the cloud lighter than the adjoining air. The vapour in the upper part of the air being thus removed by conversion into water, no longer presses as vapour, or with the same force on that below; and the lower vapour consequently rises more freely to the height of the cloud. Both the air and vapour are also (speaking in popular language) drawn up by the ascending cloud, and fresh air flows in from adjoining low levels, forming what in some parts is called the sea breeze. Cloud more or less thick is now formed, more heat is liberated, and a larger mass of air heated, which being forced upwards expands and makes the whole atmospheric column lighter, and reduces the pressure on the surface below. Under ordinary circumstances this process proceeds while the sun acts with considerable power on the surface of the earth, which is generally from 10 A.M. to 4 P.M., when day-cloud ceases to form. In this way, from 10 in the morning till 4 in the afternoon, the barometer is caused to fall, through the condensation of vapour in the upper part of the atmosphere making the column of air warmer and lighter. But now as vapour no longer ascends, cloud ceases to form, but that cloud which had been formed remains suspended in the air, where it begins to cool from the influence of evaporation of the particles of water that form the cloud. When it cools sufficiently, it becomes heavier and sinks, and additional air flows towards and over it, increasing the weight of the whole column in the locality and causing the barometer to rise. By 10 the heavy air produced by cloud evaporation has partly descended and diffused itself on the surface of the earth, forming what is called the land breeze; and during the same time the cold of the surface condenses some of the vapour into dew, when the atmosphere becomes somewhat lighter up to about 4 or 5 in the morning.

As we proceed from the equator towards higher latitudes, we find less vapour in the atmosphere, and its influence on atmospheric pressure is less marked. At Padua the fall of the barometer from 10 to 4 in the day is not much more than one-fourth the extent that it is at the equator, and at St. Petersburg it is very small. In situations where there is not sufficient vapour in the atmosphere to form any daily cloud, it is to be presumed that if a barometrical registration were to be made, there would be no double movement exhibited showing a fall from 10 A.M. to 4 P.M., and a rise from 4 to 10 P.M., because there would be no condensation and warming to produce the former, nor evaporation and cooling to cause the latter.

The heating effects of condensing vapours may however be traced even in comparatively dry latitudes, such as that of Toronto, as shown in Col. Sabine's report to the British Association in 1844. There was no fall of the barometer at that place from 4 to 10 in the morning, although the temperature had risen from 39°·20 to 46°·35, above 7°; but in the middle of the day, from 10 to 4, with an increase of temperature from 46°·35 to 50°·55, being only 4°·20, the gaseous as well as the general atmospheric pressure was materially reduced! notwithstanding that the increase in the quantity of vapour during this time must have been as great as it was in the preceding period; and if this increased quantity had remained in the atmosphere, its pressure must have been added to that which previously existed. We are then obliged to suppose that the reduction of the pressure which took place immediately after 10 o'clock, arose from a cause which came into operation at that time; and that cause it is contended can be found only in the heating of the atmosphere by the condensation of vapour.

The great defect of the temperature theory is, that it fails to account for the fall of the barometer from 10 A.M. to 4 P.M., and its subsequent rise from 4 to 10 P.M., though this is the oscillation for which we have particularly to account; whilst the theory here maintained points out the cause of these, as well as the other diurnal, and also of the casual movements of the barometer. We are therefore at liberty to conclude that the semi-diurnal fluctuations of the barometer can be accounted for only on the condensation theory.

XXXII. *On the Principles to be applied in explaining the Aberration of Light. By the* Rev. J. Challis, *M.A., Plumian Professor of Astronomy in the University of Cambridge**.

THE aberration of light having been brought before the notice of the readers of this Journal by several recent communications, I am unwilling to let the subject drop without saying a few more words respecting the principles to be applied in the explanation of the phænomenon, which possibly may appear, after all that has been said, to be involved in uncertainty. I propose to answer the question, Is the aberration of light to be attributed to known causes, or must we, to explain it, have recourse to hypothesis?

The first attempts to explain aberration referred it to the combined effect of the motion of the earth and the temporaneous transmission of light, and accordingly proceeded on the principle of attributing it to known causes. It must, however, be admitted that every attempt to show *how* the observed effect resulted from these causes, what was the particular *modus operandi*, was unsatisfactory. Some idea appropriate to the subject was still wanting. This idea I consider that I have succeeded in supplying. I have argued, as had not been argued before, that because the direction of a celestial object is necessarily referred to the direction of a terrestrial object, light from the one as well as light from the other must be taken account of in considering the question of aberration. It is self-evident, that if at any instant two objects appear in the same direction, whatever course the light from the more distant may have taken before it reaches the nearer, it subsequently pursues a common course with light from the latter, and the two portions of light enter the eye at the given instant simultaneously. The direction in which the light comes is therefore judged to be the same as the direction at that instant of the nearer object from the eye. But during the interval the light takes to pass from the nearer or terrestrial object to the eye, this object is carried by the earth's motion away from the direction of the progression of light, and the two directions, at the time they are judged to be coincident, are in reality separated by a certain angle. This angle is aberration. I may refer to my communication in the February Number for a proof, which I venture to say is as cogent as any proof in the elements of geometry, that according to the principles just stated, an astronomical instrument employed to measure the *earth's way*, as it is called, would measure a smaller angle.

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* Communicated by the Author.

The difference, or aberration, is readily calculated from knowing by observations of the eclipses of Jupiter's satellites, the ratio of the earth's velocity to the velocity of light. Being so calculated the amount is found to be the same as the amount of aberration independently determined by astronomical observation. It follows from this accordance, not only that the aberration of light is entirely accounted for on these principles, but also, as a corollary, that the direction of the progression of light from a star, as it enters the eye, is the true direction of the star. Whether it be the star, or the terrestrial object to which it is referred, that is *seen* in its true place, is a curious question, not readily answered, and not in the least degree necessary to be answered in the present inquiry.

Sufficient reasons have now, I think, been adduced for coming to the conclusion, that the question I proposed to consider must receive the following categorical answer:—The aberration of light is entirely due to known causes, viz. the motion of the earth and the temporaneous transmission of light, and does not require for its explanation any hypothesis whatever.

What then becomes of the theories which have been framed to account for aberration on the hypothesis of certain motions of the æthereal medium? As explanations of aberration they can be of no value, it being an acknowledged principle in philosophy, that an hypothesis is not to be sought for to explain what may be explained by known causes. All that is left for the theorist to do, supposing, as it appears necessary to suppose, that the æther is in some way put in motion by the motion of the earth, is to show that *no* aberration results from such motion, the whole being attributable to the earth's motion. This problem I have considered in my two former communications, not because it was necessary to do so to complete the explanation of aberration, but with the view of removing an objection that might be raised against the undulatory theory of light. By taking account both of the light from the star and the light from the terrestrial object to which the star's direction is referred, I found that no aberration would result from the motion of the æther, provided it satisfied certain not improbable analytical conditions. A different conclusion would be arrived at by the same reasoning, if the light from the star, as is commonly done in treating of aberration, were alone considered.

With these remarks I dismiss the subject of aberration, having attained the object I had in view in taking it up, if I have succeeded in extricating the explanation of the phænomenon from hypothesis and conjecture, and placing it on its true basis.

Cambridge Observatory, Feb. 17, 1846.

XXXIII. *On the Cause of the Circulation of the Blood.* *By* John William Draper, *M.D., Professor of Chemistry in the University of New York.*

AMONG physiological problems there is none of greater interest, or of more importance in its relations to the well-being of man, than that which proposes to determine the true cause of the circulation of the blood, and the various other liquids which pass from one portion of living systems to another. Unquestionably one of the most important discoveries ever made by any physician was that of the route of the circulation by Harvey. The clearness with which he and his successors developed that doctrine not only fully established his views, but gave rise to a serious error which is scarcely removed in our times.

That error relates to the action of the heart. These earlier writers regarded the circulation of the blood as a hydraulic phænomenon, supposing that the heart simulated exactly the action of a pumping machine. It is now on all hands conceded that this organ discharges a very subsidiary duty. The whole vegetable creation, in which circulatory movements of liquids are actively carried on without any such central mechanism of impulsion; the numberless existing acardiac beings belonging to the animal world; the accomplishment of the systemic circulation of fishes without a heart; and the occurrence in the highest tribes, as in man, of special circulations which are isolated from the greater one, have all served to demonstrate to physiologists that they must look to other principles for the cause of these remarkable movements.

When we reflect how large a portion of the human family is destroyed by diseases dependent on derangements of the circulation, and to how great an extent the practice of medicine, as a scientific pursuit, must depend on just views of this important function, a natural philosopher can scarcely be more profitably employed than in attempting a solution of this problem.

I am persuaded that the phænomenon may be accounted for upon physical principles in a satisfactory manner; that we can co-ordinate together, and arrange as examples of one common law, the various forms of circulatory movements, whether they occur among vegetables or animals, among insects, or fishes, or mammals; and that the facts which we meet in derangements of these motions, or their cessation, as in fainting, coughing, and the different forms of disease, or such as take place after hanging, the inhalation of protoxide of nitrogen, or alcoholic drunkenness, or in that most remarkable of all results, the restoration from death by drowning; in all these and many other such cases we can give the most felicitous explanation.

The principal facts which I design here to establish are,—

First. The systemic circulation is due to the de-oxidation of arterial blood.

Secondly. The pulmonary circulation is due to the oxidation of venous blood.

And, in conclusion, I shall offer some explanatory remarks on the phænomena of the coagulation of the blood.

Several physiologists have already made an approach to the doctrine which will be developed in this memoir. Among well-informed writers it is conceded that we must look to the relations between the blood and the tissues for the true cause of the circulation. Thus Dr. Alison attributes the effect to a "series of vital attractions and repulsions," created by the operations to which the blood in the capillaries is subservient, an idea which Dr. Carpenter has rendered more explicit, by suggesting that these forces may not be "essentially different from those which are witnessed in Physics and Chemistry" (Carpenter's Human Physiology, vol. ii. p. 417). But these views do not communicate a definite idea of the true mechanism of the motion, nor do they exhibit that phænomenon as clearly connected with well-known chemical changes occurring in living systems. Should it appear, as I shall endeavour to prove, that the circulation is a necessary result of the alternate oxidation and deoxidation of the blood, we exchange at once a loose and ill-defined conception for a precise and definite fact.

It will be perceived that I speak of the oxidation and deoxidation of the blood as the great facts to be regarded, and leave out of consideration the spontaneous changes which that fluid itself undergoes; those minor effects which it impresses on the tissues, and those which they reciprocally impress on it. For the blood experiences in the systemic circulation an incessant change, discharging a double function. Its plasma serves for nutrition, its discs for the production of heat. But whilst the final function of the plasma and discs is different, there is an intimate relationship between them. It is from the plasma that the discs arise, and at its expense they grow. Moreover, the tissues themselves, in their metamorphoses, impress changes on the blood; the cells of which they are composed have an ephemeral existence, they dissolve, and the circulating fluid removes their remains and forms new ones in their stead.

I doubt very much whether animals obtain ready-formed fibrine from the vegetable world. During the incubation of an egg we see this substance arising from albumen, and the analogy is probably continued in higher forms of existence. Neither is it by any means certain that fibrine exists in a state of solution in the blood. But, as we shall presently see, the probabilities are that it coagulates as it is produced by the metamorphosis of the blood, that metamorphosis being originally due to the act of respiration. Under an accelerated respiration, the discs oxidize with corresponding rapidity and the amount of fibrine increases; but if the supply of oxygen be limited, there is a restraint on the change of the discs, and the amount of fibrine declines.

The ultimate products of these metamorphoses include of course all the results of the intervening stages, and those ultimate products are chiefly water, ammonia and carbonic acid. We are justified therefore in these physiological discussions in looking at the whole process as one of oxidation, and neglecting intermediate metamorphoses we regard only the final action, and that action is the transmutation of oxygen into carbonic acid, of hydrogen into water, of nitrogen into ammonia.

Explanation of the General Physical Principle.—If, in a vessel containing some water, a tube of small diameter be placed, the water immediately rises to a certain point in the tube and remains suspended.

Let the tube be now broken off below that point, and replaced in the cup of water; the liquid rises as before, but though it reaches the broken extremity it does not overflow. A capillary tube may raise water to its highest termination, *but a continuous current cannot take place through it.*

Now, suppose a rapid evaporation of the liquid to ensue from the broken extremity of the tube, as fast as the removal of one portion is accomplished others will rise through the tube, and in the course of time the vessel will be emptied. By evaporation from the upper extremity a continuous current is established; a spirit-lamp, with its cap removed, is an example of this fact.

Or, if the liquid which has risen to the upper end of the tube be of a combustible nature, oil for example, and be there set on fire, as the process of combustion goes on a current will be established in the tube, as in a common oil-lamp in the act of burning.

The principle which I wish to draw from these well-known facts is, that though ordinary capillary attraction cannot determine a continuous flow of a liquid through a tube, there are very many causes which may tend to produce that result.

Let *a*, *b*, *c* be a capillary tube filled with a certain liquid, between which and the tube there are at different points affinities differing in intensity. Suppose at *a* the affinity between the liquid and the tube is intense, that it becomes feebler and feebler towards *b*, and at *c* has ceased altogether. Under these circumstances there will be a continuous flow through the tube from *a* to *c*.

To make this quite plain, let us imagine the tube *a c* to be formed of combustible matter of any kind, and at the point *a* an oxidizing liquid enters it. The liquid, as it passes along the tube, exerts its oxidizing agency, which at the expense of the tube is gradually satisfied. In successive portions of such a tube the affinity is constantly declining. It is greatest at *a*, diminishes as it passes along, and ceases altogether at *c*. Under these circumstances there will be a constant flow along the tube.

A tube with an included liquid which is thus incessantly varying in its relations will give rise to a continuous movement. At the point of entrance, the liquid, powerfully attracted by the tube, rises with energy; but the chemical changes that set in, satisfying and neutralizing that attraction, to use a common expression, it loses its hold on the tube as it goes, and new quantities, arriving behind, continuously press out those which are before them.

These various results may be expressed in the following general terms.

If a given liquid occupies a capillary tube, or a porous or parenchymatous structure, and has for that tube or structure at different points affinities which are constantly diminishing, movement will ensue in a direction from the point of greater to the point of less affinity.

Or thus:

If a given liquid occupies a capillary tube, or a porous or parenchymatous structure, and whilst in that tube or structure changes happen to it, which tend continually to diminish its attraction for the surface with which it is in contact, movement will ensue in a direction from the changing to the changed fluid.

Application of this principle to the Circulation of the Blood.—Let us now apply these principles to some of the circulations which take place in the human system, and select for that purpose the four leading forms, the systemic, the pulmonary, the portal and the placental circulation.

THE SYSTEMIC CIRCULATION.—The arterial blood, which moves along the various aortic branches, contains oxygen which has been obtained in its passage over the air-cells of the lungs, an oxidation which is indicated by its bright crimson tint. On reaching its final distribution in the tissues, it effects their oxidation, producing heat; and as it loses its oxygen, and receives the metamorphosed products of the tissues, it takes on the blue colour characteristic of venous blood.

If now we contrast the relations of arterial and venous blood to the tissues, it is obvious that the former, from the fact that it can oxidize them, must have an intense affinity for them; but the latter, as it is the result of that action after all affinities have been satisfied, must have an attraction which is correspondingly less.

Arterial blood has therefore a high affinity for the tissues; venous blood little or none. But the change from arterial to venous blood takes place in the manner I have just indicated; and therefore, upon the first of the foregoing general rules, motion will take place, and in a direction from the arterial to the venous side.

By the deoxidizing action of the tissues upon the blood, that liquid ought upon these principles to move from the arteries into the veins, in the systemic circulation. The systemic circulation is therefore due to the deoxidation of arterial blood.

THE PULMONARY CIRCULATION.—In this circulation venous blood presents itself on the sides of the air-cells of the lungs, not to carbonaceous or hydrogenous atoms, but to oxygen gas, which being the more absorbable of the constituents of the air, is taken up and held in solution by the moist walls of those cells. Absorption of that oxygen takes place, and arterialization is the result. The blood from being blue turns crimson.

What now are the relations between venous and arterial blood and oxygen gas? For that gas venous blood has a high affinity, as is shown by its active absorption; but this affinity is satisfied and has ceased in the case of arterial blood.

The change from venous to arterial blood, which takes place on the air-cells which are charged with oxygen gas, ought upon these general principles to be accompanied by movement in a direction from the venous to the arterial side.

The pulmonary circulation is due to the oxidation of venous blood, and ought to be in a direction from the venous to the arterial side. These considerations therefore explain the cause of the flow in opposite directions in the systemic and the pulmonic circulation; in the former the direction is from the arterial to the venous side, in the latter from the venous to the arterial. It arises from the opposite chemical reactions which are taking effect in the system and in the lungs; in the former, as respects the blood, it is a de-oxidation, in the latter an oxidation.

THE PORTAL CIRCULATION.—Two systems of forces conspire to drive the portal blood out of the liver into the ascending cava.

1st. The blood which is coming along the capillary portal veins, and that which is receding by the hepatic veins, compared together as to their affinities for the structure of the liver, have obviously this relation—the portal blood is acted upon by the liver, and there are separated from it the constituents of the bile; the affinities which have been at work in producing this result have all been satisfied, and the residual blood over which the liver can exert no action constitutes that which passes into the hepatic veins. Between the portal blood and the structure of the liver there is an energetic affinity, betrayed by the circumstance that a chemical decomposition takes place, and bile is separated; and that change completed, the residue, which is no longer acted upon, forms the venous blood of the hepatic veins. In the same manner, therefore, that in the systemic circulation arterial blood in its passage along the capillaries becomes deoxidized, in consequence of an affinity between its elements and those of the structures with which it is brought in contact, and drives the inert venous blood before it, so too, in the portal circulation, in consequence of the chemical affinities and reactions which obtain between the portal blood and the substance of the liver, affinities and reactions which are expressed by the separation of the bile, that blood drives before it the inert blood of the hepatic veins.

2nd. The blood of the hepatic artery, after serving for the œconomic purposes of the liver, is thrown into the portal plexus. Hence arises a second force. The pressure of the arterial blood in the hepatic capillaries upon this is sufficient not only to impel it into the capillaries of the portal veins, but also to give it a pressure in a direction towards the hepatic veins; for any pressure which arises between the arterial blood of the hepatic, and its corresponding venous blood, must give rise to motion towards the hepatic veins, no regurgitation can take place backward through the portal vein upon the blood arriving from the chylopoietic viscera, because along that channel there is a pressure in the opposite direction, arising from the arterial blood of the aortic branches. The pressure therefore arising from the relations of the hepatic arterial blood conspires with that arising from the portal blood, and both together join in giving rise to motion towards the ascending cava.

THE PLACENTAL CIRCULATION.—The umbilical arteries carry in their spiral courses, as they twist round the umbilical vein, the effete blood of the fœtus, and distribute it by their ramifications to the placenta. In that organ it is brought in relation with the arterial blood of the mother, which oxidizes it, becoming by that act deoxidized itself. The fœtal blood now returns along the ramifications of the umbilical vein, and finally is discharged from the placenta by that single trunk.

That this is truly a change similar to that which is accomplished in the adult lungs, is shown by the circumstance that the blood of the umbilical arteries becomes brighter on its passage into the umbilical vein.

As the venous blood of the fœtus is thus oxidized by the arterial blood of the mother, movement must of necessity ensue in it, on the same principle that it ensues in the adult lung, and must take place in the same direction, that is to say, from the venous to the arterial side.

The fœtal circulation offers a very close resemblance to the circulation of fishes, and is merely a refined variety of that type. The true difference is that in fœtal life the condition of immobility is observed. In fishes the venous blood is brought to the gills, and subjected in their fibrillary tufts to the oxidizing agency of the air dissolved in the surrounding water. In these organs it therefore becomes arterialized, and is pushed into the pulmonary veins. These empty directly into the aorta, no systemic heart intervening, and the mechanical impulse received by the blood during its oxidation is found sufficient to carry on the aortic circulation: the heart therefore may be and is dispensed with. A fish, by spontaneously changing its position, or by the mechanical establishment of currents in the surrounding medium, can obtain new surfaces of water for the oxidation of its blood; but for the motionless fœtal mammalian a higher mechanism is required, a mechanism which can bring the oxidizing-maternal-arterial blood in relation with the branchial or placental vessels. It is true an intricate apparatus consisting of five different classes of vessels is the result, but the play of that apparatus is precisely the same as in the simpler contrivance of fishes.

Of the Mechanical Force with which these Motions are accomplished.—The force by which these motions are established is not alone in the proper direction, but also of sufficient intensity. Some years ago I made experiments with a view of establishing this point. Some of them are inserted in the Phil. Mag. for Oct. 1838. I found that water, under such circumstances as are here considered, would pass through a piece of peritoneum, though resisted by a pressure of nearly two atmospheres; and the same facts were observed even in the case of gases. Thus sulphurous acid gas would pass through a piece of India rubber against a pressure of seven and one-third atmospheres; carbonic acid against a pressure of ten atmospheres; and sulphuretted hydrogen, though resisted by more than twenty-four atmospheres.

Explanatory Remarks on the Coagulation of the Blood.—When blood recently drawn is kept in a vessel for a space of time it spontaneously separates into two well-defined portions, the one liquid and the other a soft solid—the serum and the clot.

Physicians generally regard this as due to the death of the blood. Whilst it is in the system it is under the influence of the vital force; but when removed it spontaneously undergoes the change in question, and, unable to keep its primitive condition, coagulates and dies. Accordingly this partial solidification of the blood is looked upon as a mysterious phænomenon, and though from time to time many experiments have been made and explanations offered, that which refers it to the presence or absence of the vital principle appears to be most generally received.

But it is very doubtful whether any such special power as a vital force exists. In the instance under consideration I cannot comprehend how a loss of vitality in the blood can in any manner elucidate or indeed have anything to do with the fact of its coagulation.

It appears to me that what occurs to the blood when drawn is precisely the same as that which occurs to it continually when in the system. If its fibrine coagulates in the receiving cup, it tends equally so to do in the peripheral circulation. I can see no difference in the two cases. And if this be true, it obviously is a fruitless affair to be seeking for an explanation of a difference in habitudes in and out of the system, when those differences in reality have no existence in nature.

If, when blood flows into a cup, we could by any mechanism withdraw the particles of fibrine as they agglutinate together, the phænomenon of coagulation would never be witnessed; and this is precisely the result in the living mechanism. The fibrine, as it passes into the proper condition, is removed by a series of events which will be hereafter explained. But whether it be in those states which physiologists designate living or dead, it exhibits continually the same tendency.

When we remember that the average amount of fibrine in blood scarcely exceeds one-five-hundredth part of its weight, and that this minute quantity is sufficient, by entangling the blood-discs, to furnish so voluminous a clot, we have little difficulty in understanding the cause of the false importance which has been attached to the fact of its coagulation. When we also remember that the phænomenon is one which, far from taking effect instantaneously, requires a considerable length of time, and estimate duly the demand that is made for fibrine by the system upon the blood, we shall have no difficulty in perceiving the truth of the observation which I thus wish to bring into a clear point of view,—that the tendency to coagulation in the system is as great as it is out of it, and that the true difference in the two cases is, that in the former the resulting solid is taken up and appropriated to the wants of the œconomy; in the latter it remains undisposed of, and, entangling the blood-discs in its meshes, produces a voluminous and therefore deceptive clot.

It is with this matter of the coagulation of blood precisely as it was formerly with putrefaction. Many of the older physiologists defined a living body as a mechanism having the quality of resisting external changes. After death its parts were ultimately resolved into water, ammonia, and carbonic acid. But better views on these topics are now entertained, and we know that the living body undergoes these putrefactive changes just as much as the dead, but then in it there are appointed routes by which the resulting bodies may escape; the carbonic acid through the lungs, the nitrogenized compounds through the kidneys, the water through both these organs and the skin. It is in this as in the coagulation of the blood, there is no difference in the chemical changes taking place, the difference consists in the disposal finally made of the resulting products.

That coagulation tends to take place equally in the living system as out of it, there is abundant proof. What are all the muscular tissues which constitute by far the larger portion of the soft parts, but fibrine which has thus been separated from the blood? And those muscular tissues every moment are wasting away, and giving origin to the metamorphosed products that we find escaping from the lungs, the kidneys, the liver; from what source then do they repair their waste, if not from fibrine coagulated from the blood during the act of life? Every muscular fibre is a living witness against the doctrine that it is death that brings on the coagulation of the blood.

That the truth of this view, which at first sight may appear indefensible, may be more clearly made out, let us consider under what circumstances the blood is placed whilst moving in the system. We have to remember that coagulation is not an instantaneous phænomenon, but one which requires a considerable lapse of time. And now, assuming the doctrine which I am advancing to be true, there are very obvious reasons that the blood, so long as it moves in the system, has its tendency to coagulate satisfied in a very partial manner. Let us observe its course. It leaves the left ventricle of the heart, one pulse-wave succeeding another with rapidity, and is distributed through all the aortic branches. It takes but a few seconds for this movement to be complete, a period far too short to allow coagulation to take place; it now passes on through the capillaries, or moves through parenchymatous structures; and here, even though a great delay may occur, inasmuch as the passages are so sinuous and often so minute that the discs can move but in a single file at a time, how is it likely, under such circumstances, that coagulation should ensue? For that to take place, it is needful that there should be a free communication throughout the mass, that each particle of fibrine brought into relation with those around it may exert its plastic power and join itself to them. But in the peripheral circulation it is isolated, the cells over which it is moving, or the narrow tubes through which it goes, protect it from other particles around, and on escaping into the commencement of the venous trunks, it is hurried in the torrent of the circulation at once to the heart. Without delay the right auricle and ventricle pass it forward to the lungs, and if any tendency to set had been exhibited during the brief moment of its passage, it is again distributed upon the capillaries of the lungs, and is situated precisely as it was when in the capillaries of the peripheral system.

In this manner I regard the coagulation of blood as a simple mechanical result, having no connexion with life or death, or the fictitious principle of vitality. At the two extremes of the circulation, the peripheral and the pulmonary, there is a sorting process continually going on. If a man were to agitate a quantity of this liquid in a tube, having a contrivance at each extremity to keep the particles of fibrine as they passed apart from one another, their plastic tendency to cohere could never be satisfied, and coagulation could never ensue. And this condition of things is, to a certain extent, approximated to in the mechanism of the body.

It thus appears that by the intervention of two capillary circulations, one in the lungs and the other in the system, the coagulation of blood must be greatly retarded, though the tendency to produce that result is quite as great as when the fluid is removed from the system. And with such an obvious explanation before us, why should we resort to any occult agency, or envelope the phænomenon in mystery, when it is plainly a mechanical affair?

Physiologists have never given a full value to the facts, that the setting of the blood requires time and a free communication through all parts of the fluid mass. If it be subjected incessantly to a mechanism which divides it into portions of inconceivable tenuity, and every moment isolates each particle from all its fellows, its coagulation must be greatly restrained. It is upon the same principle that the expressed juices of carrots and turnips deposit a fibrinary clot, as M. Liebig and others have observed. Whilst they are enveloped in the cells of those vegetables coagulation cannot take place, for each granule of fibrine is shut out from the others. What need is there to resort to a vital principle to explain for the human œconomy a result which equally obtains in the case of those humble plants, or why with some physiologists impute to the nervous system the quality of maintaining fluidity in the blood? These vegetables have no nerves.

The application of the principles here set forth furnishes a very felicitous explanation of a great number of effects which we witness, to some of which I may briefly refer. It is well known that after ordinary death, whilst the arteries are empty, the systemic veins and also the right cavities of the heart are full of venous blood. The reason is clear, although the ordinary theory, that the heart acts like a pumping machine, fails, as is well known, to explain it. As long as arterial blood is deoxidizing it will move to the venous side, a movement which must continue until the arteries are empty.

But it may be asked, why do not the right auricle and ventricle relieve the veins, and by their hydraulic action in the last moments of life push the accumulating blood through the pulmonary system? Again the reason is clear. *Movement through the lungs cannot take place except when oxidation is going on.* The systemic capillaries continuing their action long after the last breath is drawn, they make the blood accumulate in the veins, and from them there is no escape.

In the same way, in fainting, the blood leaving the arteries accumulates on the venous side, and as its flow is dependent on the push of the arterial blood entering the capillaries, so soon as no more enters no pressure is exerted on the venous trunks, and if a vein is opened there is no discharge, and under such circumstances hemorrhages at once stop.

After ordinary death, although the systemic arteries are empty, the pulmonary artery is full. That this should be the case is indicated upon our principles, for the blood cannot pass from the terminal ramifications of the pulmonary artery into the veins except by being oxidized. Respiration having ceased oxidation cannot take place, the movement is checked, and the blood remains in the artery.

In a paroxysm of asthma the lungs become obstructed with mucous secretions, and the rapidity of oxidation is therefore interfered with. Under such circumstances the passage of the blood is retarded, as is shown by the great dilatation of the jugular veins.

Whatever therefore deranges the process of oxidation deranges the flow of the blood. In violent expirations, such as in coughing, the observations of Haller show that the blood moves tardily in the lungs, and in delicate persons its retardation is so complete that it regurgitates in the great veins.

In a violent and continuous explosion of laughter, the jugular veins become excessively distended; the right cavities of the heart having no power to push the venous blood through the pulmonary capillaries, and owing to the expulsion of air from the air-cells, the blood itself fails to effect the passage with its usual speed. In this instance it must again accumulate in the veins.

The various cases here cited depend on retarded oxidation. I might now consider the reverse of this, or where oxidation goes on too rapidly, as when protoxide of nitrogen is breathed. Owing to the great solubility of this gas in serum, and its power of supporting combustion, we should expect to find it exert that control over the circulation which is well known to be one of its peculiarities. This paper is however extended to so great a length, that here I must stop, though I have made no allusion to the movements in the lymphatics or lacteals, or to the flow of sap in trees, or to the circulatory movements of the lower animals. These can all be explained upon the same principle; thus the descent of the sap follows as a necessary consequence of the decomposition of carbonic acid in the leaf. Nor have I said anything of the obvious control which certain classes of nerves have over the systemic oxidation. There are many facts which prove that the nervous system regulates this operation, and can either facilitate it or keep it in check. In this there is nothing extraordinary. A piece of amalgamated zinc exhibits no tendency to oxidize in acidulated water, but by the touch of silver or platina it is made to submit itself to the action of that medium. The act of blushing, and all local inflammations, show that changes in the relations of the nervous system control the oxidizing action of arterial blood; but to these things I propose to return on a future occasion. What is here stated is sufficient to illustrate the general principle to which I wish to draw attention, that *the chemical changes which are impressed on these circulating fluids are the true causes of their flow.*

XXXIV. *On the Existence of Finite Algebraic Solutions of the general Equations of the Fifth, Sixth, and Higher Degrees*[*]. *By* JAMES COCKLE, *M.A., Cantab.; Special Pleader*[†].

7. WHEN $y = \Lambda' x^{\lambda'} + \Lambda'' x^{\lambda''} + \ldots + \Lambda^{\text{xi}} x^{\lambda^{\text{xi}}} \quad . \quad . \quad (\text{n.})$
and $\quad {}_3\text{Y}_n = h_1{}^3 + h_2{}^3, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{o.})$

h_1 and h_2 having the forms of the quantities *squared* in (b.)[‡], what is the limit of n?

8. Make $\quad {}_3\text{Y}_n = h_1{}^3 + \text{j}'\Lambda' + \mathfrak{L}'' \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{p.})$

then[§] $\quad \text{j}' = \text{J}_2^{(2)} + \text{J}_4^{(2)} + \&\text{c.}; \quad . \quad . \quad . \quad . \quad . \quad (\text{q.})$

but $\text{J}_2^{(2)} = 0$ and Λ'', Λ''', disappear from j', if[||]

$$y = \Lambda' x^{\lambda'} + \text{L}'' + \text{L}^{\text{iv}} + \text{L}^{\text{v}} + \ldots + \text{L}^{\text{xi}} \quad . \quad . \quad (\text{r.})$$

and $\quad \text{L}^{\text{m}} = \Lambda^{\text{m}}\left(x^{\lambda^{\text{m}}} - \dfrac{\gamma_m}{\gamma_3} x^{\lambda'''}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{s.})$

So, $\quad \mathfrak{L}'' = \mathfrak{h}_2{}^3 + \text{j}''\Lambda'' + \text{I}^{\text{iv}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{t.})$

9. Let $y_r = L' x_r^{\lambda'} + L''\left(x_r^{\lambda''} - \dfrac{\gamma_2}{\gamma_3} x_r^{\lambda'''}\right) + \text{l}_r, \quad . \quad . \quad (\text{u.})$

$\text{l}_n = 0$, $L = \Lambda + l$, and $l =$ a constant, then[¶],

$$\left.\begin{aligned} 0 = \text{j}' &= [\text{l}_1 . \text{l}_{n-1}]_1{}^2 \\ 0 = \text{j}'' &= [\text{l}_1 . \text{l}_{n-1}]_2{}^2 \\ 0 = \text{I}^{\text{iv}} &= [\text{l}_1 . \text{l}_{n-1}]^3 \end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\alpha.)$$

$$\therefore n - 1 > 3, \text{ or } n > 4. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{v.})$$

10. Again, $\text{j}' = 0$ is equivalent to[**]

$$\left.\begin{aligned} \gamma_2^{\text{iv}}\Lambda^{\text{iv}} + . + \gamma_2^{\text{xi}}\Lambda^{\text{xi}} &= 0 \\ \gamma_3^{\text{vi}}\Lambda^{\text{vi}} + . + \gamma_3^{\text{xi}}\Lambda^{\text{xi}} &= 0 \\ \gamma_4^{\text{viii}}\Lambda^{\text{viii}} + . + \gamma_4^{\text{xi}}\Lambda^{\text{xi}} &= 0 \\ \gamma_5^{\text{x}}\Lambda^{\text{x}} + \gamma_5^{\text{xi}}\Lambda^{\text{xi}} &= 0 \end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad (\beta.)$$

$\text{j}'' = 0$, on eliminating $\Lambda^{2\text{r}+1}$, to[††]

$$\left.\begin{aligned} {}^{\text{iv}}\gamma_1\Lambda^{\text{iv}} + . + {}^{\text{x}}\gamma_1\Lambda^{\text{x}} &= 0 \\ {}^{\text{viii}}\gamma_2\Lambda^{\text{viii}} + {}^{\text{x}}\gamma_2\Lambda^{\text{x}} &= 0 \end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\gamma.)$$

$\text{I}^{\text{iv}} = 0$, on eliminating Λ^{vi}, Λ^{x}, to[‡‡]

$$ {}_{\text{iv}}\gamma_1\Lambda^{\text{iv}} + {}_{\text{viii}}\gamma\,\Lambda^{\text{viii}} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\delta.)$$

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[*] See my presumed solution of the equation of the fifth degree, at page 125 of the last volume of this Magazine. I there used the ratios z_1, z_2 ... of the quantities Λ', Λ'', .. to one of their number, but have here employed other ratios, or, more properly speaking, the quantities themselves.—J. C.

[†] Communicated by the Author.

[‡] Phil. Mag., this vol., p. 132.

[§] Ibid. S. 3. vol. xxvii. p. 126.

[||] Ibid. p. 293 (16.).

[¶] Ibid. p. 126, note ‡, and this vol., p. 132, par. 2.

[**] Phil. Mag. S. 3. p. 126, line 9.

[††] Ibid. (*f.*) and (*g.*)

[‡‡] Ibid. (*h.*)

11. Let* $\mathrm{L}^{\mathrm{iv}} + \mathrm{L}^{\mathrm{vi}} + \ldots = \grave{p}_0 + \grave{p}_1 x + . + \grave{p}_{n-1} x^{n-1}$, (w.) then†, ${}^{\mathrm{iv}}\Sigma^{\mathrm{xi}}.\,\mathrm{L} = (1+p)(\grave{p}_0 + \ldots) + . + q_{n-2} x^{n-2}$; . (x.) and we have, in general, n quantities $1 + p$, q_0, ., q^{n-2}, to satisfy 3 homogeneous equations $0 = \mathrm{j}' = \mathrm{j}'' = \mathrm{l}^{\mathrm{iv}}$, or, n quantities $\grave{p}_0, \grave{p}_1, ., \grave{p}_{n-1}$ to satisfy 3 other homogeneous conditions (γ.) and (δ.), using the former n quantities to satisfy the group (β.), non-homogeneous with respect to them; hence

$$n > 3. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(y.)}$$

12. (v.) and (y.) are not inconsistent, for, if

$$\mathrm{A}_0 = 1, \frac{\mathrm{A}_m}{\mathrm{A}_{m-1}} = \frac{n-r+m}{n\,m} \quad . \quad . \quad . \quad . \quad . \quad \text{(z.)}$$

and

$$_r\mathrm{Y}'_n = p_r - \mathrm{A}_1 p_{r-1} p_1 + \mathrm{A}_2 p_{r-2} p_1^2 + \ldots \pm \frac{n(r-1)}{r} \mathrm{A}_{r-1} p_1^r, \quad \text{(aa.)}$$

then this accented value of Y is a critical value from which $\grave{p}_0$, q_0, . . . disappear, and, since (2.) and (3.)‡ are respectively

$$_3\mathrm{Y}'_5 = 0, \text{ and } {}_4\mathrm{Y}'_5 - \frac{1}{5}\,{}_2\mathrm{Y}_5'^2 = 0, \quad . \quad . \quad . \quad \text{(ab.)}$$

my solution would fail if $n-1$ were < 4. Hence, for all critical functions (y.) degenerates into (v.); and, after solving critical equations, we shall have quantities $\grave{p}_0$, q_0 . . left for satisfying (other) conditions whose degrees are unaffected by our previous operations.

13. However numerous might be the groups (β.), (γ.), (δ.), or the relations forming those groups, it would seem that some of the Λ's being lost at each descending step, the limit will not be proportionally elevated. We may make

$$\Sigma_r = (1+p)\,{}^{(r)}\Sigma_{r-1} + \ldots + q^{(r)}_{n-2} x^{n-2}, \quad . \quad . \quad \text{(ac.)}$$

for the Λ's introduced as we ascend the groups.

14. Λ' and Λ'' will introduce new values of $1+p$ and q into the (now to be combined) equations (2.) and (3.), or (ab.), but, as it appears to me, no new difficulty. The γ's may be derived from the τ's§, and from each other by one operation θ, and if

$$\gamma = \Theta(\tau), \text{ then } \grave{\gamma} = \Theta(\gamma), \text{\&c.} \quad . \quad . \quad . \quad \text{(ad.)}$$

Grecian Chambers, Devereux Court,
January 31, 1846.

JAMES COCKLE, Jun.

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* See Sir W. R. Hamilton's "Inquiry, &c." into Mr. Jerrard's method (Sixth Report of the British Association) from [4.] p. 301 to line 9 of p. 304.

† Ibid. p. 303.

‡ Phil. Mag. S. 3. vol. xxvii. p. 125.

§ Ibid. pp. 292, 293.

XXXV. *On some New Species of Animal Concretions.* *By* THOMAS TAYLOR, *Surgeon.*

[Continued from p. 46.]

Resino-bezoardic Acid Calculi.

VERY shortly after commencing the examination of the calculi in the College collection, my attention was drawn to several concretions which possessed the easy fusibility and general characters of a resin, and which were described in the MS. Catalogue as "false West Indian Bezoars," on the supposition that they were artificial compounds. The peculiar characters however of the resin of which they consisted, and their finely laminated structure, which it would be impossible to imitate, left no doubt on my mind of their being genuine bezoars, and in January 1841 I described them to the Museum Committee as consisting of a *vegetable* resin, derived most probably from the resinous juices of the plants on which the wild goats of the East had fed. In the same year a very interesting paper appeared in the *Annalen der Chemie und Pharmacie*, by M. Goebel, describing a new species of calculus which he had found in the Zoological Museum at Dorpat, and to which, on the supposition of its being a biliary concretion, he gave the name of *lithofellinic acid.* A similar calculus from the Pathological Museum at Göttingen was shortly after examined by Professor Wöhler.

The similarity in chemical characters of the concretions examined by these chemists with the resinous concretions previously examined by myself, rendered it certain that they were identical in composition; but as it was important to determine whether they were biliary calculi or simply intestinal concretions, derived from the materials of the food, I repeated at some length my experiments, but without coming to any other conclusion than that formerly expressed. The reasons which have induced me therefore to place the calculi among the intestinal calculi in the College Catalogue are as follows.

In the first place, the greater number of them contain, as the subjoined analysis will show, a small quantity of a soft viscid resin, resembling a vegetable balsam.

Secondly. They resemble all other concretions formed in the intestines, by having a foreign body, as a piece of wood or a seed, for their nucleus.

Thirdly. They frequently attain a very large size, quite inconsistent with the notion of their being biliary concretions, or having been contained in the gall-bladder. There is one specimen in the Museum which measures three inches and a half in length, and the same in its greatest breadth. This calculus is of a rude triangular figure; it has evidently been accompanied by other calculi, as both of its extremities possess the smooth depressed surfaces found in concretions which have been in contact with others. Another specimen, of an oval figure, is four inches in length by three in breadth. Against the notion that these concretions may have been formed from the natural or the altered constituents of the bile concreting around foreign bodies in the intestines, it may be remarked that we have no other instance of a biliary calculus being so formed*. The large biliary concretions which are sometimes passed *per anum* by the human subject, have undoubtedly received no increase in bulk while in the intestine, but have made their way into the intestine either through an ulcerated opening or through the dilated biliary duct, which is capable of undergoing dilatation to a much greater extent than is generally imagined.

The circumstance of the Oriental Bezoar being composed of a vegetable acid, as I have shown in a former paper, together with the assertion of most Oriental travellers, that the resinous concretions are found in the stomach of the animal (not a very likely spot for a biliary calculus), adds considerable weight in favour of their vegetable origin. It is however right to state that I have not been able to detect the presence of resino-bezoardic acid in several of the known resins. Our acquaintance with these substances is however so limited that it would require a very extended series of experiments to determine this question in the negative. In its chemical relations, resino-bezoardic acid closely resembles the pimaric acid of M. Laurent, which that excellent chemist has recently shown to be the natural acid of the fir. This fact, coupled with the circumstance that the calculi are not very uncommon, and that vast forests of pines abound in the regions inhabited by the goats, render it not improbable that this resin is derived from some of the fir tribe. As the term *lithofellinic acid* gives therefore an erroneous idea of the origin of these concretions, I have ventured to substitute that of *resino-bezoardic acid*, which does not differ materially from that of "résine animale bézoardique" given to them by Fourcroy. This name will also serve to identify the circumstances under which it was first discovered, should its natural source be hereafter ascertained.

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* Ambergris perhaps forms an exception to this statement. This substance is found in the intestines of the Spermaceti Whale, or floating on the sea. In the Catalogue I have placed it among intestinal concretions, although I have pointed out at the same time that it is a biliary product; its principal constituent, ambreine, bearing the same relation to the bile of the Whale as cholesterine does to that of Man.

Resino-bezoardic acid calculi are usually of an oval figure. Their external surface is smooth and polished, and has generally a greenish yellow, green, or a light brown colour. They are made up of thin concentric layers, which are frequently of a deeper tint than the exterior. In the centre of the calculus some foreign body is invariably found which forms the nucleus. These calculi are exceedingly brittle; the fracture is conchoidal, and has a resinous lustre. They vary considerably in size, but are usually larger than the ellagic acid species. One specimen in the Museum measures nearly ten inches in circumference. They melt like resin in the flame of a candle, and when more highly heated, give off white vapours, which have an aromatic odour, catch fire, burn with a brilliant flame, and leave behind a small shining carbonaceous ash.

Resino-bezoardic acid calculi readily dissolve in alcohol, with the exception of a small quantity of flocculent matter. The alcoholic solution varies in colour in different calculi, but is usually of a red or greenish-red tint. The solution gradually deposits small crystals, which, when examined by the microscope, are seen to consist of low six-sided prisms with flattened extremities. When the alcoholic solution is mixed with water the resin is thrown down. The precipitate appears under the microscope in the form of small crystalline tufts.

Digested in solution of potass these calculi readily dissolve, the solution is of a brownish green colour, and when neutralized by an acid, a thick curdy precipitate is produced, which by agitation adheres together, and while warm may be kneaded between the fingers or drawn into threads like cobbler's-wax. The viscidity of this precipitate is owing to another resinous matter which the calculi contain; for the pure resino-bezoardic acid similarly treated forms an amorphous precipitate which cannot be made to adhere together. They dissolve in solutions of ammonia and its carbonate. In concentrated sulphuric acid they also dissolve. The solution is of a red colour, and is rendered turbid by the addition of water. The precipitate is not crystalline, like that from its solution in alcohol, but consists of minute transparent yellow particles. Nitric acid acts with energy upon these calculi, nitrous acid is evolved, and a light red solution is formed, which quickly becomes yellow.

Analysis.

About 400 grains were reduced to a fine powder, mixed with distilled water, and subjected to distillation in a glass retort until about two ounces had passed over. The distilled liquid was quite transparent, and possessed the peculiar aromatic odour of the calculus, but no volatile oil could be detected. The powder was separated from the rest of the water by filtration, and dried at 200° Fahr. It was dissolved in twelve ounces of boiling alcohol. The solution was of a bright red colour when viewed by transmitted light, and had a greenish tinge by reflected light; with the exception of a small quantity of flocculent matter it was quite transparent.

In order to separate the insoluble matter, the liquid while still hot was filtered, and the matter on the filter washed with a fresh portion of alcohol and dried. This matter was of a dirty brown colour, with a shade of green. It was when quite dry rather soft, so as to admit of being moulded between the fingers. When heated on platina foil it did not fuse, but softened, caught fire, and burnt briskly, emitting at the same time the odour of heated Indian rubber. It was insoluble in water, either hot or cold.

That this substance was not caoutchouc, was shown by its not being dissolved or softened when acted upon by absolute æther or oil of turpentine. A solution of caustic potass extracted some of its colour, but did not appear to dissolve it. The exact nature of this matter I am unable to decide; its vegetable nature is rendered probable by the total want of any animal odour while burning. It amounted to about two per cent.

The filtered alcoholic solution became slightly turbid on cooling; after standing a short time small crystals were deposited, and a crystalline crust formed upon its surface. Some of the crystals when examined by the microscope had the form of very regular six-sided plates, and others that of six-sided prisms. When a drop of the liquid was allowed to evaporate on a glass plate, and the residue examined by the microscope, crystals were formed, whose figure was not very distinct, but appeared to be that of a six-sided prism lying on its side; occasionally a six-sided plate was also visible.

The liquid was put into a retort, and about two-thirds of its bulk distilled over. It was transparent while hot, but on cooling deposited an abundant crop of small crystals. These crystals had the form of three-sided plates; when carefully fused upon a slip of glass, they were converted into six-sided plates.

The crystals obtained at different times were purified by being repeatedly crystallized from their alcoholic solution, which removed nearly the whole of their colouring matter. They possessed all the characters of the lithofellinic acid of Professor Goebel, and constituted the bulk of the calculus.

The mother-liquor was mixed with water, when a precipitate separated which by agitation was converted into a viscid turpentine-looking substance that adhered to the sides of the glass. When a drop of the mother-liquor was evaporated on a glass plate, very few crystals could be detected, but a great number of thick, viscid oily-like drops: by heating the glass vapours arose, and a hard uncrystalline resin was left. When the alcoholic solution of the crystals was mingled with water, a crystalline precipitate was thrown down, which beneath the microscope appeared in the form of small irregularly-shaped prisms, arranged in stellate groups. This difference in the character of the two precipitates appeared to indicate that the mother-liquor contained either a volatile oil or some soft resin in addition to the crystalline resin previously described. To determine this question the whole was placed in a retort, and submitted to distillation; the spirit came over quite free from any essential oil, merely retaining the peculiar odour of the calculus: the last portions smelt much stronger, and were slightly turbid. The precipitate had melted and formed a deep red oil, which adhered to the sides of the retort; when cold it was soft and ductile between the fingers. It was readily soluble in solutions of potass and ammonia, the solutions were rendered milky by the addition of an acid, but no precipitate fell. The milky liquor when examined by the microscope gave the appearance of oily globules. The soft resin remaining in the retort was now divided into two portions; to the one solution of ammonia was added, and to the other æther.

The ammoniacal solution was perfectly clear and of a bright red tint; it was neutralized by muriatic acid, a viscid precipitate separated, which was collected together, washed, broken into fragments, and put into a glass tube together with æther. It only partially dissolved, and after standing some days six-sided prisms were found adhering to the tube, the æthereal solution was evaporated, and a resinous matter more fusible than the former was left.

That portion of the soft resin which had been digested with cold æther did not entirely dissolve, but left some crystals of resinous matter undissolved; the æthereal solution was evaporated, and the residue, which was quite similar to that which had been previously treated with ammonia, was mixed with it and both dissolved in alcohol, sp. gr. 0·840. The tincture was set aside for some weeks; only a small quantity of crystalline matter was deposited, together with a little soft resin; it was therefore distilled, and the residue again treated with absolute æther, in which, with the exception of a very small quantity of resin, it entirely dissolved. On distilling off the æther a semi-fluid viscid balsam of a dark-red colour was left, which did not solidify at the temperature of the air, and acquired a pellicle on its surface by exposure to air. When heated a portion of it was volatilized, giving off at the same time the odour of melted caoutchouc. It readily caught fire and burnt brightly; its combustion was unaccompanied by the slightest trace of the odour given out by animal matter. It readily dissolved in caustic potass, and the addition of an acid threw it down unchanged: it possessed a biting acrid taste, felt particularly about the fauces; by exposure to the air it became a hard resin.

When submitted to distillation in a small tube retort, no oil passed over until the resin had acquired a temperature at which it began to decompose, when an empyreumatic oil came over: the quantity submitted to distillation was, however, too small to render the experiment quite satisfactory.

The only conclusions that can be drawn from this analysis are, that the principal constituent of the calculus is a vegetable resin, which is characterized by crystallizing in the form of six-sided prisms; that it is accompanied by a small quantity of a soft resin, probably containing volatile oil; that in addition to these it contains some other substances, as colouring and extractive matter, the precise nature of which it is impossible to determine, but which are doubtless also of vegetable origin.

M. Goebel detected in the concretion examined by him a small quantity of the colouring matter of the bile. In no one of the concretions examined was I able to satisfy myself of the presence of that substance. It is probably therefore only an accidental constituent. Its presence is however no proof of their biliary origin, since the colouring matter and other constituents of the bile are frequently found in hair-balls and other concretions known to be formed in the intestine.

Resino-bezoardic acid, when freed from the other substances with which it is mixed in the calculus, possesses the following properties:—It slowly dissolves in cold alcohol, more rapidly in hot; according to Goebel, one part of resino-bezoardic acid requires 29·4 of alcohol to dissolve it at 68° of Fahrenheit and 6·5 of boiling alcohol; in cold æther it is very sparingly soluble, 444 parts being required, but only 47 when boiling. Its alcoholic solution has an acid reaction, and the resin is slowly deposited from it in the form of short six-sided prisms. The crystals are exceedingly small; they have generally a yellow tint, but may be obtained quite colourless by previously digesting the alcoholic solution with animal charcoal: they are hard, brittle, and easily reduced to powder, inodorous, and have a bitter resinous taste; their summits are generally quite flat, but are sometimes bevelled at their edges. Three-sided prisms are occasionally deposited, which apparently result from an extension of each alternate face of the six-sided prism.

The crystallized acid fuses at 401° Fahrenheit, and when not heated beyond that temperature becomes on cooling an opake crystalline mass. If the fused acid be heated only a few degrees above 401° Fahrenheit, it forms when cold a transparent glass, without the slightest trace of crystalline structure: when alcohol is poured over the fused mass a number of minute cracks are suddenly formed, which possess considerable regularity. If a thin layer of alcohol is allowed to remain over it, the whole is quickly converted into an aggregated mass of regular crystals. The most remarkable circumstance is that the melting-point of the vitreous or amorphous resino-bezoardic acid is nearly 180° lower than that of the crystallized acid, Prof. Wöhler having determined that the crystallized acid melts at 400° Fahrenheit, while the amorphous fuses at a temperature between 220° and 230° Fahrenheit. In this respect resino-bezoardic acid resembles sugar, sulphur, amygdaline and silvic acid; all of which bodies have two distinct fusing-points, according as they are either in a crystalline or amorphous state: this property Wöhler believes to be possessed by all dimorphous bodies. The above fact may be readily observed in the following manner, which serves as a very characteristic test of resino-bezoardic acid. Let a few grains of the powdered resin be strewed over a thin slip of glass and held over the flame of a candle until a portion only of the resin is melted: if the edges of the semi-fused portion be examined by the microscope groups of very regular six-sided plates are seen; the perfectly fused portion is glassy and devoid of crystalline structure. This test does not always succeed with the raw calculus, owing to the foreign substances which it contains. When heated beyond its melting-point this acid gives off white vapours, which have an aromatic odour; it finally catches fire and burns like resinous bodies in general.

Resino-bezoardic acid is insoluble in water and muriatic acid. It is thrown down from its alcoholic solution by water as a white precipitate, which under the microscope appears in the form of small prismatic crystals arranged in stellate groups. It is readily soluble in a solution of potass, soda, ammonia, and carbonate of ammonia, and is precipitated on the addition of an acid. The precipitate at first forms a dense white coagulum, but shortly becomes pulverulent; when examined by the microscope it is not crystallized, but consists of minute transparent amorphous particles. It melts at 220° Fahrenheit, and is evidently the amorphous state of the acid.

When the potass solution is evaporated a transparent gummy mass is left, which is insoluble in solution of potass, but dissolves in pure water. When the potass solution is concentrated by boiling, the compound of the resin and alkali separates from the liquid and swims on its surface; when cold it forms a hard yellowish mass like resin, which dissolves in æther, alcohol and water. When the ammoniacal solution of this acid is evaporated, the resin separates unaltered. Nitric acid decomposes this acid; nitric oxide gas is evolved, and a beautiful red solution formed, which quickly becomes yellow.

Concentrated sulphuric acid dissolves the resino-bezoardic acid: the resin is precipitated unaltered on the addition of water in the amorphous state.

3·777 grs. of the crystallized acid, which had been rendered perfectly colourless by digestion with animal charcoal, when dried at 180° Fahr. in a current of dry air and burnt with chromate of lead, gave 3·64 water and 9·793 carbonic acid. This result agrees with the analyses of Messrs. Ettling and Will and Professor Wöhler, who found—

	Ettling and Will.		Wöhler.		T. Taylor.
Carbon .	71·19	70·80	70·83	71·09	70·71
Hydrogen	10·85	10·78	10·60		10·71
Oxygen .	17·96	18·42	18·57		18·58
	100·00	100·00*	100·00†		100·00

Messrs. Ettling and Will, who have analysed some of its salts, regard the formula of the crystallized acid as $C_{42} H_{74} O_7 + HO$, while Professor Wöhler represents it as $C_{40} H_{70} O_7 + HO$.

Resino-bezoardic concretions were first examined by Fourcroy and Vauquelin. Their account is very slight and imperfect, but is accompanied by a very accurate drawing of a fragment of one of them. Fourcroy states, without mentioning his authority, that they are taken from some unknown species of Asiatic or African animals, and believes them to be formed from the resinous juices of the plants on which these animals fed.

In the College Catalogue I have described them as being the true *Occidental Bezoar*. Subsequent consideration how-

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* *Ann. der Chem. und Pharm.*, xxxix. 242.

† Poggendorff's *Ann. der Phys. und Chem.*, liv. 259.

ever inclines me to believe that the true Occidental Bezoar consisted of diphosphate of lime, and that these concretions, which Kæmpfer states were termed in Persia Lapis Bezoar Occidentalis, on account of their similarity to the concretions brought from South America, were so called from their exterior possessing the same smooth polished exterior as the diphosphate of lime concretions. The concretions described by Kæmpfer under the name of *Coagulum Resinosum Bezoarticum*, are evidently identical with resino-bezoardic acid calculi; for he says that the Swedish ambassador, on his departure from Ispahan, purchased some specimens, which, when thrown upon burning coals, melted and gave out an aromatic odour like that of colophony or olibanum. In the work of Clusius there is a figure of the occidental bezoar which is quite characteristic of this calculus, and Monardes asserts that they were taken from the wild goats of Persia. It is not however probable that any particular species of concretion was confined exclusively to the animals of one or the other hemisphere, since the resinous and bitter juices from which the concretions are formed exist in the plants of both divisions of the globe.

[To be continued.]

XXXVI. *On the Winter Storms of the United States.* *By* Lieut.-Colonel SABINE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

YOUR meteorological readers, and especially those who take an interest in the law of storms, will, I am sure, be glad to have their attention drawn to a second memoir by Professor Loomis of New York, on the phænomena of the great storms which are experienced in the United States during the winter months. In this memoir (Art. IV. of vol. ix. of the Transactions of the American Philosophical Society) two storms are investigated, one of which occurred about the 3rd of February, 1842, and the other about the 15th of the same month of the same year. The method of investigation is the same which Professor Loomis adopted in his account of the great storm of December 20th, 1836, viz. the assemblage in one view of the atmospherical circumstances simultaneously observed over the whole extent of the United States, both during the continuance of the storm and for one or two preceding days. It is by this path that we may confidently hope to attain to a knowledge of the causes which produce these great atmospherical derangements; and, thanks to the spirit of co-operative labour which distinguishes the present time, we have every prospect of seeing this path successfully pursued. Although the memoir itself is not very long, the illustrations which accompany it are many, and its republication in this country without them would not convey the full amount of instruction to be derived from the text and plates conjointly. Memoirs of this description scarcely admit of an abstract; there are however certain circumstances which present themselves in so striking a manner as common to the three storms above mentioned, as to induce the belief that they may be viewed as the characteristics of a particular class of storms which occur in the United States in the winter months of every year. The circumstances alluded to may admit of a brief notice in the light of a first generalization; and it may have the additional advantage of attracting the attention of some of your readers to the original memoir in the Transactions of the American Philosophical Society.

We may picture to ourselves, in the first instance, a normal state of the atmosphere over the United States, in the departure from which we may trace the successive phases of derangement which constitute the storm. In this normal state the wind is from the west, or a few degrees south of west, in the lower as well as in the upper current, with the thermometer and barometer at or near their respective mean heights for the time and place: the whole body of the air from the surface of the earth to its upper limit, is proceeding harmoniously in the one direction, and having blown across the greater part of the continent of America before it reaches the middle states of the union, it is extremely dry, and the atmosphere perfectly clear.

The interruption to this normal state, which in the order of time appears first to present itself, is *a change in the direction of the lower stratum of the air*, which becomes southerly in the countries situated in the north of the Gulf of Mexico, and south-easterly in the south-eastern states. The change in the direction of the lower stratum of air is speedily followed, or perhaps it should rather be said is accompanied, by cloud, and by a rise of temperature, which progressively increase, the one in extent and the other in intensity, attended by a falling barometer. The cloud condenses into rain or snow, the area of which progressively extends till but a comparatively small margin of cloud remains without precipitation. The thermometer continues to rise, the barometer to fall, and the rain or snow to descend, until the instant when the abnormal winds from the south and east give place to a more violent rush of air from the west and north-west, by which the phænomena of the storm are swept onwards, and transferred successively from the middle to the eastern states, and thence to the sea, with a velocity which in different instances has been noted to vary from about twenty to thirty-six statute miles an hour. The maximum of the thermometer and minimum of the barometer coincide generally (and with great exactness in the eastern states) with the change of the wind, the derangement of the temperature being so great as 20° and even occasionally 30° above its normal state.

The description thus given was written after reading the second memoir, and was consequently drawn principally from the phænomena of the two storms of 1842, with only a general recollection of the similarity of the circumstances of the storm of December 1836, described in Mr. Loomis's previous memoir, which I had not looked into for some months. On its reperusal since, I find a condensed view of the facts of that storm at once so graphical, and by its accordance with the description drawn from the two other storms exemplifying so well their common character, that I am induced to insert it.

"The principal characteristics were as follows:—After a cold and clear interval with barometer high, the wind commenced blowing from the south. The barometer fell rapidly, the thermometer rose, rain descended in abundance. The wind veered suddenly to the north-west, and blew with great violence: the rain is succeeded by hail or snow, which continues but a short time; the barometer rises rapidly; the thermometer sinks as rapidly. These changes are not experienced everywhere simultaneously, but progressively from west to east."

Such then are the phænomena, and such the order of their occurrence, in a class of storms which in the winter season, and in the localities referred to, are of frequent occurrence; that which has been described as the normal state of the atmosphere, and that which has been described as the interruption to it, appearing to follow each other in repeated succession. The facts being thus before us, it is for meteorologists to consider of their explanation.

When it is remembered that the temperature of the surface water of the Gulf of Mexico which washes the southern shores of the United States is considerably higher than the ordinary temperature of the surface water of the ocean in the same parallel; and that the gulf-stream which coasts the south-eastern states conveys heated water into parallels where its relative difference from the ordinary ocean-temperature is even greater than in the Gulf of Mexico, we should be prepared to expect that the abnormal southerly and south-easterly winds should be *extremely* humid as well as warm; whilst the normal westerly wind, which has crossed the rocky mountains as well as a wide extent of continent, must be *extremely* dry as well as cold. Now the warm and moist air being once conveyed to the previously cold and dry localities where the storm appears to originate, the subsequent order and succession of the phænomena are sufficiently intelligible. The fact of which it seems most difficult to render an explanation, and to which therefore attention may be profitably directed, is that of the apparent tendency of the south and south-easterly winds to insinuate themselves in the lower stratum of the air, and to prevail over the regular and normal west wind, whenever the latter has moderated after its temporary violence. The phænomenon is confined to the lower stratum of the air, as the direction of the upper clouds is preserved steadily from the west. Mr. Loomis suggests in explanation that the momentum which the westerly wind acquires at its period of violence causes it to overblow itself, and produces a reaction, each storm having thus, as he conceives, a direct tendency to produce its successor. Is it not possible that the elastic force of the vapour rising over the heated surface of the ocean to the south and south-east of the United States, and making its way to the dry interior of the continent, may have a tendency to impede and counteract the current of air proceeding from an opposite direction? It is not inconsistent with the notion of the independence of air and vapour when at rest, that when in motion either should affect the other. It is I believe a common opinion that air in motion carries vapour with it; the supposition here made is the counterpart of this. I remember to have heard that at Newfoundland,—where the north-west (the prevailing) wind is particularly cold and dry, and where the surface of the sea to the south-east is of unusually high temperature for the latitude, owing to the gulf-stream, the sea fog, as it is called,—frequently makes its way from seaward against the wind; and that the wind then gradually dies away and is succeeded by a gentle breeze from the opposite or sea quarter.

But whatever may be the fate of conjectures which may be hazarded before the true explanation of the phænomena shall be arrived at and generally accepted, the very clear and lucid manner in which Mr. Loomis has arranged and combined the facts which he has collected together, and the ability and true philosophical spirit in which he has discussed them, call for our grateful acknowledgements, and cannot fail to operate as a stimulus to the co-operators in the United States to persevere in their meteorological observations. Mr. Loomis has expressed an earnest wish that the co-operation should be extended towards the north into the countries occupied by the Hudson's Bay Company; and it cannot fail to be seen, on reading his memoir, how much observations in that quarter are wanted for the elucidation of questions which arise. We may hope that his wishes in this respect will not be disappointed.

Believe me, sincerely yours,

Woolwich, February 12th, 1846. EDWARD SABINE.

XXXVII. *On the Anthracite and Bituminous Coal-Fields in China.* *By* RICHARD COWLING TAYLOR, *F.G.S.*[*]

WE have seen the recent announcement of the sailing, from hence, of a vessel containing 308 tons of Pennsylvania anthracite, destined for Hong-Kong in China. Some very natural speculations have arisen from this circumstance, as to the probability of that remote country furnishing a market for American anthracite. As no details accompany the statement alluded to, we are not in possession of any material facts whereby an estimate can be formed of the probable success of the undertaking, in a commercial sense; and we are not sure but the coal may have been employed for convenience merely, as ballast.

In the East Indies various depots of European coal have been established, for the service of the British government steamers. This fuel, for the most part, it is understood, consists of the anthracitous and partially bituminous coals of South Wales, of course obtained at great expense. It appears that 5000 tons of English coal, at a freightage of about £2 per ton, are annually imported into Bombay, for the Company's steamers. Bituminous coals have been derived from much less distant sources; among which the Burdwan coal-field, in the vicinity of Calcutta, may be named. Mergui Island, also, in the Bay of Bengal, has lately furnished some steam coal to Singapore. The steam ships on the China seas, during the war with that vast country, were supplied from these various sources.

I do not propose to discuss the profitableness, or otherwise, of a Chinese market for our American anthracite. But as during the process of collecting statistical information for a proposed volume on "The Geological and Geographical Distribution of Coal and other Mineral Combustibles[†]," some

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[*] From a pamphlet communicated by the Author.

[†] See in Philosophical Magazine, vol. xxvi. p. 263, the Prospectus of this work, by Mr. R. C. Taylor, for which subscriptions are received by Messrs. Wiley and Putnam.—ED.

notes reached me, of an interesting character, which are not generally accessible to the majority of readers, with relation to the Chinese coal-fields, it has struck me that a portion of these details, in an abridged form, might be just now acceptable, particularly as the intercourse with that country is on the increase. I venture even to omit, for the present, the authorities for the facts I shall have to communicate; reserving them in detail for the volume adverted to. It must, nevertheless, be premised that to the Jesuit Fathers, the French Missionaries who were permitted to reside at Pekin during the 18th and preceding centuries, we are indebted for details of the highest interest, not alone on this subject, but on many other objects of philosophical inquiry in that little-known region.

It is probable that coal was discovered, and was in common use in China, long before it was known in the western world. It is mentioned by a noble traveller of the 13th century, as abounding throughout the whole province of "Cathay," of which Pekin is the capital, "where certain black stones are dug out of the mountains, which stones burn when kindled, and keep alive for a long time, and are used by many persons, notwithstanding the abundance of wood."

The good missionaries were fully capable of describing the coals which were supplied to Pekin, since they there erected a furnace or stove, in which they experimented on the properties of those combustibles; particularly with reference to the ordinary domestic uses, and for the warming of apartments and the purposes of their laboratory.

Among the people of Pekin three kinds are in use.

1. That employed by the blacksmiths. It yields more flame than the other qualities; is more fierce, but is subject to decrepitate in the fire; on which account, probably, the blacksmiths use it pounded in minute particles.

2. A harder and stronger coal, used for culinary purposes, giving out more flame than the other sorts so employed; it is less quickly consumed, and leaves a residuum of gray ashes. There are several gradations of these. The best are hard to break, of a fine grain, a deep black colour, soiling the hands less than the others. It sometimes is sufficiently siliceous to give fire with steel. Others have a very coarse grain, are easily broken and make a bright fire, leaving a reddish ash. Another species crackles or decrepitates when first placed on the fire, and falls down, almost entirely, in scales, which close the passage of the air, and stifle the fire.

3. A soft, feebly burning coal, giving out less heat than the 2nd class; consuming more quickly, it breaks with greater facility, and in general is of deeper black than the sorts previ-

ously mentioned. It is commonly this description which, being mixed with coal-dust and a fourth part of clay, is employed to form an artificial is œconomical fuel. This being moulded in the form of bricks and balls is sold in the shops of Pekin. Wagon-loads of coal-dust are brought to that city for this sole purpose.

The coal merchants have also an intermediate quality between the classes 2 and 3.

We cannot in this place recite the numerous details which are furnished by these intelligent Fathers. Suffice it to add, that nearly the whole of the properties and applications are now in every-day use in the United States, and are familiar to all. They are, in fact, the natural results suggested by qualities possessed in common by the combustibles of remote parts of the same globe. Even the modern method of warming all the apartments of our dwellings, which we view as the result of superior practical and scientific investigation, was in use, with very little deviation, centuries ago by the Chinese. Many a patented artificial fuel compound both in Europe and America, has been in practical operation in China at least a thousand years.

4. ANTHRACITE.—Another description of coal abounding about thirty leagues from Pekin, but which was not then in such general use there as the other kinds, is called by the Chinese Che-tan. Che means a stone, but tan is the name they give to wood-charcoal. Therefore, according to the genius of the Chinese language, this compound word signifies a substance resembling or having the common properties of stone and charcoal. There can be little difficulty here in recognising the variety of coal which in our day has been denominated anthracite, a compound word of similar meaning.

The Chinese *glance coal* forms a remarkable exception to the unfavourable conclusion prevailing against Oriental coal; and, according to more recent authority than those we before cited, deserves to rank at the head of the list, in respect of its purity as a coke, although in specific gravity it does not come up to the character of the Pennsylvania or Welsh fuel; neither has it the spongy texture which contributes much to the glowing combustion of the latter.

So late as 1840, a Russian officer has described the coal formations of the interior, as occupying the western mountain range of China, in such abundance that a space of half a league cannot be traversed without meeting with rich strata. The art of mining is yet in its infancy among the Chinese; notwithstanding which, coal is thought to be at a moderate price in the capital. Anthracite occurs in the western range of mountains at about a day's journey, or thirty miles only from Pekin. The coal formation is largely developed, in which thick beds of coal occur. They appear to be of various qualities. Some of this coal, occurring in shale beds, is singularly decomposed, and its particles have so little cohesion, that they are almost reduced to a state of powder. Beneath these coal shales are beds of ferruginous sandstone, and below those occur another series, consisting of much richer seams of coal than the upper group.

In this range are seen also both horizontal and vertical beds of conglomerate, accompanied by seams of coal which have the conglomerate for the roof and diorite or greenstone for the floor. As might be expected, this coal very much resembles anthracite. It is shining, of compact texture, difficult to ignite, does not flame in burning, or give out any smoke. Its substance is entirely homogeneous. Every thing respecting it leads to the belief that there had been a great development of heat at the period of its formation, or subsequently. The horizontal coal beds are the most important and valuable, and are denominated large; but no greater thickness than three and a half feet is quoted. The blacksmiths and those who work in copper, prefer this coal, on account of the intense heat which it gives out.

Throughout the whole of this mountain range may be continually seen the outcrops of this combustible, where they have never, as yet, been touched by the hand of man.

In those parts of China where wood is very dear, coal is worked on a great scale for the Pekin market: but the process of mining is very little understood by those people, who excel in the preparation of charcoal.

Coal in other parts of China.—The Missionaries and others inform us that coal is so abundant in every province of China, that there is perhaps no country in the world in which it is so common. The quays at Nankin are stored with the finest native coal. Some of the coal which was brought down to the coast, from the Pekin country, to the Gulf of Pe-tchee-lee, was anthracite, partaking of the character of plumbago or graphite. Coal, apparently of the brown coal species, exists extensively in the direction of Canton; while all the coals seen on the Yang-tse-kiang river, south of Nankin, resembled cannel coal. Nearer to Canton it possessed the comparatively modern character of the brown coal. It was abundantly offered for sale in the different cities through which Lord Amherst's embassy passed, between the lake Po-yang-how and Canton, and the boats were largely supplied with it. It is there obtained by means of pits, like wells; and we infer that, like nearly all the brown coal deposits, the beds were horizontal, and at no great depth. A sulphurous coal, interstratified with slate, and in the vicinity of red sandstone, also prevails towards Canton.

Thus, therefore, we possess evidence, the main object which this communication was designed to exhibit, that extending over large areas in China, are beds of tertiary or brown coal, of cannel coal, a dozen varieties of bituminous coal, of anthracite, glance coal, and graphitic anthracite; all of which, for ages, have been in common use in this remarkable country, and have been there employed for every domestic purpose known to civilized nations of all times; including gas lighting, and the manufacture of iron, copper, and other metals.

Mode of Mining Coal in China.—It might be expected that in China, where most of the practical arts have from time immemorial been carried on with all the perseverance of that industrious people, the operations of mining coal would be conducted with some regard to science, in relation to sinking, draining, and extraction. We have, however, good authority, especially in regard to the environs of Pekin, for stating that the process is still in a very imperfect state. Machinery to lighten labour is there unknown. They have not even an idea of the pumps indispensable to draw off the water. If local circumstances allow, they cut drainage galleries; if not, they abandon the work whenever the inundation has gained too far upon them. The mattock and shovel, the pick and the hammer, are the mining instruments—the only ones, in fact, which the Chinese employ in working the coal. The water of the mine is emptied by the slow process of filling small casks, which are brought up to the surface by manual labour. Vertical shafts are not used. In working horizontal coal seams, the timbering is expensive, and the materials cost about two copecs per poud, = $8,50 per ton, English wood being sold by weight in China.

The coal, when mined, is put into baskets and drawn upon sledges, which are raised to the surface by manual strength. Each basket contains about three pouds of coal, and one man can raise about eight baskets in a day. This is equivalent to 1032 Russian pounds, or to 12 cwt. English per day. The miners' wages are at the rate of 30 copecs a basket; which is equal to 240 copecs (copper currency), or 46 cents of United States currency, per day; being $0,76 U. S. per ton.

Prices at Pekin.—At the pit's mouth, this coal is sold for 60 copecs per poud, = $4,63 per ton of 20 cwt. It is then conveyed on the backs of mules, through the mountains, and thence on camels to Pekin, where the price is 1½ rouble,

=1¼ franc,=29 cents United states, per poud; which, if our calculation be correct, is equivalent to $11,60 United States, or £2 8*s.* 3*d.* per ton of 2240 pounds English. We perceive, therefore, that the best of fuel is expensive at Pekin, and hence the necessity for resorting to the artificial compounds and substitutes to which we briefly alluded.

There is, however, a kind of coal sold in that city at a much lower price, particularly when it is mixed with one-half of coal-dust. This coal, in 1840, sold for one rouble per poud, which is at the rate of $7,75,=£1 12*s.* 3*d.* per ton. It is of indifferent quality, however; giving out but little heat, and is quickly consumed.

The compound fuel, consisting of coal-dust and clay, is still prepared after the mode described by the Missionaries last century; but its use is chiefly confined to the indigent classes.

Coal Gas Lighting in China.—Whether, or to what extent, the Chinese artificially produce illuminating gas from bituminous coal, we are uncertain. But it is a fact that spontaneous jets of gas, derived from boring into coal-beds, have for centuries been burning, and turned to that and other œconomical purposes. If the Chinese are not manufacturers, they are, nevertheless, gas consumers and employers on a large scale; and have evidently been so ages before the knowledge of its application was acquired by Europeans. Beds of coal are frequently pierced by the borers for salt water; and the inflammable gas is forced up in jets twenty or thirty feet in height. From these fountains the vapour has been conveyed to the salt-works in pipes, and there used for the boiling and evaporation of the salt; other tubes convey the gas intended for lighting the streets and the larger apartments and kitchens. As there is still more gas than is required, the excess is conducted beyond the limits of the salt-works, and there forms separate chimneys or columns of flame.

One cannot but be struck with the singular counterpart to this employment of natural gas, which may be daily witnessed in the Valley of the Kanawha, in Virginia. The geological origin, the means of supply, the application to all the processes of manufacturing salt, and of the appropriation of the surplus for the purposes of illumination, are remarkably alike at such distant points as China and the United States. Those who have read, even within the present month, the account of the recent extraordinary additional supply of gas, and the services it is made to perform at the Kanawha salt-works, must be impressed with the coincidence of all the circumstances with those which are very briefly stated in the previous paragraph in relation to China. In fact the parallel is complete.

To the coals and combustible minerals of China, I cannot further advert here. But what a conviction irresistibly presses upon the mind, as to the incalculable utility of the *railroad system*, and coal-mining improvements, in such an empire! If ever there were concentrated at one point all the circumstances especially and unequivocally favourable to that system, and imperiously calling for improvements of the character suggested, it seems to be presented in the case of the city of Pekin. Here, with its enormous population of 1,500,000 souls, it is situated only at a day's journey—computed at thirty miles—from an immense region of coal, comprising several varieties. Yet its inhabitants cannot purchase the best qualities of this coal, brought from the mountains on the backs of mules and camels, under $11,60 per ton, and the very worst for less than $7,75 per ton.

Without making unnecessary or invidious comparisons, it might not unreasonably be suggested, that a Pekin railroad, in connection with the coal mines, would be a far more profitable enterprise in its results, than the transportation of American coals to China.

I will only add one circumstance, which had nearly escaped me. Borneo, "the largest island in the world," which is only twenty degrees due south of Canton, has lately come into repute for the great quantity of coal which it contains, not only accessible to ships along the coast, but extensively occurring in the mountains of the interior. Much information has also been acquired from the natives; and the facts which are already elicited are regarded as of considerable importance, in respect to the facilitating the steam navigation in the China seas. Philadelphia will, of course, have her share in the enlarged commercial intercourse with China. Would it, then, be asking too much of those who are personally interested in this improving trade, to communicate any additional facts, which are either unknown to, or have been omitted by, the author of these scanty notes?

Respectfully,

Philadelphia, April 28, 1845. RICHARD C. TAYLOR.

NOTE.—The prices and admeasurements which are quoted in the foregoing article, were reduced to the United States and English currencies and measures from the Russian, as furnished by the Engineer Kovanko; who, in like manner, converted them into the Russian from the Chinese standards. In consequence of this triple conversion of standards, additional care has been taken to avoid error in these calculations.

XXXVIII. *On the Conversion of the solid Ferrocyanide of Potassium into the Sesqui-ferrocyanide.* By C. F. SCHŒNBEIN[*].

IN a former notice I have shown that a solution of the yellow prussiate of potash in water, placed in contact with an atmosphere of ozone, instantaneously destroys the latter, and is converted into the red sesqui-ferrocyanide. Since that communication was made I have ascertained that even the solid yellow salt very readily absorbs ozone and is changed into the red one. If a crystal of the common prussiate is suspended in a balloon containing an atmosphere strongly charged with ozone, and kept in that state by means of phosphorus and water, it will soon assume the colour peculiar to the red cyanide, just in the same way as it would do when held in air containing chlorine. The surface of the crystal, after having remained in the ozonized air for about twelve hours, is changed into the red salt, which may be easily separated from the yellow nucleus by mechanical means. A crystal of about a cubic inch in bulk appeared after thirty-six hours' suspension in ozonized air covered with a crust of the red cyanide, at least one line thick; and in another case I saw a smaller crystal of the yellow salt entirely converted into the red one. I hardly need say that by changing the yellow compound into the sesqui-ferrocyanide, the cohesive state of the former undergoes a material alteration. The red crust surrounding the yellow nucleus is rather brittle, and consists of a heap of small crystals of the sesqui-ferrocyanide. It is worthy of remark, that under the circumstances mentioned the yellow prussiate becomes moist, and exhibits in that state a very strong alkaline reaction.

XXXIX. *On the Decomposition of the Yellow and Red Ferrocyanides of Potassium by Solar Light.* By C. F. SCHŒNBEIN[*].

A SOLUTION of the yellow prussiate of potash kept in the dark does not change its colour, but when exposed to the action of solar light it becomes of a deeper yellow. To render that change very perceptible, a weak, *i. e.* nearly colourless, solution must be used, in which case the liquid will assume a yellow colour after having been acted upon by strong sunlight only for a few minutes. If the bottle containing the solution be closed and not quite filled with the liquid, an odour of prussic acid is perceptible, and at the same time

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[*] Communicated by the Author.

a reddish yellow sediment subsides, which seems to be the peroxide of iron. The decomposition of the cyanide takes place much more rapidly when strips of filtering paper or linen are immersed in a solution of the salt and exposed to the action of solar light. In a very short time that part of the strip turned towards the sun becomes yellow, whilst the opposite side remains colourless, or nearly so. If strips of paper moistened with the solution of the common prussiate of potash are closed up in glass bottles containing air, they also turn yellow by exposure to the sun, and a strong smell of prussic acid is perceptible in the vessels after a short time. In the shade no such action takes place. A large piece of linen cloth drenched with a solution of the yellow salt, after having been exposed in the open air to the action of solar light for thirty-six hours, had turned deeply yellow, and yielded, when treated with distilled water, a deep yellow solution, which on being filtered and heated to boiling became turbid, and deposited flakes of peroxide of iron. The same solution exhibited a stronger alkaline reaction than the solution of the common prussiate does. From the facts stated, it appears that the yellow ferro-cyanide is decomposed by light into prussic acid, oxide of iron and potash, and a compound formed yielding with water a yellow solution. Is that compound carbonate of potash and peroxide of iron; and do the constituent gases of the atmosphere take part in the decomposition besides the solar light? Further experiments must answer those questions. A limpid solution of the red cyanide also becomes turbid when exposed to the action of solar light, prussic acid being evolved and peroxide of iron thrown down.

XL. *A Reference to former Contributions to the Philosophical Magazine, on Physical Optics. By* Prof. POTTER, *A.M., F.C.P.S., late Fellow of Queen's College, Cambridge, &c.*[*]

WITHOUT the slightest wish to interfere in the controversies of others, I now beg to refer the readers of the Philosophical Magazine to my papers in the Magazines for January 1840 and May 1841. In the former, at page 20, I have shown Mr. Green's formula for the intensity of reflected light to fail entirely as a representation of nature; and in the latter I have shown the peculiar refraction near the optic axes of biaxal crystals not to be represented by Sir William Hamilton's analytical deductions from Fresnel's equation to the wave surface in biaxal crystals.

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[*] Communicated by the Author.

The anonymous correspondent Jesuiticus in the last Number, refers to those analytical researches triumphantly in favour of the undulatory theory of light. I do not write to disturb the philosophical opinions of Jesuiticus, but to remind the readers of the Magazine where they will find the discussion of the points referred to.

XLI. *On Differentiation as applied to Periodic Series: with a few Remarks in reply to* Mr. Moon. *By* J. R. Young, *Professor of Mathematics in Belfast College*[*].

If in the general expression at p. 430 of my paper on Periodic Series, in the last volume of this Journal, A be made equal to -1, we shall have the identity

$$\frac{1}{2}=\cos\theta-\cos 2\theta+\cos 3\theta-\&c.\pm\frac{\cos(n+1)\theta+\cos n\theta}{2(1+\cos\theta)};$$

and if we multiply this by $d\theta$, and integrate, we shall further have

$$\frac{\theta}{2}=\sin\theta-\frac{1}{2}\sin 2\theta+\frac{1}{3}\sin 3\theta-\&c.$$
$$\pm\int\frac{\cos(n+1)\theta+\cos n\theta}{2(1+\cos\theta)}d\theta.$$

Now it is demonstrable, from other and independent principles, that, when n is infinite, the right-hand member of this equation, omitting the integral, is the true development of $\frac{\theta}{2}$, for all values of θ not exceeding π. Hence we may infer that, for $n=\infty$, this integral is necessarily zero. If we suppress it therefore, we shall commit no error in the expression for $\frac{\theta}{2}$; but a very considerable error will be introduced if we attempt to derive from that expression, thus limited to the particular case of $n=\infty$, a series of other equations, by the aid of differentiation, as is commonly done. If the evanescent integral be restored, we may then apply the process of differentiation as far as we please: our resulting equations will all be identical equations; holding, whatever be the value of n, and supplying the necessary corrections of those erroneous developments which, in the case of $n=\infty$, are so commonly met with in analysis.

I have elsewhere observed that differentiation fails to be applicable to the series

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[*] Communicated by the Author.

$$\frac{\theta}{2} = \sin\theta - \frac{1}{2}\sin 2\theta + \frac{1}{3}\sin 3\theta - \frac{1}{4}\sin 4\theta + \ldots.$$

in the isolated case of $n = \infty$; and it is plain that in this case $\frac{1}{n}\sin n\theta$ is 0, though $\sin n\theta$ is itself indeterminate; the indeterminateness is therefore rendered nugatory. But if differentiation be allowed, this indeterminateness reappears in $\cos n\theta$, in the right-hand member of the result, though the left-hand member remains determinate, which is absurd. Still we are not precluded from applying differentiation to the general forms above, since these are universally true; they comprehend *all* values of n, and are *identical*. It is in virtue of this identity, and of this alone, that the results of differentiation may safely be extended to $n = \infty$, although for this isolated value of n differentiation be inapplicable.

I have very little to say in reference to Mr. Moon's attack in the last Number of this Magazine. The papers which have called it forth,—whether justly or not, I leave others to determine,—Mr. Moon confesses that he does not understand; and humiliating as such a confession may seem, the whole tenor of his remarks shows that he is sincere.

I beg to say, that I did not write expressly for Mr. Moon, and Mr. Moon therefore cannot reasonably expect that I should attend to his demand, and define the terms I use. I have employed nothing but the recognised language of analysis, and I cannot undertake to encumber the pages of this Journal with a glossary of scientific terms for Mr. Moon's especial benefit; if he will only take the trouble to turn to the Penny Cyclopædia, Mr. De Morgan will fully instruct him in all these things.

The occasion of my mentioning Mr. Moon's name was this: —I found Mr. Moon, in the June Number of this Magazine, floundering amidst difficulties which he showed himself unable to cope with. I had long previously contemplated a paper, of which the main object was to remove those difficulties, and in drawing it up for this Journal, I could not well avoid the mention of Mr. Moon's name. But I mentioned it with the most scrupulous courtesy and respect; I was especially anxious on this point, on account of the peculiarities which Mr. Moon had so often displayed in his published communications; so anxious indeed was I to avoid offence, that—at the risk of losing all credit for discrimination—I even went the length of calling him "an able contributor to this Journal!" As I have already said, I did not certainly write expressly for Mr. Moon, but the instruction conveyed to him through my short papers, was precisely that of which he obviously stood in need. Instead of accepting this with thanks, he ungratefully turns round and bites the hand that brings him aid; and, not content with this, he is ungenerous enough, and unjust enough, to say, that everything in those papers, which is not erroneous, has already been given by himself!

Belfast, February 9, 1846. J. R. Young.

[We omit the remainder of Mr. Young's letter, in which he animadverts upon Mr. Moon in terms which the communications of the latter seem well-calculated to provoke. The same discretion has been exercised with regard to some parts of Mr. Moon's letter in the present number, as from the character which the controversy has assumed, we are not disposed to devote any more of our space to its continuance.—Edit.]

XLII. Mr. Moon *in Reply to* Jesuiticus*.

AFTER the notice which appeared in the last Number of this Journal respecting his previous papers, there would be an obvious impropriety in the writer of the following remarks attempting to force on the Editors of the Magazine any matter which would tend to produce further discussion† on the subject to which he has of late called attention, except so far as he be driven to do so in self-defence. As, however, the views to which the Editors afforded the means of publication have been openly attacked in this Journal, their author conceives he has a right to say a few words in their behalf.

An anonymous writer, who subscribes himself Jesuiticus, commences certain animadversions on my first paper on Fresnel's Theory of Double Refraction, by the remark, that "the hypothesis on which Fresnel's Theory of Double Refraction is based is the following:—'That the displacement of a molecule of the vibrating medium in a crystallized body is resisted by different elastic forces according to the different directions in which the displacement takes place.'"

He then proceeds to make some remarks on the reasonableness of this hypothesis, which it is not my present purpose to dispute; but I must beg to observe, *en passant*, that the above is *not* the hypothesis on which Fresnel's Theory of

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* Communicated by the Author.

† We omit some portions of Mr. Moon's communication, where he appears to us to have lost sight of his declared purpose of confining himself to self-defence, and has introduced matter "tending to produce further discussion."—Edit.

Double Refraction is based. It rests on a lower level still. The true basis of the theory is, that the æthereal medium consists of particles separated by finite intervals (to use a well-known, but improper mode of expression), and acting upon each other by their mutual attractions. From this principle, the so-called fundamental hypothesis of Jesuiticus is a sufficiently easy inference: I have thought it necessary to remark upon this inaccuracy, however, as from the extraordinary want of precision of the writers on this subject, it is somewhat difficult to say what is their real starting-point; at the same time, that in order to make a proper estimate of Fresnel's theory, and of the skill and judgement with which he has worked it out, it is very desirable that that fact should be clearly ascertained.

Jesuiticus afterwards goes on to say, "It is then proved, that if any particle of the æther be suddenly displaced, *the other particles remaining quiescent*, the force of restitution developed by such disturbance will not in general be in the direction of the displacement, but only when such displacement is in the direction of the aforesaid axes of elasticity. The elegant demonstration of Mr. Smith, quoted by Mr. Moon, is by Mr. Moon's own showing fully adequate to establish the theorem as I have enunciated it, which is doubtless the sense in which Fresnel conceived it."

I admit that Mr. Smith's demonstration is fully adequate to establish the theorem as Jesuiticus has enunciated it, but I must beg to assure Jesuiticus, that unless the demonstration establishes a great deal more than the theorem so enunciated, it is not, for the purpose for which it is adduced, worth the paper it is written upon. What is the use of considering the impossible case of a single particle suddenly disturbed while all the other particles remain quiescent, and then reasoning upon what takes place in the *beginning of the motion* in *that case*, as if the same held good *throughout the whole motion* in the *actual case*, when all the particles are vibrating together, when it is perfectly certain that it does not?

Jesuiticus says, "Any one who understands the subject must at once acknowledge that any theory of light must be, to a considerable extent, imaginative; and that theory which can explain the greatest number of facts ought to claim the attention of the philosopher more than any other." Of the justice of the remark contained in the first part of the above sentence, Fresnel's theory is no doubt a remarkable confirmation; in the sentiment of the second clause of it I am disposed to concur, with the reservation that some portion of the credit due to a theory depends on its antecedent probability. But mark what follows:—"It is to this that the undulatory theory owes its great celebrity, and of all parts of the undulatory theory, that of double refraction is the most extraordinary. It ought to be regarded as a stupendous monument of human ingenuity. It must not be forgotten how admirably the properties of uniaxal crystals follow from the general investigation of the biaxal class; but above all, how from this same investigation, conical and cylindrical refraction were discovered by Sir William Hamilton."

I would ask of Jesuiticus, what is the hypothesis upon which Fresnel professes to explain the separation of the ray? Whether it is not substantially what I have stated it to be in the early part of this paper? And if so, I appeal to the world whether I have not shown incontrovertibly in my two papers on this subject contained in the last two Numbers of the Philosophical Magazine, that Fresnel entirely fails to explain the separation of the ray on that hypothesis. It may be true that some of Fresnel's expressions for the disturbance in doubly refracted and other polarized waves may involve in them certain elements of truth (though for my own part I should be sorry to answer for any of them); but they do not on that account afford any evidence of the truth of his principles, for this plain reason, that they do not follow from them. It may happen, that from the ruins to which this great theory must soon, if it be not already reduced, may be gathered some useful fragments which may form part of a new and more durable edifice; but Jesuiticus may take my word for it, or if he do not choose to do that, he will not have long to wait for the verification of the prediction, that the time is at hand when Fresnel's theory will be considered as a "stupendous monument" of anything else but ingenuity. As to the supposed discoveries of conical and cylindrical refraction, if Jesuiticus had been aware of their very doubtful character, he would hardly have ventured to have brought them so prominently forward.

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As to the investigation which I examined in the first of my two papers, I do not doubt that Mr. Airy considered it merely as an illustration; but even in that point of view, and without adverting to the error which I pointed out in his reasoning, it would be entirely worthless, as it is obvious that the state of things he contemplates could only exist for a single moment, whereas the results he deduces are supposed to be always subsisting. His object is to show that an undulation consisting of transversal vibrations might be propagated according to a certain law, when even on his own premises it is quite obvious that if the disturbance originally communicated were of that character, it would immediately cease to be so, or in short, that a transversal undulation (if I may be permitted the expression) would not be propagated according to any law. With a full sense of the value of Mr. Airy's contributions to other departments of science, I cannot shut my eyes to the fact, that by allowing such investigations as the one under consideration (which but for its adopted parentage would not be worth a comment) to pass not merely without censure, but with apparent sanction, he has introduced an absence of precision,—a laxity of principle (so to speak) into mathematical inquiries, which has produced the most injurious effects both in the mixed and the pure sciences.

But to come to the error which Jesuiticus imagines he has found in my reasoning. He says, "that in substituting for u,

$$u - \frac{d\,u}{d\,x} h + \frac{d^2 u}{d\,x^2} \frac{h^2}{1\,.\,2} - \&c.$$

and for u',

$$u + \frac{d\,u}{d\,x} h + \frac{d^2 u}{d\,x^2} \frac{h^2}{1\,.\,2} + \&c.$$

the substitutions stopping at h^2, merely require that h should be small in comparison with the *length of a wave*, not in respect to u."

It is true that if we suppose the initial disturbance to be represented by $\alpha \sin \frac{2\pi}{\lambda}(v\,t - x)$, the substitutions stopping at h^2 are defensible on the ground suggested by Jesuiticus; but does Jesuiticus conceive that when Mr. Airy wrote out this demonstration, he ever thought about the length of the wave, or any other circumstance connected with the initial vibration? If he does, I can only say that he is a very extraordinary person. In my paper I took Mr. Airy's investigation for what it purported to be, namely a proof that a certain hypothesis as to the disposition of the particles and the nature of their mutual action, without reference to the form of the initial disturbance, leads to the conclusion that transversal undulations may be propagated; and in that point of view I have no hesitation in saying it entirely fails; and, independently of all others, on the ground I have pointed out, *i. e.* of false approximation. If Jesuiticus has any doubt as to whether Mr. Airy did or did not consider himself to have proved the proposition generally, I would recommend to his attention Art. 127 of Mr. Airy's Tract, in which he takes the general integral of the equation

$$\frac{d^2 u}{d t^2} = \frac{1}{n}\left(1 - \frac{1}{2^{\frac{n}{2}}}\right)\frac{m}{h}\frac{d^2 u}{d t^2},$$

to wit, $u = \phi\,(r\,t - \mu\,x)$,

which he would not have been justified in doing if he had not proved the general proposition. * * * *

Liverpool, February 10, 1846.

XLIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 15, 1846. "ON the Viscous Theory of Glacier Motion." By James D. Forbes, Esq., F.R.S. &c. Part II*. "An attempt to establish by observation the Plasticity of Glacier Ice."

The two first sections of the present memoir are occupied with a critical examination of the theory advanced by De Saussure to account for the progressive motion of glaciers, which he considered as formed of masses of rigid and inflexible ice, and with the further explanations of that theory given by Ramaud, Bischoff, Agassiz, and Studer. The author, on the other hand, regarding these masses as possessing a considerable degree of plasticity, explains on that supposition the phenomena they present; and, in the third section of the paper, he relates a series of experiments which he carried on in the Mer de Glace, near Chamouni, in the summer of 1844, with a view to determine by direct measurement the relative motion of different parts of the glacier. This he accomplished by selecting a spot on the western side of the Mer de Glace, between Trelaporte and l'Angle, where the ice was compact and free from fissures, and erecting on the surface a row of posts at short distances from one another, in a line transverse to the general direction of the moving mass. He was thus enabled to discover by trigonometrical observations the movements of different points in this line; and he ascertained that they advanced more and more rapidly in proportion as they were distant from the sides of the glacier; and that when not under the influence of neighbouring *crevasses*, these motions were gradual and uninterrupted; as was shown by the lines carried through the posts forming, after the lapse of a few days, a continuous curve, of which the convexity was turned towards the lower end of the glacier.

"An Account of the Southern Magnetic Surveying Expedition." By Lieut. H. Clerk, R.A., in a letter to Lieut.-Colonel Sabine, R.A., F.R.S. Communicated by Lieut.-Colonel Sabine.

The letter, which is dated from the Magnetical Observatory at the Cape of Good Hope, June 28, 1845, reports the return to the Cape of the Pagoda from her voyage to the high southern latitudes after the successful completion of the magnetical service on which she had been employed by direction of the Lords Commissioners of the Admiralty, at the request of the President and Council of the Royal Society.

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* An Abstract of Part I. will be found at p. 538, vol. xxvi., of this Journal.

January 22.—"On the Supra-renal, Thymus and Thyroid Bodies." By John Goodsir, Esq. Communicated by Richard Owen, Esq., F.R.S. &c.

In this paper, the author enters on the developement of the theory he advanced two years ago with regard to the origin and nature of the supra-renal, thymus and thyroid bodies, and the correctness of which, with certain modifications, he has been enabled to confirm by subsequent observation and reflection. His hypothesis was that the three organs in question are the remains of the blastoderma; the thyroid being the developement of a portion of the original cellular substance of the germinal membrane grouped around the two branches of the omphalo-mesenteric vein; the supra-renal capsules, the developements of other portions grouped around the omphalo-mesenteric arteries; and the thymus, the developement of the intermediate portion of the membrane arranged along the sides of the embryonic visceral cavity. He has since ascertained, however, that the thyroid body derives its origin in a portion of the included *membrana intermedia* remaining in connexion with anastomosing vessels between the first and second aortic arches, or carotid and subclavian vessels. He considers these organs as essentially similar in their structure, as well as in their origin in continuous portions of the blastoderma situated along each side of the spine, and extending from the Wolfian bodies to the base of the cranium: the developement of the supra-renal capsules having relation to the omphalo-mesenteric vessels; the thymus, to the jugular and cardinal veins and ductus Cuvieri; and the thyroid gland, to the anastomosing branches of the first and second aortic arches. The functions of these organs he regards as being analogous to those of the blastoderma; with this difference, however, that as the blastoderma not only elaborates nourishment for the embryo, but absorbs it also from without, that is, from the yolk, the developed organs only elaborate the matter which has already been absorbed by the other parts, and is now circulating in the vessels of the more perfect individual.

January 29.—"On the Use of the Barometric Thermometer for the determination of Relative Heights." By James R. Christie, Esq. Communicated by S. Hunter Christie, Esq., Sec. R.S., &c.

The objects of this communication, as stated by the author, are, first, to show the theoretical foundation of the very simple law pointed out by Professor Forbes, according to which the difference of the boiling temperature of water at two stations is connected with their difference of level; and next, to test the accuracy of this law by a comparison of results deduced from his own observations on the boiling-point of water at different stations among the Alps of Savoy, Piedmont and Switzerland, with the heights of the same stations as determined by other observers and by different means; and thus to arrive at a just conclusion with respect to the value of the barometric thermometer as an instrument for determining differences of level.

Combining DeLuc's formula reduced to English units,

$$b = \frac{99}{\cdot 899} \log 10\beta - 60\cdot 804,$$

where b is the variable boiling-point on Fahrenheit's scale and β the corresponding barometric pressure, with the formula of Laplace for the determination of the difference in level of two stations from barometric observations, he obtains the formula

$$H = 547{\cdot}99\ (b - b')\ \left\{1 + (t - 32^\circ)\ {\cdot}00222\right\},$$

where b and b' are the boiling-points on Fahrenheit's scale at the two stations, t the mean temperature of the air at the stations, and H their difference of level in English feet.

The author describes the particular instrument he employed in his observations, and his mode of determining the correction which it required; and then gives, in a table, the observations he made on the boiling-point of water at thirty-eight different stations in the Alps; the heights of the corresponding stations above the sea level, deduced from these observations; and, for the purpose of comparison, the heights of the same stations deduced by other observers. The difference between these and some of the author's results are considerable; but as they are not greater than would probably arise from ordinary barometric measurements, and as there is a close accordance between his results and the determinations on which the greatest reliance can be placed, he concludes that the results are on the whole satisfactory. Considering it, however, desirable to obtain some test of the accuracy of each observation independently of the rest of the series, the author avails himself of the barometric observations made at the Observatory at Geneva and at the Convent of the Great St. Bernard; and determining from these the corresponding temperature of boiling water, deduces the difference of level between each of his stations and these two places considered as fixed points: the sum of the height above Geneva and the depression below the Great St. Bernard should in all cases be the difference of level between the two fixed stations. Although there are here again considerable discrepancies, yet in most cases, where the height of the station may be considered as well-established, the height deduced from the observations agrees with it in a very remarkable manner.

In another table, the author gives the difference of level between the Observatory at Geneva and the Great St. Bernard, deduced from the recorded observations at those places simultaneous with his own at his various stations; and then remarks that the differences of height determined by the two methods do not differ from one another, in any single case, by so large a quantity as do the greatest and least differences of height deduced from the barometric observations; while in many cases the accordance is almost perfect.

The conclusion drawn from the comparisons in these tables is, that the barometric thermometer is capable of affording highly accurate and satisfactory results, perhaps even more so than the common form of barometer, but that there is considerable uncertainty attached to its indications. This uncertainty, far from being wholly attributable to the imperfections of the instrument as a measure of the atmospheric pressure, might, the author thinks, arise from an extreme susceptibility to rapid changes in that pressure, which remain unindicated by the more sluggish barometer.

"On the Decomposition and Analysis of the Compounds of Ammonia and Cyanogen." By Robert Smith, Esq., Ph. D. Communicated by Captain William Henry Smyth, R.N., F.R.S.

This paper is divided into four parts; the first relates to the decomposition of ammonia and its compounds; by the compounds of chlorine, and the collection and measurement of the nitrogen gas which is disengaged, the amount of which the author considers as furnishing a ready and accurate mode of estimating the quantity of ammonia in the solution subjected to analysis. The chloride of lime was the salt usually employed for this purpose: this method is regarded by the author as being peculiarly applicable to the analysis of organic substances.

The second part treats of the decomposition and estimation of hydrocyanic acid and its compounds by means of chloride of lime, yielding nitrogen gas and carbonate of lime; a process which occupies but a few seconds. In some cases, the employment of chloride of soda is preferable to that of chloride of lime, on account of the solubility of all the compounds that are formed. The author found the same method applicable also to the analysis of the salts of cyanogen; for the cyanides of the alkalies are decomposed by it as rapidly as the pure acid itself. The ferro-cyanides are also very readily decomposed.

The author, in the third part of his paper, relates the results of his trials of the hypochlorites as agents for the decomposition of uric acid, which proved so satisfactory as to induce him to believe that these salts might be advantageously used as solvents of uric calculi in the living bladder. He also proposes the employment of chloride of lime as a ready and accurate mode of estimating the quantity of nitrogen contained in urine, from the amount of gas disengaged by its action on the nitrogenous compounds. In the last part, the apparatus used in the experiments is described.

"On a point connected with the dispute about the invention of Fluxions." By Augustus De Morgan, Esq., M.A., F.R.A.S., &c. Communicated by Samuel Hunter Christie, Esq., Sec. R.S., &c.

An assertion made by Sir Isaac Newton in a letter to Conti, published in Raphson's History of Fluxions, that the materials of the *Commercium Epistolicum* were "collected and published by a numerous Committee of gentlemen of *different nations*, appointed by the Royal Society for that purpose," appeared to be at variance with the list of the Committee as it was appointed on the 6th of March, 1711–12, and which only contains the names of Arbuthnot, Hill, Halley, Jones, Machin and Burnet, who were all English. But on further search of the records of the Society with the aid of Mr. Weld, the Assistant Secretary, the author ascertained that other members were subsequently added to the Committee, among whom were Bonet, the Prussian minister, and De Moivre, both of whom were foreigners; thus showing that the imputations which might have been cast on Newton's veracity are groundless.

February 5, 1846.—"On the Secretory Apparatus and Function of the Liver." By C. Handfield Jones, M.D. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

The author is led by his researches into the minute structure of the liver, to results which confirm the view of Mr. Bowman, in opposition to those of Mr. Kiernan on this subject: and particularly with regard to the absence of real tubercular ducts from the interior of the lobules. He concludes that the secreting process commences in the rows of epithelial cells surrounding the central axis of the lobule, and that the fluid there secreted is transmitted to the cells forming the margin of the lobule, where it is further elaborated, and, by the bursting of these cells, is conveyed into the cavity of the surrounding duct. A few diagrams are annexed, illustrative of the descriptions of microscopic structure given in the paper.

"An Account of some Experiments on the Electro-Culture of Farm Crops." By Mr. William Sturgeon. Communicated by S. Hunter Christie, Esq., Sec. R.S., &c.

Grass grown on a parallelogram of land, fifty-five yards long by twenty-two yards wide, enclosed by underground wires, was found to be much more abundant than in any other part of the field; especially in a plot "upwards of fifty yards long, whose breadth was within the wires, and nearly at right angles to the axis of the parallelogram." This plot of grass was principally on the western side of the wires, and extended but a very little way on the eastern side. The axis of the wire-enclosed parallelogram was in the magnetic meridian.

"On the Comet of 1844–45." By John Collingwood Haile, Esq. Communicated by Charles Terry, Esq., F.R.S.

The author gives a series of observations, accompanied by a diagram, made by him at Auckland, in New Zealand, on the comet of 1844–45, which there appeared on the 20th of December 1844 and disappeared on the 30th of January following, having been visible forty-two days. Its most remarkable feature was that during its greatest brilliancy, the nucleus was not surrounded by the nebulous matter, but was situated at the very extremity of the head, and at times even appeared quite detached.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxvii. p. 307.]

June 13, 1845.—Observations of the Solar Eclipse of 1845, May 5, and of the Transit of Mercury of 1845, May 8, in a Letter from W. Lassell, Esq.

"Starfield, Liverpool, 10th June, 1845.

"I send you such observations as I have been able to make of the late solar eclipse and transit of Mercury, for which the weather was, in some respects, but very unfavourable.

"May 5, 1845. With a very unpromising sky I prepared to observe the first contact of the solar eclipse, by placing that part of the sun's limb (then very indistinct) which the moon would first touch, between the parallel threads of the micrometer, applied to the nine-feet equatoreal, with a power of ninety-six times.

"As the time of contact approached the sky somewhat cleared, and the moon's first impression took place at 23h 14m 37s·8 sidereal time, or 20h 18m 24s·17 mean time at the observatory. During the greatest part of the obscuration the sky was very cloudy, but towards the end it cleared, and the last contact was well observed at 1h 33m 48s·9 sidereal, or 22h 37m 12s·45 mean time. No phænomena beyond what is usual occurred; nor was there, to my senses, any perceptible diminution of light on the landscape.

"May 8. For the transit of Mercury the appearances a short time before it began were still more unpromising. During the forenoon we had several showers, with a most gloomy sky; and even as late as half-past three P.M. we had a smart shower of close, small rain. A little change for the better occurred shortly before four, and I had just time to set the micrometer by the sun's limb after he became visible, and get settled at the telescope, when the first notch was cut out by the planet. The sun's limb was beautifully sharp, but occasionally obscured by passing clouds. From the time, however, of the first impression until the planet had advanced about two of its diameters upon the disc of the sun, it was generally unclouded, and the atmosphere remarkably tranquil. The first contact took place at 7h 13m 36s·3 sidereal time, or 4h 8m 12s·36 mean time. The internal contact, or complete immersion of the planet, took place at 7h 16m 48s·7 sidereal, or 4h 11m 24s·24 mean time. Both times were carefully and, I believe, accurately noted. Whilst the planet was traversing the *edge* of the sun, an apparent distortion took place, the parts of the sun's edge, or limb, in contact with the planet, appearing *rounded off*; and a moment or two before the complete immersion of the planet, an appearance analogous to *Mr. Baily's beads* took place,—the planet apparently breaking contact two or three times with the sun's limb before the final separation occurred. Mercury had also, to my eye, somewhat of a pear-like shape previously to his entering quite within the sun's disc. When he had advanced two or three of his diameters, the clouds rapidly thickened, and I saw him no more.

"I take this opportunity of stating, that a late redetermination of the longitude of my Observatory depending upon the lately determined longitude of the Liverpool Observatory, inclines me to adopt finally 11m 47s·34 as my longitude west of Greenwich, which differs scarcely a quarter of a second from that given in my paper contained in the forthcoming volume of the Society's Memoirs.

"The latitude I have also redetermined lately by transits of seven stars over the prime vertical, giving 53° 25′ 3″·5 as the mean result."

Observations of the Transit of Mercury made at Aylesbury by Thomas Dell, Esq. Communicated by Dr. Lee.

"*Transit of Mercury.*—The first contact of the two limbs was invisible, the sun being obscured by heavy clouds; but almost immediately afterwards it broke through them, and the interior contact of the limbs was well observed at $4^h\ 18^m\ 33^s$ mean time. The sun was covered by light fleecy clouds, through which it was distinctly visible, and the discs of both the sun and Mercury were most beautifully and sharply defined until $5^h\ 12^m$, when the whole sky became densely overcast, and continued so until after sunset. The time of interior contact is, I believe, accurate to a second, as the error of the chronometer had been determined at the sun's transit at noon."

Mathematical Society.—After the conclusion of the business of the Ordinary Meeting, a Special General Meeting was held to take into consideration a subject, of which due notice had been given to the Fellows by the following circular:—

"Somerset House, June 5th, 1845.

"Sir,—I have the honour of notifying to you, that in pursuance of a Resolution of the Council, passed on Friday, the 23rd of May last, a Special General Meeting of this Society will be held at the Society's apartments on Friday, the 13th day of June instant, immediately after the business of the Ordinary Meeting to be held on that day is concluded, for the purpose of taking into consideration and deciding upon a recommendation of the Council to suspend upon that occasion the Bye-laws relative to the Election of Fellows, and to elect as Fellows of this Society the remaining Members of the Mathematical Society (now reduced to nineteen in number, of whom three are already Fellows), without payment of the usual Admission Fees and Annual Contributions (or compositions in lieu thereof), the Mathematical Society having announced its resolution to transfer its valuable Library, with its Records and Memorials, to the Royal Astronomical Society.—I have the honour to be, Sir, your most obedient servant,

"Robert Main, Secretary."

It was then moved by Professor De Morgan, and seconded by Mr. Galloway, and resolved unanimously,—

"That the recommendation of the Council in the circular now read be approved and adopted by this Meeting; and that, on the Library, Records, and Memorials of the Mathematical Society being delivered over to this Society, the remaining Members of the Mathematical Society be admitted Fellows of the Royal Astronomical Society without payment of the admission fees or annual contributions required by the Bye-laws."

November 14.—The President announced that the whole of the books of the late Mathematical Society had been delivered over to this Society, and had been arranged by Mr. Stratford, who would acquaint the meeting with a few of the particulars.

Mr. Stratford stated that the books received consisted of

76	volumes	folio
622	..	4to.
1442	..	8vo.
311	..	12mo.

131 books not bound or catalogued; and that 6 volumes were yet to be delivered: that the Council had this day determined to complete the deficient sets of the most valuable works, to rearrange the library, and to prepare a new catalogue, uniting the books of the two societies as early as possible.

It was then moved by the Rev. R. Sheepshanks, seconded by Mr. Drach, and resolved unanimously, that the warm thanks of the meeting be given to Mr. Stratford, for the trouble which he had taken in behalf of the Society, in carrying into effect the resolution of the last meeting with regard to the Mathematical Society.

Sir J. Herschel exhibited to the meeting a model of the surface of the moon, constructed by Frau Hofräthinn Witte, a lady resident in Hanover, from her own observations made with an achromatic telescope by Fraunhofer, placed in a small observatory on the roof of her dwelling-house, in that city. The model is composed of a mixture of mastic and wax, forming a globe 12 inches 8½ lines, Paris measure, in diameter, on which the positions and general outlines of the craters, and other remarkable features of the moon's surface, were in the first instance laid down from the latitudes and longitudes given by Messrs. Baer and Mädler in their work entitled *Der Mond*, and from their chart of the moon, and the modeling performed (with the aid of a magnifying-glass) from the actual appearance of the objects as presented in the telescope above mentioned. The globe in question is the ten millionth part of the actual diameter of the moon, in which proportion, therefore, the horizontal linear dimensions of the several mountains, &c. are laid down. But, in respect of the height, a double proportion is adopted, since otherwise the relative heights would have been with difficulty distinguishable on so small a model. Sir J. Herschel having explained the nature and mode of construction of this admirable work (of which only one other exists, now in the Royal Museum of Berlin—both being originals, and attempts to multiply copies by taking plaster casts having hitherto failed), pointed out several of the principal craters, and explained the nomenclature adopted by Messrs. Baer and Mädler in their work referred to, in describing the several characteristic peculiarities of the moon's surface. The model was, on the breaking up of the meeting, submitted to the closer inspection of the members.

December 12.—On a Direct Method of determining the Distance of a Comet by Three Observations. By J. J. Waterston, Esq.

The following is the author's explanation of his method:—

"It is well known that three observations of a comet afford sufficient data for computing its distance from the earth independently of any assumption as to the orbit in which it moves. The formula is, I believe, originally due to Lambert, and appears in the works of the principal mathematicians who have given analytical solutions of the problem by the differential method. It is unfortunate that the nature of the equation does not admit of much precision in the results of the calculation, which are consequently apt to be greatly affected by small errors of observation. The disturbing power of these unavoidable inaccuracies varies much according to the conditions of the problem, and it is, perhaps, impossible to recognise it in the analytical expression without a much greater effort of the attention than can be given when merely computing an orbit. With good observations the method has the advantage of revealing any obvious tendency to an ellipse or hyperbola; and, besides, it will in most cases afford a useful approximation to begin with in computing the parabolic formulæ. As to the expediency of putting the observations to this preliminary test in all cases, there would, perhaps, be little difference of opinion, if the labour of computation in doing so were available in the last part of the process, and if the conditions upon which the degree of accuracy depends could be easily distinguished.

"The object of this paper is to submit to the Astronomical Society an account of a method which has occurred to me of solving the equation by means of a constant curve, and to show how the preliminary calculation may be made available in Olbers's parabolic method, and likewise in a differential method, without requiring the original equatoreal position in either case to be transferred to the ecliptic. The conditions of accuracy also become so apparent in using this curve, that the effect of an error of right ascension or declination may be estimated by inspection.

"The method of solution is derived from the projection of the three observations on the plane perpendicular to the direction of the motion of the comet at the middle epoch. The earth's orbit being projected, its deflection, caused by the sun's attraction, is brought into view, and since its apparent direction is the same as that of the sun, and the projected direction of the sun from the comet is the same as at the earth, the radii vectores of both being identical on the projection; it is clear if the differentials at the middle time are alone considered that the deflection of the orbit of the comet, as it appears on the plane of projection, coincides in direction with the projected deflection of the earth's orbit, and that its magnitude depends on a function of the angle at the comet. We thus obtain the means of forming an equation for the angle at the comet in terms of the deflection of the earth's orbit; and this equation, although derived from a simple geometrical construction, appears to be similar to that which is given in the analytical discussion of the problem by Laplace, Lagrange, Legendre, and Airy. It depends wholly on the effect of the sun's centripetal force during the elapsed time as it appears on the plane of projection; and, as this, in the short differential period of a few days, bears but a small proportion to the projection of its chord, or velocity, the results are much more liable to be affected by the unavoidable errors of observation than if the equation expressed the same unknown quantity in terms of the velocity. But in the last case we have to suppose the nature of the conic section known, in the first no assumption of the kind is required, the deflecting effect of the sun's force being necessarily the same in all orbits at the same central distance.

"The equation for the angle at the comet is solved by drawing one line on the constant curve, and the preliminary computation required to do so affords an expression for the ratio of the distances at the first and third observations on the usual assumption that the chord is divided in the ratio of the times.

"This expression may be converted into the elegant form given by Olbers, so that it is identical with the value of M in his formulæ, and is expressed in terms that are likewise required in drawing the line on the constant curve.

"An example is given from the Trevandrum observations of the great comet of 1843. The formulæ are also applied to Göttinger's observations of the second comet of 1813. A copy of the constant curve is given upon a separate sheet, and the lines of these examples drawn. The co-ordinates of the curve consist of the cotangent and cube of the sine. It is easily constructed by the common tables. If drawn with ordinary care, it will give the reading of the angle at the comet to greater nicety than even the best observations can afford.

"I have appended a modification of Olbers's formulæ for the radii vectores and chord adapted to equatoreal positions, and involving the use of the angular quantities already computed for the use of the constant curve. The additional work of computation does not appear to be so great as that which is required to convert the right ascension and declination into latitude and longitude; and, besides, it is easier to compare observations with the computed elements when the latter are referred to the equator. The inclination of the orbit and position of the nodes are transferred to the ecliptic by the solution of one spherical triangle.

"In the recent improvements which Olbers has made in his method, by expanding Euler's formula into a series and reversing, the means are afforded of constructing a small table which shortens considerably the process of finding the distance by trial and error. Another improvement consists in the new expression given for the chord being more favourable to accurate computation. I have included a form of the same kind in terms of the right ascension and declination which is almost wholly made up of angular quantities that have already been prepared and used with the constant curve.

"In the last part of the paper an expression for the angle at the comet is given to be used with the differential method which, in solving by trial and error, requires only five tabular references."

Extract of a Letter from Sir John Herschel to the President, dated Collingwood, November 29, 1845.

"Being on the subject of the satellites of Saturn, I will mention here a singularity which, though obvious enough, has not (so far as I am aware) been noticed before, viz. that the periodic time of the first satellite (first in order of the ring) is *precisely* half that of the third, and the periodic time of the second *precisely* half that of the fourth. This is far too remarkable and close a coincidence to be merely casual, and (the second satellite being a certainty) the extension of the law to the first (a law so out of the way and unlikely) would of itself be evidence of its real existence, even had it not been (as it now certainly has been) re-observed. If such atoms perturb one another's motions, there must be some very odd secular equations arising from this singularity. It is not worth while to make a formal communication of such a thing to the Astronomical Society; but if you think it worth your verbal mention at the meeting, it may be interesting to those (if any) who are busy about satellitary perturbations."

On a new Double-image Micrometer, communicated in a Letter to the President by Professor Powell.

"My dear Sir,—The following suggestion for a very simple double-image micrometer occurred to me a few years ago; but not having much practical acquaintance with these matters, I should hardly have supposed it to possess novelty or prospect of utility enough to render it worthy the notice of the Astronomical Society, had you not encouraged me to communicate it.

"The optical principle is merely that of a ray of light, refracted obliquely through a plate of glass with parallel surfaces, and emerging parallel to, but not coincident with, its original direction.

"If, then, such a plate intercept half the cone of rays going from the object-glass of a telescope to its focus, there will be formed, at the focus, besides the direct, a deviated image; and the angular deviation will be dependent on the inclination, the thickness, and the refractive power of the glass, involving a constant factor to be found by observation for the particular instrument, agreeably to the following formula, which may be easily tabulated for all inclinations.

"If ϕ and ϕ' be the angles of incidence and refraction, and t the thickness of the plate, a moment's consideration will shew that the oblique path of the ray within the plate $= t \,.\, \sec \phi'$; and, for the angular space θ between the direct and the deviated ray, c being the constant for the instrument, we have

$$\theta = c \,.\, t \,.\, \sec \phi' \sin (\sin \phi - \phi').$$

"If such a plate be placed within the tube of a telescope between the object-glass and its focus, so that a variable inclination can be given to it, and a graduated circle be read off outside; then, when the plate is perpendicular to the axis there will be no deviation; but, when it is inclined, the deviation, found as above, will give the measurement of a small angular space, as in other double-image micrometers.

"The less the thickness of the glass, the greater will be the range of the scale for a very small deviation.

"The idea has as yet been put to trial only in a very rough manner; and I offer it without at all being able to say whether serious practical difficulties may not arise, which can only be decided on a more accurate construction; or should no such objection occur, it still remains to be seen whether this suggestion may afford any useful addition to the micrometrical resources already in the hands of the observer, so as at least to be available in some cases: but these are points on which the practical astronomer alone can judge; and it is mainly in the hope that it may receive such examination that I submit this idea to the Astronomical Society.—I remain, &c.,

"BADEN POWELL,
"Savilian Professor of Geometry."

"Oxford, December 7th, 1845."

XLIV. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS ON THE SPOTS ON THE SUN.

AT the meeting of the American Philosophical Society, June 20, 1845, Prof. Henry of Princeton, U.S., made the following communication of a series of experiments made by himself and Prof. Alexander relative to the spots on the sun.

His attention was directed to this subject, by an article in the September number of the *Annales de Chimie*, by M. Gautier, upon the influence of the spots on the sun on terrestrial temperature. It is well known that Sir William Herschel entertained the idea, that the appearance of solar spots was connected with a more copious emission of heat, and that the seasons during which they were most abundant were most fruitful in vegetable productions; and pursuing this idea, he was led to trace an analogy between the price of corn and the number of solar spots during several successive periods. The result of this investigation, so far as it was extended, seemed to favour the views of this distinguished philosopher. A mode of investigation of this kind, however, is not susceptible of any great degree of accuracy; the price of corn is subject to so many other causes of variation besides that of solar temperature, that little reliance can be placed on it.

M. Gautier has attempted to investigate the influence of the solar spots on terrestrial temperature, by comparing the temperature of several places on the earth's surface, during the years in which the spots were most abundant, with those in which the smallest number were perceptible. From all the observations collected, it seems to be indicated, that during the years in which the spots were the greatest in number, the heat has been a trifle less; but the results are far from being sufficiently definite to settle the question: and M. Gautier remarks, that a greater number of years of observation at a greater number of stations, will be necessary to establish a permanent connexion between these phænomena.

The idea occurred to Prof. Henry, that much interesting information relative to the sun might be derived from the application of a thermo-electric apparatus to a picture of the solar disc, produced by a telescope, on a screen, in a dark room. This idea was communicated to Prof. Alexander, who readily joined in the plan for reducing it to practice. It was agreed that they should first attempt to settle the question of the relative heat of the spots as compared with the surrounding luminous portions of the sun's disc. The first experiments were made on the 4th of January 1845. Mr. Alexander had observed, a few days previous, a very large spot, more than 10,000 miles in diameter, near the middle of the disc. To produce the image of this spot, a telescope of four inches aperture, and four and a half feet focus, was placed in the window of a dark room, with a screen behind it, on which the image of the spot was received. The instrument was placed behind the screen, with the end slightly projecting through a hole made for the purpose, and a small motion of the telescope was sufficient to throw the image of the spot off or on the end of the pile. The spot was very clearly defined, and might have been readily daguerreotyped, had the telescope been furnished with an equatorial movement. The form of the penumbra of the spot, as it appeared on the screen, was that of an irregular oblong, about two inches in one direction, and an inch and a half in the other. The dark central spot within the penumbra was nearly square, of about three-fourths of an inch on the side, and a little larger than the end of the thermo-pile.

The method of observation consisted in first placing, for example, a portion of the picture of the luminous surface of the sun in connexion with the face of the pile, and after noting the indication of the needle of the galvanometer, the telescope was then slightly moved, so as to place the dark part of the spot directly on the face of the pile, the indication of the needle being again noted. In the next set of experiments the order was reversed; the picture of the spot at the beginning of the experiment was placed in connexion with the pile, and afterward a new part of the luminous portion of the disc was made to occupy the same place.

The thermo-electrical apparatus used in these experiments was made by Ruhmkorff of Paris; and in order to render the galvanometer more sensitive, two bar magnets, arranged in the form of the legs of a pair of dividers, were placed with the opening downwards, in a vertical plane, above the needle, so that, by increasing or diminishing the angle, the directive power of the needle could be increased or diminished, and, consequently, the sensibility of the instrument could be varied, and the zero point changed at pleasure.

In the present experiments, in order to mark more definitely the difference in temperature, after the needle had been deflected by the heat of the sun, the magnetic bars above mentioned were so arranged as to repel it back to near the zero point, so that it might, in this position, receive the maximum effect of any variation in the electrical current.

Twelve sets of observations were made on the first day, all of which, except one, gave the same indication, namely, that *the spot emitted less heat than the surrounding parts of the luminous disc.* The following is a copy of the record made at the time of the observations. The degrees are those marked on the card of the galvanometer, and are of course arbitrary.

Spot, 3°¼.
Sun, 4°½.

Sun, 3°.
Spot, 1°¾.

Spot, 2°.
Sun, 3°.

Sun, 2°½.
Spot, 2°.

Spot, 2°.
Sun, 2°¼.

Sun, 5°¼.
Spot, 4°.

Spot, 4°½.
Sun, 5°.

Sun, 4°½.
Spot, 3°¾.

Sun, 2°.
Spot, 3°¼*.

Spot, 0°¾.
Sun, 2°½.

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* At this observation a slight cloud probably passed over the sun's disc.

Spot, $4^\circ\frac{3}{4}$.	Sun, $1^\circ\frac{1}{4}$.
Sun, 5°.	Spot, 0°.

The change in the temperature during the intervals of observation, is due to the variations in the temperature of the room differently affecting the two extremities of the pile.

In consequence of cloudy weather, another set of observations were not obtained until the 10th of January, and at this time the spot had very much changed its appearance; the penumbra, while it retained its dimensions in one direction, was much narrowed in the other, and the dark part was separated into two small ones; also the sky was not perfectly clear, and therefore the results were not as satisfactory as those of the previous observations; the indications were, however, the same as in the other sets, exhibiting a less degree of heat from the spots.

Cloudy weather prevented other observations on the heat of different parts of the sun, particularly a comparison between the temperature of the centre and the circumference of the disc, which would have an important bearing on the question of an atmosphere of the sun. The observations will be continued, and any results of interest which may be obtained will be communicated to the Society.

METHOD OF PURIFYING OXIDE OF URANIUM FROM NICKEL, COBALT AND ZINC. BY PROF. WOHLER.

When the oxide of uranium, in its preparation from the pitchblende, has been so far purified as to be dissolved in carbonate of ammonia, sulphuret of ammonia is carefully and gradually mixed with the solution as long as a black precipitate falls. In this way nickel, cobalt and zinc are entirely separated, without any uranium being thrown down.—Liebig's *Annalen*, Oct. 1845.

ON SOME NEW DOUBLE HALOID SALTS. BY M. POGGIALE.

Protochloride of Antimony and Chloride of Ammonium.—The protochloride of antimony combines with chloride of ammonium in two proportions. When protochloride of antimony is added to a solution of that salt, it dissolves readily, and only a slight turbidness, arising from the formation of some oxychloride, is perceptible. On evaporating the liquid at a gentle heat, at first beautiful rectangular prisms are obtained, and subsequently hexahedrons or hexahedral pyramids. The first are $3NH^3\,HCl, SbCl^3 + 3HO$, and the latter $2NH^3\,HCl, SbCl^3 + 2HO$. Both salts are colourless and transparent; they become yellow and opake in moist air, but are very permanent in dry air, and are coloured yellow in the mother-ley when heated; they are likewise decomposed by a large quantity of water.

Protochloride of Antimony and Chloride of Potassium.—This salt is deliquescent, becomes yellow on exposure to the air, is decomposed by water and also by heat; it forms laminar crystals, the composition of which is $3KCl, SbCl^3$. The mother-ley yields, on spontaneous evaporation, hexahedral crystals of $2KCl, SbCl^3$.

Protochloride of Antimony and Chloride of Sodium forms laminar crystals, having the composition $3NaCl, SbCl^3$.

Protochloride of Antimony and Chloride of Barium.—When the solution of the chloride of barium is very dilute, the two salts separate on cooling, the chloride of barium crystallizes in tablets, while the protochloride of antimony decomposes the water. It is therefore necessary, in order to obtain this compound, to use concentrated solutions. It is obtained in minute radiately-grouped needles, the composition of which is represented by $2BaCl, SbCl^3 + 5HO$. The protochloride of antimony combines in the same way with chloride of strontium, chloride of calcium, and chloride of magnesium.

Protochloride of Tin and Chloride of Ammonium forms beautiful fascicular needles, which are permanent in the air, but are decomposed by water. The analysis of this salt, which had been previously obtained by Jacquelain, gave the formula $2NH^3, SnCl, HCl + 3HO$.

Protochloride of Tin and Chloride of Potassium is obtained by direct combination of the two salts, and crystallizes in beautiful long needles, which are isomorphous with the preceding salts. Its formula is $2KCl, SnCl + 3HO$.

Protochloride of Tin and Chloride of Barium yields, on spontaneous evaporation, beautiful prisms, the *protochloride of tin and chloride of strontium* long needles. They are represented by the formulæ $BaCl, SnCl + 4HO$ and $SrCl, SnCl + 4HO$.

Chloride of Sodium and Magnesium consists of $NaCl, 2MgCl + 2HO$.

Iodide of Silver and Ammonium.—Iodide of ammonium dissolves iodide of silver, forming with it a deliquescent double salt. It contains $2NH^3, HI, AgI$.

Iodide of Lead and Sodium crystallizes in yellow shining laminæ. It is obtained by adding a slight excess of iodide of sodium to a hot solution of iodide of lead, and placing the liquid in a warm spot. Its formula is $NaI, 2PbI$.

Iodide of Zinc and Sodium yields, on spontaneous evaporation, prismatic radiately-grouped needles. It is colourless, readily soluble in water and deliquescent. Its formula is NaI, ZnI.

Chloride and Iodide of Lead is obtained by dissolving iodide of lead in a solution of chloride of ammonium. On cooling, numerous yellowish crystals separate, which assume the form of needles, and consist of $PbI, 2PbCl$. On evaporating the mother-ley, crystals are obtained of $2NH^3 HCl, PbI + 2HO$, in the form of minute, silky, ramified needles; they become yellow in the air, and are decomposed by water.

Chloride and Acetate of Lead.—This is formed when chloride of lead is boiled in a porcelain dish with teracetate of lead, to which subsequently a slight addition of acetic acid is made. The solution is evaporated at a gentle heat, when colourless shining crystals separate on cooling. The salt has a sweetish astringent taste, effloresces readily in the air, and melts at 180°; it loses its water of crystallization at 228°. The salt is readily soluble in water; alcohol decomposes it, and precipitates chloride of lead. Several analyses yielded the formula $PbCl, 5PbO\,C^4 H^3 O^3 + 15HO$.

Iodide and Carbonate of Lead is prepared by digesting carbonate of lead with iodide of lead until the excess of iodide of lead has dissolved. This salt is yellow and insoluble in water. Its formula is PbI, PbO, CO^2.—*Comptes Rendus*, p. 1180.

ON THE VOLATILE ACIDS OF CHEESE. BY MM. ILJENKO AND LASKOWSKI.

The authors cut fifty pounds of Limbourg cheese, which possessed a very strong odour, into small pieces, mixed them with water, and submitted the mixture to distillation in a large alembic, water being occasionally added during several days. By this operation a somewhat turbid ammoniacal liquor was obtained, which was supersaturated with sulphuric acid and again distilled. The product was afterwards saturated with barytes water; the salt obtained was evaporated to its crystallizing point; the acid was again separated and converted into a salt of silver. Analysis showed that this volatile acid was entirely valerianic acid.

The residue was afterwards saponified by means of potash, the soap decomposed by potash, and subjected to a fresh distillation, and there was thus obtained an acid liquid which was saturated with barytes and evaporated to crystallize; it yielded a mixture of several salts of barytes, which were separated by means of their different solubility in water. The rough salt was mixed with seven parts of water and heated to boiling; the caproate of barytes dissolved, and afterwards separated in crystalline tufts of considerable size, whilst the butyrate remained in solution; this was converted into a salt of silver and analysed.

The barytic salts, which were not dissolved by the seven parts of boiling water, were composed of caproate and caprylate of barytes; and they also were separated by their different solubility.

It appears then that cheese contains the following volatile acids:

Butyric acid	$C^4 H^8 O^2$
Valerianic acid..	$C^5 H^{10} O^2$
Caproic acid....	$C^6 H^{12} O^2$
Caprylic acid ..	$C^8 H^{16} O^2$
Capric acid	$C^{20} H^{20} O^2$

Valerianic acid occurs in the largest quantity, and its presence had been previously discovered by M. Balard in the cheese of Roquefort. All these acids, it will be observed, are homologous substances.

The authors also performed some experiments on the fused portion of cheese; they obtained by means of boiling alcohol perfectly crystalline margarine from it; it was fusible at 127° Fahr., and margaric acid was obtained from it. The rough margarine was mixed with some liquid glycerine. Unaltered caseine was also present, soluble in boiling water and insoluble in alcohol. There was also present lime, a little magnesia, soda, potash, traces of iron, phosphoric acid, chlorine and sulphuric acid.—*Journ. de Pharm. et de Ch.*, Decembre 1845.

ON THE DOUBLE SALTS OF THE MAGNESIAN GROUP.

M. J. Isidore Pierre has paid particular attention to the salts of this group, including those of magnesia, oxide of copper, zinc, nickel, cobalt, manganese and iron.

The author observes, that it is well known that Prof. Graham has stated, with respect to the sulphates of the above-named bases, that one of the equivalents of water cannot be eliminated, except at a much higher temperature than is required for the others; that this equivalent may be replaced by an equivalent of a salt, so that the double salt formed contains one equivalent less of water than if each of the two simple sulphates had brought all its water of crystallization into the molecule of the double salt which results from their combination.

M. Pierre states that the results which he has obtained do not confirm those of Prof. Graham; he found that sulphate of zinc contains, as generally admitted, 43·72 per cent., or seven equivalents of water; and he ascertained that by exposing it for a long time to a temperature of 230° Fahr. and a current of dry air, that it lost 43·6 per cent., or the whole of its water, which is at variance with Graham's result, who found that it required a heat of 400° Fahr. to expel the seventh equivalent of water.

Double Sulphate of Zinc and Potash.—This salt is readily prepared by mixing together hot solutions of equivalents of sulphate of zinc and bisulphate of potash, and allowing crystallization to take place. The crystals are beautiful small, milk-white parallelogrammic tables; this salt is soluble in two and a half times its weight of boiling water, but much less soluble in cold water, for it crystallizes abundantly on the cooling even of an unsaturated solution.

When exposed gradually to a heat of 356° to 392° Fahr., it effloresces without fusing in its water of crystallization, which it loses completely and pretty rapidly at this temperature, the amount being 27·49 per cent. The author's analysis gives as the formula of this salt, ZnO, SO^3; $KaO, SO^3 + 7HO$, which indicates, as he shows, 27·32 per cent. of water instead of 24·49, the experimental result.

In this case it is therefore to be remarked, that the sulphate of zinc retains the seven equivalents of water which it possessed in its simple state.

Double Sulphate of Zinc and Magnesia.—M. Pierre observes, that it is generally supposed that these two sulphates may combine in all proportions; having found that sulphate of zinc in the double salt which it forms retains its seven equivalents of water, the author observes that if sulphate of magnesia did the same, the double salt should contain fourteen equivalents of water.

This salt is readily obtained by mixing its equivalents and crystallizing; it forms very fine oblique rhombic prisms, which are by pressure separated into very fine needles; when quickly heated to 212° to 248° Fahr., it loses part of its water; at 392° Fahr. it retains two equivalents of water, and these cannot be expelled at a temperature lower than 482° to 500° Fahr.

When heated slowly and progressively, this salt effloresces without fusing in its water of crystallization; it may in this mode be deprived of the whole of its water of crystallization without being fused; it merely agglutinates slightly.

The formula of this salt derived from analysis is $ZnO, SO^3, MgO, SO^3 + 14HO$, which indicates 47·17 per cent. water; the loss of water by experiment was 47·12; the salt heated to 392° Fahr. retained ten equivalents of water.

From the preceding and the analyses of various other salts which the author prepared, he arrives at the following among other conclusions:—

1. Sulphate of zinc containing seven equivalents of water, retains the whole of it in the compound which it forms with the alkaline or alkalino-earthy sulphates.

2. The sulphates of zinc and magnesia combine equivalent to equivalent, and the resulting compound contains a quantity of water equal to the sum of the quantities which both salts contained when separate, that is to say, fourteen equivalents, if the double salt crystallizes at common temperatures.

3. The simple sulphates of zinc, copper and nickel yield all their water at a little above 212° in a long-continued current of air, instead of retaining one equivalent, as stated by Prof. Graham, at 400° Fahr.

4. The simple sulphates of zinc, magnesia, copper and nickel combine with other sulphates, or with each other without elimination of the water.—*Ann. de Ch. et de Phys.*, Fevrier 1846.

PREPARATION OF HYPOPHOSPHITES.

M. A. Wurtz prepared almost the whole of these salts, which he analysed, by the double decomposition of soluble sulphates with hypophosphite of barytes; the most œconomical method of preparing the last-mentioned salt, is to boil a solution of sulphuret of barium with phosphorus, until gas ceases to be evolved. If the ebullition has been long continued, the sulphuret of barium is almost entirely decomposed, and a slight excess only is left, which may be separated by carbonate of lead. Sometimes, however, the excess of sulphuret is more considerable; it is then proper to separate it by cautiously adding small quantities of sulphuric acid to the hot filtered liquor, as long as sulphuretted hydrogen is disengaged. If the liquor becomes acid, it must be neutralized, before evaporation, by a little carbonate of barytes.

Hypophosphite of Potash.—This salt was prepared by the double decomposition of hypophosphite of barytes and sulphate of potash. The aqueous solution was evaporated to dryness, and the residue, treated with hot alcohol, deposited hypophosphite of potash on cooling. The crystals of this salt are hexagonal tables. They are very deliquescent, very soluble in weak alcohol, less so in absolute alcohol, and insoluble in æther. They lose no water at 212° Fahr.

Adopting with M. Pelouze 400 as the atomic weight of phosphorus, M. Wurtz gives as the formula of this salt,

$$PO, KO, 2HO = PH^2O^3, KO.$$

Hypophosphite of Ammonia.—This salt was prepared like the preceding. It crystallizes in large irregular hexagonal laminæ; it is less deliquescent than the salt of potash, and unalterable at 212° Fahr. At about 394° Fahr., it fuses into a transparent liquid without losing water, and becomes a crystalline mass on cooling. It does not decompose under 464° Fahr., at which temperature, like other hypophosphites, it disengages a little water and spontaneously inflammable phosphuretted hydrogen. Its formula, as determined by analysis, appeared to be

$$PO, H^3N\ 3HO = PH^2O^3, NH^4O.$$

Hypophosphite of Strontia.—This salt was prepared like that of barytes, by boiling a solution of sulphuret of strontium with phosphorus, and decomposing the excess of sulphuret by carbonate of lead, or by sulphuric acid added in sufficient quantity to render the liquid slightly acid. By evaporation the hypophosphite of strontia crystallizes in the mammillated form by the juxtaposition of small laminæ round a common centre. These crystals are unalterable in the air, and lose no water at 212°. They are very soluble in water, and insoluble in alcohol. The formula of this salt is

$$PO, SrO, 2HO = PH^2O^3, SrO.$$

Hypophosphite of Magnesia.—This salt was prepared by double decomposition with sulphate of magnesia and hypophosphite of barytes; it crystallizes, as stated by M. H. Rose, in very brilliant regular octahedrons, which effloresce in dry air. The formula of the crystallized salt is $PH^2O^3, MgO + HO + 5Aq$; of the salt dried at 212°, $PH^2O^3\ MgO + HO$; and lastly, the formula of the salt dried at 360° Fahr., is PH^2O^3, MgO.

Hypophosphite of Zinc.—This salt was obtained of two different forms. It crystallizes sometimes in very efflorescent regular octahedrons, and sometimes in small rhombic crystals unalterable in the air. When a moderately concentrated solution of this hypophosphite is submitted to spontaneous evaporation, the first-mentioned crystals are usually formed. They are so efflorescent, that they lose water during pressure between folds of paper, previous to analysis. The formula of the rhombic crystals is

$$PH^2O^3\ ZnO, + HO.$$

Hypophosphite of Iron.—This salt crystallizes in large green octahedrons, which effloresce by exposure to the air and become a white powder. When exposed to the air, the moist salt absorbs oxygen from it rapidly. The formula of the crystallized salt is $PH^2O^3, FeO + 6HO$.

Hypophosphite of Chromium.—This salt was prepared by double decomposition by mixing solutions of sulphate of chromium and hypophosphite of barytes. By evaporation there was obtained an amorphous, cracked mass of a very deep green colour. This salt loses water by drying when it has been heated to 392° Fahr.; it is not soluble either in water or dilute acids. The formula of this salt is $2PH^2O^3, Cr^2O^3 + 4HO$.

Hypophosphite of Manganese.—The crystals of this salt are of a rose colour, brilliant and unalterable in the air. They do not lose water at 212° Fahr., but at about 302° Fahr. they part with one equivalent. The formula of this salt is $PH^2O^3, MnO + HO$.

Hypophosphite of Cobalt.—This salt forms large crystals of a deep red colour, which effloresce in the air. At 212° Fahr., they lose six equivalents of water of crystallization, and become a pale rose-red powder. The formula of this salt, which agrees with that of M. H. Rose, is $PH^2O^3, CoO + 6HO$.

Hypophosphite of Nickel.—The crystals of this salt are regular octahedrons, and smaller than those of hypophosphite of cobalt. When the aqueous solution is evaporated at the temperature of 212° Fahr., it is partially reduced to metallic nickel with the disengagement of hydrogen. This reduction takes place perfectly when the crystals of this salt, broken and slightly moistened, are exposed to the air at a temperature of 248° Fahr. The formula of this salt is $PH^2O^3, NiO + 6HO$.

Hypophosphite of Copper.—The solution of this salt is readily prepared by decomposing sulphate of copper with hypophosphite of barytes. It is not a permanent salt. At about 140° Fahr. it becomes turbid, and deposits hydrate of copper. By evaporating the solution *in vacuo*, small blue crystals of this salt were once obtained. These crystals decompose quickly, and with projection of the entire mass when heated to 149° Fahr.; and phosphuret of copper is formed. The formula of this salt is PH^2O^3, CuO.—*Ann. de Ch. et de Phys.*, Fevrier 1846.

BIELA'S COMET.

The following is an abstract of a letter addressed by Prof. Challis to the editor of the *Times*:—

"As I was preparing to observe Biela's comet, on the evening of the 23rd of January, I discovered a smaller comet in its immediate neighbourhood, and ascertained by my observations that evening that the two comets had the same apparent motion. A double comet is a celestial phænomenon which, I believe, has never before been witnessed, and cannot fail to arrest the attention of astronomers. It will be a matter of very great scientific interest to determine the relative motions of these two singular bodies, and the nature of the influence they mutually exert on each other. The following relative positions I have succeeded in obtaining by means of the Northumberland telescope. They are either derived from separate determinations of the places of the comets, or from direct measurements of angles of positions and differences of North Polar distance. The smaller comet is north of the other, and precedes it.

	Mean time. h	Difference of R.A. s	Difference of N.P.D. sec.	Angle of position. d. m.	Distance. sec.
Jan. 23	7·1	5·18	122·9	327·43	145·4
... 24	7·0	5·11	126·6	328·48	148·0
... 27	6·4	5·84	144·6	328·48	169·1
... 28	6·3	5·77	151·1	330·12	174·1
... 29	7·4	5·44	154·4	332·10	174·6
Feb. 11	7·4	7·38	249·7	336· 8	273·1
... 12	7·1	7·86	252·7	335· 3	278·7
... 13	7·5	8·30	264·9	334·54	292·6

"The observations on the 11th and 12th of February were obtained with great difficulty on account of the faintness of the comets from the effect of moonlight. The light of the larger comet spreads over a considerably greater extent than that of the other, but is not intrinsically much brighter."

METEOROLOGICAL OBSERVATIONS FOR JAN. 1846.

Chiswick.—January 1. Fine. 2, 3. Frosty: fine: overcast. 4. Rain. 5. Sharp frost: cloudy: clear and frosty. 6. Drizzly. 7. Overcast and mild throughout the day and night. 8. Cloudy and fine. 9. Uniformly overcast. 10. Overcast: drizzly rain. 11. Hazy and drizzly. 12. Cold haze. 13. Hazy: very fine. 14. Foggy: overcast and fine. 15. Fine. 16. Thick fog: rain at night. 17. Hazy: drizzly: cloudy and mild. 18. Foggy: rain at night. 19. Constant rain: boisterous, with rain at night. 20. Clear and fine. 21. Rain: densely clouded and mild: boisterous, with rain at night. 22. Boisterous, with rain: densely clouded. 23. Heavy showers. 24. Hazy and mild. 25. Rain. 26. Showery: heavy rain at night. 27. Clear: cloudy: rain at night. 28. Rain: cloudy: very high tide in the Thames: clear. 29. Rain. 30. Overcast. 31. Cloudy: windy at night.

Mean temperature of the month	43°·54
Mean temperature of January 1845	38 ·69
Average mean temperature of Jan. for the last twenty years	36 ·46
Average amount of rain for the last twenty years	1 ·60 inch.

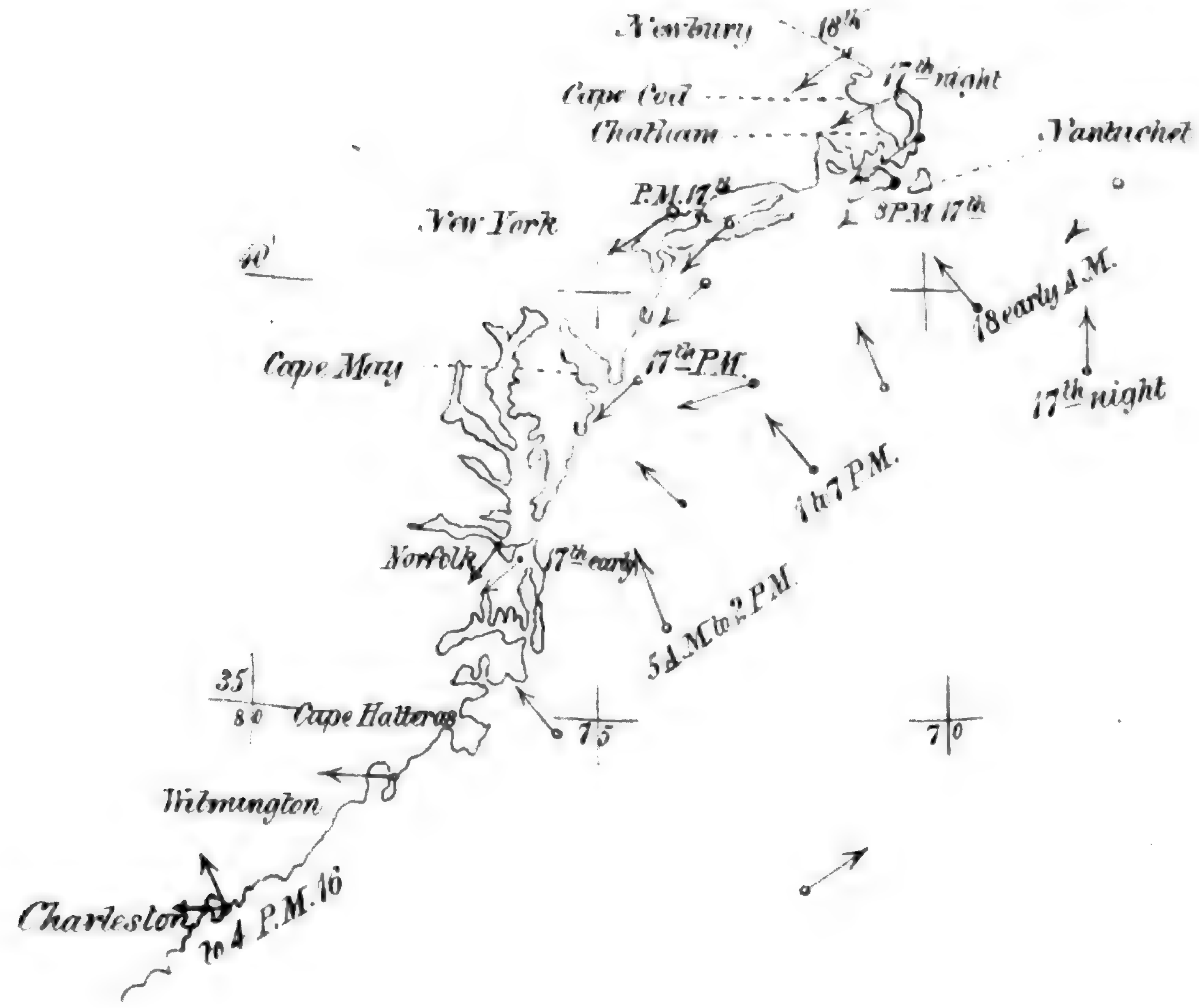
Boston.—Jan. 1. Stormy: rain last night. 2. Fine. 3. Cloudy. 4. Rain. 5. Fine. 6. Rain. 7. Cloudy. 8. Fine. 9—13. Cloudy. 14, 15. Fine. 16. Foggy. 17. Cloudy: rain A.M. and P.M. 18. Foggy. 19. Rain: rain early A.M.: rain P.M. 20. Windy: rain early A.M. 21. Cloudy: rain P.M. 22. Cloudy and stormy: rain early A.M. 23. Fine. 24. Cloudy: rain early A.M. 25. Fine: rain early A.M. 26. Cloudy: rain early A.M. 27. Fine. 28, 29. Rain. 30, 31. Cloudy.—N.B. Not so warm a January since January 1834: the average of that month was 44°·3.

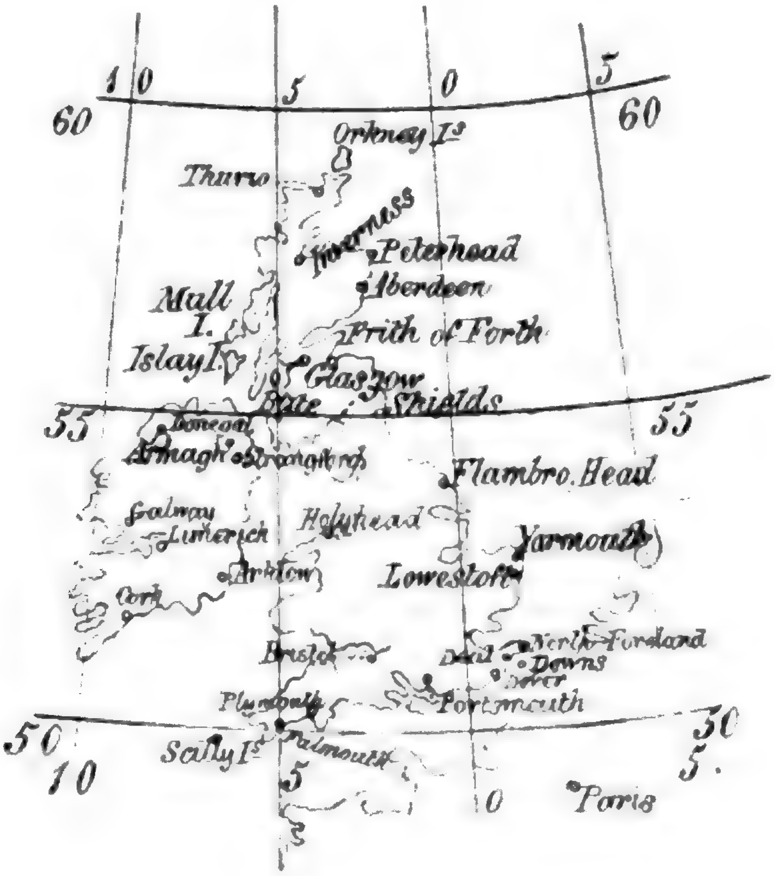
Sandwick Manse, Orkney.—Jan. 1. Snow-showers. 2. Fine: frost: cloudy. 3. Cloudy: clear. 4. Clear: showers. 5. Bright: showers. 6. Damp: clear. 7. Cloudy: showers. 8. Showers: clear. 9. Cloudy: clear. 10. Rain: cloudy. 11. Drizzle: damp. 12. Drizzle: hazy. 13. Bright: cloudy. 14. Damp: cloudy. 15. Rain: drizzle. 16. Clear. 17. Damp. 18. Bright: cloudy. 19. Damp: showers. 20. Rain: drizzle. 21. Rain: clear. 22. Damp: rain. 23. Fine: damp. 24. Fine: frost: damp: aurora. 25. Rain: cloudy. 26. Damp. 27. Damp: rain: clear. 28. Cloudy: showers. 29. Showers. 30. Cloudy: rain. 31. Drizzle: showers.

Applegarth Manse, Dumfries-shire.—Jan. 1. Snow-showers. 2. Frost: clear and fine. 3. Wet all day. 4. Fine A.M.: shower P.M. 5. Frost A.M.: rain P.M. 6, 7. Showery. 8. Fair. 9, 10. Slight drizzle. 11. Slight drizzle: fog. 12. Fair and mild. 13. Fair A.M.: rain P.M. 14. Fair: one slight shower. 15. Wet A.M.: cleared: fine. 16. Frost, slight: fine. 17. Fair A.M.: slight shower P.M. 18. Fair, but cloudy. 19. Rain nearly all day. 20. Rain all day: flood. 21. Fair, but cloudy. 22. Drizzling rain. 23. Rain and fog. 24. Thick fog. 25. Heavy rain: flood. 26. Drizzling rain. 27. Rain A.M.: fair: rain P.M. 28—31. Rain.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. Jan.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8¾ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8¾ p.m.								
1.	30·051	29·692	29·23	29·51	29·80	29·38	29·77	46	30	42·5	45½	37	36½	36½	nw.	w.	w.	n.	......	·31	......	·40
2.	30·475	30·192	29·85	30·12	30·23	30·18	30·16	41	22	34	37	30	32	37	n.	calm	n.	s.	......	......	......	·06
3.	30·440	30·335	30·10	30·10	29·83	29·92	29·62	43	38	34	45½	33	41	45	s.	calm	s.	s.	·06	......	......	·16
☽ 4.	30·008	29·828	29·60	29·61	29·84	29·52	29·70	43	22	38·5	45½	37	40½	40	sw.	w.	sw.	w.	·04	·09	......	......
5.	30·134	30·085	29·75	29·95	29·85	29·92	29·67	39	21	32	39½	31	39	40	sw.	calm	ssw.	sse.	......	·16	......	·19
6.	30·060	29·998	29·70	29·70	29·74	29·58	29·51	47	44	36	49	30½	45	49½	sw.	w.	sw.	wsw.	·02	·04	......	·08
7.	30·327	30·203	29·75	29·88	30·00	29·55	29·69	48	43	45	49½	45	50	49	w.	w.	sw.	ssw.	......	·03	0·40	·07
8.	30·576	30·451	30·03	30·15	30·31	30·02	30·19	50	41	43	50	46	46	45	sw.	calm	sw.	wsw.	......	......	......	·19
9.	30·621	30·529	30·21	30·39	30·30	30·26	30·06	49	47	40·5	49	43	46	45½	sw.	calm	ssw.	ne.	......	......	......	·04
10.	30·477	30·374	30·10	30·18	30·14	29·92	30·13	42	37	40	48	42½	46	43	sw.	calm	sw.	nw.	·01	......	......	......
11.	30·317	30·237	29·92	30·13	30·04	30·16	29·96	42	32	43	48	35	41	43	sw.	calm	sw.	sse.	·01	......	......	·19
○ 12.	30·067	29·846	29·71	29·83	29·60	29·80	29·69	36	31	37	43½	38½	45	43	se.	calm	se.	se.	......	......	......	·04
13.	29·654	29·491	29·39	29·47	29·30	29·56	29·50	48	35	35	45	33½	40	45	se.	calm	ese.	se.	......	......	......	·05
14.	29·524	29·429	29·21	29·28	29·36	29·42	29·52	50	37	39	48	39	46	40	se.	calm	se.	se.	......	......	0·21	......
15.	29·866	29·729	29·39	29·39	29·67	29·46	29·63	49	36	43	48	42	43	46	s.	calm	s.	sw.	......	......	......	·02
16.	29·838	29·722	29·53	29·69	29·58	29·74	29·75	51	40	39	47	34	40	40	e.	calm	s.	se.	·04	......	......	·37
17.	29·726	29·658	29·38	29·57	29·58	29·80	29·85	51	36	42	46½	41½	43	42	sw.	calm	e.	se.	·01	......	......	......
18.	29·734	29·604	29·37	29·55	29·47	29·81	29·68	53	35	39	47	40	40½	39	sw.	calm	e.	ese.	·24	·12	......	......
19.	29·194	28·943	28·95	29·10	28·70	29·42	29·17	54	41	43	46½	37	41	42	sw.	s.	e.	e.	·44	·04	......	·05
☾ 20.	29·484	29·309	28·78	28·71	29·05	28·98	29·01	51	40	45	48½	43	42	41½	sw.	w.	w.	e.	·14	·56	......	·34
21.	29·313	29·145	29·00	29·12	28·90	29·06	29·06	55	50	45	46	39	43	36	sw.	calm	sw.	se.	·33	......	1·18	·17
22.	29·228	29·053	28·52	28·60	28·90	28·97	28·97	56	41	52	47	42½	40½	41½	sw.	w.	sw.	se.	·02	·45	......	......
23.	29·223	29·154	28·84	28·95	28·99	29·00	29·12	51	42	43	47	42½	40½	39	sw.	calm	s.	......	·27	......	......	·20
24.	29·644	29·330	28·98	29·24	29·30	29·35	29·38	53	41	45	47	39	38	37½	sw.	calm	s.	ese.	·24	·07	......	·01
25.	29·339	29·143	28·85	28·94	28·90	28·96	28·95	56	46	50·5	49	42	43	43	sw.	calm	sw.	se.	·59	·27	......	·47
26.	29·242	29·168	28·73	28·86	28·89	28·95	28·94	55	41	48	49	45½	45	44½	w.	calm	s.	se.	·08	·03	......	·20
● 27.	29·624	29·359	28·98	29·07	29·29	28·98	29·22	54	40	43	50½	44	45	44½	sw.	calm	sw.	ssw.	·02	......	......	·04
28.	29·584	29·394	29·02	29·05	29·11	29·04	29·00	56	39	45	49½	43	44	42	w.	calm	sw.	se.	·06	·01	......	·10
29.	29·780	29·344	29·13	29·10	29·40	28·90	29·26	54	37	44	50	43½	44	45½	sw.	w.	sw.	sw.	·23	·15	......	·20
30.	29·919	29·863	29·48	29·55	29·60	29·35	29·28	53	40	42	51	42½	43½	46	sw.	calm	wsw.	w.	......	·17	1·45	·13
31.	29·965	29·804	29·45	29·52	29·28	29·15	29·19	55	44	51	51½	46	47	42	sw.	calm	sw.	w.	......	......	......	·61
Mean.	29·852	29·600	29·38	29·493	29·514	29·487	29·504	49·38	37·71	41·9	47·2	39·3	42·48	42·37					2·85	2·60	3·24	4·38


Newbury
18th
17th night
Cape Cod
Chatham
Nantucket
P.M. 17th
New York
8 P.M. 17th
40
18 early A.M.
17th night
Cape May
17th P.M.
1 to 7 P.M.
Norfolk
17th early
5 A.M. to 2 P.M.
35
80
Cape Hatteras
75
70
Wilmington
Charleston
to 4 P.M. 16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Orkney Is
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Glasgow
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Donegal
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Strangford
Flambro. Head
Galway
Limerick
Holyhead
Yarmouth
Arklow
Lowestoft
Cork
Bristol
Deal
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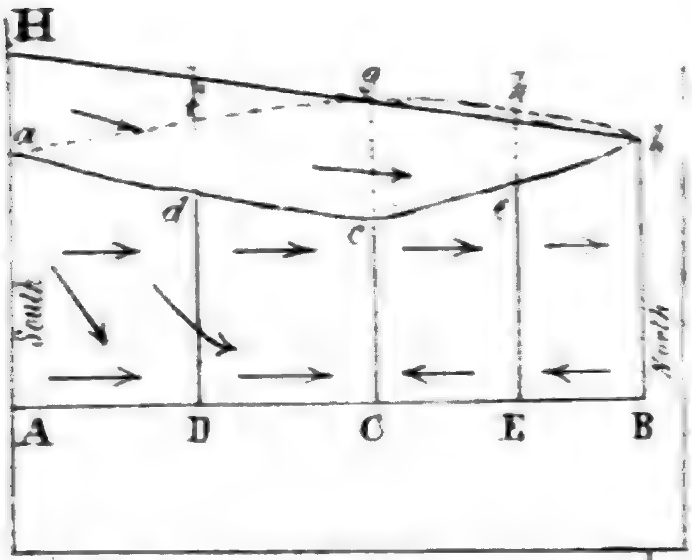
Fig. 1.



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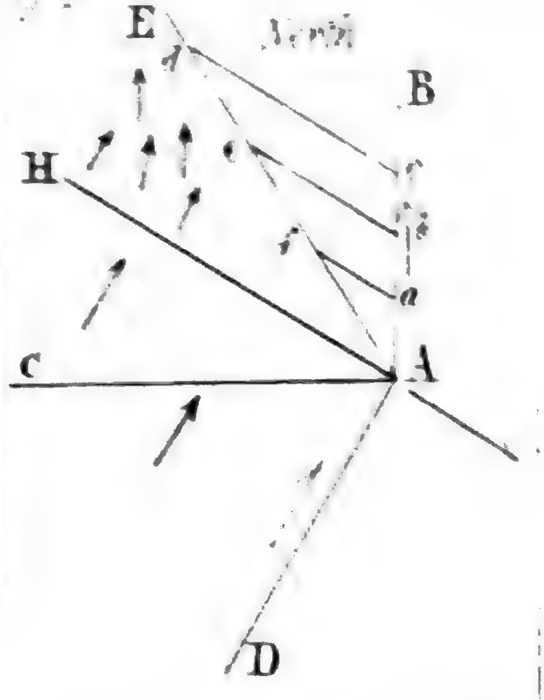


Fig. 3.

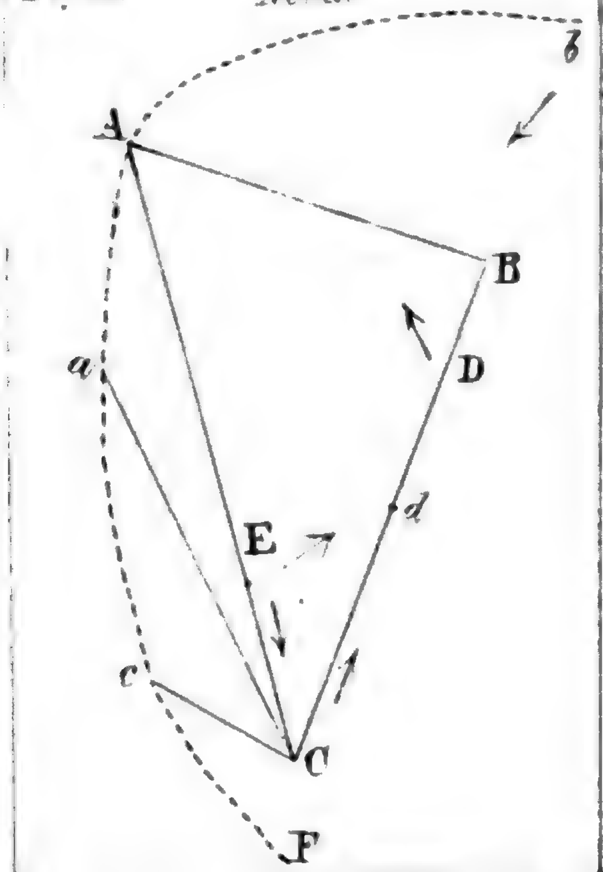
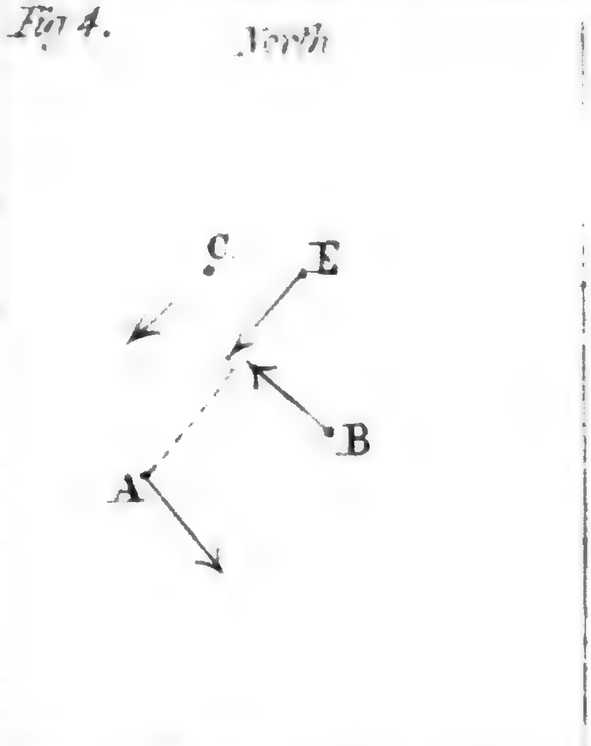


Fig. 4.



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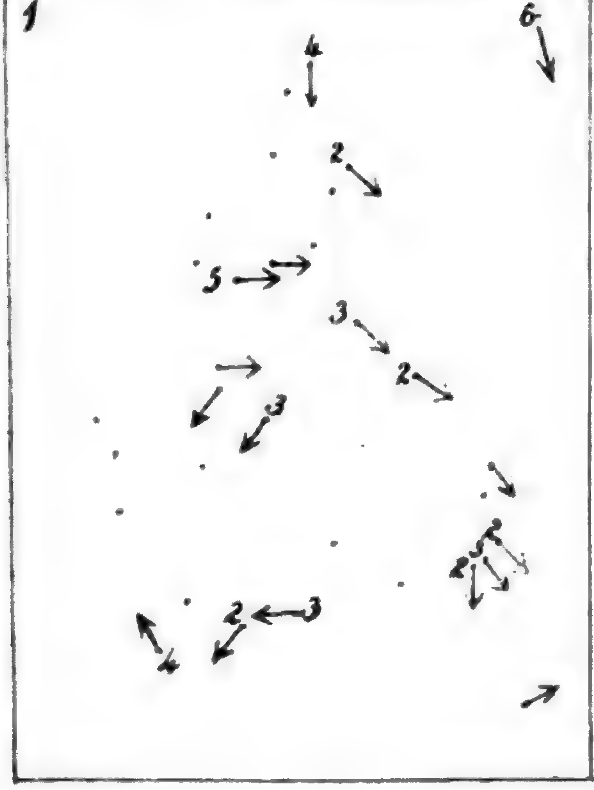

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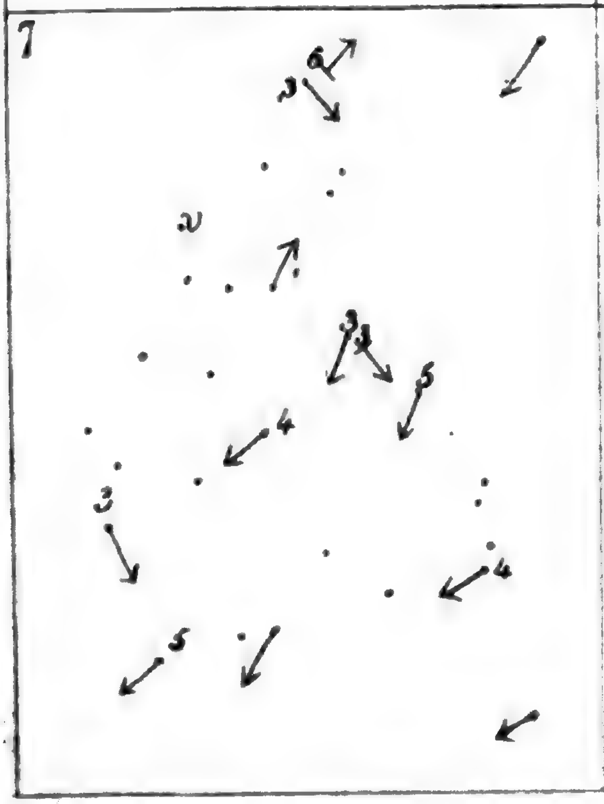

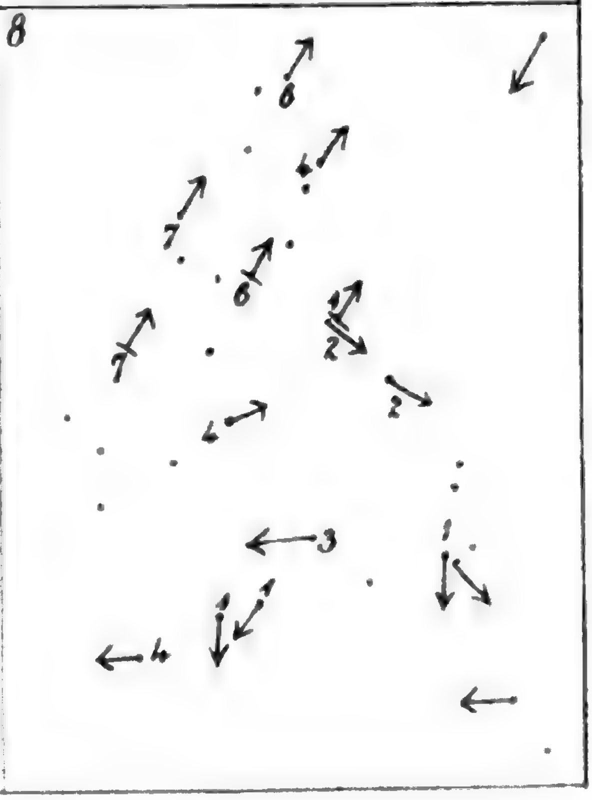

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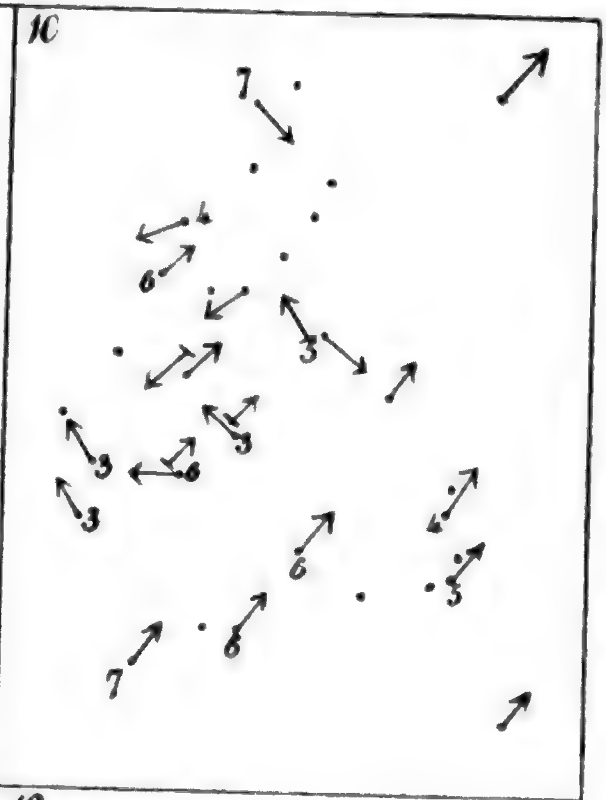

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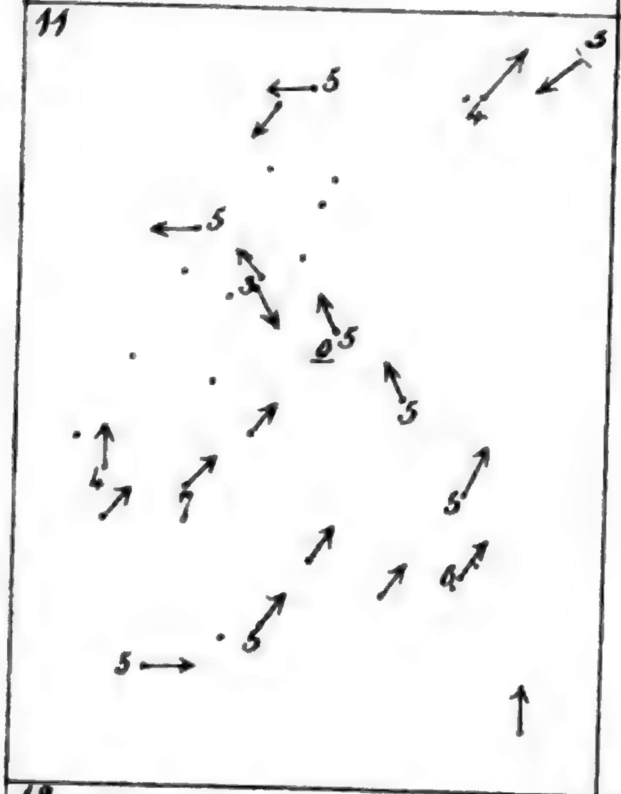

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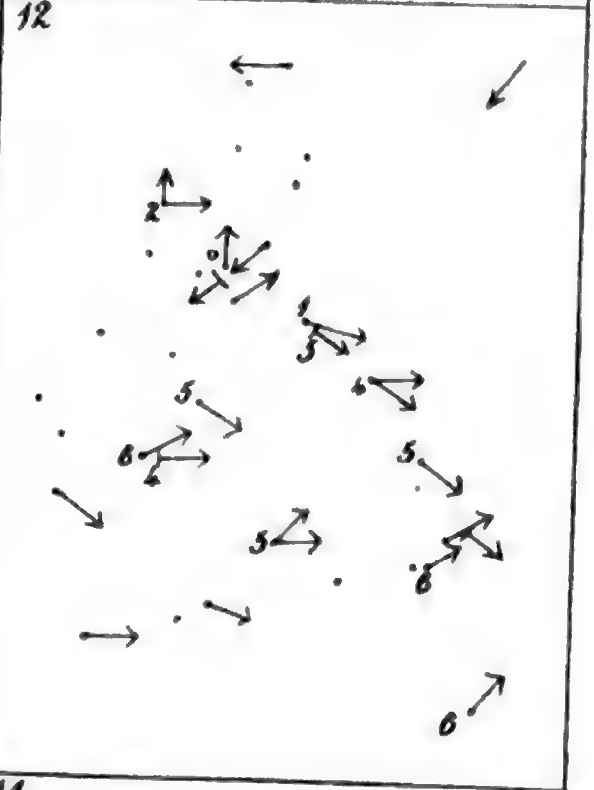

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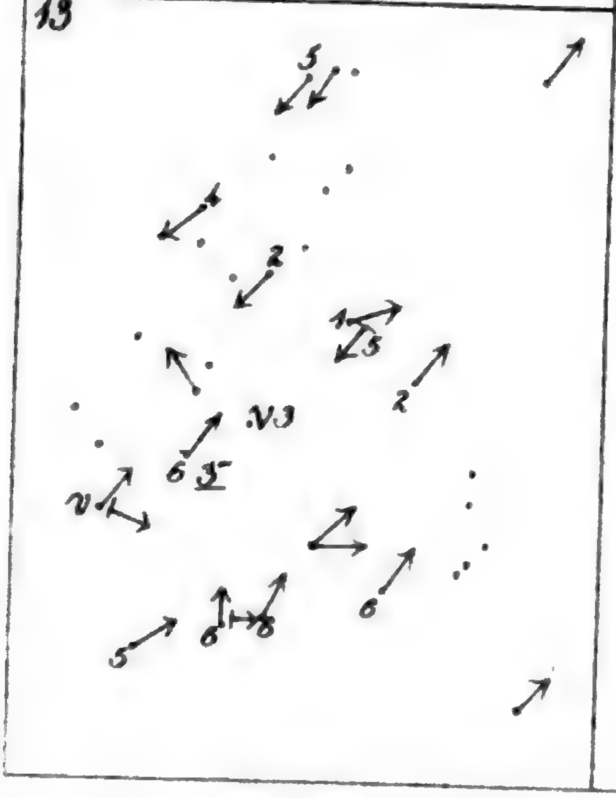

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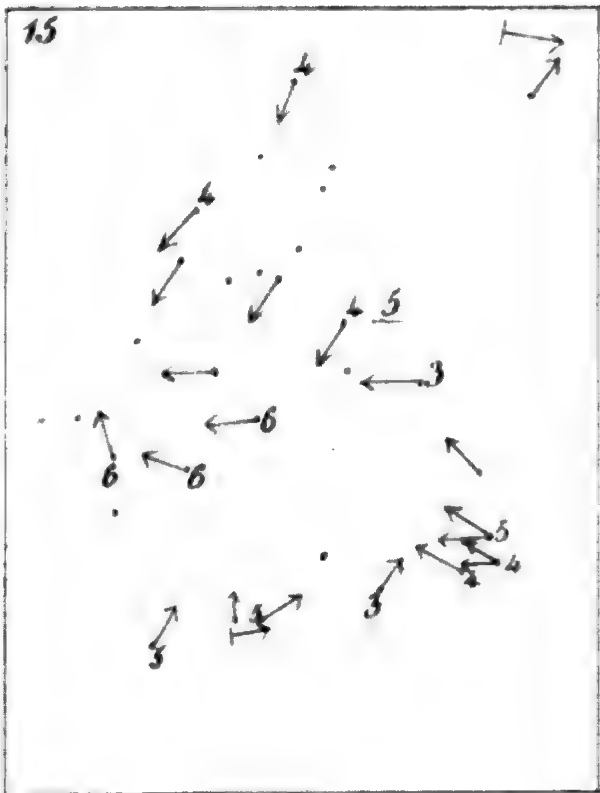

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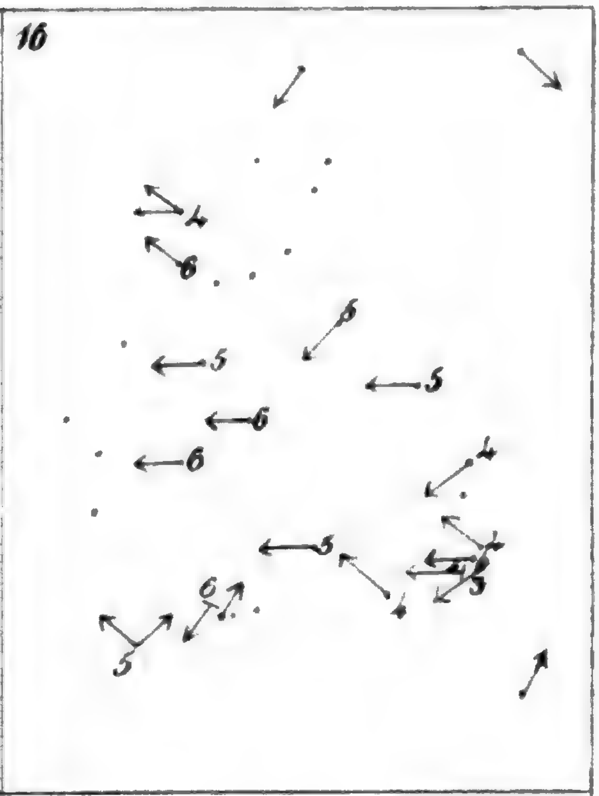

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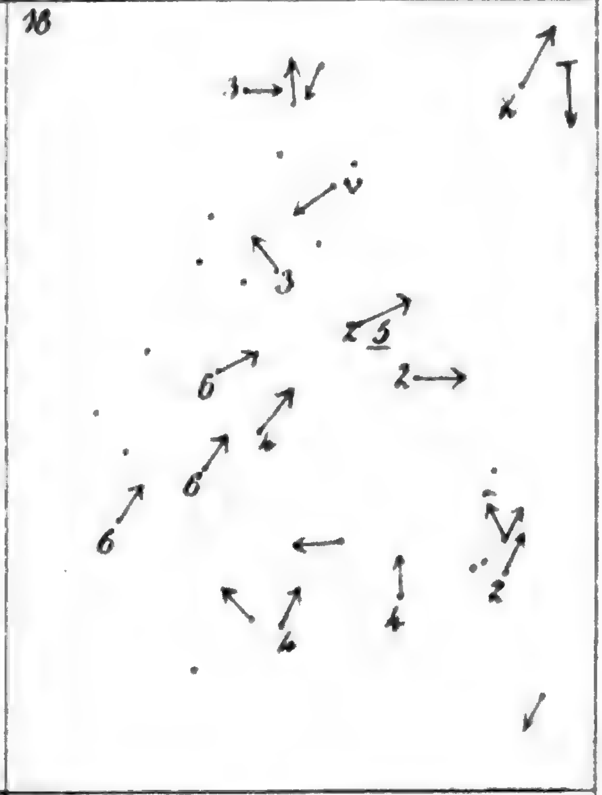

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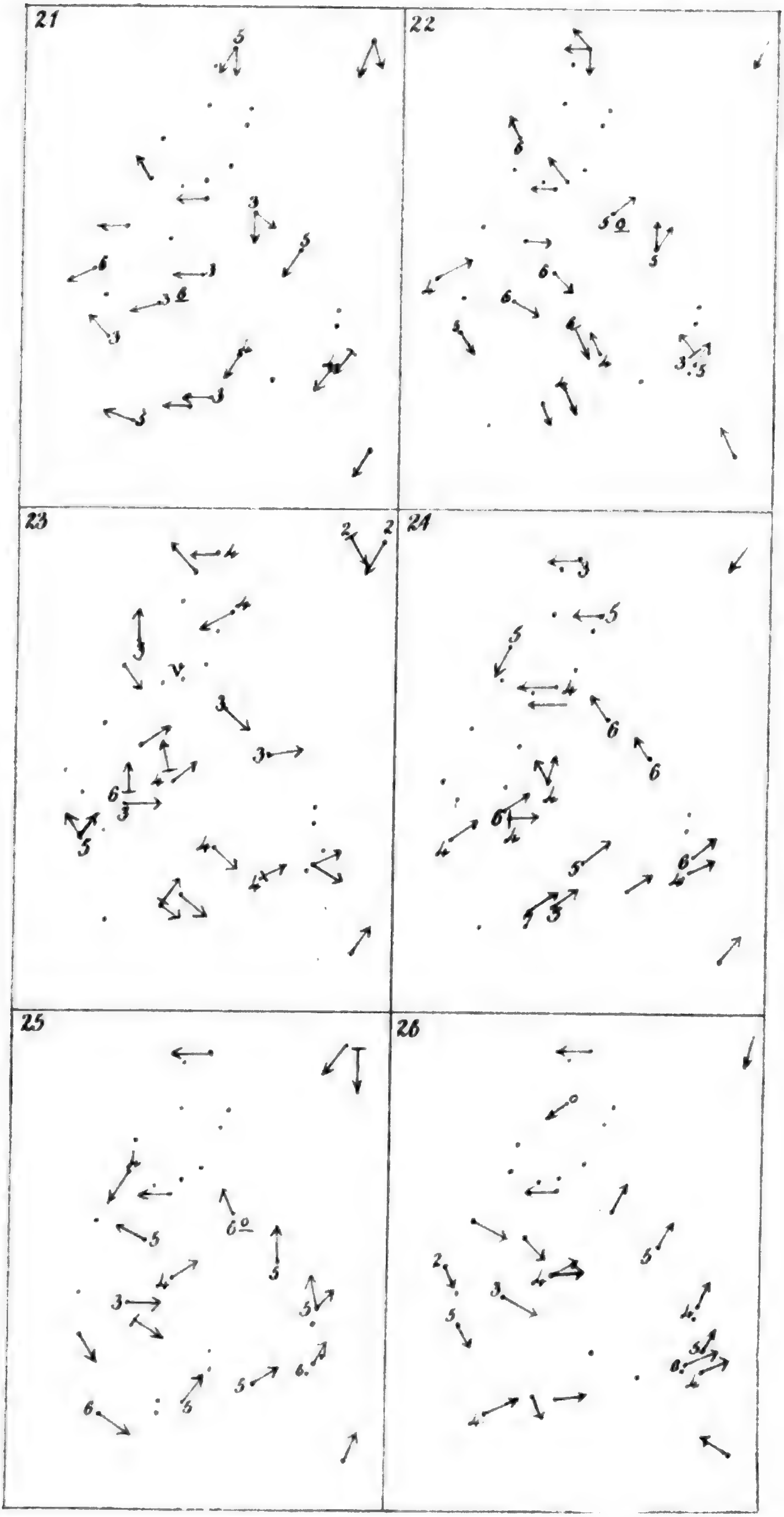
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THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

APRIL 1846.

XLV. *On the Oscillations of the Barometer, with particular reference to the Meteorological Phænomena of November 1842*[*]. *By* WILLIAM BROWN, *Jun.*[†]

[With Six Plates.]

IN order to illustrate and confirm the views I have before advanced in this Magazine (vols. xx. and xxiii.), on the connexion between the direction of the currents of the atmosphere and the oscillations of the barometer, I have endeavoured to make a direct application of them to the explanation of the various phænomena presented by the winds in this country during a great portion of the month of November 1842, that month including an extremely unsettled and stormy period of weather. For this purpose, I have collected observations showing the state of the wind and the barometer in various parts of this kingdom, and also at Christiania in Norway, and at Paris; and exhibited the direction of the wind on diagrams, with the variations of the barometer in hundredths of an inch annexed. The data from which the diagrams are constructed, and which are given p. 262, with the exception of those for North Shields, the place of my own register, are extracted from the Shipping and Mercantile Gazette Newspaper; a newspaper containing daily reports from most of the parts of Great Britain and Ireland, of the state of the wind and weather, on which nautical men are accustomed to rely for that kind of information; and the accordance of the observations at places situated near each other is sufficiently marked to confirm their general correctness, and thus give confidence in those of more isolated localities.

The barometrical observations have been collected with

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[*] This essay, with the exception of some additions, although only now published, was written soon after this period.

[†] Communicated by the Author.

great care: those for London and the Orkneys are taken from the tables published in the Philosophical Magazine and Athenæum, and those for Paris from the *Annales de Chimie*; but in general I have been indebted for them to the kindness of the observers themselves. I sought those from Christiania, which are from the register kept under the superintendence of Prof. Hansteen, in order to enlarge the field of the observations; it appears however from them that their locality is too far to the west to throw light on those of this country, except in a few instances; for this reason they are placed at the foot of the columns, contrary to the general order of the positions of the observations; and when reference is made to the observations as a whole, they are never included, except when specifically mentioned.

The whole of the facts brought to light by this investigation may, I think, be resolved into this general principle; that all winds may be ultimately referred to the action of one or both of two contrary currents caused by the unequal distribution of temperature on the surface of the earth; the one arising from the flow of colder and therefore denser air towards warmer, in the lower regions of the atmosphere; and the other from the descent to the surface of the earth of an opposite current belonging to the upper regions of the atmosphere, and formed by the elasticity or total weight of the atmosphere at any elevation in the warmer regions, being greater than that at the same elevation in colder, because of the greater height of the atmospheric column in the former than in the latter; the air in both being supposed of the same pressure at the surface of the earth. Thus in fig. 1, Plate V., the outline H *b* B A represents the general figure of a portion of the atmosphere, of which the temperature decreases from A to B (A lying on the equatorial and B on the polar side of C, or A being south and B north): the lower current of heavy air will therefore set in from B towards A. But the pressure being equal at the surface of the earth, and greater at any equal elevation in the column H A than in *b* B, at some certain height above A the pressure or elasticity of the air will be so much greater than at the same height above B, that its force will there overcome the pressure of the colder air of the column B *b*; and hence above this the air will flow from H A towards *b* B. But these currents can only be maintained by air descending and ascending in some part of them: let the upper current descend to the earth at A, it will continue to flow towards C, as represented in the figure, by the momentum acquired in its original position. These currents are the north and south winds.

From this may be deduced the following results, which may be applied to the explanation of the general atmospheric phænomena of all latitudes of both hemispheres, though when the cardinal points are referred to, they are given in terms adapted only to the northern, and to extra-tropical latitudes; for the sake of convenient reference to each, they are placed in numbered paragraphs.

1. An equality in the pressure of the atmosphere can only be maintained by the flow of these currents in their proper, that is, in their original positions, the one above and the other below; but on the descent of the upper current, which still, by its momentum, maintains wholly, or in part, its original direction, the lower is either more or less retarded, or entirely pushed back: thus whilst the air is carried away, either from the upper parts, or whole of the atmospheric columns, the flow to the lower is prevented, and consequently a diminution of air, or decrease in the atmospheric pressure, takes place in the regions where the descending current prevails on the surface of the earth: hence the great oscillations of the barometer in high latitudes, or the region of the "variable winds," and the maintenance of the equality of the atmospheric pressure in the region of the "trade winds," the ascent and descent of the air taking place at the extremities of the latter, and consequently without interruption to the course of the currents.

2. In the extent of the descending current, and for a space in front of it, following the line of its direction, the pressure of the atmosphere upon the surface of the earth will be distributed according to the curved line *a d c b*, Plate V. fig. 1; and the lines A*a*, D*d* and C*c*, &c. will represent the pressure at the several points on which they are drawn, C being supposed the point at which the descending current terminates and meets the opposite or lower one, as shown by the arrows, whilst the southerly current is blowing above the north from *c* to *b*; the minimum pressure will be near C, or the place of meeting of the two currents. That this will be the case is evident from the consideration, that the descending current advances by reason of the superiority of its force to that of the lower one, which it drives back; but this superiority is constantly diminishing by the rarefaction of the air produced by its flowing from *c* to *b*, which rarefaction at last reduces the force of this current below that of the resistance of the opposite one in front of it; hence it is clear that where the descending current terminates in advancing from the place of its descent, or is overcome, as at C, the pressure must be at the minimum, increasing from this point in both directions,

but in the greatest degree towards B, because of the resistance given to the upper current in its flow northwards after passing this point, and the increasing density of the air in the colder current. In my first paper on this subject (vol. xx.), I gave an illustration by a figure, in which for want of due consideration a rise of the barometer was conceived to occur at the point C; but in a subsequent paper on the "Storms of the Tropics" (vol. xxiii.), it is assumed that this is, as here expressed, the point of the greatest barometric depression, though I omitted to notice the discrepancy. As this part of the subject is of great importance, it may be proper to give a further explanation of it. The upper current is supposed to descend to the surface of the earth between A and C, on which it will flow to a certain distance dependent on its power to overcome the opposite one from B, and C is the point at which it meets this current and advances upon it; here therefore there will be an influx of air from both sides at the surface, whilst that above is carried away in a continuous current from *c g* to *b*. Now it will at once be evident that in this position of the currents, any change produced in the atmospheric pressure will depend upon their relative velocities; if that of the upper one is so much the greatest, that notwithstanding the check given it by the opposite force, more air is carried off from the higher parts of the atmosphere above C towards *b* than is brought to that point in the lower, the pressure at C must diminish. But the diminution of pressure thus begun by the force of the descending current, will go on until this force is reduced, by the loss of pressure sustained, to an equality with that of the opposite one, and then its momentum being destroyed it will cease to advance, and the latter will begin to advance upon it, to restore the equilibrium of the atmosphere; hence the point of its furthest advance and first cessation must be near that where the diminution of the atmospheric pressure is greatest, or the point C; and at this point, in great storms, there will be a comparative calm throughout a certain extent of the atmosphere. That the conclusion resulting from this reasoning is in consonance with observed fact, may be seen from the observations of P. J. Espy, who has shown that the space between the opposite sides of a storm is in reality the place of minimum pressure in those storms of America which he has investigated*.

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* This, according to this observer, is the position of the fall of rain which occurs during storms, the fact upon which he has founded his theory; but it will be seen that in this case the conditions are precisely such as, according to the general opinion of meteorologists, are requisite to produce rain,—these are, the meeting of two currents in precisely opposite conditions

3. It is not meant however in the foregoing paragraph that the greatest depression of the barometer throughout the course

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with regard to temperature and the quantity of aqueous vapour they contain; and hence the true nature of the connexion between the occurrence of rain and a falling barometer, both consequences of one common cause.

The phænomena attending the fall of rain are extremely dependent on geographical position, and are by no means sufficiently known to enable us fully to carry out these principles to the explanation of them, although I am firmly persuaded that when better known we shall be able to do so. I will however just refer to a few cases similar to that mentioned, of which they do give a sufficient explanation, and which are notorious weather laws:—the occurrence of rain;—just before a change of the wind, or at the time of the change, whether it be from north to south, or from south to north, though the most conspicuous in the former case; during a north-east wind with a falling barometer (§ 5), and with a south or south-east wind (occasioned by the junction of a north-east and south wind, see § 11). The last of these is the most conspicuous in the portions of storms to which § 16 refers, which in those parts where the wind is from south to south-east are always accompanied by abundance of rain. It may be thought that our dry winds from north-west (also formed by a north and south wind (§ 11)) are an exception to these results; but it is by no means necessary that rain should always occur at the meeting of these currents, for if the lower current greatly predominates in dryness or in quantity, then it is evident that there need be no precipitation of vapour in the form of rain. But there is another reason why these winds should be in general free from rain. The occurrence of rain in showers with squalls of wind, when the other portions of the day are fine, is a case to which the principle before us strikingly applies, for these squalls almost always blow in a direction somewhat different from that of the wind in the intervals between their occurrence; thus showing that they arise from an immediate onset of one or other of the opposite currents: now it is very easy to conceive that two bodies of air may meet so as to produce rain, although their relative temperatures and quantities of vapour may be so adjusted, that the resulting temperature is sufficient to maintain the same quantity of vapour; for if the collision be sudden, by the law of the diffusion of gases and vapours the vapour of the warm air will rush at once into the cold air, not waiting for the mixture to take place; and hence, being subjected to its temperature, it is immediately condensed and the rain is produced. Now (§ 11) the north-west wind is one of the most constant winds, hence one of the most favourably disposed for the gradual mixture of the opposite currents. This also explains the occurrence of fine weather with a steady barometer, for stability in the pressure of the atmosphere can only be produced by the stability of the currents. The formation and disappearance of clouds without rain may be explained in the same manner,—the precipitated vapour not being sufficiently dense to form rain is again aërified when the cold air acquires the temperature of the mixture. [For a full description of the differences and relations of the distinct atmospheres of air and vapour by which the globe is surrounded, and on which this reasoning is based, I need scarcely refer the reader to the 'Meteorological Essays' of the late Professor Daniell, where they are set forth with great perspicuity and precision.]

But perhaps the fact most remarkably in accordance with this application of the principle set forth in this essay is that general one, established by W. Snow Harris by induction from a great number of particular instances, that thunder-storms result from the collision of opposite currents; for the of a wind or storm* is at once attained at the point C, which is there supposed to be that of the furthest extent of the descending current from the *first* place of its descent, and the place of the barometric minimum in the parts *then* occupied by the wind. The pressure continues to decrease for some time by the progressive motion of the storm. It is evident that air having begun to descend at A and to flow forward with considerable velocity in the same direction as before its descent, the lower air on the south of A must at once begin to flow towards A to supply its place; but as this air is either at rest or in a state of motion in another direction, it cannot at once begin to flow with sufficient velocity to supply the deficiency; therefore the rarefaction thus produced will cause the upper current to descend into it, and thus the space upon which it flows will gradually extend itself backward from A, or southward. But at the same time that this is going on behind A, the advanced portion of the descending current has begun to retreat from C; for its force there, at first superior to that of the opposite one, is at last overcome by it, and the heavy current of cold air then advances upon the receding wind, flowing with a force in some degree proportioned to the degree of the rarefaction, and restores the air to its ordinary pressure†, according to one general law of storms, that when the wind changes from south to north the barometer begins to rise. Thus the point of minimum pressure, C, or the furthest advanced portion of the storm, and the point of its first occurrence or descent, A, both move in one direction, from north to south, but not equally; for it is obvious that the portions of air which descend after the first have an advantage over the latter in this, that the opposition in front being partially removed by the first portions and the diminution of pressure begun, they flow towards a rarefaction; and thus the force of the storm and the diminution of pressure at C are increased, and the motion of this point southward is retarded, whilst the wind is progressing on the south from A. But this disproportionate motion of the two extremities cannot remain, for with the increase of the rarefaction there is an increase of the force of the

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torrents of rain which frequently fall during their occurrence seem to manifest that the only difference between this and the fore-mentioned cases is in intensity.

* The only distinction here inferred between wind and storm is that of force; in many instances I use the term storm, because of the phænomena being sufficiently striking only when the wind has great force.

† In the hurricanes of the tropics, the returning current is a second storm, and it is sometimes so in those of high latitudes; but in the latter the rarefaction of the atmosphere is so extended, that the restoration of the pressure is frequently very gradual and produced by moderate winds.

north current, which will after a time carry backward the point C with increasing velocity, and gradually put a period to the storm*.

This is more conspicuous in storms of temperate regions than in those of the tropics, because the former consist, as it were, of one deep wide depression of the atmosphere, the progressive motion of the storm being, so far as regards the movement from north to south, apparently in great measure an enlargement of this depression southward, whilst the latter occasion a much smaller one.

4. As a direct consequence of the foregoing, and as also proved by P. J. Espy, the greatest reduction of atmospheric pressure in storms is not where the wind is most violent, but where its velocity is reduced by the resistance in front of it; and the depression of the barometer at any given place depends on its position with regard to the place of the minimum, or the point C, fig. 1, as well as on the violence of the storm.

5. A further consequence of the same result is, that a considerable diminution of the pressure of the atmosphere, and consequently, fall of the barometer, takes place on the locality where a north-east wind is blowing, when this wind is immediately on the north of the northern range of the south wind which occasions the fall (as at the point E in the current from B towards C), though, as explained in § 2, to a less extent than in the localities occupied by the south wind. This result explains a fact of very frequent occurrence, the falling of the barometer during a north-east wind.

6. As the impetus of the south wind may have reduced the elasticity or pressure of the air, in the column C *c* at any elevation of the atmospheric columns, below that of the air of the same elevation on the north; a very rapid increase of the pressure of the atmospheric columns on the north may give so great a check to the upper current at C, as to cause the air to accumulate so rapidly, that the increase of pressure or rise of the barometer extends to a great distance on the south; but it is yet evident, from the state of the atmospheric columns shown by the figure, that the south wind will continue to flow from A because of the greater pressure there; but the barometer will rise on account of the accumulation of air taking place at C and extending in a diminishing degree towards A; hence a frequent phænomenon, the rise of the barometer during the continuance of the south wind; as also

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* The deflection from south by the rotation of the earth is for the present left out of consideration, see § 15.

the beginning of the rise of the barometer, frequently some time previous to the setting in of the northerly current.

7. But an atmospheric pressure above the mean will also result from the opposition of these currents, but an opposition differing from that of § 2 in this respect; that whereas in that case the force of the descending current, originally much the greatest, is reduced at the place of its termination to an equality with that of the opposite one by the diminution of the pressure of the atmospheric columns composing it; in this, where a rise of the barometer takes place, the force of the lower, or current of gravity, is equal or superior to that of the descending one when at its full pressure, and the former is advancing upon the latter. Let then the two currents so circumstanced meet, as in fig. 1, at C, there will be at this station either simply a condensation of the air produced by the pressure of the two currents, or the air of the lower one will ascend, carrying with it an impetus which would tend to carry it on still in its first direction. In the case of § 2, air brought to the place of meeting is carried off in the upper current by the force of that current, but in this, if the air of the lower current does in this way ascend, it will simply check that flowing above in the contrary direction, and cause an accumulation of air to take place exactly similar to that of water occasioned by partially damming a stream. But even when the force of the former is in some degree inferior to that of the latter, it may yet be sufficient to retain so much air of the upper current by its opposition as to accomplish the same effect though in a less degree: thus the pressure will be represented by the dotted line *a g b* in fig. 1, and an elevation of the barometer will ensue in the localities where the south wind is blowing, as well as in those which have the north; hence great elevations of the barometer occur with south winds as well as north.

8. The foregoing paragraph is intended to explain the great atmospheric pressure sometimes produced by strong north-east winds, and calms or very gentle breezes, as one or other of these is produced (§ 12) at places situated near the collision of the two currents, especially when the effect is increased, as shown in a previous essay, by reduction of temperature; but increase of pressure will arise from other causes; as in some locality sufficiently far to the north of a strong southerly wind to be out of reach of its influence in depressing the barometer, and upon which the air from the depression flows; though probably in this case the barometer will not rise in a great degree, on account of the air, whose removal causes a deficit of pressure in one locality, being so extensively

spread by the flowing of the upper current on others; the reason why elevations of a degree corresponding to depressions at any given place have never been found by comparative observations*.

9. An elevation of the barometer may also be the consequence of a previous reduction; for let the pressure be reduced, as at C, fig. 1, the returning air or northerly current which sets in after the cessation of the southerly one, will occupy a portion at least of the higher regions of the atmosphere where it is not wont to flow, as is evident from the figure. On the restoration therefore of the usual pressure, the north current will be blowing not only in its own proper region, but also in part of that which properly belongs to the southerly one, and will continue there some time by reason of its acquired velocity after the original impulse has ceased to act; and thus the upper current, not resuming at once the whole of its action, and consequently the air not being allowed to flow from the upper parts of the atmospheric columns as rapidly as it is brought to the lower, will accumulate. In like manner, the elevation of the barometer may be the cause of giving to the upper current a great velocity, for an elevation being anywhere produced, the force by which the lower current causing it was urged on, must sooner or later be overcome by the increase in the pressure of the air towards which it flowed. But this current being overcome, the overplus of pressure will then increase the velocity of the upper one, and probably determine the flow of the air at the surface of the earth in the same direction; which indeed is frequently the way in which an elevation of the barometer subsides; and in the observations given in this essay, in which are included two periods of stormy weather, both began with the occurrence of a southerly storm after a high barometer.

10. The direction of the wind when one current alone prevails, is determined by the relative situations of the warm air and cold, and the deflection of the current thus produced, by the rotation of the earth, as the "trade winds" and monsoons of the tropics, and the north-east wind of high latitudes; but when the opposite currents come into collision, the direction is the resultant of their forces, and thus in the latter regions we have winds from every point of the compass, as has been pointed out by Prof. Kæmtz.

11. The action of these currents meeting together and producing the various winds may be considered as follows:—The south-west and north-east winds blowing from two stations, A and B, and meeting together at a station between them, C,

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* Daniell's Meteorological Essays.

may cause a wind in any direction, according to their force and their inclination to one another; if it be on the eastern side from any point of the compass between a north-eastern point and some point between south and west, it will be very liable to change; for winds from these points being produced by the direct collision of the descending current with that from north-east, when the latter is blowing on the surface of the earth from some more northern station, require the continuance and stability of a current whose direction, inasmuch as it is from east, depends upon its actual velocity, and which, in some parts of it at least, must be more or less interfered with by the flowing of the resulting wind; hence the winds from due south, or south of east, are the most inconstant and the least frequent of all the winds. But the case is very different with the winds on the contrary or western side of the compass; for as the direction of the southerly current is formed in the upper regions of the atmosphere, and consequently is not interfered with by that of the resulting wind below, and the opposition of the air which ought to form the northerly current being simply that of a pressure from north when not actually flowing towards the south, and only in some degree affected by the rotation of the earth when the wind is north-west, the conditions which are necessary to produce the westerly winds are much more capable of giving them stability and duration. When these winds, instead of meeting with their forces directed more or less obliquely to each other, meet in direct opposition with nearly equal strength, a calm or very light wind is the consequence.

12. But a rarefaction of the air being anywhere produced, the direction of the wind may be further modified by the situation of this rarefaction, with regard to the atmospheric columns adjoining it, where the rarefaction does not exist; thus as storms in high latitudes move towards east (§ 15), the returning current in some parts of the storm is deflected from west.

13. The cause of these currents being the difference of temperature of adjacent portions of air, their force will depend upon the amount of this difference; hence it is much greater in winter than in summer, when the great length of day in high latitudes lessens very greatly this difference*; hence also as the degree to which the reduction of the atmospheric pressure can be carried by the flowing of the upper current (§ 2) depends on the force of that current, and the degree in which air can be accumulated (§ 7) likewise depends on that of

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* See Phil. Mag. S. 3, vol. xx. p. 467, "On the Oscillations of the Barometer."

the lower one, both the depressions and elevations of the barometer are the greatest in winter.

14. The forces which urge on these currents are accelerating forces; but the lower current being exposed to the friction occasioned by its flowing along the surface of the earth*, as also to checks given by inequalities of temperature, its force is unequal to that of the upper one; hence storms are by far the most frequent and violent from south.

15. The progressive motion of a south-west wind or storm has been in part previously considered (§ 3), but requires further notice; it will be according to figure 2 of Plate V. Let the upper current descend upon a station A (the top of the page in this figure and those of § 16 and 17 being supposed the north) with more or less force; as shown in § 3 and the essay on "The Storms of Tropics," the storm moves or recedes from A, in the direction from B to A; but blowing from south, it is carried by the rotation of the earth towards east, or as if impelled in the direction C A; hence its actual path is the resultant of these motions, or that shown by the arrow at A. In this direction, therefore, the storm will arrive at the places which it visits, or as the line A D moving in a direction perpendicular to its length; hence also H A will be a section of the two currents at their place of meeting, and consequently the line or parallel of the line of the minimum atmospheric pressure (§ 3), extending in the same direction, or that shown by the arrow. The progressive motion however will not be in the same direction throughout; for let A be the place at which the current descends on the arrival of the storm, it will advance a certain distance along the surface. In the progressive motion just considered, there is a comparatively rapid motion from west, because although this motion is opposed by the air in front of it, yet it is principally on the east and by air at rest, for as the storm recedes and one portion of air descends behind the previous one, the opposition on the north is in part removed by the first descending air from that which descends after it; but when the air, as in this part of the storm, advances from south to north, this opposition is felt at every degree of its progress; hence the path taken by the wind in this case is simply the resultant of the directions of the two forces, but that from south-west being much the strongest, it is from a point much nearer to this than to that from which the opposite force is directed. Now

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* See Phil. Mag., October 1843, "On the Storms of the Tropics," p. 277. I may here correct an error in the note on that page: it stated, that omitting the two months in which the change of the monsoons occurs, the difference of the atmospheric pressure of the two seasons at Canton is nearly one-third of an inch: it ought to have been nearly half an inch, or 0·44.

agreeably with this, we find that the direction of the wind in such cases is generally at first from S.S.E., but as the storm continues, it changes to S. or S.W.; according to the explanation given of the lateral motion of the receding wind (one portion removing the opposition for that behind). Therefore let us suppose the direction of the advancing air to be from due south or along the line A B; then *a*, *b*, *c* will be stations at which it arrives in its progress; but at the time it reaches any of them the storm will have moved on a certain distance from A in the direction of the arrow; therefore suppose A to be moved back along the line H A, then the wind will arrive on each point of the line E A from a point immediately south of it on the line H A; and if the intervals of time which have elapsed on its arrival at stations equidistant with *a*, *b*, *c*, from the line H A in a direction due north, or parallel to A B, be severally represented by the lines *a f*, *b e*, and *c d*, parallel to the arrow, the wind will arrive at the stations *a*, *b*, *c*, or every point on the line A B, as *f*, *e*, *d*, or the corresponding points of the line E A, supposing it to move together with the line D A. Thus as fresh portions of air will advance as the point A moves forwards, the storm may be represented by a moving body of air, within which the wind is S.W., S. or S.E., and whose progressing front has the shape D A E; and we may name that part of it represented by A E the advancing portion*, and that by D A the receding portion, according to the nature of the motion.

16. In storms of the tropical regions, and in those of high latitudes commencing with the usual atmospheric pressure, the former of these is apparently insignificant; but in some cases this portion of the storm extends over so great a space, and the phænomena presented by it are so peculiar, that it will require a distinct consideration. It is evident that its extent, or the distance to which the wind advances, will depend not only on its force, but also on the greater or less resistance of the air, or in other words, the greater or less pressure of the atmosphere in front of it (the resistance from the difference of temperature being supposed the same in all cases); hence we find that the cases in which it is traced in the following observations to a considerable distance, and to which this paragraph is intended principally to apply, are those in which the height of the barometer has been much reduced by

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* This must not be confounded with the second period of storms, in which the north wind advances upon the receding south. For a description of the winds in *both* the parts, A C and C B, fig. 1, of a progressing body of air, see this Magazine, vol. xxiii. p. 214; if the several directions there given be reversed, it will apply to this case. It is to tropical storms that we must look for an exhibition of these phænomena in their greatest simplicity.

a previous storm, and not restored on the arrival of the advancing storm, which approaches from atmospheric columns on the south but little affected by the preceding one; which difference indeed is apparently the cause of the storm; it occurs however after the returning or northerly current has set in in the north, and a little raised the height of the barometer there; and on its termination, the height of the barometer in the south is reduced as low as that in the north, in some parts of which it is depressed to a greater degree than before, the fall beginning in the south and advancing towards north as the wind itself. But not only this, the minimum height to which the barometer is reduced, and the setting in of the north wind (though always from north-west), occur first in the south, but on the western side (by the south being now meant the parts along the line H A of fig. 2, Plate V., where the upper current is supposed, with regard to localities north of them, first to descend); so that, in the south, the north wind is blowing and the barometer rising, whilst the south wind is blowing and the barometer falling in the north.

The advance of the south wind northwards after its cessation on the south, admits of very easy solution on the supposition of the storm being carried forwards by means of portions of air descending from an upper current flowing in its proper position, and which the reduction of the height of the atmospheric columns towards which it is moving, allows to flow with its velocity little checked; thus the south wind continues below, not by the force of the original impulse received at its outset from the line H A, but by that of successive impulses received during its course: moreover, the rapid increase of the heights of the atmospheric columns in the south by the influx of air from north-west, still maintains the upper current of the atmosphere.

The setting in of the northerly current from north-west in the south, and the consequent rising of the barometer before these changes occur in the north, may be explained by a consideration of the form of the space, on which the depression of the barometer previous to the occurrence of the second one produced by the advancing storm, exists. Suppose the greatest depression of the barometer to be produced along the line A B, fig. 3, Plate V. (H A of fig. 2), according to § 15, and the full effect of the first storm in depressing the barometer to have taken place; as that storm moved from A to B, the pressure will be in some degree restored on the parts of this line towards A; hence the depression will decrease from B to A; but it also decreases towards C (§ 2); hence the form of this space on the west will be somewhat like that bounded by the line F*c* A *b*, the pressure increasing from B towards all parts of that line. Under these circumstances, then, the advancing storm occasioned by the greater height of the atmospheric columns about C than of those about B, occurs, and reduces the barometer at C, so as to make it the place of the greatest depression, the line *c* C now representing the line H A of fig. 2. But whilst the south current is thus setting in from C, the north-east is blowing from *b*, and the two meet somewhere between these points; and there, as at D, fig. 3, the wind is south-east (§ 11), though south-west in the localities south of it; but as the direction of the current which carries off the air so as to produce the depression is from south-west, the depression will be produced in that direction also, or along the line C B. Now if we consider the storm to have advanced from any line drawn from C towards the circumference of the depression as C *c*, it is evident that whatever part of this we suppose a particular portion of the current to set out from, its condition will be the same, that of having at the time when the pressure of the atmosphere is reduced to the lowest point, a greater atmospheric pressure on the north-west side, or along lines drawn from it to the circumference *c* A *b*, as C *a* and C A, because of that side being always adjacent to atmospheric columns less and previously affected by the storm; hence on the line C *c*, the occurrence of the minimum in order of time will be in the direction from *c* to C, or that of the receding storm (§ 15). Let then the barometer be reduced to its minimum height at C (supposed the place where the storm is most intense), or to the limit of its equality with the resistance of the air in the direction from A to C, and let the air at *d* be reduced to the same degree of rarefaction, or to one (as afterwards to be noticed) not quite so great. These stations are subject to the influence of the pressure increasing along the line C A; hence from any station E on that line, the air will tend to flow towards them, as indicated by the arrows at E, the effect of which in the case of C is to oppose the southerly current, but in that of *d* rather to assist it; hence whilst at C it is overcome and the wind sets in from north-west, it continues its course at *d*. The commencement of the restoration of the pressure of the atmosphere advancing thus from C towards D, whilst its advance from west is, as before stated, along the line *c* C or A C, it is evident that every point of the line C D B in succession from C will be related to a line parallel to A C, precisely as the point C is related to the line A C at the time of the setting in of the north wind; and the restoration of the pressure will commence as the line A C advances along the line A B, except that as it approaches B, from the great extension of the minimum depression in the north, the point A may move more slowly than the point C; near this time, however, the north wind begins to set in directly in front of the storm from north-east, and then the restoration of the atmospheric pressure proceeds throughout.

But not only does the minimum of the barometer occur first at the point C, its depression is also sometimes the greatest at this point; thus at 9 A.M. on the 11th, its height at Cork was 28·93, at Belfast 29·10, and at Plymouth (to the south-east) 29·11. Thus it appears that the barometer in this instance was about one-tenth of an inch lower at a station similar to the point C than at the point *d**. In § 2 it is stated merely that the minimum height of the barometer is near the point of meeting of the opposite currents, in order to simplify the reasoning of that paragraph; but it is evident that it will be rather to the south of it, for at this point the resistance of the air in front of the south wind at the surface of the earth, is either equal to its force, in which case it advances no further, or if not equal to it, it is yielding to it and retreating. In either case the opposition will cause more or less condensation where it is immediately felt, but the opposition decreases towards the higher regions of the atmosphere; hence (§ 2) the upper current, whose force is only exhausted by the destruction of its momentum, carries off the air from the point *d* (which, for the sake of illustration, let us suppose the limit of the south wind), and the positions south of it, so as still to carry on the reduction of pressure; but in the greatest degree a little south of *d* (or at C), on account of the condensation decreasing from *d* towards C. Now in storms in which the advancing portion is insignificant, the minimum pressure will be very near the point C of fig. 1; but in such as those now in consideration, it is apparently at a great distance, though the difference of the pressure at the point of the meeting of the currents, and that of the minimum pressure, is very slight. The distance, however, is in a great degree only apparent, being occasioned by the nature of the advance of the storm.

17. In high latitudes, the warmer regions, except when they

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* This appears to me a strong confirmation of the belief, that the origin of the south wind is that upon the supposition of which this theory is founded, for how upon any other than that of air descending from a current which flows by an acquired velocity, could a current flow from one station, C (fig. 3), to another, *d*, of a colder temperature and greater pressure? In this paragraph, as also in those which follow it, it will be observed that I have been obliged to depart from the form of the reasoning in the others, that of simple deduction from the principles stated at the outset, or from the results of previous paragraphs.

are influenced by geographical situation, have their position, with regard to colder, constantly on the equatorial side of them, both in winter and summer; but in tropical regions these positions in summer are reversed by the change in the point over which the sun is vertical; hence the currents also are either permanently reversed during this season, as in the monsoons of the Indian ocean; or, as in the "trade winds," their position is altered and their constancy interrupted, and sometimes at least their direction reversed*; thus the tropical hurricanes of the Atlantic, which occur only during summer, and the storms of the Indian ocean, which occur during the same season, that is when the south-west monsoon is blowing, have their relative parts precisely the reverse of those of high latitudes, the descending current being from north, from which quarter the storm commences, and the returning one from south, whilst the progressive motion is towards north-west.

These storms visit only the western parts of the Atlantic, a fact which, on the supposition of their origin being the descent of the upper current, is readily explained by a glance at the position of the continents of Africa and America; the western part of the Atlantic having the latter stretching out on the north of it and radiating the heat of the summer's sun, whilst the former extends on the south of the eastern parts, having only the waters of the ocean on the north. But these storms near the boundary of the tropics change their direction† and pass along the eastern coast of North America and parts adjacent, and present phænomena different from those of storms which take their rise in extra-tropical latitudes, but to which the explanation given in the foregoing paragraph of the phænomena of the advancing portion of storms in the longitudes of Europe, may with some modification be applied. In my paper on the Storms of the Tropics, I have referred to two storms of this kind, which advanced along the coast of the United States in a direction from S.S.W. to N.N.E., of which the data collected by W. C. Redfield are given in Col. Reid's work on Storms. As a general explanation only was before given of their phænomena, it may now be proper to give one

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* For a particular explanation of the phænomena of these storms I must refer the reader to my essay in this Magazine, vol. xxiii. I may observe here, however, that the identity of the phænomena of the storms of the Atlantic and Indian ocean is sufficient evidence of the same condition of the currents in the former as in the latter at the time of the occurrence of the hurricane; but in the western part of the Atlantic, which is the locality of the hurricanes, the south is the prevalent wind in summer. (See an Essay on the Climate of Barbadoes, by Robert Lawson, in the Edinburgh Phil. Journal for July 1845.)

† On the Storms of the Tropics, Phil. Mag. vol. xxiii. p. 206.

more explicit. I have therefore quoted below the principal data of the second of these storms (Law of Storms, p. 18), and from them constructed a chart (Plate IV.), showing the direction of the wind at the different localities at the *onset* of the storm, and the time occupied by the first period of it.

"At Charleston (S. C.), on the 16th, the gale was from the S.E. and E. till 4 P.M., then N.E. and round to N.W.

"At Wilmington (N. C.) the storm was from the E., and veered subsequently to the W.

"In the vicinity of Cape Hatteras, at sea, the storm was very heavy from S.E., and shifted to N.W.

"Early on the morning of the 17th, the gale was felt severely at Norfolk, and also in Chesapeak Bay from the N.E.

"Off the Capes of Virginia, on the 17th, in lat. 36° 20′, long. 74° 2′, a 'perfect hurricane' from S. to S.S.E. from 5 A.M. to 2 P.M., then shifted to N.W.

"Off Chincoteaque (M.d.), precise distance from the coast unknown, the gale was severe between S.S.E. and N.N.E.

"Off the coast of Delaware, in lat. 38°, long. 72°, 'tremendous gale,' commencing at S.E. at 1 P.M. on the 17th, and blowing six hours, then changed to N.W.

"At Cape May (N. J.) the gale was N.E. off Cape May, in lat. 39°, long. 74° 15′; heavy gale from E.N.E. on the afternoon of the 17th of August.

"Near Egg Harbour, coast of New Jersey, the gale was heavy at N.E. on the same afternoon.

"Off the same coast, in lat. 39°, long. 73°, the gale was at E.N.E.

"In the same latitude, long. 70° 30′, 'tremendous gale,' commencing at S.S.E. and veering to N.

"At New York and on Long Island Sound, the gale was at N.N.E. and N.E. on the afternoon and evening of the 17th.

"Off Nantucket Shoals, at 8 p.m., the gale commenced severe at N.E. by E.

"In the Gulf-stream, off Nantucket, in lat. 38° 15′, long. 67° 30′, on the night of the 17th, 'tremendous hurricane,' commencing at S., and veering with increasing severity to S.W., W., and N.W.

"At Elizabeth Island, Chatham, and Cape Cod (Mass.), the gale was severe, at N.E., on the night between the 17th and 18th.

"On the 18th, heavy gale from N.E. at Salem and Newbury Port (Mass.).

"Early on the 18th, in lat. 39° 51′, long. 69°, severe gale from S.E., suddenly shifting to N.

"In lat. 41° 20′, long. 66° 25′, 'tremendous hurricane' from N.N.E. on the 18th."

I cannot refer to the charts given by Col. Reid, because the directions of the wind marked out in them do not indicate the direction in any regular order with regard to the two periods of the storm.

Now the general order of the phænomena of the storms (for it is the same in both, the second being selected merely because of the information concerning it being more full than in the first one, apparently on account of its position being more westerly, and therefore including a larger portion of the United States) are as follows:—The storm commences at S.E. or N.E., but in both cases terminates at N.W., excepting in a few instances—principally near the limit of the storm in the north—where the direction of the wind at both the onset and termination of the storm is from N.E. The veering of the wind is sometimes from S.E. to N.E. and then to N.W., but more generally at once from S.E. to N.W.; and when the onset is from N.E., sometimes from N.E. to S.E., and afterwards to N.W., but more frequently directly from N.E. to N.W. The differences between the storms now under consideration, and those to which § 16 specially applies, are, —1st, that in the former the rarefaction of the atmosphere is much more suddenly produced, on account of the much greater force of the wind, and hence the extent and duration of each continuous portion of the storm is much less than in the latter; thus instead of one wind prevailing at once over a large extent of surface, as from the extreme south to the north of England, and for a long period, an American storm, over the same length of tract, consists of many alternate portions, in which the direction of the wind varies, as shown in the account given of its changes, and continues only for a few hours; and, 2ndly, in British advancing storms the collision of the north and south current takes place from the flow of the opposite currents towards a rarefaction produced by a previous storm; but in American storms the rarefaction in the first instance is occasioned by the recession of the storms from localities where the directions of the atmospheric currents are the reverse of those which are now concerned in it. Now from the distribution of the arrows in the chart, we perceive that the localities where the wind is S.E. have a constant position with regard to those where it is N.E.; and if we select any three or four positions in the track of the storm, at the time when the wind at the most southerly one has changed to N.W., the wind at the same instant of time will be blowing according to the directions shown by the arrows at A B C E, Plate V. fig. 4[*]. Now the upper current by which these storms begin within the tropic is from north-east, but after this change in their course the upper current is from south-west and the lower one north-east; let us then suppose A the station at which the storm first arrives after the change in the direction of the atmospheric currents, the upper one being now S.W., the rarefaction of the atmosphere is first produced here by the flowing of the air from it towards south, according to the recession of tropical storms, and into this rarefaction the south current descends, but instead of restoring the atmospheric pressure, it still further increases its diminution by its momentum, and extends northwards and eastwards to B. But it is evident that the rarefaction will extend to a much greater distance in front of the storm than on the western side of it[†], hence the pressure on the line A C, fig. 3 (but now less inclined from C B), overcomes at length the force of the south wind at A where the depression of the barometer is the greatest and the wind sets in from N.W., as shown in the figure, and restores the pressure, so as to maintain by again raising the height of the atmospheric columns at A, the velocity of the upper current now flowing to more northerly localities.

But whilst this is going on, the north-east wind is blowing at C and E, being produced there by two causes, its recession from A, and the flow of air produced by the rarefaction at the limit of the south wind, as at E, fig. 1, Plate V.; and as the direction of the upper current is from south-west, the diminution of the atmospheric pressure is of course carried on in that direction; and hence a rarefaction is maintained, into which the south current flows from columns at a point eastward of A, as B, which as yet is not subject in so great a degree as A, to the opposition of the air on the line A C of fig. 3. But the south wind meeting the north, the wind is S.E. But as the line A C advances, its pressure prevails both at B and C, and

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[*] I would just remark in passing, how well these positions would accord with the hypothesis of the wind moving in a whirl, could the fourth quarter wanting, when the wind should be south-west, be found; but scarcely one observation of the wind from south-west occurs in the direct path of the storm, for when the wind is stated as blowing from south-west, it is either previous to the change in the progressive motion, or to the west of the "hurricane tract of the storm." The occurrence of the southerly current as a south-east wind in front of the north-east, as exhibited by the diagrams, is a grand illustration of § 11.

[†] This is evident from the direction of the progressive motion, but an observation given in the data of the storm of 1821 (Law of Storms, p. 16) shows how abruptly the storm terminates on the west, for "at Wilmington there was no gale," but "a severe gale was experienced thirty miles outside of the American coast off Wilmington (N. Carolina)."

changes the wind at both places to north-west, whilst the phænomena previously in existence at those stations are removed to others further north. Now it is obvious that whilst at a station C, fig. 4, where the wind is north-east, it may change directly to north-west, at another, E, more to the east, it will change first to south-east if the south wind has the greatest force; but if the north-east become the strongest, which at any particular spot may be the case, the wind in the first part of the storm from south-east may change to north-east before changing to north-west, as shown by some of the data;—a particular instance may be given.

From the data of the hurricane of 1821 (Law of Storms, p. 17), "At Cape Henlopen, Delaware, the hurricane commenced at 11½ A.M. from E.S.E.; shifted in twenty minutes to E.N.E. and blew very heavy for nearly an hour. A calm of half an hour succeeded, and the wind then shifted to the W.N.W. and blew, if possible, with still greater violence." "At Cape May, New Jersey" (a little to the north-east of the previous locality), "commenced at N.E. at 2 P.M. and veered to S.E." Thus it appears that at two stations situated with regard to each other as A and E, the phænomena were as follows:—At A the storm arrived at 11½ A.M., and blew as an E.S.E. wind, but about 12 P.M. the north current had increased in force and the wind changed to E.N.E., from which point it blew for an hour, or till 1 P.M. All this time it appears there was no storm at E., but the N.E. wind had receded to it at 2 P.M., and began to blow at that time, but the S. wind soon arrived with greater strength and the wind changed to S.E. But if this be the true explanation of the action of these storms, then according to that given of the mode of progression of receding storms, they ought to increase on the western side towards north-west by the recession of the north-east current; and on the south-west towards south-east by the recession of the south wind. Now with regard to the increase on the west side; as the direction of the north wind is opposed to that of the upper current, it is evident that by extending itself to the west, it cannot extend the rarefaction; and this being produced in a direction from south-west to north-east by the flow of the upper current, the resistance on the west side would soon overcome the advance from east: moreover, the north-east wind is not caused simply by its recession from south, but by the production or increase of the atmospheric rarefaction, as at E, fig. 1. § 2. None of these causes however operate as obstacles to its increase towards south-east, and hence we find that the storm actually does increase towards east, and that throughout its whole extent a south wind progresses towards south-east. This is shown by the data, together with the report of the ship Blanche, whose log is given; for on the 17th (A.M.) she was in lat. 31° 42′, long. 76° 59′, with "fresh breezes and squally" from south by west, but at this time the hurricane was "off the Capes of Virginia in lat. 36° 20′, long. 74° 2′;" again, "off Nantucket Shoals (lat. 41° 5′, long. 70°) the gale commenced at 8 P.M. of the 17th," and "off Nantucket, in lat. 38° 15′, long. 67° 30′, on the night of the 17th; also early on the 18th, in lat. 39° 51′, long. 69°."

18. It is obvious that the foregoing results, if correct, ought to enable us to explain the mode of veering of the wind, and so in great measure they will; and when they are defective, the want arises from our ignorance of the circumstances immediately contingent on the descent of the upper current. If air simply descends upon the north-east current, or meets it from a position on the south, it is evident that whether it changes towards west or towards south of east, will depend on the degree of easterly deflection the north wind has attained; hence, if the north wind be blowing briskly, the change would probably be towards south of east; and if feebly, towards west. Also, if a station upon which the north-east wind is blowing receive a south wind approaching it as the line A D (fig. 2), it is obvious that it could not change to north-west and then to south-west, for the air sweeping along the surface in the direction of D A would gradually draw the air adjacent to it into its own direction; consequently the wind would change first to south-east. Now this change does generally occur, but not always; for on the 7th and 8th of the month chosen for these observations, the wind changed from north-east to north-west, and blowing in that direction some time, afterwards changed to south-west on the arrival of a storm moving like that of fig. 2. From this therefore we may infer, that portions of the upper current were already descending when it arrived in full force as a south-west storm. The change of the wind from south to north, however, is not so much dependent on circumstances. The position of the line A C, fig. 3, which must always exist with more or less inclination to the direction of the storm or D A, fig. 2, and C D B, fig. 3 (its peculiar effect in the case of § 16 being occasioned by the peculiarity in the distribution of the pressure of the atmosphere on the line C D), determines it in this case; thus we find that in the southern and western portions of the locality of a south wind, the south wind first changes to north-west; but as the north-east wind advances from the northern verge of the storm or wind, it changes again to

north-east,—a change from south-west to south-east in this case, in the southern localities, being almost impossible whilst the wind continues to blow on the south-east side of the locality which the storm has left. Now it is matter of general observation that the wind very seldom changes in this direction, or from south-west to south-east, being indeed termed by nautical men "backing"; there is however a particular case in which the wind sometimes changes from S.W. to S.S.E. in the northern or central portions of a space occupied by a storm; and that is in the occurrence of a storm as that of § 16, the circumstances of which fully explain the exception, for the change is the consequence of the collision of the currents by which the S.S.E. wind is produced, and hence it takes place at what for the moment is on the northern verge of the south wind, where of course a change produced by a north-east wind meeting it may make it south-east*.

Having carried out thus far the results of the principles stated at the beginning of this paper, I may now proceed to give the observations which I have collected, and first those of the wind, extracted from the Shipping Gazette.

Scotland.—Orkney, Longhope.—"November 1. N., moderate. 2. S.E., fresh breeze. 5. N.W., moderate. 8. S.W., blowing hard; rain. 11. E., strong breeze; rain. 13. N.E., squally. 15. N.N.E., fresh. 16. N.E., frosty. 19. E., strong breeze; rain. 20, 21. N. to N.E., frost and sudden squalls. 22. E. to S.E. 23. E., fresh breeze."

Pentland Frith.—"Nov. 7. N.W., moderate: night, S.W.; very strong throughout the night. 8. S.W. 18. W., moderate. 19. S.E., moderate; rain: 6 P.M., N.E., moderate."

Thurso.—"Nov. 2. S.E., moderate. 8. S.W., fresh breeze. 10. N.W., heavy gale. 11. N.E. 17. S.W. 18. N.E. 19. S.E., moderate weather. 23. S.E."

Peterhead.—"Nov. 1. N.W., light. 3. S.E. to E.S.E., light breezes. 4. E., moderate. 5. N.E., fresh breeze. 8. S.W., fresh breeze. 9. S.W., strong gale. 19. S.E. to N.E., rainy. 20. N.E., strong breeze. 23. E.N.E., fresh breeze. 24. E., strong."

Inverness.—"Nov. 19. N.E., calm and raining. 26. N.E., calm; rain."

Aberdeen.—"Nov. 17. E.N.E. 18. Variable, E.N.E."

Mull—Tobermorey.—"Nov. 3. S.E. 4. S.E., moderate breeze. 5. S.E. to E., light breeze. 7. Variable; light airs and heavy showers of rain. 8. S.W., strong gales. 9. S.W., fresh breeze, with rain at intervals. 10. E.N.E., fresh breeze. 11. E., strong breeze. 12. W. to S., light airs; variable. 13. E.N.E., fresh breeze.

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* In these paragraphs I have omitted any mention of the differences of the mean pressure on different latitudes of the surface of the earth, not because of its unimportance, but because it would merely be a transcript of my essay on that subject in this Magazine, vol. xx. p. 469.

14. E.N.E., moderate breeze. 15. E.N.E., fresh breeze. 16. E. to S.E., fresh and squally. 17. S.S.W., moderate breeze. 22. S.S.E., strong gales; heavy rain. 23. S., moderate breezes. 24. N.N.E., strong breezes."

Islay.—Bowmore.—" Nov. 2. N.E. 10. S.W., blowing hard. 15. N.E. 16. S.E., a gale. 21. S.S.E. 23. N.W. 25. N.E., fresh."

Bute.—Rothsay.—" Nov. 1. W., blowing strong. 9. S.W., blowing strong. 20. S.W., blowing strong."

Greenock.—" Nov. 1. W., fine. 7. S.W., fine. 11. N.N.W. and S.E., moderate. 12. S., light airs; calm. 14. W., moderate. 18. S.E., moderate. 19. S., light airs; rain. 22. S.E., snow and sleet. 23. Variable; light airs and calm. 24. E., fresh breezes with showers."

Glasgow.—" Nov. 3. E., moderate. 6. N.E., fine. 8. 6 P.M. wind veered round to S.W., and blowing a gale outside. 10. Shortly after we had posted our letters (on the 8th) it began to blow a heavy gale, which towards night became a hurricane with rain, which continued all day (the 9th). Today (10th) wind N.E., fair. 14. 6–30′ P.M., wind since Saturday last (12th) chiefly from N. to N.E., light. 17. N.E., light."

Leith Roads.—" Nov. 14. N.N.E., fine. 17. Variable; fair."

Ireland.—Donegal.—" On the night of the 8th, about 8 o'clock, there was a heavy gale of wind from W. to S.W. 26. W.N.W."

Strangford.—" Nov. 1. W. 2. E.S.E., fine. 3 and 4. E.S.E., fresh. 5. E. by N., fine. 14, 15, 16. E., strong breeze. 17. E.S.E., strong. 18. W.S.W., heavy breeze with rain. 19. W. 21. E. 22. W. by N. 23. W.S.W. 25. E.S.E., strong; rain. 26. N.W., rain."

Arklow.—" Nov. 6. E.N.E., fresh breezes. 9. A.M. S.W., strong gales with rain; P.M. strong gales. 10. At day-break, E., strong gales with rain: P.M. S.S.W., blowing hard. 11. A.M. S.W., strong gales. 12. A.M. S.W., strong gales and rain: P.M. W., fresh breeze. 13. A.M. W.S.W., fresh gales with rain; P.M. S.W., moderate. 14. A.M. N.E., strong gales; P.M. E.N.E. to E.S.E., a gale after sunset with heavy incessant rain. 15. E. by S., a heavy gale with torrents of rain. 16. E., heavy gale. 17. E., strong gales: P.M. E.N.E., fresh gales. 18. A.M. S.W., strong gales; P.M. S.W., strong gales. 19. S.W., strong gales. 20. A.M. E.N.E., fresh gales: P.M. S., hard gales and rain. 21. E.N.E., A.M. moderate: P.M. freshened to a gale. 22. W.N.W., fresh gale. 23. A.M. W., moderate: P.M. S., squally. 24. W.S.W., fresh gales; P.M. W., more moderate. 25. A.M. W., moderate; P.M. W.N.W., moderate. 26. W. by N., moderate."

Waterford.—" Nov. 2. W.S.W., S.E., and S.S.E. 3. S.E. to S.S.E. 4. E. to S.E., E., and N.E. 14. W. by N. to W.S.W., S.S.W., and W."

Youghall.—" Nov. 11. N.E., light breeze; hazy with rain. 12. W.S.W., fresh breeze; rain. 20. E. by S., fresh breeze, with rain."

Galway.—" Nov. 3. E.S.E. 5. E., fine. 6. E.S.E., moderate. 21.

Hard gales from E.N.E. for some days past. 22. Much rain last night; wind strong from E.N.E.; today more moderate; wind W.S.W. 26. N.N.W., light airs."

Limerick.—" Nov. 10. S.S.E., moderate. 11. S., moderate. 14. S.S.E., moderate. 15. S.S.E., blowing hard, with rain."

Cove of Cork.—" Nov. 2. Strong gales with rain. 3. S.E., stormy, with rain. 4. S.E., strong breezes. 5. N.E., a gale. 6. N.N.E., fresh breeze. 7. N.N.W., moderate. 9. S.W., stormy, with rain. 10. S.E., moderate; rain. 11. S.S.W., showery. 12. W.N.W., clear. 13. S.S.W. to W.N.W., variable; squally; heavy showers. 17. E., strong breeze. 18. S.W., stormy, with rain. 19. S.W., moderate. 20. E., strong breeze; rain. 21. S.E., moderate. 22. N.N.W., strong breeze; fair. 23. S.E., strong breeze. 24. W.S.W., fresh; heavy showers. 25. N.N.W. 26. N.W., stormy and showers."

England, West Coast.—Holyhead.—" Nov. 1. N. to N.E., moderate. 2. Variable and fine. 4 and 5. E. to E.N.E., strong; squally. 6. E. by N., fresh breeze. 7. N.E., fresh breeze. 8. W.S.W., fine breeze. 9. S.W., strong gale; hazy; wet. 10. A.M. E.S.E., fine breeze; rainy: P.M. S. to S.S.W., strong breeze and squally. 11. S., moderate. 12. W.N.W., strong breeze. 13. Variable; moderate; rain. 14. E., fine breeze. 15. E., strong gale; showery: 9 P.M. continued. 16. E., blowing excessively hard. 17. S.E., fine breeze: 8 P.M. S., moderate. 18. S.W., fresh breeze. 19. W.S.W., strong; rainy; night variable. 20. E.S.E., fresh breeze: 8 P.M. E., fresh. 21. E., moderate. 22. N.W., strong gale. 23. Last night N.W., strong gale; today veered to W.S.W., fine breeze: 8 P.M. S.S.E., fine breeze. 24. Variable from S.E. to S.S.W., fresh breeze; showery. 25. W.S.W. to S.W., fresh; showery. 26. W.S.W. to W., fresh; rain."

Beaumaris.—" Nov. 11. S.W., much rain. 12. S.W., fresh; rain. 13. S.W., much rain. 14. E., fresh. 17. E., fine. 18. S.S.W., fresh; rain. 19. S.S.W., fresh; rain. 20. S.S.E. 22. W., fine. 23. S.E., fresh. 25. S.W."

Bristol.—" Nov. 4. E., strong. 8. E., moderate. 10. 4 P.M. S.S.W., very strong. 11. S.W., showery. 13. It has continued squally from S.W. to W. since my last, and a great quantity of rain has fallen. 14. Variable; moderate. 14. (second report) E., strong. 16. E., strong; constant rain. 19. S.W., a gale. 20. E., fresh. 21. N.E., fresh. 22. The wind this morning blew fresh from S.S.E. until 10 o'clock, when it gradually veered round to N.N.W., and afterwards it was very strong from that point; a quantity of rain fell last night. 23. N.W., fresh. 24. W.S.W., strong; showery."

Scilly Islands.—St. Mary's.—" Nov. 1. S.E., fresh breeze. 2. S.E., strong wind and rain. 3. S.E., strong gales, with rain. 4. E.S.E., strong gales; rain. 5 and 6. E., strong breeze. 7. E.N.E., strong, showers. 8. E., fresh breeze. 9 and 10. S.S.W., strong gales; rain. 11. W., strong; rain. 12. W. 13. W.S.W. 14 and 15. S.W. 16. S.W. to S.E., strong, with rain for the last four days. 17. E., fresh breeze. 19. S.W., strong; rain. 20. W.N.W., rain. 21. E.S.E., strong; rain. 25. W.N.W., strong gales and rain. 26. W.S.W., fresh breezes and rain."

South Coast.—Falmouth.—"Nov. 1. N.E., light breeze. 2. E.S.E. to S.E. 3. E. by S. 4 and 5. E.N.E., strong. 6. E.N.E. 7. N.E., light. 8. N., light airs. 9. S. 10. S., a gale; rain throughout. 13. 8 A.M. S., a gale; raining; in the evening wind changed to W. 14. Weather moderated. 15. S., squally; rain; afterwards W. 16. Morning S.W. and raining; evening N.E., strong gale. 17. 1 A.M. N.E., moderate; afterwards E. 18. S. 19. S.S.W., blowing hard. 20. W., light airs and rain: 10 P.M. veered to S.E. 21. E., showery. 22. N.N.W. to N.W., showery. 23. S.W., showery. 24. W. by S., heavy gale, with rain. 26. N.W., showers."

Plymouth.—"Nov. 4. N.E., moderate. 5. N.E., fresh breeze. 6. N.N.E. to E.N.E. 8. N.E., light airs. 9. S.S.W., strong, with rain. 10. S.W., strong; thick rain. 11. S.S.W., strong; rain. 12. W. by N. 13. It has blown very heavy through the night from S.S.W. with rain, which (at noon) still continues. 14. Calm; hazy. 15. W.S.W., strong. 18. S.E., fresh breeze. 19. S.S.W., dirty. 21. Easterly, moderate. 22. N.W., fresh breeze. 23. W.N.W., fresh breeze. 24. W.S.W., strong; squally. 25. W.S.W., strong breeze, with showers. 26. W., moderate."

Penzance.—"Nov. 26. W.N.W., strong gales."

Portsmouth.—"Nov. 3. E.S.E. 4. E., fresh breezes. 6. N. to N.N.E., foggy; slight showers of rain. 9. S.W., blowing fresh. 11. S.W., rain. 14. W. by S.: P.M. fresh breeze. 15. S.W., light breeze; rain all day. 17. N. by E., fresh breeze. 18. S. by E., fresh breezes. 21. N.E., hazy. 23. P.M. W.S.W., fresh breezes; rain. 24. W.S.W., rain. 25. W.S.W., rain and squalls."

Isle of Wight.—Ryde.—"Nov. 12. W.S.W. to W. 13. S.W. to W.S.W., with rain; blowing hard. 14. W.N.W., fine. 15. S.W. 16. E.S.E., fresh. 17. N.E. to E.N.E., fresh. 18. S., fine. 21. E., fine. 24. W.S.W., strong, with rain."

Deal.—"Nov. 1. W.N.W. and N. by E., moderate and fine. 2. E.N.E. to E.S.E. 3. N.E., moderate and fine. 4. S.E., light airs and rain: P.M. N.E., fresh and squally. 5 and 6. E.N.E., blowing fresh and squally. 7. E.N.E., fresh breeze; squally. 10. S.W., fresh breeze; squally. 11. S.W., blowing very strong, with rain. 12. It has blown very hard all day from W.S.W., with squalls of rain. 14. N.N.W., light airs. 16. E., fresh. 17. A.M. E., blowing hard: 6–30′ P.M. E.S.E. 18. S.W., moderate. 20. N.N.W. and N.E., moderate. 21. N.E. 22. A.M. S.S.W., blowing strong; rain: P.M. S.E., moderate; rain. 23. W.S.W. and W.N.W., light airs. 24. W.S.W., blowing fresh. 25. It blew a gale of wind last night and nearly the whole of this day from S.W. 26. It blew very strong during last night from the S.W. or W.S.W., with squalls of rain."

Dover.—"Nov. 3. 7 A.M. S.S.W., light: noon and 7 P.M. S.E., fresh. 4. N.E., light, with rain. 5. E.N.E., strong; cloudy. 6. A.M. E., fresh: P.M. E.S.E., fresh; rain. 8. N.N.W., light. 9. S.W., fresh: 7 P.M. W.S.W., strong. 10. 7 A.M. W.S.W., fresh: noon, S.S.W., strong: 7 P.M. S.W., fresh. 15. E.S.E., strong. 16. 7 A.M. E., light wind; rain: noon and 7 P.M. E., strong. 17. E., strong. 21. N.E., fresh. 24. 7 A.M. S.S.W., strong, with rain: 7 P.M. W., strong; rain. 25. S.W., strong."

East Coast.—The Downs.—"Nov. 1. N.W., light breeze. 2. Easterly, moderate. 8. N.W., very light. 9. S.W., blowing fresh. 10. S.W., blowing fresh. 14. W.N.W., light. 15. E. to S.E., fresh. 16. E.N.E., moderate breezes. 18. S.W., very light. 19. S., blowing fresh. 20. N., moderate. 21. N.E., light breezes. 23. W., light. 25. W.S.W., blowing very fresh. 26. W.S.W., blowing fresh."

North Foreland.—"Nov. 1. N.W. 2. E. to S.E., fresh. 3. Variable. 4. E., squally. 6. Blowing fresh. 12. S.W.: noon, W.N.W.; P.M. W., squally. 14. N.W. by W. to N., light. 15. E. to E.S.E., blowing strong. 16. E.S.E., blowing fresh. 17. E.S.E., fresh. 18. S.E. to S.S.W., light. 20. N. to E., moderate. 21. N.E. 22. S.E., moderate. 23. W.S.W., light. 24. S. to S.W., blowing hard; rain. 25. S.W., squally, with rain. 26. S.S.W. to S., blowing strong."

Yarmouth (*Norfolk*).—"Nov. 1. N.W., fine. 2. N.N.E. 9. S.S.W., blowing fresh. 10. S.S.W. to S.W., fresh breeze. 11. S.S.W. to S.W., blowing strong, with rain. 12. 6 P.M. N.W., strong. 14. N. to W.N.W., fine. 15. S.S.E. 16. Last night and all this day the wind has blown fresh from E.N.E. to N.E. 17. E. 25. S. to S.S.W., blowing strong. 26. S.S.W., blowing fresh."

Lowestoft.—"Nov. 1. W. 2. E.N.E., light breezes. 18. N.W., light. 21. N.N.E., light breezes."

Flamborough Head.—"Nov. 1. W. to N.W., light breezes. 2. E.S.E., light breezes. 3. E. to E.S.E., light breeze. 4. E.N.E., strong breezes. 5. E.N.E., strong breezes and squally. 6. E.N.E., strong breezes. 7. N.E., strong breezes and squally. 8. W.N.W., light breeze. 9. S.W., strong gales. 10. S.W., light breezes. 11. S. by E., strong breezes, with rain. 12. S.W. to W., fresh breezes. 13. S.W., light breezes; evening, E., light. 14. N. to N.E., moderate breezes. 15. E., moderate breezes. 16. E., strong breezes. 17. E.N.E., blowing fresh. 18. W., light breezes. 20. N.N.E., strong breezes. 21. N.E., strong breeze. 22. S.S.W. to S., strong breeze. 23. W., moderate breeze. 24. S.E., strong gale; rain. 25. S., strong breeze. 26. S.S.W. to S.W., strong breezes."

North Shields.—"Nov. 1. N.W., brisk in the morning. 2. S.S.W. morning and evening; light and S.S.E. in the middle of the day; brisk. 3. S.E. and E.S.E., moderate: evening, E. 4. E.N.E., strong. 5. N.E., strong, but moderate towards evening. 6. E.N.E., moderate. 7. 9 A.M. N.N.E., light: 9 P.M. W.N.W., fresh; light rain. 8. 9 A.M. W.N.W., light: 2 P.M. W.S.W., light: 9 P.M. S.W., almost calm. 9. Storm from S.W., which began early in the morning and continued till 5 P.M., at which time the wind had sunk down to a calm with rain; the barometer being then at its minimum, 29·276. The calm continued through the night, with a hoar frost. 10. 9 A.M. W.N.W., very light; sky clear, but soon afterwards overcast: 2 P.M., wind extremely light and variable; rain falling: 9 P.M. wind strong from S.E., which continued, with heavy rain, all night; and until afternoon of the 11th, on which day the air became again calm in the evening. 12. 9 A.M. W.N.W., very light wind: 9 P.M. strong from N.W. 13. 9 A.M. and 2 P.M. wind extremely light from W.S.W. and S.W.; in the evening strong from N.E., with rain. 14. N.E., brisk. 15. N.E., brisk, with showers during the day, and strong at night. 16. N.E. and E.N.E., strong. 17. 9 A.M. E., very light; calm during the remainder of the day. 18. W.S.W., very light during most of the day, but strong at night. 19. Morning, W.S.W., brisk: evening, light from W.N.W., slight rain. 20. N.E., rather brisk; showers in the evening and during the night. 21. N. to N.W., rather brisk in the middle of the day; showers. 22. Morning, strong from S.S.W.; changed to S.W. in the afternoon, and sunk down to a calm in the evening; rain and snow. 23. N.W., rather brisk. 24. A storm, with rain from S.S.E., which abated towards evening. 25. S.S.E., strong till evening, when the air became calm; fine showers. 26. S.S.W. and S.W., light; rain in the evening."

The Tables which follow contain the indications of the barometer from the 1st to the 26th; each day is divided into three columns; the second and third contain the observations for morning and evening, and the middle one those of the middle of the day. The hours of observation are given after the names of the places, which are placed in the order from north to south, but the western before the eastern. In a column previous to those containing the daily indications are given the mean heights of the barometer for each place, in order that the daily heights may be compared with each other*, in which comparison the difference of pressure due to the latitude ought to be borne in mind. With the exception of the observations at Paris, Christiania, and North Shields, which are reduced to the temperature of 32° Fahr., the numbers are those which are read off from the barometer; as,

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* It is not meant by this to be understood that these means are given as representing by their differences the differences of height of the several barometers as read off from each scale, supposing them placed in juxtaposition, but only to serve in the following observations for standards for the comparison of their variations with each other in the absence of any other method of doing it; but the unusual equality of the mean pressure of the atmosphere of this month over so large a space, renders the means chosen sufficiently accurate for this purpose.

however, one degree of Fahrenheit affects the barometer at 30 inches only ·003 inch, and there is seldom a greater difference than 3° between the temperatures of consecutive observations, it will not affect any of the conclusions which may be deducible from them. The numbers representing the heights at Christiania in Paris lines, and at Paris in millimetres, have been reduced to English inches.

Names of places.	Hours of observation.	Mean of the month.	1.			2.		
Orkneys ...	9½ a.m. & 8½ p.m.	29·70	30·10		30·22	30·24		30·22
Glasgow*...	9 a.m. & p.m.	29·53	30·07		30·08	30·07		30·01
Belfast......	9 a.m. & 3 p.m.	29·74	30·35	30·33		30·24	30·18	
Armagh†...	10 a.m. & p.m.	29·44			29·97	29·91		29·82
Shields......	9 a.m. & 2 & 9½ p.m.	29·68	30·14	30·17	30·21	30·21	30·19	30·16
Cork.........	9 a.m. & 3 p.m.	29·74	30·22	30·15		29·98	29·92	
Bristol......	9½ a.m. & p.m.	29·70	30·22		30·14	30·09		30·01
Plymouth...	9 a.m. & p.m.	29·71	30·27		30·16	30·09		30·00
London ...	9 a.m & 3 p.m.	29·72	30·23	30·17		30·14	30·10	
Paris	9 a.m. & 3 & 9 p.m.	29·61	30·08		30·00	29·90		29·83
Christiania .	9 a.m. & 10 p.m.	29·71	29·61		29·96	30·03		30·20

	3.			4.			5.		
Orkneys ...	30·20		30·28	30·44		30·54	30·53		30·51
Glasgow ...	30·00		30·08	30·21		30·34	30·37		30·28
Belfast......	30·16	30·18		30·38	30·45		30·57	30·55	
Armagh ...	29·86		29·97	30·12		30·23	30·30		30·23
Shields......	30·10	30·10	30·15	30·25	30·34	30·40	30·39	30·33	30·34
Cork.........	29·83	29·83		30·16	30·16		30·33	30·32	
Bristol......	29·94		29·98	30·07		30·21	30·23		30·16
Plymouth...	29·88		29·95	30·09		30·21	30·26		30·19
London ...	30·00	29·96		30·06	30·13		30·19	30·12	
Paris	29·76		29·69	29·75		29·86	29·75		29·75
Christiania .	30·27		30·36	30·42		30·32	30·28		30·27

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* These observations give a mean much below the general one, which is probably the defect of the scale. I have compared them, however, with those given in the tables of the Philosophical Magazine for Applegarth Manse in Dumfries-shire, a locality not far distant, with which their variations agree, with the exception of being a little in advance in movements coming from north, for which reason I have given them the preference here. I may add also, that the observations at Belfast were received with the information that they might be rather defective, which however is of little consequence, as those at Armagh are given. I have inserted the former, because of those at 3 P.M.

† Height of the barometer 211 feet above the level of the sea.

	6.			7.			8.		
Orkneys ...	30·49		30·43	30·30		30·00	29·83		29·42
Glasgow ...	30·24		30·27	30·23		30·07	29·87		29·58
Belfast......	30·51			30·48	30·43		30·19	30·04	
Armagh ...	30·26		30·24	30·20		30·08	29·88		29·39
Shields......	30·34	30·35	30·33	30·30	30·27	30·23	30·05	29·97	29·77
Cork.........	30·30	30·30		30·32	30·33		30·13	30·01	
Bristol				30·18		30·18	30·14		29·99
Plymouth...	30·23		30·24	30·24		30·24	30·21		30·05
London ...	30·20	30·16		30·13	30·13		30·15	30·08	
Paris	29·85	29·83	29·87	29·84		29·94	29·93		29·88
Christiania .	30·27		30·16	30·09		29·95	29·75		29·58

	9.			10.			11.		
Orkneys ...	28·70		28·90	29·29		29·50	29·37		29·12
Glasgow ...	29·04		29·21	29·45		29·33	28·95		28·77
Belfast......	29·36	29·41		29·65	29·57		29·10	29·02	
Armagh ...	29·11		29·30	29·34		28·97	28·74		28·68
Shields......	29·37	29·28	29·37	29·62	29·58	29·43	29·14	28·99	28·89
Cork.........	29·42	29·42		29·34	29·20		28·93	28·91	
Bristol	29·66		29·55	29·57		29·35	29·05		29·02
Plymouth...	29·78		29·66	29·61		29·34	29·11		29·13
London ...	29·81	29·70		29·68	29·64		29·18	29·00	
Paris	29·82	29·76	29·78	29·73		29·53	29·33		29·17
Christiania .	29·46		29·28	29·14		29·34	29·45	29·48	29·45

	12.			13.			14.		
Orkneys ...	29·05		29·14	29·26		29·44	29·67		29·86
Glasgow ...	28·74		29·01	29·08		29·13	29·65		29·72
Belfast......	29·12	29·21		29·32	29·27		29·83	29·91	
Armagh ...	28·87		29·01	29·01		29·21	29·61		29·56
Shields......	28·94		29·20	29·27	29·24	29·25	29·71	29·82	29·84
Cork..... ...	29·30	29·31		29·10	29·40		29·66	29·60	
Bristol	29·15		29·48				29·70		29·60
Plymouth...	29·30		29·63	29·44		29·49	29·75		29·61
London ...	29·12	29·33		29·51	29·26		29·72	29·80	
Paris	29·30		29·57	29·66		29·39	29·65	29·67	29·63
Christiania .	29·30		29·09	28·96	28·94	28·97	29·16		29·54

	15.			16.			17.		
Orkneys ...	29·97		30·05	30·15		30·30	30·38		30·32
Glasgow ...	29·73		29·71	29·85		30·08	30·28		30·30
Belfast......	29·85	29·82		29·96	30·06		30·47	30·51	
Armagh ...	29·53		29·48	29·66		29·97	30·20		30·25
Shields......	29·84	29·83	29·86	29·98	30·03	30·15	30·40		30·51
Cork.........	29·40	29·37		29·49	29·70		30·30	30·31	
Bristol	29·60		29·61	29·68		29·88	30·25		30·46
Plymouth...	29·61		29·67	29·63		29·78	30·25		30·47
London ...	29·69	29·62		29·76	29·79		30·23	30·36	
Paris	29·57	29·55	29·58	29·53	29·50	29·57	29·79		30·19
Christiania .	29·69		29·72	29·80		29·92	29·98	29·94	29·95

	18.			19.			20.		
Orkneys ...	30·16		30·21	29·90		29·93	29·97		29·95
Glasgow ...	30·25		30·11	29·68		29·66	29·78		29·80
Belfast......	30·48	30·37		29·93	29·86		29·94	29·91	
Armagh ...	30·18		29·82	29·68		29·64	29·64		29·64
Shields......	30·46	30·42	30·34	29·88	29·77	29·73	29·85	29·85	29·86
Cork.........	30·29	30·18		29·98	29·92		29·76	29·80	
Bristol	30·49		30·35	30·13		29·82			
Plymouth...	30·51		30·43	30·28		30·01	29·83		29·63
London ...	30·58	30·53		30·23	30·06		29·79	29·77	
Paris	30·38	30·38	30·41	30·30		30·04	29·60		29·50
Christiania .	30·08		30·14	30·00		29·82	29·64		29·56

	21.			22.			23.		
Orkneys ...	29·95		29·77	29·47		29·39	29·36		29·30
Glasgow ...	29·82		29·62	29·22		29·12	29·17		29·01
Belfast......	30·01	29·95		29·42	29·42		29·46	29·32	
Armagh ...	29·74		29·43	29·21		29·21	29·19		28·70
Shields......	29·90	29·89	29·78	29·42	29·30	29·27	29·31	29·27	29·19
Cork.........	29·84	29·83		29·58	29·58		29·34	29·10	
Bristol	29·83		29·76	29·30		29·49	29·48		29·04
Plymouth...	29·78		29·79	29·45		29·62	29·59		29·02
London ...	29·80	29·82		29·42	29·28		29·50	29·59	
Paris	29·51		29·63	29·31	29·13	29·30	29·50		29·33
Christiania .	29·56	29·54	29·56	29·58	29·55	29·56	29·61		29·63

	24.			25.			26.		
Orkneys ...	29·11		29·08	29·04		29·10	29·10		29·10
Glasgow ...	28·71		28·58	28·63		28·70	28·76		28·90
Belfast......	28·83	28·79		28·74	28·82		28·97	29·04	
Armagh ...	28·47		28·40	28·47		28·61	28·73		28·88
Shields......	28·89	28·78	28·75	28·80	28·82	28·83	28·91		29·07
Cork.........	28·54	28·54		28·79	28·80		29·00	29·04	
Bristol	28·83		28·75	28·75		28·94	29·04		29·24
Plymouth...	28·95		28·87	28·83		29·03	29 13		29·28
London ...	28·91	28·92		28·90	28·88		29·09	29·17	
Paris	28·98		29·07	28·95		29·08	29·07		29·28
Christiania .	29·65		29·67	29·66		29·58	29·58	29·57	29·60

In the columns which follow, interposed with the text, I have given the amount of the variations in 100ths of an inch between the consecutive observations given in the preceding tables, with the exception of those in which there are three daily observations; for the 3rd column always contains the difference of the extreme observations of each day, so that in casting the eye down the columns the variations may always belong to the same periods, excepting when the time of the observations varies a little from 9 o'clock; care however must be taken with regard to the first column to observe those which include a period beginning at 3 P.M. on the day previous which is the case when there is no evening observation.

For the sake of simplifying the diagrams as much as possible, I have in the first place given a chart with the names and localities marked upon it (Plate IV.), and the subsequent ones merely contain the localities marked by dots, and set off in their several places from the chart, so that by reference to it they may easily be found. The arrow representing the direction of the wind at Christiania, placed in the north-east corner, is of course out of its relative position. I have endeavoured to represent the force of the wind, as given in nautical language, by figures placed at the feet of the arrows:—0 being a calm, 1 and 2 light airs or winds, 3 moderate, 4 brisk breeze, 5 strong breeze, 6 a gale or stormy, 7 hard gale; *v* is variable: the veering of the wind backwards and forwards between points is represented by two arrows from these points, and a change of the wind in the latter part of the day, or night, by a line crossing the foot of the arrow showing its direction. A change in the force also is marked in the same way, by a line underneath the figure. The direction of the wind at Paris and Christiania is taken from the meteorological registers of those places; and I have also occasionally introduced arrows showing its direction as noted in the registers of other places, but not without caution, as I am more disposed to rely on the mercantile reports, than on observations simply made by noting the direction of one particular vane generally only once in the day. It may also be remarked, that though the variations given in the first column of each day are those which have taken place during the previous night, they may yet be generally considered as caused by the wind indicated by the diagrams for that day, because the wind in the morning is usually that of the preceding night, and when a change occurs during the day it is marked as before stated*.

Names of Places.	1.			2.			3.		
Orkneys			+·12	+·02		−·02	−·02		+·08
Glasgow			+·01	−·01		−·06	−·01		+·08
Belfast		−·02		−·09	−·06		−·02	+·02	
Armagh				−·06		−·09	+·04		+·11
Shields...........		+·03	+·07	·00	−·02	−·05	−·06	·00	+·05
Cork...............		−·07		−·17	−·06		−·09	·00	
Bristol			−·08	−·05		−·08	−·07		+·04
Plymouth			−·11	−·07		−·09	−·12		+·07
London		−·06		−·03	−·04		−·10	−·04	
Paris			−·08	−·10		−·07	−·07		−·07
Christiania			+·35	+·07		+·17	+·07		+·09

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* The shipping reports however very frequently do not state the time of the day the report refers to, when it is extremely probable the wind did not continue in the same direction the whole of the day; so as to occasion in some cases apparent discrepancies in the direction of the wind; but they may generally be removed by reference to some other report from a neighbouring locality where a change in the direction of the wind is noted.

Names of Places.	4.			5.			6.		
Orkneys	+·16		+·10	−·01		−·02	−·02		−·06
Glasgow	+·13		+·13	+·03		−·09			
Belfast	+·20	+·07		+·12	−·02		−·04		
Armagh	+·15		+·11	+·07		−·07	+·06		−·02
Shields	+·10	+·09	+·15	−·01	−·06	−·05	·00	+·02	·00
Cork	+·33	·00		+·17	−·01		−·02	·00	
Bristol	+·09		+·14	+·02		−·07			
Plymouth	+·14		+·12	+·05		−·07	+·04		+·01
London	+·10	+·07		+·06	−·07		+·08	−·04	
Paris	+·06		+·11	−·11		·00	+·10	−·02	+·02
Christiania	+·06		−·10	−·04		−·01	·00		−·11

The first very conspicuous atmospheric phænomenon is an elevation of the barometer, which at Orkney reached the highest point at the P.M. observation of the 4th, and advanced towards south. Its maximum was in the north and north-west, affording an excellent illustration of § 7; for on the 4th, the day on which the principal rise took place, we find the collision of the currents on the western and south-western side of the chart: thus at the Irish ports and the Scilly islands the wind was south-east, whilst in England and Scotland and at Paris it was north-east. Hence at the Orkneys the barometer stood at 30·54 and at Belfast 30·57, these places being situated in the line of the meeting currents; whilst at London, which, so far as the observations go, was in the line of the north-east current moving freely, the barometer reached an elevation of only 30·19; the advance of the north wind and of the elevation of the barometer likewise accorded with each other, both being from north to south.

The observations of the 1st are particularly illustrative of § 5 and § 8; in them the opposition of the southern current is seen only in the extreme south, where it is blowing with some strength, causing a fall of the barometer in the south, where the north wind is blowing immediately in front of the south wind (§ 5), and a slight rise in the north by the arrival of the air removed from the south (§ 8). It might be thought that the setting out of the currents in somewhat different directions from the western part of England might give rise to this fall, but it is evidently not so, because it decreases towards the locality where the current takes the easterly deflection. The deflection of the southern current is also worthy of remark. At the Scilly islands the south wind meeting a north-east blows from the south-east, but at Paris meeting a north-west it is south-west or west-south-west (§ 11). The cause of the north wind being north-west in the north and eastern parts, appears to be owing to a deficit of pressure in the east (the barometer at Christiania being ·39 inch below that at the

Orkneys), which on this day was rapidly removed by the air flowing towards it, for at Christiania the height of the barometer in the morning was 29·61 and in the evening 29·96. The strength of the south wind after this increases, and on the 2nd and 3rd prevailed almost throughout, occasioning a slight general fall of the barometer until 9 A.M. on the 3rd; we still however have evidence of the north-east wind blowing on the east, as on the 2nd, on the south-east coast of England, and on the 3rd in the same part, as at the North Foreland, where it was variable (the one and the other of the currents alternately prevailing), and at Deal, where it was north-east, these being almost the most easterly parts of England. After this, however, the north-east wind advanced, becoming general on the eastern side on the 4th, and raising the barometer as before described, the south wind only appearing on the west, blowing there, according to § 11, from south-east. On the 5th and 6th the north-east prevailed throughout, and the barometer was slightly depressed in all the northern stations, beginning at the Orkneys; the cause of which might be, either the subsidence of an elevation above adjacent localities on the south, by the flowing of the air towards them, or it might be the beginning of the descent of the upper current on the north, which manifested itself at the Orkneys on the 6th by changing the wind to north-west.

Names of Places.	7.			8.			9.		
Orkneys	−·13		−·30	−·17		−·41	−·72		+·20
Glasgow	−·13		−·16	−·20		−·29	−·54		+·17
Belfast		−·05		−·24	−·15		−·68	+·05	
Armagh	−·04		−·12	−·20		−·48	−·28		+·19
Shields	−·03	−·03	−·07	−·18	−·08	−·28	−·40	−·09	·00
Cork	+·02	+·01		−·20	−·12		−·59	·00	
Bristol			·00	−·04		−·15	−·33		−·11
Plymouth	·00		·00	−·03		−·16	−·27		−·12
London	−·03	·00		+·02	−·07		−·27	−·11	
Paris	−·03		+·10	−·01		−·05	−·06	−·06	−·04
Christiania	−·18		−·14	−·20		−·17	−·12		−·18

Names of Places.	10.			11.		
Orkneys	+·39		+·21	−·13		−·25
Glasgow	+·24		−·12	−·38		−·18
Belfast	+·24	−·08		−·47	−·08	
Armagh	+·04		−·37	−·23		−·06
Shields	+·25	−·03	−·19	−·29	−·15	−·25
Cork	−·08	−·14		−·27	−·02	
Bristol	+·02		−·22	−·30		−·03
Plymouth	−·05		−·27	−·23		+·02
London	−·02		−·04	−·46		−·18
Paris	−·05		−·20	−·20		−·16
Christiania	−·14		+·20	+·11		·00

The elevation of the barometer indicated by the foregoing observations, though gradually decreasing from the 4th, did not subside till the approach of a storm from south-west (§15); which began at the Orkney islands on the night of the 7th, was blowing at the island of Mull on the 8th, and arrived at Glasgow at 6 P.M. of the same day; in the north of Ireland (Donegal) at 8 P.M.; and at North Shields (a little to the north of Donegal in latitude, but on account of its more easterly position later in receiving the storm) at a very early hour on the morning of the 9th. The change of the wind as the storm progressed is well-marked by the diagrams. On the 7th, the wind remained north-east throughout the day in almost the whole of England, but at the Orkneys had changed to north-west (§ 18), and was variable at Mull island in the evening: at North Shields it changed to north-west after mid-day, and at night the storm began at the Orkneys from south-west. On the 8th, the south-west wind is blowing in the greater part of Scotland, whilst to the southward the wind is still north, but in some cases north-west. In the evening the changes before noticed take place, the wind being yet northerly in the south. On the 9th the storm became prevalent throughout, on which day the barometer attained its minimum in the north (§ 3), its height in the south being very little reduced (§ 2), although it appears that on that day the wind was blowing as strongly in the south as in the north (§ 4).

The approach of the storm from north is seen also by the falling of the barometer, as indicated by the observations. At North Shields the barometer attained its minimum (there 29·276) at 5 P.M., and at 9 P.M. it had risen 0·10, though reckoning simply from the extreme observation, it had not risen at all; whilst at Orkney it had risen 0·20, and in the south it was yet falling (§ 3). On the morning of the 10th the barometer at the Orkneys had risen 0·39, with a strong gale from north-west (§ 3 and 12); but at Shields, from 5 P.M. on the previous day to about the same hour on this, although the barometer rose, the air was almost calm. The diagram, however, together with the barometric heights, fully explains this; for we see that its position was that of the meeting of the two currents, the north current blowing on the north and the south one on the south (§ 11), the latter continued by the state of the barometer; the barometer rising however by reason of the strength of the north wind setting in in the north (§ 6). The north wind in the middle and southern parts of Scotland appears (on account of the low state of the barometer at Orkney) to arise from the impetus which it has received in blowing in the extreme north, together probably with a higher barometer on the east (shown by its easterly deflection): thus we find the wind decreases in strength as it advances, being at Mull island only brisk. The equality in the force of the two currents is not however of long continuance, for that of the south wind, evidently because of the great depression of the barometer in the north below its height in the south (Orkney, barometer 28·70, London 29·81), on the 9th, greatly increases and advances as a storm towards the north. But this difference of the atmopheric pressure requiring, on account of the great distance of the localities of the extremes of pressure, and the resistance of the opposite current blowing in the north, a long interval of time to produce its effect, does not arrive at North Shields till the latter part of the 10th as a S.E. or rather S.S.E. storm (being deflected by its collision with the contrary current) (§ 11 and 16), and continues until about the same time of the 11th.

In the phænomena now before us we have a good example of an advancing portion of a storm (§ 16). The fall of the barometer, which continues in the south whilst the rise is going on in the north, increases on the 10th, and advances, together with the wind, towards the north, where, excepting in the extreme north, it falls to a greater degree than before, and, as noticed in § 16, the greatest depression is in the south. The progressive motion of the storm may however be traced in both the directions of the figure of § 15, but the south-east movement is in the south: thus at Cork and Plymouth the minimum depression occurred on the evening of the 10th, at Bristol about noon of the 11th; for out of four observations that at noon was the lowest (29·99); and at Paris (south-east) and at North Shields (north-east) it happened simultaneously about 9 P.M. Hence if we suppose Cork to represent the point C, fig. 3, then the line *c* C prolonged would extend to Paris, the storm however diminishing in intensity, and the line C B from Cork to North Shields. The limit of this storm may be observed in the north; for at Orkney the wind continued north-east, and at Mull island east, but yet the barometer at Orkney falls, though during a north-east wind, but not to so great a degree as the next station (Glasgow) (§ 5), for on the next day (the 12th) the minimum depression, or the point C, was in the south of Scotland or north of England; hence Orkney would represent the point E. of fig. 1.

The diagrams of the 10th and 11th (Plate VII.) are also of interest as regards the deflection of the currents produced by their meeting: on the north the wind is N.E., in the south of England and in Ireland it is S.W., and at the localities between the two it is S., S.S.E., or S.E.

Names of Places.	12.			13.			14.		
Orkneys	−·07		+·09	+·12		+·18	+·23		+·19
Glasgow	−·03		+·27	+·07		+·05	+·52		+·07
Belfast	+·10	+·09		+·11	−·05		+·56	+·08	
Armagh	+·19		+·14	·00		+·20	+·40		−·05
Shields	+·05		+·26	+·07	−·03	−·02	+·46	+·11	+·13
Cork	+·39	+·01		−·21	+·30		+·26	−·06	
Bristol	+·13		+·33						−·10
Plymouth	+·17		+·33	−·19		+·05	+·26		−·14
London	+·12	+·21		+·18	−·25		+·46	+·08	
Paris	+·13		+·27	+·09		−·27	+·26	+·02	−·02
Christiania	−·15		−·21	−·13		+·01	+·19		+·38

Names of Places.	15.			16.			17.		
Orkneys	+·11		+·08	+·10		+·15	+·08		−·06
Glasgow	+·01		−·02	+·14		+·23	+·20		+·02
Belfast	−·06	−·03		+·14	+·10		+·41	+·04	
Armagh	−·03		−·05	+·18		+·31	+·23		+·05
Shields	·00	−·01	+·02	+·12	+·06	+·17	+·25		+·11
Cork	−·20	−·03		+·12	+·21		+·60	+·01	
Bristol	·00		+·01	+·07		+·21	+·37		+·21
Plymouth	·00		+·06	−·04		+·15	+·47		+·22
London	−·11	−·07		+·14	+·03		+·44	+·13	
Paris	−·06	−·02	+·01	−·05	−·03	+·04	+·22		+·40
Christiania	+·15		+·03	+·08		+·12	−·06		−·03

The phænomena on the 12th commence a period during which the restoration of the atmosphere to its usual pressure and the rise of the barometer above its mean elevation take place; but their chief interest is in their being those ensuing on the cessation of an advancing storm. The occurrence of the minimum of the atmospheric pressure in the south before its taking place in the north has already been noticed, and is also very apparent from the whole of the observations. Thus at Cork, which seems to represent the point C, § 16, as being in the line of the greatest intensity of the storm, the barometer at 9 A.M. has risen 0·39 inch, and the rise lessens in both directions towards south-east, or along the line *c* C of fig. 3 prolonged, and towards north on the line C B; and in the latter direction at Orkney the barometer still continues to fall, though very slightly. In accordance with this state of the barometer (referring to the same paragraph), the wind is blowing strongly from north-west in the south-west and middle portions, though still opposed by the south wind on the extreme south. In the north, we have clear evidence of the extensive low state of the barometer on the west, for at Christiania, about 10° to the east of the Orkney islands, the height of the barometer at 9 A.M. was 29·30 inches, and at Orkney the wind is from east, hence at the same distance on the west of these islands the barometer is probably below this. If we now suppose, whilst the wind is blowing as shown by the diagram, the point C of fig. 3, by the motion of the line A C (§ 16), to move considerably northwards, and to be a little to the north of Holyhead, where the wind is strong from north-west, the phænomena will be simply a particular case of the general result of the paragraph, for the wind is strong from north-west on the localities in the direction which would be that of the line C A, and variable between west and north-west at Flamborough-head, and west-north-west and very light at North Shields, places, which with regard to the figure would be nearly on a horizontal line with the point C; and in the north, but not extending to Orkney (where the barometer is just beginning to rise), the wind is variable between south and west, and extremely light; but at the locality nearest to Orkney, Greenock, it appears to change to north-east in the latter part of the day. This however is the extreme portion of the storm, and accordingly, soon after this, the north wind sets in briskly from north-east instead of from north-west.

It may be observed that in the south the rise of the barometer ceases at Cork whilst it continues at Plymouth; the phænomena of the next day (13th) however, explain this, for by the setting in of the current from north-west on the south-west, which opposing the south wind blowing in the more southern parts, causes a very rapid rise of the barometer in the south, but more especially in the south-west, a disproportionate pressure there is again produced, and the consequences are in some degree the same as before; for on the 13th, although the barometer rose rapidly at the Orkney islands, and the north-east wind fully set in in Scotland, the south wind blowing previously only in the extreme south, increases in strength and becomes prevalent in all the southern part, causing a considerable fall of the barometer in the south, which, as before, occurs first at Cork, confirming the view that the rarefaction of the atmosphere is greatest in the north-west. At North Shields, where on the night previous the wind was strong from the west, it changed to south-west, though extremely light, and caused a slight fall of the barometer between 9 A.M. and 2 P.M.

The phænomena presented by the diagrams of this day, together with the variations of the barometer given in the first column of the 13th, though of the same kind as in two previous instances, afford so striking an example of the case of § 2, because of the blowing of both currents being so fully pointed out, that I cannot pass it over. We see that on the south the south wind is prevalent and strong, and the north is blowing as a fresh breeze in the north, the two currents meeting and balancing one another a little to the north of the centre of the field of observation, and yet in this place the barometer falls, the fall increasing towards south on account of the greater height of the barometer there at its commencement; but at the same time a rise takes place in the north. Now it is certain that if these winds were simply the flow of exactly similar currents, the one flowing from north and the other from south to a space between them, on this space the barometer would rise. What then becomes of the air brought to the place of meeting if the southern current does not carry it off in the upper regions of the atmosphere, as shown in fig. 1 by the upper arrows between *c* and *b*? That it does not arise from any atmospheric change, originating in a central portion, such as a change in the elasticity of the atmospheric columns, causing a portion of air to roll off from their upper parts and a current to set in towards their bases, is very evident, because the diminution of pressure begins and is greatest at the most remote parts of the south wind flowing towards it; but if we admit the explanation given by § 2, the phænomena presented by the barometer are perfectly consistent with the action of the two contrary currents, which appear to have met so directly that a calm, or a state of the air nearly approaching to it, is produced*.

On the afternoon of the 13th the north wind becomes the most prevalent, and the barometer rises rapidly throughout,

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* As the north and south winds are deflected, the one from east and the other from west, the relative position of England and Scotland might at first sight give rise to the opinion, that when the north was blowing in Scotland and the south in England, they do not blow in opposition to each other, but in parallel bands; in the cases of the 13th and 15th, however, as well as others in which this opposition has been remarked, the observations in Ireland and the extreme west of England remove all doubt as to the actual collision of the currents, for we see by these the two currents blowing directly towards each other in the more remote parts, and variable winds, calms, or the deflections of the south current from east, near the place of meeting.

It is evident, however, that when the north-east wind prevails in a much greater degree on the eastern parts than on the western, as appears to be the case in some days of this period, the opposite currents may blow in parallel bands for some distance; but on the parts immediately adjacent to the north wind, the south wind will be south-east; hence a northern locality may have a south-east wind when a north-east blows on a southern one more to the east, a case frequently occurring in this country, as on the 2nd and 3rd.

although in the south during the blowing of a south wind (§ 6). On the remaining days of this series the north wind continues on the whole to gain in predominance over the south, and to cause the barometer to rise, not however without a check in the south, where the south wind again increases in strength for a time, and causes a slight depression of the barometer on the 14th and 15th, which has its limit northward in the north of England or south of Scotland, where the force of the wind is balanced by the opposite current, repeating the phænomena of the 13th, though with this difference, that on the 15th the north wind prevails to a greater degree than on the 13th, so that at North Shields the north-east wind itself is blowing; still however there is a slight fall of the barometer (§ 5). On the 16th the south wind has greatly decreased, and serves only to produce the great comparative rise of the barometer at Orkney (§ 7), where it attains a height considerably above the mean. On the 17th, the day on which the barometer attains its maximum elevation, we have very little indication of the south wind blowing on the south; it is however blowing on the west, and accordingly we find the elevation beginning in the north-west and extending itself towards the south-east; now as this is the direction in which the point of C of fig. 1, whether the point of depression or elevation moves, and this elevation differs from that of the 4th, during which also the opposition of the south wind was on the west, in extending eastward and south; and also as in many previous instances the south wind merely retreated, and did not altogether disappear when the north wind advanced; we may infer that in the present case it is yet blowing in localities southward of the latitudes included in the diagram. If not, the phænomena yet admit of easy explanation, on the supposition that the north-east wind, which is blowing with great strength, occupies (§ 9) a greater proportion of the height of the atmosphere than the equal flow of the upper current admits of.

Names of Places.	18.			19.			20.		
Orkneys	−·16		+·05	−·31		+·03	+·04		−·02
Glasgow	−·05		−·14	−·43		−·02	+·12		+·02
Belfast	−·03	−·11		−·44	−·07		+·08	−·03	
Armagh	−·07		−·36	−·14		−·04	·00		·00
Shields	−·05	−·04	−·12	−·46	−·11	−·15	+·12	·00	+·01
Cork	−·02	−·11		−·20	−·06		−·16	+·04	
Bristol	+·03		−·14	−·22		−·31	·00		
Plymouth	+·04		−·08	−·15		−·27	−·18		−·20
London	+·22	−·05		−·30	−·17		−·27	−·02	
Paris	+·19	·00	+·03	−·11		−·26	−·44		−·70
Christiania	−·13		+·06	−·14		−·18	−·18		−·08

Names of Places.	21.			22.			23.		
Orkneys	·00		−·18	−·30		−·08	−·03		−·06
Glasgow	+·02		−·20	−·40		−·10	+·05		−·16
Belfast	+·10	−·06		−·53	·00		+·04	−·14	
Armagh	+·10		−·31	−·23		·00	−·02		−·49
Shields	+·04	−·01	−·12	−·36	−·12	−·15	+·04	−·04	−·12
Cork	+·04	−·01		−·25	·00		−·24	−·24	
Bristol			−·07	−·46		+·19	−·01		−·44
Plymouth	+·15		+·01	−·34		+·17	−·03		−·57
London	+·03	+·02		−·40	−·14		+·22	+·09	
Paris	+·01		+·12	−·32	−·18	−·01	+·20		−·17
Christiania	·00		·00	+·02		−·02	+0·5		+·02

Names of Places.	24.			25.			26.		
Orkneys	−·19		−·03	−·04		+·06	·00		·00
Glasgow	−·30		−·13	+·05		+·07	+·06		+·14
Belfast	−·49	−·04		−·01	+·08		+·15	+·07	
Armagh	−·23		−·07	+·07		+·14	+·12		+·15
Shields	−·30	−·01	−·14	+·05	+·02	+·03	+·08		+·16
Cork	−·56	·00		+·25	+·01		+·20	+·04	
Bristol	−·21		−·08	·00		+·19	+·10		+·20
Plymouth	−·07		−·08	−·04		+·20	+·10		+·15
London	−·68	+·01		−·01	−·02		+·21	+·08	
Paris	−·35		+·09	−·12		+·13	−·01		+·21
Christiania	+·02		+·02	−·01		−·08	·00		+·02

Having now given a detailed explanation of the several phænomena of the preceding observations, it will be sufficient to give a very general account of those of the succeeding ones; they are however of great interest. The first in occurrence, whose approach is indicated by the P.M. observation of the barometer at the Orkneys on the 17th, is a depression of the barometer by a south wind of just sufficient strength to be called a storm, and its subsequent rise; both progressing from north-west to south-east. Before the restoration of the usual pressure of the atmosphere a second storm occurs, prevailing in Ireland on the night of the 21st, and in England on the 22nd; and whilst the storm was blowing in the latter country the returning current set in strong from north-west in the former, raising the barometer there to a height considerably above that in England (§ 16); its height however on the following day was rapidly reduced by the setting in again of the south wind, as we have before seen in cases of a disproportionate elevation of the barometer in the south, and continued the following day (the 24th). Both these storms, but more particularly the latter, approached this island from about the north of Ireland, as appears from the fall of the barometer occurring first at Armagh. It must be recollected however that the observation there is registered at 10 P.M., thus one hour and a half later than that at the Orkneys; and as the observation at 3 P.M. at Belfast shows that it occurred between these hours, the difference in time accounts for part of the difference in the variation of the barometer; but in accordance with this view of the direction of its motion, we observe that the greatest depression of the barometer, as shown by the observations, is at Glasgow, and therefore, if not precisely there, it must be somewhere between this and the Orkney islands, and hence it is that the south wind during these storms very seldom extends to the north of Scotland, and that the north wind is generally prevalent there; and when it is not, the south wind is very inconstant, as there is always a northerly direction given on the same locality, excepting when the wind is simply stated east, when there can be little doubt it was from the north of that point; and if not so, it at least shows that the south wind had little strength. On the 23rd and 24th, however, the days on which the principal depression of this period occurred, the north wind in the north is well-marked, whilst the south wind is blowing in the south. The latter of these storms offers an example of the case of § 16, but one in which there is a more equable division between both portions of the storms of § 15.

The two remaining days of the period represent by similarity, the remaining portion of the month not included in the observations given; the weather continued stormy, and the barometer fluctuating according to the prevalence of the north or south wind, the north however being on the whole predominant, so that the barometer attained its mean height on the 1st of December.

As a very remarkable depression of the barometer occurred on the 13th of January, 1843, I included the first fifteen days of this month in my collection of observations, but with the exception of the storm which occasioned that depression, they do not offer anything sufficiently worthy of notice, after what has already been given, to make it necessary to insert them here. That storm however I notice particularly, because it presents an additional illustration of the action of the storms of § 16, its phænomena confirming the account given of storms, derived from a consideration of those of the 10th and 11th of November 1842. It advanced to the north of England from a line running somewhat in the direction from Cork to Plymouth; thus at Plymouth it began at 11 P.M. of the 12th, and at North Shields about half an hour after 5 A.M. of the 13th, six and a half hours later; but in the south its progress towards south-east, or its recession along the line *c* C of fig. 3, is well-marked, for it began at Cork at 7 P.M. of the 12th, four hours sooner than at Plymouth, and at Portsmouth at 2 A.M. of the 13th, three hours later than at Plymouth. At London and Bristol it began at the same hour as at Portsmouth, 2 A.M.*, so that its progress in both directions is clearly seen.

In this case also, as in the previous one, the storm succeeded a great depression of the barometer in the north. The order of time in which the minimum height of the barometer was attained, coincides exactly with that of the beginning of the storm, and is very conspicuously marked in both directions; thus in that of the receding portion, or along the line *c* C of fig. 3, the time of the minimum at Cork was early in the morning, or during the night; at Falmouth 9 A.M.; at Plymouth 10 A.M., and at Paris about noon; again in the direction of the advancing portion, or along the line C B, it was two hours later at Bristol than at Falmouth, and at Shields five hours later than at Bristol. I have not thought it necessary to give a diagram showing the directions of the wind, because they may be so easily described. The wind was south-west in the south of Ireland and of England, east and north-east in the north of Scotland†, and south-east or south-south-east (§ 11) between the two extremes, as in the north of England and south of Scotland.

This storm affords also an illustration of § 4; for though, as on the 11th and 12th of November, 1842, the height of the barometer at its cessation was not very far from equal throughout, the reduction was greater in the north of England than in the extreme south, yet in the former region it was a storm of short continuance and no extraordinary violence, whilst in the latter it is described as a perfect hurricane.

Height of Barometer, January 1843.

Names of Places.	11.			12.		
Orkneys	28·76		28·79	28·84		28·82
Glasgow	28·75		28·65	28·74		28·70
Belfast	29·10	28·99		29·02	29·05	
Armagh	28·85		28·57	28·78		28·55
Shields	28·80		28·81	28·84	28·90	28·93
Bristol	29·14		28·78	28·84		29·13
Cork	29·08	28·88		29·20	29·20	
Falmouth	29·24		28·83	28·80	29·22	29·23
Plymouth	29·22	29·00	28·79	28·71	29·17	29·22
London	29·11	29·02		28·74	28·97	
Paris	29·12	29·03	28·79	28·67	28·77	29·11
Christiania	28·55		28·67	28·83		28·98

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* Shipping Gazette Newspaper, January 1843.

† At Tobermorey (Mull) a hard gale from E.N.E. (§ 2), at North Shields S.S.E., and in Leith Roads a gale from E.S.E.

Names of Places.	13.			Minimum on 13th.	Time of minimum.	14.		
Orkneys	28·57		28·51			28·52		28·51
Glasgow	27·90		28·12			28·56		28·61
Belfast	28·10		28·22			28·90		28·84
Armagh	27·83		28·48			28·61		28·64
Shields	28·18	28·05	28·15	28·032	4 P.M.	28·62		28·77
Bristol	28·18		28·75	27·975	11 A.M.	29·03		28·85
Cork	28·58	28·81			*	28·70	28·75	
Falmouth	28·45	28·78	29·03	28·45	9 A.M.	29·08		29·04
Plymouth	28·44	28·70	29·01	28·436	10 A.M.	29·17	28·72	29·01
London	28·35	28·32				29·05	28·74	
Paris	28·79	28·74	28·93	28·74	about noon	29·19		28·68
Christiania	28·96		28·91			28·71		28·56

Variations of Barometer.

Names of Places.	11.			12.			13.			14.		
Orkneys ...	+·28		+·03	+·05		−·02	−·25		−·06	+·01		−·01
Glasgow ..	+·19		−·10	+·09		−·04	−·80		+·22	+·44		+·05
Belfast ...	+·06	−·11		+·03	+·03		−·95	+·12		+·68		−·06
Armagh ...	−·03		−·28	+·20		−·23	−·72		+·65	+·13		+·03
Shields ...	+·01		+·01	+·03	+·09	+·09	−·75	−·13	−·03	+·47		+·15
Bristol ...	−·01		−·36	+·06		+·29	−·95		+·57	+·28		−·18
Cork		−·20		+·32	·00		−·62	+·23		−·11	+·05	
Falmouth .	−·10		−·41	−·03	+·42	+·43	−·78	+·33	+·58	+·03		−·01
Plymouth		−·22	−·43	−·08	+·46	+·51	−·78	+·26	+·57	+·16	−·45	·16
London ...	+·06	−·09		−·28	−·23	+·44	−·62	−·03		+·73	−·31	
Paris	−·08		−·33	−·12			−·32		+·14	+·26		−·51
Christiania			+·12	+·16		+·15	−·02		−·05	−·20		−·15

An extraordinary depression of the barometer (height 28·612) for that latitude occurred about the same time at Paris† as that in England; it however occurred at 6 A.M. of the day previous to that in this country, for which reason I have noticed it here, as at first sight it might seem opposed to the general order of the phænomena presented by the foregoing observations; but it did not occur during "the hurricane," for the wind in the Paris meteorological register for the 12th is quoted merely "strong," but on the following day, when the fall corresponding to the great depression in England took place, the wind is quoted "very strong," and the tables show also a fall of the barometer in England on the 12th corresponding with that at Paris.

I may now conclude this part of the subject by remarking that observations for other periods than those chosen might exhibit difficulties which do not appear in these, although I have made no selection in publishing them, but merely given those I collected on account of the period included in them

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* Time of minimum not noted but during the night of 12–13.

† *Annales de Chimie et de Physique.*

being more than usually stormy. Whatever difficulties do arise, however, it must be borne in mind that the situation of this island is peculiar, having in summer a temperature below that of parts of Europe on the north, and in winter above that of part of the continent on the south; hence a very considerable complexity in the directions of the currents must occasionally occur.

The results given in the first portion of this paper are deduced from the simple fact of the descent of the upper current of the atmosphere, and are altogether independent of the immediate cause of its descent; that it must always be descending in some portion or other of its course, to supply the place of the air flowing in the surface current towards the equator, is very evident; but the indications of the barometer show that the acting cause of its descent at any particular time is not—always at least—a deficiency or rarefaction of the air, such as would be occasioned by the flowing of the lower current, were its effect uncompensated by the arrival of air from above, for the south wind often sets in when the barometer is high. I have alluded to this subject before (Phil. Mag. Oct. 1843, p. 280), noticing the effect of the difference of the opposite currents with respect to the quantity of aqueous vapour in each; but though there appears no reason to doubt that effect being, in a greater or less degree, as there supposed, I stated a difficulty which it is probable does not exist, that of the descending air being warmer than the air previously in its place—an opinion derived simply from the fact of the upper current having, from its origin, generally the higher temperature. Now it may very often be observed, though not always, that the temperature does actually become colder immediately at the change of the wind from north to south, though it rises again on the continuance of the wind; for when once it has found its way to the surface, then of course, whatever its temperature subsequently, it will continue there until its force be overcome, one portion making way for the next following. The cases in which the temperature rises immediately on the change of the wind may be those in which the change either takes place, not from an immediate descent of the current, but simply from its advance (§ 16) from southern localities, where it has previously descended, or perhaps been blowing for some time, and advanced by reason of an increase of force; or by its recession (§ 15) from north *.

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* This is a distinction which must be carefully borne in mind during the reading of these remarks, and during all consideration of this subject, in order to guard us from drawing inferences in any one particular case of a change of wind, when its nature in this respect is not known.

Now the south wind descends or blows in the greatest degree during the winter half of the year *, and then most at night; that is, when the temperature of the earth and the atmosphere is falling by radiation; whilst in spring and summer —the temperature increasing—the lower current, or north-east wind, prevails more than at any other time of the year. Now it is obvious that were the cooling of the atmosphere unaffected by that of the earth, its upper strata, notwithstanding the facility with which heat passes through it, would be first and most cooled by radiation; but we know that within small heights—to the extent of about 100 feet—above the surface of the earth, in which such observations can be easily conducted, the lower strata are much the coldest in nights when radiation is vigorous, by reason of the cooling of the earth. It is obvious however that it does not at all necessarily follow from this that this increase of temperature in ascending, or rather as it would be after a certain height, increase compared with the general progressive decrease of the temperature, should go on towards the higher regions of the atmosphere; for it is evident, from the very great rapidity in which the temperature decreases towards the surface, that it is very much, if not almost entirely, owing to contact; but on the contrary there will be a certain elevation, perhaps not very great, at which the cooling of the atmosphere by the greater radiation of heat into space in the strata above than in those below it, is so much greater than the cooling in the air below, occasioned by radiation to the earth, that the previous relation of the temperatures of the air of the two currents may become at any time, when this difference is not very great, reversed, and the upper one, being then comparatively the colder, descends. The cooling may also be materially affected by the presence of clouds, and by the heights above the surface of the earth at which they are formed; as these would present a comparatively dense radiating body, and by cooling the particles of air in contact with them, would cause currents of cold air to

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* The blowing of the wind from west, whether north or south, may of course (except in the case (§ 12) where it is caused by the flowing of air to restore the atmospheric pressure as in storms) be taken as evidence of the descent of the upper current; thus in the cold months of the year, although the north wind is strongly urged on towards warmer regions by the greater difference of temperature between adjacent latitudes, due to the season, and frequently prevails,—it is generally from north-west.

The phænomena of tropical regions correspond to those of high latitudes; thus the time of descent, or of hurricanes, is after the sun has attained its greatest northern declination, consequently when solar radiation is decreasing; the temperature however is very little fallen, and is in somewhat of the variable state of high latitudes, the blowing of the trade-wind being then nearly suspended.

descend; and also prevent radiation from the earth and atmospheric strata beneath them. Admitting such a cause of descent of cold air, we shall have an explanation of those sudden colds often experienced, which cannot be accounted for on the supposition of the arrival of air from colder regions.

But in the summer half of the year the time of descent of the upper current is not in the night more especially than during the day.

Being desirous of confirming the opinion that the south wind prevailed most at night, I selected from my own register of the wind, made three times a day, all the changes of wind from north to south and from south to north for one year, noting the time (whether night or day) at which they occurred, and met with the unexpected result, that whilst with regard to the winter months the opinion was amply confirmed, in those of summer the result was rather the opposite to it, the change on the whole taking place most frequently in the daytime; and in order to confirm this conclusion, the changes were selected from two other years, with the same result. I have presented in a table the average number of changes for the three years, merely remarking that with scarcely an exception the same month of each of the three years gave a result in accordance with the mean one. The table also contains the changes from south to north, which are in general opposite to the contrary ones.

Number of Changes of Wind.

Month.	Night.		Day.		Month.	Night.		Day.	
	From N. to S.	From S. to N.	From N. to S.	From S. to N.		From N. to S.	From S. to N.	From N. to S.	From S. to N.
Jan.	7·3	1·7	1·7	6·0	April	2·3	5·3	6·3	2·7
Feb.	3·7	2·0	1·7	3·7	May	4·0	4·0	3·3	3·7
March	6·3	4·3	2·3	4·3	June	3·3	2·7	1·3	1·3
Oct.	4·3	3·3	2·7	3·3	July	3·3	5·3	5·3	4·3
Nov.	5·3	1·3	1·7	6·0	Aug.	3·7	5·0	5·0	3·7
Dec.	5·0	4·0	2·3	3·3	Sept.	3·7	3·0	2·3	3·3
Total	31·9	16·6	12·4	26·6	Total	20·31	25·32	23·6	19·0

The want of exact consistency which appears in the results of the summer months is easily accounted for by the lightness of the wind and its arising often from merely local circumstances; the total result however is sufficiently decided to show the difference of character of the winter months. Now in summer the temperature of the lower strata of the atmosphere with respect to those above, from the heat derived from the ground, is comparatively much greater than in winter, the difference of course being greatest in the daytime; thus then the daytime is more favourable for the descent of air from the upper strata than in winter.

Hence then I think it may be concluded that the air of the upper current becomes relatively colder than the lower strata of the atmosphere by loss of its heat by its own radiation, and that when the cold has arrived at a certain degree, it descends, if other conditions which influence its descent are favourable; these conditions being the state of the temperature and pressure of the air of adjacent latitudes, by which the force urging forward the surface current of the atmosphere is affected; and the state of the opposite currents with respect to aqueous vapour.

XLVI. *On the Derivation of the Word* Theodolite.

By Professor DE MORGAN[*].

THE word *theodolite* has puzzled all who have tried to trace it to its origin. Some have connected it with the roots of *θεάομαι* and δολιχὸς, and made it a *seer of lengths*, though the instrument neither does, nor ever did, see anything but angles. In a modern dictionary of good reputation, it is connected with *θεάομαι* and δόλος, and made a *seer of stratagems*, which might apply to a telescope: but unfortunately the use of the term theodolite was prior to the invention of the telescope.

The word is exclusively English, never having obtained any mention from foreigners till comparatively recent times. The *Encyclopédie Méthodique* (1789) does indeed give the word without allusion to its origin; but Savérien's dictionary (1753) says that the *theodotile* (as it is spelt) is an instrument used by the English, much resembling the *graphomètre*.

I find that the use of the word runs back to the "Geometricall practise named Pantometria," begun by Leonard Digges, and finished by Thomas Digges his son (published London 1571, quarto, reprinted in 1591). But it seems as if the name was not then new. Chapter 27 is on "the composition of the instrument called Theodelitus," and it is plain from various modes of speaking that the word is here an adjective or participle. This "circle called Theodelitus," or "planisphere called Theodelitus," is nothing but a graduated circle with a revolving diameter furnished with sights, and placed horizontally. Held vertically, it would have been the astrolabe of the period, and nothing else. In Leybourn's 'Compleat Surveyor,' 1657, we learn that the altitude circle was *sometimes*

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[*] Communicated by the Author.

added; and in Stone's Mathematical Dictionary (1726) that it was *sometimes* furnished with a telescope.

A ruler with sights, travelling upon a graduated circle, was a constituent part of various astronomical instruments imported into Europe from the East, and was accompanied by the Arabic term *alhidada* to express it. The word *alidade* or *alhidade* (for it is spelt both ways) is completely naturalized in France, and appears in the common dictionaries. It was also used by the English writers of the sixteenth century, and among others by Digges himself. The original *theodolite* being nothing but a graduated circle with an *alidade*, some connexion between the terms might be suspected by those to whose notice they are brought. But so different do the words appear, that I, for one, should never have been reminded of the first by the second, if I had not happened to find, in a writer contemporary with Digges, an intermediate formation, which brings the two words nearer together. William Bourne's 'Treasure for Travailers' was published in 1578; he does not use the word theodelite, but calls the instrument the "horizontall or flatte sphere." He begins by spelling the word alhidada thus, *alydeday*, but soon changes it, and keeps very steadily to *athelida*, which is the only technical term introduced in his description of what Digges calls theodelitus. From these premises, I cannot help inferring that the *theodelited* circle of Digges, and the *athelidated* circle of Bourne, which are certainly the same things, are but described by different corruptions of the Arabic word whose earliest European form is *alhidada*.

In our day such a transformation might not be easy; but when the works above-mentioned were written, nothing was more common than to spell the same word in two different ways in the course of one sentence. Bourne himself, though he sometimes spells the name of Digges's work correctly, Pantometria, yet in the first place in which it occurs, he makes *Pantometay* of it, possibly a misprint for Pantometry.

The fact seems to have been thus in this and many other instances. In the sixteenth century, before the language was well-settled, an author more accustomed to Latin than English, would try to anglicize some technical terms; and, not finding his results please his own fancy, would then fall back upon the Latin. Bourne has done this with both *athelida* and *pantometria*; and, were it worth while, I could show abundance of similar instances in other writers.

Nor is it against the connexion of the words that Digges uses them both. Instances are not wanting in which two different spellings of the same word are used by the same writers for different things. For example, the original English sense of the word *square* applies to an angle, not a figure; a right angle is a square corner; and to this day the carpenter's right angle is called a square. But I could name half-a-dozen writers of the end of the sixteenth century who use the two spellings *square* and *squire*, the former in the modern sense, the latter for the carpenter's instrument.

XLVII. *Reply to the Observations of* M. Pierre, *on the Proportion of Water in the Magnesian Sulphates and Double Sulphates. By* THOMAS GRAHAM, *Esq., F.R.S.*[*]

IN a late number of the *Annales de Chimie*, a paper by M. Isidore Pierre appears, On the Double Salts formed by the Oxides of the Magnesian Group, of which an abstract is also given in the March Number of the Philosophical Magazine, containing statements which demand some remark from myself. It presents new analyses of the sulphate of magnesia and potash, and other double sulphates of the same type, from which the author infers that these well-known double salts possess seven atoms of water crystallization, and not six atoms, as resulted from my own analyses and the analyses of all other chemists who have of late years examined these salts. The double salts in question are thus made by M. Pierre to have the same proportion of water as sulphate of magnesia itself; while the latter salt, also, is not found to retain its seventh atom of water more strongly than the other six, but to become anhydrous at 212°, or a few degrees above that temperature, in a current of dry air. The author then infers that his results are subversive of the theory which was originally published by myself, of the constitution of the magnesian sulphates, and to which I still adhere, namely that they contain an atom of water strongly attached and not easily expelled by heat, but readily replaced by an alkaline sulphate, with formation of a double salt.

Although confident of the accuracy of the analyses thus impugned, I considered it due to M. Pierre, who, although a young chemist, has afforded every evidence of habitual care and accuracy in another experimental inquiry of importance, to repeat my experiments.

Of the double sulphate of zinc and potash, 31·46 grains by drying at 212° for several days, lost 7·75 grains of water; and by fusion at a heat verging on redness, 0·08 grain of water additional, making the whole loss 7·83 grains. Hence the composition of the salt with reference to water is as follows:

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[*] Communicated by the Author.

	Experiment.	Theory of 6HO.	Theory of 7HO (Pierre).
Water	24·89	24·03	27·32
Sulphate of zinc and potash	75·11	75·97	72·68
	100·	100·	100·

The experiment obviously indicates six and not seven equivalents of water. The slight excess of 0·86 per cent. of water is not more than is usually found in crystallized salts, arising from the difficulty of divesting them entirely of water mechanically interposed between the plates of the crystals. The peculiarly high disposition of this particular class of salts to retain mechanical water, has been noted by Mitscherlich, myself, and almost every one else who has made them the subject of investigation. It has probably been the cause of the error into which M. Pierre has fallen, in over-estimating their proportion of water.

Although it is scarcely necessary to extend the inquiry to the other double salts of the class, which being isomorphous with the last have necessarily the same proportion of water, still I may be allowed to avail myself of a series of five analyses of the double sulphate of copper and potash lately executed in the laboratory of my friend Prof. Fownes, and which he has kindly communicated to me.

	Expt. 1.	2.	3.	4.	5.	Theory of 6HO.	Theory of 7HO (Pierre).
Water............	25·20	24·00	25·00	25·2	24·4	24·44	27·40
Sulph. of copper and potash ...	74·80	76·00	75·00	74·8	75·6	75·56	72·60
	100·	100·	100·	100·	100·	100·	100·

These experiments all concur in proving that six equivalents is the proportion of water in the double sulphate of copper and potash, and not seven equivalents.

Although M. Pierre gives seven atoms of water to the double sulphate of magnesia and potash, he adds, near the end of his paper, as if to qualify the statement, that when he communicated his results to M. Balard, that chemist informed him that the double sulphate of magnesia and potash contained no more than six equivalents of water, and was therefore consistent with the views of Mr. Graham.

With reference to the single atom of water strongly retained by the magnesian sulphates, an experiment was made on sulphate of zinc. The crystallized salt dried for several days at 212°, in the same circumstances as those in which the double sulphate of zinc and potash became anhydrous, still retained water. The heat being continued for three or four days after the salt ceased to lose weight, it was thereafter found to consist of—

	Experiment.		With one equivalent of water.
Sulphate of zinc .	20·42	89·20	90·03
Water	2·46	10·75	9·97
	22·88	100·	100·

It is sufficiently evident, therefore, that sulphate of zinc, which is admitted by M. Pierre to contain seven equivalents of water, retains one equivalent of water by a stronger affinity than the other six, contrary to his observation; while, moreover, this strongly retained atom of water is absent in the double sulphate of zinc and potash, the last containing only six atoms of water—the experimental data on which the view of the constitution of these salts controverted by that chemist is founded.

XLVIII. *On the Cohesion of Liquids and their Adhesion to Solid Bodies.* *By* M. F. Donny, *Agrégé à l'Université de Gand, Préparateur du Cours de Chimie.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE twenty-sixth volume of your valuable Magazine (p. 541) contains an account of two communications made by Prof. Henry to the American Philosophical Society, on the 5th of April and 17th of May 1844, both relative to the cohesion of liquids.

I have been investigating the same subject from the beginning of 1841 to the end of 1843, when I gave a full description of my experiments on cohesion and adhesion in a written communication addressed to the Académie Royale de Bruxelles. The reception of this memoir is recorded in the *Bulletin de la Séance du 2 Décembre* 1843 (tome x. p. 457), and the memoir itself is printed in the *Mémoires Couronnées et des Savants Etrangers,* tome xvii. I beg leave to direct your attention to the contents of this communication.

Having discovered, in 1841, as Prof. Henry did in 1844, that the cohesion of liquids is a powerful attraction, entirely misrepresented in the works on natural philosophy, I endeavoured to find out the cause of this misrepresentation. With this object I constructed a very simple instrument, which enabled me to observe accurately how the separation of water from water is effected, in the well-known experiment of a plate suspended from a scale-beam over a vessel of water. The use of this new instrument convinced me immediately that there is no similitude whatever between the rupture of a solid body and this mode of separating water from water. I perceived plainly that such a separation was the final result of a series of successive transformations undergone by that portion of the liquid which is lifted up during the ascension of the plate; which transformations ultimately reduce the thinnest part of that ascending liquid to so small a diameter, that it gives way, even without any further exterior exertion. The first experimenters, not being aware of this mode of acting, considered the separation of water from water as if it were similar to the rupture of a solid body; they made their calculations accordingly, and so doing, reduced to the lowest proportions that very strong molecular attraction which fixed my attention in Europe and Prof. Henry's in America.

The learned Professor has proved the magnitude of this molecular attraction by observations on soap-bubbles. I followed quite a different course, and arrived at more extensive results.

I constantly employed liquids placed in glass tubes, whose interior diameter measured from eight to ten millimetres (from three-tenths to four-tenths of an English inch). In similar circumstances, two distinct molecular forces are acting,—the attraction of water for water, or *cohesion*; and the attraction of water for glass, or *adhesion*. In my experiments, both cohesion and adhesion appeared very weak when the liquid was not deprived of that portion of air which it usually contains; and, on the contrary, proved very powerful when air was excluded.

In order to exhibit this power of attraction in airless liquids*, I have made use of two different disjunctive forces; that of mechanical traction in my first two experiments, and that of repulsive caloric in the other.

My first experiment was made on sulphuric acid deprived of air by means of a very powerful air-pump†. In that case molecular attraction proved to be superior to the weight of a column of acid, whose height was 1250 millimetres (more than 4 English feet).

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* By *airless liquids,* I mean liquids deprived of air by one of the peculiar processes described in my memoir. In this sense, distilled water, although containing less air than common water, is far from being an airless liquid.

† This pump, constructed on a new plan, without either cock or valve, was described in 1841. It is recorded in the *Rapport du Jury et Documents de l'Exposition de l'Industrie Belge en* 1841, p. 161.

The second experiment was made on airless water, and the molecular force exceeded the weight of one atmosphere.

The third experiment proved the molecular attraction of airless water to be superior to the weight of three atmospheres, and exhibited very curious phænomena. The liquid had been placed in such circumstances as to be free from any pressure whatever; its temperature was carried to + 135° Centigrade (about +275 Fahr.); and, nevertheless, it did not exhibit the least symptom of ebullition, but by still increasing heat, a part of it was suddenly vaporized with a kind of explosion.

A fourth experiment was tried by placing distilled water (not deprived of air) in a tube similar to that used in the third experiment; an external pressure equal to three atmospheres was applied to the liquid, which was then carried to the above-mentioned temperature of + 135° Centigrade: a calm, ordinary ebullition ensued, without any symptom of explosion.

In a fifth experiment, airless water was placed in a situation comparable to that of water in a steam-boiler working under low pressure. Continually increasing heat could not bring the airless liquid to ordinary ebullition; but the molecular attraction gave way from time to time by distinct explosions, becoming successively more and more violent, till a final one, blowing up the liquid mass and fracturing the instrument, put an end to the experiment.

My sixth experiment exhibited the molecular force in a still more striking form. A tube quite open at one end, half-filled with airless water, was heated over a lamp: no ebullition ensued, but a violent explosion took place, the water being at the same time suddenly projected out of the tube and converted into a cloud of vapour.

After a complete description of the experiments, a new theory of the ebullition of liquids is proposed as a consequence of the above-mentioned results, and of some peculiar considerations fully expounded in my memoir, and whereof it will be sufficient to mention here two of the most striking.

1. The molecules composing the surfaces of volatile bodies are very much inclined to assume a gaseous form, even whilst the internal molecules are kept together by a strong attraction.

2. Ordinary ebullition does not take place at once in the whole mass of a boiling liquid, the ebullitive motion being generated from some points of that portion of the boiler's internal surface which is near the source of heat; which points evolve a succession of large bubbles of vapour, tumultuously ascending through the liquid to its uppermost surface.

According to this new theory, *ebullition is a peculiar kind* *of very rapid evaporation generated on those internal liquid surfaces which surround one or more bubbles of a gaseous fluid.*

I am, Gentlemen,
Your most humble Servant,
Ghent, March 2, 1846. F. DONNY.

XLIX. *Experimental Researches in Electricity.—Nineteenth Series. By* MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Cor. Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.*[*]

§ 26. *On the magnetization of light and the illumination of magnetic lines of force*[†].

¶ i. *Action of magnets on light.*

2146. I HAVE long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their ac-

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[*] From the Philosophical Transactions for 1846, Part I., having been read November 20, 1845.

[†] The title of this paper has, I understand, led many to a misapprehension of its contents, and I therefore take the liberty of appending this explanatory note. Neither accepting nor rejecting the hypothesis of an æther, or the corpuscular, or any other view that may be entertained of the nature of light; and, as far as I can see, nothing being really known of a ray of light more than of a line of magnetic or electric force, or even of a line of gravitating force, except as it and they are manifest in and by substances; I believe that, in the experiments I describe in the paper, light has been magnetically affected, *i. e.* that that which is magnetic in the forces of matter has been affected, and in turn has affected that which is truly magnetic in the force of light: by the term magnetic I include here either of the peculiar exertions of the power of a magnet, whether it be that which is manifest in the magnetic or the diamagnetic class of bodies. The phrase "illumination of the lines of magnetic force" has been understood to imply that I had rendered them luminous. This was not within my thought. I intended to express that the line of magnetic force was illuminated as the earth is illuminated by the sun, or the spider's web illuminated by the astronomer's lamp. Employing a ray of light, we can tell, *by the eye*, the direction of the magnetic lines through a body; and by the alteration of the ray and its optical effect on the eye, can see the course of the lines just as we can see the course of a thread of glass, or any other transparent substance, rendered visible by the light: and this was what I meant by *illumination*, as the paper fully explains.—December 15, 1845 M. F.

tion*. In modern times the proofs of their convertibility have been accumulated to a very considerable extent, and a commencement made of the determination of their equivalent forces.

2147. This strong persuasion extended to the powers of light, and led, on a former occasion, to many exertions, having for their object the discovery of the direct relation of light and electricity, and their mutual action in bodies subject jointly to their power†; but the results were negative and were afterwards confirmed, in that respect, by Wartmann‡.

2148. These ineffectual exertions, and many others which were never published, could not remove my strong persuasion derived from philosophical considerations; and, therefore, I recently resumed the inquiry by experiment in a most strict and searching manner, and have at last succeeded in *magnetizing and electrifying a ray of light, and in illuminating a magnetic line of force.* These results, without entering into the detail of many unproductive experiments, I will describe as briefly and clearly as I can.

2149. But before I proceed to them, I will define the meaning I connect with certain terms which I shall have occasion to use:—thus, by *line of magnetic force*, or *magnetic line of force*, or *magnetic curve*, I mean that exercise of magnetic force which is exerted in the lines usually called magnetic curves, and which equally exist as passing from or to magnetic poles, or forming concentric circles round an electric current. By *line of electric force*, I mean the force exerted in the lines joining two bodies, acting on each other according to the principles of static electric induction (1161, &c.), which may also be either in curved or straight lines. By a *diamagnetic*, I mean a body through which lines of magnetic force are passing, and which does not by their action assume the usual magnetic state of iron or loadstone.

2150. A ray of light issuing from an Argand lamp, was polarized in a horizontal plane by reflexion from a surface of glass, and the polarized ray passed through a Nichol's eyepiece revolving on a horizontal axis, so as to be easily examined by the latter. Between the polarizing mirror and the eye-piece, two powerful electro-magnetic poles were arranged, being either the poles of a horse-shoe magnet, or the contrary poles of two cylinder magnets; they were separated from each other about two inches in the direction of the line of the ray,

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* Experimental Researches, 57, 366, 376, 877, 961, 2071.

† Philosophical Transactions, 1834. Experimental Researches, 951-955.

‡ *Archives de l'Electricité*, ii. pp. 596-600.

and so placed, that, if on the same side of the polarized ray, it might pass near them; or, if on contrary sides, it might go between them, its direction being always parallel, or nearly so, to the magnetic lines of force (2149.). After that, any transparent substance placed between the two poles, would have passing through it, both the polarized ray and the magnetic lines of force at the same time and in the same direction.

2151. Sixteen years ago I published certain experiments made upon optical glass*, and described the formation and general characters of one variety of heavy glass, which, from its materials, was called silicated borate of lead. It was this glass which first gave me the discovery of the relation between light and magnetism, and it has power to illustrate it in a degree beyond that of any other body; for the sake of perspicuity I will first describe the phænomena as presented by this substance.

2152. A piece of this glass, about two inches square and 0·5 of an inch thick, having flat and polished edges, was placed as a *diamagnetic* (2149.) between the poles (not as yet magnetized by the electric current), so that the polarized ray should pass through its length; the glass acted as air, water, or any other indifferent substance would do; and if the eyepiece were previously turned into such a position that the polarized ray was extinguished, or rather the image produced by it rendered invisible, then the introduction of this glass made no alteration in that respect. In this state of circumstances the force of the electro-magnet was developed, by sending an electric current through its coils, and immediately the image of the lamp-flame became visible, and continued so as long as the arrangement continued magnetic. On stopping the electric current, and so causing the magnetic force to cease, the light instantly disappeared; these phænomena could be renewed at pleasure, at any instant of time, and upon any occasion, showing a perfect dependence of cause and effect.

2153. The voltaic current which I used upon this occasion, was that of five pair of Grove's construction, and the electromagnets were of such power that the poles would singly sustain a weight of from twenty-eight to fifty-six, or more, pounds.

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* Philosophical Transactions, 1830, p. 1. I cannot resist the occasion which is thus offered to me of mentioning the name of Mr. Anderson, who came to me as an assistant in the glass experiments, and has remained ever since in the Laboratory of the Royal Institution. He has assisted me in all the researches into which I have entered since that time, and to his care, steadiness, exactitude, and faithfulness in the performance of all that has been committed to his charge, I am much indebted.—M. F.

A person looking for the phænomenon for the first time would not be able to see it with a weak magnet.

2154. The character of the force thus impressed upon the diamagnetic is that of *rotation*; for when the image of the lamp-flame has thus been rendered visible, revolution of the eye-piece to the right or left, more or less, will cause its extinction; and the further motion of the eye-piece to the one side or other of this position will produce the reappearance of the light, and that with complementary tints, according as this further motion is to the right- or left-hand.

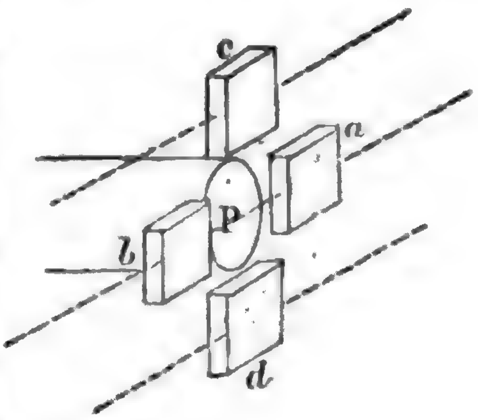
2155. When the pole nearest to the observer was a marked pole, *i. e.* the same as the north end of a magnetic needle, and the further pole was unmarked, the rotation of the ray was right-handed; for the eye-piece had to be turned to the right-hand, or clock fashion, to overtake the ray and restore the image to its first condition. When the poles were reversed, which was instantly done by changing the direction of the electric current, the rotation was changed also and became left-handed, the alteration being to an equal degree in extent as before. The direction was always the same for the same *line of magnetic force* (2149.).

2156. When the diamagnetic was placed in the numerous other positions, which can easily be conceived, about the magnetic poles, results were obtained more or less marked in extent, and very definite in character, but of which the phænomena just described may be considered as the chief example: they will be referred to, as far as is necessary, hereafter.

2157. The same phænomena were produced in the silicated borate of lead (2151.) by the action of a good ordinary steel horse-shoe magnet, no electric current being now used. The results were feeble, but still sufficient to show the perfect identity of action between electro-magnets and common magnets in this their power over light.

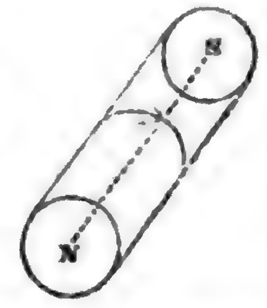
2158. Two magnetic poles were employed end-ways, *i. e.* the cores of the electro-magnets were hollow iron cylinders, and the ray of polarized light passed along their axes and through the diamagnetic placed between them: the effect was the same.

2159. One magnetic pole only was used, that being one end of a powerful cylinder electro-magnet. When the heavy glass was beyond the magnet, being close to it but between the magnet and the polarizing reflector, the rotation was in one direction, dependent on the nature of the pole; when the diamagnetic was on the near side, being close to it but between it and the eye, the rotation for the same pole was in the contrary direction to what it was before; and when the magnetic pole was changed, both these directions were changed with it. When the heavy glass was placed in a corresponding position to the pole, but above or below it, so that the *magnetic curves* were no longer passing through the glass parallel to the ray of polarized light, but rather perpendicular to it, then no effect was produced. These particularities may be understood by reference to fig. 1, where *a* and *b* represent the first positions of the diamagnetic, and *c* and *d* the latter positions, the course of the ray being marked by the dotted line. If also the glass were placed directly at the end of the magnet, then no effect was produced on a ray passing in the direction here described, though it is evident, from what has been already said (2155.), that a ray passing *parallel* to the magnetic lines through the glass so placed, would have been affected by it.



2160. Magnetic lines, then, in passing through silicated borate of lead, and a great number of other substances (2173.), cause these bodies to act upon a polarized ray of light when the lines are parallel to the ray, or in proportion as they are parallel to it: if they are perpendicular to the ray, they have no action upon it. They give the diamagnetic the power of rotating the ray; and the *law* of this action on light is, that if a magnetic line of force be *going from* a north pole, or *coming* from a south pole, along the path of a polarized ray coming to the observer, it will rotate that ray to the right-hand; or, that if such a line of force be coming from a north pole, or going from a south pole, it will rotate such a ray to the left-hand.

2161. If a cork or a cylinder of glass, representing the diamagnetic, be marked at its ends with the letters N and S, to represent the poles of a magnet, the line joining these letters may be considered as a magnetic line of force; and further, if a line be traced round the cylinder with arrow heads on it to represent direction, as in the figure, such a simple model, held up before the eye, will express the whole of the law, and give every position and consequence of direction resulting from it. If a watch be considered as the diamagnetic, the north pole of a magnet being imagined



against the face, and a south pole against the back, then the motion of the hands will indicate the direction of rotation which a ray of light undergoes by magnetization.

2162. I will now proceed to the different circumstances which affect, limit, and define the extent and nature of this new power of action on light.

2163. In the first place, the rotation appears to be in proportion to the extent of the diamagnetic through which the ray and the magnetic lines pass. I preserved the strength of the magnet and the interval between its poles constant, and then interposed different pieces of the same heavy glass (2151.) between the poles. The greater the extent of the diamagnetic in the line of the ray, whether in one, two, or three pieces, the greater was the rotation of the ray; and, as far as I could judge by these first experiments, the amount of rotation was exactly proportionate to the extent of diamagnetic through which the ray passed. No addition or diminution of the heavy glass on the *side* of the course of the ray made any difference in the effect of that part through which the ray passed.

2164. The power of rotating the ray of light *increased* with the intensity of the magnetic lines of force. This general effect is very easily ascertained by the use of electro-magnets; and within such range of power as I have employed, it appears to be directly proportionate to the intensity of the magnetic force.

2165. Other bodies, besides the heavy glass, possess the same power of becoming, under the influence of magnetic force, active on light (2173.). When these bodies possess a rotative power of their own, as is the case with oil of turpentine, sugar, tartaric acid, tartrates, &c., the effect of the magnetic force is to add to, or subtract from, their specific force, according as the natural rotation and that induced by the magnetism is right- or left-handed (2231.).

2166. I could not perceive that this power was affected by any degree of motion which I was able to communicate to the diamagnetic, whilst jointly subject to the action of the magnetism and the light.

2167. The interposition of copper, lead, tin, silver, and other ordinary non-magnetic bodies in the course of the magnetic curves, either between the pole and the diamagnetic, or in other positions, produced no effect either in kind or degree upon the phænomena.

2168. Iron frequently affected the results in a very considerable degree; but it always appeared to be, either by altering the direction of the magnetic lines, or disposing within itself of their force. Thus, when the two contrary poles were on one side of the polarized ray (2150.), and the heavy glass in its best position between them and in the ray (2152.), the bringing of a large piece of iron near to the glass on the other side of the ray, caused the power of the diamagnetic to fall. This was because certain lines of magnetic force, which at first passed through the glass parallel to the ray, now crossed the glass and the ray; the iron giving two contrary poles opposite the poles of the magnet, and thus determining a new course for a certain portion of the magnetic power, and that across the polarized ray.

2169. Or, if the iron, instead of being applied on the opposite side of the glass, were applied on the same side with the magnet, either near it or in contact with it, then, again, the power of the diamagnetic fell, simply because the power of the magnet was diverted from it into a new direction. These effects depend much of course on the intensity and power of the magnet, and on the size and softness of the iron.

2170. The electro-helices (2190.) without the iron cores were very feeble in power, and indeed hardly sensible in their effect. With the iron cores they were powerful, though no more electricity was then passing through the coils than before (1071.). This shows, in a very simple manner, that the phænomena exhibited by light under these circumstances, is directly connected with the magnetic form of force supplied by the arrangement. Another effect which occurred illustrated the same point. When the contact at the voltaic battery is made, and the current sent round the electro-magnet, the image produced by the rotation of the polarized ray does not rise up to its full lustre immediately, but increases for a couple of seconds, gradually acquiring its greatest intensity; on breaking the contact, it sinks instantly and disappears apparently at once. The gradual rise in brightness is due to the *time* which the iron core of the magnet requires to evolve all that magnetic power which the electric current can develope in it; and as the magnetism rises in intensity, so does its effect on the light increase in power; hence the progressive condition of the rotation.

2171. I cannot as yet find that the heavy glass (2151.), when in this state, *i. e.* with magnetic lines of force passing through it, exhibits any increased degree, or has any specific magneto-inductive action of the recognized kind. I have placed it in large quantities, and in different positions, between magnets and magnetic needles, having at the time very delicate means of appreciating any difference between it and air, but could find none.

2172. Using water, alcohol, mercury, and other fluids contained in very large delicate thermometer-shaped vessels, I could not discover that any difference in volume occurred when the magnetic curves passed through them.

2173. It is time that I should pass to a consideration of this power of magnetism over light as exercised, not only in the silicated borate of lead (2151.), but in many other substances; and here we perceive, in the first place, that if all transparent bodies possess the power of exhibiting the action, they have it in very different degrees, and that up to this time there are some that have not shown it at all.

2174. Next, we may observe, that bodies that are exceedingly different to each other in chemical, physical, and mechanical properties, develope this effect; for solids and liquids, acids, alkalies, oils, water, alcohol, æther, all possess the power.

2175. And lastly, we may observe, that in all of them, though the degree of action may differ, still it is always the same in kind, being a rotative power over the ray of light; and further, the direction of the rotation is, in every case, independent of the nature or state of the substance, and dependent upon the direction of the magnetic line of force, according to the law before laid down (2160.).

2176. Amongst the substances in which this power of action is found, I have already distinguished the *silico-borate of lead* (2151.) as eminently fitted for the purpose of exhibiting the phænomena. I regret that it should be the best, since it is not likely to be in the possession of many, and few will be induced to take the trouble of preparing it. If made, it should be well-annealed, for otherwise the pieces will have considerable power of depolarizing light, and then the particular phænomena under consideration are much less strikingly observed. The *borate of lead*, however, is a substance much more fusible, softening at the heat of boiling oil, and therefore far more easily prepared in the form of glass plates and annealed; and it possesses as much magneto-rotative power over light as the silico-borate itself. *Flint-glass* exhibits the property, but in a less degree than the substances above. Crown-glass shows it, but in a still smaller degree.

2177. Whilst employing crystalline bodies as diamagnetics, I generally gave them that position in which they did not affect the polarized ray, and then induced the magnetic curves through them. As a class, they seemed to resist the assumption of the rotating state. *Rock-salt* and *fluor-spar* gave evidence of the power in a slight degree; and I think that a crystal of alum did the same, but its ray length in the transparent part was so small that I could not ascertain the fact decisively. Two specimens of transparent fluor, lent me by Mr. Tennant, gave the effect.

2178. Rock-crystal, four inches across, gave no indications of action on the ray, neither did smaller crystals, nor cubes about three-fourths of an inch in the side, which were so cut as to have two of their faces perpendicular to the axis of the crystal (1692, 1693.), though they were examined in every direction.

2179. *Iceland spar* exhibited no signs of effect, either in the form of rhomboids, or of cubes like those just described (1695.).

2180. *Sulphate of baryta*, *sulphate of lime*, and *carbonate of soda*, were also without action on the light.

2181. A piece of fine clear *ice* gave me no effect. I cannot however say there is none, for the effect of water in the same mass would be very small, and the irregularity of the flattened surface from the fusion of the ice and flow of water, made the observation very difficult.

2182. With some degree of curiosity and hope, I put gold-leaf into the magnetic lines, but could perceive no effect. Considering the extremely small dimensions of the length of the path of the polarized ray in it, any positive result was hardly to be expected.

2183. In experiments with liquids, a very good method of observing the effect, is to inclose them in bottles from 1½ to 3 or 4 inches in diameter, placing these in succession between the magnetic poles (2150.), and bringing the analysing eye-piece so near to the bottle, that, by adjustment of the latter, its cylindrical form may cause a diffuse but useful image of the lamp-flame to be seen through it: the light of this image is easily distinguished from that which passes by irregular refraction through the striæ and deformations of the glass, and the phænomena being looked for in this light are easily seen.

2184. Water, alcohol, and æther, all show the effect; water most, alcohol less, and æther the least. All the fixed oils which I have tried, including almond, castor, olive, poppy, linseed, sperm, elaine from hog's lard, and distilled resin oil, produce it. The essential oils of turpentine, bitter almonds, spike lavender, lavender, jessamine, cloves, and laurel, produce it. Also naphtha of various kinds, melted spermaceti, fused sulphur, chloride of sulphur, chloride of arsenic, and every other liquid substance which I had at hand and could submit in sufficient bulk to experiment.

2185. Of aqueous solutions I tried 150 or more, including the soluble acids, alkalies and salts, with sugar, gum, &c., the list of which would be too long to give here, since the great conclusion was, that the exceeding diversity of substance caused no exception to the general result, for all the bodies showed the property. It is indeed more than probable, that in all these cases the water and not the other substance present was the ruling matter. The same general result was obtained with alcoholic solutions.

2186. Proceeding from liquids to air and gaseous bodies, I have here to state that, as yet, I have not been able to detect the exercise of this power in any one of the substances in this class. I have tried the experiment with bottles 4 inches in diameter, and the following gases: oxygen, nitrogen, hydrogen, nitrous oxide, olefiant gas, sulphurous acid, muriatic acid, carbonic acid, carbonic oxide, ammonia, sulphuretted hydrogen, and bromine vapour, at ordinary temperatures; but they all gave negative results. With air, the trial has been carried, by another form of apparatus, to a much higher degree, but still ineffectually (2212.).

2187. Before dismissing the consideration of the substances which exhibited this power, and in reference to those in which it was superinduced upon bodies possessing, naturally, rotative force (2165. 2231.), I may record, that the following are the substances submitted to experiment: castor oil, resin oil, oil of spike lavender, of laurel, Canada balsam, alcoholic solution of camphor, alcoholic solution of camphor and corrosive sublimate, aqueous solutions of sugar, tartaric acid, tartrate of soda, tartrate of potassa and antimony, tartaric and boracic acid, and sulphate of nickel, which rotated to the right-hand; copaiba balsam, which rotated the ray to the left-hand; and two specimens of camphine or oil of turpentine, in one of which the rotation was to the right-hand, and in the other to the left. In all these cases, as already said (2165.), the superinduced magnetic rotation was according to the general law (2160.), and without reference to the previous power of the body.

2188. Camphor being melted in a tube about an inch in diameter, exhibited high natural rotative force, but I could not discover that the magnetic curves induced additional force in it. It may be, however, that the shortness of the ray length and the quantity of coloured light left, even when the eyepiece was adjusted to the most favourable position for darkening the image produced by the naturally rotated ray, rendered the small magneto-power of the camphor insensible.

¶ ii. *Action of electric currents on light.*

2189. From a consideration of the nature and position of the lines of magnetic and electric force, and the relation of a magnet to a current of electricity, it appeared almost certain that an electric current would give the same result of action on light as a magnet; and, in the helix, would supply a form of apparatus in which great lengths of diamagnetics, and especially of such bodies as appeared to be but little affected between the poles of the magnet, might be submitted to examination and their effect exalted: this expectation was, by experiment, realized.

2190. Helices of copper wire were employed, three of which I will refer to. The first, or *long helix*, was 0·4 of an inch internal diameter; the wire was 0·03 of an inch in diameter, and having gone round the axis from one end of the helix to the other, then returned in the same manner, forming a coil sixty-five inches long, double in its whole extent, and containing 1240 feet of wire.

2191. The second, or *medium helix*, is nineteen inches long, 1·87 inch internal diameter, and three inches external diameter. The wire is 0·2 of an inch in diameter, and eighty feet in length, being disposed in the coil as two concentric spirals. The electric current, in passing through it, is not divided, but traverses the whole length of the wire.

2192. The third, or *Woolwich helix*, was made under my instruction for the use of Lieut.-Colonel Sabine's establishment at Woolwich. It is 26·5 inches long, 2·5 inches internal diameter, and 4·75 inches external diameter. The wire is 0·17 of an inch in diameter, and 501 feet in length. It is disposed in the coil in four concentric spirals connected end to end, so that the whole of the electric current employed passes through all the wire.

2193. The long helix (2190.) acted very feebly on a magnetic needle placed at a little distance from it; the medium helix (2191.) acted more powerfully, and the Woolwich helix (2192.) very strongly; the same battery of ten pairs of Grove's plate being employed in all cases.

2194. Solid bodies were easily subjected to the action of these electro-helices, being for that purpose merely cut into the form of bars or prisms with flat and polished ends, and then introduced as cores into the helices. For the purpose of submitting liquid bodies to the same action, tubes of glass were provided, furnished at the ends with caps; the cylindrical part of the cap was brass, and had a tubular aperture for the introduction of the liquids, but the end was a flat glass plate. When the tube was intended to contain aqueous fluids, the plates were attached to the caps, and the caps to the tube by Canada balsam; when the tube had to contain alcohol, æther or essential oils, a thick mixture of powdered gum with a little water was employed as the cement.

2195. The general effect produced by this form of apparatus may be stated as follows:—The tube within the long helix (2190.) was filled with distilled water and placed in the line of the polarized ray, so that by examination through the eye-piece (2150.), the image of the lamp-flame produced by the ray could be seen through it. Then the eye-piece was turned until the image of the flame disappeared, and, afterwards, the current of ten pairs of plates sent through the helix; instantly the image of the flame reappeared, and continued as long as the electric current was passing through the helix; on stopping the current the image disappeared. The light did not rise up gradually, as in the case of electro-magnets (2170.), but instantly. These results could be produced at pleasure. In this experiment we may, I think, justly say that a ray of light is electrified and the electric forces illuminated.

2196. The phænomena may be made more striking, by the adjustment of a lens of long focus between the tube and the polarizing mirror, or one of short focus between the tube and the eye; and where the helix, or the battery, or the substance experimented with, is feeble in power, such means offer assistance in working out the effects: but, after a little experience, they are easily dispensed with, and are only useful as accessories in doubtful cases.

2197. In cases where the effect is feeble, it is more easily perceived if the Nichol eye-piece be adjusted, not to the perfect extinction of the ray, but a little short of or beyond that position; so that the image of the flame may be but just visible. Then, on the exertion of the power of the electric current, the light is either increased in intensity, or else diminished, or extinguished, or even re-illuminated on the other side of the dark condition; and this change is more easily perceived than if the eye began to observe from a state of utter darkness. Such a mode of observing also assists in demonstrating the rotatory character of the action on light; for, if the light be made visible beforehand by the motion of the eye-piece in one direction, and the power of the current be to *increase* that light, an instant only suffices, after stopping the current, to move the eye-piece in the other direction until the light is apparent as at first, and then the power of the current will be to *diminish* it; the tints of the lights being affected also at the same time.

2198. When the current was sent round the helix in one direction, the rotation induced upon the ray of light was one way; and when the current was changed to the contrary direction, the rotation was the other way. In order to express the direction, I will assume, as is usually done, that the current passes from the zinc through the acid to the platinum in the same cell (663. 667. 1627.): if such a current pass under the ray towards the right, upwards on its right side, and over the ray towards the left, it will give left-handed rotation to it; or, if the current pass over the ray to the right, down on the right side, and under it towards the left, it will induce it to rotate to the right-hand.

2199. The LAW, therefore, by which an electric current acts on a ray of light is easily expressed. When an electric current passes round a ray of polarized light in a plane perpendicular to the ray, it causes the ray to revolve on its axis, as long as it is under the influence of the current, in the *same direction* as that in which the current is passing.

2200. The simplicity of this law, and its identity with that given before, as expressing the action of magnetism on light (2160.), is very beautiful. A model is not wanted to assist the memory; but if that already described (2161.) be looked at, the line round it will express at the same time the direction both of the current and the rotation. It will indeed do much more; for if the cylinder be considered as a piece of iron, and not a piece of glass or other diamagnetic, placed between the two poles N and S, then the line round it will represent the direction of the currents, which, according to Ampère's theory, are moving round its particles; or if it be considered as a core of iron (in place of a core of water), having an electric current running round it in the direction of the line, it will also represent such a magnet as would be formed if it were placed between the poles whose marks are affixed to its ends.

2201. I will now notice certain points respecting the degree of this action under different circumstances. By using a tube of water (2194.) as long as the helix, but placing it so that more or less of the tube projected at either end of the helix, I was able, in some degree, to ascertain the effect of length of the diamagnetic, the force of the helix and current remaining the same. The greater the column of water subjected to the action of the helix, the greater was the rotation of the polarized ray; and the amount of rotation seemed to be directly proportionate to the length of fluid round which the electric current passed.

2202. A short tube of water, or a piece of heavy glass, being placed in the axis of the Woolwich helix (2192.), seemed to produce equal effect on the ray of light, whether it were in the middle of the helix or at either end; provided it was always within the helix and in the line of the axis. From this it would appear that every part of the helix has the same effect;

and, that by using long helices, substances may be submitted to this kind of examination which could not be placed in sufficient length between the poles of magnets (2150.).

2203. A tube of water as long as the Woolwich helix (2192.), but only 0·4 of an inch in diameter, was placed in the helix parallel to the axis, but sometimes in the axis and sometimes near the side. No apparent difference was produced in these different situations; and I am inclined to believe (without being quite sure) that the action on the ray is the same, wherever the tube is placed, within the helix, in relation to the axis. The same result was obtained when a larger tube of water was looked through, whether the ray passed through the axis of the helix and tube, or near the side.

2204. If bodies be introduced into the helix possessing, naturally, rotating force, then the rotating power given by the electric current is superinduced upon them, exactly as in the cases already described of magnetic action (2165. 2187.).

2205. A helix, twenty inches long and 0·3 of an inch in diameter, was made of uncovered copper wire, 0·05 of an inch in diameter, in close spirals. This was placed in a large tube of water, so that the fluid, both in the inside and at the outside of the helix, could be examined by the polarized ray. When the current was sent *through* the helix, the water within it received rotating power; but no trace of such an action on the light was seen on the outside of the helix, even in the line most close to the uncovered wire.

2206. The water was inclosed in brass and copper tubes, but this alteration caused not the slightest change in the effect.

2207. The water in the brass tube was put into an *iron* tube, much longer than either the Woolwich helix or the brass tube, and quite one-eighth of an inch thick in the side; yet when placed in the Woolwich helix (2192.), the water rotated the ray of light apparently as well as before.

2208. An iron bar, one inch square and longer than the helix, was put into the helix, and the small water-tube (2203.) upon it. The water exerted as much action on the light as before.

2209. Three iron tubes, each twenty-seven inches long and one-eighth of an inch in thickness in the side, were selected of such diameters as to pass easily one into the other, and the whole into the Woolwich helix (2192.). The smaller one was supplied with glass ends and filled with water; and being placed in the axis of the Woolwich helix, had a certain amount of rotating power over the polarized ray. The second tube was then placed over this, so that there was now a thickness of iron equal to two-eighths of an inch between the water and the helix; the water had *more* power of rotation than before. On placing the third tube of iron over the two former, the power of the water *fell*, but was still very considerable. These results are complicated, being dependent on the new condition which the character of iron gives to its action on the forces. Up to a certain amount, by increasing the development of magnetic forces, the helix and core, *as a whole*, produce increased action on the water; but on the addition of more iron and the disposal of the forces through it, their action is removed in part from the water and the rotation is lessened.

2210. Pieces of heavy glass (2151.), placed in iron tubes in the helices, produced similar effects.

2211. The bodies which were submitted to the action of an electric current in a helix, in the manner already described, were as follows:—Heavy glass (2151. 2176.), water, solution of sulphate of soda, solution of tartaric acid, alcohol, æther, and oil of turpentine; all of which were affected, and acted on light exactly in the manner described in relation to magnetic action (2173.).

2212. I submitted *air* to the influence of these helices carefully and anxiously, but could not discover any trace of action on the polarized ray of light. I put the long helix (2190.) into the other two (2191. 2192.), and combined them all into one consistent series, so as to accumulate power, but could not observe any effect of them on light passing through air.

2213. In the use of helices, it is necessary to be aware of one effect, which might otherwise cause confusion and trouble. At first, the wire of the long helix (2190.) was wound directly upon the thin glass tube which served to contain the fluid. When the electric current passed through the helix it raised the temperature of the metal, and that gradually raised the temperature of the glass and the film of water in contact with it, and so the cylinder of water, warmer at its surface than its axis, acted as a lens, gathering and sending rays of light to the eye, and continuing to act for a time after the current was stopped. By separating the tube of water from the helix, and by other precautions, this source of confusion is easily avoided.

2214. Another point of which the experimenter should be aware, is the difficulty, and almost impossibility, of obtaining a piece of glass which, especially after it is cut, does not depolarize light. When it does depolarize, difference of position makes an immense difference in the appearance. By always referring to the parts that do not depolarize, as the black cross, for instance, and by bringing the eye as near as may be to the glass, this difficulty is more or less overcome.

2215. For the sake of supplying a general indication of the amount of this induced rotating force in two or three bodies, and without any pretence of offering correct numbers, I will give, generally, the result of a few attempts to measure the force, and compare it with the natural power of a specimen of oil of turpentine. A very powerful electro-magnet was employed, with a *constant* distance between its poles of 2½ inches. In this space was placed different substances; the amount of rotation of the eye-piece observed several times and the average taken, as expressing the rotation for the ray length of substance used. But as the substances were of different dimensions, the ray lengths were, by calculation, corrected to one standard length, upon the assumption that the power was proportionate to this length (2163.). The oil of turpentine was of course observed in its natural state, *i. e.* without magnetic action. Making water 1, the numbers were as follows:—

Oil of turpentine . . .	11·8
Heavy glass (2151.) . .	6·0
Flint-glass	2·8
Rock-salt	2·2
Water	1·0
Alcohol	less than water.
Æther	less than alcohol.

2216. In relation to the action of magnetic and electric forces on light, I consider, that to know the conditions under which there is no apparent action, is to add to our knowledge of their mutual relations; and will, therefore, very briefly state how I have lately combined these forces, obtaining no apparent result (955.).

2217. Heavy glass, flint-glass, rock-crystal, Iceland spar, oil of turpentine, and air, had a polarized ray passed through them; and, at the same time, lines of electro-static tension (2149.) were, by means of coatings, the Leyden jar, and the electric machine, directed across the bodies, parallel to the polarized ray, and perpendicular to it, both in and across the plane of polarization; but without any visible effect. The tension of a rapidly recurring, induced secondary current, was also directed upon the same bodies and upon water (as an electrolyte), but with the same negative result.

2218. A polarized ray, powerful magnetic lines of force, and the electric lines of force (2149.) just described, were combined in various directions in their action on heavy glass

(2151. 2176.), but with no other result than that due to the mutual action of the magnetic lines of light, already described in this paper.

2219. A polarized ray and electric currents were combined in every possible way in electrolytes (951-954). The substances used were distilled water, solution of sugar, dilute sulphuric acid, solution of sulphate of soda, using platinum electrodes; and solution of sulphate of copper, using copper electrodes; the current was sent along the ray, and perpendicular to it in two directions at right angles with each other; the ray was made to rotate, by altering the position of the polarizing mirror, that the plane of polarization might be varied; the current was used as a continuous current, as a rapidly intermitting current, and as a rapidly alternating double current of induction; but in no case was any trace of action perceived.

2220. Lastly, a ray of polarized light, electric currents, and magnetic lines of force, were directed in every possible way through dilute sulphuric acid and solution of sulphate of soda, but still with negative results, except in those positions where the phænomena already described were produced. In one arrangement, the current passed in the direction of radii from a central to a circumferential electrode, the contrary magnetic poles being placed above and below; and the arrangements were so good, that when the electric current was passing, the fluid rapidly rotated; but a polarized ray sent horizontally across this arrangement was not at all affected. Also, when the ray was sent vertically through it, and the eyepiece moved to correspond to the rotation impressed upon the ray in this position by the magnetic curves alone, the superinduction of the passage of the electric current made not the least difference in the effect upon the ray.

¶ iii. *General considerations.*

2221. Thus is established, I think for the first time [*], a

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[*] I say, for the first time, because I do not think that the experiments of Morrichini on the production of magnetism by the rays at the violet end of the spectrum prove any such relation. When in Rome with Sir H. Davy in the month of May 1814, I spent several hours at the house of Morrichini, working with his apparatus and under his directions, but could not succeed in magnetising a needle. I have no confidence in the effect as a *direct* result of the action of the sun's rays; but think, that when it has occurred it has been secondary, incidental, and perhaps even accidental; a result that might well happen with a needle that was preserved during the whole experiment in a north and south position.

January 2, 1846.—I should not have written "for the first time" as above, if I had remembered Mr. Christie's experiments and papers on the Influence of the Solar Rays on Magnets, communicated in the Philosophical Transactions for 1826, p. 219, and 1828, p. 379.—M. F.

true, direct relation and dependence between light and the magnetic and electric forces; and thus a great addition made to the facts and considerations which tend to prove that all natural forces are tied together, and have one common origin (2146.). It is, no doubt, difficult in the present state of our knowledge to express our expectation in exact terms; and, though I have said that another of the powers of nature is, in these experiments, directly related to the rest, I ought, perhaps, rather to say that another form of the great power is distinctly and directly related to the other forms; or that the great power manifested by particular phænomena in particular forms, is here further identified and recognised, by the direct relation of its form of light to its forms of electricity and magnetism.

2222. The relation existing between *polarized* light and magnetism and electricity, is even more interesting than if it had been shown to exist with common light only. It cannot but extend to common light; and, as it belongs to light made, in a certain respect, more precise in its character and properties by polarization, it collates and connects it with these powers, in that duality of character which they possess, and yields an opening, which before was wanting to us, for the appliance of these powers to the investigation of the nature of this and other radiant agencies.

2223. Referring to the conventional distinction before made (2149.), it may be again stated, that it is the magnetic lines of force *only* which are effectual on the rays of light, and they *only* (in appearance) when parallel to the ray of light, or as they tend to parallelism with it. As, in reference to matter not magnetic after the manner of iron, the phænomena of electric induction and electrolysation show a vast superiority in the energy with which electric forces can act as compared to magnetic forces, so here, in another direction and in the peculiar and correspondent effects which belong to magnetic forces, they are shown, in turn, to possess great superiority, and to have their full equivalent of action on the same kind of matter.

2224. The magnetic forces do not act on the ray of light directly and without the intervention of matter, but through the mediation of the substance in which they and the ray have a simultaneous existence; the substances and the forces giving to and receiving from each other the power of acting on the light. This is shown by the non-action of a vacuum, of air or gases; and it is also further shown by the special degree in which different matters possess the property. That magnetic force acts upon the ray of light always with the same character of manner and in the same direction, independent of the different varieties of substance, or their states of solid or liquid, or their specific rotative force (2232.), shows that the magnetic force and the light have a direct relation: but that substances are necessary, and that these act in different degrees, shows that the magnetism and the light act on each other through the intervention of the matter.

2225. Recognizing or perceiving *matter* only by its powers, and knowing nothing of any imaginary nucleus, abstract from the idea of these powers, the phænomena described in this paper much strengthen my inclination to trust in the views I have on a former occasion advanced in reference to its nature*.

2226. It cannot be doubted that the magnetic forces act upon and affect the internal constitution of the diamagnetic, just as freely in the dark as when a ray of light is passing through it; though the phænomena produced by light seem, as yet, to present the only means of observing this constitution and the change. Further, any such change as this must belong to opake bodies, such as wood, stone, and metal; for as diamagnetics, there is no distinction between them and those which are transparent. The degree of transparency can at the utmost, in this respect, only make a distinction between the individuals of a class.

2227. If the magnetic forces had made these bodies magnets, we could, by light, have examined a transparent magnet; and that would have been a great help to our investigation of the forces of matter. But it does not make them magnets (2171.), and therefore the molecular condition of these bodies, when in the state described, must be specifically distinct from that of magnetized iron, or other such matter, and must be *a new magnetic condition*; and as the condition is a state of tension (manifested by its instantaneous return to the normal state when the magnetic induction is removed), so the *force* which the matter in this state possesses and its mode of action, must be to us a *new magnetic force* or *mode of action* of matter.

2228. For it is impossible, I think, to observe and see the action of magnetic forces, rising in intensity, upon a piece of heavy glass or a tube of water, without also perceiving that the latter acquire properties which are not only *new* to the substance, but are also in subjection to very definite and precise laws (2160. 2199.), and are equivalent in proportion to the magnetic forces producing them.

2229. Perhaps this state is a state of *electric tension tending*

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* A speculation, &c. Philosophical Magazine, 1844, vol. xxiv. p. 136.

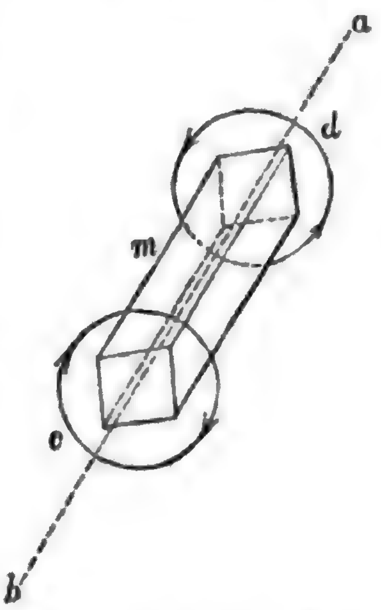
to a current; as in magnets, according to Ampère's theory, the state is a state of *current*. When a core of iron is put into a helix, every thing leads us to believe that currents of electricity are produced within it, which rotate or move in a plane perpendicular to the axis of the helix. If a diamagnetic be placed in the same position, it acquires power to make light rotate in the same plane. The state it has received is a state of tension, but it has not passed on into currents, though the acting force and every other circumstance and condition are the same as those which do produce currents in iron, nickel, cobalt, and such other matters as are fitted to receive them. Hence the idea that there exists in diamagnetics, under such circumstances, a tendency to currents, is consistent with all the phænomena as yet described, and is further strengthened by the fact, that, leaving the loadstone or the electric current, which by inductive action is rendering a piece of iron, nickel, or cobalt magnetic, perfectly unchanged, a mere change of temperature will take from these bodies their extra power, and make them pass into the common class of diamagnetics.

2230. The present is, I believe, the first time that the molecular condition of a body, required to produce the circular polarization of light, has been artificially given; and it is therefore very interesting to consider this known state and condition of the body, comparing it with the relatively unknown state of those which possess the power naturally: especially as some of the latter rotate to the right-hand and others to the left; and, as in the cases of quartz and oil of turpentine, the same body chemically speaking, being in the latter instance a liquid with particles free to move, presents different specimens, some rotating one way and some the other.

2231. At first one would be inclined to conclude that the natural state and the state conferred by magnetic and electric forces must be the same, since the effect is the same; but on further consideration it seems very difficult to come to such a conclusion. Oil of turpentine will rotate a ray of light, the power depending upon its particles and not upon the arrangement of the mass. Whichever way a ray of polarized light passes through this fluid, it is rotated in the same manner; and rays passing in every possible direction through it *simultaneously* are all rotated with equal force and according to one common law of direction; *i. e.* either all right-handed or else all to the left. Not so with the rotation superinduced on the *same* oil of turpentine by the magnetic or electric forces: it exists only in one direction, *i. e.* in a plane perpendicular to the magnetic line; and being limited to this plane, it can be changed in direction by a reversal of the direction of the inducing force. The direction of the rotation produced by the natural state is connected invariably with the direction of the ray of light; but the power to produce it appears to be possessed in every direction and at all times by the particles of the fluid: the direction of the rotation produced by the induced condition is connected invariably with the direction of the magnetic line or the electric current, and the condition is possessed by the particles of matter, but strictly limited by the line or the current, changing and disappearing with it.

2332. Let *m*, in fig. 3, represent a glass cell filled with oil of turpentine, possessing naturally the power of producing right-hand rotation, and *a b* a polarized ray of light. If the ray proceed from *a* to *b*, and the eye be placed at *b*, the rotation will be right-handed, or according to the direction expressed by the arrow-heads on the circle *c*; if the ray proceed from *b* to *a*, and the eye be placed at *a*, the rotation will still be right-handed *to the observer*, *i. e.* according to the direction indicated on the circle *d*. Let now an electric current pass round the oil of turpentine in the direction indicated on the circle *c*, or magnetic poles be placed so as to produce the same effect (2155); the particles will acquire a further rotative force (which no motion amongst themselves will disturb), and a ray coming from *a* to *b* will be seen by an eye placed at *b* to rotate to the right-hand more than before, or in the direction on the circle *c*; but pass a ray from *b* to *a*, and observe with the eye at *a*, and the phænomenon is no longer the same as before; for instead of the new rotation being according to the direction indicated on the circle *d*, it will be in the contrary direction, or to the observer's left-hand (2199). In fact the induced rotation will be added to the natural rotation as respects a ray passing from *a* to *b*, but it will be subtracted from the natural rotation as regards the ray passing from *b* to *a*. Hence the particles of this fluid which rotate by virtue of their natural force, and those which rotate by virtue of the induced force, cannot be in the same condition.

Fig. 3.



2233. As respects the power of the oil of turpentine to rotate a ray in whatever direction it is passing through the liquid, it may well be, that though all the particles possess the power of rotating the light, only those whose planes of rotation are more or less perpendicular to the ray affect it; and that it is the resultant or sum of forces in any one direction which is active in producing rotation. But even then a striking difference remains, because the resultant in the same plane is not absolute in direction, but relative to the course of the ray, being in the one case as the circle *c*, and in the other as the circle *d*, fig. 3; whereas the resultant of the magnetic or electric induction is absolute, and not changing with the course of the ray, being always either as expressed by *c* or else as indicated by *d*.

2234. All these differences, however, will doubtless disappear or come into harmony as these investigations are extended; and their very existence opens so many paths, by which we may pursue our inquiries, more and more deeply, into the powers and constitution of matter.

2235. Bodies having rotating power of themselves, do not seem by that to have a greater or a less tendency to assume a further degree of the same force under the influence of magnetic or electric power.

2236. Were it not for these and other differences, we might see an analogy between these bodies, which possess at all times the rotating power, as a specimen of quartz which rotates only in one plane, and those to which the power is given by the induction of other forces, as a prism of heavy glass in a helix, on the one hand; and, on the other, a natural magnet and a helix through which the current is passing. The natural condition of the magnet and quartz, and the constrained condition of the helix and heavy glass, form the link of the analogy in one direction; whilst the supposition of currents existing in the magnet and helix, and only a tendency or tension to currents existing in the quartz and heavy glass, supplies the link in the transverse direction.

2237. As to those bodies which seem as yet to give no indication of the power over light, and therefore none of the assumption of the new magnetic conditions; these may be divided into two classes, the one including air, gases and vapours, and the other rock crystal, Iceland spar, and certain other crystalline bodies. As regards the latter class, I shall give, in the next series of these researches, proofs drawn from phænomena of an entirely different kind, that they do acquire the new magnetic condition; and these being so disposed of for the moment, I am inclined to believe that even air and gases have the power to assume the peculiar state, and even to affect light, but in a degree so small that as yet it has not been made sensible. Still the gaseous state is such a remarkable condition of matter, that we ought not too hastily to assume that the substances which, in the solid and liquid state, possess properties even general in character, always carry these into their gaseous condition.

2238. Rock-salt, fluor-spar, and, I think, alum, affect the ray of light; the other crystals experimented with did not; these are equiaxed and singly refracting, the others are unequiaxed and doubly refracting. Perhaps these instances, with that of the rotation of quartz, may even now indicate a relation between magnetism, electricity, and the crystallizing forces of matter.

2239. All bodies are affected by helices as by magnets, and according to laws which show that the causes of the action are identical as well as the effects. This result supplies another fine proof in favour of the identity of helices and magnets, according to the views of Ampère.

2240. The theory of static induction which I formerly ventured to set forth (1161, &c.), and which depends upon the action of the contiguous particles of the dielectric intervening between the inductric and the inducteous bodies, led me to expect that the same kind of dependence upon the intervening particles would be found to exist in magnetic action; and I published certain experiments and considerations on this point seven years ago (1709—1736). I could not then discover any peculiar condition of the intervening substance or diamagnetic; but now that I have been able to make out such a state, which is not only a state of tension (2227), but dependent entirely upon the magnetic lines which pass through the substance, I am more than ever encouraged to believe that the view then advanced is correct.

2241. Although the magnetic and electric forces *appear* to exert no power on the ordinary or on the depolarized ray of light, we can hardly doubt but that they have some special influence, which probably will soon be made apparent by experiment. Neither can it be supposed otherwise than that the same kind of action should take place on the other forms of radiant agents as heat and chemical force.

2242. This mode of magnetic and electric action, and the phænomena presented by it, will, I hope, greatly assist hereafter in the investigation of the nature of transparent bodies, of light, of magnets, and their action one on another, or on magnetic substances. I am at this time engaged in investigating the new magnetic condition, and shall shortly send a further account of it to the Royal Society. What the possible effect of the force may be in the earth as a whole, or in magnets, or in relation to the sun, and what may be the best means of causing light to evolve electricity and magnetism, are thoughts continually pressing upon the mind; but it will be better to occupy both time and thought, aided by experiment, in the investigation and development of real truth, than to use them in the invention of suppositions which may or may not be founded on, or consistent with fact.

Royal Institution, Oct. 29, 1845.

L. *On the Cause of remarkably Mild Winters which occasionally occur in England. By* Lieut.-Colonel SABINE, *R.A., For. Sec. R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE unusual character of the winter which we have just experienced, together with its effects which we are now witnessing upon our gardens and fields, and its influence on the public health as evidenced by the bills of mortality, should make it an object not only of scientific, but of general interest, to endeavour to trace out the cause of so remarkable a phænomenon. By a memorandum with which the Astronomer Royal has been so obliging as to furnish me, it appears that the mean temperature in December, January and February, exceeded the mean temperature of the same months in the preceding year by the amounts respectively of 8°·7, 5°·3, 11°·2; on an average above 8° for three months. An excess of temperature of such amount and such continuance, must surely, one would suppose, have some sufficiently notable cause. I am not aware that any probable cause has yet been suggested; but should you oblige me by inserting this communication, it may at least be of use in commencing the discussion, and possibly in eliciting the opinions of others, whose views on the subject the public may naturally desire to know.

The winter which within my recollection most nearly resembled the present, was that of 1821–1822, and undoubtedly the resemblance is in many respects very striking. For the peculiarity in that year there was a cause assigned, adequate I believe to account for all the phænomena, and of which the existence was proved: I allude to the extension of the Gulf-stream in that year to the coast of Europe, instead of its terminating as it usually does about the meridian of the Azores. In the winter of 1821–1822, the warm water of the Gulf-stream spread itself beyond its usual bounds over a space of ocean which may be roughly estimated as exceeding 600 miles in latitude and 1000 in longitude, carrying with it water several degrees higher than the temperature of the sea in ordinary years in the same parallels. The facts, both in respect to the Gulf-stream, and to the peculiarities of the winter in that year, were stated in the volume of Pendulum and other observations which I published in 1825; perhaps the statement of them now will be most satisfactorily given in the words which were then used: and I have the less hesitation in introducing an extract from that work, because it was published many years ago, and is, I believe, but little known, at least in this country. The statement was as follows:—

"In the passage of the Iphigenia from England to the coast of Africa, a remarkable and very interesting evidence was obtained, by observations on the temperature of the sea, of the accidental presence in that year of the water of the Gulf-stream, in longitudes much to the eastward of its ordinary extension.

"The Iphigenia sailed from Plymouth on the 4th of January [1822], after an almost continuous succession of very heavy westerly and south-westerly gales, by which she had been repeatedly driven back and detained in the ports of the Channel; the following memorandum exhibits her position at noon on each day of her subsequent voyage from Plymouth to Madeira, and from thence to the Cape Verd Islands, the temperature of the air in the shade and to windward, and that of the surface of the sea; it also exhibits in comparison, the ordinary temperature of the ocean at that season, in the respective parallels, which Major Rennell has been so kind as to permit me to insert on his authority, as an approximation founded on his extensive inquiries; the last column shows the excess or defect in the temperature observed in the Iphigenia's passage.

Date.		Latitude N.	Longitude W.	Air.	Surface-water.		Excess or defect.
					Observed.	Usual.	
Plymouth to Madeira.	1822. Jan. 5	47° 30′	7° 30′	47°·0	49°·0	50°·0	−1
	6	44 20	9 30	52·5	55·7	52·5	+3·2
	7	41 22	11 37	54·0	58·2	54·0	+4·2
	8	38 54	13 20	54·2	61·7	55·7	+6
	9	No observation.		56·0	63·0	58·0	+5
	10	33 40	15 30	60·7	64·0	60·0	+4
Madeira to Cape Verd Islands.	19	26 00	17 50	66·0	65·5	67·0	−1·5
	20	24 30	18 50	68·0	67·0	68·4	−1·4
	21	23 06	20 00	69·0	69·0	69·5	−0·5
	22	21 02	21 27	69·5	69·5	71·2	−1·7
	23	19 20	23 00	70·6	70·2	71·6	−1·4

"It is seen by the preceding memorandum, that in the passage from Plymouth to Madeira, the Iphigenia found the temperature of the sea, between the parallels of $44\frac{1}{3}^\circ$ and $33\frac{2}{3}^\circ$, several degrees warmer than its usual temperature in the same season; namely $3^\circ{\cdot}2$ in $44\frac{1}{3}^\circ$, increasing to 6° in 39°, and again diminishing to 4° in $33\frac{2}{3}^\circ$; whilst at the same period, the general temperature of the ocean in the adjoining parallels, both to the northward and to the southward, even as far as the Cape Verd Islands in $19\frac{2}{3}^\circ$, was colder by a degree and upwards than the usual average. The evidence of many careful observers at different seasons and in different years, whose observations have been collected and compared by Major Rennell, has satisfactorily shown, that the water of the Gulf-stream, distinguished by the high temperature which it brings from its origin in the Gulf of Mexico, is not usually found to extend to the eastward of the Azores. Vessels navigating the ocean between the Azores and the continent of Europe, find at all seasons a temperature progressively increasing as they approach the sun; the absolute amount varies according to the season, the maximum in summer being about 14° warmer than the maximum in winter; but the progression in respect to latitude is regular, and is nearly the same in winter as in summer, being an increase of 3° of Fahr. for every 5° of latitude. It is further observed, that the ordinary condition of the temperature, in the part of the ocean under notice, is little subject to disturbance, and that in any particular parallel and season, the limits of variation in different years are very small; after westerly winds of much strength or continuance, the sea in all the parallels is rather colder than the average temperature, on account of the increased velocity communicated to the general set of the waters of the north-eastern Atlantic towards the south. To the heavy westerly gales which had prevailed almost without intermission in the last fortnight in November, and during the whole of December, may therefore be attributed the colder temperatures observed in the latitude of $47\frac{1}{2}^\circ$, and in those between 26° and $19\frac{1}{3}^\circ$.

"If doubt could exist in regard to the higher temperatures between $44\frac{1}{3}^\circ$ and $33\frac{2}{3}^\circ$ being a consequence of the extension in that year of the Gulf-stream in the direction of its general course, it might be removed by a circumstance well-deserving of notice, namely, that the greatest excess above the natural temperature of the ocean was found in or about the latitude of 39°, being the parallel where the middle of the stream, indicated by the warmest water, would arrive, by continuing to flow to the eastward of the Azores, in the prolongation of the great circle in which it is known to reach the mid-Atlantic.

"One previous and similar instance is on record, in which the water of the Gulf-stream was traced by its temperature quite across the Atlantic to the coast of Europe; this was by Dr. Franklin, in a passage from the United States to France, in November 1776*. The latter part of his voyage, *i. e.* from the meridian of 35° to the Bay of Biscay, was performed, with little deviation, in the latitude of 45°; in this run exceeding 1200 miles, in a parallel of which the usual temperature, towards the close of November, is about 55½°, he found 63° in the longitude of 35° W., diminishing to 60° in the Bay of Biscay; and 61° in 10° west longitude, near the same spot where the Iphigenia found 55°·7 on the 6th of January, being about five weeks later in the season. At this spot then, where the Iphigenia crossed Dr. Franklin's track, the temperature in November 1776 was 5½°, and in January 1822, 3°·2 above the ordinary temperature of the season.

"There can be little hesitation in attributing the unusual extension of the stream in particular years to its greater initial velocity, occasioned by a more than ordinary difference in the levels of the Gulf of Mexico and of the Atlantic; it has been computed by Major Rennell, from the known velocity of the stream at various points of its course, that in the summer months, when its rapidity is greatest, the water requires about eleven weeks to run from the outlet of the Gulf of Mexico to the Azores, being about 3000 geographical miles; and he has further supposed, in the case of the water of which the temperature was examined by Dr. Franklin, that perhaps not less than three months were occupied, in addition, by its passage to the coasts of Europe, being altogether a course exceeding 4000 geographical miles. On this supposition, the water of the latter end of November 1776 may have quitted the Gulf of Mexico, with a temperature of 83°, in June; and that of January 1822, towards the end of July, with nearly the same temperature. The summer months, particularly July and August, are those of the greatest initial velocity of the stream, because it is the period when the level of the Caribbean Sea and Gulf of Mexico is most deranged.

"It is not difficult to imagine that the space between the Azores and the coasts of the old continent, being traversed by the stream, slowly as it must be, at a much colder season in the instance observed by the Iphigenia than in that by Dr. Franklin, its temperature may have been cooled thereby to a nearer approximation to the natural temperature of the ocean in the former than in the latter case; and that the difference

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* Franklin's Works, 8vo, London, 1806, vol. ii. pp. 200, 201.

between the excess of 5°·5 in November, and of 3°·2 in January, may be thus accounted for.

"If the explanation of the apparently very unusual facts observed by Dr. Franklin in 1776, and by the Iphigenia in 1822, be correct, how highly curious is the connexion thus traced between a more than ordinary strength of the winds within the tropics in the summer, occasioning the derangement of the level of the Mexican and Caribbean seas, and the high temperature of the sea between the British Channel and Madeira, in the following winter!

"Nor is the probable meteorological influence undeserving of attention, of so considerable an increase in the temperature of the surface water over an extent of ocean exceeding 600 miles in latitude and 1000 in longitude, situated so importantly in relation to the western parts of Europe. It is at least a remarkable coincidence, that in November and December 1821, and in January 1822, the state of the weather was so unusual in the southern parts of Great Britain and in France, as to have excited general observation; in the meteorological Journals of the period it is characterized as 'most extraordinarily hot, damp, stormy, and oppressive;' it is stated, 'that an unusual quantity of rain fell both in November and December, but particularly in the latter;' that 'the gales from the west and south-west were almost without intermission,' and that in December, the mercury in the barometer was lower than it had been known for thirty-five years before*."

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* "The following description of this very remarkable winter is extracted from Mr. Daniell's Essay on the Climate of London (Meteorological Essays. London, 1823, pages 297 and 298), and becomes highly curious when viewed in connexion with the unusual temperature of the ocean in the direction in which the principal winds proceeded.

"'November 1821 differed from the mean, and from both the preceding years, in a very extraordinary way. The average temperature was 5° above the usual amount; and although its dryness was in excess" (the relative dryness in consequence of the increased temperature) "the quantity of rain exceeded the mean quantity by one-half. The barometer on the whole was not below the mean. All the low lands were flooded, and the sowing of wheat very much interrupted by the wet.

"'In December the quantity of rain was very nearly double its usual amount. The barometer averaged considerably below the mean, and descended lower than had been known for thirty-five years. Its range was from 30·27 inches to 28·12 inches. The temperature was still high for the season, and the weather continued, as in the last month, in an uninterrupted course of wind and rain; the former often approaching to a hurricane, and the latter inundating all the low grounds. The water-sodden state of the soil, in many parts, prevented wheat-sowing, or fallowing the land at the regular season. The mild temperature pushed forward all the early-sown wheats to a height and luxuriance scarcely ever before witnessed. The grass and every green production increased in an equal proportion.

"'January, 1822. This most extraordinary season still continued above

It is impossible to read this description of the winter of 1821-1822 without being struck with the many features which it has in common with that of the present year. The excess of heat in both amounted to several degrees, and continued through several months. There were similar floods in many parts of England in the early part and middle of this winter; and these were not confined to England only, but extended, as in 1821–1822, to many of the rivers of western Europe. The tension of the vapour conveyed to the shores of the British Channel in December, January and February last, was nearly ⅓rd part greater, as appears by the Greenwich Observations, than in the same months of the preceding year; although in consequence of the much higher temperature, the humidity of the air, or the ratio of the humidity to saturation, has been less. This was also the case in 1821–1822. We have had an unusual prevalence of westerly and south-westerly winds at the season when they are ordinarily replaced in a much greater proportion by the dry and cold winds which come to us from the interior of the great continents of Europe and Asia. If in the southern parts of Britain, and on the shores of the British Channel, we have been less severely affected by storms and extreme barometrical depressions than was the case in 1821–1822, we may possibly owe the comparative exemption to the fact that the excess of heat above the mean has been greater in the winter of 1845–1846 than it was in 1821–1822; whence we may infer perhaps that the conflict of the opposing currents of the atmosphere has been removed in the present year further to the north and north-east than on the former occasion; it is at the limits which are reached by the warm and humid current proceeding from the south-west, and in the localities where it encounters the dry and cold stream pressing from the east and north-east, that the greater atmospherical derangements are produced, and these have been experienced in the northern parts of Britain.

The similarity of the two winters having thus been shown, and specially their agreement in those features in which they differ from ordinary winters, it will naturally be asked, what evidence we have to prove or disprove an extension of the Gulf-

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the mean temperature, but the rain, as if exhausted in the preceding month, fell much below the usual quantity in this. There was not one day on which the frost lasted during the twenty-four hours.

"'Serious apprehensions were entertained, lest the wheats, drawn up as they had been by the warm and moist weather, without the slightest check from frost, should be exhausted by excessive vegetation, and ultimately be more productive in straw than corn.

"'The month of February, still 5° above the mean temperature, ended a winter which never has been paralleled.'"

stream in the present year, similar to that which took place in 1821. To this it must be replied, that strange as it may appear, this remarkable phænomenon may take place in any year without our having other knowledge of it than by its effects, although it occurs at so short a distance from our ports, from whence so many hundred vessels are continually crossing and recrossing the part of the ocean where a few simple observations with a thermometer would serve to make it known. We have no organized means of learning an occurrence which, whether it be or be not the cause of the present extremely mild winter, cannot fail whenever it does occur to affect materially and for a considerable length of time the climate of an extensive district of the globe including our own islands. History has recorded two instances in which the extension of the Gulf-stream is known to have taken place; and in both we owe our knowledge of it to the casual observations of an accidental voyager. Some one there may be in the present winter whose curiosity may have induced him, in the well-frequented passage between England and Madeira, to dip a thermometer in the sea once or twice a day, and who may therefore, perhaps unconsciously, be in possession of the very facts which it is desirable to know; in such case this communication, should it meet his eye, may be the means of inducing their publication. It is desirable however that we should not be thus altogether dependent on accident for information which may have even greater practical than scientific value; happily it is well known that suggestions of this nature, when really deserving attention, do not pass unheeded by our excellent Hydrographer, to whose department such subjects seem naturally to belong*.

But not only might we by such means be occasionally informed in November or December that we had probably to expect a continuance of very mild weather through January and February; it is not unreasonable to suppose that such winters might well be anticipated at a still earlier period of the year; ships sail faster than the Gulf-stream flows, and a more than usual difference existing in the levels of the Gulf of Mexico and the Atlantic, or a more than usual initial velocity of the stream itself, with the consequent probability of a winter of unusual mildness in Europe, might be known in England in the summer or in the early autumn; or even going

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* It is much to be wished that a society existed in England which should charge itself with the many interesting and important considerations belonging to *physical* geography. Did the object and scope of the Royal Geographical Society embrace physical as well as descriptive geography, it cannot be doubted that science and the public would be greatly benefited.

back to a yet earlier stage of the phænomenon, we might be apprised that the causes which operate in producing the derangement of the level of the Caribbean and Mexican seas were prevailing in any particular year in an unusually high degree.

I wish in conclusion to guard against the possibility of being understood to suppose that amidst the variety of incidents by which our climate is affected, there may not be others which may be influential in the production of winters of unusual mildness in an equal, or even in a greater degree than the extension of the Gulf-stream; or, that whenever the stream reaches the coasts of Europe, its influence on our climate must necessarily occasion winters like that of 1821–1822, or 1845–1846. It is reasonable to believe that there may be degrees of initial velocity between that which is usual and that which is extreme. There may also be counteracting or qualifying causes with which we are as yet wholly unacquainted. The object of this communication is rather to recall to recollection, on the occasion of the present remarkable winter, the coincidence that was discovered between the similar winter in 1821-1822, and the extension of the stream; and to promote the adoption of such simple means as may supply additional evidence, whereby we may discern between coincidence observed on a single occasion, and connexion which may be established by the observation of repeated coincidence.

I am, Gentlemen,
Your obedient Servant,
Woolwich, March 17th, 1846. EDWARD SABINE.

LI. *Observations on the Recent Researches of* Prof. Faraday.

By M. POUILLET[*].

FOR some months past there has been much talk about a new series of researches by M. Faraday, the result of which was one of the most important discoveries, the action of magnetism on light. Two authentic documents have at length reached us on this subject; one is published in the January number of the Philosophical Magazine, being an abstract of the sitting of the Royal Society of the 27th of November, the other was communicated at the last meeting of the French Academy by M. Dumas in the name of Mr. Faraday himself. Various results are announced in these two publications, but only one fact is presented with some development,

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[*] Translated from the *Comptes Rendus*, for January 26, 1846.—The researches of Prof. Faraday, referred to in this paper, are given entire in our present Number.

it is that which relates to the action of an electro-magnet on a ray of polarized light in turning its plane of polarization either to the right or left, according to the relative directions of the luminous ray and of the resultant of the magnetic actions.

This fact is justly considered by Faraday as a fundamental one, for hitherto there is nothing analogous in science; and it constitutes of itself a discovery of the very highest importance. Undoubtedly many persons have hastened to repeat and investigate it in order to ascertain its perfect accuracy, and to find out the most marked characters and the most essential conditions. Immediately after having read the Philosophical Magazine I set to work, as I stated at the last meeting of the Academy, but my first trials having been without result, and other persons not having been more fortunate than myself in the attempt, it appeared to me necessary to resume them with greater attention, varying the mode of experiment and making up in the best manner I was able for the want of precision in the directions which had come to my knowledge.

I hasten to lay before the Academy today the result of these researches, and with a twofold motive; in the first place, to render homage to the author of the discovery, and in the next place, to furnish those physicists who may desire to follow this new path of science with some indications that may be of service to them, if, as I believe, they add anything to what has hitherto been published on the subject.

The apparatus employed by me is composed,—1st, of a Bunsen's battery; 2nd, of one or more electro-magnets; 3rd, of M. Soleil's instrument for exhibiting the least angular displacements of the planes of polarization; 4th, of the various substances to be submitted to examination. The elements of the Bunsen's battery are of the ordinary size; in the majority of cases ten suffice to render the phænomenon perceptible; but to measure it and to compare the intensities with a certain approximation, forty, fifty, and even 100 elements must be employed. The electro-magnets are capable of supporting 1600 pounds when excited by a battery of twenty pairs. They are soft iron cylinders, seven to eight centimetres in diameter and about fifty centimetres in length, which are curved in a horse-shoe form, the distance of the axes of the two arms or poles being not more than fifteen to twelve centimetres. From 500 to 600 metres of copper wire, coated twice with silk, are wound round each arm. The instrument of M. Soleil is composed of two parts, one objective, the other ocular. The objective part, or that which is turned towards the light, is nothing more than a Nicol's prism, behind which is a system of two juxtaposed plates of quartz cemented by one margin, and worked together in order to fulfill a double condition of giving them exactly the same thickness and rendering each perfectly perpendicular to the axis. The surface of junction of these plates being parallel to the pencil of light and occupying the centre of its breadth, it is evident that the first half of the pencil traverses one of the plates only, and the second half the other plate; and as they were selected of opposite rotatory power, the first half of the polarized pencil is found to have its planes of polarization deviated, for instance towards the right, by a certain angle, and the second half, on the contrary, has its planes of polarization deviated towards the left by a perfectly equal angular magnitude. The magnitude of these deviations depends on the common thickness of the two plates, which is usually from five to six millimetres.

The ocular portion, or that directed towards the eye, consists, in the first place, of a thick plate of rock-crystal, likewise perpendicular to the axis, having for instance a rotatory power to the right, and a thickness of five millimetres very accurately determined by the spherometer. Behind this plate is the compensator, composed of two equal prismatic plates, provided with a similar rotatory power towards the left, *i. e.* in a contrary direction to the first. These two prisms, opposed like two wedges by their acute angle, are moved simultaneously by the same spring; they slide one upon the other, to be arranged sometimes by their less sometimes by their greater thickness, and thus always form a system equivalent to a parallel plate, but one which would vary from the thickness 0 to nearly double that of the base of each prism. To avoid the deviations which the light might experience from the variable distance of these prisms and the obliquity of the surfaces, each one is compensated by a glass prism.

Lastly, behind the compensator is a doubly refracting achromatic prism and a small Gallilean telescope, to which the eye is applied to observe the pencil of light which has passed both the objective portion, the intermediate bodies submitted to examination, and the ocular portion of the instrument.

The graduation of the compensator is easily made; and when this has once been done with sufficient care, the instrument indicates that the cause, whatever it be, which produces the deviation in the plane of polarization has an intensity equivalent to that of a plate of quartz of a known thickness; always, be it understood, on the condition that this cause exercises on the various simple lights actions comparable to that which the quartz exercises.

The instrument of M. Soleil, the construction of which I have just described, must be separated into two parts for the experiments under consideration. The objective and the ocular parts were mounted separately on my frame of diffraction*, which is most readily adapted for all the researches in which it is required to centre the apparatus on the same axis. A common lamp is placed before the objective part, and a strong magnifier gives a pencil of light closely parallel, which being propagated in the direction of the common axis, traverses successively the object-glass, the pieces subjected to the test, and the ocular; the distance between the objective and the ocular may vary between tolerably distant limits, for it may extend to nearly two metres, or only to a few centimetres, according to the nature of the observations.

It is important to remark that the pencil of light is always horizontal, and the apparatus was accidentally arranged so that the light was propagated from south to north, which may assist us to define more easily the relative positions of the polarized ray, of the electro-magnets, and of the bodies on which they act.

The electro-magnet is horizontal, that is to say, the plane of the axes of its two branches is horizontal, and precisely at the height of the pencil of light which traverses the apparatus; moreover, the vertical plane, formed by the extremities of the two branches or by the poles of the electro-magnet, is parallel to this pencil, and may approach it more or less. This being settled, if it be desired to submit to experiment, for example, a parallelopipedon of flint-glass of ten or twelve centimetres in length and terminated perpendicularly to its length by two parallel planes, this parallelopipedon is first arranged so that the ray polarized by the objective traverses it in the direction of its axis, and if the flint-glass is pure and well-annealed, as it must be for the success of the experiment, its interposition produces neither deviation nor coloration in the ray of light. The electro-magnet is then approached, arranging it in the same manner as if the piece of flint-glass were a piece of iron to close it, and there is even no inconvenience in arranging it so that the two poles of the electro-magnet are in contact with the flint-glass; the middle of the length of the latter corresponds consequently to the interval which exists between the two arms of the electro-magnet.

When these arrangements have been made, a current is passed, and suddenly it is seen that the two tints of the red image, which correspond to the two opposed plates of the quartz of the object-glass, cease to be identical. Let us sup-

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* See my *Elémens de Physique*, 4th edition, vol. ii. pl. 26.

pose, for example, that the one on the right has turned blue; if the current is passed in a contrary direction, it is that on the left which this time turns blue in the same manner. Thus, by reversing the poles of the electro-magnet, the action which it exercises on the flint-glass, or on the light which traverses it, is also suddenly reversed. Here then we see the action in question rendered evident in the most striking and incontestable manner.

In the circumstances of which I have just spoken, ten elements are more than sufficient to exhibit it to a practised eye; but with a hundred elements, it assumes such an intensity that persons the most unaccustomed to these kind of observations could not fail to perceive it as a perfectly characterized phænomenon.

Before seeking to ascertain whether this effect, at once so novel and so extraordinary, results from a direct action of the magnetic fluid upon light, or from an indirect action, in which the ponderable matter of the flint-glass intervenes, or at least the collective forces to which this matter is subjected in order to exist in molecular equilibrium, it is necessary first to determine precisely what is the nature of the effect produced, and to seek above all to measure its intensity, in order to ascertain what are the conditions under which the phænomenon is shown with the greatest energy.

For this purpose, instead of observing directly the coloured tints which the quartz gives by the lamp perpendicular to the axis, it is necessary to recompose what M. Biot has called *the tint of passage.* This is done by placing before the objective several systems of blue and greenish glasses; but I found in the cabinet of the Conservatoire some glasses very slightly coloured blue, which give to this tint a sensitiveness still greater than that obtainable by other means. When these glasses are interposed in the pencil, the tints of the quartz become of a light lilac, on which the least changes of shade are appreciable; the uncertainties which are presented by the zero of the compensator disappear, and it becomes possible not only to perceive, but to measure the effects which correspond to thicknesses of quartz of a hundredth of a millimetre.

The instrument thus modified, the compensator being at zero, and the polarizing prisms of the object-glass and of the ocular being suitably regulated in their relative positions, the experiment may be proceeded with; only there is one thing which requires mention, not to pay attention to the yellow image, but to look exclusively at the lilac image, the two halves of which are then exactly of the same shade.

As soon as the current passes, one of the halves of this image, for instance that on the right, is seen to turn blue; we observe that this tint is persistent as the current itself, and we may be convinced that, from the first instant, it acquired its whole value, that is to say, that the prolonged duration of the action adds nothing perceptible to it. The compensator is then moved in the proper direction; the difference of the tints gradually disappears in proportion as it advances, and with a little practice the point at which the equality is re-established is soon found. The number of divisions is noted down, and we thus obtain a measure, or at least an approximate measure of the effect produced,—say twenty divisions.

When subsequently the current is passed in a contrary direction, it is the other half of the tint, that to the left, which turns blue, and it is in the other direction that the compensator has to be moved to re-establish the equality. No interval of time is appreciable between the change of the current and the change of effect upon the light, and it is again instantaneously that the tint takes all its value. When the optical apparatus is well-adjusted, and the electrical communications are equally good in both directions, the ground gone over by the compensator is the same in the two cases, that is to say, that if it progressed in the first twenty divisions to the right, it should in the second proceed twenty degrees to the left.

These opposite effects and the corresponding measures may be repeated indefinitely, either with the same or a different number of pairs of the battery; and a few hours are sufficient, during which the action of the battery is nearly constant, to pass in review a great number of diaphanous substances, and to obtain a first approximation on the relative sensitiveness with which they obey the magnetic influence.

When the substances submitted to the test are more or less coloured, it is necessary to vary the systems of glasses intended to produce the tint of passage, and we do not always succeed in composing a tint equally delicate and easy of observation. It might happen consequently that some substances, even slightly coloured, when submitted to these modes of observation, would appear much less energetic than they are in reality.

Let us pause then at the diaphanous substances, and observe that in the experiment with the flint-glass cited above, it was necessary to advance the compensator twenty divisions to the right and twenty divisions to the left, according as the current passed in one direction or the other. Let us bear in mind, that if, instead of interposing on the passage of the pencil a prism of flint-glass submitted to the electro-magnet, there had been interposed, without magnetic action, a lamina of quartz perpendicular to the axis, of a proper thickness, turning to the right in the first case and to the left in the second, it is certain that the equality of the tints would have been re-established by the same movements of the compensator. Now, it is known that the effect produced by such a lamina of quartz would have been to turn the plane of polarization from right to left, whence it seems very natural and legitimate to conclude, that the flint-glass subjected to the magnetic action has produced the same effect as this lamina of quartz, that is to say, that it has also turned the plane of polarization to the right for one direction of current, and to the left for the contrary direction. This is, in fact, the conclusion to which Mr. Faraday has come, and he has characterized this new action of magnetism upon light, by stating that the magnetism turns the plane of polarization of the luminous ray submitted to its influence under certain conditions, and that the direction of this rotation is connected with that of the current.

Quartz, and the other substances which, of themselves, by their nature or structure have, without the intervention of magnetism, the permanent property of turning the planes of polarization, exert this action with variable intensities on the different elements constituting white light; and there are dispersive powers for this rotation, as there are different dispersive powers for refraction. It would be very important to make in this respect some researches upon the substances which acquire this property by the magnetic action, analogous to those very remarkable ones which M. Biot made upon the former. The apparatus which I have used must be very much modified to be adapted to this class of experiments; it serves to show the phænomena very distinctly, rather than to measure them in their more delicate details. Such an investigation, however, cannot be undertaken with phænomena so little developed as those which I have obtained; for in such limits they might perhaps be as well explained by partial depolarizations towards the right and left as by the rotation itself of the plane of polarization, which, moreover, would not detract anything, and would perhaps add to their importance.

As I just stated, the plane of polarization, in that specimen of the flint-glass which gave the most energetic effects, was diverted by the magnetic action as much as it would have been by the action of a plate of quartz two-tenths of a millimetre in thickness; now, since by changing the direction of the current, the rotation takes place in an opposite direction, it is seen that the total effect obtained by passing from the magnetic action which is exerted in one direction to that which is exerted in the other, is equal to that which would be produced by a plate of quartz four-tenths of a millimetre in thickness. Such, up to the present time, is the maximum effect which I have been able to obtain. As we have now a means of comparing the intensities of this force, it will be very easy to see how it will be modified by the different relative positions of the electro-magnet and of the piece of flint-glass.

The following are the observations I have made with respect to this point:—

1. If, instead of placing the electro-magnet in contact with the piece of flint-glass, it is removed parallel to itself in the same horizontal plane, and so that the vertical plane separating the two arms corresponds always to the middle of the flint-glass, the action diminishes, but feebly in proportion as the distance increases, so that at the distance of ten centimetres, it is still a considerable proportion of what it was when it was actually in contact.

2. If the electro-magnet is again placed in contact, and the piece of flint-glass slid in the direction of the ray of light to subject it to the action of one only of the poles of the magnet, a moment arrives when the action is wholly null; then, if it is still slid in the same direction, removing it more and more from its primitive position, until it is placed beyond the pole to which it is submitted, the action begins anew; but then it is contrary to what it was at first.

These observations appear to lead to three important consequences:—

It first results, that if we consider the unknown action of the magnet on the flint-glass as being produced by attractions and repulsions, the effect is null when the resultant of these attractive and repulsive forces is perpendicular to the direction of the polarized ray; and it is at its maximum, on the contrary, when this resultant is parallel to the ray. We may thus, from these considerations, form a just idea of the direction in which it acts, for in considering, *always hypothetically*, the piece of flint-glass as a piece of soft iron, acquiring two poles from the influence of the magnet, the movement of the plane of polarization occurs to the right when the light enters by the south pole, and proceeds from the south to the north pole, and it occurs to the left when the light enters by the north pole; consequently, whatever be the position of the piece of flint-glass, if two observations are made on it without touching it, and without disarranging the electrical apparatus, but merely turning the optical apparatus to cause the light to enter successively in the two directions, we shall see, in the first case, the effect to the right, and in the second the effect to the left, which establishes, as Mr. Faraday has pointed out, a difference, at least apparent, between the substances which have the permanent property of turning the planes of polarization and those which acquire it by magnetic action.

In the second place it results, that on experimenting in this way care must be taken to give to the pieces subjected to the electro-magnet a length greater than the distance of the axes of the two arms; for the portions which would exceed those axes would receive similar modifications among one another, and opposed to that which the central portion would receive; it may even be presumed that the compensation might obtain exactly, so that with a connecting piece exceeding the breadth of the magnet, the action might be perfectly null. This result seems to me opposed to that which is pointed out by Mr. Faraday, namely, that the effect is proportional to the length of the piece subjected to the experiments.

It results, lastly, that in order to obtain a greater effect, two electro-magnets, opposed to one another, may be presented to the piece of flint-glass, so that the poles of the same name face one another. This I have verified, and it is even by the assistance of two electro-magnets thus opposed that I have obtained the maximum effect of which I have spoken above. By placing thus several similar systems in succession on the same pencil, the effect would be, without doubt, tripled, &c.

It has appeared to me very important to examine whether the position of the plane of polarization, with relation to the horizontal plane of the electro-magnet, had any influence on the energy of the action; but whether the plane of polarization be itself horizontal, vertical or intermediate, the results appeared to me to remain perceptibly the same.

I have hitherto spoken only of flint-glass, but I have subjected to experiment all the other solid transparent bodies which I have been able to procure; for instance, various kinds of flint-glass, and doubtless of different composition, crown-glass, and glass of all kinds, coloured with copper, gold, chromium, &c., and also rock-salt. All these bodies present, although with less intensity, the same phænomena as the flint-glass; unfortunately the samples of crown-glass are sometimes so annealed as to modify the colours, and which does not allow of their being compared with other bodies; nevertheless, after the attempts which I have been able to make on some less imperfect specimens, I am led to think that the action of the crown-glass has an intensity comprised between the half and the two-thirds of that of the flint-glass.

The chloride of sodium has an action very analogous to that of the flint-glass.

I have also subjected to experiment some transparent or coloured liquids; these experiments were made in a trough formed of parallel glasses, having a length of thirteen centimetres, equal to the distance of the axes of the electro-magnets, a breadth of three centimetres, and a depth of five centimetres. The trough being empty, and the electro-magnets being in action, no sensible effect was produced by the parallel glasses which formed the extremities.

The intensity of all these liquids is very nearly equal to that of the crown-glass; the most energetic however appeared to me to be olive oil, distilled water, concentrated ammonia, and pure nitric acid; and the less energetic, acetic acid, sulphuric acid, ferrocyanide of potassium, and ferrocyanate of magnesia. It appeared to me certain that several bodies dissolved in distilled water weakened its effects.

Mr. Faraday states that manganese, chromium, and cerium are magnetic after the manner of iron, and that all the compounds of these bodies preserve this property more or less. I had long ago proved the first fact for manganese, and in the course of last summer I proved it for very pure chromium obtained by the battery, both from chromic acid and from sulphate of chromium. With regard to the magnetic compounds, I have studied them recently by a very simple and very easy process, which consists in arranging a powerful electro-magnet, with its poles at top, forming a horizontal plane; a thin paper is stretched over each pole, in contact with the iron itself, and it is then only requisite to throw upon this paper some very fine particles of the substance to be examined, and to give the paper some slight vibrations, which put them in motion. The particles arrange and fix themselves on the circle which corresponds to the terminal bar of the iron of the electro-magnet, and describe the circle with great precision. By this means I have ascertained that almost all the compounds of magnetic metals are, in fact, more or less magnetic; prussian blue and the sesquichloride of chromium (of M. Peligot) are so in a remarkable manner. There are some compounds however which are exceptions to this rule; such, for example, are the double cyanide of iron and of potassium, the chromate of silver, and the bichromate of potass.

Other metals, as platina sponge and arsenic, exhibit a perceptible action; but it would require to be verified on perfectly pure specimens.

Bismuth presents other phænomena; instead of forming a circle, like the magnetic metals, it forms two concentric circles, leaving thus a narrow white band, in the very place where the other metals form a circle, as if it were repelled by the more lively action of the iron armature of the magnet. The effect is so marked, that on mixing, for example, some sesquichloride of chromium very finely pulverized with some bismuth, likewise in very fine powder, the violet circle of the chloride is seen, and the two circles of the bismuth which are separate from them, although very near. Amber seems to give the same appearances as the bismuth, though in a much weaker degree.

No attractive or repulsive effect is observed by this means, either on very pure antimony or on the other metals, binary or other compounds (among the rare metals, I have only experimented on tellurium and the uranium of M. Peligot), nor on the alkalies, sulphur, iodine, charcoal, and diamond. I regret that I had not at my disposal either cerium or any of its compounds.

These negative results cannot invalidate in the least the general proposition of M. Faraday, who has doubtless operated with more delicate means or with more energetic magnets. I merely mention them here to point out the easy process which I have employed, and the limit of its sensibility.

There is another process for investigating the magnetic properties,—that which was employed by Coulomb when he discovered that all bodies are subject to the influence of magnets, and which has been since employed in the same view by many experimentalists, and very recently by M. Ed. Becquerel (*Comptes Rendus*, vol. xx. p. 1708). Mr. Faraday appears to have employed it; but doubtless, from the weakness of my electro-magnets, although excited by a battery of 100 pairs, I have not obtained the same results as he; in my experiments, bismuth and amber are the only two substances which took a direction perpendicular to the line of the poles, and without doubt the relation existing between this direction of the bismuth and the effect of repulsion which the fine powder of that body experiences from the part of the armature of the magnet will appear highly remarkable. These two mechanical actions of magnetism upon bodies—the attraction and repulsion of fine powders, placed almost in contact with one of the poles, and the direction given to more considerable masses, oscillating in the presence of the two poles—appear therefore to be dependent one upon the other; but in what degree are they connected with the third action, the optical action which Mr. Faraday has just discovered?

Admitting with this philosopher that all the substances which are not magnetic after the manner of iron, are *diamagnetic* or magnetic after the manner of bismuth, we should be led to conclude immediately that the optical action being concomitant with a certain mechanical action, it is at least presumable that this action is exerted upon the bodies, and not directly and immediately on the light which passes through them.

But if it happens, as in my experiments, either from the relative weakness of my magnets or from the imperfection of the methods which I have employed, or from other causes—if it happens that the various kinds of glasses, distilled water, the fatty bodies, &c., which are so sensitive to the optical action, are nevertheless insensible to the mechanical action of the magnetism, it would not be a reason to conclude that magnetism acts directly upon the light itself; a conclusion which, moreover, would only have a precise meaning in the system of emission, for in the undulatory theory, which seems at present so completely demonstrated, it is the æther of the body submitted to the experiment which would be modified by the magnetism, and it would doubtless be very difficult to recognise whether it is modified without any participation of the ponderable matter of the body with which it is so intimately connected.

LII. *On the Aberration of Light.* *By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge*[*].

I WISH to say a few words more on the subject of aberration, to prevent misapprehension. It is evident from Prof. Challis's last communication, that we differ merely as to the phænomenon which we understand by the term "aberration of light." When the position of a star has been corrected for refraction, precession, and nutation, and proper motion if it has any, let s be its mean annual place referred to the celestial sphere, s_1 the point to which the star is referred by astronomical measurement, and s_2 the point in which the sphere is cut by the line along which the light comes from the star, produced backwards, s_2 being corrected in the same manner as s_1. It is shown by observation that s_1 is displaced from s towards the point towards which the earth is moving, through an angle equal to the ratio of the velocity of the earth to that of light multiplied by the sine of the earth's way. This is the phænomenon which I understand by the *aberration of light*, and which it was the object of one of my former communi-

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[*] Communicated by the Author.

cations to account for on the theory of undulations. But it is evident that what Prof. Challis means by aberration, is the circumstance that s_1 is displaced from s_2 through the angle which I have mentioned. Prof. Challis's reasoning, by his own confession, does not explain aberration in the sense in which I used the word; for he says that it follows from *observation* (not theory alone), that s_2 coincides with s.

LIII. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF DIASPORE FROM SIBERIA.

BY M. A. DAMOUR.

THE remarkable characters of diaspore have frequently attracted the attention of mineralogists, and have been extremely well described and analysed by MM. Children, Dufrenoy, and Hess. The author observes, that he should therefore have abstained from referring to them, if he had not had occasion lately to observe a singular property of this mineral which had not been previously noticed. The diaspore is a well-known hydrate of alumina. It is shown by the experiments of M. Dufrenoy, that this mineral, even when long boiled in sulphuric acid, not only resists its action, but retains all its water. M. Damour, on repeating this experiment, obtained the same result; but he afterwards found that the diaspore, when deprived of its water by calcination, was almost totally soluble in sulphuric acid when assisted by heat.

This property is the inverse of that which chemists always observe with respect to hydrates, and in general with respect to substances which have not been calcined. In fact, the greater number of these substances lose their solubility in acids after they have been heated to redness. In this case the contrary occurs: the peculiar molecular condition of the crystallized hydrate of alumina, constituting the diaspore, appears then to be the only obstacle to the natural affinity of this hydrate for the sulphuric acid; for calcination, by destroying this arrangement of the molecules, restores the usual properties of alumina.

M. Damour took advantage of this circumstance in order to simplify the method of analysing diaspore.

The mineral was first purified by digesting it, reduced to very fine powder in dilute hydrochloric acid at a moderate heat. There was dissolved a notable quantity of oxide of iron accidentally mixed with it. The powder, after washing, was perfectly white. The proportion of water was found to be nearly similar in three different operations: to determine this the dried powder of the mineral was suffered to remain under a receiver over a stratum of pumice moistened with sulphuric acid, this powder was weighed and placed in a small covered platina crucible; in order to prevent the projection of the powdered mineral, the crucible was placed in another of the same metal; the whole being weighed, the crucibles were submitted to the highest temperature which could be produced by the flame of an alcohol eolipyle. The crucibles were cooled in a receiver with a glass stopper, containing fragments of chloride of calcium. When perfectly cool they were again weighed, and the difference between the first weighing and that after calcination was attributed to the quantity of water disengaged, and was 14·97, 14·96, and 14·90 in three experiments.

In order to act upon the diaspore deprived of water, hydrated sulphuric acid was poured upon the mineral remaining in the crucible in which it had been calcined. The whole was heated in a sand-bath so as to volatilize the greater part of the sulphuric acid; when the matter had become of a pasty consistence, water was added, which dissolved a great quantity of sulphate of alumina; the solution was poured off, and more sulphuric acid was added, and this operation was repeated five times. The aluminous solution was filtered in order to separate a small portion of a white earthy deposit; this, which had resisted the prolonged action of sulphuric acid, still contained much alumina; when moistened with nitrate of cobalt and heated to redness, it acquired a very decided blue tint, and readily dissolved in the salt of phosphorus.

The solution of sulphate of alumina was supersaturated with carbonate of ammonia; the alumina was collected, washed and heated for a long time to strong redness. It was very white, and nitrate of cobalt gave a fine blue tint to it.

One hundred parts of diaspore yielded—

Alumina	79·91	
Water	14·90	
Mineral unacted upon	5·80	100·61

M. Damour admits that this analysis is superfluous after those of MM. Dufrenoy and Hess, and gives it merely to exhibit a property worthy of attention, and which had not been previously noticed with respect to any mineral whatever.—*Ann. de Ch. et de Phys.*, Mars 1846.

ON BORACIC ÆTHER.

M. Ebelmen having ascertained that boracic acid is volatilized by the vapour of water and of alcohol, succeeded in preparing, after some trials, boracic æther by the following process:—fused and finely-powdered boracic acid was put into a tubulated retort, and an equal weight of absolute alcohol was added to it. In a few minutes the temperature of the mixture became 122° Fahr., that of the atmosphere being only 64°. The retort was heated, and a thermometer placed in it showed that the liquid did not begin to boil until heated to about 203°, and its temperature continued rising from this point. At about 230° the distillation was stopped to cohobate the distilled liquid, and it was again distilled at 230°. The boracic acid swelled much during the operation, and the liquid which covered it while the distillation was going on, had completely imbibed it the following day. The distilled liquid had the slightly alliaceous smell of absolute alcohol, became very turbid on admixture with water, deposited boracic acid, burnt with a perfectly green flame, and yielded abundant white fumes of boracic acid.

The semi-solid mass remaining in the retort was bruised and digested during twenty-four hours in anhydrous æther, which completely disintegrated it; the æthereal solution, when clear, was poured into a retort placed in an oil-bath, and fitted with a condensing apparatus. It was requisite to employ a temperature of about 392° Fahr. to obtain the last traces of æther and of alcohol. There remained in the retort a large quantity of a viscid liquid, of a slight amber colour, yielding at 392° Fahr. thick vapours in contact with the air, and which became solid on cooling.

The author considers this product as boracic æther, and it approximates in physical properties to boracic acid and the borates, which are well-known to assume the vitreous state by fusion. It is in fact a true transparent glass, but one which is rather soft at common temperatures; at about 104° Fahr. it may be drawn into fine threads. It has a weak æthereal odour and burning taste; when applied to the skin, it occasions a strong sensation of heat, and is converted into a white powder, which is hydrated boracic acid; the same effect is produced by the contact of air with the boracic æther, but when the fragments are of considerable size, it takes place slowly, eventually however they become quite opake. When boracic æther is triturated with water, it is very rapidly decomposed with the extrication of much heat; alcohol is reproduced and may be obtained by distilling the aqueous liquid.

Boracic æther is volatile, but not distillable; at about 392° Fahr. it emits thick vapours into the air; but when distillation is attempted, it is decomposed, leaving a considerable residue of fused boracic acid. When it is dissolved in absolute alcohol and the mixture distilled, the alcohol volatilizes such a quantity of boracic æther, that on the addition of water it becomes almost a solid mass.

It is combustible, and burns with a white smoke and a fine green flame, leaving a residue of fused boracic acid. It is soluble in æther and alcohol in all proportions, and retains these fluids with great affinity, for it is requisite to heat them to 392° Fahr. to remove the last traces of them; these solutions become solid masses by the addition of water.

When boracic æther is heated, it first fuses, then decomposes, swells, and becomes less and less liquid. There are simultaneously disengaged alcohol, which retains a large quantity of boracic æther, and a colourless gas which burns with a green flame before it is washed in water. After having been passed through water, the gas burns with a bright flame, and possesses all the properties of olefiant gas. The residue of the decomposition is but unmixed anhydrous boracic acid, much swelled with carbonaceous matter; it is requisite to heat it to redness for a long time to expel the inflammable gas.

Great difficulty attends the analysis of boracic æther; it was effected by converting the boracic acid, first into borate of ammonia, and afterwards into anhydrous boracic acid. The mean of several experiments gave—

Boracic acid	66·7	
Carbon	19·8	
Hydrogen	4·4	
Oxygen (estimated by loss)	9·1	100·0

The author observes that the carbon and hydrogen are in the same proportions as in æther, while the oxygen is obviously in excess. The formula $BO^6 C^4 H^5 O$ is the nearest approach to the above mean; it gives—

Boracic acid	872·0	65·4
Carbon	300·0	22·4
Hydrogen	62·5	4·7
Oxygen	100·0	7·6
	1334·5	100·0

M. Ebelmen observes that the difference between the results of experiment and calculation are too considerable to be attributed to errors of analysis. It must be admitted, he says, that the boracic æther contains a certain excess of boracic acid disseminated uniformly throughout the vitreous mass; this supposition, he further observes, is not at all improbable, when the mode of preparing boracic æther is considered.—*Ann. de Ch. et de Phys.*, Fevrier 1846.

ACTION OF BORACIC ACID ON PYROXYLIC SPIRIT.

M. Ebelmen states that the action of boracic acid upon pyroxylic spirit is similar to that which it exerts upon alcohol; when equal weights of them are mixed, great increase of temperature is produced. On heating the retort from 212° to 230° Fahr., but little distilled product is obtained; on allowing the retort to cool, and treating the matter which it contains with anhydrous æther, and operating in other respects as for boracic æther of alcohol, boracic methylic æther is obtained, the properties of which are perfectly comparable with those of boracic æther. It is soft and may be drawn into threads at common temperatures; when treated with water it is immediately decomposed, with the disengagement of much heat, into boracic acid and pyroxylic spirit; it burns like boracic æther, with a fine green flame.

Pyroxylic spirit is preferable to alcohol as a reagent for determining the presence of boracic acid by the colour of the flame; when the alcoholic solution does not contain much boracic acid, the edges only of the flame are green, and it is often difficult to discover it. But with pyroxylic spirit it requires only a small quantity of the acid to give the whole flame a green colour; this result is doubtless dependent upon the fact, that the flame of the pyroxylic spirit by itself has less colour than that of alcohol.

When pyroxylic spirit is distilled with a great excess of boracic acid, a colourless gas is obtained which is soluble in water, and whose properties resemble those of boracic methylic æther, $C^2 H^3 O$; the mode in which boracic methylic æther is decomposed is therefore entirely different from that of the corresponding compound of alcohol.

M. Ebelmen found that boracic methylic æther yielded 69·5 and 70·6 per cent. of fused boracic acid by ammonia; the acid was black and contained a small quantity of charcoal disseminated through it;

the proportion of acid correspondent to the formula $BO^6 C^2 H^3 O$ would be 75·2 per cent. The product obtained was evidently a little impure, and contained, besides some boracic methylic æther, some of the compounds of boracic acid with the pyrogenous compounds, which it is so difficult to separate from pyroxylic spirit.—*Ibid.*

ON A SIMPLE METHOD OF PROTECTING FROM LIGHTNING, BUILDINGS WITH METALLIC ROOFS. BY PROF. HENRY.

On the principle of electrical induction, houses thus covered are evidently more liable to be struck than those furnished either with shingle or tile. Fortunately, however, they admit of very simple means of perfect protection. It is evident, from well-established principles of electrical action, that if the outside of a house were encased entirely in a coating of metal, the most violent discharge which might fall upon it from the clouds would pass silently to the earth without damaging the house, or endangering the inmates. It is also evident, that if the house be merely covered with a roof of metal, without projecting chimneys, and this roof were put in metallic connexion with the ground, the building would be perfectly protected. To make a protection, therefore, of this kind, the Professor advises that the metallic roof be placed in connexion with the ground, by means of the tin or copper gutters which serve to lead the water from the roof to the earth. For this purpose, it is sufficient to solder to the lower end of the gutter a riband of sheet copper, two or three inches wide, surrounding it with charcoal, and continuing it out from the house until it terminates in moist ground. The upper ends of these gutters are generally soldered to the roof; but if they are not in metallic contact, the two should be joined by a slip of sheet copper. The only part of the house unprotected by this arrangement will be the chimneys; and to secure these, it will only be necessary to erect a short rod against the chimney, soldered at its lower end to the metal of the roof, and extending fifteen or twenty inches above the top of the flue.

Considerable discussion in late years has taken place in reference to the transmission of electricity along a conductor; whether it passes through the whole capacity of the rod, or is principally confined to the surface. From a series of experiments presented to the American Philosophical Society, by Professor Henry, on this subject, it appears that the electrical discharge passes, or tends to pass, principally at the surface; and as an ordinary-sized house is commonly furnished with from two to four perpendicular gutters (two in front and two in the rear), the surface of these will be sufficient to conduct, silently, the most violent discharge which may fall from the clouds.

Professor Henry also stated, that he had lately examined a house struck by lightning, which exhibited some effects of an interesting kind. The lightning struck the top of the chimney, passed down the interior of the flue to a point opposite a mass of iron placed on the floor of the garret, where it pierced the chimney; thence it passed explosively, breaking the plaster, into a bedroom below, where it came in contact with a copper bell-wire, and passed along this horizontally and silently for about six feet; thence it leaped explosively through the air a distance of about ten feet, through a dormer window, breaking the sash, and scattering the fragments across the street. It was evidently attracted to this point by the upper end of a perpendicular gutter, which was near the window. It passed silently down the gutter, exhibiting scarcely any mark of its passage until it arrived at the termination, about a foot from the ground. Here again an explosion appeared to have taken place, since the windows of the cellar were broken. A bed, in which a man was sleeping at the time, was situated against the wall, immediately under the bell-wire; and although his body was parallel to the wire, and not distant from it more than four feet, he was not only uninjured, but not sensibly affected. The size of the hole in the chimney, and the fact that the lightning passed along the copper wire without melting it, show that the discharge was a small one, and yet the mechanical effects, in breaking the plaster, and projecting the window-frame across the street, were astonishingly great.

These effects the Professor attributes to a sudden repulsive energy, or expansive force developed in the air along the path of the discharge. Indeed, he conceives that most of the mechanical effects which are often witnessed in cases of buildings struck by lightning, may be referred to the same cause. In the case of a house struck within a few miles of Princeton, the discharge entered the chimney, burst open the flue, and passed along the *cockloft* to the other end of the house; and such was the explosive force in this confined space, that nearly the whole roof was blown off. This effect was, in all probability, due to the same cause which suddenly expands the air in the experiment with Kinnersly's electrical air thermometer.—From the *Proc. of the American Philosophical Society*, June 20, 1845.

OBSERVATIONS ON CAPILLARITY. BY PROF. HENRY.

In 1839, the author presented the results of some experiments on the permeability of lead to mercury; and subsequent observation had led him to believe that the same property was possessed by other metals in reference to each other. His first attempt to verify this conjecture was made with the assistance of Dr. Patterson, at the United States Mint. For this purpose, a small globule of gold was placed on a plate of sheet iron, and submitted to the heat of an assaying furnace; but the experiment was unsuccessful; for, although the gold was heated much above its melting-point, it exhibited no signs of sinking into the pores of the iron. The idea afterward suggested itself, that a different result would have been obtained had the two metals been made to adhere previous to heating, so that no oxide could have been formed between the surfaces. In accordance with this view, Prof. Henry inquired of Mr. Cornelius, of Philadelphia, if, in the course of his experience in working silver-plated copper, in his extensive manufactory of lamps, he had ever observed the silver to disappear from the copper when the metal was heated. The answer was, that the silver always disappears when the plate is heated above a certain temperature, leaving a surface of copper exposed; and that it was generally believed by the workmen, that the silver evaporates at this temperature.

Professor Henry suggested that the silver, instead of evaporating, merely sunk into the pores of the copper, and that by carefully removing the surface of the latter by the action of an acid, the silver would reappear. To verify this by experiment, Mr. Cornelius heated one end of a piece of thick plated copper to nearly the melting-point of the metal; the silver at this end disappeared, and when the metal was cleaned by a solution of dilute sulphuric acid, the end which had been heated presented a uniform surface of copper, whilst the other end exhibited its proper coating of silver. The unsilvered end of the plate was next placed, for a few minutes, in a solution of muriate of zinc, by which the exterior surface of copper was removed, and the surface of silver was again exposed. This method of recovering the silver before the process of plating silver by galvanism came into use, would have been of much value to manufacturers of plated ware, since it often happened that articles were spoiled, in the process of soldering, by heating them to the degree at which silver disappears.

It is well-known to the jeweller, that articles of copper, plated with gold, lose their brilliancy after a time, and that this can be restored by boiling them in ammonia; this effect is probably produced by the ammonia acting on the copper, and dissolving off its surface, so as to expose the gold, which, by diffusion, has entered into the copper.

A slow diffusion of one metal through another probably takes place in cases of alloys. Silver coins, after having lain long in the earth, have been found covered with a salt of copper. This may be explained by supposing that the alloy of copper, at the surface of the coin, enters into combination with the carbonic acid of the soil, and being thus removed, its place is supplied by a diffusion from within; and in this way it is not improbable that a considerable portion of the alloy may be exhausted in the process of time, and the purity of the coin be considerably increased.

Perhaps, also, the phænomenon of what is called *segregation*, or the formation of nodules of flint in masses of carbonated lime, and of indurated marl in beds of clay, may be explained on the same principle. In breaking up these masses, it is almost always observed, that a piece of shell or some extraneous matter occupies the middle, and probably formed the nucleus, around which the matter was accumulated by attraction. The difficulty consists in explaining how the attraction of cohesion, which becomes insensible at sensible distances, should produce this effect. To explain this, let us suppose two substances uniformly diffused through each other by a slight mutual attraction, as in the case of a lump of sugar dissolved in a large quantity of water, every particle of the water will attract to itself its proportion of the sugar, and the whole will be in a state of equilibrium. If the diffusion at its commencement had been assisted by heat, and this cause of the separation of the homogeneous particles no longer existed, the diffusion might be one of unstable equilibrium; and the slightest extraneous force, such as the attraction of a minute piece of shell, might serve to disturb the quiescence, and draw to itself the diffused particles which were immediately contiguous to it. This would leave a vacuum of the atoms around the attracting mass: for example, as in the case of the sugar, there would be a portion of the water around the nucleus deprived of the sugar; this portion of the water would attract its portion of sugar from the layer without, and into this layer the sugar from the layer next without would be diffused, and so on until, through all the water, the remaining sugar would be uniformly diffused. The process would continue to be repeated, by the nucleus again attracting a portion of the sugar from the water immediately around it, and so on until a considerable accumulation would be formed around the foreign substance.

We can in this way conceive of the manner by which the molecular action, which is insensible at perceptible distances, may produce results which would appear to be the effect of attraction acting at a distance.—From the *Proc. of the American Philosophical Society.*

OBITUARY.—The University of Königsberg has sustained a severe loss by the death of the celebrated astronomer Bessel, who died, after long suffering, on the 17th of March, in the 62nd year of his age.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1846.

Chiswick.—February 1. Very fine: rain. 2. Fine. 3, 4. Overcast: rain. 5, 6. Very fine. 7. Overcast: windy, with showers. 8. Clear: cloudy: very clear at night. 9. Frosty: fine, but cold. 10. Frosty: cloudy and cold. 11. Frosty: fine: partially overcast. 12. Foggy: cloudy and fine. 13. Densely clouded. 14, 15. Cloudy and fine. 16. Densely overcast. 17, 18. Overcast and fine. 19. Hazy. 20. Overcast. 21. Exceedingly fine. 22. Cloudy: boisterous, with rain at night. 23, 24. Rain. 25. Heavy clouds and mild. 26. Cloudy in the morning: afterward cloudless and exceedingly fine. 27. Slight haze: showery. 28. Very fine.

Mean temperature of the month	43°·32
Mean temperature of February 1845	33 ·07
Average mean temperature for the last twenty years	39 ·36
Average amount of rain	1 ·61 inch.

Boston.—Feb. 1. Fine. 2. Fine: rain early A.M. 3. Cloudy. 4. Fine. 5. Cloudy: rain early A.M. 6. Fine. 7. Stormy: rain early A.M. 8. Fine: rain early A.M. 9. Fine: snow early A.M.: snow A.M. and P.M. 10. Fine: snow on the ground. 11. Cloudy: snow on the ground. 12. Fine: snow on the ground. 13. Cloudy: snow all gone: melted snow. 14—22. Cloudy. 23. Cloudy: rain early A.M. 24. Cloudy. 25. Fine: rain early A.M. 26. Cloudy. 27. Fine: rain A.M. 28. Fine. This month has been usually fine.

Sandwick Manse, Orkney.—Feb. 1. Sleet-showers. 2. Cloudy. 3. Cloudy: sleet-showers. 4, 5. Hail-showers. 6. Showers: rain. 7. Showers: snow-showers. 8. Snow-showers. 9. Snow-showers: frost. 10. Snow: showers. 11. Clear: cloudy. 12. Cloudy: damp. 13. Showers. 14, 15. Cloudy: showers. 16. Rain: cloudy. 17. Showers: cloudy: drizzle. 18. Showers: drizzle: cloudy: drizzle. 19. Bright: cloudy. 20. Clear: cloudy. 21. Rain: cloudy. 22. Rain. 23. Clear. 24. Damp: showers. 25—27. Clear: cloudy. 28. Cloudy: showers: clear.

Applegarth Manse, Dumfries-shire.—Feb. 1. Occasional showers. 2. Fair and fine. 3. Heavy rain. 4. Sleet and rain P.M. 5. Showers. 6, 7. Heavy showers. 8. Slight fall of snow. 9. Frost: fine: clear. 10. Frost: fine. 11. Thaw: fair: mild. 12. Slight frost. 13. Very slight frost. 14, 15. Fine. 16. Very fine. 17. Fine. 18. Frost A.M. 19. Fine, but cloudy: shower. 20. Slight showers: mild. 21. Wet and stormy. 22, 23. Damp and drizzling. 24, 25. Heavy rain. 26. Wet. 27. Remarkably fine. 28. Damp and drizzling.

Mean temperature of the month	43°·4
Mean temperature of February 1845	34 ·5
Mean temperature of Feb. for twenty-three years	37 ·0
Mean rain in February for eighteen years	2 ·0 inches.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. Feb.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
1.	29·834	29·830	29·35	29·53	29·55	29·28	29·41	51	39	44	50½	41	43	40½	nw.	w.	w.	wnw.	·52	......	......	·20
2.	29·932	29·614	29·28	29·58	29·68	29·57	29·61	48	35	38	48	39½	41	43	nw.	calm	w.	wsw.	......	·06	......	·06
☽ 3.	29·894	29·821	29·49	29·50	29·38	29·24	29·14	52	39	43	49	42	45	37	sw.	calm	w.	w.	·01	......	......	·15
4.	30·053	29·956	29·52	29·65	29·67	29·36	29·41	49	36	40·5	43½	35½	38	42	w.	w.	w.	wsw.	·31	......	......	·57
5.	29·833	29·744	29·42	29·50	29·60	29·40	29·44	49	30	41	44	38	40	38	nw.	calm	w.	w.	......	·04	......	·16
6.	29·988	29·937	29·53	29·68	29·46	29·55	29·14	50	33	38	46½	35	37½	44½	w.	calm	wsw.	wsw.	......	......	......	·17
7.	29·994	29·829	29·26	29·29	29·69	29·08	29·58	52	31	48	48½	43½	41½	34	w.	w.	wnw.	wnw.	·08	·03	0·52	·38
8.	30·129	29·979	29·54	29·79	30·00	29·83	30·14	46	29	40	41	33½	34	34	w.	calm	nw.	nne.	·04	·08	......	·14
9.	30·341	30·113	29·77	30·18	30·23	30·37	30·44	45	26	33	49	32	34½	33½	ne.	calm	n.	nne.	......	......	......	·24
10.	30·341	30·070	30·05	30·31	30·22	30·31	30·01	41	22	32	40	29½	39	44	ne.	calm	n.	nw.	......	......	......	·03
○ 11.	30·158	30·074	29·78	30·00	30·00	30·05	30·01	44	25	34	46½	35	41	43½	nw.	calm	nw.	wnw.	......	......	......	·02
12.	30·142	30·118	29·83	30·03	29·99	29·99	29·91	45	36	34	50	33½	46	46	w.	calm	wnw.	wnw.	......	......	......	......
13.	30·137	30·105	29·73	29·96	29·99	29·91	29·89	45	27	40·5	49	34	45	43½	w.	calm	nw.	wnw.	......	·25	......	·06
14.	30·185	30·118	29·70	30·01	30·10	30·06	30·13	48	24	39	49	37½	42½	38	nw.	calm	nw.	wnw.	......	......	0·00	·06
15.	30·260	30·227	29·90	30·12	30·07	30·02	29·95	50	31	38	48	33	45	46	w.	calm	w.	w.	......	......	......	·13
16.	30·194	30·182	29·80	30·09	30·09	30·07	30·12	48	41	43	51	41½	46	45	nw.	calm	w.	w.	......	......	......	·13
17.	30·177	30·010	29·77	30·05	29·93	30·04	29·97	47	39	41	49½	39½	44½	45	w.	calm	nw.	nw.	......	......	......	·05
18.	30·005	29·978	29·60	29·90	29·90	29·95	29·86	48	39	42	47½	31	45	44½	w.	calm	n.	w.	......	......	......	......
☾ 19.	30·061	29·993	29·62	29·82	29·85	29·85	29·91	46	39	42	45	39	45	45	sw.	calm	w.	w.	......	......	......	·03
20.	30·095	30·074	29·72	29·88	29·82	29·91	29·84	50	40	42	47	40	43½	42	sw.	calm	s.	sw.	......	......	......	·02
21.	30·109	30·091	29·69	29·78	29·80	29·72	29·67	58	36	45·5	48½	42	44½	46	sw.	calm	s.	s.	·01	......	0·60	·10
22.	29·988	29·861	29·55	29·50	29·50	29·42	29·25	57	47	47	53	42½	47	47	s.	calm	s.	w.	·21	......	......	·15
23.	29·800	29·690	29·38	29·49	29·45	29·41	29·50	58	50	52·5	51	47½	50	45	sw.	calm	sw.	se.	·07	·06	......	·24
24.	29·603	29·405	29·15	29·18	29·08	29·27	29·15	59	49	52·5	55½	48	46	46	s.	calm	ssw.	se.	·16	......	......	......
● 25.	29·801	29·489	29·00	28·93	29·43	28·99	29·19	58	34	52	54	47	47½	46½	sw.	w.	sw.	sw.	......	·03	......	·22
26.	29·800	29·663	29·37	29·46	29·36	29·48	29·41	60	39	45	52	42	46	46	s.	w.	w.	se.	·01	......	......	......
27.	29·648	29·623	29·30	29·40	29·44	29·45	29·55	62	38	49	56½	46	47½	47	se.	w.	w.	se.	·05	......	0·40	·03
28.	29·952	29·664	29·23	29·47	29·62	29·51	29·53	64	42	53	52	41½	47	46	sw.	w.	w.	sw.	......	·02	......	......
Mean.	30·016	29·902	29·54	29·701	29·775	29·681	29·684	51·07	35·07	42·3	48·8	39·0	43·30	42·8					1·61	0·57	1·52	3·34

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MAY 1846.

LIV. *Thoughts on Ray-vibrations.* By MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof., &c. &c.*

To Richard Phillips, Esq.

DEAR SIR,

AT your request I will endeavour to convey to you a notion of that which I ventured to say at the close of the last Friday-evening Meeting, incidental to the account I gave of Wheatstone's electro-magnetic chronoscope; but from first to last understand that I merely threw out as matter for speculation, the vague impressions of my mind, for I gave nothing as the result of sufficient consideration, or as the settled conviction, or even probable conclusion at which I had arrived.

The point intended to be set forth for the consideration of the hearers was, whether it was not possible that the vibrations which in a certain theory are assumed to account for radiation and radiant phænomena may not occur in the lines of force which connect particles, and consequently masses of matter together; a notion which, as far as it is admitted, will dispense with the æther which, in another view, is supposed to be the medium in which these vibrations take place.

You are aware of the speculation* which I some time since uttered respecting that view of the nature of matter which considers its ultimate atoms as centres of force, and not as so many little bodies surrounded by forces, the bodies being considered in the abstract as independent of the forces and capable of existing without them. In the latter view, these little particles have a definite form and a certain limited size; in the former view such is not the case, for that which represents size may be considered as extending to any distance to which the lines of force of the particle extend: the particle indeed is

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* Phil. Mag. 1844, vol. xxiv. p. 136.

supposed to exist only by these forces, and where they are it is. The consideration of matter under this view gradually led me to look at the lines of force as being perhaps the seat of the vibrations of radiant phænomena.

Another consideration bearing conjointly on the hypothetical view both of matter and radiation, arises from the comparison of the velocities with which the radiant action and certain powers of matter are transmitted. The velocity of light through space is about 190,000 miles in a second; the velocity of electricity is, by the experiments of Wheatstone, shown to be as great as this, if not greater: the light is supposed to be transmitted by vibrations through an æther which is, so to speak, destitute of gravitation, but infinite in elasticity; the electricity is transmitted through a small metallic wire, and is often viewed as transmitted by vibrations also. That the electric transference depends on the forces or powers of the matter of the wire can hardly be doubted, when we consider the different conductibility of the various metallic and other bodies; the means of affecting it by heat or cold; the way in which conducting bodies by combination enter into the constitution of non-conducting substances, and the contrary; and the actual existence of one elementary body, carbon, both in the conducting and non-conducting state. The power of electric conduction (being a transmission of force equal in velocity to that of light) appears to be tied up in and dependent upon the properties of the matter, and is, as it were, existent in them.

I suppose we may compare together the matter of the æther and ordinary matter (as, for instance, the copper of the wire through which the electricity is conducted), and consider them as alike in their essential constitution; *i. e.* either as both composed of little nuclei, considered in the abstract as matter, and of force or power associated with these nuclei, or else both consisting of mere centres of force, according to Boscovich's theory and the view put forth in my speculation; for there is no reason to assume that the nuclei are more requisite in the one case than in the other. It is true that the copper gravitates and the æther does not, and that therefore the copper is ponderable and the æther is not; but that cannot indicate the presence of nuclei in the copper more than in the æther, for of all the powers of matter gravitation is the one in which the force extends to the greatest possible distance from the supposed nucleus, being infinite in relation to the size of the latter, and reducing that nucleus to a mere centre of force. The smallest atom of matter on the earth acts directly on the smallest atom of matter in the sun, though they are 95,000,000 of miles apart; further, atoms which, to our knowledge, are at least nineteen times that distance, and indeed, in cometary masses, far more, are in a similar way tied together by the lines of force extending from and belonging to each. What is there in the condition of the particles of the supposed æther, if there be even only *one* such particle between us and the sun, that can in subtilty and extent compare to this?

Let us not be confused by the *ponderability* and *gravitation* of heavy matter, as if they proved the presence of the abstract nuclei; these are due not to the nuclei, but to the force superadded to them, if the nuclei exist at all; and, if the *æther* particles be without this force, which according to the assumption is the case, then they are more material, in the abstract sense, than the matter of this our globe; for matter, according to the assumption, being made up of nuclei and force, the æther particles have in this respect proportionately more of the nucleus and less of the force.

On the other hand, the infinite elasticity assumed as belonging to the particles of the æther, is as striking and positive a force of it as gravity is of ponderable particles, and produces in its way effects as great; in witness whereof we have all the varieties of radiant agency as exhibited in luminous, calorific, and actinic phænomena.

Perhaps I am in error in thinking the idea generally formed of the æther is that its nuclei are almost infinitely small, and that such force as it has, namely its elasticity, is almost infinitely intense. But if such be the received notion, what then is left in the æther but force or centres of force? As gravitation and solidity do not belong to it, perhaps many may admit this conclusion; but what is gravitation and solidity? certainly not the weight and contact of the abstract nuclei. The one is the consequence of an *attractive* force, which can act at distances as great as the mind of man can estimate or conceive; and the other is the consequence of a *repulsive* force, which forbids for ever the contact or touch of any two nuclei; so that these powers or properties should not in any degree lead those persons who conceive of the æther as a thing consisting of force only, to think any otherways of ponderable matter, except that it has more and other *forces* associated with it than the æther has.

In experimental philosophy we can, by the phænomena presented, recognise various kinds of lines of force; thus there are the lines of gravitating force, those of electro-static induction, those of magnetic action, and others partaking of a dynamic character might be perhaps included. The lines of electric and magnetic action are by many considered as exerted through space like the lines of gravitating force. For my own part, I incline to believe that when there are intervening particles of matter (being themselves only centres of force), they take part in carrying on the force through the line, but that when there are none, the line proceeds through space*. Whatever the view adopted respecting them may be, we can, at all events, affect these lines of force in a manner which may be conceived as partaking of the nature of a shake or lateral vibration. For suppose two bodies, A B, distant from each other and under mutual action, and therefore connected by lines of force, and let us fix our attention upon one resultant of force having an invariable direction as regards space; if one of the bodies move in the least degree right or left, or if its power be shifted for a moment within the mass (neither of these cases being difficult to realize if A and B be either electric or magnetic bodies), then an effect equivalent to a lateral disturbance will take place in the resultant upon which we are fixing our attention; for, either it will increase in force whilst the neighbouring resultants are diminishing, or it will fall in force as they are increasing.

It may be asked, what lines of force are there in nature which are fitted to convey such an action and supply for the vibrating theory the place of the æther? I do not pretend to answer this question with any confidence; all I can say is, that I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. The lines of weight or gravitating force are, certainly, extensive enough to answer in this respect any demand made upon them by radiant phænomena; and so, probably, are the lines of magnetic force: and then who can forget that Mossotti has shown that gravitation, aggregation, electric force, and electro-chemical action may all have one common connexion or origin; and so, in their actions at a distance, may have in common that infinite scope which some of these actions are known to possess?

The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavours to dismiss the æther, but not the vibrations. The kind of vibration which, I believe, can alone account for the wonderful, varied, and beautiful phænomena of polarization, is not the same as that which occurs on the surface of disturbed water, or the waves of sound in gases or liquids, for the vibrations in these cases are direct,

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* Experimental Researches in Electricity, pars. 1161, 1613, 1663, 1710, 1729, 1735, 2443.

or to and from the centre of action, whereas the former are lateral. It seems to me, that the resultant of two or more lines of force is in an apt condition for that action which may be considered as equivalent to a *lateral* vibration; whereas an uniform medium, like the æther, does not appear apt, or more apt than air or water.

The occurrence of a change at one end of a line of force easily suggests a consequent change at the other. The propagation of light, and therefore probably of all radiant action, occupies *time*; and, that a vibration of the line of force should account for the phænomena of radiation, it is necessary that such vibration should occupy time also. I am not aware whether there are any data by which it has been, or could be ascertained whether such a power as gravitation acts without occupying time, or whether lines of force being already in existence, such a lateral disturbance of them at one end as I have suggested above, would require time, or must of necessity be felt instantly at the other end.

As to that condition of the lines of force which represents the assumed high elasticity of the æther, it cannot in this respect be deficient: the question here seems rather to be, whether the lines are sluggish enough in their action to render them equivalent to the æther in respect of the time known experimentally to be occupied in the transmission of radiant force.

The æther is assumed as pervading all bodies as well as space: in the view now set forth, it is the forces of the atomic centres which pervade (and make) all bodies, and also penetrate all space. As regards space, the difference is, that the æther presents successive parts or centres of action, and the present supposition only lines of action; as regards matter, the difference is, that the æther lies between the particles and so carries on the vibrations, whilst as respects the supposition, it is by the lines of force between the centres of the particles that the vibration is continued. As to the difference in intensity of action within matter under the two views, I suppose it will be very difficult to draw any conclusion, for when we take the simplest state of common matter and that which most nearly causes it to approximate to the condition of the æther, namely the state of rare gas, how soon do we find in its elasticity and the mutual repulsion of its particles, a departure from the law, that the action is inversely as the square of the distance!

And now, my dear Phillips, I must conclude. I do not think I should have allowed these notions to have escaped from me, had I not been led unawares, and without previous consideration, by the circumstances of the Evening on which I had to appear suddenly and occupy the place of another. Now that I have put them on paper, I feel that I ought to have kept them much longer for study, consideration, and, perhaps, final rejection; and it is only because they are sure to go abroad in one way or another, in consequence of their utterance on that evening, that I give them a shape, if shape it may be called, in this reply to your inquiry. One thing is certain, that any hypothetical view of radiation which is likely to be received or retained as satisfactory, must not much longer comprehend alone certain phænomena of light, but must include those of heat and of actinic influence also, and even the conjoined phænomena of sensible heat and chemical power produced by them. In this respect, a view, which is in some degree founded upon the ordinary forces of matter, may perhaps find a little consideration amongst the other views that will probably arise. I think it likely that I have made many mistakes in the preceding pages, for even to myself, my ideas on this point appear only as the shadow of a speculation, or as one of those impressions on the mind which are allowable for a time as guides to thought and research. He who labours in experimental inquiries knows how numerous these are, and how often their apparent fitness and beauty vanish before the progress and development of real natural truth.

I am, my dear Phillips,
Ever truly yours,
Royal Institution, April 15, 1846. M. FARADAY.

LV. *On the Wax of the Chamærops.*

By J. E. TESCHEMACHER, *Esq.**

ABOUT three millions of palm leaves are annually imported into the United States of America, for the purpose of being manufactured into hats. They come tied in bundles, called in Spanish *Esteras*, each *estera* weighing from 50 to 60 pounds; these are the palmate part of the leaf with a small portion of the petiole; this last weighs one-eighth of the leaf. The palm from which the leaves are cut in Cuba and other parts of the West Indies for this purpose is a Chamærops, a low-growing species, not differing I believe from the *C. humilis* of the southern sections of the United States, except in being much more robust in habit. The *C. humilis* of the United States is too soft and yielding for this manufacture.

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* Communicated by the Chemical Society; having been read December 1, 1845.

I have cultivated the plant from Cuba for five or six years, and was unable to discover any difference in foliage; but I have never seen the fruit of either. The leaf of the Chamærops spreads out nearly horizontal with folds, precisely like those of a lady's fan. On opening these folds, when they arrive in the United States in their dried state, there is a quantity of white flaky powder, under this is the bright varnish which covers the whole surface of the leaf; both these are true vegetable wax. From one of these palm leaves I obtained, by passing the thumb down the folds, 90 grains of the white wax in powdery flakes, and by boiling the leaf, after cutting in pieces, in alcohol, 300 grains more of a grayer coloured wax.

At the manufactory the leaves are often bleached by the fumes of sulphurous acid gas, and then split by machinery into very thin strips; this division cracks off of course a large portion of the brittle varnish, which, together with the white powder, falls to the ground, is swept together and burnt or thrown away. The weight of this substance destroyed annually probably exceeds one hundred thousand pounds.

On treating this substance with a small quantity of boiling alcohol, it may, like other wax, be separated into cerine and myricine.

The powdery flakes first obtained contain about 80 per cent. myricine and 20 per cent. cerine, but the wax obtained from boiling the leaf in alcohol contains scarcely any myricine. This is easily accounted for; the flakes, being the brittle and more resinous part, break off readily; while the alcohol, which acts on the leaves, dissolves only the cerine, leaving the myricine undissolved; this might no doubt be obtained by increasing the quantity of alcohol and continuing the process, if it were desirable. In bees' wax the proportions of these two substances vary also, the cerine from 70 to 90 per cent., myricine from 10 to 30 per cent.; and it is probable that the more or less brittle quality of all wax depends on the relative quantity of these two ingredients.

The wax of *Ceroxylon andicola*, a very lofty palm, found by Humboldt at Quindin on the Andes, has been analysed, and found very nearly to resemble bees' wax in its ultimate principles.

	Bees' wax.	Palm wax.
Carbon	80·14	80·28
Hydrogen	14·08	13·20
Oxygen	5·78	6·52

To obtain this wax, the outer portion of the trunk is rasped or scraped, the raspings are heated in water, the wax swims at the top, the other parts fall to the bottom, the wax is collected, made into small balls, and dried in the sun; it has a deep yellow colour, and when the resinous part (myricine?) is melted it has the appearance of amber; after the separation of the wax and resin from the produce of Ceroxylon, there remains in the alcohol a bitter yellow substance, supposed to be a vegetable alkaloid. This yellow substance separates also from the wax of the leaf of Chamærops, but I think it is not an ingredient in the wax, but of other parts of the juices dissolved by the alcohol.

The production from the juices of plants by a purely vegetable function of wax scarcely differing from that deposited in their hives by bees is calculated to throw light on the question of the formation of this substance by these insects, and also merits the careful examination of those who are entering into the study of the various transformations of the vegetable juices at different periods of their progress towards maturity.

LVI. *Analysis of a Cobalt Ore found in Western India.*

By J. MIDDLETON, *Esq., F.G.S.*[*]

BEING engaged in analyses of the metallic ores of North-western India, with a view to the ascertainment of the constitutions of those most remarkable among them, and also with the hope of detecting others whose existence in the country has not heretofore been even suspected, I am desirous of submitting to the Chemical Society the results of my inquiries whenever they appear to me of sufficient interest to justify my troubling them with them. I may mention, that should the Society desire any information from me on this or any other subject that I may be qualified and in a position to furnish, I shall most gladly meet their wishes.

The hilly districts of Rajpootanah are remarkably prolific in metallic ores, many of these, too, exceedingly rich and abundant. Within a narrow compass in the independent state of Syepoore, are to be found the following minerals:—

Sulphuret of copper, sulphate of copper, sulphuret of cobalt, alum.

The native method of mining for the first of these ores, and which is the same as that adopted for the others, may be interesting to some of your members.

"The mine of copper is very deep, and difficult of access. The miners enter with burning lamps on their heads and with chisels, iron hammers, and baskets in their hands. They dig out the ore with their chisels by the light of their lamps, and bring it up with great labour and difficulty to the surface. They then pound and grind it small in a mill, after which

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[*] Communicated by the Chemical Society; having been read December 15, 1845.

they mix it with moist cow-dung, and this mixture being made into balls is placed in the sun to dry. When this has been accomplished the lumps are burnt, after which they are not broken up, but being mixed with an equal quantity of charcoal and as much iron filings, are put into a crucible, and a strong heat kept up by blowing with a leathern bellows till the dross separates and the copper settles at the bottom in the form of a solid disc. This product is again heated with charcoal until perfectly pure copper is produced*."

The mineral possessing greatest interest amongst those above enumerated is the sulphuret of cobalt. It is found in the copper mines in considerable abundance, and exists in a primitive schist in the form of bands and disseminated grains, the colour of which is a steel gray inclining to yellow. The grains appear to be crystallized, and are probably the cube and its derivatives. What is particularly remarkable in this ore is its purity, so far surpassing in this respect any that, so far as I am aware, is to be met with anywhere else. The only substance in combination with it, after separation of the matrix, is an iron pyrites, which is however but mechanically mixed, and so highly magnetic as to be readily removable by the magnet. The relative proportions in which these two exist are—

Cobalt pyrites	90·78 per cent.
Iron	9·22 ...

The iron pyrites consists of black amorphous granules without metallic lustre, and, as above stated, it is highly magnetic, having at the same time the low specific gravity of 2·58. It gives on analysis—

Iron	62·27 per cent.
Sulphur	37·73 ...

The analysis was carefully made, and repeated for verification, so that, notwithstanding the specific gravity is so much lower than that assigned as characteristic of iron pyrites, there can be no doubt such is the constitution of this constituent of the ore in question.

The cobalt pyrites exhibits the usual characteristic reactions, generally subject to some modifications, which do not deserve notice, as I found them to be mostly owing to the high temperature at which my experiments were made: one however is rather remarkable, and not assignable to this cause, but probably to the particular natural constitution of the mineral, which, as I have found, in other cases modifies the behaviour of substances occasionally.

Ferrocyanide of potassium produces in acid solutions a

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* This description is the translation of a native one given to me with the minerals.

bluish-green precipitate, which completely dissolves up in forty-eight hours, provided the solution be not highly concentrated, to a brilliant emerald-green fluid, which is not affected by acids or by standing, but the colour of which changes to greenish yellow, without precipitation, by ammonia.

By very careful and repeated analysis, the reduction process having been adopted for the metal, I found the proportion of the constituents to be, taking the average,—

Cobalt	64·64 per cent.
Sulphur	35·36 ...

from which it is obvious the substance is a sub-sulphuret, that its constitution is $Co_2 S$, a rather remarkable result, considering that the iron compound, doubtless of simultaneous formation, is different.

The cobalt pyrites has the specific gravity of 5·45. It is used by Indian jewellers for staining gold of a delicate rose-red colour; the *modus operandi* which they follow I have been unable to learn; it is a secret with them, which they are unwilling to disclose.

LVII. *On the Structural Relations of Organized Beings.*

By H. E. STRICKLAND, *M.A., F.G.S.*[*]

I PROPOSE to make a few observations on the Relations which subsist between different organized beings in respect of the *similarities* of their physical structures. This limitation will exclude—first, the relations between individuals, such as that of parent to offspring, for in individuals of the same species the essential points of structure are not *similar*, but *identical*; and secondly, the relations between an organized being and the external circumstances of soil, climate, or food, to which it is adapted, in other words, between structure and function; for these adaptations of the one to the other, however interesting and admirable in themselves, are not relations of *similarity*.

On comparing together the innumerable species of organized beings, we find their structures to present every possible degree of variation, from an almost perfect identity to the utmost amount of difference which the mind can conceive any two organized bodies to possess. These agreements and differences are not however devoid of laws and principles; they admit of being classed under certain general heads, and we thus discover the traces of Divine workmanship not merely

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[*] Read before the Ashmolean Society of Oxford, March 10, 1845, and communicated by the Author.

in the structure of an individual organism, but in the mutual relations of those organisms, the due combinations of which constitute the Natural Systems of Botany and Zoology.

When the human mind first began to observe and to compare the structures of organic life, to generalize the points of agreement, and thus to lay the foundation of the Science of Natural History, no inherent principles of classification were even suspected to exist, characters were compared and generalized at random, and the arrangements which resulted were of the rudest and most unphilosophical kind. The most superficial and arbitrary characters were selected as the basis of classification, and no man was able to give a reason why one mode of arrangement should not be as correct and as true to Nature as another. Thus we find the older naturalists classing Lizards, Tortoises and Frogs with terrestrial Mammalia, under the name of "Four-footed Beasts," while Serpents were made into a distinct Class; and Whales, whose physiological organization is as highly developed as in any other Mammal, were dismissed among the cold-blooded Class of Fish, into which the humble Lobster and the Oyster entered from the other side to keep them company. By some authors we find the *Echinus* and the Hedge-hog approximated, because both are covered with spines; the Ammonite and the Rock-crystal were described in the same chapter "de lapidibus"; Shrew-mice and Spiders were classed together, because both were supposed to be venomous; Bats were referred to Birds, Corals to Plants, and so on.

In the course of the seventeenth century, the few who cultivated natural science began to be conscious that these crude arrangements were not satisfactory, or consistent with the realities of Nature; and in the works of Ray and of Lister, we perceive many instances of an instinctive preference for essential instead of arbitrary characters. But it was Linnæus who first pointed out in express terms the great principle of the *Subordination of Characters.* This principle teaches us to give to each point of structure its due weight, and to attach more value to those peculiarities whose immediate influence on the mysteries of Life often renders them the most difficult for our senses to appreciate, than to those external characters which, though most conspicuous to the eye, are but remotely connected with the real Essence of the creature. This principle has been further developed by later naturalists, especially by Cuvier, and accordingly we now find that in the modern systems of Zoology the *primary* divisions of the Animal Kingdom are based on characters derived chiefly from the *nervous* system, as being the most important feature in organization, the *secondary* subdivisions are grounded on the *organs of respiration*, groups of a lower rank on the *digestive* system, and so on, the most superficial peculiarities, such as external form and colour, being reserved to characterize the ultimate groups of genera and species. These improved principles of classification are gradually bringing the systems of Zoology and Botany into a state of permanence, consistent with Nature, and satisfactory to that Truth-seeking Instinct which is inherent in the human mind.

A further advance of philosophical Classification has shown that the characters of organized beings require not only to be subordinated according to their importance, but subdivided according to their kinds. There are many instances of correspondence of structural characters in organic beings which can never by any process of subordination become elements in a natural classification, and it is important to distinguish those which *can* from those which *cannot* be so employed. Zoologists had long been aware that certain sets of characters produced an arbitrary or artificial method if employed for classification, while others seemed to lead to a natural system, but the question was involved in obscurity till the time of MacLeay, who was the first to give us clear definitions on the distinction between AFFINITY and ANALOGY. He applied his views indeed in support of a theory, the *Quinary System*, which few naturalists are now disposed to support, and with which we are not now concerned; but his elucidation of Affinities and Analogies is not the less valuable on that account. Although I am not disposed to take the same view of these principles as that of Mr. MacLeay, yet as the principles themselves are at the foundation of all sound classification, whether in Zoology or Botany, I may be allowed to make a few further remarks upon this subject.

It appears to me that the instances of resemblance or agreement of structure between any two species of organized beings should be reduced, not into *two*, but into *three* distinct classes, *Affinity*, *Analogy*, and a third, for which I propose to adopt the name of *Iconism**.

I. The highest class of these structural agreements is that of *Affinities*, which appear to be the direct result of those Laws of Organic Life which the Creator has enacted for his own guidance in the act of Creation. Affinity consists in an *essential* and physiological agreement in the corresponding parts of organic beings, resulting from a uniformity of plan

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* This term, suggested by the Rev. Dr. Ingram, President of Trinity College, appears preferable to *Mimesis*, which I had originally proposed to use.

which pervades the System of Nature*. These essential agreements of parts consist rather in a similarity of organic *composition* and of *relative situation*, than of *form*. A microscopic examination of the primary tissue, or a chemical analysis of its substance, will often demonstrate the true affinities of a structure when its external form would only mislead us. And when we have proved an affinity to subsist between the structures of two organic beings, we then apply the term to the beings themselves, and say that an affinity subsists between them, greater or less, according to the number and importance of the organs in which such affinity is shown. Take for example the long, straight weapon of offence in the Narwhal, its general appearance is that of a *horn*, and such the vulgar accordingly call it; but if we examine its organization and its chemical composition, we find that both are utterly unlike those of real horns, but correspond to the structure of teeth. Further, if we examine the mode of its connexion with the skull, we find that it is inserted into a socket like other teeth, instead of being attached in the manner of horns, and we accordingly pronounce it to be not a horn but a tooth, developed for purposes of offence to an extraordinary extent. And having thus shown that the weapon of the Narwhal has no affinity to real horns, we no longer appeal to this structure in proof of any affinity between the Narwhal and the truly horned animals. Again, the Narwhal in its external form much resembles a Fish; but when we look to its nervous, circulatory, and reproductive organizations, which rank much higher in the scale of characters than external form, we find that it is no Fish, but a true Mammal, agreeing in every essential point with the warm-blooded quadrupeds of the land, to which its affinities are real and direct. Similar instances of the discordance between outward form and real affinity might be multiplied to a great extent; and it forms a constant employment for the scientific zoologist to distinguish real affinities from apparent ones, and thus to refer every organized being to its true position in the Natural System.

It will thus be seen that every instance of asserted affinity

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* We may suppose, for instance, that it was a law of organic creation that all Birds should have the anterior extremities modified into the form of wings; and in obedience with this law we find that there is no Bird which is absolutely without wings, though there are several kinds in which the wings are perfectly incapable of flight. Again, it is a law that Mammalia have neither more nor less than seven cervical vertebræ; and we find this law to hold good, without an exception, through the whole Class of Mammals, from the slender-necked Giraffe to the Whale, which can hardly be said to have any neck at all. The above, out of countless other examples, will show what is meant by *laws of organization*.

between two organic beings is merely a corollary deduced from an observed affinity between the corresponding organs in each; and though it is not usual to apply the term *affinity* to the similarities between parts, yet as the similarity between the wholes results from the similarities of their parts, the word affinity may be as correctly applied to the one as to the other. In works of comparative anatomy it is customary to speak of those members which are essentially equivalent in two organic beings as *analogous* organs, but we shall soon see that the word analogy has a very different sense; and as the relation between equivalent organs is one of real *affinity*, and forms the sole ground on which we assert the affinity of the whole beings, we may introduce the adjective *affine* or *homologous* in place of *analogous*, when referring to structures which essentially correspond in different organic beings.

When we say that Affinity consists in an essential agreement of structure resulting from a fixity of purpose in the mind of Creative Wisdom, it must not be supposed that all affinities are equally strong, direct, and palpable. Any agreement, however slight, or however concealed by more palpable differences, which forms part of the plan of organic existence, is a true affinity; and the principle of subordination of characters before referred to is merely the arranging of these affinities in the true order of their proximities. The proximity of affinities is in the inverse ratio of their essential importance, the most important agreements of characters being those which have the widest extent, and which therefore form affinities between the remotest points in the System of Organized Beings. We will illustrate this by an example showing the successive series of affinities which the same species bears to others, commencing with the most remote, and proceeding to the closest affinity which can subsist between two distinct species. We will take as an example the species Raven (*Corvus corax*).

	A Raven has an Affinity to an	it is the same Affinity which exists between	and is derived from the Affinity between their respective	supplying the diagnostic characters of the
1.	Oak-tree;	all Animals and all Plants,	Organic Life, &c.	*Organic* EMPIRE.
2.	Locust;	Vertebrata and Insects,	nervous systems, &c.	*Animal* KINGDOM.
3.	Salmon;	Birds and Fish,	vertebral columns, &c.	PROVINCE, *Vertebrat*
4.	Swan;	Insessores and Natatores,	circulatory systems, &c.	CLASS, *Birds*.
5.	Humming Bird;	Conirostres and Tenuirostres,	structure of feet, &c.	ORDER, *Insessores*.
6.	Sparrow;	Corvidæ and Fringillidæ,	conical beaks, &c.	TRIBE, *Conirostres*.
7.	Jay;	Corvinæ and Garrulinæ,	structure of nostrils, &c.	FAMILY, *Corvidæ*.
8.	Magpie;	Corvus and Pica,	short elevated beaks, &c.	SUBFAMILY, *Corvinæ*
9.	Carrion Crow;	one species of Corvus and another,	even tails, black plumage, &c.	GENUS, *Corvus*.

The affinities in this series are seen to accumulate successively as we proceed from the remotest organism to the approximate species. The Raven and Carrion Crow not only possess that superficial resemblance of form which constitutes their generic character, but they have in addition all the other points of affinity which extend from them to a greater or less distance into the realms of organic existence. Thus we find that

The Raven has

organization	in common with all	Organized Beings.
a nervous system	...	Animals.
a vertebral column	...	Vertebrata.
a peculiar circulatory system	...	Birds.
perching feet	...	Insessores.
a conical beak	...	Conirostres.
the nostrils covered by feathers	...	Corvidæ.
ridge of the beak arched	...	Corvinæ.
an even tail	...	Corvus.
and		
a wholly black plumage	...	Carrion Crow.

It will be seen from the above example, that the whole process of classification consists in observing the affinities of structure in different beings, in estimating their importance, and in arranging them according to that estimate. It follows that a clear comprehension of *affinities*, as distinguished from the other kinds of resemblance, is essential to the objects of the scientific zoologist.

Although affinity consists in an essential and intimate agreement in the structure of certain organs, yet it by no means implies an identity of function in those organs. The modifications of external form are so various that we frequently find the same organ applied by different animals to purposes the most remote from its normal function; and on the other hand we see very different organs applied to discharge the same function. Thus, as a general proposition, it is certain that the proper function of wings is flying, of legs walking, of fins swimming; and yet we find examples where each of these organs is applied to any other function but its own, as in the case of the Bat, Seal, Ostrich, Penguin, Gurnard, and Flying Fish. Hence, although it is generally true that certain organs are destined to perform certain definite functions, yet the exceptions are so frequent as to make us attach a minor degree of importance to *function*, while we give the fullest weight to those essential properties which form the only test of real affinity.

II. We have next to consider that class of structural agreements known by the name of *Analogies*. These consist in a similarity of external form and of function connected with it, but without that agreement of essence which constitutes *Affinity*. These analogous agreements are equally the result of natural laws, but of laws of a different class from the former. Agreements of affinity are produced in conformity with the laws of the organic Creation, while analogies have a reference to the laws and properties of external and often inorganic matter. In obedience to these laws, it follows that whenever an instrument is required to produce a given effect upon external objects, or to resist their influences in a given manner, there is in general one method, and one only, of effecting the object in the best and most effectual way. Accordingly, whatever be the organ or instrument employed, that organ must have a certain and definite mechanical structure bestowed upon it to obtain the desired end. As a general rule, the same end is attained in different organic beings by means of the same set of organs; but when those organs are required for any other purpose, or are so modified as to be unfit for that special end, then some other set of organs are endowed with the requisite external structure and are called upon to act as substitutes for the legitimate instruments. Examples of this adaptation of organs to purposes remote from their normal destination are numerous and well-known; and I cannot do better than refer to the late Mr. John Duncan's work on the Analogies of Organized Beings, where there are numerous examples of such analogies arranged in a tabular and highly perspicuous form. We need only take the Elephant as an instance. We may suppose that this animal required horns for the purpose of defence, but it belongs to an order, the Pachydermata, in which horns are uniformly absent, and the laws of Affinity forbade their introduction. To supply this defect, the incisor teeth are removed from their usual duties of mastication, and are so developed as to assume the form and discharge the function of horns. Further, the great size and weight of these lengthened tusks required a great strength and shortness of neck, and the animal was consequently unable to reach the ground with his mouth. A hand was therefore required to convey the food to the mouth, but the vast weight of the animal required a massive structure in the feet, which forbade them to be adapted to the purpose of hands. To supply this want then the *nose* is lengthened out, furnished with muscles, divided at the end into a finger and thumb, and in this proboscis behold a hand! almost equal in delicacy of manipulation to the hand of Man. And thus we see the Elephant, endowed in one respect with an analogy to the Ox, and in another respect to Man, yet having no immediate affinity with either.

As then Analogy consists in an agreement of function, and only of form so far as it tends to discharge that function, it follows that real and genuine *Analogies* may take place between the works of Nature and the works of Man, while no such relation of *Affinity* can possibly exist. When, for instance, the inventive powers of Man are called upon to imitate any of the operations of Nature, the external matter to be acted on being in both cases the same, a similar arrangement of form is adopted by both. If the problem be to make a floating body adapted for rapid motion through water, Man either by practical experiment or mathematical calculation produces the form of a boat, and thus unconsciously imitates the structure of the Whale and Seal among Mammals, the Penguin among Birds, the Ichthyosaurus and Turtle among Reptiles, the Fish among Vertebrata, the Dytiscus among Coleoptera, the Notonecta among Hemiptera, Sepia among Mollusca, Physalia among Acalephæ, &c. &c. Nor is the analogy between a ship and a Fish confined to the external form only; the keel of the one represents the spine of the other, the "ribs" of both agree in name as in nature, the rudder coincides with the tail, the oars with the fins, the masts with the spinous processes, the running rigging with the tendons, the seamen with the muscles, the *look-out man* on the forecastle with the eye, and the captain in the cabin with the mental faculties in the Fishes' brain. Again, what can be more striking than the analogy between a locomotive steam-engine and a living Animal? We see in both an analogous respiratory and digestive system, the same necessity for food and drink and oxygen to sustain that internal combustion which is the source of the vital action, the same obedience of the organs of motion to the impulse of the governing mind, and the same wear and tear of the system, terminating in old age and sudden or gradual death. Yet in all these cases there is no set purpose on the part of Man to imitate the works of Nature, he merely applies the faculties which God has given him to elicit the properties which the same God has given to matter; and by this process alone he often arrives at the same or similar results to those at which Creative Wisdom had arrived before him. It appears to me therefore, that relations of Analogy, that is to say, agreements in structure in consequence solely of an agreement in the function to be performed, may be as truly and as correctly asserted to exist between artificial and natural productions, as between one object of the latter class and another. It is clear from this how much lower *Analogies* ought to stand in our estimation than *Affinities*. The latter form an essential part of that magnificent plan of Creation, which notwithstanding the amount of attention which Man has given to it, is of so transcendental a nature, that it may almost be said to be yet "to us invisible or dimly seen." Analogies, on the contrary, appear not to form any element whatever in the great System of Nature, but are merely examples of the recurrence of certain mechanical forms whenever the production of a certain mechanical action called for them; and so far from their being at or beyond the verge of human comprehension, we have seen that Man enjoys the high privilege of copying by these Analogies, at a humble distance, the far transcendent works of his Maker.

It would be an improvement in the language of Comparative Anatomy, if the term *analogous organs* were limited to the sense above defined. The serrations in the beak of a duck, for instance, are *analogous* in form and in function to *teeth*, but in their essential nature they are only a corneous modification of the *lips*. Most anatomists, however, would habitually say that the beak of a bird is *analogous* to the lips of a Mammal, though it must be evident how much more precise their language would become if they spoke of this *essential* relation as an *affinity*, and applied the word *analogous* to *formal* or *functional* relations only. A similar inaccuracy is committed by geologists in speaking of the *recent analogue* of a fossil species, meaning thereby that living species which has the nearest affinity to the extinct one. It would be more correct if they would term it the *recent affine*, or the *recent homologue*.

III. There is yet a third species of relation of structural similarity between organized beings which has usually been confounded with Analogy, but which appears to me to be distinct from it in kind, as well as far inferior to it in importance,—I refer to those cases where a resemblance in form or configuration exists, but without any perceptible identity either of essence or of function. Such, for example, are the resemblances between the flower of the *Bee Orchis* and a *Bee*, between the shell of *Murex haustellum* and a *Woodcock's head*, between a *Fungia* and a *Fungus*, *Ovulum* and an *egg*, *Haliotis* and an *ear*, &c. To this class also belong the numerous instances of *similarity of colour* between Birds whose affinities are remote, such as the resemblance of *Oriolus* to *Xanthornus*, of *Dicrurus* to *Corvus*, of *Cissopis* to *Pica*, of *Agelaius phœni-*

ceus to *Campephaga phœnicea*. Many errors of classification have been caused by mistaking these similarities for true affinities.

Not only are such cases of external resemblance unconnected with any agreement in the essential structures of the bodies compared, but there is no conceivable similarity in the functions which they are created to discharge. I think therefore that it is not going too far, nor departing from that veneration which the true naturalist will always feel for Nature's God, to call such superficial coincidences of form *accidental*. They seem to arise from the exuberant variety of the works of Nature which causes an occasional recurrence of similar forms, without any express design for such coincidences. Nothing can be inferred from such resemblances, either as to essential affinity or functional design; and they would almost have been beneath our notice, were it not that some authors have regarded them as examples of real analogies. The advocates of the Quinary theory of classification, who regard *Analogies* to be as important an element in the Natural System as *Affinities*, often speak of these mere resemblances in the light of true Analogies, and appeal to them in confirmation of their views. Regarding however, as I do, those views to be erroneous, I think it important that the distinction between *functional Analogy* and *mere resemblance* should be clearly pointed out; and to render the distinction more marked, I would distinguish the latter by the new term *Iconism*.

We must beware indeed of too hastily pronouncing an instance of resemblance to be an *Iconism*, merely because we cannot immediately detect any functional analogy. There may be real reasons for these resemblances, real agreements in the functions to be discharged, which we have not yet detected, and perhaps may never discover. A person might say, for instance, that the species of Mantis called the "walking leaf" presents a mere Iconism or accidental resemblance to true leaves; whereas it is highly probable that this very resemblance is given to the animal to enable it to remain concealed from its foes amid the verdant foliage. Such at least is undoubtedly the intention of numerous instances in which animals present an analogous colour to the surrounding surface, and even undergo corresponding changes with it, such as that of the Ptarmigan, which during summer is of a speckled gray plumage, like the lichen-covered rocks which it frequents, while in winter it becomes a pure white when those rocks are covered with snow.

I have now endeavoured to show that the relations of resemblance in organized beings are of three kinds, diminishing successively in importance; that *Affinities* are expressions of the real and elementary and esoteric Plan of Creation which the Author of Nature has been pleased to follow; that *Analogies* are coincidences of structure consequent solely upon an identity of external physical conditions; and that *Iconisms* are merely accidental recurrences of similar forms resulting from the exuberance of Nature's riches. It is evident that these distinctions must be clearly understood before we can make any progress in Natural History as a Science, and the remarks above offered may perhaps aid in drawing attention to the subject or removing the difficulties which surround it.

LVIII. *Abstract of Meteorological Observations made during the year* 1845 *at Gongo Soco, in the interior of Brazil. By* WILLIAM JORY HENWOOD, *F.R.S., F.G.S., Member of the Geological Society of France, Chief Commissioner of the Gold Mines of Gongo Soco and Catta Preta, &c. &c.*[*]

THE rich gold mines of Gongo Soco are situated in the province of Minas Geraës, about forty-eight miles north-west of the city of Ouro Preto[†] (Villa Rica), in long. 43° 30′ west and lat. 19° 58′ 30″ south, in a vale bounded on the north by the wooded mountain-range of Tejuco, and on the south by undulating grassy lowlands, which at the distance of about eight miles are terminated by the mountain-chain of the Caraças, which rises from 4000 to 5000 feet above the plain.

Barometrical measurements[‡] give Gongo Soco an elevation of about 3360 feet above the sea at Rio de Janeiro.

The thermometrical observations were made at such times as my occupations permitted, but the hours are probably not the best possible[§]. The midnight observations were made by Captains Blamey, Luke, and Guy, and the thermometer they used needed a constant correction of 2°·8 +; all the others are my own, and the thermometer I employed was a standard one (No. 89) of the British Association. The thermometer is suspended in a wooden box pierced with numerous holes, and hangs at about six feet above the ground, in a shed open at all sides, and is well protected,—as well from reflected heat as from the direct rays of the sun.

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[*] Communicated by the Author.

[†] Mr. Caldcleugh estimates the elevation of Ouro Preto at 3969 feet above the sea.—*Daniell's Meteorological Essays*, p. 345.

[‡] Made by the Austrian Mining Engineer, M. Virgil von Helmreichen.

[§] The observations at 4 and 8 P.M. give higher results than would have been afforded at 3 and 9 respectively.

Table I.

Hourly mean and extreme temperatures for every month.

	6 A.M.			9 A.M.			Noon.			4 P.M.			6 P.M.			8 P.M.			9 P.M.			Midnight.		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
January .	69°·8	64°·	66°·7	76°·2	65°·5	70°·	83°·	65°·	75°·5	84°·5	69°·	75°·9	80°·5	68°·8	73°·2	76°·8	67°·8	71°·	°......	°......	68°·5	71°·8	64°·8	67°·8
February	71·	60·8	67·2	74·6	63·	70·	81·	66·	75·4	84·	64·6	76·6	77·5	63·5	73·4	75·8	58·	71·	72·	58·	68·3	72·8	61·8	68·1
March ...	71·	62·	66·3	73·2	64·	68·6	78·5	67·	73·3	81·2	69·5	74·8	78·	67·	73·	74·2	65·	70·1	73·2	64·8	69·	70·8	60·8	66·5
April ...	68·	61·	65·3	72·5	63·	67·7	75·	65·	71·6	77·	64·5	72·5	75·	63·	70·	71·5	62·8	68·1	70·2	63·2	67·1	69·8	59·8	66·2
May......	68·	54·2	61·3	70·	61·8	65·4	75·	65·5	70·8	76·5	66·	71·5	73·	62·	67·5	69·	60·	64·3	66·	59·8	62·9	67·8	57·8	62·3
June ...	64·	47·5	52·9	65·	55·	60·	72·	60·	67·7	72·	66·5	68·5	66·5	58·	62·4	64·	54·	58·4	61·	54·5	56·3	62·8	50·8	55·4
July......	60·5	42·	54·6	67·	52·8	58·9	68·	60·	65·2	71·	62·	66·2	66·5	57·5	61·8	64·	52·2	58·1	60·2	51·5	56·2	65·8	48·8	55·2
August..	64·2	49·	58·	67·	57·	61·9	73·	61·	69·1	75·5	61·	70·7	71·6	60·	66·8	69·	58·5	62·9	65·8	57·	62·4	63·8	52·8	58·7
Sept. ...	65·2	49·9	59·4	70·	55·	63·	79·3	58·	69·9	81·	58·2	70·8	77·	57·8	67·8	71·	56·9	63·5	70·3	58·	62·8	68·8	53·8	60·2
October .	70·2	55·5	64·7	75·5	59·6	68·8	84·	64·	75·7	85·2	66·	76·3	80·	65·	73·2	74·4	62·	69·5	73·	61·8	67·5	70·8	56·8	65·8
Nov. ...	70·	61·	66·8	77·	62·6	71·1	85·	65·	77·3	87·	67·	77·8	84·	65·	74·9	76·5	64·5	71·7	74·	64·	70·4	72·8	58·8	67·5
Dec. ...	70·8	63·	66·2	77·5	67·	71·3	84·	67·	77·8	85·4	68·5	77·5	82·	67·8	76·	77·8	66·	71·4	74·5	64·4	70·	72·8	62·8	67·4

Table II.

Mean temperature of each month.

January	71°·07	May	65°·75	September ...	64°·67
February......	71·25	June	60·2	October	70·2
March.........	70·2	July	59·52	November ...	72·19
April	68·65	August	63·81	December......	72·2

Table III.—Mean temperature of each of eight hours.

6 A.M.	62·45	6 P.M.	70·
9 ...	66·39	8 ...	66·66
Noon	72·44	9 ...	65·11
4 P.M.	73·26	Midnight	63·42

The foregoing results give a mean temperature of 67°·46: as however the observations at 4 and 8 P.M. give higher results than would have been afforded if the observations had been made at 3 and 9 P.M. instead, and as we are without observations at 3 A.M., I consider the foregoing temperature about 2°·3 above the true mean, which in this case will be about 65°·14.

Circumstances have prevented my making many comparisons between the indications of the thermometer in the shade and when exposed to the direct action of the sun's rays. In the following instances, however, the instrument was suspended at about three feet above a surface of newly-turned garden-mould, of a deep red colour.

Table IV.

Date.	Therm. shade.	Therm. sunshine.	Remarks.
June 28, 3 P.M.	67·5	87·	Calm.
... 29, Noon	64·5	79·8	Calm.
Nov. 2, 2 P.M.	83·5	99·3	Light breeze E.
Dec. 29, 4 ...	82·2	97·2	Brisk breeze W.

My garden is a level spot of about one-third of an acre in area, and contains several orange and coffee trees, besides other shrubs; none of them, however, are very large; and the rain-gauge is placed on the ground at a distance from them all, in the centre of the garden.

Table V.—Quantity of rain.

	inches.
January	23·32
February	23·03
March	12·84
April	8·06
May	1·60
June	1·09
July	1·28
August	1·05
September	3·88
October	9·14
November	27·
December	12·46
Total in 1845	124·75

Although the rain is occasionally very heavy, I have seen none to compare with the results recorded by Prof. Forbes*. The heaviest showers I have seen, were

January 13, 6 P.M., when 1·12 inch of rain fell in 1 hour.
... 17, 2 ... ... 0·72 ... ... 20 minutes.
November 13, 4 ... ... 1·04 ... ... 17 ...
... 24, 5 ... ... 1·2 ... ... 25 ...
... 25, 2 ... ... 2·24 ... ... 1¼ hour.

The heaviest falls of rain during twenty-four hours were, February 22, when 3·92 inches were collected; November 26, when 3·76 inches were collected.

At the commencement of the wet season, heavy thunderstorms precede the rains for several days; they usually begin early in the afternoon, but generally pass off as evening approaches. As the season advances, they become daily later; and towards its conclusion, the time of their appearance is very irregular.

From April to August there is usually but little rain, and the continued drought enables the farmer to burn the dry stubble of his maize and beans, and to clear his grounds for tillage; for several weeks during August and September the atmosphere is filled with the smoke from these burnings; and at this time violent thunder-storms, with heavy showers, are frequent†.

For two or three weeks, about the end of January and the beginning of February, there is usually a cessation of rain and a continuance of unclouded sunshine (the *veronica*); but no such interval occurred in 1845‡.

I am unprovided with a barometer, hygrometer, and many other instruments necessary for a regular course of meteorological observation, as on leaving England my stay in Brazil was not expected to have exceeded a few months; and I have not obtained them since, as the requisite attention to them would interfere with the indispensable duties of my office. I have, however, a considerable series of observations on the

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* Reports of the British Association (1832), p. 252, and (1840) p. 113–116.

† I believe it has been long known that thunder-storms and rains follow the fires on the great prairies of North America, but I am unable to refer to my authority for the remark.

‡ A season seldom passes without heavy hail-storms in this province: during the early part of the wet season of 1844, I saw two such here; but during the present year we have had none, although there have been some of great severity in the neighbourhood.

intensity of terrestrial magnetism, but their reduction must await more leisure.

I am aware of the poverty of my remarks, and nothing but the scantiness of recorded observations in the interior of Brazil would have emboldened me to submit them to your readers.

Gongo Soco Gold Mines, W. J. Henwood.
January 16, 1846.

LIX. *On Pegmine and Pyropine, animal substances allied to Albumen. By* Robert D. Thomson, *M.D., Lecturer on Practical Chemistry in the University of Glasgow*[*].

THIS paper was written for a government report, detailing the results of an extensive series of experiments made on the influence of different kinds of food in feeding cattle during the course of 1845.

The report was drawn up last year, but has not yet been published. In reference to the reducing powers of the animal system, it is remarked that "there is only one instance with which physiologists are at present acquainted that could be adduced as evidence in favour of any substance being rendered more complex in the animal system, viz. the production of fibrine or flesh from curd or caseine. So far as chemical experiments carry us, we are not in a condition to affirm that no fibrine exists in milk; but it must be admitted that none has as yet been detected. If these be correct, then it would appear to follow that the infant fed on milk must derive its flesh from the curd of that fluid; and that as curd contains no phosphorus, while fibrine does, the curd of the milk in order to form muscular fibrine is united to phosphorus in the animal system, and is thus built up instead of being, as is the rule with other substances, reduced to a smaller number of elements. The objection to this view of the subject is, that the experiments which have been made on fibrine do not prove that it contains phosphorus. They only show that phosphoric acid can be detected in it even when it is purified in the most careful manner suggested by chemical knowledge, and it would therefore be somewhat premature to adopt any such analogy as that which we have been considering."

In support of the view first suggested by Beccaria and advocated in recent times by Prout, that the animal system merely modifies the substances which it employs as food, and does not produce them from its elements, a series of experiments made by the writer four years ago, may be quoted, hitherto unpublished, which demonstrate, that in the ocean, as

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[*] Communicated by the Author.

on land, the higher subsist on the lower animals, *because* the latter consist of the same materials of which the higher systems are composed. Without the lower animals, therefore, it is obvious the larger could not exist, and hence we may infer that the inferior organizations first peopled the earth, an argument opposed to the idea of some geologists, that animals have not been developed in succession. As it is well known that oysters serve as food for larger fishes, and these again for more powerful species, experiments were made to determine the composition of oysters, herrings, and haddocks, since it is highly probable that these prey on each other. Portions of these fishes were well-washed in water, to remove the oil and soluble matters; the white residue was then treated with alcohol and repeated digestions in æther. The resulting matter, which was considered to be pure fibrine, was found to have the following composition in the three species of fish when burned with chromate of potash:—

	Oyster.	Herring.	Haddock.
Carbon . .	53·98	53·77	53·67
Hydrogen .	46·02	7·44	7·00
Nitrogen . .		16·23	16·89
Oxygen . .			
Sulphur . .		22·56	22·44
	100·00	100·00	100·00

It is obvious, therefore, that fibrine can be obtained with the greatest facility and of the purest form from fish.

Can a substance be obtained from Albumen, &c. free from Sulphur?

In all of these kinds of fibrine, sulphur could be readily detected; nor was it found possible by any of the methods which have been hitherto described, to obtain either from fibrinous matter or from albuminous substances, a simpler body destitute of sulphur. The analysis of the milk detailed in a previous part of the report, afforded excellent opportunities of testing the accuracy of the idea supported by some of the continental chemists, that a substance can be obtained by the action of potash upon albuminous substances which contains no sulphur. On repeating the experiments that have been detailed in books upon a considerable scale with caseine or curd of milk, which were carefully conducted by William Parry, Esq., late of H.M. 4th Regiment, it was uniformly found that the resulting product contained sulphur. By this statement, certainly it is not meant to infer that such a substance may not exist, but only that the writer has not been able to procure such a substance as proteine by following most scrupulously the directions supplied by its original describer, and others who have copied his descriptions. His scepticism on this subject originated some years ago when engaged in researches on the brain, an abstract only of which has been published in Liebig's edition of Geiger's *Pharmacie.* The process of analysis for this intricate combination consisted in dissolving the albuminous part of the nervous system in dilute caustic potash; a reagent which produces no soluble power on the peculiar matter of the brain, but combines with it, forming an insoluble salt. The potash solution, on being withdrawn from the insoluble matters, yielded by neutralization with acetic acid, a substance which ought to have been *proteine*, because it was obtained by precisely the same process as that which has been described as the best for procuring that substance. But on dissolving after washing in potash, adding acetate of lead and boiling, it gave an abundant black precipitate, indicating the presence of sulphur. This experiment was shown to Prof. Liebig by the writer at the time (1842), and it is believed that that distinguished chemist considers the existence of proteine problematical.

PEGMINE.

About the same period (four years ago), the writer examined a product of the disease usually known under the name of the buffy coat of the blood, a coating of a buff colour, which usually exhibits itself on the surface of inflamed blood, and which has attracted much of the attention of writers upon pathological subjects. He found it to be a distinct body, and he has been in the habit of describing it in his lectures under the name of *pegmine* (from πῆγμα, *coagulum*). It partially dissolves by long-continued boiling in water, but may be washed in cold water, like fibrine, without undergoing any decomposition. It therefore possesses an equal right with fibrine to the character of a body *sui generis.* When dissolved in potash and precipitated with acetate of lead, and the liquid is boiled, a black precipitate of the sulphuret of lead falls. The following are the results of the analysis of this substance made in 1842, and which the writer has been in the habit of quoting in his lectures.

I. *Pegmine containing Fat.*

The first specimen was prepared by simply washing the buffy coagulum with repeated additions of cold water. It was taken from a patient affected with a violent attack of pleuritis. It is obvious from the analysis that it contained a considerable amount of fatty matter.

Carbon . . .	58·80
Hydrogen . .	8·44
Nitrogen . .	32·76
Oxygen . . .	
Sulphur . .	
	100·00

II. *Pure Pegmine.*

Another specimen procured from a different patient, also affected with an attack of inflammation of the membrane of the lungs, was treated with cold water, alcohol, and æther to remove all the fatty and oily matters mixed with it; when burned with chromate of lead, the following result was obtained:—

	I.	II.	Approximate true composition.
Carbon . .	52·07		52·07
Hydrogen . .	7·80	7·14	7·14
Nitrogen . .	14·00	14·40	14·20
Oxygen. . .	26·13		26·79
Sulphur . .			
	100·00		100·00

It is possible that the nitrogen is somewhat undervalued.

In the first analysis, the pegmine was dried at 212°, in the second at 300°. The same substance is met with in the inferior animals, especially in the horse, although not, it is believed, in the healthy state of that animal, as has been asserted, but in a similar condition of the animal to that in which it appears in the human subject—*inflammation.*

PYROPINE.

The only body which bears any resemblance in composition to the so-called proteine, is a beautiful substance which is found occasionally in the tusk of the elephant, occupying the hollow portion of the interior of that part of the animal. It possesses a fine ruby tint, and is sometimes tough, but when of the finest colour is brittle. Sections of it exhibit occasional traces of the remains of organization. It is insoluble in water, and thus differs from glue or gelatine, to which it has some affinities in its physical aspect. The writer has not been able to satisfy himself that it contains no sulphur, in consequence of its difficult solubility in caustic potash. The composition of pyropine by two analyses is as follows:—

	I.	II.
Carbon	53·33	53·50
Hydrogen . .	7·52	7·66
Nitrogen . . .	14·50	38·84
Oxygen . . .	24·65	
Sulphur . . .		
	100·00	100·00

These analyses were communicated to Prof. Liebig some years ago, and published by him in his edition of Geiger's *Pharmacie*, with the omission only of the nitrogen, which had not then been determined.

Liebig has suggested, with great plausibility to the writer, that this beautiful substance may be an altered form of blood, an idea which receives some support from the fact, that when pyropine is incinerated, it leaves 0·52 per cent. of a reddish ash,—a fact not sufficiently, perhaps, conclusive.

When pyropine is boiled in water, the liquid is not precipitated by infusion of nut-galls, a proof that it contains no gelatine or glue. Neither is it precipitated by acetate of lead. The colour of pyropine is not altered by this treatment, with the exception that a few scanty flocks of a membranous-looking matter floated about. When broken into coarse powder, it has a rich ruby colour; and hence its name (pyrōpus, a ruby). In fine powder it is brown; a minute portion of it dissolved in hot alcohol, and was deposited on cooling in the form of ferruginous flocks. The following formulæ would probably represent the relations of the preceding bodies to each other. They must, however, be considered as mere possible representations of their composition, calculated to exhibit the difference in reference to their increasing amount of oxygen.

Fibrine . . $C_{48}\,H_{39}\,N_6\,O_{16}\,Sx$
Pyropine . $C_{48}\,H_{39}\,N_6\,O_{16}\,S?$
Pegmine. . $C_{48}\,H_{39}\,N_6\,O_{17}\,Sx.$

In these formulæ, pyropine is represented as differing from fibrine in containing no sulphur, and pegmine from the preceding bodies by the presence of an additional quantity of oxygen.

The increased amount of oxygen in pegmine may be explained by the circumstance, that in inflammatory action respiration is more rapidly carried on, and in consequence a greater quantity of oxygen is introduced into the system than in the healthy condition of the body. In all cases of coagulation of blood in contact with oxygen, there is observable a light coloured portion situated on the surface of the coagulum, affording a proximate illustration of the production of the buffy coat.

LX. *On certain Definite Multiple Integrals.*
By the Rev. BRICE BRONWIN*.

IN the integrals treated of in this paper, let the limits of integration be given by the equation

$$\frac{x^2}{\alpha^2}+\frac{y^2}{\beta^2}+\ldots\ldots+\frac{\omega^2}{\lambda^2}=1, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (a.)$$

including both negative and positive values of the (n) variables. Let $P(r)=1.2.3\ldots r$; and for convenience let D, D_1, &c. stand for $\frac{d}{dg}$, $\frac{d}{dh}$, &c. respectively. The general term of the series expressing the value of

$$\psi=\iint\ldots\phi(g-x,\ h-y,\ \ldots\ldots)\,dx\,dy\ldots\ldots$$

is $\frac{D^{2p}D_1^{2q}\ldots}{P(2p)P(2q)\ldots}\phi(g,h,\ldots)\iint\ldots x^{2p}y^{2q}\ldots dx\,dy\ldots$,

the odd powers of x, y, &c. obviously vanishing. But by a well-known theorem, integrating for positive values of x, y, &c., and doubling the result; making $s=p+q+\ldots$, this term becomes

$$\frac{P\left(p-\frac{1}{2}\right)P\left(q-\frac{1}{2}\right)\ldots}{P(2p)P(2q)\ldots}\ \frac{\alpha\beta\ldots}{P\left(s+\frac{n}{2}\right)}(\alpha D)^{2p}(\beta D_1)^{2q}\ldots\phi(g,h,\ldots).$$

By another known theorem

$$P\left(p-\frac{1}{2}\right)=\frac{\pi^{\frac{1}{2}}}{2^{2p}}\frac{P(2p)}{P(p)},\ \text{\&c.};$$

and the above term is changed into

$$2^n\pi^{\frac{n-1}{2}}\alpha\beta\ldots\frac{P\left(s+\frac{n-1}{2}\right)}{P(2s+n)}\ \frac{(\alpha D)^{2p}(\beta D_1)^{2q}\ldots}{P(p)P(q)\ldots}\phi(g,h,\ldots).$$

And the sum of all the terms of the order s is

$$2^n\pi^{\frac{n-1}{2}}\alpha\beta\ldots\frac{P\left(s+\frac{n-1}{2}\right)}{P(2s+n)}\{(\alpha D)^2+(\beta D_1)^2+\ldots\}^s\phi(g,h\ldots)$$

$$=\alpha\beta\ldots f(s)\ \text{to abridge.}$$

Hence $\qquad \psi=\alpha\beta\ldots\Sigma f(s), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (b.)$

s having all integer values from zero to infinity.

Suppose $\phi(g,h\ldots)$ such that

$$(D^2+D_1^2+\ldots)\phi(g,h\ldots)=0.$$

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* Communicated by the Author.

Then, separating the symbols of operation from those of quantity,

$$D^2 + D_1^2 + D_2^2 + \dots = 0 \quad D_n^2 = -(D^2 + D_1^2 \dots + D_{n-1}^2),$$

$$D_n^4 = -D_n^2(D^2 + D_1^2 + \dots) = -(D^2 + D_1^2 + \dots)D_n^2$$
$$= (D^2 + D_1^2 + \dots)^2, \quad D_n^6 = -(D^2 + D_1^2 + \dots)^3, \text{ \&c.}$$

Therefore

$$\{\alpha^2 D^2 + \beta^2 D_1^2 \dots + \lambda^2 D_n^2\}^s = (\alpha^2 D^2 + \beta^2 D_1^2 \dots + \theta^2 D_{n-1}^2)^s$$
$$+ s(\alpha^2 D^2 + \beta^2 D_1^2 \dots + \theta^2 D_{n-1}^2)^{s-1} \lambda^2 D_n^2$$
$$+ \frac{s(s-1)}{2}(\alpha^2 D^2 + \beta^2 D_1^2 \dots + \theta^2 D_{n-1}^2)^{s-2} \lambda^4 D_n^4 + \text{\&c.}$$
$$= (\alpha^2 D^2 + \beta^2 D_1^2 \dots + \theta^2 D_{n-1}^2)^s$$
$$- s(\alpha^2 D^2 \dots + \theta^2 D_{n-1}^2)^{s-1}(\lambda^2 D^2 + \lambda^2 D_1^2 \dots + \lambda^2 D_{n-1}^2) + \text{\&c.}$$
$$= \{\alpha^2 D^2 + \beta^2 D_1^2 \dots - \lambda^2 D^2 - \lambda^2 D_1^2 \dots\}^s$$
$$= \{(\alpha^2 - \lambda^2) D^2 + (\beta^2 - \lambda^2) D_1^2 \dots + (\theta^2 - \lambda^2) D_{n-1}^2\}^s.$$

In this case therefore

$$\left.\begin{array}{l} \psi = 2^n \pi^{\frac{n-1}{2}} \alpha\beta \dots \Sigma \dfrac{P\left(s + \frac{n-1}{2}\right)}{P(2s+n)} \{(\alpha^2 - \lambda^2) D^2 \\ \qquad + (\beta^2 - \lambda^2) D_1^2 \dots\}^s \phi(g, h \dots) \end{array}\right\} . \quad . \quad . \quad (c.)$$

Change in this last $\alpha, \beta \dots \lambda$ into $a, b \dots l$, but so that

$$\alpha^2 - \lambda^2 = a^2 - l^2, \quad \beta^2 - \lambda^2 = b^2 - l^2, \text{ \&c.};$$

and let this change ψ into ϕ, we have obviously

$$\psi = \frac{\alpha\beta \dots}{ab \dots} \phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d.)$$

This is an extension of Laplace's theorem relative to the attraction of ellipsoids on a point exterior. And if α, β, &c., a, b, &c. be independent of g, h, &c., we have also

$$\left(\frac{d}{dg}\right)^m \left(\frac{d}{dh}\right)^n \dots (\psi) = \frac{\alpha\beta \dots}{ab \dots} \left(\frac{d}{dg}\right)^m \left(\frac{d}{dh}\right)^n \dots (\phi). \quad . \quad (e.)$$

To give an example or two, let

$$R = \{(g-x)^2 + (h-y)^2 \dots\}^{\frac{1}{2}},$$

and let R_1 stand for the same quantity when α, β, &c. are changed into a, b, &c.; then, since

$$\left(\frac{d^2}{dg^2} + \frac{d^2}{dh^2} \dots\right) \frac{1}{R^{n-2}} = 0,$$

n being the number of variables, we have

$$\iint \cdots \frac{dx\,dy \ldots}{\mathrm{R}^{n-2}} = \frac{\alpha\beta\ldots}{a\,b\ldots} \iint \cdots \frac{dx\,dy\ldots}{\mathrm{R}_1^{n-2}}. \quad . \quad . \quad (1.)$$

In the next two examples, by way of distinction, let x, y, &c. be changed into αx, $a x$, βy, $b y$, &c.; and let there be three variables,

$$\left.\begin{aligned}
&\iiint \frac{\alpha\beta\gamma\,dx\,dy\,dz}{\{(g-\alpha x)^2+(h-\beta y)^2+(k-\gamma z)^2\}^{\frac{1}{2}}} \\
&= \frac{\alpha\beta\gamma}{a\,b\,c}\iiint \frac{a\,b\,c\,dx\,dy\,dz}{\{(g-ax)^2+(h-by)^2+(k-cz)^2\}^{\frac{1}{2}}} \\
&= \iiint \frac{\alpha\beta\gamma\,dx\,dy\,dz}{\{(g-ax)^2+(h-by)^2+k^2\}^{\frac{1}{2}}} + \mathrm{A}c + \mathrm{B}c^2 + \&c. \\
&= \iint \frac{\alpha\beta\gamma\,dx\,dy\,\sqrt{1-x^2-y^2}}{\{(g-ax)^2+(h-by)^2+k^2\}^{\frac{1}{2}}} + \mathrm{A}c + \mathrm{B}c^2 + \&c. \\
&= \iint \frac{\alpha\beta\gamma\,dx\,dy\,\sqrt{1-x^2-y^2}}{\{(g-ax)^2+(h-by)^2+k^2\}^{\frac{1}{2}}}
\end{aligned}\right\}, \quad (2.)$$

the equation of limits for the first member being $x^2+y^2+z^2=1$, that for the second $x^2+y^2=1$. This result is obtained by developing into series relative to z, then integrating for this quantity, and lastly diminishing c without limit, the quantities A, B, &c. being finite. In the second member it must be observed, that since $c=0$, $a=\sqrt{\alpha^2-\gamma^2}$, $b=\sqrt{\beta^2-\gamma^2}$.

If we differentiate (2.) for (k), we have

$$\left.\begin{aligned}
&\iiint \frac{(k-\gamma z)\,dx\,dy\,dz}{\{(g-\alpha x)^2+(h-\beta y)^2+(k-\gamma z)^2\}^{\frac{3}{2}}} \\
&\qquad = \iint \frac{k\,dx\,dy\,\sqrt{1-x^2-y^2}}{\{(g-ax)^2+(h-by)^2+k^2\}^{\frac{3}{2}}}
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (3.)$$

By integrating the first members of (2.) and (3.) relative to z, we should obtain very singular results.

Let

$$\mathrm{R} = \{(g-x)^2+(h-y)^2+(k-z)^2\}^{\frac{1}{2}}, \quad \mathrm{V} = \iiint \frac{dx\,dy\,dz}{\mathrm{R}},$$

U the same integral when α, β, γ are changed into a, b, c. Make

$$\frac{\sqrt{(g-x)^2+(h-y)^2}}{\mathrm{R}} = \sin u, \quad \frac{g-x}{\sqrt{(g-x)^2+(h-y)^2}} = \sin v.$$

Then

$$x = g - \mathrm{R}\sin u \sin v, \quad y = h - \mathrm{R}\sin u\cos v, \quad z = k - \mathrm{R}\cos u,$$
$$dx\,dy\,dz = -d\,\mathrm{R}\sin u\,du\,dv;$$

or rather

$$dx\,dy\,dz = d\,\mathrm{R}\sin u\,du\,dv,$$

because (R) decreases while (x) increases. Therefore

$$\mathrm{U} = \iiint \mathrm{R}\,d\,\mathrm{R}\sin u\,du\,dv = \frac{1}{2}\iint \mathrm{R}^2 \sin u\,du\,dv.$$

Putting the values of x, y, and z in

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1,$$

and making

$$\mathrm{A} = \frac{1}{a^2}\sin^2 u \sin^2 v + \frac{1}{b^2}\sin^2 u \cos^2 v + \frac{1}{c^2}\cos^2 u,$$

$$\mathrm{B} = \frac{g}{a^2}\sin u \sin v + \frac{h}{b^2}\sin u \cos v + \frac{k}{c^2}\cos u,$$

$$\frac{g^2}{a^2} + \frac{h^2}{b^2} + \frac{k^2}{c^2} = 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (d.)$$

we find

$$\mathrm{R} = \frac{2\,\mathrm{B}}{\mathrm{A}}, \quad \text{and } \mathrm{U} = 2\iint \frac{\mathrm{B}^2}{\mathrm{A}^2}\sin u\,du\,dv.$$

Between $u=0$ and $u=\pi$, R will be positive, and then negative up to $u=2\pi$. We must therefore integrate from $u=0$, $v=0$ to $u=\pi$, $v=\pi$. If we leave out terms containing the first power of $\cos u$, $\cos v$, as these would give nothing in the value of the integral, we may make

$$\mathrm{B}^2 = \frac{g^2}{a^4}\sin^2 u \sin^2 v + \frac{h^2}{b^4}\sin^2 u \cos^2 v + \frac{k^2}{c^4}\cos^2 u.$$

Put

$$\left(\frac{g^2}{a^4} + \frac{h^2}{b^4}\right)\sin^2 u + \frac{2\,k^2}{c^4}\cos^2 u = m, \quad \left(\frac{h^2}{b^4} - \frac{g^2}{a^4}\right)\sin^2 u = n,$$

$$\left(\frac{1}{a^2} + \frac{1}{b^2}\right)\sin^2 u + \frac{2}{c^2}\cos^2 u = p, \quad \left(\frac{1}{b^2} - \frac{1}{a^2}\right)\sin^2 u = q;$$

and we have

$$\mathrm{B}^2 = \frac{1}{2}(m + n\cos 2v), \quad \mathrm{A} = \frac{1}{2}(p + q\cos 2v).$$

We may obviously integrate from

$$u = 0, \; v = 0 \text{ to } u = \frac{\pi}{2}; \quad v = \frac{\pi}{2},$$

multiplying the integral by 4. Therefore

$$\mathrm{U} = 16\iint \frac{m + n\cos 2v}{(p + q\cos 2v)^2}\sin u\,du\,dv = 8\pi\int \frac{mp - nq}{(p^2 - q^2)^{\frac{3}{2}}}\sin u\,du$$

$$=4\pi\int\left\{\frac{m+n}{p+q}+\frac{m-n}{p-q}\right\}\frac{\sin u\,du}{\sqrt{(p+q)(p-q)}}$$

$$=2\pi\int\left\{\frac{b}{a}\,\frac{c^4g^2\sin^2u+a^4k^2\cos^2u}{c^2\sin^2u+a^2\cos^2u}\right.$$

$$\left.+\frac{a}{b}\,\frac{c^4h^2\sin^2u+b^4k^2\cos^2u}{c^2\sin^2u+b^2\cos^2u}\right\}\frac{\sin u\,du}{\Delta},$$

where $\Delta=(c^2\sin^2u+a^2\cos^2u)^{\frac{1}{2}}(c^2\sin^2u+b^2\cos^2u)^{\frac{1}{2}}$.

If we transform this by making $\sin^2u=\frac{x}{c^2+x}$, and to abridge $\Delta=\sqrt{(a^2+x)(b^2+x)(c^2+x)}$, we find

$$\mathrm{U}=\pi abc\int\left\{\frac{g^2}{a^2}+\frac{h^2}{b^2}+\frac{1}{c^2}\,\frac{a^2k^2-c^2g^2}{a^2+x}\right.$$

$$\left.+\frac{1}{c^2}\,\frac{b^2k^2-c^2h^2}{b^2+x}\right\}\frac{dx}{\Delta},$$

the integral to be taken from $x=0$ to $x=\infty$. In this value of U, v has been taken in the plane of x and y. But if we make a and c, g and k, and then b and c, h and k change places, we shall have two other values of this quantity. Adding the three values together, dividing the sum by 3, and multiplying by $\frac{\alpha\beta\gamma}{abc}$, we find in virtue of (*d.*),

$$\mathrm{V}=\pi\alpha\beta\gamma\int\left\{\frac{2}{3}+\frac{\frac{1}{3}a^2-g^2}{a^2+x}+\frac{\frac{1}{3}b^2-h^2}{b^2+x}+\frac{\frac{1}{3}c^2-k^2}{c^2+x}\right\}\frac{dx}{\Delta}. \quad . \quad (4.)$$

As this expression is complicated, we will find V by another method. From the formulæ already given, we easily perceive that

$$\left.\begin{aligned}\iiint\frac{(k-z)\,dx\,dy\,dz}{\mathrm{R}^3}&=\iiint d\,\mathrm{R}\sin u\cos u\,du\,dv\\ &=\iint\mathrm{R}\sin u\cos u\,du\,dv=2\iint\frac{\mathrm{B}}{\mathrm{A}}\sin u\cos u\,du\,dv\\ &=\frac{4k}{c^2}\iint\frac{\sin u\cos^2u\,du\,dv}{p+q\cos 2v}=\frac{4\pi k}{c^2}\int\frac{\sin u\cos^2u\,du}{\sqrt{p^2-q^2}}\\ &=4\pi abk\int_0^{\frac{\pi}{2}}\frac{\cos^2u\sin u\,du}{\sqrt{(c^2\sin^2u+a^2\cos^2u)(c^2\sin^2u+b^2\cos^2u)}}\end{aligned}\right\} . \quad (5.)$$

And if we transform this by making $\sin^2u=\frac{x}{\sqrt{c^2+x}}$, it will become

$$2\pi a b c k \int \frac{dx}{c^2+x} \frac{1}{\sqrt{(a^2+x)(b^2+x)(c^2+x)}}.$$

We have therefore in the general case

$$\left.\begin{aligned} -\frac{dV}{dg} &= \iiint \frac{(g-x)\,dx\,dy\,dz}{R^3} = 2\pi\alpha\beta\gamma g \int \frac{dx}{(a^2+x)\Delta} \\ -\frac{dV}{dh} &= 2\pi\alpha\beta\gamma h \int \frac{dx}{(b^2+x)\Delta}, \\ -\frac{dV}{dk} &= 2\pi\alpha\beta\gamma k \int \frac{dx}{(c^2+x)\Delta}. \end{aligned}\right\} . \quad (6.)$$

If we multiply the first of these by dg, the second by dh, and the third by dk, and add them, we may certainly make the integral of the result

$$V = \pi\alpha\beta\gamma \int \left\{\lambda + 1 - \frac{g^2}{a^2+x} - \frac{h^2}{b^2+x} - \frac{k^2}{c^2+x}\right\} \frac{dx}{\Delta},$$

whether g, h, and k be implicitly contained in (6.) or not, if we suppose λ a function of g, h, k. Let us take the partial differentials of it relative to g, h and k, and equal them to their values given in (6.), we shall have, leaving out quantities which destroy one another,

$$\begin{aligned} \frac{d}{dg}\int \frac{\lambda\,dx}{\Delta} &= \int \frac{d}{dc^2}\left\{\left(1 - \frac{g^2}{a^2+x} - \frac{h^2}{b^2+x} - \frac{k^2}{c^2+x}\right)\frac{1}{\Delta}\right\} \frac{dc^2}{dg}\,dx \\ &= \int \frac{d}{dx}\left\{\left(1 - \frac{g^2}{a^2+x} - \frac{h^2}{b^2+x} - \frac{k^2}{c^2+x}\right)\frac{1}{\Delta}\right\} \frac{dc^2}{dg}\,dx \\ &= \frac{1}{abc}\left(\frac{g^2}{a^2} + \frac{h^2}{b^2} + \frac{k^2}{c^2} - 1\right)\frac{dc^2}{dg} = 0. \end{aligned}$$

In like manner,

$$\frac{d}{dh}\int \frac{\lambda\,dx}{\Delta} = 0, \quad \frac{d}{dk}\int \frac{\lambda\,dx}{\Delta} = 0.$$

Therefore $\int \frac{\lambda\,dx}{\Delta}$ is a quantity independent of g, h, k, and also of c. It must be remembered in this process that $a^2 = c^2 + \alpha^2 - \gamma^2$, $b^2 = c^2 + \beta^2 - \gamma^2$. But we cannot determine λ by making g, h, k and c infinite, for then both sides of the above equation will vanish independently of any particular value of this quantity. Nor is it easy to see how we can determine it from hence. But we see immediately from (6.) that

$$\begin{aligned} &\pi\alpha\beta\gamma \int \left\{\frac{a^2}{a^2+x} + \frac{b^2}{b^2+x} + \frac{c^2}{c^2+x}\right\} \frac{dx}{\Delta} \\ &= \frac{1}{2}\iiint \left\{\frac{a^2}{g}(g-x) + \frac{b^2}{h}(h-y) + \frac{c^2}{k}(k-z)\right\} \frac{dx\,dy\,dz}{R^3} \end{aligned}$$

$$=\frac{\alpha\beta\gamma}{2abc}\iint\left(\frac{a^2}{g}\sin u\sin v+\frac{b^2}{h}\sin u\cos v+\frac{c^2}{k}\cos u\right)d\mathrm{R}\sin u\,du\,dv$$

$$=\frac{\alpha\beta\gamma}{abc}\iint\left(\frac{a^2}{g}\sin u\sin v+\frac{b^2}{h}\sin u\cos v+\frac{c^2}{k}\cos u\right)\frac{\mathrm{B}}{\mathrm{A}}\sin u\,du\,dv.$$

Taking account of such terms only as give value in the final result, this reduces to

$$\iint\frac{\sin u\,du\,dv}{\mathrm{A}}=2\iint\frac{\sin u\,du\,dv}{p+q\cos 2v}=2\pi\int\frac{\sin u\,du}{\sqrt{p^2-q^2}}$$

$$=2\,abc^2\pi\int_0^{\frac{\pi}{2}}\frac{\sin u\,du}{\sqrt{(c^2\sin^2u+b^2\cos^2u)\,(c^2\sin^2u+a^2\cos^2u)}}$$

$$=abc\pi\int\frac{dx}{\sqrt{(a^2+x)\,(b^2+x)\,(c^2+x)}}.$$

Multiply this by the factor $\frac{\alpha\beta\gamma}{abc}$, which for convenience has been left out of the last steps, and we have $\pi\alpha\beta\gamma\int\frac{dx}{\Delta}$ for the result. By means of this result, (4.) becomes immediately

$$\mathrm{V}=\pi\alpha\beta\gamma\int\left\{1-\frac{g^2}{a^2+x}-\frac{h^2}{b^2+x}-\frac{k^2}{c^2+x}\right\}\frac{dx}{\Delta}. \quad (7.)$$

It seems not a little strange that this result should not by the direct method have come out at once. I might give many more examples in application of the theory given at the beginning of this paper; but the above may suffice, the subject being well understood.

Gunthwaite Hall, January 27, 1846.

LXI. *On the Storm-Paths of the Eastern Portion of the North American Continent.* By WILLIAM RADCLIFF BIRT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the opening articles of the first and second numbers of the second series of the American Journal of Science, Mr. Redfield has contributed some further important information to our present stock of knowledge respecting the storm-paths of the eastern portion of the North American continent and its adjacent seas. The chart No. 1, illustrating the articles, exhibits the tracks of sixteen hurricanes; eight of these form an interesting group distinguished by this striking characteristic: the paths are semi-orbital, presenting in each case the half of an ellipse of greater or less eccentricity; the apices are confined to the thirtieth parallel of latitude, and are more acuminated as they recede from the fiftieth degree of longitude west of Greenwich. The remaining eight depart considerably from this type: four appear in some measure to approximate to it, but most probably with very acuminated apices. A most remarkable one, of a parabolic form, with its apex on the twenty-sixth parallel, was observed in October 1837 (XV.). In October 1842, the north-western portion of an elliptic path with the apex still lower was traced (XIII.). The storm pursuing this path is particularly discussed in the continuation of Mr. Redfield's paper, and its identity with the northers of the Mexican coast insisted on. Mr. Redfield also speaks of other storms that had exhibited the character of northers in the Gulf of Mexico, and afterwards presented all the features of Atlantic storms. Speaking of a storm that occurred in October 1837, he says, "This norther of the Mexican coast had become in due course of progression an Atlantic storm" (see American Journal of Science, second series, No. 2, March 1846, p. 166). In October 1844, a storm passed nearly in a direct line from the Gulf of Honduras to Newfoundland (XIV.); and to the gale which is first discussed in the articles before us (XII.), a nearly direct westerly course has been assigned from all the observations that have come to hand.

It is not my intention in the present communication to enter into any examination of the particular gales above enumerated, or to attempt to substantiate or refute either the one or the other of the rival theories which have been offered as an explanation of the phænomena. Those of your readers who are acquainted with Col. Reid's work, are aware that he has most ably discussed the rotatory theory, and for the centripetal theory, I beg to refer to various papers in the American Journal of Science. I apprehend that the labours of Redfield, Loomis, and Espy in the United States, Col. Reid in England, and Piddington and Thom in India, have brought the inquiry to that point at which it becomes essential to connect it with some kindred branch of science, in order to see our way clear in resolving the interesting problems that suggest themselves, to strike out a path for working energetically in surmounting the obstacles that still retard our progress in becoming acquainted with the dynamical system of our atmosphere, and in successfully removing the desiderata as they arise.

Among the desiderata of these phænomena, and by far the most important, will be found their origin and final disappearance. Sir John Herschel has suggested, in his Report on

Meteorological Reductions*, that they may be produced by the crossing of two large atmospheric waves moving in different directions. Some interesting evidence of the existence of such atmospheric waves has been brought forward at late Meetings of the British Association. This evidence rests entirely on the barometric affections of the atmosphere over a large tract of country. In order, therefore, to extend our knowledge of the rotatory gale (Redfield) or the centripetal hurricane (Espy), especially with regard to the desiderata above-mentioned, I apprehend it will not only be important, but absolutely necessary to accompany all the observations of the direction, force, and variation of the wind with barometric readings. These readings, however, must not be confined to the mere period of the passing of the gale; evidence has been adduced of the passage of large atmospheric waves occupying from fifteen to seventeen days between the anterior and posterior troughs, or between successive crests; so that in order to detect the origin of a gale arising from the intersection of two waves, to trace it throughout its destructive course and to observe its final disappearance, it will be essential to discuss the entire system of observations appertaining to both waves, not only in time, but also in space. Our meteorological observations are approaching a degree of uniformity and system that bids fair for uniting these kindred inquiries. I have now before me Prof. Loomis's interesting and ably conceived charts for exhibiting the principal phænomena during the passage of two storms over the United States in February 1842, in which he clearly shows the barometric, thermometric, and anemonal phænomena, and exhibits in a very striking manner the extent of rain, snow, cloud, and blue sky over the whole of the United States twice in the course of each day that the storms prevailed. The barometric phænomena are shown by *lines of equal pressure*; and I apprehend that these lines of equal pressure indicate, especially in one of the two cases, that *two* waves passed over the United States; that as the posterior slope of one wave passed off, the anterior slope of a wave of a different system approached; and that in the point of intersection the storm raged. Should the entire barometric observations taken over the United States on that occasion, February 1 to 4, 1842, support the theory of atmospheric waves, I apprehend Sir John Herschel's suggestions will be partly realized.

I cannot close this notice without adverting to a most important desideratum in this interesting inquiry. Mr. Red-

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* Report of the British Association for the Advancement of Science, 1843, p. 100.

field's storm-paths are confined to the westward of the fiftieth meridian, and he is only able to give one-half of the ellipse in any case. Nearly the whole breadth of the Atlantic is a perfect blank in any storm-map. Surely several captains may be found who will be happy to make observations on their passages out and homeward, and transmit them to head quarters, either in London or some principal city in America; the only additional information required to that furnished by their logs, will be the altitudes of the barometer at given hours of the day and night, with its attached thermometer. The American steamers might furnish important and valuable information.

I have the honour to be,
Gentlemen,
Your very obedient Servant,
Cambridge Heath, April 22, 1846. W. R. BIRT.

LXII. *On the first introduction of the words* Tangent *and* Secant. *By* Prof. DE MORGAN*.

ABOUT the meaning and origin of the word *sine*, there is now no discussion. The words *tangent* and *secant*, though clear as to their meaning, have an origin which is not mentioned by historians. Nobody, in fact, knows where they came from; very few people care.

As it may appear surprising, and perhaps even doubtful, that the first inventor of names now so universally recognised should be unknown, I will begin by stating, that neither Weidler, Heilbronner, Montucla, nor Delambre, mentions the work in which the words first appear. Montucla does not allude to the question of the invention of these terms. Delambre does it, as follows, in a manner which shows that his impression on the subject was not founded on anything precise.

In speaking (*Astr. Moy.*, p. 437) of Vieta's mode of designating the trigonometrical functions, he informs us that Vieta, after stating that no elegant name had been given to what we now call the table of tangents, proposed to continue the use of the term *tabula fœcunda*, which had been given by Regiomontanus. "Ce qui," continues Delambre, "n'est pourtant pas plus élégant que le mot *tangente* qu'il réprouve; quant aux *sécantes*, il veut qu'on les appelle *hypoténuses des féconds*. Ces dénominations n'étaient pas faites pour être accueillies; on s'est décidé pour ce qui était plus naturel, plus commode, et même plus élégant, quoiqu'en dise Viète." Here it is plain that he thinks the word *tangent* to have been one of those which Vieta spoke of, when he threw his imputation of want

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* Communicated by the Author.

of elegance over all which existed. On looking at the paragraph of Vieta's *Liber Inspectionum*, which Delambre was then describing, we find not the smallest allusion to the word tangent, nor to any name for the table, except the old one of *tabula fœcunda*. So that Delambre must have proceeded upon a general impression, that the word tangent was in use in Vieta's time. But as he does not quote any authority for this impression, though much given to incidental allusion to one writer in his description of another, it is not necessary to give it any weight in the face of the positive evidence which I shall produce. I say this, because an impression on the mind of Delambre as to a usage deserves more consideration than the same thing in the case of any other mathematical historian. His memory might fail on an isolated fact, or his information might be incorrect on books which he had not seen; but he was occupied at each one time with masses of writers of one period, and came to each author fresh from that author's own contemporaries, and frequently from *very* close reading of them.

After writing the above paragraph, I happened to find a passage in a later writing (the *Responsorum liber octavus*, published in 1593) which might have left the above impression on Delambre's mind. Here Vieta distinctly names and objects to the words *tangent* and *secant*, and proposes to call the former *prosines* or *amsines*, and the latter *transsinuous* lines. Laugh if you will, he says, at the allegory of the Arabs (meaning the use of the word which is correctly Latinized by *sinus*), but either adopt it altogether, or reject it altogether. This passage strengthens the presumption, that when Vieta wrote the *Canon*, &c. he had never heard the words tangent or secant. The same impression on the part of Delambre occurs again in speaking of Pitiscus (*Astr. Mod.* ii. 33), when he says, "Il a eu le bon esprit de n'imiter ni Viète, ni Rhéticus; il a conservé les noms de sinus, de tangentes et de sécantes." But where either Rheticus or Vieta (in 1579) was to have found the last two names, we are never told.

The *Canon Mathematicus* of Vieta, to which the *Liber Inspectionum* above-mentioned is an appendix, was published in 1579. The work in which tangents and secants are first mentioned under those names, was published four years after.

Its author was Thomas Finck, of Flensborg in Denmark, who was successively professor of mathematics, rhetoric, and medicine at Copenhagen, where he died in 1656, at the age of ninety-five (*Aikin*, *Gorton*, and *Biogr. Univ.*): there are references to his astronomical observations in Tycho Brahé. The work we speak of was published at Basle, when he was a student there (and where two years before he had published an ephemeris) at the age of twenty-two. It is "*Thomæ Finkii Flenspurgensis Geometriæ Rotundi libri* xiiii. *Ad Fridericum Secundum, Serenissimum Daniæ et Norvegiæ Regem, &c. Cum Gratia et Privileg. Cæs. Majest. Basileæ, per Sebastianum Henricpetri*:" quarto. The colophon is "*Basileæ, per Sebastianum Henricpetri, anno salutis humanæ* M.D.LXXXIII. *Mense Augusto*." The work itself is well-worthy of description for its contents, independently of its being the production of so new a student. But I am here only concerned with the table of sines, tangents, and secants, and with the audacity of the young gentleman who presumed to alter the established names of the latter two. In page 73, after drawing the circle, &c., he thus proceeds:—"Erit AI tangens datæ peripheriæ. Sic vocare placuit quia sit perpendicularis extremæ diametro. ...... Geometria ipsa commodum suppeditavit nomen; nec aliunde adferri commodius poterit. Nam quod quidem numerum fœcundum rectam AI vocant, id ii videant quomodo defendant: mihi non probabunt. Damus aliquid peritissimo illi artifici Regiomontano homini Germano: qui primus hujus vocabuli author dicitur: damus etiam aliquid receptæ consuetudini. Verum id non facile damus ut verba ea in usu retineamus quibus elegantiora, breviora, significantiora, veriora habeamus."

At page 76 the secant thus makes its appearance:—"Sic peripheriæ AE secans est OEI, nempe radius OE continuatus in terminum tangentis E cum continuatione EI. Et hoc nomen huic recte accommodatum putamus. Joachimus Rheticus hypotenusam trianguli recti vocat respectu anguli recti ad A cui subtenditur. Verum cum referatur non ad angulum rectum, sed angulum in centro ad O, hoc est arcum AE: an non potius arcus secans dicatur quam recti anguli hypotenusa judicent alii. Maurolycus canonem Rhetici paulo mutatum in Messanensi Menelai editione, nomine etiam mutato edidit: et beneficum vocavit: tanquam recta OI seu numerus hanc definiens beneficus diceretur. At recta OI non magis est benefica, quam recta AI fœcunda." And again (p. 130), "Sequitur canonis hujus triangulorum pars altera quæ vulgo canon fœcundus, *nobis canon tangentium* dicitur: et canon hypotenusarum Rhetico, *nobis canon secantium* vocatur."

From these passages it is obvious that Finck gives the words for the first time, and defends them as suggestions of his own. How then does it happen that his right to them is entirely unknown? Did his learned contemporaries dislike to owe their terms to a youth of twenty-two years? or did the southerns wish to avoid acknowledging the young Dane?

The first after Finck who used the words tangent and secant was the celebrated Jesuit Clavius, whose edition of Theodosius*, with trigonometry and tables attached, was published, according to Lalande, in 1586. Blundeville, who copied these tables into his *Exercises*, and who is, as far as I can find, the first Englishman who gave complete trigonometrical tables, cites this date. These tables of Clavius are those of Finck, with the use of the terms tangent and secant; but Finck's name is entirely suppressed. Clavius does not mention the name of the condemned Protestant Rheticus: it is not surprising that he should have served the other Protestant, Finck, in the same way. I do not suspect Clavius of wanting to pass the work of another as his own; he mentions Regiomontanus and Purbach freely enough, and excludes none but persons whom a reputable Jesuit could not name, as Protestants and Copernicans. But I proceed to make good my assertion.

The tables of Clavius are to the same radius and interval as those of Finck. The sines are avowedly from Regiomontanus: the latter gave differences to every ten seconds; Clavius does the same. But Finck gave no differences: Clavius gives no differences to his tangents and secants. In the tables of this period, the tangents and secants in the last degree were often very wrong, having hardly one of what we call the decimal places right. Clavius agrees with Finck in every decimal place, and differs from Vieta and what had *then* been published by Rheticus. For instance, we take the tangent and secant of 89° 50′.

	Tan 89° 50′.	Sec. 89° 50′.	Date.
Correct value	343·77371...	343·77516...	
Vieta	343·77371...	343·77516...	1579
Rheticus	343·7829002	343·7843784	1551
{ Finck..................	343·7829002	343·7843546 }	1583
{ Clavius	343·7829002	343·7843546 }	1586

And it is the same throughout: whenever Finck differs in two or three decimal places from Rheticus, so does Clavius in the same manner. And whereas Vieta and Rheticus have the semiquadrantal form, Clavius agrees with Finck in retaining the quadrantal form.

There is a particular reason why Finck should differ from Rheticus in the secants. The former used the reciprocal of the cosine carried to more figures than it would give truly; the latter demonstrated the formula

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* I have before me the tables in the complete edition of Clavius's works, and have never seen the original edition.

$$\sec\theta = \tan\theta + \tan\left(45^\circ - \frac{\theta}{2}\right),$$

and used it. Clavius gives the same demonstration of the same formula. There is then no doubt that the celebrated tables of Clavius, the first (I believe) introduced into this country, are no other than the tables of Finck, deprotestantized by the substitution of the name of Clavius for that of Finck. If our Elizabethan mathematicians had known this, they would not have let it pass unnoticed.

The tables of Clavius were copied by Lansberg and Magini (1591 and 1592), both of whom omit all mention of Finck, though the second gives a list of the names which his several predecessors gave to the trigonometrical lines. So completely did the last name disappear from history, that it is not mentioned in its proper volume of Delambre (the *Astron. Moyenne*), except in the index, in which it is stated, speaking of the tables of secants, that "Rhéticus l'a étendue d'abord à toutes les minutes; et c'est ainsi que Finckius l'a reproduit en 1583 en citant Rhéticus mort en 1574." This is incorrect; Rheticus *published* nothing closer than to ten minutes, and Finck, we may suppose, could not have found out Valentine Otho and the manuscripts at the age of twenty-two; he would have made his final secants more correct if he could have done so. When he cites Rheticus, it is for the name which the latter adopted, not for any mode of calculating; and as I have stated, he made his own secants by his own method.

This work of Finck is so clear and concise, and so much above the usual writing of the sixteenth century, that its author ought to rank very high among the secondary authors of that period.

Being engaged in an attempt to trace the early progress of trigonometrical tables in England, I annex the results which I have obtained, in the hope that some of your readers may be able to furnish additional information. I am not aware that any one has ever investigated the point.

Thomas Digges and John Dee, in their several works on the new star in Cassiopea (both published in 1573, and in Latin), mention and use sines, but refer to foreign tables; Digges to the ten-minute canon of Rheticus, Dee to Regiomontanus. William Burroughs, in his tract on the Variation (written in 1581), mentions and uses tables of sines, but describes the doctrine of "signes and triangles" as new and strange to English ears. He professes his intention of interpolating the ten-minute canon, and publishing it, if his purpose be not forestalled by other publications. This he did not do; and to the edition of 1614 the editor appended the tables which Ralph Handson had published in his translation of the Trigonometry of Pitiscus (of the first edition of which I have not found the date). The first sines actually published, as far as I can find, were those at the end of Thomas Fale's *Horologiographia*, first published in 1593; they are to minutes, with a radius of 100000. The first complete canon which I can find is that of Blundeville, in his Exercises, first published in 1597. They are taken from Clavius, and are to every minute, with a radius of ten millions. These Exercises went through seven editions at least, and were latterly corrected from Pitiscus. John Speidell, well-known afterwards for his logarithms, published a small table in 1609, to every ten minutes, and to a radius of 1000. Briggs began his calculation of sines about 1600, in ignorance, we may suppose, of the appearance of the *Opus Palatinum* four years before. This is all I have been able to find on the matter.

There is a work expressly on the history of the trigonometrical canon, which is sometimes cited by foreign writers; it is by Frobesius, and was printed at Helmstadt about 1750; but I cannot find any copy of this work in London.

LXIII. *Complete Collection of* Kepler's *Works.*

By Dr. J. LHOTSKY,*

PROFESSOR Frisch of Stutgardt has recently published a programme of his intended collection of the works of the great German astronomer and philosopher, which, like a splendid luminary, will enliven the dim polygraphy of the present age. Considering Kepler as the real founder of modern astronomy, the collecting of his works (many of them very rare) is a tribute most due to such great merit. Our present epoch seems especially adapted to such an undertaking. Monuments are everywhere raised to the honour and memory of men who have deserved well of humanity or of their country, in one or another department of human knowledge or enterprise. In following up the track marked out by Kepler in astronomical science, a degree of accuracy and perspicuity has been acquired, which is one of the proudest trophies of the human mind. In such a time, it is impossible that the claims and memory of Kepler could be kept in abeyance any longer. He, the modest searcher and deep thinker, ought to obtain his share of attention and general recognition, so long with-

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* Communicated by the Author.

held from him,—whose discoveries have been confirmed in so splendid a way.

Something, it is true, has been done in the way of atonement towards his long-neglected memory; and it is now thirty-seven years since over his grave at Regensburg (his native city), the image of the great man was placed under the cupola of a monument, on the anniversary of his birth-day, amid the roar of cannon. It cannot, however, be said that this slight token of gratitude could suffice to the memory of one whom his coevals left in misery and distress, while occupied with the examination of the very innermost secrets of science; while at the same time the produce of a painful and thorny life, his splendid works, remained unknown and forgotten. It is true, his name is on the lips of every astronomer and philosopher; the three great *Keplerean laws* are yet the main basis of the knowledge of the heavens; still his works moulder in dust and oblivion. On the other hand, it must be acknowledged that the present position of mathematical science is far different from what it was in the seventeenth century; and problems are now solved with facility and speed which then occasioned much labour; and many a deep axiom and saying of Kepler will be more easily appreciated if presented to the reader in a more modern garb. But it is not merely the contents, but even the *form* and style of the immortal astronomer's works, which imparts value to them; and it requires but little attention to become familiar with that form, albeit hidden and enigmatic.

There are two reasons, however, which have hitherto prevented the greater spread of Kepler's works,—their *rarity* and their *external appearance*. There is hardly a library in Europe where *all* the works of Kepler are to be met with, and many where even the most important are wanting. The cause is obvious. In the then condition of typography and publishing, only a few copies could be printed, and of those many were lost in conveying them about the country and by other accidents. Thus, for instance, Kepler's *Harmonie* and *Astronomia Nova* are so scarce, that only the largest libraries can boast of their possession. The next cause of the neglect of our author's works, is the wretched type, bad paper, and the improper size of many of them. The figures, moreover, are so badly designed, and the letters thereon so indistinct, that they cannot be read without difficulty. These reasons will be deemed sufficient for making a *collection of Kepler's works.* It was the late Prof. Pfaff at Eslingen, who, in 1810, first conceived this design, which, however, did not come to maturity. Still, constant communications with this gentleman kept the conviction of the importance of such an undertaking alive, and a further investigation of the great work increased the interest for the old astronomer and philosopher. The present editor, however, Prof. Frisch, did not long enjoy the assistance of either Pfaff, or of the great philologist, Prof. Kopp, both being carried off by premature death. The labour was subsequently much aided by the head librarians of Stutgardt and Tubingen, who defrayed the preparatory expenses of the undertaking. Of especial use also was the library of Reütlingen, which contains a very complete collection of the mathematical works of the sixteenth century. All this must of necessity have led to the inquiry after the original MSS. of Kepler, a notice of which is to be found in Murr's *Journal für Kunstgeschichte und allgemeine Literatur*, vols. iii. and xvii. It is to the following effect: — Kepler's son Ludwig, who died in 1663, had preserved the MSS. of his father with the intention of publishing a selection from them. In fact, there appeared in 1634, through his endeavours, a work which, however, the father had prepared for the press, viz. *Somnium sive de Astronomiâ lunari.* All the rest remained unused, most probably because Ludwig, who was a physician, could not understand the problem of his great progenitor. From him the MSS. went to the celebrated Selenographer Hevel, and thence to his son-in-law, the common-councilman Lange at Dantzig. Of Lange they were bought in 1707 by the Leipzig mathematician, Hansch, for 100 florins. They consisted of twenty-two folio volumes, which, besides the drafts of several works already printed (for instance, the *Harmonie*, the Rudolphine Tables, &c.), contained the correspondence of Kepler with many distinguished personages, several astronomical works merely begun, and a host of miscellaneous notices. Hansch intended to publish these MSS. in a splendid form, but was only able to begin this undertaking (too costly in the form he had projected it), and the *Epistolæ ad Keplerum scriptæ, insertis ad easdem responsionibus Keplerianis*, which was patronized by the emperor Charles VI., was the only result he ever achieved. The MSS. thus published, are to be found in the Imperial Library of Vienna. As Hansch fell into poverty, he was obliged to pledge the MSS.; and as he could not redeem them, one Etringer, of Frankfort on the Maine, redeemed them for 128 florins. Thence they came (probably by inheritance) to a Mrs. Trümmer at Frankfort, where they remained unknown until 1770, when Christopher de Murr, a man deserving well of literature, called attention to them. For the purpose of recovering them from oblivion, he addressed himself to several astronomers, as Mayer, Bernouilli, Kästner, &c.; but besides laudatory commendations on his undertaking, they could not afford any substantial aid. Consequently Murr sent the catalogue of the MSS. to St. Petersburg, and solicited Euler for his intercession. On the recommendation of the latter and other savants, they were purchased in 1774 for the St. Petersburg Imperial Academy. The academicians Euler, Krafft, and Lexell received orders to peruse the MSS. and to select those worthy of publication. Lexell began the revision of a nearly completed work of Kepler's, on the motion of the moon, entitled *Hipparchus*; but there it ended, and neither the work nor the promised completion ever saw the light. These MSS. have ever since reposed, as Prof. Krafft writes, "an ornament of the Petersburg Library,"—useless, unknown. Prof. Frisch took great pains to obtain these MSS. Introduced by Baron de Regendorff, Russian Minister at Stutgardt, and Prof. Schelling, he addressed himself to the imperial government, and received the assurance that the use of them would be granted to him; and from the known scientific munificence of the Petersburg cabinet, it is to be hoped that his further request for the loan of the MSS. will be shortly granted.

As to the plan on which the works are to be edited, Prof. Frisch makes the following statement:—The original text will remain unchanged, except where palpable error has crept in, as it is intended that Kepler shall appear throughout in his truest form. The notes will be as few and concise as possible, in order not to increase the bulk of the work; they will treat either on historical points, or explain difficult passages. As most of the works of Kepler are in Latin, the adoption of that language for the notes has been deemed expedient. The introduction which will precede the whole is to contain a survey of the condition of mathematical and natural science in the century preceding the life of Kepler, and to this will be attached his biography, mostly relating to his scientific labours. But as it is possible that some of the MSS. at St. Petersburg may contain materials for the elucidation of this subject, the compiling of it will be deferred until it shall be ascertained whether permission will be granted for the use of them.

The works will, as far as possible, be printed in the order in which Kepler composed and published them. But those relating to chronology will be put together, and those consisting mostly of numerals, as the Ephemerides, the Rudolphine Tables, and the work on Logarithms, will form the last part of the collection. Prof. Frisch concludes his programme by calling upon all friends of science to aid him in an undertaking the difficulty of which he is perfectly aware of. Every communication made to him will be received with thanks. The owners of MSS. of Kepler especially might greatly assist the undertaking by the loan of them, or the transmission of accurate copies. Even of the printed works of Kepler, Prof. Frisch has not been able to see the following:—

1. Almanack of the Year 1594*.
2. *De Fundamentis Astrologiæ*, Prague 1602.
3. *Epistola ad rerum cœlestium amatores de Salis deliquio*, Prague 1605.
4. *Dissertatio cum nuncio sidereo, &c.*, Florence 1610, 4to. [The edition of Prague 1610, 4to, and Frankfort 1611, 8vo, are at hand.]
5. *Responsio ad Epistolam Bontschii*, Sagan 1609.

In conclusion, Prof. Frisch requests the loan, or offers the purchase of any of these works of Kepler.

A considerable time having elapsed since the programme of the Stutgardt Professor was published, the aim of which undoubtedly was to excite attention in, and to obtain aid also from, *this* country, which has associated itself in every useful undertaking all over the world, I have now been induced to take up this subject, as my studies at Prague had led me often in the very track of the life of Kepler, who remained eleven years in that city. But the attention of an English public cannot and ought not to be called for without searching for some *connecting link* between the activity of Kepler and those of English savants. A brief search, indeed, showed me that such existed, and I have been able to complete and correct the programme of Prof. Frisch. If we refer to the names of the various possessors of Kepler's MSS., we find that it was Hevel who obtained them from Kepler's son, and we find the number of volumes or fasciculi to have been twenty-two. It was not to be supposed that the Stutgardt Professor should have known the long array of volumes composing the Philosophical Transactions of the Royal Society, else he would have found a letter of that very Hevel addressed to the Society (or their publisher) on these very MSS., in which they are called the famous Kepler MSS. (vide Phil. Trans. vol. ix. No. 102, p. 27, anno 1674). The title of this remarkable communication is as follows:—"An Extract of M. Hevelius's Letter, lately written to the publisher, concerning the famous Kepler's MSS., together with some Observations of his about the

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* This is the first thing Kepler printed at Gratz. It is very probable that a copy of this work may exist safe in some neglected collection of rubbish.

Use of Telescopic Sights in Astronomical Observations." In looking over this letter, we find that the number of fasciculi in Hevel's possession was not twenty-two, as stated (or at least implied) by Prof. Frisch, but twenty-nine fasciculi. One thing, moreover, is certain, that Hevel must have possessed all the MSS.; deriving them from a source where certainly they had received the highest possible attention. In perusing, therefore, the catalogue of these MSS., as given by Hevel in his letter to the Royal Society, we shall find that Prof. Frisch's hopes or expectations as to what the St. Petersburg documents may contain, can be at once answered, because Hevel could never have had *less* than what is at St. Petersburg *now*; though he might have had more, which, however, would make the case worse. A very short time, in fine, will now set the matter at rest. In regard to Kepler's life, Hevel says as follows:—"At Kepleri vitam studio conscriptam non invenio; interim plurima notatu dignissima, vitam ejus spectantia passim notavi, ex quibus vita ejus possit haud obscurè depingi. Quæ verò in specie ex scriptis ejus penes me habeo, catalogus hicce indicabit" (Ibid.). Whatever fates these MSS. might have subsequently undergone, Hevel's Catalogue must ever be considered the most complete. The extract from Hevel's letters to the Royal Society (as printed in the Phil. Trans.) does not imply any especial offer or request. But there exists among the MSS. of the *British Museum*, another document relating to these MSS., and this is an *autograph* letter of Hansch to the Royal Society; it is dated Vienna, November 20, 1734. There Hansch speaks of twenty-two fasciculi, and as this was written *after* his *Epistolæ ad Keplerum* were published, it may be presumed that seven might have merged (whether entirely or partially is not known) in this undertaking. Hansch's letter contains *also* a list of the Kepler fasciculi, but they seem to have been re-arranged, as the contents of most, as given by Hevel and Hansch, do not correspond. It is, moreover, curious that Prof. Frisch says that the *Epistolæ ad Keplerum* were all that Hansch published, while the letter in the British Museum says, "Præter Epistolas, quæ in folio charta augusta prodierunt, et Librum singularem de Calendario Gregorio quem Ratisbone 1726—in folio pariter typis imprimendum curavi—reliqua MSS. REGIAM desiderant munificentiam." Hansch's letter is entirely lachrymose and supplicatory; and it is a pity to perceive that these MSS. had something ominous in them, as not only their author, but even several of their subsequent owners fell into deep distress.

Having been so far successful in my research, I resolved to see whether some of the works of Kepler, which Prof. Frisch could not discover in Germany, might not be found in our libraries, which certainly are surpassed by the richness of *especial* departments of those of the *respective* countries; and none would expect, for instance, to find more Austrian Incunabula in English libraries than there are in Vienna, &c. But taking the biographical opulence at a fair average, the balance will not be unfavourable for this small and insulated empire. The very first glance I cast in the Catalogue of the British Museum (even in its present transitory state) was encouraging, as I found No. 5 of Prof. Frisch's *Desiderata*. The full title of this little rarity is as follows:—"*Joannis Kepleri Mathematici ad Epistolam Clarissimi Viri D. Jacobi Bartschii Laubani Medicinæ Candidati Præfixam Ephemeridi in anno* 1629 *Responsio: de Computatione et Editione Ephemeridum, Typis Saginensibus* 1629." It is a small 4to pamphlet of only eleven pages, printed on paper and with a type of the then current publications of the day. The conclusion is so characteristic of the man, that we shall translate it:—"But while the storm is raging, and the shipwreck threatens public affairs, nothing remains to us but to let the anchor of our innocuous studies go down to the profound of eternity! Given at Sagan in Silesia, with our own types, anno 1628." It is known that Kepler had been in some relation with the great Wallenstein, and the place of printing is one of the possessions of the great warrior, he having been Duke of Sagan. The name of the duke is also mentioned in the contents of the work.

London, April 15, 1846.

LXIV. *On the Aberration of Light, in Reply to* Mr. Stokes. *By the* Rev. J. Challis, *M.A., Plumian Professor of Astronomy in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAD reason to expect, when I made my last communication on the Aberration of Light, that I should not have occasion to trouble you again on this subject. Mr. Stokes's remarks in the April Number compel me to say a few words more.

I can assure Mr. Stokes that I take the aberration of light in its usual acceptation, and I have no doubt that he does also. The difference between us is not in the thing explained, but in the *principles* of our explanations. My explanation, which is very simple and brief, being entirely contained in page 91 of the February Number of the Philosophical Magazine, does not even suppose the existence of an æther. On the contrary, Mr. Stokes's rests both on the hypothesis of an æther and on a gratuitous and very particular supposition respecting its motion. By Mr. Stokes's admission, I have shown on my principles, that if to the earth's way, as measured by an astronomical instrument, be added in the same plane an angle equal to the product of the ratio of the earth's velocity to the velocity of light and the sine of the earth's way, we obtain the direction in which light from a star progresses just before it enters the eye. By measures taken with astronomical instruments, it is found that if to the same angle in the same plane be added the product of 20″·42 and the sine of the earth's way, the mean place of the star is obtained. [The numerical quantity is that adopted in the British Association Catalogue of Stars.] Now it happens that the ratio of the earth's velocity to the velocity of light is known independently of the above-mentioned measures, by observations of the eclipses of Jupiter's satellites. Delambre states (*Abrégé d'Astronomie*, p. 493), that by very exact and extensive researches on the satellites of Jupiter, he found for this ratio 20″·25. The close approximation of these numerical values justifies me in concluding that the light from the star enters the eye, *quam proxime*, in the direction of a line drawn to the eye from the star's mean place; or, in Mr. Stokes's notation, that s_2 coincides very nearly with s. Mr. Stokes appears to be dissatisfied because this inference is not deduced by theory alone. I conceive that it is not the less certain because it is deduced from *facts*; and as Mr. Stokes does not contend that it is not *true*, I need say no more on this point.

The "confession" which Mr. Stokes says that I made, I am ready to make again. I allow that, anterior to the above comparison with the result of astronomical measures, it could not be anticipated that aberration would be wholly accounted for by the motion of the earth and the finite velocity of light, without reference to any theory of light. The comparison shows that it *is* so accounted for, and the inevitable consequence is, that any explanation which rests on a hypothetical motion of the æther, must be *fictitious*.

I really think that I have now said quite enough in defence of a very unexceptionable piece of reasoning, and if Mr. Stokes should have anything further to urge, I must decline answering it.

I am, Gentlemen,
Your obedient Servant,
J. Challis

Cambridge Observatory,
April 11, 1846.

LXV. *On the Finite Solution of Equations.*

By James Cockle, *M.A., Cantab.; Special Pleader*[*].

[The subject concluded from p. 191.]

15. LET $\lambda', \lambda'', \ldots, \mu$ be n unequal integers, then it might be shown[†] that x^{μ} equals

$$p_0 + p_{\lambda'} x^{\lambda'} + p_{\lambda''} x^{\lambda''} + \&c.; \quad . \quad . \quad . \quad . \quad \text{(ae.)}$$

and, hence, that[‡] $\Lambda''' x^{\lambda'''} + \Lambda^{iv} x^{\lambda^{iv}}$ may be reduced to the same form (ae.). Consequently its second and third terms will amalgamate, respectively, with the first and second terms of the right-hand side of (a.) (thus becoming unavailable), and its only effective part is

$$p_0 + p_{\lambda^{v}} x^{\lambda^{v}} + \&c. \quad . \quad . \quad . \quad . \quad . \quad \text{(af.)}$$

If, therefore, the number of terms in (af.) be < 2, we shall have (*sup.* p. 132),

$$b'' = a''_2 p_{\lambda''}, \quad b' = a'_1 p_{\lambda'} + a_1'' p_{\lambda''}. \quad . \quad . \quad . \quad \text{(ag.)}$$

16. In general, then, the transformation (b.) of that page can be effected for equations of the FOURTH degree without the necessity of fulfilling (ag.); but in *critical*[§] cases we are limited to the FIFTH and higher degrees, since p_0 disappears. On this account biquadratics cannot be reduced to a binomial form, as we might otherwise have inferred[||], for in such case we have, ultimately, to satisfy two homogeneous equations between two quantities of the form $\Lambda + p_{\lambda}$.

17. So, beyond all doubt, the transformation (o.) of p. 190 can, in general, be effected for equations of the SIXTH degree, without satisfying (ag.) by means of *one* cubic, two quadratics, and five base equations. But in critical cases we meet with the same obstacle as that mentioned in the last paragraph, and are limited to the SEVENTH and higher degrees; so that the solutions of equations of the fifth and sixth degrees present distinct difficulties[¶]. If they are absolutely insoluble, may we not hope, from a consideration of the modes in which they evade different proposed methods of solution, to arrive at a more elementary demonstration of the fact than has yet appeared?

On the Reduction of certain Functions.

In those cases, in which the length of the calculations is

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[*] Communicated by T. S. Davies, Esq., F.R.S. and F.S.A.

[†] *Sup.* p. 191, Note *.

[‡] *Sup.* p. 132.

[§] *Sup.* p. 191, par. 12.

[||] *Sup.* p. 133, par. 5.

[¶] See Sir W. R. Hamilton's "Inquiry" (cited *sup.* p. 191, Note *), p. 298, line 25, and p. 317 [9.].

not such as to render the following reductions of merely theoretical interest, we may develope the formulæ for τ, γ[*], and such other symbols as may be requisite, so as to render the operations uniform and comparatively easy. The reductions I allude to, all of which are in theory possible[†], are those of

$$f^3(3\,.\,2^m-2) \text{ to } h_1{}^3+h_2{}^3+.+h_m^3$$
$$f^4(u_m) \text{ to } h_1{}^4+h_2{}^4+.+h_m^4,$$
$$f^4(v_m) \text{ to } h_1{}^2+h_2{}^2+.+h_m^2,$$

where h_r is, in general, a linear though not homogeneous function of $m-r+1$ undetermined quantities,

$$u_{x+1}=3\,.\,2^{2u_x+1}-2;\quad v_{x+1}=3\,.\,2^{2v_x}-1;\quad u_0=v_0=1;$$

and $f^a(b)$ denotes the general function of the ath degree and bth order[‡], according to the notation which I used at p. 126 of the last volume.

Devereux Court, March 31, 1846.

LXVI. *Experimental Researches in Electricity.—Twentieth Series.* By MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Cor. Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.*[§]

§ 27. *On new magnetic actions, and on the magnetic condition of all matter*[||].

¶ i. *Apparatus required.* ¶ ii. *Action of magnets on heavy glass.* ¶ iii. *Action of magnets on other substances acting magnetically on light.* ¶ iv. *Action of magnets on the metals generally.*

2243. THE contents of the last series of these researches were, I think, sufficient to justify the statement, that a new magnetic condition (*i. e.* one new to our know-

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[*] *Sup.* p. 190. See also one of my previous papers in the Phil. Mag. S. 3. vol. xxvii. pp. 292, 293.

[†] See Mathematician, vol. ii. p. 97. Ex. xlix. for a discussion of the first reduction.

[‡] The order being the number of undetermined quantities, the degree the dimensions to which they enter. Might not the term 'simple' be advantageously applied to all equations of the first order?

[§] From the Philosophical Transactions for 1846, Part I., having been read December 18, 1845.

[||] My friend Mr. Wheatstone has this day called my attention to a paper by M. Becquerel, "On the magnetic actions excited in all bodies by the influence of very energetic magnets," read to the Academy of Sciences on the 27th of September 1827, and published in the *Annales de Chimie*, xxxvi. p. 337. It relates to the action of the magnet on a magnetic needle, on ledge) had been impressed on matter by subjecting it to the action of magnetic and electric forces (2227.); which new condition was made manifest by the powers of action which the matter had acquired over light. The phænomena now to be described are altogether different in their nature; and they prove, not only a magnetic condition of the substances referred to unknown to us before, but also of many others, including a vast number of opake and metallic bodies, and perhaps all except the magnetic metals and their compounds: and they also, through that condition, present us with the means of undertaking the correlation of magnetic phænomena, and perhaps the construction of a theory of general magnetic action founded on simple fundamental principles.

2244. The whole matter is so new, and the phænomena so varied and general, that I must, with every desire to be brief, describe much which at last will be found to concentrate under simple principles of action. Still, in the present state of our knowledge, such is the only method by which I can make these principles and their results sufficiently manifest.

¶ i. *Apparatus required.*

2245. The effects to be described require magnetic apparatus of great power, and under perfect command. Both these points are obtained by the use of electro-magnets, which can be raised to a degree of force far beyond that of natural or

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soft iron, on the deutoxide and tritoxide of iron, on the tritoxide alone, and on a needle of wood. The author observed, and quotes Coulomb as having also observed, that a needle of wood, under certain conditions, pointed *across* the magnetic curves; and he also states the striking fact that he had found a needle of wood place itself parallel to the wires of a galvanometer. These effects, however, he refers to a degree of magnetism less than that of the tritoxide of iron, but the same in character, for the bodies take the same position. The polarity of steel and iron is stated to be in the direction of the length of the substance, but that of tritoxide of iron, wood and gum-lac, most frequently in the direction of the width, and always when one magnetic pole is employed. "This difference of effect, which establishes a line of demarcation between these two species of phænomena, is due to this, that the magnetism being very feeble in the tritoxide of iron, wood, &c., we may neglect the reaction of the body on itself, and therefore the direct action of the bar ought to overrule it."

As the paper does not refer the phænomena of wood and gum-lac to an elementary *repulsive* action, nor show that they are common to an immense class of bodies, nor distinguish this class, which I have called diamagnetic, from the magnetic class; and, as it makes all magnetic action of one kind, whereas I show that there are two kinds of such action, as distinct from each other as positive and negative electric action are in their way, so I do not think I need alter a word or the date of that which I have written; but am most glad here to acknowledge M. Becquerel's important facts and labours in reference to this subject.—M. F. Dec. 5, 1845.

steel magnets; and further, can be suddenly altogether deprived of power, or made energetic to the highest degree, without the slightest alteration of the arrangement, or of any other circumstance belonging to an experiment.

2246. One of the electro-magnets which I use is that already described under the term Woolwich helix (2192.). The soft iron core belonging to it is twenty-eight inches in length and 2·5 inches in diameter. When thrown into action by ten pair of Grove's plates, either end will sustain one or two half-hundred weights hanging to it. The magnet can be placed either in the vertical or the horizontal position. The iron core is a cylinder with flat ends, but I have had a cone of iron made, two inches in diameter at the base and one inch in height, and this placed at the end of the core, forms a conical termination to it, when required.

2247. Another magnet which I have had made has the horse-shoe form. The bar of iron is forty-six inches in length and 3·75 inches in diameter, and is so bent that the extremities forming the poles are six inches from each other; 522 feet of copper wire 0·17 of an inch in diameter and covered with tape, are wound round the two straight parts of the bar, forming two coils on these parts, each sixteen inches in length, and composed of three layers of wire: the poles are, of course, six inches apart, the ends are planed true, and against these move two short bars of soft iron, 7 inches long and 2½ by 1 inch thick, which can be adjusted by screws, and held at any distance less than six inches from each other. The ends of these bars form the opposite poles of contrary name; the magnetic field between them can be made of greater or smaller extent, and the intensity of the lines of magnetic force be proportionately varied.

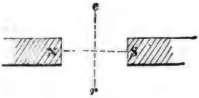
2248. For the suspension of substances between and near the poles of these magnets, I occasionally used a glass jar, with a plate and sliding wire at the top. Six or eight lengths of cocoon silk being equally stretched, were made into one thread and attached, at the upper end, to the sliding rod, and at the lower end to a stirrup of paper, in which anything to be experimented on could be sustained.

2249. Another very useful mode of suspension was to attach one end of a fine thread, six feet long, to an adjustible arm near the ceiling of the room, and terminating at the lower end by a little ring of copper wire; any substance to be suspended could be held in a simple cradle of fine copper wire having eight or ten inches of the wire prolonged upward; this, being bent into a hook at the superior extremity, gave the means of attachment to the ring. The height of the suspended substance could be varied at pleasure, by bending any part of the wire at the instant into the hook form. A glass cylinder placed between the magnetic poles was quite sufficient to keep the suspended substance free from any motion, due to the agitation of the air.

2250. It is necessary, before entering upon an experimental investigation with such an apparatus, to be aware of the effect of any magnetism which the bodies used may possess; the power of the apparatus to make manifest such magnetism is so great, that it is difficult on that account to find writing-paper fit for the stirrup above-mentioned. Before therefore any experiments are instituted, it must be ascertained that the suspending apparatus employed does not point, *i. e.* does not take up a position parallel to the line joining the magnetic poles, by virtue of the magnetic force. When copper suspensions are employed, a peculiar effect is produced (2309.), but when understood, as it will be hereafter, it does not interfere with the results of experiment. The wire should be fine, not magnetic as iron, and the form of the suspending cradle should not be elongated horizontally, but be round or square as to its general dimensions, in that direction.

2251. The substances to be experimented with should be carefully examined, and rejected if not found free from magnetism. Their state is easily ascertained; for, if magnetic, they will either be attracted to the one or the other pole of the great magnet, or else point between them. No examination by smaller magnets, or by a magnetic needle, is sufficient for this purpose.

2252. I shall have such frequent occasion to refer to two chief directions of position across the magnetic field, that to avoid periphrasis, I will here ask leave to use a term or two conditionally. One of these directions is that from pole to pole, or along the line of magnetic force; I will call it the axial direction: the other is the direction perpendicular to this, and across the line of magnetic force; and for the time, and as respects the space between the poles, I will call it the equatorial direction. Other terms that I may use, I hope will explain themselves.



¶ ii. *Action of magnets on heavy glass.*

2253. The bar of silicated borate of lead, or heavy glass already described as the substance in which magnetic forces were first made effectually to bear on a ray of light (2152.), and which is two inches long, and about 0·5 of an inch wide and thick, was suspended centrally between themagnetic poles (2247.), and left until the effect of torsion was over. The magnet was then thrown into action by making contact at the voltaic battery: immediately the bar moved, turning round its point of suspension, into a position across the magnetic curve or line of force, and after a few vibrations took up its place of rest there. On being displaced by hand from this position, it returned to it, and this occurred many times in succession.

2254. Either end of the bar indifferently went to either side of the axial line. The determining circumstance was simply inclination of the bar one way or the other to the axial line, at the beginning of the experiment. If a particular or marked end of the bar were on one side of the magnetic, or axial line, when the magnet was rendered active, that end went further outwards, until the bar had taken up the equatorial position.

2255. Neither did any change in the magnetism of the poles, by change in the direction of the electric current, cause any difference in this respect. The bar went by the shortest course to the equatorial position.

2256. The power which urged the bar into this position was so thoroughly under command, that if the bar were swinging it could easily be hastened in its course into this position, or arrested as it was passing from it by seasonable contacts at the voltaic battery.

2257. There are two positions of equilibrium for the bar; one stable, the other unstable. When in the direction of the axis or magnetic line of force, the completion of the electric communication causes no change of place; but if it be the least oblique to this position, then the obliquity increases until the bar arrives at the equatorial position; or if the bar be originally in the equatorial position, then the magnetism causes no further changes, but retains it there (2298. 2299. 2384.).

2258. Here then we have a magnetic bar which points east and west, in relation to north and south poles, *i. e.* points perpendicularly to the lines of magnetic force.

2259. If the bar be adjusted so that its point of suspension, being in the axial line, is not equidistant from the poles, but near to one of them, then the magnetism again makes the bar take up a position perpendicular to the magnetic lines of force; either end of the bar being on the one side of the axial line, or the other, at pleasure. But at the same time there is another effect, for at the moment of completing the electric contact, the centre of gravity of the bar recedes from the pole and remains repelled from it as long as the magnet is retained excited. On allowing the magnetism to pass away, the bar returns to the place due to it by its gravity.

2260. Precisely the same effect takes place at the other pole of the magnet. Either of them is able to repel the bar, whatever its position may be, and at the same time the bar is made to assume a position, at right angles, to the line of magnetic force.

2261. If the bar be equidistant from the two poles, and in the axial line, then no repulsive effect is or can be observed.

2 62. But preserving the point of suspension in the equatorial line, *i. e.* equidistant from the two poles, and removing it a little on one side or the other of the axial line (2252.), then another effect is brought forth. The bar points as before across the magnetic line of force, but at the same time it recedes from the axial line, increasing its distance from it, and this new position is retained as long as the magnetism continues, and is quitted with its cessation.

2263. Instead of two magnetic poles, a single pole may be used, and that either in a vertical or a horizontal position. The effects are in perfect accordance with those described above; for the bar, when near the pole, is repelled from it in the direction of the line of magnetic force, and at the same time it moves into a position perpendicular to the direction of the magnetic lines passing through it. When the magnet is vertical (2246.) and the bar by its side, this action makes the bar a tangent to the curve of its surface.

2264. To produce these effects, of pointing across the magnetic curves, the form of the heavy glass must be long; a cube or a fragment approaching roundness in form, will not point, but a long piece will. Two or three rounded pieces or cubes, placed side by side in a paper tray, so as to form an oblong accumulation, will also point.

2265. Portions, however, of any form, are *repelled*; so if two pieces be hung up at once in the axial line, one near each pole, they are repelled by their respective poles, and approach, seeming to attract each other. Or if two pieces be hung up in the equatorial line, one on each side of the axis, then they both recede from the axis, seeming to repel each other.

2266. From the little that has been said, it is evident that the bar presents in its motion a complicated result of the force exerted by the magnetic power over the heavy glass, and that when cubes or spheres are employed, a much simpler indication of the effect may be obtained. Accordingly, when a cube was thus used with the two poles, the effect was repulsion or recession from either pole, and also recession from the magnetic axis on either side.

2267. So the indicating particle would move, either along the magnetic curves, or across them; and it would do this either in one direction or the other; the only constant point being, that its tendency was to move from stronger to weaker places of magnetic force.

2268. This appeared much more simply in the case of a single magnetic pole, for then the tendency of the indicating cube or sphere was to move outwards, in the direction of the magnetic lines of force. The appearance was remarkably like a case of weak electric repulsion.

2269. The cause of the pointing of the bar, or any oblong arrangement of the heavy glass, is now evident. It is merely a result of the tendency of the particles to move outwards, or into the positions of weakest magnetic action. The joint exertion of the action of all the particles brings the mass into the position, which, by experiment, is found to belong to it.

2270. When one or two magnetic poles are active at once, the courses described by particles of heavy glass free to move, form a set of lines or curves, which I may have occasion hereafter to refer to; and as I have called air, glass, water, &c. diamagnetics (2149.), so I will distinguish these lines by the term *diamagnetic curves*, both in relation to, and contradistinction from, the lines called magnetic curves.

2271. When the bar of heavy glass is immersed in water, alcohol, or æther, contained in a vessel between the poles, all the preceding effects occur; the bar points and the cube recedes exactly in the same manner as in air.

2272. The effects equally occur in vessels of wood, stone, earth, copper, lead, silver, or any of those substances which belong to the diamagnetic class (2149.).

2273. I have obtained the same equatorial direction and motions of the heavy glass bar as those just described, but in a very feeble degree, by the use of a good common steel horseshoe magnet (2157.). I have not obtained them by the use of the helices (2191. 2192.) without the iron cores.

2274. Here therefore we have magnetic repulsion without polarity, *i. e.* without reference to a particular pole of the magnet, for either pole will repel the substance, and both poles will repel it at once (2262.). The heavy glass, though subject to magnetic action, cannot be considered as magnetic, in the usual acceptation of that term, or as iron, nickel, cobalt, and their compounds. It presents to us, under these circumstances, a magnetic property new to our knowledge; and though the phænomena are very different in their nature and character to those presented by the action of the heavy glass on light (2152.), still they appear to be dependent on, or connected with, the same condition of the glass as made it then effective, and therefore, with those phænomena, prove the reality of this new condition.

¶ iii. *Action of magnets on other substances acting magnetically on light.*

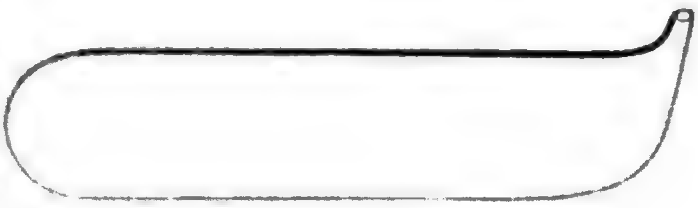
2275. We may now pass from heavy glass to the examination of the other substances, which, when under the power of magnetic or electric forces, are able to affect and rotate a polarized ray (2173.), and may also easily extend the investigation to bodies which, from their irregularity of form, imperfect transparency, or actual opacity, could not be examined by a polarized ray, for here we have no difficulty in the application of the test to all such substances.

2276. The property of being thus repelled and affected by magnetic poles, was soon found not to be peculiar to heavy glass. Borate of lead, flint-glass, and crown-glass set in the same manner equatorially, and were repelled when near to the poles, though not to the same degree as the heavy glass.

2277. Amongst substances which could not be subjected to the examination by light, phosphorus in the form of a cylinder presented the phænomena very well; I think as powerfully as heavy glass, if not more so. A cylinder of sulphur, and a long piece of thick India rubber, neither being magnetic after the ordinary fashion, were well-directed and repelled.

2278. Crystalline bodies were equally obedient, whether taken from the single or double refracting class (2237.). Prisms of quartz, calcareous spar, nitre and sulphate of soda, all pointed well, and were repelled.

2279. I then proceeded to subject a great number of bodies, taken from every class, to the magnetic forces, and will, to illustrate the variety in the nature of the substances, give a comparatively short list of crystalline, amorphous, liquid and organic bodies below. When the bodies were fluids, I inclosed them in thin glass tubes. Flint-glass points equatorially, but if the tube be of very thin glass, this effect is found to be small when the tube is experimented with alone; afterwards, when it is filled with liquid and examined, the effect is such that there is no fear of mistaking that due to the glass for that of the fluid. The tubes must not be closed with cork, sealing-wax, or any ordinary substance taken at random, for these are generally magnetic (2285.). I have usually



so shaped them in the making, and drawn them off at the neck, as to leave the aperture on one side, so that when filled with liquid they required no closing.

2280. Rock crystal.
Sulphate of lime.
Sulphate of baryta.
Sulphate of soda.
Sulphate of potassa.
Sulphate of magnesia.
Alum.
Muriate of ammonia.
Chloride of lead.
Chloride of sodium.
Nitrate of potassa.
Nitrate of lead.
Carbonate of soda.
Iceland spar.
Acetate of lead.
Tartrate of potash and antimony.
Tartrate of potash and soda.
Tartaric acid.
Citric acid.
Water.
Alcohol.
Æther.
Nitric acid.
Sulphuric acid.
Muriatic acid.
Solutions of various alkaline and earthy salts.
Glass.
Litharge.
White arsenic.
Iodine.
Phosphorus.
Sulphur.
Resin.
Spermaceti.
Caffeine.
Cinchonia.
Margaric acid.
Wax from shell-lac.
Sealing-wax.
Olive-oil.
Oil of turpentine.
Jet.
Caoutchouc.
Sugar.
Starch.
Gum-arabic.
Wood.
Ivory.
Mutton, dried.
Beef, fresh.
Beef, dried.
Blood, fresh.
Blood, dried.
Leather.
Apple.
Bread.

2281. It is curious to see such a list as this of bodies presenting on a sudden this remarkable property, and it is strange to find a piece of wood, or beef, or apple, obedient to or repelled by a magnet. If a man could be suspended, with sufficient delicacy, after the manner of Dufay, and placed in the magnetic field, he would point equatorially; for all the substances of which he is formed, including the blood, possess this property.

2282. The setting equatorially depends upon the form of the body, and the diversity of form presented by the different substances in the list was very great; still the general result, that elongation in one direction was sufficient to make them take up an equatorial position, was established. It was not difficult to perceive that comparatively large masses would point as readily as small ones, because in larger masses more lines of magnetic force would bear in their action on the body, and this was proved to be the case. Neither was it long before it evidently appeared that the form of a plate or a ring was quite as good as that of a cylinder or a prism; and in practice it was found that plates and flat rings of wood, spermaceti, sulphur, &c., if suspended in the right direction, took up the equatorial position very well. If a plate or ring of heavy glass could be floated in water, so as to be free to move in every direction, and were in that condition subject to magnetic forces diminishing in intensity, it would immediately set itself equatorially, and if its centre coincided with the axis of magnetic power, would remain there; but if its centre were out of this line, it would then, perhaps, gradually pass off from this axis in the plane of the equator, and go out from between the poles.

2283. I do not find that division of the substance has any distinct influence on the effects. A piece of Iceland spar was observed, as to the degree of force with which it set equatorially; it was then broken into six or eight fragments, put into a glass tube and tried again; as well as I could ascertain, the effect was the same. By a second operation, the calcareous spar was reduced into coarse particles; afterwards to a coarse powder, and ultimately to a fine powder: being examined as to the equatorial set each time, I could perceive no difference in the effect, until the very last, when I thought there might be a slight diminution of the tendency, but if so, it was almost insensible. I made the same experiment on silica with the same result, of no diminution of power. In reference to this point I may observe, that starch and other bodies in fine powder exhibited the effect very well.

2284. It would require very nice experiments and great care to ascertain the specific degree of this power of magnetic action possessed by different bodies, and I have made very little progress in that part of the subject. Heavy glass stands above flint-glass, and the latter above plate-glass. Water is beneath all these, and I think alcohol is below water, and æther below alcohol. The borate of lead is I think as high as heavy glass, if not above it, and phosphorus is probably at the head of all the substances just named. I verified the equatorial set of phosphorus between the poles of a common magnet (2273.).

2285. I was much impressed by the fact that blood was not magnetic (2280.), nor any of the specimens tried of red muscular fibre of beef or mutton. This was the more striking, because, as will be seen hereafter, iron is *always* and in almost *all states* magnetic. But in respect to this point it may be observed, that the ordinary magnetic property of matter and this *new property* are in their effects opposed to each other; and that when this property is strong it may overcome a very slight degree of ordinary magnetic force, just as also a certain amount of the magnetic property may oppose and effectually hide the presence of this force (2422.). It is this circumstance which makes it so necessary to be careful in examining the magnetic condition of the bodies in the first instance (2250.). The following list of a few substances, which were found slightly magnetic, will illustrate this point:—Paper, sealing-wax, china ink, Berlin porcelain, silkworm-gut, asbestos, fluor-spar, red lead, vermilion, peroxide of lead, sulphate of zinc, tourmaline, plumbago, shell-lac, charcoal. In some of these cases the magnetism was generally diffused through the body, in other cases it was limited to a particular part.

2286. Having arrived at this point, I may observe, that we can now have no difficulty in admitting that the phænomena abundantly establish the existence of a magnetic property in matter new to our knowledge. Not the least interesting of the consequences that flow from it, is the manner in which it disposes of the assertion which has sometimes been made, that all bodies are magnetic. Those who hold this view, mean that all bodies are magnetic as iron is, and say that they point between the poles. The new facts give not a mere negative to this statement, but something beyond, namely, an affirmative as to the existence of forces in all ordinary bodies, directly the opposite of those existing in magnetic bodies, for whereas those practically produce attraction, these produce repulsion; those set a body in the axial direction, but these make it take up an equatorial position: and the facts with regard to bodies generally are exactly the reverse of those which the view quoted indicates.

[To be continued.]

LXVII. *Description of a new Mercurial Trough.* *By* Professor Louyet *of Brussels**.

In small laboratories in which one of the chief points to be aimed at is œconomy, in making researches on gases soluble in water, a small porcelain trough is commonly employed capable of containing twenty to twenty-five pounds of mercury. The size of the bell-glass is proportioned to the capacity of the trough; thus only small quantities of gas can be

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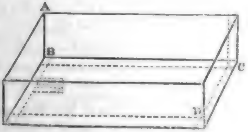
* Communicated through Prof. Grove, by the Author.

collected,—quantities often insufficient for the desired experiments. With a view to obviate this inconvenience, I have modified the mercurial trough, so as to be able, without sacrificing œconomy, to collect considerable quantities of gas; and I have thought it may be of use to give a description of this new instrument, for the service of persons engaged in particular researches. This apparatus is formed of a small oblong oak box, to the bottom of which is cemented a glass, which fits exactly over its whole extent. The external surface of this glass is carefully polished and prepared. In the centre of one of its small sides (it is rectangular), is worked a narrow and deep groove parallel to the large sides of the right angle. This opening corresponds to a hollow in the bottom of the box. This being done, I arrange the apparatus in the following way, when I desire to collect a gas over the mercury: —I procure bell-glasses made of emery-stoppered bottles, the bottom of which is removed; the edges of these receivers are prepared and rubbed with emery, and apply accurately to the ground glass, precisely like the receiver of an air-pump. The edges may be very slightly greased, or this precaution may be dispensed with. The receiver is placed on the ground glass, where it is kept fixed with one hand; with the other hand the stopper is removed, and it is entirely filled with mercury; then it is carefully re-stopped. This being done, a small quantity of mercury is poured into the box, so as to fill the small cavity, and to cover its bottom with a stratum of some millimetres. The receiver may now be moved in all directions, and may be slid until it is over the small cavity, into which the extremity of the curved tube by which the gas is disengaged, is adapted. To one of the angles of the box may be fitted a small pure iron stopcock, by which the mercury is drawn off when the operation is ended. For greater clearness, I subjoin a figure, which represents this new trough of the dimensions which I have adopted.



A B = 4½ centimetres[*]. B C = 23 centimetres. C D = 17 centimetres. Depth of the longitudinal groove, taken above the glass plate, = 2 centimetres.

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[*] The centimetre is = 0·393708 of a English inch.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Anniversary Meeting, December 1, 1845.

THE Marquis of Northampton in the Chair.

The noble President stated that the two Royal Medals had been adjudged by the Council to the Astronomer Royal, for his Inquiries into the Tides on the Coast of Ireland; to Mr. Beck, for his Investigation of the Nerves of the Uterus*; and the Copley Medal to Prof. Schwann, for his valuable work On the Analogies of Vegetables and Animals.

After presenting the Medals, the President proceeded to the biographical notices of some of the deceased members, from which we select the following:—

DR. WILLIAM HEBERDEN, the son of the eminent and accomplished author of the 'Commentaries on the History and Cure of Diseases,' was born in London in the year 1767. At the early age of seven, he was sent to school at the Charter House, and appears to

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* The report of the Committee of Physiology on the claims of Mr. Beck's paper to the award of the Medal, is as follows:—

"The paper of Mr. Beck contains the result of an elaborate anatomical investigation of the Nerves of the Uterus, together with observations on the structure and connexions of the sympathetic nerve.

"By his researches the author has cleared up various points concerning the nerves of the uterus which have hitherto been doubtful or misunderstood. He has determined more precisely than heretofore the source and mode of distribution of these nerves, and the real extent to which the organ is supplied with them. The true nature of the nervous ganglia at the neck of the uterus, and of the plexuses formed by the sympathetic and sacral nerves in the same situation, is also satisfactorily made out, as well as the fact that the branches derived from the sacral nerves are not destined for the uterus, but are distributed to adjacent organs.

"With regard to the sympathetic nerve, it is shown that there are both grey and white separate branches of communication between that nerve and the spinal nerves. This important fact has, it is true, been already pointed out in the recently published work of Todd and Bowman, but the author of the paper has nevertheless the merit of arriving at it independently, by his own observations. He has further shown that the white and grey constituents of the nerve keep distinct from each other, not only in the so-called trunk of the sympathetic, but also in its primary branches, as far as the visceral ganglia, beyond which the white and grey parts become intermixed in the nerves distributed to the viscera. The precise mode of connexion of the white and grey communicating branches with the spinal nerves is also carefully investigated. These observations appear important as tending to throw light on the constitution of the sympathetic nerve and its relation to the rest of the nervous system.

"The Committee consider the paper of Mr. Beck as a most valuable contribution to the Anatomy of the nervous system, and as affording additional and more precise data for physiological reasoning respecting the nerves to which it refers. On these grounds, as well as on account of the consummate skill and devoted perseverance displayed by the author in his arduous investigations, they have recommended that his paper be rewarded with the Royal Medal."

have there made rapid progress in the elementary branches of education. His academical studies were pursued in St. John's College, Cambridge, where he highly distinguished himself both by his mathematical and his classical acquirements. Under his father's tuition he applied himself with great diligence to his profession as a pupil of St. George's Hospital, to which, at a subsequent period, he was appointed one of the Physicians. He was elected a Fellow of this Society in the year 1791: and in 1797, became a Fellow of the College of Physicians. He died on the 19th of February, 1845, aged 77.

His contributions to the Philosophical Transactions consist of two papers; the first in 1796, on the influence of cold on the health of the inhabitants of London; in which he shows, in opposition to the popular prejudices then prevalent, that a severe winter is attended with greatly increased mortality. The second paper is entitled "On the heat of July 1825, together with some remarks on sensible cold," in which he points out the causes which influence our sensations of temperature, and more especially the powerful effect of wind in increasing the rate of cooling, and consequently of creating the sensation of cold in the human body, independently of any actual depression in the temperature of the air.

JOHN FREDERIC DANIELL was born in Essex Street, Strand, 12th of March, 1790. His father, George Daniell, Esq., Bencher of the Inner Temple, provided him with a good classical education under his own roof. At an early age he showed fondness for the pursuits of science, and was placed in the sugar refining establishment of a relative, where he introduced important improvements in the manufacture. The pursuits of business, however, were uncongenial to his tastes, and he soon relinquished this occupation. In 1813 he was elected a Fellow of the Royal Society, of which body he continued till the day of his death a zealous and active member.

The services he rendered to more than one branch of science were of no ordinary description. From an early period of his life his mind was directed to the study of meteorology, at a time when it consisted of little more than a vast accumulation of facts and observations.

In the year 1823 he published the first edition of his 'Meteorological Essays,' which constituted a new epoch in the science, and still continues the standard work of reference, the third edition of which he had nearly completed at the time of his death. This was the first attempt to embrace in a general view the scattered facts of the science, and by synthetically applying the known laws which regulate the constitution of gases and vapours, the principles of their equilibrium, and the distribution of heat among them, to give a connected account of the main phenomena of the earth's atmosphere. He insisted on the paramount importance of extreme accuracy in the construction of the instruments employed for such inquiries, and gave directions by which the needful accuracy could with certainty and facility be obtained. By the invention of the hygrometer, which bears his name, he first conferred precision on the means of ascertaining the moisture or dryness of the atmosphere, a point of cardinal importance in all investigations of this nature; his instrument still continues that which can be best depended upon for this purpose. With these accurate instruments, he for three years kept a faithful register of the various atmospheric changes; he organized the plan adopted by the Horticultural Society in their annual meteorological reports, a plan which formed the model to the admirable and more extended series of meteorological observations now issued weekly from the Greenwich Observatory under the superintendence of the Astronomer Royal.

In the year 1824 he communicated to the Horticultural Society an essay 'On Artificial Climate,' which appeared in their Transactions for that year. In this paper among other subjects he insisted on the absolute necessity of attention to the moisture of the atmosphere, as well as of that of maintaining in our hot-houses the moisture as well as the temperature of a tropical climate, if we would produce a vegetation of tropical luxuriance. The publication of this essay caused a complete change in the methods adopted for the culture of plants in general, and particularly of those contained in green-houses and hot-houses, which upon the new plans speedily outgrew the houses provided for their reception. The Society immediately awarded him their silver medal to mark their sense of the importance of his views, and now after an experience of more than twenty years, Dr. Lindley, Professor of Botany in University College, not a fortnight before his death, in an article in the Gardener's Chronicle, tracing the origin of the improvements in this branch of horticulture, ascribes the rapid advance in the practice of the art, mainly to the sound and original views promulgated in this essay.

For the purpose of making more minute and accurate observations upon variations in the atmospheric pressure, Mr. Daniell proposed to the Royal Society, in 1830, to construct a barometer in which water should be the fluid used instead of mercury. He was in consequence requested to superintend the construction of such an instrument. Great practical difficulties attended the undertaking, but these he happily surmounted, and the instrument now stands in the Hall of the Apartments of the Royal Society; he was engaged in re-adjusting it within a few weeks of his decease. On occasion of the late Antarctic Expedition under the command of Captain Sir James Ross, and the establishment by Government of the Magnetic and Meteorological observations, founded a few years since in different parts of the British Empire, when the Admiralty applied to the Royal Society for instructions as to the nature and extent of the observations to be made, Mr. Daniell was requested by the Committee of Physics of the Royal Society, to draw up the Meteorological portion of these directions. The paper which he then prepared furnished the basis of that part of the Report of the Committee, published in the year 1840, under the sanction of the Royal Society.

But it was not alone to meteorology, and its practical applications, that his labours were confined; his researches upon various chemical subjects were not less numerous or important. More than forty original papers, including thirteen on meteorology, were communicated by him to various scientific publications; among others he published several memoirs on Crystallization, and its attendant phenomena. Between the years 1830 and 1844, the Transactions of this Society were enriched by twelve papers on important subjects from his pen. He invented a process for making gas from resin for the purposes of illumination, by which the streets of New York are lighted at the present time. For this improvement he received no other acknowledgment than a vote of a few pounds' worth of books. In the year 1830, he described in the Philosophical Transactions, a new instrument for measuring high degrees of heat, such as the temperature of furnaces, and the melting-points of metals. By means of this, his pyrometer, he ascertained numerous facts of great interest both in a scientific and in a practical point of view. For the invention of this instrument, which is still the best for the objects intended, the Royal Society awarded him the Rumford Medal.

After his appointment as Professor of Chemistry in King's College, his researches were turned principally to the phenomena presented by Voltaic Electricity, and they led to the invention of his constant battery; for this the Royal Society conferred upon him the highest honour in their gift, the Copley Medal for the year 1836. The possibility of maintaining powerful and equable currents of electricity for any required period, was established by this invention. The impulse thus given to the progress of electrical research cannot be too highly estimated, and to it must be traced the numerous applications of electricity, to the blasting of rocks, the working of mines, and to submarine operations, and to the arts of electro-plating, gilding, zincing, &c., which have recently acquired such magnitude. His subsequent researches in the same field are contained in the Philosophical Transactions, and were honoured by the Society in the year 1842 by one of the Royal Medals. In 1839 Professor Daniell was placed on the Commission appointed by the Admiralty to inquire into the best method of defending the ships in the Royal Navy from lightning, and the same year the Royal Society honoured him with the office of Foreign Secretary to their body. His "Introduction to Chemical Philosophy," published during the course of this year, contributed still further to increase his reputation, and in 1842 he received from the University of Oxford the honorary degree of D.C.L. In consequence of the rapid corrosion of the copper sheathing of the vessels employed upon the African stations, the Admiralty requested him to examine the damaged sheets of metal and the waters taken up from the localities where the corrosion was the greatest; he detected the cause of this decay, showing that sulphuretted hydrogen was abundantly generated in the ocean at these spots, and succeeded in extracting from the metal plates the sulphur which had occasioned their corrosion. It is a remarkable proof of the variety and extent of Mr. Daniell's acquirements, that he received at different times all the medals in the gift of the Royal Society.

The circumstances which attended the sudden and lamented termination of his valuable life, are known to most of the Fellows of this Society. On the 13th of March 1845, after delivering his usual lecture at King's College, apparently in perfect health, he attended the Council Meeting of this Society, and shortly after making some observation upon the business of the meeting, was seized with symptoms indicating an attack of apoplexy. Several medical men who were present hastened to his relief, and he was immediately bled; not the slightest benefit, however, attended this measure, and in five minutes he was a corpse. The shock occasioned by this melancholy event, may be easier imagined than described, and cast a widespread gloom over the extensive circle of his friends and acquaintance. As a mark of respect to his memory, the Noble President postponed the ordinary meeting of the Society, which was to have been held that evening. His remains were interred at Norwood, Surrey, where during the last ten years of his life he had resided.

Mr. Daniell survived his wife eleven years, and left a family of two sons and five daughters to deplore his loss. High as were his scientific attainments, he possessed others of a still loftier and more enduring character; to the sterling qualities of a vigorous understanding, and a kind and benevolent heart, he united the humble and unobtrusive piety of a sincere christian.

Mr. GEORGE BASSEVI, an architect of distinguished reputation.

JAQUES DOMINIQUE CASSINI, Compte du Thury, was elected a Foreign Member of this Society in 1789, and at the time of his death had attained the extraordinary age of 97 years: he was the fourth in direct descent of a family which, during a period of nearly two centuries, has been singularly illustrious in the history of the sciences, and more particularly of astronomy. His great grandfather, Jaques Dominique Cassini, one of the greatest astronomers of his age, was born in 1625, and was invited by Louis XIV. from Italy to France, to preside over the magnificent Observatory of Paris, which was built under his directions: his first successor in the direction of this establishment was his son Jean Jaques, an astronomer not less eminent than himself; the second his grandson, more commonly designated as Cassini de Thury, so well known by his great Geodetical Survey and Map of France; and the third his great grandson, the subject of the present notice, who was displaced from it by the troubles of the Revolution, which involved him, at least for a time, in the common proscription of the aristocracy of France. The shock of these sad events seems to have diverted his mind from scientific pursuits, for we find his name connected with no research in astronomy or geodesy during the last half-century.

The Comte de Cassini completed the celebrated map of France which had been begun by his father. He published an account of voyages which he made in 1768 and 1769, for the trial of the marine chronometers of Le Roy, and a memoir "Sur l'influence de l'équinoxe du printemps et du solstice d'été sur les déclinaisons et les variations de l'aiguille aimantée:" he superintended and published an account of the observations which were made in 1789, by a commission appointed for that purpose, for the junction of the Observatories of Paris and Greenwich, with a special reference to the connection of the Geodetical Survey of France which had been made by his father, with the corresponding Survey of England which was at that time in progress under General Roy: he was the author likewise of "Mémoires pour servir à l'Histoire des Sciences et à celle de l'Observatoire Royal de Paris, suivis de la Vie de Jaques Dominique Cassini, premier du nom."

With him have terminated the honours of the house of Cassini, though he was not the last of his race who distinguished himself in the career of the sciences; his son, Henri Cassini, an upright and enlightened judge in the Cour Royale and the Cour de Cassation, and one of the most learned botanists of his age, fell a victim to the cholera in 1833, and with him died the last stay of the old age of his father: he was the fifth of his family who had been elected a member of the Académie des Sciences. He was a boy at the breaking out of the Revolution, and was compelled, from a regard to his personal safety, to live in the strictest retirement at his father's domain of Thury; a circumstance which turned his attention to the cultivation of Natural History and Botany, and diverted him, as he was accustomed to lament, from those studies and pursuits which formed, as it were, the proper and hereditary honours of his family.

THÉODORE DE SAUSSURE was born in Geneva the 14th of October, 1767. His father, known throughout the civilized world as the geological explorer of the Alps, who first reared observatories on heights almost inaccessible, and who inscribed his name on the eternal snows of the loftiest mountain in Europe, was by profession a physician. Being animated with an ardent love for science, which he cultivated most assiduously, it is not a matter of surprise that after entrusting his son for a short time to a private tutor, he should have undertaken personally his education, so far as to enable him to enter the Academy of Geneva, where young De Saussure soon distinguished himself. Previously to this period, his father had caused him to study medicine, mineralogy, and natural history; and had also inspired him with a taste for experimental chemistry, which he constantly required for the analyses of minerals. By degrees the son became associated with the scientific labours of his father, who records that when he resolved upon attempting the ascent of Mont Blanc, in August 1787, his son, then nineteen years of age, expressed the strongest desire to accompany him; but being apprehensive that he was not sufficiently strong, he was unwillingly obliged to leave him at the Priory at Chamouni, where he made with great care meteorological observations, simultaneously with those carried on at the summit of Mont Blanc. In the month of June of the following year, Théodore de Saussure accompanied his father in the laborious and hazardous expedition to the Col du Géant, where they remained for seventeen days, during which time young De Saussure rose every morning at four o'clock, to commence the meteorological observations, which he continued with unremitting diligence until ten o'clock each night; and so thoroughly did he enter into the scientific pursuits of his parent, that he almost importuned the latter to extend the period of his sojourn amidst those splendid scenes;

which was however effectually prevented by the guides, who, alarmed at the idea of a longer stay amidst those icy heights, destroyed all the remaining provisions, necessarily compelling the De Saussures to descend to Cormayeur.

From this period we find Théodore De Saussure always accompanying his father, who was not slow in availing himself of the great advantages derivable from his son's labours. In 1789, they made the very difficult tour of Monte Rosa, it being the first time that the gold mines of Macugnaga were visited by men of science. It was during this excursion that young De Saussure restored by experiments that confidence in the accuracy of the barometer for measuring heights, which the assertion of Bouguer had tended to weaken. He made seventy experiments at different heights, and in calculating the results, was always careful to make the necessary corrections for temperature and humidity, which Bouguer appears to have neglected. The enthusiasm and physical energy of young De Saussure were almost too great for his father, who was now aged, and weakened by various illnesses, and consequently we often find the latter compelled to resist his son's desire to prolong their arduous excursions.

The storms of the Revolution, more powerful than those of the Alps, at length put an effectual stop to these useful scientific excursions, which had been continued for so many years. Théodore de Saussure, in common with many men of his age, was compelled to leave his country. He visited this country with Alexander Marcet, who many years afterwards became his colleague in the Academy at Geneva. After travelling over England and Scotland, he returned to Geneva, and resolved henceforth to devote his life to scientific pursuits. The taste which he had acquired for chemistry under his father's tuition, had been strengthened in England and in France, where that science was eagerly cultivated; and on his return to Geneva, he determined to select the vegetable kingdom for the field of his researches, and zealously applied himself to discover by experiments the influence of the atmosphere and of soils upon plants, and the various chemical changes which they undergo. With the exception of some few accessory labours, M. de Saussure spent a long life upon this branch of science; and it may be truly said that he has done more to advance the knowledge of vegetable physiology than any other person. It is worthy of remark that he laboured seven years silently, before publishing the results of his investigations. These were comprised in his work entitled "Recherches Chimiques sur la Végétation," which appeared in 1804. He subsequently published in the Annales de Physique et de Chimie, the results of his investigations into the action of the petals of flowers upon the atmosphere. The great importance which he attached to the nutritive power of carbonic acid upon plants, directed his attention to the proportion of this gas in the atmosphere. In 1816, he published, in the first volume of the Bibliothèque Universelle, some researches on this subject, which being greatly extended, formed afterwards a paper which was published in 1830 in the

Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, under the title of "De l'action des huiles sur le gaz oxygène à la température atmosphérique." After examining the action of the green portions of plants, roots and flowers upon the atmosphere, M. de Saussure carried his investigation to the same parts of fruits. The result was a long paper published in 1821, in the Memoirs of the same Society, entitled "Influence des fruits verts sur l'air, avant leur maturité." He shows in this paper, that unripe fruit exercises the same influence as leaves upon the air.

Independently of his vegeto-physiological researches, M. de Saussure published some papers descriptive of minerals in the Journal de Physique. These are entitled, "Analyse du Sappare," "Sur une hydrophane imbibée de cire," "Analyse de la Dolomie," and "Sur le Sappare dur."

M. de Saussure was of a most reserved habit, the result probably of his solitary education: it is recorded of him that he seldom desired to converse with his friends on the scientific subjects occupying his attention; and so far did he carry this reserve, that even his most intimate acquaintances were generally ignorant of the nature of the papers which he proposed reading before the Society of Natural History. The same disposition prevented him from acting as Professor in the Academy of Geneva, though appointed to the Chair of Mineralogy and Geology in the year 1802. It was found impossible to overcome his repugnance to give the usual courses of lectures, though at the same time he gave evidence of his warm interest in the Academy by constantly attending its meetings. In 1814, he was elected a member of the Legislative Council of the Republic of Geneva, but he was too timid to take an active part in the debates of this body. In 1790 he became a member of the Agricultural Section of the Society of Arts, and always continued one of its most zealous supporters. He was a Foreign Member of the French Institute, of the Royal Academies of Naples, Turin and Munich; of the Institute of Fine Arts and Sciences at Amsterdam; of the Linnean Societies of Paris and London; the Wernerian Society of Edinburgh; and was elected a Foreign Member of the Royal Society in 1820. In 1842, M. de Saussure was unanimously elected President of the Scientific Congress, which met that year at Lyons, thus marking the high esteem in which he was held as a man of science. Having preserved throughout life the best health, M. de Saussure died on the 18th of April 1845, at the advanced age of 78, leaving behind him the reputation of a long life passed in severe and patient study, interrupted only when he came before the world with the results of his laborious experiments and researches.

Dr. Roget, reported the following Noblemen and Gentlemen as being duly elected Officers and Council for the ensuing year, viz.—

President.—The Marquis of Northampton. *Treasurer.*—George Rennie, Esq., V.P. *Secretaries.*—Peter Mark Roget, M.D., Samuel Hunter Christie, Esq., M.A. *Foreign Secretary.*—Lieut.-Col. Edward Sabine, R.A. *Other Members of the Council.*—John Bostock, M.D.; Sir William Burnett, M.D., K.C.H., V.P.; Charles Daubeny, M.D.; Bryan Donkin, Esq.; Very Rev. Dean of Ely, V.P.; Thomas Galloway, Esq., M.A.; William Robert Grove, Esq., M.A.; Leonard Horner, Esq., V.P.; Sir J. W. Lubbock, Bart., M.A., V.P.; John Forbes Royle, M.D.; William Sharpey, M.D.; William Henry Smyth, Capt. R.N.; John Taylor, Esq.; Charles Wheatstone, Esq.; Rev. Robert Willis, M.A.; Lord Wrottesley, V.P.

LXIX. *Intelligence and Miscellaneous Articles.*

NOTE BY MR. T. HOPKINS ON HIS PAPER ON THE SEMI-DIURNAL FLUCTUATIONS OF THE BAROMETER.

To Richard Taylor, Esq.,

SIR,

IN looking over my paper, inserted in the Philosophical Magazine for March, "On the Causes of the Semi-diurnal Fluctuations of the Barometer," I discovered that a mistake had been made in putting down the figures in the Plymouth table, which express the temperature at 1 P.M. The mistake arose in this way. In the tables, as inserted in the Report of the British Association for 1839, the temperature for 1 P.M. is entered 45·83. This appeared a typographical error, and in order to correct it a 5 was substituted for the 4, leaving the numbers 55·83. This temperature of the dry thermometer was then compared with that of the wet-bulb thermometer, and the influences of temperature and evaporation as thus exhibited were inferred and remarks were made upon them in the paper. But an examination of other parts of the Plymouth report now shows that the figures 45 in the thermometric column should have been transposed, when the temperature at 1 P.M. would have been found only 54·83 instead of 55·83, making the rise of temperature from 10 A.M. to 1 P.M. only 1·99, whilst the force of evaporation is only 0·89. The tabular statement of the rise of temperature and the force of evaporation at the two periods would then be from

$$5 \text{ to } 10 \left\{\begin{array}{l}5{\cdot}86 \text{ of temperature and}\\ 2{\cdot}18 \text{ of evaporation}\end{array}\right\} \text{caused a rise,}$$

$$10 \text{ to } 1 \left\{\begin{array}{l}1{\cdot}99 \text{ of temperature and}\\ 0{\cdot}89 \text{ of evaporation}\end{array}\right\} \text{caused a fall.}$$

Wishing to be correct in my statements, I have to request that you will insert this letter in the next Number of your valuable publication.

I am, Sir,
Your most obedient Servant,
Manchester, March 6, 1846. THOMAS HOPKINS.

ON SOME NEW COMPOUNDS OF PERCHLORIDE OF TIN.

BY M. LEWY.

The author remarks, that although the perchloride of tin has been the object of numerous researches, the compounds which it forms with basic chlorides and organic matters have not hitherto attracted much attention.

Compounds with Water.—It is well known that when a small quantity of water is added to perchloride of tin, the whole becomes a crystalline mass; on adding more water, the hydrate thus formed dissolves, and yields fresh crystals by slow evaporation; the form of these could not be ascertained on account of their extreme deliquescence. Their formula appeared to be $SnCl^2 + 5HO$, or to consist of—

Chlorine	40·55
Tin	33·68
Water	25·77
	100·00

When these crystals are exposed *in vacuo* over sulphuric acid, they lose a certain quantity of water of crystallization, and eventually a hydrate remains containing only two equivalents of water.

Compounds with the Chlorides.—It is well known that perchloride of tin possesses properties analogous to those of acids; it combines with basic chlorides to form double chlorides, the greater part of which crystallize very readily; they all contain equal equivalents of perchloride of tin and basic chlorides.

The compounds formed with chloride of potassium and chloride of ammonium are anhydrous. The former contains—

	By analysis.	By calculation.	Equivalents.
Chlorine	52·04	52·01	Cl^3
Tin	28·50	28·79	Sn
Potassium ..	18·76	19·19	K
	99·30	99·99	

Chloride of Tin and Ammonium.—This salt consists of—

	By analysis.	By calculation.	Equivalents.
Chlorine	57·33	58·03	Cl^3
Nitrogen	7·70	7·65	N
Hydrogen ..	7·70	2·19	H^4
Tin	32·30	32·13	Sn
		100·00	

The compounds which perchloride of tin forms with the chlorides of sodium, strontium, magnesium, calcium, and barium, all contain water of crystallization; and as far as the author's experiments have yet been carried, these double salts all contain five equivalents of water.

Double chloride of Sodium and Tin, when the analysis is corrected by calculation, appears to consist of—

		Equivalents.
Chlorine	45·54	Cl^3
Sodium........	9·94	Na
Tin	25·21	Sn
Water	19·30	Aq^5
	99·99	

The form of this salt has not been determined, but it appeared to be formed of small prisms.

Double chloride of Strontium and Tin.—The corrected analysis gave—

		Equivalents.
Chlorine	41·84	Cl^3
Strontium......	17·26	Sr
Tin	23·17	Sn
Water	17·73	Aq^5
	100·00	

This salt has the form of long channeled prisms, the summits of which are not of determinable form.

Double chloride of Magnesium and Tin.—The analysis corrected gave—

		Equivalents.
Chlorine	47·71	Cl^3
Magnesium	5·66	Mg
Tin	26·41	Sn
Water	20·21	Aq^5
	99·99	

It appears to crystallize in rhombohedrons of about 125°. This measure is however only an approximation to within one or two degrees; it was impossible to obtain a more accurate one, on account of the extreme deliquescence of the salt.

Double chloride of Calcium and Tin gave by corrected analysis,—

		Equivalents.
Chlorine	46·17	Cl^3
Calcium	8·69	Ca
Tin	25·56	Sn
Water	19·56	Aq^5
	99·98	

This salt is still more deliquescent than the preceding; the form of the crystal appears at first to be a cube, but on measuring the angles by the goniometer, one was found of 84° to 86°, and the other of 94° to 96°. It is therefore probable it also crystallizes in rhombohedrons.

Double chloride of Barium and Tin.—This gave by corrected analysis,—

		Equivalents.
Chlorine	38·13	Cl^3
Barium........	24·59	Ba
Tin	21·11	Sn
Water	16·16	Aq^5
	99·99	

The form of this salt was not determined, but as far as an opinion could be formed, it appeared to consist of small prisms.

Compounds of Bichloride of Tin and Organic Bodies.—The author prepared, as had been some years since been done by M. Kuhlman, compounds of bichloride of tin with sulphuric æther, alcohol, hydrochloric æther, and pyroxylic spirit; and M. Lewy combined it also with oxalic æther, benzoic æther, benzoate of methylene, acetic æther, acetic acid, benzoic acid, oil of bitter almonds, urea, camphor, ethal, &c. The greater part of these compounds formed very fine crystals, but their ready alteration in the air, and even *in vacuo*, as well as the difficulty of purifying them, have hitherto prevented an exact analysis of them; M. Lewy therefore endeavoured to verify the opinion of M. Kuhlman as to the composition of the compounds which he had formed.

Compound of Perchloride of Tin and Sulphuric Æther.—This forms crystals of very great beauty; it is obtained, as shown by M. Kuhlman, by mixing the two bodies either in the state of liquids or vapours. The crystals have the form of rhomboidal tables of a brilliant aspect and perfect formation. They are volatile without decomposition, dissolve readily in excess of æther, and decompose in contact with the air. This compound appeared to be formed of—

		Equivalents.
Chlorine ……	34·77	Cl^2
Tin …………	28·88	Sn
Carbon ………	23·57	C^8
Hydrogen, ……	4·91	H^{10}
Oxygen. ………	7·86	O^2
	99·99	

Compound of Perchloride of Tin and Anhydrous Alcohol.—This was formed by merely mixing the two liquids. During mixture, the temperature of the substances was always kept below 32° Fahr.; when the combination has taken place, it is to be exposed *in vacuo* to sulphuric acid and potash. After some days the compound appears in the form of small prismatic crystals, which readily dissolve in an excess of alcohol, so that it is easy to re-crystallize them. The crystals must not, however, be kept too long *in vacuo*, as they then alter readily. This compound gave, by corrected analysis,—

		Equivalents.
Chlorine ……	32·74	Cl^3
Tin …………	36·32	Sn^2
Carbon ………	14·82	C^8
Hydrogen……	3·71	H^{12}
Oxygen ……	12·36	O^5
	99·95	

Compound of Perchloride of Tin with Oxalic Æther.—This is prepared in the same manner as the preceding. When small quantities of perchloride of tin are added to oxalic æther, a moment arrives at which a crystalline mass is formed, consisting of small needles grouped round a common centre. These crystals alter readily, and it is best to analyse them as soon as formed. When mixed with water, oxalic æther is reproduced.

Analysis showed that this is a compound of equal equivalents of the perchloride and the æther, or—

		Equivalents.
Chlorine	34·94	Cl^2
Tin	29·02	Sn
Carbon	17·77	C^6
Hydrogen......	2·47	H^5
Oxygen........	15·80	O^4
	100·00	

Ann. de Ch. et de Phys., Mars 1846.

ANALYSIS OF TWO SPECIES OF EPIPHYTES, OR AIR PLANTS.

BY JOHN THOMSON, A.M.*

I. *Commelina Skinneri.*—Until about four months prior to the time this plant was examined, it had roots in some of the pots; but about that time, Mr. Murray, of the Botanic Gardens, cut off all its roots, and left it hanging on the wall to which it had been trained. I had only 353·05 grains of the young shoots to operate on, so that very great precision cannot be expected in the results. After exposing this quantity on a sand-bath to a heat of about 280°, there remained 71·91 grains of the dried plant, so that the difference, which must have been almost wholly water, amounts to 281·14 grains. The dried portion was then burned: it left a residue of 7·14 grains of ashes, which were now subjected to analysis.

After treating the ashes with water to separate the soluble from the insoluble part, and evaporating the two portions to dryness, there were obtained of matters insoluble in water 4·22 grains, and of soluble substances 3·05 grains, the whole amounting to 7·27 grains, there being thus an excess of ·13 grain.

Muriatic acid was then poured on the insoluble portion, when a violent effervescence took place, and only ·77 grain remained undissolved. By fusing this with carbonate of soda, and adding muriatic acid in the ordinary way, there were found to be ·60 grain of silica. The whole quantity dissolved in muriatic acid was now mixed, and ammonia was added. A precipitate fell, which was boiled with caustic soda to remove alumina. What remained was evidently peroxide of iron; it was dried, and found to weigh ·22 grain. The portion dissolved by the caustic soda was precipitated by the addition of muriatic acid, the excess of which was removed by adding carbonate of soda. There were thus found to be ·44 grain of alumina, or phosphate of alumina.

To the washings oxalate of ammonia was added, and after filtering and burning, the precipitate weighed 2·90, which was carbonate of lime.

The next point was to determine the composition of the salts soluble in water. This part of the process was from an accident not completely executed. The only constituents which were deter-

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* Read before the Philosophical Society of Glasgow, December 4, 1844.

mined were the sulphuric acid, the potash, and the soda, the first of which was found, by precipitating with nitrate of barytes, to weigh ·92 grain. The potash and soda were separated by means of bichloride of platinum and found to weigh respectively ·24 grain and ·94 grain. The following then is a statement of the entire results:—

	grs.		grs.		grs.
Water......	281·14				
Organic matter	64·77				
Ashes	7·14	Soluble in water	3·05	Sulph. acid .	·92
				Potash	·24
				Soda	·94
				Chlorine, &c.	·95
		Insoluble in water	4·22	Silica	·60
				Perox. of iron	·22
				Alumina ..	·44
				Carb. of lime	2·90
Entire plant .	353·05		7·27		

100 parts of the plant would contain,—

Water	79·64
Organic matter....	18·34
Ashes...........	2·02
	100·00

100 parts of the ashes again would contain approximately—

Soluble salts....	42·72	....	42·72
Insoluble	59·10	Silica	8·43
		Peroxide of iron .	3·08
		Alumina, or phosphate of alumina	6·16
		Carb. of lime ..	40·62
	101·82		101·01

II. *Vanilla planifolia.*—The following is the composition of a specimen of the *Vanilla planifolia* which I also examined. Although called an epiphyte, it had roots in some of the pots. It is a very succulent plant with a small round stem, and alternate petiolated, elliptico-lanceolate, polished leaves:—

Water	89·06
Organic matter	9·84
Ashes............	1·10
	100·00

The ashes were similar in composition to those of the *Commelina Skinneri*. They contained no alumina, and had a perceptible quantity of phosphoric acid. It is probable therefore that the alumina in the first analysis is accidental.

These analyses were conducted under the direction of Dr. R. D. Thomson in the College Laboratory of Glasgow.

ANALYSIS OF *CERADIA FURCATA* RESIN.

BY ROBERT D. THOMSON, M.D.

The plant from which this resin exudes, presents the appearance of coral, and is a native of the coast of Africa, opposite the island of Ichaboe.

The resin possesses an amber colour, and an odour similar to that of olibanum. It partially dissolves in alcohol, and is precipitated by water. Caustic ammonia produces no precipitate in the alcoholic solution. The alcoholic solution possesses a slightly acid reaction, and is not precipitated by nitrate of silver. Specific gravity 1·197, determined by my pupil, Mr. Hugh B. Tennent.

Analysis gave the following results:—

19·9 grains lost by exposure to the temperature of 212° for some days 2·11 grains. During the whole of the period its peculiar odour was emitted. Previous to being subjected to this heat it was pulverized, but it speedily became soft, and collected into a mass. In this state, when burned with oxide of copper and chlorate of potash,

6·24 grains gave 18·33 grains CO_2,
and 5·50 ... HO.

This amounts to per cent.—

Carbon	80·113
Hydrogen	9·793
Oxygen	10·094
	100·000

Calculated according to the formula $C_{10} H_7 O$, or $C_{40} H_{28} O_4$, the result would be as follows:—

Carbon	80·00
Hydrogen	9·33
Oxygen..........	10·67
	100·00

After being heated in the water-bath for some weeks, the resin still continued to emit an odour. It was then pulverized, and again heated somewhat higher, when it speedily gave out fumes, and lost its smell entirely. Its composition was then found to be as follows:—

6·52 grains gave, with oxide of copper and chlorate of potash,—

15·89 carbonic acid,
5·02 water,

which are equivalent to—

Carbon	66·46
Hydrogen	8·55
Oxygen..........	24·99

Calculated according to the formula

$$C_{40}, H_{30}, O_{11},$$

its composition will be—

Carbon	67·03
Hydrogen	8·37
Oxygen	24·60

From the *Proceedings of the Philosophical Society of Glasgow*, read February 5, 1845.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1846.

Chiswick.—March 1. Overcast. 2. Very fine. 3. Cloudy. 4. Rain. 5. Showery: clear and fine. 6, 7. Very fine. 8. Clear: cloudy: clear. 9. Frosty: fine. 10. Frosty and foggy: fine: very clear. 11. Slight fog: very fine: clear. 12. Foggy. 13. Slight haze. 14. Cloudy and windy. 15. Showery. 16. Cloudy: boisterous: heavy showers. 17. Overcast: clear: slight frost at night. 18. Frosty: overcast: clear and frosty. 19. Frosty: overcast: hazy. 20. Snow early A.M., nearly two inches deep: cloudy: clear and frosty at night. 21. Sharp frost: densely clouded: boisterous, with rain at night. 22. Clear and fine: showery. 23. Rain: cloudy and fine: clear. 24. Cloudy and fine: clear. 25. Fine: overcast: showery. 26. Cloudy and fine. 27. Clear and fine. 28. Hazy. 29. Hazy clouds: fine. 30. Slight haze: cloudy and cold: clear. 31. Dry haze: clear and fine.

Mean temperature of the month	43°·43
Mean temperature of March 1845	38 ·49
Mean temperature of March for the last twenty years...	42 ·89
Average amount of rain in March	1 ·36 inch.

Boston.—March 1. Foggy. 2, 3. Cloudy. 4. Windy: rain early A.M. 5. Cloudy: rain early A.M. 6. Cloudy: rain P.M. 7—10. Fine. 11, 12. Cloudy. 13. Cloudy: rain at noon. 14. Cloudy: rain A.M. and P.M. 15. Cloudy. 16. Windy: stormy day: rain P.M. 17. Cloudy. 18. Fine. 19. Cloudy: snow early A.M. 20. Cloudy. 21—25. Fine. 26. Cloudy: thunder-storm, with rain P.M. 27, 28. Fine. 29. Fine: rain A.M. 30. Cloudy. 31. Fine.

Sandwick Manse, Orkney.—March 1. Bright: cloudy. 2. Clear: cloudy. 3. Showers: clear. 4. Bright: clear. 5. Fine: clear. 6. Clear. 7. Bright: hail-showers. 8. Showers: clear. 9. Damp: drops. 10. Damp: cloudy. 11. Clear: halo. 12. Cloudy: drops. 13. Cloudy: showers. 14. Sleet-showers: showers: sleet. 15. Sleet-showers: cloudy: sleet. 16. Sleet-showers: sleet. 17. Sleet-showers: snow. 18. Snow-showers: snow: cloudy. 19. Snow: clear. 20. Snow: clear: snow: cloudy. 21. Snow-drift: thaw: clear. 22. Cloudy. 23, 24. Bright: clear. 25. Rain: damp: clear. 26. Showers. 27. Showers: clear. 28. Clear. 29. Showers: cloudy. 30. Clear: cloudy. 31. Snow-showers: cloudy.

Applegarth Manse, Dumfries-shire.—March 1. Fine till 10: P.M. rain. 2. Heavy showers P.M. 3. Heavy rain all day. 4. Heavy rain all day: flood. 5. Very fine. 6. Showers. 7. Showers: hail: frost. 8. Hoar frost. 9. Slight showers. 10, 11, 12. Fine: fair. 13. Wet A.M. 14. Heavy rain A.M. 15. Rain P.M. 16. Showers: hail: sleet: rain. 17. Hard frost. 18. Frost: snow-showers. 19. Hard frost: clear. 20. Hard frost. 21. Frost: snow: hail: rain: thunder. 22. Rain: hail. 23. Slight drizzle: hail. 24. Showers. 25. Wet A.M.: cleared. 26. Hoar frost: drops. 27. Showers: hail. 28. Hail: rain. 29. Frost: clear and fine. 30. Frost: clear: cloudy. 31. Frost.

Mean temperature of the month	42°·2
Mean temperature of March 1845	36 ·3
Mean temperature of March for 23 years	39 ·0
Mean rain in March for 18 years	2 ·35 inches.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. March.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8¾ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8¾ p.m.								
1.	30·056	29·952	29·60	29·75	29·57	29·68	29·50	60	45	43	52	42½	48	48	s.	calm	s.	sse.	......	......	......	·14
2.	29·953	29·928	29·50	29·60	29·42	29·51	29·49	60	41	48	52½	45½	46	45	s.	calm	sse.	se.	......	......	......	·29
3.	29·869	29·705	29·35	29·30	29·17	29·19	29·11	60	47	50	50	42½	45	46	sw.	sw.	s.	sw.	......	......	......	·06
☽ 4.	29·491	29·403	28·89	28·83	29·18	28·79	29·07	52	40	52	51½	46½	48	43	sw.	w.	s.	s.	·12	·06	......	·03
5.	29·599	29·471	29·15	29·30	29·37	29·19	29·40	55	34	46·5	51	42½	42½	41	sw.	calm	s.	s.	·27	·06	......	·07
6.	29·729	29·549	29·17	29·37	29·30	29·39	29·30	53	41	45	49	35½	42	41	w.	calm	sw.	ssw.	·02	......	......	......
7.	29·777	29·715	29·35	29·48	29·62	29·38	29·60	53	28	44	50	35	45	37½	w.	calm	sw.	wnw.	......	·06	......	......
8.	30·013	29·977	29·61	29·84	29·82	29·78	29·87	54	24	41	49	36	40	39	nw.	w.	w.	calm	......	......	1·40	·22
9.	30·305	30·105	29·75	30·00	30·14	30·00	30·12	56	27	37·5	49	34½	43	45	sw.	w.	sw.	w.	......	......	......	......
10.	30·383	30·368	29·99	30·20	30·11	30·06	29·94	58	29	41	51½	36½	49	47	nw.	w.	w.	sw.	......	......	......	·05
11.	30·544	30·407	29·99	30·27	30·41	30·38	30·39	57	28	43	52	44½	46	41	sw.	calm	e.	s.	......	......	......	......
12.	30·600	30·530	30·21	30·41	30·28	30·26	30·07	48	26	45	52	44½	47	47	ne.	calm	s.	sw.	......	......	......	......
○ 13.	30·434	30·336	30·00	30·09	30·05	29·88	29·84	51	39	43	54½	45	47	41	w.	calm	w.	w.	·03	......	......	·13
14.	30·155	30·001	29·65	29·65	29·76	29·53	29·55	56	47	49	53	43½	42	41	sw.	w.	sw.	wnw.	......	·03	......	·28
15.	29·988	29·882	29·55	29·79	29·46	29·71	29·44	57	45	48	50	38	39	37	w.	calm	s.	se.	·03	·02	0·55	·14
16.	29·570	29·267	29·04	28·96	28·78	28·60	28·65	55	31	52	48	44½	42	34	sw.	w.	wsw.	n.	·12	......	......	·51
17.	29·508	29·388	29·04	29·25	29·35	29·07	29·53	48	25	38	42	33	34	28	n.	w.	wnw.	n.	......	·16	......	·23
18.	29·576	29·513	29·25	29·43	29·50	29·56	29·62	47	23	34	36½	28½	29½	26	n.	calm	ne.	s.	......	......	......	·09
19.	29·573	29·563	29·28	29·50	29·50	29·67	29·70	47	23	30	37	22½	29	29	sw.	calm	ne.	n.	......	......	......	·20
☾ 20.	29·781	29·545	29·31	29·53	29·53	29·67	29·55	44	20	29	42	25	33	29	nw.	calm	ne.	s.	·10	......	......	·03
21.	29·643	29·258	29·30	29·13	28·87	28·96	28·90	46	35	35	40	28	33	36	sw.	calm	sse.	sse.	·09	......	......	·31
22.	29·203	29·177	28·83	28·78	28·88	28·88	28·91	53	29	43·5	47	35½	42	41	sw.	calm	sw.	se.	·09	......	......	·07
23.	29·317	29·169	28·85	28·88	28·95	29·05	29·12	55	37	39	48	35	41½	38	sw.	calm	sw.	ese.	·03	......	0·52	......
24.	29·338	29·310	28·95	29·05	29·07	29·19	29·27	53	28	43	49½	39½	43	40	s.	w.	se.	e.	......	......	......	·05
25.	29·387	29·361	29·00	29·11	29·16	29·29	29·39	53	36	45	50	38½	42½	40½	w.	calm	n—s.	sw.	·10	......	......	·07
26.	29·710	29·443	29·07	29·34	29·46	29·31	29·46	54	37	41	50	33	40½	38	w.	calm	sw.	nnw.	·02	......	......	·06
● 27.	29·790	29·734	29·36	29·50	29·48	29·45	29·66	58	27	43	48½	36	42	40½	sw.	calm	w.	nne.	......	·22	......	·39
28.	29·577	29·505	29·22	29·48	29·55	29·73	29·77	52	27	42·5	48	35	42½	37½	w.	calm	ene.	nne.	·07	......	......	......
29.	30·097	29·807	29·44	29·81	29·99	29·92	30·05	57	35	43	49	33	39	38½	w.	nw.	n.	calm	......	......	......	·05
30.	30·116	29·858	29·77	30·00	29·81	30·07	29·90	54	30	42	51	32	41½	39½	e.	calm	e.	s.	......	·03	0·65	......
31.	29·706	29·606	29·40	29·60	29·28	29·68	29·54	62	41	46	50	33½	37½	35	s.	calm	ne.	ene.	......	......	......	......
Mean.	29·832	29·704	29·38	29·251	29·510	29·510	29·539	53·80	33·06	42·6	48·6	36·9	41·35	39·03					1·00	0·64	3·12	3·47

THE
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[THIRD SERIES.]

JUNE 1846.

LXX. *Researches on the Functions of Plants, with a view of showing that they obey the Physical Laws of Diffusion in the Absorption and Evolution of Gases by their Leaves and Roots.* *By* D. P. Gardner, *M.D., Member of the Lyceum of Natural History, &c.**

1. I CONCEIVE a plant to be a porous system, containing an internal mixture of gases, or plant atmosphere, and lying in contact with common air on the one side, and with the gases dissolved in the fluid of the soil on the other. My object in the following remarks is to show that the plant atmosphere is of a fluctuating nature, and depends on the chemical action taking place; and that whatever gases are absorbed or evolved by leaves or roots, depend upon the nature of the internal atmosphere at the time. To place the evidence of these conclusions before the reader, I propose to examine the subject under five heads:—

1st. The epidermis or bounding membrane of plants is porous.

2nd. The constitution of the internal gas of plants.

3rd. The action of roots on the gases of the soil-fluid.

4th. The absorption of gases by plants is a consequence of their porosity.

5th. The action of plants on artificial atmospheres.

I. *The Epidermis or Bounding Membrane of Plants is Porous.*

2. The object in this place is to show, that the epidermis is not merely capable of transmitting particular gases, but that it obeys all the laws of a porous system. If this be found true for the bounding membrane, it will necessarily be true for the internal cellular structure.

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* Communicated by the Author.

3. Experiments were planned for the purpose of determining whether carbonic acid would penetrate into a vessel containing common air through a barrier of vegetable epidermis, and secondly, whether an inclosed mixture of gases of a theoretical composition would solicit the passage of both carbonic acid and oxygen, and at the same time evolve nitrogen.

4. A tube, five inches long and a third of an inch in bore, with a flattened and ground end, was closed by a piece of epidermis obtained from the leaf of the Madeira vine (*Basella lucida*). The tube was then immersed in a mercurial trough and filled to within an inch of the membrane; on suspending it by a wire it did not leak, though there was a pressure of three inches of mercury. Clear lime-water was admitted to displace the mercury, and the arrangement covered by a small bell-jar containing atmospheric air with 10 per cent. carbonic acid. In five hours the lime-water exhibited a distinct pellicle of carbonate of lime. The same result was obtained in more or less time with the epidermis of the cabbage, *Alanthus alata*, *Chenopodium album*, and several species of *Sedum*. Some specimens, as that from the balsam, leaked so fast as not to sustain any mercurial column, whilst others maintained four inches for thirty hours and more.

5. A similar tube was closed with epidermis, and contained an atmosphere of nitrogen 87, oxygen 13 per cent. over mercury, and was covered with a bell-jar as above. The included volume increased during nine hours from 400 to 433 measures, and on analysis consisted of N 76, O 17, CO_2 7 per cent. Hence the membrane comported itself as a simple porous tissue, allowing nitrogen to pass out and admitting oxygen and carbonic acid. This experiment was also repeated with the foregoing specimens of epidermis, and no absolute variation perceived.

II. *The Constitution of the Internal Gas of Plants.*

6. The observations hitherto made on the included gases of plants by Davy, Payen, Calvert and Ferrand, and others, cannot be quoted here, because the disturbing effects of light and other causes have not been sufficiently considered. It is not merely the gas of a cavity which is required, but the composition of that which permeates the interior during the vigorous state of the vegetable in sun-light.

7. For the purpose of obtaining this, I transplanted in the May of 1845 a number of plants of *Datura stramonium* and of blue grass (*Poa pratensis*) into tumblers, and allowed them to grow for several weeks before use. Having completed my arrangements for analysing minute quantities of gas by a sliding-

rod eudiometer, I proceeded as follows:—The plants when wanted were obtained in a perfect state by immersing the tumbler in a tub of water, and removing the garden-mould by agitating the fluid; they were thus procured without the slightest mutilation. The plant was then transferred to a convenient pneumatic trough, closed from adherent air and broken under a small receiver. This was done uniformly at 11 o'clock A.M. and as quickly as possible; the gas was immediately analysed.

8. Binoxide of nitrogen was the only substance which could be used to estimate the oxygen, and if properly prepared, is, in my experience, equal to the most intricate apparatus. Thus, in twenty-five analyses of air, made as test experiments of the excellence of my measures, there was obtained a mean of 20·83 per cent. oxygen, from which there never was a variation of 0·2 per cent.; this result closely coincides with that obtained in the elaborate researches of Dumas and Boussingault, *i. e.* 21·8 per cent.

9. Six analyses of the internal gas of Datura gave a mean of N 87·5, O 12·5 per cent. without any carbonic acid.

Four analyses of the gas from grass gave a mean of N 86·1, O 13·9 per cent. without carbonic acid. This result closely approximates to the measure published by Dr. Draper in the Philosophical Magazine for the gas drawn by the air-pump from grass.

The mean of all the observations is N 86·75, O 13·25 per cent.; and this I assume as the *normal* or plant atmosphere of the green parts at 11 to 12 o'clock A.M., during vigorous existence in the presence of bright diffused light in summer.

10. It is distinctly to be understood that this conditionally normal atmosphere is perpetually changing, and is true only for the time and place. In the preparatory examination of this subject, I obtained measures of the internal gas consisting of N 84·6, O 13·0, CO_2 2·4 per cent., but overlooked the circumstances. Messrs. Calvert and Ferrand (*Ann. de Ch., &c.,* Aout 1844) found that carbonic acid was always present at night, and give as the composition of the gas from the hollow stems of *Phytolacca decandra* at night, N 76·4, O 20·6, CO_2 0·3 per cent.

III. *The Action of Roots on the Gases of the Soil-Fluid.*

11. There are no observations on the action of roots known to me, except those of DeCandolle (*Phys. Veg.* t. i. p. 248), who asserts that uninjured roots exhale no gas, either in light or darkness. Most physiologists infer, that whatever gases exist in the soil-fluid are absorbed therewith; but this is an

unphilosophical view, for it leaves out of consideration the capacity of the sap to absorb them and its condition as to gaseous saturation. In making experiments on the subject, it is also necessary to consider the functions of the plant.

12. On the 25th of June 1844, I commenced a series of observations to determine the action of uninjured roots of Datura and blue grass on the gas dissolved in pump-water, which accurately represents the soil-fluid. The plants were obtained as detailed in section 7; they were placed in vessels resembling a bird-fountain, which were capable of being replenished with water to compensate for the evaporation of the leaves, and also of collecting any gas passing from the roots. Three sets of experiments were made: A, the roots and leaves were placed in darkness; B, both portions were exposed to bright diffused light; C, the leaves were illuminated, but the roots in darkness.

13. On the evening of the 25th of June, two sets of plants were arranged according to these plans. The Daturas, B, yielded the next morning at 11, a gas the composition of which was N 96·6, O 3·4 per cent.; these two plants were then placed in a dark cupboard for thirty-six hours and evolved no gas whatever; on again exposing them to light, they produced a mixture of N 96·2, O 3·8 per cent. as the mean of six analyses. The grass plants, B, gave off but little gas, and only enough was collected for two measures, which yielded a mean of N 96, O 4 per cent.

The plants C conducted themselves in the same way as B; the Daturas gave gas for six analyses, the mean of which was N 96·5, O 3·5 per cent.

The plants A, placed in darkness, gave no gas whatever, although they were attended to for five days.

14. We conclude that roots appear to evolve gas unequally in quantity; that the action of light on the leaves is essential to this phænomenon; and thirdly, it, the exposure of the root, does not seem to have any effect on the result. I do not believe that the gas is evolved from the interior of the plant, but that the roots disturb the equilibrium of the mixture in the water, so that all the carbonic acid is withdrawn and most of the oxygen, leaving behind the sparingly soluble nitrogen, which acquires the elastic condition. That this gaseous disturbance was not a mechanical effect of light and heat, I satisfied myself by observations at the time; and the results of Prof. Morren (*Ann. de Chimie, &c.*, Sept. 1844) show that the sun's light liberates carbonic acid and nitrogen, accumulating oxygen in the water, which is opposed to the effects here observed.

15. The gas of the pump-water was N 48, O 22, CO_2 30 per cent., therefore the roots absorbed carbonic acid and oxygen in the same way as a porous system containing the normal plant atmosphere, N 86·75, O 13·25 per cent.; this continued during daylight, or during the activity of the vegetable, but in darkness all the gas of the water is taken up without any selection.

IV. *The Absorption of Gases by Plants is a consequence of their Porosity.*

16. We are now in possession of sufficient data to state the case. A porous system lies between two media and contains a certain mixture of gases; the gases of the three systems are

	The air.	The plant-gas.	The water-gas.
Carbonic acid .	0·05	0·00	30·00
Oxygen . . .	20·80	13·25	22·00
Nitrogen . . .	79·15	86·75	48·00
	100·00	100·00	100·00

If the plant-gas were confined within an extremely delicate caoutchouc bag and surrounded by either atmosphere, it would soon be disturbed by penetration, nitrogen would be evolved, and carbonic acid and oxygen absorbed. The rapidity of the interchange would depend on the gas and the nature of the membrane.

17. That the action of roots on the water-gas is coincident with this theoretical view, I have attempted to show in the last division of the subject. The experiments detailed in article 5 were also made with this object. It must be remembered that the operation of the plant atmosphere upon the gases of the soil is not direct, as in the leaves, but through the intervention of the sap, which contains a mixture of gases dependent upon those of the interior of the plant, but has a greater capacity for carbonic acid and oxygen than for nitrogen.

18. In the case of leaves, the physical theory of porosity is more strikingly made out, because the internal gas is here in contact with atmospheric air, the epidermis only lying between. The movements witnessed in the experiments of art. 5, represent those of a vigorous plant in sun-light; carbonic acid and oxygen are absorbed and nitrogen evolved. It is not uniformly admitted that oxygen is absorbed and nitrogen evolved in plants, but an investigation of this point leaves us under the conviction that the negative is untenable. Saussure, DeCandolle, Palmer, Daubeny, Draper and others have witnessed the evolution of nitrogen. The continued absorption of oxygen is confirmed by Saussure, Davy, Gough, Achard,

Scheele, Cruickshank and others. Gough's interesting observation, that plants grown in darkness do not become green in light unless oxygen be present, is accounted for by the fact that chlorophylle is an oxidized product.

19. The movements occurring during light owe their continuance to its action. The gases absorbed are destined to equilibrate the internal atmosphere, but cannot effect this object so long as carbon and oxygen are fixed by the plant. Hence the current becomes continuous during daylight, the oxygen and carbon being removed faster than they penetrate.

20. But during darkness the stream is arrested, carbonic acid is no longer drawn from the air, but often evolved, as observed by Ingenhousz and Saussure. Oxygen is for a time required to satisfy chemical affinities, and afterwards the internal gas resembles atmospheric air; 20·6 per cent. oxygen is found, and amounts of nitrogen and carbonic acid differing with the nature of the soil-fluid, the latter gas sometimes rising to 3 per cent., when it is evolved; and at others not attaining a proportion much higher than that of the air, when it is not thrown off during night, as shown by Mr. Pepys.

21. It is the decomposition of the carbonic acid within the plant by the early sun-beams which creates the absorption from without by deranging the internal atmosphere. Carbonic acid being decomposed, a temporary excess of oxygen is produced, which causes a portion to pass outwards; but in preparing the plant for examination, any excess of this body seems to have been removed, so that it is probable that in the living organism oxygen gas is a much more important element than is usually admitted by physiologists.

V. *The Action of Plants on Artificial Atmospheres.*

22. To show beyond a doubt that the penetration of gases into plants is a physical and not vital process, we adduce the effects of artificial atmospheres. In these experiments the gases given out and absorbed are not of a definite mixture, but depend altogether on diffusion.

23. M. Marcet placed the same fungi in atmospheres of common air, oxygen, and nitrogen; and after eight to ten hours they changed 100 measures of—

	Air.	Oxygen.	Nitrogen.
into Oxygen . . .	2·0	31·3	0·0
Nitrogen . . .	77·0	24·0	96·1
Carbonic acid .	21·0	44·7	3·9
	100·0	100·0	100·0

without alteration of volume (*Ann. de Chimie, &c.,* t. lviii.

p. 407). It is evident the normal atmosphere of these fungi contained an excess of nitrogen and carbonic acid. Th. de Saussure found that seeds germinating in air absorbed nitrogen, but when placed in a mixture of N 50, O 50, they no longer did so (*Mem. de la Soc. de Genève*, t. vi. p. 545).

24. By overlooking the laws of penetration, DeCandolle, Saussure, Ingenhousz, and Plenk are thrown into contradictory positions in their experiments on the action of the green parts of plants on artificial atmospheres. Thus DeCandolle (*Phys. Veg.* t. i. p. 133), "Les parties verts laissent moins de gas oxigène dans le gas hydrogène que dans le gas azote; elles ne paraissent, contre l'assertion d'Ingenhousz, absorber ni l'un ni l'autre. Il parait aussi certain, malgré l'assertion de Plenk, qu'elles n'exhalent point de gas azote, sauf dans quelques cas, par les corolles."

25. In the summer of 1844 I tested this question by placing some specimens of the *Conferva mucosa* in pump-water and in carbonated water, and allowing them to act for several days on the same water, removing each day the gas generated during light; the plants were therefore subjected in their natural medium to different mixtures of gases dissolved in the fluid. The result was, that the plants in pump-water gave in six hours a gas consisting of O 73, N 27 per cent.; in twenty-four hours, O 53, N 47 per cent.; in forty-eight hours, O 18·6, N 81·4 per cent. In carbonated water, in six hours, O 68 per cent.; in twenty-four hours, O 63 per cent.; in forty-eight hours, O 12, N 88 per cent.; in seventy-two hours, O 3·5, N 96·5 per cent. And these plants continued healthy and acted as before when fresh water was added.

26. *Conclusion.*—From the preceding evidence I infer that plants constitute a simple porous system. The advantages resulting from this philosophical view of vegetation, both in assimilating facts hitherto insulated and in criticising experimental arrangements in vegetable physiology, constitute its chief recommendation. For illustration, we adduce two general laws springing from this theory:—1st. No hypothesis nor argument can be based on the composition of the gases expired by plants without the strictest regard to the disturbing influence of light, the gases of the soil-fluid. 2ndly. No experiments on the action of plants in sun-light can be accepted in determining the functions of leaves unless made in atmospheric air.

27. Finally, I beg to present the following summary of conclusions as fairly deducible from the preceding experiments:—

1. The epidermis of plants, so far as experiments have been made, is porous, and permits the passage of gases according to physical laws.

2. The roots, during the existence of chemical changes in plants, absorb such gases from the soil-fluids as will indirectly satisfy the requisitions of the internal atmosphere.

3. The internal gas of plants fluctuates with the forces which operate on the plant; during the active state of the green vegetable, it resembles a mixture of nitrogen 86·75, oxygen 13·25 per cent., but at night contains more oxygen and a proportion of carbonic acid.

4. The porosity of the entire plant is fully established by its action on artificial atmospheres.

Therefore the physical structure of plants is that of a porous system subject to the laws of diffusion of gases, and endowed with no vitality other than the power of forming cytoblasts and arranging cellules after a definite type.

LXXI. *On the relation of Ozone to Hyponitric Acid.*

By Dr. C. F. SCHŒNBEIN [*].

THE chemical effects produced by atmospheric air charged with hyponitric acid (NO_4) are so very like those caused by ozonized air, that some chemists are inclined to consider hyponitric acid as identical with ozone. Both decompose iodide of potassium, transform the yellow prussiate of potash into the red one, decompose sulphuretted hydrogen, colour blue the resin of guaiacum, destroy organic colouring matters, polarize negatively platinum, &c.

In spite of the similarity of chemical properties exhibited by ozone and hyponitric acid, those substances are in many other respects so entirely different that their being identical cannot be thought of: thus ozone is produced under circumstances in which an essential constituent part of hyponitric acid is absent, namely nitrogen. The analogy existing between the chemical action of the two bodies mentioned appears however so striking, that we can hardly help suspecting some connexion to exist between them, and with the view of ascertaining that connexion I have of late made many experiments, the account of which will form the substance of this paper.

The results obtained from these researches are, in my opinion, such as to speak strongly in favour of my conjecture that there exists a compound composed of $NO_2 + HO_2$. It

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[*] Communicated by the Chemical Society; having been read November 3, 1845.

appears to me probable that besides hydrate of nitric acid a peroxide of nitrogen and hydrogen is formed when water acts upon hyponitric acid. Agreeably to my view, the presence of $NO_2 + HO_2$, in the acid mixture, is the cause why the latter decomposes iodide of potassium, &c.

I think it cannot be denied that aqueous vapour will act upon vaporous hyponitric acid as liquid water does upon the liquid acid. Such being the case, and supposing that in the latter case two compounds of the formulas $NO_4 + HO_2$ and $NO_2 + HO_2$ are produced, it must then be admitted also that the same compounds are formed when the vapours of hyponitric acid are mixed with moist atmospheric air. Now if the peroxide of nitrogen and hydrogen (the first of these compounds) should happen to be a volatile substance, it follows further that in a bottle containing moist air, to which vapour of hyponitric acid had been added, an atmosphere must be produced containing some $NO_2 + HO_2$, and possessing the property of causing the reactions above mentioned. The peroxide of hydrogen contained in $NO_2 + HO_2$ would be the oxidizing agent, decomposing *e. g.* iodide of potassium, &c.

For various reasons I am inclined to consider ozone as HO_2, *i. e.* a compound isomeric with Thenard's peroxide of hydrogen. Now if this conjecture should be true, and if there should exist a compound of $NO_2 + HO_2$, moist atmospheric air being charged with vapour of hyponitric acid would owe its reactions to the presence of ozone. How far such a conjecture is founded upon facts, the experiments I am about to detail will show.

If a piece of carbonate of ammonia, having been strongly ozonized by the means of phosphorus, is suspended in atmospheric air until the latter be so charged with ammoniacal vapours as rapidly to restore the blue colour of reddened litmus paper, that atmosphere continues to enjoy bleaching powers, decomposes iodide of potassium, colours blue the paste of starch containing that salt, transforms the yellow prussiate of potash into the red one, colours the resin of guaiacum blue, discharges the colour of sulphuret of lead; continues, in one word, to possess all the properties belonging to ozone. Hence it follows that ozone is capable of co-existing with the vapours of carbonate of ammonia without suffering decomposition; and I have ascertained that pure ammonia also does not perceptibly destroy ozone. Now if there exists $NO_2 + HO_2$, we may presume from its constitution that it will likewise be able to co-exist with the vapours of the carbonate of ammonia, NO_2 being of itself inactive towards ammonia.

Hyponitric acid, or red fuming nitric acid, was gradually mixed up with so much water as to obtain a colourless liquid.

The bottom of a spacious bottle was covered with this mixture, and then a large piece of carbonate of ammonia suspended within the vessel. After the atmosphere, standing over the acid liquid, had assumed the power of colouring rapidly blue a strip of reddened litmus paper, it continued to possess the following properties:—

1. Strips of paper charged with paste of starch containing some iodide of potassium were coloured blue.

2. Strips of paper drenched with an alcoholic solution of guaiacum assumed a blue colour.

3. Strips of paper coloured blue by a solution of indigo turned white.

4. Strips of paper to which sulphuret of lead had been attached, by means of nitrate of lead and sulphuretted hydrogen, gradually turned white.

5. Strips of paper charged with a solution of the common prussiate of potash became deeply yellow.

6. Crystals of the yellow prussiate, after having been suspended for twenty-four hours within this atmosphere, were covered with a crust of the red sesqui-ferrocyanuret of potassium.

From the facts just stated, it appears that the atmosphere in question acts exactly in the same way as ozonized air does, and from the circumstances under which those reactions took place, it follows that the latter could not proceed from free hyponitric or nitrous acid, these acids not being able to coexist in a state of isolation with the vapours of carbonate of ammonia. We must therefore conclude from these facts, that there was a principle present, in the atmosphere mentioned, which acted after the manner of ozone, and conducted itself, in spite of the presence of ammoniacal vapour, as a highly oxidizing agent.

But if neither free hyponitric nor nitrous acid were the cause of the reactions mentioned, nor nitrite of ammonia, what then is the substance to which the oxidizing powers are to be ascribed? I can answer that question only by supposing that the peroxide of nitrogen and hydrogen is that agent. Before passing to another subject, I take the liberty to mention a circumstance which seems to bear upon the matter in question, and merit some attention. On breathing strongly ozonized air three or four times, a disagreeable and strangling sensation will be experienced near the throat and in the chest. This sensation is very similar to that caused by inhaling air which has stood for some time over a mixture of hyponitric acid and water, and this is the case even if the air happens to be charged with ammoniacal vapours. We observe also in such an atmosphere a peculiar and disagreeable odour, which is similar to that of common aquafortis, and slightly analogous to the smell of chlorine. It is likely that the odour mentioned belongs to the vapour of our supposed peroxide of nitrogen and hydrogen, and that it is *that* compound which, if inhaled, causes the sensation before mentioned. With regard to the subject under discussion, it seems to me that the way in which the mixture, formed from hyponitric acid and water, acts upon a solution of the yellow prussiate of potash, offers peculiar interest.

My experiments having demonstrated that the salt just mentioned (be it solid or dissolved in water) is readily transformed into the red cyanuret by ozone, and considering the acid mixture before alluded to as an aqueous solution of nitric acid and peroxide of nitrogen and hydrogen, what must happen if that mixture be put together with a solution of the yellow prussiate of potash? Supposing 1 equiv. of the said peroxide and 1 equiv. of nitric acid in the acid mixture to act upon 2 equiv. of the yellow cyanuret, we must obtain from such a reaction 1 equiv. of nitrate of potash, 1 equiv. of the red sesqui-ferrocyanuret of potassium, 1 equiv. of deutoxide of nitrogen, and 2 equiv. of water, for

$$(NO_4+HO_2)+(NO_2+HO_2)+2(2P\,Cy+Fe\,Cy)=PO\,NO_5 \\ +(3P\,Cy+Fe_2Cy_3)+NO_2+2HO.$$

If a glass tube, open at one end, be half-filled with our acid mixture and half with a dilute solution of the yellow prussiate, on mixing the liquids together a lively disengagement of gas takes place; and if the open end of the tube be put into a vessel holding water, a colourless gas will fill the upper part of the tube. On adding oxygen or atmospheric air to the gas disengaged under the circumstances mentioned, the latter will turn brownish red and exhibit all the properties of deutoxide of nitrogen. As soon as the acid mixture comes in contact with the nearly colourless solution of the common prussiate of potash, the latter turns deeply yellow, and it is very easy to ascertain that the coloured fluid contains nitrate of potash, sesqui-ferrocyanuret of potassium, and no trace of the yellow prussiate, provided a sufficient quantity of the acid mixture had been employed. The reactions indicated are therefore such as they ought to be, if, according to our supposition, the acid mixture contains nitric acid and peroxide of nitrogen and hydrogen. I need hardly say that the disengagement of deutoxide of nitrogen and the transformation of the yellow cyanuret into the red one, which take place under the circumstances mentioned, cannot originate in the nitric acid contained in the acid mixture, for it is well known that dilute pure nitric acid does not give rise to the reactions described. The cause of those effects must therefore be sought in another substance, and both hyponitric and nitrous acid not being able to coexist with free water, we are not allowed to consider either the one or the other of those acids as that cause. As I have already remarked, the reactions in question can, according to my opinion, only be accounted for in a satisfactory manner by admitting the presence of $NO_2 + HO_2$ in the acid mixture so often mentioned. My experiments have further shown that iodide of potassium, be it solid or dissolved in water, is readily decomposed by ozone, iodine being eliminated. In putting our acid mixture to a solution of the salt mentioned, deutoxide of nitrogen is abundantly disengaged, iodine precipitated, and nitrate of potash formed. As pure nitric acid containing as much water as the said acid mixture, does not act upon the solution of iodide of potassium, it cannot be the nitric acid of our mixture that causes those phænomena, nor can they, from reasons already stated, proceed from hyponitric or nitrous acids.

If we admit that there is, besides nitric acid, some $NO_2 + HO_2$ present in the acid mixture, I think we may easily account for those reactions. HO_2 oxidizes the potassium of the iodide, nitric acid unites with the base thus formed, and NO_2 is set free.

$$(NO_4 + HO_2) + (NO_2 + HO_2) + PI = PO\,NO_5\,(PO_2\,NO_4) + I + NO_2 + HO.$$

The facts I am about to state are most likely also connected with the subject under discussion.

Largely diluted pure nitric acid, not colouring in the least paste of starch containing chemically pure iodide of potassium, when put for a short time in contact with a number of metals, as zinc, iron, lead, copper, mercury, silver, &c., acquires the property—1. To colour deeply blue the paste mentioned. 2. To transform the yellow cyanuret into the red one. 3. To colour blue the resin of guaiacum. 4. To decompose sulphuretted hydrogen, &c.

Tin is an exception to the rule, for however long dilute nitric acid may have been in contact with that metal, it does not cause the reactions indicated. On the contrary, an acid having the properties mentioned, loses them when mixed in proper quantities with dilute nitric acid which has been in contact with tin. The latter acid has also the power of discharging the colour of paste of starch rendered blue by iodine. From the facts stated, it seems to follow, that when dilute nitric acid is acted upon by oxidable metals, the same oxidizing agent is produced that forms on mixing hyponitric acid with water; and it appears also that tin engenders with dilute nitric acid a deoxidizing matter, *i.e.* a nitrate of protoxide of tin.

LXXII. *On the Composition of the Fire-Damp of the Newcastle Coal Mines.* *By* Thomas Graham, *Esq., F.R.S.*[*]

SOME years ago I examined the gas of these mines, with the same result as Dr. Henry, Davy and Dr. Turner had previously obtained, namely, that it contains no other combustible ingredient than light carburetted hydrogen. But the analysis of the gas of the coal mines in Germany, subsequently published, showing the presence of other gases, particularly of olefiant gas, has rendered a new examination of the gas of the English mines desirable. The gases were,—1, from a seam named the Five-Quarter seam, in the Gateshead colliery, where the gas is collected as it issues, and used for lighting the mine; 2, the gas of Hebburn colliery, which issues from a bore let down into the Bensham seam—a seam of coal which is highly charged with gas, and has been the cause of many accidents; and 3, gas from Killingworth colliery, in the neighbourhood of Jarrow, where the last great explosion occurred. This last gas issues from a fissure in a stratum of sandstone, and has been kept uninterruptedly burning, as the means of lighting the horse-road in the mine, for upwards of ten years, without any sensible diminution in its quantity. The gases were collected personally by my friend Mr. J. Hutchinson, with every requisite precaution to ensure their purity, and prevent admixture of atmospheric air.

The usual eudiometrical process of firing the gases with oxygen was sufficient to prove that they all consisted of light carburetted hydrogen, with the exception of a few per cent. The results were as follows:—

Gateshead Gas.—Specific gravity 0·5802.

Carburetted hydrogen	94·2
Nitrogen	4·5
Oxygen	1·3
	100·0

The density of such a mixture is, by calculation, 0·5813.

Killingworth Gas.—Specific gravity 0·6306.

Carburetted hydrogen	82·5
Nitrogen	16·5
Oxygen	1·0
	100·0

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The theoretical density of this gas, deduced from its composition, is 0·6308.

The Hebburn gas was of specific gravity 0·6327.

Seventy-nine measures of the Killingworth gas, mixed with an equal volume of chlorine, left in the dark for eighteen hours, and afterwards washed with alkali, were reduced to 75 measures; from which the presence of 4 measures of olefiant gas might be inferred. But in a comparative experiment made at the same time on 25·3 measures of pure gas of the acetates, mixed with an equal volume of chlorine, a contraction occurred of 1·3 measure; that is, in exactly the same proportion as with the fire-damp.

It was observed that phosphorus remains strongly luminous in these gases, mixed with a little air, while the addition to them of one-four-hundredth part of olefiant gas, or even a smaller proportion of the volatile hydrocarbon vapours, destroyed this property. Olefiant gas itself, and all the allied hydrocarbons, were thus excluded.

Another property of pure light carburetted hydrogen, observed by myself, enabled me to exclude other combustible gases, namely, that the former gas is capable of entirely resisting the oxidating action of platinum black, and yet permits other gases to be oxidated which are mixed with it even in the smallest proportion, such as carbonic oxide and hydrogen, the first slowly and the last very rapidly; air or oxygen gas being, of course, also present in the mixture. Now platinum black had not the smallest action on a mixture of the gas from the mines with air. No moisture appeared or sensible contraction, and no trace of carbonic acid could be discovered after a protracted contact of twenty-four hours; while, with the addition of one per cent. of hydrogen, the first effects were conspicuously evident in three minutes, and with the same proportion of carbonic oxide, the gas became capable of affecting lime-water in half an hour. These experiments were repeated upon each of the three specimens of fire-damp.

Potassium fused in the fire-damp did not become covered with the green fusible compound of carbonic oxide, nor occasion any contraction. Indeed, however carefully the heat was applied to the potassium by means of an oil-bath, a slight permanent expansion always ensued. The same thing occurred in pure gas of the acetates. It appeared that potassium could not be heated above 300° Fahr. in pure carburetted hydrogen, without causing a decomposition and the evolution of free hydrogen gas.

The gas was also inodorous, and clearly contained no appreciable quantity of any other combustible gas than light carburetted hydrogen. The only additional matters present were nitrogen and oxygen; the specimen collected in the most favourable circumstances for the exclusion of atmospheric air, namely, that from the Bensham seam, still containing 0·6 per cent. of oxygen. The gases also contained no carbonic acid.

It is worthy of observation, that nothing oxidable at the temperature of the air is found in a volatile state associated with the perfect coal of the Newcastle beds. The remarkable absence of oxidability in light carburetted hydrogen appears to have preserved that alone of all the combustible gases originally evolved in the formation of coal, and which are still found accompanying the imperfect lignite coal of Germany, of which the gas has been examined. This fact is of geological interest, as it proves that an almost indefinitely protracted oxidating action of the air must be taken into account in the formation of coal; air finding a gradual access through the thickest beds of superimposed strata, whether these strata be in a dry state or humid.

In regard to measures for preventing the explosion of the gas in coal mines, and of mitigating the effects of such accidents, I confine myself to two suggestions. The first has reference to the length of time which the fire-damp, from its lightness, continues near the roof, without mixing uniformly with the air circulating through the workings. It was found that a glass jar, of six inches in length and one inch in diameter, filled with fire-damp, and left open with its mouth downwards, continued to retain an explosive mixture for twenty minutes. Now it is very desirable that the fire-damp should be mingled as soon as possible with the whole circulating stream of air, as beyond a certain degree of dilution it ceases to be explosive. Mr. Buddle has stated, "that immediately to the leeward of a blower, though for a considerable way the current may be highly explosive, it often happens that after it has travelled a greater distance in the air-course, it becomes perfectly blended and mixed with the air, so that we can go into it with candles; hence, before we had the use of the Davy lamp, we intentionally made 'long runs,' for the purpose of mixing the air." It is recommended that means be taken to promote an early intermixture of the fire-damp and air; the smallest force is sufficient for this purpose; as a downward velocity of a few inches in the second will bring the light gas from the roof to the floor. The circulating stream might be agitated most easily by a light portable wheel, with vanes, turned by a boy, and so placed as to impel the air in the direction of the ventilation, and not to impede the draft. The gas at the roof undoubtedly often acts as an explosive train, conveying the combustion to a great distance through the mine, while its continuity would be broken by such mixing, and an explosion, when it occurred, be confined within narrower limits.

Secondly, no effective means exist for succouring the miners after the occurrence of an explosion, although a large proportion of the deaths is not occasioned by fire, or injuries from the force of the explosion, but from suffocation by the after-damp, or carbonic acid gas, which diffuses itself afterwards through all parts of the mine. It is suggested that a cast-iron pipe, from eight to twelve inches in diameter, be permanently fixed in every shaft, with blowing apparatus, above, by which air could be thrown down, and the shaft itself immediately ventilated after the occurrence of an explosion. It is also desirable that, by means of fixed or flexible tubes, this auxiliary circulation should be further extended, and carried as far as practicable into the workings.

LXXIII. *Observations on the Resin of the* Xanthorœa hastilis, *or Yellow Gum-resin of New Holland. By* JOHN STENHOUSE, *Esq., Ph.D.*[*]

THIS remarkable resin, which is known in commerce as the yellow gum or acaroid resin of Botany Bay, exudes from the *Xanthorœa hastilis*, a tree which grows abundantly in New Holland, especially in the neighbourhood of Sidney. This resin was first described in Governor Phillips's Voyage to New South Wales in 1788. Mr. Phillips states that it was employed by the natives and first settlers as a medicine in cases of diarrhœa. The resin as it occurs in commerce sometimes forms masses of considerable size, but as it is very brittle, although tolerably hard, it usually arrives in the state of a coarse powder. Its colour is a deep yellow, with a slightly reddish shade, considerably resembling gamboge, but darker and less pleasing. The colour of its powder is greenish yellow. When chewed it does not dissolve or stick to the teeth, but tastes slightly astringent and aromatic like storax or benzoin. Its smell is very agreeable and balsamic. When gently heated it melts, and when strongly heated it burns with a strong smoky flame, and emits a fragrant odour resembling balsam of Tolu. The resin contains a trace of an essential oil, to which much of its agreeable smell is probably owing. This oil passes into the receiver when the resin is

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distilled with a mixture of carbonate of soda and water, but its quantity is so small that I was unable to examine it more closely. The resin is insoluble in water, but dissolves readily both in alcohol and in æther, especially in the former. Its solution in alcohol has a brownish yellow colour; the addition of water precipitates it as a dark yellow mass, but it does not crystallize out of its alcoholic solution when left to spontaneous evaporation, but remains as a varnish. When digested with strong alkaline lyes, it readily dissolves and forms a brownish red solution; and when the alkali is neutralized with muriatic acid, the resin is precipitated considerably altered as a dark brownish brittle mass. On concentrating the solution out of which the resin has been precipitated, and allowing it to cool, a quantity of impure reddish crystals resembling benzoic acid are gradually deposited. It requires repeated and long-continued digestions with the strongest alkaline lyes to remove the whole of this crystalline acid from the resin, which retains it with very great tenacity. The quantity of the acid is by no means great. It is not easily purified, as its crystals are apt to retain a trace of a reddish colouring matter, from which it is very difficult to free them. The easiest way of getting rid of it, is by dissolving the impure crystals in a small quantity of alcohol and then adding water; the greater portion of the colouring matter is retained in solution, while the crystals are precipitated tolerably white. When purified by repeated crystallizations, they become quite colourless. In appearance, taste, and smell they closely resemble benzoic acid. When dried at 212° F. and subjected to analysis,—

I. 0·2284 grm. of substance gave 0·6005 CO_2 and 0·113 HO.

II. 0·2955 grm. of substance prepared on a different occasion gave 0·790 CO_2 and 0·1505 HO.

	Found.			
	I.	II.	Cinnamic acid.	Benzoic acid.
C .	71·74	72·91	73·35	68·85
H .	5·49	5·65	5·32	4·91
O .	22·77	21·44	21·33	26·24
	100·00	100·00	100·00	100·00

It is evident from these analyses that the crystalline acid contains nearly the same amount of carbon and hydrogen as cinnamic acid, with some deficiency however in the carbon. I was led therefore to suspect that it consisted essentially of cinnamic acid, with probably a small admixture of benzoic acid, a suspicion which subsequent experiments tended fully to confirm; for on heating a quantity of the crystals with some peroxide of manganese and sulphuric acid, oil of bitter almonds was immediately evolved, and on boiling a second portion with hypochlorite of lime, the very peculiar chlorinated oil described in a former paper was also abundantly produced, thus clearly indicating the presence of cinnamic acid. A third portion of the crystals was dissolved in alcohol and left to spontaneous evaporation; it yielded after some time the fine rhombic prisms so characteristic of cinnamic acid when it is crystallized out of alcohol, mixed however with some long acicular crystals, having all the appearance of benzoic acid. I think myself warranted to conclude therefore that Botany Bay resin contains cinnamic acid mixed with a very little benzoic, in which respect it resembles balsam of Tolu, which contains both cinnamic and benzoic acids, though fortunately in much greater abundance.

Action of Nitric Acid on the Resin.

When the resin is treated with moderately strong nitric acid in the cold, a violent action ensues with the evolution of nitrous fumes. The resin is completely dissolved if the quantity of the nitric acid is considerable. The colour of the solution is dark red, but by boiling it becomes of a bright yellow colour. The liquid should be evaporated to dryness on the water-bath, to get rid of the great excess of nitric acid. The residue forms a mass of fine yellow crystals, consisting chiefly of carbazotic acid, but mixed with some oxalic and a little nitrobenzoic acids. The nitrobenzoic acid is evidently derived from the cinnamic acid in the resin. The carbazotic acid is easily separated from these other acids by converting it into carbazotate of potash, which is easily purified by one or two crystallizations, and then by decomposing the salt with muriatic acid, pure carbazotic acid may be obtained.

0·3823 grm. of the acid, dried at 212° F., gave 0·442 CO_2 and 0·049 HO.

	Found.	Calculated numbers.
Carbon . .	31·53	31·37
Hydrogen .	1·42	1·30
Oxygen . .	67·05	67·33
	100·00	100·00

0·3975 grm. of the potash salt, decomposed by sulphuric acid and then ignited with carbonate of ammonia, left 0·1300 of sulphate of potash = 17·68 per cent. of potash; calculated quantity 17·60.

The silver salt was also formed by boiling the acid with carbonate of silver. It is a very soluble salt, which crystallizes in fine red-coloured needles. 0·8975 grm. of the salt gave 0·372

Cl Ag = 31·22 Ag, or 33·53 per cent. oxide. The calculated numbers are 31·27 per cent. of silver = 33·59 oxide.

The quantity of carbazotic acid which Botany Bay resin yields when treated with nitric acid is so great, and it is so easily purified, that this resin seems likely to prove the best source of that substance. When the resin is subjected to destructive distillation in an iron or copper retort, it yields a very large quantity of a heavy acid oil mixed with a very small quantity of a neutral oil, which is lighter than water. If however the resin has been previously digested with alkaline lyes, so as to remove all the cinnamic and benzoic acids it contains, the heavy oil is obtained as before, but none of the light essential oil. The acid oil is readily soluble in potash and soda lyes; in its smell and properties it resembles creosote; when it is digested with nitric acid, it is wholly converted into carbazotic acid, and when a slip of fir-wood is dipt in it, and then moistened with either muriatic or nitric acid, the deep blue colour passing quickly into brown, so characteristic of hydrate of phenyle, is immediately produced, with which substance the oil appears completely identical. The light oil above mentioned, the quantity of which is extremely small, is separated from the hydrate of phenyle by saturating it with an alkali and distilling the mixture in a glass retort with a gentle heat. In smell and properties it resembles benzine, and is most probably a mixture of benzine and cinnamene; unfortunately the quantity obtained was so small, that I was unable to subject it to more particular examination.

LXXIV. *On the Constitution of Matter.*

By H. Sloggett, *Esq.*

To Richard Taylor, Esq.

Sir,

HAVING observed in your Journal for December 1845 some remarks on Prof. Faraday's speculation on the constitution of matter by Mr. Laming, wherein he attempts to show, that by a peculiar way of considering the theory of atoms the conducting and insulating powers of bodies appear more intelligible than on any other doctrine, I have been induced to send you a few ideas of mine on the subject, with a hope that you may not consider them unworthy of insertion.

The test of the truth of any hypothesis, is its accordance with all known facts; and any discrepancy, even a single one, between a theory and experiment, is, if not cleared up, fatal to its validity. The one-fluid theory, in electricity though preferable to its rival in perhaps all other respects, has hitherto been incapable of being generally received, on account of its giving unsatisfactory results when submitted to mathematical analysis. In endeavouring to obviate this difficulty, I conceived the object completely attained, by a supposition somewhat analogous to Mr. Laming's, though essentially different in respect to the assumption of solid atoms. As we know matter only by its properties, it certainly seems more rational to call those properties themselves matter, than to invent an imaginary substance with inseparably attached attributes.

In Mr. Laming's theory I can see no vindication of the theory of solid atoms, because it is not necessary for them to be admitted. He might consider the term "atom" to signify nothing more than "centre of attraction." With this qualification I agree with him, that different atoms are naturally associated with different quantities of electricity; arising, however, from different degrees of power in the atoms, or centres of attraction. An objection apparently arises, *in limine*, to his supposition of incomplete external strata. How are they to remain incomplete when placed in circumstances adapted to supply them with as much electricity as would be necessary to complete them? Or in other words, why should they not retain the electricity which they have once received? I have mentioned this objection because it appears to be an essential point in Mr. Laming's paper, and because it is in fact the only one, except that before mentioned, so far as it goes, in which his hypothesis differs materially from my own. It will be proper to premise, that by the word atom, I mean nothing more than a centre or combination of centres of attractive or repulsive force; those combinations of centres, when they occur, occupying the same point; implicating, in opposition to the usual notion, that matter may be penetrable. It is necessary that this be remembered, because the word in its common sense involves circumstances incompatible with another meaning.

Philosophers have long considered it established that the atoms of bodies attract each other; and it cannot but be admitted that a repulsive principle between them is just as clearly evidenced. Hence we have just grounds for the assumption that the atoms of bodies are both attractive and repulsive of each other. But this is an inconsistency if an atom be but a single principle, and as there is abundant proof of the existence of an agent distinct from matter in bodies, there are ample reasons for attributing the attracting property to this agent (electricity), and the repulsive power to the matter itself. But it remains to be shown how these are to be united in order to explain the effects which appertain to the action of bodies in general on each other, as well as those which are produced by the agency of electricity.

We assume, then, that the atoms of matter are mutually repulsive of each other, but attractive of those of electricity; and that the atoms of electricity are in like manner self-repulsive and attractive of those of matter. This hypothesis is not new, it was invented long ago to satisfy the Franklinian theory of electricity; but its application has not, to my knowledge, been successfully made. My object here is briefly to show its consistency when rightly applied.

Suppose the centres of matter far more powerful and less numerous than those of electricity. Each atom of the compound will thus consist of an atom (in the sense before stated) of matter surrounded with an atmosphere (so to speak) of electricity, of variable density, in a somewhat similar manner to the air surrounding the earth. This must have a definite limit at some distance from the centre, where the repulsive power of the whole quantity of electricity surrounding the matter on an atom of electricity equals the attractive power of the matter for the same atom; so that beyond this limit none can exist in connexion with the atom. Accordingly every particle of matter will appropriate to itself a definite quantity of electricity dependent on its inherent power; and when any excess above this quantity occurs in a body, it becomes positively electrified, and negatively electrified when there is a deficiency. This admitted, we may enunciate thus: In all bodies, in their natural state, there are two principles reciprocally combined, mutually attractive but each repulsive of itself. If there be an excess of either principle, in one instance the body in which it may occur becomes positively, and in the other negatively electrified.

This will be observed, in effect, to be expressing the two-fluid theory. A simple illustration will exemplify the similarity.

Suppose a conducting sphere charged positively. All its atoms being duly combined with as much as they can retain, it is evident that the superfluous electricity thus thrown on them must, by its elastic property, fly off from them, subject only to an inferior attractive and repulsive force, it being as it were without the effective range of the central forces. Unless retained on the body by a non-conducting medium, it would necessarily fly off entirely. This both the old theories teach us. But how will the case stand when the excitement is negative? This is a question which the partisans of the vitreous and resinous hypotheses were accustomed triumphantly to ask. Indeed I have never seen it answered; arising from no assigned position being given to electricity in its combination with matter. This being done, the solution is easy, and if it be done satisfactorily, the two-fluid theory must be estimated as nothing but a superfluity. In a sphere, the combined atoms of matter concentrate their power in its centre. Thus the exterior atoms have their atmospheres less strongly attached to them than the interior ones, since the tendency of all the combined atoms to attract electricity to the centre of the sphere is greatest at the surface, varying as the distance from the centre; and because this tendency and the force with which the atom attracts its atmosphere are in opposition. Now it is manifest that a force must act when the resistance to it is least; hence if electricity be abstracted from the sphere it must be from the surface, or rather from the atoms on the surface. If the surface be not covered with a non-conducting medium, the atoms will necessarily supply their deficiency from the contiguous conducting ones, and thus cause an equilibrium. It has however yet to be shown how negatively electrified bodies repel each other; indeed electrical attraction and repulsion generally must be explained before the validity of the theory can be assented to; but as both the received theories do this in a nearly similar manner to the present one, it need not now be entered upon. My object has been briefly to prove the sufficiency of one electric agent to elucidate what it has been considered possible to do only by two, and by so doing to furnish a basis for the explanation of the whole series of electrical phænomena. Statical electricity has alone furnished exercise for legitimate theory; and although the identity of it and voltaic electricity has never been doubted, the connexion between their effects has received no solution. The relation between the atoms of bodies and statical electrical excitement, here suggested, seems to furnish a clue by which dynamical electricity may become more intelligible. By dynamical electricity, I mean voltaic effects generally. This may perhaps more clearly appear by considering the influences to which the particles might be conceived to be subject. In order to this, we must have some standard by which we may compare different atoms or centres of matter; so we will assume an unit for that purpose. Not that an atom must consist of one unit only, but that different atoms may consist of different definite units.

Let the repulsion between one unit of matter and another be called R, the repulsion between units of electricity r, the attraction between units of electricity and matter a; let the respective quantities of matter in two different atoms be M and m, and the respective quantities of electricity E and e.

Then the repulsion which these atoms exercise on each other will be represented by

$$MmR + Eer. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

And the attraction will be

$$Mea + mEa. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B.)$$

Imagine a case of equilibrium, then

$$Mmr + Eer = Mea + mEa.$$

Now all those quantities are variable; and it is easy to perceive that the attraction and repulsion will vary with varying values of either of the quantities. At present we wish to know the effect of increase or diminution of the quantity of electricity on an atom.

Now suppose the attraction and repulsion equal, and we get

$$\underset{(1)}{Mea - MmR} = \underset{(2)}{Eer - Ema}.$$

Diminish E then (2) becomes less than (1), and consequently B becomes less than A; that is, the repulsion becomes in excess. This is on the assumption of $m a$ being greater than $e r$. Increase E, and in like manner the attraction becomes in excess. In a similar manner might it be shown that an increase or diminution of e would cause a corresponding attraction or repulsion. Still more so, then, must this occur when both E and e increase or diminish together. This is an evident reason for the repulsion existing between bodies negatively electrified. But its more important feature is the view it would give of voltaic excitement; indicating that the current is simply the appropriation of the electricity holding the elements of the liquid compound in combination, to the formation of the new compound which is essentially always in process. It will not be necessary now to trace any further the effects of this mode of considering the constitution of matter. I have said thus much merely as a preliminary necessary for its reception. Whether similar views may have been entertained before, I know not, but they have been my own for a considerable period past, and in their elucidation of all the facts on which I have tried them, there seems to be greater consistency than on many other suppositions which I have met with. It may be worth while to allude to the manner in which the property of conduction is treated by this theory: it would appear that no body is a perfect conductor; the relation between conductors and non-conductors being merely a question of time and velocity; for each atom of a body exercising an attraction and repulsion on free electricity in it, the facility of transmission will depend on the ratio and intensity of those forces, these again depending on the quantities of electricity and matter in the atoms. This has been merely hinted at, not for explanation, but to show that the theory gives the property referred to, to inherent powers in bodies themselves, and not to the space in which they are situated, and by which they are surrounded. My reasons for considering the atoms of bodies as mere centres of force, have not been given, as they are connected with other subjects on which you may not be able to afford space for entering.

I am, Sir,
Yours, &c.,
HENRY SLOGGETT.

Killigrew St., Falmouth,
Jan. 19, 1846.

LXXV. *Experiments and Observations on the Mechanical Powers of Electro-Magnetism, Steam, and Horses. By the* Rev. WILLIAM SCORESBY, *D.D., F.R.SS. L. and E., Corr. Memb. Inst. Fr., &c., and* JAMES P. JOULE, *Secretary of the Literary and Philosophical Society of Manchester, Mem. Chem. Soc., &c.*[*]

AT the last Meeting of the British Association, Dr. Scoresby described a magnetic apparatus of very great power, and gave an account of some experiments he had made with a view to test its capabilities for exciting electrical currents. The coils employed in those experiments were hastily constructed, and by no means calculated to produce a maximum effect. We agreed, therefore, to construct and try more efficient ones on the first opportunity.

Two kinds of revolving armature occurred to us as worthy of trial. One of them consisted of a hollow tube of drawn iron, 24 inches long, $1\frac{5}{8}$th inch in diameter, and $\frac{3}{16}$ths of an inch thick in the metal, bent into the shape of the letter U. It had a saw-cut along its entire length, in order to prevent the circulation of electrical currents in the substance of the iron. Each of the legs of this armature was wound with 274 feet of covered copper wire, $\frac{1}{10}$th of an inch in diameter. The other armature consisted of two bars of iron, each 20 inches long, 4 inches broad, and $\frac{3}{8}$ths of an inch thick. These bars were bent edgeways into the form of a semicircle, and then fastened together with the interposition of a piece of calico in order to prevent currents in the iron as much as possible. Each leg of this armature was furnished with two coils of covered copper wire $\frac{1}{10}$th of an inch thick. The two coils that were nearest the iron were each 276 feet long; and each of the other two coils was 296 feet long.

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[*] Communicated by the Authors.

Having placed the two straight steel magnets (each of which was 4 feet 4 inches long, 4 to 5 inches square, and had poles of 7½ square inches surface) side by side, in a horizontal position, and with two of their poles connected by a suitable armature, we placed the *hollow electro-magnetic armature* on the axis of a revolving apparatus, in such a position that the poles of the armature could revolve at the distance of about ¼th of an inch from the poles of the steel magnets. The coils were arranged for quantity, and connected by means of a proper "commutator" with platinum plates (each exposing an active surface of 5 or 6 square inches) immersed in a dilute solution of sulphuric acid. The maximum amount of decomposition was effected when the armature revolved 500 times per minute. At this velocity ¾ths of a cubic inch of the mixed gases were collected per minute.

Having removed the hollow armature, we now fastened the *flat semicircular armature* upon the axis. When this armature, with its four coils arranged for quantity, was rotated at the rate of 500 revolutions per minute, we collected as much as 1·4 cubic inch of the mixed gases per minute. With the same velocity of rotation, two inches of steel wire, $\frac{1}{90}$th of an inch thick, were raised to a bright red heat; and one inch of the same kind of wire was fused.

Great as the above effects undoubtedly are, in comparison with previously recorded results, we expect to be able to augment them very much by causing the armatures to revolve opposite the *true poles* of the magnets, and not, as heretofore, opposite their ends. It is proper also to observe, that on account of the imperfect hardness of many of the steel bars*, the magnets did not possess one quarter of the power due to Dr. Scoresby's principle of construction. We have not, however, hitherto cared to reconstruct the apparatus, because our principal object in the present research was to make experiments with the machine working as an engine, for which purpose the magnets were quite powerful enough.

The battery employed for working the machine as an engine, consisted of three cells of Daniell's constant arrangement. In each cell the copper element exposed an active surface of two

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* The bars of which the magnetic apparatus was constructed were of various lengths, but of otherwise uniform dimensions, viz. 1¼ inch broad and ¼th of an inch thick. The thickness and mass were found too great for effective hardening, at least for obtaining a degree of hardness capable of sustaining the severity of the magnetic test. Œconomy and facility of arrangement were the reasons for adopting this construction, rather than the more certain and effective one of *hard thin plates*, described by Dr. Scoresby in his "Magnetical Investigations."

square feet, and the amalgamated zinc plate a surface of ⅔rds of a square foot. A pretty correct galvanometer, consisting of a circle of thick copper wire and a magnetic needle 3 inches long, was employed for measuring the currents of electricity which were transmitted by the battery through the revolving armatures. The tangents of the deflections of the magnetic needle, corrected by a small equation, indicated the absolute quantities of transmitted electricity. The quantity of zinc consumed in the battery was deduced from the deflections of the needle; the data of the calculation being derived from previous experiments on the quantity of mixed gases evolved from acidulated water by a current capable of producing a given deflection of the needle.

Our first experiments were made with the flat semicircular revolving armature, its four coils being arranged for quantity. The deflection of the needle before the engine was allowed to start amounted to 64°, which indicated a current of 2232, calling the current corresponding to 45°, 1000. The engine, being then allowed to start, presently attained a velocity of 140 revolutions per minute. The needle was then observed to stand steadily at 43°, indicating a current of 920. The consumption of zinc in the battery was estimated to be at the rate of 205 grs. per hour.

Although we were not able to apply as exact a dynamometer as we could have wished, we were nevertheless enabled to arrive at a pretty correct estimation of the power developed, by ascertaining the weight which, when thrown over a wheel connected with the engine, was sufficient to keep it in uniform motion. In this way we found that the force developed in the above experiment was equal to raise 21,100 lbs. to the height of a foot per hour.

On making a second experiment with the same revolving armature and battery, we obtained the following results:—Current before the engine was allowed to start, 2232; current when the armature was rotating at the rate of 180 revolutions per minute, 850; consumption of zinc per hour, 190 grains; force given out per hour, 17,820 lbs. raised a foot.

Mr. J. P. Joule has already proved that the heat evolved by voltaic and magneto-electrical currents is, *cæteris paribus*, proportional to the square of their intensity*; and that the power of the electro-magnetic engine is obtained at the expense of the heat due to the chemical reactions of the voltaic battery by which it is worked. He has also shown, that if the whole of the heat developed by the consumption of a grain of zinc in a Daniell's battery could be converted into useful

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* Phil. Mag., vol. xviii. p. 308, and vol. xix. p. 260.

mechanical power, it would be equal to raise a weight of 158 lbs. to the height of a foot*. Hence, if we designate the current when the engine is *at rest* by a, and the current when the engine is *in motion* by b, the heat evolved by the circuit in a given time, will, in the two instances, be as a^2 to b^2. But the quantities of zinc consumed being as a to b, the heat, per a given consumption of zinc, will be as a to b, or directly as the currents; $a - b$ will therefore represent the quantity of heat converted by the engine into useful mechanical effect. Therefore, putting x for the mechanical effect in lbs. raised a foot high per the consumption of a grain of zinc, we have

$$x = \frac{158\,(a - b)}{a}.$$

From the above equation it is evident that the œconomical duty will be a maximum when b vanishes or becomes infinitely small in comparison with a. In this case $x = 158$, while the power of the engine will become infinitely small with regard to work performed in a given time. We must, however, observe that the equation can only be strictly correct when the current b is uniform, which it never can be exactly, in consequence of the resistance of the magnetic induction against the voltaic current varying in the different positions of the revolving electro-magnetic armature. Hence the current b is always, to a certain extent, of a *pulsatory* character, which has the effect of causing it to develope more heat than an *uniform* current of the same quantity. From this circumstance, as well as from the unavoidable existence of some slight currents in the substance of the iron of the revolving armature, the actual œconomical effect will always be somewhat below the duty indicated by our formula.

Applying the formula to our first experiment, we have for the *theoretical* œconomical effect,

$$\frac{158\,(2232 - 920)}{2232} = 92{\cdot}9,$$

while the *actual* œconomical effect was

$$\frac{21100}{205} = 102{\cdot}9.$$

In our second experiment, the theoretical œconomical effect will be

$$\frac{158\,(2232 - 850)}{2232} = 97{\cdot}8,$$

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* Phil. Mag., vol. xxiii. p. 441.

and the actual duty,

$$\frac{17820}{190} = 93 \cdot 8.$$

Taking the mean of the two experiments, we have for the theoretical duty 95·3, and for the actual performance 98·3. Here, therefore, in apparent contradiction to what we have just said, the actual exceeds the theoretical duty. This circumstance is however partly explained by the fact that the solution of sulphuric acid employed in charging the battery had been mixed immediately before the experiments were made, and was in consequence considerably heated; for Daniell has shown that the intensity of his battery increases with its temperature, and it is evident that an increase of the intensity or electromotive force of the cells of the battery must be productive of an increased œconomical effect.

The next two experiments were made with the hollow revolving armature, its two coils being arranged for quantity. In these and the subsequent experiments, the battery was charged with a *cold* solution.

Experiment 3.—Current when the engine was kept at rest, 1381; current when the armature was revolving 80 times per minute, 850; consumption of zinc, 190 grains per hour; power developed, 8800 lbs. raised a foot high per hour. From these data, the theoretical duty will be

$$\frac{158\,(1381 - 850)}{1381} = 60 \cdot 7,$$

and the actual duty will be

$$\frac{8800}{190} = 46 \cdot 3.$$

Experiment 4.—Current before the engine was allowed to start, 1381; current when the engine was revolving 102 times per minute, 678; consumption of zinc, 151 grains per hour; power developed, 9000 lbs. raised a foot per hour. Hence for the theoretical duty we have,

$$\frac{158\,(1381 - 678)}{1381} = 80 \cdot 4,$$

and for the actual duty,

$$\frac{9000}{151} = 59 \cdot 6.$$

Lastly, we made two experiments in which the engine was fitted up with two straight electro-magnets fastened parallel to the axis. Each of these straight electro-magnets consisted of a piece of drawn iron tube, 12 inches long, $1\frac{5}{8}$th inch in diameter, and $\frac{3}{16}$ths of an inch thick, cut longitudinally to prevent the circulation of electrical currents in the iron, and furnished with a coil of 210 feet of covered copper wire $\frac{1}{16}$th of an inch thick. A steel magnet consisting of a considerable number of bars was fitted up in order to excite those ends of the straight electro-magnets which were distant from the large steel magnets. The coils were arranged for quantity.

Experiment 5.—Current when the engine was kept still, 2081; current when the armature was revolving 114 times per minute, 1300; consumption of zinc, 291 grains per hour; power developed, 10030 lbs. raised a foot per hour. Hence the theoretical duty will be

$$\frac{158\,(2081 - 1300)}{2081} = 59{\cdot}3,$$

and the actual duty,

$$\frac{10030}{291} = 34{\cdot}5.$$

Experiment 6.—Current before starting, 2035; current when revolving 192 times per minute, 1000; consumption of zinc, 223 grains per hour; power developed, 12,672 lbs. raised a foot per hour. In this case the theoretical duty will be

$$\frac{158\,(2035 - 1000)}{2035} = 80{\cdot}3;$$

the actual performance will be

$$\frac{12672}{223} = 56{\cdot}8.$$

The mean of the six experiments gives a theoretical duty of 78·5, and an actual duty of 65·6. But, making allowance for the hot solution employed in the first two experiments, we may state that the actual was in general about $\frac{4}{5}$ths of the theoretical duty.

Upon the whole we feel ourselves justified in fixing the maximum available duty of an electro-magnetic engine worked by a Daniell's battery at 80 lbs. raised a foot high for each grain of zinc consumed*, or, in other words, at about half the theoretical maximum of duty.

Before we leave this part of the subject, we may state that the above experiments fully bear out the idea expressed by

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* Dr. Botto states that 45 lbs. of zinc consumed in a Grove's battery are sufficient to work a one-horse power electro-magnetic engine for 24 hours. The intensity of Daniell's battery being $\frac{3}{5}$ths of that of Grove, it follows that 75 lbs. of zinc would have been consumed had Dr. Botto employed a Daniell's battery,—a result not widely different from our own.

Dr. Scoresby in his "Magnetical Investigations," that steel magnets on his construction may be employed in the stationary part of the electro-magnetic engine with much greater advantage than electro-magnets. We have already adverted to the imperfect construction of the magnetic apparatus employed in the above experiments; had we employed one of equal weight, but constructed of thin plates of hardened steel, and furnished with armatures and batteries in proportion, we think it highly probable that a power equal to that of one horse might have been attained, the whole weight of the apparatus being considerably under half a ton.

Having thus determined the capabilities of electro-magnetism as a first mover of machinery, it will be interesting and instructive to compare it with two other sources of power, viz. steam and horses.

1. A grain of coal produces, by combustion, sufficient heat to raise the temperature of a lb. of water 1°·634. In other words, we may say that the *vis viva* developed by the combustion of a grain of coal is equal to raise a weight of 1335 lbs. to the height of one foot. Now the best Cornish steam-engines raise 143 lbs. per grain of coal; whence it appears that the steam-engine in its most improved state is not able to develope much more than $\frac{1}{10}$th of the *vis viva* due to the combustion of coal into useful power, the remaining $\frac{9}{10}$ths being given off in the form of heat.

2. A horse, when its power is advantageously applied, is able to raise a weight of 24,000,000 lbs. to the height of one foot per day. In the same time (24 hours) he will consume 12 lbs. of hay and 12 lbs. of corn*. He is therefore able to raise 143 lbs. by the consumption of one grain of the mixed food. From our own experiments on the combustion of a mixture of hay and corn in oxygen gas, we find that each grain of food, consisting of equal parts of undried hay and corn, is able to give 0°·682 to a lb. of water, a quantity of heat equivalent to the raising of a weight of 557 lbs. to the height of a foot. Whence it appears, that one quarter of the whole amount of *vis viva* generated by the combustion of food in the

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* We have been kindly informed by Mr. J. V. Gibson of Manchester, an eminent veterinary surgeon, that 14 lbs. of hay and 10 lbs. of corn is the average provender requisite to support a horse of average size, so as to enable him to work daily without any depreciation of his physical condition. We have however equalized the quantities of hay and corn, on account of the experiments on combustion having been made with a mixture containing equal portions.

animal frame, is capable of being applied in producing a useful mechanical effect,—the remaining three-quarters being required in order to keep up the animal heat, &c.

Prof. Magnus of Berlin, has endeavoured to prove that the oxygen which an animal inspires does not combine chemically with the blood, but is merely *absorbed* by it[*]. The blood thus charged with oxygen arrives in the capillary vessels, where the oxygen effects a chemical combination with *certain substances*, converting them into carbonic acid and water. The carbonic acid, instead of oxygen, is then absorbed by the blood, and thus reaches the lungs to be removed by contact with the atmosphere. Adopting this view, it becomes exceedingly probable that the *whole* of the *vis viva* due to the oxidation or combustion of the "certain substances" mentioned by Magnus is developed by the muscles. The muscles, by their motion, can communicate *vis viva* to external objects; and, by their friction within the body, can develope heat in various quantities according to circumstances, so as to maintain the animal at an uniform temperature. If these theoretic views be correct, they would lead to the interesting conclusion (which is the same as that announced by Matteucci from other considerations) that the animal frame, though destined to fulfill so many other ends, is, as an engine, more perfect in the œconomy of *vis viva* than the best of human contrivances.

LXXVI. *Experimental Researches in Electricity.—Twentieth Series.* *By* Michael Faraday, *Esq., D.C.L., F.R.S., Fullerian Prof., &c. &c.*

[Concluded from p. 406.]

¶ iv. *Action of magnets on metals generally.*

2287. THE metals, as a class, stand amongst bodies having a high and distinct interest in relation both to magnetic and electric forces, and might at first well be expected to present some peculiar phænomena, in relation to the striking property found to be possessed in common by so large a number of substances, so varied in their general characters. As yet no distinction associated with conduction or non-conduction, transparent or opake, solid or liquid, crystalline or amorphous, whole or broken, has presented itself; whether the metals, distinct as they are as a class, would fall into the great generalization, or whether at last a separation would occur, was to me a point of the highest interest.

2288. That the metals, iron, nickel and cobalt, would stand in a distinct class, appeared almost undoubted; and it will be,

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[*] [See Phil. Mag. S. 3. vol. xxvii. p. 561.]

I think, for the advantage of the inquiry, that I should consider them in a section apart by themselves. Further, if any other metals appeared to be magnetic, as these are, it would be right and expedient to include them in the same class.

2289. My first point, therefore, was to examine the metals for any indication of ordinary magnetism. Such an examination cannot be carried on by magnets anything short in power of those to be used in the further investigation; and in proof of this point I found many specimens of the metals, which appeared to be perfectly free from magnetism when in the presence of a magnetic needle, or a strong horse-shoe magnet (2157.), that yet gave abundant indications when suspended near to one or both poles of the magnets described (2246.).

2290 My test of magnetism was this. If a bar of the metal to be examined, about two inches long, was suspended (2249.) in the magnetic field, and being at first oblique to the axial line, was upon the supervention of the magnetic forces drawn into the axial position instead of being driven into the equatorial line, or remaining in some oblique direction, then I considered it magnetic. Or, if being near one magnetic pole, it was attracted by the pole, instead of being repelled, then I concluded it was magnetic. It is evident that the test is not strict, because, as before pointed out (2285.), a body may have a slight degree of magnetic force, and yet the power of the new property be so great as to neutralize or surpass it. In the first case, it might seem neither to have the one property nor the other; in the second case, it might appear free from magnetism, and possessing the special property in a *small* degree.

2291. I obtained the following metals, so that when examined as above, they did not appear to be magnetic; and in fact if magnetic, were so to an amount so small as not to destroy the results of the other force, or to stop the progress of the inquiry.

Antimony.
Bismuth.
Cadmium.
Copper.
Gold.
Lead.
Mercury.
Silver.
Tin.
Zinc.

2292. The following metals were, and are as yet to me, magnetic, and therefore companions of iron, nickel and cobalt:—

Platinum.
Palladium.
Titanium.

2293. Whether all these metals are magnetic, in consequence of the presence of a little iron, nickel, or cobalt in them, or whether any of them are really so of themselves, I do not undertake to decide at present; nor do I mean to say that the metals of the former list are free. I have been much struck by the apparent freedom from iron of almost all the specimens of zinc, copper, antimony and bismuth, which I have examined; and it appears to me very likely that some metals, as arsenic, &c., may have much power in quelling and suppressing the magnetic properties of any portion of iron in them, whilst other metals, as silver or platinum, may have little or no power in this respect.

2294. Resuming the consideration of the influence excited by the magnetic force over those metals which are not magnetic after the manner of iron (2291.), I may state that there are two sets of effects produced which require to be carefully distinguished. One of these depends upon induced magneto-electric currents, and shall be resumed hereafter (2309.). The other includes effects of the same nature as those produced with heavy glass and many other bodies (2276.).

2295. All the non-magnetic metals are subject to the magnetic power, and produce the same general effects as the large class of bodies already described. The force which they then manifest, they possess in different degrees. Antimony and bismuth show it well, and bismuth appears to be especially fitted for the purpose. It excels heavy glass, or borate of lead, and perhaps phosphorus; and a small bar or cylinder of it about two inches long, and from 0·25 to 0·5 of an inch in width, is as well fitted to show the various peculiar phænomena as anything I have yet submitted to examination.

2296. To speak accurately, the bismuth bar which I employed was two inches long, 0·33 of an inch wide, and 0·2 of an inch thick. When this bar was suspended in the magnetic field, between the two poles, and subject to the magnetic force, it pointed freely in the equatorial direction, as the heavy glass did (2253.), and if disturbed from that position returned *freely* to it. This latter point, though perfectly in accordance with the former phænomena, is in such striking contrast with the phænomena presented by copper and some other of the metals (2309.), as to require particular notice here.

2297. The comparative sensibility of bismuth causes several movements to take place under various circumstances, which being complicated in their nature, require careful analysis and explanation. The chief of these, with their causes, I will proceed to point out.

2298. If the cylinder electro-magnet (2246.) be placed vertically so as to present one pole upwards, that pole will exist in the upper end of an iron cylinder, having a flat horizontal face $2\frac{1}{2}$ inches in diameter. A small indicating sphere (2266.) of bismuth hung over the centre of this face and close to it, does not move by the magnetism. If the ball be carried outwards, half way, for instance, between the centre and the edge, the magnetism makes it move inwards, or towards the axis (prolonged) of the iron cylinder. If carried still further outwards, it still moves inwards under the influence of the magnetism, and such continues to be the case until it is placed just over the edge of the terminal face of the core, where it has no motion at all (here, by another arrangement of the experiment, it is known to tend in what is at present an upward direction from the core). If carried a little further outwards, the magnetism then makes the bismuth ball tend to go outwards or be repelled, and such continues to be the direction of the force in any further position, or down the side of the end of the core.

2299. In fact, the circular edge formed by the intersection of the end of the core with its sides, is virtually the apex of the magnetic pole, to a body placed like the bismuth ball close to it, and it is because the lines of magnetic force issuing from it diverge as it were, and weaken rapidly in all directions from it, that the ball also tends to pass in all directions either inwards or upwards, or outwards from it, and thus produces the motions described. These same effects do not in fact all occur when the ball, being taken to a greater distance from the iron, is placed in magnetic curves, having generally a simpler direction. In order to remove the effect of the edge, an iron cone was placed on the top of the core, converting the flat end into a cone, and then the indicating ball was urged to move upwards, only when over the apex of the cone, and upward and outwards, as it was more or less on one side of it, being always repelled from the pole in that direction, which transferred it most rapidly from strong to weaker points of magnetic force.

2300. To return to the vertical flat pole: when a horizontal bar of bismuth was suspended concentrically and close to the pole, it could take up a position in any direction relative to the axis of the pole, having at the same time a tendency to move upwards or be repelled from it. If its point of suspension was a little excentric, the bar gradually turned, until it was parallel to a line joining its point of suspension with the prolonged axis of the pole, and the centre of gravity moved inwards. When its point of suspension was just outside the edge of the flat circular terminating face, and the bar formed a certain angle with a radial line joining the axis of the core and the point of suspension, then the movements of the bar were uncertain and wavering. If the angle with the radial line were less than that above, the bar would move into parallelism with the radius and go inwards: if the angle were greater, the bar would move until perpendicular to the radial line and go outwards. If the centre of the bar were still further out than in the last case, or down by the side of the core, the bar would always place itself perpendicular to the radius and go outwards. All these complications of motion are easily resolved into their simple elementary origin, if reference be had to the character of the circular angle bounding the end of the core; to the direction of the magnetic lines of force issuing from it and the other parts of the pole; to the position of the different parts of the bar in these lines; and the ruling principle that each particle tends to go by the nearest course from *strong* to *weaker* points of magnetic force.

2301. The bismuth points well, and is well repelled (2296.) when immersed in water, alcohol, æther, oil, mercury, &c., and also when inclosed within vessels of earth, glass, copper, lead, &c. (2272.), or when screens of 0·75 or 1 inch in thickness of bismuth, copper or lead intervene. Even when a bismuth cube (2266.) was placed in an iron vessel 2½ inches in diameter and 0·17 of an inch in thickness, it was well and freely repelled by the magnetic pole.

2302. Whether the bismuth be in one piece or in very fine powder, appears to make no difference in the character or in the degree of its magnetic property (2283.).

2303. I made many experiments with masses and bars of bismuth suspended, or otherwise circumstanced, to ascertain whether two pieces had any mutual action on each other, either of attraction or repulsion, whilst jointly under the influence of the magnetic forces, but I could not find any indication of such mutual action: they appeared to be perfectly indifferent one to another, each tending only to go from stronger to weaker points of magnetic power.

2304. Bismuth, in very fine powder, was sprinkled upon paper, laid over the horizontal circular termination of the vertical pole (2246.). If the paper were tapped, the magnet not being excited, nothing particular occurred; but if the magnetic power were on, then the powder retreated in both directions, inwards and outwards, from a circular line just over the edge of the core, leaving the circle clear, and at the same time showing the tendency of the particles of bismuth in all directions from that line (2299.).

2305. When the pole was terminated by a cone (2246.) and the magnet not in action, paper with bismuth powder sprinkled over it being drawn over the point of the cone, gave no particular result; but when the magnetism was on, such an operation cleared the powder from every point which came over the cone, so that a mark was traced or written out in clear lines running through the powder, and showing every place where the pole had passed.

2306. The bar of bismuth and a bar of antimony was found to set equatorially between the poles of the ordinary horseshoe magnet.

2307. The following list may serve to give an idea of the apparent order of some metals, as regards their power of producing these new effects, but I cannot be sure that they are perfectly free from the magnetic metals. In addition to that, there are certain other effects produced by the action of magnetism on metals (2309.) which greatly interfere with the results due to the present property.

Bismuth.
Antimony.
Zinc.
Tin.
Cadmium.
Mercury.
Silver.
Copper.

2308. I have a vague impression that the repulsion of bismuth by a magnet has been observed and published several years ago. If so, it will appear that what must then have been considered as a peculiar and isolated effect, was the consequence of a general property, which is now shown to belong to all matter*.

2309. I now turn to the consideration of some peculiar phænomena which are presented by copper and several of the metals when they are subjected to the action of magnetic forces, and which so tend to mask effects of the kind already described, that if not known to the inquirer they would lead to much confusion and doubt. These I will first describe as to their appearances, and then proceed to consider their origin.

2310. If instead of a bar of bismuth (2296.) a bar of copper of the same size be suspended between the poles (2247.), and

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* M. de la Rive has this day referred me to the *Bibliothèque Universelle* for 1829, tome xl. p. 82, where it will be found that the experiment spoken of above is due to M. la Baillif of Paris. M. la Baillif showed sixteen years ago that both bismuth and antimony repelled the magnetic needle. It is astonishing that such an experiment has remained so long without further results. I rejoice that I am able to insert this reference before the present series of these researches goes to press. Those who read my papers will see here, as on many other occasions, the results of a memory which becomes continually weaker; I only hope that they will be excused, and that omissions and errors of that nature will be considered as involuntary.—M. F. December 30, 1845.

magnetic power be developed whilst the bar is in a position oblique to the axial and equatorial lines, the experimenter will perceive the bar to be affected, but this will not be manifest by any tendency of the bar to go to the equatorial line; on the contrary, it will advance towards the axial position as if it were magnetic. It will not however continue its course until in that position, but, unlike any effect produced by magnetism, will stop short, and making no vibration beyond or about a given point, will remain there coming at once to a dead rest: and this it will do even though the bar by the effect of torsion or momentum was previously moving with a force that would have caused it to make several gyrations. This effect is in striking contrast with that which occurs when antimony, bismuth, heavy glass, or other such bodies are employed, and it is equally removed from an ordinary magnetic effect.

2311. The position which the bar has taken up it retains with a considerable degree of tenacity, provided the magnetic force be continued. If pushed out of it, it does not return into it, but takes up its new position in the same manner, and holds it with the same stiffness; a push however, which would make the bar spin round several times if no magnetism were present, will now not move it through more than 20° or 30°. This is not the case with bismuth or heavy glass; they vibrate freely in the magnetic field, and always return to the equatorial position.

2312. The position taken up by the bar may be any position. The bar is moved a little at the instant of superinducing the magnetism, but allowing and providing for that, it may be finally fixed in any position required. Even when swinging with considerable power by torsion or momentum, it may be caught and retained in any place the experimenter wishes.

2313. There are two positions in which the bar may be placed at the beginning of the experiment, from which the magnetism does not move it, the equatorial and the axial positions. When the bar is nearly midway between these, it is usually most strongly affected by the first action of the magnet, but the position of most effect varies with the form and dimensions of the magnetic poles and of the bar.

2314. If the centre of suspension of the bar be in the axial line, but near to one of the poles, these movements occur well, and are clear and distinct in their direction: if it be in the equatorial line, but on one side of the axial line, they are modified, but in a manner which will easily be understood hereafter.

2315. Having thus stated the effect of the supervention of the magnetic force, let us now remark what occurs at the moment of its cessation; for during its continuance there is no change. If, then, after the magnetism has been sustained for two or three seconds, the electric current be stopped, there is instantly a strong action on the bar, which has the appearance of a revulsion (for the bar returns upon the course which it took for a moment when the electric contact was made), but with such force, that whereas the advance might be perhaps 15° or 20°, the revulsion will cause the bar occasionally to move through two or three revolutions.

2316. Heavy glass or bismuth presents no such phænomena as this.

2317. If, whilst the bar is revolving from revulsion the electric current at the magnet be renewed, the bar instantly stops with the former appearances and results (2310.), and then upon removing the magnetic force is affected again, and, of course, now in a contrary direction to the former revulsion.

2318. When the bar is caught by the magnetic force in the axial or equatorial position, there is no revulsion. When inclined to these positions there is; and the places most powerful in this respect appear to be those most favourable to the first brief advance (2313.). If the bar be in a position at which strong revulsion would occur, and whilst the magnetism is continued be moved by hand into the equatorial or axial position, then on taking off the magnetic force there is no revulsion.

2319. If the continuance of the electric current and consequently of the magnetism be for a moment only, the revulsion is very little, and the shorter the continuance of the magnetic force the less is the revulsion. If the magnetic force be continued for two or three seconds and then interrupted and *instantly* renewed, the bar is loosened and caught again by the power before it sensibly changes its place; and now it may be observed that it does not advance on the *renewal* of the force as it would have done had it been acted on by a first contact in that place (2310.); *i. e.* if the bar be in a certain place inclined to the axial position, the first supervention of the magnetic power causes it to advance towards the axial position; but the bar being in the same place and the magnetic power suspended and *instantly* renewed, the second supervention of force does not move the bar as the first did.

2320. When the copper bar is immersed in water, alcohol, or even mercury, the same effects take place as in the air, but the movements are, of course, not to the same extent.

2321. When plates of copper or bismuth, an inch in thickness, intervene between the poles and the copper bar, the same results occur.

2322. If one magnetic pole only be employed the effects occur near it as well as before, provided that pole have a face large in proportion to the bar, as the end of the iron core (2246.): but if the pole be pointed by the use of the conical termination, or if the bar be opposite the edge of the end of the core, then they become greatly enfeebled or disappear altogether; and only the general fact of repulsion remains (2295.).

2323. The peculiar effects which have just been described are perhaps more strikingly shown if the bar of copper be suspended perpendicularly, and then hung opposite and near to the large face of a single magnetic pole, or the pole being placed vertically, as described (2246. 2263.), anywhere near to its side. The bar, it will be remembered, is two inches in length by 0·33 of an inch in width, and 0·2 of an inch in thickness, and as it now will revolve on an axis parallel to its length, the two smaller dimensions are those which are free to move into new positions. In this case the establishment of the magnetic force causes the bar to turn a little in accordance with the effects before described, and the removal of the magnetic force causes a revulsion, which sends the bar spinning round on its axis several times. But at any moment the bar can again be caught and held in a position as before. The tendency on making contact at the battery is to place the longest moving dimension, *i. e.* the width of the bar, parallel to the line joining the centre of action of the magnet and the bar.

2324. The bar, as before (2311.), is extremely sluggish and as if immersed in a dense fluid, as respects rotation on its own axis; but this sluggishness does not affect the bar as a whole, for any pendulum vibration it has continues unaffected. It is very curious to see the bar, jointly vibrating from its point of suspension (2249.) and rotating on its axis, when first affected by the magnetic force, for instantly the latter motion ceases, but the former goes on with undiminished power.

2325. The same effect of sluggishness occurs with a cube or a globe of copper as with the bar, but the phænomena of the first turn and the revulsion cease (2310. 2315.).

2326. The bars of bismuth and heavy glass present no appearance of this kind. The peculiar phænomena produced by copper are as distinct from the actions of these substances as they are from ordinary magnetic actions.

2327. Endeavouring to explain the cause of these effects, it appears to me that they depend upon the excellent conducting power of copper for electric currents, the *gradual* acquisition and loss of magnetic power by the iron core of the electro-magnet, and the production of those induced currents of magneto-electricity which I described in the First Series of these Experimental Researches (55. 109.).

2328. The obstruction to motion on its own axis, when the bar is subjected to the magnetic forces, belongs equally to the form of a sphere or a cube. It belongs to these bodies, however, only when their axes of rotation are perpendicular or oblique to the lines of magnetic force, and not when they are parallel to it; for the horizontal bar, or the vertical bar, or the cube or sphere, rotate with perfect facility when they are suspended *above* the vertical pole (2246.), the rotation and vibration being then equally free, and the same as the corresponding movements of bismuth or heavy glass. The obstruction is at a maximum when the axis of rotation is perpendicular to the lines of magnetic force, and when the bar or cube, &c. is near to the magnet.

2329. Without going much into the particular circumstances, I may say that the effect is fully explained by the electric currents induced in the copper mass. By reference to the Second Series of these Researches (160.), it will be seen that when a globe, subject to the action of lines of magnetic force, is revolving on an axis perpendicular to these lines, an electric current runs round it in a plane parallel to the axis of rotation and to the magnetic lines, producing consequently a magnetic axis in the globe, at right angles to the magnetic curves of the inducing magnet. The magnetic poles of this axis therefore are in that direction which, in conjunction with the chief magnetic pole, tends to draw the globe back against the direction in which it is revolving. Thus, if a piece of copper be revolving before a north magnetic pole, so that the parts nearest the pole move towards the right-hand, then the right-hand side of that copper will have a south magnetic state, and the left-hand side a north magnetic state; and these states will tend to counteract the motion of the copper towards the right-hand: or if it revolve in the contrary direction, then the right-hand side will have a south magnetic state, and the left-hand side a north magnetic state. Whichever way, therefore, the copper tends to revolve on its own axis, the instant it moves, a power is evolved in such a direction as tends to stop its motion and bring it to rest. Being at rest in reference to this direction of motion, then there is no residual or other effect which tends to disturb it, and it remains still.

2330. If the whole mass be moving parallel to itself, and be small in comparison with the face of the magnetic pole opposite to which it is placed, then, though it pass through the magnetic lines of force, and consequently have a tendency to the formation of magneto-electric currents within it, yet as all parts move with equal velocity and in the same direction through similar magnetic lines of force, the tendency to the formation of a current is the same in every part, and there is no actual production of current, and consequently nothing occurs which can in any way interfere with its freedom of motion. Hence the reason that though the rotation of the bar or cube (2324. 2328.) upon its own axis is stopped, its vibration as a pendulum is not affected.

2331. That neither the one nor the other motion is affected when the bar or cube is over the vertical pole (2328.), is simply because in both cases (with the given dimensions of the pole and the moving metal) the lines of particles through which the induced currents tend to move are parallel throughout the whole mass; and therefore, as there is no part by which the return of the current can be carried on, no current can be formed.

2332. Before proceeding to the explanation of the other phænomena, it will be necessary to point out the fact generally understood and acknowledged, I believe, that time is required for the development of magnetism in an iron core by a current of electricity; and also for its fall back again when the current is stopped. One effect of the gradual rise in power was referred to in the last series of these Researches (2170.). This time is probably longer with iron not well annealed than with very good and perfectly annealed iron. The last portions of magnetism which a given current can develope in a certain core of iron, are also apparently acquired more slowly than the first portions; and these portions (or the condition of iron to which they are due) also appear to be lost more slowly than the other portions of the power. If electric contact be made for an instant only, the magnetism developed by the current disappears as instantly on the breaking of the current, as it appeared on its formation; but if contact be continued for three or four seconds, breaking the contact is by no means accompanied by a disappearance of the magnetism with equal rapidity.

2333. In order to trace the peculiar effect of the copper, and its cause, let us consider the condition of the horizontal bar (2310. 2313.) when in the equatorial position, between the two magnetic poles, or before a single pole; the point of suspension being in a line with the axis of the pole and its exciting wire helix. On sending an electric current through the helix, both it and the magnet it produces will conduce to the formation of currents in the copper bar in the contrary direction. This is shown from my former researches (26.), and may be proved, by placing a small or large wire helix-shaped (if it be desired) in the form of the bar, and carrying away the currents produced in it, by wires to a galvanometer at a distance. Such currents being produced in the copper, only continue whilst the magnetism of the core is rising and then cease (18. 39.), but *whilst* they continue, they give a virtual magnetic polarity to that face of the copper bar which is opposite to a certain pole, the polarity being the same in kind as the pole it faces. Thus on the side of the bar facing the north pole of the magnet, a north polarity will be developed; and on that side facing the south pole, a south polarity will be generated.

2334. It is easy to see that if the copper during this time were opposite only one pole, or being between two poles, were nearer to one than the other, this effect would cause its repulsion. Still, it cannot account for the whole amount of the repulsion observed alike with copper as with bismuth (2295.), because the currents are of but momentary duration, and the repulsion due to them would cease with them. They do, however, cause a brief repulsive effort, to which is chiefly due the first part of the peculiar effect.

2335. For if the copper bar, instead of being parallel to the face of the magnetic pole, and therefore at right angles to the resultant of magnetic force, be inclined, forming, for instance, an angle of 45° with the face, then the induced currents will move generally in a plane corresponding more or less to that angle, nearly as they do in the examining helix (2333.), if it be inclined in the same manner. This throws the polar axis of the bar of copper on one side, so that the north polarity is not directly opposed to the north pole of the inducing magnet, and hence the action both of this and the other magnetic pole upon the two polarities of the copper will be to send it further round, or to place it edgeways to the poles, or with its breadth parallel to the magnetic resultant passing through it (2323.): the bar therefore receives an impulse, and the angle of it nearest to the magnet appears to be pulled up towards the magnet. This action of course stops the instant the magnetism of the helix core ceases to rise, and then the motion due to this cause ceases, and the copper is simply subject to the action before described (2295.). At the same time that this twist or small portion of a turn round the point of suspension occurs, the centre of gravity of the whole mass is repelled, and thus I believe all the actions up to this condition of things is accounted for.

2336. Then comes the *revulsion* which occurs upon the cessation of the electric current, and the falling of the magnetism in the core. According to the law of magneto-electric induction, the disappearance of the magnetic force will induce brief currents in the copper bar (28.), but in the contrary direction to those induced in the first instance; and therefore the virtual magnetic pole belonging to the copper for the moment, which is nearest the north end of the electro-magnet, will be a south pole; and that which is furthest from the same pole of the magnet will be a north pole. Hence will arise an exertion of force on the bar tending to turn it round its centre of suspension in the contrary direction to that which occurred before, and hence the apparent revulsion; for the angle nearest the magnetic pole will recede from it, the broad face (2323.) or length (2315.) of the bar will come round and face towards the magnet, and an action the reverse in every respect of the first action will take place, except that whereas the motion was then only a few degrees, now it may extend to two or three revolutions.

2337. The cause of this difference is very obvious. In the first instance, the bar of copper was moving under influences powerfully tending to retard and stop it (2329.); in the second case these influences are gone, and the bar revolves freely with a force proportionate to the power exerted by the magnet upon the currents induced by its own action.

2338. Even when the copper is of such form as not to give the oblique resultant of magnetic action from the currents induced in it, when, for instance, it is a cube or a sphere, still the effect of the action described above is evident (2325.). When a plate of copper about three-fourths of an inch in thickness, and weighing two pounds, was sustained upon some loose blocks of wood and placed about 0·1 of an inch from the face of the magnetic pole, it was repelled and held off a certain distance upon the making and continuing of electric contact at the battery; and when the battery current was stopped, it returned towards the pole; but the return was much more powerful than that due to gravity alone (as was ascertained by an experiment), the plate being at that moment actually *attracted*, as well as tending by gravitation towards the magnet, so that it gave a strong tap against it.

2339. Such is, I believe, the explanation of the peculiar phænomena presented by copper in the magnetic field; and the reason why they appear with this metal and not with bismuth or heavy glass, is almost certainly to be found in its high electro-conducting power, which permits the formation of currents in it by inductive forces, that cannot produce the same in a corresponding degree in bismuth, and of course not at all in heavy glass.

2340. Any ordinary magnetism due to metals by virtue of their inherent power, or the presence of small portions of the magnetic metals in them, must oppose the development of the results I have been describing: and hence metals not of absolute purity cannot be compared with each other in this respect. I have, nevertheless, observed the same phænomena in other metals; and as far as regards the sluggishness of rotatory motion, traced it even into bismuth. The following are the metals which have presented the phænomena in a greater or smaller degree:—

Copper.
Silver.
Gold.
Zinc.
Cadmium.
Tin.
Mercury.
Platinum.
Palladium.
Lead.
Antimony.
Bismuth.

2341. The accordance of these phænomena with the beautiful discovery of Arago*, with the results of the experiments of Herschel and Babbage†, and with my own former inquiries (81.)‡, are very evident. Whether the effect obtained by Ampère, with his copper cylinder and a helix§, was of this nature, I cannot judge, inasmuch as the circumstances of the experiment and the energy of the apparatus are not sufficiently stated; but it probably may have been.

2342. As, because of other duties, three or four weeks may elapse before I shall be able to complete the verification of certain experiments and conclusions, I submit at once these results to the attention of the Royal Society, and will shortly embody the account of the action of magnets on magnetic metals, their action on gases and vapours, and the general considerations in another series of these Researches.

Royal Institution, Nov. 27, 1845.

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* *Annales de Chimie*, xxvii. 363; xxviii. 325; xxxii. 213. I am very glad to refer here to the *Comptes Rendus* of June 9, 1845, where it appears that it was M. Arago who first obtained his peculiar results by the use of electro- as well as common magnets.

† Philosophical Transactions, 1825, p. 467.

‡ Ibid. 1832. p. 146.

§ *Bibliothèque Universelle*, xxi. p. 48.

LXXVII. *On the Equations applying to Light under the action of Magnetism. By* G. B. Airy, *Esq., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

By the indulgence of Dr. Faraday, I have been able to observe in the most satisfactory way the phænomena of the rotation of the plane of polarization of light passing through boracic glass and other media under the action of magnetic currents passing nearly in the direction of the light. And in particular I have verified the very remarkable fact that, upon passing the light successively in opposite directions while the magnetic adjustments remain the same, the plane of polarization undergoes the same change of position in regard to space, or undergoes opposite changes of position in regard to the expression of "rotation to the right," or "rotation to the left," as referred to the eye of the observer.

On reflecting upon the important fact that this change is not produced except there be an intermediate diaphanous body, it seems impossible not to conceive that the effect on the light is produced mediately by the action of the magnetic forces on the diaphanous body. The object of this communication is to point out what, as I conceive, must be the form of the mathematical equations existing among the movements of the particles of the glass, &c. or its contained æther, in order to explain the phænomena on mechanical laws.

In order to justify my intruding upon you with a suggestion which is exceedingly imperfect, I think it right to state to you my opinion upon the present condition of the optical theory, and upon several steps which, though leading to nothing conclusive, have nevertheless contributed to the real intellectual progress of the science.

On the truth of the undulatory theory, as regards the geometrical representation of light by undulations based upon transversal vibrations, the resolution of which into vibrations at right angles to each other constitutes polarization, I have not the shadow of a doubt. These undulations, whatever may be the way in which they may have been originally created, I conceive to be propagated by mechanical laws applying to the attractive or repulsive forces of the particles of the medium, the assumed æther, or the medium and the æther combined. But I have seen no mechanical theory to which I attach much importance or any unqualified belief. Nevertheless I think that the investigation and publication of these mechanical theories have been advantageous to the science, by showing that mechanical laws *may be able* to explain effects never before ascribed to mechanical laws. As regards the progress of intellect, it has been very important to show that variation of velocities, as depending on the period of the oscillations, is mechanically possible; it has been very important to show that transversal vibrations are mechanically possible; it has been very important to show that crystalline separation of differently polarized rays is mechanically possible. It is not that I believe completely in any one of the mechanical explanations which have been given, but that *à priori* difficulties have been removed, and that it may now be considered that there is a fair chance of reducing the whole to mechanical explanation.

In some cases the mechanical theory has stopped at the first step, as for instance in the very remarkable equations indicated by Prof. MacCullagh as competent to represent some of the characteristic phænomena of quartz. It was here an important matter to show that there was opened even a possibility of reducing these anomalous facts to mechanical laws.

The suggestion, which it is the object of this paper to lay before you, is of the same kind as that made by Professor MacCullagh.

In order to reduce the rotation of the plane of polarization to laws, I shall follow the example of Fresnel in assuming that plane-polarized light may be considered as compounded of two beams of circularly-polarized light, one right-handed and the other left-handed, and that the rotation of the plane is produced by a difference of the velocities of the two circularly-polarized beams. And this, I take this opportunity to observe, is actually the simplest way of conceiving the change, at least in instances like that of quartz, &c., and like that before us, when the same change is produced whatever be the position of the plane of polarization (a fact which, at my request, Dr. Faraday has very carefully verified). Although the conception of a plane vibration is easier where the plane of vibration has immediate reference to the plane of reflexion, &c., yet the conception of two circular vibrations is easier where the plane of the compound vibration has no reference to any plane in the apparatus, and is in fact perfectly arbitrary.

Now let x_1 be measured in the direction in which the light is supposed to travel in the first experiment; x_2 in the opposite direction, or in the direction in which the light will travel when, the magnetic adjustments remaining the same, the relative positions of the polariser and analyser are reversed; suppose these to be horizontal: let y_1 be measured horizontally towards the right as regards the course of the light in the first experiment, y_2 towards the right as regards the course of the light in the second experiment (or opposite to y_1); z vertical, in a direction common to both experiments. Then, for the first experiment, in order to represent the displacement of particles constituting that ray of circularly polarized light, in which every particle describes a circle in the direction, viewed from the origin of light, opposite to that of the hands of a watch, and in which at any one time the position of all the particles, originally in a straight line, has become a right-handed helix (which I will call Ray No. I.); we must take the following expressions:—where τ is the period of vibration, v'_1 the velocity of transmission of the wave, and Y'_1 and Z' the displacements in the direction of y_1 and z respectively,

$$Y'_1 = a \,.\, \cos \frac{2\pi}{\tau}\left(t - \frac{x_1}{v'_1}\right),$$

$$Z'_1 = a \,.\, \sin \frac{2\pi}{\tau}\left(t - \frac{x_1}{v'_1}\right).$$

Similarly, to represent, for the first experiment, the displacement of particles constituting the ray circularly polarized in the opposite direction, or so that each particle describes a circle in the same direction, viewed from the origin of light, as the hands of a watch (which I shall call Ray No. II.), we must have

$$Y''_1 = b \,.\, \cos \frac{2\pi}{\tau}\left(t - \frac{x_1}{v''_1}\right),$$

$$Z''_1 = -\, b \,.\, \sin \frac{2\pi}{\tau}\left(t - \frac{x_1}{v''_1}\right).$$

And in the second experiment, to represent the Ray No. I. of that experiment, we must combine

$$Y'_2 = a \,.\, \cos \frac{2\pi}{\tau}\left(t - \frac{x_2}{v'_2}\right),$$

$$Z'_2 = a \,.\, \sin \frac{2\pi}{\tau}\left(t - \frac{x_2}{v'_2}\right);$$

and to represent the Ray No. II., we must combine

$$Y''_2 = b \,.\, \cos \frac{2\pi}{\tau}\left(t - \frac{x_2}{v''_2}\right),$$

$$Z''_2 = -\, b \,.\, \sin \frac{2\pi}{\tau}\left(t - \frac{x_2}{v''_2}\right).$$

And the thing which it is very important to observe is, that the same mechanical equations *referred to the same directions in absolute space* must apply to all these displacements.

In ordinary crystals or fluids possessing the property of causing rotation of the plane of polarization in the same direction as referred to the eye of the observer, whether the ray be incident on one side or on the other, mechanical equations are to be sought which will produce the result, that in both cases the velocity of Ray No. I. is greater than that of Ray No. II. (or *vice versâ*); so that if v'_1 is greater than v''_1, v'_2 will also be greater than v''_2. But in the glass affected by magnetism, if in the first experiment the velocity of Ray No. I. is greater than that of Ray No. II., then in the second experiment the velocity of Ray No. I. must be less than that of Ray No. II.; or if v'_1 is greater than v''_1, v'_2 must be less than v''_2.

Now the equation which is deduced from every mechanical supposition that accounts for the propagation of undulations, is of the form

$$\frac{d^2 Y}{d t^2} = A \,.\, \frac{d^2 Y}{d x^2},$$

$$\frac{d^2 Z}{d t^2} = A \,.\, \frac{d^2 Z}{d x^2}.$$

And it seems probable that these equations, with the addition to each of a small term, may explain the difference of velocities of the Rays No. I. and No. II.

It was pointed out by Prof. MacCullagh, that the equations

$$\frac{d^2 Y}{d t^2} = A \,.\, \frac{d^2 Y}{d x^2} + B \,.\, \frac{d^3 Z}{d x^3},$$

$$\frac{d^2 Z}{d t^2} = A \,.\, \frac{d^2 Z}{d x^2} - B \,.\, \frac{d^3 Y}{d x^3}$$

would explain this difference. I may remark here, that in the last term of the second side of each equation, any differential coefficient of an odd order would have sufficed to explain the general fact of difference of velocity; but the third order was adopted by Prof. MacCullagh in order to reconcile the expression for difference of velocity in differently-coloured rays with the fact established by experiment.

It is however necessary to inquire whether, if this assumption makes v'_1 greater than v''_1, it will make v'_2 greater than v''_2. For this purpose we must convert the various expressions into expressions referred to the same co-ordinates.

Let $x_1 = x$, $x_2 = -x$; $y_1 = y$, $y_2 = -y$:

in the first experiment let

$$\mathrm{Y}'_1=\mathrm{Y}',\quad \mathrm{Y}''_1=\mathrm{Y}'';\quad \mathrm{Z}'_1=\mathrm{Z}',\quad \mathrm{Z}''_1=\mathrm{Z}'':$$

in the second experiment let

$$\mathrm{Y}'_2=-\mathrm{Y}',\quad \mathrm{Y}''_2=-\mathrm{Y}'',\quad \mathrm{Z}'_2=\mathrm{Z}',\quad \mathrm{Z}''_2=\mathrm{Z}''.$$

Then,

in the first experiment, for Ray No. I.,

$$\mathrm{Y}' = a\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right),$$

$$\mathrm{Z}' = a\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right);$$

and Prof. MacCullagh's equations become

$$-\frac{4\pi^2}{\tau^2}a\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right)=-\mathrm{A}\frac{4\pi^2}{\tau^2}\left(\frac{1}{v'_1}\right)^2a\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right)$$

$$+\mathrm{B}\frac{8\pi^3}{\tau^3}\left(\frac{1}{v'_1}\right)^3a\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right),$$

$$-\frac{4\pi^2}{\tau^2}a\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right)=-\mathrm{A}\,.\frac{4\pi^2}{\tau^2}\,.\left(\frac{1}{v'_1}\right)^2a\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right)$$

$$+\mathrm{B}\,.\frac{8\pi^3}{\tau^3}\,.\left(\frac{1}{v'_1}\right)^3a\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right),$$

which agree in giving

$$(v'_1)^2=\frac{\mathrm{A}}{1+\mathrm{B}\dfrac{2\pi}{\tau}\left(\dfrac{1}{v'_1}\right)^3}.$$

For Ray No. II.,

$$\mathrm{Y}'' = b\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right),$$

$$\mathrm{Z}'' = -\,b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right).$$

The equations become

$$-\frac{4\pi^2}{\tau^2}b\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right)=-\mathrm{A}\,.\frac{4\pi^2}{\tau^2}\left(\frac{1}{v''_1}\right)^2b\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right)$$

$$-\mathrm{B}\,.\frac{8\pi^3}{\tau^3}\left(\frac{1}{v''_1}\right)^3b\,.\cos\frac{2\pi}{\tau}\left(t-\frac{x}{v'_1}\right),$$

$$+\frac{4\pi^2}{\tau^2}b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right)=+\mathrm{A}\,.\frac{4\pi^2}{\tau^2}\left(\frac{1}{v''_1}\right)^2b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right)$$

$$+\mathrm{B}\,.\frac{8\pi^3}{\tau^3}\left(\frac{1}{v''_1}\right)^3b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right),$$

which agree in giving

$$(v''_1)^2 = \frac{A}{1 - B\frac{2\pi}{\tau}\left(\frac{1}{v''_1}\right)^3}.$$

Hence v'_1 is less than v''_1.

In the second experiment, for Ray No. I.,

$$Y' = -a \,.\, \cos\frac{2\pi}{\tau}\left(t + \frac{x}{v'_2}\right),$$

$$Z' = a \,.\, \sin\frac{2\pi}{\tau}\left(t + \frac{x}{v'_2}\right).$$

The equations become

$$+\frac{4\pi^2}{\tau^2}a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right) = +A\frac{4\pi^2}{\tau^2}\,.\left(\frac{1}{v'_2}\right)^2 a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right)$$
$$-B\,.\frac{8\pi^3}{\tau^3}\,.\left(\frac{1}{v'_2}\right)^3 a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right),$$

$$-\frac{4\pi^2}{\tau^2}a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right) = -A\,.\frac{4\pi^2}{\tau^2}\left(\frac{1}{v'_2}\right)^2 a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right)$$
$$+B\,.\frac{8\pi^3}{\tau^3}\,.\left(\frac{1}{v'_2}\right)^3 a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right),$$

which agree in giving

$$(v'_2)^2 = \frac{A}{1 + B\frac{2\pi}{\tau}\left(\frac{1}{v''_1}\right)^3}.$$

And similarly, for Ray No. II.,

$$(v''_2)^2 = \frac{A}{1 - B\frac{2\pi}{\tau}\left(\frac{1}{v''_1}\right)^3}.$$

Hence v'_2 is less than v''_2.

Thus in both experiments (that is, whether the light passes from one side or from the other side) the Ray No. II. travels more quickly than the Ray No. I. And therefore, if in each experiment there is incident a plane-polarized ray, consisting of the combination of a Ray No. I. and a Ray No. II., the plane-polarized ray which is formed by their union after emergence will have its plane of polarization turned from the original plane of polarization, in both experiments in the same direction as the hands of a watch, or in both experiments in the direction opposite to that of the hands of a watch, as referred to the eye of a person looking in the direction of the path of the light.

This result agrees with the phænomena of quartz, turpentine, &c.; and therefore Prof. MacCullagh's equations apply to the explanation of crystalline rotation of the plane of polarization. But it does not agree with the phænomena of glass, &c. under magnetic action; and for this case new equations must be sought.

The equations which I offer as competent to represent this case are,

$$\frac{d^2\,\mathrm{Y}}{d\,t^2} = \mathrm{A}\,.\,\frac{d^2\,\mathrm{Y}}{d\,x^2} + \mathrm{C}\,.\,\frac{d\,\mathrm{Z}}{d\,t},$$

$$\frac{d^2\,\mathrm{Z}}{d\,t^2} = \mathrm{A}\,.\,\frac{d^2\,\mathrm{Z}}{d\,x^2} - \mathrm{C}\,.\,\frac{d\,\mathrm{Y}}{d\,t},$$

which are to be verified in the same manner as those applying to the phænomena of quartz, &c.

Thus, in the first experiment, for Ray No. I.,

$$\mathrm{Y}' = a\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right),$$

$$\mathrm{Z}' = a\,.\sin\frac{2\,\pi}{\tau}\left(t - \frac{x}{v_1}\right).$$

The equations become

$$-\frac{4\,\pi^2}{\tau^2}\,.\,a\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right) = -\mathrm{A}\frac{4\,\pi^2}{\tau^2}\left(\frac{1}{v'_1}\right)^2 a\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right)$$

$$+\,\mathrm{C}\frac{2\,\pi}{\tau}\,.\,a\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right),$$

$$-\frac{4\,\pi^2}{\tau^2}\,a\,.\sin\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right) = -\mathrm{A}\frac{4\,\pi^2}{\tau^2}\left(\frac{1}{v'_1}\right)^2 a\,.\sin\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right)$$

$$+\,\mathrm{C}\,.\,\frac{2\,\pi}{\tau}\,.\,a\,.\sin\frac{2\,\pi}{\tau}\left(t - \frac{x}{v'_1}\right),$$

which agree in giving

$$(v'_1)^2 = \frac{\mathrm{A}}{1 + \frac{\tau}{2\,\pi}\,\mathrm{C}}.$$

For Ray No. II.,

$$\mathrm{Y}'' = b\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v''_1}\right),$$

$$\mathrm{Z}'' = -\,b\,.\sin\frac{2\,\pi}{\tau}\left(t - \frac{x}{v''_1}\right);$$

and the equations become

$$-\frac{4\,\pi^2}{\tau^2}\,.\,b\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v''_1}\right) = -\mathrm{A}\frac{4\,\pi^2}{\tau^2}\left(\frac{1}{v''_1}\right)^2 b\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v''_1}\right)$$

$$-\,\mathrm{C}\frac{2\,\pi}{\tau}\,b\,.\cos\frac{2\,\pi}{\tau}\left(t - \frac{x}{v''_1}\right),$$

$$+\frac{4\pi^2}{\tau^2}b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right)=+A\frac{4\pi^2}{\tau^2}\left(\frac{1}{v''_1}\right)^2 b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right)$$
$$+C\frac{2\pi}{\tau}b\,.\sin\frac{2\pi}{\tau}\left(t-\frac{x}{v''_1}\right),$$

which agree in giving

$$(v'_{''})^2=\frac{A}{1-\frac{\tau}{2\pi}C}.$$

Hence v'_1 is less than $v'_{''}$.

In the second experiment, for Ray No. I.,

$$Y''=-a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right),$$

$$Z''=a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right);$$

and the equations become

$$+\frac{4\pi^2}{\tau^2}a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right)=+A\frac{4\pi^2}{\tau^2}\left(\frac{1}{v'_2}\right)^2 a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right)$$
$$+C\frac{2\pi}{\tau}a\,.\cos\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right),$$

$$-\frac{4\pi^2}{\tau^2}a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right)=-A\frac{4\pi^2}{\tau^2}\left(\frac{1}{v'_2}\right)^2 a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right)$$
$$-C\frac{2\pi}{\tau}a\,.\sin\frac{2\pi}{\tau}\left(t+\frac{x}{v'_2}\right),$$

which agree in giving

$$(v'_2)^2=\frac{A}{1-\frac{\tau}{2\pi}C}.$$

Similarly,

$$(v''_2)^2=\frac{A}{1+\frac{\tau}{2\pi}C}.$$

Hence v'_2 is greater than v''_2.

Thus if in one experiment the Ray No. II. travels more quickly than the Ray No. I., in the other experiment the Ray No. II. travels more slowly than the Ray No. I. And therefore if in each experiment there is incident a plane-polarized ray consisting of the combination of a Ray No. I. and a Ray No. II., the plane-polarized ray which is formed by their union after emergence will have its plane of polarization turned from the original plane of polarization, in one experiment in the same direction as the hands of a watch, and in the other experiment in the opposite direction, as referred to the eye of a person looking in the direction of the path of the light.

This result agrees with the phænomena of boracic glass, &c. under the action of magnetic forces.

Instead of making the second term on the right-hand side of the equation depend on $\frac{dZ}{dt}$, we might with equal success have adopted $\frac{d^3Z}{dt^3}$, $\frac{d^3Z}{dx^2 . dt}$, or any other differential coefficient of an odd order in which the number of differentiations with respect to t is odd. Different powers of τ and v will be introduced by different selections. In order to determine which of these selections is best adapted to represent the phænomena, it will be necessary to determine the deviation of the plane of polarization for light of different colours.

If $\frac{dZ}{dt}$ be adopted, the equations suggested by me will amount to this:—"The force upon any particle in the direction of one ordinate depends in part upon its velocity in the direction of the other ordinate." There is no insurmountable difficulty in conceiving that this may be true, although we have at present no mechanical reason *à priori* for believing that it is true.

To remove the possibility of misunderstanding, I will repeat that I offer these equations with the same intention with which Prof. MacCullagh's equations were offered; not as giving a mechanical explanation of the phænomena, but as showing that the phænomena may be explained by equations, which equations appear to be such as might possibly be deduced from some plausible mechanical assumption, although no such assumption has yet been made.

I am, Gentlemen,
Your obedient Servant,
G. B. Airy.

Royal Observatory, Greenwich,
May 7, 1846.

LXXVIII. *Letter to* Henry Lord Brougham, *F.R.S., &c., containing Remarks on certain Statements in his Lives of* Black, Watt *and* Cavendish. *By the* Rev. WILLIAM VERNON HARCOURT, *F.R.S. &c.*

[Continued from p. 131.]

THERE are few things more remarkable in scientific history than the manner in which Newton may be observed to have dealt with the conjectural part of philosophy. He never speaks of hypothetical speculation but in terms implying somewhat of disdain. And yet in all his works, from the announcement to the Royal Society of his first discoveries respecting light to the last revision of the Optics and Principia, an hypothesis of the highest generality holds a conspicuous place.

This apparent inconsistence is however easily explained: he doubtless was deeply impressed with the error into which his predecessor Descartes had fallen, in building a system of philosophy on superficial analogies and precarious conjectures, and looked with some dissatisfaction at the pretension of his cotemporary Hook to set aside the inductive analysis of light, on the faith of a conjectural standard of his own. With Newton the imagining hypotheses was but as child's-play compared with the labour and importance of those severe and sure processes, inductive and deductive, to which he had devoted all the efforts of his mind. He held cheap the exercise of that great faculty of imagination from which the inexhaustible riches of his philosophical invention flowed with spontaneous facility. But though he laid no stress on what he called his "guesses," no man's mind seems ever to have been more continually, as it were, *upon the guess*; and no one ever gave so eminent and instructive an example of steady persistence in that conjectural habit of mind. "To show," says Newton, "that I do not take *gravity* for an essential property of bodies, I have added one question concerning its cause, choosing to propose it by way of question because I am not yet satisfied about it for want of experiments*." After having himself achieved by a vigorous induction the most extensive generalisation to which the human intellect has ever attained, he still saw, in a stronger light than any one, reasons for doubting whether the law at which he had arrived was so simple and conformable to the rest of nature as to preclude our tracing it to some more general cause. The ascertained rule of gravitation he used but as a stepping-stone on which he might safely tread in advancing towards the great end of

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* Advertisement to Optics, 1717.

philosophy,—the reduction of all that is implied in the terms space, force, and matter, to the closest relations and the fewest agencies: he regarded this great discovery with no more partiality than he did the more undeveloped principle of molecular cohesion, with respect to which, after stating his general conception of the force, he comes to this conclusion—"there are therefore *agents* in nature able to make the particles of bodies stick together by very strong attractions; and *it is the business of experimental philosophy to find them out*[*]."

The term *attraction*, be it observed, was always employed by Newton in a provisional sense. "How these attractions may be performed," he says, "I do not here consider: what I call *attraction* may be performed by *impulse*, or by some other means unknown to me; I use that word here to signify only in general *any force by which bodies tend towards each other*, whatsoever be the cause:" thus he was content to express, in any terms that lay at hand, the mathematical law, whilst he kept the efficient cause in reserve, laying down for the order of investigation this rule—"We must learn from the phænomena of nature what bodies attract one another, and what are the laws and properties of the attraction, before we inquire the cause by which the attraction was performed[†]."

The cause of gravity, whatever it may be, he conceived must also lie at the foundation of all the other great classes of force which we observe, and till their laws and properties should have been learnt, he knew that it would be premature to attempt any deep inquiry into their causes. Nevertheless he let loose his fancy in more than one excursion into this wide field of speculation; and it is worth our while to mark the manner in which he surveyed it. For he possessed beyond other men that double power of mind which can adapt itself equally to the furthest and nearest limits of vision, and cast a glance as comprehensive over remote objects, as precise and penetrating into those that are within reach.

The widest of the generalisations to which the conjectures of Newton ascended were marked by a character far different from any which appears in the speculations of those who preceded him. Instead of loose or narrow analogies, in forming his ideas of the interior mechanism and materials of the universe, he clothed the phantoms of his philosophical vision with the most certain and general of the properties of matter: for the hooked atoms of Epicurus, the broken fragments, subtle powder, rounded globules, and feathery filaments of Descartes, he substituted the conception of particles embodying invariable powers of inertia, solidity, and hardness, with

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[*] Optics, Book 3. Qu. 31.

[†] Ibid.

forces centrifugal, or centripetal, varying with aggregation and distance. Of such particles, grouped in various modes and degrees of condensation, and variously moulded by the hand of the Creator, he thought all material things might be imagined to consist, by such, both the stability of nature and the conservation of motion might be maintained, and from such, all the great classes of phænomena might be derived.

The general name which he gave to the simplest of these particles was *æther*—a term which he used for the substance of one, or more, highly subtle and elastic fluids, capable of being combined and condensed, and taking, in different states of condensation, the form of light and ordinary matter.

His æther was not a mathematical or mechanical abstraction, but a material substance, of the actual existence of which, certain otherwise uninterpretable phænomena, especially of light, heat, and electricity, had convinced him, and which he conceived of, as being "much of the same constitution with *air*, but far rarer, subtler, and more elastic"—"not of one uniform matter, but composed, partly of the main phlegmatic body of æther, partly of other various ætherial spirits, much after the manner that air is compounded of the phlegmatic body of air intermixt with various vapours and exhalations,"—one of these spirits being the electric, another the magnetic, a third the gravitating principle. The latter he figured to himself as "not of the main body of phlegmatic æther, but of something very thinly and subtilely diffused through it (perhaps of an unctuous, gummy, tenacious or springy nature*), and bearing much the same relation to æther which the *vital aërial spirit*, requisite for the conservation of flame and vital motions, does to *air*†."

This was the first speculation of Newton respecting "the cause of the gravitating attraction of the earth." "For if such an ætherial spirit," he adds, "may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter, for the common uses of nature (adhering to the sides of those pores after the manner that vapours condense on the side of a vessel), the vast body of the earth, which may be every where to the very centre in perpetual working, may continually condense so much of this spirit as to cause it from above to descend with great celerity for a supply: in which descent it

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* Such expressions as these, used only in the earliest of Newton's speculations, appear to be in the style of the Epicurean school; but his meaning, as is evident from the variety of the terms which he uses, was only to describe in *popular language*, attractive and repulsive force.

† Registry Book of the Royal Society, vol. v. from 1675 to 1679, p. 67.

may bear down with it the bodies it pervades with force proportional to all their parts it acts upon, nature making a circulation by the slow ascent of so much matter out of the bowels of the earth in an aërial form, which for a time constitutes the atmosphere, but being continually buoyed up by the new air, exhalations, and vapours rising under, at length (some part of the vapours which return in rain excepted) vanishes again into the æthereal spaces, and there perhaps in time relents and is attenuated into its first principles. For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fluid, subtile out of gross, and gross out of subtile, some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him: and they that will may also suppose that this spirit affords, or carries with it, the solary fuel and material principle of light, and that the vast æthereal spaces between us and the stars are for a sufficient depository for this food of the sun and planets*."

How far in a geometrical and mechanical point of view a supposition which presents to us the problem of an uniform central loss of force in a sphere of "*tenacious or springy*" fluid, urged by a constant pressure, and drawing down or impelling the bodies that float in it with a force proportional to the number of their ultimate particles, can have been contemplated as tending to satisfy the conditions of the law of gravity, I leave to mathematicians to judge. This supposition preceded the public announcement of the law by ten years; but Newton has himself stated that he had deduced that law from Kepler's some twenty years before he published it†.

He soon, however, in a letter to Boyle in 1678, abandoned this form of hypothesis for one in which he supposes the æther no longer a gradually absorbed, centripetal, atmosphere, but a *stationary* fluid, "which consists of parts, differing from one another in *subtilty* by indefinite degrees," so arranged by the force with which the *pores of matter* repel the *ætherial particles* in proportion to their *magnitude*, "that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the æther is insensibly finer and finer;" and in an ætherial atmosphere so constituted he holds that bodies would be propelled towards each other by the as-

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* Registry Book of the Royal Society, vol. v. from 1675 to 1679, p. 68.

† Letter of Newton to Halley, 1686.

sumed greater repulsion of the larger particles of æther from their pores. In this letter he made a comprehensive conjectural effort to reduce the whole system of the laws of nature, whether bearing the aspect of impulse or attraction, under the dominion of *two kinds of repulsive force*, the one of mutual repulsion between the particles of æther, the other of repulsion between the particles of æther and those of ordinary matter.

In the edition of his Optics which he printed nearly forty years afterwards, in 1717, he deliberately delivered, when in full possession of the laws of gravity, another hypothesis on this subject, taking for his fundamental assumption this fact presumed from the phænomena of light, that a subtle and elastic fluid, within bodies and without them, follows some law of density which increases from their centre indefinitely into space, and merely representing the force by which they gravitate as *repulsive*. Further he has not explained himself; and it may perhaps be inferred from his subsequently omitting in an edition of the *Principia* the mention of *gravity* when he enumerates, at the end of that work, the other phænomena of molecular attraction and cohesion, electricity, light, heat, muscular motion, and nervous sensation, which he attributes to the force of "a very subtle spirit," pervading and lurking in dense bodies, but not yet sufficiently manifested by experiments,—that he was dissatisfied with his own conceptions of its gravific action, and had never reduced them into a mathematical form. Thus much however it may be worth while to remark, as deserving perhaps the attention of those who may follow Vince and Playfair in discussing the possible sufficiency of Newton's hypothesis—that in the Optics he alleges reasons for supposing the *elastic force* of ætherial particles to be *inversely proportional* to their *magnitude**. This leaves ground to believe that with the supposition of a *density* increasing with the distance he may have combined his former conjecture of an increasing *magnitude* of the particles, and so far an elasticity proportionably diminished; which gives latitude at least to the hypothesis, as making the mutual repulsion of the particles at different distances from the centre, depend on more elements than one.

But the knowledge of the experimental laws of molecular force was not sufficiently advanced to justify any serious attempt at mathematical theory, either on this subject or any other connected with them; nor did he offer these hypotheses as more than cursory hints, and specimens of a generalising and simplifying spirit of conjecture, so far illustrating nature,

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* Optics, ed. 4. book iii. p. 326.

as they embraced, and embodied, real facts and accurate conceptions of phænomena.

The subjects on which in this point of view the above-mentioned hypotheses, taken together with the questions in the Optics, threw the most important light, were the phænomena of colours, and of chemistry. I shall confine myself to his speculations on the latter subject, which lead directly to the question at issue—namely, what were the ideas of philosophers before the time of Black respecting the nature of air, and whether the *unity* of the aërial element was any part of their belief.

Most remarkable, among the *divinations* of Newton, is his *introduction* of the doctrine of chemical affinity in the optical *queries*, where he connects the phænomena of chemistry with those of electricity, as both due to *molecular forces acting at insensible distances.* He enumerates electricity among those "attractions which reach to sensible distances, and so have been observed by vulgar eyes;" he then suggests, that "there may be *others* which reach to so small distances as hitherto escape observation," and adding that "perhaps *electrical attraction may reach to such small distances, even without being excited by friction*[*]," goes on to couple it with the phænomena of *chemical affinity*, as produced by the same species of force. What is this, if it be well weighed, but the principle of all that experience has since brought to light in respect to galvanic and electro-chemical forces? here was the prophet's eye, anticipating the progress of science and the actual indications of the kind of force which he surmised.

That which follows on the point of chemical affinity itself is equally remarkable. For observe how, guided in this instance by the few obscure phænomena before him, he deals with the molecules which represent this peculiar form of attraction: they are not elementary molecules, nor molecules of equal magnitude, but *compound particles whose force of affinity is in the inverse ratio of their composition*—"the smallest particles cohering by the strongest attractions, and composing bigger particles of weaker virtue, and many of these cohering, and composing bigger particles whose virtue is still weaker, and so on, for divers successions, until the progression end in the biggest particles on which the operations of chemistry, and the colours of natural bodies depend, and which by cohering compose bodies of a sensible magnitude[†]." Have we not, in this conception of chemical affinity as depending on the *successive addition of units of force*, the principle of multiple proportions, of which the experimental demonstration was

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[*] Optics, book iii. p. 351.

[†] Ibid. p. 370.

reserved for Dalton, whose first views of that important induction were suggested perhaps by these very conjectures of Newton?

In other respects the theory of affinities is hardly laid down by him with more distinctness in this mature work, than in his younger speculations, in the earliest of which he applied it, as an universal property of bodies, to supposed ætherial fluids, and in the next to the factitious airs then recently discovered.

The chemist who remembers the modern observation, that gases (including that *vital aërial spirit* to which Newton compared his æther) are powerfully condensed in the pores of charcoal, on the surface of metals, and in the interior of a ball of spongy platina, cannot fail to be struck with the singular anticipation which the *first* of Newton's *hypotheses* display, of a close connexion between molecular attractions and chemical changes, and a subjection of the most elastic of bodies to both these forces in common. Nor will his admiration be diminished, when he finds the theory of elective and mediating affinities first broached for such a purpose as to explain the dark phænomena of muscular motion, and the material means through which the soul acts on the body, by the supposition of *relative degrees of sociableness and unsociableness* between the brain and muscles on the one hand, and on the other, a conjectural array of ætherial fluids imagined to be even rarer and more elastic than the most subtle and repulsive air *.

After this, we are not astonished to find the same master mind, in its *second* survey, so laying down the theoretical map of gaseous chemistry, that in truth the chemists who followed, down to the æra of Higgins, Dalton, and Gay-Lussac, did little more than work out by experiment the principles which Newton had assumed.

The application of chemical principles to ætherial matter is contained in a letter to Oldenburg, from which I have already given some quotations, read before the Royal Society in Dec. 1675. This elaborate communication, strange to say, has never been printed, except in the ponderous and seldom opened volumes of Birch's history of that Society, and consequently is scarcely known, even in our own country, to men of science, otherwise than by a few extracts from that part of it which relates to light, published in the Philosophical Transactions by Dr. Young.

The theory of gases, as communicated to Boyle in 1678, you will find in Birch's life of that philosopher, or in Newton's collective works. In his letter to Boyle, after supposing

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* Letter to Oldenburg, Registry of the Royal Society, vol. v.

certain atmospheres of æther to surround the particles of bodies, and describing a pressure of elastic forces, which varying with the distance produces cohesion at small distances, and repulsion at greater, he deduces among other consequences this—"that the particles of vapours, exhalations, and air, do stand at a distance from one another, and recede as far from one another as the pressure of the incumbent atmosphere will let them: for I conceive," he says, "the confused mass of vapour, air, and exhalations, which we call the atmosphere, to be nothing else but the particles of all sorts of bodies of which the earth consists, separated from one another and kept at a distance by the said principle."

He then proceeds to distinguish the *three* different ways which nature has of "transmuting gross compact substances into aërial ones"—vaporisation—volatility—and the liberation of fixed air, and to propose a theory to explain the differences. From the hypothesis, to which I before alluded, of a double repulsive force, producing unequal degrees of ætherial pressure, he deduces different spheres of cohesion and repulsion for different bodies, and their particles, in proportion to their density and size: small particles are easily detached, and easily condensed; and this is the condition of volatile substances, and of liquids—"when the particles of a body are very small, as I suppose," he says, "those of water are, the action of heat may be sufficient to shake them asunder;" and "as fast as the motion of heat can shake them off, those particles, by the said principle, will float up and down at a distance from one another, and from the particles of air, and make that substance we call vapour." "But if the particles be much larger, they then require the greater force of *dissolving menstruums* to separate them." Thus he comes to the chief object of this letter, which was to illustrate the theory of gases—of the substances, that is, then recently discovered to be more *durably fixed*, and more *durably aërial*, than vapours or volatile effluvia. For this purpose, having assumed that the essence of such substances is, that their constituent particles are relatively larger and denser, and therefore, by hypothesis, more elastic than others in the *aërial*, and more cohesive in the *fixed* condition, he brings in the doctrine of chemical affinities, elective and mediate, to liberate them from their close state of cohesion, and force them out of the proximate sphere of compression into the remoter one of repulsion. And thus, as subsidiary to a wild play of philosophical fancy, were those great principles laid down, which experience has subsequently verified, and on which the whole fabric of the chemistry of solids, liquids, and gases, has been built.

In these views the new discovery of the various permanence and condensability of the gases has a conspicuous place: "On the same difference of size," he says, "may depend the more or less permanency of aërial substances in their state of rarefaction." "This may be the reason why the small particles of vapours come easily together and are reduced back into water, unless the heat which keeps them in agitation be so great as to dissipate them as fast as they come together, but the grosser particles of exhalations raised by fermentation keep their aërial form more obstinately, because the æther within is rarer. Nor does the size only, but the density, of the particles also conduce to the permanency of aërial substances: for the excess of density of the æther *without* such particles above that of the æther *within* them is still greater: which has made me sometimes think that the true permanent air may be of a *metallic* original, the particles of no substances being more dense than those of metals. This I think is also favoured by experience: for I remember I once read in the Philosophical Transactions how M. Huygens at Paris found, that the air made by dissolving salt of tartar would in two or three days' time condense and fall down again; but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then how by the continual fermentations made in the bowels of the earth there are aërial substances raised out of all kinds of bodies, all which together make the atmosphere, you will not perhaps think it absurd, that the most permanent part of the atmosphere, which is the true air, should be constituted of *these*; especially since they are the heaviest of all others, and so must subside to the lower parts of the atmosphere and float upon the surface of the earth, and buoy up the lighter exhalations and vapours to float in greatest plenty above them. Thus I say it ought to be with the metallic exhalations raised in the bowels of the earth by the action of acid menstruums; and thus it is with the true permanent air."

These extracts show that Newton considered the hydrogen gas which Boyle had obtained from iron, and the nitrous gas which Huygens had obtained from copper, as consisting of the ultimate particles of the iron and copper themselves, brought into a state of aërial elasticity; and further, that apprehending his ætherial hypothesis to be thus strengthened by experimental facts, he proceeded to generalise so boldly, as to conclude that the whole body of the inferior atmosphere may be constituted of various metallic substances, and that the power and persistence of elastic force in different kinds of air may be proportionate to the size and density of their chemical elements.

This supposition, that the most permanent airs are of a metallic origin and nature, representing hydrogen for instance as *ferreous gas*, was set aside by the experiments of Cavendish, which proved that the gas from *iron* is identical with the gas from *zinc*, in specific gravity, in explosive power, in the quantity in which it combines with oxygen, and in the result of the combination: they went also, as far as our experiments reach, to invalidate the general supposition that the repulsive force of the particles of matter is in proportion to their weight and density; since they proved that hydrogen is, both in its *elastic* and in its *fixed* state, the lightest of bodies; unless indeed its high refractive power should be thought a stronger argument for the density, than its low combining weight for the lightness, of its molecules.

Newton seems not to have been aware, that the facts of the condensation of one gas and permanence of another, the observation of which he here ascribes to Huygens, had been established some ten years before by experiments instituted at the Royal Society, in which his correspondent Boyle had assisted—a circumstance however which was notified to the public when Huygens's paper was printed*. The experiments themselves having, I think, never been published, the interest which we equally take in tracing back the history of science, the curiosity of the experiments, and the celebrity of the experimenters, prompt me to give you some extracts on this subject from the journals of the Society.

From these it appears that on January the 4th, 1664, a year before the publication of the Micrographia, Hook exhibited to the Society "experiments to show that air is the universal dissolvent of *sulphureous* [combustible] bodies, and that

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* An account of Huygens's experiments was printed at Paris in 1674, and appears in the Philosophical Transactions, No. 119, dated November 22, 1675, under the title of—"some experiments made in the air-pump by M. Papin directed by M. Hugens." The following extract contains the facts to which Newton referred:—"The experimenter being desirous to see whether these ebullitions did make *new air*, put in the recipient a gage, and observed that when the liquors were mingled, the water in the gage rose very nimbly to the top of the gage; and drawing out the new air he made the gage-water subside again; and by this means it was seen, that all these kinds of ebullition make an air which expands itself like common air. Yet here is something that seems to be very remarkable, which is, that the air made by these ebullitions *is not of the same nature*: for it has been found experimentally, that the air formed by the mixture of *aquafortis* and copper remains always *air*, and always keeps up the water in the glass; but on the contrary, the air which has been made by the mixture of oil of tartar and oil of vitriol is almost all destroyed of itself, in the space of twenty-four hours. All these ebullitions hitherto spoken of are greater in *vacuo* than in the open air; but with lime it is not so."

this dissolution is fire, adding that this was done by a nitrous substance inherent in, and mixed with, the air."

Here was the first distinct conception, and evidence, of the composition of the atmosphere. The French physician Rey had before proved that air enters into fixed combination with solid matter: his proof rested on a capital observation which he quotes from the *Basilica Antimonii* of Hamerus Poppius: this chemist, "placing," says Rey, "a burning glass in the sun's rays, directed their focus on the apex of a cone of antimony, till the whole becomes white, when the calcination is complete. It is a wonderful thing, Poppius added, that although in this calcination the antimony loses much of its substance by the vapours and fumes which exhale copiously, yet so it is, its weight increases instead of diminishing*." The philosophical acumen of Rey seized on the truth unequivocally shown in this simplified form of calcination, in which he discerned the presence of but two ponderables, and he concluded,—1. That the increase of weight arose from the air being solidified in the antimony; 2. That the two substances combined to a definite degree of saturation; 3. That the increase of weight observed in other metals, whether by calcination or simple exposure to the air, is due to the same cause—conclusions which, if their publicity had been equal to their value, would doubtless have been recorded for the early and distinct enunciation which they contain both of a fundamental principle, and an important though as yet unanalysed fact, as the first step in this branch of science, in consequence as well as time†.

But Rey, though he recognised the ponderable and combining qualities of air, considered it with the other philosophers of his day, as an element simple in essence, though mutable in form: and the first scientific question of the accuracy of this supposition was raised by Boyle in 1654. In the same Essay in which his discovery of the factitious airs was announced, he quoted from Paracelsus the following remarkable passage:—"As the stomach converts meat, and makes part of it useful to the body, rejecting the other part, so *the lungs con-*

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[*] Essay 25.

[†] Rey's work was first published in 1630. It contains, besides the speculation here mentioned, a just correction of the view which *the schools* had taken of a fact affirmed in the Physics of Aristotle—that a blown bladder is heavier than an empty one. Rey showed that this is true only if the bladder be blown to such a degree as to *compress* the air, and that the fact, so stated, is a real proof that the air has *absolute weight*. This is perhaps the first correct *published* statement of the weight of air, as an experimental fact. It is evident however from a letter of Baliani, quoted by Venturi, that Galileo had not only taught the same doctrine, but made his experiments on the specific gravity of the air before 1630.

sume part of the air, proscribing the rest;" and having observed upon it, that "though this opinion is not, as some of the same author's, absurd, it should not be barely asserted, but explicated and proved," proceeded to relate, that "that deservedly famous mechanician and chemist Cornelius Drebell contrived for the late learned king James a vessel to go under water," and that on inquiry of Drebell's surviving relatives into the principle of his contrivance, it appeared, that "he conceived it to be not the whole body of the air, but a certain quintessence or spiritual part of it that makes it fit for respiration, which being spent, the remaining grosser body, or carcase, if I may so call it, of the air, is unable to cherish the vital flame residing in the heart; so that, for aught I could gather," says Boyle, "besides the mechanical contrivance of his vessel, he had a chemical liquor which he accounted the chief secret of his navigation; for when from time to time he perceived that the finer and purer part of the air was consumed, or overclogged by the steams and respiration of those that went in his ship, he would, by unstopping a vessel full of this liquor speedily restore to the troubled air such a proportion of vital parts, as would make it again for a good while fit for respiration*."

The experiments which Hook exhibited to the Royal Society in 1764 afforded, it must be allowed, but precarious grounds for the theory of the composition of the atmosphere which his sagacity advanced: he showed that in vessels containing a limited quantity of air, combustibles burn and waste for a limited time; and change its quality so that it is no longer capable of supporting combustion; and he showed that they undergo no loss of substance when heated without air: he took some live coals and put them under a glass vessel—"whereupon the said cole in a little time went out; but being then taken out, and exposed to the free air, recovered its burning:" sulphur in like manner would not burn when "hermetically sealed," and charcoal heated without air—"was not sensibly diminished;" he added—"that a combustible substance kept red-hot, yea in a fire as hot as to melt copper, would not waste, but as soon as fresh air was admitted did burn away and consume." Boyle proposed that trial should be made whether the extinguished combustible could be relighted by the burning-glass, or by red-hot iron; and it was found that it could not be rekindled without the admission of air.

Yet nothing can be more accurate than the theoretical account of combustion given in the Micrographia, where, laying

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* New Exp. Physico-mechanical.

down the principle that "the different volatility, or fixedness, of the parts of bodies seems to consist only in this, that the one is of a texture, or has component parts, which will be easily rarefied into the form of air, and that the other hath such as will not without much ado be brought to such a constitution," Hook states that "in the dissolution of *sulphureous* [combustible] bodies, by a substance inherent in, and mixed with, the air, which is like, if not the *very same* with, that fixed in saltpetre, a certain part of the bodies is united and mixed, or *dissolved and turned into*, the air, and made to fly up and down with it, in the same manner as a metalline or other body, dissolved into any *menstruum*, doth follow the motions and progress of that menstruum till it be precipitated."

Even the fact, afterwards proved by Cavendish, of the *density* of the gaseous product of this *dissolution*, was predicted by Hook; for in an experiment—"to prove that the substance of a candle or lamp is dissolved by the air, and the greatest part thereof reduced into *a fluid in the form of air*,"—he observes, that "the reason why this mixed body, which *certainly is otherwise heavier than the air*, and so ought to descend, doth notwithstanding ascend, is from the extraordinary rarefaction of the same by the nearness and centrality of the flame and heat, whereby it is made much lighter than the ambient air*."

"* Experiment to prove that the substance of a candle or lamp is dissolved by the air, and the greatest part thereof reduced into a fluid in the form of air—showed the Royal Society 22–29 Feb. 1671–2."—Registry of the Royal Society.

"Using a large reflecting glass, or convex refracting one, so placed in respect to my eye that a candle, set at a certain distance beyond the refracting glass, or between the eye and the surface of the reflecting glass, enlightened the whole area of the said glasses in respect to the eye, then continuing to keep the eye in that place where the area of the glasses appeared to be wholly filled with the flame of the candle, I caused another candle to be placed very near the said glasses, between the eye and the glass, or beyond where I used the refracting glass, then looking steadfastly at the flame of the last candle, it was very plain to be perceived, that the flame thereof was encompassed with a stream of liquor, which seemed to issue out of the wick, and to ascend up in a continued current or *jet d'eau*, and *to keep itself entire and unmixed with the ambient air*, notwithstanding that it was a considerable way carried above the aforesaid flame. 'T was yet further observable that the shining flame was placed in the midst of this *jet d'eau* at the lower end thereof, but that it did not ascend proportionally in height to the height of the *jet d'eau*, that where the tip of the flame ended, there ascended up a small line of an opacous body or smoke, which to a good height above the flame kept the middle of the stream. The manifestation of these phænomena was from the differing refraction of the body of the *jet d'eau* from that of the ambient air, for the flame of the first candle being but small and placed at considerable distance from the refracting and reflecting globe, the smallest variation in the refraction of the

The *production* of volatile salts in combustion, by an analogous process of combination, seems likewise to have been apprehended by him, where he represents "other parts of the combustible," not capable of the aërial form, as nevertheless so "mixing and *uniting* with the parts of the air," as "to make a coagulum or precipitation, as one may call it, which is separated from the air," but being light and volatile is carried up by its motion, till the agitation that kept it rarefied ceases, and it condenses into "a certain salt which may be extracted out of soot:" and the view thus expressed appears from the Registry to have been corroborated at one of these sessions by Boyle, who observed that "vegetables reduced in the open air yield store of volatile salt like that of hartshorn and other animal bodies, whereas in common distillations he had not found them to yield a grain."

Hook produced evidence also before the Society of that sameness of effect, by which he identified the particular ingredient in the air that supports combustion with one of the fixed constituents of nitre. To this purport he "made an experiment with charcoal enclosed in a glass, to which nitre being put, and the hole suddenly stopped up, the fire revived, although no fresh air could get in,"—and another "of gunpowder burning without air."

It is curious to remark that a similar experiment was made some fifty years before by the Cabbalist and Rosicrucian antagonist of Kepler and Mersenne, Fludd; who in proof "that the substance of saltpetre is nothing else but *air* congealed by cold*," relates that he filled an egg with it, mixed with sulphur and quick lime, and closing the aperture with wax

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medium between the first candle and the eye caused the darkness to intermix with the light, so as to exhibit the appearance of the heterogeneous *jet d'eau*. This *jet d'eau* I suppose to be nothing else but the mixture of the air with the parts of the candle which are dissolved into it in the flame. The reason why *this mixed body, which certainly is otherwise heavier than the air*, and so ought to descend, doth notwithstanding ascend with great swiftness, is first from the ascent of the flame in the middle, and next from the extraordinary rarefaction of the same by the nearness and centrality of the flame and heat, whereby it is made much lighter than the ambient air."

* "Videmus salis petræ substantiam nihil aliud esse quam *aërem* frigore congelatum, cui si accedit sulphuris aliqua portio, licet exigua, admodum strepitum ingentem edit, fulguraque artificialia emittit."—Utriusque Cosmi Historia, vol. i. tract. 1. lib. 7. cap. 6. De fulmine et tonitru, 1617. "In 2da demonstratione, candela in fundo vasis alicujus aqua repleti affigitur, cujus flamma per orificium phialæ ingrediens, depresso ejus orificio ad angulos rectos cum candela in vasis aquæ, sursum attrahet tantam aquæ proportionem quantam aëris in phiala inclusi consumpserit; aër enim nutrit ignem, et nutriendo consumitur; ac ne vacuum admittatur, aqua, hoc est tertium elementum, locum possidet aëris comesti."—Ibid. tract. 2. part. 1. lib. 3. Reg. 6.

placed the egg under water, where it exploded. Fludd also burnt a candle in a glass vessel over water, and observed that it raised the water in proportion to the quantity of air, enclosed in the vessel, which was consumed and burnt; for "air," he adds, "nourishes fire, and in nourishing consumes it." This sounds like the truth which Hook announced: but Fludd had no distinct idea of *the weight* of air, or of the great principle which led that philosopher to predict, and observe, *ponderable* products from its consumption.

Boyle supported Hook's views by "affirming that gunpowder burns very well in a receiver out of which the air has been extracted," and he afterwards took the pains to experiment with nitre compounded of nitric acid and potash out of contact with the atmosphere "*in vacuo* Boyleano," for the sake of "removing the suspicion that it does not burn without air being supplied by the numerous eruptions of the aërial particles *intercepted* by those that by their coalition make up the nitrous corpuscles*." Boyle also remarked at this discussion that "*tin* mixed with nitre will kindle it;" to which Hook added, that filings of iron will do the same. This remark was justly deemed of such importance, that the Society ordered the experiment to be tried; and it was found that "filings of tin being cast on nitre, over a fire, made it flame; though it be not known," adds the writer of the Minutes†, "that *sulphur* was ever extracted out of tin; which seems to infer that there are bodies *combustible* which are not *sulphureous*."

The only verification, in the Registry, of the intimation given in the Micrographia that the same principle in the air which supports combustion is concerned "in respiration and the preservation of life," consists in an experiment suggested by Dr. Ent, in which a bird was enclosed with a chaffer of live coals in a receiver; the Society observed the extinction of the fire to be followed by failure of vitality in the bird, which revived on the readmission of air.

After these inquiries Dr. Wilkins proposed (on the 8th of March 1764) "that the following experiment (of Dr. Wren's suggestion) might be made, viz. to put a fermenting liquor in a glass ball to which a stop-cock should be fitted, and to tie a bladder about the top of the stop-cock, by which means a certain air generated by the fermenting liquor would pass into the bladder, and upon the turning of the stop-cock be kept there in the form of air without relapsing into water. This, or the like, to be tried at the next meeting. Mr. Hook mentioned several liquors that by their working upon one another

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* New Experiments touching Flame and Air.

† Oldenburg.

would generate *an air*; viz. oil of tartar and vitriol, spirit of wine and turpentine, &c. Colonel Blunt added that oysters pounded and put into wine would make it ferment."

"On the 15th of March, the experiment of generating air was made in this manner. There was taken a common glass phial with two pipes, and some pounded oyster-shells and aquafortis; and as soon as the aquafortis was by one of the pipes poured in upon the powder, and the hole stopped with a piece of hard cement, the ebullition caused by the corrosion of the shells by the aquafortis did in a very little time blow up the bladder (tied on the other pipe) so as to swell it with air, very plump; which expansion remained till the rising of the Society, when the vessel in that posture was locked up in the box of the watch, to remain there until the next assembly." Dr. Wren made use of this experiment—"to explicate the motion of the muscles by explosion." "There was also taken a bottle containing strong ale that had been bottled awhile; and over the bottle's mouth was tied an ox-bladder out of which the air was squeezed; after which by loosening the cork by degrees the air was blown out into the bladder by the expansion of the fermenting liquor within, and the bladder was almost half-filled with an aërial spirit generated by the working liquor." Mr. Boyle, bearing perhaps in mind his anecdote of Drebell's submarine vessel, suggested that the experiment was capable of improvement for the producing of air under water, and mentioned coral, or oyster-shells, and distilled vinegar, as wholesome substances for that purpose: he moved that an animal might be put into the receiver of his engine and the air exhausted till the creature grew sickly, and that then some new air might be produced in the receiver by a contrivance of making distilled vinegar work upon coral, to see whether by this means the animal could be revived. Dr. Wilkins moved that at the next meeting the air generated by the mixture of aquafortis and the pounded oyster-shells might be blown into a dog's or cat's mouth, to see what would be the effect thereof."

"On the 22nd of March there were two experiments made for the finding out a way to breathe under water, useful for divers. The first was made by putting a bird into a rarefying engine, and with it a glass bottle with distilled vinegar and pounded oyster-shells, which whilst the vinegar is dissolving them affords a stream supposed to be *a kind of new air* fit for respiration. The bottle was also close stopped with a cork, so ordered that by pulling the stop-cock placed on the top of the receiver, the cork might, by turning it, be pulled out without admitting an ingress of the external air into the receiver at all: then the receiver being accurately cemented to the engine the air was pumped out; whereupon the bird grew sick, and when he was thought near dying, the bottle was unstopped, that the streams and supposed air that had been shut up in it during the operation might have liberty to expand themselves in the receiver for the refreshing and recovering of the animal: but here it succeeded not; in so much that though the bird was taken out of the receiver and exposed to the fresh air, yet it recovered not."

"The other experiment was made with a kitling after the manner of the former, only that instead of distilled vinegar was employed aquafortis, whereof the success was, that the air being drawn out till the cat had done struggling, and was upon the point of expiration, and the bottle being unstopped to emit the streams and supposed air into the receiver, the cat did soon begin to recover, whereupon the animal had fresh air given it, which was again exhausted, to see whether it would revive of itself, without any nitrous exhalation; but after this exhaustion the cat appeared to be dying, whereupon she was after a little while taken out into the open air wherein she revived again."

"It was also moved that a standard might be used to know what quantity of air was generated."

"The glass phial with the swelled bladder, experimented upon at the last meeting and shut up till this day, was produced, and the bladder found evidently shrunk. Ordered to be tried next day with a glass phial whelmed under water, thereby to gather all the bubbles of the air generated by the corrosion."

"On being inquired how it was known that that which was supposed to be air produced by the dissolving of pounded oyster-shells by spirit of nitre, or distilled vinegar, or aquafortis, was true air, and answer being made by the President*—that *a body rarefied by heat, and condensed by cold, was air*, the bladder was put to the fire where it expanded again as much as formerly and being removed from thence became somewhat flaccid again."

"It being moved that it might be tried whether the streams produced by the operation of distilled vinegar upon the powder of oyster-shells were convenient for respiration, the trial was made, and the bottle wherein that dissolution was performed, carried about to the company for every one to smell to it, and it was found by most of the company incommodious, as it was undiluted."

"It being moved by Mr. Hook that the air-boxes contrived

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* Lord Brouncker.

for diving might be tried by the persons bespoke by Mr. Pepys for diving, it was ordered that this diver should be sent to Mr. Hook to be instructed by him touching the use of the said boxes under water."

"On the 29th of March an experiment was made for the generating of air by putting aquafortis and the powder of oyster-shells in a small glass phial under water, and whelming a large glass filled with water over it to receive the steam to be generated by the corrosion: the success whereof was that the whelmed glass was filled about $\frac{1}{4}$th full with an aërial substance—ordered to be set by till the next meeting."

"It was moved that a way might be thought on, of producing an air that might be useful to respire."

"On the 12th of April Mr. Boyle proposed [inter alia] to try whether the eggs of silk worms and snails would be hatched, as also whether seeds would germinate and thrive, all, in an exhausted receiver."

"Dr. Goddard affirmed that plants live as much upon air as the earth."

"Mr. Hook, being called upon to give an account of one of the last days experiments touching the air generated by aquafortis and the powder of oyster-shells, reported that the greatest part of it was returned into liquor."

"The same was ordered to make, the next day, the experiment of generating air with bottled ale, supposed to be wholesome to breathe in, which the air hitherto generated is not*."

On June the 14th "an account was given of an experiment of the growth of water-cresses in a receiver." Having been kept for a week in an exhausted receiver they showed no growth; the air being admitted "they grew in the same time two or three inches."

These experiments remained unprinted: but a more complete discussion of the same subjects not long afterwards appeared. In 1668, at the early age of 23, Mayow, adopting the theory of Hook, published a tract in which "he delivered his thoughts of the use of respiration, waving those opinions that would have it serve either to cool the heart, or to make the blood pass through the lungs out of the right ventricle of the heart into the left, or to reduce the thicker venal blood into thinner and finer parts, and affirming that there is *something in the air* absolutely necessary to life, *which is conveyed*

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* On the 24th of May in this year (1764) the following record is entered: "The king had been pleased himself to make the observation (on the variation of the needle) at Whitehall, and had found no variation at all, the needle standing in the meridian."

into the blood, which whatever it be being exhausted the rest of the air is made useless, and no more fit for respiration; where yet he doth not exclude this use, that, together with the expelled air, the vapours also steaming out of the blood are thrown out. And inquiring what that may be in the air so necessary to life he conjectures that it is the more subtle and nitrous particles with which the air abounds which are communicated to the blood through the lungs, and this *aërial nitre* he makes so necessary to all life, that even plants themselves do not grow in earth deprived thereof*."

In 1673-74 he gave a fuller account of his opinions in another treatise†, the views contained in which exhibit one of the finest examples extant of the success with which a man of philosophical genius, having seized a true principle, may deduce from the observation of a few facts distinctly apprehended a whole train of real and important consequences, long before the principle itself can be deemed to have been proved by demonstrative experiments.

In reproducing the theory of the Micrographia, he took no care to give the original author of it the credit which was due; and in his own turn is passed unmentioned by Lavoisier, who did not distinguish this precursor of his own discoveries from the rest of his chemical predecessors, on whom he pronounces this general censure—that "they all allowed themselves to be carried away by the spirit of their age, which contented itself with assertions without proofs, or at least often regarded very slight probabilities as such ‡,"—a censure which it is but just to qualify by the reflection, that in experimental philosophy solid proofs are not to be discovered without the preliminary of happy conjectures.

In this tract Mayow expressly says, "Though the particles of air are very minute, *and are vulgarly taken for an element of the greatest simplicity*, it appears to me necessary to judge them to be a compound §; and he adds,—"it is manifest that the air is deprived of its elastic force by the respiration of animals much in the same manner as by the deflagration of flame." The latter assertion he made good by experiment: he not only observed, but measured, *the amount of elastic force lost in both these cases*; and he proved that animals, when confined in air which has been already diminished by combustion, *survive but half the time* that they would have lived

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* "An account of two books—Tractatus duo, prior de Respiratione, a Joh. Mayow, Oxon. 1668." Phil. Trans. No. 41. p. 833.

† Tract 5. *Med. Phys.* Imprim. Jul. 17, 1673.

‡ *Traité de Chimie.* Discours préliminaire, tome i. p. 16.

§ *Tract. de parte aërea igneaque Spiritus Nitri*, cap. 7. p. 114.

in an equal volume of common air[*]. But he advanced little beyond his predecessor in demonstrating the air to be a compound. It is not to be supposed he says that that aërial supporter of combustion is *the whole air*, but only *a part of it*, which is more active and subtle than the rest; since a light enclosed under a glass expires, even whilst the vessel still contains abundance of air: for we cannot believe that the particles of air which *were* in the said glass can be *annihilated*, nor yet *dissipated*; since they cannot pass through the glass." But this reasoning, though probable, is not conclusive; since it was certainly possible that the enclosed air might have been diminished by condensation instead of abstraction, and have become unfit to burn and to be breathed by a total vitiation, instead of a partial loss.

Yet, after all, Mayow's reasoning appears to advantage by the side of Priestley's, or Scheele's, even when in the progress of experiment his nitro-aërial spirit, or fire-air, had been actually divorced from "*its consort*," and when the latter great chemist had approached a complete analysis of the atmosphere. For so difficult did Scheele find it to interpret his own experiments, that when he had in his hands the "*liver of sulphur*" which had produced a given diminution in a given volume of air,—when he had found the specific gravity of the diminished air to be less than that of common air, and the "fire-air," which he had succeeded in separating from numerous substances, to have a greater specific gravity, as well as a greater power of supporting combustion,—when by reuniting them he had recomposed an air with all the properties of common air restored,—when he had arrived at the conclusion—"that the air consists of two different kinds of elastic fluids," and that the "*fire-air*" makes between a third and a fourth of the whole bulk,—when coming finally to the ultimate question of the analysis, he failed to find the "*lost air*" in the *liver* of sulphur. Then he gave the reins to his imagination, and embracing the idea, that heat is a compound of "fire-air" with an imaginary substance invented by Stahl, concluded that by the action of a double affinity the "fire-air" in his experiment had combined with the *phlogiston* of the liver of sulphur, and that the compound had passed through the pores of the glass by which it had before been confined. Where

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[*] *Tract. de parte aërea igneaque Spiritus Nitri*, cap. 7. p. 101. "Comperi aërem per lucernæ deflagrationem in spatium ex parte circiter tricesimâ minus quam antea reductum esse. Postquam fumi lucernæ deflagrantis, quibus cucurbita prædicta repleta est, prorsus evanuerunt, vitrumque intus æque ac prius pellucidum evasit, conatus sum secunda vice lucernam in eadem accendere, radios solares in aliam camphoræ portionem, in vitro eo pariter suspensam, uti prius conjiciendo."

weight disappears, analysis is impossible. So he left the composition of the atmosphere to be demonstrated by those who believed, with Mayow, that elastic fluids cannot penetrate glass, and who took the pains to weigh both the air and the substances by which it was diminished; whilst he went on pursuing the phantom of his imaginative genius to the examination of imponderable essences and the great discovery of the chemical forces of light, and of the distinctions between the heat of contact, and the heat of radiation*.

But what shall we say to the improvements of Priestley on the principles of Mayow? Priestley—who many months after he is said by you, and others, to have discovered oxygen gas, tells us himself, that he "had no doubt it had all the properties of genuine common air." On the 1st of August 1774, Priestley with a burning-glass, following the method of Boyle, collected this gas and observed "that a candle burnt in it with a remarkably vigorous flame, but did not give sufficient attention to the circumstance at that time—that the flame of the candle, besides being larger, burnt with more splendour and heat, than in nitrous air exposed to iron or liver of sulphur." In the October following, "I mentioned," he says, "my surprise at the air I had got, to M. Lavoisier, but at the same time had no suspicion that it was wholesome, so far was I from knowing what it was that I had really found, and taking for granted that it was nothing more than such kind of air as I had brought nitrous air to be by the processes above-mentioned." He mentioned it also to all his philosophical acquaintance at Paris and elsewhere, "having no idea at that time to what these remarkable facts would lead." On the 19th of November however, having agitated it in water, he "found that a candle still burned in it as well as in common air," though after "the same degree of agitation phlogisticated nitrous air would certainly have extinguished a candle." "In this ignorance," he adds, "of its real nature I continued from this time to the 1st of March following." "But in the course of this month I not only ascertained the nature of this kind of air, though very gradually; but was led by it, as I then thought, to the complete discovery of the constitution of the air we breathe. Till this 1st of March 1775, I had so little suspicion of its being wholesome, that I had not even thought of applying to it the test of nitrous air;" "but it occurred to me at last to make the experiment, and putting one measure of nitrous air to two measures of this air, I found not only that it was diminished, but that it was diminished quite as much as common air, and that the redness of the mixture was likewise equal to

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* Scheele's Experiments on Air and Fire.

that of a similar mixture of nitrous and common air. After this I had no doubt but that the air from *merc. calcinatus* was fit for respiration, and that it had all the other genuine properties of common air. But I did not take notice of what I might have observed if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater, than common air would have admitted. *I now concluded that all the constituent parts of the air were equally and in their proper proportion imbibed in the preparation of this substance, and also in the process of making red lead*[*],"—a conclusion identical with the ideas of Rey in 1630.

The next step in Priestley's inquiry was the employment of Mayow's mice, which convinced him that this air was *longer* respirable than common air; but his ideas of it were less accurate than Mayow's, for instead of considering it, with him, *a constituent part of nitric acid*, he thought it *a compound of nitric acid and earth*; and in December 1777, "no doubt remained on his mind that atmospheric air, or the thing that we breathe, consists of the nitrous [nitric] acid and earth, with so much phlogiston as is necessary to its elasticity, and likewise so much more as is necessary to bring it from its state of perfect purity to the mean condition in which we find it."

You now see the error into which you have fallen when you represent Priestley as discovering *before Lavoisier* that "this was a gas wholly different from all other gases formerly known," and may perhaps suspect that you are not justified in condemning as "*an unworthy and lamentable proceeding*" on Lavoisier's part, "*the intruding himself into the history of this discovery, knowing that Priestley was the sole discoverer.*" A property of this gas, which under Priestley's observation had led to nothing, in the hands of Lavoisier gave rise to one of the most important investigations in the annals of chemistry; he, it appears from your own admission, had ascertained the relations of this elementary substance to various bases and to the atmosphere, between August 1774 and March 1775, at which date the foregoing extracts show the "*sole author of the discovery*" to have "had no doubt that it had all the genuine properties of *common air*." Whoever may be called the discoverer of oxygen, whether Hook and Mayow, who first inferred its existence in nitre and in air,—or Boyle, who first *disengaged* the elastic gas from *minium*,—or Hales, who *collected* it from the same material,—or Nieuwentyt, who attri-

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[*] Experiments and Observations on different kinds of Air, vol. ii. p. 113, ed. 1790.

buted its elasticity to "the expansion of the fire particles lodged in the minium, supposing fire to be a particular fluid which maintains its own essence and figure, remaining always fire, though not always burning,"—or Priestley, who observed that it supported combustion,—or Lavoisier, who distinguished it as a gas, *sui generis*, and determined its principal combinations,—if the question be, which of these names deserves the highest place in "*the history of this discovery*," a philosopher I apprehend might be apt to hesitate,—especially perhaps between those which stand *first* in the list, and that which stands *last*.

But you have made a greater mistake in attributing to Priestley the discovery of *nitrogen*[*]; and in that mistake have again wronged Cavendish of his due. Had you taken the trouble to read a paper on this subject which I have published from his MSS.[†], you would have found that the same philosopher, who exceeded all his cotemporaries in analysing the air with accuracy, was the first who demonstrated it to contain, after burning, a mephitic gas, incapable of supporting combustion, and *distinct from fixed air*: you would have known that, some time before Priestley's publication in the Philosophical Transactions of March 1772, Cavendish communicated to him this paper, containing all the details of an experiment, in which a measured volume of air, confined under water, was passed backwards and forwards through a bent tube filled with powdered charcoal, and heated red-hot—the absorption was found to be definite, and the total loss of volume was ascertained—the fixed air was separated by soap-leys—the volume separated was observed and deducted—the specific gravity of the residual gas was examined, and it was found "rather lighter than common air;" lastly, it was found to extinguish flame, but to extinguish it, by the criterion of the watch, more slowly than fixed air. I know that you would be far from conceding to me that any experiment, however skilfully devised, carries with it its own conclusions: but then you would have known too the very words in which Cavendish conveyed those conclusions to Priestley—"The natural meaning of *mephitic* air is any air which suffocates animals; and this is what Dr. Priestley seems to mean by the word: but in all probability there are *many kinds of air* which possess this property: I am sure there are *two*—namely *fixed air*, and

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[*] This discovery has been also erroneously assigned to Rutherford, to whom Robison, who derides "the trifling or vague writings of a Nollet, a Ferguson or a Priestley," (Edinburgh Evangel. Physics,) would fain ascribe also a share in the discovery of oxygen.

[†] Report of the British Association, Append. to Address, p. 63.

common air in which candles have burned, or which has passed through the fire. Air which has passed through a charcoal fire contains a great deal of fixed air which is generated from the charcoal; but it consists *principally of common air which has suffered a change in its nature from the fire.* As I formerly made an experiment on this subject which seems to contain some new circumstances, I will here set it down*."

This important communication Priestley scarcely turned to better account than that which he afterwards received from the same skilful friend, of the composition of water; he quotes it indeed explicitly, but most defectively, in his paper in the Phil. Transactions of 1772. "Mr. Cavendish," he says, "favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 oz. measures, by passing through a red-hot iron tube filled with the dust of charcoal: this diminution he ascribed to such a *destruction* of common air as Dr. Hales imagined to be the consequence of burning: Mr. Cavendish also observed that there had been a generation of fixed air in this process, but that it was absorbed by soap-leys: this experiment I also

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* "I transferred some common air out of one receiver through burning charcoal into a second receiver, by means of a bent pipe, the middle of which was filled with powdered charcoal and heated red-hot, both receivers being inverted into vessels of water, and the second receiver being full of water, so that no air could get into it but what came out of the first receiver and passed through the charcoal. The quantity of air driven out of the first receiver was 180 oz. measures, that driven into the second receiver was 190 oz. measures. In order to see whether any of this was fixed air, some soap-leys were mixed with the water in the basin into which the mouth of this second receiver was immersed: it was thereby reduced to 166 oz.; so that 24 oz. measures were absorbed by the soap-leys, all of which we may conclude to be fixed air produced from the charcoal; therefore 14 oz. of common air were absorbed by the fumes of the burning charcoal, agreeable to what Dr. Hales and others have observed, that all burning bodies absorb air. The 166 oz. of air remaining were passed back again in the same manner as before, through fresh, burning charcoal into the other receiver: it then measured 167 oz. and was reduced by soap-leys to 162 oz.; so that this time, only 5 oz. of fixed air were generated from the charcoal, and only 4 oz. of common air absorbed. The reason of this was that since the air was rendered almost unfit for making bodies burn by passing once through the charcoal, not much charcoal could be consumed by it the second time; for charcoal will not burn without the assistance of fresh air, and consequently not much fixed air could be generated, nor much common air absorbed. The specific gravity of this air was found to differ very little from that of common air, of the two it seemed rather lighter. It extinguished flame, and rendered common air unfit for making bodies burn, in the same manner as fixed air, but in a less degree, as a candle which burnt about 80" in pure common air mixed with $\frac{8}{65}$ of fixed air, burnt about 26" in common air, mixed with the same portion of this burnt air." The gas thus obtained by Cavendish was nitrogen, with perhaps $\frac{1}{18}$ of carbonic oxide.

repeated, with a small variation of circumstances, and with almost the same result." He takes no notice of the distinction established in Cavendish's paper between "fixed air," and "common air in which candles have burnt or which has passed through the fire;" and so entirely does he misunderstand, or disregard, Cavendish's intimation of the relative levity of the latter when purified from fixed air by caustic potash, as to "conclude, after making several trials, that the air in which candles have burned" (without having been subjected to such purification) "*is rather lighter than common air*[*];" whilst with regard to the *lost air*, which the paper communicated to him described as "*absorbed by the fumes of the burning charcoal*," he represents Cavendish as having ascribed that loss to the "*destruction* of common air."

Though Priestley however here proves himself *not* to have been, as you imagine, the discoverer of nitrogen, this indistinct, but fruitful, experimenter gave in the same document three original and pregnant notifications; for he announced in it—1. the effect of vegetables in restoring the respirable quality of the air; 2. the application of the known absorbing power of nitrous gas, as a test of that respirable quality; 3. his observation that candles burn with an enlarged flame in the gas produced by the distillation of nitre[†]. This observation it is from which those who call him the discoverer of oxygen should date the discovery: for he knew as much of the gas from nitre in 1772 as of that from minium in 1774; and it was the application of nitrous gas here stated, which led, in 1780, in the hands of Cavendish, to the first accurate analysis of the atmosphere, and in 1781 to the solution of the great problem—what becomes of the air lost in the combustion of hydrogen gas?

In scientific value doubtless there can be no comparison between the experimental inductions of Cavendish, or La-

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[*] "I could not find any considerable difference in the specific gravity of the air in which candles or brimstone had burnt out. I am satisfied however that it is not heavier than common air, which must have been manifest if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose I concluded that air thus diminished in bulk is rather lighter than common air."—*Phil. Trans.* 1772, p. 164.

[†] "All the kinds of factitious air on which I have yet made the experiment are highly noxious, except that which is extracted from saltpetre or alum; but *in this even a candle burned just as in common air*. In one quantity which I got from saltpetre a candle not only burned, but *the flame was increased*, and something was heard like a hissing, similar to the decrepitation of nitre in an open fire; this experiment was made when the air was fresh made, and while it contained some particles of nitre which it would probably have deposited afterwards."—*Phil. Trans.* 1772, p. 245.

voisier, and the inferences of those earlier philosophers whose speculations we are engaged in investigating: but when we follow Mayow's deductions from the assumption, on probable grounds, that there exists in the atmosphere a gas which in the act of combining with other bodies produces the phænomena of combustion,—when we observe him concluding the identity of his gas with one of the components of *nitre* from the atmospheric production of that salt, and from the sameness of its effect in enabling substances to burn,—when we further observe him determining it to be fixed in *the acid component*[*] of nitre, and supporting this view of the subject by alleging the sameness of the effect of nitric acid and of the burning-glass, in adding to antimony weight and specific medical properties,—when we find him extending these views to other substances, stating with most remarkable accuracy the acidification, in various cases and degrees, of sulphureous and fermenting substances by atmospheric exposure, and hence inferring that this gas is the principle not only of combustion but of acidity[†],—when

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[*] "Jam vero cum pars nitri aërea in spiritu ejus acido existat, non vero in sale fixo, quod reliquam nitri partem constituit, uti supra ostendimus, concludere licet particulas igneo-aëreas nitri, quæ cum parte ejus aërea idem sunt, in *spiritu nitri* reconditas esse."—*De parte aërea igneaque Spiritus Nitri*, p. 18.

[†] After stating (*Ibid.* p. 37) that the acid of oil of vitriol is not due to any acid already existing in sulphur, of which he says there are no signs, he adds—"potius putandum est, particulas ignis nitro-aëreas, in longa illa distillatione vitrioli, cum sulphure metallico Colcotharis congredi et effervescere; unde fit quod particulæ sulphuris istius salinæ, inter particulas igneas mutuo se atterentes interpositæ, contundantur et comminuentur, ita ut eædem tandem exacuantur, et ad fluoris statum perducantur, quæ demum ignis vi in altum delatæ, oleum vitrioli componunt, haud multum secus ac spiritum sulphuris (sulphurous acid gas) per deflagrationem ejus fieri supra ostendimus." "Si vitriolum ad totalem spiritus acidi expulsionem calcinatum, aëri humido aliquamdiu expositum fuerit, idem spiritu acido de novo impprægnabitur. Nempe spiritus nitro-aëreus cum sulphure metallico *Colcotharis* lente congreditur, motuque obscuro cum eodem effervescit; unde fit, quod particulæ salinæ, aut metallicæ sulphuris istius, modo supra dicto, ad fluorem perducantur. Profecto vix concipi queat qua alia ratione spiritus iste vitriolicus in Colcothare produceretur; neque enim idem in Colcothare mox e distillatione extitit; neque putandum est eum *totaliter* ab aëre prosapiam ducere, ut alibi ostensum est." "Vitriola e lapide seu potius glebe salino-sulphurea (vulgo Marchasitam vocant) conficiuntur, e qua igni commissa flores sulphuris vulgaris, copia satis ampla, eliciuntur: postquam autem gleba ea aëri, astrisque pluviis, aliquandiu exposita est, et dein, prout ejus fert natura, sponte sua fermentata est, eadem vitriolo ubertim imprægnabitur: nimirum spiritus nitro-aëreus cum sulphure metallico *marchasitarum* istarum effervescens, partem earum fixiorem in liquorem acidum convertit, qui mox ab ortu suo particulas metallicas lapidis adoritur, evocatque; tandemque cum iisdem in vitriolum coalescit. Quinetiam *Rubigo ferri*, quæ naturalam vitriolicam obtinet particularum nitro-aërearum cum sulphure ferri *metallico* congredientium actione produci videtur." "Ani-

by an induction of the like kind we find him showing it to us as the principle by which metals gain weight from the air, and vegetables germinate and grow, and undergo an obscure fermentation [*æstum obscurum*] in their life and their decay*, —lastly, when we find him ascribing to the same principle the phænomena of respiration, and representing the reduction of this gas from the elastic to the fixed state by its union with the blood, in the lungs and elsewhere, as the cause of its change of colour, its heat and its aptness for stimulating the heart and exciting muscular motion†—in contemplating so

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madvertendum est insuper quod non tantum in rebus solidis, sed etiam in liquoribus, sal acidum, sive *achor*, spiritus nitro-aërei actione producatur." "Præterea nescio an non spiritus acidi e lignis ponderosis distillati, simili ratione per ignis operationem inter distillandum fiant." "Illud etiam obiter annotamus, quod spiritus acidi e saccharo, et melle, distillati, haud multum absimili ratione, per actionem spiritus nitro-aërei ignei, fieri videantur." "Liquorum autem fermentatio in eo consistit, quod particulæ nitro-aëreæ aut *liquori insitæ*, aut *aliunde advenientes*, cum particulis liquoris salino-sulphureis [basic] effervescunt." "Huc etiam spectat, quod vina, aut cerevisia generosiora, radiis solaribus diu exposita, aut in loco calido detenta, processu temporis in acetum commigrant." "Ex iis quæ dicta sunt haud difficile erit intellectu quomodo spiritus acidus nitri in terra generatur." "Et ita demum ostendere conatus sum, quod salia quæcunque acida a particulis salinis, spiritus nitro-aërei ope, ad fluorem sive fusionem evectis producantur." "Quoad differentiam liquorum acidorum, eam a diversitate salium e quibus iidem constituuntur procedere putandum est, uti etiam ex eo, quod salia fixa, nunc magis, nunc vero minus a spiritu nitro-aëreo alterantur, exacuenturque; et tamen inter salia acida quæcunque affinitas magna est et similitudo; inque iis omnibus particulæ nitro-aëreæ igneæque veluti in subjecto idoneo hospitantur." "Particulæ terræ salinæ hoc modo ad fluorem evectæ hospitium idoneum fiunt, in quo particulæ nitro-aëreæ recondantur detineanturque: ab iis autem utrisque strictim unitis spiritum nitri, qualis distillatione elicitur constitutum esse arbitror."

* "In ortu vegetabilium spiritus nitro-aëreus in motu et vigore positus, sulphur in statu fixo existens adoritur, quo tandem ad volatilitatem perducto, spiritus nitro-aëreus in salinis vinculis incarceratus figitur." "Nostra fert opinio etiam fermentationem ad vegetabilium interitum tendentem a particulis nitro-aëreis et salino-sulphureis, se invicem commoventibus, procedere." "Spiritus nitro-aëreus a conjuge sua salina violenter abruptus motu suo impetuoso omnia perturbat, mixtique compagem solvit." "Ea quæ spiritum nitro-aëreum excludunt res a corruptione vindicant." He instances fruits, flesh and butter, as being preserved from putrefaction, and iron from rust by things which exclude this gas, especially inflammable things, such as oil.

† "Quemadmodum particulæ nitro-aëreæ terræ spiracula lente subeuntes, ibidem cum particulis salino-sulphureis, iis vero immaturis, æstu obscuro congrediuntur, a quo vegetabilium vita dependet—ita particulæ eædem nitro-aëreæ magis confertim in cruoris massam pulmonum ministerio introductæ, particulisque ejus salino-sulphureis ad justum vigorem evectis quoad minima admixtæ, fermentationem satis insignem, qualis ad vitam animalem requisita est, efficiunt."—p. 147. He states that the colour of arterial blood has been shown by Lower, to be owing to the admixture of air with it in the lungs, and lays it down, that the heat of the body is due

just and splendid a generalisation, running parallel to the whole range of chemical induction on all those subjects which occupied the succeeding century, it is impossible not to allow that this young man handed down a bright light to all who followed him*, and made more of a few facts, than the greater part of the next generation did of many.

Mayow also examined the two kinds of air which Boyle had obtained by the action of the nitric and vitriolic acids on iron, and observed the permanence of the one gas and the partial condensation of the other. To determine whether they resembled common air in containing any of the nitro-aërial *aura*, he added them to air in which a mouse was confined, and inferred that they do not, from their not prolonging the animal's life. He then examined their relative elasticity, and finding in them the same capacities of compression and expansion as in common air, he decided that there exist various elastic fluids, and held with Newton that these, as well as that *aura* which he deemed pre-eminently elastic, and the residual gas from which it is abstracted by respiration, owe their different degrees of elasticity and permanence to elementary differences in their particles, and in the substances from which they are derived†.

The only philosopher, as far as I am aware, who dissented from these views, was the elder Bernoulli, having detailed his own respecting fixed air‡, "Mayow," he said, "after

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to the combination of these nitro-aërial particles with the blood, and the increased heat in exercise to a greater number being breathed in the same time. In like manner he accounts for febrile heat, for acid in the blood and urine, for the digestion of the food, and for muscular contraction.

* Mayow's work, besides its publication in England, was at least twice reprinted abroad; a detailed account of it was given in the Philosophical Transactions. It was repeatedly quoted by Hales, whose book was in every chemist's hands, and by other authors: it was therefore sufficiently known to have produced a real influence on the minds of men.

† *De Spir. Nit.* cap. 9. p. 163.—"Utrum aër de novo generari possit?"—In his account of Boyle's gases he says—"*Aura* prædicta haud minori vi elastica quam aër vulgaris donatur prout sequenti experimento mihi compertum est." "imili ratione experimentum feci, num aër in quo animal, aut lucerna expirassent, æque ac aër inviolatus, vi elastica pollent; et quidem mihi videtur aër iste haud minus quam aër quivis alius se expandere." "quanquam *aura* ista in qua animal aut lucerna expirarunt vi elastica æque ac aër inviolatus pollet, et tamen eadem particulis nitro-aëreis vitalibusque destituitur." "Hic etiam referre possumus quod in cap. sup. de *aura* hujusmodi aërisque vulgaris differentia annotavimus, et tamen verisimile est *auræ* istiusmodi cum aëre vulgari magnam affinitatem intercedere, vimque elasticam eorum utrorumque a causa haud multum diversa provenire. Etenim cum ferrum e particulis rigidis, item spiritus corrosivi e particulis nitro-aëreis summe elasticis constant, *aura* ex iis utrisque invicem fervescentibus conflata ab aëre vulgari haud multum diversa erit."

‡ "Allata experimenta satis, ni fallor, ostendunt existentiam aëris in cor-

various experiments concluded, that the substance itself of the globule, from which air was produced in them, is changed into an '*aura*,' as water is changed into vapour by heat: but unlike vapour, this *aura* remains *aura*, and as he himself proves by experiment, retains its elastic force. Does there exist then any other body besides air which is fluid and endowed with elastic force? I scarcely believe it. He alleges indeed as a reason for denying to this *aura* the nature of common air, that he has found by experiment that the said *aura* is incapable of supporting life: as if, because it does not support life, therefore it cannot be air! we see our atmospheric air itself in time of pestilence unfitted to support life: has it therefore ceased to be air? it would be absurd to say this. It is not to be denied that in the space E. H. [of a glass tube filled with carbonic gas] other particles besides air find room, separated perhaps by the impetuous motion of the effervescence from the acid liquor, or the solid globule, and carried

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poribus, sed et alterum nobis ostendendum est, nimirum quod aër iste sit aëre naturalis consistentiæ densior. Hoc autem sequenti experimento demonstratur. Sumatur vasculum liquore quodam acido semiplenum, ut A. C. D. B., et tubus aliquis vitreus E. F., altera parte E. clausus, altera vero F. apertus, impleatur eodem liquore; hujus vero orificio F. induatur globulus G., de luto, vel creta, in quibus nempe multæ particulæ alkali insunt, confectus; statimque, indice super orificium F. posito, invertatur tubus; et liquori in vasculo contento immergatur orificium F.; amoto digito, mox observabitur magnam effervescentiam excitari, quæ per aliquot horas durabit, donec omnis aër, intra particulas alkali contentus, solutis vinculis quibus coarctabatur, ad superiora ascenderit, et materia subsederit; tum demum animadvertitur, aërem hunc, postquam despumaverit, in suprema parte depresso liquore, magnum spatium E. H. occupare: quandoquidem autem superficies H. liquoris in tubo altior est superficie liquoris in vasculo, erit aër in spatio E. H. contentus aliquantulum rarior aëre externo; proinde, ut fiat ejusdem consistentiæ, opus est ut aut tubus altius immergatur, aut plus liquoris affundatur, donec superficies interna coincidat cum superficie exteriori; quo facto, erit quidem spatium E. H. priori paulum contractius, et aër in eo contentus naturalis consistentiæ: nihilo minus tamen adhuc majus erit, duplo, triplo, quadruplo, (pro diversitate materiæ terrestris ex qua globulus conficitur, qua scilicet plus vel minus particularum alkali in se continet,) quam quod tota moles globuli G. occupat; quod certum indicium est aërem istum, cum omnis adhuc in globo continebatur, multo densiorem fuisse quam aër externus est: posito enim globulum constare, ex una parte, materiæ terrestris, et ex una parte, pororum, quibus nempe aër condensatus inest,—vel, quod eodem recidit, spatium quod materia terrestris occupat esse æquale spatio quod aër in poris contentus replet,—si nunc spatium E. H. sit duplum spatii globuli totius, sequitur, aërem in globulo contentum quadruplo densiorem esse quam est aër externus, si triplum sextuplo, si quadruplum octuplo, et sic porro in subdupla ratione; si vero ponatur spatium materiæ terrestris non esse æquale spatio pororum, sed in alia ratione majoris vel minoris inæqualitatis, densitates aëris in globulo æque facile ad calculum revocari possunt."—Bernoulli, *Dissertatio de Effervescentia et Fermentatione*, No. 1. p. 20. 1690.

up with the air. Nor can we wonder that such an air, filled with miasmata, if breathed by animals, cannot keep them alive, especially when it is obvious that the spirit of nitre, and the globule of iron, used by the distinguished author, abound in many impure and poisonous particles, which if introduced into the system in breathing, may well corrupt the mass of the blood and induce death. If instead of the spirit of nitre he had chanced to use another acid liquor of a more *benign* quality, for instance the spirit of vitriol, and instead of a globule of iron, had taken one of an earthy kind, as in my experiment, the animal doubtless would not have perished, or at least would have lived longer. So that we may collect from this, not that the air, *as air*, destroyed the animal, but only incidentally, so far as it abounds with particles of a different kind and unfit for the support of life. But that we may make certain of one fact—namely that the substance of the globule itself is not changed by the fermentation into air, but that air really pre-existed in the globule, and was therefore *not* generated anew, the following experiment may be tried. Let the weight of an earthy globule, well-dried, be taken with perfect accuracy before the effervescence: then after the effervescence, when all the particles of the globule subside to the bottom, let the whole mass of the globule, which now lies dispersed, be carefully re-collected from the liquor; and let it be well-dried as before: lastly, let the weight also of the dried material be accurately ascertained by the help of the balance: this done, we shall find that the substance of the globule has lost nothing of its weight, or at least scarce a hundredth part, which perhaps exhaled with the air during the effervescence. But according to Mayow, it ought to have lost by far the greatest part of its weight; since it follows from his hypothesis, that the whole body of air occupying the upper part of the tube was taken from the substance of the globule; and so its weight should have been notably diminished, which nevertheless is contrary to experiment."

In this criticism Bernoulli overlooked the chief fact on which the theory of Mayow rested—the constant diminution of the volume of common air, when breathed or burnt. And his attempt at an experimental refutation of it may serve to convince you of the danger which the greatest men may incur when they venture on deciding chemical questions without a knowledge of chemistry. To give the utmost credit to the alleged result of his experiment we must presume the "acid liquor" employed in it to have been *oil of vitriol*: but any boy in a chemist's laboratory could have told him that the vitriolated lime which he collected at the end of the experiment was a different substance from the chalk with which he began it, and that the consequence therefore which he drew from the weight remaining the same was a fallacy. The compounds of sulphur, I perceive, are a stumbling-block even to you*: to Bernoulli's reputation as an experimental philosopher they are more fatal than he deemed them to animal life; for the wholesomeness of the air from so "*benign*" an acid as oil of vitriol, and "a globule of the earthy kind, as in his own experiment," was an assumption which the first trial would have discovered to be false. But it is more surprising that the computations, congenial to his own studies, which he proceeded to make, of the amount of condensation of the air, in the pores of the chalk, should not have apprised him that the globule on which he experimented *must have lost weight*, when so great a volume of condensed air was disengaged from it.

At a later period the younger Bernoulli paid so much respect to his father's opinion as to speak of the multiplicity of airs as a doubtful question. "The question," he says, "has long been agitated, whether the factitious elastic *aura* brought out of bodies, is ordinary air, or not; which question I shall not decide. If however the air of gunpowder be taken to be 1000 times denser than natural air, and 10,000 more elastic, then it follows from what precedes, that air compressed by an infinite force cannot be condensed more than 1331 times, and according to the same rule the elasticity of an air four times more dense than the natural air would be to the elasticity of natural air as $4+\frac{1}{4}$ to 1. But whether the experiments instituted by others, which make the ratio of these elasticities as 4 to 1, were conducted with sufficient accuracy, and whether the heat of the air, whilst under pressure, remained the same, I know not. It is probable however that the same *aura* which lies latent in the pores of the gunpowder is the cause of the elasticity of elastic bodies, and contractile *villous* materials; for when bodies are reduced by any force to an unnatural form, the elastic *aura* abounding in the little vacuities is compressed, and in giving the form of greatest space to those vacuities brings the body back to its original shape and extent."

In the English school, however, of pneumatic chemistry, and in the chief successor to the views and experiments of Boyle and Hook, of Mayow and Newton, there was no hesitation on this point. You have quoted the opinion of Hales, as representing, *instar omnium*, the general notion among experimental philosophers before the time of Black, that air was a single and simple element; and your inaccuracy on this point is not to be wondered at; because Hales's opinion has

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* Note to the Lives of Cavendish, Watt and Black, vol. ii. p. 511.

been over and over again mis-stated, even by eminent chemical writers. Those however, who are better occupied in making scientific discoveries than in reviewing them, may be excused, if they appear to be often less exactly acquainted with the opinions of others than with their own, so far at least as we can fully acquit them of desiring to exalt their own views, or the views of a particular æra, or a favourite author, by underrating all that has gone before.

The mistake in this case has certainly in great measure arisen from the circumstance, that the inquiries of Hales were directed more to the generic and physical properties of gases, than to their specific and chemical distinctions. He calls "airs generated in effervescences"—"true permanent air"; he has been supposed to mean that they are true *atmospheric* air; his real meaning was—that they are true elastic fluids, and, with the same permanence of constitution, possess the *same elastic force* as common air. This important fact had been before announced by Mayow, but was first ascertained with precision by Hales. "That I might," he says, "with the greater degree of certainty be assured of the degrees of compressibility of these different airs, I divided the capacities of two equal tubes into quarters of cubic inches, by pouring severally those quantities of water into the tubes, and then cutting notches with a file on the sides of the tubes at the several surfaces of the water; by which means I could see, by the ascent of the compressed water in the tubes, that both the factitious and common air were exactly alike compressible in all degrees of compressure, from the beginning till they were loaded with a weight equal to that of three atmospheres, which was the furthest I durst venture for fear of bursting the glass*." Having made this contribution to our knowledge of the physical properties of the gases, and established that at common pressures and temperatures "with equal weights they are compressed exactly in the same proportion with common air," he went on to examine whether there exists any difference of specific gravity between the air and them; but contenting himself with the single experiment to which I have already referred, where no difference could be detected†, he left to Cavendish the grand discovery of the distinctions of density in elastic fluids; and it may possibly increase your respect for that discovery to remark that his false conclusion led him into much error in computing the weight of aërial substance fixed in various bodies from the volume which they yielded, on the supposition that the density of all airs is the same.

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* Stat. Essays, Append. p. 314.

† Analysis of the Air, Exp. 77.

Hales however rendered essential service to what may be more strictly called the *chemical* philosophy of aërial fluids. I have before noticed that we owe to him the discovery of a fact in gaseous chemistry, the consequence of which it is impossible to overrate—the condensation of atmospheric air by nitrous gas, in such a manner that *the two gases were observed by him to occupy the same space.* He first also determined with *numerical* exactness, and by very ingenious methods, the volume of air absorbed in a variety of chemical processes, and stated in the clearest terms the chemical nature of that *absorption*,—a statement adopted, as I have shown, by Cavendish, and strangely misconstrued by Priestley. "They were changed," he says, "from a repelling elastic to a fixed state by the strong attraction of other particles which I call *absorbing*." He taught the chemists of the succeeding generation how to procure almost all the gases which formed the subjects of their investigation; and he taught them also the more important lesson of conducting those investigations by *measure* and *weight*. Some of his experiments led directly to the most important conclusions at which they arrived. It was not for nothing that he observed that the "Sal Tartar" (very highly calcined) with which he essayed to purify the air for respiration had "absorbed one-third of the fuliginous vapours which arose from the burning candle[*]," or that he recorded experiments on phosphorus, in which "2 grains, fired in a large receiver, flamed and filled the retort with white fumes, expanded into a space equal to 60 inches, and absorbed 28 cubic inches of air;" and "when 3 grains were weighed soon after it was burnt, it had lost half a grain of its weight[†]."

It is true that he made no advance towards analysing the air: and further, he argued, and argued justly, that "the sudden and fatal effect of noxious vapours, which has hitherto been supposed to be *wholly* owing to the loss and waste of the *vivifying spirit of air*, may not unreasonably be *also* attributed" to other causes, which he enumerates. "If," he says, "the continuance of the burning of a candle be *wholly* owing to *the vivifying spirit*, then supposing, in the case of a receiver capacious enough for a candle to burn a minute in it, that half the vivifying spirit be drawn out with half the air in 10 seconds of time, the candle should not go out at the end of those 10 seconds, but burn 10 seconds more; which it does not, therefore the burning of the candle is not *wholly* owing to *the vivifying spirit*, but to certain degrees of the air's elasticity,"—a principle which he goes on to illustrate by the "common

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[*] Analysis of the Air, edit. 1727, p. 272.

[†] Ibid. p. 169.

observation, that in very cold frosty weather fires burn most briskly*."

But we are by no means to conclude from hence that Hales had any doubt of the plurality of elastic fluids; on that point he quotes, as at once the foundation and the result of all his inquiries, the opinions expressed by Newton in the Optics:—"The illustrious Sir I. Newton," he begins, "observes, that true permanent air arises by fermentation, or heat, from those bodies which chemists call fixed, whose particles adhere by a strong attraction, and are therefore not separated or rarefied without fermentation, those particles receding from one another with the greatest repulsive force, and being most difficultly brought together which upon contact are most strongly united." "Dense bodies by fermentation rarefy into *several sorts of air*, and this air by fermentation, and sometimes without it, returns into dense bodies†," "of the truth of which," Hales adds, "we have proof from many of the following experiments." And as he begins, so he ends: for having again repeated the same quotation from Newton, he closes his "*analysis of air*," by drawing this general inference from all his researches—"Since we find in fact from these experiments that air arises from a great variety of dense bodies both by fire and fermentation, it is probable they may have very different degrees of elasticity in proportion to the different size and density of their particles, and the different forces with which they were thrown off into an elastic state."

What now, give me leave to ask, becomes of your statement, that "when D'Alembert wrote the article '*Air*' in the Encyclopédie in 1751, he gave the doctrine then universally received, that all the other kinds of air were only impure atmospheric air, and that this fluid alone was permanently elastic?" You tell us elsewhere that D'Alembert disregarded inductive philosophy, and professed himself ignorant of chemistry: and thus I should have accounted for his ignorance on this point, if I had not found on consulting the volume which you quote, that he really expressed *no such opinion* respecting air, and moreover has stated fully the views entertained of it by those, who in his own words, "supposent qu'il peut être produit et engendré, et que ce n'est autre chose que la matière des autres corps, devenue par les changemens qui s'y sont faits, susceptibles d'une élasticité permanente." D'Alembert says indeed, that *some of the ancients* considered the air *as a simple element*, but remarks with truth, that they did not attach the same sense to that term as ourselves.

I have now completed the sketch which I promised, of the

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* Analysis of the Air, edit. 1727, p. 247.

† Ibid. p. 312.

gradual advance of this branch of science, in the æra beginning with Boyle and ending with Hales, during which the prevalent theory *multiplied the number of gases beyond the truth*, by supposing them as numerous as the substances from which they were obtained. This may be regarded as the æra of the first regular school of inductive science (if we except the less perfectly methodised school of Galileo), instituted by the original members of the Royal Society, for the professed purpose of executing the grand design of Bacon.

We have been lately told by a very able and lively writer, that the sole use and effect of the *Novum Organum* of the great founder of this school, was to bring down philosophers from high but barren aims to the level of common utility,—that "the inductive method has been practised ever since the beginning of the world,"—that "it is not only not true that Bacon invented it, but that it is not true that he was the first who correctly analysed that method and explained its uses,"—and that "he greatly overrated its utility;" we have been further told that the difference between the "*instances*" which make an absurd induction, and those which constitute a sound one, "is not in the *kind* of instances, but in the *number* of instances; that is to say, the difference is not in that part of the process for which Bacon has given precise rules, but in a circumstance for which no precise rule can be given:" and this notion of philosophical induction is illustrated by asking, "Will ten instances do, or fifty, or a hundred? In how many months would the first human beings who settled on the shores of the ocean have been justified in believing that the moon had an influence on the tides? After how many experiments would Jenner have been justified in believing that he had discovered a safeguard against the small-pox? These are questions," it is added, "to which it would be most desirable to have a precise answer; but unhappily they are questions to which no precise answer can be returned*." Certainly, if the force of induction, and the inquisition and demonstration of truth, *does* depend on the "*number* of instances," and not on "the *kind*," Bacon has written in vain; but *you* know, my Lord, how it was, that in the hands of one who had better studied "the inductive method," a vague and local idea, darkened with errors that destroyed its credibility and use, passed through the *mint of a very few decisive experiments* into the *treasury* of accepted truths: that which this author esteems the inductive process had been repeated thousands of times without fruit; but when Jenner, after vaccinating a child, inoculated it, and found that it resisted the virus of

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* Life of Lord Bacon, p. 411.

inoculation, the probability that "he had discovered a safeguard" rose at once, by the force even of a single experiment, to an amount which medical experience could "precisely" assign.

Mr. Macauley considers the credulity of those whom he calls "the dupes of Mesmerism," as due, not so much to neglect of these laws of evidence, as to want of natural sagacity; but the history of science by no means justifies this view of unfounded opinions: the truth is, that all sciences, except the mathematical, had stood for centuries in the same position in which such studies as go by the names of Animal Magnetism and Craniology, appear to stand now,—the position, that is, of collections of alleged facts unscrutinized and unsifted,—of generalisations precariously deduced, and truths, where they contained any, mystified and confused.

This was the state of science when Bacon appeared. The master science of *evidence*, like every other science, requires for its perfection both *rules* and *examples*. Bacon gave the *rules*. It has been observed by one well-qualified to offer an opinion, that "he traced not merely the *outline* but the *ramifications* of science that did not yet exist*." But the chief, the all-pervading, ever-during benefit,—the force of direction, which he gave to the progress of knowledge, consisted in this —that he *did* "first analyse the inductive method correctly, that he first taught the specific value of every part of its evidence, and *that* with such precision," and such impressiveness, that a great school was founded upon his writings, who have handed down from him the torch of science, and have proceeded during the last two hundred years to practise, and mature, his rules.

Yet we shall do no more than describe a real change in the history of inductive science, if we shall proceed to speak of the experimental school of the æra which commences with Black and Cavendish, as the school of Newton: for the severe reason of the mathematician, grafted on that inductive principle of simplifying, and hedging in, ideas more complex than space and number, till they are divided and narrowed to the point of demonstration, shone forth in Newton's immortal works, and especially in his Optics, with a light as much more powerful than even the luminous lessons of Bacon, as example is more powerful than precept.

In this I believe you will agree with me, that if in our seats of learning the attentive study of *such examples of reasoning* had been made one of the essential requisites of an accomplished and sound education, we should not have seen so

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* Playfair's Dissertation, Encycl. Brit., p. 55.

many educated persons, ignorant of the laws of evidence and unconscious of their own deficiency, become as easy victims as the most ignorant, to wild paradox and blind credulity.

What the Optics were for experimental philosophy in general, that little unpretending *duodecimo* volume, of scarce a hundred pages, which Black published in 1755, on the properties of Magnesia, was to chemistry. It was, as you say, the second instance of a most beautiful example of inductive research; and the method of reasoning pursued in it deserves to be more particularly described, as constituting indeed the highest of all its merits. Not one word is there here of the *sulphureous* principle of the old chemists, or the corresponding *phlogistic* of the new: but there is, observe, *one general established principle*, reigning in the experimenter's thoughts, governing his hand, interpreting every phænomenon as it presents itself, dictating every successive experiment, and bringing forth each consequent discovery in that brief and transparent investigation.

The principle by which it was thus illuminated, was *the principle of elective affinities*,—a principle, first stated as we have seen, and generalised by Newton, experimentally noticed by Mayow, with others of the early chemists, and then recently systematised and *tabulated* by the French chemist Geoffroi. And here if we adopt such expressions as yours, in calling this "an example of *strict inductive investigation*," let us understand clearly what we mean; let us not forget that the process of what is called the *inductive* method, in its most usual applications to such a science as chemistry, does not differ from that which is called *deductive* in *mechanics*, otherwise than in the degree of our reliance on the generality of the laws to which it is applied: in mechanics we now assume the laws which we have observed, to be applicable to *all matter* whatever; in chemistry, when the *nature of the subject is widely different* from those on which we have experimented, we dare not trust the *certainty* of our generalisations: the firmest believer in ætherial matter would hesitate to presume on Newton's hypothesis of its possessing chemical affinities, as a certain truth; and *gas* was to Black what *æther* is to us. His reasonings respecting fixed air were in fact all *deductions* from the presumed principle of elective attractions; but as far as regards the chief point of his discovery—the silent transference which he remarked of the gaseous substance that, as Hales had taught him, was fixed in salt of tartar, to calcined magnesia, and again from magnesia alba to caustic lime,—*the principle* which suggested the remark and the experiments, was itself *confirmed and established*, in its extension to gaseous substances, by *the result of those experiments.* In every such course of research, whatever offers itself *fortuitously* is observed by an eye which is on the watch for the appearance of the laws, known or assumed, that fill its meditations; and the whole *design* with which each experiment is instituted, is to test the applicability of those laws, and to try the validity, or the accuracy, of principles which have more or less the character of *foregone conclusions.*

This is experimental philosophy: this is the science of observing, interrogating, and interpreting, nature—apart from that faculty of catching *far analogies* on the wings of a lively and just imagination, which constitutes perhaps the highest part of the *genius* of a philosopher, though we should be much in error, if we regarded even this high gift of Heaven as incapable of being improved by rule, example, and use.

Thus it was that Black, under the guidance of the light which a clear conception of the laws of affinity shed over his mind, proved by a short series of experiments so devised as to eliminate, one by one, *all alternative suppositions*, the following points:—1. That magnesia is a distinct substance, having its own laws of combination to distinguish it from other earths —2. that that substance, which is sometimes found in air, and sometimes fixed both in this and other absorbent earths and alkalies, is subject to the laws of chemical composition, decomposition, and transfer—3. that common air does not enter into the same combinations as fixed air;—and lastly, he inferred from the general analogy of the effects of chemical attraction, that unsaturated affinity is the *form*, as Bacon would have termed it, of *causticity*. This brief, simple, and choice specimen of synthetico-analytical research, to that time unexampled in chemistry, he completed and crowned, by denoting the law of double decomposition as dependent on "*the sum of the forces*," and fixing the place, not of magnesia only, which was as much as he at first contemplated, but of fixed air, side by side with the acids, in its own place in the *table of relative affinities**.

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* Essays and Obs. Phys. and Lit., vol. ii. pp. 221–24. The following description, by the French chemist De Lasône, in 1753, of the manner in which an aërial spirit is combined with lime and iron in the waters of Vichy, is worthy of notice, as a curious anticipation of truth since more exactly developed:—

"Toutes ces expériences prouvent évidemment que ces eaux sont alkalines, par un principe salin et par une terre absorbante; qu'elles contiennent une matière ferrugineuse; qu'elles contiennent *un principe spiritueux*, composé non seulement *d'un air sur-abondant*, comme il s'en trouve dans quelques eaux, mais encore d'une portion de cette terre subtile dont nous venons de parler, jointe au principe huileux du bitume, et *volatilisée par cet air, qui vrai-semblablement est le principal agent qui tient cette terre sus-*

Black had certainly very little ambition, and apparently little of the activity of an ardent curiosity: for here he rested, after drawing from the facts before him some pregnant inferences, as to the production, for instance, of this fixed air from charcoal, and its diffusion through the atmosphere*. He did not even measure, or collect, the air extricated in his experiments, still less did he try its density; he did not extend his inquiries at all into its elastic condition: and the consequence was that on *that point* which you take for the *stress* of his discovery, he rather retrograded from the inferences of his predecessors than advanced beyond them: for he went no further in his conclusions than this—"Quick-lime therefore does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, which is dispersed through the atmosphere, *either in the shape of an exceedingly subtile powder*, or more probably in that of an elastic fluid†." He did more, it is true, than discover the chemical affinity of *one* substance only which floats in the air, or is fixed in many earths and alkalies; for that discovery, as it limited the *number* of such substances, so it extended to the rest the probability *of a like chemical constitution*: but whether these substances are or are not *elastic*, Black, like Daniel Bernoulli, declined to decide. The demonstration of this fact—that there exists *more than one species of elastic fluid* permanent at a common temperature and pressure when not acted upon by a condensing attraction—*was reserved for Cavendish*; being the consequence of that determination of its specific gravity of which you speak so slightingly.—And here again, you see that in your haste you have denied this great philosopher his due.

And now that we have not only walked together over a part

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pendue, puisque lorsqu'on l'en chasse brusquement en secouant l'eau minérale, la terre se dépose aussi promptement, et qu'au contraire elle ne se dépose que très-lentement lorsque l'eau est bouché et que l'air ne s'évapore que lentement; que *ce même principe contient aussi une portion de la terre ferrugineuse qui existe dans ces eaux*, puisque lorsqu'elles sont dépouillées de leur air et qu'elles ont formé leur dépôt, on ne remarque plus aucun indice de matière ferrugineuse; qu'on doit encore à ce même air mêlé avec la terre et le bitume, et qu'on peut en cet état regarder, suivant la pensée de Lister, comme une espèce d'esprit, la saveur acidule qu'ont ces eaux à leur source et qu'elles perdent avec leur air sur-abondant; enfin que ce même principe aërien est la cause d'une partie de l'effervescence qui ces eaux font avec tous les acides."—*Hist. de l'Académie*, 1753, p. 174.

* Black also ascertained that the peculiar matter of fixed air combines with other bodies in more proportions than one; and Cavendish, subsequently, that *that gas* combines in proportions of which one was about *double* the other,—a fact which proved of great importance to chemical theory.

† Essays, Phys. and Lit., vol. ii. p. 198, 1765; Experiments on Magnesia, &c., 1777.

of the demesne of experimental philosophy with more deliberation than your leisure seems usually to allow you, but even ventured on searching some of the inner chambers of the art of experiment, I must appeal to you, not in the style of arch solemnity with which your "illustrious colleague" addressed you in the chamber of the Institute, as having weighed the evidence in the case of Watt *versus* Cavendish—"Avec le scrupule en quelque sorte judiciaire qu'on pouvoit attendre de l'ancien *Lord Chancellor* de la Grande Bretagne*,"—but I appeal to you, as ever you have learnt the laws of evidence from the only Chancellor of England who is of authority in philosophical questions, as ever you have listened to, and comprehended, that pupil of Bacon and Newton, the beauty of whose lectures you have so vividly described,—to take some shame to yourself, for having perused, by your own confession, the notes of Cavendish, without perceiving that all which I have said of the experiments of Black, as being so connected as clearly to manifest the whole train of the experimenter's thoughts, is still more clearly true of *these*.

You know what the problem was, on the investigation of which Cavendish was intent when he made the discovery in question. You know his aim to have been to find out what was become of "*the air lost*" in the combustion of hydrogen with common air. And what were the preliminary trials by which he searched for the lost gases? He tried—1. whether they were "*changed*" into carbonic acid;—2. whether they were "*changed*" into nitric acid;—3. whether they were *changed*

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* *Annuaire*, 1839, Note, p. 361. Lord Brougham, out of *court*, deals I fear as hastily with literature as with science; and *there* also sometimes *makes* the facts on which he reasons. Thus he criticises as "*unintelligible*" the condensed sense of that well-known line, in which Johnson, in his imitation of the Tenth Satire of Juvenal, speaks of "*patience*" as "sovereign o'er transmuted ill:" but he first *makes* it unintelligible, by substituting from his own poetical mint—"*nature*," where Johnson had written "*patience*." (Life of Johnson, p. 76.) Again, he animadverts severely on Johnson for "roaring out, 'No, Sir!' in the presence of Hume, on being asked by a common friend to let him present the Historian to the Moralist" (Life of Hume, p. 223); and he adds, "above all we have a right to complain that the associate of Savage, the companion of his debauches, should have presumed to insult men of such *pure minds* as David Hume and Adam Smith, rudely *refusing to bear them company, but for an instant*." (Life of Johnson, p. 22.) It is curious to compare this with Johnson's own account: "I was but once in Hume's company; and then his only attempt at merriment consisted in his display of a drawing too indecently gross to have delighted even in a brothel." (Hawkins.) The *real man* from whom Johnson turned on his heel, was one who added to the moral *purity* of the school of Voltaire the garb of an *ecclesiastic*,—a circumstance which perhaps may abate something of Lord Brougham's indignation at the ill-manners of Johnson.

into sulphuric acid: he *negatived* by conclusive experiments these three suppositions of *condensation*: at this crisis of his inquiry Warltire burnt inflammable gas and common air in a vessel which he imagined to be close, and finding a very sensible loss of weight, concluded, with Scheele, that ponderable matter had passed through the vessel's pores in the form of heat: at the same time he repeated an observation, made also by others, that in the combustion *water* was deposited from the air, in which he supposed it *to have been contained*: to the mind of Cavendish, deeply meditating what might be the form of matter into which the *lost airs* could have been *condensed*, this inaccurate experiment immediately suggested the light for which he was watching. Such is his own description of the manner of the discovery; and the course of experiments recorded in his note-book leave no shadow of a doubt that he has described it with truth.

In the progress of these experiments, that is to say, in his *fourth* experiment of exploding the gases, in an apparatus like Warltire's but *really close*, on the 5th day of July 1781, Cavendish arrived at the *exact volume of hydrogen* which destroyed in combustion the whole of *its own elasticity*, with the whole elasticity of the *exact volume of oxygen found* to exist in the common air with which he exploded it: in the same experiment he ascertained that the vessel, which was of such capacity as to hold 24,000 grains of water, had lost scarcely one-fifth of a grain in weight: he then varied his apparatus in such a manner as to collect a sufficient quantity of the liquid formed in these experiments, for chemical examination, and before the end of the month demonstrated it to be *pure water*.

And now point out to me, if you can, in the whole range of experimental science *three facts* the ascertainment of which was more *obviously and indubitably conclusive* of the point in question? Is there any alternative left for scepticism? The total weight undiminished—three volumes of elastic matter gone—in its place pure water—could *any man* draw *any conclusion* from such experiments but *one*? could anything but *one foregone conclusion* have led a man to institute such a course of experiment? Does *the man* who *has* instituted, and made, such experiments, want any one to come to him two years afterwards with an idle *doctrine*, or *hypothesis*, as you call it, about the connection of *water* with some *undefined kind of phlogiston*, by way of explaining to him his own investigation? What is the use of a doctrine, or a hypothesis *after an inductive demonstration*, even if the hypothesis had had any real substance or distinctness of meaning? Are we to deny the author of the demonstration the credit of understanding it, for no better reason than that in the private notes of his chain of proofs we find no shout of *εὕρηκα*?

I omit here all the multiplied precautions to ensure the most perfect accuracy in regard to every elementary material of these experiments—I omit the singular caution and sagacity which, on the unexpected intrusion of a minute quantity of nitric acid in one of his varied trials, induced Cavendish to wait till he had obtained evidence that *this* was the product of the *other ingredient* in atmospheric air, before he would publish his experiments: I put the question in a shape so simple that a child may understand it; and I ask you once more,—ought you not, with all this, clearly stated, before you, to feel some compunction for having admitted a suspicion of the good faith of Cavendish, or made a question of his having been the sole discoverer?

Again,— I have shown you, that though these experiments were communicated to Priestley as soon as they were made, and by Priestley mentioned to the public in express terms as —"*Mr. Cavendish's experiment on the re-conversion of air into water*," Priestley understood them no better than the communication which I have before mentioned of the discovery of nitrogen, and subsequently, with the aid of Watt's opinion, concluded that "water by exposing it to heat in porous earthen vessels is capable of being converted into *respirable air* by the influence of heat:" I have shown you out of that very letter of Watt, communicated to the Royal Society, on which the only real question rests—whether he understood the consequences of Cavendish's experiments nearly two years after they were finished—that Watt's *doctrine* about water and *phlogiston* was built on *this false supposition*, and that he adhered to it after Priestley had communicated to him *that* experiment which was designed to be a repetition of Cavendish's*: I have shown you that in Priestley's repetition the inflammable gas which he used cannot have contained more than one-fifteenth of its weight of hydrogen, and if it had proved anything, would have proved that water consists chiefly of *carbon*†: lastly, I have shown you that both Priestley and Watt were entirely ignorant of the distinction between *hydrogen* and the *inflammable gases on which they experimented and reasoned*; and until at a later time they were taught that distinction by Cavendish, and thus learnt what the *real basis* of water is—were obviously as incompetent to *understand*, as to *discover* its composition‡.

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* Report of the British Association, Postscript to Address, p. 24.

† Ibid. p. 27.

‡ Ibid. p. 25.

With these things before the world, you even now venture to reiterate, as your final conclusion, this most unjustifiable judgement—"It is undeniable that from less elaborate experiments Mr. Watt had before Cavendish drawn the inference then so startling, that it required all the boldness of the philosophic character to venture upon it,—the inference that water was not a simple element, but a combination of oxygen with *inflammable air thence called hydrogen gas.* That Mr. Watt first generalised the facts so as to arrive at this great truth, I think has been proved as clearly as any position in the history of physical science. It is equally certain from the examination of Mr. Cavendish's papers, and from the publication lately made of his journals, first, that he never so clearly as Mr. Watt drew the inference from his experiments; and secondly, that though those experiments were made before Mr. Watt's inferences, yet Mr. Cavendish's conclusion was not drawn privately even by himself till after Mr. Watt's inference had been made known to many others."— ! ! !

What friend of yours, my dear Lord, but must regret to see a great man trifling with his own reputation by interfering in subjects of which he thus betrays but too superficial a knowledge? I sincerely lament, for my own part, that having once been honoured by your regard, and having always respected your talents, it should have fallen to me to presume in this manner to rectify your misapprehensions. I declined to enter into controversy with *you*, partly for old acquaintance sake, and partly because I thought you on this question less *responsible* than the official writer of the Institute of France. But you *would* do battle with me; and your weapons were none of the fairest: for instead of replying to my arguments, you did me the injustice, without provocation, to compare the abilities of the obscurest lover of science in England with one of the most eminent of its cultivators in France. I know not that I shall even now have convinced you that the meanest of our philosophical chemists, in his own art, and in a just cause, may be more than a match for the most learned judge of *Patents*, or even for the ablest member of the "*Institut*," whose studies have lain in another direction. A judge in a patent cause may see his way well enough, no doubt, through intricate scientific questions, if he is but prudent in his selection of authorities: but I do not perceive that in this case you have abided by any authority better than *your own in* 1803[*]. You are even bold enough, on the strength of such authority, to differ from a deceased Secretary of the "*Institut*" itself, than whom few men were bet-

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[*] "I first stated that opinion in a published form in 1803-04," Edinburgh Review, vol. iii. Life of Lavoisier, p. 253.

ter acquainted with sciences not peculiarly his own: but though the subject is chemistry, though *you* have attended Black's lectures, and though Black's own discoveries are in question, I greatly fear that on almost every point in which you differ from Cuvier you are yourself in the wrong.

Thus, you are certainly wrong in *denying* Cuvier's assertion—that permanently elastic fluids were *measured* by Hales; and you have only to consult the 'Analysis of the Air,' to be convinced of your mistake.

Again, you are wrong in *denying* Cuvier's assertion, that "no one before Cavendish had distinguished fixed air as a separate *aëriform* substance:" and you need only look at Black's treatise to assure yourself that he declined to decide, for want of evidence, whether it was an aëriform substance, or not; and left it among the class of—"bodies of which it is difficult to say, whether they are really *combined* with the aërial particles, or are merely *suspended* in the fluid, in consequence of their being of the *same specific gravity**."

But above all, you are *most wrong* in reprehending the former Secretary of the Institute, for "*making no mention whatever of Watt in connection with the discovery of the composition of water*"—for not confounding, that is, the rights of discovery—for not falsifying the history of chemistry in one of its most material parts—for not representing Watt as the claimant of a merit to which he had not the smallest pretensions—and thus degrading, with intent to exalt, the venerable name of one who has entitled himself to the admiring gratitude of ages, by realising, beyond any other man, the vision which Bacon saw—of experimental *works of fruit*.

You have no sufficient ground, I think, for imputing to—"a person of M. Cuvier's eminent attainments, filling the high office of '*Secrétaire perpétuel*,' and charged with the delicate and important duty of recording the history of science yearly"—that "he has not read Mr. Cavendish's paper†, or Dr.

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* Cavallo on Air, p. 361. 1781. Hawksbee approached the nearest to the discovery of the different density of gases, as early as 1707;—"whether," said he, "the space deserted by the water [after an explosion of gunpowder in a close vessel] is possessed by a body of *the same weight and density*, or is of the same quality, as common air, I dare not determine; *since an experiment I have lately made seems to conclude it otherwise.*" He observed likewise "a loss, or absorption of this air, after it had reached its former temperature;" and suggested that a temporary distension of the springs or constituent parts, of the ambient air, as well as of those contained in the body of the gunpowder, may account for "*this odd phænomenon.*"—*Phil. Trans.* vol. xxv. p. 2409.

† One of Lord Brougham's reasons for thinking that Cuvier had never read Cavendish's paper is, that he says,—"Cavendish unfolded his discoveries in a manner even more striking than the discoveries themselves"—

Black's treatise." And certainly you have no ground to "lament," with respect to him, "that the history of science should be written with such *remarkable carelessness* and such *manifest inattention to the facts*,"—however true it may be, were the censure justly pointed—"that to find mistakes so very gross in the works of ordinary writers might excite little surprise; but when they are embodied in the history of the *National Institute*, and when they come to us under the name, among the very first in all sciences, of Cuvier, we may at once wonder and mourn[*]."

I only trust that the stone which you have so rashly cast at Cuvier will not recoil on any other head. I still trust sincerely that so severe a reproof will not *permanently* rest on the *present* "Perpetual Secretary" of the Institute of France; and that conformably to the known manliness of his character, and clearness of his understanding, M. Arago will yet rectify, as he knows how, the inadvertence into which he has fallen.

It now only remains for me to remark on your last words in reply to one who has supported with far greater ability than myself the same opinions which I have expressed.

I have known you, my dear Lord, more strenuously and skilfully employed than in deciding these questions for chemists; and think I remember it to have been one of the arts of a dexterous *advocate*, with which you were then familiar, to speak somewhat *largely* in an opening speech, of evidence which yet it might not be discreet to bring into court: and so I suppose it is now; for in animadverting upon the ignorance of this enemy in ambush, whom however you seem to suspect of being no ordinary man, I perceive you affirm, that you "have lying before you *fifteen pages* of statements of chemical errors in the thirty-four pages of his paper, and as these corrections are the work of *a most experienced, learned and practical chemist* whom you consulted, you have entire reliance on his report and opinion." It was some disappointment to me, at first, to find that you kept the *fifteen pages* in your pocket; but I remembered, how it happened not unfrequently of old, that in the torrent of that forensic eloquence which

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an assertion which will scarcely be disputed by any competent judge who compares the brief perspicuity of expression, and the select sequence of most exact experiment, which shines in every page of Cavendish, with the rambling, inconsequent manner of thinking and writing, general in his time, and I fear not infrequent in our own. Lord Brougham also accuses Cuvier of stating, that Cavendish established in his paper of 1764 these propositions—"*l'eau* n'est pas un élément; il existe plusieurs sortes d'air essentiellement différentes." But is not '*l'eau*', in this paragraph, merely a misprint for '*l'air*'?

[*] Brougham's Lives, vol. ii. p. 507.

so often dazzled and delighted your hearers, something that should have been kept back would occasionally slip out, of which an astute adversary did not fail to make his advantage. And even so it is still: from the *fifteen* critical pages you have allowed *one* criticism to creep out, as too good to be suppressed. And here it is:—

"I leave him" (the author of this heap of errors) "in the hands of M. Arago, who will observe with some wonder that he has been accused, and judged, and condemned, by a chemist so well-versed in that science, and so reflecting, as to announce the astonishing novelty—that the exhibition of sulphur to sulphuric acid reduces that acid, and restores it to its primitive state of sulphur! The writer had probably read somewhere that sulphuric acid is reduced to sulphurous by the process; for he is assuredly the first that had ever hit upon the acid's reduction by sulphur to 'its primitive state'*."

Now we will at least give credit to the *present* perpetual Secretary of the Institute, to whose scorn you devote the unhappy Reviewer, for having read the papers of Cavendish; and he would no doubt recollect this remarkable passage in the "experiments on factitious airs" (1764) to which the Reviewer should seem to be referring—"Sulphur is allowed by chemists to consist of the plain vitriolic acid united to phlogiston; the volatile sulphureous acid appears to consist of the same acid united to a less proportion of phlogiston than what is required to form sulphur; a circumstance which I think shows the truth of this is, that if oil of vitriol be distilled from sulphur, the liquor which comes over will be the volatile sulphureous acid." M. Arago might perhaps compare these early notions of Cavendish with the Reviewer's account of the phlogistic opinions, not in your interpolated words, but in his own—"It was concluded therefore that it was the *same* phlogiston which was derived from all those substances (charcoal, sugar, metallic bodies, &c.), however different in their nature: a similar succession of phænomena is presented by sulphur: if it be burnt, it forms sulphuric acid; but if the acid thus formed be heated with *phosphorus*, or *charcoal*, or *sugar*, or even *sulphur* itself, it is equally restored to its primitive state†," —and having read this account, supposing M. Arago for a moment to be only as experienced, as learned, and as practical a chemist, as he whom you have consulted out of court, and no more—supposing him, that is, to believe, with your anonymous friend and yourself, that the total reduction of sulphuric acid by sulphur is a laughable absurdity—M. Arago would yet see,

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* Brougham's Lives, vol. ii. p. 511.

† Quarterly Review, Dec. 1845, p. 106.

that there is nothing laughable, or ignorant, in the statement of the Reviewer; though in strictness of language it might have been more correct to say, that—'sulphuric acid heated with *phosphorus*, or *charcoal*, or *sugar*, is reduced to its *primitive* state, and even heated with *sulphur* is reduced to its *previous* state of sulphurous acid': and I think M. Arago might *wonder* a little at finding how much you, and your learned, experienced, and practical friend, have made of a mere *verbal* slip.

But *if*, as I am apt to suspect, the *perpetual Secretary* is a *better* chemist than yourself, or knows better, this time at least, on whose *information* to rely, then he will whisper to you privately, that sulphuric acid *really is reduced*, astonishing novelty as it seems to you, even by *sulphur itself*: and he will doubtless proceed to explain to you how this marvel comes to pass: he will remind you, that the great chemist of our time whose life you have written, when attempting to decompose sulphur, found it so closely united with a very considerable quantity of hydrogen, that he remained for some time in the belief, that he had effected its decomposition; and M. Arago will add, that since *hydrogen* decomposes *sulphurous acid*, it follows, that sulphuric acid cannot but be reduced by sulphur, in all the forms in which sulphur is commonly experimented with, to its *primitive* state, and that the Reviewer therefore is literally right.

And now, retaining the very sincere respect which I have always felt for one who has so laudably devoted the leisure hours of a busy life of public service to the promotion of literature and science, I hope I may have persuaded you, that it is at once more safe, and more just, for those who have not had leisure to pursue chemical studies to their foundation, to leave chemistry and chemists to themselves—at least so far as regards the minutiæ of the science, and arbitrations of the rights of discovery; and I take the license of old acquaintance to advise, that if you *will* venture on such dangerous ground, you should at least learn how to choose your authorities; and when you find even Robison, and Watt, deserting you, and *the perpetual Secretary* so tardy in coming to the rescue, you should not think it enough to reflect that in 1803—1839—1845 and 1846—you yourself stated and re-stated an opinion contrary to the public* voice of the chemists of England.

I have the honour to remain, with undiminished regard,
My dear Lord, your faithful Servant,
W. Vernon Harcourt.

N.B. An oversight with respect to a date in the part of

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* Lord Brougham expresses more surprise than is just, that I take no

this letter previously published (p. 116) requires to be thus corrected. "Priestley addressed this paper to the Royal Society on the 21st of April 1783: and therefore the communication of Cavendish's experiments, acknowledged in it as having suggested his own, must have been prior to the speculations founded thereon which Watt addressed to Priestley on the 26th of the same month, as well as to Lavoisier's experiments which followed in June."

LXXIX. *Observations on* Mr. Strickland's *Article on the Structural Relations of Organized Beings. By* Prof. OWEN, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE author of the interesting paper "On the Structural Relations of Organized Beings," in your last Number, appears,—in recommending the introduction of "the adjective *affine* or *homologous* in place of analogous, when referring to structures which essentially correspond in different organic beings" (p. 358),—not to have been aware that the term 'homologous' had been used in the sense he recommends, by comparative anatomists both in this country and abroad for some years past.

In the article *Marsupialia*, for example, Cyclopædia of Anatomy, part 21, April 1841, p. 283, he will find—"With reference to the interesting question,—What is the *homology* or essential nature of the ossa marsupialia?"—and their homologies discussed. In No. XXII. of the same Cyclopædia, article *Monotremata*, p. 375: "The interposed cartilages, which thus form a third element in the costal arch, repeat a structure common in Crocodiles, and may be regarded as the *homologues* of the costal appendages in the ribs of birds." And

notice of his having *quoted* a private letter to the son of Mr. Watt on the subject of his father's claims. I am aware that Lord Brougham says he has seen such a letter, and says also that the opinion expressed in it respecting Watt's MSS. is different from the opinion attributed to Dr. Henry by me: but I am not aware that Lord Brougham has given *any quotation* from this letter; nor if he had, would any *partial quotation* have satisfied me, that Dr. Henry's opinion was at any time different from that which he expressed to me, when I mentioned to him the sentiments which I had heard fall from M. Arago concerning the MSS. at Aston, and the insincerity of Cavendish. Dr. Henry then said, that he had seen nothing in those MSS. either to justify that impression, or to alter the received opinion respecting the discovery of the composition of water. Who indeed can doubt but that the MSS., had they contained any evidence to support an object which has been so long urged by private solicitation, would have been made public long ago?

again, p. 377:—"These clavicles are the *homologues* of the os furcatorium in the Bird." And in the note, same page:—"For a full and elaborate discussion of the various opinions which have been offered respecting the *homology* or signification of the complicated apparatus of the shoulder," &c. I could easily multiply instances in which the term homology has been applied, both substantively and adjectively, in the sense recommended by Mr. Strickland. I have been in the habit of defining, in my Introductory Lecture, the terms *homology* and *analogy*, as in the Glossary appended to the Lectures on Invertebrata, published in 1843, and of illustrating their meaning in comparative anatomy, by reference to the skeletons of the *Bird* and the *Draco volans*. The fore-limb of the Dragon being composed of essentially the same parts as the wing of the Bird, is *homologous* with it; but the wing or parachute of the Dragon, having a similar relation of function, is *analogous* to the wing of the Bird. But in thus illustrating the term *homology*, I have always felt and stated that I was merely making known the meaning of a term introduced into comparative anatomy long ago, and habitually used in the writings of the philosophical anatomists of Germany and France. Geoffroy St. Hilaire also, in defining the term, acknowledges its source:—"Les organes sont *homologues* comme s'exprimerait la Philosophie Allemande; c'est à dire qu'ils sont analogues dans leur mode de développement," &c.—*Annales des Sciences*, tom. vi. 1825, p. 341.

I have gone perhaps a little further than Oken and Geoffroy in defining the different kinds of 'homology,' which appear to me to be three, viz. 'general,' 'serial,' and 'special.' General homology is the relation in which a part or series of parts stands to the ideal or fundamental type; and thus, when the basilar process of the occipital bone in Anthropotomy is said to be the 'centrum' or 'body of the last cranial vertebra,' its *general* homology is enunciated. When it is said to repeat, in its vertebra or natural segment of the skeleton, the body of the sphenoid bone, the body of the atlas, and the succeeding vertebral bodies or centrums, its *serial* homology is indicated. When the essential correspondence of the basilar process of the occipital bone in Man with the distinct bone called 'basi-occipital' in a crocodile or a fish is shown, its *special* homology is determined.

Vicq d'Azyr began the study of 'serial homologies' in his ingenious memoir on the parallelism of the fore and hind limbs, in the Memoirs of the French Academy, 1788.

'Homologous parts' are, indeed, in one sense, 'analogous parts,' having like relations, as being repetitions of the same parts of the body, to different animals; but I have been in the habit for some years past, of expressing this kind of analogy by the term 'homology;' and I heartily join in the recommendation of your ingenious correspondent, that *all* writers on comparative anatomy and zoology should use the word in that sense, whether it be or be not coupled with the likeness of function performed by such parts; to signify which relation alone, the term 'analogy' should be restricted. As instances of parts both homologous and analogous, may be cited the pectoral limb of the Porpoise and that of the Fish: they are homologous as being constituted of essentially the same or corresponding parts; they are analogous as having the same relation of subserviency to swimming. So likewise the pectoral fin of the flying-fish is analogous to the wing of the bird; but, unlike the wing of the Dragon, it is also homologous with it. Some organs are analogous, but only partially homologous: thus the Monkey's foot is analogous to the Man's hand, as having the functions of the opposable thumb: it is also homologous with it *generally*, as being part of the radiated appendage of a hæmal arch; and *serially* as being the terminal segment of that appendage; but it is not *specially* homologous with it. The thumbless hand of *Ateles* is specially homologous with the perfect hand of Man, but the pollicate foot of *Ateles* is not so. I offer these as examples of the mode in which 'homology*' is illustrated in my own Lectures on Comparative Anatomy, in addition to those cited from my printed works.

I am, Gentlemen,
Your most obedient Servant,
RICHARD OWEN.

College of Surgeons,
May 9, 1846.

LXXX. *Observations on* Messrs. Lyon Playfair *and* Joule's *Memoir on Atomic Volume and Specific Gravity. By* Prof MARIGNAC *of Geneva*†.

THE authors in this paper have determined the density of a large number of bodies, and have arrived, by a comparison of their atomic volumes, at laws which would be rather curious if they could be regarded as proved. Their investigations have been principally directed to the soluble salts, and they have sought to determine not only the atomic volume of

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* In geometry those sides of similar figures which are opposite to equal and corresponding angles are sometimes said to be *homologous*, as being proportional to each other.

† Translated from the *Bibliothéque Universelle*. Feb. 15, 1846. The memoir referred to will be found at p. 453 of the previous volume of this Journal. [ED. *Phil. Mag.*]

these salts in the solid state, but also their volume in the state of solution, that is to say, the increase in volume which the water in which they are dissolved undergoes. Their method consists in introducing a determined weight of salt into water contained in a glass bulb, capable of holding 1000 to 4000 grains of water, surmounted by a graduated stem, in which they measure the increase in volume of the water after the solution of the salt. The same apparatus served to measure the volume of the salts in the solid state, either by employing a saturated solution of the salts or a liquid in which they were insoluble, such as the oil of turpentine.

We shall examine successively the results relative to the volumes occupied by the salts in a state of solution and in a solid state.

In 1840 Dalton observed that sugar and certain hydrated salts, on solution in water, increased its volume by a quantity precisely equal to the volume of water they themselves contained. He generalised this observation, extending it to all the hydrated salts, and he thence concluded that the anhydrous salts did not increase the volume of the water in which they were dissolved.

Messrs. Playfair and Joule confirmed this interesting result for a sufficiently large number of salts, but not for all*. Those which obey this law are in general the salts containing a large proportion of water of crystallization, as for instance—

The sulphates of the magnesian group with .... 5, 6 or 7 equiv. water
The chlorides of calcium, strontium and magnesium with... 6 equiv. water
The alums† with .... 24 equiv. water
The phosphates or arseniates of soda, neutral or basic, with 24 equiv. water
The carbonate, borate, sulphate, pyrophosphate of soda with 10 equiv. water
The sulphate of alumina with...................................... 18 equiv. water

Lastly, cane-sugar, considering as water the 11 equiv. oxygen and hydrogen which it contains.

For all these compounds the increase in volume of the water in which they are dissolved is precisely equal to the volume of water they contain; so that if these salts be used in the anhydrous state, they dissolve in the water without causing any change of volume.

Several other salts, both anhydrous and hydrated, follow different laws. Messrs. Playfair and Joule advance the fol-

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* This had been previously done to a certain extent by Mr. Holker; see his paper in vol. xxvii. p. 207 of this Journal.—Ed. Phil. Mag.

† There is however an exception, for the ammoniacal alums possess a volume equal to 25 equiv. water and not 24 like the potash alums, although they contain like them only 24 equiv. water of crystallization.

lowing law in respect to them:—The volume occupied by an equivalent of any salt whatever in solution in water, is always an exact multiple of the number 9 representing the atomic volume of water.

It is difficult to conceive whence this simple relation between the volume of the salts and the volume of the water arises; nevertheless, if by adopting this hypothesis we were led to represent by similar formulæ the volume of analogous compounds this law would be interesting, but the following examples will suffice to show how many anomalies we meet with. We will here compare the number of volumes of water which some groups of analogous compounds occupy.

Bisulphate of soda	2	Nitrate of copper	2
... of potash	4	... of soda	3
... of ammonia	5	... of potash	4
Bicarbonate of soda	2	... of ammonia	5
... of potash	4	Chloride of potassium	3
... of ammonia	4	Iodide of potassium	5

But let us pass over these objections and see whether really the circumstance indicated as fact is sufficiently established by experiment. The atomic volume of a salt in solution is not a constant magnitude; it varies with the temperature and with the relative proportions of the salt and of the water; we will examine successively these two influences. That of the temperature is very considerable; this is proved by the experiments of Messrs. Playfair and Joule, who measured the volume occupied by a salt at different temperatures. The following are some of their results:—

	Volume of one equivalent of salt in solution.			
Sulphate of magnesia with 7 equiv. water	at 0°	60·88	at 29°	63
... of zinc	0	56·12	32	63
... of iron	0	61	27	63
Anhydrous sulphate of potash ...	2¾	14·4	27	18
Sulphate of potash and copper ...	0	65·2	22	72
... of potash and magnesia ...	0	61·8	27	72

Now at what temperature should these volumes be compared with one another? We are totally unable to answer this question, but certainly nothing authorizes us to choose for each salt a different and arbitrary temperature, as Messrs. Playfair and Joule have done, by taking only the numbers inscribed in the second column, because these alone satisfied the law which they wanted to prove. For other bodies, however, they have admitted experiments made at low temperatures; thus for the alums of iron and chromium the experiments were made at 2¾°, for the sulphate of alumina at 10°.

When it is seen that the atomic volume of sulphate of potash and magnesia may vary more than 10 between 0° and 27°, it will readily be conceived that it will never be a difficult task to make the volume of each compound an exact multiple of 9; for this purpose it is merely requisite to make the experiment at a suitable temperature.

The relative proportions of water and of salt likewise cause the volume occupied by the latter in solution to vary. Messrs. Playfair and Joule are fully aware of this; they made experiments on the very subject, and proved this influence by the following results relative to the volume occupied by an equivalent of sugar in solution in different quantities of water:—

Relative proportions of sugar and water.	Temperature.	Volume of one equivalent of sugar in solution.
1 : 120	15°·5	99
1 : 10	11	105·89
1 : 1	11	107·01
3 : 1	11	108·06

The difference between the first number and the following would have been still greater if the experiments had been made at the same temperature.

What then are the relative proportions of salt and water which should be employed in order that the results may be capable of comparison? We know not, and the authors leave us in total ignorance of the subject; we only find that in their experiments they have not restricted themselves to uniform conditions; thus they employed

	Salt.	Water.	Proportion.
for the sulphate of alumina and potash	59	1000	1 : 17
... ... of iron and ammonia	30·06	1000	1 : 33
... ... of oxide of chrome and potash ...	32	4100	1 : 129
... ... of alumina and ammonia............	20	4100	1 : 205

Evidently, if under such circumstances they have found an agreement between the volumes of these salts, it may be concluded that had they operated under uniform conditions they would not have found the least trace of one. In short, the atomic volume of a salt in solution depends both on the temperature and the relative proportions of water and salt, and these circumstances cause the volume to vary within considerable limits. As long as these influences are neglected, or we do not operate so that they act in all cases in the same manner, the results obtained cannot be compared in any possible way with one another.

Let us now pass to the atomic volume of the salts in the solid state. Messrs. Playfair and Joule have advanced for these the following law:—The atomic volume of any salt whatever (anhydrous or hydrated) is a multiple of 11, or of a number near to 11, or a multiple of 9·8 (the atomic volume of ice); or again, the sum of a multiple of 11 or of 9·8.

This law appears to us to resemble very much the preceding, except that the indecision as to the choice between the multiples of two different numbers renders it still less probable.

We do not in this case meet in the same degree with the objections above set forth for the case of dissolved salts; the temperature cannot cause any great variation in their density, and the experiments were made at temperatures varying too little to have any separate influence,—if it be admitted, which however is far from being proved, that with respect to the solid bodies their densities should be compared at the same temperatures.

We will however make one remark relative to the process by which the densities were determined; it appears to us little suited to give accurate results. It is not stated what was the volume of the liquid to which the salt whose density was to be determined was added; but as it was the same apparatus which had served for the preceding experiments, we may suppose that it contained at least 1000 grains of water. The quantity of salt employed in each experiment was from 40 to 60 grains, and there thence resulted an increase in the volume of the liquid corresponding to about 20 to 40 grains of water; in a great number of cases indeed we find an increase of only 10 to 20 grains, that is to say, of from 1 to 2 per cent. of the total volume. It is evident that by this process it is extremely difficult to avoid serious errors produced by the slightest variations of temperature, which tend to alter the volume of so large a liquid mass, and of errors probably still more important, which might result either from the solution of a portion of the salt in the liquid, if this was not accurately saturated, or from the precipitation of a portion of the salt contained in the liquid, if it were more than saturated. The experiments of Gay-Lussac prove indeed that both these circumstances may readily occur.

These causes of error might perhaps be neglected if the volume of the liquid were very inconsiderable; but when, on the contrary, its proportion is so large relatively to the solid salt, they become too serious for any confidence to be placed on the densities obtained by this process.

We should add, that on reviewing the formulæ which Messrs. Playfair and Joule have established, based on the preceding law, they do not appear to us to indicate any very great probability for this law. Along with certain analogies which do not surprise us, for they result simply from the fact of the equality of the atomic volumes with respect to the isomorphous compounds, we meet with a number of most striking anomalies. For the chlorides of calcium, strontium and magnesium, the atomic volume is equal to 11 multiplied by 6, *i.e.* the number of equivalents of water of crystallization of those salts, but for the alums it is 11 × 25, while there are only 24 equiv. water. For the sulphate and borate of soda with 10 equiv. water the volume $=11 \times 10$, but for the pyrophosphate with 10 equiv. water it is 11×11; and for the carbonate likewise, with 10 equiv. water, it is $9{\cdot}8 \times 10$; for the anhydrous carbonate of soda the factor 11 is taken, and for the hydrated carbonate 9·8; on the contrary, for the anhydrous sulphate of soda the authors prefer 9·8, and for the hydrated sulphate 11. The bromide of potassium $=4 \times 11$, the bromide of sodium $=5 \times 11$, the chloride of potassium $=4 \times 9{\cdot}8$, the chloride of sodium $=3 \times 9{\cdot}8$.

These instances we think will suffice to show that the hypothesis of Messrs. Playfair and Joule is not confirmed by an analogy of formulæ such as ought to be expected, and that the coincidence which does exist between the calculated and the observed densities merely result from the easy way in which the authors select at will the factor 9·8 or the factor 11, or even of combining them for one and the same body, as they have done in a large number of cases.

LXXXI. *Remarks on* Dr. Faraday's *Paper on Ray-vibrations.*

By G. B. Airy, *Esq., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE communication which accompanies this was sketched before my attention was called to Dr. Faraday's leading paper in your Number for the present month. I need not to say that I read that paper with great interest and great pleasure. Yet I will ask your indulgence, and I am sure that I shall receive the forgiveness of Dr. Faraday, while I comment on the principal points of that paper somewhat critically. I am desirous of examining, or of suggesting grounds for examination by others, as to how far the fundamental suppositions of Dr. Faraday are necessarily limited by recognised phænomena, and as to how far the subject is metaphysical or physical.

The paper, as I understand, treats of two subjects:—

1. The possibility of explaining phænomena of radiation,

more especially of light, by supposing that when there is no body obviously occupying the path of the light, &c., the vibrations which are assumed as the foundation of the undulations producing the phænomena are transmitted on the *lines of force* by what (for want of a received term) may be called *lateral shakes.*

2. The possibility of removing the idea of *substance* and substituting for it that of *centres of force.*

I shall treat of these in the order in which I have written them above.

1. With regard to the transmission of light through the planetary spaces.

Dr. Faraday and myself agree in receiving the undulatory theory of light with transversal vibrations, as applicable to those phænomena which present themselves in ordinary optical experiments. Without any wish therefore to dogmatize on this matter, I shall assume the undulatory theory in all the following remarks.

It is admitted that vibrations forming progressive undulations are required for the explanation of certain crystalline and other phænomena. But I must claim somewhat more. Progressive undulations (leaving the nature of their vibrations undetermined) are required to explain the phænomena of *diffraction*; and these progressive undulations must not be of the nature of radial shakes, where each shake derives its virtue or existence from the momentary influence of the distant origin, but they must be true waves, of which the mechanical characteristic is that the motion of a succeeding set of particles is determined by the relative motion of the preceding set of particles; the order of "preceding" and "succeeding" not being confined to a radial line or to any lines whatever, but being such that the motion of particles may be origin of motion to other particles extending round them through a very large solid angle. I defy any one to put together a theory of radial lines subject to lateral shakes which shall explain diffraction; and I say that it will be found absolutely necessary to admit, in the theory explanatory of diffraction, that each disturbance of particles produces a swell (to use language derived from the motion of water), which swell is propagated in all directions through at least a very large solid angle. Now the consequences of this are very important. Diffraction takes place in air; therefore the vibrating medium exists in air, and the undulations are transmitted by *it*, and not by radial shakes. As far as we can perceive air in its utmost degree of tenuity, it produces *refraction*; refraction inexorably requires for its explanation a change of velocity of the undulations, and a power at the same time of changing the direction of progress in a degree exactly corresponding to the change of velocity: these changes are in the simplest and most natural manner possible explained by the theory of true waves in which the swell produced by every particle is propagated in all directions through a very large angle, while (as I apprehend) it will be found somewhat difficult to modify a theory of radial shakes so as to explain them; therefore I conceive it demonstrated that the propagation of true waves takes place through the air to its utmost borders. Beyond the existence of sensible air we can make no experiments; and I am free to concede that if we supposed the air and its accompanying æther [if different] to terminate at a distinct frontier, and if we supposed the transversal shakes to be propagated radially through the planetary spaces to that frontier, and then supposed each shake, as it presented itself, to be the origin of a spreading swell through the æther, the phænomena of light would be explained. But here a remarkable circumstance forces itself on our minds. A moment's consideration will show that at this frontier the course of the light will be subject to refraction, in just the same way as if the incident light had consisted of waves, and following the same law as depending on the velocity of propagation. Now it is abundantly established that at the boundary of our air there is no sensible refraction, that is, that the velocity of the propagation is not sensibly altered. Now is it not a very curious circumstance that there should be a system of radial shakes outside and a system of true waves inside which propagate the undulations with *exactly* the same velocity? Is there any philosopher who would be inclined to receive as true this suggestion of two independent causes of velocity, and this exact adjustment of independent velocities, when the adjustment will *necessarily* exist if the same vibrating medium or æther occupies all space? Not I, certainly. However well-disposed I might be to admit any such *saltus* of nature at the surface of glass or crystal where the phænomena of light are totally changed, I cannot bring myself to believe in it as existing either *through the air* where the change of phænomena is gradual, or *at the limits of the air* where there is no change at all. In a word, I must have the same theory of light for the planetary spaces as for the air in which our experiments on diffraction are made; and that theory must be the theory of true waves.

I do not insist on the novelty of the conception, that lateral influences take place in a travelling succession along a radial line, in a manner different from anything whatever that we know. I am perfectly aware that the theory is merely sketched by Dr. Faraday as the result of hasty thought, and that it might be in some measure modified in its details on further consideration by its author. But while the distinctive features of the theory are retained, it will be, for the reasons which I have given, inadmissible to me. The theory is however, in my opinion, a fair subject for the consideration of the natural philosopher.

2. With regard to the substitution of centres of force for matter.

This speculation, in its general character, differs little from the celebrated inquiry regarding Substance and Accidents. In the latter the question is, whether, when we have found a lump of matter to possess certain form, colour, weight, and other properties, we can satisfy ourselves by saying that this lump of matter is a combination of such a form, such a colour, such a weight, &c.? And the answer has usually been that the mind is not satisfied unless we describe the lump of matter as *something* possessing the properties of such a form, such a colour, such a weight, &c. In the speculation before us, the question is, whether instead of matter which exerts certain actions upon other matter, we may assume that there is nothing but a number of centres of force producing these actions? I think that most persons would say that the mind is not satisfied with this assumption, and that it requires the idea of a *something* as foundation for these centres of force. But this question, in my opinion, is purely a metaphysical question, entirely removed from the province of the natural philosopher.

To a great extent I am willing to admit that the supposition of centres of force is satisfactory. Mechanical attraction or repulsion (including weight under the former term), colour, radiation of every kind where the existence of something intermediate between the radiating body and the body receiving the radiation is not apparently demonstrated; all these may, I think, be received without scruple as the results of mere centres of force. But there is one property, to which by chance Dr. Faraday has not alluded in his paper, that appears to me irreconcileable with the notion of centres of force; I mean the property of *inertia.* And I believe that the general notion of *substance* is really founded upon the perception of *inertia.* Construct for any one a mass of matter possessing invariable form, colour, and other attributes, even attraction; if he finds that this mass yields to muscular or other force without perceptible resistance (it matters not whether it continually retain the same velocity or not), he will scarcely scruple to admit that there is no *substance*. While the resistance to force remains, it seems scarcely possible to get rid of the idea of *substance*.

Perhaps it may be said that even inertia may be represented by centres of force, only supposing the development of the force to be dependent in some way upon time. Such, however, is not the character of forces that we know best; and the introduction of this idea appears to give greater complexity to the force-centre-theory than is given by the idea of substance in the material theory.

Now I say that, in the wave-theory of light, and in all theories of waves where the amplitude of the vibrations does not diminish transcendentally with relation to the distance passed over by the wave, the supposition of inertia (or something equivalent) is absolutely necessary. This will be evident to any mathematician who compares the results obtained from the different suppositions of inertia or no inertia. For instance; in the theory of the transmission of heat by conduction, no inertia is supposed; the equation then has the form $\frac{dh}{dt} = \mathrm{A} . \frac{d^2 h}{dx^2}$, of which the solution (supposed to be periodic) is, $h = \mathrm{B} . \varepsilon^{-\alpha x} . \cos(nt - \beta x)$. But in the theory of the transmission of sound, where the vibrating particles are supposed to possess inertia, the equation is $\frac{d^2 \mathrm{X}}{dt^2} = \mathrm{A} . \frac{d^2 \mathrm{X}}{dx^2}$, of which the solution (similarly restricted) is $\mathrm{X} = \mathrm{B} . \cos(nt - \beta x)$. The former result certainly does not represent anything like the law of diminution of light; the latter does represent its general constancy of intensity (the distance of the source being very great). I infer therefore that the supposition of inertia is absolutely necessary.

Combining this inference with that obtained above regarding the universality of undulations in space, I am led to the conclusion that all space with which we are acquainted contains something which exhibits the property that we call *inertia*. The reasons which have led me to this conclusion appear to me decisive, but I admit them to be fair subjects for doubt and discussion by natural philosophers. Whether we are to infer from this that there is *matter* through all space, is, in my opinion, a metaphysical question.

But the remarks that I have just made will enable me to answer one paragraph of Dr. Faraday's paper. "Perhaps I am in error in thinking the idea generally formed of the æther is that its nuclei are almost infinitely small, and that such force as it has, namely its elasticity, is almost infinitely intense. But if such be the received notion, what then is left in the æther but force or centres of force?" To this I reply, that *almost infinitely* has no meaning but *finitely*, and therefore that the supposed æther, under this description, is precisely in the same category as all other fluids. But I add, in regard to the latter sentence, that the mathematical considerations which I have detailed above, show that there is something in the æther besides force or centres of force, namely *inertia*. And I repeat the expression of my own opinion, that it is easier to conceive this as indicating *substance* (however obscure the idea may be), than to frame a system of laws applying to centres of force which shall represent its effects equally well.

I am, Gentlemen,
Your obedient Servant,
G. B. Airy.

Royal Observatory, Greenwich,
May 12, 1846.

LXXXII. *Explanation of the Vorticose Movement, assumed to accompany Earthquakes. By* Robert Mallet, *C.E., M.R.I.A., Ph.D., &c., Secretary of the Geological Society of Dublin*[*].

IN our progress to the ascertainment of physical knowledge, the removal of error is next in importance to the discovery of that which is true, inasmuch as by the former the road is cleared, by which the difficult journey towards truth is to be accomplished. The substitution, therefore, of a true for a false explication of phænomena, however in themselves unimportant, is never to be neglected; and with this view it was that I some time since addressed myself to the discovery of what I believe to be the true explanation of a somewhat singular and heretofore puzzling circumstance attendant upon the effects of earthquakes upon buildings, which has been frequently observed, and has been hitherto explained, so far as it has been attempted to be explained at all, by the assumption of a vorticose or gyratory movement having been in some inexplicable way given to the ground. The phænomenon alluded to, is the displacement of the separate stones of pedestals or pinnacles, or of portions of masonry of buildings by the motion of earthquakes, in such a manner that the part moved presents evidence of having been *twisted* in its bed *round a vertical axis.*

The first notice I find recorded of such a peculiar motion, is in the Philosophical Transactions, in an account of the

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[*] Communicated by the Author.

earthquake at Boston, in New England, of November 18th, 1755, communicated by John Hyde, Esq., F.R.S. He says, "the trembling continued about two minutes; near one hundred chimneys were levelled with the roofs of the houses, and many more shattered. Some chimneys, though not thrown down, are dislocated or broken several feet from the top, and partly turned round as on a swivel. Some are shoved to one side horizontally, jutting over, and just nodding to fall," &c. This author does not seem to have been struck with this odd circumstance of the twisting round of the chimneys, and offers no explanation. The next instance that I have found is in the account of the great earthquake of Calabria, in 1783, as recorded by the Royal Academy of Naples, quoted by Mr. Lyell, in his Principles of Geology, vol. i. page 482. After describing several other remarkable phænomena, tending to show the great velocity of the shock, such as that many large stones were found, as it were, shot out of their beds in the mortar of buildings, so as to leave a complete cast of themselves in the undisturbed mortar; while in other instances the mortar was ground to powder by the transit of the stone, he says, "Two obelisks (of which he has given figures) placed at the extremities of a magnificent façade in the convent of St. Bruno, in a small town called Stephano del Bosco, were observed to have undergone a movement of a singular kind. The shock, which agitated the building, is described as having been horizontal and vorticose. The pedestal of each obelisk remained in its original place, but the separate stones above were turned partially round, and removed sometimes nine inches from their position without falling." This is all that Lyell says upon the subject; he contents himself apparently with the vorticose account of the Neapolitan Academy.

I have found some few other notices of similar phænomena in old books of travels. Two additional instances, however, will be sufficient. The first will be found in the quarterly journal of the Royal Institution, in a narrative of the earthquake in Chili, of November 1822, communicated by F. Place, Esq.

The church of La Morceda, at Valparaiso, built of burnt bricks, stood with its length north and south. [The houses are built of adobes, or sun-dried bricks.] "The church tower, sixty feet high, was levelled; the two side-walls, full of rents, were left standing, supporting part of the shattered roof, but the two end-walls were entirely demolished. On each side of the church were four massive buttresses, six feet square, of good brickwork; those on the western side were thrown down and broken to pieces, as were two on the eastern side.

The other two were twisted off from the wall in a north-easterly direction, and left standing." The direction of the shocks was thought to be either from the south-west, or from the north-west.

We shall see hereafter evidence in the twisting of the two remaining buttresses, that the former was the real direction of the shocks, and that there was no vorticose motion, (indeed, the idea of two vortices, with centres only a few feet apart, is absurd upon the face,) but that the twisting of the buttresses is accounted for simply by a straight line movement, in connection with the attachment of the buttresses at one side, to the flank wall of the church.

The last instance I shall quote is from the pages of the able and delightful Darwin, in his Journal of a Naturalist's Voyage (Colonial Library, edit. p. 308), in describing the effects of the great earthquake of March 1835, upon the buildings in the town of Conception; and after noticing, also, the evidences of immense velocity in the shock, by which the projecting buttresses from the nave walls of the cathedral had been cut clean off close to the wall, by their own inertia, while the wall, which was in the line of shock, remained standing; he proceeds,—"Some square ornaments on the coping of these same walls were moved by the earthquke into a diagonal position. A similar circumstance was observed after an earthquake at Valparaiso, Calabria and other places, including some of the ancient Greek temples" (for which he quotes Arago, in *L'Institut.* 1839, p. 337, and Miers's Chile, vol. i. p. 392).

"This twisting displacement," he proceeds, "at first appears to indicate a vorticose movement *beneath each point*, thus effected; but this is highly improbable. May it not," he adds, "be caused by a tendency in each stone to arrange itself in some particular position with respect to the lines of vibration, in a manner somewhat similar to pins on a sheet of paper when shaken?"

The sagacity of Darwin at once showed him that the vorticose hypothesis was most improbable, and that in order to its being able at all to account for the phænomenon, a separate vortex must be admitted for every separate stone found twisted, the axis of rotation of the vortex having been coincident with that of the stone: besides this paramount improbability, therefore, a little further reflection would have led either Lyell or Darwin to estimate the necessarily inconceivable velocity of motion, at the extremity of the radius of one of these vortices, even if assumed at no more than a few hundred feet, in order that its velocity, within a few inches of the centre, should be so great as to wrench out of its mortared bed, and twist a block of masonry by merely its own inertia.

Considering these circumstances, on lately reading the foregoing passages of Darwin, I was soon led to see that the twisting phænomena observed could be readily accounted for upon the established principles of mechanics, without having recourse to either vortices or vibrations, arranging blocks of many hundred weights, after the manner of pins on paper, or sand on one of Chladni's acoustic plates,—an explanation which, with all my admiration of Darwin, appears quite as far from probability as its predecessor.

I assume, then, nothing more than what is universally admitted, that during earthquakes a motion of some sort takes place, by which the ground itself, and all objects resting upon it, are shaken or moved back and forwards, by an alternate horizontal motion, within certain narrow limits, which, for all present evidence to the contrary, may be a straight line motion, though possibly variable in direction at different, and sometimes closely successive times, and the velocity of which is sufficient to throw down or disturb the position of bodies supported by the earth, through their own inertia.

Let us now apply this to the cases described of stones twisted on their bases, and the explanation will at once come to light.

If a stone, whether symmetrical or otherwise, rest upon a given base, and that motion be suddenly communicated horizontally to that base in any direction, the stone itself will be solicited to move in the same direction, and the measure of force with which the movement of the base is capable of affecting the stone or other incumbent body, is equal to the amount of friction of the latter upon its base—a function of its weight which, without the intervention of cement, may be from one-fifth to one-tenth of the weight of the body, for cut stone resting on cut stone, but may be increased to any amount by the intervention of cement.

The stone, however, is possessed of weight, and therefore of inertia; that is to say, being at rest, its whole mass cannot be instantly brought into motion by the plane, and if the amount of adhesion between the stone and its bed be less than the inertia due to any given velocity of horizontal movement of the bed, the bed will move more or less from under the stone, or the stone will appear to move in a contrary direction to that of the motion of its bed.

Now the inertia of the stone, which is here the resisting force, may be considered to act at the centre of gravity of the body.

The impelling force is the grasp of the stone, which its bed holds of it by friction or adhesion; and this may also be referred to some one point in the surfaces of contact, which we might call *the centre of adherence.*

If, then, a stone or other solid body rest upon a horizontal plane, which is suddenly moved with sufficient velocity to effect motion in the incumbent body, three several conditions of motion of the body may occur, according to the respective position of the centre of gravity of the stone, and of the centre of adherence.

1st. The centre of gravity of the stone may be at such a height above the base, that it shall upset by its own inertia. This is the case with houses, towers, walls, &c., when they fall by earthquakes, accompanied also by dislocation of their parts.

2nd. The centre of adherence may be in a point of the base, plumb under the centre of gravity of the stone; or in a vertical plane, passing through the centre of gravity of the stone, and in the direction of motion of the base.

In this case, the stone will appear to move in the opposite direction to that in which the base has moved; that is to say, the stone may have acquired more or less the direction of motion of the base, according as the motion of the latter has been longer or shorter continued, or less or more rapid; but, in so far as the movement in the opposite direction has taken place, the base, in reality, has slipped from under the stone.

3rd. The centre of adherence may neither be plumb under the centre of gravity of the stone, nor in the plane of motion passing through its centre of gravity, but in some point of the base outside the line of its intersection by this plane; in which case, the effect of the horizontal rectilineal motion of the base will be to twist the stone round upon its bed, or to move it laterally, and twist it at the same time, thus converting the rectilineal into a curvilineal motion, in space; the relative amount of the two compounded motions being dependent upon the velocity and time of movement of the base, and upon the perpendicular distance measured horizontally at the surface of adherence, between the centre of adherence and the centre of gravity of the stone.

This latter case is that which applies to the twisted stones of Calabria, South America and Greece; and affords, as I feel assured, the true explanation of the phænomena.

The relation of these forces, which have taken so many words to state correctly, might, of course, have been expressed algebraically in three lines; but as this would not be universally intelligible, I have preferred the more tedious and in-elegant statement of words; and to render the matter quite familiar, have prepared a model of one of the Calabrian pedestals, figured by Mr. Lyell, which will exhibit to the eye all the phænomena already adverted to, by giving by the hand a rectilineal horizontal motion to the base*.

I have now proved that no vorticose motion is requisite to account for the twisting of obelisks, &c., as observed in earthquakes, and that nothing more than a simple horizontal rectilineal motion is demanded; but, it may be asked, if this rectilineal horizontal motion in earthquakes be an alternate one also—if the earth shake both back and forwards—how is it that these and other displaced bodies are not moved back into their places again by the reverse motion, by the same sort of motion, acting in the contrary direction?

This question is, I believe, fertile in consequences, and its consideration has led me to some further conclusions as to the nature of earthquake motions. The first reason obviously is, that as the forward movement has by displacement produced a new set of conditions as to the centres of gravity and of adherence of the stone and base, so it can scarcely by possibility ever occur that there shall be precisely such as to give rise to such a new form of twisting motion as shall neutralize that first produced, although it is quite probable that *some* second twisting may be produced by the backward stroke or motion; for this view I am indebted to my friend Dr. Apjohn. But this alone is not sufficient. After looking through a great number of authors, on earthquakes, I have not been able to find one that has endeavoured, far less succeeded, in shaping to himself any distinct notion as to what the precise nature of the earthquake movement is. The ancients, appealing to their senses, so far as these could guide them, thought that it was like the shaking of a sieve, as the word σεισμὸς tells us. The moderns in general are not more exact in their notions: a trembling, a vibration, a concussion, a movement, and so forth, are the words we find scattered through even scientific authors. Mitchell, Lyell and Darwin, with some others, although they obviously have formed no distinct idea on the subject, use the word "undulation," and in so far, have come nearer to the truth; for it appears to me, that the fact, that displaced bodies are not occasionally replaced, in earthquakes, is conclusive evidence of either one of two things: either the motion is limited to horizontal direct movement, in one or more directions; and, if so, the whole mass of the disturbed country must be pushed bodily forward, and remain so, of which there

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* Exhibited at the Geological Society of Dublin, from whose Transactions this paper is extracted.—Read 8th December 1845.

is no evidence; and all bodies must, as the effect of one shock, fall in the one direction, and not in opposite directions, which is contrary to observed facts: or, on the other hand, if the movement be an alternate horizontal motion, as all observations go to prove it is, then the motion in one direction must be slower than in the other, or attended with other differences of circumstances. The backward motion must be different from the forward motion, or otherwise displaced bodies would be replaced by the recurrence, in the opposite sign of forces similar and equal to those that first set them in motion; but they are not found so replaced.

Now, of all conceivable alternate motions, the only one that will fulfill the requisite conditions observed, namely, that shall move with such an immense velocity as to displace bodies by their inertia, or even shear close off great buttresses from the wall, they sustained, (Darwin) or project stones out of their beds, by inertia; that shall have a horizontal alternate motion, either much quicker in one direction than in the other, or different in its effects; and that shall be accompanied by an upward and downward motion at the same time—a circumstance universally described as attendant on earthquakes—the only motion, I say, that will fulfill these conditions, is the transit of a great solitary wave of elastic compression, or of a succession of these, in parallel or in intersecting lines through the solid substance and surface of the disturbed country.

The general idea of the nature of earthquake motion, viz. that it consists of a wave of some sort, is not however new, although so entirely neglected by the mass of recent geological authors. To the Rev. John Mitchell, M.A., Fellow of Queen's College, Cambridge, the merit of this idea appears originally due. In a paper communicated to the Royal Society, read in 1760, and published in the 51st vol. of the Philosophical Transactions, part 2nd, he treats at length of the origin and phænomena of earthquakes, and distinctly enunciates the following view:—

That the motion of the earth is due to a wave, propagated along its surface, from a point where it has been produced by an original impulse. This impulse, he conceives, to arise from the sudden production or condensation of aqueous vapour, under the bed of the ocean, by the agency of volcanic heat, the supposed mechanism of which he minutely describes; but while he was so far right in his conception of an elastic wave of *some sort*, I expect to be able shortly to prove that he has wholly mistaken the nature of the wave that actually occurs, and that a wave, such as he assumes, can have no existence consistently with the physical structure of our globe, with the observed facts of earthquake motions, or even with the conditions of his own hypothesis.

LXXXIII. *On the Causes to which Musical Sounds produced in Metals by discontinuous Electric Currents are attributable.* *By* Prof. Elie Wartmann[*].

SINCE the discovery made in 1837 by Dr. Page, and verified the following year by Prof. Delezenne, of the possibility of producing a musical sound by electricity, this interesting phænomenon had scarcely been studied, when in 1844 MM. Marian, Beatson, Gassiot, and De la Rive all at once made known the various conditions of its production. The interesting memoir of the last gentleman, printed in vol. v. p. 500 of the *Archives de l'Electricité*, contains a great number of very valuable results. But the theoretical part of the subject has not yet been presented under a precise and general form, and it is with a view to supply, if possible, this gap, that I have undertaken the following experiments. I imagined them in the month of August 1845, in consequence of a meeting at which M. de la Rive exhibited his curious apparatus to Prof. Dove and myself.

A well-annealed soft-iron wire, $1^{m}\cdot7$ long and $2^{mm}\cdot5$ in diameter, was fixed in a horizontal position on a thick trencher of hard wood inserted into the wall. One of its extremities was held back by the jaws of a clamp, whilst the other supported a weight of 24 kilogrammes. Upon a cork, pierced by friction with the wire, I arranged a small plane mirror, with parallel faces, made at the Optical Institute of Munich and intended to reflect, into a telescope furnished with cross wires, the divisions of a scale placed at a distance of two metres. This arrangement, similar to that of the magnetometer, exhibits the least deviations of the reflecting surface, when it is not displaced parallel to itself. The iron wire passed through a wooden reel, the bore of which was five centimetres in diameter, and on which were rolled three copper wires enveloped with silk, $23^{m}\cdot6$ long and 3 millimetres in diameter. I employed a Bunsen's battery of eleven pairs, and a mercurial rheotome or contact-breaker; these two instruments were enclosed in an ante-room adjoining the laboratory.

According to the place which the wire occupies, it becomes the seat of greater or less transversal vibrations, whose plane may be varied at will. In general, in any position of the wire, the intensity of the effect varies at different points of its length, as is perceived on bringing the mirror to such points. The

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[*] Communicated by the Author.

amplitude of the vibrations is not the same for different parts of the wire subjected similarly to the reel. M. de la Rive found this by the comparison of the sounds obtained. These phænomena result from the attraction exerted upon the wire by the parts of the coil which are the nearest to it: they cause a distinct class of sounds. But there exists another cause of vibrations in the wire, the effect of which is more or less independent of this lateral attraction. Longitudinal vibrations are produced in it, with which correspond sounds of a peculiar character. If the axis of the reel was identical with that of the wire, supposing it exactly rectilinear and cylindrical, a transverse deviation would no longer take place. But even then, the molecules on which the electro-magnetic action is exerted are attracted right and left of the centre of the axis of the reel towards this central point, as a steel needle is seen to be drawn into it as soon as it is introduced into the hollow of the helix. It is this internal vibration which, by the discontinuity of the electric current, is rendered periodical in two opposite directions, that determines the second class of sound.

Let us now pass to the case of the current transmitted by the wire. In order to study it, I substituted for the mirror the spherical and perfectly polished bulb of a small mercurial thermometer. The optical axis of the telescope, passing through the intersection of the crossed wires, was directed on the brilliant image of a luminous point reflected very obliquely at the upper part of the convexity. This arrangement discovers any change in form of the wire, even in the direction of its length. I was not able to perceive any elongation of the wire under the electric action, although it gave a very distinct sound. I attribute the principal cause of this sound to the polary arrangement which the molecules undergo in order to give passage to the electricity. This arrangement is manifest in many cases, and I have elsewhere pointed out a very great number of them*. It is the result of a struggle between the molecular forces which constituted the primitive state of equilibrium of the body and the new activity which the dynamical condition of the fluid excites. If the flow of the latter is continuous, this struggle is instantaneous, and the noise which it occasions is null or nearly so; but it recommences with each closing of the circuit if the flow is periodical.

It is already known from the experiments of M. Peltier †,

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* Memoir on the Electric Diathermansie of Voltaic Pairs: *Archives de l'Electricité*, vol. i. page 74.

† *Comptes Rendus des Séances de l'Acad. des Sciences de Paris*, Jan. 6th, 1845.

and of various scientific men, that the prolonged passage of the electricity by metallic wires alters essentially their tenacity. It seemed to me very probable that the elasticity of wires subjected for some time to the intermittence of currents which renders them sonorous, must be altered in a permanent manner.

Since the experiments just mentioned were made, M. Wertheim has published[*] a very interesting note, in which he describes a process of observation analogous to mine, although less delicate, and indicates the causes to which he attributes the sounds produced. Although I agree with him on most points, I differ from him both as to what relates to the attraction exerted from the two sides of the centre of the helix, an attraction which he does not mention, and in the explanation of the case in which the wire is directly traversed by the discontinuous current. The skilful experimentalist whom I have just named attributes the sound produced to the heat engendered by the current. Nevertheless my wire indicated no perceptible heat. It results from the experiments of M. de la Rive and my own, that the sonorous state continues with more than 600 interruptions a second. How shall we admit that the elevation of temperature and the diminution of elasticity which accompany it can disappear in $\frac{1}{600}$th of a second? The current of a pile of eleven pairs certainly does not alter the thermical state of a bar of a centimetre square in section, as I have directly established[†]: nevertheless, if it is discontinuous, it renders it sonorous. I may add, finally, that this heating does not take place when the reel is employed, as any one may convince himself by placing a bismuth and iron pair in its hollow, connected with a very delicate rheometer. Nevertheless the sonorous property may be the same as with the wire directly subjected to the current.

Lausanne, March 16, 1846.

LXXXIV. *An Account of various Substances found in the Guano Deposits and in their Vicinity.* *By* E. F. TESCHEMACHER, *Esq.*[‡]

REPORTS having been circulated that large quantities of saltpetre (nitrate of potash and nitrate of soda) were to be found of a very good quality in the neighbourhood of the guano deposits on the coast of Africa, numerous vessels were

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[*] *Comptes Rendus*, Feb. 23, 1846.

[†] Phil. Mag., Oct. 1843; *Archives de l'Electricité*, vol. ii. page 601.

[‡] Communicated by the Chemical Society; having been read December 1, 1845.

despatched both from London and Liverpool in search of those valuable substances, particularly as it was considered they might be obtained upon the same terms as Ichaboe guano, namely, for nothing but the labour and expense of fetching. No favourable accounts however have as yet been received as to the success of these undertakings. The evidence of such deposits existing there at all was very unsatisfactory; the circumstance much relied upon was the existence of large beds of nitrate of soda in the neighbourhood of the coast of South America, and large deposits of guano similar in many respects to the deposits of guano on the African coast: there was certainly an abundance of animal matter and ammoniacal salts to furnish the nitric acid, and a temperature high enough to effect the decomposition, but the source from whence the alkaline bases of potash and soda were to be derived was not very evident. The principal source of saltpetre in the East Indies is from numerous districts of nitrous earth found on the surface of the soil, which being compounds of lime and magnesia with nitric acid, they are dissolved out, and the saltpetre subsequently formed by the decomposition of these nitrous compounds by potash salts. The nitrate of soda saltpetre beds in the Province of Tarapaca near Iquiqua on the coast of South America, are the only instances known of the occurrence of saltpetre ready-formed in extensive beds, but even this deposit contains the salt in a state of great impurity.

These explorations, however, on the African coast have brought to light various other substances which have been found there, the details of which are more particularly the object of this communication.

The substances which I shall now describe are found in the guano beds, or in their vicinity, either in a crystalline state, or in distinct masses. The first substance is a crystalline salt, perfectly transparent, with a cleavage and brilliant faces in one direction only; it gives a yellow precipitate with nitrate of silver; gives off ammonia upon application of caustic potash, and when heated to redness loses about 50 per cent. of water and ammonia; I consider it therefore to be *phosphate of ammonia*. The portion of salt I examined consisted only of a few grains, and was consequently too small a quantity to analyse with exactness.

The next substance was also a crystalline salt a little mixed with guano in its cavities; it possessed a cleavage with brilliant planes in two directions: upon examination with the reflecting goniometer, it gave 112° as the measurement of the angle formed by the meeting of the adjacent planes. Upon analysis I found it to consist of—

21·0	parts of	Ammonia.
55·50	...	Carbonic acid.
23·50	...	Water.
100·00		

being nearly equivalent to 1 atom of ammonia, 2 atoms of carbonic acid, and 2 atoms of water.

Formula $NH_3 + 2CO_2 + 2HO$,

and is consequently a *bicarbonate of ammonia*.

The third substance was found at Saldanha Bay on the coast of Africa, imbedded in patches in the mass of guano. It is found in distinct crystals with numerous modifications, many of the planes possessing sufficient brilliancy to enable me to measure the angles by the reflecting goniometer. I have given the measurements of one crystal, from which it appears the primary form is the right rhombic prism of 57° 30′ and 122° 30′: it has a cleavage parallel to plane M*. Upon analysis I find this substance to be composed of—

14·30	parts of	Ammonia.
17·00	...	Magnesia.
30·40	...	Phosphoric acid.
38·10	...	Water.
99·80		

which is nearly equivalent to 1 atom ammonia, 1 atom magnesia, 1 atom phosphoric acid, 5 atoms water.

Formula NH_3, MgO, $PO_5 + 5HO$.

It is therefore the *ammonio-magnesian phosphate*. The specific gravity is 1·65, hardness 2; it falls to powder before the blowpipe, giving off water and ammonia. It occurs white, translucent, sometimes coloured brown by the guano; it readily dissolves in weak acids.

This substance is clearly derived from the guano; but being insoluble in water, it must have been held in solution by some of the organic acids of the guano, and deposited therefrom in large crystals, as they are found, but disseminated in patches only of the guano, in various parts of the beds.

This substance not having been found before in a native state, but hitherto only known as one of the artificial products of the laboratory, must be considered as a new mineral body; I therefore propose to give it the mineralogical name of *Guanite*, this name being derived from the circumstances and locality of its formation.

The source from which the first two substances, namely, the phosphate of ammonia and the bicarbonate of ammonia,

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* See the angular measurements subjoined.

are derived, is clearly the percolation of water through the guano beds dissolving out these salts, which running into lower situations may be detained in lagoons and hollows of rocks, where being subject to the high temperature of the climate they would be evaporated down, leaving these salts in the crystalline state described. As guano contains abundance of these two salts, it is possible there may exist considerable masses of them; should this be the case, it is evident that to the chemist in particular it would be of great interest as an additional source of these valuable salts.

The chance of finding any considerable quantity of guanite in the state of crystals is not great, but as it forms one of the ingredients of guano it is a substance of some importance. The application of it as a manure in combination with other ingredients is likely to be highly beneficial, it being a compound containing two important substances in an insoluble state, namely, ammonia and phosphoric acid; these may be taken up by plants only as they may be required, and not be liable to be dissolved out of the soil or evaporated like other ammoniacal salts.

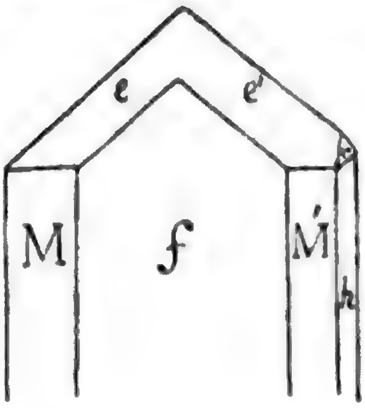
The last substance which I shall describe was also found imbedded in the guano from Saldanha Bay; it consists of small globular particles composed of concentric laminæ slightly adhering together, of a yellowish white colour, containing in places portions of a similar nature, which on fracture have appearances of an organic structure like bone, but on examination by the microscope proved to be portions of shells resembling Nummulites. On analysis I found the substance to be composed of—

37·50	parts	Carbonate of lime.
32·50	...	Carbonate of magnesia.
12·00	...	Phosphate of lime.
12·00	...	Water with a little ammonia and animal matter.
3·00	...	Sand.
2·50	...	Alkaline sulphates and chlorides.
99·50		

There does not appear to be any great quantity of this substance. How it has been formed it is difficult to imagine; the composition is so very different either from that of bones or shells, particularly in regard to the large quantity of carbonate of magnesia which it contains. It is however probable that both bones and shells form the base of this substance, and that partial decomposition having taken place, the magnesia may have subsequently entered into combination with the carbonate and phosphate of lime.

Measurements of Guanite.

M on M′	57°·30	M′ on *e*′	142°·10
M on *f*	118°·30	*h* on *c*	133°·20
M′ on *f*	118°·30	*e* on *e*′	91°·50
M′ on *h*	151°·00	*e* on *f*	112°·20
f on *h*	89°·30	*e*′ on *f*′	112°·20
M on *e*	142°·10	*e* on *c*	142°·10



LXXXV. *Notes on the Preparation of Alloxan.* By WILLIAM GREGORY, *M.D., F.R.S.E.*[*]

IN an interesting and able paper on alloxan and its derivatives, the first part of which appears in Liebig's *Annalen* for September 1845, Schlieper enters into minute details concerning the most advantageous method of preparing alloxan, and after describing the results which he obtained on repeating the process given by me, proposes a new method of his own, which he considers in every way preferable, as yielding, with greater facility and certainty, a larger proportion of alloxan. Professor Liebig in his Lectures (Lancet 1845) also recommends Schlieper's method as the best in every respect.

I am still, notwithstanding, inclined to give a decided preference to my own process, when carefully performed, and that on the grounds of its superior simplicity, facility and productiveness. A brief comparison of the two methods, with their results, will enable the reader to judge for himself.

I must first of all, however, observe, that Schlieper, in repeating my process, has not obtained results so favourable as I had formerly announced; so that, in his hands, his own method has been the more productive. I formerly obtained from 100 parts of uric acid 90 of crystallized (hydrated) alloxan, perfectly pure, not reckoning the portion of alloxan remaining in the mother-liquids. Schlieper, on the other hand, from 15 ounces of uric acid, treated by my process, obtained, including the contents of the mother-liquids, 8 ounces hydrated alloxan, $1\frac{3}{4}$ ounce alloxantine (= $2\frac{1}{4}$ ounces alloxan), and $\frac{3}{4}$ ounce parabanic acid; in all equivalent to about $11\frac{1}{4}$ ounces of alloxan. This only amounts to 75 per cent.; whereas I obtained 90 per cent., exclusive of the mother-liquids, which I find on an average to yield fully one-tenth more; in all, therefore, at least 100 per cent. I may here state that I have

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[*] Communicated by the Chemical Society; having been read December 15, 1845.

never failed to obtain this as an average result since my process was published, although I have very often repeated the process. Several of my pupils have been equally successful. I shall now, therefore, describe the process as I have for some time pursued it, and its simplicity will, I trust, be evident.

In my original account of this process, I recommended the use of nitric acid of sp. gr. 1·3 to 1·35, and it was with such acid, as I believed, that my results were obtained. But as Schlieper found it impossible to succeed with acid of less sp. gr. than 1·4 to 1·42, I suspect that I may have been mistaken as to the sp. gr. of my acid. This I cannot now ascertain; but it is rendered probable by the circumstance that, in the experiments about to be mentioned, I found an acid of 1·412 to answer my purpose perfectly, with the same appearances as I had formerly observed.

Schlieper having corrected this error proceeds to describe my process, as performed by him, with great accuracy and minuteness, and his description of the phænomena entirely agrees with my experience. I can only account for his not obtaining such favourable results as I have always done, to the circumstance of his acid being a little too concentrated. However this may be, on reading his paper I proceeded to repeat my process, and obtained the results to be hereafter stated.

The following is the process I now follow:—2 or $2\frac{1}{2}$ fluid ounces of colourless nitric acid, sp. gr. 1·412, are placed in a flat-bottomed dish or beaker glass, and as much uric acid is introduced as will lie on the point of a small spatula. This is well-stirred in to prevent the formation of lumps, and in a few minutes effervescence commences, the liquid becomes slightly warm, and the powder dissolves. More uric acid is now added, taking care never to exceed a certain small quantity, and not to allow the liquid to become warm beyond a certain degree, which is easily judged of by laying the dish on the hand. If too hot when uric acid is added, or if too much acid be added at once, the uniform steady effervescence is changed into a violent and tumultuous action, after which no alloxan can be obtained. It is proper to have a plate with cold water at hand, in which to place the dish or glass if it should seem likely to become too warm. But a little practice enables us to regulate the operation so that no external cooling is required.

After several portions of uric acid have been added, crystals of alloxan begin to appear in the warm liquid, but the addition of uric acid is to be continued, with the same precautions, till so much alloxan has been formed, that on cooling the whole becomes nearly semisolid. When this point is reached the liquid has become somewhat viscid, and this, along with the presence of the crystalline deposit of alloxan, gives a peculiar character to the effervescence toward the end of the operation. I commonly find that with 2½ fluid ounces of nitric acid the point above alluded to is reached when about 1200 grains of uric acid dried at 212° have been dissolved. It does not answer to operate on a much larger scale; it is better to use several dishes at once, each containing 2½ or at the most 3 fluid ounces of acid. For every 500 grains of uric acid 1 fluid ounce of nitric acid may be allowed.

The whole is now allowed to stand all night in a cool place, and next day the alloxan is collected on a funnel with the aid of a little asbestus. The mother-liquid drains off, and the last portions of it are cautiously displaced by ice-cold water, till the droppings are found to have only a moderately strong acid taste. The alloxan on the funnel, which is anhydrous, is then digested with just as much water at 140° or 150° F. as will dissolve it. The solution is filtered, and on cooling deposits a large crop of crystals of hydrated alloxan. [Should too much water have been added, the filtered liquid must be evaporated at from 120° to 140° F., till on cooling it crystallizes abundantly.] The mother-liquid of these crystals, evaporated at the same temperature, yields a second crop. The mother-liquor of this is added to the acid mother-liquor previously drained off, and the whole liquid treated, after the addition of two or three times its bulk of water, with sulphuretted hydrogen, till the alloxan present is reduced to the state of alloxantine. As a part is always reduced still further to dialuric acid, the liquid must be exposed to the air for a day or two, or until it deposits no more crystals. The alloxantine is purified by solution in boiling water, filtration to separate sulphur, and crystallization; and when dry three parts of it correspond to rather more than four of hydrated alloxan. If required, it may very easily be converted into alloxan; as Schlieper has described this process I need not repeat it here.

The mother-liquid of the alloxantine generally yields some parabanic acid; but very little if the process has been carefully performed.

I think it will be admitted that the above process is sufficiently simple. It will be observed that I no longer recommend the separation of the alloxan formed from the nitric acid in several successive portions, but that there is only one operation for all, in which the alloxan is collected on a funnel with asbestus. I used sometimes to divide the process into five successive operations, and generally made three of them: but I am now convinced that it is best to dissolve in the nitric acid the whole of the uric acid that is to be dissolved before collecting the alloxan.

Let us now consider the productiveness of this method. I have already stated my average of former results to have been 90 per cent. of crystallized alloxan, exclusive of the mother-liquid, which corresponded to one-tenth more. As the process now stands we have—1. The first crop of crystals of alloxan, varying with the proportion of water used to dissolve the anhydrous alloxan. 2. The second crop of the same crystals. 3. The alloxantine from the mother-liquid converted into alloxan, or calculated in that form. I take no account of the parabanic acid.

Experiment 1.—Uric acid 2600 grains; alloxan, first crop, 1950 grains, second crop, 550 grains; alloxantine, 200 grains, equivalent to alloxan, 290 grains. In all, therefore, from 2600 grains of uric acid, 2790 grains of hydrated alloxan, or 107 per cent. nearly.

Experiment 2.—Uric acid, 1130 grains; alloxan, first crop, 800 grains, second crop, 140 grains; alloxantine, 80 grains, equivalent to alloxan, 116 grains. In all, therefore, 1056 grs. of alloxan from 1130 of uric acid, or 93 per cent.

Experiment 3.—Uric acid, 1500 grains; alloxan, first crop, 1150 grains, second crop, 270 grains; alloxantine, 120 grains, equivalent to alloxan, 174 grains. In all, therefore, from 1500 grains of uric acid, 1594 grains of alloxan, or 106 per cent.

The above results, averaging 102 per cent. of pure hydrated alloxan, were obtained without difficulty. Indeed the only delicate point in the process is the attention necessary to avoid too great a rise in temperature, alloxan being decomposed by heat even when simply dissolved in water, but still more when acid is present. A little experience however makes this quite easy; and besides, this difficulty attaches equally to Schlieper's new method, as we shall see.

The formula of uric acid being $C_{10}\,N_4\,H_4\,O_6$, while that of hydrated alloxan is $C_8\,N_2\,H_4\,O_{10} + 6$ aq, it is obvious that 100 parts of uric acid can produce about 128 of alloxan. It is not likely that we shall ever obtain the full proportion without loss, but I consider my process, simple as it is, to furnish a very satisfactory approximation, considering the impossibility of separating the whole alloxan from the acid liquid in which it is formed.

If we now refer to Schlieper's account of his new method, we find that it includes the following operations:—1. The uric acid is acted on by hydrochloric acid and chlorate of potash, care being necessary, as in my process, to keep the temperature below a certain point. 2. The *whole* of the alloxan is reduced by sulphuretted hydrogen to the state of alloxantine. 3. The alloxantine is reoxidized by nitric acid, and thus reconverted into alloxan. I cannot admit that this process is either more simple or more easy than my own. On the contrary, as I obtain nine-tenths of the whole alloxan, or 90 parts from 100 of uric acid directly as alloxan, and pure, in the first crystallizations, while Schlieper first converts all his alloxan into alloxantine, and then reconverts the alloxantine into alloxan; and further, as I use no other reagent but nitric acid in preparing these nine-tenths, the advantage of simplicity and facility is entirely on my side.

From 4 ounces of uric acid, Schlieper obtains by his own process 2 ounces 7 drachms and 20 grains of alloxantine, equivalent theoretically to 3 ounces and 7 drachms of alloxan, or nearly 97 per cent. But in reconverting this alloxantine into alloxan by nitric acid, it will be found impossible to obtain, practically, the whole alloxan, since some of it must remain in the mother-liquid; and moreover, in the process of oxidation by heating with nitric acid some alloxan is very likely to be converted into parabanic acid, and thus lost. Judging from experience, I should not expect the 97 per cent. of alloxan obtained in theory to yield, in crystals, more than 90 per cent.

As far as productiveness, therefore, is concerned, I may claim also a superiority for my method. It is true that it has not succeeded so well in the hands of Schlieper, but this must I think be attributed to accidental causes, and possibly to a want of perfect familiarity with the method on the part of Schlieper, who seems to be so good an operator, that I cannot doubt that he would, after a little practice, obtain the same results as I have always succeeded in obtaining.

Finally, I beg to remind those who may wish to try my process, that what Schlieper describes as a modification of my process is the process itself, unmodified; because the only change introduced by Schlieper consists in the use of acid at 1·4 or 1·42 instead of 1·3 or 1·35, as erroneously recommended in my original process. In point of fact, the acid which I have long used for the purpose has the sp. gr. 1·412, and for this number 1·3 or 1·35 was accidentally substituted in writing or printing my former notice. In common with all chemists I am much indebted to M. Schlieper for pointing out this oversight.

LXXXVI. *Intelligence and Miscellaneous Articles.*

ON CHLOROAZOTIC ACID.

M. BAUDRIMONT remarks, that Mr. Edmund Davy published his researches on aqua regia in 1831, and concluded from them, that what he terms chloronitrous acid is composed of equal volumes of chlorine and nitric oxide gases, which combine without alteration of volume.

According to M. Baudrimont, the presence of [uncombined] chlorine in the product obtained by Mr. Davy prevented the product from being properly examined, and he therefore undertook fresh researches on the subject.

In order to prepare the active product [chloroazotic acid] of aqua regia, M. Baudrimont mixed three parts, by weight, of nitric acid, of specific gravity about 1·314, with five parts of hydrochloric acid, of specific gravity 1·156; this mixture yields a colourless liquid, which after an uncertain time becomes red, according to the temperature of the air and the intensity of the light to which it is exposed. If, however, the mixture be heated, it becomes red at about 187° Fahr., and yields vapour of the same colour; the temperature gradually increases to nearly 230° Fahr., and then remains so invariably during the whole time of the operation.

If the product of the distillation be received in a vessel properly cooled, a red liquid is obtained; but if the neck of the retort be simply passed into a receiver, a red vapour is formed which does not condense, and a colourless liquid is condensed.

This experiment shows that this distillation yields two distinct products—a red vapour which is very volatile, and a colourless product which is more fixed. It shows also that the temperature of 230° Fahr. does not indicate a boiling-point, but a fixed point of decomposition. By adopting the requisite arrangements, the red vapour may be condensed in tubes in the state of a red liquid, which boils at a very low temperature; it can be preserved only in tubes hermetically sealed. This is what the author terms chloroazotic acid; the properties of which are as follows:—

At a sufficiently low temperature it is a red limpid liquid, surmounted with vapour of the same colour; its boiling-point is about 20° Fahr.; from this it follows that it is gaseous at ordinary temperatures. In the state of gas it is red, and possesses a suffocating odour, analogous to that of chlorine, but still differs considerably from it.

The extreme volatility of chloroazotic acid presented almost insurmountable difficulties to the determination of its principal properties.

The elements of chloroazotic acid reduced to volumes, and the volumes multiplied by the corresponding specific gravities, give the following results:—

N or 2 volumes	......	$2\times0{\cdot}9727=1{\cdot}9440$
O^3 or 3 ...	......	$3\times1{\cdot}1057=3{\cdot}3171$
Cl^2 or 4 ...	......	$4\times2{\cdot}4216=9{\cdot}6864$
9 elementary volumes give	....	14·9475

One experiment on the specific gravity of chloroazotic acid gave 2·49, and another 2·45, and lead to the same result,—

$$\frac{14{\cdot}9475}{6}=2{\cdot}49.$$

Thus the 9 volumes of elementary gases which form chloroazotic acid are condensed into 6 volumes, and one volume of the acid contains $\frac{1}{3}$rd volume of nitrogen, $\frac{1}{2}$ of oxygen, and $\frac{2}{3}$rds of chlorine. The specific gravity of the liquid acid was found to be 1·3677.

Chloroazotic acid consists of—

Nitrogen ..	12·6	or 1 eq.
Oxygen....	22·4	... 3 ...
Chlorine....	65·0	... 2 ...
	100·	

The extreme volatility of chloroazotic acid renders the examination of its chemical reaction extremely difficult, and it can be effected only at very low temperatures.

With phosphorus, the acid enters into ebullition, and disappears without acting sensibly upon it; arsenic in powder is acted upon, and yields a white product; silver in powder occasions deflagration, and the liquid disappears; gold is rapidly dissolved, but platina is acted upon with more difficulty; alcohol yields an æthereal odour, analogous to that of nitric æther.

Chloroazotic acid in the gaseous state appears to have no action on phosphorus at ordinary temperatures; the latter may be even liquefied by heat, without producing any more apparent action.

Pulverized arsenic and antimony burn vividly in the gas; bismuth is immediately attacked, yielding white vapours, but unaccompanied with light; potassium is slowly acted upon at common temperatures, but the reaction is violent when heated to its fusing-point; there occurs sudden increase of temperature, accompanied with vivid light; gold is acted upon, and a plate of copper, heated to dull redness, burns very vividly; tin heated nearly to its melting-point, does not appear to be immediately attacked, but in a little time it is tarnished and rendered white; mercury is immediately acted upon; one-half of the gas disappears, and the remainder is nitric oxide, entirely absorbable by solution of protosulphate of iron.—*Ann. de Ch. et de Phys.*, Mai 1846.

NOTICES OF NEW LOCALITIES OF RARE MINERALS, AND REASONS FOR UNITING SEVERAL SUPPOSED DISTINCT SPECIES. BY FRANCIS ALGER, MEMBER OF THE BOSTON SOCIETY OF NATURAL HISTORY*.

Phacolite from New York.

This rare mineral, which comes to us principally from Bohemia and Ireland, I have discovered among a suite of specimens of various kinds found on New York Island, near Harlem, by Messrs. Mathews and Johnson, of New York city. The specimens, which eventually proved to be this mineral, were labelled stilbite; but their appearance was so peculiar, that I questioned at the time whether they had been correctly designated, and determined to examine them carefully at my earliest convenience. I have since received two other specimens, better characterized than the first, from Mr. Johnson. The crystals are in a geode form, implanted on calcareous spar, and associated with silver-coloured mica and a few scales of oligisto-magnetic iron ore. They are of a wax or honey-yellow colour, have a waxy lustre, and the smallest individuals are translucent. They are brittle, breaking with an uneven fracture, have none of the foliated structure of stilbite, and afford no indications of cleavage. Hardness superior to that of stilbite, and equal to that of chabasite. Their surfaces are roughened or pitted, so as to reflect no image by which they could be subjected to measurement by the goniometer. Before the blowpipe, a fragment of the mineral swells and intumesces slightly, like the Bohemian and Ferroe chabasite, and fuses into an opaline, blebby bead; at the moment of ignition, in the outer flame, it gives out a beautiful green phosphorescence, which I have also noticed, in a less degree, in the phacolite from Ireland. It is soluble in hydrochloric acid. The crystals, at first sight, appear to be rounded, and to have no determinate form; but, on closer examination, some of the smaller and more isolated ones are found to be nearly perfect double six-sided pyramids, precisely similar to the phacolite from Bohemia, differing from it only in colour and lustre. I cannot doubt that, like that mineral, they are secondaries to a primary rhombohedron, probably of the same measurements, and are also identical with it in composition. The absence of well-defined cleavage is unfortunate, but this is a defect which applies equally to the foreign mineral. Nor is the rhombohedral cleavage of ordinary chabasite, of which phacolite is by many supposed to be only a variety, by any means easily determined; in fact, Sir David Brewster has suggested, from optical investigations, whether the primary form of chabasite be not a prism.

Is Phacolite a variety of Chabasite, or distinct from it?

Tamnau of Berlin, in his very complete little essay on Chabasites, has given very good reasons for uniting the two; while Breithaupt has maintained them to be distinct. The primary rhombohedron of

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* From the Journal of the Boston Society of Natural History.

phacolite, according to Breithaupt, is P on P, 94°, that of chabasite P on P, 94° 24′. Phillips makes the last 94° 46′. The analyses of Anderson and Rammelsberg would seem at first to show a marked difference in their composition, a difference which is also shown by the different analyses of common chabasite, resulting in varieties having different formularic expressions. For example, acadiolite contains three per cent. more of silicic acid than common chabasite, and is a tersilicate of lime and the other isomorphous bases, instead of a bisilicate of the same bases. The mineralogical formula of acadiolite is $3\mathrm{Al\ Si}^2+(\mathrm{Cal, N, K,})\,\mathrm{Si}^3+6\ \mathrm{Aq}$, while that of chabasite is $3\mathrm{Al\ Si}^2+(\mathrm{Cal, N, K,})\,\mathrm{Si}^2+6\ \mathrm{Aq}$. Rammelsberg is inclined to regard phacolite as a mixture of acadiolite and scolecite (lime mesotype), the latter containing an additional atom of water*. By uniting the atoms of both, he thus states the chemical formula for phacolite: $2\mathrm{RO\ SiO}^3+2\mathrm{Al}^2\mathrm{O}^3\ 3\mathrm{Si}^3\ \mathrm{O}^3+10\mathrm{HO}$. As the analyses stand (compare Berzelius's and Thomson's with the two just referred to), phacolite differs from chabasite in containing three per cent. less of silicic acid, and three atoms less of water. Now it is obvious that these differences are insufficient to authorize a separation of the two minerals, unless there be a want of agreement in crystallographical and other characters, greater than that as yet pointed out. An equally valid reason could be urged for the separation of acadiolite from chabasite, on the ground of a difference in their composition, had not the examinations of Prof. G. Rose proved an exact agreement in the angles of their primary crystals. So, also, of levyne and gmelinite, which are now admitted to be only varieties of chabasite, their occurring forms all being secondaries to the same primary rhombohedron. The evidence of the identity of any two minerals is best shown by the incipient or intermediate passages of one into the other, in the same specimen. I am not aware that, in the case of the Irish or Bohemian phacolite, such evidence has been adduced; no tendency of the sort is shown in the specimens I have examined from those countries. Now one of my specimens from New York has the distinct form of chabasite (the perfect rhombohedron) and of phacolite (perfect double six-sided pyramids). The first form, however, is rare; the incipient replacements are also shown; but these crystals have not the full perfection of waxy lustre reflected by the ultimate form of phacolite,—a singular effect, attributable, probably, to the nature of the solvent in which the molecules were suspended.

Approach of twin-crystals to the Phacolite form.

These, as they are sometimes presented, would, unless carefully examined, be mistaken for the true form of phacolite. The most perfect specimens I have seen are from Nova Scotia. They consist of two rhombohedrons united in the usual manner, each crystal turned half round, but having their superior edges and lateral angles

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* See First Suppl. to his *Handwörterbuch*, p. 112. It was on these grounds that Hoffmann proposed to separate acadiolite, as well as the Gustafsberg variety, from chabasite.—Poggendorff's *Annalen*, xxv. 495.

deeply replaced. The approach to the form of phacolite is thus produced; the edges and angles not standing out in relief, as they ordinarily do in these twin forms. The striæ, parallel with the edges of the two rhombohedrons, so intersect as to show the compound nature of the crystals. Dr. C. T. Jackson has a fine specimen of this variety from the Two Islands, in Nova Scotia, of a wine-yellow colour; I have another pure white, from the same place.

Yttro-cerite.

This rare mineral is found, associated with brucite, in rolled masses of limestone, in the town of Amity, Orange county, New York. I have as yet seen but two specimens of it, which I found among some fragments of limestone containing brucite and mica, in the duplicate collections belonging to the late Dr. Horton of Edenville. It attracted my attention as being unlike fluor spar, which it was supposed to be at the time, and I have now satisfied myself that it is yttro-cerite, though I have not gone so far as to detect the yttria, the presence of which in the mineral cannot be indicated by mere blowpipe experiments alone. It has no crystalline structure, but appears in thin layers or seams, which sometimes amount to scarcely anything more than peach-blossom or purple stains, penetrating the seams of the limestone: precisely the character of this mineral in the specimens I have of it from Finbo in Sweden. With this it also agrees in hardness and colour. When heated in a glass tube, it slightly decrepitates, shows no phosphorescence, gives out moisture, and becomes milk-white; at the same time there is a perceptible burnt smell. When its powder, moistened with sulphuric acid, is placed in a platinum-crucible, hydrofluoric acid is given out by the application of heat, and the usual reaction on glass is produced. The pulverized mineral, heated with fused salt of phosphorus in an open glass tube, also shows the same reaction, the glass losing its polish where the moisture is deposited. In these experiments I was careful to separate the mineral entirely from the brucite; but I have not been able to obtain fragments sufficiently free from carbonate of lime, to enable me to give its blowpipe characters in detail, or subject it to any other trials. I hope to be able to obtain better specimens at an early day, and then to complete its examination. The mineral is very characteristic, and, in the hand specimen, cannot be distinguished from the Finbo variety.

Ottrelite identical with Phyllite.

The name of phyllite, from *φύλλον*, a leaf, was given by Dr. Thomson to a mineral which was discovered and sent to him for analysis by Prof. Nuttall. It comes from Sterling, Massachusetts, and is disseminated in small thin plates through what appears to be an argillo-micaceous slate. Some of these plates are angular and others rounded, not appearing to have any regular crystalline form; yet in a few instances they present the distinct form of rhomboidal tables. Colour brownish-black, or grayish-black: lustre, shining and semi-metallic; opake; fracture uneven. The knife makes a faint impression upon them. In strong transmitted light, the thinnest discs present a greenish colour. Before the blowpipe, on charcoal, it becomes magnetic, but does not fuse even on the edges; with double its bulk of borax, it slowly dissolves into a dark iron-green glass. Its composition, as stated by Dr. Thomson, is as follows:—

Silica..............	38·40
Alumina	23·68
Peroxide of iron	17·52
Magnesia	8·96
Potash	6·80
Water	4·80
	100·16

Ottrelite was discovered by M. Desclozeaux, and analysed by M. Damour in 1842. A full description of it is given in the *Annales des Mines* for that year, vol. ii. p. 357. It occurs in small discs or plates, of a grayish-black or greenish-black colour, with considerable metallic lustre, disseminated through a gangue which appears like a greenish argillaceous slate. These discs present no distinct form in the specimens I have examined, their edges being rounded, as in the case of the phyllite; but Desclozeaux has referred them to a hexagonal prism, or to an acute rhomboid deeply truncated by a plane perpendicular to the axis, or deeply compressed in that direction. He also obtained a cleavage parallel with that plane. Minute fragments are translucent, and show a greenish colour by transmitted light. Before the blowpipe, it fuses, alone, with difficulty, *on the edges*, into a black, magnetic globule. It dissolves slowly in borax, giving the reaction of iron, and with carbonate of soda, shows the presence of manganese.

Its constituents are as follows:—

		Oxygen.		Ratio.	Formulæ.
Silica........	43·34	22·51		4	
Alumina	24·63	11·50		2	$2\text{Al}\,\text{Si}+(\text{Fe}^3,\text{Mn}^3.)\,\text{Si}^2$
Protox. of iron	16·72	3·80	5·63		$+\text{Aq}.$
Protox. of manganese ..	8·18	1·83		1	$2\text{Al}^2\text{O}^3\,\text{SiO}^3+(\text{Fe}^3\,\text{MnO}^3.)$
Water	5·66	5·03		1	$2\text{SiO}^3+3\text{H}\,\text{O}^3$
	98·53				

Dr. Thomson's analysis affords a different formula, and, according to his method of determining the atomic proportions, phyllite is a simple silicate (the atoms of silica and bases being equal), consisting of nine atoms silicate of alumina, three atoms silicate of peroxide of iron, three atoms silicate of manganese, and one atom silicate of potash*. The occurrence of so large a proportion of potash in the mineral is not a little remarkable, and I would suggest whether it

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* Outlines of Mineralogy, &c., vol. i. p. 384. Dr. Thomson's atomic weights, founded upon the idea of Prout, that they are all multiples of the atomic weight of hydrogen vary somewhat from Berzelius's.

may not have been derived from the gangue of slate, from which it is difficult to obtain the mineral entirely free. Its infusibility before the blowpipe would seem to show this. It has been suggested, also, that a part of the iron may have been in the state of protoxide. It seems impossible, without some such supposition, that substances so closely resembling each other in all their physical characters, should differ so much in chemical composition. Now, if the potash be left out, and the peroxide of iron be changed into protoxide, the ratio between the atoms of acid and bases is nearly the same as in ottrelite, if we unite the atoms of magnesia and iron as isomorphous with each other. Ottrelite, also, is not easily separated from its matrix, but the larger size of its plates would seem to render it more easy to obtain pure specimens for analysis; and it is to be observed that Damour repeated his analysis, and obtained precisely the same result. It is remarkable that Rammelsberg has alphabetically inserted phyllite, but has given no formula for its constitution. It seems proper that the name of phyllite, on the ground of its priority, and because it expresses so well the ordinary appearance of the mineral, should stand, and that of ottrelite be abandoned*.

Dysluite identical with Automalite.

I am satisfied, from recent observations, that these two minerals, as they occur in New Jersey, should form but one species. The difference in hardness, colour, specific gravity and pyrognostic characters, can be accounted for by the well-established fact of the isomorphous replacement among the constituents of certain minerals which do not differ in crystalline form. In dysluite we have but thirty per cent. of alumina, the acting acid principle in the mineral, while in automalite we have sixty per cent. But the peroxide of iron, which is isomorphous with the alumina, amounts to nearly forty-two per cent. Now, if we suppose about thirty per cent. of this peroxide of iron to have replaced the same number of atoms of alumina in automalite, and the eight per cent. of protoxide of manganese to have replaced so much of the oxide of zinc, we make up very nearly the essential constituents as shown in the analyses of automalite by Ekeberg and Abich. It is to be observed that the latter chemist puts down the iron as *protoxide* in the Franklin automalite. If it should prove that the iron exists in dysluite in both states of oxidation, the twelve per cent. remaining out of the forty-two may be protoxide, replacing so much oxide of zinc. So that in this view of the case, the 17 per cent. oxide of zinc + 11 per cent. protoxide of iron + 7 per cent. protoxide of manganese = 35 per cent. oxide of zinc, which is nearly the exact quantity found by Abich in the crystals from Franklin. We may then state the constituents as follow:—

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[*] Brooke has supposed phyllite to be identical with gigantolite. If we compare the analysis of gigantolite with Damour's analysis above, the evidence of their identity (supposing ottrelite to be a purer variety of phyllite) is much more marked, and the ratio between the atoms of acid and bases is nearly the same in each.

		Oxygen.		Ratio.
Alumina	30·49	14·24	23·43	3
Peroxide of iron	30·00	9·19		
Protoxide of iron	11·93	2·72	7·76	1
Protoxide of manganese	7·60	1·70		
Oxide of zinc	16·80	3·34		

Here it is evident that the atoms of acid and bases are to each other as three to one, which is the case also with automalite, taking Abich's analysis, and grouping the isomorphous bases, thus:

		Oxygen.		Ratio.
Alumina	57·09	26·66		3
Oxide of zinc	34·80	6·92	8·72	1
Magnesia	2·22	·76		
Protoxide of iron	4·55	1·04		

Dr. Thomson, the only chemist who has analysed dysluite, reckons all the iron as peroxide, and as the principal basic constituent of the mineral, which, in his view, consists of the aluminates of iron, zinc and manganese. Rammelsberg, in stating the analysis, has given both oxides, and the atoms of alumina and peroxide of iron, as put down by him, are 22·80, and those of the isomorphous bases—protoxide of iron, protoxide of manganese and oxide of zinc—are 7·83 (7·89 ?); thus giving the same ratio as that above stated.

But other reasons may be urged why dysluite should be regarded only as a variety of automalite. I have seen specimens on which there were crystals well claiming the name of dysluite, as well as others equally entitled to the name of automalite; while there were yet others evidently passing from one into the other,—the bright and perfect crystals of automalite gradually losing their lustre, becoming porous, comparatively brittle and soft. I think if these circumstances had been attended to in the early history of the mineral, the name dysluite would long since have departed from the catalogue of mineral species.

Polyadelphite.

As Dana, in the new edition of his Mineralogy, has very properly included this mineral under the species garnet, I merely refer to it, to give further evidence of the correctness of his opinion from circumstances connected with its occurrence at the locality. It is evidently a granular, imperfectly crystallized yellow garnet, and the specimen which I received ten years ago from Prof. Nuttall, contains mechanical mixtures which it would be impossible to separate from it, so as to give us entire confidence in its analysis. To these, I believe, we may attribute its departure in composition from the common brown or yellow garnet, though it does not differ much from the brown garnet of Franklin, analysed both by Dr. Thomson and Mr. Seybert.

Beaumontite of Levy, and *Lincolnite* of Hitchcock.

In a paper read before the Boston Society of Natural History, and since published in their Transactions, and in the American Journal of Science (vol. xlvi. p. 235), I gave my reasons for classing these two minerals with heulandite. That beaumontite is heulandite, I believe is no longer doubted in this country or Europe. An analysis of the mineral by M. Delesse, has appeared since the publication of my paper*, and it agrees with all the other analyses of heulandite, excepting in the slight excess of silicic acid. In this respect it offers an example analogous to that of the variety of chabasite called acadiolite, in which the silicic acid forms a larger atomic proportion of the mineral, without causing any appreciable variation in the angles of the crystals. As to lincolnite, I must think that the various papers that have been called forth in relation to it since my first communication appeared, have established its indisputable identity with heulandite.

Peculiarities in the modifying planes† have given rise to a secondary form, rarely observed in heulandite. These consist in the enlargement of the planes *f* (Phillips), or ĕ (Dana), so as nearly to obliterate the primary planes *M*; being, in fact, the reverse of what we usually observe in heulandite from other localities. In the measurements by Prof. Hitchcock and Prof. Shepard, the angle of *f* on *T* was mistaken for that of *M* on *T*, and in the figure given by Prof. Hitchcock, it is evident that the planes lettered *M* should be *f*. The true value of *f* on *T* is 115° 10′ (Dana); Prof. Shepard's last measurements made it 116° 17′.

Ledererite.

I am compelled, at last, to declare my conviction that the specific nature of this mineral can no longer be maintained. Connell's analysis of an Irish gmelinite, which agrees with ledererite in all its physical and crystallographical characters, has shown also an identity in chemical composition. The phosphoric acid detected by Mr. Hayes must be viewed as an accidental constituent, varying probably in different crystals, or in some of them not existing at all. Some of the zeolites, in the Nova Scotia trap, have been found associated with small crystals of phosphate of lime, and it is not impossible that some of the minutest of these may have intercrystallized with the ledererite. We regret that we have not been able to obtain other specimens to enable Mr. Hayes to give it a re-examination. For comparison, I subjoin the analyses of ledererite and gmelinite.

	Ledererite.		Gmelinite.
Silica	49·47		48·56
Alumina	21·48		18·05
Lime	11·48		6·13
Soda	3·94		3·85
Phosphoric acid	3·48	Potash	0·39
Protoxide of iron	0·14		0·11
Water	8·58		21·66
	98·56, Hayes.		98·75, Connell.

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[*] *Ann. de Chim. et de Phys.* for 1843, t. ix. p. 395. Phillips's Min. p. 627.

[†] For the figures see Amer. Journ. of Science, vol. xlvi. p. 234, and vol. xlvii. p. 416. Corroborative evidence of the correctness of my opinions

Now, if the phosphoric acid in ledererite is united with lime as an accidental mixture, 2½ per cent. of the lime should be taken from the 11·48 per cent. found in the mineral: this brings the proportion down nearly to that obtained by Connell. Mr. Hayes was not able to determine the weight of the water with accuracy, owing to the small quantity of the mineral operated upon. As the loss (1·44 per cent.) was mostly water, we may suppose, with Rammelsberg, that ledererite is gmelinite containing ⅓ (½?) its quantity of water. The chemical formula for gmelinite and chabasite is thus:

$$(CaO, NaO, 3KO)2\,SiO^3 + 3Al^2O^3\,2SiO^3 + 18HO^*.$$

Excepting the absence of striæ, and the shorter dimensions of the prismatic planes of its crystals, the Irish gmelinite precisely resembles ledererite; their hardness, lustre, colour and blowpipe characters are the same. The appearance of hexahedral cleavage, on which Dr. Jackson originally founded the chief claim of the latter to the character of a new species, was only imperfectly produced by heating the crystals, and not by ordinary mechanical cleavage. This could not be effected, the mineral breaking in all directions with a vitreous fracture. Dr. Jackson agrees with me that it can no longer be retained as a distinct species.

While preparing my edition of Phillips's Mineralogy, I requested Mr. Hayes and Dr. Jackson to make several analyses for me with particular reference to that work. As some of these have not appeared in any other form, I wish now to make a permanent record of them, in order that they may be seen where they might not otherwise reach. The first are of the Nova Scotia chabasite (acadiolite), which Hoffmann has distinguished from common chabasite, by its containing three per cent. more silica, and for which Rammelsberg has given a formula differing somewhat from that of chabasite. (See first part of this article.)

Silica......	52·02	52·20
Alumina ..	17·88	18·27
Lime......	4·24	6·58
Potash	3·03	2·12
Soda......	4·07	
Water	18·30	20·52
	99·60, Hayes.	99·69, Hayes.

These results agree with those obtained by Hoffmann[†] in his analysis of the same mineral, the specimens of which were presented to him by Charles Cramer, Esq. of St. Petersburg.

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by the editors of the Amer. Journ. of Science, may be seen at the pages here referred to.

[*] *Handwörterbuch*, i. 150. Rammelsberg unites chabasite and gmelinite, the first as soda chabasite, the last as lime chabasite. This is in accordance with Tamnau, who has established their identity on crystallographical grounds. The close relation of the two minerals was, however, first shown by Prof. Mohs. See his Mineralogy, vol. ii. p. 105.

[†] Amer. Journ. of Science, vol. xxx. p. 366.

Washingtonite of Shepard, analysed by Mr. J. S. Kendall under the direction of Dr. Jackson, gave these results:—

		Oxygen.	Ratio.
Titanic acid........	25·28	4·82	1
Peroxide of iron	51·84	10·36	2
Protoxide of iron ..	22·86	5·08	1
	99·98		

The atomic proportions are thus nearly one atom titanic acid, two atoms peroxide of iron, one atom protoxide of iron; or, a trititaniate of iron, consisting of two atoms trititaniated peroxide and one atom trititaniated protoxide. If we unite the magnesia and lime with protoxide of iron in the following analysis of an ilmenite from Arendal[*], by Mosander, we obtain precisely the same result. The crystalline form of the two varieties is also the same, and there can be no doubt of their identity as one species[†].

Titanic acid................		24·19
Peroxide of iron		53·01
Protoxide of iron	19·91	20·92
Magnesia and lime ..	1·01	

By referring to the analyses of ilmenite from other localities, it will be seen that the essential constituents, titanic acid and the two oxides of iron, so interchange with each as to produce different varieties, but all having the same crystalline form.

NOTICE ON CERTAIN IMPURITIES IN COMMERCIAL SULPHATE OF COPPER. BY MR. S. PIESSE.

One source of the sulphate of copper of commerce is the treatment of brass and German silver articles technically called dipping, which consists in plunging them for a short time into a mixture of nitric and sulphuric acids, an operation which removes the coat of oxide from the surface of the metal, and leaves the latter in a clean state proper for the reception of varnish or other finishing. In time this dipping liquid becomes in great measure saturated, and after neutralization with old copper yields on evaporation in leaden pans a large quantity of sulphate of copper in crystals. According to the author not less than 100 tons of dipping liquid are thus disposed of annually at Birmingham by the makers of buttons and other arti-

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[*] The hystatite of Breithaupt.

[†] An acute rhombohedron, P on P 86° 10′, for the ilmenite. Shepard, employing varnished planes of the washingtonite, makes P on P 86°. Prof. Shepard founds the distinction on other than crystallographical characters; for, he says, it is not thus "shown to be distinct, in any essential manner, from the axotomous iron ore of Mohs, or from crichtonite (including ilmenite): indeed, it appears most probable that all these minerals are not only identical in their angles, but are isomorphous with specular iron."—Amer. Journ. vol. xliii. p. 365. The analysis, now, would seem to destroy the groundwork for any distinction.

cles. The crystallized sulphate of copper so obtained is often largely contaminated with sulphate of zinc, which may sometimes be seen in the form of slender white needles on the surface of the dark blue crystals, and in some of the applications of this salt may prove injurious. Sulphate of nickel, sulphate of lead, arsenic, and chlorides are also sometimes present.—*From the Proceedings of the Chemical Society.*

ON A NEW EUDIOMETRIC PROCESS. BY PROF. GRAHAM.

Professor Graham described a new eudiometric process for the rapid absorption of oxygen gas from atmospheric air and other gaseous mixtures containing oxygen. It consists in the employment of a solution in ammonia of a sulphite of the suboxide of copper and ammonia. This salt falls as a granular powder, when a stream of sulphurous acid gas is conveyed into a cold solution of the ammoniacal sulphate of copper. When dissolved in ammonia it absorbs oxygen with singular avidity, and when employed in this form in eudiometry gives results of considerable uniformity.—*From the Proceedings of the Chemical Society.*

EQUIVALENT OF CHLORINE.

M. Gerhardt observes, that M. Marignac has made some observations and experiments tending to show that the atomic weight of chlorine is not thirty-six times that of hydrogen, as he (M. Gerhardt) had concluded, but somewhat less. M. Marignac's conclusions are derived from the weight of chloride of potassium yielded by the calcination of chlorate of potash; to these results M. Gerhardt makes the following objections:—

It is the residue of chloride of potassium obtained, and not that of the oxygen gas, which is weighed; and from the following causes the quantity of chloride might be too small, and would diminish the atomic weight of chlorine:—a trace of moisture on the salt; a portion of chlorate or chloride carried off by the current of oxygen gas; if the oxygen were impure, and contained, as M. Marignac has stated, a trace of chlorine.

Thus, observes M. Gerhardt, all the errors which can be committed in these determinations are referrible to the chlorine, and give it in too small quantity.

From his experiments M. Gerhardt concludes, in opposition to those of M. Marignac, that the equivalents of chlorine, silver and potassium, are exact multiples of the equivalent of hydrogen, that is to say,—

Chlorine	36
Silver	108
Potassium	39

Journ. de Pharm. et de Ch., Avril 1846.

ON HIPPURIC ACID, BENZOIC ACID, AND THE SUGAR OF GELATINE.

M. Dessaignes remarks, that hippuric acid has already been the subject of numerous researches; its metamorphoses are nevertheless so interesting as to leave something for those to glean who will study it. M. Liebig has shown, that when it is dissolved in boiling hydrochloric acid, it crystallizes on cooling without having been altered; but if the ebullition be prolonged for about half an hour, it is decomposed, and yields, according to M. Dessaignes, benzoic acid equal nearly in quantity to that indicated by theory. The benzoic acid was separated by the filter, and the filtered liquor gave by evaporation long, acid, nitrogenous prismatic crystals, into the composition of which hydrochloric acid entered as a constituent part. These crystals were neutralized by carbonate of soda or carbonate of lead; and after getting rid of the solution of chloride of sodium or chloride of lead, fresh crystals of a very saccharine and azotized matter were obtained; these were neutral to reagents, and formed crystalline compounds with oxide of silver, and with nitric, sulphuric, and oxalic acids.

M. Dessaignes soon found out that he had thus produced, by a metamorphosis which might have been foreseen, the sugar of gelatine discovered by M. Braconnot.

Reckoning $C=150$, $H=6{\cdot}25$, and $N=17{\cdot}5$, if from

$$\begin{array}{ll} & C^{18}\,H^{18}\,N^{2}\,O^{6} \\ \text{we subtract} & C^{14}\,H^{12}\quad\; O^{4} \\ \hline \text{we obtain} & C^{4}\,\;H^{6}\,\;N^{2}\,O^{2} \end{array}$$

to which it is sufficient to add $1\frac{1}{2}$ equivalent of water to obtain sugar of gelatine. M. Dessaignes is inclined to the opinion, that 2 equivalents of water should be added, and that the true equivalent of sugar of gelatine is $C^{4}\,H^{10}\,N^{2}\,O^{4}$, as already indicated by M. Gerhardt.

All the reactions and beautiful crystallizations which M. Dessaignes obtained with the saccharine azotized matter from hippuric acid, convinced him of the identity of this substance with the sugar of gelatine obtained from isinglass; but in order to convince chemists of this fact, he thinks it requisite to analyse the sugar of hippuric acid. The metamorphosis which gives rise to this body is very distinct; no gas is evolved during the reaction; the only two products are benzoic acid and hydrochlorate of sugar. From 100 of dry hippuric acid M. Dessaignes obtained,—

Benzoic acid (dry)	67·49
Hydrochlorate of sugar (dried over sulphuric acid)	59·08
	126·57

Nitric acid boiled for twenty minutes with hippuric acid, converts it into benzoic acid, and nitro-saccharic acid, which crystallizes in magnificent truncated tables. Nitro-saccharic acid, prepared with the sugar of isinglass, yielded precisely similar crystals.

Sulphuric acid diluted with twice its volume of water also effects the metamorphosis of hippuric acid without the disengagement of gas, and without colouring the solution. The benzoic acid obtained is very easily purified, and also a compound, from which, by means of chalk or carbonate of lead, sugar of gelatine may be procured.

M. Dessaignes combined, equivalent to equivalent, sulphuric acid $SO^3 H^2 O$, and the sugar obtained from hippuric acid, giving as the formula of the latter $C^4 H^{10} N^2 O^4$; and he obtained a solution which crystallized in large prisms of great splendour, to the last drop.

A very concentrated solution of oxalic acid boiled for two hours with hippuric acid, converts it into benzoic acid and oxalate of sugar, which crystallizes in fine prisms. Lastly, an excess of potash or soda, boiled for half an hour with hippuric acid, converts it into alkaline benzoate and sugar, which was obtained in the form of hydrochlorate, after having treated the mixture of benzoate and sugar with hydrochloric acid.—*Ann. de Ch. et de Phys.*, Mai 1846.

COMPARATIVE ANALYSES OF ORIENTAL JADE AND TREMOLITE.

BY M. DAMOUR.

The jade selected for analysis had been worked in India; it was of a milk-white colour and semi-transparent, and had the appearance of white wax, or perhaps rather of spermaceti. Its fracture is splintery; it scratches glass, but feebly. Its specific gravity was found to be 2·970. Its tenacity is very great; when reduced to powder and heated in a glass tube, its appearance was not altered, and it yielded no water. In the flame of the blowpipe it swells up, and fuses slowly into a milk-white enamel. Borax dissolves it without colour; the salt of phosphorus dissolves it, leaving a skeleton of silica. It is not sensibly acted upon by hydrochloric acid.

Two analyses gave the following results:—

Silica	58·46	58·02
Lime.............	12·06	11·82
Magnesia	27·09	27·19
Protoxide of iron ..	1·15	1·12
	98·76	98·15

M. Damour having observed that this is precisely the composition of tremolite (white amphibole), submitted this substance to the same process of analysis as that adopted with the jade. The specimen which he selected was from St. Gothard, and in colourless crystals, very perfect and associated with granular dolomite, which was separated by hydrochloric acid previously to analysis.

It yielded,—

Silica............	58·07
Lime	12·99
Magnesia	24·46
Protoxide of iron ..	1·82
	97·34

From the similarity of these results, M. Damour is of opinion that this jade may be ranked with tremolite; and if this opinion should be adopted, he observes, that in collections oriental jade will hereafter be classed as *compact tremolite*. — *Ann. de Ch. et de Phys.*, Avril 1846.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1846.

Chiswick.—April 1. Fine. 2. Cloudy: showery. 3. Clear and windy: cloudy and fine. 4. Hazy: heavy rain. 5. Heavy rain: clear. 6. Heavy rain: cloudy. 7. Slight rain: densely overcast. 8—10 Fine. 11. Dry haze. 12, 13. Cloudy and fine. 14. Clear: dry haze: overcast. 15. Densely clouded: dry haze: densely overcast. 16. Slight dry haze. 17. Foggy. 18. Rain. 19. Cloudy and cold: clear. 20. Showery: frosty at night. 21. Foggy: cloudy and fine. 22. Foggy. 23. Heavy clouds: rain. 24. Rain: dark haze: cloudy. 25. Hazy and damp: showery: hazy: foggy. 26. Extraordinary fall of rain early A.M.: dense clouds: overcast at night. 27. Clear and fine. 28. Very fine. 29, 30. Clear: very fine: overcast.

Mean temperature of the month	47°·36
Mean temperature of April 1845	48 ·41
Average mean temperature of April for the last twenty years	47 ·19
Average amount of rain in April	1 ·47 inch.

Boston.—April 1. Fine. 2. Rain. 3 Windy: rain P.M. 4. Cloudy: rain P.M. 5. Cloudy: rain A.M. and P.M. 6. Cloudy. 7. Rain. 8. Cloudy: rain early A.M. 9, 10. Fine. 11. Cloudy: rain P.M. 12. Fine. 13. Fine: rain early A.M. 14. Fine: rain P.M. 15. Cloudy: rain early A.M. 16—18. Cloudy. 19. Cloudy: rain A.M. 20—22. Fine. 23. Fine: rain A.M. 24, 25. Fine. 26. Cloudy: rain A.M. 27. Fine: rain A.M. 28. Cloudy. 29. Fine: ice this morning. 30. Cloudy.

Sandwick Manse, Orkney.—April 1. Snow: clear. 2. Showers: clear. 3. Snow-showers. 4. Snow-showers: frost: snow showers. 5. Showers. 6. Showers: clear: aurora. 7. Clear: drops. 8. Cloudy: clear. 9. Bright: cloudy. 10. Bright: showers. 11. Bright: rain. 12. Fog: damp. 13. Damp: drizzle. 14. Clear. 15. Fog: cloudy. 16. Cloudy. 17. Cloudy: damp: fog. 18. Rain: clear. 19—21. Fine: clear. 22. Clear: cloudy. 23, 24. Clear. 25. Cloudy. 26. Sleet showers: hail-showers. 27. Bright: cloudy. 28. Hail-showers: cloudy. 29. Snow-showers: clear. 30. Cloudy: clear.

Applegarth Manse, Dumfries-shire.—April 1. Wet. 2. Wet A.M.: cleared and fine. 3. Wet A.M.: cleared. 4. Slight showers: frost A.M. 5. Fair, but chilly. 6. Fair, but very bleak. 7. Fair. 8. Fair: frost A.M. 9. Fair: frost: fine. 10. Fine. 11. Rain all day. 12. Rain P.M.: thunder. 13. Frequent heavy showers. 14. Frequent heavy showers: hail: fine P.M. 15. Frequent heavy showers: rain all day. 16. Very fine spring day. 17, 18. Dropping day. 19. Fair, though chilly. 20. Frost, slight: fine. 21. Hoar-frost: rain P.M. 22. Slight showers. 23, 24. One slight shower. 25. Heavy shower: fair P.M. 26. Slight shower: fine. 27. Slight shower: frost A.M. 28. Frost A.M.: fine. 29. Frost A.M.: a slight shower. 30. A dropping day.

Mean temperature of the month	45°·6
Mean temperature of April 1845	48 ·2
Mean temperature of April for twenty-three years	44 ·2
Mean rain in April for eighteen years	1 ·69 inch.

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

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. April.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	29·591	29·445	29·10	29·19	29·10	29·36	29·30	62	45	52	48	41½	38	36½	sw.	calm	sse.	ne.	·03	......	......	·09
2.	29·284	29·122	28·89	29·08	28·93	29·26	29·30	59	40	49	58	43	39½	38	sw.	calm	ne.	ne.	·06	......	......	·12
☽ 3.	29·725	29·486	28·80	29·00	29·37	29·35	29·50	55	39	48	49	41½	40	36½	w.	w.	ne.	n.	·06	·38	......	......
4.	29·916	29·250	29·24	29·43	29·24	29·51	29·49	53	40	40	44½	31½	38½	35	se.	calm	ene.	ne.	·46	·08	......	·15
5.	29·222	29·142	28·90	29·21	29·17	29·43	29·40	55	40	43	44½	35	42	41	sw.	calm	ne.	nne.	·45	·66	......	·31
6.	29·058	28·984	28·69	29·14	29·20	29·46	29·41	51	34	47	47	36½	43½	36	sw.	calm	ne.	calm	·30	·12	......	·18
7.	29·136	29·060	28·75	29·22	29·22	29·38	29·45	48	40	43	47	32	42½	41	w.	n.	nne.	e.	·12	·76	......	·02
8.	29·411	29·198	28·89	29·20	29·37	29·50	29·58	56	31	46·5	52	39	44½	40½	w.	calm	nne.	calm	......	·13	0·57	......
9.	29·799	29·645	29·26	29·51	29·57	29·62	29·62	57	29	44	54	32½	44	41½	w.	calm	w.	calm	......	......	......	·18
10.	29·835	29·736	29·40	29·60	29·57	29·60	29·63	60	39	50	54	36	43½	42	sw.	calm	wsw.	w.	......	......	......	......
○ 11.	29·542	29·337	29·22	29·36	29·16	29·54	29·40	61	36	49	52	37½	46	44	s.	s.	e.	e.	·06	......	......	·02
12.	29·507	29·370	28·96	29·16	29·28	29·29	29·36	65	45	55	59	47	46	45½	sw.	calm	e.	se.	·09	·13	......	·18
13.	29·758	29·509	29·05	29·25	29·44	29·46	29·44	61	34	57	57	47	50	46	sw.	sw.	e.	se.	·02	·14	......	......
14.	29·715	29·565	29·32	29·60	29·63	29·65	29·83	61	42	55	60	46½	51	45	ne.	calm	se.	se.	·10	......	......	·14
15.	29·786	29·590	29·21	29·68	29·86	29·95	30·09	57	49	53	53	46	46	45	e.	calm	se.	se.	·05	·52	0·72	......
16.	30·027	29·939	29·57	29·95	29·89	30·05	29·95	62	44	50·5	60	46	46	46	e.	calm	se.	s.	......	......	......	......
17.	29·952	29·883	29·50	29·83	29·84	29·94	29·99	57	44	53	53	47	46	45	sw.	calm	nne.	se.	·09	......	......	......
☾ 18.	30·003	29·893	29·55	29·91	29·98	30·09	30·20	53	43	48	52½	44	46	42	n.	calm	n.	se.	·02	......	......	·25
19.	30·127	30·019	29·74	30·12	30·15	30·25	30·28	53	34	43	51	41	45½	42	n.	calm	ne.	calm	......	......	......	·07
20.	30·108	30·072	29·74	30·13	30·01	30·25	30·15	54	27	47	55	33½	46	41	ne.	calm	ne.	ese.	·03	·11	......	......
21.	30·049	29·955	29·61	29·95	29·87	30·07	30·08	57	30	47	55	35½	46	41	ne.	calm	e.	e.	......	......	......	......
22.	29·873	29·840	29·52	29·87	29·90	30·17	30·16	57	37	51·5	52	40	47	45	e.	calm	e.	ne.	......	......	......	......
23.	29·815	29·773	29·46	29·90	29·81	30·16	30·15	53	42	49	52½	37½	47	42	ne.	calm	e.	ene.	·27	......	0·57	......
24.	29·865	29·746	29·40	29·82	29·80	30·19	30·19	57	34	53	50	40	50½	43	ne.	calm	ne.	ene.	·05	·07	......	......
● 25.	29·901	29·784	29·53	29·88	29·85	30·15	30·08	60	42	51	56	38½	47	40½	e.	calm	ese.	nne.	1·40	......	......	......
26.	29·818	29·762	29·40	29·82	29·84	29·98	29·92	53	30	47	51	40½	42	38½	n.	nw.	ne.	nne.	·04	......	......	......
27.	29·909	29·885	29·47	29·81	29·76	29·82	29·81	54	36	43	49	32½	42	38½	n.	nw.	w.	n.	·03	·04	......	·08
28.	29·986	29·867	29·50	29·79	29·94	29·85	29·97	61	27	48	52½	32	40	39	w.	calm	nw.	nw.	......	·09	......	......
29.	30·232	30·193	29·78	30·04	30·07	30·01	30·16	60	33	47	54	32	38	35½	e.	calm	w.	nw.	......	......	......	·14
30.	30·298	30·283	29·87	30·15	30·10	30·20	30·20	61	44	49	53	39	42½	36	w.	calm	sw.	ese.	......	......	0·08	·02

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SIR ROBERT KANE, M.D. M.R.I.A.

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

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

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THE
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[THIRD SERIES.]

JULY 1846.

I. *On the Satellitary Nature of Shooting Stars and Aërolites.*
By H. E. Strickland, *M.A., F.G.S.*[*]

IN the truly philosophic work Cosmos, in which the profound Humboldt embodies the results of his life-long studies, he expresses some opinions on the subject of *shooting stars*, which there appears to me considerable difficulty in adopting. If we assume with him that the observations of Benzenberg and Brandes on the parallax of shooting stars are correct, it appears that these bodies have a velocity of from 17 to 36 geographical miles per second, that their elevation above the earth is from 16 to 140 geographical miles, and their diameters from 80 to 2600 feet. It may further be taken for granted, that these bodies revolve in orbits according to the laws of gravitation, that they are ordinarily invisible, but become momentarily luminous whenever they plunge into the earth's atmosphere; and that aërolites are fragments projected or swept from these asteroids (possibly by the resistance of the atmosphere), and hurled to the earth by terrestrial attraction.

Admitting these premises, the next question is to determine the nature of the orbits in which these mysterious bodies revolve, and the influences to which they are subjected in their course. Humboldt here adopts the opinion first propounded by Chladni, that shooting stars and meteors are planetary bodies revolving round the sun in elliptic orbits, and only rendered visible to us at the nodes, where the orbits of the earth and of these asteroids intersect. Their number on this view of the subject must be prodigious, as it is only those whose orbits happen to traverse the earth's orbit, and which

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[*] Read before the Ashmolean Society of Oxford, March 1846, and communicated by the Author.

happen to pass at the moment when the earth is crossing their node, which can ever be visible to us. How then, let me ask, is it brought about that these innumerable planetary bodies, which are so continually entering our atmosphere and passing within a few miles of the earth, never come in contact with it? for be it remembered that aërolites are not regarded as being the shooting stars themselves, but only as fragments left behind them in their course. Can we suppose that our earth, a body of nearly 8000 miles diameter, should be incessantly forcing its way through showers of these planetary bodies, hundreds of which daily approach in their circumsolar revolutions within from 16 to 140 miles of the earth, and yet that they should never impinge upon its surface? Should we not in that case continually hear of these fiery masses, with diameters from 80 to 2600 feet, and velocities of 36 miles a second, dashing into the body of our earth like cannon-balls into an earthen rampart? If, in order to meet this objection, it be asserted that the real diameter of the shooting stars has been over-estimated, and that aërolites are not fragments of, but are identical with these bodies themselves, which accordingly really do fall upon the earth's surface, still on the doctrine of chances it would follow that the earth's disc, which presents a far larger surface than that portion of its atmosphere which surrounds and projects beyond its limb, must receive a proportionally larger number of these projectiles.

The attraction of the earth would still further increase the amount of those asteroids which would come in contact with it, as compared with those which pass through and escape from the atmospheric stratum; yet, what is the real proportion between the two classes of phænomena? We find in reality that shooting stars, that is, asteroids rendered visible by atmospheric contact, occur to the amount of scores, sometimes of hundreds, every night, while the fall of aërolites upon the earth's surface is a phænomenon of very much rarer occurrence. It seems evident therefore that there is some cause which renders the circulation of asteroids in orbits approximately parallel to the earth's surface, the normal condition, to which the fall of aërolites to the ground (whether we regard them as being the entire *nuclei*, or merely detached fragments of these meteors) forms only a casual exception.

To what then must we attribute this constant flight of asteroids in lines closely approximating, yet not impinging upon, the earth's surface? It seems evident that we cannot regard them as solar *planets*, pursuing their course through the system regardless of intervening obstacles, as they must inevitably in that case come into very frequent contact with our earth. Why then may we not suppose them to be *satellites*, revolving rapidly round the earth in orbits more or less eccentric, and occasionally plunging into the upper regions of the atmosphere? It does not follow because these bodies move with "planetary velocity," that they must therefore be planets. The satellites nearest to the bodies of Jupiter and Saturn revolve round those planets with a velocity of about ten miles per second, which is not very greatly inferior to that assigned to some shooting stars; and as the velocities of satellites increase with their proximities, we may well suppose that satellites revolving within 150 miles of their primary would have very high velocities. The alleged velocities of shooting stars accord sufficiently well (allowing for the perturbations to which the proximity of the earth may give rise) with Kepler's law, that the squares of the times are proportionate to the cubes of the distances. By applying this law to the known velocity of the moon, it results that a satellite revolving round our earth at 5000 miles from the centre, or about 1000 miles from the surface, would have a velocity of about 40 miles per second, which is even greater than that hitherto assigned to shooting stars.

We may surely then conceive these bodies to be of the nature of satellites, having all their elements so adjusted as to ensure a perpetual revolution round the earth, into whose atmosphere they occasionally dip and undergo a momentary ignition.

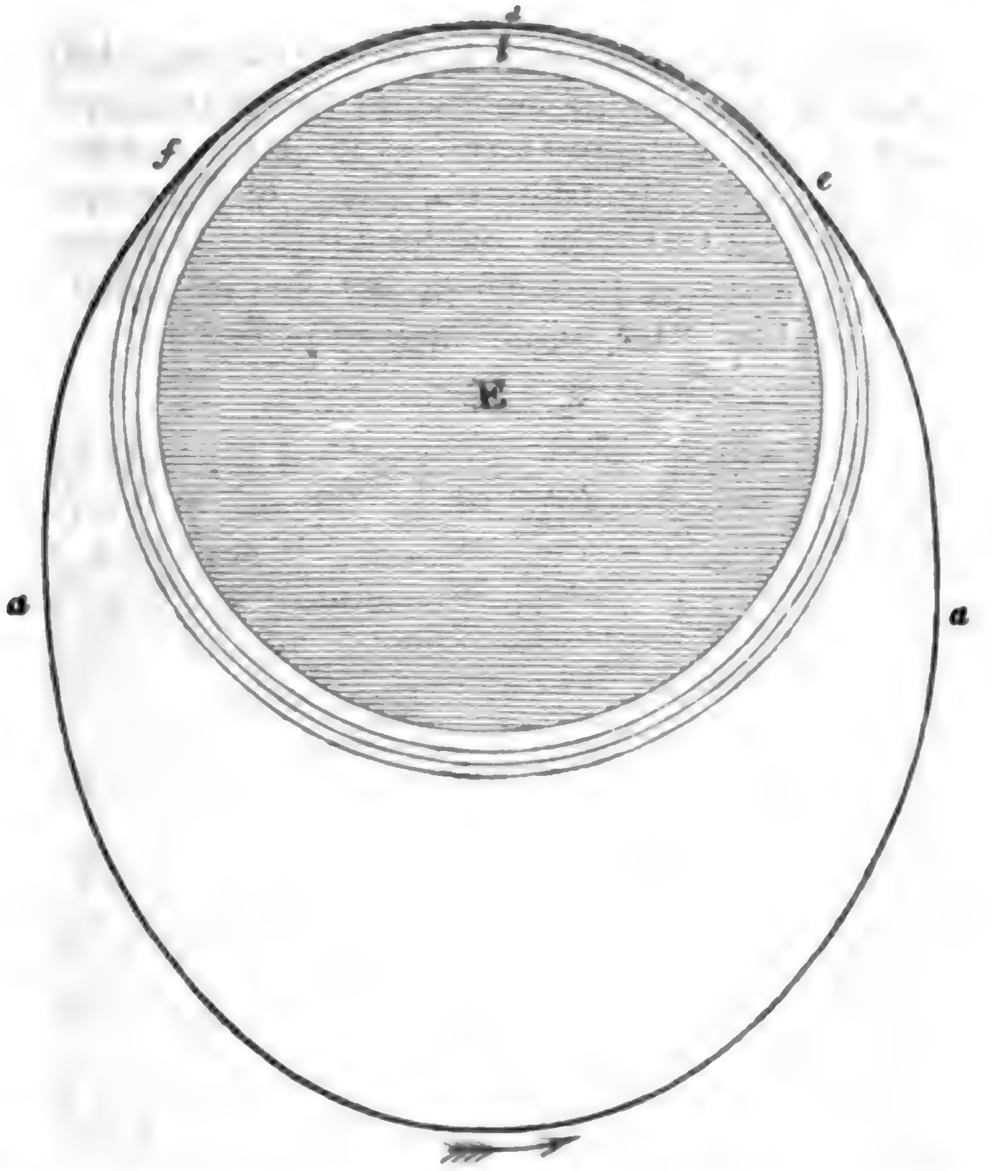
It appears moreover difficult to conceive, that if the motion of meteors is of a planetary nature, such small bodies could pass within a few miles of the earth, and then proceed on their course round the sun, comparatively uninfluenced by the terrestrial attraction. The perturbation produced by the earth's mass on a planet of only a few hundred feet in diameter passing within 100 or 150 miles' distance, would surely be so enormous as wholly to destroy the original orbit of the minor body, and the most probable effect would be to convert it into a satellite and to retain it permanently within the earth's attraction. So that even admitting that these asteroids may have once been in the condition of planets, and that many such bodies may still, unknown to us, be revolving in circumsolar orbits, we must yet regard all the shooting stars which ordinarily make their appearance within our atmosphere, as being at present in the condition of satellites.

The main objection, and it is certainly a very important one, to the satellitary theory of shooting stars, is founded on the fact of the nearly (though not quite regular) periodical recurrence of an increased number of these meteors at certain annual epochs. This has been explained by supposing that the earth at these periods intersects certain zones or orbital rings, in which vast numbers of asteroids are constantly revolving round the sun. On the supposition that these bodies are satellitary, and not planetary, it is certainly difficult to account for the fact of their becoming visible in greater numbers at one season of the year than at another. It is however conceivable that the luminosity of shooting stars may be caused by their coming in contact, not with our gaseous atmosphere, but with an electrical atmosphere, which may extend far beyond the limits of the gaseous one; and it may be further conjectured, that from unknown cosmical causes this electric atmosphere may at certain points in the earth's orbit receive quantitative or qualitative modifications, which may enable it at those seasons to illuminate a larger numerical proportion of the meteoric satellites.

At these annual epochs the showers of meteors are said to have apparently proceeded during several hours of observation from the same point in the heavens, viz. the constellation Leo.

But this alleged fact seems irreconcilable with either the planetary or the satellitary theory of shooting stars. Even admitting an approximate constancy in the directions in which these bodies approach our atmosphere, yet as their distance when rendered visible is considered to be not greater than about 150 miles, it is evident that their parallax (whether viewed simultaneously by two distant observers, or at successive intervals of a few hours by the same observer) would be so great as to destroy the appearance of perfect uniformity in the point of the starry heavens, where they make their first appearance. We must therefore suppose that the amount of this uniformity has been overstated; still it is possible that there may be a prevailing direction in which the majority of these bodies enter the atmosphere, and the predominance of this direction may still be in some degree apparent, notwithstanding the influence of parallax. Such a predominance of direction (if it really exist) does not however necessarily prove the shooting stars to be planets, but may be equally explained on the satellitary theory in the following manner:—

Let *a a* be an elliptical ring composed of great numbers of these satellitary bodies revolving in parallel curves round E, the earth; let *b* be the limit of the gaseous, and *c* that of the electric atmosphere in its normal condition. It is evident that while this condition lasts, the meteors in the ring *a a* will be wholly external to the electric atmosphere *c*, and will be consequently invisible. The only meteors which would be seen in these circumstances, would be such stray ones as may revolve in minor orbits occasionally intersected by the circle *c*. But let the electrical atmosphere from some annually recurring cause be temporarily extended to *d*, then while this condition lasts, the meteors revolving in the ring *a a* would be rendered visible during their course from *e* to *f*, and (the ring *a a* remaining always parallel to itself) the meteors would appear (allowing for the effects of parallax) to proceed from nearly the same points of the heavens.



This explanation must however be regarded only as a rude conjecture to remove an apparent difficulty; and as this difficulty, if a real one, may be equally explained on the planetary and satellitary theory of meteors, it need not prevent us from giving the preference to the latter, if the arguments which I have adduced in its favour are of any weight.

II. *On the Constitution of the Luminiferous Æther, viewed with reference to the phænomenon of the Aberration of Light.* By G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge**.

IN a former communication to this Magazine (July 1845), I showed that the phænomenon of aberration might be explained on the undulatory theory of light, without making the startling supposition that the earth in its motion round the sun offers no resistance to the æther. It appeared that the phænomenon was fully accounted for, provided we supposed the motion of the æther such as to make

$$u\,dx + v\,dy + w\,dz \quad . \quad . \quad . \quad . \quad . \quad (a.)$$

an exact differential, where u, v, w are the resolved parts, along three rectangular axes, of the velocity of the particle of æther whose co-ordinates are x, y, z. It appeared moreover that it was necessary to make this supposition in order to account in this way for the phænomenon of aberration. I did not in that paper enter into any speculations as to the physical causes in consequence of which (a.) might be an exact differential. The object of the present communication is to consider this question.

The inquiry naturally divides itself into two parts:—*First*, In what manner does one portion of æther act on another beyond the limits of the earth's atmosphere? *Secondly*, What takes place in consequence of the mutual action of the air and the æther?

In order to separate these two questions, let us first conceive the earth to be destitute of an atmosphere. Before considering the motion of the earth and the æther, let us take the case of a solid moving in an ordinary incompressible fluid, which may be supposed to be infinitely extended in all directions about the solid. If we suppose the solid and fluid to be at first at rest, and the solid to be then moved in any manner, it follows from the three first integrals of the ordinary equations of fluid motion, obtained by M. Cauchy, that the motion of the fluid at any time will be such that (a.) is an exact differential. From this it may be easily proved, that if at any instant the solid be reduced to rest, the whole of the fluid will be reduced to rest likewise; and that the motion of the fluid is the same as it would have been if the solid had received by direct impact the motion which it has at that instant. Practically however the motion of the fluid after some time would differ widely from what would be thus obtained, at least if the

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* Communicated by the Author.

motion of the solid be progressive and not oscillatory. This appears to be due to two causes; first, the motion considered would probably be unstable in the part of the fluid behind the solid; and secondly, a tangential force is called into play by the sliding of one portion of fluid along another; and this force is altogether neglected in the common equations of hydrodynamics, from which equations the motion considered is deduced. If, instead of supposing the solid to move continuously, we supposed it first to be in motion for a very small interval of time, then to be at rest for another equal interval, then to be in motion for a third interval equal to the former, and so on alternately, theoretically the fluid ought to be at rest at the expiration of the first, third, &c. intervals, but practically a very slight motion would remain at the end of the first interval, would last through the second and third, and would be combined with a slight motion of the same kind, which would have been left at the end of the third interval, even if the fluid immediately before the commencement of it had been at rest; and the accumulation of these small motions would soon become sensible.

Let us now return to the æther. We know that the transversal vibrations constituting light are propagated with a velocity about 10,000 times as great as the velocity of the earth; and Mr. Green has shown that the velocity of propagation of normal vibrations is in all probability incomparably greater than that of transversal vibrations (Cambridge Philosophical Transactions, vol. vii. p. 2). Consequently, in considering the motion of the æther due to the motion of the earth, we may regard the æther as perfectly incompressible. To explain dynamically the phænomena of light, it seems necessary to suppose the motion of the æther subject to the same laws as the motion of an elastic solid. If the views which I have explained at the end of a paper On the Friction of Fluids, &c. (Cambridge Philosophical Transactions, vol. viii. part 3) be correct, it is only for extremely small vibratory motions that this is the case, while if the motion be progressive, or not very small, the æther will behave like an ordinary fluid. According to these views, therefore, the earth will set the æther in motion in the same way as a solid would set an ordinary incompressible fluid in motion.

Instead of supposing the earth to move continuously, let us first suppose it to move discontinuously, in the same manner as the solid considered above, the æther being at rest just before the commencement of the first small interval of time. By what precedes, the æther will move during the first interval in the same, or nearly the same, manner as an incompressible fluid would; and when, at the end of this interval, the earth is reduced to rest, the whole of the æther will be reduced to rest, except as regards an extremely small motion, of the same nature as that already considered in the case of an ordinary fluid. But in the present case this small motion will be propagated into space with the velocity of light; so that just before the commencement of the third interval the æther may be considered as at rest, and everything will be the same as before. Supposing now the number of intervals of time to be indefinitely increased, and their magnitude indefinitely diminished, we pass to the case in which the earth is supposed to move continuously.

It appears then, from these views of the constitution of the æther, that (*a.*) must be an exact differential, if it be not prevented from being so by the action of the air on the æther. We know too little about the mutual action of the æther and material particles to enable us to draw any very probable conclusion respecting this matter; I would merely hazard the following conjecture. Conceive a portion of the æther to be filled with a great number of solid bodies, placed at intervals, and suppose these bodies to move with a velocity which is very small compared with the velocity of light, then the motion of the æther between the bodies will still be such that (*a.*) is an exact differential. But if these bodies are sufficiently close and numerous, they must impress either the whole, or a considerable portion of their own velocity on the æther between them. Now the molecules of air may act the part of these solid bodies. It may thus come to pass that (*a.*) is an exact differential, and yet the æther close to the surface of the earth is at rest relatively to the earth. The latter of these conditions is however not necessary for the explanation of aberration.

There is one curious consequence of the theory contained in my paper of last July, which I did not at the time observe. On referring to this paper, it will be seen that if the motion of the æther is such that (*a.*) is an exact differential, the change in the direction of the normal to a wave of light, as the wave passes from a part of space where the disturbance of the æther due to the motion of the earth is insensible to another part where the disturbance is sensible, is given by the equation

$$\alpha_2 - \alpha_1 = \frac{u_2}{V},$$

which is what (6.) becomes when u_1 and v_1 are each put equal to zero; and the plane passing through the direction of the light and the direction of the motion of the æther at the point considered is taken for the plane $x\,z$. Now, in consequence of the motion of the æther, the direction of the light in space will deviate from the normal to the wave through the angle $\frac{u_2}{V}$ in the contrary direction, as may be very easily shown (see Phil. Mag. for February 1846, p. 78). Hence the direction of the light coming from a star is the same as that of a right line drawn from the star, not merely at such a distance from the earth that the motion of the æther is there insensible, and again close to the surface of the earth, where the æther may be supposed to move with the earth, but *throughout the whole course* of the light; so that a ray of light will proceed in a straight line even when the æther is in motion, provided the motion be such as to render (*a*.) an exact differential. The orthogonal trajectory represented in fig. 2 of my paper of July, must not be confounded with the path of a ray of light. In that paper I supposed that the æther close to the surface of the earth was at rest relatively to the earth; in fact, the very object of the paper was to get rid of the apparent necessity of supposing the æther to pass through the whole atmosphere and through the earth itself. It should be observed, however, that the phænomenon of aberration allows us to suppose that the æther passes through the atmosphere and through the earth with any velocity, either constant, or varying from point to point, provided only (*a*.) be an exact differential.

P.S. I take this opportunity of adding a few words on the subject of Prof. Challis's last communication. There is nothing so far as I can make out in which we differ, except the sense in which we use the expression *explaining* a phænomenon from certain causes. According to my use of the term, a person would be said to explain a phænomenon when he has shown that certain causes being assumed, the phænomenon would necessarily follow. In this sense we explain the formation of images in common optics, assuming the properties of rays. We are able to show what must be the form, &c. of the image. In this sense Prof. Challis has not explained aberration by assuming *merely* the motion of the earth and the velocity of light, since, for aught that appears from his reasoning, a star might be displaced through double the angle through which it is observed to be displaced. It was for this reason, that in order to allow that Prof. Challis had explained aberration, I attached, in a former communication, a peculiar meaning to the word *aberration.*

We may divide the causes which we might conceive concerned in the production of aberration into three:—(1), the motion of the earth; (2), the velocity of light; (3), the change in the direction of the light coming to the earth. Professor Challis has shown that a certain apparent displacement of a star would result from the first two causes; and *as this happens* to be the whole displacement observed (neglecting a quantity which may be considered insensible), so that there is none left to be attributed to the third cause, he says that he has explained aberration, assuming merely the first two causes. It is evident that the two senses attached to the words, *to explain a phænomenon*, are quite different. According to the sense in which I used the words, the explanation of the absence of any change in the final direction of the light would have to be included in any theory which professed to explain aberration by means of the first two causes *only*. In the present communication I have used the words in my own sense, for I believe that there is no impropriety in it; but Prof. Challis may, if he pleases, consider the object of my July paper to have been the explanation, not of aberration, but of the absence of any change in the final direction of the light. Whichever of these results be arrived at, the other readily follows.

III. *On Electrical Endosmose.* *By* Mr. JAMES NAPIER[*].

THAT two dissimilar solutions, separated by a porous partition, will pass the one into the other, is a phænomenon long observed, the only necessary condition being that the liquids have a strong tendency to combine, and that the one is more capable of entering into or wetting the porous partition than the other. The liquids eminently fitted to effect this are alcohol and water, and saturated solutions of some salts and pure water. But the phænomenon of endosmose takes place also when an electric current passes through liquids separated by a porous partition. When all the above conditions are absent, and even when these conditions are present, the endosmotic current will follow the electric, although in a contrary direction to that which would take place were there no electric current passing, showing that, under these circumstances, it has its origin in the passage of the electric force. This fact was first made known by Mr. Porrett in the Annals of Philosophy for 1816.

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[*] Communicated by the Chemical Society; having been read December 15, 1845.

The object of this paper is not to define the cause, but to point out the different conditions of electrical endosmose, and the important part it plays in electro-chemical investigation.

The conditions under which electrical endosmose are observed, are, that the two metals constituting a battery, or the two electrodes of a battery, be placed in separate vessels or divisions of the same vessel; one of the vessels, or partition, being composed of a material sufficiently close in texture to prevent the mixture of the two liquids, and porous enough to allow the electricity to permeate through it. The substances generally used are bladder, parchment, unglazed porcelain, &c. The last is what I have generally used in the experiments to be detailed.

The general effects of electrical endosmose are, a portion of the positive solution passes along with the electric current into the negative solution, not by electrolytic action, as has been generally supposed, but by endosmotic action; and this endosmotic current is confined to the direct influence of the electric current, or facing the metals composing the electrodes or battery. There are some circumstances in which the positive solution gains in quantity, making an apparent current contrary to the electrical; these will be referred to as we proceed.

Electrical endosmose manifests itself in two ways, or rather is of two kinds, which may be distinguished as the *measurable* and the *unmeasurable*, the former being the result of the transfer of water from one cell to another, the latter of a salt or acid held in solution, and which is being decomposed; this may take place to an extent equal to the whole salt held in solution in the positive cell without the quantity of the two solutions being materially altered; and that this transfer is from endosmose will be best illustrated by detailing a few experiments, stating however that the investigation necessarily involves a great number of experiments, the whole of which I cannot detail, and the conclusions come to have been the result of these. It may be stated here that the vessel or division in which the positive electrode or metal is placed is termed the positive solution, and the negative division the negative solution.

I. 20 ounces of water, in which were dissolved 500 grains of sulphate of copper, were made the positive solution, and other 20 ounces of water, with 500 grains of sulphate of copper, were made the negative solution. A copper electrode, previously weighed, was put into each of these solutions and connected with a Wollaston's battery of nine pairs, the current allowed to pass until the negative solution was completely exhausted of copper, which required sixty-four hours, the battery not renewed during the experiment. At the end of the experiment, the electrodes being again weighed, the positive had lost 257 grains, the negative had increased 250 grains. The positive solution had lost 6 ounces by measure, the negative had gained 4 ounces. The greatest portion of this change took place within the last ten hours. The 2 ounces lost are mostly from evaporation. The positive solution was evaporated and crystallized, and there were obtained 905 grains of sulphate of copper, 95 less than was originally dissolved; but the partition being saturated with salt may account for a portion of the loss. The 500 grains which were originally in the negative solution would only contain 127 grains of metallic copper; but there is nearly double of this deposited, showing that the salt had been transferred by some means from the positive to the negative, in quantity amounting to nearly the whole of the salt originally dissolved in the positive solution.

II. 100 grains of sulphate of copper were dissolved in 4 ounces of water and put into each division, in the same manner as the last experiment. The partition in this case was much closer in texture than the other; the same battery power was used, and continued till the negative solution was exhausted, which was nineteen hours. The negative electrode being weighed, it had increased 34·4 grains; the negative solution had increased one-fourth of an ounce; the positive solution had lost three-eighths of an ounce. Comparing this experiment with the last, there is a great discrepancy, the only known cause being the closer texture of the diaphragm; the transfer of the salt is only in the proportion of one-third that of the former, and the measurable endosmose is still further out of proportion, being only one-sixteenth; while the amount of decomposition is only one-seventh of the 500 grains. The electrodes in both experiments were the same in size.

The question now suggested itself whether the transfer of the copper salt from the positive to the negative cell was the result of endosmose or of electrolysis, corresponding with the results of the late Professor Daniell and Professor Miller, who supposed that certain bases underwent electrolytic transfer in fractional proportions, and that these proportions might vary according to the texture of the diaphragm, or that kind of peculiar resistance given to the passage of a current when passing from one solution to another.

In order to determine which was the true cause, the negative cell was charged with a weak solution of caustic potash, the positive being sulphate of copper; gas was freely evolved from the negative electrode; in a little time the porous diaphragm facing the electrodes became coated with oxide of copper, which greatly retarded the current, and made it necessary to use a more powerful battery. After twelve hours, the oxide which had accumulated upon the diaphragm and at the bottom of the positive cell was carefully collected, washed, dried, and weighed 7·3 grains. The potash of the positive solution being neutralized by muriatic acid, had a solution of chloride of barium added, which gave a precipitate of sulphate of barytes weighing 26 grains, equal to about 9 grains of sulphuric acid. From this it appeared that the copper had passed from the positive to the negative as sulphate of copper, and therefore not by electrolysis.

In repeating similar experiments with very dilute alkaline and earthy salts in the negative cell, I have observed that the oxide of copper formed being a conductor of electricity, it often floated between the diaphragm and negative electrode as a kind of spongy fibre, and conducted the electricity through it as a solid, constituting its extremity in connexion with the porous partition into the electrode, the result being the reduction of the oxide in and upon the diaphragm, closing it up with metallic copper, similar to that observed in the operations of electro-metallurgy when any of the electrodes are allowed to touch the diaphragm. It has often surprised me how minute a fibre would connect the electrode and cell and produce a wide-spreading result.

A solution of cyanide of potassium was next tried in the negative cell, having sulphate of copper in the positive; the current passed four hours. The cyanide solution smelt strongly of hydrocyanic acid; the solution was found to contain both copper and sulphate of potash; the quantities were not determined.

The next object was to use two salts of such a character that if that from the positive cell passed to the negative by endosmose it would remain in solution; if by electrolysis, the base would be reduced to the state of an insoluble oxide. The negative was accordingly charged with a solution of caustic potash, and the positive with cyanide of copper and potassium, the latter in excess. After eight hours, during which gas was freely evolved from the negative electrode, and upon it was a small portion of copper powder, no oxide was formed; the solution contained cyanide of potassium and copper, the latter, being precipitated by hydrochloric acid and fused, gave 3·5 metallic copper, with strong smell of hydrocyanic acid.

From this it appeared that both the copper and potassium salt were transferred by endosmose. The double cyanides of potassium and silver and of potassium and gold were next substituted for the copper in the positive cell with similar results. Nitric acid was also substituted for the caustic potash in the negative cell, with the double cyanides of copper, silver and gold in the positive, which would give the contrary result of last experiments, namely, precipitating the metals if transferred by endosmose, but dissolving or holding them in solution if transferred by electrolysis. In a short time, in each experiment, the surface of the porous diaphragm facing the negative electrode became coated with the cyanide of the metal, which accumulated and dropped to the bottom of the vessel. At the termination of the experiments the acid solution smelt strongly of hydrocyanic acid, and contained much nitrate of potash. With the gold and silver salts, the acid solution had not the slightest indication of their presence in solution, but with the copper there was a considerable portion present, which was owing to the cyanide of that metal being decomposed by nitric acid; but this experiment being repeated with the copper cyanide in the positive and sulphuric acid in the negative, no trace of copper in solution was found in the acid.

The next experiment was to determine if any portion of the potassium of the potash salt was transferred by electrolysis; for this purpose two porous vessels were employed, one filled with a solution of cyanide of potassium, the other with dilute nitric acid; these were placed in a glass vessel containing a solution of nitrate of silver; the positive electrode was placed in the cyanide of potassium, the negative in the nitric acid. The cyanide of potassium which passed from the positive cell to the silver solution in the glass vessel was decomposed, producing cyanide of silver and nitrate of potash. After twelve hours, the cyanide of silver formed was carefully collected, washed and dried; it weighed 65·6 grains; the remaining silver solution had as much muriatic acid added as precipitated the silver. The clear filtered solution was now evaporated to dryness and kept for some time at the point of fusion. There were obtained 28 grains of nitrate of potash, which is 4 grains less than the equivalent of cyanide of silver obtained; but the nitric acid in the negative cell also contained a little nitrate of potash and silver, which had passed from the glass vessel, and will account for this loss. From these, and a variety of other experiments of a similar kind with different salts, I consider that no base of an electrolyte is transferred by electrolytic action, but that salts being electrolysed are all transferred more or less from the positive to the negative electrodes by endosmose, and that the amount of this varies according to the texture of the porous diaphragm, the power of the electric current, and various other modifying circumstances, which will be apparent as we proceed with the inquiry into the cause and circumstances of electrical endosmose.

In referring to the first experiment, it was observed that the greatest amount of measurable endosmose took place during the last ten hours when both solution and battery were nearly exhausted. 500 grains of sulphate of copper were again put into each cell, the solutions accurately measured, and the current of 9 pairs passed for twenty hours. The deposit on the negative electrode was 116·3 grains; this solution had increased in measure three-eighths of an ounce, the positive had lost half an ounce; being evaporated and crystallized, the negative gave 226 grains crystals, the positive gave 749 grains. In this experiment we have 184 grains of the salt transferred, and only three-eighths of an ounce of measurable endosmose.

960 grains of sulphate of copper were dissolved in 20 ounces of water and put into the negative cell, while the positive was charged with dilute muriatic acid. A battery of 12 pairs was connected and kept in action twenty-four hours; it was then found that the negative electrode had increased in weight 178 grains, and the solution had gained in measure 2½ ounces. A small portion had nitrate of silver added, which gave a slight milkiness, but not amounting to a precipitate. The whole solution of the negative cell, being evaporated and crystallized, gave sulphate of copper 463 grains. This shows a transfer of sulphate of copper from the positive solution equal to 162 grains. The positive solution was of a deep blue colour, it had lost by measure 2¼ ounces; the electrode was covered with a white powder, and had lost 212 grains. Here again the two kinds of endosmose are perfectly distinct. But we have another curious result, viz. the muriatic acid apparently refusing to be transferred from the positive to the negative solution. This attracted particular attention, and the next experiment was conducted in the following manner:—100 grains of sulphate of copper were dissolved and put into a small porous vessel, the solution measuring 2½ ounces; this was supported at the top of a deep glass vessel filled with dilute muriatic acid, so that several inches of solution were under the porous vessel, the surface of the two solutions being level; by this arrangement the copper salt formed at the positive electrode by the current sunk to the bottom by its own gravity and remained under the porous vessel, so that no transfer of salt could take

place. Two copper electrodes were used, and the current continued until the negative solution was exhausted of copper. When this was completed the negative electrode had gained in weight 25·1 grains: the solution had increased in measure half an ounce. On testing this by nitrate of silver, only a slight milkiness was obtained. The positive solution was deep blue at bottom, but only a slight tinge above the bottom of the porous vessel; the positive electrode was coated nearly to one-sixteenth of an inch with a white pasty matter, a great portion had also fallen to the bottom of the vessel: the electrode had lost in weight 132 grains. Here we have measurable endosmose amounting to half an ounce without any transfer of the acid in solution.

I now took two porous vessels, the one charged with 2½ ounces by measure of a solution of sulphate of copper, the other with dilute muriatic acid; these were placed in a glass vessel filled with a measured quantity of distilled water, the cells placed 1 inch apart; two copper electrodes were used: the muriatic acid cell made the positive solution, the sulphate of copper the negative. A current from 9 pairs was kept up for eighteen hours. At first the decomposition was exceedingly slow, but it afterwards increased; the results of this experiment were—

Positive solution lost three-eighths of an ounce, pole covered with white powder, and had lost in weight 37 grains; solution contained sulphate of copper. The solution in the glass vessel was tinged blue by sulphate of copper, had lost in measure 1½ ounce, and gave a copious precipitate with nitrate of silver, showing a transfer of muriatic acid from the positive cell.

The negative cell had increased in measure 1¼ ounce. Nitrate of silver gave no indication of muriatic acid, the electrode had increased in weight 26 grains.

A similar experiment was again repeated, the negative cell and glass vessel being both charged with distilled water, the positive with dilute muriatic acid; platinum electrodes were used. A 9-pair battery was attached for sixteen hours. The current passing was sufficient to keep deflected a galvanometer needle, but no evolution of gas was observed till nearly the end of the experiment, when the poles became covered with small bubbles of gas not large enough to be evolved. The results of this experiment were, positive cell lost in measure one-eighth of an ounce. The solution in the glass vessel was slightly acid, giving a precipitate with nitrate of silver, and had lost in measure 1½ ounce. The negative cell had increased in measure 1½ ounce, did not change the colour of blue litmus paper, nor give any precipitate with nitrate of silver: here also, as in the last experiment, we have muriatic acid being transferred in small quantities from the positive to the intervening liquid. But another feature presents itself, the great amount of measurable endosmose and the almost entire absence of electrical decomposition; at the same time the endosmose being principally confined to the two vessels containing water, as if the acid in the cell had only acted the part of an electrode. On reversing the condition of the last experiment, the positive and glass vessel being charged with distilled water, the negative with the dilute muriatic acid, 9-pair battery, for sixteen hours, the positive vessel lost 1½ ounce; the glass vessel had increased 1¼ ounce, while the negative or acid solution underwent no alteration, showing again the endosmose only between the two cells containing water.

Several experiments were repeated both with the single and double cells, having muriatic acid as the positive solution, with sulphate of copper, water and alkalies as the negative solution; in some I had transfer of acid in small quantity, in others no indication of transfer; I believe the cause of difference to be in the texture of the partition; when a diaphragm was used less porous, the endosmose was considerable, in one case amounting to 4 grains in thirty hours. Nitric and sulphuric acids, which are much more easily transferred than muriatic acid, have also had their transfer resisted by two closely-textured diaphragms, passing through one into the mid-division but not into the negative solution: however, these results show that all kinds of salts or acids are not transferred with the same facility, probably from their power of conduction.

While operating with the double cells, having an acid in the positive and water in the middle and negative cells, I have several times observed that both positive and negative solutions increased in bulk at the expense of the middle solution; in one instance the positive solution had gained 1 ounce, the negative gained three-fourths of an ounce, and the middle solution had lost 1¾ ounce. The negative and positive solutions originally contained only 2 ounces, the electrodes measured 2 by 1 inches; nevertheless in these instances the transfer of the acid from the positive to the negative was considerable. The different acids gave different results of this sort; with sulphuric acid the increase was greatest, muriatic acid one-half less, nitric acid about one-fourth of the sulphuric; whether this be the result of any fixed law I cannot say, the times I have observed it being too few. As I have never observed these results except with the acids, I am inclined to think the cause to be what has been already stated, namely, the solution acting as the electrode, thus exposing a surface of acid to water in a state of excitement or tension sufficient to attract by its exalted affinity that fluid, and cause an increase of bulk in the cell, in a similar manner, as I shall endeavour to show in a forthcoming paper, to that which causes a greater amount of action at the positive electrode than at the negative in any electrolyte, and with muriatic acid and copper, as already referred to in a former part of this paper, amounting occasionally to five times that of the negative.

I shall now give the results, in a tabular form, of a few experiments made to determine the relation, if any, between the measurable endosmose and electrical decomposition.

The experiments were all made with the double cells of three compartments; the battery used being Wollaston's of 9-pair intensity; the time of each experiment was sixteen hours; the electrodes of platinum. The acids used were diluted as 1 to 25 water; solution of sulphate of copper the same strength in all.

Positive solution.	Glass vessel (intermediate).	Negative solution.	Loss in positive.	Change in glass vessel.	Gain in negative.	Amount of deposit on decomposition.	Amount of measurable endosmose.
			Ounces.	Ounces.	Ounces.	Grains.	Ounces.
SO^3	Water	Water	none	loss $\frac{1}{4}$	$\frac{4}{8}$	not measurable	$\frac{4}{8}$
Water	Water	SO^3	$\frac{1}{4}$	none	$\frac{3}{8}$	....	$\frac{3}{8}$
NO^5	Water	Water	$\frac{1}{8}$	loss $\frac{1}{4}$	$\frac{7}{8}$	....	$\frac{1}{8}$
Water	Water	NO^5	1	gain $\frac{5}{8}$	non	....	$\frac{6}{8}$
HCl	Water	Water	$\frac{1}{8}$	loss $1\frac{1}{4}$	1	....	1
Water	Water	HCl	$1\frac{3}{8}$	gain $1\frac{1}{4}$	none	....	$1\frac{1}{4}$
Water	Water	SO^3CuO	$\frac{1}{4}$	gain $\frac{1}{8}$	$\frac{1}{8}$	1	$\frac{1}{4}$
Water	Water	SO^3CuO	1	gain $\frac{1}{8}$	$\frac{7}{8}$	10	1
SO^3CuO	SO^3	SO^3	$\frac{1}{16}$	gain $\frac{1}{8}$	$\frac{1}{8}$	33	$\frac{1}{4}$
SO^3	SO^3	SO^3CuO	$\frac{3}{16}$	gain $\frac{1}{8}$	$\frac{1}{16}$	24	$\frac{3}{16}$
SO^3CuO	Water	SO^3	none	loss $\frac{1}{8}$	$\frac{1}{4}$	11	$\frac{1}{4}$
SO^3	Water	SO^3CuO	$\frac{1}{8}$	loss 1	$1\frac{1}{8}$	30	$1\frac{1}{8}$
HCl	Water	SO^3CuO	$\frac{3}{16}$	loss $1\frac{3}{8}$	$1\frac{1}{4}$	27	$1\frac{1}{4}$
SO^3CuO	SO^3CuO	SO^3CuO	$\frac{1}{4}$	loss $\frac{1}{16}$	$\frac{1}{8}$	53	$\frac{1}{8}$
HCl	HCl	SO^3Cu	$\frac{3}{16}$	gain $\frac{1}{4}$	$\frac{1}{16}$	43	$\frac{3}{16}$
SO^3	Water	Water	gain $\frac{1}{8}$	loss $\frac{3}{4}$	$\frac{1}{4}$	not measurable	$\frac{3}{4}$
Water	Water	SO^3	loss $\frac{3}{8}$	loss $1\frac{1}{2}$	1	evolution of gas	1
HCl	Water	Water	gain $\frac{1}{4}$	loss $1\frac{3}{4}$	1	not measurable	$1\frac{1}{4}$
NO^5	Water	Water	gain $\frac{1}{8}$	loss $\frac{1}{8}$	$\frac{1}{8}$	not measurable	$\frac{1}{8}$
SO^3	Water	Water	loss $\frac{1}{8}$	loss $\frac{3}{4}$	$\frac{7}{8}$	not measurable	$\frac{7}{8}$
Common salt ...	Water	Water	none	loss 1	1	not measurable	1

The following table is of the same kind as the last, but the time of action and the power of the battery varying, as stated.

Positive solution.	Solution of glass vessel.	Negative solution.	Loss in positive.	Change in glass vessel.	Gain in negative.	Amount of deposit.	Amount of measurable endosmose.	Time on.	Power of battery.
						Grs.		hours	
SO^3	SO^3	SO^3CuO	none	loss $\frac{1}{16}$ oz.	$\frac{1}{16}$ oz.	21	$\frac{1}{16}$oz.	2	2 p. large* pls.
HCl	HCl	SO^3CuO	$\frac{1}{16}$ oz.	loss $\frac{1}{16}$ oz.	$\frac{1}{8}$ oz.	12	$\frac{1}{8}$ oz.	2	Ditto
NO^5	NO^5	SO^3CuO	none	none	none	23	none	2	Ditto
HCl	HCl	SO^3CuO	...	...	...	9	...	2	2 p. small pls.
SO^3	SO^3	SO CuO	...	...	...	15	...	2	Ditto
NO^5	NO^5	SO^3CuO	...	...	...	16	...	2	Ditto
SO^3	SO^3	SO^3CuO	...	...	...	8	...	2	1 p. large pls.
HCl	HCl	SO^3CuO	...	...	...	7	...	2	Ditto
NO^5	NO^5	SO^3CuO	...	...	...	8	...	2	Ditto
SO^3	SO^3	SO^3CuO	...	...	...	6	...	2	1 p. small pls.
HCl	HCl	SO^3CuO	...	...	...	5	...	2	Ditto
NO^5	NO^5	SO^3CuO	...	...	...	7	...	2	Ditto
CyK	CyK	SO^3Cu	...	loss $\frac{1}{4}$ oz.	$\frac{1}{8}$ oz.	10	$\frac{1}{8}$ oz.	4	Ditto
CyH	CyH	SO^3CuO	$\frac{3}{4}$ oz.	gain $\frac{1}{4}$ oz.	$\frac{1}{4}$ oz.	27	$\frac{1}{8}$ oz.	4	9 p. small pls.

* In those marked large plates, the zinc plate of battery measured 6 by 4 inches; in those marked small, zinc measured 2 by 2½ inches.

The result of these experiments, as well as those given before in detail, shows that there is no relation between the measurable endosmose and the amount of decomposition in the cells; so that the two phænomena must depend upon somewhat different causes, which now became an object of inquiry. Professor Faraday in his researches, mentions that a current of electricity may be made to pass through solutions without decomposing them, and Mr. Sturgeon gives it as his opinion that there is always an undecomposing current passing with a decomposing current. These observations being applied to the results of the present inquiry into measurable and unmeasurable endosmose, seemed to throw some light upon the distinct character of these phænomena, for, as will be observed, the measurable endosmose seems to be greatest when the current has the greatest difficulty to pass through, and when the decomposition is least; and on the contrary, the unmeasurable endosmose is greatest when the battery is powerful and the current passing freely, or rather decomposition going on freely.

In order to compare the two, I now tried a few experiments with different powers of battery, under constant circumstances as regards the decomposition cells. Each division of a decomposition cell had put into it 100 grains of sulphate of copper dissolved in 3 ounces of water; copper electrodes were used, and the current allowed to pass until the negative solution was exhausted of copper. The following is the mean of several trials.

With a 1-pair battery the negative solution exhausted in forty-one hours, deposited upon pole 29 grains, being 3·4 grains more than the equivalent of 100 grains of sulphate, which is equal to 8·5 grains of sulphate which have passed by endosmose, not including water of crystallization, as I believe the salt passes without water. The negative solution had increased in bulk 1 ounce.

With a 2-pair battery the negative solution exhausted in nineteen hours, deposited upon pole 32 grains, being 6·4 grains more than the equivalent of 100 grains of sulphate, and equal to 16 grains of sulphate passed by endosmose. The solution had increased in bulk five-eighths of an ounce.

With a 4-pair battery the negative solution exhausted in twelve hours, deposited upon pole 35·6 grains, being 11 grains more than the equivalent of 100 grains of sulphate, and equal to 27·5 grains of sulphate passed by endosmose. The solution had increased half an ounce.

With a 6-pair battery the negative cell exhausted in seven hours, deposited upon pole 39 grains, being 14·4 grains more than the equivalent of 100 grains of sulphate, equal to 36 grains of sulphate passed by endosmose. The solution increased in bulk three-eighths of an ounce.

The two divisions of the decomposing cell were now charged with distilled water; two platinum electrodes were used; the current was also made to pass through a solution of sulphate of copper to ascertain if sufficiently strong to effect any decomposition; but in none of the experiments was any deposit obtained. Each experiment was continued thirty hours, when the cells stood thus:—

With one pair,	positive lost	1 oz.	negative gained $\frac{3}{4}$ oz.
...	two pairs ...	... 1 oz.	... ... $\frac{3}{4}$ oz.
...	four pairs ...	... $1\frac{1}{4}$ oz.	... ... $\frac{7}{8}$ oz.
...	six pairs ...	... $1\frac{3}{8}$ oz.	... ... $1\frac{1}{8}$ oz.
...	nine pairs ...	... $1\frac{5}{8}$ oz.	... ... $1\frac{3}{8}$ oz.

With these, and all other experiments with water, a similar vessel to the decomposing cells was placed alongside, filled with water, to note the loss by evaporation, which in this case was three-eighths of an ounce, accounting for the loss in the two cells above.

A similar experiment was made with thirteen pairs of a Grove's battery, the current passed four hours; a gentle flow of gas was evolved from the electrode. No copper solution was used in this experiment. The cells stood thus: positive lost 2 ounces, negative gained $1\frac{3}{4}$ ounce.

The two divisions of the decomposition cell being again filled with distilled water, into one was put a piece of zinc and into the other a piece of platinum, connected by a wire;

in forty-eight hours the zinc cell had lost three-quarters of an ounce, the platinum cell had gained half an ounce. This experiment was repeated many times with similar results.

Two large cells were filled with distilled water, and a piece of zinc, measuring 4 by 6 inches, carefully weighed was put into one division and a piece of copper of the same size was put into the other division, the current passed through a delicate galvanometer, which kept deflected about 3°; the positive solution was kept at a given height, the negative was taken out as it increased, keeping the two solutions as nearly level as possible; this was kept up for forty days, when there was found to have passed through from the positive to the negative a bulk equal to 32 ounces, allowing for the evaporation, which was known by a similar vessel placed alongside. The zinc was covered with a gray film and had increased in weight 12 grains; this being carefully dissolved off by ammonia, water and the zinc again weighed, it was found to have lost 36 grains, which we may take as the amount of oxidation during the experiment.

The general conclusions which may be drawn from these experiments respecting endosmose are,

(1.) That the current of positive electricity passing through a liquid is always accompanied with a current of the liquid in the same direction.

(2.) If the liquid contains a salt or an acid that is undergoing decomposition, the endosmotic current is principally, if not wholly, confined to that salt or acid, unaccompanied with water, and therefore adds little or nothing to the bulk of the liquid into which it passes.

(3.) When the quantity of electricity which a battery is capable of giving off is greater than the salt or acid can conduct, the extra quantity, if we may so term it, passes through the water, taking with it, and thus inducing a flow of that liquid, into the negative cell, increasing the quantity; the same effect being produced with water when no salt or acid is in solution: hence the well-known fact, that endosmose is greatest with pure water, and even with currents that give no apparent decomposition, or rather a decomposition so minute as to be unobserved.

IV. *On the Permanent State of Heat in a Thin Uniform Wire of any form, acted on by two sources of heat of equal intensity at its extremities. By* G. W. Hearn, *B.A. of Cambridge, and Professor of Mathematics, Royal Military College, Sandhurst*[*].

WE suppose the wire homogeneous and of the same thickness throughout, ε^2 the area of a transverse section, K the interior, and h the exterior conductivity; v the temperature at a point distant s from one extremity measured along the wire.

The excess of the flux of heat between s and $s+\delta s$, is

$$k\,\varepsilon^2 \frac{d^2 v}{d s^2}\,\delta s,$$

and the quantity emitted by this portion is

$$h\,c\,v\,\delta s,$$

c being the circumference of the transverse section,

$$\therefore \qquad \mathrm{K}\,\varepsilon^2 \frac{d^2 v}{d s^2} = h\,c\,v;$$

or if

$$\frac{h\,c}{\mathrm{K}\,\varepsilon^2} = k^2,$$

$$\frac{d^2 v}{d s^2} = k^2 v,$$

$$\therefore\ v = \mathrm{A}\,e^{-ks} + \mathrm{B}\,e^{ks}.$$

Let the equal temperatures of the extremities be represented by 1, and let a be the length of the wire;

$$\therefore\ \mathrm{A} + \mathrm{B} = 1,$$

$$\mathrm{A}\,e^{-ka} + \mathrm{B}\,e^{ka} = 1,$$

or

$$\mathrm{A} = \frac{e^{ka}-1}{e^{ka}-e^{-ka}}, \qquad \mathrm{B} = \frac{1-e^{-ka}}{e^{ka}-e^{-ka}},$$

$$v = \mathrm{A}\left\{e^{-ks} + e^{-k(a-s)}\right\}. \quad \ldots\ldots \quad (1.)$$

Now suppose the temperature at the middle point to have been determined by observation $= t_2$.

Then

$$t_2 = \mathrm{A}\left\{e^{-\frac{ka}{2}} + e^{-\frac{ka}{2}}\right\} = 2\,\mathrm{A}\,e^{-\frac{ka}{2}},$$

or

$$t_2 = 2\,\frac{e^{ka}-1}{e^{ka}-e^{-ka}}\cdot e^{-\frac{ka}{2}} = \frac{2}{e^{\frac{ka}{2}} + e^{-\frac{ka}{2}}};$$

from which

$$e^{\frac{ka}{2}} = \frac{1+\sqrt{1-t_2^2}}{t_2}.$$

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[*] Communicated by the Author.

Now the extreme temperatures being represented by 1, it is evident that t_2 will be some proper fraction. Let therefore

$$t_2 = \sin\alpha,$$

$$\therefore\ e^{\frac{ka}{2}} = \cot\frac{\alpha}{2}, \text{ and } \ e^{-\frac{ka}{2}} = \tan\frac{\alpha}{2}.$$

Hence

$$v = \frac{1}{2}\,t_2 \cot\frac{\alpha}{2}\left\{\left(\tan\frac{\alpha}{2}\right)^{\frac{2s}{a}} + \left(\tan\frac{\alpha}{2}\right)^{\frac{2(a-s)}{a}}\right\},$$

or $$v = \frac{1}{2}\,t_2\left\{\left(\tan\frac{\alpha}{2}\right)^{1-2n} + \left(\cot\frac{\alpha}{2}\right)^{1-2n}\right\},$$

where $$n = \frac{s}{a}.$$

Now let t_3 be another observed temperature at the distance of $\frac{1}{3}\,a$ from either extremity,

$$t_3 = \frac{1}{2}\,t_2\left\{\left(\tan\frac{\alpha}{2}\right)^{\frac{1}{3}} + \left(\cot\frac{\alpha}{2}\right)^{\frac{1}{3}}\right\},$$

whence $$\tan\frac{\alpha}{2} = \frac{\{t_3 - \sqrt{t_3^2 - t_2^2}\}^3}{t_3^3};$$

so that from those two observed temperatures we can easily calculate α, and thence v.

To adapt the formulæ to logarithmic calculation, let

$$\frac{t_2}{t_3} = \sin\eta,$$

then $$\tan\frac{\alpha}{2} = 8\sin^6\frac{\eta}{2}.$$

Moreover, make

$$\left(\tan\frac{\alpha}{2}\right)^{1-2n} = \tan\theta, \therefore \left(\cot\frac{\alpha}{2}\right)^{1-2n} = \cot\theta,$$

then $$v = t_2 \operatorname{cosec} 2\,\theta.$$

Hence we have only to calculate

$$\log\sin\eta = \log t_2 - \log t_3,$$

$$\log\tan\theta = (1 - 2\,n)\left\{\log 8 + 6\log\sin\frac{\eta}{2}\right\},$$

$$\log v = \log t_2 + \log\operatorname{cosec} 2\,\theta.$$

It is also evident, since $e^{\frac{ka}{2}} = \cot\frac{\alpha}{2}$, we can obtain k, and thence $\frac{h}{\mathrm{K}}$ the ratio of the exterior to the interior conductivity.

The quantity k will be usually very great, and $\therefore\ \alpha$ very small; and hence when $n < \frac{1}{2}$, we shall have very nearly

$$v = \frac{1}{2}\, t_2 \left(\cot\frac{\alpha}{2}\right)^{1-2n},$$

and when $n > \frac{1}{2}$,

$$v = \frac{1}{2}\, t_2 \left(\tan\frac{\alpha}{2}\right)^{1-2n}.$$

Let v and v' be two temperatures corresponding to

$$n\left(<\frac{1}{2}\right) \text{ and } n + \frac{1}{2},$$

$$\therefore\ v = \frac{1}{2}\, t_2 \left(\cot\frac{\alpha}{2}\right)^{1-2n}, \quad v' = \frac{1}{2}\, t_2 \left(\tan\frac{\alpha}{2}\right)^{-2n};$$

$$\therefore\ v v' = \frac{1}{4}\, t_2^2 \cot\frac{\alpha}{2}.$$

Hence the product of the temperatures of points distant from each other by half the length of the wire (provided neither are very near the extremity) is nearly constant.

There is another curious relation which is not merely approximative, but accurately true, and which is easily verified by experiment. It is as follows:—

Having, as above determined, θ, find m from the formula

$$(\tan\theta)^{\frac{1-2m}{1-2n}} = \tan\left(\frac{\pi}{4} - \theta\right),$$

and then $s' = a\, m;$

and if v and v' be the temperatures at s and s',

$$v = t_2 \operatorname{cosec} 2\theta,$$
$$v' = t_2 \sec 2\theta;$$

$$\therefore\ \sin 2\theta = \frac{t_2}{v}, \qquad \cos 2\theta = \frac{t_2}{v'};$$

$$\therefore\ \frac{1}{v^2} + \frac{1}{v'^2} = \frac{1}{t_2^2},$$

or the sum of the reciprocals of the squares of the temperatures at such points is constant.

It is to be observed that Newton's law of cooling *in vacuo* is the basis of the above investigation. Also, that as the extreme temperatures of the wire are supposed equal, the two extremities may be joined and subjected to a single source of heat, in which case it will be a closed curve*.

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* Vide Fourier, *Théorie de la Chaleur*, and Kelland on Heat.

V. *Remarkable Property of Mica.* *By* P. Riess*.

A PIECE of clean mica becomes covered like other substances with a vast number of small drops of water at very small distances from each other, when it is breathed upon or held over steaming water. The layer of bubbles impairs the reflecting properties of the mica, and renders objects placed beneath it imperfectly visible; if by means of a sharp knife a very thin leaf be pealed off the mica, and thus a new surface given to it, and it then be again exposed to the aqueous vapours, the fresh surface remains perfectly transparent whilst the surrounding parts become dim as before. This curious fact is not accounted for by supposing no aqueous vapour to be condensed upon the fresh surface, for a distinct play of colours may be observed upon that part, showing it to be covered with a continuous thin layer of water. A drop of water, which remains in a globule upon an old surface of mica, immediately spreads out and moistens the whole extent of a fresh surface.

From its extreme purity, therefore, the fresh mica surface exerts so strong an affinity for aqueous vapour as to condense it in a continuous film, whereas, if it has been exposed for some time to the air, it can only condense the vapour in separate little globules. This may be distinctly seen under the microscope.

A fresh mica surface shows this property not only in an atmosphere nearly saturated with aqueous vapour, as is produced by an exhalation, but likewise in common air when far removed from a saturated state. It is well known that mica is one of the best non-conductors of electricity; a piece was chosen which did not in the least affect a charged electrometer during the space of a minute; on pealing off a thin layer, the fresh surface discharged the electrometer in a few seconds, the old surface still remaining a non-conductor. That the power of conducting was due to a continuous layer of water, and could be removed by its evaporation, was proved by the following experiments.

A fresh mica surface placed in a box, the bottom of which was covered with chloride of calcium, could not, after a minute or two, be found to conduct electricity; it became however a conductor on the removal of the drying material and the renewal of the air in the box. Whilst heated, a similar leaf of mica did not conduct, but regained that power during cooling.

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* Translated from Poggendorff's *Annalen*, No. 3, 1846, and communicated by Dr. E. Ronalds.

The two metallic caps of a high insulated glass cylinder were connected by a freshly split leaf of mica in the interior of the cylinder: either cap discharged an electrometer on touching the other with the hand. When however the air was rarefied in the cylinder to two lines barometrical pressure, the electrometer was not discharged, for then the leaf of mica insulated completely. The insulation lasted for twenty-two hours, when on admitting a small quantity of air insulation was again destroyed, and the mica conducted as well as before.

Exposed to the air, mica retains this property but a short time. In a few hours parts of the fresh surface become dim when breathed on; in a day or two the whole surface, and the mica then insulates to a considerable extent.

The property of not becoming dim when breathed on is common to the scaly varieties of gypsum, and probably to other minerals. Calcareous spar possesses the property in a very slight degree, and loses it in a few minutes.

VI. *On Quaternions; or on a New System of Imaginaries in Algebra. By* Sir William Rowan Hamilton, *LL.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, Member of several other Scientific Societies in these and in Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from vol. xxvi. p. 224.]

18. THE separation of the real and imaginary parts of a quaternion is an operation of such frequent occurrence, and may be regarded as being so fundamental in this theory, that it is convenient to introduce symbols which shall denote concisely the two separate results of this operation. The algebraically *real* part may receive, according to the question in which it occurs, all values contained on the one *scale* of progression of number from negative to positive infinity; we shall call it therefore the *scalar part*, or simply the *scalar* of the quaternion, and shall form its symbol by prefixing, to the symbol of the quaternion, the characteristic Scal., or simply S., where no confusion seems likely to arise from using this last abbreviation. On the other hand, the algebraically *imaginary* part, being geometrically constructed by a straight line, or radius vector, which has, in general, for each determined quaternion, a determined length and determined direction in space, may be called the *vector part*, or

simply the *vector* of the quaternion; and may be denoted by prefixing the characteristic Vect., or V. We may therefore say that *a quaternion is in general the sum of its own scalar and vector parts*, and may write

$$Q = \mathrm{Scal.}\,Q + \mathrm{Vect.}\,Q = \mathrm{S.}\,Q + \mathrm{V.}\,Q,$$

or simply

$$Q = \mathrm{S}Q + \mathrm{V}Q.$$

By detaching the characteristics of operation from the signs of the operands, we may establish, for this notation, the general formulæ:

$$1 = \mathrm{S} + \mathrm{V};\quad 1 - \mathrm{S} = \mathrm{V};\quad 1 - \mathrm{V} = \mathrm{S};$$
$$\mathrm{S.S} = \mathrm{S};\quad \mathrm{S.V} = 0;\quad \mathrm{V.S} = 0;\quad \mathrm{V.V} = \mathrm{V};$$

and may write

$$(\mathrm{S} + \mathrm{V})^n = 1,$$

if n be any positive whole number. The scalar or vector of a sum or difference of quaternions is the sum or difference of the scalars or vectors of those quaternions, which we may express by writing the formulæ:

$$\mathrm{S}\Sigma = \Sigma\mathrm{S};\quad \mathrm{S}\Delta = \Delta\mathrm{S};\quad \mathrm{V}\Sigma = \Sigma\mathrm{V};\quad \mathrm{V}\Delta = \Delta\mathrm{V}.$$

19. Another general decomposition of a quaternion, into factors instead of summands, may be obtained in the following way:—Since the square of a scalar is always positive, while the square of a vector is always negative, the algebraical excess of the former over the latter square is always a positive number; if then we make

$$(\mathrm{T}Q)^2 = (\mathrm{S}Q)^2 - (\mathrm{V}Q)^2,$$

and if we suppose $\mathrm{T}Q$ to be always a real and positive or absolute number, which we may call the *tensor* of the quaternion Q, we shall not thereby diminish the generality of that quaternion. This *tensor* is what was called in former articles the *modulus*[*]; but there seem to be some conveniences in not

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[*] The writer believes that what originally led him to use the terms "modulus" and "amplitude," was a recollection of M. Cauchy's nomenclature respecting the usual imaginaries of algebra. It was the use made by his friend, John T. Graves, Esq., of the word "constituents," in connexion with the ordinary imaginary expressions of the form $x + \sqrt{-1}\,y$, which led Sir William Hamilton to employ the same term in connexion with his own imaginaries. And if he had not come to prefer to the word "modulus," in this theory, the name "tensor," which suggested the characteristic T, he would have borrowed the symbol M, with the same signification, from the valuable paper by Mr. Cayley, "On Certain Results respecting Quaternions," which appeared in the Number of this Magazine for February 1845. It will be proposed by the present writer, in a future article, to call the *logarithmic modulus* the "mensor" of a quaternion, and to denote it by the foregoing characteristic M; so as to have

$$\mathrm{M}Q = \log.\mathrm{T}Q,\quad \mathrm{M.}QQ' = \mathrm{M}Q + \mathrm{M}Q'.$$

obliging ourselves to retain here a term which has been used in several other senses by writers on other subjects; and the word tensor has (it is conceived) some reasons in its favour, which will afterwards more fully appear. Meantime we may observe, as some justification of the use of this word, or at least as some assistance to the memory, that it enables us to say that the *tensor of a pure imaginary*, or vector, is the number expressing the *length* or linear *extension of the straight line* by which that algebraical imaginary is geometrically constructed. If such an imaginary be divided by its own tensor, the quotient is an imaginary or vector *unit*, which marks the *direction* of the constructing line, or the region of space towards which that line is *turned*; hence, and for other reasons, we propose to call this quotient the *versor* of the pure imaginary: and generally to say that *a quaternion is the product of its own tensor and versor factors*, or to write

$$Q = TQ \, . \, UQ,$$

using U for the characteristic of versor, as T for that of tensor. This is the other general decomposition of a quaternion, referred to at the beginning of the present article; and in the same notation we have

$$T.TQ=TQ; \quad T.UQ=1; \quad U.TQ=1; \quad U.UQ=UQ;$$

so that the tensor of a versor, or the versor of a tensor, is unity, as it was seen that the scalar of a vector, or the vector of a scalar, is zero.

The tensor of a positive scalar is equal to that scalar itself; but the tensor of a negative scalar is equal to the positive opposite thereof. The versor of a positive or negative scalar is equal to positive or negative unity; and in general, by what was shown in the 12th article, the versor of a quaternion is the product of two imaginary units. The tensor and versor of a vector have been considered in the present article. A tensor cannot become equal to a versor, except by each becoming equal to positive unity; as a scalar and a vector cannot be equal to each other, unless each reduces itself to zero.

20. If we call two quaternions *conjugate* when they have the same scalar part, but have opposite vector parts, then because, by the last article,

$$(TQ)^2 = (SQ + VQ)(SQ - VQ),$$

we may say that the *product of any two conjugate quaternions*, $SQ+VQ$ and $SQ-VQ$, is equal to the *square of their common tensor*, TQ; from which it follows that *conjugate versors are the reciprocals of each other*, one quaternion being called the *reciprocal* of another when their product is positive unity.

If Q and Q′ be any two quaternions, the two products of their vectors, taken in opposite orders, namely VQ . VQ′ and VQ′ . VQ, are conjugate quaternions, by the definition given above, and by the principles of the 9th article; and the conjugate of the sum of any number of quaternions is equal to the sum of their conjugates; therefore the products

$$(\mathrm{S}Q+\mathrm{V}Q)(\mathrm{S}Q'+\mathrm{V}Q') \text{ and } (\mathrm{S}Q'-\mathrm{V}Q')(\mathrm{S}Q-\mathrm{V}Q)$$

are conjugate; therefore T. QQ′, which is the tensor of the first, is equal to the square root of their product, that is, of

$$(\mathrm{S}Q+\mathrm{V}Q)(\mathrm{T}Q')^2(\mathrm{S}Q-\mathrm{V}Q), \text{ or of } (\mathrm{T}Q)^2(\mathrm{T}Q')^2;$$

we have therefore the formula

$$\mathrm{T}.QQ' = \mathrm{T}Q.\mathrm{T}Q',$$

which gives also

$$\mathrm{U}.QQ' = \mathrm{U}Q.\mathrm{U}Q';$$

that is to say, the *tensor of the product* of any two quaternions is equal to the *product of the tensors*, and in like manner the *versor of the product* is equal to the *product of the versors*. Both these results may easily be extended to any number of factors, and by using Π as the characteristic of a product, we may write, generally,

$$\mathrm{T}\Pi Q = \Pi \mathrm{T} Q; \quad \mathrm{U}\Pi Q = \Pi \mathrm{U} Q.$$

It was indeed shown, so early as in the 3rd article, that the modulus of a product is equal to the product of the moduli; but the process by which an equivalent result has been here deduced does not essentially depend upon that earlier demonstration: it has also the advantage of showing that *the continued product of any number of quaternion factors is conjugate to the continued product of the respective conjugates of those factors, taken in the opposite order*; so that we may write

$$(\mathrm{S}-\mathrm{V}).QQ'Q''\ldots = \ldots(\mathrm{S}Q''-\mathrm{V}Q'')(\mathrm{S}Q'-\mathrm{V}Q')(\mathrm{S}Q-\mathrm{V}Q),$$

a formula which, when combined with this other,

$$(\mathrm{S}+\mathrm{V}).QQ'Q''\ldots = (\mathrm{S}Q+\mathrm{V}Q)(\mathrm{S}Q'+\mathrm{V}Q')(\mathrm{S}Q''+\mathrm{V}Q'')\ldots,$$

enables us easily to develope S Π Q and V Π Q, that is, the scalar and vector of any product of quaternions, in terms of the scalars and vectors of the several factors of that product. For example, if we agree to use, in these calculations, the small Greek letters α, β, &c., with or without accents, as symbols of vectors (with the exception of π, and with a few other exceptions, which shall be either expressly mentioned as they occur, or clearly indicated by the context), we may form the following table:—

$$2\,\mathrm{S}.\alpha = \alpha - \alpha = 0; \qquad 2\,\mathrm{V}.\alpha = \alpha + \alpha = 2\alpha;$$
$$2\,\mathrm{S}.\alpha\alpha' = \alpha\alpha' + \alpha'\alpha; \qquad 2\,\mathrm{V}.\alpha\alpha' = \alpha\alpha' - \alpha'\alpha;$$
$$2\,\mathrm{S}.\alpha\alpha'\alpha'' = \alpha\alpha'\alpha'' - \alpha''\alpha'\alpha; \quad 2\,\mathrm{V}.\alpha\alpha'\alpha'' = \alpha\alpha'\alpha'' + \alpha''\alpha'\alpha;$$
$$\&\mathrm{c}. \qquad \&\mathrm{c}.$$

of which the law is evident.

21. The fundamental rules of multiplication in this calculus give, in the recent notation, for the scalar and vector parts of the product of any two vectors, the expressions,

$$\mathrm{S}.\alpha\alpha' = -(xx' + yy' + zz');$$
$$\mathrm{V}.\alpha\alpha' = i(yz' - zy') + j(zx' - xz') + k(xy' - yx');$$

if we make

$$\alpha = ix + jy + kz, \quad \alpha' = ix' + jy' + kz',$$

x, y, z and x', y', z' being real and rectangular co-ordinates, while i, j, k are the original imaginary units of this theory. The *geometrical meanings* of the symbols $\mathrm{S}.\alpha\alpha'$, $\mathrm{V}.\alpha\alpha'$, are therefore fully known. The former of these two symbols will be found to have an intimate connexion with the theory of *reciprocal polars*; as may be expected, if it be observed that the equation

$$\mathrm{S}.\alpha\alpha' = -a^2$$

expresses that *with reference to the sphere of which the equation is*

$$\alpha^2 = -a^2,$$

that is, with reference to the sphere of which the centre is at the origin of vectors, and of which the radius has its length denoted by a, *the vector* α' *terminates in the polar plane of the point which is the termination of the vector* α. The latter of the same two symbols, namely $\mathrm{V}.\alpha\alpha'$, denotes, or may be constructed by a straight line, which is in direction perpendicular to both the lines denoted by α and α', being also such that the rotation round it from α to α' is positive; and bearing, in length, to the unit of length, the same ratio which the area of the *parallelogram under the two factor lines* bears to the unit of area. The *volume of the parallelepipedon* under any three coinitial lines, or the *sextuple volume of the tetrahedron* of which those lines are conterminous edges, may easily be shown, on the same principles, to be equal to the *scalar of the product of the three vectors* corresponding; this scalar $\mathrm{S}.\alpha\alpha'\alpha''$, which is equal to $\mathrm{S}(\mathrm{V}.\alpha\alpha'.\alpha'')$, being positive or negative according as α'' makes an obtuse or an acute angle with $\mathrm{V}.\alpha\alpha'$, that is, according as the rotation round α'' from α' towards α is positive or negative. To express that two proposed lines α, α' are rectangular, we may write the following *equation of perpendicularity*,

$$\text{S.}\,\alpha\alpha' = 0; \text{ or } \alpha\alpha' + \alpha'\alpha = 0.$$

To express that two lines are similar or opposite in direction, we may write the following *equation of coaxality*, or of parallelism,

$$\text{V.}\,\alpha\alpha' = 0; \text{ or } \alpha\alpha' - \alpha'\alpha = 0.$$

And to express that three lines are in or parallel to one common plane, we may write the *equation of coplanarity*,

$$\text{S.}\,\alpha\alpha'\alpha'' = 0; \text{ or } \alpha\alpha'\alpha'' - \alpha''\alpha'\alpha = 0;$$

either because the volume of the parallelepipedon under the three lines then vanishes, or because one of the three vectors is then perpendicular to the vector part of the product of the other two.

[To be continued.]

VII. *On Cryptolite.* *By* F. Wöhler*.

THE new substance for which, from the hidden manner in which it occurs, I propose the name of cryptolite, is phosphate of the oxide of cerium. It has only been found as yet imperceptibly disseminated in the massive greenish or reddish apatite of Arendal in Norway. It becomes apparent when the apatite is placed in large pieces in dilute nitric acid, appearing, as the apatite dissolves, in the form of fine, parallel, crystalline needles of about a line in length. The apatite from which I obtained it, contained not more than 2 or 3 per cent.; it neither occurred in all the varieties, nor was it equally disseminated throughout the mass, but appeared chiefly confined to those parts having a red colour.

A complete description of cryptolite can only be given when it shall be found in distinct isolated specimens. For the present it can only be stated, that it crystallizes in transparent, apparently six-sided prisms of a very pale wine colour. Specific gravity about = 4·6. At a moderate red heat it undergoes no change.

Finding it to contain nothing but phosphoric acid and protoxide of cerium, with a mere trace of protoxide of iron, it was analysed by means of strong hot sulphuric acid, which acts upon it in the same manner as upon cerite.

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* Translated from Poggendorff's *Annalen*, No. 3, 1846, and communicated by Dr. E. Ronalds.

100 parts of cryptolite gave,—

Peroxide of cerium . .	73·70
Protoxide of iron. . .	1·51
Phosphoric acid . . .	27·37
	102·58

The excess arises from the cerium having been determined as peroxide instead of protoxide, in which latter form it evidently exists in the mineral. Whether the oxide of cerium is mixed with the oxides of didymium and lanthanium must at present remain undetermined, no sure method of separating these oxides from each other being known. The colour of the oxide obtained was dark cinnamon-brown, and evidently contained no zirconia or thorina. The absence of this latter distinguishes the mineral from monazite and edwardsite. Comparative experiments made with monazite and cryptolite left no doubt that they were perfectly distinct.

In conclusion, I may mention that cryptolite is either not quite insoluble in nitric acid, or the apatite of Arendal contains another cerium mineral which is soluble; it may possibly be monazite. On separating cryptolite and precipitating the saturated solution of apatite in nitric acid by an excess of oxalate of potash, washing and heating the precipitate to redness, and dissolving the carbonate of lime thus obtained in nitric acid, there remained a grayish earthy powder undissolved. Muriatic acid dissolved this powder, leaving a quantity of fluoride of calcium. The solution, evaporated down and mixed with a boiling-hot saturated solution of sulphate of potash, deposited in considerable quantity a powdery, pale, amethyst-coloured precipitate of the double salt of potash and cerium-oxide, which, decomposed by potash, gave a cinnamon-brown coloured oxide of cerium.

VIII. *On certain Criteria of Imaginary Roots of Equations. By* J. R. Young, *Professor of Mathematics Belfast College*[*].

At page 450 of vol. xxiii. of this Magazine, I gave certain new criteria for the imaginary roots of numerical equations. These, in conjunction with the long-neglected criteria of Newton, were shown to be of some value in facilitating inquiries into the character of the roots of an equation, as by their aid the mere inspection of the coefficients would often supply the information not otherwise to be obtained without a lengthy and laborious analysis. The formulas referred to

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[*] Communicated by the Author.

admit of extension: giving rise to other criteria that may sometimes apply where known tests fail, and which, as well on this account as for the purpose of giving completeness to the former set, it may be worth while to record.

The general numerical equation being

$$A_n x^n + A_{n-1} x^{n-1} + A_{n-2} x^{n-2} + A_{n-3} x^{n-3} + \dots A_1 x + A_0 = 0,$$

the criteria already investigated are

$$\begin{gathered} A_n A_{n-2} > A_{n-1}^2, \\ A_{n-1} A_{n-3} > A_{n-2}^2, \\ A_{n-2} A_{n-4} > A_{n-3}^2, \\ \vdots \\ A_2 A_0 > A_1^2, \end{gathered}$$

which are to be applied for the detection of imaginary roots exactly as the criteria of Newton are applied, and as explained in the volume above referred to. To give the proposed extension to these forms, we have only to multiply the several coefficients of the equation—in the manner so often employed by Newton and Maclaurin—by the terms of an arithmetical progression, as 1, 2, 3, &c., and to replace the original coefficients by the results: we shall thus have the following set of criteria to be applied in the same way as those above:—

$$\begin{gathered} 3A_n A_{n-2} > 4A_{n-1}^2, \\ 8A_{n-1} A_{n-3} > 9A_{n-2}^2, \\ 15A_{n-2} A_{n-4} > 16A_{n-3}^2, \\ \vdots \\ (n^2-1) A_2 A_0 > n^2 A_1^2. \end{gathered}$$

But these are only particular cases of the following more comprehensive forms, obtained by means of the general arithmetical progression k, $k+1$, $k+2$, &c.:—

$$\begin{gathered} k(k+2) A_n A_{n-2} > (k+1)^2 A_{n-1}^2, \\ (k+1)(k+3) A_{n-1} A_{n-3} > (k+2)^2 A_{n-2}^2, \\ (k+2)(k+4) A_{n-2} A_{n-4} > (k+3)^2 A_{n-3}^2, \\ \vdots \\ (k+n-2)(k+n) A_2 A_0 > (k+n-1)^2 A_1^2. \end{gathered}$$

and in which we may, if we please, write A_0, A_1, A_2, &c. in the place of A_n, A_{n-1}, A_{n-2}, &c., and *vice versâ*.

It will be observed that these criteria are really distinct from the former set, and are not comprehended in that set; for although whenever any one of these holds the corresponding one in the former set must hold also, yet one of these may fail without any failure in the corresponding form of the preceding set.

The latter forms, when expressed in words, furnish the following rule, which it will be easy to remember:—

Commencing with the second coefficient and proceeding towards the right, or with the last but one and proceeding towards the left, multiply the successive coefficients by any series of consecutive whole numbers, and square the results: the square of each coefficient will thus be multiplied by an integral square. Let the product of the adjacent coefficients, on each side, be now multiplied by the same square *minus* 1; and compare these results with the former, as in Newton's rule.

Of course criteria of this kind, which are to be applied immediately to the coefficients of the given equation, cannot always be expected to make known the exact number of imaginary roots entering that equation; yet our chances of obtaining this knowledge become multiplied with the number of our distinct and independent tests for the detection of such roots. At present we have no easily applied, and at the same time completely decisive tests of this kind which extend beyond an equation of the third degree; yet, as respects equations of the fourth degree, I think a little may be added in this way, as inferences from the following general expression for two roots of the biquadratic equation—

$$x^4 + px^2 + qx + r = 0,$$

when the other two x_1, x_2 are given:—

$$-\frac{x_1+x_2}{2} \pm \sqrt{\left\{\left(\frac{x_1-x_2}{2}\right)^2 + \frac{q}{x_1+x_2}\right\}}\,{}^{*}.$$

The inferences adverted to are these, viz.—

Supposing q positive, which is always allowable, we see from this expression that,—

1. If two real roots occur in the positive region, the other two roots must be real also.

2. If two imaginary roots are indicated in the negative region, the others must be imaginary. Therefore,

3. If two real roots are detected, without regard to their situation, and the remaining two are indicated in the negative region, these must be real also; and if two imaginary roots are indicated in either region, and the remaining two are indicated in the positive region, these must be imaginary also.

Belfast, June 11, 1846.

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[*] Analysis, &c. of Cubic and Biquadratic Equations, p. 235.

IX. *On the Use of the Word* Homology *in Comparative Anatomy.* *By* H. E. Strickland, *M.A., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

WHEN I recommended in a recent paper on the Structural Relations of Organized Beings (p. 358), that comparative anatomists should use the term *affine* or *homologous* in place of *analogous*, when speaking of structures which essentially correspond in different organized beings, I was quite aware, though I accidentally omitted to say so, that the word *homologous* had been occasionally used in that sense. I therefore by no means intended to imply that *homologous* was a word of my own coining, but I merely expressed a wish to see it, or its synonym *affine*, adopted uniformly in a technical sense, where it had already been adopted partially. But from what Prof. Owen says (p. 525), it is clear that the term *homology* has only been introduced into this country in the last four or five years, and by few if any authors besides himself. We may henceforth, however, hope to see the distinction between analogy and homology, a distinction clearly comprehended by the mighty mind of Aristotle (Hist. Anim. lib. i. c. 1.), but which has been and still is very imperfectly understood by many naturalists to the present time, become every day more strongly impressed on the minds of students, and implanted in their memories by exactness of definition and of nomenclature.

Of the two synonymous words *affinity* and *homology*, the latter appears preferable, as being constructed on a similar plan to its antithesis *analogy*. It is to be regretted therefore that the modern zoologists, to whom we are indebted for the first clear definitions on this subject, did not adopt the term *homology* instead of *affinity*, but the latter word is now so well established in systematic zoology, that it is perhaps too late to alter it. We may however be thankful to Prof. Owen for having been the first to introduce the convenient and useful word *homology* into the language of comparative anatomy in this country, as well as for the philosophical definitions which he has given of its several kinds.

I am, Gentlemen,
Your most obedient Servant,
H. E. Strickland.

Oxford, June 17, 1846.

X. *Note to the* Rev. WILLIAM VERNON HARCOURT'S *Letter to* Lord Brougham.

To Richard Taylor, Esq.

DEAR SIR,

HAVING in a contribution to the last volume of the Philosophical Magazine touched on certain views of the *evidence* of inductive philosophy, which I apprehend to be erroneous, I wish, with your permission, to explain in a somewhat fuller degree, though briefly, my own conceptions of the real nature of that evidence; and request you to subjoin these explanations as a supplementary note, either in the Magazine or in the *separate* publication, which you have been so kind as to propose, of my account of the discovery of the gases and of the composition of water.

I remain, dear Sir,
Yours faithfully,
Bolton Percy, June 15. W. VERNON HARCOURT.

The ideas which Mr. Macauley appears to entertain of inductive and probable evidence*, as depending on the *number* of instances, have obtained currency in some of our academical schools, chiefly, I believe, from the authority of a great moral and metaphysical writer, whose studies did not lie in the direction of natural and experimental philosophy. "A low presumption," says Bishop Butler, "often repeated, will amount to a moral certainty. Thus a man's having observed the ebb and flow of the tide today, affords some sort of presumption, though the lowest imaginable, that it may happen again tomorrow: but the observation of this event for so many days and months and ages together, as it has been observed by mankind, gives us a full assurance that it will†."

It is the greatest of Butler's merits, that on subjects of the most abstruse and important speculation, formerly discussed by *à priori* reasoning, he substituted the inductive for the hypothetical method, and argued with just precaution and due corrections from what *is* to what *probably, or not improbably, may be*. "Into the nature," however, "the foundation and measure of probability, it is not my design," he adds, "to inquire further; this belongs to the subject of logic, and is a part of that subject which has not yet been fully considered." Nevertheless since the soundness of all our practical judgements, and all our intellectual conclusions depend on our understanding well the grounds whereon they rest, it is of the utmost consequence to

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* See Phil. Mag., vol. xxviii. p. 513.

† Butler's Analogy, Introduction, p. 1.

disentangle, as far as we can, the knot into which three distinct questions—namely the investigation of the laws of nature, the prospect of their continuance, and the degrees of certainty and presumption on which we judge, and on which we act—are bound and mixed up together in the passages now referred to: let us then take the principles of evidence here assumed by Butler, and subject them to a strict analysis.

For this purpose we will vary the *subject* of his illustration. We will suppose a man to have observed, instead of the ebb and flow of the tide, a flash of lightning, or the fall of a stone from the air. Shall we say that the having observed one of these phænomena today affords some sort of presumption, though the lowest imaginable, that it may happen again tomorrow? On the supposition that we are in the dark concerning the causes and circumstances which determine these events, does not the doctrine of chances teach us, on the contrary, that there are an infinite number of presumptions to one, that the same event will not happen again tomorrow, or on any other day that can be named? The solitary event does indeed suggest to us, not "the lowest imaginable presumption," but the highest possible certainty, that there are causes in operation which *may* at any time produce the same result; but apart from *the suspicion of some permanent cause*, it does not afford a shadow of a presumption that the same event *will* happen again at all.

Dismissing then the notion that a certainty is a bundle of low presumptions from single facts, let us consider what the nature of the evidence really is which is furnished by the repetition of an event.

If a die, presumed to be on all sides of uniform weight, be thrown, there is no presumption that the same number will come up on the next throw, the presumptions being on the contrary as many against *that* as against any one of the numbers marked on the die. If however in *several successive throws* the same number *does* come up, a presumption *does* presently arise, and increases rapidly with the repetition of the throws, that the same number will continue to come up. The origin of this presumption is obvious. The violation of the *indifference* of the chances has indicated a definite cause, which is conceived to determine the result of the throw.

If I have seen the crater of a volcano constantly smoking for fifty years, I shall entertain an expectation of its smoking tomorrow: but the presumption that it will smoke tomorrow, or that it will continue to smoke for fifty years to come, is very far from being as *violent* as the presumption that a die which I have thrown fifty times with the like result will continue to show the same face fifty times more.

The distinction between the two cases is evidently no other than this. In the latter I have inferred an *invariable* cause, namely that of weight, as determining the event; in the former I have inferred that causes are operating, which I presume from the frequent recurrence of the event to operate with *some degree of permanence*, but of which the duration, till I can trace the laws and circumstances which govern the eruptions of volcanoes, is so doubtful and incalculable, that the cessation of the event is almost as likely as its continuance.

The ebb and flow of the tide, the rising and setting of the sun, suggest the same kind of thoughts as the determinate fall of the die: only in these cases it is the *generality* of the phænomena rather than their *precision* which guides the mind to its conclusion that *here is a law of nature*—a conclusion which once arrived at, from however few observations, produces a certainty that the event will continue, equal to the observation of ages.

But the amount of rational certainty is carried much further, when the mind rises to the consideration of a geometrical path for the sun or the earth, and has ascertained those precise and universal laws which regulate the movements of the heavenly bodies. By whatever means this knowledge is obtained, the assurance of the future event is carried by it to the highest pitch, and that often after very few observations indeed; as any one will see, who considers the great degree of certainty which a few sights of a comet, if we abstract the chance of disturbing forces, can give of its periodical return.

In all these cases it is evident that the real medium through which we connect the *actual* with the *future* is the apprehension of an *efficient*, and if we would probe the subject to the bottom, an *intelligent cause*. This is the principle, issuing from the inward analogies of our own minds, which lies at the foundation of philosophy, and gives force to inductive evidence; this is the principle which turns sequences into causes and effects, gathers individual facts into laws, and connects, so far as it is possible to connect, the past, the present, and the future.

Those who clearly perceive the truth here stated, will see that there is not the least ground for considering our expectation of the rising of the sun as an *ultimate fact* in the constitution of our minds, or for resolving a reasonable belief into an instinctive and implicit credulity in *the continuance of the laws of nature*.

What the instinctive process may be by which irrational beings are led to pursue a course of conduct conformable to that which in those endowed with reason proceeds from reasonable inference, is another question. It is also another question, what is the difference in the degrees of certainty required to satisfy the mind of the philosopher, and to regulate the conduct of the man. It is sufficiently plain that a wise man will not build his house on the edge of a volcano, however low the presumption may be that its eruptions will last.

The idea that there is any absolute certainty of *future* physical events rests on no grounds of reason. Mathematics have an abstract and absolute certainty of their own. The certainty of physics is absolute only as respects facts and laws that *actually* exist. Our views in regard to the future are necessarily, in natural knowledge, qualified and conditional, for the highest no less than the lowest of the presumptions which it contemplates. We believe that the sun rises and the tide flows, as the volcano smokes, in conformity with laws whether simple or complex, known or unknown, which will not lightly be changed. Among the causes of these effects, no rational philosopher ever overlooked the FIRST: and since we cannot calculate the course of HIS secret operations, we must be content to allow that however great the difference may be in the value of physical presumptions as establishing actual laws, there is no such thing as physical certainty for *the time to come.*

The certainties of the *actual* laws of nature are attained to by inductive observation. Where there is any regularity discernible in events, a few observations, on the principle above explained, indicate a cause. Then the business of the inductive philosopher is to investigate that cause, not by repeating the observation, from which he would gain neither light nor certainty, but by varying it till the possible causes of the event by the process of *elimination* are reduced to one. Even when for accuracy observations appear to be repeated, the value of the repetition consists in the presumed variation of the circumstances, by means of which the accidental errors of disturbing causes destroy one another.

In *experiments*, where the circumstances are diversified at our will, and we proceed, in the language of Bacon, to bind the Proteus, and force nature to deliver her oracles, the more dexterous and accurate the experimenter, the less need he has either to repeat, or vary, his experiments. The difference between a learned and an unlearned experimentalist, the advantage which a disciple of Bacon and Newton, and Black and Cavendish and Lavoisier, possesses over men uninstructed in the science of induction, is—that the former has learnt the art of cross-examining nature, and the latter has not. The difference between the man who has a genius for inductive philosophy, and the man who has none, is—that the one has a sagacity, which the other wants, in discovering *media* of proof, and driving his interrogatories *to a point*. Little or nothing depends on the multiplication of experiments, everything on the selection; and the only guides to selection are, first, a quick analogical perception—and, secondly, a just and sound appreciation—of the *causes* of phænomena.

Errata in Vol. xxviii.

Page 489, line 24, *for* 1764 *read* 1664.
... 492, ... 5, *for* "consumes it" *read* "it is consumed."
... 492, ... 36, *for* 1764 *read* 1664.
... 495, ... 43, *for* 1764 *read* 1664.
... 514, ... 5, *for* "duodecimo volume, of scarce a hundred pages" *read* "treatise of scarce seventy pages."

XI. *On some Chemical Effects produced by Platinum.*
By Dr. C. F. Schœnbein*.

SOME time ago I published an account of a series of experiments made with the resin of guaiacum, from which it appeared that the substance named is instantly rendered blue, not only by chlorine and nitrous acid, but also by bromine, iodine, ozone, and a number of metallic peroxides.

Free oxygen, be it pure or mixed with nitrogen, hydrogen, and carbonic acid gas, does not act in the dark upon that resinous matter, and comparatively very little when exposed to the action of solar light. From these facts, it becomes manifest that oxygen must have assumed a peculiar condition of chemical excitement before it is capable of causing the reaction mentioned. The beautiful experiments both of Davy and Dœbereiner have demonstrated that platinum has the power to occasion the oxidation of a number of substances under circumstances in which that chemical action would not take place without the agency of that metal. The blue coloration which the resin of guaiacum assumes under certain circumstances is most likely dependent upon a partial oxidation of that substance, and the latter being so very sensible to oxygen, that happens to be chemically excited, it could easily be conjectured that platinum in a state of minute mechanical division put in contact with the resinous substance mentioned, might cause the oxidation of the latter in the

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* Communicated by the Chemical Society; having been read December 1, 1845.

same manner as that metal occasions the oxidation of hydrogen, æther and alcohol. The facts I am going to state will show that the correctness of this conjecture is fully borne out by experiment.

Newly-prepared spongy platinum being placed upon a piece of filtering paper that had previously been drenched with an alcoholic solution of resin of guaiacum, caused rather rapidly the appearance of blue spots at the place where the metal had been in contact with the resinous solution. My experiments have further shown that that reaction takes place the more rapidly and intensely the more divided the platinum happens to be of which we make use in the experiment described. What is called Platinum Black acts therefore more energetically than spongy platinum does. From the facts stated, it appears that platinum in a state of minute mechanical division conducts itself towards resin of guaiacum like the simple halogenous bodies, ozone and a number of metallic peroxides. These facts demonstrate also that the coloration of the resinous matter being caused by platinum, belongs to that series of phænomena which takes place when that metal is put in contact with a mixture of oxygen and hydrogen, oxygen and vapour of æther, &c.

In the paper above alluded to, I have pointed out the remarkable coincidence that all the substances having the power of rendering blue the resin of guaiacum possess also the property of decomposing iodide of potassium, transforming the yellow prussiate of potash into the red one, and I add, decomposing sulphuretted and ioduretted hydrogen, transforming sulphurous acid into sulphuric acid, and destroying organic colouring matters. We shall presently see that platinum in a state of minute mechanical division has the same power.

If a crystal of pure iodide of potassium be put upon a piece of filtering paper that has previously been moistened with distilled water and spongy platinum be placed upon that paper, the spot touched by the metal assumes rather rapidly a brownish red colour. This coloration does not result from free iodine, but is most likely due to a compound consisting of periodide of platinum and iodide of potassium. That conjecture is founded upon the following facts. If a solution of iodide of potassium is put in contact with spongy platinum, or platinum black, the former assumes a perceptibly red colour, which disappears on heating the solution to its boiling-point. Now it is well known that the compound before mentioned yields with water a red solution, the colour of which is destroyed by heat. Neither the red solution nor the brownish-red spots before mentioned are able to render blue paste of starch, another proof that there is no free iodine in the case. The reaction described is most likely brought about in the following manner: the chemically excited oxygen surrounding the spongy platinum decomposes iodide of potassium, a peculiar peroxide of potassium being formed and iodine eliminated. The latter in its nascent state combines with platinum to produce the periodide of that metal, which itself unites with iodide of potassium into that compound, yielding with water a red solution. It is worthy of remark, that a solution of iodide of potassium having been treated with spongy platinum, enjoys the property of colouring blue the paste of starch on being mixed with dilute and pure sulphuric acid. The same reaction is exhibited by the same solution after it has been treated either with ozone or peroxide of lead. If some drops of a weak, *i. e.* colourless solution of the yellow prussiate of potash, be added to platinum black, that solution assumes a perceptibly yellow colour, and yields with a solution of chemically pure sulphate of protoxide of iron a blue precipitate.

From these facts, it seems to follow that platinum has the power to transform the yellow prussiate into the red one. Some years ago I tried to show that spongy platinum being placed in an atmosphere of sulphuretted hydrogen, loses its property of acting upon detonating gas, on account of a film of sulphur being deposited on the surface of the metal. Such being the case, it would follow that spongy platinum has the property to decompose sulphuretted hydrogen.

Colourless hydriodic acid, on being mixed up with some platinum black, assumes a brownish yellow colour, which reaction indicates an elimination of iodine.

Several chemists, particularly Dœbereiner, Phillips, and Brunner, have ascertained that spongy platinum produces sulphuric acid on being placed in contact with moist oxygen and sulphurous acid, and there is no doubt that platinum black put into sulphurous acid gives rise to the formation of sulphuric acid. Moist filtering-paper being coloured by a solution of indigo and put in contact with spongy platinum for about twenty-four hours, appears entirely bleached at those spots which had touched the metal. I have repeated this experiment more than thirty times, and always with the same result. This remarkable fact proves that platinum in a state of minute mechanical division, has the power of destroying organic colouring matters, and acts as a real bleaching agent. Before passing to other subjects, I must not omit to mention a circumstance which seems to me meriting some attention. It is a curious fact, which has not escaped the notice of chemists, that in more than one case platinum acts exactly like common electricity, both of them determining at the common temperature, for instance, the oxidation of free hydrogen. Now it being well known that nitric acid is formed if electrical sparks are made to pass through moist air, it seemed to me within the reach of possibility, that the same acid might be produced by platinum, if that metal in a state of minute mechanical division were placed in contact with moist atmospheric air. With the view of ascertaining the correctness of that conjecture, I put a piece of moist litmus paper in close contact either with spongy platinum, or with platinum black. In some cases part of the paper exhibited a slight reddish coloration, part of it proved to be entirely bleached, or nearly so. I must, however, not omit to state, that in the great majority of my experiments I obtained bleaching effects only, and no reddening of the litmus paper whatever. I am unable to account for the difference of the results mentioned. Was the reddening of the litmus paper caused by some traces of nitric acid formed under the circumstances indicated? I am not prepared at all to answer that question. If nitric acid should however happen to be produced under the circumstances mentioned, it would be a fact, in my opinion, not very difficult to be accounted for. In whatever state the oxygen surrounding platinum may be, certain it is that that state is such as to render oxygen very apt to combine at the common temperature with a number of oxidable substances that would not be oxidized by common oxygen without the presence of platinum. The formation of nitric acid taking place under the circumstances mentioned, would indeed be a fact very similar to the combustion of detonating gas caused by platinum. I repeat, however, that I consider the generation of nitric acid brought about by the agency of platinum, as far from being established by decisive facts.

The voltaic character of bodies being so intimately connected with their chemical nature, that in most if not in all cases we may infer the one from the other, the fact I am going to state merits our attention. Chlorine, bromine, iodine, ozone, and a number of metallic peroxides, enjoy considerable electro-motive powers, which are of such a kind as to render those bodies what is commonly called electro-negative. Hence it comes that a piece of metal being covered with any one of the bodies named, bears to another common piece of the same metal the same voltaic relation as copper does to zinc. According to the experiments of De la Rive and some other philosophers, platinum foil being covered with some spongy platinum, is negative to common platinum plate, a fact which proves that in a voltaic point of view there exists a great analogy between the simple halogenous bodies, ozone and metallic peroxides on one side, and spongy platinum on the other.

After having stated a number of facts which demonstrate the highly oxidizing powers of platinum, we ask in what manner does that metal exalt the chemical activity of oxygen? This question has occupied many philosophers, and been answered in very different ways. Faraday and Dœbereiner ascribe to platinum the power of condensing oxygen so much as to deprive that element of its gaseous condition, and think that condensed state to be the true cause of the oxidizing powers of platinum. Others (De la Rive and Gmelin) presume that oxygen is capable of chemically uniting with that metal, and with those philosophers it is the oxide of platinum that occasions the oxidation of hydrogen, &c. Berzelius holds the opinion that the oxidations caused by platinum are catalytical phænomena, *i. e.* effects produced by some unknown force being innate to that metal and exalting the chemical attractive powers of oxygen.

It is not my intention to enter into a discussion of those opinions; I shall confine myself to a few general remarks upon that interesting subject. As the common oxides of platinum are not acted upon (at the common temperature) by free hydrogen, as platinum foil or wire, that causes at a moderate temperature the combustion of detonating gas, exhibits a perfect metallic surface, while the thinnest film of an oxide diminishes or destroys the lustre of any metal, and as platinum is a body that has a very weak affinity for oxygen, I think that that metal is not capable of combining directly with the oxygen of the air, and that De la Rive's view of the subject is erroneous. But how is it with Faraday's and Dœbereiner's theory of the matter in question? It seems indeed to be the true one. Satisfactory however as that hypothesis may appear, it is nevertheless possible that the oxidizing action of platinum may depend upon a cause different from what the philosophers mentioned consider as such.

We know that phosphorus being put in contact with moist atmospheric air gives rise to the formation of a highly oxidizing agent, which, as it has been shown elsewhere, seems to be a peculiar compound of water and oxygen, and enjoys the power of oxidizing a great number of substances at the common temperature. Now if phosphorus enjoys that property, it seems possible that some other substances, for instance platinum and iridium, may do the same. In other terms, it appears possible that platinum has the power to engender out of free oxygen and water a highly oxidizing peroxide, which surrounding that metal empowers the latter to cause all the oxidations above mentioned. In comparing the chemical properties of platinum, when minutely divided, with those of ozone, we cannot help being struck by their great similarity, as will appear from the following statements:—

1. Both substances possess a negative electro-motive power.
2. Both of them destroy organic colouring matters.
3. Both of them render the resin of guaiacum blue.
4. Both of them decompose iodide of potassium.
5. Both of them change the yellow prussiate of potash into the red one.
6. Both of them transform sulphurous acid into sulphuric acid.
7. Both of them decompose oxalic and formic acids.
8. Both of them act in a similar way upon æther and alcohol.

Great as the similarity of properties may be, it does not follow that platinum owes its oxidizing powers to a film of peroxide of hydrogen being formed round the metal by a catalytical action of the latter. I have not yet succeeded in producing, by the means of spongy platinum and moist air, an atmosphere exhibiting the peculiar electrical smell, bleaching power and oxidizing properties which belong to ozone. Considering the great volatility of the last-named substance, we should suppose that it ought to disperse into the surrounding medium as soon as formed; or should ozone be retained by platinum in a way similar to that in which we think oxygen is attached to and condensed around that metal? Supposing oxygen to be an odoriferous substance, it is manifest that the oxygen actually condensed by platinum could not affect the olfactory nerves.

Another objection might be raised to the conjecture, that it is a film of peroxide of hydrogen, to which platinum owes its oxidizing powers, from the fact that Thenard's oxygenized water is really decomposed by platinum. Considering however that ozone is in some respects strikingly different from Thenard's compound, having for instance a peculiar odour, being insoluble in water, transforming both metallic silver and its basic oxide into a peroxide, it appears possible that ozone is capable of existing in the closest contact with platinum without suffering decomposition. I am not aware of spongy platinum or platinum black having been treated with anhydrous oxygen, and I do not know whether it has been ascertained if the latter is absorbed as easily by that metal as moist oxygen. It is equally unknown to me whether platinum, after being placed in dry oxygen or air, exhibits the same properties as platinum after being exposed for some time to the same gases when moist. If the oxidizing powers of platinum should depend upon a film of peroxide of hydrogen attached to that metal, it is obvious that spongy platinum, freed from its adhering water and placed within completely dry oxygen, could not assume oxidizing properties. Supposing however that spongy platinum acquires oxidizing powers under the circumstances mentioned, we may nevertheless imagine that those powers depend upon a film of peroxide of hydrogen surrounding that metal. De la Rive's and Marignac's experiments have shown that oxygen obtained from fused chlorate of potash, on being exposed to the action of electrical sparks, yields perceptible quantities of ozone. Now, taking that odoriferous substance for a peroxide of hydrogen, we must admit that even that oxygen, which is considered as absolutely anhydrous, still contains traces of aqueous vapour. Taking for granted the humidity of what is called dry oxygen, we may easily conceive how platinum brought into an anhydrous mixture of oxygen and hydrogen could cause the combustion of the latter. Out of some oxygen and the traces of water still contained in what is considered anhydrous detonating gas, a film of peroxide of hydrogen would be formed around the spongy platinum; that peroxide, in the very moment of its being engendered, would oxidize a neighbouring portion of free hydrogen. The heat resulting from that oxidation would determine another portion of hydrogen to unite with oxygen. The heat proceeding from that chemical union would occasion the combustion of an additional portion of hydrogen, and so on until the whole of the detonating gas should be consumed. The minute quantity of the peroxide of hydrogen attached to the spongy platinum would act like a small common or electrical spark, which, as we well know, is capable of setting the largest volume of detonating gas on fire. Electrical sparks acting upon a mixture of oxygen and hydrogen, exactly in the same way as spongy platinum does, and it being a well-ascertained fact that ozone makes its appearance on causing common electricity to pass through (moist) oxygen, it is possible that electricity and platinum occasion the oxidation of hydrogen, because both of them are able to produce ozone, and that it is to the agency of that odoriferous substance that we are to ascribe the chemical effect mentioned. That conjecture must become still more plausible, if we take into account the fact that spongy platinum acts in a variety of other cases exactly like ozone. Taking this view of the case, we could not admit that an electrical spark has the power to cause directly the formation of water out of detonating gas, but should be obliged to consider the oxidation as occasioned by the ozone being formed under electrical influence out of aqueous vapour and oxygen. In other terms, we are obliged to ascribe the oxidation to the same cause from which we derive the decomposition of iodide of potassium, the transformation of the yellow prussiate of potash into the red one, the destruction of vegetable colouring matters, the turning blue of the resin of guaiacum, the transformation of the protoxide of lead into the peroxide, &c., brought about by the electrical brush. Now, as it can hardly be doubted any longer that the chemical effects just stated are due to ozone produced by electricity, the conjecture, according to which free hydrogen may also be oxidized by electrical ozone, seems to be very probable. Indeed, if potassium, the hydrogen of colouring matters, the oxide of lead, &c., are oxidized by the oxygen of electrical ozone, why should free hydrogen make an exception to the rule?

But however that may be, the subject under discussion seems to be interesting, and I add, still obscure enough to offer an inducement to chemists to apply themselves to its elucidation by further investigations. The matter merits our attention the more that it bears so close a relation to that series of chemical phænomena which are called catalytical actions, and which certainly belong to the most enigmatical facts of our science.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxviii. p. 416.]

Feb. 12, 1846. "A PRACTICAL Extension of the Application of the Law of Mortality announced by B. Gompertz, Esq., in the Philosophical Transactions for 1823." By A. M. Drach, Esq. Communicated by B. Gompertz, Esq., F.R.S.

In endeavouring to verify the theoretical law of mortality, announced by Mr. Gompertz, by means of constants derived from the results of the English census of 1841, which are contained in the sixth Annual Report of the Registrar-General, and which furnish an extended basis for computation, the author found the accordance between the two to be so remarkably close as to justify the assumption that Mr. Gompertz's formula expresses the true law of the decrement of human life. The paper is occupied with the analytical details of this investigation.

"On Spontaneous Nitrification." By C. F. Schœnbein, Professor of Chemistry in the University of Bâle. Communicated by Michael Faraday, Esq., D.C.L., F.R.S., &c.

From various facts adduced by the author, he is led to the conclusion, that during the slow combustion of phosphorus in moist atmospheric air, while ozone is produced, there is also formed a quantity of nitric acid; and that in all cases where both these compound bodies are simultaneously generated, however different may be the concomitant circumstances of the experiment, there is strong reason to suspect that the formation of the one is in some way connected with that of the other.

"On the Process of Etching, or Engraving, by means of Voltaic Electricity." By James H. Pring, M.D. Communicated by P. M. Roget, M.D., Sec. R.S.

The author, referring to an account which he gave of his method of etching on hardened steel plates, or other polished metallic surfaces, by means of Electricity, in the Philosophical Magazine for November 1843, offers some additional observations relating to the theory of the process, and states some further practical remarks in its application to engraving. A specimen of a steel plate, and of a razor, on which ornamental designs were engraved by this method, were laid before the Society, in illustration.

February 19.—"On the Mechanism of Respiration." By Francis Sibson. Communicated by Thomas Bell, Esq., F.R.S.

This paper is almost entirely occupied with anatomical details, collected from an extensive series of dissections of the muscles and bones concerned in the act of respiration in man and the lower animals, for the purpose of elucidating the mechanism of their action both in inspiration and in expiration; accompanied by a great number of illustrative diagrams and drawings. The author commences with the serpent tribes, which present the simplest form of ribs, being attached only at their vertebral ends, while their anterior ends are free. When these ribs are brought forwards by the action of the levatores costarum and external intercostal muscles, the chest is expanded; and when drawn backwards by the long depressors, internal intercostals and transversales, expiration is effected. In birds there are added to the former apparatus a sternum, and a series of sternal ribs, the respiratory movements of which are performed in directions the reverse of those of the vertebral ribs. During inspiration, the angles between the vertebral and sternal ribs become more open; the sternum moves forwards, and the spinal column slightly backwards, by the combined action of the scaleni and sterno-costal muscles on the first vertebral and first sternal ribs respectively; of the levatores costarum and external intercostal on all the lower vertebral ribs, and of the sternal intercostals on all the lower sternal ribs. On expiration these movements are reversed by the action of the internal intercostals, the external and internal oblique, recti, transversales and other muscles. The mechanism in the Mammalia is further assisted and modified by the addition of a large and powerful diaphragm. The thoracic ribs are articulated with the sternum by the medium of cartilages corresponding to the sternal ribs of birds: those ribs which are connected with the inferior curve of the dorsal arch have floating cartilages, and may be considered as a diaphragmatic set of ribs. When raised, the former approach each other, and the latter recede from each other anteriorly. Intermediate to these are the longer ribs connected with the dorsal arch, having their cartilages united, and articulated with the lower end of the sternum. The scaleni muscles invariably act during the whole time of inspiration. The external intercostals between the thoracic ribs are also throughout inspiratory; but those portions which are situated between their cartilages are expiratory; and those between the diaphragmatic ribs are inspiratory behind, expiratory to the side, and in front, and inspiratory between their cartilages. Between the intermediate ribs, they are for the most part slightly inspiratory between the ribs, and expiratory in front, between the cartilages. The external intercostals of the thoracic ribs are expiratory behind, and inspiratory in front, if the ribs approach these, and are inspiratory between their costal cartilages. Between the diaphragmatic and intermediate sets of ribs, and between their cartilages they are throughout expiratory. The levatores costarum draw the posterior portion of the lower ribs backwards. In the ass and the dog, the upper fasciculi of the serratus magnus are expiratory, the lower inspiratory, and the intermediate neutral. In man, the greater part of the fasciculi of this muscle is expiratory. In the ass, the lower fibres of the serratus posticus inferior are inspiratory, and the upper fibres expiratory. In the dog and in man, all are throughout expiratory.

February 26.—"Illustrations of the Viscous Theory of Glaciers." By James David Forbes, Esq., F.R.S. &c. Part III.

The author inquires, in this part of his paper, into the motion of those comparatively small isolated glacial masses, reposing in the cavities of high mountains or on *cols*, and called by De Saussure *glaciers of the second order*. A glacier of this description in the neighbourhood of the Hospice du Simplon, lodged in a niche on the northern face of the Schœnhorn, immediately behind the Hospice, and at an elevation of about 8000 feet above the sea, was selected for observation. The average velocity of its descent was found to be about one inch and a half in twenty-four hours; those parts in which the slope was 20° moving with a velocity about one-third greater than those in which the slope was 10°. The author next enters into general views on the annual motion of glaciers, and on the influence of seasons; and gives tabular details of the observations made with reference to these questions at two stations; the one on the Glacier des Bossons, and the other at the Glacier des Bois, which is the outlet of the Mer de Glace towards the valley of Chamouni. In both these glaciers, the motion in summer exceeds that in winter in a greater proportion as the station is lower, and consequently exposed to more violent alternations of heat and cold. He also found that the variations of velocity due to season are greatest where the variations in the temperature of the air are greatest, as in the lower valleys; excepting that variations of temperature below the freezing-point produce scarcely any appreciable change in the rate of motion of the ice. He concludes with some general illustrations of the plastic or viscous theory of glacier motion. A glacier, he contends, is not a mass of fragments or parallelopipedons; neither is it a rigidly solid body; and although it may be extensively intersected by crevices, these "crevasses" are comparatively superficial, and do not disturb the general continuity of the mass in which they occur. The water contained in these crevices is only the principal vehicle of the force which acts upon it: and the irresistible energy with which the whole icy mass descends from hour to hour with a slow but continuous motion bespeaks of itself the operation of a fluid pressure acting on a ductile or plastic material.

March 5.—"On the Physics of Media that are composed of free and perfectly elastic Molecules in a state of Motion." By J. J. Waterson, Esq. Communicated by Captain Beaufort, R.N., F.R.S.

This memoir contains the enunciation of a new theory of heat, capable of explaining the phænomena of its radiation and polarization, and the elasticity of various bodies, founded on the hypothesis of a medium consisting of a vast multitude of minute particles of matter endowed with perfect elasticity, and enclosed in elastic walls, but moving in all directions within that space, with perfect freedom, and in every possible direction. In the course of these motions, the particles must be supposed to encounter one another in every possible manner, during an interval of time so small as to allow of their being considered infinitesimal in respect to any sensible period; still, however, preserving the molecular *vis viva* constant and undiminished.

The author then enters into extensive analytical investigations; first, of the conditions that determine the equilibrium of such a homogeneous medium, as is implied by the hypothesis, and of the laws of its elasticity; secondly, of the physical relations of media that differ from each other in the specific weight of their molecules; thirdly, of the phænomena that attend the condensing and dilating of media, and of the mechanical value of their molecular *vis viva*; fourthly, of the resistance of media to a moving surface; fifthly, of the vertical equilibrium of a medium surrounding a planet and constituting its atmosphere; and lastly, of the velocity with which impulses are transmitted through a medium so constituted.

In an Appendix, the author enters into a full explanation of a table of gases and vapours, drawn up with reference to the subjects discussed in his paper.

March 12.—"On the Blow-hole of the Porpoise." By Francis Sibson, Esq. Communicated by Thomas Bell, Esq., F.R.S.

The external opening of the air-passage of the porpoise is so situated at the upper part of the head as to admit of the animal's breathing while only a small portion of its head is above the water. In its descent through the skull, between the cranial and facial bones, the tube is divided by a thin plate of bone into two nasal canals, which form, below this partition, a single muscular tube opening at its lower part into the pharynx by a constricted aperture, through which the larynx projects upwards quite through the pharynx, dividing it into two channels. A series of pouches, five in number, capable of great dilatation, and provided with a muscular apparatus for retaining or expelling their contents, communicate by large orifices with the nasal canals, and appear to correspond in situation with the antra, frontal sinuses and ethmoid cells. The author gives a minute anatomical description of these muscles, and an account of their modes of action; the adjustments of the apparatus being such that the outer passage may be closed or opened above or below the anterior pouches. When the outer passage is closed, the posterior pouches can be distended and the anterior emptied; while the converse may be effected when the passage is open. The use of the pouches appears to be to buoy up the head, so that on the porpoise rising from deep water, the opening for breathing comes first to the surface and admits of the animal's sleeping in that position, while its whole body remains immersed in the water.

"On Motion in the Lumbar Division of the Spine in Birds." By George Oakley Fleming, M.D., F.L.S. Communicated by Thomas Bell, Esq., F.R.S.

The author gives quotations from the works of Cuvier, Blumenbach, Tiedemann, Macartney, Vicq d'Azyr, Carus, Earle, and Roget, in proof of its being the prevalent opinion among comparative physiologists that the dorsal and lumbar portions of the spine form altogether a rigid structure, not admitting of the least perceptible flexion. But from his observations of the form of the articulating surfaces of the lumbar vertebræ, which appear to be adapted to lateral motion, the author was led to conclude that, although flexion in the mesial plane is effectually prevented, some degree of lateral flexion actually takes place. The number of articulations in this part of the spine, he observes, varies in different birds: thus in the sea-gull, there are several articulations in the dorsal and lumbar portions; while, in the peacock, there is only one moveable vertebra; the remaining dorsal being united together, and all the lumbar vertebræ being consolidated and anchylosed with the sacrum; thus forming two firm and immoveable pieces between which the moveable vertebra is placed. The flexion of the spine forwards is prevented by the great breadth of the spinous processes and their projections at right angles to the bodies of the vertebræ; and frequently also by the addition of a number of thin, flat long bones which are applied by their flat surfaces on each side of the spinous processes; and also by strong flat ligaments, situated between each spinous process, like the ligamentum nuchæ of herbivorous quadrupeds. For the purpose of guarding against pressure on the spinal cord during the lateral flexion of this part of the spine, the spinal canal is enlarged laterally at the centres of motion. The paper is illustrated by drawings of the parts described.

March 19.—"Investigation of the Power consumed in overcoming the Inertia of Railway Trains, and of the Resistance of the Air to the motion of Railway Trains at high velocities." By P. W. Barlow, Esq., F.R.S., M.I.C.E.

The object of the author in this inquiry is to obtain a more correct knowledge than has hitherto been possessed of the resistances which the air opposes to the motion of locomotive engines at great velocities, and of the loss of force arising from increased back pressure and the imperfect action of the steam. For this purpose he institutes a comparison between the velocities actually acquired by railway trains with those which the theory of accelerated motion would have assigned; and his experiments are made not only on trains propelled by a locomotive engine, but also on those moving on the atmospheric railway, which latter affords valuable results, inasmuch as the tractive force is not subject to the losses at high velocities necessarily incident to locomotive engines. A table is given of the theoretical velocities resulting from calculation founded on the dynamical law of constant accelerating forces, in the case of trains of various weights, impelled by different tractive forces, moving from a state of rest; and is followed by another table of the observed velocities in Mr. Stephenson's experiment on the Dalkey line; the result of the comparison being that in a distance of one mile and a quarter, the loss of velocity is about one-half of the observed velocity.

A series of experiments on locomotive lines is next related; but the comparison is less satisfactory than in the former case, because the tractive force cannot be so accurately estimated; it is however sufficiently so to establish the fact, that the power lost by the locomotive engine below the speed of thirty miles per hour is so small as to be scarcely appreciable, and that the time and power which are absorbed in putting a railway train in motion are almost entirely required to overcome the inertia of the train, and do not arise from any loss or imperfection of the engine. It appears, from these experiments, that above one-fifth of the whole power exerted is consumed in putting the train in motion at the observed velocity. The author then enters into some general remarks on the effects arising from this source of loss of power, and the practical application of the knowledge thus obtained. In the atmospheric railway, he finds that the tractive force of a fifteen-inch pipe is so small (being less than half that of a locomotive engine), that the time of overcoming the inertia must limit the amount of traffic which can be carried on a single line, especially with numerous stations. When a great velocity is obtained, the tractive force of the locomotive is much reduced, and therefore a much greater velocity can be attained on an atmospheric railway.

The inquiries of the author into the amount of resistance exerted by the air on railway trains lead him to the conclusion that in the atmospheric railway the loss of tractive power of the piston from friction, &c. is very inconsiderable; and that the resistance of the air is less than had been hitherto estimated, not exceeding, on an average, ten pounds per ton on the average weight of trains.

A tabular statement is then given of the results of the experiments made by the British Association, for the purpose of comparing them with those obtained by the author from his own observations, and more particularly from his experiments on the Croydon Atmospheric Railway. The general conclusion which he draws from this investigation is that the resistance of the air in a quiescent state is less than had been previously estimated, and that the ordinary atmospheric resistance in railway progression arises from the air being generally itself in motion, and as the direction of the current is almost always oblique, from its producing increased friction in the carriages themselves. This kind of resistance will not increase as the square of the velocity; and as it is the principal one, it follows that the resistance to railway trains increases in a ratio not much higher than the velocity, and that the practical limit to the speed of railway travelling is a question, not of force, but of safety.

March 26.—"On the Muscularity of the Iris." By Professor Maunoir, of Geneva. Communicated by P.M. Roget, M.D., Sec. R.S.

The author has satisfied himself, from the result of his own dissections, as well as from the concurrent testimony of a great number of anatomists, that the iris is provided with two sets of muscular fibres, the one orbicular, immediately surrounding the pupillary margin and acting as a sphincter; the other, extending in a radiated direction from the exterior circumference of the former to their insertions into the ciliary ligament, their action being to enlarge the pupil. One-fourth of the disc of the iris is occupied by the orbicular, and the remaining three-fourths by the radiated muscle. The author has examined the structure of the iris in a great number of animals, and states the results obtained by M. Lebert, whom he applied to on this occasion, from numerous dissections of the eyes of animals belonging to each class of vertebrata. He also refers to a work which he published in the year 1812, entitled "Mémoire sur l'Organisation de l'Iris," for evidence of the muscularity of the iris which he obtained by applying galvanism to the human eye immediately after decapitation; and he concludes with the narrative of the case of a woman in whose iris there had been formed, by an accidental wound with the point of a knife, a triangular aperture below the pupil. This aperture became dilated when the pupil was contracted, and *vice versâ*; thus furnishing a proof that its movements were effected by muscular action.

April 2.—Contributions to the Chemistry of the Urine. Part II. "On the Variations in the Alkaline and Earthy Phosphates in Disease." By Henry Bence Jones, M.D., Fellow of the Royal College of Physicians. Communicated by Thomas Graham, Esq., F.R.S., &c.

The analyses, of which the results are given in a series of tables, were made by the author, chiefly from the urine of patients labouring under different diseases in St. George's Hospital, and therefore nearly under the same circumstances as far as exercise was concerned. He found that the variations in the earthy phosphates were in general independent of the nature of the disease. In fractures of the spine and paraplegia, however, the total amount of these salts was slightly above the healthy standard during the early period, and when inflammatory action might be considered as prevailing; but when this action had subsided, and the affection had become chronic, the total quantity of phosphatic salts was less than natural. In chronic diseases of the brain, and in chronic and even in acute diseases of the membranes, no increase of these salts was observed.

In fractures of the bones of the skull, when inflammation of the brain supervened, there was a slight increase of the total amount of phosphates; but no such increase occurred when the head was not affected, even although acute inflammation of other organs existed. In acute inflammation of the brain there was an excessive secretion of phosphates, which returned to the natural quantity as soon as the inflammation passed into the chronic state. In some functional diseases of the brain, attended with delirium, the secretion of the salts was excessive; but the excess ceased with the disappearance of that symptom. In other functional diseases, as in fevers, no excess was observable. In delirium tremens, when food could be taken, there was neither excess nor deficiency; but in the most violent cases, where no food could be taken, the quantity of the phosphates was diminished in a most remarkable degree. In the general paralysis of the insane, no increase of phosphates was observed. One case of acute paroxysm of mania showed a small increase during the paroxysm; in two other cases of mania there was a diminution of phosphates approaching to that occurring in delirium tremens. Bright's disease, even attended with acute inflammation, showed no increase. When only a few ounces of urine were secreted, as in dropsy, no increase was observed; and none also in a very extreme case of exostosis. In the case of mollities ossium, there was a decided increase of the earthy phosphates; and at last, the alkaline phosphates were also in excess, although there was no indication of affections of the nervous structures.

The following are the general conclusions which the author draws from his inquiries: first, that acute affections of the nervous substance, organic and functional, are the only diseases in which an excess of phosphatic salts appears in the urine; and in acute inflammation of the brain, its amount is proportional to the intensity of the inflammation; secondly, that in a large class of functional diseases of the brain, of which delirium tremens presents the most marked example, the secretion of phosphates is most remarkably diminished; and lastly, that no chronic disease exhibits any marked excess in the total quantity of phosphatic salts secreted, at least as far as the mode of analysis employed by the author can be regarded as conclusive.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxviii. p. 229.]

January 9, 1846.—The following communications were read:—

Announcements of the Discovery of the new Planet Astræa; with Observations, Elements, &c.

The addition of a new planet to the solar system is a fact so interesting and important in astronomy, as to require that the numerous communications of which it has already been the subject should be treated and discussed in the publications of this Society with a greater regard to classification and arrangement than is necessary, or indeed always practicable, in other cases of less prominent interest. Instead, therefore, of giving an abstract of each separate communication that has been received respecting the new planet Astræa, it is proposed to give, first, a brief historical notice of its discovery, and of the manner in which the search after it was prosecuted; secondly, a tabular statement of the observations of the planet which have been received; and, thirdly, the elements which have been computed. With regard to its history, the first fact that occurs to us, as equally creditable to its discoverer and instructive to amateur astronomers and others who are desirous to extend the bounds of astronomical science, is, that its discovery was by no means accidental, but the reward of long and well-directed search. Perhaps many persons are apt, on occasions like the present, to regard the discovery of any new body in our system as a lucky accident, resulting from a casual or careless sweep among the stars, though the fact with regard to the greater number of such discoveries is directly the opposite of this. The four asteroids which have been for years recognised as belonging to our system, were the reward of three or four successive steps of true philosophical inquiry. An obvious analogy existed in the distances of the primary planets from the sun, which, though not the result of any known law of nature, was yet sufficiently evident in fact to draw the attention of astronomers to its equally remarkable failure at one particular distance, viz. that at which those bodies were afterwards discovered. It was at least worthy of an attempt at its verification; and such was accordingly instituted by several astronomers. Lambert appears first to have suggested the idea of the existence of a yet undiscovered body, and Bode's celebrated empirical law was published by him in 1772; but no serious attempt by means of co-operation was made to effect its discovery till the autumn of the year 1800, when an Association of twenty-four astronomers was formed, having Schröter for their president and Zach for secretary, who engaged to observe thoroughly every star visible within the zodiacal limits. The announcement of the discovery of Ceres by Piazzi, on Jan. 1, 1801, was made to Lalande, Bode, and Oriani on the 24th of January following, very soon after the formation of this Association*, and its planetary nature was soon recognised. This discovery was speedily followed by that of Juno by Harding, and of Pallas and Vesta by Olbers†. The last planet, it is well known, was discovered through a search suggested by the curious yet natural hypothesis of the bodies being fragments of a large planet which had been shattered into fragments. Uranus also, to use Sir John Herschel's own words, "was discovered by Sir W. Herschel, in the course of a review of the heavens, in which every star visible in a telescope of a certain power was brought under close examination, when the new planet was immediately detected by its disc, under a high magnifying power."

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* It is a singular fact that Piazzi was not a member of this Association, and that, in spite of such an organisation, the discovery was, in fact, accidental.

† Pallas was discovered in 1802; Juno in 1804; and Vesta in 1807.

Such, briefly, is the well-known history of the discovery of those planets of our system which required the aid of optical resources and of persevering search among the stars for the detection of their existence; and it is interesting to find that we are indebted for our knowledge of Astræa to a similar sagacity and perseverance. The discoverer, M. Hencke, of Driessen in Prussia, is a gentleman who, at one period of his life, was employed in the post-office of that town, but who, being gifted with a taste for astronomical pursuits, has, for the last fifteen years, been rendering himself familiar with the general features of the heavens, for the express purpose of discovering such a body as has now rewarded his exertions. The circumstances which attended the discovery of the other four asteroids rendered it, antecedently, extremely probable that others yet remained to be detected; and the difficulty lay in conducting a search of such a nature. The body to be discovered would be probably of a brightness equivalent to a star of from the 8th to the 10th magnitude, and the only sensible circumstance in which it would differ from a star would be its motion. But the motion of a body can be detected only by comparisons between its situations on different days; and there would be nothing to direct the choice of the objects to be tried amongst the hundreds that one sweep of the telescope would present to the observer. Nothing, then, it is evident, but a complete familiarity with the part of the heavens under review, and a knowledge of the relative positions of all the stars in it, to the limits of the lowest magnitude above-specified, would suffice to assure the observer of the presence of an object in a particular position on one evening which did not occupy that position on a preceding. M. Hencke, while examining a portion of the heavens in the fourth hour of R. A., on December 8, was immediately aware of the presence, directly between two stars of the 9·10th magnitude, marked on the Berlin maps, and denoted by the positions

$$\left.\begin{array}{ll}\text{R.A. ....} & 4^h\ 18^m\ 45^s \\ \text{N.P.D..........} & 77^\circ\ 18'\ \ 1''\end{array}\right\}$$

$$\text{And }\left.\begin{array}{ll}\text{R.A. ....} & 4^h\ 20^m\ 20^s \\ \text{N.P.D..........} & 77^\circ\ 28'\ \ 9''\end{array}\right\}$$

of a star of the 9th magnitude, not marked on the maps; and, from his familiarity with this part of the heavens, he felt assured that the star did not previously exist there. He wrote immediately to Professor Encke, and soon afterwards to Professor Schumacher (the letter to Schumacher was received by him December 13), announcing his suspicions of the discovery of a new planet, and giving the position of the star in question for the time of his observation, viz.—

Dec. 8, 8^h Berlin mean time.

$$\begin{array}{ll}\text{R.A. ....} & = 65^\circ\ 25' \\ \text{N.P.D. ....} & = 77\ \ 19\end{array}$$

Professor Encke and Professor Schumacher immediately made public M. Hencke's communication; and we are indebted to them for the principal sources of our information relating to the discovery, by letters to the Astronomer Royal and Mr. Hind, dated respectively December 15 and December 16. It is well known with what interest the intimations of the discovery were received in England, and the personal zeal of the Astronomer Royal in immediately circulating the notice which he had received by every possible means, needs no comment.

Encke's elements are,—

Epoch of mean longitude, 1846, January 0, 0^h, Berlin mean time 94° 48′ 11·8″

Mean anomaly	319 2 54·8	
Long. of the perihelion ...	135 45 17·0	Mean equinox, 1846, Jan. 0.
Longitude of the node ...	141 10 6·7	
Inclination	5 20 7·2	
Angle of eccentricity......	11 16 30·4	$e = 0·195520$
Log. semi-axis major	0·413564	
Mean daily motion	850″·473	

Time of revolution, 1524 days.

Elements by Mr. Hind, deduced by Gauss's Method from the Observations at Berlin on December 14; at Altona, on December 17; and at Mr. Bishop's Observatory, on December 24. They are only a rough approximation sufficient to give a general idea of the orbit.

Epoch 1846, January 1·0, Greenwich mean time.

	° ′ ″	
Mean longitude	87 16 41	
Long. of perihelion on orbit.	142 9 50	Mean equinox, Jan. 1·0.
Long. of ascending node ...	138 41 16	
Inclination	5 37 15	
Angle of eccentricity	6 9 46	
Log. semi-axis major	0·4044914	

Period, 1477 days.

On the Periods of the Satellites of Saturn. By S. M. Drach, Esq.

The author has extracted the periods and daily motions of the satellites of Saturn from Maëdler's Astronomy, in confirmation of the curious law announced by Sir J. Herschel. He observes that perturbations of the fifth order must often occur, and, in conclusion, proposes the following question:—"Has the action of the ring caused the *exact* duplication of periods not to take place? and has the absence of this appendage, in the case of Jupiter, allowed the absolutely rigorous formula $n_1 - 3n_2 + 2n_3 = 0$ to exist?"

On the Reduction of the Sextant Observations of the Distance of the Great Comet of 1843 from bright Stars. By the Rev. R. Main, one of the Secretaries of the Society.

In the introduction to this paper the author drew the attention of the meeting to the remarkable zeal with which cometary observations and calculations had been pursued during the last few years, both in this country and on the continent. In proof of this, he stated that the fifth and sixth volumes of the Society's Monthly Notices (commencing with the year 1840) contain announcements of the discovery of 13 comets, and that the number of communications received by the Society respecting them amounted to 140. The greater number of these comets were telescopic; but there were three (viz. the Great Comet of 1843, that of 1844–45, and the second Great Comet of 1845) which were visible to the naked eye, and therefore capable of being observed with instruments of the smallest optical power. A considerable number of sextant observations of each of these comets had been communicated, but especially of the comet of 1843, made chiefly by naval officers well-accustomed to the use of that instrument. The number of these observations was so great, that it was deemed proper by the Council, in the beginning of the year 1844, to appoint a committee to provide for their discussion and reduction; and the author, in conjunction with Mr. Galloway and Mr. Stratford, undertook this responsibility. The task of performing the computations was given to Mr. Harris, the late Assistant Secretary; and the author took upon himself the preparation of the necessary formulæ and the arrangement of the steps of the calculations. The author took occasion, in this part of the paper, to point out to the meeting how satisfactorily Mr. Harris had performed the work entrusted to him, the errors detected in a minute examination of it being far fewer than might reasonably be expected in a mass of work of so laborious and troublesome a character. Mr. Main then proceeded to give an account of the observations which had been reduced, and an explanation of the different steps of the processes employed. With very few exceptions, the observations are contained in the fifth and sixth volumes of the Monthly Notices, and the total number of results deduced from them was 199; and in a table is given the page of the volume at which the observations under discussion are to be found, together with the place and circumstances of observation, &c. Mr. Main then proceeded to mention in detail the principal processes of the calculation. The principal step that needs mention here is the formula which has been employed for correcting the observed distance from the effects of refraction and parallax. It was shown by a mathematical investigation, that if Z and z be the zenith distances of the comet and one of the stars of comparison, and the vertical refractions be $\alpha_c \tan Z$ and $\alpha^{s} \tan z$; if, also, Π be the horizontal parallax of the comet, and d the distance of the comet from the star, then the whole correction of the distance will be,

$$(\alpha_c - \Pi \cos Z) . \left(\frac{\cos z}{\cos Z} - \cos d\right) + \alpha_s \left(\frac{\cos Z}{\cos z} - \cos d\right).$$

After the computation of the approximate zenith distances of the comet and stars, this formula was found to be of very easy application. It was also explained how the values of the barometer and thermometer readings used in the work had been conjecturally supplied in cases (the greater number were such) where they had not been given by the author.

With respect to the deduction of the R. A. and N. P. D. of the comet from the corrected distances, the author first gave the following theoretical solution, which, though of no direct practical utility, may be interesting to the mathematician.

Let Δ, Δ', and σ be the N.P.D.'s and distance of the stars of comparison; ϖ the N.P.D. of the comet; d and d' the observed corrected distances; then,

$$\left.\begin{array}{r}\sin^2\Delta\sin^2 d'\\ +\sin^2\Delta'\sin^2 d\\ +\sin^2\sigma\sin^2\varpi\end{array}\right\} + 2\left\{\begin{array}{r}\cos\Delta\cos\varpi\,(\cos d-\cos d'\cos\sigma)\\ +\cos\Delta'\cos\varpi\,(\cos d'-\cos d\cos\sigma)\\ +\cos d\cos d'\,(\cos\sigma-\cos\Delta\cos\Delta')\\ +\cos\Delta\cos\Delta'\cos\sigma\end{array}\right\} = 2.$$

Again, if the difference of R. A. of the star whose N. P. D. $= \Delta$ and the comet be x, and the angle at the comet subtended by that star and the pole be x'; α the difference of the R. A.'s of the stars, and β the angle subtended by them at the comet; then,

$$\frac{\cot x' - \cot\beta}{\cot x - \cot\alpha} = \frac{\sin\Delta'\sin\alpha}{\sin d\sin\beta}$$

and

$$\frac{1+\cot^2 x}{1+\cot^2 x'} = \frac{\sin^2\Delta}{\sin^2 d},$$

from which two equations $\cot x'$ may be eliminated, and a quadratic equation will result for the determination of x.

The author then proceeded to explain the practical rules which had been given for the solution of the spherical triangles requisite to determine the R. A. and N. P. D. of the comet.

In conclusion he animadverted upon several imperfections in the observations which had been discussed, and particularly on the want of sufficient explanation of several circumstances materially affecting their accuracy.

Extract of a Letter from C. Piazzi Smyth, Esq., to the President, Dec. 1845, accompanying some drawings of the Great Comet of 1843.

The drawings in question are in Indian ink, and comprise five naked-eye views of the Great Comet on the nights of March 3rd, 4th, 9th, 18th, and 25th; and six telescopic views of its head on the nights of March 3rd, 4th, 6th, 9th, 22nd, and 31st.

The former are 6·4 by 5 inches, containing 57° of altitude and 45° of azimuth; the latter are in circular spaces, 0°·5 in diameter, on a scale of 2·7 inches to 1°.

The writer remarks on the extensive and regularly progressive character of the changes of the comet's appearance in both series of drawings, and especially (as being directly contrary to the general opinion) on the concavity of the tail to the direction in which the body was proceeding; its angular advance on the line passing through the sun and the nucleus; and its continual increase in length: all the observations being subsequent to the perihelion passage.

He remarks also on the manner in which the principal phænomena seem to be accounted for by Sir John Herschel's mathematical theory (published in the sixth volume of the Memoirs); the very simple nature, or rather the reasonableness of that theory; the impossibility of refusing the little that is assumed in it; and the important characteristic which it has now acquired of being a true theory, inasmuch as, being framed to suit one class of comets, viz. round telescopic ones, it is found, without any addition, to apply to a class at first sight totally different, viz. the tailed comets, of which the great one of 1843 is so extreme an example.

Not only then does there now seem to be a chance, by pursuing the usual method of astronomical inquiry (comparing prediction with numerical observation), of ascertaining the laws of these apparently most capricious phænomena, but even of proving whether, though so diverse from everything else in our system, they are regulated by the theory of planetary gravitation.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxvii. p. 229.]

April 14, 1845.—On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids. By G. G. Stokes, M.A., Fellow of Pembroke College.

The theory of the equilibrium of fluids depends on the fundamental principle, that the mutual action of two contiguous portions of a fluid is normal to the surface which separates them. This principle is assumed to be true in the common theory of fluid motion. But although the theory of hydrostatics is fully borne out by experiment, there are many instances of fluid motion, the laws of which entirely depend on a certain tangential force called into play by the sliding of one portion of fluid over another, or over the surface of a solid. The object of the first part of this paper is to form the equations of motion of a fluid when account is taken of this tangential force, and consequently the pressure not supposed normal to the surface on which it acts, nor alike in all directions.

Since the pressure in a fluid, or medium of any sort, arises directly from molecular action, being in fact merely a quantity by the introduction of which we may dispense with the more immediate consideration of the molecular forces, and since the molecular forces are sensible at only insensible distances, it follows that the pressure at any point depends only on the state of the fluid in the immediate neighbourhood of that point. Let the system of pressures which exists about any point P of a fluid in motion be decomposed into a normal pressure p, alike in all directions, due to the degree of compression of the fluid about P, and a system S of pressures due to the motion. The author assumes that the pressures belonging to the system S depend only on the relative velocities of the parts of the fluid immediately about P, as expressed by the nine differential coefficients of u, v and w with respect to x, y and z. [The common notation is here employed.] He assumes, further, that the relative velocities due to any arbitrary motion of rotation may be eliminated without affecting the pressures of the system S. Choosing for the motion of rotation that for which the angular velocities are $\frac{1}{2}\left(\frac{dw}{dy}-\frac{dv}{dz}\right)$ about the axis of x, with similar expressions for the axes of y and z, the residual relative motion depends on only six independent quantities. Considering only this residual relative motion, the author shows that there are always three directions, which he names *axes of extension*, at right angles to one another, such that if they be made the axes of x_1, y_1, z_1, the resolved parts of the relative velocity of the point P′, whose relative co-ordinates are x_1, y_1, z_1, will be $e' x_1$, $e'' y_1$, $e''' z_1$ along the three axes of extension respectively, the point P′ being supposed indefinitely near to P. Thus the system of pressures S is made to depend on the three quantities e', e'', e''', which in the case of an incompressible fluid are connected by the equation $e' + e'' + e''' = 0$. Moreover, on account of the symmetry of the motion, the pressures on planes perpendicular to the axes of extension will be normal to those planes. They will here be denoted by p', p'', p'''.

By what precedes, any one of these pressures, as p', will be expressed by $\phi(e', e'', e''')$, the function ϕ being symmetrical with respect to the second and third variables. For reasons stated in the paper itself, the author was led to take, as the form of the function ϕ, $\varsigma e' + \varsigma' (e'' + e''')$. The general expressions for the pressures would thus contain two arbitrary constants (or rather functions of the pressure and temperature), which in the case of an incompressible fluid would unite into one. But it is shown by the author, that in all probability $p' = 0$ when $e' = e'' = e'''$; and he accordingly makes this assumption, which reduces the two constants to one, even in the case of a gas. The expression for p' finally adopted is $\frac{2}{3} \mu (e'' = e''' - 2 e')$.

The pressures on three planes passing through P being known, the pressure on any other plane passing through that point may be found by the consideration of the motion of an indefinitely small tetrahedron of the fluid. Thus expressions are obtained for the pressures on planes parallel to the co-ordinate planes. These expressions, however, contain quantities which refer to the axes of extension; and it is necessary to transform them into others containing quantities which refer to the axes of co-ordinates. This transformation is easily effected by means of an artifice, and then no difficulty remains in forming the equations of motion. When μ is supposed to be constant, a supposition which it is shown may in many cases be made, the equations thus obtained are those which would be obtained from the common equations by subtracting

$$\mu \left(\frac{d^2 u}{dx^2} + \frac{d^2 u}{dy^2} + \frac{d^2 u}{dz^2} \right) + \frac{\mu}{3} \frac{d}{dx} \left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \right)$$

from $\frac{dp}{dx}$ in the first, and making similar changes in the other two.

The particular conditions which must be satisfied at the boundaries of the fluid are then considered, and the general equations applied to a few simple cases.

On considering these equations the author was led to observe, that both Lagrange's and Poisson's proofs of the theorem that $u\,dx$

$+ v\,dy + w\,dz$ is always an exact differential when it is so at any instant (the pressure being supposed equal in all directions), would still apply, whereas the theorem is manifestly untrue when the tangential force is taken into account. This led him to perceive that one objection to these proofs is of essential importance. He has given a new proof of the theorem, which however was not necessary to establish it, as it has been proved by M. Cauchy in a manner perfectly satisfactory.

The methods employed in this paper in the case of fluids apply with equal facility to the determination of the equations of equilibrium and motion of homogeneous, uncrystallized, elastic solids, the only difference being that we have to deal with relative velocities in the former case, and with relative displacements in the latter. The only assumption which it is necessary to make, is that the pressures are linear functions of the displacements, or rather relative displacements, the displacements being throughout supposed extremely small. The equations thus arrived at contain two arbitrary constants, and agree with those obtained in a different manner by M. Cauchy. If we suppose a certain relation to hold good between these constants, the equations reduce themselves to Poisson's, which contain but one arbitrary constant.

The equations of fluid motion which would have been arrived at by the method of this paper if the two constants $\mathfrak{C}$, $\mathfrak{C}'$ had been retained, have been already obtained by Poisson in a very different manner. The author has shown, that according to Poisson's own principles, a relation may be obtained between his two constants, which reduces his equations to those finally adopted in this paper.

There is one hypothesis made by Poisson in his theory of elastic solids, by virtue of which his equations contain but one arbitrary constant, which the author has pointed out reasons for regarding as improbable. He has also shown that there is ground to believe that the cubical compressibility of solids, as deduced by means of Poisson's theory from their extensibility when formed into rods or wires, is much too great, a conclusion which he afterwards found had been previously established by the experiments of Prof. Oersted.

The equations of motion of elastic solids with two arbitrary constants, are the same as those which have been obtained by different authors as the equations of motion of the luminiferous æther in vacuum. In the concluding part of his paper the author has endeavoured to show that it is probable, or at least quite conceivable, that the same equations should apply to the motion of a solid, and to those very small motions of a fluid, such as the æther, which according to the undulatory theory constitute light.

May 12.—On the Aberration of Light. By G. G. Stokes, M.A., Fellow of Pembroke College.

In the common explanation of aberration, it is supposed that light comes in a straight line from a heavenly body to the surface of the earth, except in so far as it is bent by refraction. This, of course, would follow at once from the theory of emissions; but it appears at first sight difficult to reconcile with the theory of undulations, unless we make the startling supposition that the æther passes freely through the earth as the earth moves round the sun. The object of this paper is to show that if we make the following suppositions, that the earth in its motion pushes the æther out of its way, that the æther close to the surface of the earth is at rest relatively to the earth, and that light is propagated through the disturbed æther as we suppose sound to be propagated air in motion, the observed law of aberration will still result, provided the motion of the æther be such that $u\,dx + v\,dy + w\,dz$ is an exact differential, where u, v, w are the resolved parts of the velocity of any particle of the æther along the rectangular axes of x, y, z.

On the Pure Science of Magnitude and Direction. By the Rev. H. Goodwin, Fellow of Caius College, and of the Cambridge Philosophical Society.

This memoir may be considered in some degree supplementary to the preceding one by the same author, "On the Connexion of the Sciences of Mechanics and Geometry." In that memoir it was argued, that if the views there advanced were sound, there must be such a science as that of *pure direction*, or rather a *pure science of magnitude and direction* which should include within itself the sciences of geometry, of kinematics, and of mechanics; in this the attempt is made to establish mathematically the fundamental proposition of such a science.

By making use of De Moivre's formula, the author conceives himself to have established this proposition, that if P represents the magnitude of any cause which varies uniformly and continuously into its exact opposite, *i. e.* into $-$ P while its direction varies uniformly from a given direction to the exactly opposite direction; and if θ be the angle which the direction of P makes with a given direction, then P is equivalent to two causes, $P \cos\theta$ in that given direction, and $P \sin\theta$ in the direction perpendicular to it.

The author is aware of the improbability which may appear to exist, that so general a proposition should be susceptible of proof without reference to particular instances, and has therefore endeavoured to obviate some objections, which will be more or less strongly felt, according to the nature of the philosophy of knowledge adopted by the mind which makes them, and which in some cases will probably be invincible.

The memoir concludes with some remarks on the general question of the transition of a quantity from the + to the $-$ affection, which the author conceives to be illustrative of his general design.

December 8.—On the Heights of the Aurora Borealis of September 17 and October 12, 1833. By Professor Potter, A.M., of Queen's College.

The data for the calculations are almost entirely taken from the conspectuses of the observations printed and distributed in 1833 by the British Association; and although so long time has elapsed, no calculations of the heights of the phænomena, which are the first steps to be taken in finding the nature of the meteor, have, to the author's knowledge, been hitherto published; the only imperfect discussion being given in the Number of this Magazine for December 1833.

The observers of the display of September 17, were Mr. J. Phillips, at York; Mr. Clare, Mr. Hadfield and the author, at or near Manchester; Professor Airy, at Cambridge; and the Hon. C. Harris, near Gosport.

The observers of that on October 12, were Professor Sedgwick, at Dent; Mr. W. L. Wharton, near Guisborough; Mr. J. Phillips, at York; Mr. Clare, Mr. Hadfield and the author, at or near Manchester; Dr. Robinson, at Armagh; Professor Airy, at Cambridge; and the Hon. C. Harris, at Heron Court.

The observations of the aurora of September 17 at Cambridge at $8^h\ 25^m$ Greenwich time, taken with those at Manchester at $8^h\ 24^m$, give the height of the lower edge of the arch 56 English miles, and of the upper edge 71 miles.

The observations of another arch, seen from $10^h\ 49^m$ to $11^h\ 19^m$ at York, and from $10^h\ 49\frac{1}{2}^m$ to $11^h\ 4\frac{1}{2}^m$ near Gosport, give the height of the lower edge 389 miles.

The observations on October 12, at $7^h\ 56^m$ at York and at $7^h\ 54^m$ at Cambridge, give the height of the upper edge of an arch 72·2 miles.

The observations at Guisborough at $8^h\ 20^m$, and at Heron Court at $8^h\ 22^m$, give the height of the under edge of the arch seen at that time 70·9 miles, and of the upper edge 85·5 miles.

The observations at Dent at $8^h\ 55^m$, taken with others at Manchester at $8^h\ 54^m$, give the height of the upper edge of that arch 84·97 miles.

The last arch remained stationary about a quarter of an hour, and therefore the observations are the more valuable; but combining an observation at Armagh with those at Manchester, the height comes out only 64·47 miles; and even with the utmost allowable latitude to the deductions from the observations, the height comes out 66·5 miles.

The last arch having been noticed to have risen to a higher altitude at the same places, a calculation with the corresponding data gives the height 65·4 miles.

These last three results are remarkably in accordance with each other, but considerably different from those for other places at nearly the same time; so that probably the method which was used, of obtaining a base line by projecting the places of observation upon an intermediate magnetic meridian, is only approximately correct, from the course of the arch over the earth's surface, rather than geometrical reasons.

Another arch was noted by most of the observers from $10^h\ 34^m$ to $10^h\ 45^m$. The observations at Dent at $10^h\ 40^m$, and Heron Court at $10^h\ 37^m$, give the height of the upper edge 59·4 miles.

An observation made by the author on the extent of the arch, on September 17, upon the horizon at $8^h\ 40\frac{1}{2}^m$, and its altitude, for application to the formula he has given in the Edinburgh Journal of Science, before it was joined with the Philosophical Magazine, for determining the height from observations at one place by the help of an hypothesis, gave the height 53·9 miles, which is a near approximation to the height found by the trigonometrical method for $8^h\ 25^m$.

The author concludes that the meteor occurs immediately beyond the ordinary limits assigned to the earth's atmosphere, and from that to very much greater altitudes, as shown by many other calculations; and states his conviction that the meteor will be some time observed with much more accurate means than hitherto, from its connexion with the earth's magnetism.

February 23, 1846.—Analytical Investigation of the Disease prevalent in the Potato during the year 1845. By Geo. Kemp., M.D., Pet. Coll.

This communication may be resolved into two parts; the analysis of the diseased portion of the potato, as compared with Boussingault's analysis of the healthy tuber, and certain deductions derived from the empirical formulæ proposed as representing their respective compositions.

The author recognises three stages of the disease: the first appearing as dark brown patches under the skin; the second as striæ of the same colour proceeding towards the centre; and the third as a soft, pultaceous, blackish, and offensive mass, in which all traces of organization are lost.

From the impossibility of isolating the portions affected by the disease, in the first two stages, from the surrounding sound parts, the examination was principally directed to the third stage.

A potato having been selected in which the above characters were well-developed, a sufficiently large portion for comparison still remaining perfectly sound, gave the following results as indicative of the relative proportions of organic and inorganic matter in the sound and unsound part.

Of the sound portion,—

I.	247 milligrammes gave	10·5	milligrammes of ash.
II.	205·5	... ...	8·5	... ...

Of the unsound portion,—

I.	311 milligrammes gave	18	milligrammes of ash.
II.	234	... ...	13	... ...
III.	236	... ...	13·5	... ..

Or, reducing to 100 parts, the sound portion consists of—

	I.	II.
Organic matter....	95·75	95·86
Inorganic matter ..	4·25	4·14
	100·00	100·00

whilst the unsound portion gives,—

	I.	II.	III.
Organic matter	94·22	94·45	94·29
Inorganic matter....	5·78	5·55	5·71
	100·00	100·00	100·00

The mean of the former is 4·19 per cent. of ash, that of the latter 5·68, making an excess of 1·49 per cent. arising from loss of organic matter.

The ultimate analysis of the unsound portion afforded the following results:—

I.	155 milligrammes gave carbonic acid	238·5,	water	98.
II.	132	202	...	78.
III.	163	251,	water not estimated.	

Mean of two analyses for nitrogen, after the method of MM. Varrentrapp and Will, 1·23 per cent.

These data furnish the following summary:—

	I.	II.	III.
Carbon	42·09	41·73	41·99
Hydrogen ..	7·02	6·56	7·02
Nitrogen....	1·23	1·23	1·23
Oxygen	43·98	44·80	44·08
Ash........	5·68	5·68	5·68
	100·00	100·00	100·00

The analysis of the sound potato, by Boussingault, is as follows:—

Carbon	44·1
Hydrogen	5·8
Nitrogen..........	1·2
Oxygen	43·9
Ash	5·0
	100·0

The object of the second part of the communication is to show, that, while Boussingault's analysis of the sound potato may be expressed by an empirical formula, representing the elements of proteine, starch, and cellulose, the analysis of the tuber, after undergoing the action of the late prevalent disease, admits of no such solution; but may be expressed by an empirical formula, representing proteine, starch, and butyric acid, with a very large excess of the elements of water.

Butyric acid has been found in the diseased potato by Mr. Tilley; but the author's principal object is to connect the changes developed by his analyses with the researches of Erdmann, Marchand, and Scharling, on the germination of seeds and tubers. These researches are totally independent and irrespective of the disease in question, whilst it is clear that the same changes occur in both cases. After reviewing the physical circumstances with respect to soil and culture, which have proved remarkably favourable to the development of the morbid changes, the author arrives at the general conclusion, that the disease in question essentially consists in an unnatural tendency to premature germination.

GEOLOGICAL SOCIETY OF DUBLIN.

March 11, 1846.—On some Secular and Diurnal Motions of the Earth's Crust, &c. By Robert Mallet, Esq., Mem. Ins. C.E., M.R.I.A., President of the Society.

The President said, that on taking the chair for the first time, he could not better mark his sense of the great honour which the Society had conferred on him in electing him their President, than by laying before them a plan which he had for some time conceived for the organization and carrying out of arrangements for a great, combined, and comprehensive movement for the promotion of a particular branch of geological discovery.

Geology might be divided into topographical and physical, both of which require for their study and development, the confluent aid of several distinct branches of natural inductive science, as well as of the exact sciences properly so called. Physical geology, again, might be divided into several distinct branches, for the cultivation of all of which, the application of physics, mechanics, and chemistry, in their largest senses, is indispensable. For a considerable period it had appeared to him that one of the most important directions in which physical geology could be advanced was, in placing it in connexion with a mode of investigation, new as respected geology, although old as regarded other sciences, namely, measurement; by the organization and solution of such questions as were capable, in terrestrial physics, of having an answer in measure, number or weight. The application of measures to geology might be called experimental geology—a branch of the science hitherto, he might say, unexplored. Geologists had hitherto contented themselves with observing what nature had presented to them, and that must, from the nature of geology, at all times form the staple of its investigations; nevertheless, whenever it was possible to use the experimental method, it might be pronounced not only as the most certain, but also the most rapid means of advancing geological, as it had always been of every other branch of physical science. In order to illustrate what he meant by measurement in geology, and to show its feasibility, and even simplicity of application, and to what important consequences it at once led, he would briefly give an example of the sort of inquiry alluded to. Taking any large island, such as Ireland, let them suppose the following data respecting it to be obtained in measures—the entire amount of water annually discharged into the sea by all its rivers, and the entire amount of soluble and suspended matter carried down to the sea by them, and the chemical constitution of these. From these data they should be able to pronounce on the amount and on the character of deposits annually taking place round the coasts. They should then be in a condition, with the assistance of their tidal and other knowledge, to predicate with a considerable degree of accuracy the nature, character, extent, form, and relative locality, of the deposits so taking place; and hence to predict what forms of rock would at a future period be most likely to occur round their shores; to draw direct conclusions from thence as to the changes in progress, or hereafter to occur, to animal and vegetable marine organization on our coasts; to indicate by the most ample and certain data the changes of soundings, of shoals, of harbours, beaches and bars, which concern the mariner and the civil engineer; and to draw conclusions as to the relation and balance between the wear of the land by detrital action, both meteorological and tidal, and its replacement by forces of elevation, whether slowly or convulsively.

He mentioned this as one instance of experimental geology; but the direction in which he was at present anxious to have geology advanced, by obtaining the aid of a co-operative movement in applying this method of investigation, was rather different. It had become certain of late years that the crust of their globe, far from being of that immoveable character which was popularly supposed, was, in fact, one of the most unstable and changeable parts of creation, that there was nothing immutable upon it, but the very mutability that marks and agitates it, was subject to almost continual motion in both a vertical and a horizontal direction; the latter resulting, at uncertain intervals, principally from the cause which produced earthquakes, the former occurring at occasional epochs from the same cause; and regularly recurring, in all probability, in the form of annual and daily motions of expansion and contraction in the earth's crust, due to alterations of temperature by changes of season, and of day and night. To measure these motions appeared of the very highest importance, at the present epoch, to the advancement of geology. A communication from Sir William R. Hamilton, which he (Mr. M.) had in his hand related to one of these phænomena; but to take matters in the order of their relative importance, they should first consider these movements which related to the earthquake oscillation of which he had spoken.

It would be necessary, in order that the Society might understand his meaning, and see more clearly the bearing of the motion as to scientific co-operation, which he intended having the honour of laying before the Council at a subsequent period, to detain them with a slight sketch of the movements due to forces of elevation and depression which were known to be always taking place in the earth's surface.

That any portion of the earth's crust was absolutely permanent as to level was uncertain; that there were some portions which were neither rising nor falling with respect to an assumed plane, at a fixed distance from the earth's centre, was possible; but that large tracts were slowly rising, and that others were periodically caused to rise; again, that other large tracts were slowly sinking (for instance the great coral region of the South Sea), and some suddenly depressed (usually at periods of sudden neighbouring elevation), was certain. The forces which produce these changes of level also give rise to earthquakes. It was necessary for him to refer, very briefly, to some particulars respecting the subject of earthquakes, on which he had recently the honour to lay a paper before the Royal Irish Academy. Every earthquake was in fact a succession, or a combination rather, of waves taking place in the solids forming the crust of the earth, and in the fluids incumbent upon it, and resulting from a rupture or disturbance occurring at some one point of that crust. If they supposed an earthquake taking place under the sea, three sets of waves started at the same moment from the centre of rupture. One of these was the wave of elastic compression, which was propagated in every direction, and was transmitted with a velocity proportioned to the varying elasticity and density of the crust. The passage of this wave constitutes the true earthquake shock. There was also a wave of sound transmitted through both the solid and the liquid portions of the earth's surface, and also a wave of sound transmitted through the air by the impulse communicated to it from the first-mentioned waves; and last of all, the great sea wave, produced by the commotion at the bottom of the sea, which reached the land long after the shock itself had passed it. Here were three classes of motion, which in general were talked of as connected with a very formidable phænomenon, but seldom viewed as movements capable of measurement. It was a fact, that, no matter where an earthquake took place, the wave of elastic compression which was produced actuated every portion of the earth's crust, and its occurrence could be detected, by sufficiently accurate instruments, at any part of the globe.

In some of the observatories connected with the great magnetic survey, now in operation at various points over a great portion of the whole world, and in the organization of which our fellow-member, Dr. Lloyd, has had so large a share, there existed a necessity for certain magnetic instruments, which were, in fact, though not intentionally so, ready-made seismometers: and in the magnetic observatory of Dublin, Dr. Lloyd has frequently observed a sudden vertical oscillation communicated at the same instant to all the instruments, and which he believes to be the effect of slight earth tremors, propagated from remote centres of disturbance.

It would be of the highest service, not only to physics in general, but also to geology, if the circumstances of earthquake shocks could be observed and their velocity measured; since, if they could tell the velocity of the wave through the earth's crust, and were at the same time to determine by proper experiments the elasticity of the superficial rocks of the earth's surface, they should be able to infer, from the two data so obtained, the nature of the geological deposits under the bed of the ocean, concerning which nothing whatever was known at present. Topographical geology as yet extends but to a small portion of the land; future examinations may embrace the whole of this. But of the nature and boundaries of the formations which compose the bed of the ocean, we know absolutely nothing; so that in fact two-thirds of the whole surface of the globe is in this respect a geological blank. The method of investigation here proposed, however, although it may not give us minute information as to the character of the ocean bed, will enable us to gather some general and certain knowledge of the architecture of those fathomless abysses of the ocean, which no eye can ever behold. He would, therefore, hereafter by the aid of the Council ask, perhaps through the medium of the General Council of the British Association, that all the observatories in connexion with the magnetic survey should, in addition to their magnetic observations, record observations as to the occurrence of earthquake shocks. This might be very readily done, since the magnetic instruments at present in operation in some of the observatories were quite adapted to afford the necessary indications with respect to the shocks, and other instruments required might be easily added, namely, those which it will be necessary to contrive to register the altitude of the crest of the wave of elastic compression, at the moment of its passage—an element in this inquiry of the first importance.

But it happened, that magnetic observatories which were furnished with these sorts of instruments, did not generally exist in places in which earthquakes were frequent. It thus became necessary to establish, in some places, at least, observatories specially devoted to geological purposes as their primary object. It would be highly desirable to have such observatories in the volcanic regions of South America—the land of earthquakes; since, in addition to observing the velocity of the shock wave, the elasticity of the neighbouring rocks, through which the shock had passed, could experimentally be determined. He conceived that it would be quite possible to get observatories established there, and he should submit to the Council the means by which such a desirable object could most probably be accomplished. Shocks of earthquake were of greatly more frequent occurrence than was generally supposed, and occurred in every part of the world. Violent ones, producing destructive effects, and which were only experienced at limited distances from the centre of disturbance, were comparatively rare; but minor shocks were of almost daily occurrence; and, indeed, in a district in Scotland, the number of such slight shocks occurring within a certain period had been observed. The proposed observatories, therefore, will be by no means unprovided with work.

There was another class of motion to which the earth's crust was subject, and to which Sir William Hamilton's note related. Before reading it, he (Mr. Mallet) might perhaps mention that as much as four years ago, upon an occasion of his dining in company with Dr. Robinson, Astronomer Royal of Armagh, he mentioned to him (Mr. Mallet) and others, that the observatory of Armagh had been observed to be subject to very slow and minute annual motions, not only to one which was manifested by the whole observatory being lifted in summer and depressed in winter, but also to one by which it appeared to move in azimuth. Dr. Robinson mentioned the fact as one which had not been explained, and it occurred to him (Mr. Mallet), and he stated at the moment, his anticipation, that it would be found that the motions were due to expansions of the earth's crust, caused by the alternations of temperature of summer and winter. Very recently, conversing with Sir William Hamilton on the subject, he (Mr. Mallet) perceived that a similar idea in connexion with it had independently presented itself to his mind; and, in fact, Sir William Hamilton was to be considered as the discoverer of this class of motion, since the man who first observed a fact, and at the same time gave a true explanation of it, was entitled to the honour of discovery. Mr. Mallet then read the following note which he had received from Sir William Hamilton, and to which he had referred :—

"Observatory of Trinity College, March 2, 1846.

"My dear Sir,—The only thing which seemed to me original in what I observed to you and others at the geological dinner last month, was the proposition for instituting in new, and multiplying in old observatories, observations with a leveling instrument, for the purpose of acquiring accurate data respecting some of the expansions, whether periodical or secular, of the crust of the earth. I thought that by fixing the chief attention on the variations of a long spirit level, very carefully and steadily mounted, and from time to time reversed, as in an astronomical observatory, perhaps with precautions as to original erection and subsequent use, which sidereal checks render not so necessary to the astronomer, and possibly, too, by using two pairs of pillars for two different vertical planes, a gentleman might at a moderate expense of money and trouble make in his own lawn or house observations useful to geology; and if I remember rightly, under this conception I talked of founding geological observatories, on which you remarked, that if your paper on earthquakes had been read to the end at the Academy, it would have been found to contain a similar suggestion, though based upon reasons not in all respects the same.

"I also mentioned the fact, that in this observatory, the western end of a transit level, supported on pillars peculiarly favourable for the accurate examination of a point of this kind (see an account of them by Dr. Ussher, in the first volume of the Transactions of the Royal Irish Academy), was always a little higher in summer than in winter; and that in answer to an inquiry of mine, Mr. Cooper's first assistant had by that morning's post informed me that the axis of the instrument at Markree Observatory showed (such as I conjectured that it might) an opposite phænomenon, though this was to be accounted for by mechanical rather than geological consideration. I remember, also, acknowledging that Dr. Robinson had long ago remarked to me that the whole hill on which the Armagh Observatory stands is found to have a motion with the seasons, but that I had been in the habit of conceiving Dr. Robinson to deduce this from observations of the azimuth, rather than of the level; and that my own conjecture, perhaps a very wild one, had been, that Ireland as a whole expanded, and thereby rose somewhat more out of the sea in summer than in winter; which expansion, if it were admitted to exist, would account for the western end of the astronomical level rising a little on the east and sinking on the west coast of the island. Indeed, as a mode of conjecturally accounting for what has been noticed in this observatory, the notion has long been in my mind, and has been put forward by me, though with the necessary diffidence, to some of the astronomical students of the University in one of my lectures last summer, if not at an earlier date. The conversation in which I was engaged with you on this and similar subjects, at the last anniversary dinner of the Geological Society, interested and excited me at the time very much indeed; and if you think the foregoing memorandum, which I have drawn up at your desire, worthy of being incorporated in any communication of your own to the Society, it is perfectly at your service for that purpose.

"I remain, my dear Sir, very truly yours,
"WILLIAM R. HAMILTON."

Mr. Mallet would remind the Society, that the earth's surface was the medium through which two great waves of heat were continually transmitted; one proceeding from the sun, by part of which the atmosphere was heated, and the other coming from the interior of the earth towards the surface. In every climate there was a plane lying between the influences of these two waves, which never changed its temperature, and which varied in its depth below the earth's surface. As there was thus a transmission of heat from the interior of the earth towards its surface, and also from the surface towards the centre, the plane of constant temperature, or isogeothermal plane as it was called, would be found in any given locality at a determinate depth; and if the supplies of external and internal heat were constant, it would always be found at the same depth in the same place. But inasmuch as the earth's surface was exposed to temperatures, varying with the winter and summer seasons, the level of that plane must rise and fall in proportion to the force of the variable wave. The average depth of this plane in their latitudes was about sixty feet, but was far greater in the tropical climates, in some of which the heating power of the sun ranged to a depth of nearly 500 feet below the earth's surface.

The result of the varying intensity of the external wave of heat must be the contraction and expansion of the earth's crust due to the difference between the temperatures of summer and winter, and hence a certain annual motion in the earth's crust, besides which, from similar causes, acting in shorter periods, and in less degree, there must be a diurnal motion. To measure this systematically, would be, it appeared to him, highly important, not perhaps directly to those more obvious parts of geology which treat of the elevation of mountain ranges and the depression of valleys, and so forth; but inasmuch as it would lead to a region of investigation which was at present absolutely unknown; and by penetrating which it would probably be found in this, as in so many other parts of terrestrial physics, that forces and motions the most minute, and scarcely to be detected, when taken in aggregate were essential and potent parts of the universal machine. There had yet been no investigation of the rate of expansion of any rock, the tables of the rate of expansion of the very few solids hitherto published having reference only to substances which were of value to the astronomer or the engineer. Therefore with respect to the measurement of these motions, the facility with which the observations necessary for that end could be made, and the likelihood of important information being obtained in the pursuit of it, were grounds sufficient to warrant an undertaking of the object. He had now said sufficient to put the Society in possession of the nature of the views he entertained with respect to the opening in a new region of a new campaign in geology, if he might so speak; and which he hoped to do by inducing his fellow-members of the Council to communicate with such members of the great magnetic body as were connected with Dublin—as, for instance, Prof. Lloyd—and through them to the Council of the British Association, to enable certain experiments as to the elasticity of the earth's crust to be made, and the velocity of the elastic wave through it determined; to induce hereafter observations to be made in all the magnetic observatories (and possibly some new geological observatories founded), and observatories continued, both during the continuance of the magnetic survey and after it had terminated, for the use of the geologist, viz. observations of all the motions, whether diurnal, secular, or cataclysmal, that take place in the earth's crust. He believed that such observations would be replete with interest to geology and to physics; and he conceived that the application would be met by the British Association with that ready response which had always been given by it to every project that seemed to be calculated for the benefit of science.

GEOGRAPHICAL SOCIETY OF PARIS.

The first general meeting of this Society was held on Friday, the 22nd of May last, when, on the report of the special committee appointed to decide on the most important discovery in Geography during the year 1843, the gold medal of the Society was awarded to our countryman Dr. Beke, the author of *Origines Biblicæ*, and an old correspondent of this Journal[*].

It may be in the remembrance of our readers, that at the general meeting last year of the Royal Geographical Society of London, Dr. Beke was honoured with a similar award of the gold medal of that Society.

These marks of approbation conferred on Dr. Beke by the two principal geographical societies of the world, are the best proofs of the value of the additions made by him to our knowledge of a portion of Eastern Africa, which has of late attracted so much attention both in England and in France. The following summary of the researches thus appreciated and rewarded, was given by Sir Roderick Murchison, the late President of the Royal Geographical Society, in his anniversary address in May 1844:—

"Of all Abyssinian travellers since the days of Bruce, Dr. Beke, as an individual, having most improved our geographical acquaintance with that country, I may be permitted to say a few words explanatory of his labours. He landed at Tajurrah in November 1840,

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[*] See Phil. Mag., vol. iii. p. 103; iv. p. 107, 280; vi. p. 401; vii. p. 40; viii. p. 506; ix. p. 34; xi. p. 66, 344; xiv. p. 426.

and left Massowah in May 1843, having been exactly two years and a half in Abyssinia and the lowlands adjoining it.

"In his arduous endeavours to construct a map of a large tract, Dr. Beke carried a series of thermometric levels across nearly seven degrees of latitude (from Tajurrah to Banja), having been the first to ascertain the remarkable depression of the salt lake Assal, which he roughly estimated at 760 feet (since ascertained by Lieut. Christopher to be 590) below the level of the sea; and he has fixed by astronomical observations the latitude of upwards of seventy stations.

"Whilst in Shoa he visited and mapped the water-shed between the Nile and the Hawash, along a line of nearly fifty miles northward of Ankober; and he obtained information of the existence of the river Gojeb.

"After leaving Shoa he proceeded westward across the Abai, into the plateau of Gojam, where he remained in all a year and a quarter, so traversing it in various directions as to be able to construct a map of the country.

"He is the first traveller since the time of Bruce who has described the sources of the Abai (the Nile of Bruce), and I rejoice to say that he completely sustains the accuracy of the narrative of the great explorer of Abyssinia. By reaching the river Abai at various points around Gojam and Damot, he has determined its course approximatively; and it may be mentioned that near Mota he discovered a second bridge over that stream, described by no previous traveller.

"During a long stay in the neighbourhood of Baso, in the hope of being able to penetrate from thence southward, he collected information respecting the countries to the south of the Abai, from which he has constructed a rough map comprising near 70,000 square miles of country, hitherto very partially explored by one of the brothers d'Abbadie, and as yet, of course, very imperfectly laid down.

"On his way from Gojam to Massowah, Dr. Beke took a hitherto untrodden road, passing by Mahhedera Mariam, Debra Tabor, Ebenat, and Sokota to Antalo; and from thence again by a route never travelled by other Europeans, round by Takirákira (a place described by Rüppell) to Adowa. On this route he crossed the Takkaze much higher up to the south than others had done, by which the course of that river in the maps is corrected; whilst by this *new* line through the heart of Abyssinia, an important addition is made to the general map of that country. Dr. Beke's maps and journals have been handed over to the Royal Geographical Society, and a small portion of them has already been published in our Journal*. Various other portions of the information obtained by him

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* The map of Dr. Beke's route and the remainder of his Itinerary have since appeared in vol. xiv. of the Society's Journal, part 1. For other information published by him, see vol. xiii. part 2 of the same Journal; the Friend of the African, vol. i. and ii. *passim*; and A Statement of Facts relative to the Transactions between the Writer and the late British Political Mission to the Court of Shoa.

have appeared in the Friend of Africa, published by the African Civilization Society; but the greater mass, comprising a description of the manners and customs of the inhabitants, as well as his personal adventures, he is, I understand, now engaged in preparing for the press.

"It may be added, that he has collected vocabularies of thirteen languages and dialects spoken in Abyssinia and the countries to the south*, and he has made numerous drawings illustrative of the country and its inhabitants."

XIII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOUNDS OF BORACIC ACID WITH ÆTHER.

BY MM. EBELMEN AND BOUQUET.

PROTOBORATE *of Æther.—Boracic Æther.*—Chloride of boron was obtained by passing a current of chlorine gas over a mixture of boracic acid and charcoal heated to redness in a porcelain tube; by this operation, as shown by M. Dumas, there are obtained chloride of boron and oxide of carbon which are liberated together; this mixture of gases was passed into a bottle two-thirds filled with alcohol, which absorbs a large quantity of chloride of boron; the liquor becomes hot, and the bottle in which it is absorbed should be immersed in cold water. After a certain time, dependent upon the quantity of alcohol and the rapidity of the current of gases, the liquor separates into two portions, the upper of which alone contains the new product; the lower portion is of a yellow colour, and is strongly impregnated with hydrochloric acid. As soon as the separation of the liquid into two portions occurs, the gases which are continued to be passed contain a large quantity of hydrochloric acid. The operation is to be stopped when the presence of chlorine is suspected in the gaseous products.

When the upper portion of liquid is poured off, and distilled with a few drops of absolute alcohol, a small quantity of very acid alcohol passes over, and then the boiling-point rapidly rises. At 239° Fahr. the receiver is to be changed, and a very abundant product, nearly equal to the whole of the liquid, is to be separately received, between 239° and 248° Fahr. There remains in the retort a product of a vitreous appearance, presently to be noticed.

By rectifying the distilled liquid between 239° and 248°, a neutral product is obtained, the boiling-point of which remains fixed at 246° Fahr.

This new compound is a very moveable liquid, perfectly colourless, with a peculiar and rather agreeable smell, and a burning bitter taste. Its density, taken at 32° with M. Regnault's apparatus, is 0·8849. It dissolves immediately in water, but in a short time boracic acid is deposited. It combines with alcohol in all proportions. Moist air converts it into hydrated boracic acid. It burns with a

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* Since published in the Transactions of the Philological Society, vol. ii. pp. 89–107.

fine green flame, accompanied with thick fumes of boracic acid, but without leaving any solid residue.

By analysis corrected by calculation, the substance appeared to be composed of—

Boracic acid......	23·91	436·2
Carbon..........	49·35	900·0
Hydrogen........	10·28	187·5
Oxygen	16·46	300·0
	100·00	1823·7

Its formula is $BO^3\,3C^4H^5O$.

The results of analyses were confirmed by those of the vapour of the æther, which gave the following numbers:—

1	volume of the vapour of Boron	1·506
12	... of Carbon	10·054
30	... of Hydrogen	2·076
6	... of Oxygen	6·636

$$\frac{20{\cdot}272}{4} = 5{\cdot}068.$$

The reaction which gives rise to borate of æther is the same as that from which silicate of æther is derived:—

$$BCl^3 + 3C^4H^6O^2 = BO^3\,3C^4H^5O + 3HCl,$$
$$SiCl + C^4H^6O^2 = SiO\ \ C^4H\ \ O + HCl.$$

If SiO^3 be admitted to represent the formula of silica, the two formulas would have been perfectly similar. But it must be observed that in this notation $SiCl^3$ would represent 3 volumes of chloride of silicium, whilst the formula BCl^3 corresponds to 4 volumes of chloride of silicium. This difference, stated by M. Dumas, in the condensation of the elements, also occurs in the densities of the fluorides of boron and silicium. It also occurs, as just shown with boracic and silicic æthers, for $SiOC^4H^5O$ corresponds to 1 volume of vapour only. It will be observed, that in both cases 1 volume of vapour of chlorine produces 1 volume of vapour of æther.

Vitreous Borate of Æther.—It has been stated, that after preparing the above-described compound, there remains in the retort a substance which becomes solid, assuming a vitreous aspect. It is transparent and possesses the same odour as the protoborate, and a very bitter taste, which distinguishes it perfectly from boracic æther $(BO^3)\,3C^4H^5O$, the taste of which is burning. The latter is also decomposed by water much more readily than the vitreous product.

By analysis it yielded 26·7 carbon and 6·0 hydrogen, which evidently is the same proportions as in æther, C^4H^5O. This substance requires, however, to be further examined.

The authors state, that they had supposed that by distilling the protoborate with alcohol slightly diluted, they should produce compounds analogous to those which are obtained by the same method with protosilicate of æther, that is to say, æthers containing increasing quantities of acid. They did not, however, succeed. Borate of æther heated with alcohol of 36°, and entirely distilled at a temperature below 248° Fahr., left a slight residue of boracic acid in the retort.

Protoborate of Methylen.—MM. Ebelmen and Bouquet found that the action of chloride of boron on pyroxylic spirit was perfectly similar to that upon alcohol. The gas is absorbed with the production of heat, and the bottle containing the spirit must be carefully kept cool. When the disengaged gases after passing the liquor are very acid, the liquor separates into two portions; the lower one is brown and extremely acid, and the upper one is limpid and colourless, and alone contains the new product. It is to be poured off and distilled with a thermometer in the retort. The receiver is to be changed when the boiling-point of the liquid has become constant. The first products contain some hydrochloric acid; two rectifications are sufficient to obtain a neutral product, the boiling-point of which remains stationary at about 162° Fahr.

If instead of employing pyroxylic spirit which has been rectified from chloride of calcium, the article of commerce rectified merely from quicklime be used, the liquid, traversed by the chloride of boron, becomes of a brown colour, but without separating into two portions towards the end of the operation. When this product, containing a large quantity of chloride of boron, is cautiously distilled, and the product received in a vessel surrounded with ice, separation takes place. That portion only of the liquid which distils below 176° Fahr. ought to be preserved; what remains in the retort at this temperature is a black viscid liquid, equal to more than half the liquid submitted to distillation. By rectifying the upper portion of the liquid obtained in the cooled receiver, a pure product is obtained.

Borate of methylen is a colourless liquid, which is very moveable, and has a penetrating odour, slightly resembling that of pyroxylic spirit; it mixes with water, but soon deposits boracic acid. Its density at 32° is 0·9551, and its boiling-point about 162°; it burns without residue and with a green flame.

It appeared to be composed of—

Boracic acid......	436·2	33·58
Carbon..........	450·0	34·65
Hydrogen........	112·5	8·66
Oxygen	300·0	23·11
	1298·7	100·

Its formula is $BO^3\,3C^2\,H^3\,O$.

By experiment and calculation it appears to be composed of—

1	volume of vapour of	Boron....	1·506
6	...	Carbon	5·027
18	...	Hydrogen	1·245
6	...	Oxygen	6·636
			14·414

$$\frac{14{\cdot}414}{4} = 3{\cdot}603.$$

The mode in which the elements are condensed is therefore the same as in the protoborate of æther. 1 volume of chloride of boron produces 1 volume of borate of methylen.

Protoborate of Amylen.—When chloride of boron is passed through potato-oil, the liquid soon separates into two portions, and at the same time hydrochloric acid begins to be disengaged. By pouring off the upper portion, distilling and keeping the products separate, nearly the whole of the liquid passes over between 500° and 536° Fahr. This product, when rectified, yields a liquid, the boiling-point of which is constant between 518° and 527° Fahr.

Borate of amylen is a colourless liquid of an oily appearance, the weak smell of which resembles that of the potato-oil. Its density at 32° is 0·870; water decomposes it and dissolves the boracic acid; ammonia also decomposes it; it burns with a white flame with green edges, and the production of boracic acid.

Analysis showed that it is composed of—

BO^3,	Boracic acid....	426·2	12·8
30C,	Carbon........	2250·0	66·2
33H,	Hydrogen......	412·5	12·1
3O,	Oxygen........	300·0	8·9
		3398·7	100·0

The preceding results, the authors are of opinion, tend to show that boracic acid may form with each simple æther at least two compound æthers; one of which is tribasic and contains as much oxygen in the acid as in the base, while the other contains six times as much oxygen in the acid as in the base. The tribasic æthers $BO^3, 3C^4H^5O$, $BO^3, 3C^2H^3O$, are similarly constituted to crystallized boracic acid $BO^3, 3HO$. Boracic æther $(BO^3)^2 C H^5 O$ is analogous to anhydrous borax $(BO^3)^2, NaO$.—*Ann. de Ch. et de Phys.*, Mai 1846.

HYDRATED BORACIC ACID.

MM. Ebelmen and Bouquet remark that it is stated in the greater number of chemical treatises, that crystallized boracic acid effloresces and loses half of its water at 212°; the authors state that they have not verified this statement, but that by long exposure to a heat of 320° Fahr., crystallized boracic acid containing 35·7 per cent. of water, is converted into a well-fused substance of a vitreous appearance, which is perfectly homogeneous, and retains one-sixth of the water contained in the crystallized acid. The formula of this hydrate is $(BO^3)^2, H$. When exposed to a higher temperature, it is converted into anhydrous acid, swelling up much and emitting, very sensibly, the fumes of boracic acid. When the acid has been once rendered anhydrous, it may be kept long on fusion in the crucible without any notable loss.—*Ann. de Ch. et de Phys.*, Mai 1846.

NEW BOOKS.

Scientific Memoirs, Part XV., containing articles by Biot, Neumann, Kœne, Riess, Dove, and Seebeck.

A Practical Manual, containing a Description of the General, Chemical and Microscopical Characters of the Blood, and Secretions of the Human Body. By J. W. Griffith, M.D., F.L.S.

The Catalogue of Stars of the British Association for the Advancement of Science; containing the Mean Right Ascensions and North Polar Distances of Eight Thousand Three Hundred and Seventy-seven Fixed Stars, reduced to January 1, 1850: together with their Annual Precessions, Secular Variations, and Proper Motions, as well as the Logarithmic Constants for computing Precession, Aberration and Nutation. With a Preface explanatory of their Construction and Application, by the late Francis Baily, Esq.

The Report of the Fifteenth Meeting of the British Association for the Advancement of Science, held at Cambridge in June 1845.

METEOROLOGICAL OBSERVATIONS FOR MAY 1846.

Chiswick.—May 1. Dry haze: overcast. 2. Overcast. 3. Dry haze. 4. Cloudy and fine. 5. Very fine: showers. 6. Showery: cloudy and fine. 7. Cloudy and fine. 8. Overcast: exceedingly fine: clear at night. 9. Very fine. 10. Slight rain: cloudy: clear. 11, 12. Very fine. 13. Light clouds: overcast: rain at night. 14. Clear: cold and dry. 15. Cloudless: light clouds and fine: clear and cold at night. 16. Uniformly overcast: dry haze: densely overcast at night. 17. Overcast: rain: clear. 18. Rain: cloudy: boisterous. 19. Very fine: thunder-showers: densely overcast. 20. Rain: heavy showers. 21—23. Very fine. 24. Slight fog: overcast and fine. 25, 26. Very fine. 27. Cloudless: very fine: overcast. 28, 29. Very fine. 30. Hot and dry. 31. Cloudless: hot and sultry: clear.

Mean temperature of the month	56°·16
Mean temperature of May 1845	50 ·04
Mean temperature of May for the last twenty years	54 ·77
Average amount of rain in May	1·84 inch.

Boston.—May 1. Fine. 2—4. Cloudy. 5, 6. Fine: rain P.M. 7. Cloudy. 8, 9. Fine. 10. Cloudy: rain early A.M. 11, 12. Fine. 13. Cloudy. 14—16. Fine. 17. Cloudy: rain early A.M. 18. Cloudy: rain A.M.: thunder P.M. 19. Fine. 20. Fine: hail and rain A.M. and P.M., with thunder and lightning. 21. Cloudy. 22. Fine: rain P.M. 23, 24. Fine. 25, 26. Cloudy. 27—29. Fine. 30. Fine: 3 o'clock P.M. 75°. 31. Fine.—N.B. The warmest May since 1833: it was 62°·8.

Sandwick Manse, Orkney.—May 1. Drizzle: damp. 2. Bright: clear. 3. Clear: aurora. 4. Bright: rain. 5, 6. Rain: cloudy. 7. Fine: rain. 8. Cloudy. 9. Clear: cloudy. 10. Clear: thunder and hail. 11. Bright: drops. 12. Bright: cloudy. 13, 14. Bright: fog. 15. Bright: cloudy. 16. Bright: clear. 17. Rain. 18. Fog: cloudy. 19—21. Damp: cloudy. 22. Rain: damp. 23. Showers: bright: cloudy. 24. Showers: clear. 25. Rain: cloudy. 26. Showers. 27, 28. Showers: cloudy. 29. Cloudy. 30. Rain: cloudy. 31. Cloudy: fine.

Applegarth Manse, Dumfries-shire.—May 1. Dropping day. 2. Fair and fine. 3. Fair and very fine. 4. Rain all day. 5. Heavy showers. 6. Showers. 7, 8. Fair and fine. 9. Fair and fine: a few drops P.M. 10. Heavy rain during the night. 11. Fair and fine. 12. Slight shower: growing weather. 13—16. Fair and fine. 17. Showery morning: cleared. 18. Showery all day. 19. Rain P.M. 20. Rain during the night: cleared. 21. Showers: thunder. 22. Drizzly all day. 23—25. Very fine day. 26. Very fine day: droughty. 27, 28. Very droughty. 29—31. Very warm.

Mean temperature of the month	52°·6
Mean temperature of May 1845	50 ·0
Mean temperature of May for 23 years	51 ·0
Mean rain in May	1·96 inches.
Mean rain in May for 18 years	1·73 „

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. May.	Max.	Min.		9 a.m.	9 p.m.	9¾ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¾ a.m.	8½ p.m.								
1.	30·285	30·263	29·83	30·12	30·01	30·03	29·77	56	51	55	53½	44½	46	49	w.	calm	sw.	sw.	......	......	......	......
2.	30·208	30·125	29·64	30·00	30·02	29·81	29·97	68	52	57	58½	50	49	46	sw.	w.	sw.	w.	......	......	......	·09
☽ 3.	30·083	29·932	29·59	30·00	29·98	30·04	30·10	70	52	62·5	60½	46½	46	42	e.	calm	e.	......	......	......	......	......
4.	29·866	29·721	29·50	29·80	29·60	30·03	29·87	66	46	51	50	44	47½	43	e.	e.	e.	se.	......	......	......	......
5.	29·724	29·544	29·20	29·52	29·38	29·71	29·60	66	45	61	60	43	42	42	s.	calm	ese.	e.	·03	......	......	·15
6.	29·548	29·494	29·00	29·30	29·38	29·46	29·41	63	43	57	60	46	44½	45	sw.	w.	sw.	se.	·13	·14	......	·09
7.	29·750	29·665	29·15	29·49	29·52	29·44	29·41	66	43	55	57	38	47½	47	sw.	calm	sw.	se.	......	·01	......	·15
8.	29·931	29·730	29·35	29·67	29·80	29·62	29·82	69	42	59	58	44	49	47	w.	calm	nw.	......	......	......	0·83	·15
9.	29·974	29·863	29·49	29·80	29·55	29·79	29·52	72	49	61	62½	37½	51½	48½	sw.	calm	w.	sse.	·03	......	......	......
10.	29·962	29·787	29·26	29·51	29·68	29·42	29·56	70	40	56	57½	47½	49	47	w.	calm	sw.	w.	·04	·07	......	......
○ 11.	30·093	30·058	29·50	29·81	29·82	29·73	29·80	72	34	58·5	59	44	53½	49	nw.	calm	ssw.	sse.	......	......	......	·10
12.	29·994	29·836	29·49	29·85	29·85	29·87	29·92	69	48	60·5	64	43½	54	48½	e.	calm	sse.	sse.	......	......	......	......
13.	29·734	29·713	29·35	29·86	29·98	30·04	30·19	69	46	58	60	47	47½	46	e.	e.	e.	sse.	·22	......	......	......
14.	29·995	29·880	29·50	30·08	30·06	30·19	30·12	62	38	57	62½	44	49½	44	e.	ne.	e.	w.	......	......	......	......
15.	29·968	29·859	29·53	29·98	29·80	29·97	29·78	63	34	57	62	36	58	48	e.	e.	e.—s.	sse.	......	......	0·35	......
16.	29·695	29·360	29·29	29·67	29·40	29·74	29·68	63	46	57	62	39½	51	44½	e.	e.	e.	ese.	·34	......	......	......
17.	29·218	29·184	28·69	29·10	29·03	29·42	29·13	61	36	52	56	45	46½	47	s.	w.	ne.—s.	e.	·01	·40	......	·15
☾ 18.	29·139	29·023	28·64	28·94	28·90	29·11	29·16	62	46	53	54½	43	49	47	sw.	s.	ese.	se.	·17	......	......	·26
19.	29·545	29·481	28·95	29·18	29·19	29·20	29·30	63	49	58	59	46	51	49	sw.	w.	s.	se.	·12	·15	......	......
20.	29·558	29·444	28·96	29·12	29·34	29·32	29·56	60	44	58·5	61½	47	53	48	sw.	sw.	s.	se.	·26	......	......	·03
21.	29·949	29·677	29·22	29·65	29·88	29·77	29·93	71	39	60	65	45	54	50	se.	calm	ssw.	se.	......	·65	......	......
22.	30·203	30·120	29·57	30·00	30·02	29·94	30·01	71	46	64	56	45½	54	48	s.	calm	s.	w.	......	......	......	·04
23.	30·212	30·201	29·61	30·10	30·12	30·12	30·12	71	47	63	64	49½	49½	48	w.	calm	wnw.	w.	......	·03	0·78	·02
24.	30·232	30·202	29·67	30·12	30·11	30·04	30·07	75	45	64	60½	45½	51	47½	w.	calm	nw.	w.	......	......	......	·01
● 25.	30·207	30·102	29·61	30·05	30·00	29·87	29·84	75	53	57·5	63	52½	50	48	w.	calm	wnw.	w.	......	......	......	·25
26.	30·138	30·115	29·54	30·00	30·00	29·85	29·82	73	39	57	60	48	50	48	nw.	calm	w.	w.	......	......	......	·03
27.	30·109	30·046	29·52	29·97	29·98	29·88	30·03	71	42	61	58½	46½	47	46	w.	w.	wnw.	nnw.	......	......	......	·26
28.	30·243	30·153	29·63	30·10	30·15	30·12	30·17	67	41	61	64	42	48¾	49	e.	calm	w.	w.	......	......	......	·03
29.	30·320	30·305	29·74	30·20	30·18	30·13	30·04	73	39	62	65	41	57	51¼	s.	calm	wsw.	sw.	......	......	......	·01
30.	30·300	30·154	29·72	30·16	30·15	30·04	30·17	79	44	66	[illegible]	45	[illegible]	[illegible]	ssw.	calm	wsw.	se.	......	......	[illegible]	[illegible]
31.	30·178	30·130	[illegible]	30·12	[illegible]	[illegible]	30·11	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	......	......	......	......

THE
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[THIRD SERIES.]

AUGUST 1846.

XIV. *On the Annual Motions of the Earth's Crust.*

By the Rev. T. R. Robinson, *D.D., &c.*

To Richard Taylor, Esq.

Dear Sir,

I SEE in the last Number of the Philosophical Magazine, that Mr. Mallet has, in a paper on the Secular and Diurnal Motions of the Earth's Crust, referred to my observations respecting the annual variations of my instruments. In a brief notice, which will be found in the Royal Irish Academy's Transactions, vol. xix. p. 193, I expressed a belief that these are of local origin, being produced by changes of temperature acting in the immediate vicinity of the observatory; and I gave the index corrections of the circle during the year 1839 to show their magnitude and character. The subject of which I was then treating did not permit any fuller explanation; but the following Table, in which I have comprised the results of the last eight years, may prove useful to any of your readers who pursue the inquiry suggested by Mr. Mallet.

Its second column shows for each month the mean elevation of the west end of the transit-instrument's axis, as determined by the level: this operation is in general performed every day on which observations are made; and whenever the adjustment has been altered, the amount of change was carefully measured. As however in such a case the surfaces which are moved do not assume a permanent bearing for some time, a slight degree of inaccuracy may exist as compared to

The third column, which gives the change of the horizontal point of the mural circle, or (expressing it similarly to the preceding) the elevation of the northern extremity of a line, on the meridian face of the pier, supposed horizontal at the season of greatest cold. At first it was derived from direct and reflected observations of stars, latterly from those of the lines of the telescope; and each of these columns is based on more than 2000 observations.

The fourth gives the mean temperature of each month as shown by a standard thermometer in a northern aspect, noted at 10 o'clock in the morning and evening.

Month.	West.	North.	Temperature.
January	0″·00	+0″·59	40°·0
February	+0·21	+0·00	38·7
March	+0·24	+0·12	41·7
April	+1·22	+1·08	47·1
May	+1·85	+2·22	51·2
June	+2·56	+4·35	56·5
July................	+1·94	+5·65	56·5
August	+2·43	+5·85	57·0
September	+2·63	+5·21	54·1
October	+1·81	+4·62	47·0
November	+1·12	+3·35	42·9
December	+1·12	+1·88	41·3

Besides these the transit instrument is disturbed in azimuth when any sudden change of temperature occurs, and probably also with an annual period; of this however I have seldom obtained measures, as whenever observed it is corrected by the meridian mark. Cold makes the western pivot move to the south.

On inspecting the table, it is evident that both these movements depend on the temperature, having their maxima and minima at the times of greatest heat and cold; the second exactly, the first less precisely, but still with a decided approximation. Nor do they vary according to the same law, which may probably arise from some secondary force acting more powerfully in one direction than the other. Can it be the infiltration of water into the subjacent strata? When sinking a well many years since, no water was found till the limestone (which under the observatory is covered by about 100 feet of clay) was bored through, when it rose with great violence about 40 feet. The hydrostatic pressure, varying with the supply at different times, seems capable of such an action. The motion of the western point is such as might be explained in kind, though not perhaps in magnitude, by Mr. Mallet's hypothesis of a general expansion, and consequently increased curvature of the surface of Ireland; but that of the northern seems to me exactly the reverse, unless we assume that the expansion of the trap rocks which predominate in

Antrim is so much greater than that of the clay-slate which is found to the south, as not merely to overcome it, but to exhibit a large residual action. If we regard the two as resultants of an inclination of the surface, its intersection with the horizon is determined by the equation tang $A = \frac{5''\cdot 85}{2''\cdot 63}$, and must make with the meridian an angle of 65° 48′ east, which is, I believe, pretty nearly that of the junction of the limestone and slate a little south of Armagh.

I may add that such changes are not peculiar to this observatory; in fact few will be found exempt from them. The late Mr. Henderson found that the Edinburgh transit changed so regularly with the temperature, shown by a thermometer sunk twenty-four feet in the rock, that it was a matter of indifference whether he corrected by the level or the thermometer. Comparing the months of February and August there, W. sinks $5''$ and N. rises $3''$ in summer. At Cambridge, the same months give by a mean of 1838 and the four following years, that W. sinks $1''\cdot 72$ and N. sinks $5''\cdot 14$; and at Greenwich W. sinks about $2''$ and N. rises about $2''\cdot 5$. This diversity seems to show that each station has its peculiarities, and that no general system can include the whole. It may also, I think, be inferred, that the agency, whatever it be, is not necessarily of wide diffusion; and therefore it might be possible in any future construction of a standard observatory, to select, by preliminary experiments, a locality free from such influence. But to do this with effect would require a much larger collection of facts than we at present possess.

Believe me,
Yours sincerely,
T. R. Robinson.

Observatory, Armagh,
July 11, 1846.

XV. *On Conjugate Points in relation to* Prof. Young's *paper on this subject, Philosophical Magazine for August* 1845. *By* H. S. Warner, *Esq.**

I HAVE been induced to consider the subject of conjugate points, from observing that what appears to me to be an important error in Prof. Young's paper "On Imaginary Zeros and the Theory of Conjugate Points" (Phil. Mag., vol. xxvii. pp. 91–97), has not been noticed in any subsequent number of the Philosophical Magazine.

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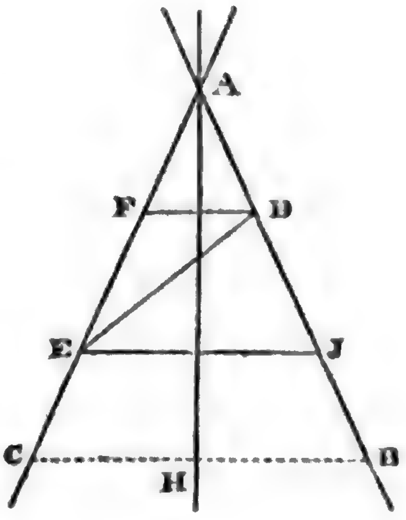
* Communicated by the Author. [The publication of this paper has been delayed, from a misgiving as to some of the views contained in it, which perhaps may not meet with acceptance from mathematicians.—Ed.]

1. Prof. Young says (p. 92), that in the extreme case of the ellipse (the minor axis diminishing till it vanishes), the curve degenerates into a finite straight line, the equation in this ultimate state being $y = \frac{0}{a}\sqrt{a^2-x^2}$. He thus continues: "Between the limits $x=-a$ and $x=+a$, the evanescent quantity on the right is *real*, implying that every point on the axis of x, which does not lie without these limits, is a real point of the locus. But if x exceed a, in either the positive or negative direction, the evanescent quantity is *imaginary*; and consequently in accordance with the ordinary interpretation, the corresponding points on the axis of x are beyond the bounds of the locus." Now for any point in the axis of x, at which x is greater than a, positively or negatively, the equation is of the form $y = 0\sqrt{-1}$; and as he says that this point is beyond the bounds of the locus, he must consider $0\sqrt{-1}$ as unassignable: *in this consists his error*, for this expression is equal to neither more nor less than 0; thus

$$0\sqrt{-1}=\sqrt{(-1\times 0^2)}=\sqrt{-0}=\sqrt{0}\ (\text{since } +0=-0)=0:$$

and hence for any point in the axis of x (whether at that point x is greater or less than a) $y=0$, so that the locus is an infinite straight line coinciding with the axis of x, and not, as he asserts, a finite straight line of the length of the major axis, or $2a$.

2. Perhaps however the geometrical consideration of the case will render this more evident. If A B C be a section of a cone by a plane passing through the axis A H, and if another plane at right angles to this one intersect it in the line D E, then the line of intersection of the conic superficies and this latter plane will be an ellipse, of which the major axis a will be $=$ D E; and if D F, E J be lines drawn across the triangle A B C, perpendicular to A H, it is well known that the minor axis b of the ellipse will be $=\sqrt{\text{DF}\cdot\text{EJ}}$. If now the points D and E be supposed to move along the lines B A, A C (D approaching A, and the distance D E remaining unchanged, which is equivalent to supposing that in the equation to the ellipse b diminishes while a remains constant) till $b=0$, this will be the case *only when* D *coincides with* A; and *then* the conic section will coincide with the side



A C of the triangle A B C, that is, it will be an infinite straight line. Even if the imaginary quantity were considered as a real quantity, measured along a line perpendicular to the plane of the co-ordinates x and y, which is what is meant, I suppose, by "the imaginary curve being viewed as real curve out of the plane of co-ordinates," the expression $0\sqrt{-1}$ would indicate a point in, and therefore assignable on, that plane, for it would indicate a point neither *above* nor *below*, and therefore *in*, it.

3. Similarly also, the hyperbola will become in the ultimate state an infinite straight line, and not, as Prof. Young says, "two infinite straight lines *in directum* with the axis of abscissas, but separated from each other by an interval equal to the fixed principal diameter." Having, I think, made it evident that it is an error to consider $0\sqrt{-1}$ as unassignable, it being really equivalent to 0, I shall now consider the theory of conjugate points.

4. If $y = \phi(x)$ be the equation to a curve, and we can put $\phi(x)$ under the form $f_1(x) + f_2(x)$, so that $y = f_1(x) + f_2(x)$, and if we lay down the curve which is the locus of $y = f_1(x)$, then to determine the locus of $y = \phi(x)$, for any value of x, we shall merely have to measure along the ordinate drawn at that value of x, a distance equal to the value of $f_2(x)$ corresponding to the given value of x; measuring from the intersection of the ordinate and the curve $y = f_1(x)$; this curve we may call the *axial curve*, the other one, $y = \phi(x)$, being derived by measuring from it as from an axis. If at any point of the axial curve $f_2(x)$ is $= 0$, then that point is a point in the locus of $y = \phi(x)$ also; this is evident. Also, if for any value of x, $f_2(x)$ is impossible, then at that value of x the curve $y = \phi(x)$ will be unassignable, for we cannot assign any distance to be measured along the ordinate, and hence cannot exhibit the curve.

5. If now, for a continuous series of values of x, $f_2(x)$ is of the form $A + B\sqrt{-1}$, the curve will be unassignable for all these values of x (by No. 4), for $f_2(x)$ will be impossible; but if continuing the series, we at last reach a value of x at which $B = 0$, so that $f_2(x)$ is of the form $A + 0\sqrt{-1}$, the curve will be assignable, for $A + 0\sqrt{-1} = A + 0 = A$ (by No. 1); and if further $A = 0$, it will be a point of the axial curve corresponding to the same value of x, that will be the locus. If now, still continuing the series of values, B continues $= 0$, or $f_2(x)$ ceases to be impossible, the imaginary quantity $\sqrt{-1}$ disappearing, the locus will be assignable; and thus we shall have a real curve commencing at that ordinate at which $B = 0$. But if, on the other hand, on continuing the series, the ima-

ginary $\sqrt{-1}$ does not disappear, and its coefficient B has a value, whether positive or negative, different from 0, then the curve is unassignable on this side of the ordinate at which $B=0$ as well as on the other; and hence the equation expresses at this portion of the locus a single point, isolated and unconnected with any (assignable or real) part of the curve that is a conjugate point.

6. Hence the following rule:—Having put the equation under the form $y=f_1(x)+f_2(x)$, then if for any particular value of x such as a', $f_2(x)$ becomes of the form $A+0\sqrt{-1}$, while both for a value of x a little greater and for one a little less than a' (such as $a'\mp h$) it is of the form $A+B\sqrt{-1}$, there will be a conjugate point determined by the co-ordinates $x=a'$, $y=f_1(a')+A$; but if $f_2(x)$ is of this form only on one side of a', there will be a cusp determined by these co-ordinates.

In the case of an implicit equation: if any values of x and y, such as $x=a'$, $y=b'$, satisfy the equation, while both when x is taken a little greater and a little less than a', y must be of the form $A+B\sqrt{-1}$; to satisfy the equation there will be a conjugate point at $x=a'$, $y=b'$; but there will be a cusp instead of a conjugate point if it is only on one side of a', that y is of the form $A+B\sqrt{-1}$.

7. In dividing $\phi(x)$ into the two other functions, $f_1(x)$, $f_2(x)$, we must take care so to assume $f_1(x)$ that it may be possible for all the parts of the locus that we wish to examine in regard to conjugate points. If $\phi(x)$ cannot be divided into two functions such as we desire, we can always obtain the *necessary* form of equation $y=f_1(x)+f_2(x)$ by taking $f_1(x)=0$, and $f_2(x)=\phi(x)$, so that the equation is $y=0+\phi(x)$, and the axial curve or locus of $y=f_1(x)$ is the locus of $y=0$, that is, a straight line coinciding with the axis of x.

8. As an example, let us take the equation

$$\left(\frac{y-x}{a-x}\right)^2 = x-2a,$$

whence we have $y=x+(a-x)\sqrt{x-2a}$;

then $f_1(x)=x,\quad f_2(x)=(a-x)\sqrt{x-2a}.$

Now when $x=a,\quad f_2(x)=0\sqrt{-a}=0\sqrt{-1}$;

while for $x=a\mp h$ (h being less than a) it is equal to

$$\pm h\sqrt{a\pm h}\,.\,\sqrt{-1};$$

hence there is a conjugate point determined by

$$x=a,\quad y=f_1(a)=a.$$

Again, for $x = 2a$, $f_2(x) = 0 = 0 + 0\sqrt{-1}$,
while for $x = 2a + h$, it is $= -(a+h)\sqrt{h}$;
and for $2a - h$,

$$f_2(x) = -(a-h)\sqrt{-h} = -(a-h) \,.\, h^{\frac{1}{2}} \sqrt{-1};$$

or possible for $2a + h$, and impossible for $2a - h$; hence there is a cusp determined by

$$x = 2a, \quad y = f_1(2a) + 0 = 2a.$$

9. It will be seen that these rules are substantially the same as those of Prof. Young; the only difference is, that he says that we must find a value of y in which the symbol $0\sqrt{-1}$ appears, and then suppress this evanescent imaginary quantity; while I say that it suppresses itself, being equal to 0. I have considered only the imaginary $\sqrt{-1}$, but it will be found that the same principle applies to other imaginary expressions, such as $\log -1$, $\sin \sqrt{-1}$, $\sin^{-1} \sqrt{-1}$, &c., namely that each of these when multiplied by 0, is equal to 0; the reason being that no quantity but ∞ can, when multiplied by 0, produce anything but 0, and these imaginaries are not equal to ∞.

Perhaps in a future Number I may consider some other points connected with these subjects.

Trinidad, January 3, 1846.

Postscript.

Port of Spain, Trinidad,
April 3, 1846.

I find that I inadvertently made use of the term *cusp* in speaking of that point at which a curve ends abruptly. This certainly was an error, but I know not what name to give to this point instead of cusp; for there appears to be no recognised appellation, as yet, in consequence of the little attention that has been given to this peculiar kind of point. Neither the "*abrupt termination*" of Prof. De Morgan, nor the "*point d'arrêt*" of the French writers seems to me to be a good name for it. Query: why should we not call it *a terminus*? We might then define *a cusp* as the common *terminus* of two or more branches of a curve.

H. S. WARNER.

XVI. *On the Connexion of the Circle and Hyperbola; and on the Geometrical Interpretation of Imaginary Exponentials.* *By* H. S. Warner, *Esq.*[*]

THE following investigations relate to an interesting branch of modern science; but as it is dangerous to claim, at this time, anything as one's own without a careful examination of preceding writings, and as I am not acquainted with the recent progress of research in regard to what were called imaginary quantities, I send these observations for publication, merely on the chance that they are new; hoping that if they are not so, I may be informed of the fact through the columns of the Philosophical Magazine.

1. The assumption on which I proceed is that,—

The proper geometrical interpretation of $A\sqrt{-1}$ *is a line of the length* A, *measured in a direction perpendicular to that in which it would have been measured if it had not been multiplied by* $\sqrt{-1}$.

2. Taking the equation of the equilateral hyperbola (the centre being the origin, and, for simplicity, the semi-axis being the unit of length) $y^2=x^2-1$, we find, that for a value of x (positive or negative) greater than unity, y is possible, determining the opposite hyperbolas; but that for a value of x less than unity, the equation assumes the form $y^2=-1\times(1-x^2)$ or $y=\sqrt{-1}\times\sqrt{(1-x^2)}$. Hence ($y=\sqrt{1-x^2}$ being the equation of a circle whose radius $=1$) by the assumption above, the hyperbola will be represented between $x=+1$ and $x=-1$, by a circle (radius 1) whose ordinates are measured perpendicular to the plane of xy; that is by a circle standing upright, or a circle described on a plane at right angles to the plane of xy, the intersection of the two planes coinciding with the axis of x. The perpendicular plane I must, for want of a better name, call the *imaginary plane.*

3. Considering the circle in the same way, we shall find that for positive or negative values of x greater than unity, it is represented by a pair of opposite hyperbolas, drawn on the imaginary plane. Hence we arrive at this remarkable result, *that the equilateral hyperbola and the circle are similarly related to planes at right angles to one another*, or *that to change the one curve into the other, we have merely to take the imaginary plane as the real one, and vice versâ.* The figure will illustrate this: C A X is the axis of x, C Y the axis of y, and C K a line perpendicular to the plane of xy, or what may be called the axis of imaginaries: C K and C L are the asymp-

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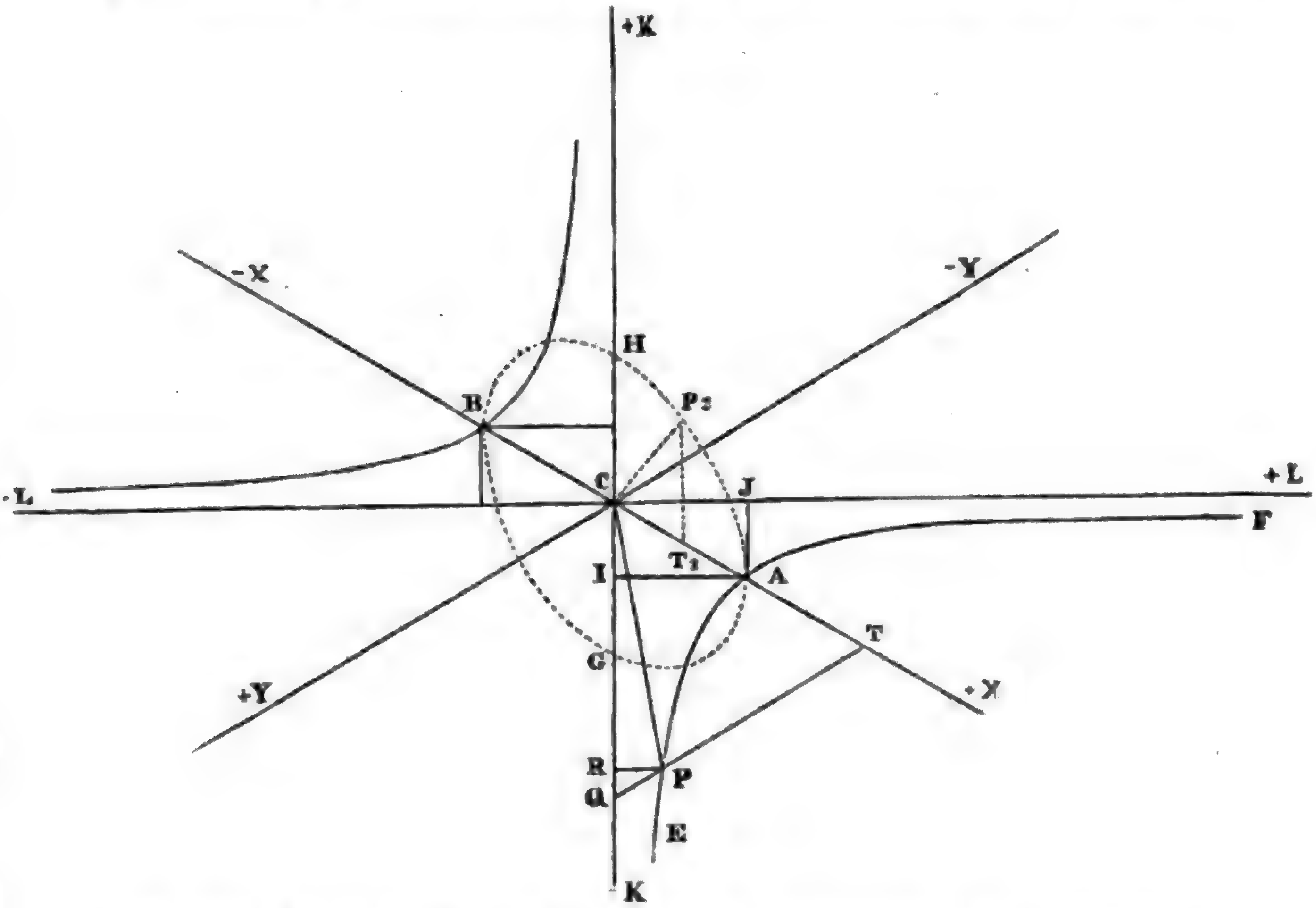
[*] Communicated by the Author.

totes (from the perspective used the axis of imaginaries appears to coincide with one of the asymptotes), and C I A J is the inscribed square of the asymptotes. The dotted lines (representing a circle as commonly drawn) are the lines on the one plane, while the black ones (representing the common form of the hyperbola) are those on the other plane: hence if the black lines be considered as those on the real plane, the dotted circle will be on the imaginary plane, and the figure will be the locus of the *hyperbola*: if, on the other hand, the dotted lines be considered as being on the real plane, the black ones will be imaginary, and the figure will be the locus of the *circle*.



4. If we take any point P on the hyperbola (regarding for the present the black lines as those on the real plane) determined by the co-ordinates $CT = x$, $TP = y$, and draw the line P R parallel to C L, then by the property of the hyperbola we have

$$\frac{\text{area } IAPR}{\text{area } CIAJ} = \log \frac{CR}{CI};$$

but $IAPR = ACP, \quad CIAJ = \frac{1}{2}, \quad CI = \sqrt{\frac{1}{2}};$

$$\therefore 2\,(ACP) = \log\,(CR \times \sqrt{2}). \quad . \quad . \quad . \quad . \quad \text{(a.)}$$

The equation of the hyperbola being $y = \sqrt{x^2 - 1}$, and of the asymptote $y = x$, we have

$$CQ = x\sqrt{2}, \quad QR = \sqrt{\frac{1}{2}} \times PQ = \sqrt{\frac{1}{2}} \times (x - \sqrt{x^2-1}),$$

$$CR = CQ - QR = x\sqrt{2} - (x - \sqrt{x^2-1})\sqrt{\frac{1}{2}}$$

$$= (x + \sqrt{x^2-1})\sqrt{\frac{1}{2}} \quad \therefore \; CR\sqrt{2} = x + \sqrt{x^2-1}$$

$$= CT + TP,$$

hence we have

$$2\,(ACP) = \log\,(CT + TP) = \log\,(x + y). \quad . \quad . \quad . \quad \text{(b.)}$$

5. Let us now observe the variations of the quantities that form this equation, as the point P moves along the curve, or is assumed at different points of it.

As P approaches A, both the area A C P and C T + T P diminish till at A we have $2 \times 0 = \log 1$.

Suppose P to be taken on the branch A F, then A C P and T P will both be negative, and the algebraic sum of C T and T P will be their arithmetical difference, which will be always less than 1; hence the logarithm of a number less than unity is negative.

Let us now suppose that a point on the imaginary circle is taken, as at P_2. Then $2\,(A C P_2) = \log\,(C T_2 + T_2 P_2)$; but if the arc $A P_2 = \theta$, we have

$$2\,(A C P_2) = \theta, \quad C T_2 = \cos\theta, \quad T_2 P_2 = \sin\theta;$$

but $C A P_2$ and $T_2 P_2$ being both on the imaginary plane are affected with the symbol $\sqrt{-1}$, while $C T_2$ being on the real plane is unaffected with it; hence finally we have

$$\left.\begin{array}{l} \theta\sqrt{-1} = \log\,(\cos\theta + \sqrt{-1}\,.\,\sin\theta) \\ \text{or} \quad \varepsilon^{\theta\sqrt{-1}} = \cos\theta + \sqrt{-1}\,.\,\sin\theta \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad \text{(c.)}$$

Similarly, if P_2 be taken on the imaginary branch below the real plane so that θ is negative, we have

$$\varepsilon^{-\theta\sqrt{-1}} = \cos\theta - \sqrt{-1}\,.\,\sin\theta. \quad . \quad . \quad . \quad \text{(c'.)}$$

Let θ be $= \frac{\pi}{2}$ so that P_2 falls on H, then $C T_2$ or $\cos\theta$ vanishes, so that

$$\frac{\pi}{2}\sqrt{-1} = \log\sqrt{-1} \;\text{ or }\; \varepsilon^{\frac{\pi}{2}\sqrt{-1}} = \sqrt{-1}. \quad . \quad . \quad . \quad \text{(d.)}$$

When P_2 reaches B,

$$\pi\sqrt{-1} = \log\,(-1) \;\text{ or }\; \varepsilon^{\pi\sqrt{-1}} = -1. \quad . \quad . \quad . \quad . \quad \text{(e.)}$$

And at G,

$$\frac{3\pi}{2}\sqrt{-1} = \log(-\sqrt{-1}) \text{ or } \varepsilon^{\frac{3\pi}{2}\sqrt{-1}} = -\sqrt{-1}. \quad . \quad \text{(f.)}$$

$$\text{At A,} \quad 2\pi\sqrt{-1} = \log 1 \text{ or } \varepsilon^{2\pi\sqrt{-1}} = 1.. \quad . \quad . \quad \text{(g.)}$$

From A we may suppose P_2 to run over the circle a second time and then a third time, and so on to an *m*th time; so that in the formulas (c.), (d.), &c. we may increase the coefficient of $\sqrt{-1}$ in the exponent of ε, by $2m\pi$ without changing the result; also we may suppose P_2 on arriving at A after the *m*th revolution, to proceed along the real branch of the curve so that the logarithm of any quantity X may be represented by tabular $\log X + 2m\pi\sqrt{-1}$.

6. Perhaps it may not be unimportant to remark, that in describing the properties of the hyperbola, the proper way of enunciating the connexion of hyperbolic spaces with logarithms is the one I have used, namely $2(\text{A C P}) = \log(\text{C T} + \text{T P})$; for this expression holds good as well for the imaginary as for the real branch of the hyperbola, as I have shown above, while the common expression $\frac{\text{I A P R}}{\text{C I A J}} = \log\frac{\text{C R}}{\text{C I}}$ is quite unintelligible when applied to the imaginary circle, being derived from the consideration of the asymptotes, which are no part of the curve, though usually drawn with it.

7. There are several other points arising out of the connexion of the circle and hyperbola which it would exceed my limits to touch upon here, but which I shall probably make the subject of another article, should it turn out that the preceding results are new.

8. In conclusion, I may remark that this investigation is as yet incomplete, or imperfect, for the most general formula arising from it is $\log X = Y + 2m\pi\sqrt{-1}$ (Y being the arithmetical logarithm of X), while the most general formula obtained by other means is

$$\log X = \frac{Y + n\pi\sqrt{-1}}{1 + 2m\pi\sqrt{-1}}.$$

See De Morgan's Calculus, p. 384.

Port of Spain, Trinidad,
April 2, 1846.

XVII. *On the unequal Decomposition of Electrolytes, and the Theory of Electrolysis.* *By* Mr. JAMES NAPIER[*].

IN a paper which I submitted to the Society last session upon this subject[†], it was mentioned that the fundamental law laid down by electro-chemists, "That there can be no inequality of decomposition in any part of an electrolyte," is not applicable under all circumstances, especially when the negative element of the electrolyte can combine with the positive electrode, there being under such circumstances an increased tendency in the electrode to combine with the electrolyte, which causes a greater amount of decomposition or chemical action at the positive than at the negative electrode.

That the extra amount of decomposition at the positive electrode is not the result of the ordinary solvent powers of the electrolyte for the metal composing the electrode, was shown by pieces of the same metal being put into the same solutions as the electrolyte the same length of time as the battery was in action, and which were in some cases not affected, and in others only a small fraction of that which had taken place at the positive electrode above what the current of electricity passing accounted for; showing that the increased affinity between the elements of the electrolyte and the electrode had its origin in some influence communicated by the battery, and was not accounted for by the amount of electricity passing, measured by the deposition of a metal upon the negative electrode, and as experiments which were being made showed, without any relation to that current.

I had observed, while experimenting upon electrical endosmose, that there seemed some relation between the cause of measurable endosmose and the phænomenon under consideration: it became therefore probable that the element of an electrolyte liberated at the negative pole, such as a deposited metal, might not be, as I had formerly thought, an accurate measure of the whole electricity passing through the solution under all circumstances, but that a feeble current may be also passing, which may be sufficient to give a greater disposition, if I may be allowed the term, to the solid electrode to combine with the elements of the electrolyte, but not of sufficient power to decompose all the particles of the compound fluid through which it passes to the negative electrode, the solution conducting it as a solid conductor would, or rather, as appears from the results described in my paper on

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[*] Communicated by the Chemical Society; having been read January 5, 1846.

[†] See Phil. Mag. S. 3. vol. xxvi. p. 211.

endosmose, the electricity taking the dissolved particles with it to the negative electrode and producing the phænomena of endosmose. These suppositions therefore became the object of an inquiry, which I shall now describe in detail.

I took a piece of amalgamated zinc, measuring 2 by 2½ inches, in such state of amalgamation that dilute sulphuric acid had no action upon it; this was put into a mixture of sulphuric acid and water in the proportion of 1 acid to 24 water, and surrounded with copper, which was placed as close to the zinc as would allow a free current of the solution between them, as well as the escape of gas, with the view of interrupting as little as possible the free action of the acid upon the zinc. This was kept in action for an hour, when it was found that the zinc had dissolved from it 97 grains. This I assumed as the maximum amount of chemical action which could be obtained between the zinc and acid of this strength, at least under the influence of copper. The zinc and copper being again put into this liquid, but with the two metals 1 inch apart, connected by a slip of copper at the surface of the liquid, in one hour there was dissolved from the zinc 56·5 grains. The two metals being again placed in the same position, but connected by two copper electrodes of equal size placed into a solution of sulphate of copper, at the expiration of an hour there was deposited upon the negative electrode 9 grains of copper, and there were dissolved from the zinc 10·7 grains. Taking another zinc and copper and putting them into the acid connected with the first in the manner practised for intensity, there were deposited in one hour 17 grains. The zinc in cell had lost 18 grains. With 4 pairs zinc and copper in acid there were deposited 26 grains, and dissolved from the zinc 27·7 grains. With 6 pairs there were deposited 34 grains, and dissolved from zinc 36 grains; and with 9 pairs there were deposited 43 grains, and dissolved from the zinc 45 grains. And thus it went on increasing in quantity by the addition of plates until the action of the acid and zinc came nearly to the same as was assumed as a maximum, namely, 97 grains; with 30 pairs there were dissolved from zinc 101 grains, with 35 pairs 99, which I consider very close to the assumed quantity.

Assuming the position which the results of these experiments suggest, I would say, that if the chemical energy induced between the acid and zinc under the influence of copper be equal to 100 grains per hour, but by distance or the interposition of a medium of inferior conducting power the chemical action is reduced to 10 grains per hour, the remaining tendency or disposition of the acid and zinc to unite will be equal to 90; and by increasing the action, either by adding intensity or decreasing the resistance, so that it amounts to 25 grains per hour, we decrease the tendency to further action to 75, and so on, the one decreasing as the other increases. Now, if this tendency for further action between the acid and zinc be the source of this feeble or undecomposing current, and this the cause of an extra chemical action at the positive over the negative electrode, then the amount of this extra action at this locality will be greatest with a weak battery, at least have some relation to the surface of zinc exposed and the amount of actual chemical action going on. That this is the case the following experiments will show. To save repetition, I may state that when small plates are mentioned the measures of the zinc are 2 by 2½ inches; when large plates are named they measure 5 by 6 inches.

1. With small plates, the electrodes being the same size as plates, both placed in sulphate of copper with a very little free acid. The current passing four hours, the results were as under:—

One pair negative gained	27 grs.	Positive lost	30·5 grs.
Two pairs ...	44 ...	...	49 ...
Four pairs ...	58 ...	...	63 ...
Six pairs ...	70 ...	...	73 ...
Nine pairs ...	85 ...	...	87 ...

Taking these results in equivalent proportion, counting the equivalent of copper 32, the extra action at positive will be—

One pair	4·1 grains.
Two pairs	3·6 ...
Four pairs	2·7 ...
Six pairs	1·3 ...
Nine pairs	0·7 ...

The next experiment was with cyanide of potassium in the proportion of 1 ounce to the pint of water. This was put into one division of a decomposition cell, the other being charged with sulphate of copper; silver positive electrodes were used in the cyanide solution. The battery used was of small plates, the current passing two hours.

One pair negative gained	14 grs.	Positive lost	57 grs.
Two pairs ...	14 ...	...	50 ...
Four pairs ...	20 ...	...	70 ...
Six pairs ...	21 ...	...	71 ...

The equivalent of deposit being again taken, the proportions of extra solution of positive stand thus:—

One pair	9·1 grs.
Two pairs	6·8 ...
Four pairs	5·7 ...
Six pairs	0·9 ...

I now took a strong solution of cyanide of potassium and divided it into three equal parts: into two were placed porous vessels filled with sulphate of copper, in which were put the negative electrode of the battery, one connected with 9 pairs of small plates, the other with 1 pair of large plates. The same sort of electrodes were used in both experiments. The third portion of cyanide of potassium solution had a piece of copper placed in it of the same size with the electrodes. The battery current was continued one hour. The results were—

9 pairs deposited 12 grains, dissolved 15 grains; equivalent 8 grains.

1 pair deposited 4 grains, dissolved 7 grains; equivalent 24 grains.

Piece of metal in cyanide potassium lost 1·2 grain.

Similar experiments were made with various electrolytes in the positive division, using sulphate of copper in all cases in the negative, the amount deposited being taken as the measure of the decomposing current.

1st. Positive cell charged with a weak solution of cyanide of potassium.

1 pair deposited 7 grains, dissolved 10 grains; equivalent proportion 13·7 grains.

9 pairs deposited 26 grains, dissolved 31 grains; equivalent proportion 6·1 grains.

2nd. Muriatic acid 1 to 24 water in positive solution.

1 pair deposited 15 grains, dissolved 23 grains; equivalent 17 grains.

9 pairs deposited 54 grains, dissolved 69 grains; equivalent 8·8 grains.

3rd. Positive solution, nitric acid 1 to 24 water.

1 pair deposited 17 grains, dissolved 20 grains; equivalent 5·6 grains.

9 pairs deposited 26 grains, dissolved 29 grains; equivalent 3·7 grains.

4th. Positive solution, sulphuric acid 1 to 24 water.

1 pair deposited 20 grains, dissolved 23 grains; equivalent 4·3 grains.

9 pairs deposited 49 grains, dissolved 55 grains; equivalent 3·9 grains.

The next series of experiments are of the same kind, using

1 and 2 pairs of large and small plates; but two porous vessels were used, put into a vessel charged with the same liquid as the positive, the negative in this, as in the last, being charged with sulphate of copper, the electrodes being 2 by $2\frac{1}{2}$ inches in all the experiments.

1st. Positive solution, sulphuric acid 1 to 24 water.

1 pair small plates deposited 9 grains, dissolved 11 grains; equivalent 7 grains.

1 pair large plates deposited 9 grains, dissolved 12 grains; equivalent 10·6 grains.

2 pairs small plates deposited 16 grains, dissolved 18 grains; equivalent 4 grains.

2 pairs large plates deposited 19 grains, dissolved 22 grains; equivalent 5 grains.

2nd. Positive solution, muriatic acid 1 to 24 water.

1 pair small plates deposited 5 grains, dissolved 6 grains; equivalent 6·3 grains.

1 pair large plates deposited 7 grains, dissolved 9 grains; equivalent 9·1 grains.

2 pairs small plates deposited 9 grains, dissolved 12 grains; equivalent 9·4 grains.

2 pairs large plates deposited 11 grains, dissolved 16 grains; equivalent 14·5 grains.

3rd. Positive solution, nitric acid 1 to 24 water.

1 pair small plates deposited 8 grains, dissolved 9 grains; equivalent 4 grains.

1 pair large plates deposited 8 grains, dissolved 10 grains; equivalent 8 grains.

2 pairs small plates deposited 14 grains, dissolved 15 grains; equivalent 2·3 grains.

2 pairs large plates deposited 19 grains, dissolved 21 grains; equivalent 3·3 grains.

I now took three separate pairs of small plates excited by 1 sulphuric acid to 24 water, and to each attached copper electrodes, which were placed in a solution of sulphate of copper: the electrodes of one were one-half size of the battery plates, of the other equal size, and of the third twice the size of the battery plates. The current passed sixteen hours, the results were as follows:—

Small negative electrode gained 25·4 grains, positive lost 29·3 grains; equivalent 4·9 grains.

Equal-sized negative electrode gained 33 grains, positive lost 37 grains; equivalent 3·8 grains.

Large negative electrode gained 42 grains, positive lost 44 grains; equivalent 1·5 grain.

A piece of zinc and copper, each measuring 3 inches by 3, were placed in distilled water, and two small electrodes, 1 by 2 inches, attached and placed in a solution of sulphate of copper. The current passing was sufficient to deflect a delicate galvanometer needle 3°: the current was allowed to pass seventy-two hours. The electrodes being again weighed, the negative had undergone no change, the positive had lost 2·6 grains.

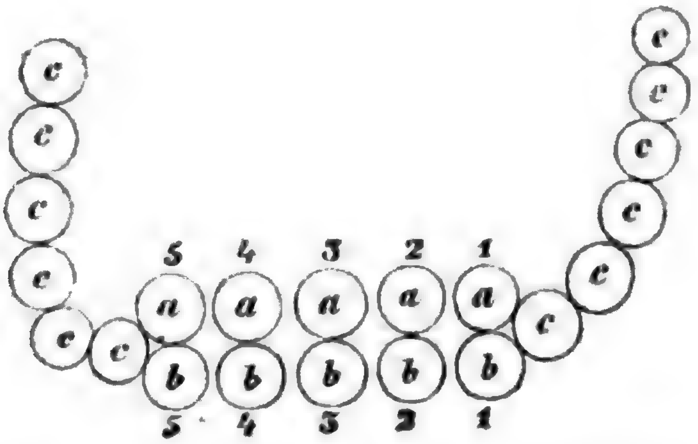
Another experiment of the same kind, but the electrodes placed in dilute sulphuric acid, 1 to 100 water, the current passed forty-eight hours; the negative electrode was found unchanged, the positive had lost 2 grains. A piece of copper suspended in the acid solution during the same time remained unchanged.

These experiments, which are only a few of a great many that were made, all bearing more or less on the point, I think sufficiently prove the view taken both of the origin of the undecomposing current of electricity, and also that it is that current which produces the phænomenon under discussion; namely, a disposition in the positive electrode and in the zinc in the battery to combine with the negative element of the electrolyte up to a given point, varying according to the strength of acid, and the negative properties, if we may so term it, of the metal in contact with the zinc, through the influence of which the chemical action is induced.

The results of the experiments of this as well as my preceding paper upon electrical endosmose, are applicable to the explanation of various natural and chemical phænomena, the investigations into some of which I am at present engaged with. At present I shall confine myself to a few remarks upon the philosophy of electrolytic action which these experiments suggest.

The manner in which electricity passes through and decomposes solutions, is a subject that has occupied the attention of electro-chemists since its power to do so was first known, and the opinions published are almost as numerous as the investigators; the greater part of which having been already collected and published by Professor Faraday in his Fifth Series of Researches, need not be repeated here. Suffice it to say, that the whole of these theories being based upon the supposition that there is a mutual transfer of both the negative and positive elements of an electrolyte, and that supposition being now found incorrect, these theories cease to be tenable.

From the first time that I observed that the base of an electrolyte was not transferred, my mind became impressed with a theory of electrolytic action, which seemed to agree with the results of my every-day experience in electro-metallurgy, and also with various experiments made on a small scale, in order to test these views more fully. During these trials I was favoured with a copy of the paper of the late Professor Daniell and Professor Miller on the decomposition of secondary compounds, in which the non-transfer of the basic element is observed, but the statement made at the same time that this is not universal, some elements, such as potassium, sodium, &c., being transferred in certain proportions. These statements interfering with my views, induced me to investigate the subject more closely. The paper now submitted to the Society forms a portion of these investigations, the results of which induce me to think it probable that the results obtained by these gentlemen were due to what I term unmeasurable endosmose rather than to electrolytic transfer; and that no basic element of an electrolyte is ever transferred by electrolytic action, but that the base of an electrolyte which is being decomposed is the medium or conductor of the electricity through the solution from electrode to electrode. And the manner in which this takes place I conceive to be as follows:—



The double row represents a line of compound atoms forming an electrolyte, *a* the acid or negative element of the electrolyte, *b* the base or positive element of the electrolyte, *c c* the wires or solid conductors of the electricity from the battery to the decomposition cell; the last particles in contact with the electrolyte may be viewed as the electrodes. The *a b* particles are held together by their affinity for each other.

Now let it be supposed that an equivalent of electricity leaves the positive terminal of the battery and passes along the solid particles of the conductor, that particle upon which the electricity rests must be for the time in a higher state of excitement than the other particles. When the electric current comes to the last particle of the solid chain which is in contact with the electrolyte, its increased excitement causes it to attract and combine with the acid particle nearest it, *a* 1; when these unite the electricity passes to the first basic particle *b* 1, giving it an exalted excitement, which causes it to unite with the acid particle *a* 2, the electric force passing to *b* 2, which becomes excited in turn and takes the particle *a* 3; and so on through the chain till the last particle *b* 5, which, having no further acid to combine with, gives its electricity to the solid conductor which passes along to the battery. If the last particle *b* 5 be a metal that can exist under the circumstances in which it is left, such as copper, silver, &c., it accumulates upon that electrode as a deposit; if not such as can exist under these circumstances, such as potassium, &c., it decomposes water and hydrogen is evolved. By this we observe that every equivalent of decomposition will carry an equivalent of acid to the positive electrode. This is exactly what is found by experiment to be the case. That these decompositions and combinations amongst the particles of a salt may produce a current of that salt in the direction of the electric current producing endosmose, can very easily be conceived.

Whether this be the true philosophy of electrolytic action is yet to be further investigated; in the mean time it does not appear inconsistent with any experiments I have yet investigated: but should it be found not to account for electrolytic action under all circumstances, it will be gratifying should it only prove a stepping-stone to a clearer view of the subtile action of this power.

It appears to me that the phænomena observed and described in these papers favour the idea that electricity is but one power or substance, and that that power is identical with chemical affinity. But the further consideration of this point must be deferred till another opportunity.

XVIII. *On a convenient Instrument for graduating Glass Tubes, invented by* Prof. Bunsen *of Marburg*[*].

THIS instrument consists of a mahogany board $5\frac{1}{2}$ feet long, 7 inches wide, three-quarters of an inch thick. Throughout its centre is a groove 1 inch wide, half an inch deep, arched at bottom, for the reception of tubes. At one part, 5 inches from the end, is placed a brass plate, $1\frac{1}{3}$ foot long and 2 inches wide, in such a position that when screwed down its edge comes one-half over the groove. It is furnished

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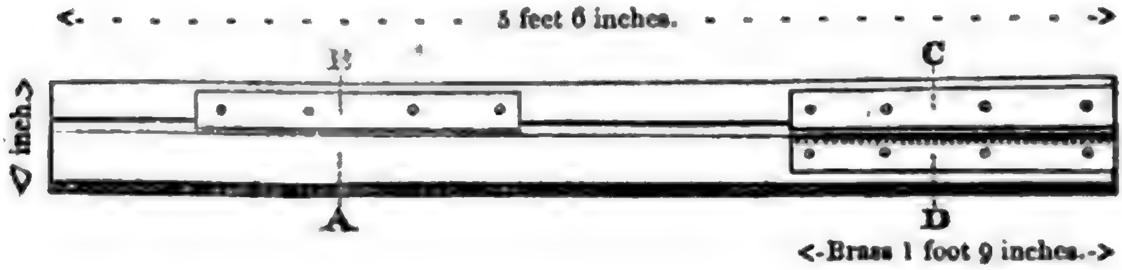
[*] Communicated by the Chemical Society; having been described by Dr. Lyon Playfair January 19, 1846.

with 4 screw-nuts, passing through a cut portion of the plate, a quarter of an inch long, so as to allow a certain advancement or withdrawal of the plate at pleasure. C and D are two similar plates, placed at the other end of the wooden board, C having the same amount of motion as B, and being precisely similar in every respect. D is a brass plate of the same dimensions as B and C, but the screws go through a hole of the same size as themselves into the wood. It is cut, at intervals of five millimetres, into notches, every alternate one being one-twentieth and one-tenth of an inch deep. The instrument is provided with a wooden rod, 3 feet long, 1 inch broad, and half an inch thick, E. This is provided with two steel points, placed by screws at half an inch from either end. One of these, F, is in the form of a knife, the other, G, of a bradawl. The instrument is furnished with a screw-driver, that these may be removed at pleasure.

Scale six-tenths of an inch to the foot.



Half natural size.

Half natural size.

When a tube is to be graduated, it is covered with a thin layer of melted wax and turpentine, by means of a camel's-hair pencil, and is placed in the groove between C and D, which are then screwed down in their places, so as to retain the tube firmly in its position. A standard tube, previously mathematically divided into millimetres (the most convenient division), is now placed in the groove under B, which is then screwed upon it. The rod E is now used, the pointed steel G being put in one of the millimetre marks on the standard tube; the knife-formed steel F is now upon the waxed tube, and is made to make a mark upon it, the length of which is regulated by the distance between the edges of C and D. The pointed steel is now removed back one millimetre on the standard tube, and the corresponding mark made on the waxed one; and thus we proceed until the whole of the waxed tube is divided into millimetres. The object of the notches is, that a longer mark may be made at every five millimetres, and a still longer one at every ten, in order to aid the eye in reading. The waxed tube is now removed to a leaden chest, containing pounded fluor spar and sulphuric acid, slightly heated, which etches it more successfully than a solution of hydrofluoric acid. Previously, however, to being etched, it is desirable to figure the number of millimetres at the space of every ten; and this is conveniently done by the steel pointer G after being removed from E.

We have thus an accurate measure of length etched upon the tube, which should have been one of pretty uniform calibre. The next point is to determine the true value of each of the divisional marks. This is done by calibrating it throughout all its length with small portions of mercury, say equal in bulk to five grains of water. By this means the relative value of each mark may be determined, and the proportion which it bears to any given standard. The only possible error is in the assumption that the tube is of even calibre between the space occupied by the mercury; but the quantity of this added is so small that any such error becomes quite inappreciable.

The convenience of this graduator is so great, that a long tube may be beautifully divided in the course of a quarter or half an hour. The standard tubes should be made of glass, but the original divisions from which this standard is made may be made on wood or any other material.

XIX. *On the Gallo-Nitrate of Silver of* Mr. Fox Talbot, *and its Action upon Iodized Paper.* *By* Geo. S. Cundell*.

SOME of the properties of this remarkable compound have been described by its inventor, and are sufficiently well known; the relations however of its constituent parts, and the means by which the action of the compound may be regulated (some knowledge of which is indispensable to the using it with success), are less generally understood, and have not, so far as I am aware, yet been described.

When solution of the nitrate of silver is applied to iodized paper, a surface is obtained which is extremely sensible to the impressions of light: and, if gallic acid have been added to the solution, the sensibility of the surface will be increased in a remarkable manner.

Preparations however of this kind are subject to a spontaneous and rapid change, independently of light; and, where

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* Communicated by the Author.

such have been employed, after a certain time (longer or shorter, according to the strength of the solutions), on developing an impression in the usual manner, it will be found that the *whole* of the surface, whether acted upon by light or not, will become dark and discoloured; and such preparations seem therefore to be of little or no value in photography.

By adding to them, however, a little acetic acid, the tendency to spontaneous decomposition is immediately lessened; and by combining the three constituents in due proportion, a "gallo-nitrate" may be formed of any degree of stability (or, inversely, of any degree of sensibility) required.

There are, no doubt, many proportions in which the ingredients may be united with good effect; but, for general use, no proportions perhaps are better than the original ones given by the inventor. It is to be observed however of his formula, that it is adapted to certain external conditions, and is not equally suitable where these are different. It is not equally suited to every climate or temperature of the air; and although well-adapted to excite paper for immediate use, it is perhaps less so when the paper is intended to be kept: hence the cause of many miscarriages in its use, and the necessity for something more than a mere formula for its preparation.

If the acetic acid be deficient (which is frequently the case from its uncertain strength), the spontaneous decomposition already noticed will take place with more or less rapidity; and, unless the paper be used without delay, the impression will be lost in the general darkening of the surface.

On the other hand, if the acetic acid be in excess, there will be less tendency to decomposition; the paper will also be less sensitive, but it will much better preserve its whiteness in the excited state, and may be used successfully after many hours.

By thus *tempering* the compound with acetic acid, according to circumstances, it will be found that its action may in a great degree be brought under control.

If the gallo-nitrate of silver of Mr. Talbot be applied to iodized paper, an action commences, which, unless very quickly arrested, instead of merely exciting the surface and preparing it to receive an impression, will speedily darken and render it useless. It is most probably on that account that the paper is directed to be "dipped into water;" the effect of which is *to arrest* the action by diluting the gallo-nitrate, and by removing, perhaps, nine-tenths of it from the paper. The extent however to which the destructive action may have gone, and to which the gallo-nitrate may *thus* have been diluted or removed, is all to the last degree uncertain; and it seems a more satisfactory proceeding to apply the gallo-nitrate *in the* *diluted state*, and to dispense with the dipping in water altogether.

The diluting is a very much simpler and more œconomical method, and it is evidently a more certain one.

If the gallo-nitrate be diluted not more than ten times, it imparts a high degree of sensibility, but in that case the paper must be used immediately. It may be diluted as much as forty times, and in favourable circumstances will still give an intense picture. In diluting to that extent the paper becomes less sensitive, but it is less subject to the spontaneous change, and preserves its useful properties much longer.

To develope an impression, the gallo-nitrate ought to be of the full strength, particularly in gallic acid, upon which the depth and intensity of the picture chiefly depend, and which in a saturated solution will be present in greater or less quantity, according to the temperature of the air, by which the general chemical action is also affected.

London, July 7, 1846.

XX. *Microscopic Observations on Hail.*

By A. Waller, *M.D.*[*]

IN a paper read before the Royal Society, June 18, 1846, I have presented some observations on the microscopic examination of globules of water, as they exist in fumes of steam, fogs, clouds, and mists. I found that they consist of particles of water perfectly spherical, without the least appearance of a vesicular structure, as is generally taught. I now proceed to examine, in a similar manner, water in some of the other forms in which it presents itself in meteorology, and commence with hail.

Some confusion seems to exist between different authors respecting the distinction between hail and sleet, which it is very desirable to avoid. Kaemtz, in his Manual of Meteorology, states that "very small hailstones are termed sleet." Pouillet, *Elémens de Physique*, who is generally very precise in his terms, gives the following definition: "Le gresil [sleet], which we have the opportunity of observing in our climates almost every year during the months of March and April, has doubtless an origin similar to that of snow. It is also congealed water, or rather small needles of ice pressed and interlaced together, forming a kind of ball of rather a compact nature, and sometimes surrounded with a real coating of transparent ice." In one dictionary I find sleet termed "a fall of rain and snow together," and in another, "a kind of

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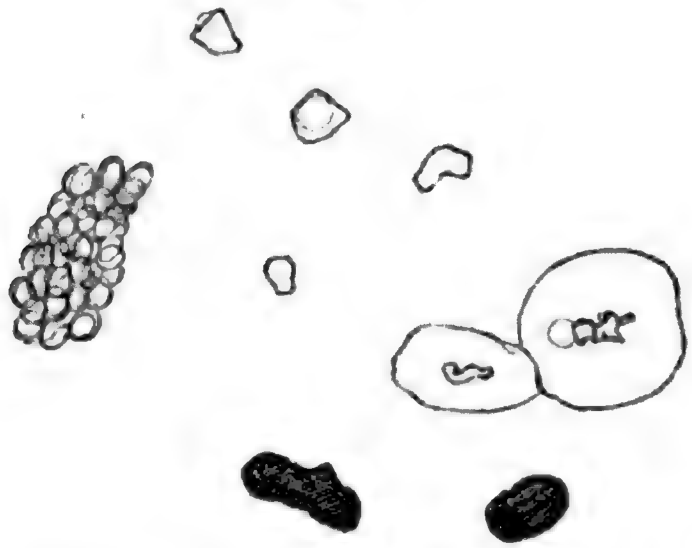
[*] Communicated by the Author.

smooth small hail or snow not falling in flakes, but single particles." The cause of this confusion arises from our inability to give any character visible to the naked eye, applicable on all occasions, for distinguishing hailstones, sleet, and globular masses of snow from each other. In ordinary cases, particularly in the summer months, no difficulty is found in the use of the term hailstones. We might then define them as consisting generally of fragments of ice, hard, nearly opake, and producing various physical effects in proportion to their velocity; but a more minute examination soon shows, that they are generally accompanied with some sufficiently small to deserve the name of sleet according to Kaemtz, and others with no more consistency than flakes of moistened snow; and if we were to accept the definition of sleet by M. Pouillet, in this country, where hail is generally much smaller than in warmer climates, hail storms would be of comparatively very rare occurrence. In showers of sleet and snow the same difficulties are sometimes experienced; if particles of sleet invariably presented themselves under the form of transparent globules or particles of ice, no possibility of confusion would exist between them and the other two, but we also find them sometimes mingled with some that are more or less opake and possessing a greater size. This uncertainty respecting the precise meaning attached to the words hail and sleet, is found in the accounts of many meteorological phænomena; and it would be desirable, in order to avoid it, to define the exact sense in which they are employed, or what would be still preferable, to describe with more precision the principal physical characters presented by the particles themselves. I fear it will be impossible at present to propose any set of terms which in every case would be free from objection, any more than with regard to the different forms of clouds or other meteorological phænomena, which pass with gradual transition from one form to another; but I would propose to confine the term sleet to all particles of water which are transparent throughout, whatever may be their size, and that of hail to those which are merely semi-transparent throughout, or possess at any part the power of irregularly dispersing light, which, as we shall see hereafter, arises from their being composed of minute globules. The term snow will be applied to all those particles of water in the atmosphere in which we can detect a crystalline appearance by aid of the microscope. With regard to hail, which possesses no cohesion and loses its form when it comes in contact with any solid body, I term it amorphous hail, from its having no definite form; and the snow, whose crystalline forms have been altered by moisture or by caloric, I call on the same principle amorphous snow.

I will now proceed to mention the results of a few observations I have made upon hail.

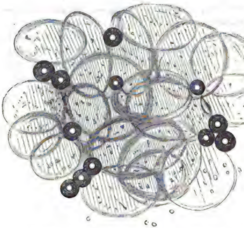
March 22, 1846.—There was a fall of hail which lasted several minutes. The hailstones were generally of a lenticular shape, about half the size of a French bean, of a dull white colour, and semi-transparent. As they fell they were received in a blanket on this and on the following occasions, as when folded up in it, they might be preserved much longer from melting. One of them placed under the microscope was found to disengage bubbles of air in great abundance; its surface presented several irregular fissures by reflexion. After liquefaction, the drop of water resulting was found to contain numerous particles of various forms. Some of these were perfectly spherical, transparent, and agglomerated together in groups. Some were single, with a central nucleus, others were completely opake, black, and globular. Other hailstones contained, besides particles similar to the above-described, some of a siliceous appearance, causing a gritty feel when compressed between surfaces of glass. Shortly after the hail there followed a fall of rain: some drops of it being examined in the same way, were found to contain numerous irregular particles, some of which were apparently particles of soot, but I could detect no globular particles, nor any resembling sand.

Fig. 1.



March 23.—There occurred a fall of hail more considerable than the former, but the hailstones were smaller, though of similar shape. At the commencement of the fall, the hailstones, as soon as they struck the earth, split into numerous fragments, which remained loosely adhering together, much like wet sand or the ices of pastry-cooks. The others which fell after did not break up in this way. Several of them, examined as before, disengaged, as they melted, bubbles of air in abundance. When the stone was reduced to a thin disc of ice, it became sufficiently transparent to be viewed by transmitted light. It was then perceived to consist of numerous particles of either a globular or ovular shape, closely adhering together, and presenting, on what appeared their surface, numerous very minute black points. The drop of water which resulted contained very slight traces of foreign matter of the most minute dimensions. In a few instances I perceived a collection of spherical globules of a green colour about $0^{\mathrm{mm}}{\cdot}005$. One or two deposited a particle apparently siliceous, which appeared to be in the centre of the hailstone.

Fig. 2.



April 3, 2 P.M.—There fell some hail for about five minutes. The weather warm, sun strong, occasional showers during the day. About half an hour previously I observed brilliant, thin, white cirro-strati at various points of the sky: before the hail fell, there appeared a second cloud over head, apparently much lower than the former, very dense and extending rapidly. The first hailstones so completely lost their cohesion and liquefied so quickly, even as they fell on my coat, that I at first took them for drops of rain, with which they were accompanied. Those which I collected resembled a grain of rice in size and colour. The dark cloud quickly disappeared after the fall of hail; the upper ones still remained about the same. I had not an opportunity of using the microscope.

April 6, 5 P.M.—A heavy fall of rain with hailstones about the size of a sweet pea. Under the microscope, the hailstones, when sufficiently thin, appeared to consist of spherical globules of various sizes, as in fig. 1, mixed with others of an angular outline. Air-bubbles as usual.

The foreign matter was found to consist of particles like sand, and others resembling microscopic lichens.

May 10, 3 P.M.—Wind N.W. an hour or two previous, bringing numerous masses of clouds, detached portions of which rapidly disappeared under the action of the sun's rays. Temperature warm; shortly before the fall of hail, the wind blew violently in the most opposite directions. A few hailstones fell of nearly a globular form. Under the microscope they gave off air-bubbles as usual, and all contained minute amorphous particles, some of an organic appearance, as in fig. 2. The thin disc of ice, besides the globular particles, presented many minute circular spots of different sizes, which as the ice melted, appeared to form the air-bubbles. These presented a different appearance from the globular particles before mentioned; they were much less defined and more irregular in their size. In some instances I could detect what appeared to be globular particles imbedded in a matrix of ice.

Air contained within Hailstones.

It is remarkable that, in all the accounts of hailstones which I have met with, I have not found any mention of the quantity of air which is contained in their interior. Under the microscope, the rapid formation of air-bubbles is the first point which attracts the attention, giving rise to the idea that a chemical action disengaging a gaseous body must be taking place. As I have invariably observed this in all the numerous hailstones which I have examined, I regard it as intimately connected with all those which, like those I have met with, are composed of separate globules, and have a semi-transparent aspect. Although the greater part of this air is disengaged during the process of liquefaction, some part of it remains in the state of small air-bubbles, on the surface of the drop of water resulting from the fusion of the ice, and may be perfectly distinguished by the unassisted eye. Hitherto I have not been able to determine the amount of air which a hailstone is capable of containing, but as far as I have been able to judge, it is more than the volume of the hailstone itself. We must furthermore bear in mind, that as the density of the atmosphere is less as we ascend, the original volume of the air, supposing it to have been confined at the moment the hail was formed, must have been much more considerable than at the time we examine it. Hail, as we know, is abundantly formed at the elevation of Mont Blanc, and probably much higher. Supposing the air in our observations had been confined at that height, its volume would have been about double that which it presented at the time it was liberated, supposing the temperature on both occasions to be the same. The influence of temperature would no doubt operate in a different direction, as hailstones must be considered to take their origin at a temperature below zero; and when examined, even after their fall, they have been found generally, by M. Pouillet, −3° or − 4° below 0· Cent., whereas when the air escapes, they may be considered exactly at 0· Cent. But admitting the temperature to have been −10° Cent. when they were formed, the dilatation of the air would be about $\frac{1}{26}$th of the volume, which is far from compensating for the decreased pressure. By subjecting hailstones while kept several degrees below the freezing-point to a decreased pressure or a vacuum in the air-pump, we might decide whether the air has any communication with the external atmosphere, and with what force it is retained.

In observation May 10, the air appeared to be fixed within the hailstone, under the form of globules, very distinct in their appearance from the solid spherules. Their diameters varied from the one-fifth of a millimetre to sizes considerably smaller. As they disappeared they seemed to form the air-bubbles, which are invariably to be seen escaping. In the other cases I could only detect, under the highest power, minute dark points at most of the size of $0^{\mathrm{mm}}\cdot005$, which appeared to be on the surface of the spherules. I can only conjecture them to consist of air, because after fusion I could perceive nothing like them in the drop of water. If it should result from future observation with the air-pump, as I have suggested, that the particles of ice merely retain air from the same cause as other porous bodies, the transparent condition of ice may enable us to form some idea of the manner in which porous bodies, such as charcoal, act in fixing gaseous substances within them; and whether the gases are reduced to a state of liquid by their molecular attraction with the solid.

Solid Globular Particles.

In the observations given above, it is found that, with the exception of the first, all the hailstones have been seen under the microscope to be composed entirely of minute spherules of ice agglutinated together; and even the discrepancy in that instance may be accounted for, from my attention having been principally engaged with the numerous air-bubbles that were escaping. At the time I commenced these observations I was not aware that this structure of the hailstone had ever been ascertained before, but I now find that it has been already stated by several authors. Descartes, who admitted the existence of clouds composed of solid globules, explained the formation of hailstones by some of these globules being detached, and I believe agglomerated together. Berzelius, who gives in his Treatise on Chemistry a succinct and excellent account of hail, attributes it to the "agglomération de plusieurs grains," "arrondis et non en cristaux comme la neige." In the *Encyclopædia Britannica*, art. Hail, we find also that hail is formed "by many little spherules of ice agglutinated together." It appears surprising, that with such correct descriptions of hail there should still exist in works of a more ambitious character, others so much more imperfect and erroneous. In fig. 1, I have traced the forms of these globules: some of them appear more ovular than globular, and I believe that such is the real shape of some; although at the same time we must bear in mind, that a transparent globule of the high refractive powers of water will appear much distorted if part is seen through others before it, while the rest of it is perfectly exposed. The average size of those I have represented is about one-third of a millimetre; they do not vary greatly from this standard, and I have found them generally the same on other occasions.

It is evident that these globules must have existed in a solid state previous to their agglomeration to form hail, and therefore their maximum temperature at that time must have been below the freezing-point.

In what manner are we to account for the formation of these globules previous to their agglomeration?

In the experiments on congelation under the microscope, the account of which has been given in the paper before referred to, we have already seen with what facility these globular forms of ice are produced by the condensation of the moisture from the atmosphere on a surface of glass or Canada balsam. A curious and unforeseen analogy was found between their appearance in my experiments and their formation in nature. In winter a refrigerating mixture causes a deposit from the air, in which crystalline forms, frequently of a very regular appearance, were generally perceived. Only at a few points were discovered globular forms of ice, and these were generally surrounded on their surface with minute pyramidal crystals, giving them a mace-like appearance.

In summer the deposit was more confusedly crystalline, liable to liquefy from various external causes, and then to return again to a solid condition. The globular forms of solid particles were in consequence very numerous. The cause of this difference was, that in one case the quantity of moisture in the air was small, the deposition gradual, and the transitions of temperature from one extreme to the other very small; in the other, the moisture was abundant, the temperature of the air greatly lowered, and the crystalline action rapid. We find the same difference between the crystals obtained from a concentrated solution in a liquid exposed to a rapid decrease of temperature, and those which are formed in a solution less concentrated and progressing more gently. A still greater analogy is found in the crystals of naphthaline, iodine, camphor, and other substances volatile at the common temperature, when allowed to crystallize in empty bottles without the application of artificial heat. When either of these bodies is driven off in a state of vapour, the fumes that are formed when condensed on glass, present the form of either minute globules or of crystals, according to the temperature in which they are exposed. When the fumes are given off from either of these substances in a state of ebullition, it is found that the globules are of much larger dimensions than any which are formed at a temperature below the boiling-point. On a former occasion I have fully demonstrated that such is likewise the case with regard to the fumes of water. I will not attempt at present to offer any explanation of this fact, which is so invariably the case, that at any time, by the microscopic inspection of the globules, it is easy to decide whether they are formed from boiling water or at a temperature below.

The above observations confirm what we might have already anticipated, that the production of solid globular particles arises from the same cause as that which produces confused crystallization; and that in fact it is simply confused crystallization of separate particles. The most efficient cause of this is rapid congelation, which apparently does not allow the constituent particles sufficient time to group themselves in geometrical forms, but obliges them to assume that which their molecular attraction tends to give them. Between the two extremes, of a globular form and a perfect hexagonal prism, frequent shapes which the crystals assume in frost, we have various intermediate forms which may be considered as the resultants of the crystalline and molecular powers. Although by our experiments we may observe the circumstances in which these globules are formed, it would be impossible to ascertain by their means, with any precision, the condition of temperature requisite in one instance to form small crystals, and in the other minute globules. But in nature we not unfrequently meet with experiments performed for us, which if examined with care would, I believe, furnish us with the desired information.

In the winter months, during a hoar frost, we sometimes find, adhering to minute filaments on the hedges, such as those of the spider's web, very minute globules of transparent ice, such as exist in hail and sleet. On other occasions, instead of globular forms, we find others of crystalline shapes. Careful observations made as they are in process of formation, with the instruments we possess for examining the state of temperature and the movements of the atmosphere, would no doubt give us the formula of these two deposits. The same observations would likewise enable us to determine, whether the temperature of the air around is such that any existing mist or fog must contain its globules in a solid state, according to the hypothesis advanced by Descartes. I am inclined to believe that such is not unfrequently the case.

It is evident that these globules, with the exception of their being formed on the surface of bodies which are stationary, are in all respects exactly similar to the particles of sleet. Frost, which forms in the same way, presents under the microscope crystalline forms, which are so much like those of snow that I have scarcely been able to distinguish one from the other. But if such deposits formed on the surface of the earth present analogies so great to those formed in the atmosphere, that they almost deserve the names of "ground sleet" and "ground snow," it naturally suggests that something like hail may also occasionally be produced in the same way, or in other words, the globules of ice may be found in a state of agglomeration. I have seen somewhere a definition of hoar frost, which describes it as a collection of spherules of ice; and according to the view I take, I believe that such is sometimes the case, although the observations I have hitherto made, I must confess, have not yet enabled me to detect this structure in hoar frost*.

Kensington, June 1846.

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* Since making the foregoing observations, I have had an opportunity of witnessing another storm, which occurred on June 23, 1846, when a violent shower of rain fell, and having continued for about a quarter of an hour, it became mixed with hail, which had been preceded for a few minutes by thunder. The largest hailstones attained the size of a French bean, and were accompanied by others of all sizes down to that of a pea. Their shape was generally biconical; colour, a dull, without any external envelope of transparent ice, resembling in all respects those I had previously examined on other occasions, with the exception of one, *which was*

XXI. *On the Theory of Parallel Lines.*
By Mr. Henry Meikle.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOUR correspondent X. Y., who, in the Philosophical Magazine for September 1844, has been pleased to take notice of my paper on parallel lines, inserted in the Edinburgh Philosophical Journal for April 1844, alleges that the reasoning depends on the assumption, that no triangle can have the sum of its angles inappreciably small. Now if this is really supposed to need any proof, the following I presume will be quite satisfactory.

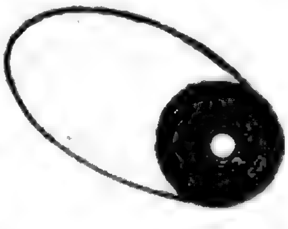
If the sum of the angles of a triangle could ever be so small that it could not be multiplied to exceed any given angle, then an infinite number of copies of that triangle could be placed around a point, and yet these would all lie within a single circle whose radius is their longest side. Consequently, whilst owing to the minuteness of the sum of the angles, the area of each included triangle would be a maximum, or the greatest which any triangle could have*, the sum of an infinite number of such areas would be infinite; and yet the whole of them would not nearly cover the finite area of the circle, which is absurd.

Your very obedient Servant,
Henry Meikle.

Maitland Street, Edinburgh,
June 20, 1846.

completely transparent throughout one half and the other portion of an opake white. Under the microscope, however, I found that they presented some difference, appearing to consist almost entirely of irregular fragments of ice, which presented numerous dark points, such as are seen in fig. 1. I clearly detected numerous bubbles of air in the stone previous to its fusion, some perfectly spherical, others elongated like particles of mercury on a plain surface. The spherules of ice so constantly met with in the other observations were very scarce, and each contained a globule of air placed near its circumference, as in fig. 3. The hailstone, which was partly transparent, was found on the transparent side to be free from globules of air or spherules of ice, with all the characters of a transparent particle of ice; the opake part presented the same appearances as the other hailstones, of which about seven or eight were examined.

Fig. 3.



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* This, in the circumstances, is an obvious inference from my second proposition, which X. Y. admits to be satisfactorily demonstrated, namely, that if in one triangle the sum of the angles differed from two right angles, so it would in every triangle; the difference would always have the same sine, and (as in spherics) be proportional to the area of the triangle.

XXII. *On Quaternions; or on a New System of Imaginaries in Algebra.* *By* Professor Sir WILLIAM ROWAN HAMILTON, *LL.D., Corresponding Member of the Institute of France, and Royal Astronomer of Ireland.*

[Continued from p. 31.]

22. THE *geometrical considerations* of the foregoing article may often suggest *algebraical transformations* of functions of the new imaginaries which enter into the present theory. Thus, if we meet the function

$$\alpha\,\mathrm{S}.\alpha'\alpha'' - \alpha'\,\mathrm{S}.\alpha''\alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

we may see, in the first place, that in the recent notation this function is algebraically a pure imaginary, or *vector form*, which may be constructed geometrically in this theory by a straight line having length and direction in space; because the three symbols α, α', α'' are supposed to be themselves such vector forms, or to admit of being constructed by three such lines; while $\mathrm{S}.\alpha'\alpha''$ and $\mathrm{S}.\alpha''\alpha$ are, in the same notation, two *scalar forms*, and denote some two real numbers, positive, negative, or zero. We may therefore equate the proposed function (1.) to a new small Greek letter, accented or unaccented, for example to α''', writing

$$\alpha''' = \alpha\,\mathrm{S}.\alpha'\alpha'' - \alpha'\,\mathrm{S}.\alpha''\alpha. \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Multiplying this equation by α'', and taking the scalar parts of the two members of the product, that is, operating on it by the characteristic $\mathrm{S}.\alpha''$; and observing that, by the properties of scalars,

$$\mathrm{S}.\alpha''\alpha\,\mathrm{S}.\alpha'\alpha'' = \mathrm{S}.\alpha''\alpha\,.\,\mathrm{S}.\alpha'\alpha''$$
$$= \mathrm{S}.\alpha''\alpha'\,.\,\mathrm{S}.\alpha''\alpha = \mathrm{S}.\alpha''\alpha'\,\mathrm{S}.\alpha''\alpha,$$

in which the notation $\mathrm{S}.\alpha''\alpha\,\mathrm{S}.\alpha'\alpha''$ is an abridgement for $\mathrm{S}(\alpha''\alpha\,\mathrm{S}.\alpha'\alpha'')$, and the notation $\mathrm{S}.\alpha''\alpha\,.\,\mathrm{S}.\alpha'\alpha''$ is abridged from $(\mathrm{S}.\alpha''\alpha)\,.\,(\mathrm{S}.\alpha'\alpha'')$, while $\mathrm{S}.\alpha'\alpha''$ is a symbol equivalent to $\mathrm{S}(\alpha'\alpha'')$, and also, by article 20, to $\mathrm{S}(\alpha''\alpha')$, or to $\mathrm{S}.\alpha''\alpha'$, although $\alpha'\alpha''$ and $\alpha''\alpha'$ are not themselves equivalent symbols; we are conducted to the equation

$$\mathrm{S}.\alpha''\alpha''' = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

which shows, by comparison with the general *equation of perpendicularity* assigned in the last article, that *the new vector α''' is perpendicular to the given vector α''*, or that these two vector forms represent two rectangular straight lines in space. Again, because the squares of vectors are scalars (being real, though negative numbers), we have

$$\alpha(\alpha\,\mathrm{S}.\alpha'\alpha'')\,.\,\alpha' = \alpha^2\alpha'\,\mathrm{S}.\alpha'\alpha'' = \alpha'(\alpha\,\mathrm{S}.\alpha'\alpha'')\,.\,\alpha,$$
$$\alpha'(\alpha'\,\mathrm{S}.\alpha''\alpha)\,.\,\alpha = \alpha'^2\alpha\,\mathrm{S}.\alpha''\alpha = \alpha(\alpha'\,\mathrm{S}.\alpha''\alpha)\,.\,\alpha';$$

therefore the equation (2.) gives also

$$\alpha\alpha'''\alpha' = \alpha'\alpha'''\alpha; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

a result which, when compared with the general *equation of coplanarity* assigned in the same preceding article, shows that *the new vector* α''' *is coplanar with the two other given vectors,* α and α'; it is therefore perpendicular to the vector of their product, $\mathrm{V}.\alpha\alpha'$, which is perpendicular to both those given vectors. We have therefore two known vectors, namely $\mathrm{V}.\alpha\alpha'$ and α'', to both of which the sought vector α''' is perpendicular; it is therefore parallel to, or coaxal with, the vector of the product of the two known vectors last mentioned, or is equal to this vector of their product, multiplied by some scalar coefficient x; so that we may write the transformed expression,

$$\alpha''' = x\,\mathrm{V}(\mathrm{V}.\alpha\alpha'.\alpha''). \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

And because the function α''' is, by the equation (2.), homogeneous of the dimension unity with respect to each separately of the three vectors α, α', α'', while the function $\mathrm{V}(\mathrm{V}.\alpha\alpha'.\alpha'')$ is likewise homogeneous of the same dimension with respect to each of those three vectors, we see that the scalar coefficient x must be either an entirely constant number, or else a homogeneous function of the dimension zero, with respect to each of the same three vectors; we may therefore assign to these vectors any arbitrary lengths which may most facilitate the determination of this scalar coefficient x. Again, the two expressions (2.) and (5.) both vanish if α'' be perpendicular to the plane of α and α'; in order therefore to determine x, we are permitted to suppose that α, α', α'' are three coplanar vectors: and, by what was just now remarked, we may suppose their lengths to be each equal to the assumed unit of length. In this manner we are led to seek the value of x in the equation

$$x\,\mathrm{V}.\alpha\alpha'.\alpha'' = \alpha\,\mathrm{S}.\alpha'\alpha'' - \alpha'\,\mathrm{S}.\alpha''\alpha, \quad . \quad . \quad (6.)$$

under the conditions

$$\mathrm{S}.\alpha\alpha'\alpha'' = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

and

$$\alpha^2 = \alpha'^2 = \alpha''^2 = -1; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

so that α, α', α'' are here *three coplanar and imaginary units.* Multiplying each member of the equation (6.), as a multiplier, *into* $-\alpha''$ as a multiplicand, and taking the vector parts of the two products; observing also that

$$\mathrm{V}.\alpha'\alpha'' = -\mathrm{V}.\alpha''\alpha', \text{ and } -\mathrm{V}.\alpha\alpha'' = \mathrm{V}.\alpha''\alpha;$$

we obtain this other equation,

$$x\,\mathrm{V}.\alpha\alpha' = \mathrm{V}.\alpha''\alpha.\mathrm{S}.\alpha'\alpha'' - \mathrm{V}.\alpha''\alpha'.\mathrm{S}.\alpha''\alpha: \quad . \quad . \quad (9.)$$

in which the three vectors $\mathrm{V}.\alpha''\alpha$, $\mathrm{V}.\alpha''\alpha'$, $\mathrm{V}.\alpha\alpha'$ are coaxal, being each perpendicular to the common plane of the three vectors $\alpha, \alpha', \alpha''$; they bear therefore scalar ratios to each other, and are proportional (by the last article) to the areas of the parallelograms under the three pairs of unit-vectors, α'' and α, α'' and α', and α and α', respectively; that is, to the sines of the angles a, a', and $a' - a$, if a be the rotation from α'' to α, and a' the rotation from α'' to α', in the common plane of these three vectors. At the same time we have (by the principles of the same article) the expressions:

$$-\mathrm{S}.\alpha''\alpha = \cos a; \quad \mathrm{S}.\alpha''\alpha' = -\cos a';$$

so that the equation (9.) reduces itself to the following very simple form,

$$x \sin(a' - a) = \sin a' \cos a - \sin a \cos a', \quad . \quad . \quad (10.)$$

and gives immediately

$$x = 1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

Such then is the value of the coefficient x in the transformed expression (5.); and by comparing this expression with the proposed form (1.), we find that we may write, for *any three vectors*, $\alpha, \alpha', \alpha''$, not necessarily subject to any conditions such as those of being equal in length and coplanar in direction (since those conditions were not used in *discovering the form* (5.), but only in *determining the value* (11.),) the following *general transformation*:

$$\alpha\,\mathrm{S}.\alpha'\alpha'' - \alpha'\,\mathrm{S}.\alpha''\alpha = \mathrm{V}(\mathrm{V}.\alpha\alpha'.\alpha''); \quad . \quad . \quad . \quad (12.)$$

which will be found to have extensive applications.

23. But although it is possible thus to employ geometrical considerations, in order to *suggest* and even to *demonstrate* the validity of many general transformations, yet it is always desirable to know how to obtain the same *symbolic results*, from the *laws of combination of the symbols*: nor ought the *calculus of quaternions* to be regarded as complete, till all such *equivalences of form* can be deduced from such symbolic laws, by the fewest and simplest principles. In the example of the foregoing article, the symbolic transformation may be effected in the following way.

When a scalar form is multiplied by a vector form, or a vector by a scalar, the product is a vector form; and the sum or difference of two such vector forms is itself a vector form; therefore the expression (1.) of the last article is a vector form, and may be equated as such to a small Greek letter; or in other words, the equation (2.) is allowed. But every vector form is equal to its own vector part, or undergoes no change of signification when it is operated on by the characteristic V;

we have therefore this other expression, after interchanging, as is allowed, the places of the two vector factors $\alpha' \alpha''$ of a binary product under the characteristic S,

$$\alpha''' = \mathrm{V}\,(\alpha\,\mathrm{S}.\,\alpha''\alpha' - \alpha'\,\mathrm{S}.\,\alpha''\alpha). \quad \ldots \quad (1.)'$$

Substituting here for the characteristic S, that which is, by article 18, symbolically equivalent thereto, namely the characteristic $1 - \mathrm{V}$, and observing that

$$0 = \mathrm{V}\,(\alpha\,\alpha''\alpha' - \alpha'\alpha''\alpha), \quad \ldots \ldots \quad (2.)'$$

because, by article 20, $\alpha\,\alpha''\alpha' - \alpha'\alpha''\alpha$ is a scalar form, we obtain this other expression,

$$\alpha''' = \mathrm{V}\,(\alpha'\,\mathrm{V}.\,\alpha''\alpha - \alpha\,\mathrm{V}.\,\alpha''\alpha'). \quad \ldots \quad (3.)'$$

The expression (1.)$'$ may be written under the form

$$\alpha''' = \mathrm{V}\,(\alpha\,\mathrm{S}.\,\alpha'\alpha'' - \alpha'\,\mathrm{S}.\,\alpha\,\alpha''); \quad \ldots \quad (4.)'$$

and (3.)$'$ under the form

$$\alpha''' = \mathrm{V}\,(\alpha\,\mathrm{V}.\,\alpha'\alpha'' - \alpha'\,\mathrm{V}.\,\alpha\,\alpha''), \quad \ldots \quad (5.)'$$

obtained by interchanging the places of two vector-factors in each of two binary products under the sign V, and by then changing the signs of those two products; taking then the semisum of these two forms (4.)$'$, (5.)$'$, and using the symbolic relation of article 18, $\mathrm{S} + \mathrm{V} = 1$, we find

$$\alpha''' = \frac{1}{2}\,\mathrm{V}\,(\alpha\,\alpha'\alpha'' - \alpha'\alpha\,\alpha'')$$

$$= \mathrm{V}\left(\frac{1}{2}\,(\alpha\,\alpha' - \alpha'\alpha).\,\alpha''\right); \quad \ldots \quad (6.)'$$

in which, by article 20, $\frac{1}{2}(\alpha\,\alpha' - \alpha'\alpha) = \mathrm{V}.\,\alpha\,\alpha'$; we have therefore finally

$$\alpha''' = \mathrm{V}\,(\mathrm{V}.\,\alpha\,\alpha'.\,\alpha''): \quad \ldots \ldots \quad (7.)'$$

that is, we are conducted by this purely symbolical process, from laws of combination previously established, to the transformed expression (12.) of the last article.

24. A relation of the form (4.), art. 22, that is an *equation between the two ternary products of three vectors taken in two different and opposite orders*, or an evanescence of the scalar part of such a ternary product, may (and in fact does) present itself in several researches; and although we know, by art. 21, the *geometrical interpretation* of such a symbolic relation between three vector forms, namely that it is the condition of their representing *three coplanar lines*, which interpretation may suggest a transformation of one of them, as a *linear function with scalar coefficients*, of the two other vectors, because any one straight line in any given plane may be treated as the

diagonal of a parallelogram of which two adjacent sides have any two given directions in the same given plane; yet it is desirable, for the reason mentioned at the beginning of the last article, to know how to obtain the same general transformation of the same symbolic relation, without having recourse to geometrical considerations.

Suppose then that any research has conducted to the relation,

$$\alpha\,\alpha'\,\alpha'' - \alpha''\,\alpha'\,\alpha = 0, \quad \ldots\ldots \quad (1.)$$

which is not in this theory an identity, and which it is required to transform. [We propose for convenience to commence from time to time a new numbering of the *formulæ*, but shall take care to avoid all danger of confusion of reference, by naming, where it may be necessary, the *article* to which a formula belongs; and when no such reference to an article is made, the formula is to be understood to belong to the *current series* of formulæ, connected with the existing investigation.] By article 20, we may write the recent relation (1.) under the form,

$$\mathrm{S}.\,\alpha\,\alpha'\,\alpha'' = 0; \quad \ldots\ldots\ldots \quad (2.)$$

and because generally, for any three vectors, we have the formula (12.) of art. 22, if we make, in that formula, $\alpha'' = \mathrm{V}.\,\beta\beta'$, and observe that $\mathrm{S}(\mathrm{V}.\beta\beta'.\alpha) = \mathrm{S}(\alpha\,\mathrm{V}.\beta\beta') = \mathrm{S}.\alpha\beta\beta'$, we find, for *any four vectors* $\alpha\alpha'\,\beta\beta'$, the equation:

$$\mathrm{V}(\mathrm{V}.\alpha\alpha'.\mathrm{V}.\beta\beta') = \alpha\,\mathrm{S}.\alpha'\beta\beta' - \alpha'\,\mathrm{S}.\alpha\beta\beta'; \quad \ldots \quad (3.)$$

making then, in this last equation, $\beta = \alpha'$, $\beta' = \alpha''$, we find, for *any three vectors*, $\alpha\,\alpha'\,\alpha''$, the formula:

$$\mathrm{V}(\mathrm{V}.\alpha\alpha'.\mathrm{V}.\alpha'\alpha'') = -\alpha'\,\mathrm{S}.\alpha\alpha'\alpha''. \quad \ldots\ldots \quad (4.)$$

If then the scalar of the product $\alpha\,\alpha'\,\alpha''$ be equal to zero, that is, if the condition (2.) or (1.) of the present article be satisfied, the product of the two vectors $\mathrm{V}.\alpha\alpha'$ and $\mathrm{V}.\alpha'\alpha''$ is a scalar, and therefore the latter of these two vectors, or the opposite vector $\mathrm{V}.\alpha''\alpha'$, is in general equal to the former vector $\mathrm{V}.\,\alpha\,\alpha'$, multiplied by some scalar coefficient b; we may therefore write, under this condition (1.), the equation

$$\mathrm{V}.\,\alpha''\,\alpha' = b\,\mathrm{V}.\,\alpha\,\alpha', \quad \ldots\ldots\ldots \quad (5.)$$

that is,

$$\mathrm{V}.\,(\alpha'' - b\,\alpha)\,\alpha' = 0, \quad \ldots\ldots \quad (6.)$$

so that the one vector factor $\alpha'' - b\alpha$ of this last product must be equal to the other vector factor α' multiplied by some new scalar b'; and we may write the formula,

$$\alpha'' = b\,\alpha + b'\,\alpha', \quad \ldots\ldots\ldots \quad (7.)$$

as a transformation of (1.) or of (2.). We may also write, more symmetrically, the equation

$$a\alpha + a'\alpha' + a''\alpha'' = 0, \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

introducing *three* scalar coefficients a, a', a'', which have however only *two arbitrary ratios*, as a symbolic transformation of the proposed equation $\alpha\,\alpha'\,\alpha'' - \alpha''\,\alpha'\,\alpha = 0$. And it is remarkable that while we have thus *lowered by two units the dimension of that proposed equation*, considered as involving three variable vectors $\alpha, \alpha', \alpha''$, we have at the same time *introduced* (what may be regarded as) *two arbitrary constants*, namely the two ratios of a, a', a''. A converse process would have served to *eliminate two arbitrary constants*, such as these two ratios, or the two scalar coefficients b and b', from a linear equation of the form (8.) or (7.), between three variable vectors, at the same time *elevating the dimension of the equation by two units*, in the passage to the form (2.) or (1.). And the analogy of these two converse transformations to *integrations and differentiations of equations* will appear still more complete, if we attend to the intermediate stage (5.) of either transformation, which is of an *intermediate degree*, or dimension, and involves *one arbitrary constant* b; that is to say, *one more* than the equation of highest dimension (1.), and *one fewer* than the equation of lowest dimension (7.).

25. As the equation $\mathrm{S}.\alpha\,\alpha'\,\alpha'' = 0$ has been seen to express that the three vectors $\alpha\,\alpha'\,\alpha''$ represent coplanar lines, or that any one of these three lines, for example the line represented by the vector α, is in the plane determined by the other two, when they diverge from a common origin; so, if we make for abridgement

$$\left.\begin{aligned} \beta &= \mathrm{V}(\mathrm{V}.\alpha\alpha'.\mathrm{V}.\alpha'''\alpha^{\mathrm{iv}}), \\ \beta' &= \mathrm{V}(\mathrm{V}.\alpha'\alpha''.\mathrm{V}.\alpha^{\mathrm{iv}}\alpha^{\mathrm{v}}), \\ \beta'' &= \mathrm{V}(\mathrm{V}.\alpha''\alpha'''.\mathrm{V}.\alpha^{\mathrm{v}}\alpha), \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (1.)$$

the equation

$$\mathrm{S}.\beta\beta'\beta'' = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

may easily be shown to express that *the six vectors* $\alpha\alpha'\alpha''\alpha'''\alpha^{\mathrm{iv}}\alpha^{\mathrm{v}}$ *are homoconic*, or represent *six edges of one cone of the second degree*, if they be supposed to be all drawn from one common origin of vectors. For if we regard the five vectors $\alpha'\alpha''\alpha'''\alpha^{\mathrm{iv}}\alpha^{\mathrm{v}}$ as given, and the remaining vector α as variable, then first the equation (2.) will give for the locus of this variable vector α, some cone of the second degree; because, by the definitions (1.) of β, β', β'', if we change α to $a\,\alpha$, a being any scalar, each of the two vectors β and β'' will also be multiplied by a, while β' will not be altered; and therefore the function $\mathrm{S}.\beta\beta'\beta''$ will be multiplied by a^2, that is by the square of the scalar a, by which the vector α is multiplied. In the next place, this conical locus of α will contain the given vector α'; because if we suppose $\alpha = \alpha'$, we have $\beta = 0$, and the equation (2.) is satisfied: and in like manner the locus of α contains the vector α^{v}, because the supposition $\alpha = \alpha^{\text{v}}$ gives $\beta'' = 0$. In the third place, the cone contains α'' and α^{iv}; for if we suppose $\alpha = \alpha''$, then, by the principle contained in the formula (4.) of the last article, we have

$$\beta'' = -\mathrm{V}(\mathrm{V}.\alpha^{\text{v}}\alpha''.\mathrm{V}.\alpha''\alpha''') = \alpha''\mathrm{S}.\alpha^{\text{v}}\alpha''\alpha''';$$

and by the same principle, under the same condition,

$$\begin{aligned}\mathrm{V}.\beta\beta' &= \mathrm{V}(\mathrm{V}(\mathrm{V}.\alpha'''\alpha^{\text{iv}}.\mathrm{V}.\alpha'\alpha'').\mathrm{V}(\mathrm{V}.\alpha'\alpha''.\mathrm{V}.\alpha^{\text{iv}}\alpha^{\text{v}}))\\ &= -\mathrm{V}.\alpha'\alpha''.\mathrm{S}(\mathrm{V}.\alpha'''\alpha^{\text{iv}}.\mathrm{V}.\alpha'\alpha''.\mathrm{V}.\alpha^{\text{iv}}\alpha^{\text{v}});\end{aligned}$$

but $\mathrm{S}(\mathrm{V}.\alpha'\alpha''.\alpha'') = \mathrm{S}.\alpha'\alpha''\alpha'' = 0$; therefore $\mathrm{S}.\beta\beta'\beta'' = \mathrm{S}(\mathrm{V}.\beta\beta'.\beta'') = 0$; and in like manner this last condition is satisfied, if $\alpha = \alpha^{\text{iv}}$, because β and $\mathrm{V}.\beta'\beta''$ then differ only by scalar coefficients from α^{iv} and $\mathrm{V}.\alpha^{\text{iv}}\alpha^{\text{v}}$, respectively, so that the scalar of their product is zero. Finally, the conical locus of α contains also the remaining vector α''', because if we suppose $\alpha = \alpha'''$, we have

$$\beta = \alpha'''\mathrm{S}.\alpha'\alpha'''\alpha^{\text{iv}}, \quad \beta'' = \alpha'''\mathrm{S}.\alpha''\alpha'''\alpha^{\text{v}},$$

and therefore in this case $\mathrm{S}.\beta\beta'\beta'' = 0$, because the scalar of the product of α''' and $\beta'\alpha'''$ is zero. The locus of α is therefore a cone of the second degree, containing the five vectors α', α'', α''', α^{iv}, α^{v}; and in exactly the same manner it may be shown without difficulty that *whichever of the six vectors* $\alpha\,..\,\alpha^{\text{v}}$ *may be regarded as the variable vector, its locus assigned by the equation* (2.), of the present article, *is a cone of the second degree, containing the five other vectors.* We may therefore say that this equation,

$$\mathrm{S}.\beta\beta'\beta'' = 0,$$

when the symbols β, β', β'' have the meanings assigned by the definitions (1.), or (substituting for those symbols their values) we may say that the following equation

$$\left.\begin{aligned}\mathrm{S}\{\mathrm{V}(\mathrm{V}.\alpha\alpha'.\mathrm{V}.\alpha'''\alpha^{\text{iv}}).\mathrm{V}(\mathrm{V}.\alpha'\alpha''.\mathrm{V}.\alpha^{\text{iv}}\alpha^{\text{v}}).\\ \mathrm{V}(\mathrm{V}.\alpha''\alpha'''.\mathrm{V}.\alpha^{\text{v}}\alpha)\} = 0,\end{aligned}\right\} \quad . \quad . \quad (3.)$$

is the *equation of homoconicism*, or of *uniconality*, expressing that, when it is satisfied, one common cone of the second degree passes through all the six vectors $\alpha\alpha'\alpha''\alpha'''\alpha^{\text{iv}}\alpha^{\text{v}}$, and enabling us to deduce from it all the properties of this common cone.

26. The considerations employed in the foregoing article might leave a doubt whether *no other* cone of the same degree could pass through the same six vectors; to remove which doubt, by a method consistent with the spirit of the

present theory, we may introduce the following considerations respecting conical surfaces in general.

Whatever four vectors may be denoted by α, α', β, β', we have

$$\mathrm{V}\,(\mathrm{V}.\,\alpha\alpha'.\,\mathrm{V}.\,\beta\beta') + \mathrm{V}\,(\mathrm{V}.\,\beta\beta'.\,\mathrm{V}.\,\alpha\alpha') = 0\,; \quad . \quad (1.)$$

substituting then for the first of these two opposite vector functions the expression (3.) of art. 24, and for the second the expression formed from this by interchanging each α with the corresponding β, we find, for any four vectors,

$$\alpha\,\mathrm{S}.\,\alpha'\beta\beta' - \alpha'\,\mathrm{S}.\,\alpha\beta\beta' + \beta\,\mathrm{S}.\,\beta'\alpha\alpha' - \beta'\,\mathrm{S}.\,\beta\alpha\alpha' = 0. \quad . \quad (2.)$$

Again, it follows easily from principles and results already stated, that the scalar of the product of three vectors changes sign when any two of those three factors change places among themselves, so that

$$\left.\begin{aligned} \mathrm{S}.\,\alpha\beta\gamma &= -\,\mathrm{S}.\,\alpha\gamma\beta = \mathrm{S}.\,\gamma\alpha\beta \\ = -\,\mathrm{S}.\gamma\beta\alpha &= \mathrm{S}.\,\beta\gamma\alpha = -\,\mathrm{S}.\,\beta\alpha\gamma. \end{aligned}\right\} \quad . \quad . \quad . \quad (3.)$$

Assuming therefore any three vectors ι, $\varkappa$, λ, of which the scalar of the product does not vanish, we may express any fourth vector α in terms of these three vectors, and of the scalars of the three products $\alpha\varkappa\lambda$, $\iota\alpha\lambda$, $\iota\varkappa\alpha$, by the formula:

$$\alpha\,\mathrm{S}.\,\iota\varkappa\lambda = \iota\,\mathrm{S}.\,\alpha\varkappa\lambda + \varkappa\,\mathrm{S}.\,\iota\alpha\lambda + \lambda\,\mathrm{S}.\,\iota\varkappa\alpha. \quad . \quad . \quad (4.)$$

Let α be supposed to be a vector function of one scalar variable t, which supposition may be expressed by writing the equation

$$\alpha = \phi\,(t)\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

and make for abridgement

$$\frac{\mathrm{S}.\,\alpha\varkappa\lambda}{\mathrm{S}.\,\iota\varkappa\lambda} = f_1\,(t)\,; \quad \frac{\mathrm{S}.\,\iota\alpha\lambda}{\mathrm{S}.\,\iota\varkappa\lambda} = f_2\,(t); \quad \frac{\mathrm{S}.\,\iota\varkappa\alpha}{\mathrm{S}.\,\iota\varkappa\lambda} = f_3\,(t)\,; \quad . \quad (6.)$$

the forms of these three scalar functions $f_1\,f_2\,f_3$ depending on the form of the vector function ϕ, and on the three assumed vectors $\iota\,\varkappa\,\lambda$, and being connected with these and with each other by the relation

$$\phi\,(t) = \iota f_1\,(t) + \varkappa f_2\,(t) + \lambda f_3\,(t). \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

Conceive t to be eliminated between the expressions for the two ratios of the three scalar functions $f_1\,f_2\,f_3$, and an equation of the form

$$\mathrm{F}\,(f_1\,(t), \;\; f_2(t), \;\; f_3(t)) = 0\,. \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

to be thus obtained, in which the function F is scalar (or real), and homogeneous; it will then be evident that while the equation (5.) may be regarded as the *equation of a curve in space* (equivalent to a system of three real equations between the three co-ordinates of a curve of double curvature and an auxiliary variable t, which latter variable may represent the *time*, in a motion along this curve), the *equation of the cone* which passes through this arbitrary curve, and has its vertex at the origin of vectors, is

$$\mathrm{F}(\mathrm{S}.\alpha\kappa\lambda,\quad \mathrm{S}.\iota\alpha\lambda,\quad \mathrm{S}.\iota\kappa\alpha) = 0. \quad . \quad . \quad . \quad . \quad (9.)$$

Such being a form in this theory for the equation of an *arbitrary conical surface*, we may write, in particular, as a *definition of the cone of the* nth *degree*, the equation:

$$\Sigma\,(\mathrm{A}_{p,q,r}\,(\mathrm{S}.\alpha\kappa\lambda)^p\,.\,(\mathrm{S}.\iota\alpha\lambda)^q\,.\,(\mathrm{S}.\iota\kappa\alpha)^r) = 0;\ . \quad (10.)$$

p, q, r being any three whole numbers, positive or null, of which the sum is n; $\mathrm{A}_{p,q,r}$ being a scalar function of these three numbers; and the summation indicated by Σ extending to all their systems of values consistent with the last-mentioned conditions, which may be written thus:—

$$\left.\begin{aligned} \sin p\pi = \sin q\pi = \sin r\pi = 0; \\ p \geqq 0, \quad q \geqq 0, \quad r \geqq 0; \\ p + q + r = n. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (11.)$$

When $n = 2$, these conditions can be satisfied only by *six* systems of values of p, q, r; therefore, in this case, there enter only six coefficients A into the equation (10.); consequently *five* scalar ratios of these six coefficients are sufficient to particularize a cone of the second degree; and these can in general be found, by ordinary elimination between five equations of the first degree, when five particular vectors are given, such as α', α'', α''', α^{iv}, α^{v}, through which the cone is to pass, or which its surface must contain upon it. Hence, as indeed is known from other considerations, it is in general a determined problem to find the particular cone of the second degree which contains on its surface five given straight lines: and the general solution of this problem is contained in the equation of homoconicism, assigned in the preceding article. The proof there given that the six vectors $\alpha\,.\,.\,\alpha^{\mathrm{v}}$ are homoconic, when they satisfy that equation, does not involve any property of conic sections, nor even any property of the circle: on the contrary, that equation having once been established, by the proof just now referred to, might be used as the basis of a complete theory of conic sections, and of cones of the second degree.

27. To justify this assertion, without at present attempting to effect the actual development of such a theory, it may be sufficient to deduce from the equation of homoconicism assigned in article 25, that great and fertile property of the circle, or of the cone with circular base, which was discovered by the genius of Pascal. And this deduction is easy; for the three

auxiliary vectors β, β', β'', introduced in the equations (1.) of the 25th article, are evidently, by the principles stated in other recent articles of this paper, the respective lines of intersection of three pairs of planes, as follows:—The planes of $\alpha\,\alpha'$ and $\alpha'''\,\alpha^{\text{iv}}$ intersect in β; those of $\alpha'\,\alpha''$ and $\alpha^{\text{iv}}\,\alpha^{\text{v}}$ in β'; and those of $\alpha''\,\alpha'''$ and $\alpha^{\text{v}}\,\alpha$ in β''; and the form (2.), art. 25, of the equation of homoconicism, expresses that these three lines, $\beta\,\beta'\,\beta''$, are coplanar. *If then a hexahedral angle be inscribed in a cone of the second degree, and if each of the six plane faces be prolonged* (if necessary) *so as to meet its opposite in a straight line, the three lines of meeting of opposite faces*, thus obtained, *will be situated in one common plane*: which is a form of the theorem of Pascal.

[To be continued.]

XXIII. *On the Influence exerted by Electricity, Platinum and Silver upon the Luminosity of Phosphorus. By Dr.* C. F. SCHŒNBEIN[*].

SOME time ago I tried to show that the shining of phosphorus in atmospheric air is intimately connected with the formation of that highly oxidizing agent I have called ozone. The correctness of that view is confirmed by the fact that phosphorus never becomes luminous if the production of ozone be prevented, or that luminous phosphorus grows dark if the ozone be removed. It is well-known that phosphorus remains dark at low temperatures, and I have ascertained that under these circumstances no ozone is produced; my experiments have further shown that phosphorus still shines in ozonized air at a temperature at which in pure air phosphorus exhibits not the slightest emission of light.

According to the results of my former researches, ozone is formed during electrical discharges taking place in atmospheric air, the electrolysis of water and the action exerted by phosphorus upon moist mixtures of oxygen and nitrogen, oxygen and hydrogen, oxygen and carbonic acid gas.

These facts taken together led me to suspect that phosphorus might become luminous in atmospheric air within which electrical discharges had been effected, at a temperature at which phosphorus does not shine in common air. How far that conjecture is well-founded will appear from the facts I am going to state.

1. If, at a temperature of 2° to 5° R. below zero, a piece of phosphorus, about an inch long, and having a clean surface,

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[*] Communicated by the Chemical Society; having been read March 2, 1846.

be laid upon a wooden board, and the free end of a metallic wire connected at its other end with the conductor of an electrical machine be placed at the distance of a few lines before the dark phosphorus, the latter will become luminous as soon as the electrical brush makes its appearance at the free end of the wire; and in addition, a luminous tail of 4 to 6 inches long will be perceived behind the phosphorus. The brush is no sooner made to disappear than the shining tail disappears also, whilst the phosphorus itself remains luminous for a second or two more. The brush being called forth again, the phænomena described will repeatedly take place.

2. The same piece of phosphorus, at a temperature of about 4° R. below zero, being placed within a coil made up of one extremity of a copper wire, so that the end of that coil projects (in the shape of a point) about a line beyond the phosphorus, and the other end of the wire being connected with the conductor of the electrical machine, a very interesting phænomenon will take place as soon as the brush is caused to appear at the metallic point that projects beyond the phosphorus. From the centre of the brush proceeds a luminous cone, the apex of which lies in the middle of the brush. The length of that cone varies with the size of the brush, the longer the latter the longer the cone. I have often obtained cones at least two feet in length with brushes being hardly an inch long. I am sure the admirable machine of the Polytechnic Institution would yield tails of extraordinary length, and it is really worth while to make the experiment with that powerful apparatus. I must not omit to mention that the phænomenon resembles exactly the tail of a comet, and cannot be perceived but in complete darkness; but it is hardly necessary to add that the luminous cone disappears invariably and simultaneously with the brush. If the finger be approached to the brush so as to change the position of the latter, the cone in its whole length is also put in motion. In the experiments described use was made of the positive brush; in changing the positive electricity for the negative a tail is likewise obtained, but that cone is thin and less lively.

3. If, at a temperature of 2° R. below zero, a piece of phosphorus be placed in a bottle so arranged that the electrical brush may at pleasure be produced within that vessel, the phosphorus becomes luminous so soon as the brush makes its appearance, and that phosphorescence is the more lively the stronger the brush and the longer the latter had been made to play. The emission of light continues to take place for a short time after the cessation of the play of the brush. The phosphorus having grown dark, becomes luminous again along with the reappearance of the brush.

4. According to my experiments, chemical and voltaic ozone are instantaneously destroyed by a number of gaseous substances, *ex. gr.* by olefiant gas, sulphurous acid, fumes of hyponitric acid, vapour of æther, &c. Hence it comes that phosphorus placed in atmospheric air, mixed with small quantities only of any of the substances named, does not produce ozone, and it is well known that phosphorus remains dark under the same circumstances. If the experiment be made as indicated under § 3, the electrical brush, lively as it may happen to play within the air of the bottle, does not call forth in phosphorus the slightest sign of phosphorescence, provided the atmospheric air surrounding phosphorus contain some olefiant gas, hyponitric acid, &c.

5. Some time ago I ascertained the fact that either platinum black or spongy platinum has the power of destroying indigo, of colouring blue the resin of guaiacum, of decomposing iodide of potassium, in short, of producing oxidizing effects very similar to those brought about by the electrical brush or spark. That similarity of action made me suspect that, with regard to phosphorus, platinum being in a state of minute mechanical division, might conduct itself like electricity, and the results of my experiments have proved the correctness of my conjecture. At a temperature of 4° R. below zero, newly-prepared platinum black was placed upon a watch-glass; now, as soon as a piece of phosphorus (having previously been wiped with filtering-paper) was made to touch the metallic powder it became luminous, first at the point of contact, and immediately afterwards along its whole surface. On breaking that contact, the phosphorus turned dark again.

6. Spongy silver, as it is obtained from the acetate of that metal, acts upon phosphorus as powerfully as spongy platinum does; for no sooner has phosphorus been touched by the silver than the former becomes luminous, even at a temperature of 6° R. below zero.

7. Iron, lead, copper, antimony, bismuth, tin, in a state of minute mechanical division, have no effect upon phosphorus. With gold, iridium, and the rest of the metals, I have not yet made any experiment.

XXIV. *On Struvite, a new Mineral.* *By* G. L. Ulex*.

NUMEROUS crystals were found in digging out the ground of St. Nicholas church, in the middle of our town; the largest of which are about one inch long, and weigh

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* Communicated by the Chemical Society; having been read March 2, 1846.

from 1·4 to 1·8 grm. Their primary form is a right rhombic prism. [The admeasurements were made by Prof. Marx.]

M on M . . .	95° 10′
M on O . . .	132° 25′
O on P . . .	138° 25′
s on *s* . . .	63° 30′
h on *t* . . .	143°
h on *h* . . .	57° 10′

Fig. 1.

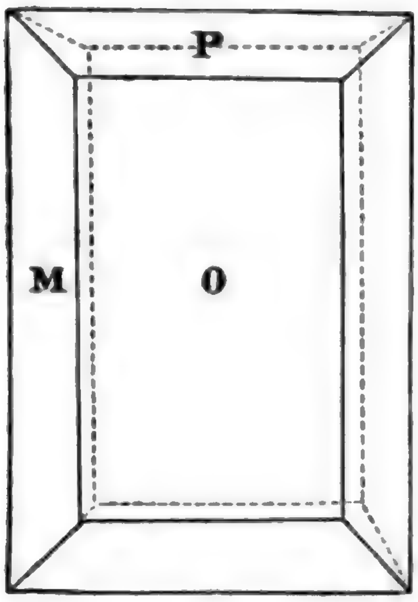

Fig. 4.

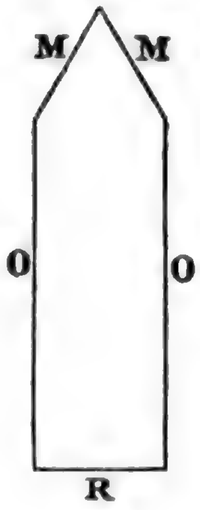

Fig. 2.

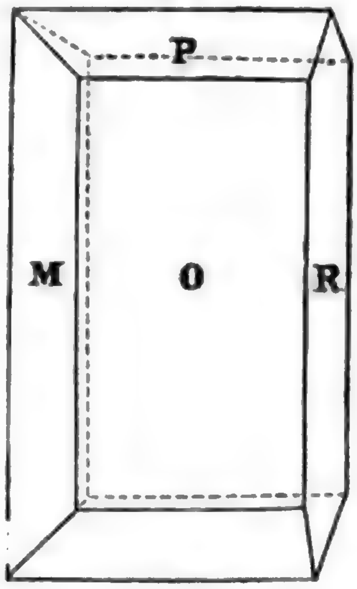

Fig. 5.

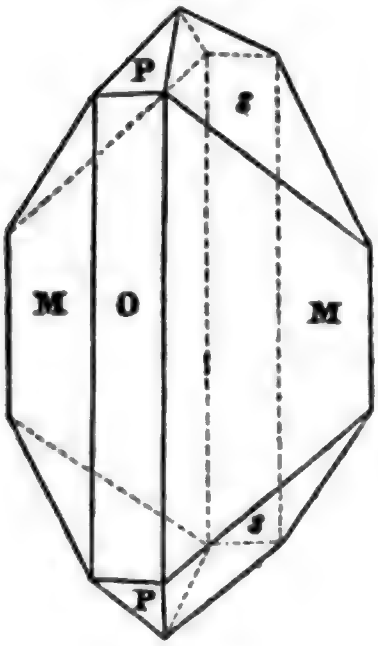

Fig. 6.

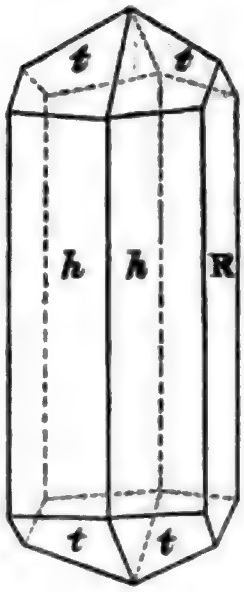

Fig. 7.

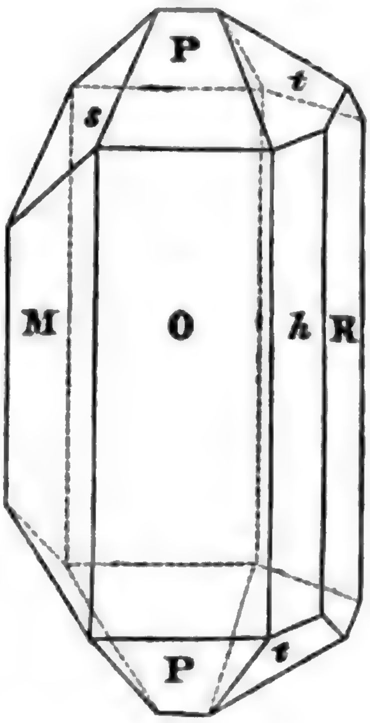

Fig. 1. Represents a crystal of struvite in its simplest form.

Fig. 2. The crystal as it most frequently occurs.

Fig. 4. A segment.

Figs. 5, 6. Crystals with some secondary planes, but generally only segments occur; the plane *s* on both sides large and distinct: the planes *h* and *t* very small.

Fig. 7. A view of the planes collectively as exhibited by some crystals.

They cleave parallel to the plane of O. A peculiarity in these crystals is, that they occur almost always in halves, and appear to have rested or been formed on planes which would have passed through the centre of the entire crystal. One of these natural segments is shown in fig. 2.

The crystals are transparent, and of a somewhat yellow colour; most of them inclose organic matter, and are thereby rendered more or less dark and opake. They are harder than talc, but are scratched by gypsum. Their specific gravity is 1·7.

They are very sparingly soluble in water, and in consequence tasteless. When heated in a tube they give out much water and ammonia (the black crystals empyreumatic water and ammonia) without flying into pieces. When heated to redness, they exhibit the phænomenon of phosphorescence: the passing of *c* into *b*, phosphoric acid. Heated before the blowpipe they melt into a colourless glass, which on cooling forms a white enamel.

The constituent parts of the salt are phosphoric acid, magnesia, ammonia and water.

Dissolved in hydrochloric acid and precipitated by ammonia, they yield a sandy powder, which under the microscope exhibits the well-known form of the phosphate of ammonia and magnesia.

The salt loses by ignition 54·7 to 55·5 per cent. (the difference is caused by the small quantity of organic matter). The ammonia was estimated by means of chloride of platinum; by dissolving the salt in hydrochloric acid, 6·9 to 7·1 per cent. of ammonia were found. The residue of 44·5 to 45·3 per cent. should be the same combination of phosphoric acid with magnesia as is contained in the phosphate of magnesia and ammonia, because if the crystals are dissolved in hydrochloric acid and precipitated by ammonia, the fluid which is filtered from the precipitate gives no reaction either with sulphate of magnesia or with phosphate of soda. It consists therefore of

$$(NH_4O) + 2MgO + PO^5) + 12HO.$$

The crystals are the same salt which is found in many animal secretions, and in putrifying urine.

The salt is not altered by the air. Mr. Graham has informed us, that if the artificial salt is heated to 212° F. it loses 10 at. of water and no ammonia. The natural crystals can bear a temperature of 248° F., and they give out the same quantity of water and no ammonia. The products of distillation were conducted into a solution of the nitrate of the protoxide of mercury, which was not blackened.

Concerning the locality where the crystals are found, we perceive below the upper strata, at a depth of 6 to 12 feet, a large quantity of cattle-dung mixed with straw, in a state of putrefaction. This passes by degrees into a black peat earth, which extends to a depth of 26 feet and rests upon gravel. The peat earth, of a thickness of 10 to 12 feet, consists of a homogeneous impalpable mass, mixed here and there with small parts of vegetable remains (parts of grasses; Sphagnum and other mosses could not be detected). This is the true matrix of the crystals, in which here and there blue iron earth (protophosphate of iron or vivianite) is also found.

By drying in the air it loses 40–50 per cent. of water, and is not to be distinguished from the heavy black turf.

Water dissolves very little from it. The solution is of a light brown colour, without any reaction upon litmus. Heated, it gives off at first some ammonia; in other respects it smells and burns like turf, with a bright flame. The ashes which remain vary in weight from 20–30 per cent.; moistened with water it does not act upon litmus.

A quantity dried at 212° F. was subjected to analysis; 100 parts of it gave—

2·0	per cent. soluble in æther (principally chlorophylle).
1·5	... ... alcohol (principally resinous matter).
1·5	... ... water (principally salts of humous acids).
36·0	... ... liquor potassæ (principally humous acids).
36·0	... organic residue (principally humine and vegetable fibrine).
23·0	... inorganic residue, consisting of—
0·3	... soluble in water (sulphate of potass and chloride of sodium, no phosphates of alkali).
11·5	... soluble in hydrochloric acid (principally alumina and phosphate of magnesia and lime, less peroxide of iron and sulphate of lime).
12·2	... insoluble in hydrochloric acid (sand; calcined with soda and decomposed by hydrochloric acid, it gave 11 silica, 1·2 alumina, and peroxide of iron).

The analysis shows that we have a humous mass, which has been formed from organic matters by putrefaction and decay. It is probable that these matters were principally the excrements of Herbivora; a presumption which is supported by the analysis of the ashes, from the quantities of phosphate of magnesia and lime.

The solid excrements of the Herbivora are characterized by abundance of phosphate of magnesia, and the deficiency of ammoniacal salts; the fluid excrements (urine) have, on the contrary, abundance of ammoniacal salts (from the decomposition of urea) and are deficient in earthy salts; relations which make it probable that the formation of such large crystals in such quantities (they occur in thousands) was caused by a reaction of the urine upon the solid excrements, where the first gave the ammonia, the latter the phosphate of magnesia.

The locality where the crystals are found confirms this assumption. The place where St. Nicholas's church is built was occupied 800 years ago by the New Castle (Neue Burg), which was burnt and destroyed with the whole city of Hamburg, in 1072, by Kruko, tyrant of the Wenden. Now it is most probable that the ditch of the castle was used as a reservoir for rubbish and manure by the inhabitants of the new-built city, who preferred trade, as more profitable, to agriculture. So by degrees the ditch was filled, and covered partly with houses, and a small part of it formed till a late period an open dung-pit, which was emptied from time to time. The crystals are found principally below the dung-pit, and appear to be formed by the infiltration of urine through a soil consisting of vegetable matters.

The crystals forming a mineral which has never yet been described, are named Struvite, in honour of the minister Von Struve, well-known to mineralogists, and highly meritorious from the great interest he takes in the advancement of science in the town of Hamburg.

XXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 54.]

April 2, 1846. "ON the Effects produced by Poisonous Fish on the Human Frame." By Sir William Burnett, M.D., K.C.H., Vice-President of the Royal Society.

The author communicates a report which he lately received from Mr. Jameson, the surgeon of the flag ship at the Cape of Good Hope, of the rapidly fatal consequences ensuing from eating small portions of the liver of a fish, known at the Cape by the name of the *Bladder* or *Toad fish*, the *Aptodactylus punctatus*, or *Tetrodon* of Cuvier. The symptoms were chiefly pain and burning sensation at the epigastrium, constriction and spasm of the fauces and muscles of deglutition, rigidity of the tendons, coma, paralysis and convulsions, following one another in quick succession, and terminating in death within twenty minutes after the poisonous food had been taken. Several other instances of the same kind are next related; and a narrative is subjoined of the case of a seaman who lost his life, with similar symptoms, from the bite of a water-snake in Madras roads; the *Coluber laticaudatus* of Linnæus (*Hydrus colubrinus* of Shaw); and also of a ship's company who were all severely affected by eating portions of a large *Banacuda* (*Perca major*).

The author ascribes the symptoms induced by these deleterious substances to their action on the nervous system alone, there being evidence of congestion only, but not of inflammation, in the stomach and other viscera.

"Further Researches on the Nervous System of the Uterus." By Robert Lee, M.D., F.R.S., Fellow of the Royal College of Physicians.

The author states, that on the 8th of April 1838, he discovered, in dissecting a gravid uterus, structures which had a striking resemblance to ganglionic plexuses of nerves; and, in the following December, he traced, in another gravid uterus, the sympathetic and spinal nerves into these new structures. He requested several distinguished anatomists to examine these dissections, and to compare them with similar dissections of the unimpregnated uterus, which he had made in the course of the same year. He then quotes, at some length, the opinions given by these several referees after their examination; and which appear, for the most part, to be favourable to the views of the author, namely, that the structures in question are not mere fibrous tissues, but that they possess the character of nerves, and that they augment in size with the enlargement of the uterus during pregnancy. Among those to whom the preparations were submitted for examination, however, two persons declared it to be their opinion, which they founded on observations with the microscope, that the filaments regarded by the author as nerves, are bands of elastic tissue only, and not plexuses of nerves; and the author, on receiving this intimation, withdrew the paper which he had presented to the Royal Society, and which had been read on the 12th of December 1839, in which paper the appearances displayed in his dissections were minutely described and delineated.

The author next proceeds to give the history of his subsequent researches on the same subject, which he extended to the corresponding parts in some of the larger quadrupeds; and from all these he obtained accumulated evidence of the truth of his original opinions. He also adduces the testimony of various observers, in addition to those he had before cited, which are all in accordance with his own views, as they are expressed in his paper, printed in the Philosophical Transactions for 1841, an Appendix to which was published in the volume of the same work for 1842. Later observations and dissections have served only to confirm him in his opinions; and he considers them as establishing the fact that the nerves of the uterus are considerably enlarged during the gravid state of that organ.

The author concludes his paper by giving a report, drawn up by Mr. John Dalrymple, of the results of his microscopic examination of the uterine nerves in preparations furnished by the author, which tend to corroborate his views.

May 7.—"On the Anatomy and Physiology of the Vascular Fringes in Joints, and the Sheaths of Tendons." By George Rainey, Esq., M.R.C.S. Communicated by John Simon, Esq., F.R.S., Assistant-Surgeon to the King's College Hospital, and Demonstrator of Anatomy in King's College, London.

It has been generally believed that the folds of synovial membrane which project into the articular cavities in the form of fringes, contain merely globules of fat, and are subservient only to the mechanical offices of filling up spaces that would otherwise be left vacant during the movements of the joints. By a careful examination of their real structure with the aid of the microscope, the author has found that they present an arrangement of vessels quite peculiar to themselves, and bearing no resemblance whatever to that of the vessels which secrete fat; together with an epithelium, remarkable by its form and disposition, and characteristic of organs endowed with the function of a special secretion. He has traced the presence of these synovial fringes in all cavities which contain synovia; that is to say, not only in the joints, but also in the sheaths of tendons, and in the bursæ mucosæ. When well-injected, they are seen, under the microscope, to consist of two parts; namely, a convolution of blood-vessels, and an investing epithelium. These convoluted vessels do not enclose, by their anastomoses, spaces like those capillaries which secrete fat, and which are of a much smaller size than the former; and the epithelial investments, besides enclosing separately each packet of convoluted vessels, sends off from each tubular sheath secondary processes of various shapes, into which no blood-vessels enter. The lamina itself, forming these folds and processes, consists of a very thin membrane studded with flattish oval cells, a little larger than blood-corpuscles, but destitute of nucleus or nucleoli; presenting none of the characters of tessellated epithelium, but corresponding more to what Mr. Goodsir has termed *germinal membrane*. From all these facts the author concludes that the proper office of this structure is to secrete synovia; an office which Clopton Havers had assigned to them as long ago as the year 1691, although his opinion has not been generally adopted by later physiologists.

"Description of a Process for extracting the Palladium which exists in combination with the Gold of the Gongosocho and other Mines in the Brazils." By Percival N. Johnson, Esq., in a letter to the President, by whom it is communicated to the Society.

The process consists in melting the gold, obtained from its matrix by the ordinary processes of stamping and washing, with three times its weight of silver, granulating it in cold water, and refining it by the process of quartation, or separation of its alloys by nitric acid. The nitric solution contains the silver, palladium, copper, and a small proportion of iron and lead.

May 14.—Contributions to Terrestrial Magnetism. No. VII. By Lieut.-Colonel Sabine, R.A., For. Sec. R.S.

From the discovery which was made of the non-coincidence of the locality of the maximum magnetic intensity, within the Arctic circle, with that in which the magnetic direction is vertical, it followed that the generally prevailing opinions respecting the distribution of magnetic force at the surface of the earth were erroneous, and that even the broad outline of the picture of terrestrial magnetism required to be recast. For the purpose of obtaining sufficiently copious and accurate materials, by means of which so desirable an object could be accomplished, the British Association for the Advancement of Science requested, in the year 1835, a report to be prepared, in which the state of our knowledge, collected from a great variety of sources, with regard to the variations of the magnetic force at different parts of the earth's surface, should be reviewed, and properly discussed, and suggestions offered as to the best means of extending the inquiry. In the report so obtained, it was recommended that magnetic surveys of that portion of the North American Continent, which is comprised within certain isodynamic lines, should be procured. The present paper contains the results of an expedition towards the accomplishment of this object, recommended by the President and Council of the Royal Society to be undertaken under the auspices and with the assistance of the Hudson's Bay Company. Lieutenant Lefroy, of the Royal Artillery, who had received an appointment to the Toronto Observatory, with a special view to this survey, was entrusted with the conduct of an expedition in conformity with that recommendation.

The author gives a circumstantial narrative of the expedition, together with minute details of the instruments employed, and the methods of observation adopted; and extensive tables of the observations themselves, both as regards intensity and inclination, at the different stations where they were made, occupying altogether about 120 folio pages of manuscript.

It results from the calculations founded on the data furnished by these observations, that the geographical position of the point of maximum intensity, where its amount is 1·88, is 52° 19′·3 north latitude, and 268° 01· longitude. The angle which the major axis of the ellipse makes with the parallel of geographical latitude is 57° 49′·5; and the values of the semi-axes of the ellipse of 1·875 are 290 and 110 geographical miles respectively.

May 28.—"An Account of the desquamation and change of colour in a Negro of Upper Guinea, West Africa." By the Rev. Thomas S. Savage, M.D., Corr. Member of the Boston Natural History Society, &c. Communicated by Richard Owen, Esq., F.R.S.

The subject of this narrative, named Tahtoo Duari, is a member of the Grebo tribe, the aboriginal inhabitants of Cape Palmas and its vicinity. His parents were members of the same tribe and natives of the same region. The father was of a decidedly black complexion, while the mother was what is termed *yellow*, the two extremes observable in the tribe, and between which there is found every variety of shade. In March 1844, when about twenty-five years of age, Tahtoo was attacked with a quotidian ague, having previously been in perfect health. The febrile symptoms subsided in the course of a week, but were followed by a general desquamation of the cuticle, leaving the subjacent skin of a dingy yellow hue. A month afterwards, the same process, preceded by a similar febrile attack, recurred, and was followed by still greater whiteness of the newly-formed skin, resulting in the complete conversion of a negro to a white man, retaining the characteristic features and hair of an Ethiopian. This change was accompanied with great sensibility in the skin to the heat of the sun and of fire, exposure to which readily excited irritation, and even inflammation; but the general health soon became completely re-established. In the course of three months, subsequently to this change, numerous spots of a chestnut-brown colour made their appearance, first on the wrists, then on the back of the arms, head and neck, and successively on the other parts of the body, forming by their extension dark patches of various sizes; which, being scattered over the whole surface, presented a singularly mottled appearance; and as the black colour became predominant, the white portions of the skin seemed like patches of irregular shape formed in the natural negro skin. With the colour of the skin, that of the hair, which had also become white, has been gradually restored to its former black hue. During this process of return to the natural colour the health has been remarkably good.

June 11.—"On the Physiology of the Human Voice." By John Bishop, Esq., F.R.S.

After premising a brief description of the system of organs which are subservient to the voice, the author proceeds to consider the several theories which have been devised to account for its various modifications. These theories have, for the most part, been founded on the laws which regulate the vibratory movements of stretched membranous surfaces; and the investigation of those laws has accordingly occupied the attention of many eminent mathematicians, such as Euler, Bernoulli, Riccati, Biot, Poisson and Herschel; but it is a subject requiring the most profound analysis, and involving the resolution of problems of much greater complexity than the laws of the vibrations of either strings or bars. The assumptions which are necessary in order to bring the subject within the reach of analysis, namely, that the membrane is homogeneous in its substance, and of equal thickness and elasticity throughout its whole extent, are at variance with the actual conditions of the vocal organs, which are composed of tissues differing in thickness, density and elasticity, and of which the tension is indeterminate; circumstances which present insuperable obstacles to the attainment of a mathematical theory of their vibrations.

The author, after giving a critical account of the experiments made by Biot, Willis, Müller, Cagniard la Tour and De Kempelin on the vibrations of membranous laminæ, examines the various actions of the vocal organs during the production of the more simple tones; and considers more especially the office of the vocal ligaments, in regulating the pitch of the voice, which he considers as resulting from variations in their length and tension conjointly. By applying to the chordæ vocales the formulæ of vibrating cords, he traces the influence which is exerted on their movements by the mucous membranes; and finds that they obey, to a certain extent, the laws of vibrating strings.

The analogy between the action of the glottis and that of a reed is next examined, and an opinion expressed that the movements of the glottis in the vocalization of the sound, partake of the nature of the reed, during the partial opening and shutting of the rima-glottidis.

The author next investigates the acoustic relations between the actions of the glottis and that of the vocal pipe, and the acoustic effects of flexible membranous tubes on a column of air vibrating within it, and finds that the structure of the trachea and of the soft parts above and below the larynx is adapted to vibrate synchronously with any note that may be formed in the larynx. The falsetto voice may be produced either by the partial closing of the glottis, or by a nodal division of the vocal chords; the pitch of the sound in the production of this peculiar modification of the voice, being such that the column of air in the vocal tube is of the precise length requisite to vibrate in unison with the larynx. The inquiry is further extended to the sources of the various tones of the voice in singing, such as the *bass*, *tenor*, *contralto*, and *soprano*; together with their subdivisions of *barytone*, *mezzo-soprano*, and *soprano-sfogato*; and to the places which they occupy in the musical scale. Independently of the falsetto, the compass of the natural voice rarely exceeds two octaves; although in some cases, as in those of Malibran and Catalani, it may extend even beyond three. The voice in singing is modulated by the contraction or relaxation of the velum, uvula and fauces. The author lastly adverts to the attempts that have, at various times, been made by the Abbé Mical, Faber, Kratzenstein, De Kempelin, Willis, Wheatstone and others, to imitate articulate sounds by mechanism.

Having thus examined the human voice as resulting from the vibration of membranous ligaments, in obedience, first, to the laws of musical strings; secondly, to those of reeded instruments; and thirdly, to those of membranous pipes; he arrives at the conclusion that the vocal organs combine, in reality, the actions of each of these instruments, and exhibit in conjunction, the perfect type of every one of them.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 60.]

February 13, 1846.—*Extracts from the Report of the Council of the Society to the Twenty-sixth Annual General Meeting.*

At the last annual meeting, the Astronomer Royal, then President, in announcing the reasons upon which the Council had awarded the medal to Captain Smyth for his Bedford Observations, made some remarks upon the difficulty which arose from the original observations not being before the Society, and concluded by expressing strongly his opinion that those observations should be given up to the Council, that they might have full means of justifying their award. The slips of paper containing the results of the measures had been transferred to Dr. Lee, with the Bedford instruments. In March, however, they were forwarded to our Secretary by Captain Smyth, with the consent of Dr. Lee, and are now the property of the Society, which owes its thanks to both the gentlemen named, for their ready accession to the President's request. The Council, fully concurring in the Astronomer Royal's remarks, have great satisfaction in making this announcement.

It has been for some time understood that it was the intention of our respected colleague, Dr. Pearson, to bequeathe to the Society the stock and plates of his most valuable work on Practical Astronomy. Some months since he expressed his desire that the Society should take charge of 500 copies, till such time (a distant one we hope) as his bequest should take effect. This request being readily granted, the copies in question were deposited in our apartments: the plates still remain in Mr. Woodfall's custody.

Among the other presents received during the past year may be mentioned the manuscript observations of the Paramatta Catalogue of 7385 Stars, presented by Sir Thomas Brisbane.

To Mr. Turnor the Society is indebted for the gift of the curious manuscript Almanacs described in the recently published volume of our Memoirs by our lamented Assistant-Secretary.

The Society has to regret the loss, by death, of the following Fellows:—Admiral Greig, Lieutenant A. P. Greene, General Sir Robert Dundas, Charles Shearman, Esq., and the Rev. Henry Coddington.

The post of Director of the Edinburgh Observatory, vacant by the lamented death of Mr. Henderson, has been conferred by Government on Mr. Charles Piazzi Smyth, the second son of our President, who, as is well known to all here present, has been engaged for the last ten years as assistant to Mr. Maclear, at the Cape of Good Hope. Mr. Piazzi Smyth has lately arrived in England, and is now settled at Edinburgh, and engaged in the performance of his duties. Educated in so good a school, and having hitherto shown so much zeal in the pursuit of practical astronomy, we cannot doubt that he will do additional honour to the name which he bears; and in that persuasion, the Council feel sure this meeting will join them in congratulating our President on the prospects which have opened to his son.

The Council have awarded the gold medal to the Astronomer Royal for his recent publication of the reductions of the Greenwich Planetary Observations from 1750 to 1830. This work was described generally in the last Annual Report, as about to appear. The President will undertake, at the close of the ordinary proceedings of this day, to describe and justify the grounds on which the award was made. The Council will add nothing except their entire conviction, that it rarely happens that so valuable a present is made by the practical to the theoretical astronomer; and seldom indeed, that it is made by one who unites both characters in so remarkable a degree.

The corresponding series of lunar observations is now so far advanced, that only a small revision of some of the later years is wanting. Ten years' reductions in Right Ascensions and North Polar Distance are printed, the Government having supplied the necessary funds; and the formation of the equations for the correction of the elements is in progress. It is remarkable that, though all the lunar tables now existing to which observation has contributed, or which have not been formed from theory alone, have been founded on data procured from Greenwich, that Observatory is yet able to show that its records are not exhausted. Singularly connected as it has always been with the progress of lunar astronomy, its history has never presented so remarkable an epoch as the present time. The reduction, in the most systematic and usable form, of its old meridian observations, is the work of the Astronomer Royal, who suggested and undertakes the extra-meridional course of observation mentioned in the last report.

The altitude and azimuth instrument intended for this purpose has now left the engineer's hands, and is in those of Mr. Simms, for the completion of the micrometers, levels, graduations, &c. It is in a very forward state. The peculiar tables which the reduction of the observations will require are in preparation, and no long time will elapse before it is in use.

The connexion of Pulkowa, Altona, and Valentia with the Observatory of Greenwich has the calculations now completed. The result for Pulkowa is not yet printed; that for Altona differs very little from the one which was found many years ago. The result for Valentia has been compared with that derived from Colonel Colby's survey, the elements for the figure of the earth being those which were given some years ago by the Astronomer Royal, in the *Encyclopædia Metropolitana.* It appears that the geodetic longitudes of Liverpool and Kingston are a very little too large, that of Valentia a very little too small; being just what would take place if the ordinary assumptions employed in determining the figure of the earth were disturbed by an unknown source of attraction beneath the surface of Ireland. A memoir on these results is now in preparation by the Astronomer Royal.

The ordinary star-observations at Greenwich have been, during the past year, in a great measure devoted to filling up the blanks in one element (whether Right Ascension or North Polar Distance) which exist in the recently published catalogue of 1439 stars.

By information received from Colonel Estcourt, who is employed upon the survey of the North American boundary, it appears that the latitudes of various points have been obtained by the use of excellent twelve and fifteen-inch altitude and azimuth instruments. The differences of longitude have been found (the country presenting no more such difficulties as those described in last year's Report) by instantaneous signals of gunpowder flashes compared with transit observations, made at intervals of three minutes. The accuracy of the results may be inferred from this statement, that the extreme discordance of the determinations of difference of longitude from sixty signals was only four-tenths of a second. The intermediate parts have been filled up by the usual operations of surveying. There is no doubt that the map of this wild tract will be comparable in accuracy to that of any country in Europe. A general map is in preparation on a scale of four inches to a mile, with special maps on that of twelve inches to a mile.

At Cambridge, the observations of comets and of the new planet have, for the present, superseded those of double stars. The volume for 1842 has just been published. Besides the usual matter, it contains the accurate places of a large number of double stars. During the last year, the meridian observations were confined to stars; for the most part, either double stars or stars observed with comets. By this means Professor Challis hopes to complete a second Cambridge Catalogue, at the same time that he effects the reduction of the large number of comet observations which he has taken.

The last year has been marked to the astronomer by the appearance of the Catalogue of the British Association. This inestimable volume, which contains the mean places for 1850 of 8377 stars, with all the requisite aids for obtaining their apparent places for any other epoch, is an extension and completion of the Catalogue published in 1827 by this Society, and so well known as the Astronomical Society's Catalogue. Both were published under the same superintendence, and both bear the name of Francis Baily on their title-pages; but though upon the same plan, yet the second appears with such additions and improvements as render it a new work. For, first, the number of stars is nearly trebled; secondly, from the particular attention which has been paid of late years to this branch of astronomy (chiefly owing, it may be said, to our own Catalogue), the accuracy of the mean places has been immensely increased; thirdly, the secular variations of the precession have been added, as well as the annual proper motions, so far as they are known; and lastly, in the nomenclature, in the exact citation of authorities, and, it must be added, in the beauty of the getting-up, every thing has been done which the greatest experience and zeal could suggest.

The preface contains a clear account of the process of composing the catalogue, of the formulæ employed or to be employed, of the motives which directed the author in his variations from antecedent authorities,—in short, all that would have been expected from Francis Baily on the subject which had occupied him principally, though not exclusively, for a quarter of a century. The work was completed, with the exception of some notes, and partly printed, at the author's decease, and the task of bringing the whole to the desired end has been ably performed by his friend and coadjutor in the labour of constructing and printing our former catalogue, Lieutenant Stratford.

But though this volume is, and will be for years, the text-book of the practical astronomer, it must not be received as a perfect work, but as a step—an immense step, to be sure—towards perfection.

Our knowledge of the sidereal heavens is yet in its infancy. That every thing must be in motion, in accordance with the laws of gravitation, may indeed be looked upon as demonstrated; but, except the broad notion that our system is moving towards some point of the present apparent heavens, the knowledge of the approximate distances of two stars, certainly of 61 Cygni and probably of α Centauri, nothing is accurately known of the things which must become data before the velocity and direction of motion of the centre of gravity of our system can be determined. The late Astronomer Royal, Mr. Pond, was of opinion, and his judgement has recently been confirmed by our illustrious associate Bessel, that a variation in the proper motions of some stars has been detected by observation, and if this be confirmed, the first step is made*. But the exceeding minuteness of the difference of motions, which themselves require many years to rise to an appreciable amount, presents a difficulty only to be overcome by immense labour and the most scrupulous accuracy. For each star there are required, at least, three different and well-defined epochs; and where are we, as yet, to look for such data?

The Council cannot but mention what is one of the most remarkable astronomical events of the year, though perhaps no one of the parties concerned in it gave our science a thought. They allude to the repeal of the excise duty on glass, which might be called with perfect truth an astronomical window-tax. Every person here present must be aware that the nature of the regulations under which it was found or thought necessary to place the manufacture of this important article, rendered experiments for the improvement of optical glass almost impracticable and a great deal too expensive. It may now be confidently hoped, that in a few years our country will not be obliged to admit that we are surpassed by foreigners in this particular. But it must be remembered that it is not likely that the object will be attained, unless systematic attention be directed towards it. The Council hope, therefore, that those who feel interested in astronomy, and who have directed their attention to the chemistry of manufactures, will not forget to keep their thoughts fixed upon the great importance of procuring good optical glass in large quantities. The end to be gained is not merely the acquisition of large object-glasses in greater numbers. The want of sufficient supply, easily obtained, is found in regard to smaller lenses,

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* It may be worth consideration whether a complete recomputation of all Bradley's observations of stars might not be advantageous. The *Fundamenta*, as edited by Bessel, does not include *all* the observations, and the epoch of the observations is not given. This most important element was first added by the present Astronomer Royal, and the yearly partial catalogues which appear in the annals of our observatories are exceedingly valuable from supplying the date. In making the above suggestion we do not forget what we owe to Bessel's *Fundamenta*, nor how much of the reasonableness of our expecting yet more benefit from Bradley's observations lies in the consequences of the impulse which the publication of that mighty work gave to accurate astronomy, and the means which it supplies for still further improvement.

and no one can positively say, or even reasonably suppose, that the whole manufacture is not capable of great improvement.

An attempt has been made by Mr. Simms to furnish the object-glass for the Liverpool equatoreal from his own workmanship; but the indifferent quality of all the optical glass which is on sale (the Munich unworked glass is not to be bought) did not allow him to attain the degree of perfection which he aspired to. In this dilemma, the Town Council have requested the Astronomer Royal to purchase the object-glass, tube, and eye-glasses, from Merz and Co. of Munich. The mounting which the Astronomer Royal proposes to adopt is that sometimes called the English mounting,—namely, a transit telescope between two supports, which, united at the top and bottom, form the polar axis. Mr. Airy considers that by making these supports of sheet-iron, having a section something like that of a chord and arc, he will get great stiffness and solidity with moderate weight, and at a comparatively small expense. When thus equipped, the Liverpool Observatory will doubtless furnish a plentiful supply of valuable extra-meridional observations. This excellent institution will then be a model of what the science may reasonably look for at the hands of those who attend to the interests of navigation at the principal ports,—namely, ample means of giving time and regulating chronometers, with power to the superintendant of employing the time he has to spare about that class of observations in which a private observer can make himself really useful to the progress of the science.

It was mentioned in last year's Report that Mr. Sheepshanks had undertaken to finish the construction of the standard yard measure commenced by Mr. Baily. On examining the state of progress (for Mr. Baily's last illness seized him when he had hardly commenced his operations) and the points which were left not satisfactorily determined by preceding measures, Mr. Sheepshanks, with the consent of the Committee, resolved upon a more extensive series of experiments than was at first contemplated. It was found by the officers of the Ordnance survey, and by Mr. Simms when repeating the operation, that the hypothesis of equal expansions of metals for equal increments of temperature, as shown by the mercurial thermometer, is far from exact. Mr. Baily's preliminary measures also showed that the differences between certain standards had varied sensibly since the time when he constructed the standard scale of the Society. The method of properly supporting a bar has been recently considered by the Astronomer Royal, in a paper contained in our newly-published volume. This is not a proper place or time for entering upon the details of processes which must be tried before they are approved of. The apparatus is well-advanced, and the objects proposed are, to ascertain the best material for a measure, with the law of expansion of the material; to perpetuate the measure itself, although the material may undergo slow changes from molecular action; and to leave commencing data for a more complete inquiry into the subject, to be made in another generation. Mr. Sheepshanks, up to this time, sees no reason to doubt of success, though in such delicate researches difficulties and anomalies are pretty sure to present themselves. So far as he is able he will tread in Mr. Baily's steps, faithfully recording everything as it presents itself, without hypothesis, and having recourse to every available source of information. The Fellows of this Society, well-aware as they are of the perfect familiarity with every class of instrumental means which Mr. Sheepshanks possesses, will look upon his labours as they proceed with no ordinary interest.

The circumstances relating to the discovery of the planet Astræa were detailed in the last number of the Society's Monthly Notices, and it will therefore be unnecessary to detain the meeting by any lengthened remarks on the subject. When the planet was first observed in this country it shone with the brilliancy of a star of between the 9th and 10th magnitudes. At the end of December a very material decrease in its brightness had occurred, and at the commencement of the present month the planet could hardly be reckoned brighter than the 11th magnitude. There is a hazy, ill-defined appearance about Astræa, which makes it a matter of no great difficulty to distinguish her, even should there be several small stars in the field with her. Professor Encke was the first to calculate the elements of the new planet; his second elements were marvellously exact, though based on observations extending over thirteen days only. There is a very near agreement in the results of different astronomers, which shows that we have already closely approximated to the orbit of the planet.

In duly acknowledging the service rendered to astronomy by this discovery, we must not forget that we owe much to the exertions of Professor Encke, who detected the planet again on the 14th of December, and observed it very carefully at every opportunity. On these observations is based M. d'Arrest's excellent Ephemeris, which has been of great assistance to astronomers during the past month.

Since the last Anniversary of the Society three comets have been discovered, and the periodical comets of Encke and Biela have returned agreeably to prediction.

Father de Vico, director of the Observatory at Rome, and one of the Associates of the Society, detected a telescopic comet on the night of February 26, 1845. It did not become visible to the naked eye, and was not observed after the end of April.

At the beginning of June a fine comet made its appearance in the constellation Perseus. It was followed by astronomers until the end of the month, when it was lost in the twilight. There is strong evidence in favour of the identity of this comet with that of 1596, observed by Tycho Brahé; an elliptical orbit of 249 years' period satisfies the observations very well. Father de Vico saw Encke's comet on two days in the month of July, but it does not appear that other astronomers were so fortunate.

The comet of Biela was found at Berlin with the great Fraunhofer refractor on Nov. 29, and at Cambridge, by Professor Challis on Dec. 1. At this time it was excessively faint, but the agreement between the observed places and the positions given by Santini's elements is very satisfactory. In the middle of last month the comet became double and presented a most singular appearance, for the nebulosities surrounding the two nuclei scarcely joined each other, and the whole object exactly resembled two comets in very close proximity. This phænomenon was first observed by Professor Challis, with the Northumberland telescope, on Jan. 15. It was, however, seen in America on the 13th. As viewed in Mr. Bishop's refractor on Jan. 24, the comet was much like the *dumb-bell* nebula in Vulpecula seen under small optical power. The distance between the two comets has been since rapidly increasing, and the deviations of the positions of both comets from those deduced from Santini's Elements are now very great.

On the 24th of January last, Father De Vico discovered a telescopic comet in Eridanus; this is the third comet detected by our indefatigable associate. On the subject of comets the Council received a letter from our distinguished Associate Professor Schumacher, dated May 5, requesting that the observations made in England might be immediately forwarded to him, and promising a like return. The Council accordingly directed that all observations which came into their hands should be immediately sent to M. Schumacher; and they hope that those Fellows who observe comets will lose no time in communicating their observations through Mr. Hind, who has undertaken to forward them to Professor Schumacher.

The return of Mr. Piazzi Smyth to Europe enables the Council to furnish the meeting with a short account of Mr. Maclear's geodetical operations.

The anomalies known to exist in Lacaille's southern arc, and which Colonel Everest, when he visited the scene of operation, suspected to have arisen from local attraction on the plumb-line, have been the means of leading Mr. Maclear into a very extensive measurement of the same kind, of which, in such a report as the present, we can only give a passing notice. When, in 1837, Lacaille's Observatory at his southern end was trigonometrically connected with the Royal Cape Observatory, there was found to be error in the latitude of the former, certainly of the kind which the local attractions would cause, but by no means sufficient in amount to explain all the difference between the theoretical and the measured degree. But even this partial explanation was destroyed by Mr. Maclear's subsequent measures with the zenith sector at both ends of Lacaille's arc, which produced results agreeing (as to the length in the heavens of that arc) almost exactly with that of Lacaille. The discordance being thus thrown upon the trigonometrical part of the operation, Mr. Maclear (1840–41) carefully measured a base of 42,000 feet nearly on the site of Lacaille's and then re-observed all his triangles, feeling confident that the former stations had been recovered in every instance to within a few feet. The length of the degree thus found halved the difference between Lacaille and the modern state of the theory, being about 200 feet less than that of the former, and about as much more than that of the latter. But, the old triangles being but ill-chosen for causing errors of observation to produce their least effects, Mr. Maclear took another and a more advantageously related system, and found a result agreeing almost exactly with his former one. Still the effect of attraction at the southern end only remained imperfectly compensated, and that at the northern not at all. Mr. Maclear then resolved to extend the arc to the length of the usual European ones, to place the terminal stations in the best positions which could be found, to swing the pendulum on every mountain, and to fix the positions of all such points as might be useful in future land or coast surveys. This work has occupied him during the course of the years from 1843 to the present time, and it is not yet completed. Either of four stations may be used independently at the extreme southern end of the arc—the Royal Observatory, Lacaille's Observatory in Cape Town, one on the mountain Zwarte Kop, twenty miles south of the observatory, and one on Cape Point, the extreme south-west of Africa. The most northern station is on the Khamiesberg, giving a total length of about 4½ degrees, with an intermediate astronomical station on the top of Heer Logiments-berg. From the extreme south to the middle station two sets of triangles are formed; from thence to the northern, one and a part of another. From the Royal Observatory extends an eastern chain of triangles, for the settlement of the position of Cape l'Aguillhas and the measurement of an arc of parallel. The sides of the triangles vary from 50 to 90 miles. The country to the north of Khamiesberg is now under examination, to try the feasibility of carrying the triangulation up to a station at which a perfectly unexceptionable latitude can be obtained. The physical difficulties of the northern part of the triangulation have been enormous. The houses at 20 miles distance from each other—the natives themselves imperfectly, and sometimes not at all, acquainted with the mountains through which the surveyors had to explore difficult passes in most inclement weather—the difficulty of finding water, and the scanty quantity of it when found—the irksomeness and danger of carrying the instruments up ascents which a free and active man can only surmount with difficulty—the endurance of all temperatures, from sand in the plains at 140° of Fahrenheit to ice and sleet on the heights—and the possibility of return being cut off by the gathering of snow in the ravines, —present a picture far from inviting, and form a measure of the resolution of Mr. Maclear and his assistants, as well as of the strength of the principle which carried them into those wilds. The Society will join the Council in hoping that their success may be equal to their desert.

The President (Capt. W. H. Smyth, R.N.) then addressed the Meeting on the subject of the award of the Medal, as follows:

The Report, Gentlemen, which you have just heard, has acquainted you that your Council have awarded the Medal of this Society to Mr. Airy, the energetic Astronomer Royal, for his reductions of the Planetary Observations made at Greenwich between the years 1750 and 1830, by which an immense magazine of dormant facts, contained in the annals of the Royal Observatory, are rendered available to astronomical use. It may be proper, in expressing the grounds of this adjudication, to allude to the extensive nature and elevated character of that extraordinary work, as well as to the peculiar incidents connected with its production, which have brought it within the recognition of your Council.

It is, of course, understood, and has always been acted upon, that work, however excellent and useful, does not enter into competition when it only follows the necessary duty of the author. Our medal was primarily instituted as a mark of approbation on individual exertion, on labours of love; and not to note our sense of the official merits of public men, or of the rectitude and ability with which they may acquit themselves in their respective offices. Now the weighty reductions in question come before us as executed, at the expense of her Majesty's Government, by the Astronomer Royal. It remains, however, to be added, that the undertaking was proposed by that distinguished individual long before his appointment to Greenwich. After his attention had been particularly called to the planetary theory, by his taking charge of the Cambridge Observatory,—having already investigated the errors of the Solar Tables and the long inequality due to Venus,—he saw the immediate necessity of a complete computation of all the older *exact* observations, which is nearly equivalent to saying of those made at Greenwich since the erection of the new transit instrument by Bradley in September 1750, and no others. The importance of this object does not seem to have struck the authorities on Mr. Airy's first proposal; but after the British Association was formed, a deputation of that body waited upon the Chancellor of the Exchequer in 1833, and obtained his consent to defray the necessary expenses. Mr. Airy's offer was, that if the Government would meet the cost of reduction and printing, he would undertake the entire preparation and supervision of the work gratuitously; so that he undeniably has the merit of originating, pressing, carrying the proposal into successful operation, and bringing it to a most satisfactory conclusion, almost solely and exclusively, and without any pecuniary advantage or official call. The publication of the Reductions was ordered by the Lords Commissioners of the Admiralty, in consequence of a recommendation made to them by the Board of Visitors of the Royal Observatory of Greenwich, on the 25th of June, 1841.

This is the real origin of the reductions, and we find displayed in the execution of the task qualities of a higher order than that mere zealous and patient perseverance so often consumed in the mechanical department of computation. For raised as he was by his own talents and application to an almost unrivalled height over the field of astronomy, Mr. Airy was not only able to take in at one view the actual existing state of the science, but could also look back and trace all the wanderings of former inquirers, each proceeding in his own narrow foot-path according to his own peculiar views of the various constants of refraction, nutation, aberration, and other elements of reduction. Through this wilderness he cut a broad highway, connecting all those separate labours, and making each of them serviceable to the common end; as when a drain is cut through a marshy soil, and the waters of numberless useless pools are collected and led into one straight channel, which carries the improver through the before impracticable region, ready to apply his whole energy on the space beyond. But although much of this may be said of the Greenwich documents, still the original observations were so fairly and fully stated, and are so completely preserved, that they now admit of reduction with the same exactness as at the time when they were made. Still it is by no means a simple task to make one's self conversant, not only with the merits, but with all the peculiarities, the weaknesses, and the failings of both instruments and observers for a period of eighty years, as the Astronomer Royal has done.

The contents of this precious work are comprised in a quarto volume of above 700 pages, excellently printed, and divided into five sections. The first is devoted to the ascertaining of the clock errors, comprising the intervals of the wires, corrections for imperfect transits, and the position of the instrument, the effects of personal equation, and the rules by which the fundamental and determining stars were chosen; the second section contains the investigation of index errors and zenith points of the mural quadrants and circles; the third exhibits the computation of the geocentric places of the planets, as deduced from actual observations; the fourth shows the computation of the geocentric places of the planets, from the best existing tables of each of the bodies; and the fifth section gives the comparison of the observed with the tabular places, and expressions for the discordances in terms of the heliocentric errors of the earth and planets. These sections are briefly yet luminously described in an introduction of only thirty-four pages: but, though short, it would be difficult to name another specimen of astronomical writing of equal clearness and excellence. There are then twenty pages of the errata detected, either in the published volumes of the Greenwich Observations, or in the fundamental tables on which the planetary computations are founded; and the book concludes with nineteen skeleton forms of the schemes employed in reducing the matter. The whole is so lucidly and methodically arranged, that any one with a very moderate amount of elementary knowledge can take up an observation, and conduct the process to the end; for he will be able to check every important step in the result by a reference to the volume itself, to the formulæ, and to the skeleton forms.

But if I may venture to express my opinion on this point, I would add, that it is very improbable that a casual examination of the steps of the processes should produce any useful result beyond that of gratifying a laudable curiosity, or familiarising the student with calculations and investigations of this kind. In all cases of doubt, the original documents have been consulted, and every effort which great experience and singular acuteness could suggest, has been made to clear up the mistakes which will always be found in large quantities of observation. The mode of exhibiting the results is a great safeguard against errors of computation, and in passing through the press every portion of the work has been subjected to a most scrupulous revisal. The advantage of accuracy need not be insisted upon here, and I feel confident that the present book approaches in this respect as near perfection as, humanly speaking, that limit can be attained.

Such is the work which your Council have considered it their duty thus to appreciate. The masses of results obtained by this operose application of thought and labour may be likened to the extraction of ore from the dross of an ancient furnace; and I am gratified in having already had the satisfaction of publicly expressing my high sense of the vast importance of the task. Every stage of the undertaking has stamped the Astronomer Royal as being fully in command of the whole process; and in exhibiting the rare union of large conception, profound learning, and pains-taking practice, prove him worthy of being the leader of the astronomical establishments of the kingdom. From the well-directed exertion of such talents the scientific world now reaps the benefit of the enormous calculations before us, the whole utility of which cannot yet, probably, be specifically stated. But we know, that whatever improvement may be made in the theory or constants of any one of the planets, or of the earth, the effect will be shown in its corresponding influence on the heliocentric places of the earth and planet. Now, the equations which connect the variations of these elements with errors found by observation are all tabulated and grouped ready for the geometer's use, without requiring him to look beyond the fifth section, and with a degree of precision which no single computer could ever hope to arrive-at. And as in botany we assume a new æra from the time when Linnæus reduced the disjointed facts and instances of his day into a methodical system, by which men were able to make themselves masters of every truth which had been discovered up to that period, and turn their attention to something certainly new and useful; so may we, perhaps, date a new epoch in planetary astronomy from the appearance of the great work before you, since the desiderata in this department of the science are rendered visible, and the necessity for exact instants, a greater refinement of methods, and more delicate observations made apparent.

The following Fellows were elected Officers and Council for the ensuing year, viz.—

President: Captain W. H. Smyth, R.N., K.S.F., D.C.L., F.R.S. —*Vice-Presidents*: Samuel H. Christie, Esq., M.A., F.R.S.; Bryan Donkin, Esq., F.R.S.; Thomas Galloway, Esq., M.A., F.R.S.; Rev. Robert Main, M.A.—*Treasurer*: George Bishop, Esq.—*Secretaries*: William Rutherford, Esq.; Rev. Richard Sheepshanks, M.A., F.R.S.—*Foreign Secretary*: Sir John F. W. Herschel, Bart., K.H., M.A., F.R.S.—*Council*: G. B. Airy, Esq., M.A., F.R.S., Astronomer Royal; Solomon M. Drach, Esq.; Rev. George Fisher, M.A., F.R.S.; John Russell Hind, Esq.; Manuel J. Johnson, Esq., M.A.; John Lee, Esq., LL.D., F.R.S.; Augustus De Morgan, Esq.; Edward Riddle, Esq.; William Simms, Esq.; Lieut. William S. Stratford, R.N., F.R.S.

XXVI. *Intelligence and Miscellaneous Articles.*

ON AN IMPROVED METHOD OF DETECTING ALUMINA.

BY MR. J. C. NESBITT.

THE process depends on the insolubility of the phosphate of alumina in acetic acid, and the two following experiments will show the advantages of this test:—

1st. Two grs. of alum were dissolved in 1000 grs. of water, and a single drop of this solution was mixed with a little solution of phosphate of soda, to which acetate of ammonia and acetic acid had been added; in a few minutes a precipitate of phosphate of alumina was produced.

Another drop was mixed with ammonia, and the usual tests were employed to detect alumina, but no precipitate was produced.

Some ordinary solution of potash was mixed with a strong solution of muriate of ammonia. The mixture showed no change after the lapse of two days.

Another portion of the same solution of potash was mixed with a solution of muriate of ammonia, containing a few drops of solution of phosphate of soda. A precipitate was produced in the course of twenty minutes or half an hour.

In order to detect alumina in the ashes of plants, or in substances containing similar ingredients, it is merely necessary to precipitate the oxide of iron (if any be present) and the alumina as phosphates, by the addition, if necessary, of phosphate of soda, acetate of ammonia and acetic acid. This precipitate is to be boiled with a solution of pure caustic potash; the phosphate of alumina will be dissolved, and may be again precipitated by muriate or acetate of ammonia and acetic acid.—*Proceedings of the Chemical Society*, part xvii.

ON A NEW SUBSTANCE FROM COCHINEAL.

At the Meeting of the Chemical Society for March 16, 1846, Mr. Warren De la Rue described a new body which he had obtained from cochineal, a subject he has been for some time past investigating, and which bears a remarkable similarity to a substance which Liebig has lately produced by the action of potassa on caseine, to which he assigns the composition C_{16}, N, H_9, O_5, at the same time stating that the formula requires confirmation. Though the analyses of the new substance differ somewhat from this formula (its composition appearing to be C_{18}, N, H_{11}, O_6), yet the agreement of its properties with those assigned by Liebig to the substance described by him, leaves but little doubt as to the identity of the two bodies. A specimen of Liebig's substance, furnished by Dr. Hofmann, agrees perfectly in its physical characters.

The new body is obtained from cochineal by the following means:—The colouring principle being first separated from an infusion of cochineal, the mother-liquor is to be carefully evaporated in a water-bath to the consistence of a syrup, when there appears floating in it a small quantity of granular chalky-like masses, which being collected on a filter is kept warm, and, when drained, well-washed with cold water; they are then dissolved in boiling water and recrystallized; again well-washed, and finally dissolved in as small a quantity of boiling water as possible; a little animal charcoal is to be added, and the ebullition continued for a short time; on filtration and cooling, the new body crystallizes as a bulky assemblage of tufts, filling the vessels; on drying they form into paper-like masses, of a perfectly white colour and a beautiful silky lustre.

This body is sparingly soluble in cold water, considerably more so in hot water; soluble in ammonia, from which it crystallizes as the ammonia is driven off by heat; it is likewise soluble in acids.

From the process pursued in separating this substance from cochineal there is no doubt that it pre-exists, and is not produced by the operation; it may, however, be a product of oxidation of some part of the insect during its preparation for commerce. Three hundred parts of cochineal yield one part of the new body.

SULPHUROUS ÆTHER.

This product was obtained by MM. Ebelmen and Bouquet: by pouring absolute alcohol on protochloride of sulphur, heat is disengaged, with an abundant production of hydrochloric acid and deposition of sulphur; the alcohol is to be added till all action ceases, the mixture is then to be distilled; there passes at first at about 176° Fahr., an abundant product, which is merely alcohol containing much hydrochloric acid; the temperature of the liquid afterwards rises rapidly; it becomes clear by the fusion of the sulphur which it contained in suspension, assumes a reddish-brown colour, and yields, between 302° and 338° Fahr., a product which is to be separately received. There remains in the retort a considerable deposit of fused sulphur. The liquor distilled between 302° and 338° was twice rectified and yielded a limpid colourless product, of a peculiar æthereal odour, somewhat resembling that of mint; its taste is at first cooling, afterwards burning, and there is left an after-taste of sulphur; its boiling-point is stationary at 320°.

According to M. Pierre, the density of sulphurous æther is 1·085 at 60° Fahr. and 1·106 at 32°. It mixes with alcohol in all proportions; water decomposes the mixture, redissolving the deposit very slowly, and exhaling the odour of sulphurous acid. Æther which had been prepared for some time and kept in badly stopped bottles, is similarly decomposed by the moisture of the air. An alcoholic solution of potash dissolves this æther rapidly. The liquor diluted with water gives no further precipitate, and when saturated with hydrochloric acid, a strong smell of sulphurous acid is produced. Sulphurous æther does not burn when exposed to a body in combustion, unless it is previously made hot. Its flame is bluish, accompanied with a strong sulphurous odour.

In analysing this æther, the sulphur was determined by dissolving a known weight of it in an alcoholic solution of potash; the solution was diluted with water, boiled to expel the alcohol, and then saturated with chlorine, which converted the sulphurous into sulphuric acid; the liquor after being rendered acid with hydrochloric acid was precipitated by chloride of barium; the hydrogen and carbon were determined in the usual manner.

It appeared to be composed of—

Sulphur	23·18	200·0
Carbon	34·77	300·0
Hydrogen......	7·28	62·5
Oxygen	34·77	300·0
	100·00	

Its formula is $SO^2 C^4 H^5 O$.

In the state of vapour calculation gives—

2	volumes of	Sulphurous acid ..	4·424
4	...	Carbon	3·318
10	...	Hydrogen	0·691
1	...	Oxygen	1·106

$$\frac{9{\cdot}539}{2} = 4{\cdot}769$$

The formula $SO^2 C^4 H^5 O$ represents, therefore, 2 volumes of vapour, the same as carbonic æther, $CO^2 C^4 H^5 O$. It is to be observed that carbonic acid and sulphurous acid have corresponding formulas, and both contain a volume of oxygen equal to their own.—*Ann. de Ch. et de Phys.*, Mai 1846.

ON THE PREPARATION AND PROPERTIES OF DIGITOLEIC ACID AND ITS SALTS.

M. C. Ph. Kosmann prepared this acid from the *Digitalis purpurea* by digesting the leaves of the plant in cold water and treating the solution with tri-acetate of lead slightly in excess; the precipitate obtained was boiled for a quarter of an hour with a solution of carbonate of soda; the filtered solution, which was of a brown colour, was treated with dilute sulphuric acid; the flocculent precipitate obtained was perfectly washed and dried, and treated with alcohol; the alcoholic solution left by evaporation a residue, which became a crystalline mass; this was six times treated with cold æther, which dissolved a green oil that remained after the evaporation of the æther. This last left a substance A, undissolved. The green oil becomes a granular crystalline mass, and is covered with groups of small stellated needles of a green colour, which have a bitter acrid burning taste, and occasion headache; the odour is aromatic and not disagreeable; it melts at 86° Fahr., forming greasy spots on paper; it is but slightly soluble in water, but dissolves readily in alcohol and in æther. In order to obtain it pure, it is to be combined with bicarbonate of soda in solution, which dissolves it, then precipitated by acetic acid, carefully washed, treated with æther, and to be evaporated; the alcoholic solution reddens litmus paper, and decomposes the carbonate of potash with the effervescence of carbonic acid gas.

The facts detailed prove that this oily substance is a true acid, to which the author has given the name of *digitoleic acid*; and he attempted to determine its equivalent by means of the salts of lead and barium, procured by the double decomposition of the neutral salt of potash, and the chloride of barium and nitrate of lead; the barytic salt was converted by calcination into carbonate, and the salt of lead into sulphate. Four operations carefully conducted gave 4153·895 as the mean atomic weight of the digitoleic acid.

The salt of lead consists of—

Digitoleic acid....	74·866
Oxide of lead	25·134
	100·000

and the barytic salt is composed of—

Digitoleic acid	81·28
Barytes	18·72
	100·00

Several salts of the new acid were prepared and examined by M. Kosmann.

Digitoleate of Potash.—The acid was treated cold with bicarbonate of potash and water; the solution is slowly effected, but takes place in a few hours with the slow disengagement of carbonic acid; the filtered solution was evaporated to dryness by the water-bath, and the residue tested with cold alcohol of 85 per cent., which dissolved the potash-soap; the alcoholic solution filtered and evaporated left a greenish-brown residue, which became a confused crystalline mass intermixed with needles, the odour of which was aromatic, and the taste bitter and acrid; it does not effervesce with acid, and yields by the action of heat an odorous matter and carbonate of potash. The aqueous solutions froth like soaps.

Digitoleate of Soda.—This salt was prepared like the preceding, with bicarbonate of soda; the alcoholic solution left by evaporation a scaly mass of a brownish-green colour, without any trace of crystallization; the aqueous solution, frothed by agitation, does not effervesce with acid, and has a bitter acrid taste. This soap is soluble in æther, and by the action of heat emits an odorous vapour, and yields a residue of carbonate of soda.

Digitoleate of Morphia was prepared by the double decomposition of digitoleate of potash and acetate of morphia. The properties of this salt are, that it is a greenish precipitate, which adheres strongly to the sides of the vessel. After repeated washing, it was treated with alcohol, which dissolved it and formed a bright green solution, that deposited by slow evaporation fine flattened rectangular prisms with square bases, the colour of which was greenish with a metallic lustre; they were insoluble in water.—*Journ. de Chémie Médicale*, June 1846.

ON A REMARKABLE FELSITE FROM MARIENBERG.

BY A. BREITHAUPT.

This felsite occurs in veins of tinstone, traversing gneiss at Martersberge, and at Wilsberge near Marienberg in Saxony, accompanied by quartz, which appears to be a more ancient formation, and tinstone, which is more recent. It was long taken for brown spar (dolomite), for it is red or reddish-white in crystals, and presents in its combinations P, *x*, T, and *l*, so that the last three forms, with nearly equally extended surfaces, resemble a rhombohedron.

Freshly broken, the crystals are semi-transparent, cleavage rectangular, in the direction of the faces P and M; hardness 7¾ (of Breithaupt's scale); specific gravity 2·441 to 2·445.

Decomposed by hydrofluosilicic acid, M. Kröner obtained as its chemical composition,—

Silica	66·43
Alumina	17·03
Oxide of iron	0·49
Potash	13·96
Soda	0·91
Lime	1·03
Magnesia and manganese	traces

Poggendorff's *Annalen*.

CHEMICAL ANALYSIS OF COPPER BLENDE. BY C. F. PLATTNER.

Breithaupt's copper blende has recently been said to be identical with tennantite. The former however is distinguished by its red streak and lower specific gravity. In tennantite from Cornwall, Kudernatsch found—

Sulphur	27·76
Arsenic	19·10
Copper	48·94
Iron	3·57
Silver	trace
Quartz	0·08
	99·45

The chemical formula for which is—

$$\left.\begin{matrix}(Fe\,S)^4\\(Cu\,S)^4\end{matrix}\right\}As_2\,S_3+2(Cu_2\,S)^4,\,As_2\,S_3,$$

or more simply, according to M. Frankenheim,

$$\left.\begin{matrix}(Cu\,S)^4\\(Fe\,S)^4\end{matrix}\right\}As_2\,S_3,$$

Copper blende from the Prophet Jonas Mine, Freiberg, I found to contain—

Sulphur	28·111	201·16 = 0·139	= 139
Arsenic	18·875	470·04 = 0·040	= 40
Copper	41·070	395·69 = 0·104	= 104
Zinc	8·894	403·23 = 0·022	= 29
Iron	2·219	339·21 = 0·007	
Lead	0·341		
Antimony and silver	traces		
	99·510		

If we consider, with M. Frankenheim, Cu_2S to be isomorphous with ZnS and FeS, the following simple formula results:—

$$\left.\begin{matrix}(Cu_2S)^4\\(Zn\,S)^4\\(Fe\,S)^4\end{matrix}\right\}As_2S_3.$$

Copper blende essentially differs from tennantite in having a portion of Cu_2S replaced by Zn S.—Poggendorff's *Annalen*.

LOXOCLASE (*FELSITES LOXOCLASIUS*), A NEW MEMBER OF THE FELSITE GENUS. BY A. BREITHAUPT.

Amongst a number of North American minerals, for which I have to thank Mr. Shepard, were several of the genus Felsite, and one in particular which at first sight I took for pegmatolith, but which turns out to be a new and remarkable species. This mineral in many respects resembles oligoclase: I have therefore, in the following description, pointed out those peculiarities which render it distinct. Lustre, between vitreous and greasy; pearly on the most perfect cleavage-surface.

Orthoclastic.—Primary form, oblique rhombic prism, $+P\overline{\infty}(P) = 63°\ 38'$ towards the chief axis; $-P\overline{\infty}(x) = 65°\ 37'$ towards the chief axis; $P:y=3x=99°\ 45'$; $P:T=112°\ 30'$; $P:l=112°\ 50'$; $P:M=90°\ 0'$ (oligoclase $P:M=86°\ 45'$); $M:T=120°\ 15'$; $M:l=119°\ 25'$; $T:l=120°\ 20'$. Cleavage, perfect hemidomatic; very distinct in the direction of the short diagonal; indistinct approaching distinctness in the direction of the long diagonal; hemiprismatic, in fragments. Fracture, uneven to conchoidal and hackly. Hardness = $7\frac{1}{2}$ to $7\frac{3}{4}$ (of Breithaupt's scale) (oligoclase $7\frac{3}{4}$ to $8\frac{1}{2}$). Specific gravity = 2·609 to 2·620 (oligoclase 2·644 to 2·662). Colour, yellowish-gray, yellowish-white, pea yellow, bluish-gray. Translucent in thin laminæ to transparent. The oblique cleavage in the direction of the long diagonal is characteristic of loxoclase, although not always very distinct, hence its name. Its specific gravity is also higher than any other of the orthoclastic felsites. It appears subject to decay by exposure to the atmosphere.

Two analyses performed by Prof. Plattner; the one by means of hydrofluoric acid (*a*), the other by carbonate of soda (*b*); the mean of the two (*c*) showed it to possess the following composition:—

	a.	*b.*	*c.*
Silica		63·50	63·50
Alumina	20·48	20·10	20·29
Oxide of iron	0·65	0·70	0·67
Potash......................	3·03		3·03
Soda	8·76		8·76
Lime	3·40	3·04	3·22
Magnesia....................	trace	trace	
Water and fluoride of silicon..		1·23	1·23

for which the general formula is $RO, SO_3 + R_2O_3, (SO_3)^3$, as also for oligoclase. Loxoclase affords therefore a remarkable example of dimorphism, for oligoclase is plagioelastic, and yet both must be classed under the same genus. Loxoclase fuses before the blowpipe with difficulty (fusibility between adular and labrador), and shows in the outer flame an intense soda reaction. (Oligoclase melts pretty easily.) Heated in a small glass bulb it gives out a little water and fluoride of silicon, and is very imperfectly decomposed by hot muriatic acid. Loxoclase occurs with pyroxen, graphite, and calcareous spar, upon the two former covered by the latter, at Hammond, in the State of New York.—Poggendorff's *Annalen*.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1846.

Chiswick.—June 1, 2. Slight haze: cloudless. 3. Slight haze: very dry air: clear and fine. 4—6. Hot and dry. 7. Sultry. 8, 9. Cloudy and fine. 10. Overcast. 11, 12. Very fine. 13, 14. Hot and dry. 15. Cloudless. 16—18. Hot and dry, with slight haze. 19. Foggy: excessively hot: clear at night. 20. Hot and sultry. 21. Uniformly overcast: fine. 22. Sultry: excessively hot: rain: at night thunder, lightning, and heavy rain. 23, 24. Densely clouded. 25. Fine. 26. Overcast: heavy showers. 27, 28. Cloudy and fine. 29. Fine, with clouds: windy: clear at night. 30. Cloudy and fine: overcast.

Mean temperature of the month	66°·63
Mean temperature of June 1845	62 ·14
Average mean temperature of June for the last twenty years	60 ·88
Average amount of rain in June	1·88 inch.

Boston.—June 1. Cloudy. 2. Fine: half-past 11 o'clock A.M. thermometer 75°: 2 o'clock P.M. 78°. 3. Fine: 3 o'clock P.M. thermometer 80°. 4. Fine. 5. Cloudy. 6. Fine: quarter-past 2 o'clock P.M. thermometer 82°. 7. Fine. 8. Cloudy: lightning A.M. 9, 10. Cloudy. 11—17. Fine. 18. Fine: quarter-past 11 o'clock A.M. thermometer 80°. 19. Fine. 20. Cloudy. 21. Fine. 22. Cloudy: rain P.M., with thunder and lightning. 23. Rain: rain early A.M.: rain P.M. 24. Fine. 25. Fine: rain and hail, with thunder and lightning P.M. 26—28. Cloudy. 29, 30. Fine.—The past month has been considerably warmer than *any* month of my observations.

Sandwick Manse, Orkney.—June 1—3. Fine. 4. Fine: bright: fine. 5. Fine: bright: cloudy. 6. Damp. 7. Damp: cloudy. 8. Cloudy. 9. Damp: fog: cloudy. 10. Cloudy: rain. 11. Bright: rain. 12, 13. Bright: clear. 14, 15. Bright: cloudy. 16. Bright: clear. 17, 18. Fine. 19. Thunder and hail*: cloudy. 20. Clear: fine. 21. Clear. 22. Cloudy: fog. 23. Rain and thunder: thunder and rain. 24. Drizzle: thunder and drops. 25, 26. Clear. 27. Rain: clear. 28. Bright: cloudy. 29. Bright: drops. 30. Bright: showers.

Applegarth Manse, Dumfries-shire.—June 1—6. Very fine. 7. Very fine: thunder. 8. Fine soft rain. 9, 10. Slight shower. 11. Fair, but cloudy. 12—16. Fair and fine. 17. Fair and fine: warm. 18. Fair and fine: thunder. 19. Slight drizzle: thunder. 20. Dry and withering. 21. Very warm and withering. 22. Very warm: showers: thunder and hail. 23. Very heavy rain. 24, 25. Showers: fair P.M. 26—29. Heavy rains. 30. Very heavy rain.

Mean temperature of the month	63°·2
Mean temperature of June 1845	56 ·5
Mean temperature of June for twenty-three years	55 ·7
Mean rain in June for eighteen years	3 inches.

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* The most severe thunder-storm ever remembered: one man was killed, others knocked down, and the lightning struck various places. It was at its height between six or seven o'clock A.M.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. June.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·187	30·153	29·66	30·10	30·11	30·14	30·13	79	46	62	76	45	56	54	e.	calm	sw.	sw.	......	......	......	......
☽ 2.	30·210	30·188	29·71	30·10	30·11	30·12	30·19	82	43	70	77½	50	68	61	e.	calm	sw.	sw.	......	......	......	......
3.	30·216	30·177	29·65	30·16	30·12	30·18	30·13	85	45	73	80½	51	60	59	e.	calm	sw.	e.	......	......	......	......
4.	30·178	30·159	29·60	30·10	30·09	30·14	30·09	83	49	74	80	55½	68	57	e.	calm	sw.	se.	......	......	......	......
5.	30·143	30·115	29·56	30·08	30·04	30·03	30·10	84	49	73	80	51	75	56	e.	calm	se.	w.	......	......	......	......
6.	30·084	30·035	29·48	30·00	29·98	30·04	30·06	89	53	76	77½	53	56	56	se.	calm	sw.	e.	......	......	......	·01
7.	29·978	29·883	29·33	29·95	29·73	30·07	30·00	90	56	78	80½	56½	55	53	sw.	calm	nne.	se.	......	......	......	·04
8.	29·871	29·806	29·29	29·80	29·71	29·91	29·80	79	50	63·5	65½	56	55½	53	sw.	calm	s.	se.	......	......	0·00	......
○ 9.	29·835	29·772	29·19	29·67	29·67	29·68	29·60	77	51	64	69	52½	56	56	sw.	calm	s.	s.	......	......	......	......
10.	29·995	29·912	29·34	29·70	29·68	29·65	29·64	77	56	67	67½	57	60	53	sw.	w.	s.	se.	......	......	......	·02
11.	30·195	30·149	29·53	29·97	30·03	29·83	30·00	81	53	72	66	53	54½	53	w.	w.	s.	w.	......	......	......	·30
12.	30·222	30·126	29·64	30·09	30·05	30·02	29·90	85	50	70	70	47½	60	56	w.	calm	ssw.	sw.	......	......	......	·03
13.	30·096	30·094	29·55	30·04	30·04	29·95	30·01	86	52	73	70	56	57	55	ne.	calm	w.	sw.	......	......	......	......
14.	30·106	30·074	29·47	30·07	30·04	30·04	30·07	82	50	77	75	55	56	52½	e.	calm	w.	sw.	......	......	......	......
15.	30·243	30·155	29·52	30·12	30·18	30·10	30·20	87	56	72	75	52	58	54	e.	calm	wnw.	w.	......	......	0·15	......
☾ 16.	30·320	30·289	29·64	30·25	30·25	30·26	30·29	87	50	77·5	79	50	56	54	e.	calm	se.	......	......	......	......	......
17.	30·314	30·243	29·65	30·24	30·20	30·26	30·23	88	53	76	83	51½	69	61	e.	calm	se.	w.	......	......	......	......
18.	30·210	30·145	29·59	30·16	30·09	30·21	30·12	88	54	76	83	58	70	62	se.	calm	sw.	se.	......	......	......	......
19.	30·106	30·054	29·45	30·04	30·04	30·03	30·22	93	53	75	82	59½	63½	50½	se.	calm	sw–n.	nnw.	......	......	......	·22
20.	30·181	30·087	29·52	30·18	30·22	30·32	30·30	87	62	68	69½	57½	54	49	e.	n.	ne.	e.	......	......	......	......
21.	30·190	30·061	29·65	30·16	30·02	30·20	30·05	81	56	68	77	48	57	46½	e.	calm	e.–s.	se.	......	......	......	......
22.	29·957	29·649	29·40	29·88	29·70	29·96	29·78	93	59	66	81	56	58½	56½	se.	calm	sw.	se.	·59	......	......	·18
● 23.	29·644	29·577	28·87	29·40	29·30	29·61	29·27	69	48	66	65	56	58	57	sw.	calm	w.	e.	·05	·32	0·10	·17
24.	29·517	29·401	28·95	29·22	29·30	29·12	29·29	67	48	64	61	48	54	53	sw.	w.	w.	......	......	·13	......	·16
25.	29·643	29·497	28·99	29·39	29·48	29·48	29·65	70	45	62	64	43	59½	53½	w.	calm	ssw.	e.	......	......	......	......
26.	29·681	29·647	29·22	29·48	29·41	29·69	29·67	67	54	63	59	44½	58½	54	sw.	calm	sse.	ese.	·09	·63	......	......
27.	29·797	29·629	29·12	29·44	29·54	29·57	29·62	73	50	65·5	65½	51	55½	54	sw.	calm	ssw.	se.	·01	......	......	·12
28.	29·850	29·800	29·32	29·61	29·43	29·70	29·62	74	58	66	66½	55	60	55½	sw.	calm	ssw.	se.	·03	......	......	......
29.	29·814	29·786	29·21	29·47	29·40	29·46	29·34	76	57	68	64	55	62	56	sw.	w.	sw.	se.	......	......	......	·35
30.	29·978	29·888	29·27	29·49	29·65	29·33	29·36	75	48	67	62	54	62	55	sw.	w.	wsw.	s.	·03	......	4·60	......

THE

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[THIRD SERIES.]

SEPTEMBER 1846.

XXVII. *On the Magnetic Affection of Light, and on the Distinction between the Ferromagnetic and Diamagnetic Conditions of Matter.* By MICHAEL FARADAY, *F.R.S., Foreign Associate of the Academy of Sciences, &c.*[*]

WHEN a ray of polarized light and lines of magnetic force pass simultaneously and parallel to each other through a transparent solid or liquid medium not possessing forces of double refraction, the ray is rotated according to a simple law of action, which I have expressed in the last part of the Philosophical Transactions[†]. When such a ray passes through certain specimens of rock crystal, oil of turpentine, &c., it is also rotated according to a natural law well-known, without any reference to magnetic force. A very striking distinction exists between these two cases of rotation, though they at first appear to be the same; for the former rotation is dependent in its direction upon the lines of magnetic force, and not upon the position of the observer or the course of the ray of light, whereas the latter is dependent upon the position of the observer or the course of the ray.

Upon consideration it appeared that the peculiar character of the magnetic rotation might be made available in exalting the final effect of the magnetic force upon the ray, and also in demonstrating many important points in a more marked manner and higher degree than had yet been possible; and upon referring the idea to experiment, it was found to be true. The following pages contain some of the results.

A parallelopiped of heavy glass 0·7 of an inch square and 2·5 inches long, had the two ends polished and silvered. The silvering was then removed from a space about 0·1 of an inch wide along one of the edges of one end, and also from a corre-

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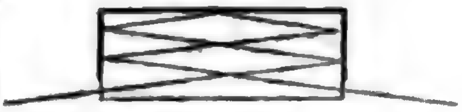
[*] Communicated by the Author.

[†] 1846, part i. pp. 4, 5. [Phil. Mag. vol. xxviii. pp. 298, 299.]

sponding space on the other end, except that the parts cleared were on the contrary sides of the parallelopiped; so that each end was furnished with a good plane reflector, but these overlapped each other (fig. 2). In consequence of this arrangement, a ray of light could be transmitted diagonally across the length of the piece of glass; or the ray, after entering at one end, could be reflected two or more times within the glass and then passed out.

A similar piece of heavy glass was silvered at the two ends and one side of the prism; and the silvering was then removed at the ends for the space of 0·1 of an inch from those edges which were the furthest from the silvered side (fig. 1). A ray of light passing in at the unsilvered part of one end with a certain degree of obliquity, could be reflected at the other end, then at the side, and again at the first end, passing thus three times along the glass and finally out at the second end. At other inclinations the ray would pass five, seven, nine, eleven, or a greater number of times along the glass before it issued forth on its course through the air to the eye of the observer.

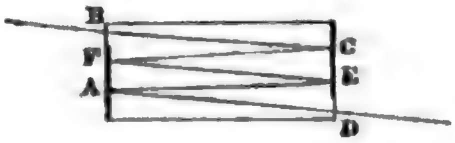
Fig. 1.



Either of these pieces of glass could produce the desired result of repeated reflexions within, but the first form was found most convenient in use. When a strong light was employed, it was not difficult to follow the series of images produced by successive reflexion up to the ninth or tenth image, these corresponding of course to a transit of the ray seventeen or nineteen times along the substance of the glass. A little change of position of the silvered glass between the Nichol's prisms used as the polarising and analysing apparatus, was sufficient to bring any one of these images into view, the glass being at the same time under the full influence of the electro-magnet, or the helix, employed to generate lines of magnetic force. A further advantage is gained if the ends of the piece of glass are not quite parallel to each other, the sides proceeding from the edges where the ray enters and issues forth being in a very slight degree different in length. This arrangement causes the series of reflected images to open out if seen at one end and to close up if seen at the other, and thus the observation of a particular image or the simultaneous comparison of two or more images, is favoured.

On considering the effect of this arrangement, it is evident that if ABCD represent a trough of solution of sugar, or any other body having the ordinary rotating influence over a polarized ray, then a ray sent in at D and passing out at A

Fig. 2.



would be rotated to a certain amount. But if, instead of proceeding onwards at A, it were reflected by the surface A F to E, and were there observed, it would be found to have received no rotation, for the effect produced in going from D to A would be exactly compensated by its return from A to E. Or if the reflexions were made more numerous and recurred at E F and C, so that the ray should traverse the body five times, still an amount of rotation equal only to that which its passage once along the substance could effect would be finally produced.

Such would not be the case if A B C D were a diamagnetic, rotating the ray by means of magnetic force; for then, whichever way the ray was passing, it would still be rotated in the *same* direction in relation to the lines of force. So if observed issuing forth at A, it would have an amount of rotation (which we may call right-handed) equal to what one transit across the diamagnetic could produce; if observed at E, it would have an amount of left-handed rotation double the amount of the first or unit quantity; if observed at F, it would have three times the first amount of right-handed rotation; if observed at C, four times the amount of left-handed rotation; and at B would possess five times the original amount of right-handed rotation.

This was confirmed by the result of an experiment. The great magnet described in the Philosophical Transactions* was employed, and the parallelopiped of glass (fig. 2) submitted to its powers; the direct ray, or that producing the first image of the luminous object, acquired a right-handed rotation equal to 12°. Moving the glass a little the second image was brought into view, or that produced by the ray which had traversed the glass three times, and its rotation was 36°. The third image was then observed, and the rotation of the ray producing it was 60°, as nearly as my rough apparatus could measure angular quantities. The same general results were obtained with the second piece of glass described.

The experiment proves in a very striking manner, that whichever way the ray of light is passing through the diamagnetic, the direction of its rotation depends essentially and alone upon the direction of the lines of magnetic force.

It also proves and manifests in a manner not to be mistaken, the difference in this respect between the magnetic rotation of

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* 1846, p. 22, par. 2247. Phil. Mag. vol. xxviii. p. 398.

the ray and that produced by quartz, sugar, oil of turpentine, and such bodies.

Either by independent or by conjoint observations of the different images, it proves that the effect is proportionate to the length of ray submitted to the magnetic force (Experimental Researches, 2163); for the unit length and multiples of the unit length may be observed at once, the intensity of the magnetic force and other circumstances remaining unchanged.

It permits the attainment of a far greater degree of accuracy in the measurement of the amount of rotation of a given ray, or in the estimation of the comparative degrees of rotation of the different coloured rays.

The form of the arrangement makes a short piece of any given diamagnetic, as a crystal, &c., sufficient for an experiment, which would not suffice if the ray were passed but once through it.

It allows of the concentration of the magnetic force by an approximation of the poles, when a magnet is used, so as to exalt the effect; or to render a weak magnet equivalent to a stronger one, so that even good ordinary magnets may now be made available. Or if a helix be used, a much shorter and weaker one than that which before was necessary, may now be employed.

[*Note.*—A heavy domestic affliction having suddenly taken the author's attention away from this paper, the remaining part must be deferred to the next number of the Phil. Mag.]

XXVIII. *On the Blue Compounds of Cyanogen and Iron.*

By ALEXANDER W. WILLIAMSON, *Ph.D.*[*]

IT is a well-known fact that the different substances which pass by the general name of prussian blue, when prepared in the usual way, are not pure combinations of iron, but invariably contain potassium, of which the quantity varies according to the circumstances under which they are formed. It has been the subject of frequent experiment to decide whether this potassium should be considered as an admixture or as an essential constituent, and in the latter case to discover what part it plays in the constitution of the compound. Among those various researches we may confine ourselves to the consideration of those of Berzelius[†] and Gay-Lussac[‡].

Gay-Lussac found that prussian blue retains potassium so

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[*] Communicated by the Chemical Society; having been read March 16, 1846.

[†] Poggendorff's *Annalen*, vol. xxi. p. 490.

[‡] Ibid. vol. xxv. p. 385.

tenaciously that the latter cannot by mere washing be removed from it; for as soon as the precipitate had been freed from all potassium, it was found to consist no longer of prussian blue, but hydrated oxide of iron. From this fact he concludes that the purest prussian blue contains potassium. Berzelius, on the contrary, by washing continuously for three weeks a precipitate of ferrocyanide of potassium with a persalt of iron, succeeded in obtaining a substance, which, though retaining the colour of the original compound, was perfectly free from potassium. The water passing through in the later periods of the operation contained this potassium in the form of ferridcyanide, the formation of which salt he attributes to a process of oxidation resulting from the action of the air; thus admitting that a decomposition of the original precipitate accompanied the removal of the potassium. Without the proof afforded by an analysis, or some characteristic reaction, we cannot venture to conclude that the residue after this decomposing reaction retained the composition of prussian blue. Berzelius thus agrees with Gay-Lussac in the fact that a decomposition of the prussian blue is a necessary condition for the removal of the potassium. The difference consisted only in the kind of decomposition which took place: in Berzelius's experiments the residue after decomposition still maintained a blue colour, while in Gay-Lussac's, in which by the action of a water which he himself concluded must have been alkaline, the residue consisted of peroxide of iron. Both lead however to the same conclusion.

Berzelius analysed two kinds of prussian blue containing potassium. The first, which was soluble in water, he found, after separation by means of alcohol from the red and yellow cyanides, which were simultaneously formed, to contain for every 12 atoms of iron 1 of potassium. The other, which was insoluble in water as it remained on the filter, from which the soluble salt had been washed away, contained iron and potassium in the proportion of 8 equivalents to 1. The former of these substances he considers as a combination of 2 atoms of the yellow prussiate with 3 atoms of prussian blue,—

$$2(\mathrm{Fe\,K_2\,Cy_3}) + 3(\mathrm{Fe_7\,Cy_9}).$$

The second is a combination of 1 atom yellow prussiate with 2 prussian blue,—

$$\mathrm{Fe\,K_2\,Cy_3} + 2(\mathrm{Fe_7\,Cy_9}).$$

This great chemist admits however, what indeed the complex nature of these formulæ sufficiently indicates, that further light is needed on the nature of these compounds than these analyses afford.

I shall now describe the results of some experiments which I have performed in the laboratory of Professor Liebig, for the purpose of gaining some further insight into the composition and nature of these salts, and of deciding the question of the existence of the pure compounds of iron of which the formulæ have been given on purely theoretical grounds.

It is well known that when ferrocyanide of potassium is boiled with dilute sulphuric acid, only a part of the cyanogen is given off as hydrocyanic acid, and that a greenish substance remains behind containing all the iron and a considerable quantity of the potassium in combination with cyanogen; also that this substance by exposure to the air gradually changes into a deep blue. This process is much favoured, as Gay-Lussac observed, by the presence of free acid, and is accompanied by the formation of a potassium salt of this acid. This green substance consists, according to the analysis of Everitt, which M. Geiger repeated in this laboratory, of 3 equivalents cyanogen, 2 iron and 1 potassium. My analysis confirms this composition. 2·372 grammes ferrocyanide of potassium were distilled with water, and sulphuric acid equivalent to the quantity of potassium in the salt, and the distillation was continued until no more prussic acid passed over. The residue, after this treatment, was washed out on a filter, air being excluded, dried and cautiously heated in a platinum crucible with concentrated sulphuric acid, which decomposed all the cyanogen and converted the iron and potassium into sulphates. The aqueous solution of these salts was decomposed by ammonia, filtered, evaporated and heated to redness. 0·532 gramme sulphate of potash was thus obtained, which corresponds to 10·08 per cent. of potassium instead of 10·67 per cent., which the above formula requires.

The compound may be considered as ferrocyanide of potassium, in which 1 atom of potassium is replaced by iron, and may be expressed by the following formula:—

$$\mathrm{Cfy}\left\{\begin{matrix}\mathrm{Fe}\\ \mathrm{K}\end{matrix}\right.$$

Its reactions are also such as we must expect from such a compound, for by treating it with caustic potash, a solution of pure ferrocyanide of potassium is formed whilst protoxide of iron is set free. It will be designated in the following lines, ferrocyanide of iron and potassium. The blue compound into which this salt is changed by oxidation has been incorrectly considered as prussian blue. It, however, differs materially in its composition from that body, as I will proceed to show. Its formation takes place very easily, for it is caused by every reagent, such as chlorine, nitric acid, sulphuric acid, with oxygen, &c., which eliminates potash from the white salt. For its preparation, dilute nitric acid, which consists of 1 volume concentrated acid and 20 of water, may be used with most advantage. This liquid, in which the white salt, prepared as above described, is suspended, is heated in an open basin, during which the liquid is frequently stirred. At a low temperature no apparent action ensues; but when the liquid has nearly reached the boiling-point, an evolution of nitric oxide commences, which gradually increases in briskness, and is accompanied by a speedy change of the white colour to dark blue. As soon as this action has commenced, the basin must be removed from the fire to prevent too violent an action, which would cause the admixture of another substance. If the correct proportion of white salt to acid has been used, the action continues without further heating until the transformation into the blue compound is complete. If, however, the colour should indicate that some of the white salt remains, a small quantity more acid must be added, and the mixture, if necessary, again heated. In order to proceed with certainty, a small portion of the blue substance should be decomposed with potash; should the peroxide of iron which is thus set free contain protoxide, the treatment with nitric acid must be continued; the presence of ferridcyanide of potassium in the solution indicates, on the contrary, that the action has gone too far, in which case the operation must be recommenced with a fresh portion. In this manner a body of a beautiful violet blue colour is formed, containing less potassium than the preceding one. The liquid contains nitrate of potash, but no trace of iron. The substance prepared in this way was thrown on a filter and washed out until the washings left no residue on polished platinum. The compound was not in the least degree affected by this treatment. It retained its original colour, and contained no peroxide of iron soluble in muriatic acid. After being carefully dried at 100° C., it was analysed in the manner which I shall now proceed to describe.

The cyanogen was completely oxidated by a gradually increasing heat applied to it in an open flat-bottomed porcelain crucible. It was thus converted into a mixture of peroxide of iron and carbonate of potash, from which the alkali was extracted by repeatedly boiling with water, and determined as chloride. The residue was determined as peroxide of iron. The cyanogen was determined indirectly by suspending a weighed quantity of the compound in water and decomposing it with potash. The peroxide of iron thus separated necessarily corresponded to the quantity of percyanide, as all protocyanide remained in solution in the form of yellow prussiate.

I. 3·417 grms. gave 0·694 chloride of potassium and 1·630 peroxide of iron, corresponding to 10·6 per cent. potassium and 32·4 per cent. iron.

II. 4·225 grms. gave 0·871 chloride of potassium and 1·986 peroxide of iron, corresponding to 10·87 per cent. potassium and 32·5 per cent. iron.

Determination of Cyanogen.

I. 3·744 grms. gave 0·940 peroxide of iron, corresponding to 17·4 per cent. iron as cyanide.

II. 6·125 grms. gave 1·585 peroxide of iron, corresponding to 17·9 per cent. iron as cyanide.

The formula $Fe_4 Cy_6 K + 4Aq$ requires—

		I.	II.
Fe_4	108·8=32·0	32·4	32·5
K	39·2=11·5	10·6	10·9
precipitated by potash—			
Fe_2	54·4=16·28	17·4	17·9.

The deficiency of potassium and corresponding excess of iron are sufficiently accounted for by the imperfect separation by water. The excess of peroxide of iron, corresponding to the cyanide, was caused by its containing some of the potash employed in its separation. In order, however, to remove all doubt of the correctness of the formulæ deduced from these numbers, several combustions with chromate of lead were performed with the greatest accuracy. These are as follows:—

I. 0·427 grm. gave 0·331 CO_2 and 0·046 water, corresponding to 21·14 per cent. carbon and 10·78 water.

II. 0·320 grm. gave 0·247 CO_2 and 0·036 water, corresponding to 21·05 per cent. carbon and 11·15 water.

III. 0·721 grm. gave 0·559 CO_2 and 0·081 water, corresponding to 21·14 per cent. carbon and 11·3 water.

		Calculated.	I.	II.	III.
C_{12} . .	72	21·17	21·14	21·05	21·14
4Aq .	36	10·59	10·78	11·15	11·3

The approximation here is sufficient.

This compound is particularly remarkable for its brilliant violet-blue colour. In a finely divided state, as is obtained by suspension in a large quantity of water, it is transparent with a green colour. It does not possess when dry the coppery lustre so characteristic of prussian blue. It may be viewed as ferridcyanide of potassium, in which 2 atoms of potassium are replaced by iron, and it bears the same relation to the above-described white compound, $Cfy\left\{\begin{matrix}Fe\\K\end{matrix}\right.$, as Gmelin's salt to the yellow prussiate. The manner of its formation is also the same, for 2 atoms ferrocyanide of iron and potassium unite, giving up 1 atom of potassium, just as in the case of yellow prussiate.

$$2\left(Cfy\left\{\begin{matrix}Fe\\K\end{matrix}\right.\right) + Cl = 2Cfy\left\{\begin{matrix}2Fe\\K\end{matrix}\right. + KCl.$$

We may call it, according to the customary nomenclature, ferridcyanide of iron and potassium. It has been mentioned above, that on treating it with potash, ferrocyanide of potassium is formed, and hydrated oxide of iron is set free. It is evident that in this reaction 2 atoms of iron are replaced by 3 of potassium, whilst the oxygen of the potash combines with the iron.

On heating this blue compound with a solution of yellow prussiate it changes it into ferridcyanide; if the blue compound be in excess no trace of yellow prussiate remains behind. 3·473 grms. ferridcyanide of iron and potassium were suspended in an excess of ferrocyanide, and digested with it for a considerable time. Collected on a filter, it was of a pale blue colour and gave 1·39 grm. sulphate of potash, corresponding to 0·624 potassium, of which, before the treatment with ferrocyanide, it only contained 0·411. Hence it is evident that the reaction consists in potassium being taken from the yellow prussiate, and half of the ferridcyanide of iron and potassium changed into ferrocyanide. This reaction may even be applied with advantage to the preparation of larger quantities of pure red prussiate, for it presents neither of the disadvantages attending the decomposition by chlorine, viz. the admixture of chloride of potassium, which is unfavourable to crystallization, and also the well-known green substance, which is formed by the slightest excess of chlorine. The blue salt may of course be used repeatedly for this operation, as it is immediately restored by warming with nitric acid.

By long-continued boiling with nitric acid, this blue compound is changed into a rich dark green, which contains a greater proportion of cyanogen and a small quantity of potassium. It is reduced by the action of light, which gradually changes its colour to a blue. For analysis, it was heated with concentrated sulphuric acid until all cyanogen was destroyed; the iron and potassium were determined as peroxide and sulphate of potash.

I. 2·966 grms. gave 1·527 peroxide of iron and 0·147 sulphate of potash, corresponding to 35·72 per cent. iron and 2·23 per cent. potassium.

II. 3·172 grms. gave 1·637 peroxide of iron and 0·159 sulphate of potash, corresponding to 35·97 per cent. iron and 2·25 per cent. potassium.

Burned with chromate of lead it gave the following numbers:—

I. 0·421 grm. gave 0·361 carbonic acid and 0·058 water, corresponding to 23·3 per cent. carbon and 13·6 water.

II. 0·151 grm. gave 0·121 carbonic acid and 0·020 water, corresponding to 23·2 per cent. carbon and 13·3 water.

This quantity of potassium is too small to justify the conclusion that it is essential to the composition of the salt, as it only amounts to about 1 equiv. to 24 iron. Its equivalent of iron was therefore added to the other iron, and in this manner numbers found which correspond to the formula $Cy_7\ Fe_5 + 5\ aq$, as is shown by the following:—

	Calculated.	Found. I.	Found. II.
C_{14}	$84 = 23{\cdot}14$	23·3	23·2
Fe_5	$136 = 37{\cdot}5$	37·17	37·27
Aq_5	$45 = 12{\cdot}4$	13·6	13·3

On treating this body with potash, peroxide of iron is separated and a brownish-red liquid formed, in which proto- and persalts of iron give a blue precipitate. This liquid is decomposed by boiling, peroxide of iron is precipitated, and the colour of the precipitate becomes much lighter. I am not aware, however, what is the nature of the decomposition. The brown-red colour of the liquid has much similarity with that obtained by mixing ferridcyanide of potassium with a persalt of iron. The green substance which Pelouze obtained by decomposing yellow prussiate by chlorine, and for which he gives the empirical formula $Fe_3\ Cy_4$, is decomposed in a similar manner by potash. If, in accordance with the view of many chemists, we consider this substance analogous to magnetic iron ore, as $Fe_2\ Cy_3 + Fe\ Cy$, the compound which I have described may be viewed as containing a double quantity of percyanide, $2(Fe_2\ Cy_3) + Fe\ Cy$. I will not however venture to express any opinion as to the propriety of considering it as a peculiar compound or not.

It is well known that for technical purposes an esteemed blue colour, which is sometimes called Turnbull's blue, is prepared by decomposing ferridcyanide of potassium with a protosalt of iron. In this compound, which by its colour may be easily distinguished from prussian blue, the presence of potassium has been discovered, but without bringing to light any connection with the composition of the body. It appeared to me not devoid of interest to examine the properties of this substance and subject it to analysis. To a dilute aqueous solution of ferridcyanide of potassium, which had been purified by frequent crystallizations, was added a solution of sulphate of iron, the red salt remaining in excess. In washing out this precipitate I proceeded with particular care, as, whilst removing all substances mixed with the compound, it was necessary to avoid any action upon it which might render soluble any constituents of the salt, as was the case with Berzelius's washing in the air. This object I completely succeeded in attaining, simply by means of washing with distilled water in a vessel from which air was excluded. The precipitation had been performed in a tall glass cylinder, which was completely filled by the liquids, and could be perfectly closed by a glass plate adjusted to its ground edge. In this vessel the precipitate was washed by subsidence and decantation, during the first period of which the water ran off colourless, or only slightly coloured by the remaining quantity of red prussiate. As soon, however, as the greater part of the soluble salts had been washed away, the liquid no longer deposited all the blue compound suspended in it, but even after standing several hours ran off with a blue colour. In the supposition that I had here a solution of prussian blue, I threw it upon a filter and found that it ran through unchanged. Unwilling however, upon the simple testimony of this fact, to conclude that it was a solution, I transferred it to a filtering apparatus, so contrived that by means of a hydrostatic pressure, which could be varied at pleasure, it was driven through a sixfold layer of filtering paper supported by a linen cloth. This experiment proved that the blue compound was indeed not in solution, but merely in a state of suspension, so fine as to pass through the pores of a simple filter, for it was completely separated by the denser mass of the compressed folds of paper, and a colourless liquid passed through. The washing was continued until the water filtered through in this manner left no visible residue on evaporation on polished platinum. Subsequent experiments showed that this operation may, without affecting the result, be performed more quickly and easily by the use of hot water, as in this case the precipitate assumes a denser form and deposits more easily from the liquid.

I. 1·706 grm. of this substance gave 0·828 peroxide of iron and 0·189 sulphate of potash, corresponding to 33·6 per cent. of iron and 4·96 per cent. of potassium.

II. 1·870 grm. gave 0·909 peroxide of iron and 0·221 sulphate of potash, corresponding to 33·7 per cent. of iron and 5·3 per cent. potassium.

The average of these numbers, namely 33·6 iron and 5·1 potassium, are in the proportion of 9·3 equivalents iron to 1 potassium. It will be shown later how these numbers are to be considered; we see in the meanwhile that they indicate a different composition from that of ferridcyanide of iron and potassium.

I next endeavoured to precipitate in such a manner as if possible to replace all the potassium of Gmelin's salt by iron, which was done by forming the precipitate in an excess of protochloride of iron, and digesting it for several hours with that salt. The substance thus prepared was washed out in the manner described in the preceding instance, and was found, though not absolutely free from potassium, to contain so extremely small a quantity of this element, as to render a determination of it next to impossible; so that the difference produced by neglecting it fell far within the limit of the ordinary limit of analysis. After drying at the ordinary temperature over sulphuric acid it was subjected to analysis.

I. 0·765 grm. gave 0·383 peroxide of iron, corresponding to 34·6 per cent. of iron.

II. 0·763 grm. gave 0·379 peroxide of iron, corresponding to 34·4 per cent. of iron.

Burnt with chromate of lead,—

I. 0·535 grm. gave 0·344 carbonic acid and 0·155 water, corresponding to 17·5 per cent. carbon and 28·9 water.

II. 0·563 grm. gave 0·359 carbonic acid and 0·157 water, corresponding to 17·4 per cent. carbon and 27·9 water.

III. 0·477 grm. gave 0·307 carbonic acid and 0·135 water, corresponding to 17·5 per cent. carbon and 28·2 water.

The proportion of 5 equivalents of iron to 6 of cyanogen requires to 34·5 of iron, which is the average found, 18·2 carbon instead of 17·5, as is found. If it be considered that the error of iron and carbon determinations are here added together, the accordance will I think be held to be sufficient. My reason for not calculating any formula for the water was, that owing to the readiness with which the compound is decomposed, I was compelled to analyse it without drying at an elevated temperature, which would have produced an evolution of hydrocyanic acid. On decomposing this substance in the fresh prepared state by potash, proto- and peroxides of iron are set free and ferrocyanide of potassium formed. A portion of this compound was, after carefully washing out and without drying, decomposed by carbonate of potash. The proto- and peroxides of iron, thus separated, after long-continued heating in the air, weighed 1·627 grm. of peroxide. The liquid boiled in and treated with sulphuric acid gave 1·077 grm. peroxide of iron. The proportion of 3 to 2 requires 1·623 and 1·082. Of 5 equivalents of iron 3 were separated in combination with 4 atoms of oxygen of the potassium, which takes their place, while the other two being contained in the composition of the radical remained in solution.

I will now pass to the consideration of the manner in which we may consider these elements to be combined. It is well known that ferridcyanide of potassium is reduced by the action of sulphuretted hydrogen, that is to say, it takes up hydrogen, and is thus converted into a mixture of 3 atoms ferrocyanide of potassium and 1 atom hydroferrocyanic acid (or ferrocyanide of hydrogen).

$$\left.\begin{array}{l}\mathrm{4Cfy\;6K}\\ \mathrm{2HS}\end{array}\right\}=\left\{\begin{array}{l}\mathrm{3Cfy\;2K}\\ \mathrm{Cfy\;2H}\\ \mathrm{2S}\end{array}\right.$$

The fact of the red cyanide being thus reducible is however a property which may in the above instance exercise a considerable influence on the composition of the product, as is shown by the following instructive experiment of Liebig. To a boiling solution of ferridcyanide of potassium was added a small quantity of protosulphate of iron, which was not nearly sufficient to decompose all the cyanide. Ferrocyanide of potassium, which remained in solution, and precipitate of prussian blue (3Cfy 4Fe), were formed:—

$$\left.\begin{array}{l}\mathrm{4Cfy\;6K}\\ \mathrm{4FeO}\end{array}\right\}=\left\{\begin{array}{l}\mathrm{3Cfy\;4Fe}\\ \mathrm{Cfy\;2K}\\ \mathrm{4KO}\end{array}\right.$$

This reaction leads to a new way of considering the above analysed compound, which is adapted to its composition when containing and also when free from potassium, for if one atom more of protosalt of iron be added than assumed in the above formula of decomposition, it forms, by decomposition with the yellow prussiate, ferrocyanide of iron and potassium, which divided in the already-formed prussian blue, produces a mixture containing iron and potassium in the proportion found by analysis:—

$$\left.\begin{array}{l}\mathrm{4Cfy\;6K}\\ \mathrm{5FeO}\end{array}\right\}=\left\{\begin{array}{l}\mathrm{3Cfy\;4Fe}\\ \mathrm{Cfy}\left\{\begin{array}{l}\mathrm{Fe}\\ \mathrm{K}\end{array}\right.\\ \mathrm{5KO}\end{array}\right.$$

On adding an excess of iron salt the second equivalent of potassium is also replaced by iron, and a mixture of prussian blue and ferrocyanide of iron formed, which contains cyanogen and iron in the proportion of 6 to 5, as found. This view rests upon the supposition that prussian blue (3Cfy 4Fe) is always formed by the decomposition at the ordinary temperature, as was found to be the case at the boiling-point. In the contrary case we must assume that the potassium of the red cyanide is simply replaced by iron:—

$$\mathrm{2Cfy\;3K + 3FeCl = 2Cfy\;3Fe + 3KCl}$$

$$\mathrm{4Cfy\;6K + 5FeCl = 4Cfy}\left\{\begin{array}{l}\mathrm{5Fe}\\ \mathrm{K}\end{array}\right. \mathrm{+\;5KCl}$$

In the first case we have ferridcyanide of iron, in the second a double salt of the same with ferridcyanide of iron and potassium.

The separation of Fe_3O_4 by potash seems to speak in favour of the view that the precipitate is a mixture of prussian blue and ferrocyanide of iron, as these substances would give this reaction; for if the compound were a ferridcyanide of iron, it would be expected that potash would set free protoxide of iron and reproduce the red cyanide. It might indeed be replied to this that the red cyanide is reduced by the protoxide in proportion as it is set free:—

$$\left.\begin{matrix}\mathrm{2Cfy\,3K}\\ \mathrm{KO}\end{matrix}\right\} + \mathrm{3FeO} = \mathrm{2(Cfy\,2K)} + \mathrm{Fe_3O_4}.$$

I described above an experiment of Liebig, showing that by the action of protoxide of iron the red prussiate is reduced to the yellow, but I find that the reverse process takes place with equal facility, that is, the peroxide of iron decomposes the yellow cyanide, forming the red one. A small quantity of perchloride of iron was added to a boiling solution of ferrocyanide of potassium, and the mixture heated for some minutes. The liquid filtered off clear gave a deep blue precipitate with protosulphate of iron:—

$$\left.\begin{matrix}\mathrm{3(Cfy\,2K)}\\ \mathrm{Fe_2O_3}\end{matrix}\right\} = \left\{\begin{matrix}\mathrm{2Cfy\,3K}\\ \mathrm{Cfy\,2Fe}\end{matrix}\right\} + \mathrm{3KO}.$$

I have now endeavoured, as far as possible, to state the two ways of considering these compounds, and will leave the decision to more competent judges.

I next endeavoured to apply the method which had proved so serviceable in obtaining Turnbull's blue free from potassium, viz. digesting the precipitate with an excess of the iron salt, to the preparation of prussian blue of equal purity. A weak solution of yellow prussiate was poured into a great excess of perchloride of iron, and the mixture allowed to stand for some hours exposed to a gentle heat. This precipitate, after being completely washed in the usual manner, gave, on analysis, to 0·707 peroxide of iron, 0·057 sulphate of potash, which numbers are in the proportion of 27 iron to 1 potassium. If this were to be considered as a peculiar compound, it must be,—

$$\mathrm{3(3Cfy\,4Fe)} + \mathrm{3Cfy}\left\{\begin{matrix}\mathrm{3Fe}\\ \mathrm{K}\end{matrix}\right.$$

All my endeavours to obtain a precipitate free from potassium were unavailing, as long as this element was present at the formation; I had therefore at last no alternative left but that of precipitating by pure hydroferrocyanic acid. This acid was prepared by mixing a solution of yellow prussiate, from which, by boiling, all atmospheric air had been expelled, with about an equal volume of muriatic acid which had been similarly freed from air, and precipitating by æther. After it had been filtered off and washed with æther, the acid only needed to be dissolved in absolute alcohol and again precipitated by æther to be obtained perfectly pure. It was then dried, dissolved in water, and precipitated with an excess of perchloride of iron. The precipitate thus formed may be distinguished from common prussian blue by its darker colour. It was washed in the usual manner and dried at a temperature of from 30° to 40°.

I. 1·400 grm. burnt in an open porcelain basin gave 0·6487 peroxide of iron, corresponding to 32·1 per cent. iron.

II. 1·0365 grm. gave 0·481 peroxide of iron, corresponding to 32·0 per cent. iron.

III. 1·0035 grm. gave 0·463 peroxide of iron, corresponding to 31·8 per cent. iron.

Burned with chromate of lead,—

I. 0·6934 grm. gave 0·4405 carbonic acid and 0·1919 water, corresponding to 17·3 per cent. carbon and 27·9 per cent. water.

II. 0·415 grm. gave 0·269 carbonic acid and 0·127 water, corresponding to 17·7 per cent. carbon and 28·6 water.

III. 0·304 grm. gave 0·233 carbonic acid and 0·119 water, corresponding to 17·4 per cent. carbon and 20·0 water.

Nitrogen determination according to Will's method :—

I. 0·4065 grm. gave 0·3077 chloride of platinum and ammonium, corresponding to 20·2 per cent. nitrogen and 0·574 platinum.

II. 0·543 grm. gave 1·780 chloride of platinum and ammonium, corresponding to 20·6 per cent. nitrogen and 0·780 platinum.

The following table shows the degree of approximation to the formula $Fe_7\ Cy_9$:—

	Calculated.	Found.		
		I.	II.	III.
Fe_7	190·4=31·56	32·1	32·0	31·8
C_{18}	108·0=17·87	17·3	17·7	17·4
N_9	126·0=20·83	20·2	20·6	
Aq_{20}	180·0=29·74	27·9	28·6	28·0

In each combustion fresh-prepared substance was taken. The slight excess of iron and loss of carbon and nitrogen are doubtless to be attributed, in spite of the precautions taken to prevent it, to a decomposition having taken place during the drying, by which hydrocyanic acid was set free, as indeed might be recognised by the smell, and peroxide of iron formed. The oxygen combined with iron is in the table calculated as water, hence the deficiency of water. This decomposition, which takes place very easily, explains the formation of the so-called basic prussian blue, a substance considered as a compound of prussian blue and peroxide of iron, but of which little is known.

$$3Cfy\ 4Fe + 3aq - 3CyH = 2Cfy\ 3Fe + Fe_2\ O_3.$$

By the action of light a process of reduction takes place, as was remarked above in the case of another compound. With other cyanogen compounds a similar action occurs, of which I will mention a striking instance further on. On decomposing this substance by potash, ferrocyanide of potassium is formed and peroxide of iron set free. Experiments, in which I endeavoured to determine quantitatively the proportion of iron separated by potash, invariably gave more than the formula of the pure substance requires, but this seems to be accounted for by the formation of the above-described basic compound.

In aqueous oxalic acid the prussian blue is very easily soluble. By the addition of carbonate of potash to this liquid the colour is changed to a red-brown, but no iron is precipitated at the ordinary temperature; as soon however as the liquid is boiled, peroxide of iron falls down in the proportion of three-fifths of that contained in the blue compound.

An unweighed quantity of dried prussian blue was dissolved in oxalic acid, and by boiling with carbonate of potash 0·208 peroxide of iron were precipitated. The liquid contained 0·137. The proportion of 3 to 2 requires 0·207 and 0·138. On a second determination 0·274 were precipitated by potash, whilst 0·174 remained in solution. The proportion of 3 to 2 requires 0·268 and 0·179. This reaction with oxalic acid might reward further examination. A remarkable circumstance connected with it is that the liquid after being filtered off from the peroxide of iron, separated by potash, is precipitated blue by muriatic acid.

I have now endeavoured to describe the formation of prussian blue under different circumstances, and the influence which these exercise on its composition, giving particular attention to the presence of potassium.

It appears to me that this circumstance of the presence of potassium is not a matter of mere scientific interest, but is also of great importance to the dyer, for it materially affects the colour and dyeing power of the product in the manner I shall now proceed to state. The greater the quantity of potassium contained in the cyanide, the lighter and more approaching to violet is its colour; and, on the other hand, in proportion as the quantity of potassium is diminished the colour becomes deeper and more powerful. Of the different blue compounds described above the brightest and most striking is that formed from the residue of the distillation of prussic acid, $\left(2\mathrm{Cfy}\left\{\begin{matrix}2\mathrm{Fe}\\ \mathrm{K}\end{matrix}\right.\right)$. Next to this in order stands the precipitate formed by proto-sulphate of iron in an excess of ferridcyanide of potassium, $4\mathrm{Cfy}\left\{\begin{matrix}5\mathrm{Fe}\\ \mathrm{K}\end{matrix}\right.$. The next link in the chain is a compound formed by the decomposition of ferridcyanide of potassium by sulphuric acid, which contains still less potassium, and which I will presently describe. Darkest of all are the compounds which contain no potassium. The dyeing power of these substances, by which term I intend to designate the relative power of imparting a blue colour to any colourless substance with which they may be mixed, is in the inverse ratio to their quantity of potassium; but in such a manner that those containing potassium have a smaller colouring power than, after deducting the cyanide of potassium contained in them, would belong to the remainder; whence we see that the potassium plays no indifferent part in their colouring power.

When a solution of ferridcyanide of potassium is treated with concentrated sulphuric acid a green precipitate is formed, consisting of percyanide of iron and a small quantity of cyanide of potassium. By continued boiling with an excess of sulphuric acid its colour is changed into a rich blue. It is advisable to continue boiling for a considerable time after the change of the colour has taken place, in order to ensure the complete transformation. Ammonia, which may be easily proved to exist in the liquid, is here formed at the expense of a part of the cyanogen. This compound was washed and dried in the manner described in former instances, and gave on analysis to 1·962 peroxide of iron 0·049 sulphate of potash, which is one proportion of potassium to about sixty iron, too small a quantity of potassium to be considered as essential.

I. 1·464 grm. gave 0·702 peroxide of iron, corresponding to 33·25 per cent. iron.

II. 1·152 grm. gave 0·556 peroxide of iron, corresponding to 33·45 per cent. iron.

III. 0·549 grm. gave 0·266 peroxide of iron, corresponding to 33·61 per cent. iron.

Burned with chromate of lead,—

I. 0·481 grm. gave 0·306 carbonic acid and 0·131 water, corresponding to 17·3 per cent. carbon and 27·2 water.

II. 0·347 grm. gave 0·219 carbonic acid and 0·95 water, corresponding to 17·7 per cent. carbon and 27·0 water.

The following table shows the degree of approximation to the formula $Fe_5 Cy_6 + 13Aq$:—

	Calculated.	Found.		
		I.	II.	III.
Fe_5 . .	136 = 33·2	33·2	33·4	33·6
C_{12} . .	72 = 17·6	17·3	17·7	
N_6 . .	84 = 20·5			
13Aq .	117 = 28·6	27·2	27·0	

It is evident, from the quantity of water found, that less hydrogen was in the substance than calculated, and that a portion of peroxide of iron was contained in it.

It is decomposed by potash in the same manner as Turnbull's blue, which has been dried in the air; peroxide of iron is set free, and hydroferrocyanic acid combines with the potash, as described under that compound. The cause of this reaction, which seems irreconcileable with the composition 2Cfy 3Fe, lies in an oxidation of the iron taking place by the action of the air. We have seen above that Turnbull's blue retains a considerable quantity of water, and may assume that its elements are divided between the radical and the iron. Its formation is as follows:—

$$\left.\begin{array}{l}2\mathrm{Cfy}\,3\mathrm{K}\\3\mathrm{FeO}\\4\mathrm{Aq}\end{array}\right\} = 2(\mathrm{Cfy\,H_2})\,\mathrm{Fe_3O_4} + 3\mathrm{KO}.$$

The fresh precipitate behaves as such a compound; but after it has been dried in the air potash separates from it pure peroxide of iron instead of the magnetic oxide. This proves that, what indeed ensues with all protosalts of iron, oxidation takes place on exposure to the air. A consideration of the analytical results immediately shows that such an admixture of peroxide of iron is contained in it. The process of oxidation consists in 2 atoms of the cyanide taking up 1 atom of oxygen:

$$2\{2(\mathrm{Cfy\,H_2})\,\mathrm{Fe_3O_4}\} + \mathrm{O} = 4(\mathrm{Cfy\,H_2}) + 3\mathrm{Fe_2O_3}.$$

This formula expresses the elements contained in the substance after drying in the air. A more probable expression for its composition is however obtained, if the elements of three such compound atoms are arranged to 4 atoms of prussian blue and 1 atom peroxide of iron. An analogous compound to the pure unoxidated Turnbull's blue is that which I described under the name of ferridcyanide of iron and potassium, but which may as correctly be considered as a combination of 2 atoms hydroferrocyanic acid with 1 peroxide of iron and 1 potash. A strong support of this view is the fact that it contains 4 atoms of water, which are not expelled at 100°.

I have also examined a great number of precipitates formed by ferrocyanide of potassium with other metallic salts, and universally found, that whatever may be the method of precipitation, it is not possible to replace all the potassium by the metal made use of. Ferrocyanide of potassium was added to a great excess of sulphate of copper; the precipitate, which had the well-known red-brown colour, was found after complete washing to contain a considerable quantity of potassium.

The corresponding dingy yellow precipitate of ferridcyanide also contains potassium. On decomposing it by potash, ferridcyanide of potassium and oxide of copper are obtained, which proves that it is a true ferridcyanide, and not analogous to Turnbull's blue.

The reducing action of light shows itself most strikingly in the case of this precipitate. It had, after drying, been kept in large pieces in a glass bottle which stood for some time near a window. All the outsides of the pieces turned towards the light, became of a red-brown colour from the formation of ferrocyanide.

On treating the ferrocyanide of copper above described with sulphuretted hydrogen, no action at first seemed to take place; after some time however it commenced; and on the decomposition being completed a strongly acid solution was formed, which became gradually blue on exposure to the air. It gave a blue precipitate with perchloride of iron, but did not possess the characteristic reaction of hydroferrocyanic acid, for it was not precipitated by æther. On adding muriatic acid this reaction however immediately appeared. By standing over sulphuric acid it dried into a blue mass with a coppery lustre, which with water formed a liquid not unlike a solution.

On analysis it gave to 0·487 peroxide of iron 0·261 sulphate of potash. The formula, $4\mathrm{Cfy}\left\{\begin{matrix}7\mathrm{H}\\ \mathrm{K}\end{matrix}\right.$, requires 0·270 sulphate of potash to the quantity of iron found.

XXIX. *On the Symbol $\sqrt{-1}$ in Geometry.*

By A CORRESPONDENT OF TWENTY-FIVE YEARS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

DURING the progress of the papers of Sir William Hamilton, one in your own Magazine and the other in the Cambridge and Dublin Mathematical Journal, it would be unseemly to enter into a discussion of the general question respecting the correctness of the views entertained by this distinguished analyst, or even respecting their *originality*. This must be reserved till we have their full development before us. However, the limited case of which some small use has been attempted in your last Magazine by Mr. Warner, is fairly open to discussion, since the doctrine itself is one of considerable standing (almost eighty years) in the mathematical world. To this, therefore, I shall confine my remarks at the present moment, and they shall be as brief as possible.

A paper by Foncenex in the *Turin Mémoires* (if my memory be correct, for 1778), is devoted to a discussion of the notion of this symbol which is now so frequently adopted—that it is the symbol of perpendicularity. Who the author was whose views Foncenex combated, he does not state; but very likely it was to the Abbé Sauri that he referred. The doctrine was again broached by the emigrant priest Buée, in the Philosophical Transactions for 1806; and it was extended shortly after by Français so as to include *essentially* all the interpretations which I have yet seen put upon it, in the *Annales des Mathématiques.* English speculatists should read more than they usually do.

I will not, of course, adopt the logical fallacy of undertaking "to prove a negative," but I may broadly state as my conviction, that the appearance of the symbol $\sqrt{-1}$ expresses *impossibility*, and nothing more. Such a conclusion is, of course, only inductive, but founded on this fact, that I have never met with a case which was not in strict conformity with it, or easily reducible to such a one.

It is admitted on all hands, that when *the data of a problem are incompatible* with each other, the quæsitum of the problem will always be of the form $\alpha \pm \beta \sqrt{-1}$; and conversely, that whenever this form appears in the solution, the data of the problem are incompatible with each other. This is not exclusively the case in geometrical problems, but appertains to all algebraic researches indiscriminately, whatever be the subject-matter of the inquiry. The principle is fundamental and universal.

In algebra, the sole difference between a theorem and a problem is this:—that in the theorem the equivalence of two different expressions (or functions of different forms) is affirmed, whilst in the problem one side of that equality, subject to assigned conditions, is demanded. The processes of solving the proposition either as a theorem or a problem, have not the slightest logical difference of character. Thus we may propose as a theorem to be proved, that x being indeterminate,

$$a^x = 1 + \frac{\mathrm{A}\,x}{1} + \frac{\mathrm{A}^2 x^2}{1\,.\,2} + \ldots$$

where $$\mathrm{A} = (a-1) - \frac{1}{2}(a-1)^2 + \ldots;$$

or we may propose to expand a^x in a series of integer positive powers of x. The actual process of investigation will be the same in either case, although certain hints are furnished by proposing the inquiry as a theorem, which may facilitate our discovery of a method of research adapted to the purpose in view.

As another instance, we may propose *to prove the theorem*

$$\frac{1}{2}\{e^{\theta\sqrt{-1}} + e^{-\theta\sqrt{-1}}\} = 1 - \frac{\theta^2}{1\,.\,2} + \frac{\theta^4}{1\,.\,2\,.\,3\,.\,4} - \ldots;$$

or we may propose to find u and v such that

$$1 - \frac{\theta^2}{1\,.\,2} + \frac{\theta^4}{1\,.\,2\,.\,3\,.\,4} - \ldots = v\,\{u^\theta + u^{-\theta}\}.$$

Treating this proposition as a theorem, we shall have to establish the identity of value of the two sides of the equation; and treating it as a problem to find the values of u and v, which will fulfill the identity: and the two processes differ not the least in logical character, and are only varied in the order of their subordinate details. From both forms of the proposition, we learn, however, that the series cannot be converted into the form $v\,\{u^\theta + u^{-\theta}\}$, whilst u and v are real numbers positive or negative; and that the condition can only be fulfilled by the co-existence of the incompatible operations expressed by $\sqrt{\ }$ being performed upon $-$. In other words, the operations which give the value in a finite algebraic form are inconsistent with each other; and the imposed conditions are therefore incongruous.

Precisely the same remark applies to Demoivre's as to Euler's theorem, viz. that ($\cos\theta$ and $\sin\theta$ being abbreviations for two specified series)

$u^\theta = \cos\theta + v\sin\theta$ is the problem, and

$e^{\theta\sqrt{-1}} = \cos\theta + \sqrt{-1}\,.\sin\theta$ is the theorem.

Also from this the same conclusion respecting congruity is deducible in the same way.

It is too familiarly known to require being insisted on here, that the *coefficient* of $\sqrt{-1}$ in a result, does *in general* furnish some information as to the method by which the relative magnitudes in the data may be so modified as to render the proposed problem soluble, or so that the enunciated theorem shall be expressible by means of congruous operations. For if that coefficient can be reduced by any such relations amongst the data to zero, the expression itself disappears in consequence; thus giving the extreme case of solvibility of the proposition composed of data of the specified kind. And, again, if that coefficient can be made to take the form $\sqrt{-\gamma}$, the symbol $\sqrt{-1}$ will not appear in the result at all, since $\sqrt{-\gamma}$

$\times \sqrt{-1}$ is a real quantity involving no incongruous operations. This, however, is more immediately and more frequently the case in the resolution of problems; the cases of this possible transformation of the conditions under which a theorem involving incongruous operations (whether in pure analysis or its applications to geometry or physics) can be congruously expressed being comparatively rare. Be it remembered, however, that whether the preceding views be admitted or not, they do not actually bear upon the *direct argument* which I am about to urge, although according to my view they strongly bear upon its *illustration*.

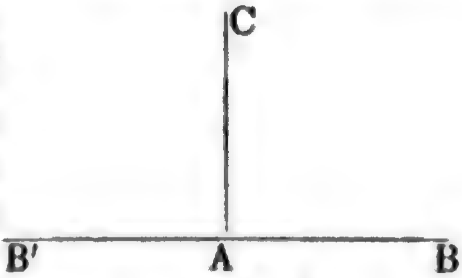
My proposition is this,—

The symbol $\sqrt{-1}$ does not express perpendicularity, but only incongruity amongst the geometrical conditions from which the expression was derived.

1. When we attach the signs + and − to the symbols of two lines, it expresses that those lines have a *contrary direction* estimated from a given point in a given indefinitely prolonged line. Their positions become then fixed and incapable of an altered position without cancelling the hypothesis of their existence.

2. When we define a rectangle, we do it as a parallelogram which has these two lines placed *at right angles to each other* for adjacent sides. These two lines then become fixed and incapable of an altered position, without cancelling the hypothesis of their existence.

3. Let A B be the same in both cases; then A B′ is the second line of the first hypothesis, and A C is the second line of the second hypothesis. Whence AB′ and AC cannot coincide without cancelling one hypothesis or the other. Let A B, A B′, A C be of equal magnitude.



4. In saying that the rectangle $+a \times -a = AC^2 = a^2$, we allege that $-a$ can have the position A B′ and A C *at the same time*; and in this way we get $AC = \pm a\sqrt{-1}$.

5. In this we have manifestly only prescribed incongruous operations, those of giving to the same line two different positions at the same time.

6. Perpendicularity is a *real property*, or the result of a possible and performable series of operations, whilst this alleged expression of it results from incongruous operations. The expression $\sqrt{-1}$ cannot, therefore, be that of perpendicularity; and it has been proved to be that of incongruous operations.

Our proposition is therefore proved; and all the conclusions that follow from the contrary doctrine are at once destroyed. In truth, could we for one moment expect this contradiction between the parts of the hypothesis to result in anything else than the *symbol of impossibility*? Might it not, indeed, be more properly called the *symbol of contradiction*?

Of course with the fundamental "assumption," as Mr. Warner very truly calls it, all that is built upon it falls; and it might seem almost unnecessary to say another word on the subject. However, with respect to the conjugate hyperbolas which are so often quoted in discussions of this kind, it may be worth while to remark, that they *cannot be brought under the same equation with the primary ones*. Neither (though this is generally lost sight of) do they belong to the *same geometrical system*. They cannot be cut from a pair of opposite right cones conjugate to those from which the primary ones were cut. They are, in fact, the produce of a second geometrical hypothesis as much as their equation is.

Points out of the co-ordinate plane X Y are as foreign to the original hypothesis respecting a plane curve, as the point C is foreign to that of $AB = +a$ and $AB' = -a$. In short, the entire speculation is unworthy of the ability which has been employed upon it; and its prosecution throws discredit on the scientific honours which many of its cultivators have deservedly won.

August 5, 1846. SHADOW.

XXX. *Note on* Mr. Warner's *Paper on Conjugate Points. By* J. R. YOUNG, *Professor of Mathematics Belfast College.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I THINK with you, that Mr. Warner's views on the subject of conjugate points—as delivered in the last Number of the Philosophical Magazine—are not likely to meet with acceptance among mathematicians; and I am persuaded that even Mr. Warner himself will feel inclined to abandon them when he reflects upon the consequences to which they lead. His fundamental *analytical* principle is, that "$+0 = -0$;" from which it follows, taking the reciprocal, that $+\infty = -\infty$; and therefore, transposing, that $2\infty = 0$, that is, that a quantity infinitely great is nevertheless equal to *nothing*. His fundamental *geometrical* principle is of like character: it is, that a finite and determinate line (D E), merely by being turned about a little, suddenly becomes infinite. Mr. Warner's principles therefore require that a quantity, however small—even zero itself—is equivalent to another infinitely great; and "in this"—to use Mr. Warner's own words—"in this consists *his* error."

I am, Gentlemen,
Your obedient Servant,
Belfast, August 10, 1846. J. R. YOUNG.

XXXI. *On two Extraordinary Meteors.*
By Prof. ELIE WARTMANN [*].

I OBSERVED a few months ago two meteorological phænomena which I consider to be of very unfrequent occurrence. The first is *an extraordinary rainbow*, which was seen on the 25th of last April during a partial eclipse of the sun. I was at Paudex, a little village on the shore of the Lake of Geneva, nearly two miles east of Lausanne. The sky, which was cloudy in the morning, cleared up in the afternoon. A single band of cumulo-strati, at a slight elevation, rested on the ridge of the Jura, in the west, where, half concealing the sun, it received from it a golden light scarcely supportable by the eye, and which prevented the observation of that luminary. Towards 5 o'clock a double iris was perceived, presenting the usual interior and exterior arcs; the first accompanied by six exterior supernumerary bands, red, green, red, green, reddish, pale green. At this moment there was no rain between the sun and my eye, and the surface of the lake was smooth as a mirror; but a hard shower fell upon the rocks of Meillerie, situated on the opposite shore, and veiled their summits. By degrees the nimbus or rain-cloud went off to the S.S.W. I returned home in the direction of the sun; then, after a few minutes, wishing to examine again the rainbow, I no longer observed any secondary bands, nor the two concentric arcs above mentioned. A brilliant column of the brightest colours was formed over the lake, which appeared to penetrate to its bottom. This column was violet on the side of the sun, and was *double the width* of the ordinary interior arc. It rose, dome-shaped, to nearly nine to ten degrees, then it branched out into two distinct arcs, which left between them a dark space similar to a spherical angle of about six degrees aperture. [I had no instrument with me, and I only give these as approximate estimations.] The inferior arc was the ordinary interior one; it was more brilliant and more developed than the upper one which merged gradually in the general light, and had also the red outwards. At the limit of the wide band, in which the two partial arcs originated, the

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[*] Communicated by the Author.

colours sparkled so that it was impossible to determine the spot where the separation of the tints between the two arcs took place. The development of these various phases commenced at a quarter to six o'clock; the last continued eight to ten minutes, and all vanished some instants before sunset. Mr. Frederick Chavannes, mathematical instructor in the Gymnasium of Lausanne, and member of the Society of Natural Sciences in that city, witnessed this phænomenon, and agreed in describing it as I have done.

Does this extraordinary eccentric arc proceed from a second illuminating point placed at the same height as the sun, or from a very resplendent nubilous band? I am inclined to think that it is produced by the reflexion of the luminous rays on the surface of the lake. This circumstance, in diminishing their intensity, must have caused all trace of the external arc to disappear. The little height of the eccentric above the common arc will be explained, on this hypothesis, by the short distance of the luminary from the horizon. Lastly, we may compare the appearance of which I have spoken to that of other bows which have been observed in analogous circumstances, such as that of the 10th of August 1665, observed by the Canon-Etienne on the banks of the Eure, near Chartres; that of the 17th of August 1698, which was observed by Halley at Chester, in the vicinity of the Dee; that of the 8th of August, 1743, so well described by Celsius, who witnessed it at Husbi, in Dalecarlia, on the left bank of the Dale, &c. All these meteors, and that of the 25th of April, occurred between the hours of six and seven in the evening. But the very regular spectral coloration of the portion common to the two arcs has still to be accounted for. The cause of its being more brilliant than the rest is explained by the superposition of the refracted rays in relation to the observer; but how are the two partial spectres expanded so as only to form one of twice the width?

The second meteorological phænomenon was seen in all its beauty on Saturday the 30th of May last. The Bise had blown the whole day, and not a vapour obscured the perfect clearness of the sky. The moon was only five days old. Shortly after sunset I remarked and pointed out to several persons who were with me, a very luminous band of that deep red which sometimes tinges the clouds in the west. It was single, vertical, and about 35° high. It presented no appearance of divergence; on the contrary, its sides, exactly parallel, were distant $1\frac{1}{2}°$ to 2°. It rested on the perfectly defined ridge of the Jura, near the spot where the sun went down. From this inferior limit, its brilliancy remained sensibly constant (in reality it diminished with the height, but the decreasing clearness in the atmosphere compensated this diminution of visibility) up to an elevation of about 25° above the horizon; still higher the red tint diminished rapidly in intensity, and finally merged in the blue of the sky, without its being possible to determine exactly this upper limit. It was exactly a quarter to eight o'clock when I made this observation; the sun had disappeared eight to ten minutes before. Possibly the band existed some instants before I perceived it. It continued to be visible for more than forty-five minutes, moving towards the north, as the sun did, but without ceasing to *appear* vertical, without inclining perceptibly toward the horizon; only it diminished progressively in brilliancy, and consequently in length.

This phænomenon was remarkably splendid, and excited the admiration of a great number of persons. Dr. H* * *, showing it to his son, exclaimed, "Look! there is the pillar of fire that shone by night in the camp of the Israelites!" At Aigle, east of Lausanne, people thought that the chief town was in flames.

From a rapid survey of the polar star, I found that this light was 50° west of the geographical meridian. Its dependence on the sun, its distance from the magnetic meridian, and the absence of abnormal perturbations in the magnetometer, remove any idea of an aurora borealis. On the other hand, the very decided parallelism of its lateral borders on an extent of 35°, places this phænomenon out of the class of ordinary crepuscular rays. It is distinguished from those which were particularly investigated by M. L. A. Necker, by its appearance after sunset, by the absence of dark bands on its right and left, by its gigantic dimensions, and lastly by its character of constant width and solitude. If the point of view from which it was observed took from it the illusory appearance of divergence of the crepuscular rays, it seems necessary to attribute its production to other causes than the clouds, to which that able geologist and mineralogist attributed the common origin of these rays.

The following day, May 31, this same light was visible, but very feebly. The sky, although clear, had not the rare transparency of the preceding evening. The sun disappeared at about twenty-five minutes past seven; the band was seen towards ten minutes past eight 7° more to the north than the point of setting, and traversed about 2° in five minutes. Various accounts which I have received agree in tracing back the first appearance of this phænomenon to the middle of last April. The 21st of May is stated as the day when the meteor was visible for an hour and a half. On the 23rd the sky was very clear, the air calm, the meteor less brilliant than the evening before; it was seen suddenly on the horizon at a quarter past seven, and did not disappear till thirty-five minutes past eight. Lastly, on the 28th, the Bise was strong, and fleecy clouds were perceived around the luminous band, which lasted until forty-two minutes past eight. It appears certain that it was always seen single, vertical, and with parallel edges.

Lausanne, June 30, 1846.

XXXII. *On the Cause of Endosmose and Exosmose.*

By George Rainey, *M.R.C.S.*[*]

THE phænomena of endosmose and exosmose were attributed by Dutrochet to the passage of currents of electricity of unequal intensity through a membrane situated between two fluids of different densities, these currents being supposed to carry with them unequal quantities of fluid. Although the facts of endosmose and exosmose, as described by this philosopher, are admitted by all to be correct, yet his explanation of them has not been generally received; nor am I aware that they have ever been explained in a manner generally satisfactory, and therefore I shall be obliged if you will insert in your valuable journal the following explanation.

It is a fact which probably no one will question, that when two fluids of unequal density are brought into contact in minute quantities, without any membrane being interposed, they will intermix by diffusion, the rarer fluid intermingling itself with the denser one, and the denser fluid diffusing itself through the rarer one, until the one becoming gradually more dense, and the other less so, the whole mass acquires one uniform consistency. However, should this fact be doubted, it can be well illustrated by introducing between two surfaces of glass, placed almost in contact, some thick mucilage, and a little thin coloured fluid of any kind, for instance common writing-ink, and observing with the microscope the part where they become united, when the two fluids will be seen gradually to intermix, and to become so blended together, that their line of union will become lost.

The same fact can be shown by introducing some mucilage into a piece of thermometer tube, and after it, at the same end, some writing-ink. The coloured fluid will at first pass only in a small column through the mucilage, along the centre of the tube, leaving the mucilage in contact with its parietes, but afterwards they will become gradually blended together.

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[*] Communicated by the Author.

Also by filling a piece of thermometer tube with water, closing its upper extremity, and placing it vertically with its lower one in a solution of gum coloured with logwood, when the latter will gradually ascend and colour the former, showing that the diffusion is independent of gravity, and therefore may be inferred to be the result of that attraction which is exerted universally between the particles of matter, varying in the inverse ratio of the squares of their distances, and in the direct ratio of their masses; the particles in this instance, upon which the difference of density of the two fluids depends, being contained in a medium which allows of their free motion one upon another, cannot therefore come to a state of rest until every one of them is attracted equally on all sides, that is, until all the fluid is of one density. It is also a fact equally unquestionable, that if solutions of the same substance in the same menstruum, but of different densities, be filtered through the same membrane, the more rare fluid will pass through it with greater rapidity than the denser one. This is one of the effects of the same principle of attraction, the dense fluid being in this case more strongly attracted by the material of which the membrane is composed than the rare one.

Now, when two fluids of different densities are separated by a porous membrane, the porosities being merely a multitude of capillary tubes, like the space between the pieces of glass, or the tube of the thermometer, will allow of the passage through them of the particles of one solution into the other; and they will become mixed together by diffusion the same as if no regular membrane were present, as in the experiments just detailed; so that electricity may just as well be regarded as the cause of diffusion as of endosmose, these being the same. However, the membrane being traversed by an immensity of extremely minute porosities, will extend the surface of the two fluids between which it is placed, and in this way, bringing into close proximity a great number of particles, place them under circumstances very favourable for the operation of the force of attraction of one particle for another. But the characteristic feature of endosmose and exosmose is, that the rarer fluid passes through the interposed membrane more rapidly than the denser one; and therefore that an accumulation takes place on that side of it on which the latter was placed. The reason of this is obvious; for had it been otherwise, or even had these quantities been equal, then the rarer fluid must have repassed through the interposed membrane, after its density had been increased by its intermixture with the denser fluid, as rapidly as it passed through it before its density had been thus augmented; or from the commencement of the operation the dense fluid must have passed with the same velocity through the membrane as the rare one, which is contrary to the second fact. Therefore, in case of diffusion of two fluids of unequal densities through a porous partition, the fluid MUST *accumulate* on that side of the partition on which is situated that fluid which passed through it most slowly.

This explanation is applicable in all cases in which the rarer fluid on the one side of the membrane has the same chemical properties as that on the other side, their difference being only in their degree of density.

If these fluids be dissimilar in respect to their chemical qualities, still the accumulation will take place on that side of the membrane on which was situated the fluid which passed through it most slowly, although its density may be less than the other; thus, for instance, when a bladder full of air is surrounded with carbonic acid, the latter enters faster than the former escapes, and the bladder bursts. Now it is found by experiment, that carbonic acid passes through wet membrane quicker than atmospheric air, although of a greater density.

In such instances the relative velocities of the passage of different fluids through membrane will be influenced by their chemical attraction for the material of which the membrane is composed, as well as by that attraction which is regulated by quantity of matter; and in some cases the existence of the latter force may be so modified by the former as to be wholly inappreciable; but still that force, being an universal agent, and acting upon all matter according to invariable laws, is in operation in every instance where the conditions requisite for endosmose and exosmose are present, although in some cases chemical attraction or electricity may have no demonstrable existence; and therefore it may be looked upon as the essential cause of endosmose and exosmose, chemical affinity, electricity, &c. being merely the modifiers of its operation, and in respect to it, secondary agents.

XXXIII. *On some Formulæ which serve to indicate the limits of the application of Indeterminate Methods to the solution of certain Problems. By* JAMES COCKLE, *M.A., of Trinity College, Cambridge**.

THE equations distinguished below by unaccented numbers will respectively be found at p. 344, or one of the four succeeding pages, of the Report of the Sixth Meeting of

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* Communicated by Sir George Cayley, Bart.

the British Association for the Advancement of Science, to which, for the purpose of avoiding prolixity, the author begs leave to refer the reader of this paper. It will be seen that, using the notation there employed,

$$m(0,1,1) = 1 + m(1,1) = 4, \quad . \quad . \quad . \quad (336'.)$$

$$m(0,0,1,1) = 1 + m(1,1,1) = 6, \quad . \quad . \quad . \quad (337'.)$$

since $$m(1,1,1) = 5, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (307.)$$

$$m(0,0,0,1,1) = 1 + m(1,1,1,1) = 12, \quad . \quad (338'.)$$

since $$m(1,1,1,1) = 11; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (317.)$$

10 may however be written for 11 in this last equation, for reasons which will be manifest on referring to page 348 of the above-mentioned Report; so that we may make

$$m(0,0,0,1,1) = 11. \quad . \quad . \quad . \quad . \quad . \quad . \quad (339'.)$$

So $$m(0,0,0,0,1,1) = 1 + m(1,1,1,1,1) = 48, \quad . \quad (340'.)$$

for $$m(1,1,1,1,1) = 47; \quad . \quad . \quad . \quad . \quad . \quad (327.)$$

and

$$m(0,0,0,0,0,1,1) = 1 + m(1,1,1,1,1,1) = 924, \quad (341'.)$$

since $$m(1,1,1,1,1,1) = 923. \quad . \quad . \quad . \quad . \quad (328.)$$

Now each of the newly-valued functions may for convenience be represented by $m(0^{r-1}, 1^2)$,—a very obvious abbreviation. Let $n(0^{r-1}, 1^2)$ denote the corresponding function when, instead of the process to which the above equations refer, we apply that which the writer of this paper has used in the last two volumes of the present series of this Magazine. Then, by investigations similar to those pursued in the particular cases already therein treated, he has been conducted to the general condition,

$$n(0^{r-1}, 1^2) = 2r. \quad . \quad . \quad . \quad . \quad . \quad . \quad ('' .)$$

This last condition, which the author hopes to discuss on some future occasion, gives

$$n(0,1,1) = 4, \quad . \quad . \quad . \quad . \quad . \quad (336''.)$$

$$n(0,0,1,1) = 6, \quad . \quad . \quad . \quad . \quad . \quad (337''.)$$

$$n(0,0,0,1,1) = 8, \quad . \quad . \quad (338''.) \text{ or } (339''.)$$

$$n(0,0,0,0,1,1) = 10, \quad . \quad . \quad . \quad . \quad . \quad (340''.)$$

$$n(0,0,0,0,0,1,1) = 12. \quad . \quad . \quad . \quad . \quad . \quad (341''.)$$

On the condition marked (338″.) or (339″.) is founded a proposition announced at p. 405 of the last (44th) volume of the Mechanics' Magazine. The more general one, which will be found at p. 36 of No. 1196 of that work, is based upon the above condition (″.)

Of course the consideration of the above propositions will require some extensions of the formulæ given by the writer at p. 396 of the last volume of the Philosophical Magazine. We shall have, for instance, to reduce $f^5(w_m)$ to the form $\Sigma_m(h^5)$. To determine w_m we must employ a new equation of finite differences, viz.

$$w_{x+1} - 3 \,.\, 2^{2u_{2w_x}+1} + 2 = 0,$$

when $w_0 = 1$ and u has the same meaning as at the page last cited. Were it desirable, it would not be difficult to generalize this equation of differences so as to obtain the corresponding one for functions of any degree.

Grecian Chambers, Devereux Court,
July 20, 1846.

XXXIV. *Facts and Observations relating to the Science of Phonetics* (No. IV.). *By* R. G. Latham, *M.D., Cant.*[*]

In a previous Number it was stated that the mutes *s* and *z* had certain peculiarities. In explaining the nature of these it is necessary to enter into some points of classification and nomenclature.

The sounds *p*, *b*, *t*, *d*, *k*, *g*, *s*, *z* are called *lene* sounds (or *Lenes*), as opposed to *f*, *v*, þ, ð, χ, γ, σ, ζ, which are called *aspirate* sounds (or *Aspirates*). This division is as old as the time of the classical grammarians, and is generally recognised. Hence the division,—

Lene.			Aspirate.	
p	.	*b*	*f*	*v*
t	.	*d*	þ	ð
k	.	*g*	χ	γ
s	.	*z*	σ	ζ

By the side of this should be placed the division of sounds into what is called *explosive* and *continuous*. If we isolate the sound of *p*' as much as possible, so as to join with it no vowel whatsoever, we find that the articulation is only of a momentary duration, "taking place during a sudden change in the conformation of the mouth, and not capable of prolongation." (Müller's Physiology, Baly's translation, p. 1045.) The same is the case with the sounds of *b*, *t*, *d*, *k*, *g*.

In contrast to this we may *isolate* such a sound as *f*'. Here the sound can be "prolonged, *ad libitum*, as long as a particular disposition of the mouth and a constant expiration are maintained" (Ibid.).

It is upon these differences that the names *explosive* (*strepitus incontinuus explosivus*) and *continuous* (*strepitus continuus*) are given by even the earlier writers upon these subjects; and,

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[*] Communicated by the Author.

undoubtedly, they may with great propriety and convenience be retained.

Now the divisions into *lene* and *aspirate* and the divisions into *explosive* and *continuous* more or less coincide. Besides the vowels, semivowels, and liquids, *all* the aspirates are *continuous*; and *nearly* all the lenes (*p*, *b*, *t*, *d*, *k*, *g*) are *explosive*. But *s* and *z* are at once both *lene* and *continuous*; and this constitutes their peculiarity, a peculiarity which I am satisfied with indicating, being, at present, unable to explain.

In asking how far the different classes of articulate sounds have the characters of *explosiveness* or *continuity*, the answers are as follows:—

1. That all the *vowels* are *continuous*.
2. That both the *semivowels* are *continuous*.
3. That *h* is *continuous*.
4. That all the liquids are *continuous*.
5. That *all* the *aspirate mutes* are *continuous*.
6. That *two* of the *lene mutes* are *continuous*.
7. That the remaining *lene mutes* are *explosive*; and that nothing else is so.

* * * *

The same question that has just been put respecting the distribution of the *explosive* and *continuous* sounds may now be put respecting the sounds called, after the fashion of the Sanskrit grammarians, *surd* and *sonant*; the former term denoting sounds like *p*, *t*, *k*, *s*, *sh*, &c., where the utterance is in a whisper; the latter denoting sounds like *b*, *d*, *z*, *l*, *a*, &c., where the utterance is *vocal*, loud, or at the natural pitch of the human voice. Now,—

1. All the vowels are *sonant*.
2. Both the semivowels are *sonant*.
3. All the liquids are *sonant*.
4. Half the mutes are *sonant*, viz. *b* . *v* . *d* . ð . *g* . γ . *z* . ζ.
5. Half the mutes are *surd*, viz. *p*, *f*, *t*, þ, *k*, χ, *s*, σ.
6. The aspirate *h* is surd.

Surd sounds are formed by breath unmodified by any action of the larynx whatever.

Sonant sounds are formed by breath thrown into vocalism by some action of the larynx, the precise nature of which has yet to be determined.

The power of *h* is the type of surd sounds. The superaddition of certain motions of the *lips*, *tongue*, and *velum palati* convert this simple breathing in *p*, *f*, *t*, &c.

No form of vocal sound is so simple as that of a mere breathing. Herein the air passes *from* the lungs through the throat and mouth without any action of the *chordæ vocales*.

It is almost doubtful whether it be convenient or not to call this sound an articulation. However, it enters into the constitution of speech, and is expressed in English by the letter *h*.

A sound equally simple may be formed by drawing *in* a breath, when the sound is formed by a current of air passing *from* the mouth *to* the lungs. This is the sound of *h* formed by means of an inspiration. The existence of this sound in *speech* is problematical. In some of the Hottentot languages described as containing, amongst their fundamental sounds, inarticulate clicks and catches, it may possibly exist.

By breathing an expiration through the nose instead of through the mouth, we may form a nasal variation of *h*.

By drawing a breath through the nose, instead of through the mouth, we may form a nasal variation of the second sound.

Hence, of even the simplest breathing, there are four possible forms.

I. Oral, where the breath passes through the mouth.
    a. The breath expired = *h*.
    b. The breath inspired.

II. Nasal, where the current of breath passes through the nose.
    a. The breath expired.
    b. The breath inspired.

Of these four possible varieties of the simple breathing, it is only the first that is *known* to be used in speech. The sounds formed by mere breathings are of course both surd and continuous. The consideration of the sounds formed by passing a current of air through the nostrils is important.

The possible varieties of a simple current of unmodified breath have thus been enumerated. The difference between *ex*pired and *in*spired forms is of little practical importance, since the articulations of real language are, with a few problematical exceptions, all *ex*pired. On the other hand, the difference between air *ex*pired through the mouth and air *ex*pired through the nostrils, has (as will be shown in some future communication) been much underrated.

XXXV. Newton's *Letters to* Oldenburg *and* Boyle *respecting his Theory of Light and Colours.*

To Richard Taylor, Esq.

Dear Sir,

THE letter to Oldenburg, of which I gave an account in one of your former Numbers, as containing the first draught of Newton's speculations respecting an ætherial medium, certainly deserves more notice than it has obtained. It has scarcely, I think, been quoted, except by Dr. Young; and its existence is but little known, even among the best-informed scientific men, notwithstanding its publication (with some verbal inaccuracies) by Dr. Birch in his History of the Royal Society. Under these circumstances, I have no doubt that a reprint of it would be very acceptable as a curious matter of history, and especially as tending to throw light on the character of Newton's mind.

The letter to Boyle is more accessible; but the circumstance of its containing the earliest attempt, as I have pointed out in my letter to Lord Brougham, to explain the nature of gaseous substances, has failed to attract the notice which it merits at the hands of the historians of chemistry.

I remain, dear Sir,
Yours faithfully,
Bolton Percy, August. WILLIAM VERNON HARCOURT.

Mr. Isaac Newton's *Letter, Hypothesis, Observations and Experiments touching his Theory of Light and Colours; in confirmation and illustration of his former discourse on the same subject*[*].

SIR,

I HAVE sent you the papers I mentioned by John Stiles. Upon reviewing them, I find something so obscure as might have deserved a further explanation by schemes, and some other things I guess will not be new to you, though almost all was new to me when I wrote them. But as they are, I hope you will accept of them, though not worth the ample thanks you sent. I remember in some discourse with Mr. Hook, I happened to say that I thought light was reflected, not by the parts of glass, water, air, or other sensible bodies, but by the same confine or superficies of the ætherial mediums which refracts it, the rays finding some difficulty to get through it, in passing out of the denser into the rarer medium, and a greater difficulty in passing out of the rarer into the denser; and so, being either refracted or reflected by that superficies as the circumstances they happened to be in at their incidence, make them able, or unable, to get through it. And for confirmation of this, I said further, that I thought the reflexion of light at its tending out of glass into air, would not be diminished or weakened by drawing away the air in an air-pump, as it ought to be, if they were the parts of air

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[*] Read before the Royal Society, December 9, 1675, and some following days of their meeting.

that reflected; and added that I had not tried this experiment, but thought he was not unacquainted with notions of this kind. To which he replied, that the notion was new, and he would, the first opportunity, try the experiment I propounded. But, upon reviewing the papers I send you, I found it there set down for tried, which makes me recollect that about the time I was writing those papers, I had occasionally observed in an air-pump here, at Christ's College, that I could not perceive the reflexion of the inside of the glass diminished in drawing out the air. This I thought fit to mention, lest my former forgetfulness, through having long laid aside my thoughts on these things, should make me seem to have set down for certain what I never tried.

Sir, I had formerly purposed never to write any hypothesis of light and colours, fearing it might be a means to engage me in vain disputes; but I hope a declared resolution to answer nothing that looks like a controversy (unless possibly at my own time upon some other by-occasion) may defend me from that fear. And therefore considering that such an hypothesis would much illustrate the papers I promised to send you, and having a little time this last week to spare, I have not scrupled to describe one so far as I could on a sudden recollect my thoughts about it, not concerning myself whether it be thought probable or improbable, so it do but render the papers I send you, and others sent formerly, more intelligible. You may see, by the scratching and interlining, it was done in haste, and I have not had time to get it transcribed, which makes me say I reserve the liberty of adding or altering it, and desire that you would return those and the other papers when you have done with them. I doubt there is too much to be read at one time, but you will soon know how to order that. At the end of the hypothesis you'll see a paragraph to be inserted, as is there directed. I should have added another or two, but I had not time, but such as it is, I hope you will accept it.

Sir, I am,
Your humble Servant,
(Signed) I. Newton.

Mr. I. Newton's *Letter sent to* H. O.[*]

An Hypothesis explaining the Properties of Light discoursed of in my several papers.

Sir,

In my answer to Mr. Hook you may remember I had

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[*] Registry Book of the Royal Society, vol. v., from 1675 to 1679.

occasion to say something of hypotheses where I gave a reason why all allowable hypotheses in their genuine constitution should be conformable to my theories, and said of Mr. Hook's hypothesis, that I took the most free and natural application of it to phænomena to be this:—"That the agitated parts of bodies, according to their several sizes, figure and motions, do excite vibrations in the æther of various depths or bignesses, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours, much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which according to those bignesses make several tones in sound, &c.*" I was glad to understand, as I apprehended from Mr. Hook's discourse at my last being at one of your assemblies, that he had changed his former notion of all colours being compounded of only two original ones, made by the two sides of an oblique pulse, and accommodated his hypothesis to this my suggestion of colours, like sounds, being various, according to the various bigness of the pulses. For this I take to be a more plausible hypothesis than any other described by former authors; because I see not how the colours of thin transparent plates, or skins, can be handsomely explained without having recourse to ætherial pulses. But yet I like another hypothesis better, which I had occasion to hint something of in the same letter in these words:—"The hypothesis of light's being a body, had I propounded it, has a much greater affinity with the objector's own hypothesis than he seems to be aware of, the vibrations of the æther being as useful and necessary in this as in his. For assuming the rays of light to be small bodies emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the æther as stones do in water when thrown into it. And supposing these vibrations to be of several depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities, of what use they will be for explicating the manner of reflexion and refraction, the production of heat by the sun beams, the emission of light from burning, putrifying, or other substances whose parts are vehemently agitated, the phænomena of thin transparent plates and bubbles, and of all natural bodies, the manner of

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* Birch's History of the Royal Society, vol. iii. p. 248.

vision, and the difference of colours, as also their harmony and discord, I shall leave to their consideration who may think it worth their endeavour to apply this hypothesis to the solution of phænomena*." Were I to assume an hypothesis, it should be this, if propounded more generally so as not to determine what light is, further than that it is something or other capable of exciting vibrations in the æther; for thus it will become so general and comprehensive of other hypotheses as to leave little room for new ones to be invented; and therefore because I have observed the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted an hypothesis to explain them by, and found that some, when I could not make them take my meaning when I spake of the nature of light and colours abstractedly, have readily apprehended it when I illustrated my discourse by an hypothesis; for this reason I have here thought fit to send you a description of the circumstances of this hypothesis, as much tending to the illustration of the papers I herewith send you; and though I shall not assume either this or any other hypothesis, not thinking it necessary to concern myself whether the properties of light discovered by me be explained by this, or by Mr. Hook's, or any other hypothesis capable of explaining them, yet while I am describing this, I shall, sometimes to avoid circumlocution and to represent it more conveniently, speak of it as if I assumed it and propounded it to be believed. This I thought fit to express, that no man may confound this with my other discourses, or measure the certainty of the one by the other, or think me obliged to answer objections against this script; for I desire to decline being involved in such troublesome, insignificant disputes.

But to proceed to the hypothesis.—1. It is to be supposed therein, that there is an ætherial medium, much of the same constitution with air, but far rarer, subtiler, and more strongly elastic. Of the existence of this medium, the motion of a pendulum in a glass exhausted of air almost as quickly as in the open air is no inconsiderable argument†. But it is not to be

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* Letter to O. Camb. July 11, 1672. Phil. Trans. No. 88. p. 5087–8.

† Either we must presume that a word is here omitted in the manuscript, and that the sentence should stand thus—"the motion of a pendulum '*ceasing*' in a glass exhausted of air almost as quickly as in open air," and suppose this statement made on the authority of Boyle's experiments, in which the difference of the times of vibration in the two cases was "scarce sensible" (New Exp. Phys.-Mech., Exp. 26),—or we must conclude that Newton had already, in 1675, arrived, by experiments of his own, at the same conclusion to which Derham and Hawksbee came in 1704, that in consequence of the extension of the arcs of vibration, the vibrations, though quicker in their rates, are in their times "slower in the exhausted

supposed that this medium is one uniform matter, but composed partly of the main phlegmatic body of æther, partly of other various ætherial spirits, much after the manner that air is compounded of the phlegmatic body of air intermixed with various vapours and exhalations. For the electric and magnetic effluvia, and the gravitating principle, seem to argue such variety. Perhaps the whole frame of nature may be nothing but various contextures of some certain ætherial spirits or vapours, condensed as it were by precipitation, much after the manner that vapours are condensed into water, or exhalations into grosser substances, though not as easily condensable, and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, which, by virtue of the command increase and multiply, became a complete imitator of the copy set her by the Protoplast. Thus perhaps may all things be originated from æther.

At least the electric effluvia seem to instruct us that there is something of an ætherial nature condensed in bodies. I have sometimes laid upon a table a round piece of glass about two inches broad, set in a brass ring, so that the glass might be about one-eighth or one-sixth of an inch from the table, and the air between them inclosed on all sides by the ring, after the manner as if I had whelmed a little sieve upon the table. And then rubbing a pretty while the glass briskly with some rough and raking stuff, till some very little fragments of very thin paper laid on the table under the glass began to be attracted and move nimbly to and fro, after I had done rubbing the glass, the papers would continue a pretty while in various motions, sometimes leaping up to the glass

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than in the unexhausted receiver" (Phil. Trans. No. 294. p. 1785). In the 6th Section of the 2nd Book of the *Principia*, in the scholium to the 31st Prop., Newton has given the results of an experiment made by him for the purpose of determining whether the vibration of bodies affords any indication of a resisting medium as present in their internal pores, independent of the resistance which the air makes to the movement of their surfaces. The principal object of this experiment seems to have been, to demonstrate that there exists no resistance of this description equal to that which Descartes's theory of a *plenum* would require. Newton found the resistance of the internal parts of the box with which he made the experiment at least more than 5000 times less than that of its surface. A box, first empty, and then loaded with metal, was swung by a thread of 11 feet long; the resistance of the full box compared with that of the empty, by the ratio of the weights to the number of oscillations within measured distances, was found to be in a proportion not greater than that of 78 to 77. The circumstances of the experiment, however, did not admit of such accuracy as to carry its import beyond the negative object for which it was instituted; and Newton, in the reasons which he has subsequently assigned for admitting the existence of an æther, made no use of this.

and resting there awhile, then leaping down and resting there, then leaping up, and perhaps down and up again, and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also they would leap up in one arch and down in another divers times together, without sensible resting between, sometimes skip in a bow from one part of the glass to another without touching the table, and sometimes hang by a corner and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind, and be otherwise variously moved, every paper with a divers motion. And upon sliding my finger on the upper side of the glass, though neither the glass nor the enclosed air below were moved thereby, yet would the papers as they hung under the glass receive some new motion, inclining this way or that way, accordingly as I moved my finger. Now whence all these irregular motions should spring I cannot imagine, unless from some kind of subtile matter lying condensed in the glass, and rarefied by rubbing, as water is rarefied into vapour by heat, and in that rarefaction diffused through the space round the glass to a great distance, and made to move and circulate variously, and accordingly to actuate the papers, till it returns into the glass again and be recondensed there. And as this condensed matter by rarefaction into an ætherial wind (for by its easy penetrating and circulating through glass I esteem it ætherial) may cause these odd motions, and by condensing again may cause electrical attraction with its returning to the glass to succeed in the place of what is there continually recondensed; so may the gravitating attraction of the earth be caused by the continual condensation of some other such like ætherial spirit, not of the main body of phlegmatic æther, but of something very thinly and subtilely diffused through it, perhaps of an unctuous, or gummy tenacious and springy nature; and bearing much the same relation to æther which the vital aërial spirit requisite for the conservation of flame and vital motions does to air. For if such an ætherial spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter for the continual uses of nature (adhering to the sides of those pores after the manner that vapours condense on the sides of a vessel), the vast body of the earth, which may be everywhere to the very centre in perpetual working, may continually condense so much of this spirit as to cause it from above to descend with great celerity for a supply: in which descent it may bear down with it the bodies it pervades with force proportional to the superficies of all their parts it acts upon, nature making a circulation by the slow ascent of so much matter out of the bowels of the earth in an aërial form, which for a time constitutes the atmosphere, but being continually buoyed up by the new air, exhalations, and vapours rising underneath, at length (some part of the vapours which return in rain excepted) vanishes again into the ætherial spaces, and there perhaps in time relents and is attenuated into its first principle. For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtile out of gross, and gross out of subtile, some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him: and they that will may also suppose that this spirit affords or carries with it thither the solary fuel and material principle of light, and that the vast ætherial spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of ætherial natures by the bye.

In the second place, it is to be supposed that the æther is a vibrating medium like air, only the vibrations far more swift and minute; those of air made by a man's ordinary voice, succeeding one another at more than half a foot or a foot distance, but those of æther at a less distance than the hundred-thousandth of an inch. And as in air the vibrations are some larger than others, but yet all equally swift (for in a ring of bells the sound of every tone is heard at two or three miles distance in the same order that the bells are struck), so I suppose the ætherial vibrations differ in bigness, but not in swiftness. Now these vibrations, besides their use in reflexion and refraction, may be supposed the chief means by which the parts of fermenting and putrifying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion, are shaken asunder like a ship by waves, and dissipated into vapours, exhalations, or smoke, and light loosed or excited in those bodies, and consequently by which a body becomes a burning coal, and smoke flame; and I suppose flame is nothing but the particles of smoke turned by the access of light and heat to burning coals, little and innumerable.

Thirdly, the air can pervade the bores of small glass pipes, but yet not so easily as if they were wider, and therefore stands at a greater degree of rarity than in the free aërial spaces, and at so much greater a degree of rarity as the pipe is smaller, as is known by the rising of water in such pipes to a much greater height than the surface of the stagnating water into which they are dipped*. So I suppose æther, though it pervades the pores of crystal, glass, water, and other natural bodies, yet it stands at a greater degree of rarity in those pores than in the free ætherial spaces, and at so much greater a degree of rarity as the pores of the body are smaller. Whence it may be that spirit of wine, for instance, though a lighter body, yet having subtiler parts, and consequently smaller pores than water, is the more strongly refracting liquor. This also may be the principal cause of the cohesion of the parts of solids and fluids, of the springiness of glass and other bodies whose parts slide not one upon another in bending, and of the standing of the mercury in the Torricellian experiment, sometimes to the top of the glass, though a much greater height than twenty-nine inches. For the denser æther which surrounds these bodies must crowd and press their parts together, much after the manner that air surrounding two marbles presses them together if there be little or no air between them. Yea, and that puzzling problem, *by what means the muscles are contracted and dilated to cause animal motion, may receive greater light from hence than from any other means men have hitherto been thinking on.* For if there be any power in man to condense and dilate at will the æther that pervades the muscle, that condensation or dilatation must vary the compression of the muscle made by the ambient æther, and cause it to swell, or shrink, accordingly; for though

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* The imprinted paper of "Observations" which accompanied this, and which was the first form of the "Optics," contained a passage on the present subject, omitted in that publication, which deserves to be quoted for the acknowledgment it contains of the merit of Hook. In the third prop. of the second book, Newton remarks:—"To the increase of the opacity of these bodies it conduces something, that by the 23rd observation, the reflexion of thin transparent substances are considerably stronger than those made by the same substances of a greater thickness." Here the paper subjoins,—"And to the reflexion of solid bodies it may be further added, that the interstices of their parts are void of air. For that for the most part they are so is reasonable to believe, considering the inaptitude which air hath to pervade small cavities, as appears by the ascension of water in slender glass pipes, paper, cloth, and other such like substances, whose pores are found too small to be replenished with air, and yet large enough to admit water, and by the difficulty wherewith air pervades the pores of a bladder, through which water finds ready passage. And according to the 11th observation, the cavities thus void of air will cause the same kind of effect, as to reflexion, which those do that are replenished with it; but yet something more manifestly, because the medium in relation to refractions is rarest when most empty of air, as Mr. Hook hath proved in his *Micographia*, in which book he hath also largely discoursed of this and the precedent proposition, and delivered many other very excellent things concerning the colour of thin plates, and other natural bodies, which I have not scrupled to make use of as far as they were for my purpose."

common water will scarce shrink by compression and swell by relaxation, yet (so far as my observation reaches) spirit of wine and oil will; and Mr. Boyle's experiment of a tadpole shrinking very much by hard compressing the water in which it swam, is an argument that animal juices do the same: and as for their various pression by the ambient æther, it is plain that that must be more or less, accordingly as there is more or less æther within to sustain and counterpoise the pressure of that without. If both æthers were equally dense, the muscle would be at liberty as if pressed by neither: if there were no æther within, the ambient would compress it with the whole force of its spring. If the æther within were twice as much dilated as that without, so as to have but half as much springiness, the ambient would have half the force of its springiness counterpoised thereby, and exercise but the other half upon the muscle; and so in all other cases the ambient compresses the muscle by the excess of the force of its springiness above that of the springiness of the included. To vary the compression of the muscle therefore, and so to swell and shrink it, there needs nothing but to change the consistence of the included æther; and a very little change may suffice, if the spring of æther be supposed very strong, as I take it to be many degrees stronger than that of air.

Now for the changing the consistence of the æther, some may be ready to grant that the soul may have an immediate power over the whole æther in any part of the body, to swell or shrink it at will; but then how depends the muscular motion on the nerves? Others therefore may be more apt to think it done by some certain ætherial spirit included within the *dura mater*, which the soul may have power to contract or dilate at will in any muscle, and so cause it to flow thither through the nerves; but still there is a difficulty why this force of the soul upon it does not take off the power of springiness, whereby it should sustain more or less the force of the outward æther. A third supposition may be, that the soul has a power to inspire any muscle with this spirit, by impelling it thither through the nerves; but this too has its difficulties; for it requires a forcible introducing the spring of the æther in the muscles by pressure exerted from the parts of the brain; and it is hard to conceive how so great force can be exercised amidst so tender matter as the brain is; and besides, why does not this ætherial spirit, being subtile enough, and urged with so great force, go away through the *dura mater* and skins of the muscle, or at least so much of the other æther go out to make way for this which is crowded in? To take away these difficulties is a digression, but seeing the subject is a deserving one, I shall not stick to tell you how I think it may be done.

First, then, I suppose there *is* such a spirit; that is, that the animal spirits are neither like the liquor, vapour, or gas, of spirits of wine; but of an ætherial nature, subtile enough to pervade the animal juices as freely as the electric, or perhaps magnetic, effluvia do glass. And to know how the coats of the brain, nerves, and muscles, may become a convenient vessel to hold so subtile a spirit, you may consider how liquors and spirits are disposed to pervade, or not pervade, things on other accounts than their subtilty; water and oil pervade wood and stone, which quicksilver does not; and quicksilver, metals, which water and oil do not; water and acid spirits pervade salts, which oil and spirit of wine do not; and oil and spirit of wine pervade sulphur, which water and acid spirits do not; so some fluids (as oil and water), though their parts are in freedom enough to mix with one another, yet by some secret principle of *unsociableness* they keep asunder; and some that are *sociable* may become *unsociable* by adding a third thing to one of them, as water to spirit of wine by dissolving salt of tartar in it. The like *unsociableness* may be in ætherial natures, as perhaps between the æthers in the vortices of the sun and planets; and the reason why air stands rarer in the bores of small glass pipes, and æther in the pores of bodies, may be, not want of subtilty, but *sociableness**; and on this ground, if the ætherial vital spirit in a man be very *sociable* to the marrow and juices, and *unsociable* to the coats of the brain, nerves, and muscles, or to any thing lodged in the pores of those coats, it may be contained thereby, notwithstanding its subtilty; especially if we suppose no great violence done to it to squeeze it out, and that it may not be altogether so subtile as the main body of æther, though subtile enough to pervade readily the animal juices, and that as any of it is spent, it is continually supplied by new spirit from the heart.

In the next place, for knowing how this spirit may be used for animal motion, you may consider how some things unsociable are made sociable by the mediation of a third. Water, which will not dissolve copper, will do it if the copper be melted with sulphur. Aquafortis, which will not pervade gold, will do it by addition of a little sal-ammoniac or spirit of salt. Lead will not mix in melting with copper; but if a little tin, or antimony, be added, they mix readily, and part again of their own accord, if the antimony be wasted by throwing

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* In the third book of the Optics, Newton states more accurately the true theory of capillary attraction; but the *germ* of that theory is certainly contained in these expressions.

saltpetre, or otherwise. And so lead melted with silver quickly pervades and liquifies the silver in a much less heat than is required to melt the silver alone; but if they be kept in the test till that little substance that reconciled them be wasted or altered, they part again of their own accord. And in like manner the ætherial animal spirit in a man may be a mediator between the common æther, and the muscular juices, to make them mix more freely, and so by sending a little of this spirit into any muscle, though so little as to cause no sensible tension of the muscle by its own force, yet by rendering the juices more sociable to the common external æther, it may cause that æther to pervade the muscle of its own accord in a moment more freely and more copiously than it would otherwise do, and to recede again as freely, so soon as this mediator of sociableness is retracted; whence, according to what I said above, will proceed the swelling or shrinking of the muscle, and consequently the animal motion depending thereon.

Thus may therefore the soul, by determining this ætherial animal spirit or wind into this or that nerve, perhaps with as much ease as air is moved in open spaces, cause all the motions we see in animals; for the making which motions strong, it is not necessary that we should suppose the æther within the muscle very much condensed, or rarefied, by this means, but only that its spring is so very great that a little alteration of its density shall cause a great alteration in the pressure. And what is said of muscular motion may be applied to the motion of the heart, only with this difference; that the spirit is not sent thither as into other muscles, but continually generated there by the fermentation of the juices with which its flesh is replenished, and as it is generated, let out by starts into the brain, through some convenient *ductus*, to perform those motions in other muscles by inspiration, which it did in the heart by its generation. For I see not why the ferment in the heart may not raise as subtile a spirit out of its juices, to cause those motions, as rubbing does out of a glass to cause electric attraction, or burning out of fuel to penetrate glass, as Mr. Boyle has shown, and calcine by corrosion metals melted therein.

Hitherto I have been contemplating the nature of æther and ætherial substances by their effects and uses, and now I come to join therewith the consideration of light.

In the fourth place, therefore, I suppose light is neither æther, nor its vibrating motion, but something of a different kind propagated from lucid bodies. They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies at great distances one after the other, but yet without any sensible interval of time, and continually urged forward by a principle of motion, which in the beginning accelerates them, till the resistance of the ætherial medium equal the force of that principle, much after the manner that bodies let fall in water are accelerated till the resistance of the water equals the force of gravity. God, who gave animals motion beyond our understanding, is without doubt able to implant other principles of motions in bodies which we may understand as little. Some would readily grant this may be a spiritual one; yet a mechanical one might be shown, did not I think it better to pass it by. But they that like not this, may suppose light any other corporeal emanation, or an impulse or motion of any other medium or ætherial spirit diffused through the main body of æther, or what else they imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy; only whatever light be, I would suppose it consists of successive rays differing from one another in contingent circumstances, as bigness, force, or vigour, like as the sands on the shore, the waves of the sea, the faces of men, and all other natural things of the same kind differ, it being almost impossible for any sort of things to be found without some contingent variety. And further, I would suppose it diverse from the vibrations of the æther, because (besides that were it those vibrations, it ought always to verge copiously in crooked lines into the dark or quiescent medium, destroying all shadows, and to comply readily with any crooked pores or passages as sounds do) I see not how any superficies (as the side of a glass prism on which the rays within are incident at an angle of about forty degrees) can be totally opake. For the vibrations beating against the refracting confine of the rarer and denser æther must needs make that pliant superficies undulate, and those undulations will stir up and propagate vibrations on the other side. And further, how light, incident on very thin skins or plates of any transparent body, should for many successive thicknesses of the plate in arithmetical progression, be alternately reflected and transmitted, as I find it is, puzzles me as much. For though the arithmetical progression of those thicknesses, which reflect and transmit the rays alternately, argues that it depends upon the number of vibrations between the two superficies of the plate, whether the ray shall be reflected or transmitted, yet I cannot see how the number should vary the case, be it greater or less, whole or broken, unless light be supposed something else than these vibrations. Something indeed I could fancy towards helping the two last difficulties, but nothing which I see not insufficient.

Fifthly, it is to be supposed that light and æther mutually act upon one another, æther in refracting light, and light in warming æther, and that the densest æther acts most strongly. When a ray therefore moves through æther of uneven density, I suppose it is most pressed, urged, or acted upon by the medium on that side towards the denser æther, and receives a continual impulse or ply from that side to recede towards the rarer, and so is accelerated if it move that way, or retarded if the contrary. On this ground, if a ray move obliquely through such an unevenly dense medium (that is, obliquely to those imaginary superficies which run through the equally dense parts of the medium, and may be called the refracting superficies), it must be incurved, as it is found to be by observation in water*, whose lower parts were made gradually more salt, and so more dense than the upper. And this may be the ground of all refraction and reflexion. For as the rarer air within a small glass pipe, and the denser without, are not distinguished by a mere mathematical superficies, but have air between them at the orifice of the pipe running through all intermediate degrees of density; so I suppose the refracting superficies of æther between unequally dense mediums to be not a mathematical one, but of some breadth, the æther therein at the orifices of the pores of the solid body being of all intermediate degrees of density between the rarer and denser ætherial mediums; and the refraction I conceive to proceed from the continual incurvation of the ray all the while it is passing the physical superficies. Now if the motion of the ray be supposed in this passage to be increased or diminished in a certain proportion, according to the difference of the densities of the ætherial mediums, and the addition or detraction of the motion be reckoned in the perpendicular from the refracting superficies, as it ought to be, the sines of incidence and refraction will be proportional according to what Descartes has demonstrated.

The ray therefore in passing out of the rarer medium into the denser, inclines continually more and more towards parallelism with the refracting superficies; and if the differing densities of the mediums be not so great, nor the incidence of the ray so oblique as to make it parallel to that superficies before it gets through, then it goes through and is refracted; but if through the aforesaid causes the ray becomes parallel to that superficies before it can get through, then it must turn back and be reflected. Thus, for instance, it may be observed

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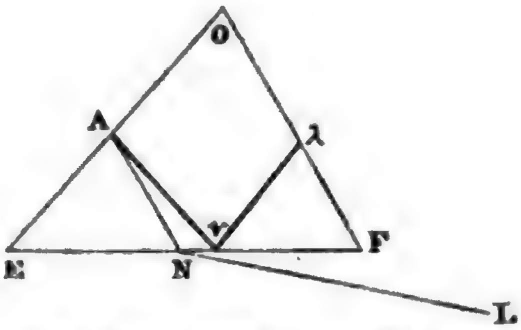
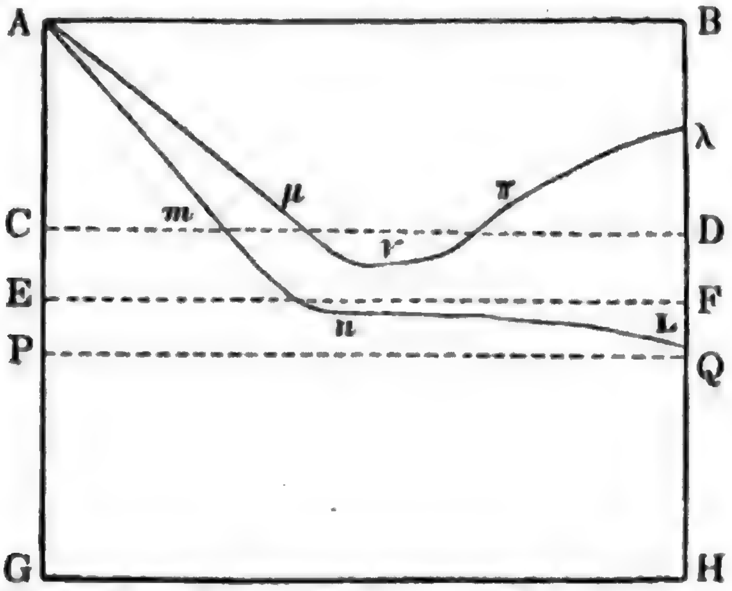
* Mr. Hook's Micrographia where he speaks of the inflexion of rays.

in a triangular glass prism O E F, that the rays A N that tend out of the glass into air, do, by inclining them more and more to the refracting superficies, emerge more and more obliquely till they be infinitely oblique, that is, in a manner parallel to the superficies, which happens when the angle of incidence is about 40°; and then if they be a little more inclined, are all reflected, as at A ν λ, becoming, I suppose, parallel to the superficies before they can get through it.



Let A B C D represent the rarer medium, E F H G the



denser, C D F E the space between them or refracting physical superficies, in which the æther is of all intermediate degrees of density, from the rarest æther at C D to the densest at E F; A *m n* L a ray, A *m* its incident part, *m n* its incurvation by the refracting superficies, and *n* L its emergent part. Now if the ray A *m* be so much incurved as to become at its emergence *n*, as nearly as may be, parallel to C D, it is plain that if that ray had been incident a little more obliquely, it must have become parallel to C D before it had arrived at E F, the further side of the refracting superficies, and so could have got no nearer to E F, but must have turned back by further incurvation, and been reflected as it is represented at A μ ν λ: and the like would have happened if the density of the æther had further increased from E F to P Q, so that P Q H G might be a denser medium than E F H G was supposed; for then the ray in passing from *m* to *n*, being so much incurved as at *n* to become parallel to C D or P Q, it's impossible it should ever get nearer to P Q, but must at *n* begin by further incurvation to turn back, and so be reflected. And because if a refracted ray (as n L) be made incident, the incident (A m) shall become the refracted, and therefore if the ray A $\mu\nu$, after it is arrived at ν, where I suppose it parallel to the refracting superficies, should be reflected perpendicularly back, it would return back in the line of incidence $\nu\mu$ A; therefore going forward, it must go forward in such another line $\nu\pi\lambda$, both cases being alike, and so be reflected at an angle equal to that of incidence.

This may be the cause and manner of reflexion, when light tends from the rarer towards the denser æther; but to know how it should be reflected when it tends from the denser towards the rarer, you are farther to consider, how fluids near their superficies are less pliant and yielding than in their more inward parts, and if formed into thin plates or shells, they become much more stiff and tenacious than otherwise. Thus things which readily fall in water, if let fall upon a bubble of water, they do not easily break through it, but are apt to slide down by the sides of it, if they be not too big and heavy. So if two well-polished convex glasses, ground on very large spheres, be laid one upon the other, the air between them easily recedes till they almost touch, but then begins to resist so much that the weight of the upper glass is too little to bring them together, so as to make the black (mentioned in the other papers I send you) appear in the midst of the rings of colours. And if the glasses be plain, though no broader than a twopence, a man with his whole strength is not able to press all the air out from between them, so as to make them fully touch. You may observe also that insects will walk upon water without wetting their feet, and the water bearing them up; also motes falling upon water will often lie long upon it without being wetted. And so I suppose æther in the confine of two mediums is less pliant and yielding than in other places, and so much the less pliant by how much the mediums differ more in density; so that in passing out of denser æther into rarer, when there remains but a very little of the denser æther to be passed through, a ray finds more than ordinary difficulty to get through, and so great difficulty where the mediums are of a very differing density as to be reflected by incurvation after the manner described above, the parts of æther on the side where they are less pliant and yielding, acting upon the ray much after the manner that they would do were they denser there than on the other side; for the resistance of the medium ought to have the same effect on the ray from whatsoever cause it arises. And this I suppose may be the cause of the reflexion of quicksilver and other metalline bodies. It must also concur to increase the reflective virtue of the superficies when rays tend out of the rarer medium into the denser; and in that case therefore the reflexion having a double cause ought to be stronger than in the æther, as it is apparently. But in refraction this rigid tenacity or unpliableness of the superficies need not be considered, because so much as the ray is thereby bent in passing to the most tenacious and rigid part of the superficies, so much is it thereby unbent again in passing on from thence through the next parts gradually less tenacious.

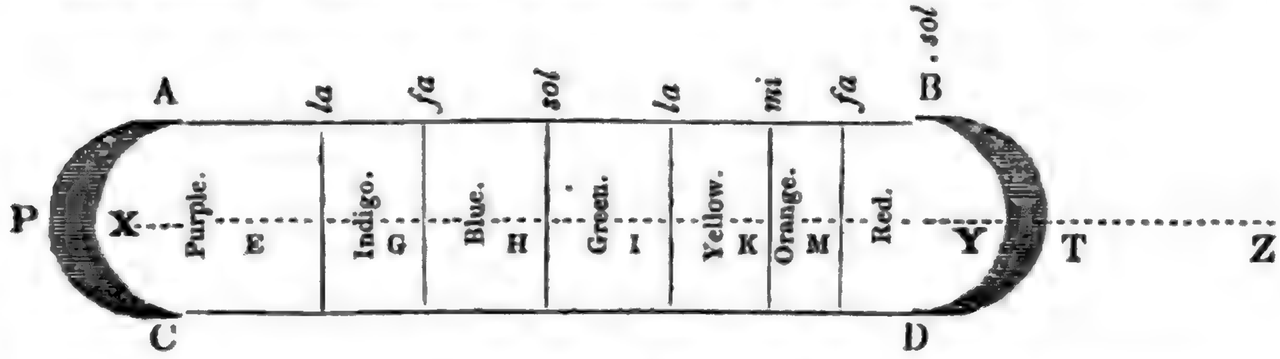
Thus may rays be refracted by some superficies and reflected by others, be the medium they tend into denser or rarer. But it remains further to be explained, how rays alike incident on the same superficies (suppose of crystal, glass or water), may be at the same time, some refracted, others reflected; and for explaining this, I suppose that the rays when they impinge on the rigid resisting ætherial superficies, as they are acted upon by it, so they react upon it, and cause vibrations in it, as stones thrown into water do in its surface, and that these vibrations are propagated every way into both the rarer and denser mediums as the vibrations of air which cause sound are from a stroke, but yet continue strongest where they began, and alternately contract and dilate the æther in that physical superficies. For it's plain by the heat which light produces in bodies that it is able to put their parts in motion, and much more to heat and put in motion the more tender æther, and it's more probable that it communicates motion to the gross parts of bodies by the mediation of æther than immediately; as, for instance, in the inward parts of quicksilver, tin, silver, and other very opake bodies, by generating vibrations that run through them, than by striking the outward parts only without entering the body. The shock of every single ray may generate many thousand vibrations, and by sending them all over the body, move all the parts, and that perhaps with more motion than it could move one single part by an immediate stroke; for the vibrations, by shaking each particle backward and forward, may every time increase its motion, as a ringer does a bell by often pulling it, and so at length move the particles to a very great degree of agitation, which neither the simple shock of a ray nor any other motion in the æther, besides a vibrating one, could do. Thus in air shut up in a vessel, the motion of its parts caused by heat, how violent soever, is unable to move the bodies hung in it with either a trembling or progressive motion; but if air be put into a vibrating motion by beating a drum or two, it shakes glass windows, the whole body of a man, and other massy things, especially those of a congruous tone; yea, I have observed it manifestly shake under my feet a cellared free-stone floor of a large hall; so as I believe the immediate stroke of five hundred drum-sticks could not have done, unless perhaps quickly succeeding one another at equal intervals of time. Ætherial vibrations are therefore the best means by which such a subtle agent as light can shake the gross particles of solid bodies to heat them. And so supposing that light impinging on a refracting or reflecting ætherial superficies puts it into a vibrating motion, that physical superficies being by the perpetual appulse of rays always kept in a vibrating motion, and the æther therein continually expanded and compressed by turns; if a ray of light impinge upon it while it is much compressed, I suppose it is then too dense and stiff to let the ray pass through, and so reflects it; but the rays that impinge on it at other times, when it is either expanded by the interval of two vibrations, or not too much compressed and condensed, go through, and are refracted.

These may be the causes of refractions and reflexions in all cases, but for understanding how they come to be so regular, it's further to be considered, that as in a heap of sand, although the surface be rugged, yet if water be poured on it to fill its pores, the water, so soon as its pores are filled, will evenly overspread the surface, and so much the more evenly as the sand is finer; so, although the surface of all bodies, even the most polished, be rugged, as I conceive, yet when that ruggedness is not too gross and coarse, the refracting ætherial superficies may evenly overspread it. In polishing glass or metal, it is not to be imagined that sand, putty, or other fretting powders should wear the surface so regularly as to make the front of every particle exactly plain, and all those planes look the same way, as they ought to do in well-polished bodies, were reflexion performed by their parts; but, that those fretting powders should wear the bodies first to a coarse ruggedness, such as is sensible, and then to a finer and finer ruggedness, till it be so fine that the ætherial superficies evenly overspreads it, and so makes the body put on the appearance of a polish, is a very natural and intelligible supposition. So in fluids it is not well to be conceived that the surfaces of their parts should be all plain, and the planes of the superficial parts always kept looking all the same way, notwithstanding that they are in perpetual motion, and yet without these two suppositions, the superficies of fluids could not be so regularly reflexive as they are, were the reflexion done by the parts themselves, and not by an ætherial superficies evenly overspreading the fluid.

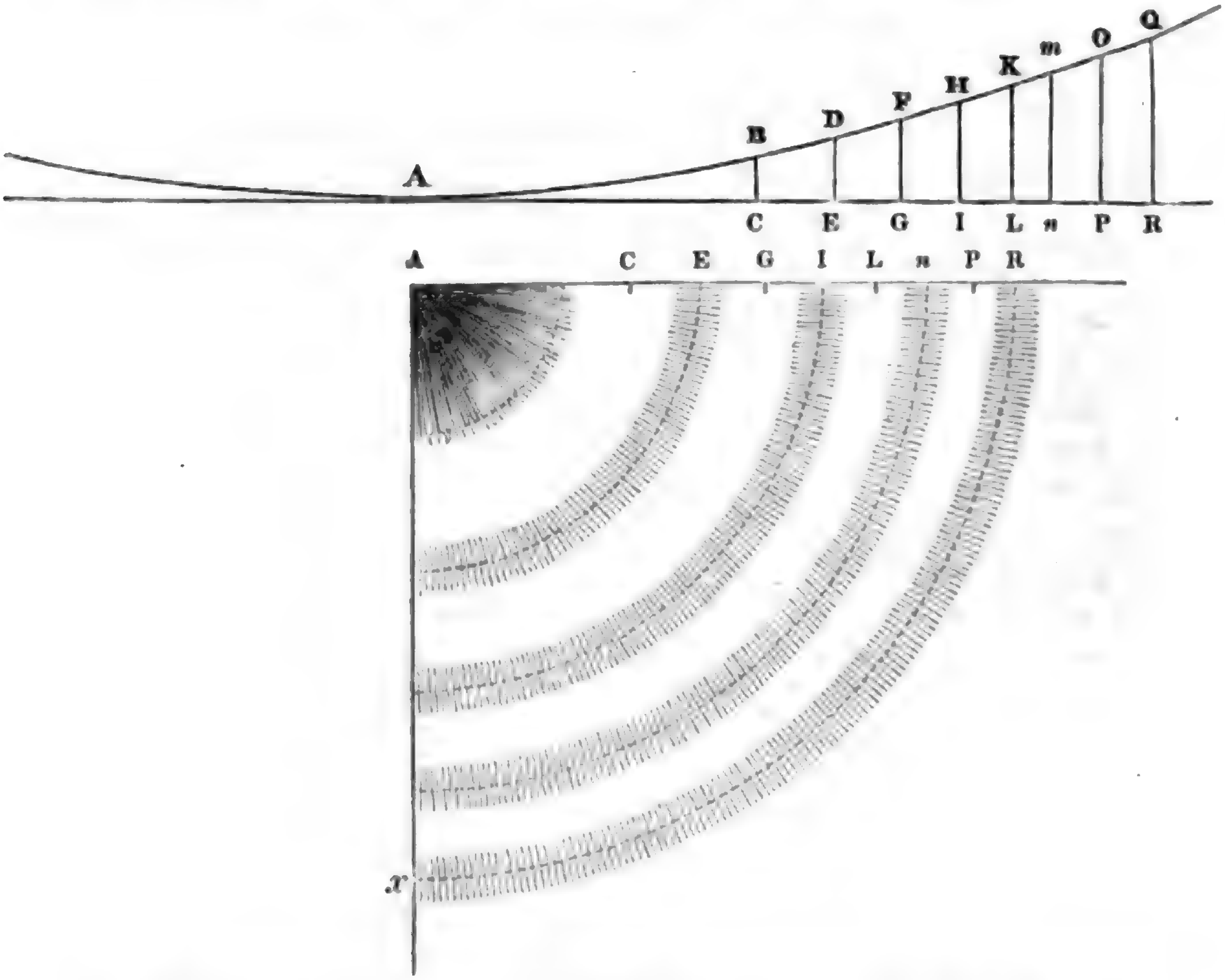
Further, concerning the regular motion of light, it might be suspected whether the various vibrations of the fluid through which it passes may not much disturb it; but that suspicion I suppose will vanish by considering, that if at any time the foremost part of an oblique wave begin to turn it awry, the hindermost part by a contrary action must soon set it straight again.

Lastly, because without doubt there are in every transparent body pores of various sizes, and I said that æther stands at the greatest rarity in the smallest pores; hence the æther in every pore should be of a differing rarity, and so light be refracted in its passage out of every pore into the next, which would cause a great confusion, and spoil the body's transparency; but considering that the æther in all dense bodies is agitated by continual vibrations, and these vibrations cannot be performed without forcing the parts of æther forward and backward from one pore to another by a kind of tremor, so that the æther which one moment is in a great pore, is the next moment forced into a less; and, on the contrary, this must evenly spread the æther into all the pores not exceeding some certain bigness, suppose the breadth of a vibration, and so make it of an even density throughout the transparent body, agreeable to the middle sort of pores. But where the pores exceed a certain bigness, I suppose the æther suits its density to the bigness of the pore or to the medium within, and so being of a divers density from the æther that surrounds it, refracts, or reflects light in its superficies, and so makes the body where many such interstices are, appear opake.

Thus much of refraction, reflexion, transparency, and opacity;—and now to explain colours. I suppose that as bodies of various sizes, densities, or tensions, do by percussion or other action, excite sounds of various tones, and consequently vibrations in the air of various bignesses; so, when the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the æther, those rays, whatever they be, as they happen to differ in magnitude, strength, or vigour, excite vibrations of various bignesses; the biggest, strongest, or most potent rays, the largest vibrations, and others shorter according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which front or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations; which vibrations (like those of sound in a trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerves into the sensorium (which light itself cannot do), and these I suppose affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all, with white; much after the manner that in the sense of hearing nature makes use of aërial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed. And further, as the harmony and discord of sounds proceed from the proportions of the aërial vibrations, so may the harmony of some colours, as of a golden and blue, and the discord of others, as of red and blue, proceed from the proportions of the ætherial. And possibly colour may be distinguished into its principal degrees: red, orange, yellow, green, blue, indigo, and deep violet, on the same ground that sound within an eighth is graduated into tones. For, some years past, the prismatic colours, being in a well-darkened room, cast perpendicularly upon a paper about two-and-twenty foot distant from the prism, I desired a friend to draw with a pencil lines across the image or pillar of colours, where every one of the seven aforenamed colours was most full and brisk, and also where he judged the truest confines of them to be, whilst I held the paper so that the said image might fall within a certain compass marked on it. And this I did, partly because my own eyes are not very critical in distinguishing colours, partly because another to whom I had not communicated my thoughts about this matter could have nothing but his eyes to determine his fancy in making those marks. This observation we repeated divers times, both in the same and divers days, to see how the marks on several papers would agree; and comparing the observations, though the just confines of the colours are hard to be assigned, because they passed into one another by insensible gradation; yet the differences of the observations were but little, especially towards the red end; and taking means between those differences that were, the length of the image (reckoned not by the distance of the verges of the semicircular ends, but by the distance of the centres of those semicircles, or length of the straight sides, as it ought to be) was divided in about the same proportion that a string is between the end and the middle to sound the tones in an eighth. You will understand me best by viewing the annexed figure, in which A B and C D represent the straight sides about ten inches long, A P C and B T D the semicircular ends, X and *y* the centres of those semicircles, X Z the length of a musical string double to X *y*, and divided between X and *y* so as to sound the tones expressed at the side (that is X H the half, X G and G I the third part, *y* K the fifth part, *y* M the eighth part, and G E the ninth part of X *y*); and the intervals between these divisions express the spaces which the colours written there took up, every colour being most briskly specific in the middle of those spaces. Now for the cause of these and such like colours made by refraction, the biggest or strongest rays must penetrate the refracting superficies more freely and easily than the weaker, and so be less turned awry by it, that is less refracted; which is as much as to say, the rays which make red are least refrangible, those which make blue, or violet, most refrangible, and others otherwise refrangible according to their colour. Whence if the rays which come promiscuously from the sun be refracted by a prism, as in the aforesaid experiment, those of several sorts being variously refracted, must go to several places on an opposite paper or wall, and so parted, exhibit every one their own colours, which they could not do while blended together. And because refraction only severs them, and changes not the bigness or strength of the ray, thence it is, that after they are once well-severed, refraction cannot make any further changes in their colour. On this ground may all the phænomena of refractions be understood. But to explain the colours made by reflexions, I must further suppose, that though light be unimaginably swift, yet the ætherial vibrations excited by a ray move faster than the ray itself, and so overtake and outrun it, one after another. And this I suppose they will think an allowable supposition, who have been inclined to suspect that these vibrations themselves might be light. But to make it the more allowable, it's possible light itself may not be so swift as some are apt to think; for notwithstanding any argument that I know yet to the contrary, it may be an hour or two, if not more, in moving from the sun to us. This celerity of the vibrations therefore supposed, if light be incident on a thin skin or plate of any transparent body, the waves excited by its passage through the first superficies, and taking it one after another till it arrive at the second superficies, will cause it to be there reflected or refracted, accordingly as the condensed or expanded part of the wave overtakes it there. If the plate be of such a thickness that the condensed part of the first wave overtake the ray at the second superficies, it must



be reflected there; if double that thickness, that the following rarefied part of the wave, that is, the space between that and the next wave overtake it, there it must be transmitted; if triple the thickness, that the condensed part of the second wave overtake it, there it must be reflected; and so where the plate is five, seven, or nine times that thickness, it must be *reflected* by reason of the third, fourth, or fifth wave overtaking it at the second superficies; but when it is four, six, or eight times that thickness, that the ray may be overtaken there by the dilated interval of those waves, it shall be *transmitted*, and so on; the second superficies being made able or unable to reflect accordingly as it is condensed or expanded by the waves. For instance, let A H Q represent the superficies of a spherically convex glass laid upon a plain glass, A I R and A I R Q H the thin plano-concave plate of air between them, and BC, DE, FG, HI, &c. thicknesses of that plate or distances of the glasses in the arithmetical progression of the numbers 1, 2, 3, 4, &c., whereof B C is the distance at which the ray is overtaken by the most condensed part of the first wave; I say the rays incident at B, F, K and O ought to be *reflected* at C, G, L and P; and those incident at D, H, M and Q

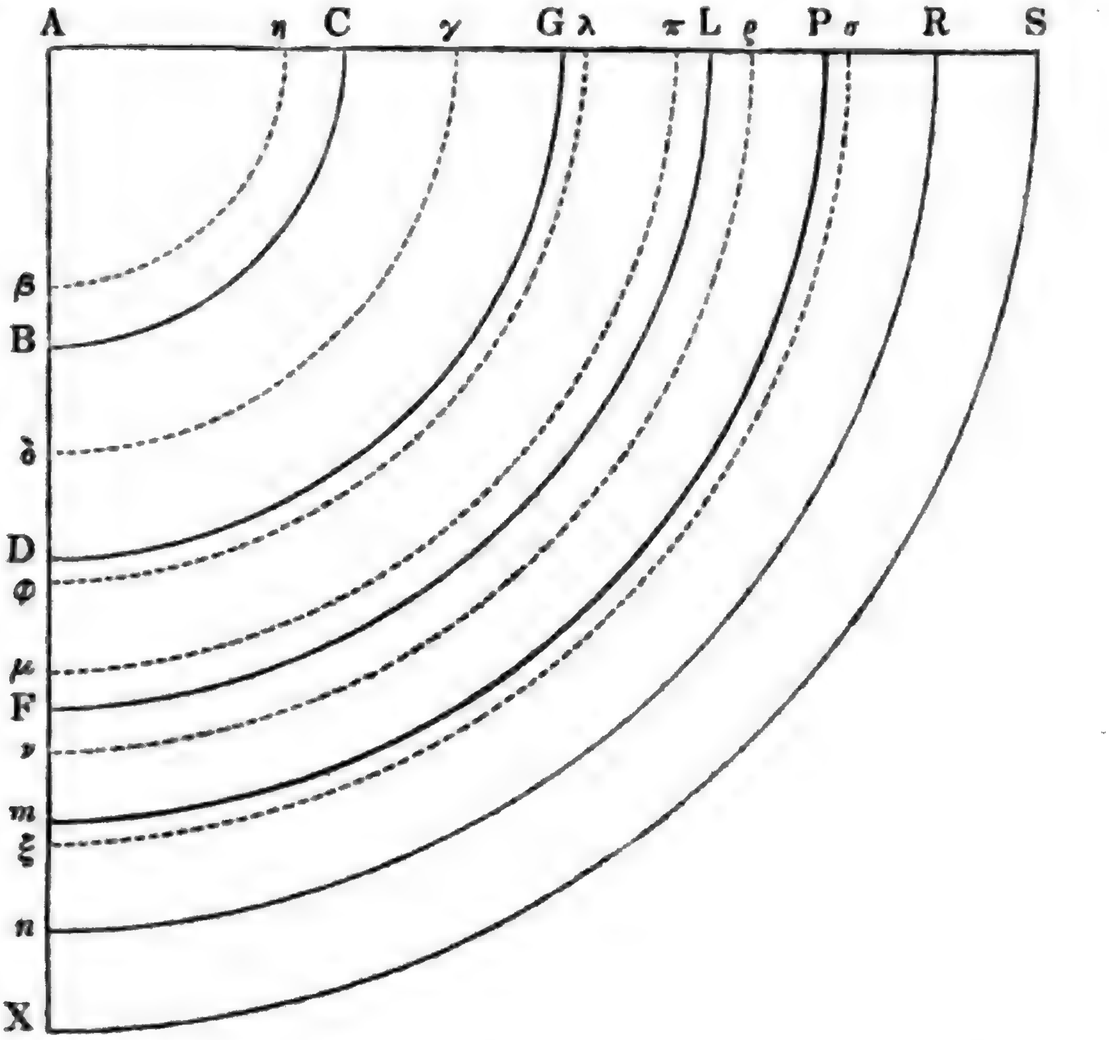


ought to be transmitted at E, I, *n* and R; and this because the ray B C arrives at the superficies A C, when it is condensed by the first wave that overtakes it; D E when rarified by the interval of the first and second; F G when condensed by the second wave; H I when rarefied by the interval of the second and third, and so on for an indeterminate number of successions; and at A, the centre, or contact of the glasses, the light must be *transmitted*, because there the ætherial mediums in both glasses are continued as if but one uniform medium. Whence if the glasses in this posture be looked upon, there ought to appear at A, the contact of the glasses, a black spot, and about that many concentric circles of light and darkness, the squares of whose semi-diameters are to sense in arithmetical progression. Yet all the rays without exception ought not to be thus reflected or transmitted: for sometimes a ray may be overtaken at the second superficies by the vibrations raised by another collateral, or immediately succeeding ray; which vibration being as strong, or stronger than its own, may cause it to be reflected or transmitted when its own vibration alone would do the contrary. And hence some little light will be reflected from the black rings, which makes them rather black than totally dark; and some transmitted at the lucid rings, which makes the black rings appearing on the other side of the glasses not so black as they would otherwise be. And so at the central black spot, where the glasses do not absolutely touch, a little light will be reflected, which makes the spot darkest in the middle, and only black at the verges. For thus I have observed it to be, by tying very hard together two glass prisms which were accidentally (one of them at least) a very little convex, and viewing by divers lights this black spot at their contact. If a white paper was placed at a little distance behind a candle, and the candle and paper viewed alternately by reflexion from the spot, the verges of the spot which looked by the light of the paper as black as the middle part, appeared by the stronger light of the candle lucid enough, so as to make the spot seem less than before; but the middle part continued as absolutely black in one case as in the other, some specks and streaks in it only excepted, where I suppose the glasses through some unevenness in the polish did not fully touch. The same I have observed by viewing the spot by the like reflexion of the sun and clouds alternately.

But to return to the lucid and black rings; those rings ought always to appear after the manner described, were light uniform. And after that manner, when the two contiguous glasses A Q and A R have been illustrated in a dark room by light of any uniform colour made by a prism, I have seen the lucid circles appear to about twenty in number, with many dark ones between them, the colour of the lucid ones being that of the light with which the glasses were illustrated. And if the glasses were held between the eye and the prismatic colours cast on a sheet of white paper, or if any prismatic colour was directly trajected through the glasses to a sheet of paper placed a little way behind, there would appear such other rings of colour and darkness (in the first case between the glasses, in the second on the paper) oppositely corresponding to those which appeared by reflexion. I mean that whereas by reflected light there appeared a black spot in the middle, and then a coloured circle; on the contrary, by transmitted light, there appeared a coloured spot in the middle, then a black circle; and so on, the diameters of the coloured circles made by transmission equalling the diameters of the black ones made by reflexion.

Thus, I say the rings do and ought to appear when made by uniform light, but in compound light it is otherwise. For the rays which exhibit red and yellow, exciting, as I said, larger pulses in the æther than those which make blue and violet, and consequently making bigger circles in a certain proportion, as I have manifoldly found they do, by illuminating the glasses successively by the aforesaid colours of the prism in a well-darkened room, without changing the position of my eye or of the glasses; hence the circles made by illuminating the glasses with white light, ought not to appear black and white by turns, as the circles made by illustrating the glasses for instance with red light, appear red and black; but the colours which compound the white light must display themselves by being reflected, the blue and violet nearer to the centre than the red and yellow, whereby every lucid circle must become violet in the inward verge, red in the outward, and of intermediate colours in the intermediate parts, and be made broader than before, spreading its colours both ways into those spaces which I call the black rings, and which would here appear black, were the red, yellow, blue, and violet, which make the verges of the rings, taken out of the incident white light which illustrates the glasses, and the green only left to make the lucid rings. Suppose C B, G D, L F, P *m*, R *n*, S X represent quadrants of the circles made in a dark room by the very deepest prismatic *red* alone; and ηβ, γδ, λφ, πμ, ρν, σξ the quadrants of like circles made also in a dark room, by the very deepest prismatic *violet* alone; and then if the glasses be illuminated by open daylight, in which all sorts of rays are blended, it is manifest that the first lucid ring will be

$\eta\beta$, BC; the second $\gamma\delta$, DG; the third $\lambda\phi$, FL; the fourth $\pi\mu$, mP; the fifth $\rho\nu$, nR; the sixth $\sigma\xi$, XS, &c.: in all which the deepest *violet* must be reflected at the inward edges



represented by the pricked lines, where it would be reflected were it alone, and the deepest *red* at the outward edges represented by the black lines, where it would be reflected were it alone, and all intermediate colours at those places in order between these edges at which they would be reflected were they alone; each of them in a dark room parted from all other colours by the refraction of a prism. And because the squares of the semi-diameters of the outward verges AC, AG, AL, &c., as also of Aη, Aγ, Aλ, &c., the semi-diameters of the inward are in arithmetical progression of the numbers 1, 3, 5, 7, 9, 11, &c.; and the squares of the inward are to the squares of the outward ($A\eta^2$ to AC^2, $A\gamma^2$ to AG^2, $A\lambda^2$ to AL^2, &c.) as 9 to 14 (as I have found by measuring them carefully and often, and comparing the observations); therefore the outward *red* verge of the second ring, and inward *violet* one of the third, shall border upon one another (as you may know by computation and see them represented in the figure), and the like edges of the third and fourth rings shall interfere, and those of the fourth and fifth interfere more, and so on; yea the colours of every ring must spread themselves something more both ways than is here represented, because the quadrantal arcs here described represent not the verges, but the middle of the rings made in a dark room by the extreme violet and red; the *violet* falling on both sides the pricked arches, and *red* on both sides the black line arches. And hence it is that these rings or circuits of colours succeed one another continually without any intervening black, and that the colours are pure only in the three or four first rings, and then interfering and mixing more and more, dilute one another so much, that after eight or nine rings they are no more to be distinguished, but seem to constitute an even whiteness; whereas when they were made in a dark room, by *one* of the prismatic colours alone, I have, as I said, seen above twenty of them, and without doubt could have seen them to a greater number, had I taken the pains to make the prismatic colour more uncompounded. For by unfolding these rings from one another by certain refractions expressed in the other papers* I send you, I have even in daylight discovered them to above a hundred, and perhaps they would have appeared innumerable, had the light or colour illustrating the glasses been absolutely uncompounded, and the pupil of my eye but a mathematical point, so that all the rays which came from the same point of the glass might have gone into my eye at the same obliquity to the glass. What has been hitherto said of these rings is to be understood of their appearance to an unmoved eye; but if you vary the position of the eye, the more obliquely you look upon the glass the larger the rings appear. And of this the reason may be, partly that an oblique ray is longer in passing through the first superficies, and so there is more time between the waving forward and backward of that superficies, and consequently a larger wave generated; and partly that the wave in creeping along between the two superficies, may be impeded and retarded by the rigidness of those superficies bounding it at either end, and so not overtake the ray so soon as a wave that moves perpendicularly across.

The bigness of the circles made by every colour and at all obliquities of the eye to the glasses, and the thickness of the air or intervals of the glasses, where each circle is made, you will find expressed in the other papers I send you, where also I have more at large described how much those rings interfere or spread into one another, what colours appear in every ring, where they are most lively, where and how much diluted by mixing with the colours of other rings, and how the contrary colours appear on the back side of the glasses by the

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* Obs. 24.

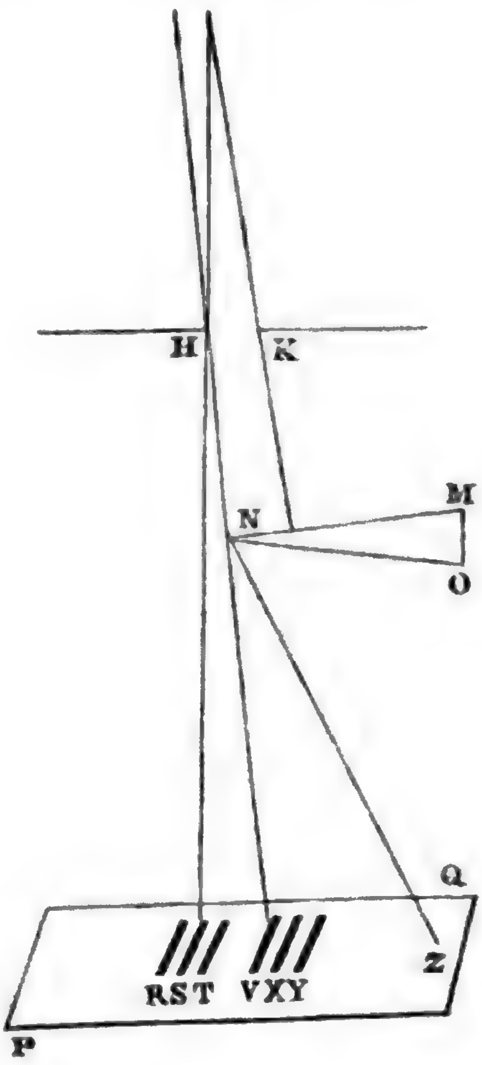
transmitted light, the glasses transmitting light of one colour at the same place, where they reflect that of another. Nor need I add anything further of the colours of other thinly plated mediums, as of water between the aforesaid glasses, or formed into bubbles and so encompassed with air, or of glass blown into very thin bubbles at a lamp furnace, &c.; the case being the same in all these, excepting that where the thickness of the plates is not regular, the rings will not be so, that in plates of denser transparent bodies the rings are made at a less thickness of the plate, (the vibrations, I suppose, being shorter in rarer æther than in denser), and that in a denser plate surrounded with a rarer body, the colours are more vivid than in the rarer surrounded with the denser; as for instance, more vivid in a plate of glass surrounded with air, than in a plate of air surrounded with glass; of which the reason is, that the reflexion of the second superficies, which causes the colours, is, as was said above, stronger in the former case than in the latter; for which reason also the colours are most vivid when the difference of the density of the medium is greatest.

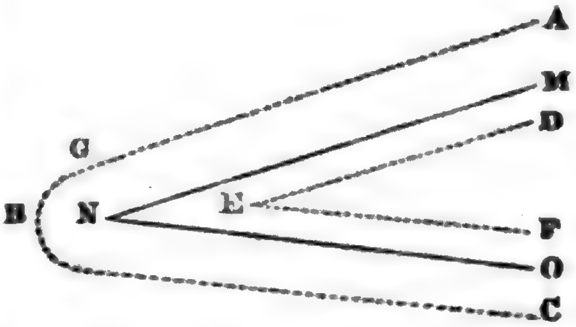
Of the colours of natural bodies also I have said enough in those papers, showing how the various sizes of the transparent particles of which they consist is sufficient to produce them all, those particles reflecting or transmitting this or that sort of rays, according to their thickness, like the aforesaid plates, as if they were fragments thereof. For, I suppose, if a plate of an even thickness, and consequently of an uniform colour, were broken into fragments of the same thickness with the plate, a heap of those fragments would be a powder much of the same colour with the plates. And so, if the parts be of the thickness of the water in the black spot at the top of a bubble described in the seventeenth of the observations I send you, I suppose the body must be black. In the production of which blackness, I suppose, that the particles of that size being disposed to reflect almost no light outward, but to refract it continually in its passage from every part to the next, by this multitude of refractions the rays are kept so long straggling to and fro within the body, till at last almost all impinge on the solid parts of the body, and so are stopped and stifled; those parts having no sufficient elasticity, or other disposition to return nimbly enough the smart shock of the ray back upon it.

I should here conclude; but that there is another strange phænomenon of colours, which may deserve to be taken notice of. Mr. Hook, you may remember, was speaking of an odd straying of light, caused in its passage near the edge of a razor, knife, or other opake body in a dark room; the rays, which pass very near the edge being thereby made to stray at all angles into the shadow of the knife.

To this Sir William Petty, then president, returned a very pertinent query, whether that straying was in curved lines? and that made me, having heard Mr. Hook some days before compare it to the straying of sound into the quiescent medium, say, that I took it to be only a new kind of refraction, caused perhaps by the external æther's beginning to grow rarer a little before it came at the opake body, than it was in free spaces, the denser æther without the body, and the rarer within it, being terminated not in a mathematical superficies, but passing into one another through all intermediate degrees of density: whence the rays, that pass so near the body, as to come within that compass where the outward æther begins to grow rarer, must be refracted by the uneven denseness thereof, and bended inwards toward the rarer medium of the body. To this Mr. Hook was then pleased to answer, that though it should be but a new kind of refraction, yet it was a new one. What to make of this unexpected reply I knew not, having no other thought, but that a new kind of refraction might be as noble an invention as anything else about light; but it made me afterward, I know not upon what occasion, happen to say, among some that were present to what passed before, that I thought I had seen the experiment before in some Italian author. And the author is Honoratus Faber, in his dialogue *De Lumine*, who had it from Grimaldo; whom I mention, because I am to describe something further out of him, which you will apprehend by the opposite figure. Suppose the sun shine through the little hole H K into a dark room upon the paper PQ, and with a wedge MNO intercept all but a little of that beam, and you will see upon the paper six rows of colours, R, S, T, V, X, Y, and beyond them a very faint light spreading either way, such as rays broken, like HNZ, must make. The author describes it more largely in divers schemes. I have time only to hint the sum of what he says.

Now for the breaking of the ray H N Z, suppose, in the next figure, M N O be the solid wedge, A B C the inward bound of the uniform rarer æther within, between which bounds the æther runs through all the intermediate degrees; and it is manifest, that, if a ray come between B and N, it must in its passage there bend from the denser medium towards C, and that so much the more, by how much it comes nearer N. Further, for the three rows of colours V X Y, those may perhaps proceed from the number of vibrations (whether one, two, or three) which overtake the ray in its passage from G, till it be about the midway between G and H, that is at its nearest distance to N, so as to touch the circle described about N, with that distance; by the last of which vibrations, expanding or contracting the medium there, the ray is licensed to recede again from N, and go on to make the colours; or further bent about N, till the interval of the next wave overtake it, and give it liberty to go from N, very nearly in the line it is then moving, suppose toward Z, to cause the faint light spoken of above. You will understand me a little better, by comparing this with what was said of the colours of thin transparent plates, comparing the greatest distance that the ray goes from GBH towards N, to the thickness of one of those plates. Something too there is in Descartes's explication of the rainbow's colours, which would give further light in this. But I have no time left to insist further upon particulars; nor do I propound this without diffidence, having not made sufficient observation about it.





Letter from Newton *to* Oldenburg, *dated Jan.* 25, 1675–76.

Sir,

I received both yours, and thank you for your care in disposing those things between me and Mr. Sinus. I suppose his friends cannot blame you at all for printing his first letter, it being written, I believe, for that end, and they never complaining of the printing of that, but of the not printing of that which followed, which I take myself to have been, *per accidens,* the occasion of, by refusing to answer him. And though I think I may truly say I was very little concerned about it, yet I must look upon it as the result of your kindness to me that you was unwilling to print it without an answer.

As to the paper of observations which you move in the name of the Society to have printed, I cannot but return them my hearty thanks for the kind acceptance they meet with there, and know not how to deny anything which they desire should be done. Only I think it will be best to suspend the printing of them for a while, because I have some thoughts of writing such another set of observations for determining the manner of the productions of colours by the prism, which if done at all ought to precede that now in your hands, and will do best to be joined with it. But this I cannot do presently by reason of some incumbrances lately put upon me by some friends, and some other business of my own, which at present almost take up my time and thoughts.

The additions that I intended, I think I must, after putting you to so long expectations, disappoint you in; for it puzzles me to connect them with what I sent you; and if I had those papers, yet I doubt the things I intended will not come in so freely as I thought they might have done. I could send them described without dependence on those papers; but I fear I have already troubled your Society and yourself too much with my scribbling, and so suppose it may do better to defer them till another season. I have therefore at present only sent you two or three alterations, though not of so great moment that I need have stayed you for them; and they are these:—

Where I say that the frame of nature may be nothing but æther condensed by a fermental principle, instead of these words write, that it may be nothing but various contextures of some certain ætherial spirits or vapours condensed, as it were, by precipitation, much after the manner that vapours are condensed into water, or exhalations into grosser substances, though not so easily condensable; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, who, by virtue of the command increase and multiply, became a compleat imitator of the copies set her by the Protoplast. Thus perhaps may all things be originated from æther, &c.

A little after, when I say the ætherial spirit may be condensed in fermenting or burning bodies, or otherwise inspissated in the pores of the earth to a tender matter, which may be, as it were, the succus nutritius of the earth, or primary substance, out of which things generable grow; instead of this you may write, that that spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter, for the continual use of nature, adhering to the sides of those pores after the manner that vapours condense on the sides of a vessel.

In the same paragraph there is, I think, a parenthesis, in which I mention volatile salt-petre; pray strike out that parenthesis, lest it should give offence to somebody.

Also where I relate the experiment of little papers made to move curiously with a glass rubbed, I would have all that struck out which follows, about trying the experiment of leaf-gold.

Sir, I am interrupted by a visit, and must in haste break off.

Yours,

January 25, 1675-6. ISAAC NEWTON.

Letter from Newton *to* Boyle.

HONOURED SIR,

I have so long deferred to send you my thoughts about the physical qualities we speak of, that did I not esteem myself obliged by promise, I think I should be ashamed to send them at all. The truth is, my notions about things of this kind are so indigested, that I am not well satisfied myself in them; and what I am not satisfied in, I can scarce esteem fit to be communicated to others; especially in natural philosophy, where there is no end of fancying. But because I am indebted to you, and yesterday met with a friend, Mr. Maulyverer, who told me he was going to London, and intended to give you the trouble of a visit, I could not forbear to take the opportunity of conveying this to you by him.

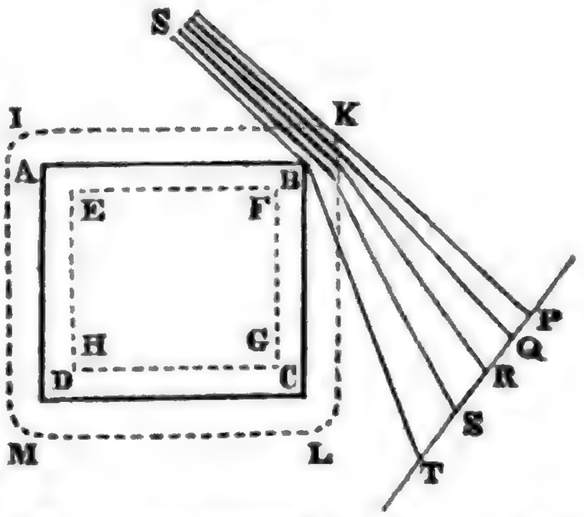
It being only an explication of qualities which you desire of me, I shall set down my apprehensions in the form of suppositions as follows. And first, I suppose, that there is diffused through all places an ætherial substance, capable of contraction and dilatation, strongly elastic, and, in a word, much like air in all respects, but far more subtile.

2. I suppose this æther pervades all gross bodies, but yet so as to stand rarer in their pores than in free spaces, and so much the rarer, as their pores are less; and this I suppose (with others) to be the cause why light incident on those bodies is refracted towards the perpendicular; why too well-polished metals cohere in a receiver exhausted of air; why ☿

stands sometimes up to the top of a glass pipe, though much higher than 30 inches; and one of the main causes why the parts of all bodies cohere; also the cause of filtration, and of the rising of water in small glass pipes above the surface of the stagnating water they are dipped into; for I suspect the æther may stand rarer, not only in the insensible pores of bodies, but even in the very sensible cavities of the pipes; and the same principles may cause menstruums to pervade with violence the pores of the bodies they dissolve, the surrounding æther, as well as the atmosphere, pressing them together.

3. I suppose the rarer æther within bodies, and the denser without them, not to be terminated in a mathematical superficies, but to grow gradually into one another; the external æther beginning to grow rarer, and the internal to grow denser, at some little distance from the superficies of the body, and running through all intermediate degrees of density in the intermediate spaces; and this may be the cause why light, in Grimaldo's experiment, passing by the edge of a knife, or other opake body, is turned aside, and as it were refracted, and by that refraction makes several colours. Let ABCD be a dense body whether opake or transparent, EFGH the outside of the uniform æther, which is within it, IKLM the inside of the uniform æther, which is without it; and conceive the æther, which is between EFGH and IKLM, to run through all intermediate degrees of density between that of the two uniform æthers on either side. This being supposed, the rays of the sun SB, SK, which pass by the edge of this body between B and K, ought in their passage through the unequally dense æther there, to receive a ply from the denser æther, which is on that side towards K, and that the more by how much they pass nearer to the body, and thereby to be scattered through the space PQRST, as by experience they are found to be. Now the space between the limits EFGH and IKLM, I shall call the space of the æther's graduated rarity.

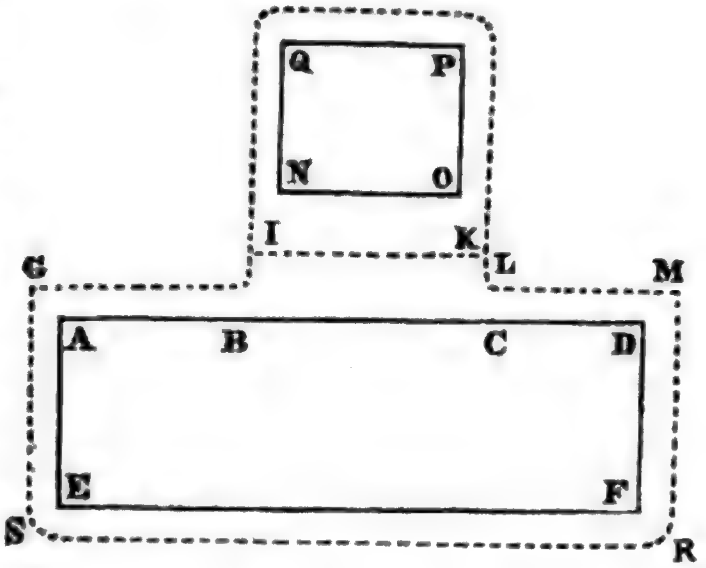
Fig. 1.



4. When two bodies moving towards one another come near together, I suppose the æther between them to grow rarer than before, and the spaces of its graduated rarity to extend further from the superficies of the bodies towards one another; and this, by reason that the æther cannot move and

play up and down so freely in the straight passage between the bodies, as it could before they came so near together: thus if the space of the æther's graduated rarity reach from the body ABCDFE only to the distance GHLMRS, when no other body is near it, yet may it reach further, as to IK, when another body NOPQ approaches; and as the other body approaches more and more, I suppose the æther between them will grow rarer and rarer. These suppositions I have so described, as if I thought the spaces of graduated æther had precise limits, as is expressed at IKLM in the first figure, and GMRS in the second; for thus I thought I could better express myself. But really I do not think they have such precise limits, but rather decay insensibly, and, in so decaying, extend to a much greater distance than can easily be believed or need be supposed.

Fig. 2.



5. Now from the fourth supposition it follows, that when two bodies approaching one another come so near together as to make the æther between them begin to rarefy, they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another; which reluctance and endeavour will increase as they come nearer together, because thereby they cause the interjacent æther to rarefy more and more. But at length, when they come so near together that the excess of pressure of the external æther which surrounds the bodies, above that of the rarefied æther, which is between them, is so great as to overcome the reluctance which the bodies have from being brought together; then will that excess of pressure drive them with violence together, and make them adhere strongly to one another, as was said in the second supposition. For instance, in the second figure, when the bodies E D and N P are so near together that the spaces of the æther's graduated rarity begin to reach one another, and meet the line I K, the æther between them will have suffered much rarefaction, which rarefaction requires much force, that is, much pressing of the bodies together; and the endeavour which the æther between them has to return to its former natural state of condensation, will cause the bodies to have an endeavour

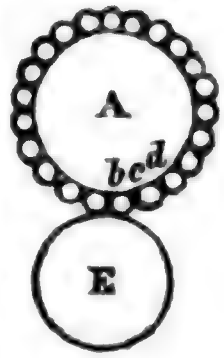
of receding from one another. But, on the other hand, to counterpoise this endeavour, there will not yet be any excess of density of the æther which surrounds the bodies, above that of the æther which is between them at the line I K. But if the bodies come nearer together, so as to make the æther in the mid-way line I K grow rarer than the surrounding æther, there will arise from the excess of density of the surrounding æther a compressure of the bodies towards one another, which when by the nearer approach of the bodies it becomes so great as to overcome the aforesaid endeavour the bodies have to recede from one another, they will then go towards one another and adhere together. And, on the contrary, if any power force them asunder to that distance, where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now hence I conceive it is chiefly that a fly walks on water without wetting her feet, and consequently without touching the water; that two polished pieces of glass are not without pressure brought to contact, no, not though the one be plain, the other a little convex; that the particles of dust cannot by pressing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging substances and salts dissolved in water do not of their own accord concrete and fall to the bottom, but diffuse themselves all over the liquor, and expand still more if you add more liquor to them. Also, that the particles of vapours, exhalations, and air do stand at a distance from one another, and endeavour to recede as far from one another as the pressure of the incumbent atmosphere will let them; for I conceive the confused mass of vapours, air, and exhalations which we call the atmosphere, to be nothing else but the particles of all sorts of bodies, of which the earth consists, separated from one another, and kept at a distance, by the said principle.

From these principles the actions of menstruums upon bodies may be thus explained: suppose any tinging body, as cochineal or logwood, be put into water; so soon as the water sinks into its pores and wets on all sides any particle which adheres to the body only by the principle in the second supposition, it takes off, or at least much diminishes, the efficacy of that principle to hold the particle to the body, because it makes the æther on all sides the particle to be of a more uniform density than before. And then the particle being shaken off by any little motion, floats in the water, and with many such others makes a tincture; which tincture will be of some lively colour, if the particles be of all the same size and density; otherwise of a dirty one, for the colours of all natural bodies whatever seem to depend on nothing but the various sizes and densities of the particles, as I think you have seen described by me more at large in another paper. If the particles be very small (as are those of salts, vitriols, and gums), they are transparent; and as they are supposed bigger and bigger, they put on these colours in order, black, white, yellow, red; violet, blue, pale green, yellow, orange, red; purple, blue, green, yellow, orange, red, &c., as it is discerned by the colours, which appear at the several thicknesses of very thin plates of transparent bodies. Whence, to know the causes of the changes of colours, which are often made by the mixtures of several liquors, it is to be considered how the particles of any tincture may have their size or density altered by the infusion of another liquor. When any metal is put into common water, it cannot enter into its pores, to act on it and dissolve it. Not that water consists of too gross parts for this purpose, but because it is unsociable to metal. For there is a certain secret principle in nature, by which liquors are sociable to some things and unsociable to others; thus water will not mix with oil, but readily with spirit of wine, or with salts; it sinks also into wood, which quicksilver will not; but quicksilver sinks into metals, which, as I said, water will not. So aquafortis dissolves ☽, not ☉; aqua regia ☉, not ☽, &c. But a liquor, which is of itself unsociable to a body, may, by the mixture of a convenient mediator, be made sociable; so molten lead, which alone will not mix with copper, or with regulus of Mars, by the addition of tin is made to mix with either. And water, by the mediation of saline spirits, will mix with metal. Now then any metal put in water impregnated with such spirits, as into aquafortis, aqua regia, spirit of vitriol, or the like, the particles of the spirits, as they, in floating in the water, strike on the metal, will by their sociableness enter into its pores and gather round its outside particles, and by advantage of the continual tremor the particles of the metal are in, hitch themselves in by degrees between those particles and the body, and loosen them from it; and the water entering into the pores together with the saline spirits, the particles of the metal will be thereby still more loosed, so as by that motion the solution puts them into, to be easily shaken off, and made to float in the water: the saline particles still encompassing the metallic ones as a coat or shell does a kernel, after the manner expressed in the annexed, in which figure I have made the particles round, though they may be cubical, or of any other shape.

Fig. 3.

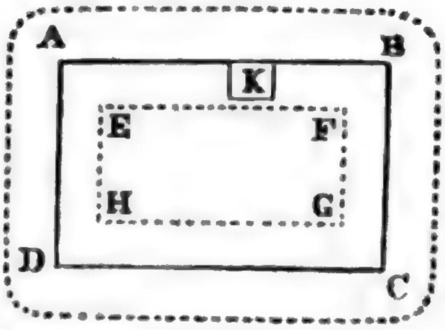
If into a solution of metal thus made be poured a liquor, abounding with particles, to which the former saline particles are more sociable than to the particles of the metal (suppose with particles of salt of tartar), then so soon as they strike on one another in the liquor, the saline particles will adhere to those more firmly than to the metalline ones, and by degrees be wrought off from those to enclose these. Suppose A a metalline particle, inclosed with saline ones of spirit of nitre, E a particle of salt of tartar, contiguous to two of the particles of spirit of nitre, *b* and *c*; and suppose the particle E is impelled by any motion towards *d*, so as to roll about the particle *c* till it touch the particle *d*, the particle *b* adhering more firmly to E than to A, will be forced off from A; and by the same means the particle E, as it rolls about A, will tear off the rest of the saline particles from A one after another, till it has got them all, or almost all, about itself. And when the metallic particles are thus divested of the nitrous ones, which, as a mediator between them and the water, held them floating in it, the alcalizate ones, crowding for the room the metallic ones took up before, will press these towards one another, and make them come more easily together: so that by the motion they continually have in the water, they shall be made to strike on one another; and then, by means of the principle in the second supposition, they will cohere and grow into clusters, and fall down by their weight to the bottom, which is called precipitation. In the solution of metals, when a particle is loosing from the body, so soon as it gets to that distance from it, where the principle of receding described in the fourth and fifth supposition begins to overcome the principle of acceding, described in the second supposition, the receding of the particle will be thereby accelerated; so that the particle shall as it were with violence leap from the body, and putting the liquor into a brisk agitation, beget and promote that heat we often find to be caused in solutions of metals. And if any particle happen to leap off thus from the body, before it is surrounded with water, or to leap off with that smartness as to get loose from the water, the water, by the principle in the fourth and fifth supposition, will be kept off from the particle, and stand round about it, like a spherically hollow arch, not being able to come to a full contact with it any more; and several of these particles afterwards gathering into a cluster, so as by the same principle to stand at a distance from one another, without any water between them, will compose a bubble. Whence I suppose it is, that in brisk solution there

Fig. 4.



usually happens an ebullition. This is one way of transmuting gross compact substance into aërial ones. Another way is by heat; for as fast as the motion of heat can shake off the particles of water from the surface of it, those particles, by the said principle, will float up and down in the air, at a distance both from one another, and from the particles of air, and make that substance we call vapour. Thus I suppose it is, when the particles of a body are very small (as I suppose those of water are), so that the action of heat alone may be sufficient to shake them asunder. But if the particles be much larger, they then require the greater force of dissolving menstruums to separate them, unless by any means the particles can be first broken into smaller ones. For the most fixed bodies, even gold itself, some have said will become volatile, only by breaking their parts smaller. Thus may the volatility and fixedness of bodies depend on the different sizes of their parts. And on the same difference of size may depend the more or less permanency of aërial substances, in their state of rarefaction. To understand this, let us suppose A B C D to be a large piece of any metal, E F G H the limit of the interior uniform æther, and K a part of the metal at the superficies A B. If this part of particle K be so little that it reaches not to the limit EF, it is plain that the æther at its centre must be less rare than if the particle were greater; for were it greater, its centre would be further from the superficies A B, that is, in a place where the æther (by supposition) is rarer; the less the particle K therefore, the denser the æther at its centre; because its centre comes nearer to the edge AB, where the æther is denser than within the limit E F G H. And if the particle were divided from the body, and removed to a distance from it, where the æther is still denser, the æther within it must proportionally grow denser. If you consider this, you may apprehend how, by diminishing the particle, the rarity of the æther within it will be diminished, till between the density of the æther without, and the density of the æther within it, there be little difference; that is, till the cause be almost taken away, which should keep this and other such particles at a distance from one another. For that cause explained in the fourth and fifth suppositions, was the excess of density of the external æther above that of the internal. This may be the reason then why the small particles of vapours easily come together, and

Fig. 5.



are reduced back into water, unless the heat, which keeps them in agitation, be so great as to dissipate them as fast as they come together; but the grosser particles of exhalations raised by fermentation keep their aërial form more obstinately, because the æther within is rarer.

Nor does the size only, but the density of the particles also, conduce to the permanency of aërial substances; for the excess of density of the æther without such particles above that of the æther within them is still greater; which has made me sometimes think that the true permanent air may be of a metallic original; the particles of no substances being more dense than those of metals. This, I think, is also favoured by experience, for I remember I once read in the Philosophical Transactions, how M. Huygens at Paris, found that the air made by dissolving salt of tartar would in two or three days' time condense and fall down again, but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then, how by the continual fermentations made in the bowels of the earth there are aërial substances raised out of all kinds of bodies, all which together make the atmosphere, and that of all these the metallic are the most permanent, you will not perhaps think it absurd, that the most permanent part of the atmosphere, which is the true air, should be constituted of these, especially since they are the heaviest of all others, and so must subside to the lower parts of the atmosphere and float upon the surface of the earth, and buoy up the lighter exhalations and vapours to float in greatest plenty above them. Thus, I say, it ought to be with the metallic exhalations raised in the bowels of the earth by the action of acid menstruums, and thus it is with the true permanent air; for this, as in reason it ought to be esteemed the most ponderous part of the atmosphere, because the lowest, so it betrays its ponderosity by making vapours ascend readily in it, by sustaining mists and clouds of snow, and by buoying up gross and ponderous smoke. The air also is the most gross unactive part of the atmosphere, affording living things no nourishment, if deprived of the more tender exhalations and spirits that float in it; and what more unactive and remote from nourishment than metallic bodies?

I shall set down one conjecture more, which came into my mind now as I was writing this letter; it is about the cause of gravity. For this end I will suppose æther to consist of parts differing from one another in *subtilty* by indefinite degrees; that in the pores of bodies there is less of the grosser æther, in proportion to the finer, than in open spaces; and consequently, that in the great body of the earth there is much less of the grosser æther, in proportion of the finer, than in the regions of the air; and that yet the grosser æther in the air affects the upper regions of the earth, and the finer æther in the earth the lower regions of the air, in such a manner, that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the æther is insensibly finer and finer. Imagine now any body suspended in the air, or lying on the earth, and the æther being by the hypothesis grosser in the pores, which are in the upper parts of the body, than in those which are in its lower parts, and that grosser æther being less apt to be lodged in those pores than the finer æther below, it will endeavour to get out and give way to the finer æther below, which cannot be, without the bodies descending to make room above for it to go out into.

From this supposed gradual subtilty of the parts of æther some things above might be further illustrated and made more intelligible; but by what has been said, you will easily discern whether in these conjectures there be any degree of probability, which is all I aim at. For my own part, I have so little fancy to things of this nature, that had not your encouragement moved me to it, I should never, I think, have thus far set pen to paper about them. What is amiss, therefore, I hope you will the more easily pardon in

Your most humble servant and honourer,

Cambridge, Feb. 28, 1678–9. ISAAC NEWTON.

XXXVI. *A New Analytical Demonstration of the* "*Parallelogram of Forces.*" *By* THOMAS WEDDLE, *Esq., Newcastle-on-Tyne*[*].

THE following investigation of this fundamental proposition, whatever may be its defects, has at least one advantage—the most general case is discussed at once. In all the analytical proofs that I have seen, a particular case only is established analytically; thus Laplace (*Mécanique Céleste*) and Pontécoulant (*Système du Monde*) consider the forces to act at right angles; and Poisson (*Mécanique*) first finds the resultant of two equal forces, and afterwards thence deduces, by *geometrical* considerations, that of any two forces. I have here attempted to conduct the whole investigation analytically, and to do so without first establishing a particular case.

I shall assume that the resultant of two equal forces bisects

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[*] Communicated by the Author.

the angle contained by their directions, and is never greater than their sum.

Let the forces P, Q, R, acting upon the point O in the directions O P, O Q, O R, be in equilibrium, so that R, acting in the opposite direction R O, is the resultant of P and Q. Denote the angles Q O R, R O P, and P O Q by α, β, and γ. Make $< ROQ' = < ROQ = \alpha$, and $< ROP' = < ROP = \beta$; and to the point O apply in the direction OP', a force $P' = P$; in OQ', a force $Q' = Q$; and in O R, a force $R' = R$; hence the forces P', Q', R' are respectively equal to P, Q, R, and the angles contained by the directions of the former forces are equal to those contained by the directions of the latter; now P, Q, R are in equilibrium, hence P', Q', R' are also in equilibrium; wherefore the forces P, P', Q, Q', and $R + R'$, whose directions are O P, OP', O Q, OQ', and O R, must balance each other, and consequently the resultant of the first four forces must be equal and opposite to $R + R'$.

Moreover, since $< ROP = < ROP' = \beta$, RO (produced, if necessary) will bisect the angle POP'; hence the resultant R_1 of the equal forces P and P' will act in R O, and R_1 will be a function of P and β; now, since the numerical values of R_1 and P vary with the unit of force, and that of β is independent of it, the ratio $\frac{R_1}{P}$ must be independent of this unit, and consequently a function of β only; denote this function by $2\,\phi\,\beta$, $\therefore R_1 = 2\,P \, . \, \phi\,\beta$.

Now R_1 cannot, for any value of β, exceed $P + P'$ or $2\,P$, wherefore if $\psi\,\beta = \cos^{-1}\phi\,\beta$, the value of $\psi\,\beta$ will always be real, and we may therefore assume

$$R_1 = 2\,P \, . \cos\psi\,\beta.$$

Hence also, if R_2 be the resultant of the equal forces Q and Q', we must have

$$R_2 = 2\,Q \, . \cos\psi\,\alpha.$$

Moreover, since R_1 and R_2 act in the same straight line, $R_1 + R_2$ is the resultant of P, P', Q, Q'; and consequently $R_1 + R_2 = R + R' = 2\,R$,

$$\therefore R = P\cos\psi\,\beta + Q\cos\psi\,\alpha. \quad . \quad . \quad . \quad . \quad (1.)$$

Similarly, $$Q = P\cos\psi\,\gamma + R\cos\psi\,\alpha, \quad . \quad . \quad . \quad . \quad (2.)$$

and $$P = Q\cos\psi\,\gamma + R\cos\psi\,\beta, \quad . \quad . \quad . \quad . \quad (3.)$$

α, β and γ being connected by the equation

$$\alpha + \beta + \gamma = 2\pi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

Eliminating P and Q from (1. 2. 3.), R will disappear at the same time, and there results,

$$\left.\begin{array}{l} 1 - \cos^2\psi\alpha - \cos^2\psi\beta - \cos^2\psi\gamma \\ \quad - 2\cos\psi\alpha \, . \cos\psi\beta \, . \cos\psi\gamma = 0. \end{array}\right\} \quad . \quad . \quad . \quad . \quad (5.)$$

Solve this equation for $\cos\psi\gamma$,

$$\begin{aligned} \therefore \cos\psi\gamma &= -\cos\psi\alpha \, . \cos\psi\beta \pm \sqrt{(1-\cos^2\psi\alpha)(1-\cos^2\psi\beta)} \\ &= -\cos\psi\alpha \, . \cos\psi\beta \pm \sin\psi\alpha \, . \sin\psi\beta \\ &= -\cos\{\psi\alpha \pm \psi\beta\}; \end{aligned}$$

$$\therefore (2m+1)\pi \mp \psi\gamma = \psi\alpha \pm \psi\beta,$$

or $$\psi\alpha \pm \psi\beta \pm \psi\gamma = (2m+1)\pi, \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

m (as well as n, p, q and r employed below) being a positive or negative whole number. Again, when $\alpha = \beta = \gamma = \frac{2}{3}\pi$, we have

$$(1.\ 2.\ 3.),\ \cos\psi\left(\frac{2}{3}\pi\right) = \frac{1}{2} = \cos\left(2n \pm \frac{1}{3}\right)\pi,$$

$$\therefore \psi\left(\frac{2}{3}\pi\right) = \left(2n \pm \frac{1}{3}\right)\pi. \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

Now the left-hand member of (6.) may be written four different ways: $\psi\alpha + \psi\beta + \psi\gamma$, $\psi\alpha + \psi\beta - \psi\gamma$, $\psi\alpha - \psi\beta + \psi\gamma$, or $\psi\alpha - \psi\beta - \psi\gamma$; but when $\psi\alpha = \psi\beta = \psi\gamma = \psi\left(\frac{2}{3}\pi\right)$, the last three give

$$\psi\left(\frac{2}{3}\pi\right) = (2m+1)\pi, \text{ or } -(2m+1)\pi,$$

which is inconsistent with (7.); hence we must have

$$\psi\alpha + \psi\beta + \psi\gamma = (2m+1)\pi. \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

Differentiate (8.) first with respect to α, and then with respect to β, observing that (4.) $\frac{d\gamma}{d\alpha} = \frac{d\gamma}{d\beta} = -1$,

$$\therefore \psi'\alpha = \psi'\beta = \psi'\gamma, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

where, as usual, $\psi'\alpha = \frac{d}{d\alpha} . \psi\alpha$. Now two of the quantities α, β, γ being arbitrary (4.), it is evident that (9.) cannot be satisfied but by $\psi'\alpha = \text{constant} = c$; hence integrating

$$\psi\alpha = c\alpha + c_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

To determine the constants c, c_1, we observe that when

$\alpha = 0$ and $\beta = \gamma = \pi$, the forces all act in the same straight line, P in one direction and Q and R in the opposite; hence $P = Q + R$: and we derive from (1. 2. 3.),

$$\cos\psi 0 = -1 = \cos(2p+1)\pi \text{ and } \cos\psi\pi = 1 = \cos 2q\pi,$$
$$\therefore \psi 0 = (2p+1)\pi \text{ and } \psi\pi = 2q\pi;$$

hence (10.) gives

$$c_1 = (2p+1)\pi, \text{ and } c\pi + c_1 = 2q\pi, \text{ or } c = 2(q-p-1) + 1 = 2r+1 \text{ (suppose)},$$

$\therefore$ (10.) becomes $\quad \psi\alpha = (2r+1)\alpha + (2p+1)\pi,$

and

$$\cos\psi\alpha = \cos\{(2r+1)\alpha + (2p+1)\pi\} = -\cos(2r+1)\alpha. \quad (11.)$$

Moreover, if

$$\alpha = \frac{\pi}{2r+1}, \text{ and } \beta = \gamma = \pi - \frac{1}{2r+1} \cdot \frac{\pi}{2},$$

we shall have (11.),

$$\cos\psi\alpha = 1 \text{ and } \cos\psi\beta = \cos\psi\gamma = 0,$$
$$\therefore (1.2.3.)\ Q = R \text{ and } P = 0;$$

hence the equal forces Q, R acting at an angle $\frac{\pi}{2r+1}$, balance each other; but this is impossible, unless $r = 0$, when they act in the same straight line in opposite directions. Make therefore $r = 0$ in (11.), and we have finally,

$$\cos\psi\alpha = -\cos\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

The equations (1, 2, 3.) therefore become,

$$R = -P\cos\beta - Q\cos\alpha. \quad . \quad . \quad . \quad . \quad . \quad (13.)$$
$$Q = -P\cos\gamma - R\cos\alpha. \quad . \quad . \quad . \quad . \quad (14.)$$
$$P = -Q\cos\gamma - R\cos\beta. \quad . \quad . \quad . \quad . \quad (15.)$$

Multiply these equations by R, Q and P respectively, and we readily get

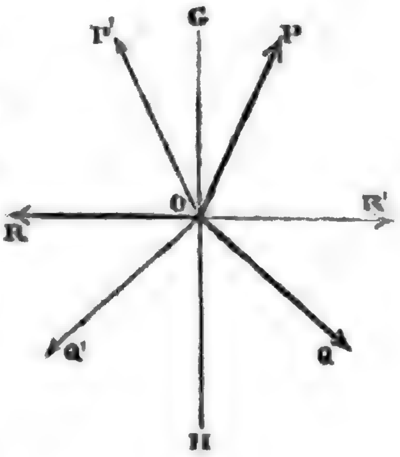
$$R^2 = P^2 + Q^2 + 2PQ\cos\gamma. \quad . \quad . \quad . \quad (16.)$$

Eliminate P, and then Q, from any two of the equations (13. 14. 15.), and reduce by means of (4.),

$$\therefore \frac{P}{\sin\alpha} = \frac{Q}{\sin\beta} = \frac{R}{\sin\gamma}. \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

After having (by any means) obtained the form of $\phi\alpha$ $(= \cos\psi\alpha)$, the formula (17.) may be found in the following manner:—

Produce R O to R′, draw G O H perpendicular to R O R′,



and make $< R'OP' = < ROP$, and $< R'OQ' = < ROQ$. In the directions OP', OQ' and OR', apply the forces P', Q' and R' equal to P, Q and R respectively; these six forces will, as before, be in equilibrium, and R and R' being equal and opposite may be removed; and hence P, P', Q and Q' will balance each other. Now since

$$< POH = < P'OH = \frac{3\pi}{2} - \beta,$$

and $$< QOG = < Q'OG = \frac{3\pi}{2} - \alpha,$$

the resultant of the equal forces

$$P \text{ and } P' = 2P \, . \, \phi\left(\frac{3\pi}{2} - \beta\right),$$

and that of $$Q \text{ and } Q' = 2Q \, . \, \phi\left(\frac{3\pi}{2} - \alpha\right);$$

hence, as there is equilibrium, we must have

$$2P \, . \, \phi\left(\frac{3\pi}{2} - \beta\right) = 2Q \, . \, \phi\left(\frac{3\pi}{2} - \alpha\right);$$

$$\therefore \frac{P}{\phi\left(\frac{3\pi}{2} - \alpha\right)} = \frac{Q}{\phi\left(\frac{3\pi}{2} - \beta\right)};$$

similarly, it is found that

$$\frac{Q}{\phi\left(\frac{3\pi}{2} - \beta\right)} = \frac{R}{\phi\left(\frac{3\pi}{2} - \gamma\right)},$$

$$\therefore \frac{P}{\phi\left(\frac{3\pi}{2} - \alpha\right)} = \frac{Q}{\phi\left(\frac{3\pi}{2} - \beta\right)} = \frac{R}{\phi\left(\frac{3\pi}{2} - \gamma\right)}.$$

Since $\phi\alpha = -\cos\alpha$, this agrees with (17.).

XXXVII. *On the Action of Hyponitric Acid upon Aqueous Solutions of Bromine and Chlorine.* *By* Dr. C. F. SCHŒNBEIN*.

THE hydrobromic and hydrochloric acids being decomposed by nitric acid into water, hyponitric acid and romine or chlorine, it appears little likely that by the com-

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* Communicated by the Chemical Society; having been read April 6, 1846.

bined action of bromine and hyponitric acid water will be decomposed, and the hydrobromic and nitric acids formed. Such however seems to be the case, as will be seen from the following statements.

1. If the fumes of hyponitric acid are made to pass into an aqueous solution of bromine, the brown yellow colour of that fluid will be soon discharged, and its odour of bromine as well as its bleaching power destroyed. The same solution on being mixed either with chlorine or peroxide of lead reassumes its former colour, bromine being eliminated under those circumstances. It is hardly necessary to add, that an aqueous solution of bromine is also discoloured by nitric acid holding some hyponitric acid dissolved. Having added to aqueous bromine a sufficient quantity of hyponitric acid, the liquid obtained contains no trace either of bromine or hyponitric acid, and is in every respect like a mixture of dilute nitric and hydrobromic acids. From the facts stated, it appears that at the common temperature bromine and hyponitric acid on their being put in contact with water are transformed into hydrobromic and nitric acids, and according to the theory of the day, we must account for that reaction by admitting that water is decomposed under the circumstances mentioned.

2. A yellow aqueous solution of chlorine treated with hyponitric acid in the way indicated under § 1, loses its colour, odour and bleaching power, and has exactly the same properties enjoyed by a mixture of dilute hydrochloric and nitric acids. Hence it follows that hyponitric acid acts upon aqueous chlorine in the same way as it does upon an aqueous solution of bromine.

3. If into a bottle, having previously been charged with hydrochloric acid gas, some strong and pure nitric acid is introduced, the latter will, even at a temperature of 15° R. below zero, be rapidly decomposed, chlorine and hyponitric acid being eliminated and water formed. Adding to that mixture a sufficient quantity of water, both chlorine and hyponitric acid will entirely disappear, *i. e.* hydrochloric and nitric acids be reformed.

From the facts above stated, it follows,—

a. That concentrated nitric and hydrochloric acids decompose each other into hyponitric acid, chlorine and water, even at very low temperatures.

b. That a sufficient quantity of water being present, chlorine or bromine, conjointly with hyponitric acid, decompose water (according to the theory of the day), forming nitric and hydrochloric or hydrobromic acid.

c. That nitric and hydrochloric or hydrobromic acid being dissolved in a sufficient quantity of water, can at the common temperature coexist without decomposing each other.

XXXVIII. *Notices respecting New Books.*

Elements of Physics. By C. F. PESCHEL, *Principal of the Royal Military Academy at Dresden. Translated from the German, with notes, by* E. WEST. 3 vols. foolscap octavo.

THIS is a work of great merit. It contains a condensed account of almost every branch of the extensive subject on which it treats, with the most approved formulæ, conveniently arranged, for computing in every department, and it is copiously illustrated with woodcuts.

The first volume treats of the properties of *ponderable* bodies,—mass, density, attraction, motion, mechanical and chemical action, &c.; the construction of machines, the laws of force, the theory of undulation, and a multitude of other subjects connected with matter having sensible *weight*, and all discussed with an ability which leaves little to be desired in a work which, though essentially scientific, is yet decidedly *popular*.

We should have been glad to see from the author, *or the translator*, a chapter on the measurement of heights from the temperature of boiling water, a subject which we think has not received the attention which it is entitled to.

The second and third volumes, that of *imponderable* bodies—light, heat, electricity, magnetism, &c.; and here again we trace the hand of a master, who has placed before his readers, in the most lucid order, those branches of science in their modern improved state.

The work is a little encyclopædia of physical science, and we heartily recommend it as a work by which the public will benefit.

XXXIX. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 144.]

Ancient Eclipses of the Sun, computed by M. Rumker.

May 8, 1846. M. HEISS, of Cologne, has calculated the particulars of two eclipses during the Peloponnesian war which are mentioned in history, viz. one on August 3, 431 A.C., and another on March 21, 424 A.C.

M. Rumker has given the particulars of four more, in the hope that they may have been noticed by some ancient writer:—

A.C.		Beginning.	Middle.	End.	Digits eclipsed.	Place of Observ.
		h m s	h m s	h m s		
418	June 11.	11 43 4 A.M.	0 28 9 P.M.	1 14 5 P.M.	1·9	Athens.
416	Oct. 14.	4 33 0 A.M.	5 2 7 A.M.	5 32 5 A.M.	1·4	Syracuse.
411	Jan. 27.	10 7 2 A.M.	10 59 5 A.M.	11 52 0 A.M.	2·9	Athens.
409	June 1.	1 49 5 P.M.	3 7 6 P.M.	4 14 0 P.M.	7·5	Athens.

Eclipses of the sun visible at Athens, occurred Nov. 4, 426 A.C. and Oct. 25, 417 A.C.

The lunar eclipses visible at Athens during the war are,

A.C.		A.C.		A.C.	
428	June 17	421	Feb. 2	413	Aug. 27
425	April 15	421	July 28	407	Oct. 21
425	Oct. 9	418	Nov. 21	406	April 15

On the Diminution of Brightness in β Ursæ Minoris.

Extract of a letter from Sir J. F. W. Herschel to the President:—

Allow me to call your attention to the remarkable diminution which has taken place in the apparent brightness of β Ursæ Minoris since 1840 and 1841. In the course of a series of observations which I carried on in those years, on the comparative brightness of the stars, the star in question was always ranked above Polaris, and far above β Ursæ Majoris and α Coronæ. To the last-named star it is still superior, and perhaps a little (but very little) to the former; but with Polaris it will bear no comparison. That the change is in β, and not in Polaris, is evident. The latter retains its rank; it remains (as I have always observed it) much inferior to α, ε, and η Ursæ Majoris; but when compared with ζ, there is some doubt. I incline to place it a little higher in the scale than that star; and I find on consulting my lists, that on the 30th April, 1840, it was judged then also superior to ζ, but that on the next night this order was reversed; proving that then, as now, a difficulty existed in deciding the relative claims of the two stars to superiority.

β Ursæ Minoris must therefore clearly claim a place among the slowly variable stars, a class of especial interest; and I hope this notice may suffice to induce some of our astronomical fraternity to keep an eye on its future progress.

On the Double Stars, δ Cygni, γ Leonis, &c. By J. R. Hind, Esq.

In 1783 Sir W. Herschel estimated the distance of the stars of δ Cygni to be nearly $2''{\cdot}5$; in 1802 and 1804 he could not see the companion at all. It was rediscovered in 1826 by M. Struve, who found the distance to be nearly $2''$, whence he conjectured the periodic time was about 40 years. This guess was, however, contradicted by subsequent observations; and M. Struve now accounts for the anomalous disappearance by supposing the light of the smaller star to be variable. Mr. Hind has computed two orbits which satisfy the observations pretty well, and which nearly agree, except in the node and inclination. The method of calculation is that described by Sir J. F. W. Herschel in the Memoirs of the Astronomical Society, vol. v. p. 171, &c. Though these elements are only approximate, yet they give a general idea of the form of the orbit. The periodic time is about 180 years.

By the same mode of calculation, Mr. Hind has obtained a *first* approximation to the orbit of γ Leonis, with a periodic time of nearly

300 years. As the angle of position has only altered 24° in 63 years, the result is rather uncertain.

Mr. Hind points out several double stars which are promising subjects for investigation; ξ Scorpii, 12 Lyncis, ζ Aquarii, 36 Andromedæ, 85 Persei, η Tauri, υ^1 Cancri, σ^2 Ursæ Majoris, 157 Lyncis, 49 Serpentis, ο Draconis, and Σ 460, 520, 1126, 1457, 1757, 1819, 2107, 2120, 2438. He conceives that ξ Scorpii *probably* revolves in a very elongated ellipse.

Suggestions for Improving the Manufacture of Optical Glass. By James Nasmyth, Esq.

Mr. Nasmyth, after remarking, for our encouragement, that the problem is possible, since large discs of homogeneous glass are made elsewhere, expresses his conviction that the proper materials in sufficient purity are at our command, and that the difficulty is in the mode of effecting perfect combination and vitrification.

He proposes to carry the heat of the furnace to the highest practicable degree, thus ensuring perfect fusion and fluidity, and then, by maintaining the heat for a considerable time, to give the particles time to arrange themselves in their order of density. He would then lower the heat so gradually as to avoid disturbing their arrangement, but not so slowly as to endanger its vitreous quality. The melting-pots should be cylindrical in form, and as deep as prudence will permit. The mass, when cool, is to be sawn across in parallel slices. In this way Mr. Nasmyth conceives that discs nearly homogeneous would be procured, and at any rate that the density would be uniform through each horizontal section, which perhaps would be sufficient for optical purposes. If there be any tendency to unite in definite proportions, it is clear that the circumstances described would favour the combination.

Without venturing to give a positive opinion on such a subject, the method seems founded on rational principles. It is high time some vigorous attempt should be made to manufacture optical glass in this country. The foreign supply is scanty, and often of indifferent quality. At Munich, *finished* object-glasses, *only*, are furnished; the price is high, and the telescopes not always of corresponding quality. It is nearly a century since Dollond discovered the principle of achromatism, and constructed his admirable telescopes; and it may be doubted whether the manufacture of optical glass has since advanced one step in this country. The great bar to improvement, the vexatious excise laws, is removed; and the first person who can restore this country to her original pre-eminence in optical glass-making, will not merely derive a large pecuniary recompense, but be entitled to the gratitude of every lover of astronomical science.

At the close of the meeting Mr. Sheepshanks gave a short account of the origin and construction of instruments on the *repeating* principle, expressing an opinion that, though the improved construction and division of astronomical and geodesical instruments had in later years taken away much of the original value of this invention, yet that, under some circumstances, a repeating stand for a theodolite or an astronomical circle repeating in altitude might be useful.

When the causes of error in measuring an angle, terrestrial or celestial, are separately considered, the sum of the errors of division and of reading off will in some cases be much larger than the error of bisecting the object or of reading off the level. Whenever this is so, then, *supposing no fresh error to be introduced*, the repeating principle may be advantageously applied, since the errors of division and reading off are divided by the *whole* number of observations, and not by the *square root* of the number, as is the case in non-repeaters.

The defect most to be feared in repeating instruments is the slipping of a part which should be fixed while another part is moved. This may probably be avoided in the *repeating stand* for a theodolite, by making the lower motion exceedingly heavy while the upper motion is exquisitely light, and by affixing the clamp of the stand at as large a distance as may be from the centre of motion. By measuring the angle twice over, first moving the theodolite and stand the same way, and again moving them the contrary way, any error from slipping will be detected. The coincidence of results by the two methods will be a satisfactory test of their truth.

When the repetition is in altitude, the level, *if attached to the circle*, is a sufficient protection against slipping. It is, perhaps, owing to the mistake of fixing the level to the vertical axis, and polishing the limb so highly that the clamp could not bite hard, that the repeating circle lost much of its reputation.

In Borda's circle, the clamp for moving the vertical circle is inconveniently placed; *two* observers are required (unless the support of the instrument is immoveable), and the telescope, in the original construction, is very loosely held. The last fault is easily amended, and by sacrificing the motion round the upper axis (which is utterly useless in an astronomical instrument), a convenient situation may be given to the circle clamp. Mr. Sheepshanks exhibited a form of the instrument which can be used single-handed. The line of sight of the telescope is turned by a prism down the tube which forms the axis of the vertical circle; thus the observer, without changing his position, can read off the level immediately after the bisection of the star; but the instrument has not as yet been tested experimentally.

To measure the effect of flexure in the telescope, which seems to be the only *fixed* source of error in this class of instruments, Mr. Sheepshanks proposes either to measure the angle of 180° between two collimating telescopes, after Bessel's method, or to observe the circum-meridian altitudes of the same star in three fashions: first in the ordinary manner, in which the flexure tells one way; secondly, observing the star in mercury, where the nadir distances will be affected by flexure to the same amount but the *contrary* way from the zenith distances; and thirdly, by stepping from the star seen directly to the star seen by reflexion, which is not affected by flexure at all. A careful set of observations would probably give the constant of flexure with great nicety.

ZOOLOGICAL SOCIETY.

June 23, 1846.—Professor Owen read a Memoir (Part II.) on the Dinornis, descriptive of parts of the skeleton transmitted from New Zealand since the reading of Part I. (Annals, vol. xiv. p. 59.)

The bones referable to species defined in that communication were first described. Among these were the cranial portion of the skull of *Dinornis struthoides* and a corresponding portion of the skull of *Dinornis dromioides*, which in general form more resembled that part of the skull of the Dodo than of any existing bird; but they are remarkable for the great breadth of a low occipital region, which slopes from below upwards and forwards; the almost flat parietal region is continued directly forwards into the broad sloping frontal region; the temporal fossæ are remarkably wide and deep; the orbits small; the olfactory chamber expanded posteriorly, but not to so great an extent as in the Apteryx; the plane of the foramen magnum is vertical. Many other characteristics in the cranial organization of the genus *Dinornis* were described, and the specific distinction of the two mutilated crania pointed out.

The tympanic bone of the *Dinornis giganteus* was described in detail and compared with the same bone in existing birds.

Different cervical and dorsal vertebræ, referable to the species *Din. giganteus*, *ingens*, *struthoides* and *crassus*, were described. These vertebræ were remarkably entire, and with some of the best-preserved bones of the extremities, described in a subsequent part of the Memoir, had been obtained from a turbary formation on the coast of the Middle Island, near Waikawaite.

One of the most interesting of the novel acquisitions from this locality was an almost entire sternum, referred by Prof. Owen to the *Din. giganteus*. It is a subquadrate, keel-less, shield-shaped bone, broader than long, with the posterior angles and the xiphoid process prolonged, as in the Apteryx, but without the anterior emargination. The coracoid depressions very small. This bone was minutely described and compared with the keel-less sternums of the existing Struthious birds; that of the Apteryx being demonstrated to be most like the sternum of *Dinornis*.

The following bones of the extremities, imperfectly or not at all known in 1843, were next described:—

The entire femur of *Dinornis giganteus*. Entire tibiæ and tarso-metatarsi of *Din. giganteus*, indicating a robust variety of this stupendous bird to have existed in the Middle Island.

The tarso-metatarsus of *Dinornis ingens* from the North Island, distinguished by a rough depression indicative of a fourth or back-toe, and consequently a genus (*Palapteryx*) distinct from *Dinornis*.

Femora, tibiæ and tarso-metatarsi of a Dinornis of the height of the *Din. ingens*, but of more robust proportions, from the Middle Island; with a feeble indication of a surface for a back-toe.

The tibiæ and tarso-metatarsi of *Dinornis* (*Palapteryx*) *dromioides* from the North Island, confirming by their long and slender proportions the conjecture hazarded in the author's former memoir (Zool.

Trans. vol. iii. pp. 252, 264). The tarso-metatarsus also shows the rough elliptical surface for the attachment of the back-toe, indicating the *Din. dromioides* to belong to the same generic or subgeneric section as *Din. ingens* from the North Island.

Femora, tibiæ and *tarso-metatarsi*, from the Middle Island, were next exhibited and described, which establish a new species, for which Prof. Owen proposed the name of *Din. casuarinus*: a small and feeble depression, five lines by three lines, indicates that this species had a back-toe in the corresponding position with that in the Apteryx, but more rudimental.

A very remarkable femur and tarso-metatarsal bone, also from the Middle Island, were exhibited, belonging to an additional tridactyle species, to which the name of *Dinornis crassus* was given. Of this species the author remarks: "With a stature nearly equal to that of the Ostrich, the femur and tarso-metatarsus present double the thickness in proportion to their length. It must have been the strongest and most robust of birds, and the best representative of the pachydermal type in the feathered class."

The third new species is comparatively a small one, being intermediate in size between the *Dinornis didiformis* and the *Din. otidiformis*; it was founded on remains exclusively from the North Island, and was called by the author *Dinornis curtus*.

The paper (which was illustrated by numerous figures) concluded by some general comparisons and remarks on the geographical distribution of the different species of *Dinornis*.

July 14, 1846.—Prof. Owen communicated, as an 'Appendix to his Memoir on the Dinornis,' some observations on the skull and on the osteology of the foot of the Dodo (*Didus ineptus*).

After a brief summary of the history of this remarkable extinct brevipennate Bird, in which the reduced highly finished figure by Savery, in his famous painting of 'Orpheus charming the Beasts,' now in the collection at the Hague, was particularly noticed; and the recent discovery of the skull of the Dodo amongst some old specimens in the Museum of Natural History at Copenhagen was mentioned, he proceeded to demonstrate the peculiarities of the Dodo's skull, by a comparison of the cast of the head of the bird in the Ashmolean Museum at Oxford with those of other recent and extinct species of Birds.

The Dodo's skull differs from that of any species of *Vulturidæ*, or any Raptorial Bird, in the greater elevation of the frontal bones above the cerebral hemispheres, and in the sudden sinking of the interorbital and nasal region of the forehead; in the rapid compression of the beak anterior to the orbits; in the elongation of the compressed mandibles, and in the depth and direction of the sloping symphysis of the lower jaw. The eyes of the Dodo are very small compared with those of the *Vulturidæ* or other *Raptores*. The nostrils, it is true, pierce the cere, but are more advanced in position; this however seems essentially to depend upon the excessive elongation of the basal part of the upper mandible before the commencement of the uncinated extremity; the nostrils are pierced near the commencement of this uncinated part as in the *Vulturidæ*, but are nearer the lower border of the mandible in the Dodo.

The resemblance between the skull of the Dodo and that of the Albatros is chiefly in the compression and prolongation of the curved mandibles: there are no traces in the Dodo of the hexagonal space on the upper surface of the cranium of the Albatros, so well defined there by the two supra-occipital ridges behind, the two temporal ridges at the sides, and the two converging posterior boundaries of the supra-orbital glandular fossæ in front. There is no sudden depression of the frontal region in the skull of the Albatros; the nostrils are near the upper surface of the basal third of the beak in the Albatros; and the Dodo's cranium is thrice as broad in proportion to the breadth of the mid-part of the mandible as in that of the Albatros.

More satisfactory evidence of the affinities of the Dodo was obtained from a comparison of the bones of the foot, which have recently been very skilfully and judiciously exposed by the able Curator of the Ashmolean Museum.

The tarso-metatarsal bone most resembles in its thickness and general proportions that of the Eagles, especially the great Sea-Eagles (*Haliaëtus*); it is much stronger than the tarso-metatarsus of any of the *Vulturidæ*, or than that of the Cock, the *Crax*, or any of the *Gallinæ* or existing *Struthionidæ*; the stronger-footed species of *Dinornis* most resemble it in the general proportions of the tarso-metatarsus, but greatly differ in the particular configuration of the bone, and in the absence, or feebler indication, as in the subgenus *Palapteryx*, of the articulation for the metatarsal bone of the back-toe. The relative size of this bone is greater in the Dodo than in any other known bird. The Eagles make the nearest approach to it in this respect; as also in the shape of the hinder supplemental metatarsal, the breadth of its distal end, and its peculiar twist backwards and outwards, so as to form a bridge or pulley against which the flexor tendon of the hind-toe plays. This half-twist of the rudimental hind-metatarsus is feebly repeated in the *Gallinæ*, but the bone is much less expanded at its lower articular end, especially in the *Crax*; whilst the more typical *Gallinæ* are further distinguished from the Dodo by their spur.

The Apteryx is the sole existing Struthious bird which possesses the hind-toe; but it is very much smaller than in the Dodo, and the supporting metatarsal bone is devoid of the distal twist and expanded trochlea. The upper end of the tarso-metatarsus of the Dodo is remarkable for the great development of its calcaneal process, from which a strong ridge descends, gradually subsiding, half-way down the bone. The posterior surface of the calcaneal process is broad, triangular, vertically grooved and perforated at its base. In the Eagle the corresponding calcaneal process is a compressed, subquadrate ridge, whose base of attachment is not much longer than the obtuse end, and this is neither grooved nor perforated. In the *Cathartes Californianus* the calcaneal process is thicker than in the Eagle, shaped more like that of the Dodo, with a ridge descending upon the metatarsus, but it has a double groove behind.

In the Common Cock the calcaneal process more resembles that in the Dodo than the Vulture's does, but it is not so broad.

With regard to the first or proximal phalanx of the hind-toe, that of the *Haliaëtus* is larger and broader, especially at its base, stronger in proportion to its length, but longer in proportion to the sustaining metatarsus.

In the Vultures the proximal phalanx is not only longer in proportion to the metatarsus, but is more slender than in the Dodo. The same bone is also longer and more slender in proportion to the small supporting metatarsal bone in the Cock, the *Crax*, and all other *Gallinæ*; in fact, the Dodo is peculiar among Birds for the equality of length of the metatarsus and proximal phalanx of the hind-toe. With regard to the three trochlear extremities of the principal coalesced metatarsals, the middle one in all *Gallinæ* is longer in proportion than in the Dodo, in which the inner one is nearly as long as the middle one, the outer one being the shortest. In the Eagle the inner division is of quite equal length with, or is longer than the middle trochlea; the proportions of the three trochleæ in the Vultures corresponding best with those in the Dodo. Another character by which the Dodo resembles the Vulture more than the Eagle is manifested by the proportions of the proximal phalanx of the second toe (innermost of the three anterior ones); this is very short, and is often anchylosed to the second phalanx in the Eagles: it is almost as long in the Vultures as in the Dodo.

Upon the whole, then, the Raptorial character prevails most in the structure of the foot, as in the general form of the beak, of the Dodo, compared with Birds generally; and the present limited amount of our anatomical knowledge of the extinct terrestrial Bird of the Mauritius would lead to support the conclusion that it is an extremely modified form of the Raptorial Order.

Devoid of the power of flight, it could have had small chance of obtaining food by preying upon the members of its own class; and if it did not exclusively subsist on dead and decaying organized matter, it most probably restricted its attacks to the class of Reptiles, and to the littoral fishes, Crustacea, &c.

The author concluded by recommending search to be made for bones of the Dodo in the superficial deposits, the alluvium of rivers, and the caves in the islands of Mauritius and Rodriguez; little doubting that an active exploration would be as richly rewarded as similar investigations have been in the islands of New Zealand, by the recovery of the remains of the great extinct species of terrestrial birds which formerly inhabited them.

ROYAL IRISH ACADEMY.

At the Annual Meeting on the 16th of March last, the following Address was delivered from the Chair by Sir William Rowan Hamilton, L.L.D. &c. &c.

"My Lords and Gentlemen of the Royal Irish Academy,—Although it is, I believe, well known to most, perhaps to all, of you, that it has been for a considerable time my wish and intention to retire this evening from the Chair to which, in 1837, your kindness called me, on the still lamented event of the death of my distinguished predecessor, the late admirable Dr. Lloyd, and in which your continuing confidence has since replaced me on eight successive occasions, yet a few parting words from me may be allowed, perhaps expected; and I should wish to offer them, were it only to guard against the possibility of any one's supposing that I look upon my thus retiring from your Chair as a step unimportant to myself, or as one which might be taken by me with indifference, or without deliberation. It was under no hasty impulse that I resolved to retire from the office of your President into the ranks of your private members, nor was it lightly that I determined to lay down the highest honour of my life.

"My reasons have been stated in an Address delivered in another place, at a meeting of some members of your body. They are, briefly, these: that after the expiration of several years, I have found the duties of the office press too heavily upon my energies, indeed, of late, upon my health, when combined with other duties; and that I have felt the anxieties of a concentrated responsibility—exaggerated, perhaps, by an ardent or excitable temperament—tend more to distract my thoughts from the calm pursuits of study, than I can judge to be desirable or right in itself, or consistent with the full redeeming of those pledges which I may be considered to have long since given, as an early Contributor to your Transactions.

"When I look back on the aspirations with which first I entered on that office from which I am now about to retire, it humbles me to reflect how far short I have come of realizing my own ideal; but it cheers me to remember how greatly beyond what I could then have ventured to anticipate, the Academy itself has flourished. Of this result I may speak with little fear, because little is attributable to myself. Gladly do I acknowledge that it has been my good fortune, rather than my merit, to have presided over your body during a period in which, through the exertions of others much more than through my own (though mine, too, have not been withheld), the Academy is generally felt to have prospered in all its departments. The original papers which have been read; the volumes of Transactions which have been published; the closer communication which has been established with kindred societies of our own and of foreign countries; the enhanced value of our Library and Museum, which have been, at least, as much enriched in the quality as in the quantity of their contents; the improved state (as it is represented to me) of our finances, combined with an increased strength of our claims on public and parliamentary support; the heightened interest of members and visiters in our meetings, which have been honoured on four occasions, during my presidency, by the presence of representatives of Royalty; even the convenience and appropriate adornment of the rooms in which we assemble;—all these are things, and others might be named, in which, however small may have been the share of him who now addresses you, the progress of the Academy has not been small, and of which the recollection tends to console one who may, at least, be allowed to call himself an attached member of the body, under the sense, very deeply felt by him, of his own personal and official deficiencies.

"Whoever may be the member elected by your suffrages, this evening, to occupy that important and honourable post which I am now about to resign, it will, of course, become my duty to give to that future President my faithful and cordial support, by any means within the compass of my humble power. But if it be true, as I collect it to be, that your unanimous choice will fall upon the very member whom, out of all others, I should have myself selected, if it could have been mine to make the selection—with whom I have been long connected by the closest ties of College friendship, strengthened by the earnest sympathy which we have felt in our aspirations for the welfare of this Academy, which has already benefited by his exertions in many and important ways—then will that course, which would have been in any event my *duty*, be in an eminent degree my *pleasure* also.

"And now, my Lords and Gentlemen, understanding that an old and respected member is prepared to propose for your votes, as my successor, the friend to whom I have ventured to allude—very inadequately, as regards my opinion of his merits, yet, perhaps, more pointedly than his modesty will entirely forgive or approve of,—I shall detain you no longer from that stage of the proceedings of the evening which must be the most interesting to all of us, but shall conclude these words of farewell from this Chair, by expressing a hope that my future exertions, though in a less conspicuous position, shall manifest, at least in some degree, that grateful and affectionate sense which I must ever retain of the constant confidence and favour which you have, at all times, shown towards me."

After which it was Resolved,—That the thanks of the Academy be given to Sir William R. Hamilton, and that the Academy desire to express their entire sense of the value of his services as President, of his high and impartial bearing in the Chair, and of his untiring efforts to advance the interests of the body; and they also wish to record their satisfaction that he has determined to remain in the Council of the Academy.

The following officers were then elected:—The Rev. Prof. Lloyd, President, in the place of Sir William Hamilton; Dr. Todd, Secretary; the Rev. Charles Graves, Secretary of Council; and the Rev. Mr. Butcher, Secretary of Foreign Correspondence; in the rooms respectively of Dr. MacCullagh, and Sir Robert Kane, resigned, and of the Rev. Dr. Lloyd, who was elected President.

XL. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF A CALCAREOUS ROCK ON WHICH FUCI AND VARECHS VEGETATE. BY M. LASSAIGNE.

THE rocks on which these substances grow on a part of the western shore of France, are covered daily with the water of the ocean. In order to examine the nature of these rocks, some fragments were detached from the surface, to which several of the marine plants remained attached.

The substance which constitutes these rocks possesses considerable cohesion; externally it is yellowish-white, and internally chalk-white. When dried in the air, it has the appearance of moderately hard chalk, and is friable. Its porosity having occasioned the absorption of a small quantity of sea-water, it retained a portion of its salts; superficially this rock has a saline taste, which does not exist at the depth of about one inch and a half to two inches.

The unquestionable existence of iodine, in the state of iodide of sodium in the greater part of marine plants, and its absence from sea-water, as proved by the experiments made by M. Gualtier de Claubry in 1813, and those since published by Davy, induced the author to search for this element in the rock on which the plants grow.

About 462 grains reduced to fine powder were treated with cold alcohol of 90 per cent. and afterwards with water. These two liquids, submitted separately to evaporation, left a slight dirty white residue, weighing only a small fraction of a grain; this was redissolved in water and found to be chloride of sodium, with traces of sulphate of soda and of lime. The presence of an iodide in this residue could not be detected either by solution of nitrate of silver or solution of starch, either in contact with concentrated sulphuric acid, or with a small quantity of a very weak solution of chlorine. The first of these reagents produced a curdy-white precipitate which was entirely soluble in ammonia, and was consequently entirely pure chloride of silver; the second reagent was not at all coloured under the circumstances described; and lastly, the solution of bichloride of palladium, which discovers the smallest quantity of iodine, produced no effect.

The portion of rock which had been subjected to the successive action of alcohol and water, was mixed with water and treated with weak nitric acid, which gradually dissolved it with brisk effervescence, except a small white residue, which was collected on a weighed filter. A part of this dried residue adhered to the filtering paper, and had the soapy and soft feel of alumina; it adhered slightly to the tongue, and was in fact a combination of silica and alumina; it weighed a small fraction of a grain; the nitric solution was slightly acid and colourless; solution of starch gave no indication of iodine; when saturated with ammonia it yielded slight traces of alumina; the solution exposed to the contact of the air became gradually turbid and brown, and deposited brown flocculi, which, after some days, were collected and found to be peroxide of manganese; it probably existed as protocarbonate in the rock. The solution was then treated with oxalate of ammonia, which precipitated the lime, and potash afterwards threw down a little magnesia.

From the facts above stated, M. Lassaigne concludes,—

1st. That the calcareous rock obtained west of the city of Dieppe, on which the fuci or varech grow, contains no compound of iodine.

2nd. That the rock is essentially composed of carbonate of lime, with a small quantity of carbonate of magnesia and of manganese.

3rd. That the rock contains a small proportion of argillaceous earth in a state of mixture, which separates when the rock is dissolved by a weak acid at common temperatures.

4th. That additional researches are required to ascertain from what substances marine plants obtain the iodine which they contain, if we admit, with the chemists above-quoted, that it is not found in sea-water, even in very minute quantity.—*Journ. de Chim. Méd.*, Juin 1846.

PREPARATION OF PROTIODIDE OF IRON. BY M. C. CALLOUD.

To determine the nature of this preparation, as usually obtained, the author examined several specimens of it, and he found not only that the iodide had suffered great change, but even entire decomposition; in the latter case the substance appeared to be soluble in water, but this solubility was owing to the accidental presence of a saline iodide, entirely different from iodide of iron; he found that some which had been recently prepared was half-decomposed; some which had deliquesced, was decomposed to the extent of three-fifths, while a portion which had been dried after deliquescence was totally decomposed.

One hundred parts of the dry iodide of iron of druggists were treated with distilled water; the solution, after being filtered to separate the insoluble portion, was treated with solution of potash, which precipitated the portion of iron remaining combined; calculating from the portion remaining insoluble in water, and from that precipitated by potash, the composition of the iodide appeared to be,

Iodide of iron, presumed to be in the state of protiodide	54·20
Iodine uncombined	34·80
Sesquioxide of iron	11·00
	100·00

The second specimen appeared to consist of—

Protiodide of iron	38·80
Iodine uncombined	41·70
Sesquioxide of iron	14·50
Hygrometric moisture	5·00
	100·00

Both these preparations yielded turbid solutions of a deep colour, and possessed the odour of iodine; they did not exhibit the properties of persalts of iron when submitted to the action of tests; they gave a bright blue precipitate with the ferrocyanide of potassium, and a green one with the alkalies.

The iodide which had been dried after deliquescence, was, as already stated, entirely decomposed, and all the iron had become oxyiodide, forming with the reduced iodine a mixture totally insoluble in water; no trace of iron could be detected in the solution by the most sensible tests, but merely manganese, which existed in the state of sesquioxide; this was derived from the iron. It is therefore evident that iodide of iron obtained by evaporation to dryness is a very imperfect preparation, and that its employment is attended with some inconveniences.

During the evaporation of a solution of protiodide of iron, disengagement of hydriodic acid gas takes place, and from this results the first decomposition of the salt; and a product is obtained containing a corresponding quantity of oxyiodide, mixed with undecomposed iodide; and this decomposition of the solution into hydriodic acid and oxyiodide does not cease until the aqueous vapour is produced in sufficient quantity to prevent the contact of the air with the salt in solution.

M. Calloud observes, that by analogy with the chlorides of iron the periodide of iron should be a more stable compound than the protiodide; experience however shows that this is not the case, for the iodide of iron, being constantly subjected to the hygrometric and oxygenating action of the air, becomes completely decomposed, even at the usual temperature of the air; and when the iodide of iron of the shops is examined, laminæ of crystallized iodine, adhering to a bluish friable insoluble mass, may be seen by the naked eye; this substance is merely a mixture of reduced iodine and sesquioxyiodide of iron.—*Journ. de Chim. Méd.*, Août 1846.

ON THE SOLUBILITY OF ALUMINA IN SOLUTION OF AMMONIA.

MM. J. Malaguti and J. Durocher state that in analysing the laumonite it was found an enormous loss of alumina was sustained; it is well known, they observe, that this mineral is a silicate of lime and of hydrate of alumina; on account of the presence of lime the authors diluted the acid solution before precipitating it with ammonia. They were surprised on obtaining in this manner only 8 out of 100 of alumina, notwithstanding the presence of some sal-ammoniac in the solution; and one of the authors states that several years since, in making numerous analyses of kaolin by hydrofluoric acid (a process in which it is requisite that the solution of the alumina of the mineral should be very dilute), he was obliged to give up the use of liquid ammonia as a precipitant and to employ sulphuret of ammonium, on account of the large quantity of alumina which remained in solution.

It is, indeed, well known that alumina is never precipitated perfectly from its solutions by ammonia, without the presence of ammoniacal salts. The quantity which remains in the liquid, far from being scarcely appreciable, may on the contrary exist in considerable proportion, and which is increased, not only by the excess of ammonia, but also according as the solution is more or less diluted with water.

Alumina is indeed precipitated entirely when ammoniacal salts are present; but the quantity of these salts ought to be greater as the solution of alumina is more dilute. Alumina may be separated entirely from a solution, even when very dilute, and notwithstanding an excess of ammonia and the absence of ammoniacal salts, provided a considerable time is allowed to elapse between that of the precipitation and filtration.

Lastly, alumina may be precipitated immediately and completely by sulphuret of ammonium, even when the solution is very dilute and no ammoniacal salts are present.

The authors observe that nothing is more easy than to show the great solubility of alumina in solution of ammonia, and the very strongly marked precipitating power of sulphuret of ammonium. Prepare two solutions, one of 2 grammes of ammoniacal alum in 50 grammes of water, and the other of 2 grammes of the same salt in 200 grammes of water, and let the same volume of solution of ammonia, say 15 cubic centimetres, be poured into both solutions, it will be seen that the precipitate produced in the solution, the total volume of which is 65 cubic centimetres, is incomparably greater than that produced in the solution the volume of which is 215. It is sufficient to filter the most bulky solution, to divide into two equal portions, and to expose one to ebullition and the other to a current of sulphuretted hydrogen, to produce in both cases a considerable precipitate of alumina.

If, instead of passing sulphuretted hydrogen into the alumino-ammoniacal solution, a current of carbonic acid be sent through it, a large precipitate of alumina will also be obtained; but whereas, in the first case, the whole of the alumina is precipitated, in the second only a considerable portion is thrown down. In fact if the alumina precipitated by the sulphuretted hydrogen be separated by the filter, and the filtered liquor be even long boiled, not the slightest trace of alumina will appear; but if the solution which has been treated with carbonic acid be boiled, a sensible quantity of flocculent alumina will appear, when all the carbonate of ammonia has been volatilized. The same is also the case if the alumino-ammoniacal liquor be treated with sal-ammoniac.

When 10 grammes of sal-ammoniac in powder are added to 250 cubic centimetres of alumino-ammoniacal liquor, prepared by the precipitation of 2 grammes of ammoniacal alum, by 15 cubic centimetres of solution of ammonia, a considerable deposit of alumina is formed: let this alumina be separated by filtration, and again add 10 grammes of sal-ammoniac to the filtered solution, and there will be no fresh appearance of alumina; but if another 10 grammes of the salt be added, more alumina, though small in quantity, is thrown down.

It is then evident that in order to precipitate the last portions of alumina immediately, they require a much larger quantity of sal-ammoniac than the first, and the quantity is perhaps greater than chemists generally suppose. If also, instead of immediately filtering the precipitate obtained by carbonic acid and sal-ammoniac, it be suffered to remain for some days in close vessels, all the alumina will be precipitated.—*Ann. de Ch. et de Phys.*, Août 1846.

ON THE BIBASIC ARSENIATE OF AMMONIA AND MAGNESIA.

BY M. LEVOL.

The author observes, that, notwithstanding the striking analogies presented by arsenical and phosphoric compounds, it does not appear to have been ascertained whether an arsenical compound exists, corresponding to the most important of the ammoniaco-magnesian double phosphates, or that which occurs among certain products of animal organization, and which is so frequently employed in chemical analyses, on account of its insolubility, to determine the quantity of phosphoric acid.

The author thought it would be interesting to determine if it were possible to produce an arseniate analogous to this phosphate, and to apply it, on account of its insolubility, to ascertain the quantity of arsenic acid; and he found, in fact, that such a salt exists, and that it resembles in every respect the bibasic phosphate of the same bases, and its formula is similar to that which Berzelius assigns to the phosphate $2NH^3, 2MgO, AsO^5+10HO$.

This salt is obtained like the corresponding phosphate, that is to say by pouring a soluble double ammoniaco-magnesian salt into a solution of arsenic acid, after having rendered it ammoniacal; like the phosphate, it does not immediately appear, but only after the lapse of a few seconds, unless it be agitated; it deposits in the same way in the form of very small crystals on the sides of the vessel; its insolubility may also be compared to that of the phosphate: one part of arsenic acid, diluted with 56818 parts of ammoniated water, was rendered sensible soon after the addition of a few drops of a concentrated solution of ammoniaco-magnesian sulphate; and the author is of opinion that it would be difficult to mention two salts more exactly comparable each to the other, than the phosphate and arseniate under consideration.

The new salt was applied in a very difficult case of chemical analysis, that of the quantitative separation of arsenious and arsenic acids, the former not yielding a double insoluble salt with ammonia and magnesia. When the precipitate is collected, it is to be dried and calcined to redness, taking care that it is not exposed to any reducing influence; there remains $2MgO, AsO^5=55{\cdot}74$ of 100 of the new salt, which represent 41·02 of arsenic acid. The author suggests that the ammoniaco-magnesian double salts might be employed as remedies in cases of poisoning by arsenic acid.—*Ann. de Ch. et de Phys.*, Août 1846.

EXAMINATION OF A FOSSIL RESIN FOUND NEAR EU (SEINE-INFERIEURE). BY MM. LASSAIGNE AND CHEVALLIER.

This substance may be mistaken for amber, which it resembles in its yellowish colour, transparency, fusibility and combustibility. One specimen of a pale yellow colour, was of sp. gr. 1·094; the other, which was reddish-yellow, had a sp. gr. of 1.

The first of these specimens was reduced to fine powder, and macerated during twelve hours in sulphuric æther, the action of which was assisted by repeatedly shaking the vessel, and eventually heating it to about 90° Fahr.

After this reaction the æther was filtered and had acquired a slight golden-yellow colour; it was introduced into a retort with a small quantity of water, and distilled with a gentle heat. After the distillation of the æther there remained a fluid resin of an amber-yellow colour, possessing the consistence and viscidity of common turpentine, and part of it solidified on cooling. The water in which this resin floated was poured off, it was colourless and reddened litmus paper strongly; it was slowly evaporated at the usual temperature, by placing under a glass with a vessel containing concentrated sulphuric acid. The product of this evaporation was an inodorous colourless mass, crystallized in small laminæ, possessing first a bitter and afterwards an acrid taste.

This crystallized residue reddened litmus paper strongly, its aqueous solution yielded no precipitate with barytes water, but gave a white flocculent precipitate with acetate of lead, readily soluble in excess of the acetate. A small quantity of this acid, saturated with ammonia, yielded a crystallizable salt, the solution of which precipitated persulphate of iron in yellowish-coloured flocculi. Lastly, a portion of the same acid, heated in a small tube, first fused, afterwards partially decomposed and sublimed in needles at a little distance from the heated portion of the tube.

The portion of the resin dissolved by the æther, again submitted to distillation with a little water, yielded traces of volatile oil, which were deposited on the surface of the water contained in the receiver in the state of a light pellicle. This resin, after cooling, had the form of a yellow transparent mass, which was so friable that it was pulverizable by slight pressure between the fingers. Digested in cold alcohol a part only of it was dissolved, and there remained a whitish insoluble resinous matter.

The residue upon which the æther had first acted, was treated first with cold and afterwards with hot alcohol, and it remained insoluble. This property resembles that of some fossil resins, and among others the insoluble resin stated by M. Berzelius to exist in amber, and which he has described as the bitumen of amber.

On recapitulating the facts arising during this examination, it will be observed that this fossil resin found in the vicinity of Eu, resembles amber in physical characters, in density, and in the different organic principles which have been separated from it.

MM. Lassaigne and Chevallier obtained the following substances, and in the annexed proportions, from 100 parts of the fossil resin:—

Resin insoluble in æther and alcohol	83·3
Resin soluble in æther and alcohol	14·2
Resin soluble in æther and insoluble in alcohol	2·0
Succinic acid	0·5
Traces of volatile oil.	
	100·0

Journ. de Chim. Méd., Juillet 1846.

ON THE OCCURRENCE OF FLUOR SPAR, APATITE AND CHONDRODITE IN LIMESTONE. BY JAMES D. DANA.

The analyses of corals by Mr. B. Silliman, jun.*, have shown that although ordinary corals consist mainly of carbonate of lime, there is present a small proportion of phosphates and fluorides, with some silica, alumina and oxide of iron. It is also probable, from some trials by Mr. Silliman, that these constituents exist also in many shells.

From the results obtained in these analyses, it appears that the fluorides and phosphates amount, on an average, to about ¼ per cent., or 0·25 part in a hundred parts of coral; and the amount in the same manner of the phosphates, is 0·05 per cent. A cubic foot of coral, as deduced from the average specific gravity ascertained by Mr. Silliman, weighs 157 pounds. Consequently in each cubic foot there are 6¼ ounces of fluorides and 1¼ ounce of phosphates; in each cubic rod, 1700 pounds of fluorides and 340 pounds of phosphates. These fluorides are fluorides of calcium and magnesium, and the phosphates are phosphates of lime and magnesia. In the same manner we ascertain that the amount of fluorides in a reef of coral, a mile long, half a mile wide and a hundred feet deep, amounts to more than 500,000,000 pounds. The proportion of silica is a little less than that of the fluorides.

Late geological researches have placed it beyond doubt, that the various limestones consist mainly, like coral limestone, of animal remains, among which corals in many instances hold a conspicuous place. These limestones often contain crystallizations of fluoride of calcium (fluor spar); and in other beds which have evidently been acted upon and crystallized by heat, there are also apatite (phosphate of lime) and chondrodite (composed of fluoric acid, magnesia and silica). Moreover these are the most common minerals of these limestones.

The above deductions supply us with a full explanation of the origin of these minerals. The fluorine, phosphoric acid, lime, magnesia and silica present, are adequate for all the results, without looking to any other sources. Instead therefore of being extraneous minerals introduced into the limestone rock, their elements at least are an essential part of its constitution; and they have been separated from the general mass by a segregation of like atoms under well-known principles, and it may be arranged anew, in some cases, according to their affinities. Fluoride of calcium may crystallize out when under water without much or any heat; and it is an interesting fact, that this fluoride has been lately proved by Mr. G. Wilson, to be soluble to some degree in pure cold water†. Mr. G. Wilson has also shown that fluorides actually exist in sea-water, as had been suggested by Mr. Silliman some months before the dis-

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* See the volume of the Exploring Expedition on Zoophytes, p. 712; and Silliman's American Journal, Second Series, vol. i. p. 189.

† See Chem. Gazette, No. 85, May 1846, p. 183.

covery, in his memoir on the composition of corals. Apatite and chondrodite require heat, as they are found only in *granular* limestones. The chondrodite is not supposed to exist as such in coral, but to form from the mutual action of its elements (which are present) during the slow action of the heat that gives the crystalline character to the limestone.

The magnesia of magnesian limestones is not attributable to the corals, as the proportion obtained by the analyses is less than one per cent*. It is derived probably from a foreign source; and this may be true, in part at least, for the magnesia of the chondrodite, although there is enough of this constituent present for a large amount of this mineral. The silica may also be in part foreign, or may proceed from the earthy impurities which were mixed with the limestone at its formation.—Silliman's *American Journal*, July 1846.

REMARKABLE DISCOVERIES IN ISOMORPHISM.

BY M. SCHEERER †.

M. Scheerer has just found that in compounds containing magnesia, protoxide of iron, oxide of nickel and other oxides isomorphous with magnesia, a part of the base may be wanting without a change of crystalline form, provided that this part be replaced by a quantity of water which contains three times as much oxygen as this part of the base. For example, the compounds $\dot{Mg}^3 \dddot{Si}$, $\dot{Mg}^2 \dddot{Si} + 3\dot{H}$, and $\dot{Mg} \dddot{Si} + 6\dot{H}$ in accordance with this principle, are isomorphous. Thus *chrysolite and serpentine may be isomorphous.* The composition of the first, $\dot{Mg}^3 \dddot{Si}$, is anhydrous and constant. Serpentine is hydrated and has a varying composition, wherever found, not affording a chemical formula. But examined with reference to M. Scheerer's views, we observe that in all the best analyses of serpentine, the oxygen of the magnesia and of the protoxide of iron, added to *one-third* the oxygen of the water, is equal to that of the silica; and consequently serpentine is a variable mixture of two isomorphous silicates, $\dot{Mg}^3 \dddot{Si}$ and $\dot{Mg}^2 \dddot{Si} + 3\dot{H}$. M. Scheerer has brought forward numerous other examples from among silicates, sulphates, &c.

M. Scheerer has also discovered that oxide of copper may be replaced in an isomorphous manner by *two* atoms of water.

We may now see clearly why so many hydrated minerals have never given uniform results, even with the most careful analyses.

The memoir of Scheerer will appear in two or three months, in Poggendorff's *Annalen*. The facts here briefly stated were communicated by him to the Academy of Sciences at Stockholm, at its last session.—*Ibid.*

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* Silliman's American Journal, New Series, i. 189, 198.

† In a letter to B. Silliman, jun., from Berzelius, dated March 10, 1846.

SCIENTIFIC MEMOIRS.

Part XVI. of this publication, completing vol. iv., will appear in the course of this month, and will contain the following articles:—Müller on the Structure and Characters of the Ganoids, and on the Natural Classification of Fish; Regnault on the Elastic Forces of Aqueous Vapour; Regnault's Hygrometrical Researches; Berzelius on the Composition of Organic Substances; Fresnel on the Colours produced in Homogeneous Fluids by Polarized Light, &c.

METEOROLOGICAL OBSERVATIONS FOR JULY 1846.

Chiswick.—July 1. Fine: cloudy. 2. Densely clouded. 3. Cloudy: clear and fine. 4. Sultry: hot and dry. 5. Excessively hot: showers: cloudy. 6. Fine: heavy showers. 7. Overcast and fine. 8. Overcast: rain. 9. Overcast: heavy rain. 10. Showery. 11. Overcast: light clouds and fine. 12. Clear and fine. 13. Cloudless and hot. 14. Hot breeze. 15. Overcast: fine. 16. Rain: densely overcast. 17. Cloudy and fine. 18. Constant rain: cloudy and fine. 19. Showers. 20. Very fine. 21. Fine: cloudy. 22. Hot and dry. 23. Cloudy: clear and fine. 24. Overcast: rain. 25, 26. Very fine. 27—29. Cloudy and fine. 30, 31. Very hot.

Mean temperature of the month	65°·46
Mean temperature of July 1845	61 ·43
Mean temperature of July for the last twenty years	62 ·96
Average amount of rain in July	2·38 inches.

Boston.—July 1. Fine: rain P.M. 2. Fine. 3. Cloudy: 3 o'clock P.M. thermometer 80°. 4. Fine: 4 o'clock P.M. thermometer 84°. 5. Fine: 10 o'clock A.M. thermometer 87°: 12 o'clock A.M. 90°: hail and rain, with thunder and lightning, accompanied with a tremendous wind P.M.* 6. Windy: rain P.M. 7. Windy. 8. Cloudy: rain P.M. 9. Rain: rain P.M. 10. Rain. 11. Cloudy. 12. Fine. 13. Cloudy. 14. Fine: 3 o'clock P.M. thermometer 81°. 15. Fine. 16. Fine: rain P.M. 17. Fine. 18. Cloudy: rain P.M. 19. Cloudy. 20. Fine. 21. Cloudy: rain P.M. 22, 23. Cloudy. 24. Cloudy: rain P.M. 25, 26. Fine. 27. Cloudy: rain early A.M. 28. Cloudy: 2 o'clock P.M. thermometer 81°. 29—31. Fine.—I cannot find so hot a month in all my journal except last month.

Sandwick Manse, Orkney.—July 1. Cloudy. 2. Cloudy: rain: clear. 3. Rain: cloudy. 4. Drizzle: clear. 5. Cloudy: rain. 6. Cloudy: drizzle: showers. 7. Drizzle: showers. 8. Bright: clear. 9. Cloudy: clear. 10. Bright: cloudy. 11. Cloudy: rain. 12. Fog. 13. Fog: rain. 14. Cloudy. 15. Showers: clear. 16. Bright: clear. 17. Cloudy: showers. 18. Bright: drizzle. 19. Showers: clear. 20, 21. Bright: showers. 22. Fog: showers: clear. 23. Cloudy: drops: clear. 24. Cloudy: clear. 25. Bright: drops. 26. Clear: cloudy. 27. Bright: cloudy. 28. Showers: clear. 29. Clear: fine. 30. Bright: fine. 31. Fog: fine.

Applegarth Manse, Dumfries-shire.—July 1. Showers all day. 2. Showers A.M.: fine P.M. 3. Wet morning: cleared. 4. Fair and fine. 5. Fine A.M.: thunder and rain P.M. 6. Tremendous rain. 7. Very fine. 8. Rain P.M. 9. Rain. 10. Fair and fine. 11. Fine: slight drizzle. 12. Wet morning: cleared. 13. Fair, but threatening. 14. Very fine. 15. Showers P.M. 16. Showery. 17. Fine: showers. 18. Slight showers. 19. One slight shower. 20. Fine: slight shower. 21, 22. Showery all day. 23. Wet all day. 24. Showers. 25. Fair and fine. 26. Rain P.M. 27. Drizzly. 28. Dropping day. 29—31. Fine: fair.

Mean temperature of the month	59°·2
Mean temperature of July 1845	56 ·2
Mean temperature of July for 23 years	58 ·1
Mean rain in July for 18 years	3·90 inches.
Mean rain in July	5·79 ,,

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* The hottest day since 31st July 1826.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. July.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8¼ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8¼ p.m.								
☽ 1.	30·014	29·973	29·43	29·70	29·60	29·60	29·66	70	58	65	60	51	56½	55	sw.	calm	sw.	......	·04	......	......	·14
2.	30·074	29·988	29·37	29·75	29·87	29·76	29·87	75	59	67	65	51	60	54½	w.	w.	sw.	......	·01	·09	......	......
3.	30·165	30·129	29·52	29·92	29·95	29·80	29·78	80	46	69	63	58	62	61	sw.	calm	sw.	ssw.	......	......	......	·10
4.	30·154	29·990	29·50	29·99	29·94	29·87	29·91	92	66	72·5	67½	57	63½	56	s.	calm	sw.	w.	......	......	......	·05
5.	29·768	29·608	29·14	29·70	29·40	29·88	29·65	95	50	80	81	50	60	52	s.	calm	sw.	e.	·24	......	......	·02
6.	29·630	29·448	28·97	29·24	29·36	29·45	29·50	71	55	65	59	53	54½	49	sw.	sw.	sw.	n.	·13	·37	......	·42
7.	29·876	29·744	29·16	29·56	29·68	29·60	29·73	72	49	62	63	50	50	50	w.	sw.	sw.	nw.	......	·22	......	·04
○ 8.	29·850	29·764	29·34	29·69	29·65	29·81	29·86	68	56	59	59	45	51	50	s.	calm	ne.	se.	·08	......	3·10	·10
9.	29·669	29·641	29·18	29·64	29·65	29·86	29·82	72	56	61	63	52½	56	53	sw.	calm	ne.	se.	·16	·10	......	......
10.	29·987	29·734	29·20	29·79	29·90	29·87	29·90	73	45	64	67	53	55½	52½	s.	calm	nw.	w.	·39	·45	......	......
11.	30·140	30·083	29·55	29·96	29·98	29·92	29·86	73	49	62	62	50	55	53	w.	calm	nw.	sw.	......	......	......	·07
12.	30·142	30·070	29·55	29·89	29·90	29·78	29·87	78	49	67	64½	53	57½	55	w.	w.	sw.	w.	......	......	......	·30
13.	30·063	29·717	29·47	29·90	29·81	29·87	29·75	87	58	69·5	65½	59½	59	55½	s.	calm	s.	sw.	......	......	......	·06
14.	29·771	29·664	29·12	29·68	29·66	29·67	29·65	84	54	73	71	55	60	54	s.	calm	w.	w.	......	......	......	·22
☾ 15.	29·907	29·812	29·25	29·73	29·70	29·68	29·70	76	55	69	68	55½	55	53½	sw.	nw.	sw.	s.	......	......	0·27	·05
16.	29·763	29·504	29·10	29·50	29·25	29·46	29·34	70	55	68	63	58	58½	55	sw.	sw.	sw.	......	·02	......	......	·04
17.	29·511	29·406	28·80	29·13	29·18	29·21	29·13	71	52	65	65½	56	56	53	sw.	w.	w-sw.	wsw.	·06	·07	......	·02
18.	29·450	29·294	28·82	29·10	29·02	29·18	29·12	71	51	58	60	48	59	53	sw.	sw.	se-sw.	s.	·22	......	......	·01
19.	29·828	29·574	29·80	29·32	29·52	29·16	29·54	74	52	63	64	46	55	53½	sw.	w.	w.	sw.	·05	·14	......	·05
20.	29·995	29·828	29·30	29·68	29·72	29·64	29·72	78	51	65	64½	54	61	55½	sw.	calm	w.	......	......	......	......	......
21.	29·905	29·833	29·24	29·58	29·51	29·66	29·50	73	55	68	63½	54½	59	55½	sw.	w.	w.	se.	·04	......	......	·05
22.	29·923	29·873	29·25	29·61	29·68	29·55	29·69	76	54	63	65	53	57	54	sw.	w.	w.	......	......	·07	......	·06
○ 23.	29·888	29·825	29·26	29·55	29·43	29·56	29·33	80	58	67·5	61½	56	59	56	sw.	sw.	sw.	s.	......	......	0·90	·03
24.	29·793	29·425	29·20	29·58	29·61	29·46	29·56	71	47	66	55	55	61½	57	w.	calm	sw.	s.	·32	......	......	......
25.	30·067	29·957	29·35	29·70	29·80	29·60	29·77	76	45	65	65½	52½	64½	56	sw.	nw.	sw.	sw.	......	·40	......	......
26.	30·152	30·123	29·53	29·97	29·97	29·93	29·85	76	56	66·5	67	53	59½	56	w.	calm	sw.	sse.	·02	......	......	·04
27.	30·202	30·157	29·54	29·83	29·93	29·72	29·83	75	63	65	66	56½	59	58	sw.	w.	sw.	se.	......	·07	......	·19
28.	30·218	30·126	29·58	29·98	29·98	29·90	29·95	83	53	74	68	59	59½	56½	w.	calm	sw.	w.	......	......	......	·07
29.	30·092	29·981	29·40	30·01	30·01	30·04	30·16	88	60	75·5	62	58	60	57	se.	calm	sw.	sw.	......	......	......	......
30.	29·975	29·937	29·34	30·01	30·08	30·25	30·26	89	62	74	74½	66	61	54	se.	ne.	sw.	s.	......	......	1·52	......
☽ 31.	[illegible]	[illegible]	[illegible]	30·07	[illegible]	30·18	[illegible]	80	64	70	71	[illegible]	[illegible]	[illegible]	[illegible]	ne.	[illegible]	se.	......	......	......	......

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[THIRD SERIES.]

OCTOBER 1846.

XLI. *On the Magnetic Affection of Light, and on the Distinction between the Ferromagnetic and Diamagnetic Conditions of Matter. By* MICHAEL FARADAY, *F.R.S., Foreign Associate of the Academy of Sciences, &c.*

[Continued from p. 156.]

HAVING ascertained the great advantage which this form of apparatus possessed for the examination of many substances which would give no sensible results by the process I formerly described, I proceeded to apply it to the cases of air and some doubly refracting bodies (Experimental Researches, 2237). For this purpose I made the faces of the magnetic poles reflectors, by applying to each a polished plate of steel; and as the poles were moveable, their reflecting surfaces could be placed at any distance and in any position required, the substance experimented on being between them.

Air.—I could obtain no signs of action upon the ray when air was between the magnetic poles, even with the fourth, fifth and sixth images.

Rock-crystal.—The cubes of this substance formerly described (Experimental Researches, 2178) were submitted to examination; but I could detect no trace of action on the ray of light when passing through them, although they were 0·75 of an inch in the side, and the ray was observed after passing seven, and even nine times across them. The cubes were examined in all directions.

Iceland Spar.—A cube of this substance (Experimental Researches, 2179) was examined in the same manner, but I obtained no effect.

Heavy glass presented the expected phænomena easily and well.

Failing to procure any positive result in these trials, either with air or with doubly refracting crystals, I silvered the latter in the manner that had been employed for the heavy glass, that the magnetic poles might be brought as close as possible; still no evidence of any magnetic action on the ray could be observed.

A natural six-sided prism of rock-crystal, 2·3 inches in length, was polished and silvered at the ends: no magnetic effect upon the light could be observed with this crystal with either the first, second or third image.

M. E. Becquerel thinks that he has observed an effect produced in doubly refracting crystalline bodies; and it is probable that his apparatus is far more delicate for the observance of optical changes than mine. In that case, if combined with the procedure founded on repeated transits of the ray, it perhaps would produce very distinct results: but the latter process alone has not as yet given any evidence of the action sought after.

Certain indications led me to look with interest for any possible effect which the crossing of the reflected rays might produce in the arrangement of reflectors and glass represented in fig. 1; but I could find no difference of action between it and the other arrangement, fig. 2, in which no such crossing occurred.

Near the close of last year I sent to the Royal Society two papers On the Magnetic Condition of all Matter*, in which I believed that I had established the existence of a magnetic action new to our knowledge; antithetical in its nature to the magnetism manifested by iron in any of its forms or conditions, strong or weak, or to that magnetism which iron could, in any quantity or under any circumstances, produce. Further, that all bodies not magnetic as iron, were magnetic according to this new mode of action; and that as *attraction* by the magnet marked the magnetic condition of iron, however small its quantity, or whatever its state might be, so *repulsion* was the distinctive characteristic of all those bodies which were naturally fitted to acquire the new state, and develope this new form of power.

M. Becquerel has sent a note to the Academy of Sciences†, in which he states certain results of his own much anterior to mine, due to ordinary magnetic action, and in which the position of the substances was *across* the magnetic axis. I need not quote the whole, but will select the following words at the end:—" From these facts it results that the magnetic effects

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* Philosophical Transactions, 1846, p. 21, or Phil. Mag., vol. xxviii. 1846.
† *Comptes Rendus*, 1846, p. 147.

produced in steel or in soft iron by the influence of a magnet, differ from those which occur in all bodies, in this; that in the first the distribution of magnetism is always in the direction of the length, whilst in tritoxide of iron, wood, gumlac, &c., it occurs generally in a transverse direction, especially when they are formed into needles. This difference of effect is due to the circumstance, that the magnetism being very weak in these bodies, we may neglect the reaction of the particles of the bodies on themselves." These words, and the time of their publication (January 1846), sufficiently show that M. Becquerel does not admit the new form or condition of magnetic action, which I supposed I had previously demonstrated.

M. E. Becquerel, in a communication to the Academy of the date of June 1846*, after confirming the action on light which I had announced, touches the question of magnetic action on all bodies; and at this late period is still of the same opinion as his father. I need quote only a few brief lines here and there to show this state of his mind. After speaking of Coulomb's results, and of his father's in 1827, he says, "These experiments have been made nearly twenty years, and nevertheless M. Faraday has announced anew the phænomenon of transverse direction, has made of the bodies which place themselves thus a new class of substances, and has named them diamagnetic substances. . . . . I ask myself how, under these circumstances, one can push the spirit of classification thus far, when one may give to the same substance all the various positions of longitudinal, transverse or oblique. In fact, place the two extremities of two strong magnets opposite each other, at 0·08 or 0·12 of an inch apart, and at about 0·04 from their surface, suspend by means of cocoon silk, a small needle of wood or of copper, about 0·04 in diameter and 1·9 or 2·3 inches long, it will place itself transversely. If the needle be cut in two and again divided, &c., we shall end by having a fragment, which will place itself in the line of the poles. It is simply a phænomenon of the resultant forces; for we may give to one and the same substance these different positions, according to its form, by modifying the distance of the poles." Then, speaking of the small quantity of iron which he found in certain specimens, and of his former statement, that these substances behaved as a mixture of inert matter and magnetic particles, or as ferruginous mixtures, he says, "all these conclusions still subsist at this day in all their generality."

These conclusions from two profound philosophers, so well

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* *Comptes Rendus*, 1846, p. 595.

able in every respect to judge any question arising on such a subject as magnetism, made me anxious on two accounts; for first, I thought it possible I might really be in error respecting the broad and general principle of magnetic action, which I supposed I had discovered; and next, that if right on that point, I must have been sadly deficient in describing my results not to have conveyed a better impression to the minds of those so competent to receive and understand. I therefore, for my own sake, entered into an examination of this point; for though I am nearly indifferent to the fate of any speculative or hypothetical view I may venture to send forth, I am far from being so as regards the correctness of any announcement I may make of a law of action or a new fact; and having carefully experimented on one or two of the cases of transverse position assumed by certain bodies magnetic, as iron, I now give the general result.

Some good uniform peroxide of iron (being one of the substances which M. Becquerel experimented with) was prepared and introduced regularly into a thin glass tube, 0·25 of an inch in diameter and 1·4 inch long; it was then suspended by a long filament of cocoon silk, and could be brought into any position relative to one of the poles of a strong electro-magnet, which could be made to assume various forms by the use of terminal pieces of iron. As peroxide of iron can occasionally receive and retain a feeble magnetic state, it is necessary the experimenter should be aware of the possibility, and guard against its effect in producing irregular results.

The pole of the magnet was in the first place a cone, of which the base was 1·5 inch in diameter, its axis being in a horizontal line. The cylinder of peroxide of iron was advanced towards the cone with its centre of suspension in a line with the axis. When within the influence of the magnet, and transverse to the axial line, it retained that position: but this was a position of unstable equilibrium; for if the cylinder became oblique to the axial line on either side, then the end nearest to the cone approached towards it, being attracted, and at last went up to and remained against it. But whether directly across the axial line, and so in the position of unstable equilibrium, or in any other position, the centre of gravity of the whole was always *attracted*; an effect easily appreciated with an electro-magnet by interrupting and renewing the exciting current.

As a contrast with diamagnetic bodies, I will state, that if a similar cylinder of phosphorus, bismuth, or heavy glass be placed in precisely the same circumstances, then the transverse position is a position of *stable* equilibrium: if the cylinder be moved from it, it returns by vibrations into it; and during the whole time the centre of gravity of the mass is *repelled*.

A square end was now given to the pole of the magnet, the face opposed to the cylinder of peroxide being 1·75 inch wide and of an equal height. The axial line is that which passed horizontally from the middle of this face, and took its course through the centre of gravity of the cylinder, which was also its centre of rotation. When the cylinder was at any distance less than 0·3 of an inch from the face of the pole, it stood parallel to the face, and therefore transverse to the axial line: being moved out of this position, it resumed it, so that the position was one of stable equilibrium. At distances a little greater this became a position of unstable equilibrium, and two positions of stable equilibrium were found equally inclined on the opposite sides of the transverse position, becoming more and more oblique to it as the distance was increased. Both the transverse positions and the oblique positions were easily referable to the concentration of the lines of magnetic force at the edges of the square end of the magnet. Effects due to the same cause have already been pointed out in the Experimental Researches (2298, 2299, 2384)*.

In every position of the cylinder of peroxide before this magnetic pole, the peroxide, as a mass, was *attracted*.

By using another termination of iron, the end of the pole opposite the peroxide was enlarged in its horizontal dimensions to 2·5 inches. All the former phænomena recurred; but the distance between the face of the pole and the tube of peroxide could be increased to one inch or more, before the tube began to assume the oblique positions.

A third termination presented a face having 3·5 inches of horizontal extension: the phænomena were here precisely the same, but the distance could be increased to 1·75 inch before the cylinder ceased to be parallel to the face, and began to acquire an oblique position.

For the complete comprehension of this and other effects due to the form of the face of the pole, and the concentration of the lines of magnetic force passing through the air near the edges, I will describe the positions assumed by the cylinder of peroxide, when its centre of suspension was preserved at a constant distance from the face of the pole, but was carried into different positions on one side or the other of the axial line. These are represented in the figure; by which it will be seen, that as the peroxide was carried to one side or the other of the axial line it became inclined to that line, in

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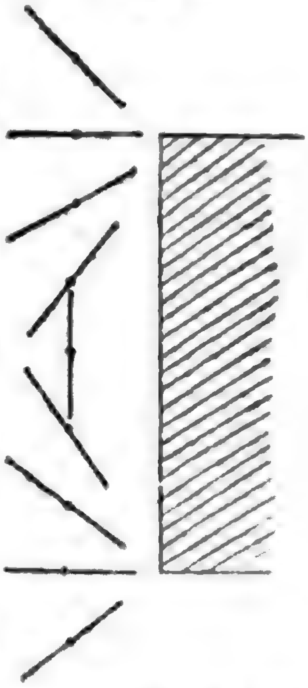
* Philosophical Transactions, 1846, pp. 32, 48.

a manner and to a degree easily comprehensible by those who consider the concentration of magnetic force at the edges of the face. The same was the case with the former face of 2·5 inches. Either end of the cylinder of peroxide might be the end nearest to the face of the pole; but the centre of gravity of the cylinder was in every case *attracted* by the magnet.

Fig. 3.



Other cylinders of peroxide of iron of different diameters and lengths were employed; and when they were smaller in length than the opposed faces of the magnetic poles, the results were precisely the same.

A cylinder of paper, magnetic through the presence of iron, was used and produced the same results.

Having been led to think that the disaggregation of the peroxide of iron had considerable influence over these phænomena, by obstructing and preventing the communication of magnetic induction from particle to particle, and was far more influential than the mere weakness of magnetic force, I took some substances, even more weak in magnetic power than the peroxide, and formed them into cylinders. The substances were solution of proto-sulphate of iron, muriate of cobalt and muriate of nickel, which were introduced into thin glass tubes 0·25 of an inch in diameter and 1·4 inch long. These, when suspended before the poles with their centres of suspension in the axial line, did not act as the peroxide of iron or the paper. They could indeed be kept in a position parallel to the face of the pole, but this was a position of unstable equilibrium; and when the least removed out of that position, the end nearest the magnet continued to approach until it came in contact, and then the whole remained unchanged. The action was precisely like that of a piece of iron, but very inferior in energy.

A saturated solution of proto-sulphate of iron was diluted with five times its volume of water, and still showed precisely the same phænomena as the stronger solution: yet its magnetic power was very far inferior to the magnetic force of the peroxide of iron, as was fully manifested by the extent of the attraction of the centre of gravity of the two portions of the substances. When the peroxide was under experiment, the suspension-thread was drawn twice or even three times further from the perpendicular than when the solution was used.

If a piece of iron wire be substituted for the cylinder of peroxide or the magnetic solutions, it will not remain parallel to the face of the pole, or oblique and *not* touching it; for one end will always go up to the face of the pole; or if it be very short, and then by loading, or otherwise be prevented from coming in contact with the pole, one end will point towards the part of the pole face nearest to it. In this respect it is as the magnetic solution, and not as the peroxide: however weak the magnetic pole may be, if it have power to affect the iron wire at all, it will produce the *same* effect. Further, if the iron wire be rendered perfectly free from magnetism, by making it red-hot, either end may be made that which is nearest to the face of the pole.

A piece of hæmatite, separated, not by an iron tool but by an agate, or otherwise in a careful manner, pointed in the manner of iron, though of course not with the same power, *i. e.* it did not take up a stable position either parallel to the face of the magnet or inclined, but not touching it; for one end or the other always went up to and remained in contact with the metal of the pole. The hæmatite, being powdered and put into a small tube, acted in the same way as when whole.

A piece of bottle-glass tube, which was magnetic from the iron it contained, acted as the hæmatite, either when whole or powdered up and put into a flint-glass tube: it therefore was unlike the peroxide of iron.

Pure peroxide of manganese appeared to take aplace between these bodies and the peroxide of iron. Generally speaking, the end nearest to the flat face of the pole went up to it and remained there; but when one end was opposite the edge of the face and the other end nearly opposite the middle, at the time the magnet was made active, the latter end, though nearer than the former, would recede, and the former end come up and remain in contact. If the latter end was still nearer, it would approach from the first; and, there was no place of stable equilibrium for the tube in which it remained parallel to the face, or nearly so, and neither end touching it.

A piece of thick platinum wire acted as the hæmatite or green glass, and not as the peroxide, in Becquerel's experiment. Spongy platina pressed into a tube acted as the peroxide of manganese, giving like it in certain situations, the beginnings of an action like that of the peroxide of iron.

The centre of gravity of all these bodies was attracted by the magnet, whatever part of the face they were placed in juxtaposition with. In no case was anything like a repulsion shown.

Now I do not see how any difficulty can arise in referring all these results of position to the ordinary action of magnets upon matter ordinarily magnetic, as iron is. All depends upon the shape and size of the poles, and upon the state of unity or disintegration, more or less complete, of the magnetic matter; for matter which is much weaker in magnetic power than peroxide of iron, as the solution of nickel, or dilute solution of iron or the metal platina, does not act as the peroxide, but as metallic iron. Still, in every case, the phænomena are phænomena of *attraction*; for not only is the centre of gravity of the whole mass attracted, but the parts of the cylinder of peroxide of iron, as well as of the other magnetic substances, are in those positions which the resultants of attractive force would necessarily give them. This is precisely the reverse of what takes place with diamagnetic bodies, for there the centre of gravity of the whole mass is *repelled*; and whatever form the mass may have, its parts take those positions which are most consistent with the direction and degree of the *repulsive* force.

All ambiguity and doubt may be removed in such cases as the above by the use of a single pole, either conical, wedge-shaped or round. It is true, that if a wire of iron, two or three inches long, be placed with its middle part near the apex of a conical or wedge-shaped pole, it will stand at right angles across the axis of the cone or wedge, vibrating like a balance-beam; and also that if a cylinder of bismuth, phosphorus or heavy glass be placed in the same position, it will take a similar position. But no magnetician who looks at the effect can for a moment confound the phænomena; for he will see at once that the iron as a mass is attracted, and the diamagnetic is, as a mass, repelled: and then, if for this observation of the latter phænomena he will take small spheres of iron, peroxide of iron, or solution of muriate of iron on the one hand, and of bismuth or phosphorus, copper or wood on the other, he will have the phænomena in the simplest state, and the fundamental fact will be before him; for the magnetics will be attracted, the diamagnetics will be repelled.

I cannot find any case of transverse position which does not enter into one or other of the two kinds referred to above; that is to say, which is not either a magnetic or a diamagnetic result. Even as regards the effect of ordinary magnetism in peroxide of iron and such like bodies, I see no reason to accept the statement of M. Becquerel, that the distribution of magnetism tends to take place in a transverse direction. It appears to me that the destruction of the continuity of the mass in these cases, combined with the degree of magnetic force in the substance, prevents the transmission of the force by induction or conduction from particle to particle, to the same extent as when the continuity exists, and so consecutive poles at short distances and in different directions are produced; and hence the reason why the solution of iron or nickel, or the platina, does not behave as the peroxide of iron, though weaker in magnetic force than it, but acts as metallic iron.

If it had not been for the remarkable relation of a vacuum, and with it of those attenuated forms of matter, air, gases and vapours, which I have for this very reason amongst others insisted on in the Experimental Researches (2432, &c.), it might have become questionable whether those bodies which I have called diamagnetics, were not acted upon strictly in the same manner as magnetic bodies; and the result, whether of attraction or repulsion, a consequence of a difference of degree only between the body observed and the medium surrounding it (2438, &c.). But I cannot help looking upon a vacuum as presenting a zero point in the phænomena of attraction and repulsion: and as magnetic bodies are attracted, and diamagnetic bodies repelled (2406, 2436) by a magnet, when surrounded by and in relation to it, so I believe that these two conditions represent two antithetical forms of magnetic force. This is the conclusion I have set forth in my original papers, and notwithstanding my very great respect for the judgement of MM. Becquerel, it is that which by the facts I am encouraged still to maintain*.

When heavy glass is submitted to the action of a powerful electro-magnet, the maximum degree of rotation of the ray is not obtained at once, but requires a sensible interval of time (Experimental Researches, 2170); this I have attributed to the gradual rising in intensity of the force of the magnet, and

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* I take the liberty in this note to refer to a similar point in the philosophy of static electricity. I have often been asked for the proof of an absolute natural zero between positive and negative electricity; and in reference to M. Peltier's views, that the earth is negative to the space around it, which in its turn is positive, have been told that if all parts of a portion of its plane surface were equally negative, we on that surface could not tell that it was not in a zero state. But such is not the case. A surface which is truly negative may appear, in comparison, to be positive to one still more highly negative; or a negative surface may seem to be in a zero state in relation to two other surfaces, one of which is more negative and the other less so, or even positive; but if referred to a true standard its real state is shown at once, and this standard is given by the inside of any metallic vessel, from which, by its shape or depth, external influence is excluded. Such a vessel always presents the same normal condition within, whatever charge its external surface may have; and by comparing the surface of the earth with the inside of such a vessel, which is easily done by the use of carriers, such as Coulomb employed, any one may tell for himself whether that surface is in a negative or a zero state.

the corresponding rising of the effect of that force in the glass. M. E. Becquerel does not agree with me in the above explanation, but thinks it is due to this; that the particles of the diamagnetic itself require time to assume their new state. That they may require time is, I think, very probable. I do not know any state the acquiring of which does not need time. This time is however most probably exceedingly small in the instance of diamagnetic bodies; and that the phænomena referred to by me are not due to such a cause, is, I think, shown by two considerations. The first is, that the electro-magnet is well known by other kinds of proof, as the induction of currents, &c., to require time to develope its maximum force due to a given electric current. I have shown that the rotation of the ray must increase during the time the magnet is thus rising; and I found that the power to induce currents exists simultaneously with the increasing rotation. The next consideration is, that when the diamagnetic is submitted, not to the action of an electro-magnet, but of a helix, the rotation does not increase in the same gradual manner as before, but is instantly at a maximum (Experimental Researches, 2195): hence my reasons for adhering to the explanation I gave in the papers themselves last year.

But on subjects so new as these, differences of opinion must inevitably arise upon many points; and it is better for the inquiries themselves that it should be so, for the facts in consequence receive a more close investigation. I therefore leave many points of difference between myself and others unnoticed for the present; believing that new and connected facts will rapidly accumulate, and that time, with his powerful aid, will in a very few years give both facts and opinions their right places.

Royal Institution, August 11, 1846.

XLII. *On the Composition and Resolution of Forces.* By G. W. Hearn, *Esq., B.A. of Cambridge, and Professor of Mathematics in the Royal Military College, Sandhurst* *.

1. EQUAL forces are such as being applied to the same point in contrary directions make equilibrium.

2. The resultant of any number of forces acting on a point is a single force, statically equivalent to the whole of them, that is a force such as would produce the same pressure in the same direction.

3. If two equal forces inclined to each other act on a point, their resultant is in the plane of the two forces, and bisects

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* Communicated by the Author.

the angle formed by their directions; and half that resultant is defined to be the 'resolvent' of either of the equal forces.

4. When any number of forces act on a point, if any straight line be drawn through that point, and planes be drawn through this line and the respective directions of the forces; and if in each plane a force equal to the force in that plane be applied to the point, making an angle with the straight line equal to that which the other force makes, but on the contrary side of the line, such a system of forces is called 'supplementary' to the former.

5. A system of forces acting on a point is in equilibrium when their resultant is zero.

Prop. I. When a system of forces is in equilibrium, the sum of the resolvents in the direction of any straight line through their point of application is zero.

The system being in equilibrium, the supplementary system will also be in equilibrium; for this latter system is nothing more than the former turned through 180° round the assumed line.

The two systems are therefore also in equilibrium, and consequently the resultant zero. But this resultant consists of the (algebraic) sum of the several resultants of the pairs of equal forces in the several planes through the assumed line, and half the resultant is therefore the sum of the resolvents of the given system. The resultant being zero, the sum of the resolvents is also zero.

Prop. II. Denoting the resolvent of a force P in the direction of a line making an angle θ with the direction of the force by $\mathrm{P}f\theta$, it is required to assign the form of $f\theta$.

It is an immediate consequence of our definition of resolvent that $f\theta = 1$ when $\theta = 0$, and that $f\theta = 0$ when $\theta = \frac{\pi}{2}$.

Moreover, that $f\theta$ cannot $= 0$ for any value of $\theta < \frac{\pi}{2}$.

Also that $f\theta$ is a periodical function going through all its values, as θ increases from 0 to 2π; and that $f\theta$ can never be infinite, and must have some determinate *single* value for every value of θ, and is therefore a function which can be expanded in integral positive powers of θ.

Let two equal forces keep a point in equilibrium. Sum of resolvents $=0$ gives

$$\mathrm{P}f\theta + \mathrm{P}f(\pi+\theta) = 0,$$

or

$$f\theta + f(\pi+\theta) = 0.$$

This equation is easily resolved, but is too general for our present purpose.

To obtain further information as to the form of $f\theta$, let P, Q, R be three forces in equilibrium, and therefore necessarily in the same plane, and let a line be drawn through their point of application also in the same plane, inclined at angles θ, $\alpha+\theta$, $\beta+\theta$ to P, Q, R respectively, then sum of resolvents $=0$ gives

$$\mathrm{P}f\theta+\mathrm{Q}f(\alpha+\theta)+\mathrm{R}f(\beta+\theta)=0$$

for all values of θ.

On equating to zero the coefficients of the several powers of θ, we have the conditions

$$\mathrm{P}f0+\mathrm{Q}f\alpha+\mathrm{R}f\beta=0. \quad . \quad . \quad . \quad . \quad (1.)$$
$$\mathrm{P}f'0+\mathrm{Q}f'\alpha+\mathrm{R}f'\beta=0. \quad . \quad . \quad . \quad . \quad (2.)$$
$$\mathrm{P}f''0+\mathrm{Q}f''\alpha+\mathrm{R}f''\beta=0. \quad . \quad . \quad . \quad . \quad (3.)$$
$$\mathrm{P}f'''0+\mathrm{Q}f'''\alpha+\mathrm{R}f'''\beta=0. \quad . \quad . \quad . \quad . \quad (4.)$$
$$\&c. \qquad \&c.$$

But the ratios $\frac{\mathrm{Q}}{\mathrm{P}}, \frac{\mathrm{R}}{\mathrm{P}}$ must be determinate functions of α and β, since the forces proportionally altered will still be in equilibrium. Hence the preceding conditions must be equivalent to two only.

Now if (1.) and (2.) were identical, or equivalent equations, all the rest would be equivalent to them; since 3 is derived from 2 in the same manner as 2 from 1, &c. The whole set would then reduce to only one, which is insufficient for the determination of $\frac{\mathrm{Q}}{\mathrm{P}}, \frac{\mathrm{R}}{\mathrm{P}}$.

Hence (3.) must be a consequence of (1.) and (2.), so that if λ and μ be certain determinate constants, we have

$$f''\alpha+\lambda f'\alpha+\mu f\alpha=0,$$
$$f''\beta+\lambda f'\beta+\mu f\beta=0.$$

And since these conclusions are independent of any specific values of α and β, we have generally

$$f''\theta+\lambda f'\theta+\mu f\theta=0. \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{A}.)$$

And it is easy to see that from this (4.) will be a consequence of (2.) and (3.), &c., and the whole system of conditions will be equivalent to two only, *videlicet* the two first.

Now if a and b be the roots of

$$x^2+\lambda x+\mu=0,$$

the solution of (A.) is

$$f\theta=\mathrm{A}\,e^{a\theta}+\mathrm{B}\,e^{b\theta};$$

but since $f\theta$ must be periodical, a and b will be imaginary; let therefore $a=m+n\sqrt{-1}$ and $b=m-n\sqrt{-1}$. Then

$$f\theta=e^{m\theta}\{\mathrm{A}\cos n\theta+\mathrm{B}\sin n\theta\};$$

and that this may be periodical m must $=0$. Also, since

$$f\theta+f(\pi+\theta)=0,$$

n is restricted to be an odd integer.

Also when $\theta=0\ f\theta=1$, $\therefore\ \mathrm{A}=1$; and when $\theta=\frac{\pi}{2}\ f\theta=0$.

But n being odd $\cos n\frac{\pi}{2}=0$ $\sin n\frac{\pi}{2}=\pm 1$. Hence $\mathrm{B}=0$, and therefore

$$f\theta=\cos n\theta;$$

but $\cos n\theta=0$ when $n\theta=\frac{\pi}{2}$, or $\theta=\frac{\pi}{2n}$; but $f\theta$ cannot $=0$, for $\theta<\frac{\pi}{2}$. Hence n is restricted to be $=1$, and therefore, finally,

$$f\theta=\cos\theta.$$

XLIII. *On the Substances contained in the* Roccella tinctoria. *By* Edward Schunck, *Esq.*[*]

THE *Roccella tinctoria* derives its interest from the fact of its being that species of lichen from which the finest kind of archil is prepared. It has been examined by Heeren and Kane. The former discovered in it a peculiar substance, which he called *Erythrine*, and a fat acid named by him *Roccellic acid*. The latter extracted from the plant a substance which he called *Erythriline*, similar in properties to Heeren's erythrine, and a body to which he applied the name of *Erythrine*, but which possesses all the properties of the substance called by Heeren *Pseuderythrine*, and supposed by him to be a product of the action of boiling alcohol on his erythrine My results do not agree entirely with those of either of these chemists.

The lichen which I employed for my investigation was obtained from Angola and Madagascar, and was pronounced by a distinguished botanist, to whom I submitted it, to be the *Roccella tinctoria* var. *fuciformis*. In order to extract the various substances contained in it, I submitted it to the following operations:—

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[*] Communicated by the Chemical Society; having been read April 6, 1846.

The plant was cut into pieces and treated in a capacious vessel with boiling water. After the boiling had continued for some time, the fluid, which had acquired a yellowish-brown colour, was strained through cloth. On cooling there was deposited from it a white substance in flocks and minute crystals. It was separated by filtration and washed with cold water. After drying it had a grayish appearance. In order to purify it, it was only necessary to dissolve it in boiling alcohol, which left behind an inconsiderable black or brown residue. On cooling it separated as a white crystalline mass. This substance resembles Heeren's erythrine and Kane's erythriline. I shall call it *Erythric acid.*

The fluid from which this substance had separated was of a light brown colour. On evaporation it became dark brown and muddy, and left at length a dark brown viscid mass, having a sweetish bitter taste. This mass, after standing for some time, became solid and crystalline. It was treated with cold water, which left behind a crystalline substance of a brownish-white colour and a bitter taste. This substance is a product of the action of boiling water on erythric acid. I shall call it *Picro-erythrine.* In order to purify it, it must be, after washing it with a little cold water, dissolved in boiling water, to which is added some animal charcoal. After filtration and evaporation there remains a yellowish mass, which soon becomes crystalline and white. On treating this mass with cold water, the picro-erythrine remains behind perfectly white and in a state of purity.

To the brown fluid obtained by treating the dark brown mass with cold water, a solution of sugar of lead was added, which produced a copious grayish-brown precipitate. This precipitate, after being separated by filtration and washed with water, was decomposed by a stream of sulphuretted hydrogen gas. A light yellow fluid was obtained, which, on evaporation, became gradually brown and deposited a dark brown substance, insoluble in water but soluble in alcohol and alkalies. A brown viscid mass was left at last, having an acid reaction and a strongly acid and at the same time bitter taste. It showed no trace of anything crystalline even after long standing. It was soluble in alcohol, but insoluble in æther. Its aqueous solution was precipitated by a solution of glue, by lime and baryta water, and acetate of copper, and was rendered muddy and gradually precipitated by sulphuric acid and common salt. It therefore consisted probably of some kind of tannic acid. Through the fluid separated from the precipitate produced by sugar of lead sulphuretted hydrogen gas was passed until all the lead was precipitated.

The filtered fluid gave a slight flocculent precipitate on the addition of alcohol, but none with lime and baryta water or sulphuric acid. On evaporation it left a clear brown syrup, which soon changed into a crystalline mass, having a sweetish bitter taste. It contained besides picro-erythrine, to which it owed its bitter taste, a quantity of orcine, which was detected in the following manner:—A part of it was dissolved in boiling water, and during the boiling a quantity of magnesia was introduced into it. After filtration the fluid was evaporated to dryness. It left a brown mass, which no longer became crystalline on standing. The picro-erythrine had entered into combination with the magnesia, and on treating the mass with æther in the cold, a yellowish-brown fluid was obtained, which on evaporation gave crystals of orcine, recognisable by its well-known properties. The dark brown viscid mass obtained from the mother-liquor of the erythric acid left on being burnt a considerable quantity of ashes, consisting of sulphate of soda, chloride of sodium, oxide of iron and carbonate of magnesia, with a trace of carbonate of lime.

The lichen, after being extracted with boiling water, had lost its grayish-white colour and become green. It was dried and then treated with boiling alcohol. The alcohol acquired during the process a dark emerald-green colour. It was strained through cloth while still hot. On becoming cold it deposited a green flocculent substance, which was separated by filtration. The green colour of this substance could not be removed by washing with cold alcohol. On drying it became dark green and coherent, but when powdered it was light green again. This substance is a kind of fat; it cannot be fused without being decomposed. When heated on platinum foil it melts to a brown fluid, giving off at the same time a strong smell of burning fat, and burns without leaving any ashes. Heated in a tube closed at one end it melts and gives a brown, oily sublimate, which soon solidifies, but without becoming crystalline. It is left behind on evaporating its alcoholic solution as an amorphous green mass. It is precipitated from its solution in alcohol by water, and also by an alcoholic solution of acetate of lead. It is insoluble in boiling muriatic and dilute sulphuric acids. Concentrated sulphuric acid dissolves it. Boiling nitric acid, even if dilute, destroys its green colour and makes it yellow. If treated with concentrated nitric acid, it is dissolved and decomposed with an evolution of nitrous acid; by degrees there collect on the surface of the fluid yellow oily drops, which solidify on cooling. It is very little soluble in boiling caustic ley, and insoluble in ammonia. The dark green fluid from which this substance had separated was evaporated to dryness, when it left a dark green viscid residue, interspersed with yellowish crystalline grains. This residue was treated with boiling water, which removed some picro-erythrine contained in it, and then with cold alcohol, which left behind a quantity of the greenish-white fat just described, and acquired a dark green colour. The alcoholic fluid had an acid reaction. It contained roccellic acid and a dark green, easily fusible fat. In order to separate the roccellic acid, an alcoholic solution of sugar of lead was added. This produced a greenish-white flocculent precipitate, which was separated by filtration and washed with alcohol. This precipitate, consisting of roccellate of lead, was decomposed by dilute nitric acid, which left the roccellic acid behind of a green colour. The latter, after being washed with water to remove the nitrate of lead, was dissolved in boiling alcohol, to which some animal charcoal was added. After filtering the solution, the roccellic acid separated on cooling in white crystalline needles. By adding water to the green alcoholic fluid from which the roccellate of lead had been precipitated, it became milky, and on boiling, dark green drops of a fatty substance collected at the bottom. This substance is a kind of fat, which is easily fusible at the temperature of boiling water. Its green colour is no doubt owing to the presence of chlorophylle, which cannot however be separated from it. It imparts no colour to boiling muriatic or dilute sulphuric acid. Boiling nitric acid destroys its green colour and changes it into yellow, after which it dissolves in alcohol with a yellow colour. It is soluble in alkalies, and is precipitated again by acids.

The lichen, after extraction with water and alcohol, was treated with dilute caustic ley at a boiling heat. A dark brown fluid was obtained, to which, after filtration, muriatic acid was added. This produced a dark brown flocculent precipitate, which, after filtering, washing with water and drying, appeared as a light brown powder. When heated it burns without leaving any ash. On being treated with strong caustic potash at a boiling heat, it gives off a slight smell of ammonia. It is soluble in alkalies, but insoluble in alcohol. The acid fluid from which it was precipitated deposited on evaporation a dark brown substance, in the same way as a solution of tannin or extractive matter. It is doubtful indeed whether the brown substance precipitated by the acid from the alkaline fluid is contained in the plant as such, or whether it is formed by the conjoint action of the air and alkali on some other substance in the lichen.

The *Roccella tinctoria* is easily reduced to ashes. These ashes are grayish-white. They consist of sulphate of soda, chloride of sodium, oxide of iron, alumina, carbonate of lime and carbonate of magnesia.

I shall now proceed to describe more fully several of the substances just mentioned.

Erythric Acid.

This body is the most important of those existing in the plant, as it is that one which gives rise to the colouring matters for the production of which the lichen is employed. It is not possible however to obtain much from the plant, since by the action both of boiling water and alcohol it undergoes a rapid change. By the method described above I obtained in one case from 1 lb. of the lichen 60 grains of it. If prepared without the intervention of alkalies, erythric acid is perfectly white and tasteless. It is soluble in water, alcohol and æther. 1 part dissolves in 240 parts of boiling water, from which a great part separates on cooling in flocks, or as a crystalline powder. Its solubility in æther distinguishes it from Heeren's erythrine, and its solubility in water from Kane's erythriline. Its solutions redden litmus paper. From a concentrated solution in boiling alcohol it is deposited on cooling in needles and star-shaped masses, which consist of minute crystals. It is precipitated from its alcoholic solution by water as a jelly. If the alcoholic solution however be boiled for a length of time, it is converted into erythric æther, in the same way as lecanoric acid is converted by boiling alcohol into lecanoric æther; and if water be now added to the solution no precipitate is formed, but the erythric æther gradually crystallizes in needles from the solution. By the continued action of boiling water erythric acid is converted into picroerythrine. Heated on platinum foil, it melts and burns away without leaving any residue: heated in a tube closed at one end, it gives an oily sublimate, which after some time crystallizes; this sublimate consists of orcine. Erythric acid is easily soluble in caustic and carbonated alkalies, and in lime and baryta water, and it is reprecipitated from these solutions by acids in form of a jelly, unless they have previously been boiled or left to stand for a considerable time. If a solution of it in baryta water be boiled, carbonate of baryta is deposited, and acids now produce no precipitate of erythric acid. If the excess of baryta be removed by a stream of carbonic acid gas, and the filtered solution be evaporated, there are obtained prismatic crystals, which are easily recognised as consisting of orcine by their intensely sweet taste, by their being volatilizable without any residue, by their solution precipitating basic acetate of lead, reducing chloride of gold, and giving a red colour with ammonia and red flocks on boiling with nitric acid. Erythric acid then, like lecanoric, is converted by alkalies into orcine and carbonic acid. A solution of erythric acid in ammonia exposed to the air soon becomes of a dark-red or purple colour. It is the basis, and I believe the only one, of the colouring matters derived from the plant. An alcoholic solution of erythric acid is not precipitated by nitrate of silver, but the addition of nitrate of silver to an ammoniacal solution produces a white precipitate, which on boiling becomes black, a mirror of silver being formed at the same time on the sides of the glass. Chloride of gold added to an alcoholic solution is not changed even on boiling. With perchloride of iron an alcoholic solution strikes a deep purple colour; on the addition of ammonia the colour is changed into yellow, but the oxide of iron is not precipitated, unless the fluid be boiled. It is not precipitated by an alcoholic solution of acetate of lead, but basic acetate of lead produces immediately a copious precipitate.

On combustion with oxide of copper the following results were obtained:—

I. 0·5400 grm. dried at 212° gave 1·1640 carbonic acid and 0·2530 water.

II. 0·3640 grm. gave 0·7835 carbonic acid and 0·1820 water.

These numbers lead to the following composition:—

			Calculated.	I.	II.
34 equivs.	Carbon .	2550·0	59·47	58·78	58·70
19 ...	Hydrogen	237·5	5·53	5·20	5·55
15 ...	Oxygen .	1500·0	35·00	36·02	35·75
		4287·5	100·00	100·00	100·00

The lead compound was prepared by precipitating an alcoholic solution of erythric acid with basic acetate of lead, filtering, washing the precipitate with cold water and drying *in vacuo*.

I. 0·6340 grm. gave 0·6435 carbonic acid and 0·1215 water. 0·3465 gave 0·0075 lead and 0·1970 oxide of lead.

II. 0·3195 grm. gave 0·3160 carbonic acid and 0·0630 water. 0·4345 gave 0·0945 lead and 0·2520 oxide of lead.

This gives—

			Calculated.	I.	II.
34 eqs.	Carbon . .	2550·0	27·08	27·68	26·97
15 ...	Hydrogen .	187·5	1·99	2·12	2·19
11 ...	Oxygen . .	1100·0	11·69	11·04	11·72
4 ...	Oxide of lead	5578·0	59·24	59·16	59·12
		9415·5	100·00	100·00	100·00

The decomposition of erythric acid by means of alkalies is therefore as follows:—

1 equivalent of erythric acid . $=C_{34}\ H_{19}\ O_{15}$
-2 equivalents of carbonic acid . $=C_{2}\ O_{4}$
$+3$ equivalents of water . . . $=H_{3}\ O_{3}$, gives
2 equivalents of crystallized orcine $=C_{32}\ H_{22}\ O_{14}$.

Erythric Æther.

This body has all the properties ascribed to pseuderythrine by Heeren and Kane, and as I found its composition to differ very little from that given by Liebig for Heeren's pseuderythrine, and by Kane for his erythrine, it can hardly be supposed that it is a different body. It is easily prepared by the action of boiling alcohol on erythric acid, in the same way as lecanoric æther from lecanoric acid. Indeed its formation takes place so easily and rapidly in this way, that it is almost impossible to extract erythric acid from the plant by means of boiling alcohol, nothing being obtained by endeavouring to obtain it in this way but erythric æther. In its appearance and properties erythric æther can hardly be distinguished from lecanoric æther, and as its composition in 100 parts happens to be almost the same, the one may easily be mistaken for the other. Erythric æther is at first tasteless, but after being kept for some time in the mouth, it produces a burning sensation on the tongue. It is soluble in boiling water. If more of the substance be taken than the water can dissolve, the excess melts, forming drops like oil, which sink to the bottom. On cooling the solution becomes milky, and a great part of the æther crystallizes out in needles and plates. It is easily soluble in alcohol and æther. On allowing the alcoholic solution to evaporate spontaneously, it is obtained in prismatic crystals. When heated in a tube it melts, and is almost completely volatilized, the vapour condensing in the colder parts of the tube to a fluid which soon crystallizes. It is soluble in caustic and carbonated alkalies, and in lime and baryta water. From these solutions it is precipitated unchanged by acids, unless they have previously been boiled for a length of time. It reduces nitrate of silver on the addition of ammonia and boiling, and chloride of gold without the addition of any alkali. It gives a precipitate with basic acetate, but none with neutral acetate of lead. On dissolving a quantity of it in caustic potash and subjecting the fluid to distillation, I obtained in the receiver a fluid, from which, on the addition of dry carbonate of potash, a thin layer of alcohol separated, which was easily recognised by its peculiar spirituous smell and its burning with a blue flame. The fluid in the retort was neutralized with sulphuric acid and evaporated to dryness. The residue was treated with alcohol, which on evaporation gave crystals of orcine. Its products of decomposition with alkalies are therefore the same as those of lecanoric æther. Its analysis gave the following results:—

I. 0·5380 grm. gave 1·1965 carbonic acid and 0·2970 water.

II. 0·4085 grm. gave 0·9095 carbonic acid and 0·2260 water.

These numbers correspond to the following composition:—

			Calculated.	I.	II.
38 equivs.	Carbon .	2850·0	61·45	60·65	60·72
23 ...	Hydrogen	287·5	6·19	6·13	6·14
15 ...	Oxygen .	1500·0	32·36	33·22	33·14
		4637·5	100·00	100·00	100·00

Its rational formula is $C_{14} H_{18} O_{14} + C_4 H_5 O$. It is therefore formed from erythric acid by the substitution of 1 equivalent of oxide of ethyle for 1 equivalent of water.

Picro-erythrine.

This substance is a product derived from erythric acid. To it must be attributed the highly bitter taste of all the extracts made of the lichen, whether aqueous or alcoholic. In its properties, however, it agrees strictly neither with the erythrine-bitter of Heeren nor the amarythrine of Kane. It approaches nearest to the telerythrine of the latter, according to the description given by him of that substance. It is a product of the action of water on erythric acid. If pure erythric acid, as prepared by the process described above, be treated with boiling water for a short time, it dissolves, the fluid acquires a bitter taste, and on cooling deposits no erythric acid. On evaporation it yields a brownish, viscid mass, leaving a taste between bitter and sweet. This mass, after some time, becomes crystalline. It may then be treated with cold water, which leaves the picro-erythrine behind white and pure. I have described above how it may be obtained as a secondary product in the preparation of erythric acid. In operating on the plant with boiling water much more is obtained of it than of erythric acid. On extracting the lichen also with boiling alcohol, a considerable quantity is found in the extract. Picro-erythrine has the following properties. It has a strong but not disagreeably bitter taste. It is soluble in water, alcohol and æther. Its solubility in æther distinguishes it from Kane's amarythrine and telerythrine, which are insoluble in that fluid. It does not dissolve readily in cold water, but easily in boiling water. Its solution in the latter, however, does not re-deposit it on cooling. On evaporating a solution of it in water or alcohol, it is left behind as a white crystalline mass, but never in well-defined crystals or needles. Its solutions redden litmus paper slightly. It does not undergo any further change on being treated with boiling water, nor does it form an æther on being treated with boiling alcohol, as erythric acid does; it has therefore little or no claim to be considered as an acid. Heated on platinum foil it melts to a yellow fluid, is decomposed, and burns with a bright flame, leaving no ash. Heated in a tube closed at one end it gives a sublimate of orcine. It is decomposed by boiling nitric acid with an evolution of nitrous acid. Concentrated sulphuric acid dissolves it, and on boiling decomposes it with a disengagement of sulphurous acid. It dissolves in caustic alkalies, and in lime and baryta water in the cold. Its solution in baryta water deposits carbonate of baryta on boiling, and the solution then contains nothing but orcine. Its solutions in alkalies speedily become red on exposure to the air. Its aqueous solution is not precipitated by neutral acetate, but copiously by basic acetate of lead. With perchloride of iron it gives a deep purple colour, which is destroyed by ammonia, the oxide of iron being immediately precipitated. Nitrate of silver is not changed by it on boiling, but on the addition of ammonia a precipitate is produced, which on boiling the fluid is reduced, the silver being deposited as a metallic mirror on the sides of the glass. Chloride of gold is slowly reduced by it on boiling, the gold being deposited in the shape of metallic scales; if caustic potash be added the reduction is effected instantaneously, the gold being deposited as a black powder. It gives no precipitate with a solution of glue. Its analysis gave the following:—

I. 0·5090 grm. gave 0·9830 carbonic acid and 0·2850 water.
II. 0·4690 grm. gave 0·8970 carbonic acid and 0·2510 water.

These numbers give the following composition:—

			Calculated.	I.	II.
34 equivs.	Carbon . .	2550	52·57	52·86	52·16
24 ...	Hydrogen .	300	6·18	6·22	5·94
20 ...	Oxygen . .	2000	41·25	40·92	41·90
		4850	100·00	100·00	100·00

The compound with oxide of lead, prepared by precipitation with basic acetate of lead, gave the following results:—

0·2575 grm. gave 0·1465 carbonic acid and 0·0430 water.
0·3185 grm. gave 0·0220 lead and 0·1960 oxide of lead.

This corresponds to—

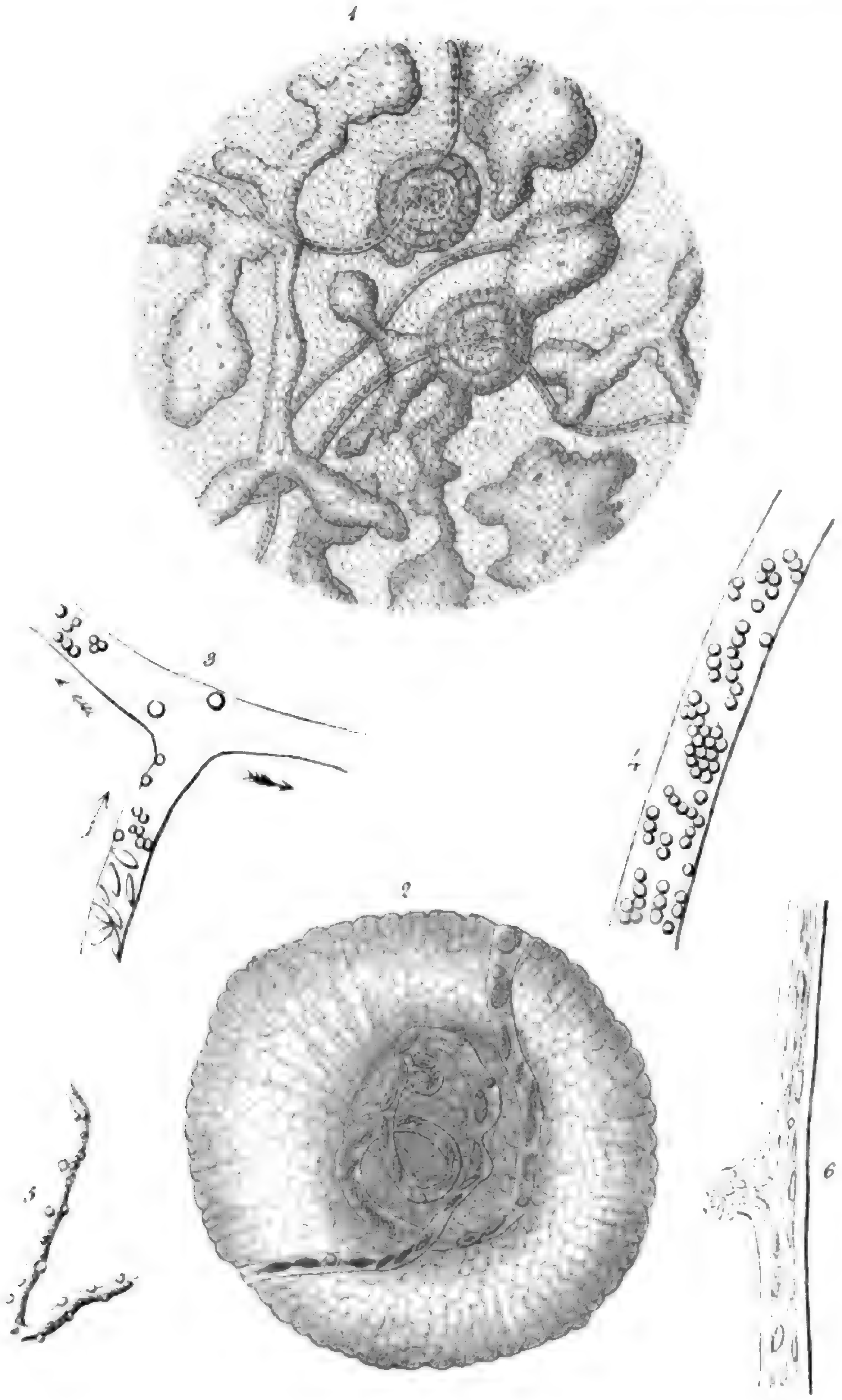
			Calculated.	Found.
34 equivs.	Carbon . .	2550	15·93	15·51
24 ...	Hydrogen .	300	1·87	1·85
20 ...	Oxygen . .	2000	12·51	13·70
8 ...	Oxide of lead	11156	69·69	68·94
		16006	100·00	100·00

Erythric acid, therefore, in its conversion into picro-erythrine, takes up the elements of 5 equiv. of water.

Roccellic Acid.

This acid was discovered by Heeren. If the lichen be extracted with ammonia in the cold, a yellow fluid is obtained which contains erythric acid and roccellic acid dissolved in ammonia. The roccellic acid may be separated by adding chloride of calcium to the fluid, by which a precipitate of roccellate of lime is produced, or by precipitating the two acids with muriatic acid and treating the precipitate with boiling water, which dissolves the erythric acid and leaves the roccellic acid behind. But by this method the acid is not obtained so pure as by extraction with alcohol, as the ammonia takes up at the same time a brown substance from the plant, from which it is afterwards difficult to separate the acid. I therefore prefer the method which I have described above.

Roccellic acid is a species of fat acid. I have nothing to correct in, and little to add to, the description given of it by Heeren. It is insoluble in water, but easily soluble in alcohol and æther. From a hot concentrated solution in alcohol it crystallizes on cooling in small needles, forming when dry a white, shining crystalline mass. By the spontaneous evaporation of its alcoholic solution, it is obtained in larger and more defined crystals. Its solutions redden litmus paper strongly. Heated on platinum foil it melts to a transparent fluid, which, if allowed to cool, congeals again to a crystalline mass. If further heated it is decomposed, giving off a smell like burning fat, and burns with a bright flame, leaving no residue. Heated in a tube closed at one end, it melts and gives an oily sublimate, leaving little or no residue; the oily sublimate is soon changed into a crystalline mass, but on being again sublimed it remains fluid. Roccellic acid is insoluble in dilute mineral acids, but soluble in alkalies. When caustic potash is poured on it, it swells up to a gelatinous mass, which is insoluble in the caustic ley, but soluble in water. The solution on boiling foams like a solution of soap; strong acids re-precipitate the roccellic acid in flocks. The solution on evaporation leaves a crystalline, saponaceous mass. Ammonia behaves in a similar manner. It is also soluble in



Dr Waller del.

J. de C. Sowerby lith.

Hullmandel & Walton Lithographers

carbonated alkalies, carbonic acid being disengaged on boiling, but it is insoluble in lime and baryta water. The solution in ammonia gives with chloride of calcium and chloride of barium flocculent precipitates, consisting of roccellate of lime and baryta. A solution of roccellic acid in alcohol is precipitated by an alcoholic solution of acetate of lead, but not by a solution of nitrate of silver. A solution of it in ammonia gives with nitrate of silver a white gelatinous precipitate, which becomes brown when the fluid is boiled, but is not completely reduced. An alcoholic solution of roccellic acid does not reduce chloride of gold on boiling.

On being ignited with oxide of copper—

I. 0·42100 grm. gave 1·0200 carbonic acid and 0·4025 water.
II. 0·2815 grm. gave 0·6795 carbonic acid and 0·2720 water.

From these numbers the following composition may be deduced:—

		Calculated.	I.	II.
24 equivs. Carbon . .	1800·0	66·97	66·07	65·83
23 ... Hydrogen	287·5	10·69	10·62	10·73
6 ... Oxygen .	600·0	22·34	23·31	23·44
	2687·5	100·00	100·00	100·00

The lead salt was prepared by dissolving the acid in a small quantity of ammonia and precipitating with acetate of lead.

I. 0·4475 grm. gave 0·5530 carbonic acid and 0·2070 water.
0·3405 grm. gave 0·0705 lead and 0·0990 oxide of lead.
II. 0·4360 grm. gave 0·5490 carbonic acid and 0·2025 water.
0·2860 grm. gave 0·0730 lead and 0·0675 oxide of lead.

These numbers correspond to the following composition:—

		Calculated.	I.	II.
24 equivs. Carbon . .	1800	33·55	33·70	34·34
22 ... Hydrogen .	275	5·12	5·13	5·16
5 ... Oxygen . .	500	9·34	9·81	9·42
2 ... Oxide of lead	2789	51·99	51·36	51·08
	5364	100·00	100·00	100·00

This salt is therefore basic. I have been prevented by want of material from examining the other salts of the acid.

XLIV. *Microscopic Examination of some of the principal Tissues of the Animal Frame, as observed in the Tongue of the living Frog, Toad, &c. By* Augustus Waller, *M.D.*[*]

[With a Plate.]

THE peculiar structure and disposition of the tongue of the frog affords great facilities for its examination under the

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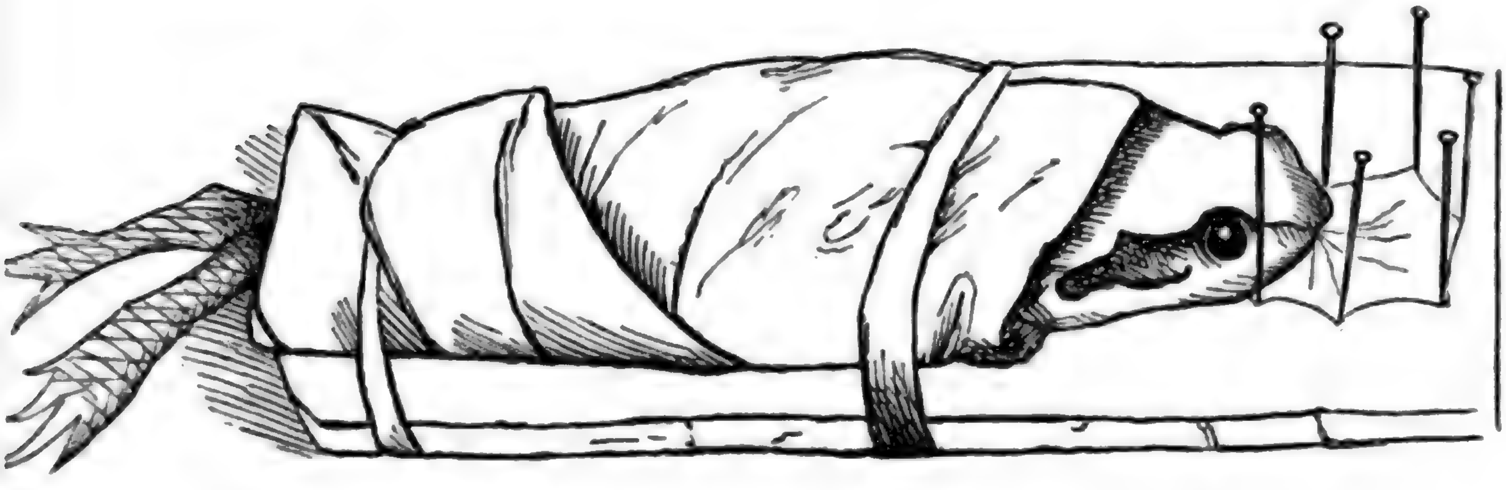
[*] Communicated by the Author.

strongest magnifying powers we possess. It is inserted by the base to the concavity of the lower jaw-bone, and the point hangs down the pharynx perfectly free from any adherence. It presents great resemblance to the uvula in the human subject in size, shape and colour, which also has its lower extremity free and hanging down the pharynx. The peculiar habits of the frog completely explain the apparently anomalous disposition of the tongue; as, independently of its being an organ of taste, it likewise serves the purpose of one of prehension for supplying the animal with food. As is well known to naturalists, it subsists on flies and small insects which it seizes in their flight. On these occasions it darts its head forward, at the same time projecting the tongue out of the mouth, the upper surface of which being covered with a slimy exudation, comes in contact with the insect, folds round it and draws it back into the mouth. In this little manœuvre the tongue may be considered as performing a movement of rotation of 180° round the lower jaw-bone, so that the surface which is uppermost in the mouth becomes lowermost when in contact with the object. This fact must be borne in mind when the tongue is examined under the microscope, for then likewise the inferior surface is uppermost, and the superior or papillary surface below.

I take this opportunity of stating my claim of priority with regard to this experiment, as a very erroneous impression might be created by M. Donné's statement in his *Cours de Microscopie*. It was first performed by me in Paris in 1839. The idea occurred to me while making some dissections of the frog for the purpose of comparative anatomy; and at first I merely applied it to test M. Dumas's well-known theory of muscular contraction. Subsequently I repeated it in the presence of several of my friends, among whom I may particularly mention Mr. Thomas Cooper. Success in this point encouraged me to attempt to observe the circulation in the human subject; and as the prepuce in some cases is nearly semi-transparent, I fixed upon that membrane as the subject of experiment. For an obvious reason I was desirous of not making these experiments alone, and obtained the assistance of M. Donné. After a few attempts we finally abandoned the subject; although I am still of opinion that they would be successful if tried under favourable circumstances. While engaged in this pursuit I stated to him that I had already discovered a living membrane, containing muscles and glands in its natural state, without the aid of the scalpel. From the interest with which I found this observation received by him, I was induced to repeat the experiment as described, and we determined to prosecute it in common. My surprise therefore was great, when a few days after I was informed (without any intimation from M. Donné) that he had communicated the experiment to the Société Philomatique. At the next meeting of the Society I attended, and found that in the *procès verbal* of the preceding meeting, in giving an account of the experiment, M. Donné had omitted to mention my name and co-operation. I seized the opportunity to rectify this mistake, and to state the arrangement that I had made with M. Donné, and to disclaim any precedence on his part. I have only to add, that these statements were made before a very full meeting of the members, that M. Donné was present, and that they were received by him without contradiction. I should not have deemed it necessary to enter into these details if I had not seen at page 108 of his *Cours de Microscopie* an account of these observations almost literally the same as inserted in the minutes of the Société Philomatique, with the following slight comment:—"Voici comment se prépare cette belle expérience dont l'idée première m'a été donné par un élève étranger, qui suivait mes cours, M. Waller."

To prepare the animal for the experiment, it must be rolled up in a strip of linen, leaving the head and the extremities of the legs uncovered. Arranged in this kind of swaddling bandage with a little care, it is incapacitated from making any violent movement, which is indispensable for the success of the experiment, while at the same time it is perfectly able to breathe. We must next provide a slip of sheet-cork rather longer than the frog: at one extremity a circular piece about



the size of a shilling is to be cut out, so as to form an opening to allow of the passage of light when the tongue has been stretched over the opening. The frog is now to be placed on the slip of cork, where it may be secured by a few turns of string, or by a couple of elastic bands, which I have found more convenient. The animal's tongue may now be easily discovered as described, and drawn out of the mouth with a pair of tweezers. It will now be perceived (as I first observed while dissecting one of these animals) that this organ possesses an extraordinary degree of elasticity, and that with care it may be extended in all directions until it is so attenuated as to become nearly transparent. A few pins will enable us to fix it over the opening made in the cork, where we can examine it at our leisure.

When the tongue is first extracted from the mouth it is observed to be extremely pale, and the only vessels visible are two veins with accompanying arteries, corresponding to the lingual arteries in man, which, after ascending the throat in a parallel direction, penetrate into the tongue, where they are chiefly distributed at its inferior surface. In the substance of the tongue they may be described in general terms as proceeding by their main trunks towards the two tubercles, which are found at its anterior extremity, and communicating in their course by frequent anastomoses. After several minutes' exposure they become more apparent from the general vascular distention that takes place, and we observe the same phænomena as when the conjunctiva is exposed to the contact of some foreign substance. Thus the vessels become more distended and apparently increased in number, at the same time they appear to terminate in the tissue of the organ, like the minute fibres of roots.

This vascular engorgement appears to arise in the first instance entirely from the vital irritability of the organ, as it is seen to commence as soon as the tongue is seized by the tweezers, even before it is extracted from the mouth. After its preparation this injection is further increased by the impediments which exist to free respiration and muscular action. The peculiar manner in which this animal, devoid of ribs, breathes, is well known. The number of respirations in a minute depends much upon the external temperature, and varies from four to thirty. The pulsations of the heart in the same period are about sixty, which we may ascertain by observing the skin over the heart as it rises at each contraction. Acquaintance with these facts will be found useful in the subsequent inquiry. Nervous trunks, in colour of a shining white, are found accompanying the vessels, and are best seen when the extended tongue is viewed between the light.

On the upper or papillary surface the eye detects minute elevations above the rest of the membranes, which very much resemble the granulations often existing on the inner surface of the eyelids: when this surface is covered with ink, these minute elevations become still more apparent.

Microscopic Examination.—It will be found advisable to commence this examinatiom by means of the simple microscope, as it offers us at one view the principal phænomena to be observed; and for this purpose a lens of moderate power will suffice.

The blood-vessels, which to the naked eye appeared to terminate so abruptly, are seen to be continuous canals carrying blood in opposite directions. In the arteries the direction of the blood appears to be centripetal, from the inversion caused by the instrument, and the globules are pale. The veins are more voluminous than the arteries, and contain blood much darker, moving more slowly.

The minute elevations on the upper surface are seen to be of two kinds; one like small transparent grapes, within which there is a very active circulation; and the other of a conical shape, adherent at the base and destitute of internal circulation. The first are minute glands, the latter are the papillæ in which probably resides the power of taste.

The nerves are recognised by their wavy direction, their dark outlines by transmitted light, and their whiteness when viewed by reflexion.

Epithelium.—The scales of epithelium form a layer which covers every part of the upper surface of the tongue, and are the first parts which offer themselves to the microscopic observer. With a strong magnifying power they present a most interesting appearance, like a mosaic pavement, or a complete scaly armour covering the papillæ and the glands, descending into the interior of the latter, and into all the deeper inequalities that are found on the surface of the tongue. They may be seen equally well whether the animal is alive or dead; but if alive, it is preferable to choose one whose tongue is pale, and does not become rapidly injected when extracted from the mouth. With a little care of this kind, which applies equally to most of the following observations, it will be found easy to submit this organ to the strongest magnifying powers. The scales are much smaller than those found in the same region in man, or those which constitute the epidermis. In size they average from $\frac{1}{3000}$th to $\frac{1}{3700}$th of an inch. In form they are not perfectly regular, but generally rhombic or hexagonal, and sometimes of an elongated prismatic shape. They are mostly placed side by side in close juxtaposition, frequently overlapping each other. With a magnifying power of 600 diameters these bodies present on their surface an irregular punctuated appearance, such as might arise from the existence of small filaments projecting upwards in a vertical direction, and which are probably the ciliary filaments with which they are studded.

The flattened appearance of the epithelial scales, I believe, is owing to the manner in which the light is transmitted through the membrane, which gives a view of their horizontal section, but conveys no idea of their projection upwards. I am led to this conclusion from the appearance of the papillæ and glands, which project upwards into the mucous membrane, and are found to present at their borders several projections, such as we should expect from small bodies shaped like those figured by Henle, as existing on the inner side of the cheek in man. In Plate I. figs. 1 and 2, may be seen these toothed projections corresponding in size to the scales, which corroborates the view of regarding each of them as forming a small conical projection above the membranes. The scales on the under surface are much smaller and more indistinct, but are the same in other respects.

Cilia and ciliary movements.—These may be seen with great facility at the upper surface, when a sharp border of the tongue is examined under strong magnifying power. For this purpose a portion of the tongue is to be placed between two slips of glass with a little water: the foreign particles in suspension will then show by their agitation the existence of the ciliary motion, which will be seen at the borders of the membrane. There will be perceived, under favourable circumstances, minute projections or filaments; but more frequently there will be seen merely a kind of flittering movement, without the cilia which give rise to it being distinguished separately. These cilia were not included in the communication to the Société Philomatique, and their detection is due to M. Donné at a subsequent period.

Tongue of the Toad.—It is covered with more irregular epithelia, generally not so large as those of the Frog; in other respects they are much alike.

Tongue of the Lizard.—It is very remarkable for the thick and horny papillæ with which it is covered. These are all protected by a covering which is much like the epidermis. The layer of papillæ presents no appearance of circulation on the surface, and is too opake to allow any passage of light; but when the tongue is retained out of the mouth I have often observed the circulation at the borders; and the globules of blood are sufficiently voluminous to be easily seen by the simple microscope.

Papillæ, Glands, &c.—On the frog's tongue, immediately beneath the layer of epithelian scales, are seen various small bodies of irregular curved outlines, which may be considered as the second anatomical stratum of the tongue; these consist of papillæ, small glands and mucous follicles.

The papillæ are small bodies of a cylindrical, conical or club-like shape, connected at the base so as to be continuous throughout. In thickness or breadth they vary between about $0^{m}\cdot25$ and $0^{m}\cdot05$: their length cannot be accurately determined, as they have no fixed line of demarcation at their point of insertion on the mucous membrane. No appearance of any circulation is found in any of these papillæ: the blood-vessels run by them, sometimes forming vascular loops, but are rarely seen to penetrate through them; and even in those cases the vessel is frequently external to the papilla, which is sufficiently transparent to allow the blood-vessel to be seen through it. In inspecting the surface of the tongue, especially when the high powers are used, a mistake may be easily made, against which I wish to guard those who may repeat these experiments: it consists in the appearance of numerous curved lines running in every direction, perfectly black, and apparently very defined. These lines have been supposed to be loops of nervous fibres; and M. Donné appears still to labour under that erroneous idea, when at page 116, *Cours de Microscopie*, he says, "d'autres taches grises se remarquent. . . . . Je les crois formées par des houppes nerveuses." These lines are nothing more than the outlines of the papilla of fig. 1, seen not exactly at the right focus.

From the nature of their structure and functions we should expect to find some nervous filaments in the interior of the papillæ; and probably such do exist, though I have not yet been able to detect them. The nervous fibres, which are found below this layer, will be examined hereafter. They never present any opening; the dark points sometimes seen at their apex belong to some central spots of the epithelial scales, with which they are covered as with a glove; nor do they ever appear flaccid or empty.

Glands *.—The glands of the frog's tongue differ from the papillæ by the very active circulation which they present in their interior. They are always of a globular shape, more prominent and larger on the surface than the papillæ, being generally about the $\frac{1}{300}$th of an inch. The circulation in these small glands is very interesting to witness, as we can trace the progress of the particles of the blood as they enter into the centre of the gland, and follow them in all their movements, until they make their exit in the capillary vessel which

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[*] These will be shown hereafter to resemble the fungiform papillæ in the human subject.

conveys them to the venous branches. The movement of the blood in the gland may be compared to that in small whirlpools; and at first it does appear as if the blood were animated by a circular movement, in a small spherical cavity at the centre of the gland, without its being contained in any capillary vessel. This appearance however is illusory, and created by the rapid motion of the blood in a coil of transparent vessels, which are twisted up on this spot into a spherical shape. The limits of this vascular nucleus are well-defined, and are generally found to occupy half the diameter of the gland, or about $\frac{1}{1000}$th of an inch. When engorged with blood it is easy to perceive the coils of capillaries; and their continuity may be traced to the ends, which, as in fig. 2, bring the blood and convey it away. The two extremities are generally found at opposite sides of the gland, as in fig. 2. When perfectly empty it is almost impossible to detect these coils; but there may be seen, however, a distinct outline, which probably consists of the membrane, upon which is secreted the fluid contained in the external transparent area. The rate at which the blood moves in these coils is constantly varying; sometimes it is so quick that the globules are only perceptible by a flitting appearance, and in a few moments afterwards it becomes stationary. In favourable circumstances, when the red globules are scanty and the circulation slow, one of them will arrive in the gland, and may be followed in the various gyrations it performs; at one moment ascending in the superficial curves, at other times descending and passing into others placed deeper. Some idea may be formed in this way of the considerable extent of the capillary convolutions. The blood does not circulate with equal rapidity in different glands at the same moment; for there may be seen at the same time some in which the blood is stationary, and others in which it moves with extreme quickness. The external area, which, divested of the epithelial scales, would be transparent, comprises the other half of the diameter of the gland: within this the contents are perfectly motionless. In some there appears a radiated structure distinct from the epithelial covering, constituting separate compartments or cells in its interior: they contain a fluid substance void of granules or other particles perceptible by the microscope.

In general no opening is to be perceived on the surface of the gland for the passage of the fluid after it has been secreted; but in favourable instances a small opening with scales of epithelium around its borders can be seen, descending apparently into the cavity of the gland. Although it is difficult to demonstrate the existence of an aperture or duct, it is easy to ascertain that the secretion which lubrifies the surface of the tongue is very abundant over these bodies, and that it appears to arise from them. On examining the fluid, it was found to be a thick mucus, without any of the globules commonly known to characterize it.

The tongue of the toad presents likewise numerous papillæ and glands similar to those of the frog. In addition, numerous depressions in the mucous membrane are frequently seen, which are of the same nature as the mucous follicles or crypts in man. Around them and on their exterior we find a very abundant network of capillaries.

Vascular System and Circulation.—Having examined the glands, which may be considered as appendages of the vascular system, I now proceed to describe other phænomena of the same system, as observed in its general distribution throughout the tongue. The blood, we are aware, consists of a transparent fluid holding in suspension numerous particles, most of which are red and of a flattened shape, while a few others are colourless, and spherical in form. The various movements and appearances presented by these particles in their circuit from the arteries to the veins, constitute the principal phænomena to be observed by the microscope. The tongue offers great facility for observing the two opposite currents in the veins and arteries, as they run together side by side for a considerable distance in that organ.

I have already mentioned the rapid injection of the tongue as soon as it is subjected to the experiment, from the irritation caused by extracting it from the mouth. The degree of rapidity with which this takes place varies in different subjects, even when they are selected as similar as possible with regard to age, size, &c. These varieties are to be attributed to certain constitutional differences or idiosyncrasies, such as we find in human beings, and which are found to exist in a minor degree throughout the animal kingdom, and are inseparable from animal life. While preparing the tongue, we find likewise some of these constitutional peculiarities; for in some subjects it may be expanded without the least laceration, while in others it is extremely liable to tear with the slightest force used. In proportion as we penetrate more closely into the structure of this organ, we find numerous peculiarities of structure referable to the same cause; but particularly when examining the changes which occur in the vascular system, these evidences of constitutional peculiarities are more apparent. As this point is intimately connected with the vital functions, and may lead us to a better acquaintance with the laws by which they are governed, it is unnecessary to point out the interest with which it deserves to be examined.

We may estimate in some measure the changes which occur in the tongue when under the influence of irritation, by examining it in an animal recently dead, and without its having been subjected to any previous irritation. The capillaries visible are then found to be comparatively very few in number, and less engorged with blood; but as the blood in this case is perfectly motionless, it is still better to prepare a subject as speedily as possible, when the increase of the number of capillaries may be easily watched. Vessels which before were invisible, are then revealed by the passage of a few particles of blood in a certain defined direction, gradually becoming more distinct. I have never been able to satisfy myself on these occasions of the formation of any new channels for the blood; on the contrary, they have always appeared to me to be perfectly defined and distinct from the first moment of their appearance. The most remarkable changes to be observed in the tongue are the following:—

1. *Engorgement of the Capillaries.*—The capillaries are rapidly injected with blood, which becomes stagnant. Circulation is only seen at a few points. The arteries still continue to pour their contents, while the current in the vein becomes more and more languid, until at length it ceases entirely. The vessels appear to contain a red substance, in which we cannot easily detect the separate red particles. The lymph-globules are sometimes seen distinct within the vessels, but they are not generally very abundant: whenever a muscular movement is made, it is found to give rise to a brisk movement of the blood, which continues several instants afterwards.

2. *An increased action in the Capillaries.*—The stagnation in the capillaries is very slowly produced, for many hours we may witness the admirable sight of these vascular currents moving rapidly in all directions. At one place are the discs, passing through canals sufficiently large to allow several to pass at a time: by tracing this canal towards its extremity we find its calibre gradually diminish, until at length the discs or spherules can only pass one at a time. In these vessels may be frequently seen, as it has been described, the state of oscillation of the fluid column, the temporary stagnation of some loop, which is afterwards succeeded by the globules passing with great rapidity, while the adjoining loops in their turn become inactive; at other times the direction of the current in the loop takes an opposite course, which may continue as long as the examination lasts.

The most efficient cause in producing these changes in the capillary circulation is, without doubt, muscular contraction; for whenever that occurs, a state of great activity is produced in the circulation for several moments, and the inertia of several of the fluid columns is overcome. In recognising the influence of muscular action on the circulation, we must not forget that this is merely auxiliary to the great action of the heart, and that it only occurs at irregular intervals.

The action of the heart itself may be traced into the minutest capillaries, and even sometimes to the large venous trunks, by the periodical acceleration of the vascular column. I have already described the manner to detect the pulsations of the heart of the frog: I found the average number to be sixty-four in a minute. By counting the acceleration of blood in the artery, these two are found to correspond with great precision. In the same way, by accurately noting the time, we may in most cases observe a similar acceleration in the minute capillaries, and even in the veins. Müller has already noticed an oscillating movement in the capillaries, when the animal is in a state of great exhaustion; but in my experiments I have been able to detect it from the commencement without any impairment of the vital powers. In these instances the number of accelerated movements in a minute is about thirty-eight, and they are synchronous with the contractions of the heart: in the accompanying vein at the same time I have also perceived a slight oscillating movement of the blood. By a little practice the synchronous action of the capillaries and the heart will scarcely ever fail to be perceived.

3. *Corpusculation of Capillaries.*—The peculiar manner in which the lymph-globules, or corpuscles, conduct themselves in the capillaries, when in an organ in a state of irritation, has of late engaged much attention. The experiments of Mr. W. Addison of Malvern, have greatly contributed to show these important functions in inflammation. In the tongue of the frog and toad they may be frequently seen circulating with the red particles in the vessels, down to the minutest capillaries. As it has already been pointed out, these spherules are generally found, when they come in contact with the parietes of the vessels, to retain their adherence with greater force than is manifested by the red particles in the like circumstances; as in fig. 3, where the current was observed to continue for many minutes without displacing the globules near the sides of the vessel. Thus we frequently see a lymph-globule remain in the same place, notwithstanding the current of red particles sweeping and pushing by it. From its globular form we should have expected, *à priori*, that the contrary would have been the case. The appearance in the larger vessels of these spherules, adherent to their inner surface, has been very aptly compared

to so many pebbles or marbles over which a stream runs without disturbing them. Another arrangement of them in the capillaries is shown in fig. 4, where the vessel is occupied solely by these spherules. This is only to be found near the borders of the tongue, where we may suppose that the greatest irritation exists. The spherules are there found to be quite motionless, and not a single red particle can be detected at any point of the vessel, which, in the case in question, was followed for a considerable length. The corpuscles, which are transparent, are occasionally seen to be granulated. It appears to be very doubtful whether this corpusculation (if I may be allowed the expression) is an invariable accompaniment of all cases of irritative congestion or inflammatory action, as has been maintained. As far as I have been enabled to judge, the corpusculation only occurs after a rapid circulation has existed for some time in the vessels of the part irritated; but if the congestion and stagnation occur quickly, this collection of corpuscles is not to be seen, which would appear as if they required to be eliminated from a considerable mass of blood.

4. *Extra Fibrination of the Capillaries.*—Under this term I denominate another effect produced by increased influx of blood, which, if not so striking as the collection of the corpuscles, is still more constantly observed. It consists in a kind of condensation of the tissue on the exterior of the vessel, and appears to be produced by the exudation of the fibrinous parts of the blood, from increased pressure exerted by the blood on the capillaries. While this is taking place the vessel is less distinctly seen; minute globular forms may be perceived in this condensed tissue, consisting probably of particles of fibrine. I have already shown in a former paper upon the structure of vapours, &c., the great tendency which liquids have to assume a globular shape. It would be convenient to apply some term, such as I have used above, to distinguish this action. Sometimes, as in fig. 5, these globules are very distinctly seen.

5. *Extravasation of Blood.*—When this occurs, the whole surface of the tongue becomes covered with numerous spots of blood, similar to those which appear on the human body. It appears to take place more frequently in the glands than at other places, and the blood appears to be affected at their exterior surface. Generally the current of blood is stationary at the point of rupture and in the adjoining vessels. The appearance presented by the vessel at the ruptured point is shown in fig. 6. There is found a conical tumour projecting from the sides of the vessel; and at its extremity are seen a few

globules that have escaped. This fact is interesting, as the appearance of the rupture shows how much the tissue of the capillary is capable of being distended: either the blood does not always escape through the vessel, or the opening becomes soon closed up; for I have frequently seen small tumours, similar to that in fig. 6, and often larger, into which the blood penetrated and was seen to circulate*.

Muscles.—Muscular tissue is known to consist of minute fibres, which present under the microscope the appearance of straight tubes or cylinders, generally of the same thickness throughout, but varying in different fibres. In general these cylindrical fibres are closely packed together in a parallel direction, and adhere together by means of a semifluid glutinous substance possessing the properties of cellular tissue. According to Krause, these fibres, which are also frequently known under the name of primitive fasciculi, vary in diameter from $\frac{1}{240}$th to $\frac{1}{29}$th of a line. In the human pharynx, according to Schwann, they are from $\frac{1}{47}$th to $\frac{1}{40}$th of a line.

In certain parts of the body we find that these fibres present under the microscope dark lines or streaks in the direction of the transverse diameter, resembling the steps of a ladder; other fibres, on the contrary, are simple tubes without any striæ, and perfectly plain. The striated fibres were supposed for some time to belong exclusively to the muscles endued with voluntary action, and the latter to those removed from the influence of the will. But although such is generally the case, it has been found that this rule is not without exception. It has been remarked, that all the involuntary muscles with striated fibres are very red and fleshy, and that the voluntary muscles which are without them are pale and thin.

The muscular striæ have been the subject of great controversy. By some they are considered to consist of globules, which by their close arrangement in a line create a striated appearance. A hollow cylinder, such as the fibre appears to be, is supposed to contain these globules within it: the intermediate spaces are supposed to be light, from the absence of the globules, and to be occupied by some transparent fluid. This is the opinion advocated by Prevost, Dumas, Müller, Edwards, Home, &c.

Having given these brief preliminary details, I will now describe the muscular parts as they are seen in the tongue of the frog and toad.

Below the epithelium and papillæ are seen the muscular

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* At some points there exist in the minuter veins considerable constrictions. These are the valvules of the veins, and are not to be confounded with any abnormal condition.

fibres crossing in every direction, like straight lines or cylinders, of an even outline and without any subdivisions: at the inferior surface they are still more distinctly seen, from the greater thickness of the epithelial covering and the absence of papillæ: and from the mode of arrangement they may be compared to the threads of a piece of linen, or the beams of a raft. Thus we generally find one layer of fibres arranged transversely in a parallel direction, and with considerable intervals between them. Below these, by varying the focus slightly, appear other fibres cutting them at right angles, but otherwise arranged in a similar way. In this manner appear successive layers of muscular fibres at different angles. In addition to these layers is the genio-glossus proceeding from the convexity of the lower jaw-bone, and composed of round fibres arranged together like a bundle of reeds, gradually expanding in a curved direction towards the anterior extremity of the tongue.

For a long period all the muscular fibres of the tongue were considered to be destitute of striæ, and to belong to the fibres of the second class, such as those which line the intestinal tube. Such is the description still given of them by M. Donné at page 113, *Cours de Micros.*

As the other muscles of the frog which are under the influence of the will are known to present the characteristic striæ, it appeared to me a curious anomaly that the muscular fibres of the tongue, which are equally voluntary, should be without them. For that reason I was induced to examine the muscular parts of the tongue of the toad, to ascertain whether the like exception existed there: I found the striæ to be as distinctly marked as they are in any of the other voluntary muscles of the body. I have since on various occasions succeeded in observing them equally distinctly in the muscular fibres of the frog's tongue.

I will now mention what I have been able to observe with respect to the presence or absence of these striæ, as the subject may be deemed interesting from its bearing upon some of the theories of muscular contraction. The characteristic striæ are always to be perceived upon the muscular fibre of the toad's tongue; but in order to detect them conveniently (the tongue of this animal not being so expansive as that of the frog), a few slight incisions may be made, which can be done without injuring any large vessel. In the young animals of this species we may frequently be able to draw aside, and secure with a pin, a transparent, membrane-like cellular tissue, in which an active circulation can be observed for a long time, and the distinctive streaks of muscular fibre over its entire surface likewise. After these membranes have been subjected some time to the experiment, the striæ become indistinct and finally disappear. In some instances the muscular fibre appeared to have its contents broken up in separate fragments, each of which included several of the muscular striæ. Similar results were obtained from the examination of the tongue of the frog, except that frequently none of the striæ were to be seen in any portion of it. Their non-appearance I at first attributed to their more rapid disappearance; but notwithstanding every care I took to perform the experiment expeditiously, I found that in many instances they were entirely wanting: when examined after death (if the tongue has not been previously experimented upon) I have always succeeded in detecting them. Dilute nitric acid in these cases was found to render the striæ more distinct. The striæ, as they appear in these animals, are sometimes perpendicular to the axis of the fibre, at other times inclining slightly to it; and even in some instances the lines are bent in the middle. Their distance from each other, though generally equal, was sometimes irregular.

In all my observations I have never detected any angular deviation when the muscular fibres contracted, as asserted by Prevost and Dumas. During the contraction the sides of the fibres are merely seen to become more dark and distinct, and perhaps more thick; but in general, with high magnifying powers, the movement is too quick to be followed, and the fibre loses the exact focus of the instrument: neither have I detected any displacement or alteration of the striæ, such as many anatomists have imagined to take place.

In the preceding observations I have endeavoured to show some of the results to be obtained by a careful microscopic observation of this organ, so important in its functions, and containing within it all the principal tissues of the animal frame. I have yet to treat of the nerves and lymphatics, whose ultimate distribution is beautifully displayed. It is unnecessary to point out all the facts which I consider novel in this communication; but as some of the most interesting, I may refer to the alterations in muscular fibre during life, the circulation in the glands, and the entire separation, in certain cases, of the spherical from the other particles of the blood.

Kensington, August 15, 1846.

Additional Observations.

Recent observations have enabled me to decide the much-agitated question as to the formation of pus, and its origin from the extravasation of the colourless or spherical corpuscles from the capillaries. In a future communication I purpose to enter fully into this subject, but at present I will confine myself to the establishment of this fact by the two following observations extracted from my notes.

August 1st.—A toad was examined after it had been dead two or three hours; the abdominal cavity and cellular tissue were found distended by a limpid dropsical collection of serum, containing numerous white corpuscles, with a few blood-discs. The mesentery was arranged for microscopic inspection, and its capillaries were found filled with corpuscles and discs all at rest. Upon examining the membrane I observed corpuscles within the vessel, which disappeared from the spots where they had previously been detected, and after a few minutes were no longer to be found. The only traces of their former situation were curved indentations in the vessel, of the same size as the corpuscles, and a solution in the continuity of the parietes of the vessel at those points. At other parts, where several of the corpuscles had existed together, the capillary tube seemed completely interrupted, as if it had been entirely dissolved. While the corpuscles were disappearing in this manner from the vessel the red discs remained stationary.

August 29th.—The tongue of the live frog was distended in the ordinary way. After the observation had continued for half an hour, numerous corpuscles were seen outside the vessels, together with a very few blood-discs, in the proportion of about one to ten of the former. The extravasated particles were equally diffused over most part of the tongue. No appearance of rupture could be seen in any of the vessels. The corpuscles were generally distant about $0^{\mathrm{mm}}\cdot03$ from their parietes. After the experiment had lasted about two hours, thousands of these corpuscles were seen scattered over the membrane, with scarcely any blood-discs. The process by which they passed out of the vessel could be best observed in a capillary containing stationary blood particles. Generally, at a slight distance from it, some extravasated corpuscles could be detected, and at the nearest opposite point of the tube a small concave depression was presented. Frequently, near this depression, numerous corpuscles were collected within the tube, as if about to follow the rest which had escaped. These were frequently agitated by a movement of oscillation, which showed that there was no open point in the tube. In other spots some of these corpuscles were seen protruding half out of the vessel. Whenever the current re-occurred in a vessel presenting these appearances, the depressions and unevenness quickly disappeared, and no trace of the corpuscular extravasation could be seen, except the presence of the corpuscles themselves.

I consider therefore as established,—1st, the passage of these corpuscles "de toute pièce" through the capillaries; 2ndly, the restorative power in the blood, which immediately closes the aperture thus formed. It would lead me too far at present to explain how I have obtained purulent matter in these animals similar in all respects to that in the human subject.

Kensington, Sept. 21, 1846.

XLV. *On an Improvement in the Daguerreotype Process by the application of some new compounds of Bromine, Chlorine and Iodine, with Lime.* *By* R. J. Bingham, *Chemical Assistant in the Laboratory of the London Institution*[*].

ALL persons who have practised the Daguerreotype must have remarked, that in warm weather a considerable deposition of moisture takes place upon the glass or slate cover used to confine the vapour in the bromine or accelerating pan. This moisture must also necessarily condense upon the cold metallic surface of the plate during the time it is exposed to the bromine vapour. In fact, I have been informed by a number of professional Daguerreotypists (and I have experienced the difficulty myself), that they were unable to obtain perfect pictures during the excessive heat of the late season; and a very clever and enterprising operator, who last year made a tour on the continent, and brought home some of the finest proofs I have ever seen, entirely failed this season in obtaining clear and perfect pictures, from the constant appearance of a mist or cloud over the prepared surface. This appears to be caused by the deposition of moisture upon the plate, arising from the water in which the bromine is dissolved. To obviate this some have recommended the pan to be kept at a low temperature in a freezing mixture; and M. Daguerre, in a communication to the French Academy of Sciences, recommends the plate to be heated: but in practice both these plans are found to be unsuccessful. (See Lerebour's *Traité de Photographie.*)

It appeared to me, that if we could avoid the use of water altogether in the accelerating mixture, not only would the difficulty I have mentioned be avoided, but a much more sensitive surface would be obtained on the plate. With this view I endeavoured to combine bromine with lime, so as to form a compound analogous to bleaching powder. In this I

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[*] Communicated by the Author.

was successful, and find that bromine, chloride of iodine and iodine, may be united with lime, forming compounds having properties similar to the *so-called* chloride of lime.

The bromide of lime* may be produced by allowing bromine vapour to act upon hydrate of lime for some hours: the most convenient method of doing this is to place some of the hydrate at the bottom of a flask, and then put some bromine into a glass capsule supported a little above the lime. As heat is developed during the combination, it is better to place the lower part of the flask in water at the temperature of about 50° F.: the lime gradually assumes a beautiful scarlet colour, and acquires an appearance very similar to that of the red iodide of mercury. The chloro-iodide of lime may be formed in the same manner: it has a deep brown colour. Both these compounds, when the vapour arising from them is not too intense, have an odour analogous to that of bleaching powder, and quite distinguishable from chlorine, bromine or iodine alone.

Those Daguerreotypists who use chlorine in combination with bromine, as in Wolcott's American mixture, or M. Guérin's Hungarian solution, which is a compound of bromine, chlorine and iodine, may obtain similar substances in the solid state, which may be used with great advantage. By passing chlorine over bromine, and condensing the vapours into a liquid, and then allowing the vapour of this to act upon lime, a solid may be obtained having all the properties of the American accelerator; or by combining the chloro-iodide of lime with a little of the bromide, a mixture similar to that of M. Guérin's may be produced: but I greatly prefer, and would recommend the pure bromide of lime, it being, as I believe, the quickest accelerating substance at present known. By slightly colouring the plate with the chloro-iodide, and then exposing it for a proper time over the bromide, proofs may be obtained in a fraction of a second, even late in the afternoon. A yellow colour should be given by the use of the first substance; and the proper time over the bromide is readily obtained by one or two trials. With about a drachm of the substance in a shallow pan, I give the plate ten seconds the whole of the first day of using the preparation, and add about three seconds for every succeeding one. The compound should be evenly

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* I call this substance bromide of lime, although there is a difficulty as to the composition of bleaching powder, and which would also apply to the compounds I describe. Some chemists regard the *chloride of lime* to be a compound of lime, water and chlorine. Balard thinks it is a mixture of hypochlorite of lime and chloride of calcium; and the view of Millon and Prof. Graham is, that it is a peroxide of lime, in which 1 equivalent of oxygen is replaced by 1 of chlorine.

strewed over the bottom of the pan, and will last with care for about a fortnight.

The great advantage of this compound is, that it may be used continuously for a fortnight without renewal; and, unlike bromine water, its action is unaffected by the ordinary changes of temperature.

I have hastened to communicate this during the present fine weather, believing that it will be acceptable to all interested in this beautiful application of science.

September 14, 1846.

XLVI. *On the Constitution of Aqueous Solutions of Acids and Alkalies.* By John Joseph Griffin, Esq.*

An inquiry has been instituted of late into the *volume* that is assumed by a given weight of any chemical substance on being dissolved in water; and the chemists who have instituted this inquiry have promptly drawn conclusions and established laws, very general and comprehensive in character, and susceptible of important applications, provided they are founded on well-established facts.

Thus, "in the year 1840," I quote from Messrs. Playfair and Joule, "Dalton made the interesting discovery, that sugar and certain salts on being dissolved in water increase its bulk only by the amount of water pre-existing in them. He generalized this observation by asserting that all hydrated salts dissolve in water, increasing its bulk merely by their amount of water of hydration, while anhydrous salts do not at all increase the bulk of the water in which they are dissolved.

"But it must not be forgotten, that when Dalton published this paper he was much enfeebled by illness, and on this account it does not derogate from the acuteness of the philosopher, that Mr. Holker was unable to confirm Dalton's results in repeating the experiments in 1843. He did so however in the case of sulphate of magnesia, and approximately in that of one or two other salts."—*Philosophical Magazine*, vol. xxvii. p. 456.

The experiments that were particularly described by Dalton, were made with sugar and sulphate of magnesia. But he declares that his hypothesis was not founded on those experiments alone. He says, "I have tried the carbonates, the sulphates, the nitrates, the muriates or chlorides, the phos-

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* Communicated by the Chemical Society; having been read April 20, 1846

phates, the arseniates, the oxalates, the citrates, the tartrates, the acetates, &c. &c., and have been uniformly successful. Only the water adds to the *bulk* and the *solid matter* adds to the weight."—*Essay on the quantity of Acids, Bases and Water in the different varieties of Salts; with a new method of measuring the* WATER *of Crystallization, as well as the* ACIDS *and* BASES.

Dr. Dalton knew well the importance of the principle he was attempting to establish, for he declares "It is the greatest *discovery* that I know of next to the atomic theory." —*New and Easy Method of Analysing Sugar.*

In May 1845, Messrs. Playfair and Joule presented to the Chemical Society a memoir, in which they have recorded the results of an elaborate experimental examination of Dalton's hypothesis. (See Philosophical Magazine, vol. xxvii. p. 453.) Their conclusions are no less startling than were those of Dalton, on which the inquiry proceeded. They find his doctrine in relation to sugar to be true. "It is," they say, "an extraordinary fact, that the twelve atoms of carbon in sugar cease to occupy space in solution, and that the bulk of an atom of sugar is just the bulk of its oxygen and hydrogen considered as water. And this," they say, "is a matter of supreme interest, and cannot fail to lead to important results when we come to the consideration of organic compounds." —*Pages* 530 *and* 474.

They next confirm Dalton's statement with regard to sulphate of magnesia and other analogous salts, namely, that when anhydrous they occupy no space in solution, but when hydrated they add the bulk of the water of hydration to the bulk of the water of solution.

Thirdly, they find that other compounds dissolved in water increase its volume for every equivalent, either by 9 or by multiples of 9. [9 is their standard number for the volume of an equivalent of water.]

These statements affirm three main propositions, namely,—

1. That certain anhydrous substances when dissolved in water *occupy no space*. The volume of the solution is the same as the volume of the water contained in it.

2. That other anhydrous substances when dissolved in water *assume a fixed and characteristic volume*; so that the volume of the solution is not the same as that of the water contained in it, but greater or smaller, in exact proportion to the atomic volume of the substance dissolved in it.

3. That when organic substances are dissolved in water their carbon occupies no space, but their hydrogen and oxy-

gen, if in the proportions proper to form water, occupy exactly the space of an equivalent weight of water.

Other subjects than these are discussed in the memoir of Messrs. Playfair and Joule, but in order to avoid complication I shall confine myself entirely to what relates to the atomic volume of substances in solution.

If the first two of the foregoing propositions are true, they give us important information respecting the constitution of aqueous solutions. If, for example, we are furnished with the specific gravities of anhydrous substances, and with their atomic volumes in solution, then, knowing their atomic weights, and taking for granted the specific gravity of water and the temperature of the several materials, we shall have in our hands the whole laws that regulate the specific gravities of aqueous solutions, and we shall be enabled to calculate tables of solutions perfectly free from the inaccuracies that beset all tables that are prepared from experiments on individual solutions. We shall be rendered by these doctrines so completely master of details, that, if the specific gravity of any solution be given, we shall be able to tell its chemical strength; or if the chemical strength be given, we shall be able to declare what must be its specific gravity. These doctrines lead, therefore, to very important practical results, and they are the more deserving of regard, that the previous researches of chemists in this department have been entirely unsuccessful. All that we know of the relation of the chemical strength of solutions to their specific gravity has been derived from practical trials, and we have no general principle to guide us beyond the limits of those trials, no rule by which to test the accuracy of the experimental data, or by which to determine either the chemical power of a solution from its density, or the density from its chemical power. The laws announced by Dalton, and reaffirmed by Messrs. Playfair and Joule, come apparently to clear away this difficulty, and coming supported by the results of a series of elaborate and ingenious experiments, if they are too startling to command the immediate assent of chemists, they at least merit a rigorous examination.

In the course of an inquiry into the subject of centigrade testing (dosage), I have had occasion to examine the constitution of a great number of solutions of the principal acids and alkalies. The facts elicited by that examination afforded collectively a sharp test of the accuracy of these new doctrines respecting the atomic volumes of substances in solution. I compared therefore the doctrines with the facts carefully, and I feel bound to declare that they are at variance. Among a dozen chemical substances, whose solutions I have examined, I find only one that appears to have a fixed atomic volume in solution in water. That substance is ammonia. All the other substances that I have examined vary constantly in their liquid volume, according to the state of dilution, and I am forced to the conclusion, in so far at least as concerns the substances that I shall enumerate, that the doctrine of a fixed atomic volume in solution is fallacious and of no use. While I think it right to state this opinion explicitly, I do so with considerable diffidence. It is possible that I may have overlooked some circumstance essential to the full comprehension of the question. I by no means deny the accuracy of the experiments upon which the opposite opinion has been founded. The difference in our conclusions results from different ways of interpreting the experiments. The conclusions are however incompatible; and if the doctrines of Messrs. Playfair and Joule shall prove to be correct, serious fallacies must exist in my calculations or mode of reasoning.

To place the evidence on this subject before chemists, I proceed to detail my researches into the constitution of solutions of acids and alkalies. An investigation of this sort has the unfortunate property, that, dealing with a great variety of separate facts, an account of it must necessarily be long. I shall shorten it to the utmost by throwing as much as possible into the form of Tables; and in order to render the condensed account intelligible, I shall begin by describing the plan of the Tables, and explaining such terms and other particulars as seem to require it.

Of the Measures used.

I have in all cases examined the constitution of the same volume of solution, namely, the tenth part of an imperial gallon, or the bulk of one avoirdupois pound, or 7000 grains of pure water at the temperature of 62° F. I propose to call this measure a DECIGALLON.

For the sake of having a manageable term for a small volume of liquid, I divide the decigallon into 1000 parts, each of which contains 7 grains of water, and for which measure I propose the name of SEPTEM.

A complete scheme of decimal measures thus founded on the imperial gallon is exhibited in the following Table:—

Imperial Liquid Measure. Divided Decimally.

Gallon.	Deci-gallons.	Centi-gallons.	Milli-gallons.	Septems.	Avoirdupois weight of water at 62° F.	
					Grains.	Pounds.
1·	10·	100·	1000·	10000·	70000·	10·
·1	1·	10·	100·	1000·	7000·	1·
·01	·1	1·	10·	100·	700·	·1
·001	·01	·1	1·	10·	70·	·01
·0001	·001	·01	·1	1·	7·	·001

1 Quart = 2500 Septems. 1 Fluid ounce = 62·5 Septems.
1 Pint = 1250 Septems. 1 Cubic inch = 36·06543 Septems.

The relation of the decigallon to the septem is the same as that of the litre (kilogramme of water) to the cubic centimetre (gramme of water).

1 litre contains 2200 septems.

Chemical Standards.

In speaking of chemical substances with reference to atomic weights, I have used the numbers contained in the tables of Berzelius, oxygen = 100. In order to be able to speak without vagueness of certain absolute quantities of substances, I use the term *Test Atom* to signify Berzelius's atomic weight of a substance *weighed in English grains.* By the term *Degree,* I mean the hundredth part of a test atom. Thus,—

5·01165 grs. is 1° of SO^3.
501·165 ... is 1 test atom, or 100° of SO^3.

In applying these measures and weights to centigrade testing, I prepare *normal test liquors* of 100° of strength, in all cases, by dissolving 1 test atom of the substance in so much water as produces 1 decigallon of solution at 62° F.; that is to say,—

To prepare Carbonate of Soda of 100°.—I dissolve 667·335 grs. of pure anhydrous carbonate of soda in distilled water, dilute the solution to the bulk of about 995 septems, bring it to the temperature of 62° F., and then adjust the volume to exactly 1000 septems.

To prepare Nitrate of Silver of 10°.—I dissolve 212·864 grs. of pure crystallized nitrate of silver in distilled water, and in like manner dilute the solution at 62° F. to the bulk of 1000 septems.

A centigallon of test liquor of 100° contains the tenth part of a test atom of any substance. My alkalimeter, or centigrade pouret, has the capacity of a centigallon, and is graduated into 100 septems: hence every degree of this instrument indicates the 1000th part of the test atom of the reagent, while, as each degree measures the 10,000th part of an imperial gallon, it is easy to find the relation between the chemical strength and the physical mass of a given solution.

Methods of Experimenting.

The experiments that are distinguished in the following Tables by the letter G were made in my laboratory, except in those cases where other authorities are specified. The chemical solutions were prepared in stoppered graduated glasses at the temperature of 62° F. The requisite analyses were all effected by the method of centigrade testing. Various precautions were taken to ensure accuracy, but I forbear to enter into details, because they would occupy too much space, and I reserve them for a separate work on Centigrade Testing, which I purpose to publish for the use of chemical manufacturers. The specific gravities were determined by weighing the solutions in light bottles, of which three sizes were used, capable of containing respectively 3000, 700, and 350 grs. of water. The balance in which the large bottle was weighed turned with half a grain when loaded. That in which the small bottles were weighed turned with ·01 gr. when loaded. The weighings were all taken when the liquors were at the temperature of 62° F. The greater part of the experiments were made by my assistant, Mr. Joseph White of Nottingham; the others by myself.

The tables of mineral acids were calculated from the experiments already published by Dr. Ure and other chemists.

Atomic Volume of Water.

For the sake of subsequent reference I shall state the atomic volume of water, as derived from the atomic weights both of Dumas and of Berzelius.

TABLE I.—*Water.*
H^2O = 112·48 grs.: Berzelius.
= 112·5 grs.: Dumas.
Temperature 62° Fahrenheit.

Authorities.	Specific gravity of the liquid.	Grains of H^2O in 1000 septems.	Atoms of H^2O in 1000 septems.	Septems containing 1 atom of H^2O.
B	1·000	7000·0	62·233	16·0686
D	1·000	7000·0	62·222	16·0714

The last column of this table shows the *atomic volume of water,* or the bulk of 1 test atom at 62° Fahr., expressed in septems.

TABLE II.—*Sulphuric Acid.*

$SO^3 = 501{\cdot}165$ grs. Temperature 62° F.

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of SO^3 in 1000 septems.	Atoms of Water to 1 atom SO^3.	1 SO^3		Septems of solution containing 1 SO^3.
				Occupies: Septems.	Condenses: Septems.	
B	1·97	27·516	0·0	36·34	0·0	36·343
BM	1·896	23·81	0·5	33·96	2·38	41·999
G	1·848	21·081	1·0	31·37	4·97	47·437
CU	1·8484	21·00	1·02	31·19	5·15	47·619
	1·8393	20·00	1·27	29·63	6·71	50·000
	1·8212	19·00	1·51	28·37	7·97	52·632
	1·7961	18·00	1·75	27·37	8·97	55·556
G	1·78	17·16	2·0	26·14	10·20	58·276
CU	1·7636	17·00	2·004	26·68	9·66	58·824
	1·7279	16·00	2·265	26·10	10·24	62·500
	1·6888	15·00	2·55	25·67	10·67	66·667
	1·6487	14·00	2·873	25·26	11·08	71·429
G	1·6321	13·623	3·0	25·19	11·15	73·403
CU	1·6086	13·00	3·245	24·78	11·56	76·923
	1·5672	12·00	3·67	24·33	12·01	83·333
	1·522	11·00	4·155	24·14	12·20	90·909
	1·4795	10·00	4·75	23·64	12·70	100·00
	1·4362	9·00	5·475	23·13	13·21	111·11
	1·3926	8·00	6·377	22·52	13·82	125·00
	1·349	7·00	7·54	21·74	14·60	142·86
	1·3033	6·00	9·062	21·04	15·30	166·67
	1·2567	5·00	11·19	20·25	16·09	200·00
	1·2112	4·00	14·39	18·80	17·54	250·00
	1·1603	3·00	19·61	18·16	18·18	333·33
	1·1078	2·00	30·01	17·69	18·65	500·00
	1·088	1·60	37·86	16·59	19·75	625·00
	1·0567	1·00	61·30	14·89	21·45	1000·0
	1·0286	·50	123·6	14·21	21·13	2000 0
	1·0151	·25	248·2	11·20	25·14	4000·0
	1·0066	·10	621·9	5·59	30·75	10000·
	1·0043	·06	1037·	− 0·07	36·41	16667·
	1·0037	·05	1245·	− 2·41	38·75	20000·
	1·0032	·04	1556·	− 8·40	44·74	25000·
	1·0026	·03	2075·	−15·07	51·41	33333·
	1·002	·02	3112·	−28·41	64·75	50000·
	1·0014	·01	6227·	−68·41	104·8	100000·
1	2	3	4	5	6	7

AUTHORITIES.—B = Bussy. The substance is the anhydrous acid = SO^3. BM = Bussy for the sp. gr.; Mitscherlich for the chemical constitution. It is the fuming sulphuric acid of Nordhäusen = SO^3, $H^2O + SO^3$. G = Graham and Gmelin. CU = my calculations, after the nearest numbers in Ure's table.

Description and Plan of Table II.

I shall give a full description of the table of sulphuric acid to render the plan intelligible, and to enable me to dispense with descriptions of many of the following tables constructed on the same model.

Every horizontal line shows the composition of a decigallon of solution. Column 2 shows the specific gravity of the solution, or the weight in avoirdupois pounds of a decigallon. By multiplying the specific gravity by 7000, we obtain the weight of a decigallon in grains.

Column 3 shows the absolute weight of anhydrous acid contained in a decigallon of each solution, expressed in test atoms. To find the weight in grains, the numbers in this column must be multiplied by 501·165, the number given at the head of the table as the weight of a test atom.

These two columns of each table contain the physical and chemical data from which all the particulars given in the other columns are derived.

The numbers contained in column 5, which show the "*volume in solution*" occupied by 1 test atom of anhydrous acid at different stages of dilution, are determined by the following calculation. The absolute weight in grains of the anhydrous acid contained in a decigallon of solution is deducted from the absolute weight in grains of the same volume of solution: the difference is the weight in grains of the water. Dividing this difference by 7, we have the volume of the water expressed in septems. Deducting this volume from 1000 (the number of septems contained in a decigallon), the difference shows the number of septems occupied by the known weight of anhydrous acid. Dividing this number of septems by the number of test atoms constituting the known weight of anhydrous acid, the product is the *number of septems occupied by one test atom of the anhydrous acid.* As this calculation is important, I shall illustrate it by two examples.

First Example.—Acid of sp. gr. 1·8393, having a chemical strength of 2000°, or 20 test atoms per decigallon.

1839·3 = sp. gr. of the solution.
Mult. by 7
12875·1 = weight of 1000 septems of solution in grs.
Deduct 10023·3 = weight of 20 test atoms of acid in grs.
7) 2851·8 = weight of the water in grs.
407·4 = volume of the water in septems.
20) 592·6 = volume of the acid in septems.
29·63 = volume of 1 test atom of acid in septems.

Second Example.—Acid of sp. gr. 1·0037, chemical strength 5°, or 0·05 test atom per decigallon.

1003·7 = sp. gr. of the solution.
7
7025·9 = weight of 1000 septems of solution in grs.
25·058 = weight of $\frac{1}{20}$th test atom of acid in grs.
7)7000·842 = weight of the water in grs.
1000·1203 = volume of the water in septems.

In this case the water present is in greater quantity than would suffice to *fill* the decigallon measure if water alone was present. The 5° of acid (25·058 grs.) condense their own volume and ·1203 septem besides. The effect produced by 100° of acid, or 1 test atom, is of course 20 times as much, namely, 2·406 septems. I have put this and similar negative quantities in the table with the *minus* sign (−) prefixed to them.

By this calculation we effect a complete analysis of the solution. We begin with—

$$\left.\begin{array}{r}\text{the volume}\\ \text{the weight}\end{array}\right\}\text{of the solution,}$$

$$\left.\begin{array}{r}\text{and we find the volume}\\ \text{the weight}\end{array}\right\}\text{of the water,}$$

$$\left.\begin{array}{r}\text{and the volume}\\ \text{the weight}\end{array}\right\}\text{of the anhydrous acid.}$$

The numbers in column 6, which represent the condensation that takes place, are found by subtracting the numbers contained in column 5 from the number which represents the volume of the test atom of uncombined acid SO^3, which number is placed at the head of column 5.

The numbers in column 6 represent the actual condensation effected in each mixture by the chemical power of one test atom of SO^3. I do not mean to ascribe the diminution of bulk to the acid, but to the compound which the acid helps to form. There is consequently a fallacy in the statements given in column 5. It is not *absolutely true* that SO^3 *occupies* the volume there assigned to it in different solutions. It is only true if the actual condensation that occurs is reckoned solely upon the acid, and none ascribed to the water or the resulting hydrate; but in reality the hydrate alone suffers the whole condensation. This compound has the property of existing, at a given temperature, within a smaller volume than that occupied by its uncombined elements. None of the condensation therefore can be properly ascribed either to the acid or to the water individually. This being understood, the fallacy in the table can lead to no error. The plan adopted has the advantage of rendering the effects of different substances acting on water readily comparable with one another.

The numbers contained in column 7 represent the atomic volume of the different solutions, that is to say, the volume of solution that contains a test atom of the anhydrous acid. To find these numbers we divide 1000 by the numbers contained in column 3, or by the degree expressed in test atoms.

The numbers contained in column 4, which represent the state of hydration expressed in atomic proportions, are determined by the following process. The numbers in column 7 show the volume in septems of every solution equivalent to 1 test atom of anhydrous acid. Deducting from that number the volume occupied by 1 test atom of anhydrous acid, as quoted in the same horizontal line in column 5, the residue is the quantity of associated water expressed in septems. Dividing this residue by the atomic volume of water, 16·07, the product shows the number of atoms of water that occur in combination with 1 atom of the anhydrous acid.

The range of the experiments recorded in this and the following tables, is from a solution saturated at 62° F. to a solution containing 1° of acid or other substance per decigallon, or 1 test atom in 10 imperial gallons of solution. I do not stop at that stage of dilution because the condensing power of the chemical substances is exhausted, for I find no limit to that power. I stop because experiments performed on liquors further diluted do not give results of sufficient accuracy for this inquiry. It is impossible to determine the exact specific gravity of solutions running between sp. gr. 1·001 and sp. gr. 1·000.

Observations on some of the prominent features of the Table of Sulphuric Acid.

Messrs. Playfair and Joule (Philosophical Magazine, vol. xxvii. p. 480) consider the atomic volume of sulphate of water to be equal to twice the atomic volume of water. Deducting the water, this leaves 16·07 as the volume in solution of the anhydrous sulphuric acid. This agrees with a solution of about 160° of chemical strength, but disaccords with every other solution, weaker or stronger. In fact, anhydrous sulphuric acid has no fixed volume in solution. Its measure extends from + 33·96 septems to − 68·41 septems, changing with every change in the composition of its hydrates, so that a single drop of water cannot be added to any solution without diminishing the bulk, by increasing the condensing power, of the anhydrous acid.

The amount of the condensation effected by anhydrous sulphuric acid in its various stages of hydration is remarkable. The atomic volume of the test atom of acid is very nearly 36 septems, or 1 cubic inch. With half an atom, or 8 septems of water, it condenses $2\frac{2}{3}$ septems. With 1 atom, or 16 septems of water, it condenses 5 septems. With 2 atoms, or 32 septems of water, it condenses 10 septems. In all these cases the condensation is equal to nearly one-third part of the entire bulk of the water. With 6227 atoms, or 100105 septems of water, it condenses 105 septems. In this case the condensation is equal to only one-thousandth part of the bulk of the water, but it is equal to three times the bulk of the uncombined anhydrous acid.

It is curious to compare this chemical condensation with that which is effected in water by simple pressure. A weight of 1 lb. avoirdupois applied to a cube of water of the bulk of 1 septem diminishes its height to the extent of 50 millionths, whereas 1 lb. weight of sulphuric acid, = 14 test atoms, applied to 450 septems of water, = 28 test atoms, occasions a condensation equal to 143 septems, and if applied to 100000 septems of water, it occasions a condensation equal to 1400 septems.

If, as some chemists affirm, dilute solutions are not to be held as chemical compounds, but only as mixtures of acid and water, in what manner shall we account for this enormous condensation? We universally admit, as chemical compounds, the hydrate of sulphuric acid with 1 atom of water (oil of vitriol), and the crystallizable hydrate with 2 atoms of water; but when the compound contains more water than 2 atoms, some of us, for that reason alone, begin to question the occurrence of chemical action. Yet surely the evidence afforded by progressive condensation is not to be disregarded merely because the condensation occurs in a dilute liquor? and if a condensation equal to *ten* septems is taken as a proof of chemical action in the formation of the second hydrate, how can we reasonably declare that a condensation equal to *a hundred* septems, effected by the same quantity of acid, but with more water, is evidence of *no* chemical action?

The rate of condensation does not seem to accord with any arithmetical or geometrical progression. I cannot reduce it to any useful formula. I can perceive no particular relation that it bears to the real specific gravity of the solutions, nor to their chemical strength, nor to the state of hydration of the acid, nor to any other general principle. I am unable to construct a formula with which to determine either the chemical strength of a solution from its specific gravity, or the specific gravity from its chemical strength. But this probably arises from the fact that the subject has not been sufficiently examined; for a glance at the Tables cannot but suggest the opinion, that the condensation effected by every particular substance will have an invariable progression.

In the mean time there is one particular in which we can approach a little towards a knowledge of the laws that regulate specific gravities, and that is, in determining the exact degree in which condensation increases the mean specific gravity of a solution. What I have to say on this head applies to solutions of all substances, and has therefore a general interest.

If we deduct 1 from the specific gravity of anhydrous sulphuric acid, and divide the residue by the number of test atoms of anhydrous acid that fill a decigallon at 62° F., the product is a fraction, which, when added to 1, represents the mean specific gravity of a solution containing 1 test atom of acid per decigallon. Thus—

$$\frac{\text{sp. gr. } 1{\cdot}97-1}{27{\cdot}516}=\frac{{\cdot}97}{27{\cdot}516}={\cdot}035252.$$

The product ·035252 + 1 = 1·035252 is the mean specific gravity of a solution of acid of 100°.

The same decimal fraction multiplied by any given number, say 20, and then added to 1, gives the mean specific gravity of a solution of the chemical strength indicated by the given number. Thus—

$$({\cdot}035252\times 20)+1=1{\cdot}70504,$$

which is the mean specific gravity of a solution of acid of 2000°.

If now we compare the mean specific gravities thus calculated with the real experimental specific gravities of the several solutions, and take the *differences*, we obtain a series of numbers which show the precise extent to which the specific gravities are increased by the condensation resulting from the chemical action of the acid upon the water. I give these differences in the following Table.

TABLE III. Showing the increase of Specific Gravity occasioned in Solutions of SO^3 by condensation.

Degree in atoms.	Increase in spec. grav.	Degree in atoms.	Increase in spec. grav.	Degree in atoms.	Increase in spec. grav.
23·81	·056650	17·00	·164316	11·00	·134228
21·081	·104853	16·00	·163868	10·00	·126980
21·00	·108108	15·00	·160020	5·00	·080440
20·00	·134260	14·00	·155172	3·00	·054544
19·00	·151412	13·623	·151862	1·00	·021448
18·00	·161564	13·00	·150324	0·10	·003075
17·16	·175076	12·00	·144176	0·01	·001048

It is commonly stated, in reference to the hydrates of sulphuric acid, that the greatest condensation occurs in the formation of the hydrate $SO^3+3H^2O=1362{\cdot}3°$; but this table shows that the greatest condensation occurs in the formation of the hydrate $SO^3+2H^2O=1716°$. What I mean by the greatest condensation is that which occurs *in a given mass of solution*; for, speaking absolutely, the greatest condensation effected by 1 atom of SO^3 (or of any other substance) is that which occurs when it combines with the largest possible quantity of water. The extent to which this holds true is best seen on examining the specific gravity of a very dilute solution, for example, that of 1°:

1·00000000 = specific gravity of water.
0·00035252 = fraction indicating the mean specific gravity of 1° of acid.
0·00104748 = increase of specific gravity due to condensation.
1·00140000 = real specific gravity of acid of 1°.

The effect produced by condensation is here three times as great as that due to the mere difference of weight between the anhydrous acid and the water which it replaces.

The common attribution of the greatest condensation to the third hydrate is nevertheless true, provided we take the protohydrate as the point of comparison. Thus, 47·437 septems of SO^3, H^2O added to 16·07 septems of water = 63·507 septems, whereas the atomic measure of $SO^3 + 2H^2O$ is only 58·276 septems, showing a condensation of 5·231 septems. Again, 47·437 septems of SO, H^2O added to 32·14 septems of water = 79·577 septems, whereas the atomic measure of $SO^3 + 3H^2O$ is 73·403 septems, showing a condensation of 6·174 septems, which exceeds the former condensation by 0·943 septems.

The extent to which the mean specific gravity of a solution is increased by condensation depends necessarily on the actual amount of the condensation. As it is easy to ascertain that amount, it will be proper to point out the exact relation between condensation and the term expressing the real specific gravity of a solution.

If we suppose 500 septems of acid to combine with 500 septems of water, and to condense so much as to form only 999 septems of solution, we shall require 1 septem more of water to fill the standard measure of a decigallon. This adds 7 grs. to the weight of the solution, and therefore increases its specific gravity by ·001. If the condensation is such as to require 5 septems of water to complete the decigallon measure, the weight of the liquor will be increased by 35 grs. and its specific gravity by ·005. For every septem of condensation per decigallon, that is to say, for the condensation of every thousandth part by measure of the solution, the increase in specific gravity will be ·001. This law is universal. The nature of the chemical substance that causes the condensation, the quantity of it contained in the solution, its specific gravity, its atomic weight, the volume of its anhydrous atom, are all matters of indifference. The sole regulating particular is the extent of condensation per decigallon expressed in septems. *Every septem of condensation per decigallon, effected in any solution, increases the specific gravity by* ·001 (water being = 1·000).

It is easy to test the accuracy of this principle by facts. If we divide the differences given in Table III. by ·001, we find the number of septems that correspond to the condensation per decigallon in each solution. And if the condensation per decigallon is divided by the number of test atoms known to be present in a decigallon of each solution, the product shows the amount of condensation effected by each test atom, and that product corresponds with the number given in column 6 of Table II.

The following calculation, relating to acid of 10 atoms or 1000° of strength, also substantiates this principle.

To . . . . . ·035252 = fraction indicating the mean specific gravity of 100° of acid,
Add . . . . ·012698 = condensation per atom in solutions of 1000° of acid: see Table III.
·047950
Multiply by 10 = number of test atoms of acid per decigallon.
Gives . . . ·479500
Add . . . . 1·000000 = specific gravity of water.
Result . . . 1·479500 = real specific gravity of acid of 1000°.

I hold these results to prove that anhydrous sulphuric acid has no fixed atomic volume in solution. There is, nevertheless, a means of indicating the volume of an atom of anhydrous acid in solution, which is free from fallacy, and well-adapted to practical purposes,—I allude to that exhibited in column 7 of Table II. The numbers in this column are all chemically equivalent, and show how many measures of any strong acid must be taken to produce, by dilution with water, the atomic measure of another weaker acid. Thus, 47·437 septems, the atomic measure of SO^3, H^2O, diluted with water to the bulk of 58·276 septems, produces the hydrate SO^3, $2H^2O$; or diluted to the bulk of 73·403 septems, it produces the hydrate $SO^3 + 3H^2O$; or diluted to the bulk of 1000 septems, it produces acid of 100°. The condensation that occurs in these dilutions has been estimated and thrown out, so that we deal only with the practical results. The *atomic measure of a solution* is therefore a definable manageable quantity.

The opinion entertained by Dalton, that anhydrous salts dissolve in water without adding to its bulk, is singularly inapplicable to sulphuric acid. Half an atom of water, measuring 8·035 septems, saturated with SO^3, so as to form the fuming acid of 2381°, acquires the volume of 41·999 septems, or 1000 septems of water produce 5227 septems of fuming acid. Thus an anhydrous substance can increase the volume of its solvent water from 8 to 42, or from 1 to $5\frac{1}{4}$, the temperature remaining constant. I shall show, in treating of soda, a result entirely different from this, and yet equally at variance with the hypothesis of Dalton.

The hydrate of sulphuric acid that distils without change in composition is the protohydrate, $SO^3 + H^2O$. Hence the fuming acid, $SO^3, H^2O + SO^3$, cannot be prepared by the concentration of dilute acids.

Table IV.—*Muriatic Acid.*

$H^2Cl^2 = 455{\cdot}13$ grs. Temperature 62° F.

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of H^2Cl^2 in 1000 septems.	Atoms of water to 1 H^2Cl^2.	1 H^2Cl^2.		Septems of solution containing 1 H^2Cl^2.
				Occupies: Septems.	Condenses: Septems.	
F	1·27	19·533	0·0	51·20	0·0	51·196
G	1·6963	26·089	0·0	38·33	0·0	38·350
D	1·2109	7·9021	5·49	38·33	?	126·55
U	1·2	7·5259	5·88	38·44	?	132·87
T	1·203	7·5229	5·91	38·03	0·30	132·93
B	1·192	7·0216	6·52	37·68	0·65	142·42
CU	1·1891	7·00	6·53	38·00	0·33	142·86
CU	1·1662	6·00	8·05	37·32	1·01	166·67
CU	1·1408	5·00	10·15	36·86	1·47	200·00
CU	1·1148	4·00	13·3	36·32	2·01	250·00
M	1·094	3·1969	17·25	35·62	2·71	312·80
CU	1·0878	3·00	18·52	35·75	2·58	333·33
U	1·0599	2·00	28·93	35·07	3·26	500·00
CU	1·0307	1·00	60·1	34·32	4·01	1000·0
CU	1·0157	·50	122·4	33·62	4·71	2000·0
CU	1·0079	·25	246·8	33·42	4·91	4000·0
CU	1·0032	·10	620·2	33·02	5·31	10000·
U	1·002	·06288	987·	33·21	5·12	15904·
CU	1·0016	·05	1242·	33·30	5·03	20000·
CU	1·0013	·04	1554·	32·52	5·81	25000·
CU	1·001	·03	2072·	31·72	6·61	33333·
CU	1·0006	·02	3109·	35·02	3·31	50000·
CU	1·0003	·01	6221·	35·02	3·31	100000·
1	2	3	4	5	6	7

Authorities.—F = Faraday. This line refers to the gaseous acid condensed to the liquid form by the pressure of 40 atmospheres at 50° Fahr. G = Griffin. This line shows the measure of muriatic acid on the supposition that it could exist in an uncombined state at 62° F. and 30 inches Bar. in the same volume that it occupies in the solution of 7·9021, sp. gr. 1·2109. B = Berzelius. D = Davy. M = Mitscherlich, after Dalton. T = Thomson. U = Ure. CU = calculated from the nearest numbers in Ure's table.

Observations on the Table of Muriatic Acid.

Muriatic acid has only one-third part of the solubility of sulphuric acid. A saturated solution of the latter contains about 24 test atoms, that of the former only about 8 test atoms per decigallon. The condensing power of muriatic acid is only a tenth part of that of sulphuric acid, and though always progressing with dilution, yet when the acid forms the hydrate containing 6000 atoms of water, where the dilution is about 800 fold, the entire condensation does not amount to 10 septems. The atomic measure at the highest is about $2\frac{1}{3}$ times that of water, and at the lowest is not less than twice that of water.

The limited solubility of certain substances in water has been sometimes ascribed to the action of cohesion, but it is difficult to conceive in what manner cohesion can limit the solubility of muriatic acid gas. The two upper lines in Table IV. exhibit a singular illustration of the effects of cohesion. The compression of 40 atmospheres at 50° F. is required to reduce muriatic acid gas to a liquid of sp. gr. 1·27, in which condition the test atom measures 51·2 septems, and the decigallon contains $19\frac{1}{2}$ test atoms. The compressive power of water acting on the gas at about the same temperature, and under the ordinary pressure of the atmosphere, reduces the anhydrous acid to sp. gr. 1·6963, in which condition its test atom measures only 38·33 septems, and the decigallon contains 26 test atoms. We may judge from this fact that the limited solubility of muriatic acid in water results, not from the cohesion of its particles, but from a quality the very opposite to cohesion.

The fraction which $+1$ indicates the mean specific gravity of 100° of muriatic acid is $(·6963 \div 26·089 =)$ ·026688.

TABLE V. Showing the increase of Specific Gravity occasioned in Solutions of H^2Cl^2 by condensation.

Atoms of H^2Cl^2.	Increase in spec. grav.	Atoms of H^2Cl^3.	Increase in spec. grav.	Atoms of H^2Cl^2.	Increase in spec. grav.
7·00	·002284	4·00	·008048	2·00	·006524
6·00	·006072	3·1969	·008680	1·00	·004012
5·00	·007360	3·00	·007736	0·10	·000531

The point of greatest condensation in muriatic acid is at that hydrate which distils without alteration in constitution. Mitscherlich (perhaps after Dalton) quotes this hydrate as sp. gr. 1·094, composition $= H^2Cl^2 + 17\frac{1}{4} H^2O$. Professor Clark states the specific gravity to be 1·110, and its composition $= H^2Cl^2 + 16\frac{2}{3} H^2O$. It may possibly be $H^2Cl^2 + 16 H^2O$. The hydrate, where the greatest density is united with the greatest chemical strength, has 5½ atoms of water, or one-third of the quantity which belongs to the hydrate that sustains the greatest amount of condensation.

Messrs. Playfair and Joule (Phil. Mag., vol. xxvii. pp. 495, 521, 527) are of opinion that the atomic measure of muriatic acid is, in concentrated solutions, equal to twice the atomic volume of water (p. 521); but that in dilute solutions it is sometimes twice (p. 495) and sometimes only once (pp. 521, 527) the atomic volume of water. They do not specify the point at which they conceive the measure to change suddenly from 2 volumes to 1 volume.

TABLE VI.—*Nitric Acid.*

$N^2O^5 = 677{\cdot}036$ grs. Temperature 62° F.

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of N^2O^5 in 1000 septems.	Atoms of water to 1 N^2O^5.	1 N^2O^5 Occupies: Septems.	1 N^2O^5 Condenses: Septems.	Septems of solution containing 1 N^2O^5.
N^2O^5	1·6651	17·216	0·0	58·09	0·0	58·087
G	1·521	13·485	1·0	58·09	?	74·154
CU	1·5091	13·00	1·2	57·56	0·53	76·923
CU	1·4948	12·00	1·73	55·49	2·60	83·333
G	1·485	11·524	2·0	54·64	3·45	86·772
CU	1·4746	11·00	2·32	53·57	4·52	90·909
CU	1·452	10·019	3·0	51·60	6·49	99·812
CU	1·4518	10·00	3·01	51·54	6·55	100·00
CU	1·4256	9·00	3·84	49·43	8·66	111·11
G	1·42	8·8203	4·0	49·10	8·99	113·37
CU	1·3962	8·00	4·84	47·19	10·90	125·00
CU	1·4	7·9069	5·0	46·13	11·96	126·47
CU	1·3602	7·00	6·07	45·26	12·83	142·86
CU	1·322	6·00	7·69	43·05	15·04	166·67
T	1·322	5·8579	8·0	42·16	15·93	170·71
CU	1·2792	5·00	9·9	40·88	17·21	200·00
T	1·2844	4·9799	10·0	40·12	17·97	200·81
CU	1·2305	4·00	13·12	39·09	19·00	250·00
T	1·2012	3·5484	15·0	40·78	17·31	281·82
CU	1·1766	3·00	18·39	37·85	20·24	333·33
CU	1·1207	2·00	28·85	36·37	21·72	500·00
CU	1·0619	1·00	60·06	34·82	23·27	1000·0
CU	1·0314	·50	122·3	33·92	24·17	2000·0
CU	1·0158	·25	246·8	33·52	24·57	4000·0
CU	1·0064	·10	620·2	32·72	25·37	10000·
CU	1·0032	·05	1242·	32·72	25·37	20000·
CU	1·0026	·04	1554·	31·71	26·38	25000·
CU	1·002	·03	2072·	30·05	28·04	33333·
CU	1·0013	·02	3109·	31·72	26·37	50000·
CU	1·0007	·01	6221·	26·72	31·37	100000·
1	2	3	4	5	6	7

AUTHORITIES.—G = Graham. T = Thomson. CU = Calculated after the nearest numbers in Ure's table. The specific gravity of the anhydrous acid is calculated on the supposition that it could exist in a free state at 62° F., under the same volume that it occupies in the protohydrate.

Observations on the Table of Nitric Acid.

The greatest chemical strength of nitric acid in solution is 1348½°. This constitutes the protohydrate of sp. gr. 1·521. The volume of a test atom of acid, as it exists in this hydrate, is 58·09 septems. The condensation which occurs in diluted solutions extends to 31 septems per test atom in a solution of 1°, where the dilution is 1348 fold. Both as respects solubility and power of condensation, nitric acid stands between sulphuric acid and muriatic acid.

The fraction which + 1 indicates the mean specific gravity of 100° of nitric acid is (·6651 ÷ 17·216 =) ·038634.

TABLE VII. Showing the increase of Specific Gravity occasioned in Solutions of N^2O^5 by condensation.

Atoms of N^2O^5.	Increase in spec. grav.	Atoms of N^2O^5.	Increase in spec. grav.	Atoms of N^2O^5.	Increase in spec. grav.
13·00	·006858	8·8203	·079228	4·00	·075964
12·00	·031192	8·00	·087128	3·00	·060698
11·00	·049626	7·00	·089762	2·00	·043432
10·00	·065460	6·00	·090196	1·00	·023266
9·00	·077894	5·00	·086030	0·01	·000314

The point of greatest density in the hydrates of nitric acid coincides with that of greatest chemical strength, namely, in the protohydrate. The hydrate that distils without alteration in constitution is $N^2O^5, 4H^2O$. The hydrate that sustains the greatest amount of condensation is $N^2O^5, 8H^2O$.

Messrs. Playfair and Joule's account of the atomic volume of nitric acid is as follows (Phil. Mag., vol. xxvii. p. 521):—"Nitrate of water in the acid of sp. gr. 1·42 seems to affect four volumes. . . . . Nitrate of water calculated on weak acids has three volumes; but there being no good fixed point upon which to make the calculation, we must leave at present this point undetermined."

This estimate of the atomic measure of the acid in solution of sp. gr. 1·42 is nearly correct. Four volumes for the hydrate is three volumes for the anhydrous acid, and 3 × 16·07 = 48·21, while the number given in Table VI. is 49·10 septems. Three volumes is also correct for the hydrate in a *single* weak solution; for 16·07 × 2 = 32·14 septems is nearly the atomic measure of the anhydrous acid in a solution of 4°. But there is no sudden fall in the atomic volume from 4 to 3, nor do these numbers apply to any solutions save the two that are named specially. They are only separate links of a long chain, whose parts are all of different magnitudes.

TABLE VIII.—*Acetic Acid.*

$C^4 H^6 O^3 = 643 \cdot 19$ grs. Temperature 62° F.

Specific gravity of the solution.	Atoms of $C^4 H^6 O^3$ in 1000 septems.	Atoms of water to 1 $C^4 H^6 O^3$.	1 $C_4 H_6 O_3$		Septems of solution containing 1 $C_4 H_6 O^3$.
			Occupies: Septems.	Condenses: Septems.	
1·067645	11·62	0·0	86·06		86·063
1·057	9·79	1·000	86·06	?	102·14
1·058286	9·55	1·178	85·78	0·28	104·71
1·068800	9·00	1·672	84·24	1·82	111·11
1·068857	9·00	1·673	84·23	1·83	111·11
1·071043	8·50	2·124	83·52	2·54	117·65
1·073657	8·20	2·430	82·90	3·16	121·95
1·074428	8·00	2·640	82·58	3·48	125·00
1·073971	7·50	3·193	82·02	4·04	133·33
1·073286	7·00	3·823	81·42	4·64	142·86
1·069029	6·00	5·370	80·38	5·68	166·67
1·063800	5·00	7·521	79·12	6·94	200·00
1·056386	4·00	10·72	77·79	8·27	250·00
1·044043	3·00	15·94	77·20	8·86	333·33
1·031514	2·00	26·38	76·13	9·93	500·00
1·016343	1·00	57·53	75·54	10·52	1000·0
1·015029	·90	64·46	75·19	10·87	1111·1
1·013714	·80	73·13	74·74	11·32	1250·0
1·012114	·70	84·26	74·58	11·48	1428·6
1·010314	·60	99·07	74·69	11·37	1666·7
1·008629	·50	119·8	74·63	11·43	2000·0
1·007057	·40	150·9	74·24	11·82	2500·0
1·005029	·30	202·7	75·12	10·94	3333·3
1·003429	·20	306·5	74·74	11·32	5000·0
1·002743	·15	410·3	73·60	12·46	6666·7
1·001943	·10	617·8	72·46	13·60	10000·
1·001771	·09	686·9	72·20	13·86	11111·
1·001600	·08	773·4	71·88	14·18	12500·
1·001457	·07	884·6	71·07	14·99	14286·
1·001286	·06	1033·	70·46	15·60	16667·
1·001129	·05	1240·	69·31	16·75	20000·
1·000914	·04	1551·	69·03	17·03	25000·
1·000700	·03	2070·	68·55	17·51	33333·
1·000529	·02	3107·	65·46	20·60	50000·
1·000286	·01	6219·	63·31	22·75	100000·
2	3	4	5	6	7

Observations on the Table of Acetic Acid.

The whole of this table, with the exception of the first two lines, is calculated from my experiments. The acid on which the experiments were made was prepared for me by Mr. Charles Glassford, chemist to Messrs. Turnbull and Co., Camlachie. It was very pure.

Upon crystallizing a quantity of this acid and pouring off the mother-liquor, I found the latter to have a strength of 910°. The crystals were melted, the solution recrystallized, the crystals pounded, the mother-liquor drained off, the crystals remelted, recrystallized, abstracted once more from the mother-liquor, and finally brought into solution in a closed bottle at 62° F., and not heated higher than that point. The specific gravity of the solution thus procured was 1·058286, and its chemical strength 955°, which gives the formula $C^4 H^6 O^3 + 1{\cdot}178\, H^2 O$, showing nearly one-fifth part of an atom more water than belongs to the protohydrate. Repeated trials brought me no nearer to the protohydrate, which I imagine is partly owing to the difficulty of separating the mother-liquor entirely from the crystals. Van der Toorn (Report of the Fourth Meeting of the British Association, Edin. 1834) states the specific gravity of the protohydrate at 59° F. to be 1·057, which makes its strength to be 979°. This is possibly correct, and I have inserted it in the Table, in company with the specific gravity of the anhydrous acid, calculated from Van der Toorn's number. Of course the anhydrous acid is hypothetical.

The weaker solutions of acetic acid were prepared by dilution of the strong acid in stoppered graduated glass cylinders. The quantity of acid contained in each diluted solution was determined by neutralization with weak ammonia of known strength, applied by means of a pouret; the mixing being effected in a stoppered glass bottle, and solution of litmus being employed to test the point of neutralization. All the adjustments, measurements and weighings were effected at 62° F.

The fraction which + 1 indicates the mean specific gravity of 100° of acetic acid is (·067645 ÷ 11·6194 =) ·005822.

TABLE IX. Showing the increase of Specific Gravity occasioned in Solutions of $C^4 H^6 O^8$ by condensation.

Atoms of $C^4 H^6 O^3$.	Increase in spec. grav.	Atoms of $C^4 H^6 O^3$.	Increase in spec. grav.	Atoms of $C^4 H^6 O^3$.	Increase in spec. grav.
9·55	·002686	6·00	·034097	2·00	·019870
9·00	·016402	5·00	·034690	1·00	·010521
8·00	·027852	4·00	·033098	0·10	·001361
7·00	·032532	3·00	·026577	0·01	·000228

In acetic acid, the greatest chemical strength, the greatest density and the greatest condensation characterize three different hydrates. None of the mineral acids possess this singular divergence of properties. The greatest chemical strength is the property of the protohydrate of 979° (Van der Toorn), or according to my experiments at 62° F., of the solution of 955°. The greatest density is at about 800°, where the hydrate has nearly 3 atoms of water. The greatest condensation occurs at about 500°, where the hydrate has about $7\frac{1}{2}$ atoms of water.

The measure of the test atom of acetic acid is 86·06 septems. It is therefore much larger than the measure of any of the mineral acids, while its specific gravity is greatly below that of any of them. The peculiarities of acetic acid depend upon these two properties.

Acetic acid of 979° has a specific gravity of 1·057. The addition of as much water as reduces its strength to 800° increases its specific gravity to 1·074428. The addition of as much more water as reduces its strength to 400° brings down its specific gravity to 1·056386, which is nearly the same as the specific gravity of the strongest solution of 979°. These singular properties admit of a simple explanation.

The density of anhydrous acetic acid being not much greater than that of water, little change in the specific gravity of its solutions is produced by simply replacing a proportion of one constituent by the other; yet, if no condensation occurred, the density of all the solutions would no doubt run in regular progression from the density of the strongest acid, 1·057, to the density of water, 1·000. But in fact, the specific gravity rises from 1·057 to nearly 1·075, and then descends towards 1·000. This irregularity results entirely from the condensation that occurs in the formation of the series of hydrates. So great is this condensation, in proportion to the density of the acid, that many of the hydrates are rendered by it not only denser than the protohydrate, but denser than even the anhydrous acid itself. The mean density of acid of 800° is 1·046576 $[=(\cdot 005822 \times 8)+1]$. When we add to this the effect of condensation, which is ·027852 $(=3\cdot 4815 \times 8)$, we obtain the actual specific gravity of the solution, 1·074428. The mean density of acid of 500° is 1·029110 $[=(\cdot 005822 \times 5)+1]$, which, added to the density occasioned by condensation, ·034690 $(= 6\cdot 94 \times 5)$, gives the real specific gravity of acid of that strength, 1·063800. Hence the laws which regulate the specific gravities of solutions rule in the same way over acetic acid that they do over sulphuric acid. The singular rise and fall in the specific gravity of solutions of acetic acid are due to the existence of no extraordinary property in that acid, but to the regular operation of a simple universal principle that acts alike upon all substances whatever.

Dalton's hypothesis, that organic substances on being dissolved in water increase its bulk only by the amount of water pre-existing in them, or by the oxygen and hydrogen which they contain, is at variance with the properties of anhydrous acetic acid. According to that hypothesis, the measure of the test atom of this acid in solution should be equal to that of 3 atoms of water = 48·21 septems (= 16·07 × 3), whereas the measure actually varies in different solutions from 86·06 septems to 63·31 septems. It follows, that Dalton's hypothesis can never be used in the analytical examination of organic substances of unknown composition.

[To be continued.]

XLVII. *On the Discovery of Footmarks in the Greensand of the Isle of Wight.* *By* S. M. Saxby*.

UNDER the impression that footmarks of *extinct* animals have not before been found in any part of the world at a higher stratum level than that of the new red sandstone, I think it may be interesting to communicate to your readers, that some time since, while examining the fallen blocks of crag and freestone on the shore at Ventnor, I discovered distinct impressions of the feet of animals of various kinds, strongly indented in the *flinty blue rag which forms the bed of the freestone.* With the exception of the marks left by the crustaceans on the oolite near Bath, I am not aware that within the range of 3000 feet (which may be the average depth between the freestone and the new red sandstone) any traces of animal footmarks have been found. After thoroughly searching the locality, I found and removed to my residence the blocks described as under.

Block 1 bears a good and interesting impression of the feet of a three-toed creature, very similar in its general character to the *Ornithichnites giganteus*, as figured by Dr. Buckland in the Bridgewater Treatise, plate 26^{b}; differing however in the angles subtended by the toes, which in the above plate measure about 33°, while in this the angle exceeds 50°; and the mark of a JOINT in the middle toe in this specimen is much more strongly given; and indeed the impression altogether *cannot be mistaken* for an *accidental* resemblance to the foot-print of an animal. On the same block there are other marks, which, although evidently those of animals, are not such as I will venture to describe. The length of the above three-toed impression is about 7 inches. There is some indication of a hind-toe.

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* Communicated by the Author.

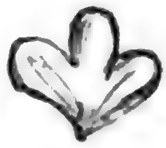
Block 2 is a large block, weighing 3½ cwt., and has confused impressions, apparently of feet of a large three-toed creature, which has trodden more than once on the same mark. There is also a very plain footmark of a triangular form about 2 inches broad, apparently of an animal whose foot has three soft prominences.

Block 3 bears the deep impression of apparently a web-footed creature, having three toes set somewhat obliquely; the right toe being much the thickest, but shortest. All three toes had claws, and the angles between them measure about 20° and 35° respectively, the larger angle being between the middle toe and the short thick toe. The extreme length from the middle toe to the tapered "heel" is 2¼ inches. There are other not easily described impressions on this stone also.

Block 4 contains a deep impression of the footstep of a quadruped, not much unlike that which would have been left by a large dog. It measures in length 2¾ inches and in breadth 2¼ inches.

Block 5 has an impression something similar to the one on block 4.

Block 6 shows the footmark of a three-toed animal (toes broad and short, and making angles of about 50° with each other), and from the sharpness of the narrowed heel may be compared to a fleur-de-lis.



The above blocks have been detached from the superposed freestone, by their having fallen from the cliff during a landslip; and having lodged below high-water mark, the constant action of the surge and shingle, perhaps for centuries, has denuded the ancient surface of the rag, leaving however portions of the freestone in the footmarks above-described.

It is extremely difficult to separate the lower portion of the freestone from the rag; they in general are so gradually blended, that it is not easy, in cutting, to ascertain *which really is the surface* of the rag. My attempts to clear other portions of the bed, when aided by experienced masons, were a complete failure.

On many surfaces of the ragstone, which have been cleaned by the action of the waves, vast quantities of gigantic mollusks are visible; and indeed I at first mistook the confused markings on Block 2 for the remains of a "lobated Siphonia," until

I noticed the *risings of the once soft mass*, round the impressions, indicating with certainty that some *weight* had caused the *deep* markings alluded to.

Belle Vue, Ventnor, Isle of Wight, S. M. S.
February 20, 1846.

XLVIII. *On Nitraniline, a new Product of Decomposition of Dinitrobenzole.* *By* JAMES SHERIDAN MUSPRATT, *Ph.D.* *and* AUGUSTUS WILLIAM HOFMANN, *Ph.D.*[*]

A GREAT number of investigations have proved that the action of nitric acid upon organic bodies produces changes in their composition in two ways. First, there is sometimes simply an accession of oxygen to the elements of the organic matter, the nitric acid being reduced to nitric oxide or nitrous acid and expelled. Thus, by treating *indigo* with nitric acid it becomes converted into *isatine*, the composition of which differs from that of indigo only by containing two equivalents of oxygen more than the latter. *Cinnamic acid* passes into *hydride of benzoyle*, water being formed, and the excess of carbon, combining with the oxygen of the nitric acid, escapes in the form of carbonic acid. By a further supply of oxygen the hydride of benzoyle is converted into *benzoic acid.* In all other cases the action of nitric acid differs in no respects from that of other means of oxidation, *ex. gr.* chromic acid, peroxide of manganese and sulphuric acid, &c.

The action of nitric acid however upon organic matters is in other cases more complex; it does not consist solely in the supply of oxygen, but besides that element, nitrogen also enters into the newly-formed compound. *Indigo,* subjected for a long time to the action of concentrated nitric acid, undergoes a series of metamorphoses, and the last product which is formed, namely, *carbazotic acid* (nitrophenisic acid), contains to the same quantity of carbon a much larger proportion of nitrogen than the indigo from which it is derived.

Cinnamic acid, hydride of benzoyle, and *benzoic acid,* none of which contain nitrogen, furnish, as the last product of the action of nitric acid, a nitrogenous acid, which is known as *nitrobenzoic* (*nitrobenzinic*) *acid.*

A great many of the nitrogenous compounds produced in this manner possess the properties of acids. Besides the abovementioned we might enumerate *nitrocinnamic acid, nitrosalicylic acid* (anilic acid), and many others similarly constituted.

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[*] Communicated by the Chemical Society; having been read March 16, 1846.

Some chemists have on this account, without hesitation, assumed the presence of nitric acid in these bodies. Berzelius represents the carbazotic acid as a copulated compound of hydrated nitric acid with a sort of saline body, in which another equivalent of nitric acid is combined with an organic oxide having the formula

$$C_{12}\,H_2\,NO_3.$$

Carbazotic acid is therefore a copulated acid of similar constitution with the sulphovinic acid (bisulphate of oxide of ethyle), and would be represented as follows:—

$$C_{12}\,H_2\,NO_3,\,NO_5 + HO,\,NO_5.$$

Also, in the other acids of this nature, which contain only *one* equivalent of nitrogen, Berzelius assumes the presence of nitric acid. According to his view, nitrobenzoic and nitrosalicylic acids would be represented as follows:—

Nitrobenzoic acid . . $C_{14}\,H_4\,O_2 + HO,\,NO_5$
Nitrosalicylic acid . . $C_{14}\,H_8\,O_4 + HO,\,NO_5$.

There is no direct proof for this assumption, and therefore other chemists believe that the nitrogen in these compounds does not exist as nitric acid but as nitrous acid, NO_3; thus nitrobenzoic acid, according to Mulder, is $C_{14}\,H_4\,O_4,\,HO,\,NO_3$. The compounds produced by the contemporaneous action of nitrogen and oxygen upon organic bodies are however not always acid; a great class of bodies furnish under such circumstances products in which the acid properties of the nitric acid have entirely disappeared: to these belong a number of organic bodies, which consist only of carbon and hydrogen. For example, if *benzole* is brought into contact with fuming nitric acid, they combine with separation of water and form an indifferent body, *i. e.* nitrobenzole (nitrobenzide) discovered by Mitscherlich.

Naphthalole (naphthaline), *toluole*, and a considerable number of carbides of hydrogen, behave in a like manner.

In which form is the nitrogen contained in these bodies, as, for instance, in nitrobenzole?

The perfect neutrality of this substance, its ether-like taste and odour, induced some chemists to suppose that its elements might be similarly arranged to those of the compound ethers. Nitrobenzole, according to this view, may be considered as a compound of an organic oxide with nitrous acid, corresponding to nitrous ether.

Nitrite of oxide of ethyle $C_4\,H_5\,O,\,NO_3$, nitrous ether.
Nitrite of oxide of benzide $C_{12}\,H_5\,O,\,NO_3$, nitrobenzole.

These views, which appear at the first glance very reasonable, lose however by a closer examination a considerable portion of their probability. Independent of the fact that oxide of benzide is merely an hypothetical compound while we know the oxide of ethyle in an isolated state, the behaviour of these two bodies towards potassa is sufficient to show a decided dissimilarity in their constitution. Nitrous ether, like all the other compound ethers, is decomposed by potassa, giving rise to nitrite of potassa and alcohol (Liebig). Nitrobenzole, when similarly treated, yields no acid of nitrogen; an aqueous solution of potassa leaves this body quite intact, and when employed in alcoholic solution, the latter is oxidized and *azobenzole* (*azobenzide*), *i. e.* nitrobenzole minus oxygen, is formed[*]. We see here that all analogy between nitrous ether and nitrobenzole disappears entirely.

Nitrobenzole undergoes still another change, which does not in any way agree with the benzide theory. According to Deville[†], this body, when repeatedly treated with fuming nitric acid, yields a crystalline substance, to which he has assigned the name binitrobenzole (binitrobenzide). This body[‡], according to the theory in question, was considered to be a compound of 2 equivalents of nitrous acid with a higher oxide of benzide, *i. e.* that its constitution must be according to the formula $C_{12} H_5 O_2, 2NO_3$. That his view is not tenable, without mentioning other objections to it, is evident, inasmuch as the above formula does not express the composition of the new compound; for, according to Deville's analysis, we find that instead of five it only contains four equivalents of hydrogen.

Recently certain French chemists, Laurent, for example, have represented these bodies under a different aspect. A great number of investigations had led to the conclusion that the hydrogen of many compounds may be wholly or partly replaced by a corresponding number of equivalents of chlorine and bromine without altering the fundamental properties. This law had been established for a number of acids and indifferent bodies, and an investigation published some months since[§] by one of us has proved that the same law extends also to bodies possessing basic properties, which at first sight appeared improbable.

The same view may be applied to many products which have assumed nitrogen and oxygen from nitric acid. These compounds may indeed be regarded as simple products of

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[*] *Ann. der Chemie und Pharm.*, vol. liii. p. 28.

[†] *Ann. de Chem. et de Phys.*, 3 ser., t. iii. p. 157.

[‡] Berzelius, *Jahresbericht*, 1843.

[§] Phil. Mag., vol. xxvi. p. 385.

substitution, in which the hydrogen, instead of being replaced by chlorine or bromine, is replaced by a compound radical, namely by hyponitric acid. Whilst in the assumption of chlorine into an organic body, the replaced hydrogen is taken up by another equivalent of chlorine, forming hydrochloric acid; in the employment of nitric acid, the fifth equivalent of oxygen combines with the replaced hydrogen, forming water. Benzoic acid, $HO, C_{14} H_5 O_3$, furnishes, when treated with chlorine, an acid,

$$HO, C_{14} \left\{ \begin{matrix} H_4 \\ Cl \end{matrix} \right\} O_3,$$

which can scarcely be distinguished from the original compound.

Moreover, nitrobenzoic acid, which is produced by the action of nitric acid upon benzoic acid, has been in many cases confounded with benzoic acid. The constitution of this acid, according to the theory of substitution, is

$$HO, C_{14} \left\{ \begin{matrix} H_4 \\ NO_4 \end{matrix} \right\} O_3;$$

and it therefore may be regarded as strictly analogous to chlorobenzoic acid.

In a similar way we have chloro-, bromo- and nitro-salicylic acid.

Salicylic acid . . .	$HO, C_{14} H_5 O_5$
Chlorosalicylic acid .	$HO, C_{14} \left\{ \begin{matrix} H_4 \\ Cl \end{matrix} \right\} O_5$
Bromosalicylic acid .	$HO, C_{14} \left\{ \begin{matrix} H_4 \\ Br \end{matrix} \right\} O_5$
Nitrosalicylic acid .	$HO, C_{14} \left\{ \begin{matrix} H_4 \\ NO_4 \end{matrix} \right\} O_5.$

Carbazotic acid contains not less than 3 equivalents of nitrogen; now if we suppose the whole of this element to exist in the form of hyponitric acid, its empirical formula, $HO, C_{12} H_2 N_3 O_{13}$, may be theoretically represented by the following:—

$$HO, C_{12} \left\{ \begin{matrix} H_2 \\ 3NO_4 \end{matrix} \right\} O,$$

and thus it would be a simple product of substitution of the compound $HO, C_{12} H_5 O$, which was discovered by Runge in coal-gas naphtha, described by him under the designation of carbolic acid, the same body which was subsequently analysed by Laurent, and termed by him hydrate of phenyle. In fact this substance loses under the influence of chlorine, bromine and nitric acid, one equivalent of hydrogen after another, into the place of which a corresponding number of equivalents of the elements or the compound NO_4 enter. We obtain in this way a series of products of substitution, the last member of which, on the one side, is chlorophenusic acid, and on the other carbazotic acid.

$$\left.\begin{matrix}\text{Hydrate of Phenyle}\\ \text{Phenole}\end{matrix}\right\} HO,\ C_{12}\, H_5\, O.$$

$$\left.\begin{matrix}\text{Bromophenasic acid}\\ \text{Bromophenole}\end{matrix}\right\} HO,\ C_{12}\left\{\begin{matrix}H_4\\ Br\end{matrix}\right\}O.$$

$$\left.\begin{matrix}\text{Chlorophenesic acid}\\ \text{Dichlorophenole}\end{matrix}\right\} HO,\ C_{12}\left\{\begin{matrix}H_3\\ Cl_2\end{matrix}\right\}O.$$

$$\left.\begin{matrix}\text{Chlorophenisic acid}\\ \text{Trichlorophenole}\end{matrix}\right\} HO,\ C_{12}\left\{\begin{matrix}H_2\\ Cl_3\end{matrix}\right\}O.$$

$$\left.\begin{matrix}\text{Chlorophenusic acid}\\ \text{Pentachlorophenole}\end{matrix}\right\} HO,\ C_{12}\, Cl_5\, O$$

$$\left.\begin{matrix}\text{Nitrophenesic acid}\\ \text{Dinitrophenole}\end{matrix}\right\} HO,\ C_{12}\left\{\begin{matrix}H_3\\ 2NO_4\end{matrix}\right\}O.$$

$$\left.\begin{matrix}\text{Nitrophenisic acid}\\ \text{Trinitrophenole}\\ \text{Carbazotic acid}\end{matrix}\right\} HO,\ C_{12}\left\{\begin{matrix}H_2\\ 3NO_4\end{matrix}\right\}O.$$

The same view may be taken of the products resulting from the action of nitric acid upon benzole.

Nitrobenzole is simply benzole, in which 1 equivalent of hydrogen is replaced by hyponitric acid, and in dinitrobenzole 2 equivalents of hydrogen are replaced, as the name implies.

$$\text{Benzole . . .}\quad C_{12}\, H_6.$$

$$\text{Nitrobenzole .}\quad C_{12}\left\{\begin{matrix}H_5\\ NO_4.\end{matrix}\right.$$

$$\text{Dinitrobenzole.}\quad C_{12}\left\{\begin{matrix}H_4\\ 2NO_4.\end{matrix}\right.$$

The chemical character of these three compounds is not essentially different.

As the substitution of chlorine and bromine for the hydrogen of organic bodies has only recently been extended to bases, so until now the replacement of hydrogen by hyponitric acid has been limited to acids and neutral bodies; it therefore appeared to be a point of great interest to ascertain whether indeed NO_4 also could replace hydrogen in basic compounds without destroying their electro-positive character. There have been, in fact, some investigations made in this direction. Laurent lately discovered a new base among the products of distillation of hydrobenzamide, remarkable for its high atomic weight. This base, which may be obtained in fine crystals, and is by Laurent called lophine, is converted, when boiled with nitric acid, into a yellow crystalline powder, which manifestly must be considered a product of substitution of lophine. Laurent's analysis gives the following composition to this base: $C_{46}H_{16}N_2$, and the yellow powder is represented by

$$C_{46}\left\{\begin{matrix}H_{13}\\3NO_4\end{matrix}\right\}N_2.$$

In this body (nitrolophyle, trinitrolophine), according to the view in question, 3 equivalents of hydrogen are replaced by hyponitric acid; but with this interchange lophine has lost all its basic properties, trinitrolophine being an indifferent substance.

In pursuing this track of investigation, we endeavoured to supply the hiatus existing here by finding a basic compound into which NO_4 might enter without affecting its properties. The various ways in which aniline is formed, its characteristic properties, and above all having succeeded in replacing hydrogen by bromine and chlorine in aniline, we deemed this body the best suited to our experiments.

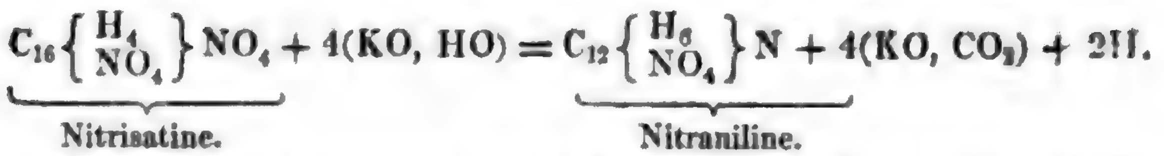
In the first place, we attempted to accomplish our purpose by the direct action of nitric acid on aniline, but we did not in this way succeed: when aniline is mixed with pretty strong nitric acid, a mass of crystals are formed, which are nitrate of aniline. On heating this salt in the presence of free nitric acid it dissolves with violent action and the escape of nitrous fumes, and is, as we know, ultimately converted, on continuing the treatment, into carbazotic acid. Were it possible to obtain nitraniline by this process, it obviously could only be by interrupting the action of nitric acid at the proper time, but after a number of experiments with acid of every variety of strength, and adding water at every stage of the process, we could not succeed; either the aniline separated unchanged, or its molecular constitution underwent an entire change, and the water separated resinous matters rather of an acid than a basic character. We soon abandoned this method, and remembering that the chlorine and bromine bases were produced only indirectly, we sought to accomplish our present purpose in the same manner.

After the fact had been established, that isatine was converted into aniline by fusion with caustic potassa, the production of the chlorine and bromine bases followed as a matter of course; in the same way, if we could succeed in producing nitrisatine,

$$C_{16}\left\{\begin{matrix}H_4\\NO_4\end{matrix}\right\}NO_4,$$

by replacing the hydrogen of isatine with hyponitric acid, the distillation of this compound with potassa might yield nitraniline,

$$\underbrace{C_{16}H_5NO_4}_{\text{Isatine.}} + 4(KO, HO) = \underbrace{C_{12}H_7N}_{\text{Aniline.}} + 4(KO, CO_2) + 2H$$

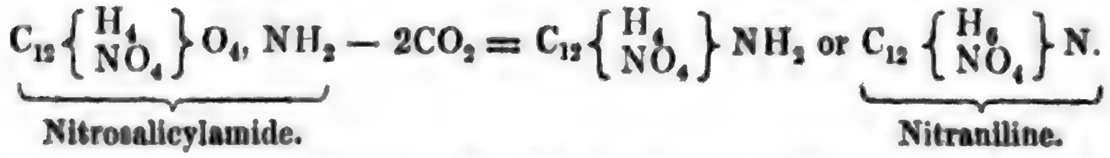


$$\underbrace{C_{16}\left\{\begin{matrix}H_4\\NO_4\end{matrix}\right\}NO_4}_{\text{Nitrisatine.}} + 4(KO, HO) = \underbrace{C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N}_{\text{Nitraniline.}} + 4(KO, CO_2) + 2H.$$

On studying however the action of nitric acid upon isatine, we soon found that the yellow powder which is formed is not simply a substitution product of isatine, but belongs evidently to one of the families of bodies containing less carbon, which proceed from the indigo series. It was therefore obvious that by distilling this compound with hydrate of potassa, we could not effect our purpose.

A third mode for the preparation of nitraniline occurred to us. One of the manifold sources of aniline is the destructive distillation of anthranilic acid, which simply loses 2 equivalents of carbonic acid; isomeric with this acid is salicylamide, recently described by Cahours*. The composition of these two bodies is represented by the formula $C_{14}H_7NO_4$. Now if the salicylamide underwent the same decomposition by heat as anthranilic acid, it might obviously be supposed that the nitraniline would result from the distillation of nitrosalicylamide, a substance also lately prepared by Cahours.

$$\underbrace{C_{14}H_5O_4NH_2}_{\text{Salicylamide.}} - 2CO_2 = C_{12}H_5NH_2 \text{ or } \underbrace{C_{12}H_7N}_{\text{Aniline.}}$$



$$\underbrace{C_{12}\left\{\begin{matrix}H_4\\NO_4\end{matrix}\right\}O_4, NH_2}_{\text{Nitrosalicylamide.}} - 2CO_2 = C_{12}\left\{\begin{matrix}H_4\\NO_4\end{matrix}\right\}NH_2 \text{ or } \underbrace{C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N.}_{\text{Nitraniline.}}$$

There were some researches made in this direction, the results of which we published at the beginning of last year†.

The exceedingly small amount of aniline, however, which the distillation of salicylamide had yielded (the principal products being hydrate of phenyle and ammonia), could never warrant a similar treatment of nitrosalicylamide, in order to obtain a sufficient quantity of nitraniline for analysis and a complete study of its properties.

There yet remained to us one resource. We have already mentioned a compound which is produced by the continued action of nitric acid upon nitrobenzole, *i. e.* dinitrobenzole,

$$C_{12}\left\{\begin{matrix}H_4\\2NO_4\end{matrix}\right.$$

which we may consider as nitrobenzole, in which a second equivalent of hydrogen is replaced by hyponitric acid. Chemists are well-acquainted with the interesting transformation which nitrobenzole suffers with reducing agents. By assuming

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* *Ann. der Chem. und Pharm.* vol. xl. p. 64.

† Phil. Mag., vol. xxvi. p. 581.

hydrogen and eliminating oxygen, it is perfectly converted into *aniline*.

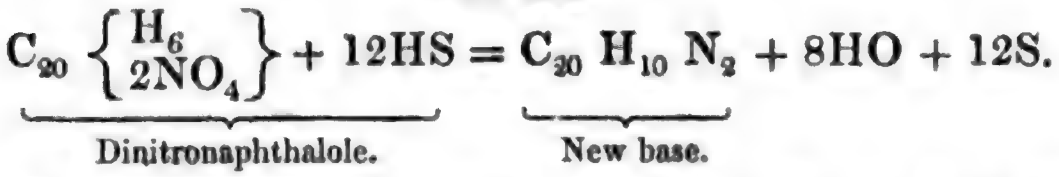
What, we may ask, would be the action of reducing means upon dinitrobenzole? Various changes might be expected. If all the oxygen were eliminated we might form a substance possessing the formula $C_{12} H_4 N_2$; but it was also conceivable that the reduction would extend only to 1 equivalent of hyponitric acid, and that the other equivalent might form part of the new body; in other words, that we might produce nitraniline,

$$C_{12}\left\{\begin{matrix} H_6 \\ NO_4 \end{matrix}\right\}N.$$

We were just about to make the experiment, when some researches of Zinin* were published, on the action of hydrosulphuric acid upon *dinitronaphthalole* (nitronaphthalese) and dinitrobenzole, which appeared very unfavourable to our views.

Zinin found that by the action of hydrosulphuric acid upon dinitronaphthalole, a new base is formed containing no oxygen. By analogy this body should possess the composition—

$$\underbrace{C_{20}\ H_{10}\ N_2}_{\text{New base.}}$$



$$\underbrace{C_{20}\left\{\begin{matrix} H_6 \\ 2NO_4 \end{matrix}\right\}}_{\text{Dinitronaphthalole.}} + 12HS = \underbrace{C_{20}\ H_{10}\ N_2}_{\text{New base.}} + 8HO + 12S.$$

But Zinin found, on analysing the double salt of this base with bichloride of platinum, that its equivalent was only half,

$$\frac{C_{20}\ H_{10}\ N_2}{2} = C_{10}\ H_5\ N,$$

showing that a division had taken place, whence he gave the name seminaphthalidine to the new body.

Seminaphthalidine is a well-characterized organic alkaloid, forming with acids fine crystallizable salts. Zinin obtained by the action of hydrosulphuric acid upon dinitrobenzole the same result, but in this case far less decided. A difficultly crystallizable substance is formed, also possessing a basic nature, which however could not be obtained in the state of purity. An approximative analysis of this substance showed it to be analogous to the seminaphthalidine, *i. e.* $C_6\ H_4\ N$. Zinin himself remarked, that this substance required a closer examination. These researches appeared to cut off all hopes of our ever obtaining nitraniline; nevertheless we considered a continuation of our experiments in every way desirable, so

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* Erdmann's *Journ. für Prakt. Chemie*, vol. xxxiii. p. 29.

as at least to solve the questions that remained unanswered in Zinin's experiments. By doing so we very soon obtained results which had escaped the observation of Zinin, and which brought us to the point we aimed at.

For our research we had to prepare a large quantity of dinitrobenzole. The transformation of nitrobenzole into dinitrobenzole proceeds very slowly, even when it is boiled repeatedly with the strongest nitric acid. It is obtained however very speedily by dropping benzole or nitrobenzole into a mixture composed of equal parts of fuming nitric acid and concentrated sulphuric acid, as long as the liquid remains homogeneous. The liquid is now boiled for a few minutes and then allowed to cool. A thick mass of dinitrobenzole is formed, which by washing with water is freed from all adhering acid. One recrystallization from alcohol furnishes this body in long, shining, needle-shaped prisms of absolute purity.

A combustion of these crystals gave us the following results:—

0·2978 grm. dinitrobenzole gave 0·4728 grm. carbonic acid and 0·0650 grm. water.

These results afford in 100 parts—

Carbon . . .	43·26
Hydrogen . .	2·42

				Calculated.	Found.
12	equivs. of	Carbon . .	900·00	42·77	43·26
4	...	Hydrogen .	50·00	2·37	2·42
2	...	Nitrogen . .	354·00	16·82	
8	...	Oxygen . .	800·00	38·04	
			2104·00	100·00	

Preparation of Nitraniline.

When an alcoholic solution of dinitrobenzole is saturated with ammoniacal gas it assumes a dark red colour, and by then passing a stream of hydrosulphuric acid gas through this solution, a large quantity of crystals of sulphur are deposited. By continuing to pass hydrosulphuric acid gas through the solution until only a small quantity of sulphur precipitates, it will contain very little undecomposed dinitrobenzole. By adding hydrochloric acid to this solution and boiling, a new portion of sulphur, mixed with a trace of dinitrobenzole, is thrown down. The whole is now filtered, and to the filtrate potassa is added, a brown matter separates, which unites and settles in a resinous mass at the bottom of the vessel. We washed this matter with cold water until all the alkali was removed. It dissolved completely in alcohol and ether, imparting to them a reddish-brown colour; hot water dissolved the greater part of it, but even after continued boiling in this solvent small quantities of a brown resin remained undissolved. The hot aqueous solution possessed a fine orange-yellow colour; upon cooling it afforded beautiful yellow crystals about an inch in length, which, when recrystallized from water, were perfectly pure.

Composition of Nitraniline.

The crystalline matter prepared in the process described, possesses all the properties of a true organic base, obviously quite dissimilar to that obtained by Zinin. The physical properties of the two bodies could scarcely be more unlike. Elementary analysis proved immediately the difference of the two substances, and showed that the crystals, in fact, belonged to the body which we had endeavoured to procure in such a variety of ways.

By a combustion of the yellow crystals with oxide of copper the following results were obtained :—

I. 0·3035 grm. of substance gave 0·5865 grm. of carbonic acid and 0·1275 grm. of water.

II. 0·3748 grm. substance gave 0·7119 grm. carbonic acid and 0·1493 grm. water.

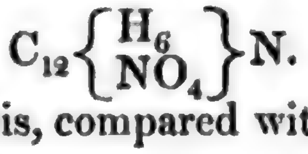
III. The nitrogen was determined by Dumas's method in an atmosphere of carbonic acid.

Results.—0·4260 grm. of substance gave 80 cubic centimetres of moist nitrogen. Barometer $324'''$, Thermometer 16° centig.

These numerical results represent the following per cents.

	I.	II.	III.
Carbon . .	52·70	51·80	
Hydrogen .	4·66	4·42	
Nitrogen . .	...	...	20·52

agreeing exactly with the formula of nitraniline,



$$C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N.$$

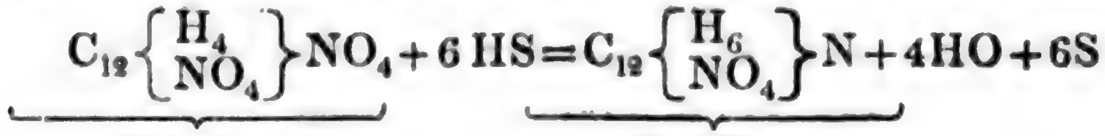
The mean of our analysis, compared with the calculated numbers, stands as follows:—

		Theory.		Mean of the exp.
12 equivs.	Carbon	900·00	52·05	52·25
6 ...	Hydrogen	75·00	4·33	4·54
2 ...	Nitrogen	354·00	20·47	20·52
4 ...	Oxygen	400·00	23·14	
		1729·00	100·00	

In order to control this formula, we prepared the double compound of the new base with bichloride of platinum. On igniting this salt 0·5240 grm. gave 0·1500 grm.=28·62 per cent. of platinum.

Atomic weight from experiment . . 1739·5
Atomic weight, theoretical . . . . 1729·0.

The metamorphosis of dinitrobenzole under the influence of reducing agents is thus perfectly analogous to the transformation of nitrobenzole. In fact the decomposition is very simple, if we regard dinitrobenzole as nitrobenzole, in which one equivalent of hydrogen is replaced by hyponitric acid.



$$\underbrace{C_{12}\left\{\begin{matrix}H_4\\NO_4\end{matrix}\right\}NO_4}_{\text{Dinitrobenzole.}}+6\,HS=\underbrace{C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N}_{\text{Nitraniline.}}+4HO+6S$$

Properties of Nitraniline.

Nitraniline, as we have stated, is obtained from a hot aqueous solution, in the form of long yellow crystals. It is very sparingly soluble in cold water, so that nearly the whole is separated by crystallizing from this solvent. Alcohol and ether also dissolve this base; from the former it crystallizes in silky needles, and from the latter in the same form, but not so well defined. Nitraniline dissolves in all acids, from which potassa separates it in yellow flakes, which under the microscope have a confused, needle-like structure. At the ordinary temperature the base possesses no odour, but when slightly warmed, it evolves a peculiar aromatic odour, which somewhat resembles aniline. Its taste is burning. When the crystals are heated they melt into a deep yellow oil, which passes into a yellowish vapour, condensing in beautiful iridescent plates upon the cold interior surface of the vessel. Nitraniline sublimes without fusing when heated in a water-bath, yielding a sublimate of most beautiful crystals. At a higher temperature the base distils, leaving no residue, and the liquid which passes over solidifies in the receiver or in the neck of the retort into a scaly mass. The boiling-point of the base lies higher than 285° C. (545° Fahr.), and the fusing-point at about 110° C. (230° Fahr.). Its vapour burns with a smoky flame.

Nitraniline is specifically heavier than water; it affords not the slightest reaction with test papers; even rose paper, which is so exceedingly sensitive for alkalies, is not at all affected by it. The properties of nitraniline agree in many respects with those of aniline, chlor- and bromaniline. Like these bases it possesses in a high degree the property of imparting an intense yellow colour to fir-wood. This base also stains the skin in no less degree than carbazotic acid, nitrophenesic acid, &c.; but it does not afford the beautiful reaction with hypochlorite of lime which characterizes aniline.

Nitraniline displaces none of the metallic oxides from their acid compounds; its basic properties are exceedingly weak. Aniline displaces this base from all its salts.

Compounds of Nitraniline.

Although nitraniline is so weak a base, it combines with acids, affording crystallizable salts, which possess the same constitution as the corresponding salts of aniline. All its salts are decidedly acid to test papers. They are decomposed by the caustic and carbonated alkalies, nitraniline separating in a crystalline form. We have investigated the subjoined salts in order to be completely satisfied with respect to the composition and nature of the base.

Hydrochlorate of Nitraniline, $C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N, HCl$.—It is obtained by evaporating a solution of nitraniline in hydrochloric acid. The colour of nitraniline entirely disappears in this solvent, and fine scales are obtained, shining like mother-of-pearl. This salt is extremely soluble in water and alcohol.

Analysis.—0·4055 grm. of salt gave 0·3350 grm. chloride of silver, or 20·37 per cent of chlorine.

				Theory.	Found.
12	equivs.	Carbon . .	900·00	41·23	
7	...	Hydrogen .	87·50	4·00	
2	...	Nitrogen .	354·00	16·22	
4	...	Oxygen . .	400·00	18·32	
1	...	Chlorine .	442·65	20·23	20·37
			2184·15	100·00	

Binoxalate of Nitraniline, $C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N, HO, 2(C_2O_3, HO)$.—This salt is obtained on mixing a solution of nitraniline in alcohol with oxalic acid dissolved in the same solvent. This salt separates in the form of yellowish crystals, which are washed with ether and dried upon a porous tile.

The combustion of this compound with chromate of lead afforded the following numbers:—

0·4985 grm. of salt gave 0·7550 grm. carbonic acid and 0·1790 grm. water. In 100 parts—

Carbon	41·30
Hydrogen . . .	3·99

			Theory.	Found.
16 equivs.	Carbon . .	1200·00	40·45	41·30
9 ...	Hydrogen .	112·50	3·79	3·99
2 ...	Nitrogen .	354·00	11·93	
13 ...	Oxygen . .	1300·00	43·83	
		2966·50	100·00	

The above numbers show the slight excess of carbon which often occurs with the substitution products of hyponitric acid, especially when the combustion is carried on rather quickly. Unfortunately we had no more of the base to prepare this salt again, but there is little doubt but that the analysed salt corresponds with the binoxalate of chloraniline*.

Double Salt of Hydrochlorate of Nitraniline and Bichloride of Platinum, $C_{12}\left\{\begin{matrix} H_6 \\ NO_4 \end{matrix}\right\}N$, HCl, Pt Cl_2.—An aqueous solution of hydrochlorate of nitraniline is not precipitated by deutochloride of platinum; but when an alcoholic solution of the former is mixed with the latter, a yellow crystalline double salt is formed, which is exceedingly soluble in both water and alcohol, and therefore must be washed with ether. We have already alluded to the determination of the platinum in this salt when treating of the atomic weight of the base. We append the calculated composition of this salt along with the platinum per-centage found.

			Theory.	Found.
12 equivs.	Carbon . .	900·00	20·91	
7 ...	Hydrogen .	87·50	2·03	
2 ...	Nitrogen .	354·00	8·23	
4 ...	Oxygen . .	400·00	9·30	
3 ...	Chlorine .	1327·95	30·87	
1 ...	Platinum .	1233·50	28·66	28·62
		4302·95	100·00	

We have not examined the other salts of nitraniline.

Products of the Decomposition of Nitraniline.

We have not as yet made many experiments in this direction, owing to the great difficulty of obtaining the base in sufficient quantity; we can therefore only give a few reactions.

Nitric acid violently decomposes nitraniline, and after continued boiling affords an acid which we have not more closely examined. There is little doubt but that it is carbazotic acid.

When this base is treated with bromine violent action takes place, accompanied by an elevation of temperature and a strong disengagement of hydrobromic acid. After some time

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* Phil. Mag., vol. xxvi. p. 199.

the whole is changed into a brown resinous mass, which crystallizes from alcohol in the form of yellowish needles. This body is insoluble in water and perfectly neutral, combining neither with acids nor alkalies. It is manifestly nitrodibromaniline, corresponding with tribromaniline. We may regard the following as the formula, although as yet we have not analysed the substance:—

$$C_{12}\left\{\begin{matrix}H_4\\NO_4\\Br_2\end{matrix}\right\}N-(?)$$

The production of nitraniline appears to us to be interesting, in reference to the question of substitution, which is still in debate amongst chemists. If we reject that theory, we can scarcely understand the constitution of this base and its relations to aniline.

The group of aniline compounds has thus been again augmented by the discovery of nitraniline. The following is a synopsis of the different members of this family:—

Aniline	$C_{12}\,H_7\,N.$
Chloraniline	$C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N.$
Dichloraniline . . .	$C_{12}\left\{\begin{matrix}H_5\\Cl_2\end{matrix}\right\}N.$
Trichloraniline . . .	$C_{12}\left\{\begin{matrix}H_4\\Cl_3\end{matrix}\right\}N.$
Chlorodibromaniline .	$C_{12}\left\{\begin{matrix}H_4\\Cl\\Br_2\end{matrix}\right\}N.$
Bromaniline	$C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N.$
Dibromaniline . . .	$C_{12}\left\{\begin{matrix}H_5\\Br_2\end{matrix}\right\}N.$
Tribromaniline . . .	$C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N.$
Nitraniline	$C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N.$
Nitrodibromaniline . .	$C_{12}\left\{\begin{matrix}H_4\\NO_4\\Br_2\end{matrix}\right\}N.\,(?)$

In concluding this memoir, we may remark that the body which Zinin obtained by treating dinitrobenzole in the same manner, *i. e.* the compound $C_6\,H_4\,N$, is evidently the last product of the action of the hydrosulphuric acid upon dinitrobenzole, our nitraniline being the first. Zinin's compound will doubtless be procured by the further action of reducing agents upon nitraniline. We shall shortly ascertain this.

$$\underbrace{C_{12}\left\{\begin{matrix}H_6\\ NO_4\end{matrix}\right\}N}_{\text{Nitraniline.}} + 6HS = \underbrace{2C_6H_4N}_{\text{Semianiline,}} + 4HO + 6S.$$

The small quantity of resinous matter which remains behind upon dissolving crude nitraniline in boiling water is probably Zinin's semianiline.

XLIX. *On Quaternions; or on a New System of Imaginaries in Algebra.* *By* Professor Sir WILLIAM ROWAN HAMILTON, *LL.D., Corresponding Member of the Institute of France, and Royal Astronomer of Ireland.*

[Continued from p. 122.]

28. THE known and purely *graphic* property of the cone of the second degree which constitutes the theorem of Pascal, and which expresses the coplanarity of the three lines of meeting of opposite plane faces of an inscribed hexahedral angle, may be transformed into another known but purely *metric* property of the same cone of the second degree, which is a form of the theorem of M. Chasles, respecting the constancy of an anharmonic ratio. This transformation may be effected without difficulty, on the plan of the present paper; for if we multiply into $\mathrm{v}.\gamma\gamma'$ both members of the equation (3.) of the 24th article, and then operate by the characteristic s., attending to the general properties of scalars of products, we find, for *any six vectors* $\alpha\alpha'\beta\beta'\gamma\gamma'$, the formula

$$\mathrm{s}(\mathrm{v}.\alpha\alpha'.\mathrm{v}.\beta\beta'.\mathrm{v}.\gamma\gamma') = \mathrm{s}.\alpha\gamma\gamma'.\mathrm{s}.\alpha'\beta\beta' - \mathrm{s}.\alpha'\gamma\gamma'.\mathrm{s}.\alpha\beta\beta'; \quad (1.)$$

which gives, for any *five* vectors $\alpha\alpha'\alpha''\gamma\gamma'$, this other:

$$\mathrm{s}(\mathrm{v}.\alpha\alpha', \mathrm{v}.\alpha'\alpha''.\mathrm{v}.\gamma\gamma') = \mathrm{s}.\alpha\alpha'\alpha''.\mathrm{s}.\gamma\alpha'\gamma' \quad . \quad . \quad (2.)$$

If, then, we take six arbitrary vectors $\alpha\alpha'\alpha''\alpha'''\alpha^{\mathrm{iv}}\alpha^{\mathrm{v}}$, and deduce nine other vectors from them by the expressions

$$\left.\begin{array}{l} \alpha_0 = \mathrm{v}.\alpha\alpha',\ \alpha_1 = \mathrm{v}.\alpha'\alpha'',\ \alpha_2 = \mathrm{v}.\alpha''\alpha''', \\ \alpha_3 = \mathrm{v}.\alpha'''\alpha^{\mathrm{iv}},\ \alpha_4 = \mathrm{v}.\alpha^{\mathrm{iv}}\alpha^{\mathrm{v}},\ \alpha_5 = \mathrm{v}.\alpha^{\mathrm{v}}\alpha, \\ \beta = \mathrm{v}.\alpha_0\alpha_3,\ \beta' = \mathrm{v}.\alpha_1\alpha_4,\ \beta'' = \mathrm{v}.\alpha_2\alpha_5; \end{array}\right\} \quad . \quad . \quad . \quad (3.)$$

we shall have, *generally*,

$$\left.\begin{array}{l} \mathrm{s}.\beta\beta'\beta'' = \mathrm{s}.\alpha_0\alpha_2\alpha_5.\mathrm{s}.\alpha_3\alpha_1\alpha_4 - \mathrm{s}.\alpha_3\alpha_2\alpha_5.\mathrm{s}.\alpha_0\alpha_1\alpha_4 \\ \quad = \mathrm{s}.\alpha_0\alpha_1\alpha_4.\mathrm{s}.\alpha_2\alpha_3\alpha_5 - \mathrm{s}.\alpha_3\alpha_4\alpha_1.\mathrm{s}.\alpha_5\alpha_0\alpha_2 \\ \quad = \mathrm{s}.\alpha\alpha'\alpha''.\mathrm{s}.\alpha^{\mathrm{iv}}\alpha'\alpha^{\mathrm{v}}.\mathrm{s}.\alpha''\alpha'''\alpha^{\mathrm{iv}}.\mathrm{s}.\alpha^{\mathrm{v}}\alpha'''\alpha \\ \quad - \mathrm{s}.\alpha'''\alpha^{\mathrm{iv}}\alpha^{\mathrm{v}}.\mathrm{s}.\alpha'\alpha^{\mathrm{iv}}\alpha''.\mathrm{s}.\alpha^{\mathrm{v}}\alpha\alpha'.\mathrm{s}.\alpha''\alpha\alpha''' \\ \quad = \mathrm{s}.\alpha\alpha'\alpha''.\mathrm{s}.\alpha''\alpha'''\alpha^{\mathrm{iv}}.\mathrm{s}.\alpha\alpha'''\alpha^{\mathrm{v}}.\mathrm{s}.\alpha^{\mathrm{v}}\alpha'\alpha^{\mathrm{iv}} \\ \quad - \mathrm{s}.\alpha\alpha'''\alpha''.\mathrm{s}.\alpha''\alpha'\alpha^{\mathrm{iv}}.\mathrm{s}.\alpha\alpha'\alpha^{\mathrm{v}}.\mathrm{s}.\alpha^{\mathrm{v}}\alpha'''\alpha^{\mathrm{iv}}, \end{array}\right\} \quad . \quad . \quad (4.)$$

Thus if, in particular, the six vectors $\alpha \,..\, \alpha^{\text{v}}$ are such as to satisfy the condition

$$\text{s}.\beta\beta'\beta''=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

they will satisfy also this other condition, or this other form of the same condition:

$$\frac{\text{s}.\alpha\alpha'\alpha''}{\text{s}.\alpha\alpha'''\alpha''} \cdot \frac{\text{s}.\alpha''\alpha'''\alpha^{\text{iv}}}{\text{s}.\alpha''\alpha'\alpha^{\text{iv}}} = \frac{\text{s}.\alpha\alpha'\alpha^{\text{v}}}{\text{s}.\alpha\alpha'''\alpha^{\text{v}}} \cdot \frac{\text{s}.\alpha^{\text{v}}\alpha'''\alpha^{\text{iv}}}{\text{s}.\alpha^{\text{v}}\alpha'\alpha^{\text{iv}}}; \quad . \quad . \quad (6.)$$

and reciprocally the former of these two conditions will be satisfied if the latter be so.

These two equations, (5.) and (6.), express, therefore, each in its own way, the existence of one and the same geometrical relation between the six vectors $\alpha\,\alpha'\,\alpha''\,\alpha'''\,\alpha^{\text{iv}}\,\alpha^{\text{v}}$: and a slight study of the *forms* of these equations suffices to render evident that they both agree in expressing that these six vectors are *homoconic*, in the sense of the 25th article; or in other words, that the six vectors are sides (or edges) of one common cone of the second degree. Indeed the equation (5.) of the present article, in virtue of the definitions (3.), coincides with the equation (2.) of the article just cited, the symbols β, β', β'' retaining in the one the significations which they had received in the other. The recent transformations show, therefore, that the *equation of homoconicism*, assigned in article 25, may be put under the form (6.) of the present article, which is different, and in *some* respects simpler. The former expresses a *graphic* property, or relation between *directions*, namely that the three lines β, β', β'', which are the respective intersections of the three pairs of planes $(\alpha\alpha', \alpha'''\alpha^{\text{iv}})$, $(\alpha'\alpha'', \alpha^{\text{iv}}\alpha^{\text{v}})$, $(\alpha''\alpha''', \alpha^{\text{v}}\alpha)$, are all situated in one common plane, if the six homoconic vectors be supposed to diverge from one common origin; the latter expresses the *metric* property, or relation between *magnitudes*, that the ratio compounded of the two ratios of the two pyramids $(\alpha\,\alpha'\alpha'')$ $(\alpha''\,\alpha'''\alpha^{\text{iv}})$ to the two other pyramids $(\alpha\alpha'''\alpha'')$ $(\alpha''\alpha'\alpha^{\text{iv}})$, or that the product of the volumes of the first pair of pyramids divided by the product of the volumes of the second pair, does not vary, when the vector α'', which is the common edge of these four pyramids, is changed to the new but homoconic vector α^{v}, as their new common edge, the four remaining homoconic and coinitial edges $\alpha\alpha'\alpha'''\alpha^{\text{iv}}$ of the pyramids being supposed to undergo no alteration. The one is an expression of the property of the *mystic hexagram* of Pascal; the other is an expression of the constancy of the *anharmonic ratio* of Chasles*. The calculus of Quaternions (or the

* Although the foregoing process of calculation, and generally the method of treating geometrical problems by quaternions, which has been extended by the writer to questions of dynamics and thermology, appears to him to be

method of scalars and vectors) enables us, as we have seen, to pass, by a very short and simple symbolical transition, from either to the other of these two great and known properties of the cone of the second degree.

[To be continued.]

L. *Intelligence and Miscellaneous Articles.*

ON THE DETERMINATION OF CARBONIC ACID IN SALINE COMPOUNDS. BY C. BRUNNER, SEN.

THE estimation of carbonic acid in its combinations is generally effected by ignition, when the compound is one of those which part with the whole of the acid at a red heat. When water is disengaged at the same time, its quantity must either be determined by a separate experiment, and subtracted from the loss experienced on ignition, or collected in a suitable apparatus during the calcination, and so calculated. With those compounds where this method cannot be applied, it is customary to expel the carbonic acid by a stronger acid, for instance sulphuric acid, and to determine its amount from the loss, taking care in this case to retain the water accidentally carried over with it by some suitable substance. Apparatus for this mode of determination have been described by Rose, and recently by Fresenius.

It is readily seen that, according to these methods, the result is always obtained in a negative manner, that is to say, by a loss in weight. As we should certainly endeavour to exchange all such negative methods for positive, I will here communicate one which appears to me applicable in most cases. The substance to be examined is placed in the little flask *a*, and a suitable quantity, for instance an ounce, of water poured over it; upon which the flask is closed with a tight-fitting cork provided with three tubes, and when necessary coated with cement. The straight tube terminates above in a small funnel, through which the sulphuric acid is poured; the second, provided with two bulbs, is connected by its rectangular bend with a tube *b c*, from a third to half an inch wide; and, in order to gain space, likewise furnished with two expansions containing asbestos moistened with sulphuric acid; the third is bent

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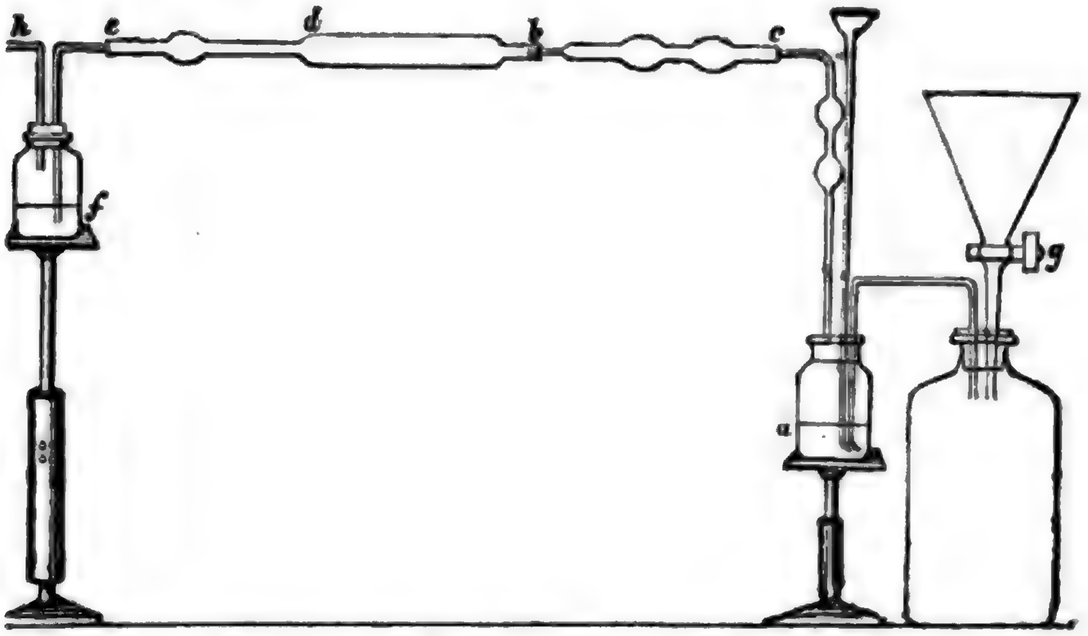
new, yet it is impossible for him, in mentioning here the name of Chasles, to abstain from acknowledging the deep intellectual obligations under which he feels himself to be, for the information, and still more for the impulse given to his mind by the perusal of that very interesting and excellent History of Geometrical Science, which is so widely known by its own modest title of *Aperçu Historique* (Brussels, 1837). He has also endeavoured to profit by a study of the Memoirs by M. Chasles, on Spherical Conics and Cones of the Second Degree, which have been translated, with Notes and an Appendix, by the Rev. Charles Graves (Dublin, 1841); and desires to take this opportunity of adding, that he conceives himself to have derived assistance, as well as encouragement, in his geometrical researches generally, from the frequent and familiar intercourse which he has enjoyed with the last-named gentleman.



twice, and terminates in the large empty flask. To adapt it more easily, it may be cut anywhere in the middle, and the two parts connected by caoutchouc. The two tubes dipping in the liquid *a* are drawn out at their lower ends into fine, somewhat laterally curved points; the tube *b e* contains, in the expanded portion *b d*, which is from three-quarters to an inch in width, well-burnt lime, which may be readily moistened with water, from *d* to *e*, asbestos, or fragments of pumice-stone, drenched with sulphuric acid, and separated from the lime by a light stopper of asbestos; the small Woulf's flask contains lime-water.

The analysis is now performed in the following manner:—A small quantity of sulphuric acid is poured into the flask *a* through the funnel-tube, the stopcock *g* being closed; as this will not descend of itself, it is made to do so by drawing gently with the mouth at *h*. The evolution of gas, which is rendered perceptible by the ascending bubbles, and by the air passing through the lime-water in *f*, is now waited for; another portion of acid is then added, and this continued until it may be assumed that a tolerable excess of acid has been introduced. This being done, some water containing a little caustic potash in solution is allowed to flow into the flask by opening the stopcock *g*, whereby a current of air is passed through the vessel *a*, which carries the carbonic acid, contained partly in the liquid partly in the upper space of the vessel, into the tube *b e*; but since this would never be completely effected without the application of heat, the vessel *a* towards the end of the experiment is immersed in a small dish filled with water, which is kept warm over a small lamp as long as is found requisite.

It is best to regulate the current of air so that about 2 bubbles of gas pass through the lime-water in a second. It will however never be found to become in the least turbid. It is scarcely necessary to observe that the object of the sulphuric acid in *b c* is to retain the moisture carried by the gas from *a*; also that the potash added to the water which flows into the large flask is to absorb the carbonic acid of the atmospheric air; and, lastly, that, the increase in weight of *b e* yields the result sought for.

Several analyses made according to this method yielded highly satisfactory results. 1·771 grm. of recently ignited carbonate of potash, which was allowed to cool in a closed platinum crucible, gave 0·564 carbonic acid. Taking the atom of potash at 590, that of the carbonic acid at 275, 0·563 ought to have been obtained.

1·705 grm. of very pure magnesite gave 0·870 carbonic acid = 51·026 per cent.—Poggendorff's *Annalen*, No. vi. 1846.

ON THE ACTION OF NITRIC ACID UPON CHOLIC ACID.

BY A. SCHLIEPER.

The interesting connexion which has recently been shown to exist between the products of decomposition of cholesterine and choloidic acid by Prof. Redtenbacher[*], led to the supposition that it might also extend to the other products of the bile if they were submitted to a similar treatment. With this view I have been induced to examine the action of nitric acid on Demarcay's cholic acid.

The cholic acid was prepared according to the process described by Theyer and Schlosser; bile, freed from mucus, fat and colouring substance, was retained for several days at a boiling temperature with a tolerably strong solution of potash, and then concentrated until a soapy mass, which became hard on cooling, separated from the liquid. After complete separation, it was dissolved in water, filtered, and treated with acetic acid, which separates the impure cholic acid in thick white flakes, which unite, forming resinous masses. If the eliminated resin be heated with water to boiling, it assumes all at once a granular crystalline structure, and is then easily reduced to powder; the latter was dried, and washed on a funnel with æther until it appeared white, and then pure white cholic acid obtained from it by dissolving and crystallizing it from alcohol. Cholic and nitric acids do not act on one another in the cold, however concentrated the latter may be; but if a mixture of the two be heated in a retort, a very violent reaction soon ensues, the mass ascends, frothing considerably, while large quantities of nitrous acid escape. When the first action is over, the retort contains a dark yellow liquid, on which float some drops of oil, which on cooling solidify, and are nothing more than unaltered cholic acid. As the nitric acid which distilled over possessed a peculiar odour, it was nearly saturated with an alkali and again distilled; but although the aqueous distillate still retained the peculiar smell, none of the volatile products which Redtenbacher discovered in submitting cholesterine and choloidic acid to a similar treatment, could be detected in it. The yellowish liquid which remained in the retort was evaporated on the water-bath to expel the excess of nitric acid, when it dried to a yellowish transparent gum, which exhibited in its external properties the greatest resemblance to the cholesteric acid recently described by Redtenbacher. To separate this body from some still undecomposed cholic acid, it was repeatedly dissolved in water, filtered, and again evaporated until the residue dis-

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[*] Chem. Gaz., vol. iv. p. 269.

solved to a clear solution in water. To ascertain the presence of cholesteric acid, the silver salt was prepared by dissolving the acid gummy mass in water, neutralizing with ammonia, and precipitating with nitrate of silver; it was deposited in thick white flakes. Oxalic acid was not present. To obtain the silver salt in a crystalline state, some nitrate of ammonia was added to the mother-ley from which it had been precipitated, in order to increase the solubility of the silver salt in it, then boiled with the precipitate, when the greater portion dissolved; on cooling, the cholesterate of silver separated from the hot filtered liquid, on the bottom and sides of the glass, in granular crystalline yellowish crusts; more of the salt was obtained by evaporating the mother-ley. The salt, dried at 212°, yielded 57·69 oxide of silver, 23·81 carbon, and 2·35 per cent. hydrogen; leading to the formula $AgO, C^8 H^4 O^4$, which is that of the cholesterate of silver.

The contemporaneous occurrence of cholesteric acid in the products of decomposition of choloidic acid, cholesterine and cholic acid by nitric acid exhibits the close relationship of these three bodies as respects their constitution.—Liebig's *Annalen*, lviii. p. 375.

ON A NEW PROPERTY OF LIGHT EXHIBITED IN THE ACTION OF CHRYSAMMATE OF POTASH UPON COMMON AND POLARIZED LIGHT. BY SIR D. BREWSTER*.

The chrysammate of potash, which crystallizes in very small, flat rhombic plates, has the metallic lustre of gold, whence it derives its name of golden fluid. When the sun's light is transmitted through the rhombic plates it has a reddish yellow colour, and is wholly polarized in one plane. When the crystals are pressed with the blade of a knife on a piece of glass, they can be spread out like an amalgam. The light transmitted through the thinnest films thus produced, consists of two oppositely polarized pencils,—the one of a bright carmine red and the other of a pale yellow colour. With thicker films, the two pencils approach to two equally bright carmine red pencils. It is to the reflected light, however, and its new properties, that I wish to direct attention. Common light, reflected at a perpendicular incidence from the surfaces of the crystals, or of the films, has the colour of virgin gold. It grows less and less yellow as the incidence increases, till it becomes of a pale bluish white colour at very great incidences. The compound pencil, thus reflected and coloured, consists of two oppositely polarized pencils,—one polarized in the plane of reflexion, and of a pale bluish white colour at all incidences, and the other polarized perpendicular to the plane of reflexion, and of a golden yellow colour at small incidences, passing successively into a deeper yellow, greenish yellow, green, greenish blue, blue, and light pink, as the angle of incidence increases. This very remarkable property, which I have discovered also in some other crystals, is not caused by any film of oxide formed upon the natural surface of the crystal, nor is it the

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[*] Read at the Southampton Meeting of the British Association.

result of any change produced upon the surface by external causes. It is exhibited, under the usual modifications, if the surface of the chrysammate is in optical contact with fluids and with glass; and when the crystal is in the act of being dissolved, or when a fresh surface is exposed by mechanical means, the superficial action of the crystal upon light is in both cases the same. When the chrysammate is re-crystallized from an aqueous solution, it appears in tufts of prisms of a bright red colour, the golden reflexion being overpowered by the transmitted light; but when these tufts are spread into a film by pressure, the golden yellow colour re-appears. When the crystals of chrysammate are heated with a spirit-lamp, or above a gas-burner, they *explode* with a flame and smoke like gunpowder; and, by continuing the heat, the residue melts and a crop of colourless amorphous crystals is left. I have found the same explosive property in the aloetinate of potash.—*Athenæum.*

INFUENCE OF NITROUS OXIDE GAS ON VEGETATION.

BY M. VOGEL, JUN.

It is generally admitted that nitrous oxide gas is incapable of supporting life, although a candle which has been blown out is relighted by immersion in this gas, and burns with greater brilliancy in it than in atmospheric air. The rapid combustion of the candle is owing to the ready decomposition of this gas at a high temperature,—a decomposition which cannot be effected in the lungs by respiration; whereas the air, which is a mere mixture of oxygen and azotic gases, readily yields its oxygen in the act of respiration.

The author obtained the nitrous oxide, used in his experiments, from pure nitrate of ammonia; and into a vessel containing this gas he introduced seeds of cress spread on a moist sponge. The introduction of the seed was effected under water, and so as to prevent the access of atmospheric air, and it was expelled from the sponge by slight pressure; sufficient water was left in the bottle for germination; and after the sponge and seeds had been introduced, it was hermetically closed.

In order to make a comparative experiment, a sponge with cress seed was also placed in a bottle containing atmospheric air. After some days the seed in this bottle began to germinate and to form leaves; but the reverse was the case in the bottle containing the seed in nitrous oxide. Not the slightest appearance of germination was perceptible: the seeds, nevertheless, were swelled and covered with mucus, but without any change which indicated germination. After two weeks had elapsed, the seed and sponge were removed from the nitrous oxide, and as soon as they came into the air they began to germinate; from this it follows that the seed had not lost its power of germinating by exposure to nitrous oxide; whereas seeds cease to germinate after they have been in contact for a short time with several other gases.

The nitrous oxide gas in which the seed had remained for two weeks, underwent no alteration; it did not contain even a trace of carbonic acid, and re-lighted a candle.

To ascertain the influence of this gas on plants previously developed, a sponge with plants of cress completely formed were placed in a bottle filled with it. In two days the plants suffered no change; but on the third day they became of a yellow colour, and in a week they drooped: when removed to the atmosphere, they recovered their colour and ceased to droop.

It is to be observed, that the experiments described were made, not only with seeds, but also with plants, in nitrous oxide exposed to the direct rays of the sun and in the shade, and that in both cases no decomposition of the gas occurred.

It results from the experiments described,—

1. That nitrous oxide is not favourable either to the germination of seed, or the vegetation of plants previously formed.
2. That this gas is not decomposed by the chlorophylle of plants, even when the apparatus is exposed to the direct rays of the sun.
3. That seeds which have remained for some time in this gas, do not thereby lose the power of germinating in the air.—*Journ. de Pharm. et de Ch.*, Août 1846.

METHOD OF DETERMINING THE QUANTITY OF TUNGSTIC ACID.

M. Marguerite remarks that the usual process for ascertaining the quantity of tungstic acid has been admitted to be inaccurate by all chemists; and in analysing the supersalts of this acid, the limit of error which may be committed in employing it, may occasion such a difference in their centesimal composition as may make the formula vary one equivalent of acid, or at any rate leave it undecided: in fact, a difference of 1 or 2 per cent. constitutes a tungstate with four or five equivalents of acid.

M. Berzelius, in his *Annuaire* for 1841, gives a precise method for estimating tungstic acid in neutral tungstates, by means of nitrate of mercury. M. Marguerite was however apprehensive, that by precipitating the acid tungstates in this manner, the nitric acid set free might act upon the precipitated tungstate of mercury, and render the process inexact.

The following process M. Marguerite found to be rigorously exact and extremely simple. Put the salt to be analysed in a small platina capsule, and add to it several times its weight of pure concentrated sulphuric acid; subject the mixture at first to a gentle, and subsequently to a red heat.

After the calcination, the residue consists of an acidulous sulphate and free tungstic acid. It is to be mixed and washed on a filter, with water containing hydrochlorate of ammonia, which possesses the property of preventing the tungstic acid from becoming a hydrate, and passing through the filter,—an effect which is sometimes produced, even after it has been calcined.

When the last washings cease to precipitate chloride of barium, the residue is to be calcined to expel the hydrochlorate of ammonia, and a few drops of nitric acid are to be added to oxidize any small particles of reduced tungsten which may have appeared, and to dissipate the last traces of carbon which the filter may have left. In this manner the water may be ascertained by simple calcination, the tungstic acid directly, and the alkali by difference.

Having attempted to estimate the alkali by the quantity of sulphuric acid fixed, it was found that tungstic acid, like the silicic and boracic acids, decomposed the neutral sulphate at the temperature requisite to destroy the bisulphate: the tungstic acid then combines with the alkali, and forms a tungstate which is more or less acid; it is therefore important not to carry on the operation so far. The bisulphate possesses the advantage of presenting sulphuric acid to those parts of the salt which would not be decomposed, which at this high temperature acts energetically.

That the salt has been completely attacked, and that the tungstic acid has not been hydrated, and consequently dissolved, may be ascertained by treating the first washings with a plate of zinc and hydrochloric acid. They do not in this case produce the slightest blue tint, though capable of ascertaining the smallest quantity of tungstic acid.—*Ann. de Ch. et de Phys.*, Août 1846.

PREPARATION OF CHLORIC ACID. BY M. BŒTTGER.

The author observes, that the preparation of chloric acid by means of hydrosilicic acid is attended with a great number of inconveniences; he therefore proposes to replace it with oxalic acid, this acid also yielding a salt of soda which is but slightly soluble in water.

The following is the process recommended. First prepare a solution of tartrate of soda, by dissolving seven parts of crystallized carbonate of soda and seven and a half parts of tartaric acid in boiling water; add to this solution while boiling a solution, also boiling, of six parts of chlorate of potash in sixteen parts of water: let it cool, so as to allow the tartar to deposit; then filter, and add to the solution six parts of oxalic acid dissolved in six parts of water, heated to a temperature not exceeding 133° F.; agitate the mixture briskly, and put it into a freezing mixture, made with crystallized sulphate of soda and muriatic acid; by this the oxalate of soda is deposited, and it requires only to be separated by the filter.

The chloric acid thus obtained is not chemically pure; but to procure it pure and concentrated, it is to be treated with recently precipitated carbonate of barytes, crystallizing the chlorate formed, and decomposing it with sulphuric acid.—*Journ. de Pharm. et de Ch.*, Juillet 1846.

OBSERVATIONS ON YEST.

M. F. W. Ludersdorff, in order to decide the question whether yest is an organized substance, and whether, if it be, it occasions fermentation in consequence of it, made the following experiments:—He triturated a portion of it on glass so perfectly, that the microscope could not detect any kind of globular texture; two parts of grape-sugar were dissolved each in ten parts of distilled water; one of them was mixed with the triturated yest, and the other with a similar portion of the same yest, not triturated; both were exposed to a temperature of 95° F. The liquid containing the untriturated yest began to ferment in half an hour, and the action continued uninterruptedly for two days, when all the sugar was decomposed; but the liquid containing the triturated and disorganized yest did not yield the smallest bubble of gas during the whole of the time.—*Ibid.*

METEOROLOGICAL OBSERVATIONS FOR AUG. 1846.

Chiswick.—August 1. Uniformly overcast: hot and dry: 2 P.M. almost continued thunder: at 3 P.M. rain in torrents: at 4^h 40^m vivid lightning and rain, mixed with large hail: overcast at night 2. Sultry: thunder and rain: clear. 3. Rain: showery. 4. Cloudy and fine. 5. Heavy rain. 6. Cloudy and fine. 7. Overcast. 8, 9. Cloudy and fine. 10, 11. Very fine. 12. Rain: cloudy. 13. Cloudy: heavy rain. 14. Very fine. 15. Clear: fine: rain. 16, 17. Cloudy: fine. 18. Fine: rain. 19. Fine: drizzly. 20. Overcast: rain. 21. Densely clouded: rain. 22—25. Cloudy and fine. 26. Overcast. 27. Fine. 28—31. Very fine.

Mean temperature of the month .... 64°·16
Mean temperature of Aug. 1845 .... 59 ·30
Average mean temperature of Aug. for the last twenty years 62 ·23
Average amount of rain in Aug. .... 2·41 inches.

Boston.—Aug. 1. Cloudy: rain P.M., with thunder and lightning. 2. Fine: rain P.M. 3. Fine: rain, with thunder and lightning A.M. 4. Fine: rain and lightning P.M. 5. Cloudy: rain P.M. 6, 7. Cloudy. 8. Fine. 9. Cloudy: rain A.M. 10. Fine: rain P.M. 11, 12. Fine. 13. Cloudy: whirlwind, with rain A.M. 14. Fine. 15. Fine: rain A.M. 16. Fine. 17. Fine: rain early A.M. 18. Fine. 19. Rain. 20. Fine: rain A.M. and P.M. 21. Rain. 22. Cloudy. 23. Cloudy: rain P.M. 24—27. Cloudy. 28. Fine. 29. Cloudy. 30, 31. Fine. —The past month has been extraordinary warm.

Sandwick Manse, Orkney.—Aug. 1. Cloudy: fine. 2. Fog: hot: fine. 3. Bright: hot: fog. 4. Clear: hot: fog. 5. Bright: cloudy. 6. Fog: cloudy. 7. Cloudy. 8. Bright: fog. 9. Fog: rain. 10. Bright: rain: clear. 11. Showers: clear. 12. Showers: cloudy. 13. Rain. 14. Bright: clear. 15. Rain. 16. Showers: small rain. 17. Cloudy: fine. 18. Cloudy. 19. Bright: hot: fog. 20. Cloudy: rain. 21. Cloudy: damp. 22. Damp: drizzle. 23. Cloudy. 24. Bright: cloudy. 25. Clear: aurora: fine. 26. Clear: fine. 27. Clear: aurora: fine. 28. Clear: fog. 29. Cloudy. 30. Bright: rain. 31. Rain: clear.

Mean temperature of the month .... 58°·82
Mean temperature of Aug. for nineteen preceding years ...... 54 ·76
Mean temperature of Aug. 1845 .... 53 ·16

Applegarth Manse, Dumfries-shire.—Aug. 1. Remarkably warm. 2. Very fine. 3. Very fine: thunder. 4. Fine: one shower. 5. Heavy shower: fine. 6. Fair and fine. 7. Rain, heavy: thunder. 8. Wet: thunder. 9. Wet P.M.: fair A.M. 10. Showers. 11, 12. Slight showers. 13. Very heavy rain: flood. 14. Fine: one shower. 15. Showers P.M. 16. Wet A.M.: cleared. 17. Very fine harvest day. 18. Rain nearly all day. 19. Fine harvest day. 20. Fine harvest day: thunder. 21—23. Fine harvest days. 24, 25. Fine harvest days: threatening. 26, 27. Fine harvest days: clear. 28, 29. Fine harvest days: threatening. 30. Fine harvest day. 31. Rain: cleared P.M.

Mean temperature of the month .... 61°·25
Mean temperature of Aug. 1845 .... 56 ·4
Mean temperature of Aug. for twenty-three years .... 57 ·0
Mean rain in Aug. for eighteen years .... 3·61 inches.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. Aug.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8¼ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8¼ p.m.								
1.	29·752	29·670	29·18	29·85	29·79	30·02	29·99	92	60	70	77½	58½	63½	59	e.	calm	ene.	ese.	1·23	......	......	......
2.	29·830	29·642	29·05	29·74	29·75	29·99	29·99	81	56	73·5	76	62	66	62	s.	calm	ene.	e.	·65	·73	......	......
3.	29·925	29·802	29·24	29·78	29·80	29·98	29·98	77	51	73	78½	63	70	62	w.	calm	e.	ese.	·04	·03	......	......
4.	29·942	29·923	29·34	29·83	29·85	30·03	30·00	79	56	70	73½	58	68	60	se.	s.	e.	se.	·85	·46	......	......
5.	29·920	29·827	29·29	29·89	29·89	30·00	29·96	80	62	68	72	56	65	60	e.	s.	ne.	e.	·43	·37	......	......
6.	29·906	29·835	29·28	29·90	29·64	29·98	29·97	83	64	68·5	76	52	61½	63	ne.	n.	s.	calm	......	·04	......	......
○ 7.	29·747	29·692	29·24	29·76	29·61	29·96	29·91	83	60	66·5	69½	58	62	60	sw.	calm	e—se.	e.	......	......	......	......
8.	29·728	29·701	29·07	29·58	29·58	29·82	29·73	74	55	71	71	61	64	61	sw.	sw.	sw.	e.	·01	......	1·00	......
9.	29·969	29·844	29·19	29·65	29·75	29·67	29·68	71	58	68	67	59	61½	60	sw.	calm	w.	se.	......	......	......	·22
10.	30·022	30·007	29·38	29·76	29·77	29·75	29·75	71	55	70	71	56	63½	56	sw.	w.	sw.	se.	......	·02	......	·30
11.	30·073	30·039	29·37	29·76	29·82	29·59	29·76	73	57	66	63	54½	58	55	w.	calm	sw.	w.	·26	·02	......	·57
12.	30·066	30·038	29·49	29·92	29·83	29·87	29·87	75	51	66	64	55	57½	55	nw.	calm	sw.	wsw.	·01	......	......	·13
☾ 13.	29·792	29·682	29·16	29·43	29·59	29·51	29·56	66	44	65	61½	54	54½	51½	sw.	sw.	sw-w.	nw.	·22	......	......	·49
14.	29·912	29·879	29·38	29·75	29·75	29·74	29·82	77	49	60·5	64	48	56½	52½	sw.	nw.	ssw.	s.	......	·22	......	·16
15.	29·740	29·716	29·22	29·55	29·53	29·70	29·53	79	50	67	63	55	54½	56	s.	calm	s.	se.	·07	......	3·05	·12
16.	29·863	29·792	29·19	29·58	29·65	29·50	29·56	72	49	67	65	53	58	55½	w.	calm	sw.	w.	......	·02	......	·21
17.	29·855	29·746	29·29	29·64	29·53	29·67	29·70	70	53	64	66	57	61	56	sw.	calm	sw.	e.	·27	·24	......	·06
18.	29·570	29·539	28·99	29·39	29·44	29·74	29·73	72	54	63	66	56	60	57	sw.	sw.	e.	e.	·07	......	......	......
19.	29·648	29·578	29·00	29·57	29·64	29·72	29·73	70	52	60	70	51	60½	58	w.	calm	n.	calm	·01	......	......	·19
20.	29·829	29·720	29·23	29·64	29·61	29·67	29·69	64	59	65	62	55	58	58	sw.	nw.	e.	se.	·33	·41	......	......
● 21.	29·954	29·744	29·23	29·76	29·93	29·89	30·00	70	54	61	68	56	57	56	ne.	calm	se.	w.	·03	·47	......	·11
22.	30·055	29·994	29·49	29·98	29·99	30·03	30·06	67	48	62	65½	48	62	57½	n.	calm	sse.	w.	......	·01	......	......
23.	30·123	30·082	29·54	30·08	30·11	30·19	30·23	71	59	65·5	63	59	58½	56	n.	calm	ene.	w.	......	......	0·48	·04
24.	30·210	30·138	29·62	30·16	30·20	30·26	30·31	68	46	63	61½	54	57½	56	ne.	n.	ne.	nw.	......	·04	......	......
25.	30·210	30·194	29·70	30·24	30·21	30·31	30·26	69	52	60·5	68½	46	57½	54	ne.	calm	ne.	s.	......	......	......	......
26.	30·148	30·051	29·60	30·17	30·08	30·20	30·15	74	56	62	70	45	61	57	ne.	calm	se.	se.	......	......	......	......
27.	29·998	29·950	29·48	30·05	30·00	30·14	30·09	76	57	64·5	69	46½	60	55	e.	ene.	se.	se.	......	......	......	......
28.	29·936	29·921	29·38	30·05	30·05	30·13	30·18	78	56	69	72	49½	60	57	e.	e.	ene.	w.	......	......	......	......
☽ 29.	30·010	29·929	29·34	30·03	29·98	30·15	30·06	72	48	65	70½	50	60	55	ne.	calm	ne-w.	se.	·02	......	......	......
30.	30·172	30·095	29·45	30·00	30·02	30·00	30·00	70	48	65	68½	52	61	57½	nw.	calm	sw.	se.	......	......	0·39	......
31.	30·178	30·132	29·54	30·00	30·18	30·08	30·07	79	56	63	67½	59	57	54	w.	calm	nw.	w.	......	......	......	·31

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LI. *The* Astronomer Royal *on the Bands formed by the Partial Interception of the Prismatic Spectrum.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SINCE my return from the continent I have seen a report of the transactions of the British Association at its late meeting; and my attention has been directed to a communication by my friend Prof. Powell, "On the Bands formed by the Partial Interception of the Prismatic Spectrum," which, for care in observation, caution in theory, and courtesy in the expression of doubt, merits well to be taken as a model for papers of a similar character.

Prof. Powell found that in certain points there is an apparent discordance between his observations and the result of my deductions from the Undulatory Theory. It is curious that on these very points there is the most perfect accordance. The discrepance has arisen from this circumstance: that my short paper in the Philosophical Transactions, 1841, part 1, was intended rather to show the possibility of explaining the bands, than to enter into details as to their variation under varying conditions of experiment; that I therefore investigated closely only that case in which the bands are most conspicuously formed; and that an expression used by me, with reference only to that one case, has been (not unnaturally) applied to other cases, to which it is totally inapplicable.

In page 8 of my paper, line 20, I have made this restrictive supposition: "And if $\frac{\lambda e}{h}$ be equal to the change of k corresponding to a change of 2π in R." This limitation, *which establishes a relation between the aperture of the eye or telescope* *on the one hand, and the extent of the spectrum and the thickness of the retarding plate on the other hand*, dominates over all the succeeding deductions. The expression in page 9, line 16, "the interval between the bands is $\frac{2\lambda e}{h}$," is subject to this limitation. In a word, I have stated what is the law of variation of the breadth of the bands *when the aperture of the eye is varied, and when the extent of spectrum, &c. receives a corresponding variation, so as to preserve the condition of page* 8 *line* 20: but I have not stated what is the law of variation of the breadth of the bands *when the aperture of the eye is varied without varying other parts of the experimental arrangement.* This latter limited variation was the variation made in Prof. Powell's experiments, and therefore my expression does not apply to these experiments.

I will now proceed to investigate expressions which will apply to Prof. Powell's experiments.

For this purpose I beg to refer to the diagram in page 10 (remarking that the numerical values of the ordinates are to be found in the table of page 4), which represents the different degrees of intensity of light in the successive slightly diffused images formed by the light coming from successive portions of the spectrum, when viewed under the conditions of this experiment. And I must request the reader to remember that the effect of viewing a *spectrum* (as distinguished from a collection of all the various qualities of light in one point) is, that supposing the first curve to remain in its place, the second curve will be drawn back by a certain quantity C, the third by 2 C, the fourth by 3 C, and so on; and that in order to obtain the expression for the intensity of the light received by the eye, the ordinates which are then in the same vertical line must be added together. This quantity C is absolutely constant while the extent of spectrum and thickness of retarding plate are constant, and does not in any degree depend on the aperture of the eye.

Then it must be remarked that the effect of enlarging the aperture of the eye is to diminish the horizontal extent of each of the curves in the same proportion, leaving the centre of each in the place defined by the movement which I have just mentioned; and that the effect of diminishing the aperture of the eye is to enlarge the horizontal extent of each of the curves in the same proportion, leaving the centre of each in the place as above defined.

Suppose, then, that the quantity C is equal to *three* intervals of ordinates, *as at present marked in the diagram of page* 10. Then if, in a change of experiment, the aperture of the eye be enlarged in the proportion 3 : 4, the length of each curve will be diminished in the ratio of 4 : 3, and there will be *four* intervals of ordinates in the constant space C. If, in another change, the aperture of the eye be diminished in the proportion 3 : 2, the length of each curve will be increased in the ratio of 2 : 3, and there will be only *two* intervals of ordinates in the constant space C.

I shall now proceed to apply these considerations to the circumstances of experiment.

General Condition for the three following Experiments.

The constant C is supposed to be equal to three intervals of ordinates, as at present marked in the diagram of page 10.

Special condition for the first experiment.

The aperture of the eye is such as to make the horizontal extent of each curve exactly equal to that marked in the diagram.

Since each curve is to be drawn back by the constant space C from that above it, and since C is equal to three intervals of ordinates, it follows that the aggregate of light will be obtained by combining the 1st number of the first column on page 4 with the 4th number of the second column, the 7th number of the third column, and so on; continuing the succession of columns as long as numbers can be taken by this rule (the seventh column being exactly similar to the first). Effecting this, and dividing the sums by 12, we find the following numbers to represent the intensity of light which comes to the eye:—

13646, 12829, 11295, 9227, 6875, 4524, 2456, 921, 105, 105, 921, 2456, 4524, 6875, 9227, 11295, 12829, 13646, 13646, 12829, &c.

The bands here will be most vividly bright and black.

The bands recur at an interval of 18 ordinates or 6 C.

Special condition for the second experiment.

The aperture of the eye is greater than that in the first experiment, in the ratio of 3 : 4.

Since each curve is to be drawn back by the constant space C from that above it, and since C (as above explained) is now equal to four intervals of coordinates, it follows that we must combine the 1st number of the first column with the 5th number of the second column; the 9th number of the third column, and so on. Effecting this, and dividing the sums by 9, we find the following numbers representing the intensity of light which comes to the eye:—

7659, 9164, 10512, 11617, 12399, 12801, 12801, 12399, 11616, 10512, 9164, 7659, 6093, 4586, 3238, 2135, 1353, 949, 949, 1353, 2135, 3238, 4586, 6094, 7659, 9164, 10512, &c.

The bands here are extremely well marked, but not quite so vividly bright and black as in the first experiment.

The bands recur at an interval of 24 ordinates; and as 4 ordinates are now included in the space C, the bands recur at an interval of 6 C.

Special condition for the third experiment.

The aperture of the eye is less than that in the first experiment, in the ratio of 3 : 2.

Since each curve is to be drawn back by the constant space C from that above it, and since C (as is above explained) is now equal to two intervals of ordinates, it follows that we must now combine the 1st number of the first column on page 4 with the 3rd number of the second column; the 5th number of the third column, and so on. Effecting this, and dividing the sums by 18, we obtain the following numbers for the intensity of the light which reaches the eye:—

7871, 5880, 4157, 3161, 3161, 4157, 5880, 7871, 9594, 10256, 10256, 9594, 7871, 5880, &c.

The bands here are well marked, but are not so vividly bright and black as in either of the former experiments.

The bands recur at an interval of 12 ordinates; and as 2 ordinates are now included in the space C, the bands recur at an interval of 6 C.

Thus it appears that, as result of theory, the interval of the bands is precisely the same whether the aperture of the eye be represented by 2, by 3, or by 4. I have gone through these proportions in detail; but any one who has watched the operation sufficiently to see upon what the succession of values in the numbers depends, will perceive that the recurrence of bands depends only on the recurrence in the values of R, and therefore that, whether a telescope or the naked eye be used, whether the spectrum be produced by diffraction or by refraction, and whatever be the refractive angle or dispersive power which produces it, provided the same retarding plate be used, the same bands will always be formed in the same part of the spectrum. The theoretical effect of altering the aperture of the eye (besides of course affecting the general brilliancy) is to render the bands more or less distinct without changing their places.

All these theoretical results are in absolute accordance with Prof. Powell's observations.

The fact, that the bands can sometimes be seen on only a portion of the spectrum, is a result of this circumstance, that the retarding plate cannot be placed close to the pupil of the eye. A single trial will enable any one to perceive that the further the plate is held from the eye, the smaller is the extent of visible bands; and the injurious effect of this circumstance is much greater when the pupil is small than when it is large; inasmuch as, with a very small pupil, the light which enters the eye from extreme points of the spectrum may not have touched the retarding plate at all.

I will conclude with observing that this very remarkable experiment, which long appeared inexplicable, and which at two subsequent times has presented startling difficulties to the theorist, seems destined to give one of the strongest confirmations to the Undulatory Theory.

I am, Gentlemen,
Your obedient Servant,
G. B. AIRY.

Royal Observatory, Greenwich,
Oct. 16, 1846.

LII. *Observations on the Law of Daily Temperature.* By Sir DAVID BREWSTER, *K.H., D.C.L., F.R.S., and V.P.R.S. Edin.*[*]

IT followed from the early researches of Humboldt, that the mean temperature of the equatorial regions was nearly the same under every meridian, but that in higher latitudes it declined rapidly in the New World, and under the eastern meridians of Asia, as shown in the following table:—

Latitude.	Temp. of Old World.	Temp. of New World.	Difference.
30	70°·52 Fahr.	66°·92	3°·60
40	63·14	54·50	8·64
50	50·90	37·94	12·96
60	40·64	23·72	16·92[†]

But notwithstanding the inflexion of the isothermal lines, as thus indicated, the Arctic and Antarctic poles were considered as the coldest points of the globe. Having found that in the West of Europe the mean annual temperature varied with the cosine of the latitude, and not, as had been believed,

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[*] The substance of the following article formed the concluding portion of a review of Baron Humboldt's *Asie Centrale*, published in the North British Review for August 1846, vol. v. p. 492.

[†] *Mém. d'Arcueil*, tom. iii. p. 462, and. Edin. Phil. Journal, vol. iii. pp. 4, 256; vol. iv. pp. 23, 262; and vol. v. p. 28.

with the square of the cosine, I was anxious to obtain observations in high latitudes in order to test the accuracy of his formula. Mr. Scoresby, to whom I made application, was fortunately able to state it as the result of numerous observations made in different years, that the mean temperature in the latitude of 78° was about 17°, from which he deduced that of the Pole to be 10°. But as the mean temperature in the latitude of 60° is 16° 92′ less in the Old World than the New, or in the meridian on which Mr. Scoresby approached the Pole, it is obvious that the mean temperature of the Pole, if deduced by the same process from the observations in the Old World, would be even below zero. This result was doubtless perplexing; but the perplexity was soon removed by the return of Captain Parry in 1820 from Melville Island. He found the mean annual temperature of that island, in $74\frac{3}{4}$° of latitude, to be + 1° 33′ Fahr., thus confirming the result which we had obtained and previously communicated to the Royal Society of Edinburgh[*]. This extraordinary result, taken in connexion with Mr. Scoresby's, suggested to us the idea that the coldest part of the globe was not far from Melville Island, and that there were "*two points of greatest cold* not many degrees from the poles, and in meridians nearly at right angles to that which passes through the west of Europe." From a comparison of a great number of mean temperatures both in the Old and New World in high latitudes, we were led to the conclusion that the two cold poles were situated in 80° of north latitude, and in 95° of east and 100° of west longitude[†],—the mean temperature of the eastern

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[*] Edinburgh Transactions, vol. ix. p. 212, Feb. 1820.

[†] In my treatise on Magnetism, I have adopted for the place of the American pole 100° W. long. and 73° N. lat., and the Asiatic pole in 80° E. long. and 73 N. lat., in order to represent better the observations of the Arctic voyagers, and in order to adapt them to the general formula given in the following note:—

"The formulæ for finding the mean temperature are as follows:—

$$\text{Asiatic pole, mean temp.} = 82^\circ{\cdot}8 \sin D$$
$$\text{American pole, mean temp.} = 86^\circ{\cdot}3 \sin D - 3\tfrac{1}{2}^\circ$$

The distance from the Pole, viz. D, is in the coldest meridian $D = 80^\circ -$ lat., and in the warmest meridian $\cos D = \cos 10^\circ \sin$ lat. In all intermediate meridians we have $\cos D = \frac{\cos L\,(\cos l - \theta)}{\cos \theta}$, and $\tan \theta = \cos M \tan L$, where M is the difference of longitude between the place and the pole, L the co-latitude of the isothermal pole or 10°, and l the co-latitude of the place.

"In order to obtain a general formula for the whole globe, we must suppose the temperature of both poles equal, and the cold meridians 180° apart. In this case we have

$$T = (t - \tau)\,(\sin^n \delta = \sin^n \delta') + \tau,$$

pole being + 1° Fahr., and that of the western — 3½°, and that they were nearly coincident with the magnetic poles of the earth. These views have been adopted by Humboldt, Erman, Kupffer, Dalton and others; and we have no doubt that we shall be able, by means of the accurate meteorological observations which are now making in different parts of the world, to fix more accurately the position of these remarkable points, and to determine whether or not their connexion with the magnetic poles is merely accidental.

This remarkable distribution of heat over the earth's surface, or rather in our atmosphere, is connected with the unequal distribution of heat over the year. In the cold meridians we have a hot summer and a cold winter, whereas in the warm meridians the difference between the summer and the winter temperature is greatly diminished. In order to express this conjoined effect, Humboldt adopts the form $51^\circ{\cdot}1\,\dfrac{65^\circ{\cdot}3}{38^\circ{\cdot}7}$ adding the signs of + and —, according as the numbers are above or below zero. Thus $+\,51^\circ{\cdot}1\,\dfrac{+\,65^\circ{\cdot}3}{+\,38^\circ{\cdot}7}$ expresses the temperature of Paris; the number 51°·1 being the mean annual temperature, 65°·3 the mean temperature of summer, and 38·7 the mean temperature of winter. This arrangement of the temperatures has, however, so much the appearance of a mathematical formula, that we conceive it would be more natural to place the numbers thus: + 65·3, + 51°·1, + 38°·7, or max. 65°·3, mean 51°·1, min. 38°·7, the mean temperature occupying the middle place between the two extremes, and the negative signs alone being prefixed to the numbers when they are below *zero*.

In treating of the cause of the inflexion of the isothermal lines, Baron Humboldt has endeavoured to show, by very ingenious arguments, that the form, the extent, and the altitude of the continents, and their relation to the polar ice, are the principal causes of these inflexions. We are not prepared either to controvert or to admit the accuracy of this explanation, which we have no doubt will meet with a ready and a general acceptance; but when we look at the system of isothermal curves surrounding the poles of revolution, and mark their coincidence with the magnetic poles of the earth, and their simi-

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where T is the mean temperature of any place whose distance from the two cold poles is δ and δ', t the mean equatorial temperature, τ the mean temperature at each of the poles of maximum cold. The distances δ and δ' are found by the formula for D given above. The exponent n is about five-eighths, when the formula represents the most accurate observations."

larity to the isodynamical magnetic curves*, we are disposed to view so remarkable a phænomenon as the result of a physical condition of the earth itself, and produced by causes connected with its magnetic, or galvanic, or chemical agencies.

Before we can determine, with anything like accuracy, the form of the isothermal lines, we must, in reference at least to past observations, be able to deduce from them the mean temperature of the places at which they were made; and this can only be done after we have determined the law of the distribution of heat throughout the day and the year, the *mean annual orbit* as it were, described by the summit of the mercurial column. Previous to the determination of the law of distribution, we could not obtain the mean annual temperature even from *three* daily observations, and still less from *two* or *one* observation. The mean of the *maximum* and *minimum* furnished us with an approximation; but as the instruments with which these extremes were measured were continually going wrong, the results which they yielded were always liable to error.

In order to solve this problem, I suggested to the Royal Society of Edinburgh the plan of establishing *hourly* observations at Leith on the *east* coast of Scotland. They were continued for four years, and yielded results of the most unexpected kind; but the observations for the first two years only, viz. for 1824 and 1825, have been published. We shall therefore in general confine ourselves to the results to which they lead. The first of these is the determination of the *hours of mean temperature* for the whole year, and the interval which elapses between these hours, which has been called the *Critical Interval.*

	Hour of morning, Mean temperature.	Hour of evening, Mean temperature.	Critical interval.
1824	9 h. 13 m. A.M.	8 h. 26 m. P.M.	11 h. 13 m.
1825	9 13 ...	8 28 ...	11 15
1826	9 7 ...	8 27½ ...	11 20
1827	9 12 ...	8 23 ...	11 11
Mean	9 11¼ ...	8 26 ...	11 14¾

Now it is a remarkable circumstance, that at Padua the critical interval is 11 hours 14 minutes; at Appenrade

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* This remarkable fact, which we first pointed out in the Edinburgh Transactions, vol. ix. pp. 223, 225, is fully admitted by M. Erman. "The thermometrical observations at Jakousk," says he, "confirm, in a remarkable manner, the relations which have been discovered between these temperatures and terrestrial magnetism; for at this place we arrive at once in the neighbourhood of the meridian of one of the magnetic poles, and in the meridian of the greatest cold which is known in the whole world."—*Reise um die Erde*, tom. ii. p. 250. See also the *Encyclop. Brit.* art. Magnetism, vol. xiii. p. 695.

11 hours 11 minutes; at Inverness, 11 hours 13 minutes*; at Kingussie, 10 hours 44 minutes; at Belleville, 11 hours 14 minutes; at Tweedsmuir, 11 hours 15 minutes; at Plymouth, 11 hours; at Philadelphia, 11 hours 20 minutes; at Trincomalee, 11 hours 5 minutes; at Colombo, 10 hours 55 minutes; at Kandy, 11 hours; and at Madras, 10 hours.

Another general law which we obtained from the hourly observations at Leith is, that the half sum of the mean temperatures of any two hours of the same name, differs at an average only a quarter of a degree of Fahrenheit from the mean temperature of the year, the maximum difference being 0°·421, and the minimum 0°·059, and the differences following a regular law. The following are the results of *four* years' hourly observations:—

Homonymous hours.		Differences between the half sum and the mean annual temperature.	
		Leith.	Inverness.
5 A.M.	5 P.M.	−0°·134 Fahr.	−0°·434
6	6	−0·281	−0·543
7	7	−0·372	−0·552
8	8	−0·421	−0·396
9	9	−0·285	−0·113
10	10	−0·086	+0·174
11	11	+0·176	+0·374
12	12	+0·374	+0·555
1 P.M.	1 A.M.	+0·367	+0·550
2	2	+0·366	+0·389
3	3	+0·252	+0·173
4	4	+0·059	−0·175
Mean difference		0·264	0·369

The law of the differences is here very interesting. At Leith the mean temperature of 4 h. A.M. and 4 h. P.M., and of 10 h. A.M. and 10 h. P.M., approaches nearest to that of the year; and at Inverness the hours of 9 h. A.M. and 9 h. P.M., and of 3 h. A.M. and 3 h. P.M. On this subject Humboldt remarks:—

"We are surprised, at the first glance, by the generality of this law. The *homonymous* hours are very unequally distant from the hour of the maximum of the daily temperature, and the hours of equal temperature, (we may say by analogy with the practice of astronomers in the determination of the true time, the *corresponding thermometrical altitudes,*) give for each place an epoch very different from that of the maximum. It it is a thing truly remarkable, that from the mean of two or-

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* Deduced from three years' hourly observations, made at Inverness by Mr. Thomas Mackenzie of Rayning's School, and at the expense of the British Association. The hours of mean temperature are 8 h. 28 m. A.M., and 7 h. 41 m.

dinates we may deduce the mean temperature of the whole year, that is to say, the mean of all the horary ordinates."

The next important relation which we have deduced from the hourly observations is, that which the mean temperature of each hour bears to that of the whole year. The following are the results obtained from four years' observations at Leith and three at Inverness:—

Hours.	Leith.	Inverness.	Difference between Leith and Inverness.
1 A.M.	−2°·131	−2°·553	+0°·422
2	−2·396	−2·788	+0·392
3	−2·658	−3·030	+0·372
4	−2·793	−3·086	+0·293
5	−2·844	−2·933	+0·089
6	−2·545	−2·376	−0·169
7	−1·956	−1·599	−0·357
8	−1·180	−0·557	−0·623
9	−0·160	+0·636	+0·796
10	+0·777	+1·752	+0·975
11	+1·702	+2·628	+0·926
12	+2·463	+3·347	+0·884
1 P.M.	+2·865	+3·654	+0·789
2	+3·125	+3·566	+0·441
3	+3·135	+3·376	+0·241
4	+2·927	+2·736	−0·191
5	+2·576	+2·063	−0·513
6	+1·984	+1·291	−0·693
7	+1·211	+0·495	−0·716
8	+0·362	−0·235	−0·597
9	−0·410	−0·862	−0·548
10	−0·949	−1·404	+0·455
11	−1·351	−1·880	+0·529
12	−1·713	−2·237	+0·524

The value of the preceding table is very great. It enables us in climates at least analogous to those of Leith and Inverness, and approximatively in others, to deduce the mean temperature of the year, from meteorological registers containing only a single daily observation. Thus, if the mean of all the observations in a register kept at 6^h P.M. was 51°·000, then, since the mean temperature of that hour exceeds the mean temperature of the day by 1°·984, we shall have 51° − 1°·984 = 49°·016 for the mean temperature of the place of observation. In like manner, when in meteorological registers, as in most of them, the observations have been made at two or three hours which do not give the mean temperature of the day, we can easily deduce from the preceding table the corrections which are necessary to make them give the mean annual temperature of the place of observation.

If we wish to discover at what two hours, or at what three hours, the observations should be made to discover the mean temperature, we have only to find in the above table the hours corresponding to two or three ordinates, with opposite signs whose sums shall be nothing; thus, 6 h. A.M. and 5 P.M. give −2·545 and + 2·576, whose sum is only 0°·031.

In like manner we may choose, in reference to our convenience, to select two times of observation, one of these being at a given hour, and the other between two hours; or it may be more convenient to take two times of the day, both of which are between regular hours. The following table shows these hours, and the intervals between the times of observation, as deduced from the Leith observations.

Hours of observation which give the mean.			Interval between the observations.	
1 h. A.M. and	11 h.	34 m. A.M.	10 h.	34 m.
2	11	29½	9	49½
3	12	29 P.M.	9	27
4	12	49	8	49
5	12	57	7	57
6	12	12	6	12
7	11	21	4	21
8	10	20	2	20
9	9	21	0	21
1 A.M.	5	45 P.M.	16	45
2	5	18	15	18
3	4	46	13	46
4	4	23	12	23
5	4	14	11	14
6	5	3	11	3
7	6	2	11	2
8	7	2	11	2
9	8	41	11	41
10	9	41	11	41
11	11	59	12	59
12	2	15	9	45
1 P.M.	5	0	8	0
5	5	54	11	6
6	6	57	11	3
7	7	58	11	2
8	8	48	11	12
9	9	37	11	23
10	10	11	11	49
11	10	37	12	23
12	11	1	12	59

If it is more suitable to have none of the hours an even number, we may combine the equal ordinates opposite 1 h. A.M. and 1 h. A.M. in the preceding tables, thus—

11 h. 34 m. A.M. and	5 h. 45 m. P.M.
11 49½	5 18

and so on with the other numbers.

The difference between the Leith and Inverness ordinates, as shown in the table, page 346, exhibits the effect of an increase of latitude in producing an approximation to a continental climate. The mornings at Inverness, from 1 h. A.M. till about 5 h. 20 m. A.M., are colder; and the days, from 9 h. A.M. till $3\frac{1}{2}$ h. P.M., are warmer in reference to the mean temperature than those at Leith; whereas, about the time of the morning and evening mean temperatures, it is warmer at Inverness in the morning and colder in the evening.

Having obtained the ordinates of the mean annual curve of the daily temperatures, it becomes a curious subject of inquiry if this curve has any resemblance to curves with which we are familiar. With this view we must divide it into four portions or branches, namely, that between the hours of the evening mean temperature and the minimum ordinate; that between the minimum ordinate and the morning mean; that between the morning mean and the maximum ordinate; and that between the maximum ordinate and the evening mean. In this way the mean annual curve of daily temperature will be divided into four different curves, which have a striking similarity to parabolas, as shown by the following table, which contains the mean annual hourly temperatures for 1824 and 1825, as observed, and as calculated on the supposition of the temperatures being the abscissæ of parabolas, and the horary intervals the ordinates.

Hours.	Observed temperatures.	Calculated temperatures.	Differences.
8 h. 27 m. P.M.	Mean 48°266	Mean 48°266	0°000
9	47·829	47·904	+0·075
10	47·276	47·315	+0·039
11	46·803	46·806	+0·003
12	46·398	46·374	−0·024
1 A.M.	46·134	46·021	−0·113
2	45·933	45·747	−0·186
3	45·689	45·551	−0·138
4	45·449	45·433	−0·016
5	Min. 45·394	Min. 45·394	0·000
6	45·653	45·555	−0·098
7	46·283	46·039	−0·244
8	47·079	46·845	−0·184
9	48·055	47·973	−0·082
9 13	Mean 48·266	Mean 48·266	0·000
10	49·012	49·091	+0·079
11	49·050	49·969	+0·019
12	50·777	50·653	−0·124
1 P.M.	51·149	51·141	−0·008
2	51·470	51·434	−0·036
3	Max. 51·532	51·532	0·000
4	51·239	51·422	+0·183
5	50·872	51·091	+0·219
6	50·294	50·544	+0·250
7	49·544	49·773	+0·229
8	48·624	48·783	+0·159
8 27	Mean 48·266	Mean 48·266	0·000

The coincidence between the observed and calculated results, as shown in this table, is so remarkable, that the *parabolic temperatures,* as we may call them, *never differ from the real temperatures more than* ONE QUARTER OF A DEGREE OF FAHRENHEIT.

Since we began this article we have been favoured by Robert Thom, Esq., of Ascog, a gentleman of great scientific attainments, with the results of an hourly meteorological register, which he kept for twelve consecutive years at Rothesay in the Isle of Bute, and which we have no scruple in saying forms the most important class of meteorological observations which have ever been made, not only for their number and continuity, but from the care and attention with which they were conducted. These observations possess also another interest, in so far as they exhibit the laws of temperature on the *west coast* of Scotland, and in a climate essentially different from that of the *east* coast, to which our Leith and Inverness observations relate. Our limits will not permit us to give the results of these observations so fully as we could wish, but we shall endeavour to lay the most important of them before our readers. The following are the times of mean temperature:—

	Mean of twelve years, from 1828—1832 inclusive.	Distances from noon.
Mean temp. at	8 h. 32 m. A.M. 47°·46	3 h. 28 m.
	7 39 P.M. 47°·46	7 39
Critical interval......		11 h. 7 m.

Hence it appears that the hours of mean temperature are much earlier than at Leith, and are nearly the same as those at Inverness, differing from them only by four minutes in the morning and five minutes in the evening, while the critical interval differs from that of Inverness only nine minutes.

The similarity between these two sets of observations will appear still more striking from the following law of the *homonymous* hours:—

Homonymous hours.		Difference between the half-sum and mean annual temperature.
5 h. A.M. and	5 h. P.M.	−0·30
6	6	−0·39
7	7	−0·49
8	8	−0·39
9	9	−0·36
10	10	−0·08
11	11	+0·22
12	12	+0·45
1 P.M.	1 A.M.	+0·56
2	2	+0·54
3	3	+0·30
4	4	−0·05
	Mean difference......	0·348

Hence it appears that the homonymous hours of mean temperature are 4 h. A.M. and 4 h. P.M., and 10 h. A.M. and 10 h. P.M., the one differing only 5-100ths, and the other only 8-100ths of a degree of Fahrenheit from the mean annual temperature; while the maximum deviation, exactly as at Inverness, occurs at 1 h. A.M. and 1 h. P.M., and at 7 h. A.M. and 7 h. P.M., amounting at Rothesay to + 0·56 and − 0·49, and at Inverness to 0·55, an agreement so remarkable that it cannot but surprise us.

The following table exhibits the difference between the temperatures of each hour of the day, and the mean annual temperature.

Hours.	Differences.	Difference between Rothesay and Inverness.	Hours.	Differences.	Difference between Rothesay and Inverness.
1 A.M.	−2°65	+0°10	1 P.M.	+3°78	+0°13
2	−2·82	+0·03	2	+3·90	+0·33
3	−3·02	+0·01	3	+3·63	+0·25
4	−3·13	+0·04	4	+3·02	+0·28
5	−2·87	+0·06	5	+2·28	+0·22
6	−2·26	−0·12	6	+1·48	+0·19
7	−1·49	−0·11	7	+0·52	+0·03
8	−0·50	−0·06	8	−0·28	+0·05
9	+0·44	−0·20	9	−1·16	+0·30
10	+1·54	−0·21	10	−1·69	+0·29
11	+2·52	−0·11	11	−2·08	+0·20
12	+3·29	−0·06	12	−2·39	+0·15

As Inverness is colder in the morning than Leith, so Rothesay is slightly colder than Inverness, from 1 h. A.M. till 4 h. A.M., then becomes slightly warmer till 1 h. P.M., from which time it continues colder till 12 at night.

With such a valuable series of observations as those at Rothesay, it became an object of great interest to ascertain the relation of the curve of annual temperature to the parabola. Before we saw these observations we intimated to Mr. Thom our conviction that the curve would deviate more from the parabola than that at Leith, owing *to the form of the visible horizon* at Rothesay being so different from the open horizon at Leith. If a hill rises to the north of the place of observation, by which the sun's rays are never obstructed, it can exercise little or no influence on the thermometer; but if one or more hills obstruct the sun's rays after he has risen above the true horizon, that obstruction must affect the temperature of the place of observation at the hours corresponding with the azimuth of the hill. That this cause has operated at Rothesay will be seen from the following table containing the observed temperatures, and those calculated upon the supposition that the branches of the curve are parabolic:—

Hours.	Observed temperatures.	Calculated.	Difference.
7 h. 39 m. P.M.	Mean 47°·46	47°·46	0°·00
8	47·18	47·18	0·00
9	46·30	46·48	0·18
10	45·77	45·87	+0·10
11	45·38	45·36	−0·02
12	45·07	44·95	−0·12
1 A.M.	44·81	44·64	−0·17
2	44·64	44.43	−0·21
3	44·44	44·31	−0·13
3 40	Min. 44·29	44·29	0·00
4	44·33	44·44	+0·11
5	44·59	44·53	−0·06
6	45·20	45·02	−0·18
7	45·97	45·78	−0·19
8	46·96	46·80	−0·16
8 32	Mean 47·46	47·46	0·00
9	47·90	48·14	+0·24
10	49·00	49·38	+0·38
11	49·98	50·33	+0·35
12	50·75	50·98	+0·23
1 P.M.	51·24	51·32	+0·08
1 40	Max. 51·39	51·39	0·00
2	51·36	51·38	+0·02
3	51·09	51·32	+0·23
4	50·48	50·62	+0·12
5	49·74	50·17	+0·43
6	48·94	49·33	+0·39
7	47·98	48·27	+0·29
7 39	Mean 47·46	47·46	0·00

From this very instructive table it is obvious that from eight o'clock at night till eight in the morning, that is during the average night, when the obstruction of the sun's rays by adjacent hills can have no effect upon the thermometer, the deviations from the parabola are very small, and less than those at Leith; whereas, from eight in the morning till eight at night, that is during the average day, the deviations are considerable when compared with those at Leith, arising, doubtless, from the obstruction of the sun's rays in particular azimuths. That this is the cause of the deviations may be inferred from their signs. During the night the deviations are *negative*, and during the day *positive*; that is during the night the observed temperatures are *greater* than the parabola makes them, owing to the radiation of cold from the obstructed portions of the sky being *less* than if it had not been obstructed; whereas during the day the deviations are large and *negative*, that is during the day the hills have obstructed the sun's rays in particular azimuths; and the observed temperatures are less than the parabola makes them.

These results were so important, that it became very desirable to ascertain the relation of the Inverness daily curve to the parabola, and to the form of the visible horizon. We have therefore made the necessary calculations, and obtained the following results:—

Hours.	Observed temperatures.	Calculated.	Differences.
7 h. 41 m.	Mean 46°·020	46°·020	−0°·00
8	45·785	45·97	+0·12
9	45·158	45·07	−0·09
10	44·616	44·46	−0·15
11	44·140	43·95	−0·19
12	43·783	43·54	−0·24
1 A.M.	43·467	43·23	−0·24
2	43·232	43·02	−0·21
3	42·990	42·92	−0·07
3 35	Min. 42·900	42·90	0·00
4	42·934	42·92	−0·01
5	43·087	43·21	+0·12
6	43·644	43·66	−0·02
7	44·421	44·42	0·00
8	45·463	45·45	−0·01
8 28	Mean 46·020	46·020	0·00
9	46·656	46·74	+0·08
10	47·772	47·87	+0·10
11	48·648	48·73	+0·08
12	49·367	49·39	−0·05
1 P.M.	49·674	49·64	−0·03
1 40	Max. 49·700	49·70	0·00
2	49·586	49·69	+0·10
3	49·396	49·52	+0·12
4	48·756	49·15	+0·39
5	48·083	48·57	+0·48
6	47·311	47·74	+0·43
7	46·515	46·81	+0·29
7 41	Mean 46·020	46·020	0·00

It appears from this table that in *three* of the *four* parabolic branches the deviation from the parabola is considerably less than in the Leith observations; whereas in the afternoon branch and in that part of it between the hours of four and eight, the deviation is nearly double of what it is in the other branches and in the Leith observations. The observed temperatures are less than they should be, and we can account for this only by the interposition of some obstacle in the azimuth of four, five, six, and seven o'clock P.M., or from the thermometer being better protected from extraneous influences at these particular hours*.

As there are many meteorological registers which have been kept only at two hours of the day, it became of importance to deduce from them the approximate hours of mean temperature,

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* We find upon looking at the Register, that the thermometer is placed in the shade in a window facing the north. The line of the building is 6° 15′ to the north of due west, and hence the thermometer is better protected from the sun in the afternoon than in the morning.

in order that by adopting these hours the true hours of mean temperature and the critical interval may be determined. In order to do this, we must, if we do not know it, deduce the mean temperature of the place of observation from the preceding or other tables, that is by correcting the temperatures in the register by the differences between the temperature in the table at the same hour and the mean temperature of the day. The mean of the two mean temperatures thus obtained may be considered as the mean temperature of the place. Then from the rates of ascent and descent at the times of the morning and evening mean, as given in the preceding tables, or otherwise ascertained, and the differences between the two registered temperatures and the mean temperature as already obtained, we may find the quantities to be added or subtracted from the interval between the two observations in order to obtain the critical interval.

LIII. *Observations on the Nature of Lampic Acid.*
By ARTHUR CONNELL, *Esq.*[*]

A FEW years ago I showed that this acid liquid, as prepared in the usual way by exposing the vapour of æther to the action of a coil of glowing platinum wire, contains a large quantity of formic acid[†]. The latest researches with which I am acquainted on lampic acid are those of Stass, Martens and Marchand, who, whilst they admit the presence of formic and acetic acid in it, maintain that it also contains a third acid, viz. the aldehydic. Although these researches were made some years ago, and I had seen short notices of them, it was not until lately that I had access to Erdman and Marchand's Journal of Practical Chemistry, which contains a full account of them[‡]; and I now offer a few observations upon them, with reference to the alleged existence of aldehydic acid in lampic acid.

I may observe in commencing, that even were it established that lampic acid contains a third acid besides the formic and acetic, it by no means follows that that third acid is to be viewed, as has been done by some chemists, as the peculiar and characteristic acid of the lampic mixture, and as confirming Liebig's original view, that lampic acid may be regarded as aldehydic acid. The very fact that the proportions of these

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[*] Read to the Chemical Section of the British Association, &c. held at Southampton, and communicated by the Author.

[†] Lond. and Edinb. Phil. Mag. vol. xi. p. 175.

[‡] Vols. xviii. and xix.

acids vary somewhat according to the mode of preparation, which seems to follow from the late experiments, negatives this idea; and indeed it is given up by Liebig, who now only says, in his Organic Chemistry, that aldehydic acid is *one* of the acids in lampic acid. But if we take the above-mentioned normal mode of preparing it,—the method principally used by Davy, Faraday and Daniell, then formic acid constitutes by much the larger proportion of the acid liquid so prepared; and as formic acid is well known to reduce the oxides of metals having feeble affinities for oxygen with effervescence, it is plain that this characteristic property of lampic acid may be with justice mainly ascribed to its formic constituent.

But the more important point is, whether lampic acid contains any aldehydic acid at all. On this, I think I shall be able to show that the experiments depended on are quite insufficient to establish this fact; and that my own experiments, as well as those of Mr. Daniell, are opposed to such a view. It is admitted on all hands that there are other matters besides acids present in the liquid to which the name of lampic acid is usually given. Of these aldehyde is certainly one. Now every one of the late experiments is quite explicable on the idea, which indeed is advocated by Martens, that this aldehyde is mixed or otherwise associated with the acetic acid which remains after the formic acid has been removed by means of oxide of lead or otherwise. Thus the acid supposed to be the aldehydic portion of lampic acid is described as reducing nitrate of silver without effervescence; as yielding a brown resin when heated with alkali, and as affording a copper salt, which when heated in solution gives a precipitate of suboxide of copper, whilst acetate of copper is formed and remains dissolved. Now as it is a character of aldehyde to reduce certain metals without effervescence, and to yield a resin when heated with an alkaline solution, all the above experiments are quite compatible with the idea that the acid is merely the acetic with associated aldehyde. The same view will readily explain Liebig's argument, founded on the darkening of the lampic salts on evaporation, and of the acid itself by sulphuric acid. All these are characters which the presence of aldehyde would produce.

But my own experiments, as well as those of Mr. Daniell, on the atomic weight of lampic acid, as prepared by the ordinary process, afford still more direct evidence on this point. By an analysis of the lampate of barytes I ascertained the atomic weight of the acid to be 50·35 or 629·4, oxygen = 100; and Mr. Daniell's result was quite the same. Now the atomic weight of formic acid is 37; that of acetic acid 51; that of the so-called aldehydic acid, $C^4 H^3 O^2$, 43; and that of aldehyde, $C^4 H^3 O + HO$, 44·06. It is evident then that an acid, of which a considerable part has been ascertained to be formic acid, could never have an atomic weight of 50·35 by mixture, either with acetic acid or with aldehydic acid; each of these of course taking up its own portion of base. The considerable mixture of formic acid would evidently keep the atomic weight *much* below that of acetic acid; whilst nothing short of the nearly total absence both of formic and aldehydic acids would give such an atomic weight as that found. But let us suppose the formic acid to be mixed with acetic acid, having some foreign body such as aldehyde associated with it and entering into the constitution of its salts, at least on their first formation; such a mixture would readily bring up the atomic weight of the mixed acid to the observed point. This becomes still more evident from the experiments formerly detailed, which had for their object to get quit of the associated foreign matter. The lampate of barytes was kept for a considerable time on the sand-bath at a temperature of from 300° F. to 320° F., during which operation it became dark brown, and resinous matter separated when it was dissolved in water, *but no part of the base except a mere trace was set at liberty.* The atomic weight of the acid, after repeating this process twice or thrice, was reduced from 50·35 to 44·95; and it was evident that it could have been reduced still further by repetitions of the process. The nature of this action I conceive to have been quite analogous to that of heating aldehyde with bases. It suffers decomposition and yields resinous matter. Supposing all the aldehyde to have been thus driven off, the acid united to barytes would then have been formic acid, mixed with a little acetic acid, and would have had an atomic weight a little above that of formic acid. It is the most obvious, if not the only mode of accounting for the considerable reduction of atomic weight without loss of base to suppose the expulsion of a foreign or associated matter.

I am aware, as already stated, that it has been of late alleged that the proportions of the constituents of lampic acid vary according to the mode of preparation. This is likely enough; but then I conceive that this variation only takes place in the relative proportions of formic and acetic acids, and of associated aldehyde or other foreign body. The greater the proportion of acetic acid and associated foreign matter, the more will those appearances be observed, which have led, erroneously as I conceive, to the supposition of the presence of a third acid, viz. the aldehydic. It is evident that no account of the properties and constitution of lampic acid can be correct which does not fully explain them aspossessed by the acid prepared in its ordinary way.

I adhere then to the view which I formerly gave, that lampic acid consists of formic and acetic acid, with one or more matters not of an acid nature. Of these, aldehyde is the substance which the experiments appear to point out as mixed or associated with the acetic portion of the liquid. This is probable in itself, because the formation of lampic acid is a process of oxidation of the elements of æther, whether in their separate state or in the hydrated condition of alcohol. The first stage of oxidation will produce aldehyde by removing two atoms of hydrogen. The next will convert a portion of aldehyde into acetic acid by adding two atoms of oxygen; and this acetic acid may, in the act of being formed, possibly associate itself with an undecomposed portion of aldehyde, if this association is not at a later period, when the union with a base is effected. It is at least certain that all the aldehyde is not oxidated, for free aldehyde is a part of the product. A further stage of oxidation will yield formic acid:—

$C^4 H^5 O$ æther.
$C^4 H^3 O$ anhydrous aldehyde.
$C^4 H^3 O^3$ anhydrous acetic acid.
$C^2 H O^3$ anhydrous formic acid.

It would not be difficult to state what appears to me to be the most probable view of the nature of the so-called aldehydic acid itself as obtained from other sources, but I am unwilling to make observations on a substance on which I have not experimented.

LIV. *On the great Symmetrical Barometric Wave of November.* *By* W. R. Birt.

[With a Plate.]

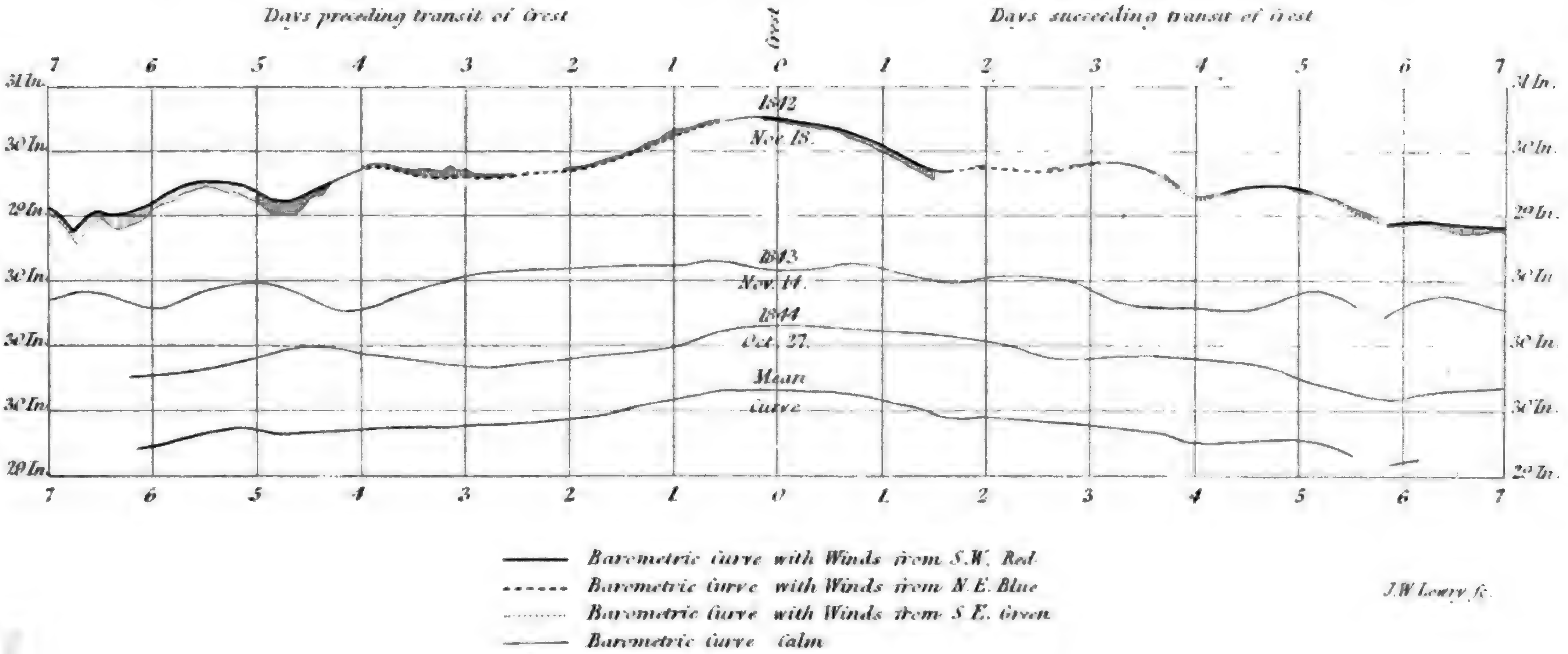
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE return of the great symmetrical barometric wave last autumn, with its appearance at Dublin in twelve out of seventeen years' observations between 1829 and 1845 inclusive, will render the barometric movements of the ensuing November highly interesting to the meteorologist. Allow me through the medium of your Journal to solicit the attention of observers to this interesting phænomenon, and to point out a few of its essential features by which it may be recognised should it revisit us during the present year.

The curves of the great symmetrical wave as observed at *London* in 1842, 1843, and 1844, with the mean of the three

Great Symmetrical Barometric Wave as observed at London in 1842 1843 & 1844 by W. R. Birt.



J. W. Lowry fc.

The extent of shading BELOW the curve is intended to shew the force of the S.W. winds, ABOVE that of the N.E. & S.E. winds.

years, are exhibited in Plate III. which accompanies this communication. This Plate illustrated my Report on Atmospheric Waves presented to the British Association for the Advancement of Science at the Cambridge meeting in 1845, and to this and my preceding report in the volume for 1844 the reader is referred for further information respecting this particular wave, and also on the subject of atmospheric waves generally. Since my second report was presented to the Association the great wave has been again observed, and the comparison of the curves for 1842, 1843, 1844, and 1845 has suggested the idea that we have obtained the *type* of the barometric oscillations during the middle portion of November. This type has been expressed in the following language:—"That during fourteen days in November, more or less equally disposed about the middle of the month, the oscillations of the barometer exhibit a remarkably symmetrical character; that is to say, the fall succeeding the transit of the maximum or highest reading designated by the word crest in the Plate, is to a great extent similar to the preceding rise. This rise and fall is not continuous or unbroken; in three out of four of the occasions on which it has been observed, it has been found to consist of *five* distinct elevations. The complete rise and fall has been termed the great symmetrical barometric wave of November, and as such has been considered to result from the transit of a large wave; but there is great reason to believe that while it may be due to the transit of a normal wave of about fourteen days' amplitude, it also exhibits the transits of *five* secondary superposed waves. At the setting in of the great November wave the barometer is generally *low*, sometimes below twenty-nine inches. This depression is succeeded by *two* well-marked undulations, varying from one to two days in duration. The central undulation, which also forms the apex of the great wave, is of larger extent, occupying from three to five days; when this has passed, two smaller undulations corresponding to those at the commencement of the wave make their appearance, and at the close of the last the wave terminates."

It is important to bear in mind that the symmetrical character of the wave is *confined* to a certain line of greatest symmetry, and that at stations on each side of this line the symmetry is more or less departed from, according to their distance from or proximity to it. It is also important to notice that this line of greatest symmetry possesses an erratic character. In the year 1842 it stretched from Dublin to Munich, Birmingham and *Brussels* being two important stations on it. Last year it appeared to be confined to the south of England;

Helstone being the station of greatest symmetry. The southern counties of England will under these circumstances present an area for the establishment of highly important stations. At the Orkneys (the extreme northern station in the series of observations last year), the symmetry was most strikingly departed from by the *anterior* portion of the wave being considerably *higher* than the posterior; while at Paris, the most southern station from which observations have yet been received, the opposite of this occurred, the *anterior* portion being the *lowest* and the *posterior* the *highest*. These phænomena are strictly in accordance with the notion of a wave coming up from the north-west and passing on towards the south-east, the direction in which the great symmetrical wave of 1842 travelled: as the anterior slope or front of the wave approached, the barometric readings would be lowest at the south-eastern stations, while the reverse would occur as the posterior slope or back of the wave passed off; the readings in the north-west would be lower than those in the south-east.

On its last return observations were made on the great wave at thirty-four stations extending from the Orkneys to Paris, and from the west of Ireland to Heligoland: should it return during the present autumn, by multiplying observations on it, especially in the southern part of England, we shall be in a much better position to examine it under every possible aspect. The Honourable the Corporation of the Trinity House have directed observations to be made at several lighthouses, and Capt. Beaufort has kindly undertaken to obtain observations from several of our surveying vessels. In the Philosophical Magazine for September 1845, the reader will find instructions for making the necessary observations; and I shall be most happy to forward copies of these instructions, with the requisite forms for recording the observations, on being applied to for that purpose. Observations from any locality will be valuable, but those from any part of England or Wales included by two lines, the northern extending from Dublin to Harwich, and the southern from the Scilly Isles to Calais, will be of great importance in order to determine the direction of the line of greatest symmetry, and to ascertain the character and amount of its variations as compared with its position in former years.

In addition to the great symmetrical wave, I beg to solicit the attention of your readers to a most remarkable depression of the barometer on or about the 28th of November, which has been observed at Dublin in fifteen out of seventeen years. For the knowledge of this depression I am indebted to Capt. Larcom of the Royal Engineers, who has most kindly furnished me with curves of the barometric oscillations during November from 1829 to 1845 inclusive: I subjoin a table of the epochs and values of this depression.

Year.	Epoch of minimum.	Value.
1829	Barometer falling.	
1830	Nov. 27	29·40
1832	... 28	29·19
1833	... 28	28·31
1834	... 28	28·97
1835	... 30	28·86
1836	... 27	28·81
1837	... 28	29·26
1838	... 28	27·77
1839	... 29	29·08
1841	... 29	28·47
1842	... 27	28·56
1843	... 26	29·15
1844	... 28	29·74
1845	... 29	29·08

I have the honour to be,
Gentlemen,
Your most obedient humble Servant,
W. R. Birt.

2 Sidney Place, Cambridge Road, Bethnal Green.
London, October 16, 1846.

LV. *Notices of the Superposition of certain Minerals in some of the Metalliferous Deposits of Cornwall and Devon. By* William Jory Henwood, *F.R.S., F.G.S., Chief Commissioner of the Gongo Soco and Catta Preta Gold Mines.*

To Richard Taylor, Esq.

My dear Sir,

THE interesting communications of Messrs. Fox[*] and Dana[†] induce me to trouble you with an abstract of observations on the superposition of certain minerals, made during my examination of the mines of Cornwall and Devon[‡].

To enumerate all the substances of which the *lodes* in that district are composed and their respective localities, would be a tedious task, and at present perhaps not a very useful one: I have therefore tabulated the order in which only the more

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[*] Lond. Edin. and Dublin Phil. Mag. vol. xxviii. p. 5. [†] Ibid. p. 49.

[‡] Cornwall Geological Transactions, vol. v. p. 214, completed in 1842, published 1843.

abundant of them occur. The nature of the (*country*) containing rock is prefixed:—the first column denotes the mineral adjoining the (*wall*) side of the *lode*; the second, that which rests on that named in the first; the third, the substance overlying the second, and so on: the last column contains the names of some of the localities. The italic letter denotes the crystalline; the Roman, the massive minerals.

Mineral adjoining *wall* of *lode*.	Mineral resting on that in first column.	Mineral resting on that in second column.	Mineral resting on that in third column.	Localities.
Containing rock—GRANITE.				
Quartz	*Quartz*	....	......	Numerous.
Quartz	Opal	....	......	Wheal Cairn.
Quartz	*Quartz*	Chalcedony	......	Pedn-an-drea.
Quartz	*Quartz*	*Arseniate of iron*	......	Wheal Gorland.
Quartz	*Quartz*	*Wolfram*	......	St. Michael's Mount.
Quartz	*Quartz*	*Arseniate of copper*	......	Wheal Unity.
Quartz	*Quartz*	*Uranite*	......	Gunnis Lake.
Quartz	*Oxide of tin*	*Tungstate of lime*	......	Wheal Friendship.
Quartz	Native copper	*Red oxide of copper*	......	Wheal Gorland.
Quartz	*Malachite*	....	......	Gunnis Lake.
Quartz	Mineral pitch	....	......	East Wheal Crofty.
Amethystine quartz.	Quartz	....	......	Wheal Bellon.
Amethystine quartz.	*Amethystine quartz*	....	......	Dartmoor.
Felspar	Phosphate of iron	....	......	Park-noweth.
Fluor	*Fluor*	*Quartz*	......	Wheal Gorland.
Oxide of tin	....	....	......	All tin mines in granite.
Oxide of tin	*Sulphuret of bismuth*	....	......	Balleswidden.
Hematite iron ore	*Specular iron ore*	....	......	Park-noweth.
Earthy brown iron ore	*Vitreous copper ore*	Black copper ore	......	Wheal Jewel.
Earthy brown iron ore	*Red oxide of copper*	....	......	Wheal Gorland.
Containing rock—GREENSTONE.				
Quartz	Stalactitic quartz	*Quartz*	......	Wheal Edward.
Quartz	*Quartz*	*Arragonite*	......	Levant.
Quartz	*Quartz*	*Hydrous oxide of iron*	......	Restormel.
Quartz	*Quartz*	*Wolfram*	......	Poldice.
Quartz	*Quartz*	*Arseniate of copper*	......	Wheal Unity.
Quartz	*Quartz*	*Arseniate of lead*	......	Wheal Unity.
Quartz	Chlorite	*Oxide of tin*	......	Wheal Vor.
Quartz	Chlorite	*Arseniate of lead*	......	Wheal Unity.
Quartz	Fluor	*Fluor*	......	Wheal Unity Wood.
Quartz	Arsenical pyrites	*Arsenical pyrites*	......	Wheal Unity Wood.
Quartz	Earthy brown iron ore	*Phosphate of copper.*	......	Gunnis Lake.
Quartz	Earthy brown iron ore	Pitch-blende	......	Wheal Edward.
Quartz	Earthy brown iron ore	*Uranite*	......	Wheal Edward.
Quartz	Earthy brown iron ore	Vitreous copper ore	......	Botallack.
Quartz	Carbonate of iron	*Spathose iron ore*	......	Botallack.
Quartz	Vitreous copper ore	*Arragonite*	......	Levant.
Quartz	Chlorite	Copper pyrites	Mineral pitch	North Roskear.
Containing rock—FELSPAR PORPHYRY (Elvan).				
Oxide of tin	Oxide of tin	....	......	Wherry.
Silicate of tin	....	....	......	Wheal Coates.
Quartz	Earthy brown iron ore	*Blue carbonate of copper*	......	Ting Tang.
Quartz	Earthy brown iron ore	*Malachite*	......	Ting Tang.
Quartz	Copper pyrites	....	......	Ting Tang.
Earthy brown iron ore	Copper pyrites	....	......	Ting Tang.
Earthy brown iron ore	Native copper	....	......	Wheal Buller.
Earthy brown iron ore	Vitreous copper ore	....	......	Ting Tang.
Earthy brown iron ore	*Red oxide of copper*	....	......	Ting Tang.
Earthy brown iron ore	*Arseniate of copper.*	....	......	Ting Tang.
Earthy brown iron ore	Crysocolla	....	......	Ting Tang.

neral adjoining wall of lode.	Mineral resting on that in first column.	Mineral resting on that in second column.	Mineral resting on that in third column.	Localities.
		Containing rock—CLAY-SLATE.		
uartz	*Quartz*	*Quartz*		Wheal Friendship.
uartz	*Quartz*	*Copper pyrites*	*Quartz*	East Crinnis.
uartz	*Quartz*	*Sulphate of barytes*		United Mines.
uartz	*Quartz*	Copper pyrites	*Copper pyrites*	United Hills.
uartz	*Quartz*	*Sulphuret of antimony*		Pengelly.
uartz	Chlorite	Oxide of titanium		Virtuous Lady.
uartz	*Quartz*	*Blende*	*Fluor*	Polberrow.
uartz	*Quartz*	*Celestine*		Binner Downs.
uartz	Fluor	Galena		Wheal Penrose.
uartz	Iron pyrites	*Quartz*		West Pink.
uartz	Iron pyrites			Numerous.
uartz	Iron pyrites	Carbonate of iron	*Spathose iron ore*	Virtuous Lady.
uartz	Iron pyrites	*Phosphate of iron*		Wheal Falmouth.
uartz	Iron pyrites	Sulphuret of silver		Dolwath.
uartz	Earthy brown iron ore	*Red oxide of copper*		Wheal Charlotte.
uartz	Earthy brown iron ore	*Carbonate of lead*		Pentire-glaze.
uartz	Earthy brown iron ore	*Phosphate of lead*		Wheal Alfred.
uartz	Earthy brown iron ore	*Sulphate of lead*		Mellanear.
uartz	Hematite iron ore	Oxide of manganese		Restormel.
uartz	*Wood tin*			Polberrow.
uartz	*Oxide of tin*			Numerous.
uartz	Native silver			Herland.
uartz	Sulphuret of silver			Wheal Brothers.
uartz	*Red silver ore*			Dolcoath.
uartz	Native copper			Numerous.
Quartz	Vitreous copper ore	*Vitreous copper ore*		Wheal Speed.
Quartz	Vitreous copper ore	*Capillary red oxide of copper*		Providence.
Quartz	*Buntkupfererz*			Wheal Falmouth.
Quartz	Copper pyrites			Numerous.
Quartz	Copper pyrites	*Sulphuret of bismuth*		Fowey Consols.
Quartz	*Tennantite*			Fowey Consols.
Quartz	Copper pyrites	*Fluor*		Polberrow.
Quartz	*Red oxide of copper*			Numerous.
Quartz	Galena	*Galena*	*Quartz*	Wheal Rose.
Quartz	*Blende*	Pearl-spar		Union Mines.
Quartz	*Blende*	*Fluor*		West Pink.
Quartz	*Galena*	*Blue lead ore*		Wheal Hope.
Quartz	Blende	*Blende*		Union Mines.
Quartz	Mineral pitch			South Towan.
Quartz	Carbonate of lime			Binner Downs.
Chlorite	*Oxide of tin*			Numerous.
Pearl-spar	*Copper pyrites*			Cann Quarry.
Fluor	Copper pyrites			Wheal Unity Wood.

As the foregoing table is a first attempt at arranging these facts, it will doubtless be found susceptible of many improvements.

For the present I abstain from mentioning inferences, though many are obvious enough.

I remain, my dear Sir,
Your most faithful, humble Servant,
W. J. HENWOOD.

Gongo Soco, Brazil, June 29, 1846.

LVI. *On the probable Conduction of Galvanic Electricity through Moist Air.* By CHARLES G. PAGE, *M.D., Prof. Chem. and Pharm., Columbian College, Washington, D.C.**

SEVEN or eight years since I observed a curious fact, which led me to the conclusion that many substances considered

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* From Silliman's Journal, September 1846.

as perfect insulators, or rather non-conductors of galvanic electricity, might under certain circumstances prove conductors. I took two large sheets of zinc and coiled them together, the two sheets being separated by a layer of India rubber cloth. The sheets of zinc were connected respectively with the poles of a battery, and when the battery consisted of a single pair, no appreciable current passed from plate to plate through the India rubber cloth. When however the battery consisted of twenty compound pairs, a slight current passed, as indicated by a delicate galvanoscope. I made no further investigation or application of this fact till 1843, when recurrence was had to this feature to solve some difficulties experienced in the projection of a line of Morse's telegraph between this place and Baltimore. Ten miles of lead pipe containing four well-insulated wires had been laid in the ground, and upon trying these wires respectively, making the wire one-half of the circuit, and the lead pipe the other half, with a battery of intensity, a current could be established through any one of the three *extra* wires. Being consulted by Prof. Morse as to the probable cause of this *cross firing*, as it was technically called, the solution seemed to me to be obvious in view of the above experiment. The reasoning was sufficiently plausible to induce Prof. Morse to abandon the undertaking of the pipe, and resort to his original plan of raising the wire upon posts. The explanation was simply this, viz. that the insulating or non-conducting material would under any circumstances conduct the current; but that in some cases the amount transmitted could not be appreciated by any known test however delicate, was a *postulate* subsequently borne out. Does not such a conclusion follow directly from the law of Pouillet, that the conducting power of wires is directly as their cross section and some inverse ratio of their length, in connexion with the well-established laws of the different conducting powers of metals? For example, copper having from four to six times the conducting power of iron, a wire of iron, to equal in this respect a wire of copper, should be of from four to six times the size. Let this rule be applied to poorer conductors, and we may infer that the poorest conductor, or what has been usually considered a non-conductor, would become a conductor, if the area of its cross section were indefinitely increased and its length remained nearly nothing. In the case of the wires in the lead pipe—the one for instance joining the two poles of the battery—is separated from that lying next to it and the lead pipe, by a layer of thin non-conducting material throughout its length; and if we suppose the width of this layer in contact to be $\frac{1}{8}$th of an inch, the length of ten miles would give a surface of 550 square feet, while the length of the conductor would be only the thickness of the material, and a constant quantity, for any length of wires and tube, and increase of surfaces in contact. A full realization of the principle appears in the fact, that although the earth is a much poorer conductor than copper, mass for mass, yet upon the telegraphic routes it is found that the earth is a much better conductor than the copper wires used, the mass of the former being indefinitely greater than the latter.

These phænomena induced me to try the following experiment, in order to ascertain if the air might not act as a conductor. The roof of the patent office building covered with copper, exposes to the air twenty-two thousand square feet of this metal, and thus affords an enormous surface for conduction. A wire was connected with the metal of the roof, another wire with a plate of zinc of about four square feet. The free ends of these two wires were connected with a galvanoscope of exceeding sensitiveness, and with matters thus arranged, the zinc plate was insulated from the earth and building in the open air, and when the upper surface of the zinc plate was moistened with water, or what proved still better, acidulated water, the needle of the galvanoscope was deflected from two to five degrees. There was a slight drizzling rain at the time. Before the zinc plate was moistened no action was noticed. The inference from this experiment seems safely to warrant the position that a moist atmosphere conducts galvanic electricity. Many years since I proposed in this Journal a plan for ascertaining the level of the water in steam-boilers consisting of a zinc plate or a pair of plates, which should indicate the failure of water in the boiler by the cessation of action upon a galvanoscope placed in any convenient position outside the boiler. I have never had opportunity to test this device, but was at the time somewhat apprehensive that pure steam might act as a conductor and thus defeat the invention. I have recently been informed, though not in a direct manner, that the experiment had been tried in Philadelphia, and that the steam acted as a conductor. Whether dry air could act at all as a conductor remains yet to be ascertained, and I shall be able soon to put it to the test. Immediately after the above experiment was tried, the zinc plate was buried in the earth, other things remaining the same. The galvanoscope is inside a window in the second story, where I am enabled to watch it through most of the day. To my surprise, as soon as connexion was made with the plates, one being the copper roofing and the other buried vertically in the earth, the needle was deflected forty degrees, and the quantity of electricity afforded was sufficient to work the magnets used in Morse's telegraph, as witnessed by the Professor, and others present. Is the conduction through the material of the building or through the earth and intervening atmosphere? Probably through the mass of the building, which is constructed of blocks of sandstone, the walls being 2½ ft. in thickness. A small piece of this stone was found not to conduct this current perceptibly, either dry or when moistened with water. But when moistened with acidulated water, the current passed feebly. The fact is interesting whether the air or sandstone becomes the conductor; for if the conduction be through the building, it is through a material which in a block of eight or ten cubic inches is apparently a non-conductor, but which in the aggregate of the immense pile of the edifice is a conductor. The extreme sensitiveness of the galvanoscope is evident from its frequent disturbances from the slightest causes. I may safely say that the needle is affected by a flash of lightning one hundred miles distant. Whenever a thunder cloud is visible, the needle is deflected at each flash of lightning, and the deflection is in one or the other direction, as the induced current varies according to the direction of the lightning. When the thunder cloud is near, the action upon the needle is very strong and has several times twisted it suddenly off from the silken fibre to which it is attached. When no cloud is visible in the horizon, the needle on certain days—particularly at noon when thunder storms most frequently occur—is subject to frequent disturbances, resembling the former. I may remark here, as evidence of the rapidity of induction, the movement of the needle and flash of lightning appear simultaneous to the eye.

There are some extraordinary influences upon the needle, having a kind of periodicity, which cannot yet be accounted for, or identified with any meteorological fluctuations. There are also regular changes, which have thus far been noticed during the day. In the morning the current is at its maximum. About 10 o'clock A.M. it declines, and gets to its minimum about half-past 2 P.M., when the needle begins to return and arrives within four or five degrees of its maximum of deflection at 8 P.M. Whether this point observed at 8 A.M. is the real maximum is not known, as I have not been able to observe it in the night. The range of variation from morning to night is about ten degrees. I have not been able to notice any irregularity in these changes except as to the time. The irregular disturbances are very interesting, and may be identical with the *magnetic storms* of Gauss. Upon certain days they are hardly perceptible, though they never cease altogether. On some days they are violent, if I may be allowed the expression. The needle does not take a sudden start and return as when influenced by lightning, but moves gradually without oscillation to some fixed point, from which it will return sometimes in two minutes and sometimes in ten or fifteen minutes. An extended series of observations will be necessary before any deductions can be safely made. If the wires should be separated by a slight interval during a thunder storm, doubtless electrical sparks would be visible. During heavy storms, a flash of lightning twenty miles distant from the wires of Morse's telegraph will induce electricity in the wire sufficient to operate the magnets and work the telegraph, sometimes recording several signals. A flash of lightning in Baltimore, forty miles distant from this place, will operate the magnet at this end of the line.

Washington, D. C.

LVII. *Value of different kinds of Vegetable Food, based upon the amount of Nitrogen.* By E. N. HORSFORD, *M.A.*[*]

SINCE Gay-Lussac's discovery of nitrogen in the seeds of plants, the conception of animal nutrition has been assuming a more and more definite character.

Already have the principal proximate ingredients of meal, by taking advantage of its physical properties, been separated from each other. Gluten, albumen and legumine, starch, gum, sugar, dextrine and woody-fibre, have been made known, and their physical as well as some of their chemical properties[†] studied. Their more accurate chemical constitution was reserved to a later period, when it was found that they might be arranged in two classes, those containing nitrogen and those containing no nitrogen; and the interesting discovery was made, that the former, as well as the latter, are among themselves identical in composition.

It is well known that labourers supplied only with food containing no nitrogen, become incapable of executing their tasks: and further, that the corporeal system, even without labour, cannot be *sustained* upon such food. The discovery of the near identity in chemical composition between vegetable albumen, fibrine and caseine, and the corresponding bodies

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[*] Communicated by the Author.

[†] Fr. Marcet found gluten consisting of 55·7 per cent. carbon, 22 per cent. oxygen, 7·8 per cent. hydrogen, and 14·5 per cent. nitrogen.—*Ann. de Ch. et de Phys.* xxxvi. 27.

found in the animal kingdom, gave the above fact its explanation.

The food must contain an ingredient suited to replace the animal matter consumed.

This being known, and the quantitative relation of the several elements of the nitrogenous compounds being also known, an estimate of the value of given kinds of food becomes, in the hands of the chemist, a problem of comparatively easy solution.

The following investigation, undertaken at the suggestion and under the direction of Prof. von Liebig in the Giessen laboratory, had for its object the determination of the relative values of the different kinds of *vegetable food*. These values are *threefold*.

The various forms of food derived from grains, herbage and roots, furnish—1, bodies containing nitrogen; 2, bodies destitute of nitrogen; and 3, inorganic salts, all of which are serviceable in the animal œconomy.

The nitrogenous bodies, from their solution in the blood, form the tissues—the actual organism. The bodies wanting nitrogen contribute, by their more or less perfect combustion, to the warmth of the animal body; and the salts of the alkalies and alkaline earths serve in building up the osseous framework, beside constituting an essential part of every organ of the animal system.

Their values for the latter purpose are in general in proportion to the phosphates their ashes contain.

Their values for the second purpose may be considered in general as in the inverted relation of their values for the first, or inversely proportioned to the per-centage of nitrogen.

Their values for the first purpose, that of ministering to the support and growth of organic tissues, have been the specific object of the hereafter enumerated determinations.

Boussingault, to whom the agriculturist is so largely indebted for investigations bearing upon the interests of husbandry, has not left this field untrodden. It was thought, however, that the worth of his table of nutrition could lose nothing by a series of carefully conducted analyses, having the same object in view. It was moreover conceived, that in substances containing so small a per-centage of nitrogen as grains and roots generally, the method of Messrs. Varrentrapp and Will for determining nitrogen would give more accurate results than that of Dumas employed by Boussingault. The analyses hereafter given of the same substance, rarely varied from each other more than one-tenth of one per cent: and yet in general the determinations which follow, and those of similar substances made by the distinguished French chemist, differ no further from each other than might be expected from productions of the same vegetable variety grown on different soils.

Buckwheat (*Polygonum Fagopyrum*) constitutes an exception to this remark. In the table of analytical results* this grain has a nitrogen per-centage of 2·40, while two ordinary varieties of wheat (*Triticum vulgare*) have 2·33 and 2·30 per cent. Buckwheat meal from Vienna gave, as shown below, 1·08 per cent. Buckwheat grains (*Polygonum tartaricum*) from the experimental field of Hohenheim Agricultural Institute, gave 1·56 per cent. of nitrogen, while the analysis of three superior varieties of wheat grown in the same field, gave respectively 2·59, 2·68 and 2·69 per cent. This species of buckwheat was further found to contain 22·66 per cent. of woody fibre.

The equivalent value of buckwheat, according to Boussingault, wheat being 100, is 108. The following analyses gave for its equivalent value 170; for that of the Vienna buckwheat meal 245.

For the following investigation, the meals, table-peas and beans and lentils, were procured by Prof. von Liebig from Vienna. The grains, with the exception of rice and *Triticum monococcum*, were furnished from the Agricultural Institute at Hohenheim in the kingdom of Würtemberg, in reply to an order for the most approved varieties of Cerealia cultivated in Europe. The roots were from Giessen.

The several grains, meals and roots, in their market condition, were dried in a water-bath at 100° C. The potatoes, beets, carrots and turnips, were cut into thin shavings with the least delay, weighed between watch-glasses and placed in the water-bath.

The combustions for carbon and hydrogen were made with oxide of copper, a mixture of chlorate of potassa and oxide having been placed at the extreme end of the combustion-tube.

It was found difficult to reduce the woody fibre of the oats, barley and buckwheat, to the requisite fineness for a complete combustion. Where the difficulty could not readily be overcome, in addition to the chlorate of potassa at the end of the tube, it was found well, in filling with mixed substances and oxide of copper, to add, at intervals of an inch and a half, a small quantity of finely pulverized and thoroughly mixed oxide of copper and chlorate of potassa. The successive evolutions of oxygen in this case thoroughly reoxidized any por-

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* Boussingault's *Landswirthschaft*, Germ. ed., p. 294.

tions of copper reduced in the progress of combustion, and secured the most satisfactory results.

Difficulty presented itself also in the combustion of the beets and other roots, arising from the extreme compactness of the substance when dried. It was however overcome by the method already mentioned.

The per-centage of woody fibre was determined in the following manner. Grains, such as had been analysed, were digested upon a sand-bath, several weeks, in diluted hydrochloric acid,—one part of acid to a thousand of water. At intervals of from eight to ten days the fluid was poured off, and the digestion resumed with diluted acid, as before. After several weeks, the woody fibre not appearing wholly freed from other substances, an equally dilute solution of caustic potassa was employed, and the digestion therewith continued. At the end of two months, the woody fibre of the oats, barley and buckwheat, were poured upon filters, thoroughly washed with distilled water, and dried at 100° C. The hulls of the peas and beans were treated with diluted caustic potassa alone, and similarly determined.

To express in 100 parts the results of analyses, the carbon, hydrogen, oxygen and sulphur of the nitrogenous ingredients were estimated from the per-centage of nitrogen[*]. The carbon and hydrogen so estimated, deducted from the whole per-centage of carbon and hydrogen, gave what belonged to the starch, gum, sugar, &c. The oxygen of the latter was estimated from the carbon by the formula $C_{12} H_{10} O_{10}$[†].

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[*] Mulder's analysis of coagulated albumen, Scheerer and Jones's analyses of legumine and gluten (*Ann. de Ch. und Pharm.* xxxix. 360), and Heldt's analysis of the gluten of rye (*Ann. de Ch. und Pharm.* xlv. 198), differ so little from each other, that a single formula has been constructed upon Mulder's analysis of coagulated albumen, and, with a little modification, employed in determining the elements of the nitrogenous ingredients of all the substances analysed. His per-centage of oxygen was reduced by the per-centage of sulphur, which, according to Dr. Rüling (*Ann. de Ch. und Pharm.* lviii. 3 Heft.), for gluten = 1·14 per cent. and for legumine 0·50 per cent. The phosphorus has been neglected, as no accurate determinations of the per-centage of this element has been made. Mucine, discovered by Berzelius, is included in the estimate for the nitrogenous ingredients, since analyses of albumen and of gluten, with and without mucine, correspond with each other.

[†] The following list of the principal organic bodies containing no nitrogen, and present in the substances analysed, and their adjoined constitution, will justify the method of calculation :—

Amylum	$C_{12} H_{10} O_{10}$.
Dextrine	
Gum	
Woody fibre (Payen)	
Cane-sugar	$C_{12} H_{11} O_{11}$.

No. 1. *Wheat-flour from Vienna.*

I. 1·0883 gramme lost at 100° C. 0·9378 grm.

II. 1·3090 grm. of the flour dried at 100° C. left, after incineration, 0·0091 grm.

III. 0·8620 grm. of the same left 0·0061 grm. ashes.

IV. 0·3805 grm. burned with oxide of copper, gave 0·6370 grm. of carbonic acid and 0·2186 grm. of water.

V. 0·4190 gr. gave 0·7020 gr. CO^2 and 0·2550 gr. HO.

VI. 0·3671 gr. gave 0·6189 gr. CO^2 and 0·2374 gr. HO.

VII. 0·8078 gr. gave, by Varrentrapp and Will's method for determining nitrogen, 0·3925 gr. ammonio-chloride of platinum.

VIII. 0·8168 gr. gave 0·3893 gr. ammonio-chloride of platinum.

These determinations correspond, expressed in 100 parts, to—

	I.	II.	III.
Carbon ...	45·66	45·69	45·97
Hydrogen	6·38	6·76	6·96
Nitrogen .	3·04	2·99	
Ashes ...	0·69	0·70	
Water ...	13·83		

Estimated in 100 parts, according to the constitution of the ingredients present, the above determinations give the following numbers :—

Nitrogenous ingredients.	Nitrogen...	3·00	= 19·15
	Carbon ...	10·53	
	Hydrogen	1·31	
	Oxygen ...	4·08	
	Sulphur ...	0·23	
Ingredients containing no nitrogen.	Carbon ...	35·23	= 79·77
	Hydrogen	5·39	
	Oxygen ...	39·15	
	Ashes..................		= 0·70
			99·62

No. 2. *Wheat-flour from Vienna.*

I. 3·6365 grms. lost at 100° C. 0·4964 grm.

II. 1·2599 gr. of substance dried at 100° C. left, after incineration, 0·0084 gr.

Pectic acid, dried at 100° C.—
(Regnault) .... $C_{12}\,H_8\,O_{11}$.
(Mulder).................... $C_{12}\,H_8\,O_{10}$.
Pectine combined with PbO.
(Fremy) .... $C_{10}\,H_9\,O_{11}$.

III. 0·3643 gr. gave 0·6015 gr. CO^2 and 0·2175 gr. HO.

IV. 0·5429 gr. gave 0·9022 gr. CO^2 and 0·1350 gr. HO.

V. 0·8705 gr. gave, by the method of Varrentrapp and Will, 0·2974 gr. ammonio-chloride of platinum.

VI. 0·698 gr. gave 0·2331 gr. ammonio-chloride of platinum.

Expressed in 100 parts, the above give—

	I.	II.
Carbon ...	45·03	45·32
Hydrogen .	6·67	6·63
Nitrogen ...	2·14	2·10
Ashes	0·67	
Water......	13·65	

Estimated in 100 parts, according to the method described at page 368, the above determinations yield the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·12	= 13·53
	Carbon ...	7·44	
	Hydrogen .	0·93	
	Oxygen ...	2·89	
	Sulphur ...	0·15	
Non-nitrogenous ingredients.	Carbon ...	37·73	= 85·37
	Hydrogen	5·72	
	Oxygen...	41·92	
	Ashes		= 0·66
			99·56

No. 3. *Wheat-flour from Vienna.*

I. 3·5345 grms. lost at 100° C. 0·4500 gr.

II. 4·4785 grs. of substance dried at 100° C. gave 0·0497 gr. ashes.

III. 0·5545 gr. gave 0·9339 gr. CO^2 and 0·3377 gr. HO.

IV. 0·3310 gr. gave 0·5655 gr. CO^2 and 0·2031 gr. HO.

V. 0·6405 gr. gave 0·3514 gr. of ammonio-chloride of platinum.

These determinations, expressed in 100 parts, give—

	I.	II.
Carbon......	45·93	46·59
Hydrogen	6·76	6·81
Nitrogen ...	3·44	
Ashes	1·11	
Water	12·73	

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	3·44	= 21·93
	Carbon ...	12·08	
	Hydrogen .	1·50	
	Oxygen ...	4·68	
	Sulphur ...	0·25	
Ingredients containing no nitrogen.	Carbon ...	34·78	= 78·03
	Hydrogen	5·28	
	Oxygen ...	37·97	
	Ashes.................		= 1·10
			101·06

Talavera Wheat (Triticum vulgare) *from Hohenheim.*

This variety is of high reputation as a winter grain. Berry yellow, of medium size, and slightly shrunk. 10 grains weigh 0·3606 grm.

I. 2·3010 grs. lost at 100° C. 0·3551 gr.

II. 3·9868 grs. of substance dried at 100° gave 0·1116 ashes.

III. 0·2796 gr. gave 0·3915 gr. CO^2 and 0·1672 gr. HO.

IV. 0·2387 gr. gave 0·3915 gr. CO^2 and 0·1427 gr. HO.

V. 0·6429 gr. gave 0·2711 gr. ammonio-chloride of platinum.

These, expressed in parts of 100, give—

	I.	II.
Carbon......	45·14	44·73
Hydrogen..	6·64	5·97
Nitrogen ...	2·59	
Ashes	2·80	
Water	15·43	

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·59	= 16·52
	Carbon ...	9·09	
	Hydrogen .	1·13	
	Oxygen ...	3·53	
	Sulphur ...	0·18	
Ingredients containing no nitrogen.	Carbon ...	35·84	= 80·78
	Hydrogen	5·12	
	Oxygen ...	39·82	
	Ashes.................		= 2·80
			100·10

Whitington Wheat (Triticum vulgare) *from Hohenheim.*

An English variety of great excellence. Berry yellow, or white, large, and slightly shrunk. 10 grains weigh 0·4239 gr.

I. 2·6221 grs. lost at 100° C. 0·3653 gr.

II. 4·6567 grs. of substance dried at 100° C. gave 0·1460 gr. ashes.

III. 0·2927 gr. gave 0·4747 gr. CO^2 and 0·1834 gr. HO.

IV. 0·3898 gr. gave 0·6377 gr. CO^2 and 0·2343 gr. HO.

V. 0·5494 gr. gave 0·2343 gr. ammonio-chloride of platinum.

Expressed in 100 parts—

	I.	II.
Carbon......	44·23	44·61
Hydrogen..	6·96	6·67
Nitrogen ...	2·68	
Ashes	3·13	
Water	13·93	

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·68	= 17·09
	Carbon ...	9·40	
	Hydrogen	1·17	
	Oxygen ...	3·65	
	Sulphur ...	0·19	
Ingredients containing no nitrogen.	Carbon ...	35·02	= 79·58
	Hydrogen	5·65	
	Oxygen ...	38·91	
	Ashes..................		= 3·13
			99·80

Sandomierz Wheat (Triticum vulgare) *from Hohenheim.*

Berry scarcely of medium size, plump and sound. This variety is known as one of the best in Germany. 10 grains weigh 0·3199 grm.

I. 4·2256 grs. lost at 100° C. 0·6545 gr.

II. 3·6555 grs. of substance dried at 100° gave 0·0886 gr. ashes.

III. 0·5754 gr. gave 0·9327 gr. CO^2 and 0·3465 gr. HO.

IV. 0·7303 gr. gave 0·3131 gr. ammonio-chloride of platinum.

Expressed in 100 parts—

	I.
Carbon ...	44·20
Hydrogen	6·68
Nitrogen	2·69
Ashes ...	2·40
Water ...	15·48

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·69	= 17·15
	Carbon ...	9·44	
	Hydrogen.	1·17	
	Oxygen ...	3·66	
	Sulphur ...	0·19	
Ingredients containing no nitrogen.	Carbon ...	34·76	= 78·89
	Hydrogen.	5·51	
	Oxygen ...	38·62	
	Ashes.................		= 2·40
			98·44

No. 1. *Rye-flour from Vienna.*

I. 3·253 grs. at 100° lost 0·4482 gr.

II. 2·889 grs. of the substance dried at 100° gave 0·0387 gr. ashes.

III. 3·2285 grs. gave 0·0427 ashes.

IV. 0·3383 gr. gave 0·5576 gr. CO^2 and 0·1983 gr. HO.

V. 0·3109 gr. gave 0·5055 gr. CO^2 and 0·1901 gr. HO.

VI. 0·6519 gr. gave 0·1941 gr. ammonio-chloride of platinum.

These correspond in 100 parts to—

	I.	II.
Carbon......	44·41	44·34
Hydrogen..	6·51	6·79
Nitrogen ...	1·87	
Ashes	1·34	1·32
Water......	13·78	

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	1·87	= 11·92
	Carbon ...	6·56	
	Hydrogen.	0·82	
	Oxygen ...	2·54	
	Sulphur ...	0·13	
Non-nitrogenous ingredients.	Carbon ...	37·81	= 85·65
	Hydrogen.	5·83	
	Oxygen ...	42·01	
	Ashes		= 1·33
			98·90

No. 2. *Rye-flour from Vienna.*

I. 7·4625 grs. lost at 100° C. 1·0956 gr.

II. 2·8000 grs. of substance dried at 100° C. gave 0·0300 gr. ashes.

III. 0·5312 gr. gave 0·8752 gr. CO^2 and 0·3116 gr. HO.

IV. 0·4577 gr. gave 0·7626 gr. CO^2 and 0·2720 gr. HO.

V. 0·7558 gr. gave 0·3537 gr. ammonio-chloride of platinum.

VI. 0·7378 gr. gave 0·3433 gr. ammonio-chloride of platinum.

These correspond in 100 parts to—

	I.	II.
Carbon......	44·94	45·44
Hydrogen..	6·52	6·60
Nitrogen ...	2·94	2·92
Ashes	1·07	
Water	14·68	

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·93	= 18·69
	Carbon ...	10·28	
	Hydrogen.	1·28	
	Oxygen ...	3·99	
	Sulphur ...	0·21	
Non-nitrogenous ingredients.	Carbon ...	34·91	= 78·97
	Hydrogen.	5·28	
	Oxygen ...	38·78	
	Ashes..................		= 1·07
			98·73

Bush-rye (Secale cereale) *from Hohenheim.*

Winter crop. In the Hohenheim catalogue is the following remark:—"Besides its other qualities, it yields such excellent straw that it deserves being mentioned." The berry is small and generally shrunken. 10 grains weigh 0·1220 grm.

I. 4·2303 grms. lost at 100° C. 0·5896 gr.

II. 4·3792 grs. of substance dried at 100° C. gave 0·1078 gr. ashes.

III. 0·4742 gr. gave 0·7879 gr. CO^2 and 0·2767 gr. HO.

IV. 0·6281 gr. gave 1·0555 gr. CO^2 and 0·3777 gr. HO.

V. 0·6807 gr. gave 0·3016 gr. ammonio-chloride of platinum.

These correspond in 100 parts to—

	I.	II.
Carbon......	45·31	45·83
Hydrogen..	6·48	6·68
Nitrogen ...	2·78	
Ashes	2·43	
Water	13·94	

Estimated in 100 parts, as previously, the above determinations give the following numbers:—

$$\begin{array}{r}\text{Nitrogenous}\\ \text{ingredients.}\end{array}\left\{\begin{array}{lr}\text{Nitrogen...} & 2{\cdot}78\\ \text{Carbon ...} & 9{\cdot}76\\ \text{Hydrogen.} & 1{\cdot}21\\ \text{Oxygen ...} & 3{\cdot}78\\ \text{Sulphur ...} & 0{\cdot}20\end{array}\right\} = 17{\cdot}73$$

$$\begin{array}{r}\text{Non-nitrogenous}\\ \text{ingredients.}\end{array}\left\{\begin{array}{lr}\text{Carbon ...} & 35{\cdot}76\\ \text{Hydrogen.} & 5{\cdot}37\\ \text{Oxygen ...} & 39{\cdot}73\end{array}\right\} = 80{\cdot}86$$

$$\text{Ashes..................} = \underline{2{\cdot}43}$$

$$101{\cdot}02$$

Rush-rye (Secale cereale arundinaceum) *from Hohenheim.*

Berry of medium size and slightly shrunken. 10 grains weigh 0·1838 grm.

I. 3·3139 grms. lost at 100° C. 0·4579 gr.

II. 1·4596 gr. of substance dried at 100° gave 0·0346 gr. ashes.

III. 0·2795 gr. gave 0·4620 gr. CO^2 and 0·1742 gr. HO.

IV. 0·2632 gr. gave 0·4381 gr. CO^2 and 0·1638 gr. HO.

V. 0·6435 gr. gave 0·2530 gr. ammonio-chloride of platinum.

These, expressed in 100 parts, give—

	I.	II.
Carbon......	45·08	45·39
Hydrogen..	6·92	6·22
Nitrogen ...	2·47	
Ashes	2·37	
Water......	13·82	

Estimated in 100 parts, the above determinations give the following numbers:—

$$\begin{array}{r}\text{Nitrogenous}\\ \text{ingredients.}\end{array}\left\{\begin{array}{lr}\text{Nitrogen...} & 2{\cdot}47\\ \text{Carbon ...} & 8{\cdot}67\\ \text{Hydrogen.} & 1{\cdot}08\\ \text{Oxygen ...} & 3{\cdot}36\\ \text{Sulphur ...} & 0{\cdot}18\end{array}\right\} = 15{\cdot}76$$

$$\begin{array}{r}\text{Non-nitrogenous}\\ \text{ingredients.}\end{array}\left\{\begin{array}{lr}\text{Carbon ...} & 36{\cdot}56\\ \text{Hydrogen.} & 5{\cdot}49\\ \text{Oxygen ...} & 40{\cdot}62\end{array}\right\} = 82{\cdot}67$$

$$\text{Ashes..................} = \underline{2{\cdot}37}$$

$$100{\cdot}80$$

Polenta Meal (Indian corn-meal) *from Vienna.*

Yellow and coarse.

I. 5·5810 grs. lost at 100° 0·7456 gr.

II. 4·3933 grs. of substance dried at 100° C. gave 0·0386 gr. ashes.

III. 3·9323 gr. gave 0·331 gr. ashes.

IV. 0·5444 gr. gave 0·9010 gr. CO^2 and 0·3330 gr. HO.

V. 0·5095 gr. gave 0·8399 gr. CO^2 and 0·2943 gr. HO.

VI. 0·9055 gr. gave 0·3093 gr. ammonio-chloride of platinum.

These correspond in 100 parts to—

	I.	II.
Carbon......	45·14	44·96
Hydrogen..	6·89	6·49
Nitrogen ...	2·14	
Ashes	0·87	0·84
Water	13·36	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·14	= 13·65
	Carbon ...	7·51	
	Hydrogen.	0·93	
	Oxygen ...	2·92	
	Sulphur ...	0·15	
Non-nitrogenous ingredients.	Carbon ...	37·53	= 84·90
	Hydrogen.	5·67	
	Oxygen ...	41·70	
	Ashes		= 0·86
			99·41

Common Yellow Maize, Indian corn (Zea mays), *from Hohenheim.*

Yellow oval berry, bright and sound. 10 grains weigh 3·5934 grms.

I. 4·5765 grs. lost at 100° C. 0·6849 gr.

II. 5·0654 grs. of substance dried at 100° C. gave 0·0974 gr. ashes.

III. 0·4164 gr. gave 0·6984 gr. CO^2 and 0·2468 gr. HO.

IV. 0·4103 gr. gave 0·6800 gr. CO^2 and 0·2454 gr. HO.

V. 0·7319 gr. gave 0·2684 gr. ammonio-chloride of platinum.

These correspond in 100 parts to—

	I.	II.
Carbon......	45·74	45·20
Hydrogen..	6·58	6·64
Nitrogen ...	2·30	
Ashes	1·92	
Water	14·96	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·30	= 14·66
	Carbon ...	8·07	
	Hydrogen.	1·00	
	Oxygen ...	3·13	
	Sulphur ...	0·16	
Non-nitrogenous ingredients.	Carbon ...	37·38	= 84·52
	Hydrogen.	5·61	
	Oxygen ...	41·53	
	Ashes................		= 1·92
			101·10

Triticum monococcum *from Hohenheim.*

I. 4·4914 grs. lost at 100° C. 0·6472 gr.

II. 7·1327 grs. of substance dried at 100° C. gave 0·1438 gr. ashes.

III. 0·6303 gr. gave 1·0288 gr. CO^2 and 0·3811 gr. HO.

IV. 0·6757 gr. gave 1·1041 gr. CO^2 and 0·4098 gr. HO.

V. 0·7105 gr. gave 0·2340 gr. ammonio-chloride of platinum.

These correspond in 100 parts to—

	I.	II.
Carbon......	44·51	44·56
Hydrogen..	6·71	6·74
Nitrogen ...	2·07	
Ashes	2·01	
Water	14·40	

Estimated in 100 parts, the above determinations give the following numbers :—

Nitrogenous ingredients.	Nitrogen...	2·07	= 13·20
	Carbon ...	7·26	
	Hydrogen.	0·90	
	Oxygen ...	2·82	
	Sulphur ...	0·15	
Non-nitrogenous ingredients.	Carbon ...	37·28	= 84·52
	Hydrogen.	5·82	
	Oxygen ...	41·42	
	Ashes		= 2·01
			99·73

Jerusalem Barley (Hordeum distichon) *from Hohenheim.*

10 grains weigh 0·5312 grm.

I. 1·9328 grm. lost at 100° C. 0·3247 grm.

II. 2·3553 grs. of substance dried at 100° C. gave 0·0670 gr. ashes.

III. 0·4457 gr. gave 0·7392 gr. CO^2 and 0·2787 gr. HO.

IV. 0·4603 gr. gave 0·7728 gr. CO^2 and 0·2913 gr. HO.

V. 0·6910 gr. gave 0·2560 gr. ammonio-chloride of platinum.

These determinations correspond in 100 parts to—

	I.	II.
Carbon......	45·23	45·78
Hydrogen...	6·94	6·79
Nitrogen ...	2·31	
Ashes	2·84	
Water	16·79	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·34	= 14·72
	Carbon ...	8·11	
	Hydrogen.	1·00	
	Oxygen ...	3·14	
	Sulphur ...	0·16	
Non-nitrogenous ingredients.	Carbon ...	37·39	= 84·80
	Hydrogen	5·89	
	Oxygen ...	41·54	
	Ashes..................		= 2·84
			102·36

Common Winter Barley (Hordeum vulgare) *from Hohenheim.*

10 grains weigh 0·3955 grm.

I. 1·5268 gr. lost at 100° C. 0·2107 gr.

II. 2·5708 grs. of substance dried at 100° C. gave 0·1409 gr. ashes.

III. 0·3244 gr. gave 0·5380 gr. CO^2 and 0·1928 gr. HO.

IV. 0·2505 gr. gave 0·4152 gr. CO^2 and 0·1611 gr. HO.

V. 0·5266 gr. gave 0·2342 gr. ammonio-chloride of platinum.

VI. 4·3619 grs. of grains (calculated upon substance dried at 100° C.) gave, by the method already described, page 368, 0·2356 gr. woody fibre and combined inorganic matter.

VII. 0·1793 gr. of the same woody fibre gave 0·0035 gr. ashes.

These, expressed in 100 parts, correspond to—

	I.	II.
Carbon......	45·23	45·20
Hydrogen...	6·60	7·14
Nitrogen ...	2·79	

Ashes	5·52
Water	13·80
Hulls	5·40
Ashes of hulls	1·90

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ..	2·79	= 17·70
	Carbon ...	9·79	
	Hydrogen	1·12	
	Oxygen ...	3·80	
	Sulphur ...	0·20	
Non-nitrogenous ingredients.	Carbon ...	35·43	= 80·64
	Hydrogen.	5·85	
	Oxygen ...	39·36	
	Ashes..................		= 5·52
			103·86

	Dried at 100° C.	Fresh.
Nitrogenous ingredients	17·70	15·26
Inorganic ingredients ...	5·52	4·76
Woody fibre	5·30	4·57
Starch, sugar, &c.	71·48	61·61
Water		13·80
	100·00	100·00

Kamskatka Oats (Avena sativa) *from Hohenheim.*

10 grains weigh 0·3446 gr.

I. 2·3657 grs. lost at 100° C. 0·3446 gr.

II. 3·1728 gr. of substance dried at 100° C. gave 0·1032 gr. ashes.

III. 0·4310 gr. gave 0·7341 gr. CO^2 and 0·2552 gr. HO.

IV. 0·4001 gr. gave 0·6830 gr. CO^2 and 0·2427 gr. HO.

V. 0·4102 gr. gave 0·1581 gr. ammonio-chloride of platinum.

VI. 0·6207 gr. of the same gave 0·2324 gr. ammonio-chloride of platinum.

These, expressed in 100 parts, correspond to—

	I.	II.
Carbon ...	46·45	46·55
Hydrogen	6·55	6·73
Nitrogen...	2·42	2·35
Ashes......	3·26	
Water ...	12·71	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·39	= 15·24
	Carbon ...	8·39	
	Hydrogen.	1·04	
	Oxygen ...	3·25	
	Sulphur ...	0·17	
Non-nitrogenous ingredients.	Carbon ...	38·11	= 86·05
	Hydrogen.	5·60	
	Oxygen ...	42·34	
	Ashes................		= 3·26
			104·55

Early White-panicled Oats (Avena sativa) *from Hohenheim.*

Berry bright, plump and sound. 10 grains weigh 0·3689 gr.

I. 1·7548 gr. lost at 100° C. 0·2271 gr.

II. 1·8486 gr. of substance dried at 100° C. gave 0·0765 gr. ashes.

III. 0·3539 gr. gave 0·6057 gr. CO^2 and 0·2144 gr. HO.

IV. 0·2410 gr. gave 0·4123 gr. CO^2 and 0·1451 gr. HO.

V. 0·4977 gr. gave 0·2236 gr. ammonio-chloride of platinum.

VI. 3·5498 grs. gave, by the method already described, 0·5916 gr. woody fibre and combined inorganic matter.

VII. 0·4197 gr. of the same woody fibre gave 0·0140 gr. ashes.

Expressed in 100 parts, these correspond with—

	I.	II.
Carbon	46·68	46·66
Hydrogen ...	6·73	6·69
Nitrogen	2·82	
Ashes	4·14	
Water.........	12·94	
Hulls	16·66	
Ashes of hulls	3·35	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	2·82	= 17·99
	Carbon ...	9·90	
	Hydrogen.	1·23	
	Oxygen ...	3·84	
	Sulphur ...	0·20	
Non-nitrogenous ingredients.	Carbon ...	36·76	= 83·08
	Hydrogen	5·48	
	Oxygen ...	40·84	
	Ashes................		= 4·14
			105·21

	Dried at 100° C.	Fresh.
Nitrogenous ingredients	= 17·99	15·67
Inorganic ingredients...	= 4·14	3·60
Woody fibre	= 16·11	14·01
Starch, sugar, &c.	= 61·76	53·78
Water....................		12·94
	100·00	100·00

As the woody fibre, = 16·66 per cent. of the whole, belongs mostly to the chaff, this grain ranks among the richest in nitrogenous compounds. 2·82 per cent. of nitrogen *with* the chaff equals 3·38 per cent. *without*.

Common Rice (Oryza sativa).

I. 7·4606 grs. lost at 100° C. 1·1301 gr.

II. 8·3670 grs. of substance dried at 100° C. gave 0·0306 gr. ashes.

III. 0·7158 gr. gave 1·1701 gr. CO^2 and 0·4244 gr. HO.

IV. 0·6095 gr. gave 0·9982 gr. CO^2 and 0·3559 gr. HO.

V. 1·5609 gr. gave 0·2892 gr. ammonio-chloride of platinum.

These determinations correspond in 100 parts to—

	I.	II.
Carbon ...	44·57	44·66
Hydrogen	6·58	6·48
Nitrogen...	1·16	
Ashes	0·36	
Water ...	15·14	

Estimated in 100 parts, these determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	1·16	= 7·40
	Carbon ...	4·07	
	Hydrogen	0·51	
	Oxygen ...	1·58	
	Sulphur ...	0·08	
Non-nitrogenous ingredients.	Carbon ...	40·54	= 91·60
	Hydrogen	6·02	
	Oxygen ...	45·04	
	Ashes		= 0·36
			99·36

Buckwheat-meal from Vienna.

I. 7·3962 grs. lost at 100° C. 1·1187 gr.

II. 5·1500 grs. of substance dried at 100° C. gave 0·0564 gr. ashes.

III. 0·5041 gr. gave 0·8194 gr. CO^2 and 0·2911 gr. HO.

IV. 0·3441 gr. gave 0·5577 gr. CO^2 and 0·2061 gr. HO.

V. 1·1295 gr. gave 0·2062 gr. ammonio-chloride of platinum.

VI. 0·9536 gr. gave 0·1561 gr. ammonio-chloride of platinum.

These, expressed in 100 parts, correspond to—

	I.	II.
Carbon ...	44·36	44·20
Hydrogen	6·42	6·68
Nitrogen..	1·14	1·03
Ashes	1·09	
Water....	15·12	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	1·08	= 6·88
	Carbon ...	3·79	
	Hydrogen	0·47	
	Oxygen ...	1·47	
	Sulphur ...	0·07	
Non-nitrogenous ingredients.	Carbon ...	40·48	= 91·52
	Hydrogen	6·07	
	Oxygen ...	44·97	
	Ashes.............		= 1·09
			99·49

Tartarian Buckwheat (Polygonum tartaricum) *from Hohenheim.*

This variety differs from the common species, *Polygonum Fagopyrum*, in that while the hulls of the latter are smooth, those of the former are covered with folds and excrescences. The whole grain, hull included, was pulverized and analysed. 10 grains weigh 0·2566 grm.

I. 2·7598 grs. lost at 100° C. 0·3916 gr.

II. 2·5924 grs. of substance dried at 100° C. gave 0·0597 gr. ashes.

III. 0·4245 gr. gave 0·7013 gr. CO^2 and 0·2503 gr. HO.

IV. 0·3401 gr. gave 0·5710 gr. CO^2 and 0·1943 gr. HO.

V. 0·5677 gr. gave 0·1407 gr. ammonio-chloride of platinum.

VI. 5·0444 grs. of the same, by the method already described, gave 1·1438 gr. woody fibre and combined inorganic matter.

VII. 0·7100 gr. of the woody-fibre gave 0·0006 gr. ashes.

These, expressed in 100 parts, correspond to—

	I.	II.
Carbon ...	45·06	45·79
Hydrogen ...	6·55	6·35
Nitrogen ...	1·56	
Ashes	2·30	
Water	14·19	
Hulls	22·67	
Ashes of hulls	0·08	

Estimated in 100 parts, the above determinations give the following numbers :—

Nitrogenous ingredients.	Nitrogen ...	1·56	= 9·94
	Carbon ...	5·47	
	Hydrogen	0·68	
	Oxygen ...	2·12	
	Sulphur ...	0·11	
Non-nitrogenous ingredients.	Carbon ...	40·29	= 90·38
	Hydrogen	5·77	
	Oxygen ...	44·32	
	Ashes		= 2·30
			102·62

Table Peas (Pisum sativum) *from Vienna.*

Bright, plump, sound and of medium size. 10 peas weigh 2·6080 grms.

I. 4·4804 grs. lost at 100° C. 0·6018 gr.

II. 2·1600 grs. substance dried at 100° C. gave 0·0687 gr. ashes.

III. 0·4527 gr. gave 0·7476 gr. CO^2 and 0·2789 gr. HO.

IV. 0·3810 gr. gave 0·6314 gr. CO^2 and 0·2272 gr. HO.

V. 0·8603 gr. gave 0·6047 gr. ammonio-chloride of platinum.

VI. 5·2332 grs. of the same, according to the method described, gave 0·4005 gr. hulls.

VII. 0·3962 gr. of hulls dried at 100° C. gave 0·0098 gr. ashes.

These correspond in 100 parts to—

	I.	II.
Carbon ...	45·04	45·20
Hydrogen	6·84	6·62
Nitrogen	4·42	
Ashes	3·18	
Water	13·43	
Hulls	7·65	
Ashes of hulls	2·47	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	4·42	= 28·02
	Carbon ...	15·51	
	Hydrogen	1·93	
	Oxygen ...	6·02	
	Sulphur ...	0·14	
Non-nitrogenous ingredients.	Carbon ...	29·61	= 67·31
	Hydrogen	4·80	
	Oxygen ...	32·90	
	Ashes		= 3·18
			98·51

Legumine, according to the analysis of Dumas and Cahours, gave for the above determinations 102·00*.

	Dried at 100° C.	Fresh.
Nitrogenous ingredients	= 28·02	24·41
Inorganic ingredients	= 3·18	2·75
Woody fibre	= 7·47	6·46
Starch, sugar, &c. ...	= 61·35	52·95
Water		13·43
	100·02	100·00

Field Peas (Pisum sativum) *from Giessen.*

Less in size than the preceding variety. 10 grains weigh 1·9829 grm.

I. 3·5904 grs. lost at 100° 0·7002 gr.

II. 2·2455 grms. of substance dried at 100° C. gave 0·0628 gr. ashes.

III. 0·6178 gr. gave 1·0267 gr. CO^2 and 0·3572 gr. HO.

IV. 0·6467 gr. gave 0·4708 gr. ammonio-chloride of platinum.

V. 31·925 grs. gave, by the method already described, 1·9510 gr. hulls.

VI. 0·7232 gr. of the hulls dried at 100° C. gave 0·0135 gr. ashes.

These correspond in 100 parts to—

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* Their analysis gave—

Carbon	50·5
Hydrogen	6·8
Nitrogen	18·5
Oxygen	24·2
	100·0

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	I.
Carbon	45·32
Hydrogen ...	6·42
Nitrogen	4·57
Ashes	2·79
Water	19·50
Hulls	6·11
Ashes of hulls	1·86

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	4·57	= 29·18
	Carbon ...	16·04	
	Hydrogen	2·00	
	Oxygen ...	6·43	
	Sulphur ...	0·14	
Non-nitrogenous ingredients.	Carbon ...	29·28	= 66·23
	Hydrogen	4·42	
	Oxygen ...	32·53	
	Ashes		= 2·79
			98·10

Legumine, according to Dumas' and Cahours' analysis, gave for the above determinations 101·06.

	Dried at 100° C.	Fresh.
Nitrogenous ingredients	29·18	23·49
Inorganic ingredients ..	2·79	2·24
Woody fibre	6·00	4·83
Starch, sugar, &c.	62·03	51·14
Water		19·50
	100·00	101·20

Table Beans (Phaseolus vulgaris) *from Vienna.*

Berry bright, plump, and of less than medium size. 10 beans weighed 3·1431 grms.

I. 2·9467 grs. lost at 100° C. 0·3953 gr.

II. 2·8422 grs. of substance dried at 100° C. gave 0·1244 gr. ashes.

III. 0·4648 gr. gave 0·7721 gr. CO^2 and 0·2747 gr. HO.

IV. 0·4334 gr. gave 0·7126 gr. CO^2 and 0·2617 gr. HO.

V. 0·9082 gr. gave 0·6457 gr. ammonio-chloride of platinum.

VI. 13·6091 grs., by the method already described, gave 0·5461 gr. hulls.

VII. 0·5510 gr. hulls dried at 100° C. gave 0·0212 gr. ashes.

These correspond in 100 parts to—

	I.	II.
Carbon......	45·30	45·84
Hydrogen ...	6·56	6·76
Nitrogen ...	4·47	
Ashes	4·38	
Water	13·41	
Hulls	4·11	
Ashes of same	3·84	
Woody fibre	3·86	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	4·47	= 28·64
	Carbon ...	15·69	
	Hydrogen	1·95	
	Oxygen ...	6·39	
	Sulphur ...	0·14	
Non-nitrogenous ingredients.	Carbon ...	29·38	= 66·70
	Hydrogen	4·68	
	Oxygen ...	32·64	
	Ashes		= 4·38
			99·72

According to the analysis of legumine by Dumas and Cahours, the estimate gives 102·99.

	Dried at 100° C.	Fresh.
Nitrogenous ingredients	= 28·54	24·71
Inorganic ingredients ..	= 4·38	3·79
Woody fibre	= 3·86	3·34
Starch, sugar, &c.	= 63·22	54·75
Water		13·41
	100·00	100·00

Large Field-beans (Vicia faba) *from Giessen.*

White, plump and sound. 10 weigh 5·289 grms.

I. 7·4054 grs. lost at 100° C. 1·1705 gr.

II. 2·7950 grs. of substance dried at 100° C. gave 0·1150 gr. ashes.

III. 0·4987 gr. gave 0·8255 gr. CO^2 and 0·3053 gr. HO.

IV. 0·7238 gr. gave 0·5291 gr. ammonio-chloride of platinum.

V. 44·5335 grs. gave, by the method already described, 1·9680 gr. hulls.

VI. 0·8335 gr. of the hulls dried at 100° C. gave 0·0624 gr. ashes.

These, expressed in 100 parts, correspond to—

	I.
Carbon	45·18
Hydrogen ...	6·80
Nitrogen ...	4·59
Ashes	4·01
Water	15·80
Hulls	4·44
Hull-ashes ...	7·48
Woody fibre	4·09

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	4·59	= 29·31
	Carbon ...	16·11	
	Hydrogen	2·00	
	Oxygen ...	6·47	
	Sulphur ...	0·14	
Non-nitrogenous ingredients.	Carbon ...	29·07	= 66·17
	Hydrogen	4·80	
	Oxygen ...	32·30	
	Ashes		= 4·01
			99·49

According to Dumas' and Cahours' analysis of legumine, the estimate from the above determinations gave 102·73.

	Dried at 100° C.	Fresh.
Nitrogenous ingredients	= 29·31	24·67
Inorganic ingredients	= 4·01	3·37
Woody fibre	= 4·09	3·44
Starch, sugar, &c.	= 62·59	52·72
Water		15·80
	100·00	100·00

Lentils (Ervum lens) *from Vienna.*

Grains bright and sound.

I. 9·3074 grs. lost at 100° C. 1·2108 gr.

II. 2·1669 grs. pulverized lost at 100° C. 0·2820 gr.

III. 1·4724 gr. of the substance dried at 100° C. gave 0·0402 gr.

IV. 0·3511 gr. gave 0·5813 gr. CO^2 and 0·2122 gr. HO.

V. 0·3863 gr. gave 0·6452 gr. CO^2 and 0·2362 gr. HO.

VI. 0·6797 gr. gave 0·5198 gr. ammonio-chloride of platinum.

The above, expressed in 100 parts, correspond with—

	I.	II.
Carbon ...	45·15	45·55
Hydrogen	6·71	6·79
Nitrogen	4·77	
Ashes ...	2·60	
Water ...	13·01	13·01

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	4·77	= 30·46
	Carbon ...	16·74	
	Hydrogen	2·08	
	Oxygen ...	6·72	
	Sulphur ...	0·15	
Non-nitrogenous ingredients.	Carbon ...	28·61	= 65·06
	Hydrogen	4·67	
	Oxygen ...	31·78	
	Ashes............		= 2·60
			98·12

According to Dumas' and Cahours' analysis of legumine, the estimate gave 101·34.

White Potatoes (Solanum tuberosum) *from Giessen.*

I. 1·1455 gr. lost at 100° C. 0·8586 gr.

II. 3·2201 grs. substance dried at 100° C. gave 0·1163 gr. ashes.

III. 0·5814 gr. gave 0·9351 gr. CO^2 and 0·3139 gr. HO.

IV. 1·1530 gr. gave 0·2843 gr. ammonio-chloride of platinum.

These, expressed in 100 parts, give—

Carbon ...	43·86
Hydrogen	6·06
Nitrogen	1·56
Ashes ...	3·61
Water ...	74·95

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	1·56	= 9·96
	Carbon ...	5·47	
	Hydrogen	0·68	
	Oxygen ...	2·14	
	Sulphur ...	0·11	
Non-nitrogenous ingredients.	Carbon ...	38·39	= 86·36
	Hydrogen	5·32	
	Oxygen ...	42·65	
	Ashes		= 3·61
			99·93

Blue Potatoes (Solanum tuberosum) *from Giessen.*

I. 2·8369 grs. lost at 100° C. 1·9558 gr.

II. 3·6446 grs. of substance dried at 100° C. gave 0·1226 gr. ashes.

III. 0·8315 gr. gave 1·3260 gr. CO^2 and 0·4711 gr. HO.

IV. 0·7625 gr. gave 1·2030 gr. CO^2 and 0·4345 gr. HO.

V. 1·3485 gr. gave 0·2587 gr. ammonio-chloride of platinum.

Corresponding in 100 parts to—

	I.	II.
Carbon ...	43·49	43·02
Hydrogen	6·29	6·33
Nitrogen	1·20	
Ashes ...	3·36	
Water ...	68·94	

Estimated in 100 parts, the above determinations lead to the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	1·20	= 7·66
	Carbon ...	4·21	
	Hydrogen	0·52	
	Oxygen ...	1·65	
	Sulphur ...	0·08	
Non-nitrogenous ingredients.	Carbon ...	39·04	= 88·20
	Hydrogen	5·79	
	Oxygen ...	43·37	
	Ashes		= 3·36
			99·22

Carrots (Daucus carota) *from Giessen.*

I. 2·6735 grs. lost at 100° C. 2·3021 gr.

II. 1·6379 gr. of substance dried at 100° C. gave 0·0946 gr. ashes.

III. 0·6668 gr. gave 1·0597 gr. CO^2 and 0·3736 gr. HO.

IV. 0·6797 gr. gave 0·1885 gr. ammonio-chloride of platinum.

V. 0·7043 gr. of the same gave 0·1790 gr. ammonio-chloride of platinum.

Expressed in 100 parts, these correspond to—

	I.	II.
Carbon ...	43·34	
Hydrogen	6·22	
Nitrogen	1·74	1·59
Ashes ...	5·77	
Water ...	86·10	

Estimated in 100 parts, the above determinations give the following numbers:—

$$\begin{array}{r}\text{Nitrogenous}\\\text{ingredients.}\end{array}\left\{\begin{array}{ll}\text{Nitrogen} \ldots & 1{\cdot}67\\ \text{Carbon} \ldots & 5{\cdot}87\\ \text{Hydrogen} & 0{\cdot}73\\ \text{Oxygen} \ldots & 2{\cdot}27\\ \text{Sulphur} \ldots & 0{\cdot}12\end{array}\right\} = 10{\cdot}66$$

$$\begin{array}{r}\text{Non-nitrogenous}\\\text{ingredients.}\end{array}\left\{\begin{array}{ll}\text{Carbon} \ldots & 37{\cdot}47\\ \text{Hydrogen} & 5{\cdot}49\\ \text{Oxygen} \ldots & 41{\cdot}63\end{array}\right\} = 84{\cdot}59$$

Ashes............ = 5·77

101·02

Red Beets (Beta vulgaris rapacea) *from Giessen.*

I. 1·6173 gr. lost at 100° C. 1·3200 gr.

II. 2·3399 grs. of substance dried at 100° C. gave 0·1505 gr. ashes.

III. 0·4875 gr. gave 0·7525 gr. CO^2 and 0·2512 gr. HO.

IV. 0·5505 gr. gave 0·2152 gr. ammonio-chloride of platinum.

Corresponding in 100 parts to—

Carbon ...	40·99
Hydrogen	5·72
Nitrogen	2·43
Ashes ...	6·43
Water ...	81·61

Estimated in 100 parts, the above determinations give the following numbers:—

$$\begin{array}{r}\text{Nitrogenous}\\\text{ingredients.}\end{array}\left\{\begin{array}{ll}\text{Nitrogen} \ldots & 2{\cdot}43\\ \text{Carbon} \ldots & 8{\cdot}53\\ \text{Hydrogen} & 1{\cdot}06\\ \text{Oxygen} \ldots & 3{\cdot}31\\ \text{Sulphur} \ldots & 0{\cdot}17\end{array}\right\} = 15{\cdot}50$$

$$\begin{array}{r}\text{Non-nitrogenous}\\\text{ingredients.}\end{array}\left\{\begin{array}{ll}\text{Carbon} \ldots & 32{\cdot}46\\ \text{Hydrogen} & 4{\cdot}66\\ \text{Oxygen} \ldots & 36{\cdot}06\end{array}\right\} = 73{\cdot}18$$

Ashes .... = 6·43

95·11

Peligot found in this beet—

10·6 per cent. sugar ; add to this
2·8 ... nitrogenous ingredients,
1·1 ... inorganic matter,
81·6 ... water, and there remain,
3·9 ... woody fibre, &c. in
100·0 parts.

Yellow French Beet (Beta cicla) *from Giessen.*

I. 1·8545 gr. lost at 100° C. 1·5255 gr.

II. 2·2840 grs. of substance dried at 100° C. gave 0·1148 gr. ashes.

III. 0·4530 gr. gave 0·6853 gr. CO^2 and 0·2587 gr. HO.

IV. 0·4057 gr. gave 0·6165 gr. CO^2 and 0·2202 gr. HO.

V. 0·5660 gr. gave 0·1635 gr. ammonio-chloride of platinum.

Corresponding in 100 parts to—

	I.	II.
Carbon ...	41·25	41·45
Hydrogen	6·34	6·03
Nitrogen	1·81	
Ashes ...	5·02	
Water ...	82·25	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen ...	1·81	= 11·54
	Carbon ...	6·35	
	Hydrogen	0·79	
	Oxygen ...	2·46	
	Sulphur ...	0·13	
Non-nitrogenous ingredients.	Carbon ...	34·74	= 78·49
	Hydrogen	5·15	
	Oxygen ...	38·60	
	Ashes		= 5·02
			99·05

Ruta Baga (Brassica napo-brassica) *from Giessen.*

I. 2·2981 grs. lost at 100° C. 1·9139 gr.

II. 2·5171 grs. of substance dried at 100° C. gave 0·1010 gr. ashes.

III. 0·6762 gr. gave 1·1226 gr. CO^2 and 0·3665 gr. HO.

IV. 0·6122 gr. gave 1·0180 gr. CO^2 and 0·3306 gr. HO.

V. 0·7625 gr. gave 0·1765 gr. ammonio-chloride of platinum.

Corresponding in 100 parts to—

	I.	II.
Carbon ...	45·27	45·35
Hydrogen	6·02	6·00
Nitrogen	1·45	
Ashes ...	4·10	
Water ...	83·28	

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	1·45	= 9·24
	Carbon ...	5·09	
	Hydrogen	0·63	
	Oxygen ...	1·97	
	Sulphur ...	0·10	
Non-nitrogenous ingredients.	Carbon ...	40·23	= 90·32
	Hydrogen	5·38	
	Oxygen ...	44·71	
	Ashes		= 4·01
			103·57

White Turnips (Brassica rapa) *from Giessen.*

I. 0·8742 gr. lost at 100° C. 0·7674 gr.

II. 1·7487 gr. of substance dried at 100° C. gave 0·1229 gr. ashes.

III. 0·4376 gr. gave 0·6953 gr. CO^2 and 0·2225 gr. HO.

IV. 0·2831 gr. gave 0·4472 gr. CO^2.

V. 0·5976 gr. gave 43·42 per cent. C. and 5·91 per cent. H.

VI. 0·7969 gr. gave 0·2523 gr. ammonio-chloride of platinum.

Corresponding in 100 parts to—

	I.	II.	III.
Carbon ...	43·33	43·08	43·42
Hydrogen	5·64	6·04	5·91
Nitrogen	1·98		
Ashes ...	7·03		
Water ...	87·78		

Estimated in 100 parts, the above determinations give the following numbers:—

Nitrogenous ingredients.	Nitrogen...	1·98	= 12·62
	Carbon ...	6·95	
	Hydrogen	0·86	
	Oxygen ...	2·69	
	Sulphur ...	0·14	
Non-nitrogenous ingredients.	Carbon ...	36·24	= 81·33
	Hydrogen	4·82	
	Oxygen ...	40·27	
	Ashes		= 7·02
			100·97

White Onions from Giessen.

I. 4·3169 grms. lost at 100° C. 4·0478 grms.

II. 0·6775 gr. of substance dried at 100° C. gave 0·0578 gr. ashes.

III. 0·6726 gr. gave 0·1272 gr. ammonio-chloride of platinum.

These determinations correspond in 100 parts to—

Nitrogen...	1·18
Ashes	8·53
Water ...	93·78

Hulls.

Name.	Undissolved residue in 100 parts (hulls ?) dried at 100° C.	Ashes of the hulls.	Woody fibre.
Common Winter Barley	5·40	1·90	5·30
Tartarian Buckwheat	22·67	0·08	22·66
Panicled Oats..................................	16·66	3·35	16·10
Table Peas	7·65	2·47	7·47
Field Peas	6·11	1·86	6·00
Table Beans	4·01	3·84	3·86
Large Field-beans	4·41	7·48	4·09

Relative worth of the individual grains according to weight and per-centage of nitrogen.

Name.	10 grains weighed in undried condition.	Relative weight of the individual grains, Bush-rye = 1.	Per cent. of nitrogen in undried condition.	Relative quantity of nitrogen in the individual grains, Bush-rye = 1.
Bush-rye	grs. 0·1220	1	2·39	1
Rush-rye	0·1838	1·5	2·13	1·3
Talavera Wheat	0·3606	3	2·19	2·7
Whitington Wheat	0·4239	3·5	2·30	2·9
Sandomierz Wheat	0·3199	2·8	2·13	2·4
Indian Corn	3·5934	29·4	1·95	24·0
Jerusalem Barley	0·5312	4·3	1·92	3·6
Common Barley................	0·3955	3·2	2·40	2·4
Kamskatka Oats	0·3446	2·8	2·08	2·4
Early-panicled Oats	0·3689	3	2·45	3·1
Tartarian Buckwheat	0·2566	2·1	1·33	1·2
Table Peas	2·6080	21·4	3·83	34·3
Field Peas	1·9828	16·3	3·68	25·1
Table Beans	3·1431	25·5	3·87	41·2
Large White Beans	5·2890	43·3	3·86	69·9

Tabular Results of Analysis.

Name.	Nitrogen.	Carbon.	Hydrogen.	Oxygen.	Sulphur.	Ash
Wheat-flour from Vienna, No. 1	3·00	45·74	6·70	43·23	0·23	0·70
Wheat-flour from Vienna, No. 2	2·12	45·18	6·65	44·81	0·15	0·69
Wheat-flour from Vienna, No. 3	3·44	46·86	6·78	42·65	0·25	1·10
Talavera Wheat from Hohenheim	2·59	44·93	6·25	43·35	0·18	2·89
Whitington Wheat	2·68	44·42	6·82	42·56	0·19	3·13
Sandomierz Wheat	2·69	44·20	6·68	42·28	0·19	2·40
Rye-flour from Vienna, No. 1	1·87	44·37	6·65	44·55	0·13	1·35
Rye-flour from Vienna, No. 2	2·93	45·19	6·56	42·77	0·21	1·07
Bush-rye from Hohenheim	2·78	45·52	6·58	43·51	0·15	0·86
Rush-rye from Hohenheim	2·47	45·23	6·57	43·98	0·18	2·37
Polenta Meal from Vienna	2·14	45·04	6·60	44·62	0·15	0·86
Indian Corn from Hohenheim	2·30	45·45	6·61	44·66	0·16	1·92
Triticum monococcum from Giessen	2·07	44·54	6·72	44·24	0·15	2·01
Jerusalem Barley from Hohenheim	2·31	45·50	6·87	44·68	0·16	2·84
Common Winter Barley from Hohenheim	2·79	45·22	6·99	42·16	0·20	5·52
Kamskatka Oats from Hohenheim	2·39	46·50	6·64	45·59	0·17	3·36
Early-panicled Oats from Hohenheim	2·82	46·66	6·74	44·68	0·20	4·14
Common Rice	1·16	44·61	6·53	46·62	0·08	0·36
Buckwheat Meal from Vienna	1·08	44·27	6·54	46·50	0·07	1·09
Tartarian Buckwheat from Hohenheim	1·56	45·42	6·45	46·50	0·11	2·30
Table Peas from Vienna	4·42	45·12	6·73	37·92	0·14	3·18
Field Peas from Giessen	4·57	45·33	6·42	38·75	0·14	2·79
Table Beans from Vienna	4·47	45·07	6·63	38·73	0·14	4·38
Large White Beans from Giessen	4·59	45·18	6·80	38·55	0·14	4·01
Lentils from Vienna	4·77	45·35	6·75	38·28	0·15	2·60
White Potatoes from Giessen	1·56	43·86	6·00	44·77	0·14	3·61
Blue Potatoes from Giessen	1·20	43·25	6·31	45·00	0·08	3·36
Carrots from Giessen	1·67	43·34	6·22	43·90	0·12	5·77
Red Beets from Giessen	2·43	40·99	5·72	39·37	0·17	6·43
Ruta Baga from Giessen	1·81	41·09	5·94	41·06	0·13	5·42
Yellow French Beet from Giessen	1·45	45·31	6·61	42·59	0·10	4·01
White Turnips from Giessen	1·98	43·19	5·68	42·96	0·14	7·02
White Onions from Giessen	1·18	....	....	....	....	8·53

Table for the Nitrogenous Ingredients.

Name.	Nitrogenous ingredients.		Water.
	Dried at 100° C.	In fresh condition.	
Wheat-flour from Vienna, No. 1	19·16	16·51	13·85
Wheat-flour from Vienna, No. 2	13·54	11·69	13·65
Wheat-flour from Vienna, No. 3	21·97	19·17	12·73
Talavera Wheat from Hohenheim	16·54	13·98	15·43
Whitington Wheat from Hohenheim	17·11	14·72	13·93
Sandomierz Wheat from Hohenheim	17·18	14·51	15·48
Rye-flour from Vienna, No. 1	11·94	10·34	13·78
Rye-flour from Vienna, No. 2	18·71	15·96	14·68
Bush-rye from Hohenheim	17·75	15·27	13·94
Rush-rye from Hohenheim	15·77	13·59	13·82
Polenta Meal from Vienna	13·66	11·53	13·36
Indian Corn from Hohenheim	14·68	12·48	14·96

Table for the Nitrogenous Ingredients (continued).

Name,	Nitrogenous ingredients.		Water.
	Dried at 100° C.	In fresh condition.	
Triticum monococcum from Giessen	13·22	11·30	14·40
Jerusalem Barley from Hohenheim	14·74	12·26	16·79
Common Barley from Hohenheim	17·81	15·35	13·80
Kamskatka Oats from Hohenheim	15·26	13·32	12·71
Early-panicled Oats from Hohenheim	18·00	15·67	12·94
The same without hulls	21·57	18·78	12·94
Common Rice	7·40	6·27	15·14
Buckwheat Meal from Vienna	6·89	5·84	15·12
Tartarian Buckwheat from Hohenheim	9·96	7·94	14·19
Table Peas from Vienna	28·02	24·41	13·43
Field Peas from Giessen	29·18	23·49	19·50
Table Beans from Vienna	28·54	24·74	13·41
Large White Beans from Giessen	29·31	24·67	15·80
Lentils from Vienna	30·46	26·59	13·01
White Potatoes from Giessen	9·96	2·49	74·95
Blue Potatoes from Giessen	7·66	2·37	68·94
Carrots from Giessen	10·66	1·48	86·10
Red Beets from Giessen	15·50	2·83	81·61
Ruta Baga from Giessen	11·56	2·04	82·25
Yellow French Beet from Giessen	9·25	1·54	83·28
White Turnips from Giessen	12·64	1·54	87·78
White Onions from Giessen	7·53	0·46	93·78

Tabular Arrangement of Nutrition Values in Equivalents. *Wheat at* 100.

Name.	Theory.		Experiment.
	Dried at 100° C.	In fresh condition.	In fresh condition.
Wheat	100	100	94
Rye	98·8	97·6	97·6
Indian Corn	115	113	108
Triticum monococcum	128	124·6	
Barley	104	102	101·5
Panicled Oats	92	90	112·7
The same without hulls	78	76·3	
Kamskatka Oats	110	106	
Common Rice	220	225	
Tartarian Buckwheat	170	166	122·7
Table Peas from Vienna	59·9	57·6	90·7
Field Peas from Giessen	57·7	60	90·7
Table Beans from Vienna	59·2	57	94·7
Large White Beans from Giessen	58·8	57	94·7
Lentils	55·5	53	
White Potatoes	169·8	565·6	429
Blue Potatoes	220·8	596·3	429
Carrots	158·6	959·4	545·4
Red Beets	109	501·5	
Ruta Baga	146	689·5	643
Yellow French Beet	182·7	919·4	589·7
White Turnips	133·8	919·4	1000
White Onions	224·6	2106·0	

The last column in the preceding table contains the average results of experiments made to ascertain the practical equivalents, as they are given in Boussingault (*Landwirthschaft*, Germ. ed. p. 292–295). One of the results with wheat was neglected, its variation from the others being so great

By comparing the results of the above investigation with each other, and with those previously known, the following conclusions have been arrived at:—

That the same species of cereal grain grown on different soils may yield unequal per-centages of nitrogen.

That one-seventh of fresh ripe cereal grains is moisture, which may be expelled at a temperature of 100° C. (212° F.).

That wheat- and rye-flours, which to the eye and sense of feeling are undistinguishable from each other, may differ by from one to three-tenths of their whole quantity of nitrogen.

That root-crops grown on different soils may yield unequal per-centages of nitrogen.

That the per-centage of moisture in edible roots of the same species is, in the fresh condition, a constant quantity.

That beets, carrots and turnips, have a larger per-centage of moisture than potatoes.

That the nutritive values of peas, beans and lentils, correspond with each other.

That more aliment is contained in a given weight of peas, beans or lentils, than in an equal weight of any other kind of vegetable food analysed.

That in several of the grains and roots analysed there are organic bodies beside those identical in composition with gluten and starch.

That the ashes of carrots, beets, turnips and potatoes, as Prof. von Liebig has already remarked, contain carbonates.

That iron is present in the ashes of all the grains and roots analysed.

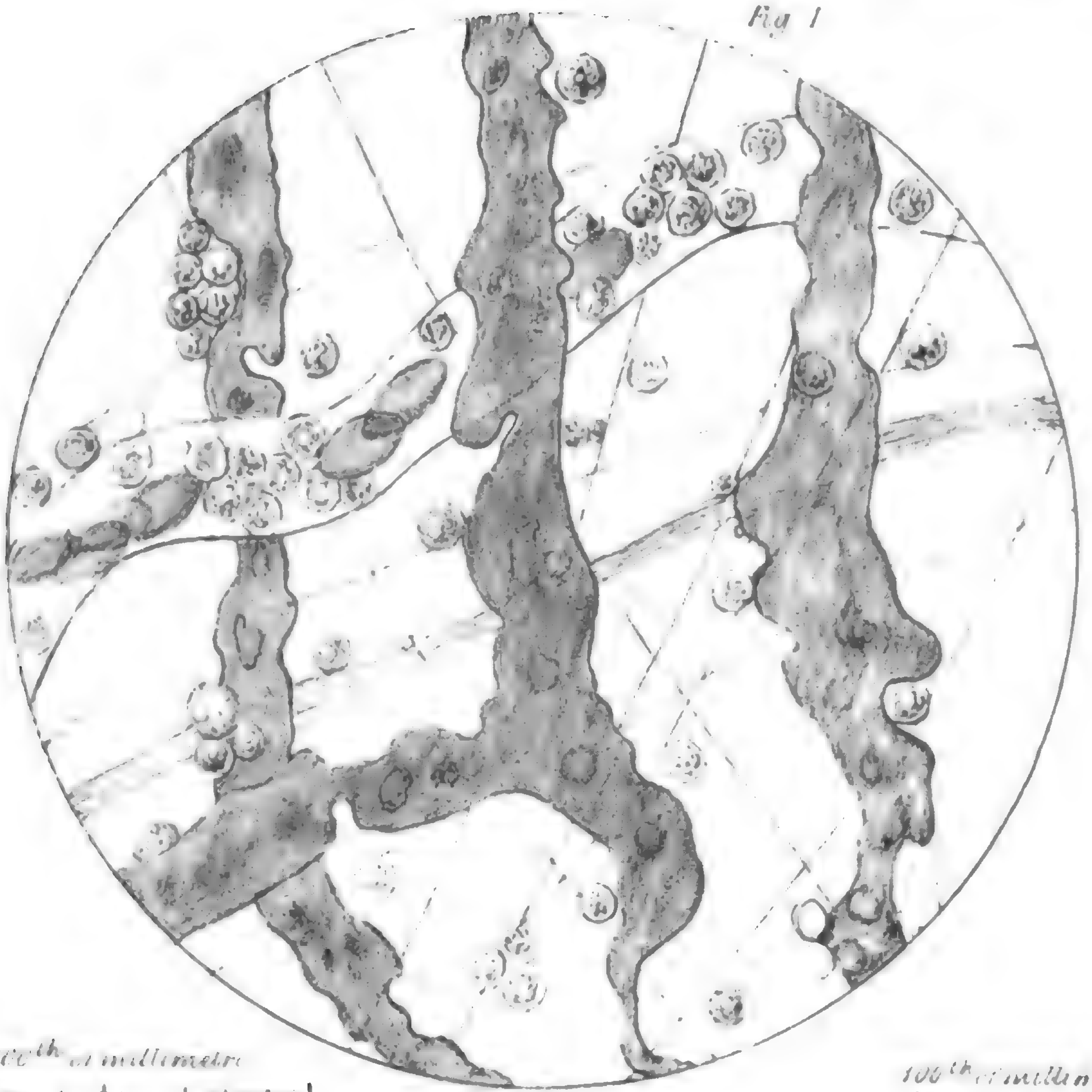
That the differences between the theoretical equivalents, as estimated from the per-centages of nitrogen and those ascertained by the experiments of stock-growers, and particularly the differences between the results of the different stock-growers, may be attributed to the following reasons:—

Because the per-centages of nitrogen and carbon in fodder grown on different soils are unequal.

Because the prominent test has been the increase or diminution in weight of the animal fed. *Increase* in weight may arise from secretion of fat, derived from the sugar and starch of the plants. *Diminution* in weight may follow unusual activity, increasing the consumption of fat already present.

Because the experiments, in but few instances, were under-

Fig. 1.



100th of millimetre

Scale of Fig. 1. Pl. II.

100th of millimetre

Scale of Fig. 1. Pl. I.

Fig. 2.

Dr. Waller del.

J. D. C. S. lith.

taken with substances whose per-centages of water and nitrogen had been ascertained.

Because theoretical equivalents have been employed in conditions unequally suited to digestion. The same food, coarse or fine, fresh or prepared for easy digestion, yields unequal measures of nutrition.

Because the conditions, whether exposed to the open air or protected in stalls, whether subjected to labour of uniform severity, or allowed the free range of pastures, have not been made alike.

Finally, because, as above intimated, some animals by nature differ greatly from others, in the facility with which fat and muscle are developed, even when the circumstances are precisely the same.

LVIII. *Microscopic Observations on the Perforation of the Capillaries by the Corpuscles of the Blood, and on the Origin of Mucus and Pus-globules. By* AUGUSTUS WALLER, *M.D.*[*]

[Continued from p. 287.]

IN the preceding Number of this Journal I have already given a brief outline of some observations respecting the manner in which the white corpuscles of the blood are seen to leave the interior of the capillary vessels. As this subject is intimately connected with the formation or origin of the globules found in mucus and purulent matter, it deserves a close and strict investigation.

In the first place, we must bear in mind that the blood consists of transparent fluid parts containing two kinds of particles in suspension; the one of a red colour and a flattened disc-like shape, and the other nearly transparent and spherical. The examination of the tongue of the frog, or of any other transparent part of a living animal, shows these globules and corpuscles generally circulating together through the capillaries.

The examination of mucus and purulent matter shows that they contain great quantities of globules, which are exactly similar to the spherical corpuscles found in the blood. In all these instances these particles are of a spherical form, transparent or whitish, granulated, and about the size of $\frac{1}{100}$th of a millimetre. In acetic acid they contract, in the alkalies they are quickly broken up, in water they swell and are disaggregated into separate minute granules.

The corpuscles of blood in the mammiferæ and in the frog are found to be of about the same size as in man.

In proof of what I have stated respecting the similarity of

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[*] Communicated by the Author.

these bodies in pus, mucus and in the blood, I may mention that I have before me drawings of mucus and purulent globules, and of the corpuscles obtained from man and from some of the cold-blooded animals. All these drawings have been carefully executed under the camera lucida, at different times; and they resemble each other so closely as to render it impossible to give any distinctive character to recognise one from the other.

In consequence of this striking resemblance, observers have already supposed that the corpuscles of the blood give rise to those found in mucus and pus, and that these are simply corpuscles extravasated or filtered from the blood. An important observation has moreover corroborated this theory, namely that of the accumulation of the corpuscles at the inner sides of vessels which are subjected to any prolonged irritation. This theory has principally been known in this country from the talented observations of Mr. W. Addison.

On the other hand, it has appeared to other physiologists so improbable to suppose the perforation of the capillary vessels by the corpuscles, that they have come to the conclusion that they are not derived from the blood, but, like semen or milk, are formed on the secreting surface, in virtue of some plastic power of the fluids which are effused upon it. This opinion is maintained, with some modifications of trifling import, by Müller, Autenreith, Donné, &c.

My observations respecting the almost exclusive accumulation of the corpuscles in the vessels of the frog's tongue which are most exposed to irritation, was already an advance in the inductive proof of the first theory; but it is especially the observations at the end of the preceding paper which give the crowning evidence in its favour. I have already had the satisfaction to find that they have been confirmed by other observers to whom I have shown them. On my own part, I have repeated them in very numerous instances with invariable success.

Let us now examine the admirable manner in which nature has solved this apparent paradox, of eliminating, from a fluid circulating in closed tubes, certain particles floating in it, without causing any rupture or perforation in the tubes, or allowing the escape of the red particles, which are frequently the smaller of the two, or that of the fluid part of the blood itself. In order to allow less latitude to the imagination, I will in the first place describe faithfully, from a number of others, one or two complete observations, as I have noted them down at the time of experimenting.

Obs. 1.—A frog's tongue was secured in the usual manner.

The capillaries became rapidly distended with blood, and the colour of the entire organ was darker from this injection. After exposure to the air for two or three hours it was again examined: the appearance presented by its inferior surface is represented in Plate II. fig. 1. The capillaries were so forcibly distended that they attained double their ordinary calibre, and by their numerous swellings and contractions resembled a knotted stick. The blood was generally stagnant, and the outlines of the separate particles could not be distinguished: the corpuscles could only be known from the discs by their whiteness. Over the whole surface of the tongue were numerous corpuscles outside of the vessels, and occasionally a few scattered discs. The extravasated blood-discs were rapidly altered: they appeared to lose their redness, and to become broken up into granules which soon disappeared. The corpuscles appeared to retain their form much longer. The engorged vessels became rapidly very irregular in their calibre, by the formation of numerous indentations or concavities throughout their length. Opposite these concavities were found one or more corpuscles which had escaped. The upper surface of the vessel likewise presented numerous dark spots of an irregular shape with very distinct dark edges. These were evidently small depressions or foramina seen in full, while the former were viewed in profile. In some instances, the manner in which the corpuscle escaped from the interior of the tube could be distinctly followed: that part of the tube in contact with the external side of the corpuscle gradually disappeared, and at nearly the same time might be seen the formation of a distinct line of demarcation between the inner segment of the corpuscle and the fluid parts of the blood in contact with it. Any slight agitation then was capable of disengaging the corpuscle from the vessel to which it was now external, and in its place a concave depression remained, which appeared sufficiently protected by some membrane, as to oppose effectually the exit of the discs and the fluid parts of the blood. In rare instances the blood-discs and corpuscles might be seen separate and distinct within the tube, while occasionally some of the latter were seen to escape through the sides; the contents of the tube oscillating synchronously with the increased action of the blood in the surrounding vessels. In general, it is observed that the corpuscles escape more frequently at a bend or angle where there is a subdivision of the tube: those in the interior are also observed to collect there in greater numbers. In some places, the indentures formed by the extravasated corpuscles were so deep that the vessel appeared to be quite disconnected; and such was sometimes really the case; but generally the diameter was only much decreased without being entirely disconnected, so as to interrupt the course of the blood. I have sometimes seen vessels of considerable size with an internal rapid current, the tubes of which at some places were contracted to one-fifth of the size which they presented elsewhere. It is easy to explain this, if we suppose that on some occasion they had been perforated by corpuscles and narrowed as I have described.

The corpuscles do not escape merely from vessels destitute of internal current. Capillaries with a most rapid current are often seen to be uneven, with slight indentations along their sides, and at a short distance external corpuscles which are more numerous opposite the indentations. In these cases, the various analogies visible with regard to the indentations and the corpuscles situated directly opposite to them outside the vessel, show that the vessel must have given them passage in the same manner as when the blood is stagnant or in a state of feeble oscillation. The larger trunks never present any distinct indentations to mark the passage of the corpuscle through them; but at their exterior corpuscles are frequently seen collected in abundance, which renders it probable that the parietes of these also may be dissolved to allow of the passage of these particles, which, however, must not be confounded with that condition which I have termed extra fibrination. A careful observer will easily distinguish the globular particles of what is probably condensed or coagulated fibrine, from the corpuscles, as they are much more minute and have no granular structure.

The results of other experiments performed in the same way are so similar to the above, that it would be a needless repetition to detail them. When an animated being is the subject, there must necessarily be some slight variation in the results. Thus in some of my observations I find a difference in the period which elapses before the injection and engorgement takes place, and the succeeding phænomena are likewise affected in proportion. I will briefly mention another observation, as it was attended with a loss of blood, and may serve in some respects to elucidate the influence of depletion on capillary circulation.

Obs. 2.—While the tongue of the frog was being secured, one of the trunks of the lingual vessel was ruptured near the jawbone and the blood escaped in abundance: from its dark colour and the continuous manner in which it flowed, a vein probably had been torn. The depletion thus produced represented on a small scale the operation of bleeding, to which we have such frequent recourse in medicine. To the naked eye the distended membrane presented but a slight appearance of injection, in comparison to that which is exhibited by it in ordinary instances. The microscope showed that there existed scarcely a vessel containing stagnant blood. The venous and internal trunks were all considerably paler and more transparent than I had ever before seen them, and their outlines were distinctly marked, instead of that dull and indistinct appearance produced in ordinary cases by the extra fibrination or transudation of the liquor sanguinis. The circulation of the blood was extremely rapid in the trunks and capillaries, so much so as to render it impossible to detect the separate particles during their course. After the lapse of three or four hours the capillaries had become in a measure engorged with stagnant blood, but to a much less extent than usual. Many of the circulating papillæ were found to contain a central nucleus, completely black and opake, occupying the whole internal area, generally containing the vascular coil. Within this dark nucleus there appeared no circulation. In one of them, the passage of the blood particles could be traced from the point of entrance on one side to the dark nucleus, and from thence to the opposite extremity of the vessel. Most of the papillæ in this subject presented this appearance; in a few other papillæ the vascular coils existed as usual. I have met with papillæ of the same appearance in numerous other instances. After the engorgement of the capillaries had continued some time, the usual signs of the escape of the corpuscles were to be detected, and they were seen diffused over various parts of the membrane mingled with blood-discs, arising from the ruptured vessel. In other experiments, where no such loss of blood occurred, the tongue became entirely covered with corpuscles, without any admixture of blood-discs.

While engaged in the examination of the same animal I made a few observations upon the muscular striæ, which I here mention, as they were very distinctly seen without any further preparation of the organ. The fibres of the genio-glossus appeared perfectly round and cylindrical, with very perfect transverse markings, the direction of which varied in the same fibre on altering slightly the focus of the instrument, thereby indicating that at different depths the striæ had different directions. At the extremity of the tongue the fibres no longer presented this rounded appearance, but seemed perfectly flat. The transverse striæ at this place did not appear to be confined within separate cylinders, but the whole surface was covered with muscular striæ and with dark parallel lines, indicating the presence of muscular fibres. The entire surface thus appeared to be of a muscular nature. The most ready and correct representation of these diffused striæ, is by covering a piece of paper with prints of the papillæ of the top of the finger, which has been previously rubbed over with a little printer's ink, in other words, making finger-marks on the paper; when, by drawing parallel lines among these printed papillæ, a most faithful image is obtained.

With regard to other appearances presented by the capillaries, they will be found described in my former communication.

Those who have patience to watch for several hours the phænomena which occur in the capillaries, will perceive the great increase that takes place in the diameters of those under the influence of prolonged irritation, particularly at the edges nearest the irritating agents employed to secure the organ. After watching these changes, it will be seen how slight a cause, such as a temporary obstruction of a vessel by a few globules, is sufficient to determine whether the increase in volume shall be confined to one vessel or be divided among two or more. Therefore we can readily comprehend how the same causes in operation on the embryo may create all those anomalies observed in the distribution of the aortic trunks in the adult, which may be found represented in Bourgery's plates.

In endeavouring to account for the fact of the passage of the corpuscles through the vessels we find considerable difficulties. It cannot be referred to the influence of vitality, as it is observed likewise to take place after death. It may be surmised, either that the corpuscle, after remaining a certain time in contact with the vessel, gives off by exudation from within itself some substance possessing a solvent power over the vessel, or that the solution of the vessel takes place in virtue of some of those molecular actions which arise from the contact of two bodies; actions which are now known as exerting such extensive influence in digestion, and are referred to what is termed the catalytic power.

While advancing these hypotheses we must not omit to bear in mind, that corpuscles are constantly seen adherent, for a long space of time, to the inner sides of a vessel without altering their appearance in the least*.

Mucus.—In ordinary circumstances, the mucus which is formed on the tongue of the frog contains no particles in suspension, but entirely consists of a thick viscous substance,

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* Acetic acid applied to an engorged vessel on the living tongue will dissolve the outer vesicle of the blood-disc and leave the nucleus completely uncovered, in the same way as when the blood is out of the vessels. The action of the acid is almost immediate.

completely transparent. After prolonged irritation, however, as in the above-mentioned experiments, when corpuscles are seen diffused over the tongue, such is no longer the case; and numerous corpuscles are then found in the mucus which exists there, and which then possesses all the characters of the substances which are thrown off from the irritated mucous membranes of the human subject. It would be unnecessary to adduce any further evidence of the common origin of these two kinds of mucus, or rather of the globules which they contain.

Purulent matter.—Although the formation of purulent matter itself has not been included in the present observations, (this subject requiring a separate examination, which I will defer to a future occasion,) the presence of its most general and constant ingredients, viz. the purulent globules and the fibro-albuminous fluid which contains them, may easily be explained by the preceding observations, in which the extra fibrination, corpusculation, and the extra corpusculation of the vessels, represent on a small scale all the principal phænomena accompanying its formation.

Absorption.—After having shown the manner in which the capillaries, engorged with stagnant blood, give off the corpuscles which they contain, it now remains to examine how they are enabled to discharge the rest of their contents. When these contents consist of blood in a fluid state, we can readily understand how, after the removal of the irritation, the engorgement would gradually cease, as is constantly observed with regard to vessels in this state. But many of these vessels, particularly those which have given off corpuscles, contain blood which appears to consist of a gelatinous mass, and is probably in a state of coagulation. The most simple notion of hydrodynamics shows the difficulty of driving a material of a semi-solid consistency through a delicate elastic tube. The question then arises as to the process by which these vessels discharge their contents and become again capable of conveying the blood to the parts which depend upon them for their nutrition. The following observations will in a great measure answer this question.

The frog of Obs. 1, after being kept twenty-four hours in water, was examined. The tongue, at first of its natural colour, became gradually injected, and presented the same appearances as were observed on the preceding day, with respect to the circulation and the escape of the corpuscles from the vessels. But in addition tubes were perceived in numerous places, containing an amorphous, colourless and nearly transparent substance, incompletely filling the cylindrical cavity which contained it. Many of these tubes appeared interrupted, and their course could only be traced by means of disjointed fragments containing the amorphous matter, which, under the highest magnifying power, appeared to consist of numerous irregular fragments loosely packed, and presenting a very considerable surface. Capillaries which contain recently coagulated blood are known to present a granulated, uneven appearance, caused by the irregular arrangement of the blood within them. The tubes were therefore the engorged capillaries of the previous day, which had been deprived of their colouring matter and some of their fluid contents by a chemical action, which is generally termed absorption; while other parts, not quite so easily acted upon, were not yet removed. The size of these granular tubes corresponded with the ordinary diameters of the capillary vessels and their trunks. Their direction likewise was the same as that pursued by vascular trunks and capillaries. Blood-vessels of about the same size could generally be seen at their sides, with an internal current of blood: some of these were probably of recent formation. The other tissues were in their usual condition: thus the muscles presented their characteristic striæ very distinctly; the nervous trunks and fibres were unimpaired; and the ramifications of the cellular fibres were also intact.

The subject of Obs. 2 was examined after the same lapse of time; the organ had nearly recovered its natural colour previous to this second examination. Numerous tubes with their amorphous contents were seen at different parts in exactly the same condition as mentioned above. The papillæ with the dark centre, as before described, were in the same state as the day before: small portions of muscular fibre were perceived, with their striæ lacerated and broken. The other tissues were unaltered.

Kensington, Oct. 15, 1846.

EXPLANATION OF PLATES.

PLATE I.

Fig. 1. Represents the dorsum of the frog's tongue covered with numerous papillæ, two of which are circular, and contain vascular coil connected with capillary vessels. These papillæ are intimately connected with the perception of taste, and are analogous with the fungiform papillæ of the mammiferæ. The other papillæ represented are destitute of internal circulation, and much more irregular in form, and correspond to the filiform or conical papillæ of the mammiferæ. The whole drawing is covered with scales of epithelium, amongst which are occasionally seen what appear to be small foramina, but which did not exist in this case.

Fig. 2. A fungiform or neuro-vascular papilla, as seen under a higher magnifying power. Within it is the vascular area. The external zone presents the radiated appearance which is sometimes seen.

Fig. 3. Blood-discs and corpuscles with a capillary; some of the latter near the sides were inspected for a long time, and remained fixed in the same situation, while a rapid current was traversing the vessel.

Fig. 4. Corpusculation of a vessel towards the end of the tongue. This vessel was traced for a considerable distance and contained no blood-discs.

Fig. 5. Extra fibrination of a vessel. The smaller globules are probably globular particles of fibrine, the others are extravasated corpuscles.

Fig. 6. Varicose swelling of a ruptured capillary giving passage to the blood. In the same subject were perceived similar swellings, but without a rupture, in which the blood circulated.

PLATE II.

Fig. 1. Represents vessels of the inferior surface of the tongue as they appear after the escape of the corpuscles, filled with stationary blood, deformed and indented at the points of escape, near which the corpuscles are generally found. A portion of a vessel with an internal current is likewise seen with discs, and internal and external corpuscles. No indentations are seen near these, probably from the force of the current, which directly restored the form of the vessel. The remainder of this figure was covered with membranous muscular striæ, which to avoid complication are not represented.

Fig. 2. Shows the process of absorption of stagnant blood. In this case the vessels themselves still remain, while their contents are unequally absorbed. Tortuous vessels ramify among them.

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 133.]

June 18, 1846. "THE Electric Fluid." By W. F. Stevenson, Esq., F.R.S.

The author denies the existence of two electric fluids, and maintains that all the phenomena are explicable on the hypothesis of a single fluid; which when present in a conducting body renders it positive, and in a non-conducting body, negative; but a body which is naturally a conductor, may, he asserts, be rendered otherwise, by changing its form.

"Observations of the Heights of the Thermometer and Barometer made at Lenham Lodge, near Maidstone, Kent, during the first nine days of the month of June 1846." By George Hunsley Fielding, M.D., F.R.S.

On Sunday the 7th of June 1846, the thermometer in the shade rose to the extraordinary height of 94° Fahr., exceeding by one degree the heat of the 13th of July 1808, which was considered to be the highest on record in this country.

"On the relative dynamic value of the Degrees of the Compass; and on the Cause of the Needle resting in the Magnetic Meridian." By Sir Graves C. Haughton, K.H., M.A., F.R.S., Foreign Associate of the Institute of France.

By ascertaining the distances at different azimuths at which a bar magnet placed with its axis directed to the centre of a magnetic compass needle caused the needle to assume the position in which its axis was in the same line with that of the magnet, the author found that these points of distance form a peculiar curve, which was nearest to the centre of the magnet at the east and west azimuths, and receded from it as it advanced to the north and south; and was twice the distance from it at the north than at the south azimuths. From this and other experiments he infers that the value of every degree of the compass is inversely as the square of the length of the ordinate or co-ordinate passing through it, the abscissa being considered as zero; and that the magnetic needle does not rest in the magnetic meridian in consequence of polar attraction, but is impelled in that direction by the effect of repulsion, from the east or west, and that its natural position is the result of an equilibrium between the eastern and western repulsions. On the other hand, the return of the needle, when it is placed in the reverse position, that is, when made to deviate 180° from its natural position, is the result of an attraction towards the east and west positions; the force of attraction in that case being quadruple the force of repulsion in the former case. He is consequently led to the conclusion that the operation of terrestrial magnetism is totally different from that of an artificial magnet, in which latter case the attractive and repulsive forces are exactly equal.

"Remarks on the Extractive Material of Urine, and on the Excretion of Sulphur and Phosphorus by the Kidneys in an unoxidized state." By Edmund Ronalds, Ph.D., Giessen. Communicated by Golding Bird, M.D., F.R.S.

In the course of an experimental inquiry in which the author was engaged with a view to ascertain whether larger quantities of carbon were discharged by the kidneys in cases in which the functions of the lungs or liver were imperfectly performed, he was led to the result, that sulphur, not combined with oxygen in the form of sulphuric acid, existed in the urine to the amount of from three to five grains in the course of a day; and also that phosphorus, not in the state of phosphate, was in the same period excreted by the same channel, to the extent of nearly six grains.

"On some peculiarities of Fœtal Digestion." By George Robinson, M.D. Communicated by William Bowman, Esq., F.R.S.

The author endeavours to show, contrary to the assertions of Dr. Robert Lee, that the human fœtus, in common with that of all the higher animals, invariably contains, during the latter periods of intra-uterine life, albuminous or nutritious matter, which undergoes digestion in the small intestines. He finds that the nature of this substance varies very much in different animals. In the earlier periods of fœtal life, the stomach contains very little, if any, nutritious matter. He is disposed to consider the salivary glands as the source of the albuminous matter found in the stomach at later periods.

"Extracts of Letters from Captain Williams, first Assistant to the

Commissioner of Arracan, and from Ensign Haukeu and Mr. Howe, Marine Assistant Commissioner, giving details of a curious phenomenon seen at sea off Ryook Phyoo." Communicated by Viscount Mahon, F.R.S., Pres. S.A.

The appearance in question, seen between five and six o'clock in the evening of the 2nd of December 1845, was that of a large flame far out at sea, flickering several times for fifteen and twenty minutes, and suddenly ceasing. It was conjectured to have been either a large ship on fire, or a volcanic eruption; but no positive data exist for determining the question.

"On the Fossil Remains of the soft parts of Foraminifera discovered in the Chalk and Flint of the South-east of England." By Gideon Algernon Mantell, Esq., LL.D., F.R.S.

By subjecting small portions of calcareous rock to the action of diluted hydrochloric acid, the calcareous earth and the shells it had enveloped were removed; the residue consisting of particles of quartz and of green silicate of iron; and also numerous remains of the soft parts of animalcules, chiefly Xanthidia and Rotaliæ. The organization of the latter appears, from the observations of Ehrenberg, to be very simple, and to have no relation with the Cephalopoda, as was formerly imagined. The body is enclosed within the shell, which is polythalamous, and it occupies not only the outer chamber, but also all the cells contemporaneously, and the shell is pierced all over with minute pores, through which tentacula protrude; and there are also several soft transparent feelers, or *pseudopodia*, which are instruments of locomotion. When the shell is removed by acid, the soft body is exposed, and is seen to extend to the innermost chamber; and there is a connecting tube occupying the place of the siphuncle of the nautilus, but which is the intestinal canal; for the cells are the receptacles of the digestive sacs or stomachs, in which monads, naviculæ, and other minute infusoria, which the animal had swallowed, may sometimes be observed. In the fossil remains, the appearance of the parts which the author supposes to be the digestive organs, is that of a series of bladders or sacs, composed of a tough flexible integument, and connected by a tube. These organs are more or less filled with a dark substance; those which are distended are always well-defined, while the empty ones are collapsed and disposed in folds, just as membranous pouches would appear under similar conditions. The sacs regularly diminish in size from the outer to the innermost cell, and vary in number from fourteen to twenty-six. In some instances, small papillæ are observable on the external surface of the integument, which are probably vestiges of the bases of the pseudopodia or tentacula.

Drawings of the various specimens described by the author accompany the paper.

"Experiments relative to Animal Temperature, showing that there must be some source of animal heat besides the combustion of the Carbon and the Hydrogen contained in the Food of Animals." By Robert Rigg, Esq., F.R.S.

The subject of these experiments was a labouring man in the employment of the author, living on his ordinary food, and working at his usual employment. A strict examination was instituted into the quantity and chemical constituents of the ingesta and egesta during ten days; at the end of which time he had gained one pound in weight. He infers from the results of this experiment, that the carbon and hydrogen contained in the food of animals, which enter into combination with the respired oxygen, forming carbonic acid and water, do not generate sufficient heat for the purposes of animal life; and that consequently there must be some other sources of heat in the animal economy, one of which he believes to be the secretion of carbon.

"Electro-Physiological Researches.—Fourth Memoir. On the Physiological Action of the Electric Current." By Charles Matteucci. Communicated by Michael Faraday, Esq., LL.D., F.R.S.

In the prosecution of his inquiries on the physiological action of electric currents, the author found it necessary to employ an apparatus, which was expressly made for him by M. Bréguet, adapted to the delicate appreciation of the intensity of the force of the muscular contractions excited by those currents; of which apparatus he gives a minute description, illustrated by a drawing. He was thus enabled to institute an exact comparison between the contractions caused by the direct, and those by the reverse currents, both at the commencement and at the termination of their action. The following are the general conclusions he deduces from the experiments thus conducted.

1. The passage of the electric current through a mixed nerve produces a variation in the excitability of the nerve, differing essentially in degree, according to the direction of the current through the nerve. This excitability is weakened and ultimately destroyed; and this takes place more or less rapidly according as the *direct current*, that is, a current circulating through the nerve from the centre to the periphery, is more or less intense. On the other hand, by the passage of the same current in the contrary direction, that is, from the periphery to the centre, or the *inverse current*, the excitability is preserved and increased.

2. The variations in the excitability of the nerve produced by the passage of the current, tend to disappear more or less rapidly on the current ceasing. If the nerve be taken from a living animal, or from one in which life is but just extinct, so that its excitability is very great, these variations last only as long as the current continues to circulate; while, if the nerve has already lost some of its excitability, they survive the cessation of the current by a period of from one to ten or fifteen seconds.

3. If the same current be made to act upon a mixed nerve, the contraction which occurs on the first moment of its introduction is very different according to its direction; the direct current always occasioning a stronger contraction than that produced by the inverse current.

"On Phlogiston and the Decomposition of Water." By W. F. Stevenson, Esq., F.R.S.

The author is of opinion that the evidence on which the modern theory of the composition of water is founded, is fallacious; and believing water to be a simple body, he conceives that it forms hydrogen by combining with the electric fluid, which he imagines to be identical with the phlogiston of former chemists. He cites the opinions of Priestley, Cavendish and Watt, as corroborating his views, and interprets their experiments in conformity with the hypothesis he has adopted.

"Suggestion intended to confirm Franklin's Theory of Electrostatics, by explaining the phenomena of Repulsion between bodies negatively electric." By James A. Smith, Esq. Communicated by S. Hunter Christie, Esq., Sec. R.S., &c.

The author conceives that in negatively electrified bodies, or bodies having less than their natural quantity of electricity, the redundant matter must have a tendency to escape, and thus the equilibrium of its cohesion is destroyed; and that two bodies in such a condition must mutually repel each other.

"On Sir Isaac Newton's Method of Finding the Limits of the Roots of Equations." By Herbert Panmure Ribton, Esq. Communicated by John George Children, Esq., F.R.S.

The author states that he has reason to believe that by generalizing from successive inductions of equations, a formula more universal than Newton's Binomial could be found.

LX. *Intelligence and Miscellaneous Articles.*

ON A NEW METAL, PELOPIUM, CONTAINED IN THE BAVARIAN TANTALITE. BY PROF. H. ROSE.

IN a former paper, on the composition of the so-called tantalic acid which occurs in the columbite of Bodenmais in Bavaria, I showed that it consisted of two acids, one of which differs so decidedly from all known metallic oxides, that I did not hesitate to regard it as the oxide of a new metal, which I named niobium*. I did not then enter into a description of the second acid, which occurs in company with the niobic acid, but merely observed that it possessed great similarity to the tantalic acid procured from the Finland tantalites.

The separation of the two acids according to the method I formerly described was exceedingly troublesome and tedious. After I had suspected a peculiar substance in the so-called tantalic acid from columbite, and had vainly attempted in various ways to isolate it, I succeeded in effecting this only approximatively on converting the acid into chloride, by mixing it with charcoal and passing a current of chlorine over the heated mixture. I obtained a yellow, readily fusible and very volatile chloride, and a white, infusible, less volatile chloride. Both were converted by water into metallic acids, which were not dissolved by the hydrochloric acid formed, but separated on boiling, and could easily be freed by washing with water from

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* The paper here referred to will be found at p. 35 of the third volume of the Chemical Gazette.

every trace of acid; but when the acid from the white infusible chloride, after I had separated it as well as possible from the yellow one, was again mixed with charcoal and treated with chlorine, I constantly obtained yellow and white chloride; less, it is true, of the first than when the mixture of the two acids as they occur in the mineral was employed; but even when the operation was very frequently repeated with the acid from the white chloride, it was not possible to obtain by this method a pure white chloride free from yellow. I observed however that the white chloride was only partially sublimed. When it was separated as much as possible from the yellow chloride, and by sublimation also from a white non-volatile residue, it at last afforded an acid, which on treatment with charcoal and chlorine yielded a tolerably pure, white, wholly volatile chloride, the chloride of niobium. The white fixed residue yielded with charcoal and chlorine a large quantity of yellow chloride; and after removing this by sublimation, again left a white non-volatile residue, which, on being again subjected to a similar treatment, afforded the same products.

On comparing this behaviour of the yellow chloride with that observed on treating a mixture of pure tantalic acid and charcoal with chlorine, I obtained a perfectly similar yellow chloride and a white non-volatile residue; but the quantity was far smaller, and its production could be entirely avoided if, in the preparation of the chloride of tantalium, every trace of humidity and atmospheric air had been carefully excluded. Moreover, the sublimed yellow chloride from the Bavarian mineral very much resembled the chloride of tantalium. This similarity likewise extended to the acids prepared from the two chlorides; they behaved so much alike, that it was only after long-continued investigation properties were discovered by which they might be separated.

Both in the preparation of chloride of tantalium with the tantalic acid from the Finland tantalite, but especially in that of the yellow chloride from the Bavarian mineral, I frequently obtained considerable quantities of a red chloride, which was still more volatile than the yellow one, and proved on examination to be chloride of tungsten. When the chlorides are exposed for some time to the air, the tungsten can be removed by digestion with ammonia as readily soluble tungstate of ammonia.

Sometimes chloride of tin and chloride of titanium were obtained in preparing the chloride; they could be readily distinguished, by their fluid state of aggregation, from the other chlorides.

The formation of the chlorides of tungsten and tin was in so far remarkable, as the acids from which the chlorides were prepared had been kept in a moist condition for a long time in contact with sulphuret of ammonium. I draw especial attention to this circumstance, because, unless perfectly freed from these impurities, the chlorides and the acids prepared from them are obtained with very different properties.

The yellow chloride from the Bavarian mineral differs therefore principally from the chloride of tantalium by its leaving a white non-volatile residue on its production, or rather on its volatilization, at a high temperature. This residue consists principally of the acid which may be obtained from the yellow chloride by decomposition with water.

In the preparation of the yellow chloride from the columbite of Bodenmais, there is formed along with it an oxychloride, which is decomposed by heat into chloride and acid, just like the tungstate of the chloride of tungsten. The formation of the oxychloride can be prevented by placing a long layer of charcoal in the anterior portion of the glass tube, in which the mixture of acid and charcoal is to be treated with chlorine. While the chlorine is passing through the tube, this charcoal is first raised to a strong red heat, and then the mixture.

The acid of the yellow chloride from the Bodenmais mineral, which is contained in it along with the niobic acid, I have named *Pelopic* acid, and the metal *Pelopium*, from Pelops the son of Tantalus and the brother of Niobe; to point out, at the same time, by this name, not only its simultaneous occurrence with the oxide of niobium, but more particularly the very great resemblance of pelopic acid to the tantalic acid from the Finland tantalites. This similarity is indeed more perfect than exists between the combinations of any other two simple metals; it is so great, that it was only after a long-continued and most minute investigation that I could decide upon publishing the results I had obtained. The combinations of niobium are, on the contrary, very different from those of pelopium or tantalium.

I will here describe the most important properties by which the compounds of tantalium differ from the corresponding compounds of pelopium, and at the same time enumerate those of niobium.

In its properties pelopic acid is intermediate between tantalic and niobic acids, just as strontia between baryta and lime. And in the same way as we are able to explain many properties of strontia, by assuming it to be a mixture of the two last-mentioned earths, we are able to determine *à priori* most of the properties of pelopic acid, by admitting it to be a mixture of a large proportion of tantalic acid with a small quantity of niobic acid; and as was the case with bromine, which, on its discovery, was considered to be a combination of chlorine and iodine, I myself was long of opinion that the pelopic acid was nothing more than tantalic acid still contaminated by a certain quantity of niobic acid, which I had not succeeded in separating. It was only by an uninterrupted investigation of this subject for several years that I became convinced of the distinctness of pelopic acid.

The chlorides of the three metals dissolve in cold concentrated sulphuric acid without any evolution of heat, but with disengagement of hydrochloric acid; but if the solution of the chloride of tantalium and pelopium is boiled, it solidifies to a jelly. Water then does not dissolve any of the tantalic acid, but a large quantity of the pelopic acid. The solution of the chloride of niobium in sulphuric acid is not rendered turbid by boiling; it even remains clear on dilution with water, but if it be now boiled, the whole of the niobic acid is precipitated from the solution.

Chloride of tantalium dissolves in hydrochloric acid in the cold to a turbid liquid, which after some length of time forms an opaline jelly, from which cold and boiling water dissolve only traces of tantalic acid. But if chloride of tantalium is treated with boiling hydrochloric acid, it does not dissolve entirely, and on cooling it does not form a jelly, but water now dissolves the whole of it to an opaline liquid, which is not rendered more turbid by boiling. Sulphuric acid produces in it, after some time, a voluminous precipitate even in the cold. The chloride of pelopium behaves in a similar manner, except that sulphuric acid does not produce a precipitate in the cold in the solution obtained by boiling and diluted with water, but only on boiling. Chloride of niobium does not dissolve in cold hydrochloric acid; scarcely anything is dissolved on the addition of water; when however chloride of niobium is boiled with hydrochloric acid, it does not dissolve in it, but on diluting with water the whole dissolves, and the niobic acid is not even precipitated from the solution by boiling. When however sulphuric acid is added, a turbidness results even in the cold, and the whole of the niobic acid is precipitated by boiling. When, on the other hand, but a small quantity of hydrochloric acid is placed in contact with the hydrates of the acids, the result is quite a different one. The same is the case when the chlorides of the three metals are treated with much water. The niobic acid is then completely separated on boiling from the chloride of niobium, and also the pelopic acid from the chloride of pelopium; but tantalic acid does not separate quite so completely from the chloride of tantalium.

Chloride of tantalium, heated with a solution of hydrate of potash, is partly dissolved; but a solution of carbonate of potash does not dissolve any tantalic acid even on boiling. Chloride of pelopium is dissolved in large quantity by solution of caustic potash, and even carbonate of potash dissolves it in tolerable abundance on boiling. Chloride of niobium is dissolved even in the cold by a solution of potash, and also by boiling in a solution of carbonate of potash.

Tantalic acid remains white on being heated to redness; pelopic acid is rendered slightly yellowish; niobic acid, dark yellow. On cooling, both again become as white as before ignition.

All three acids exhibit, when their hydrates are heated very strongly, the phænomenon of incandescence. This however is not the case when the compounds with sulphuric acid are treated with ammonia, and then heated to redness.

Tantalic acid, exposed in a current of hydrogen to a strong red heat, remains white; pelopic and niobic acids become black; but the reduction which these acids undergo is quite inconsiderable, for very doubtful traces of water are perceptible, and the blackened acids quickly become white when heated with access of air, without experiencing any perceptible increase in weight. When tantalic acid is heated to redness in a current of gaseous ammonia in a brisk charcoal fire, it is turned gray, with the formation of but slight traces of water. Pelopic and niobic acids become black, and are reduced, with the production of a considerable quantity of water.

When tantalic acid is heated in a brisk charcoal fire, and sulphuretted hydrogen gas passed over it, it becomes slightly gray, but no trace of water is perceptible. Pelopic and niobic acids are converted by the same treatment slowly but entirely into sulphurets, with formation of water and separation of sulphur.

Metallic pelopium can be prepared from the chloride by treatment with ammonia, in the same way as the metals from the chloride of tantalium and chloride of niobium. It has the greatest resemblance to tantalium.

When the ignited acids, which are insoluble in almost all reagents in the moist way, are fused in a silver crucible with hydrate of potash, they dissolve in it. The fused mass is soluble in water. Hydrate of soda behaves in a different manner. When the ignited acids are melted with it, the fused masses obtained are not clear; but an insoluble sediment is formed, which does not dissolve in any excess of the alkali. If the fused mass be treated with a moderate quantity of water, the excess of soda is removed, and a white insoluble mass remains. If, after removing the free soda, a large portion of water be poured over the insoluble mass, it dissolves, and most completely when niobic acid has been employed.

The insolubility of the three acids in excess of soda, while the potash compounds are soluble in excess of potash, essentially characterize them. In this they differ from similar acids, especially from tungstic acid. When the solutions of the soda salts are mixed with concentrated solutions of hydrate of soda, they immediately become turbid; if the mixture was made very slowly and carefully, all three soda salts may be obtained in crystals, which are deposited on the sides of the vessel. But crystals only of the niobate of soda can be easily obtained of any size. I succeeded in obtaining them half an inch and more in size, but in general they are much smaller. They are sparingly soluble in cold, more readily soluble in hot water; the solution may be boiled without becoming turbid; it can be evaporated, and the niobate of soda deprived of its water of crystallization without being decomposed. The salt is only rendered insoluble in water by being heated to redness.

The pelopate, and especially the tantalate of soda, are less stable; when their solutions are boiled, an insoluble white precipitate separates, which is an acid salt of soda.

When the niobate of soda is exposed to a red heat, and a current of dry sulphuretted hydrogen passed over it, a dark black crystalline mass is obtained, from which water removes hydrosulphated sulphuret of sodium, while crystalline sulphuret of niobium remains undissolved.

When pelopate of soda is treated in the same manner, there is also no sulphosalt formed, but only sulphuret of pelopium. The tantalate of soda remains white on treatment with sulphuretted hydrogen, but its soda is converted into sulphuret of hydrogen and sodium.

When niobic acid is fused with an excess of carbonate of soda until the fused mass no longer decreases in weight, the amount of oxygen in the expelled carbonic acid is twice that in the niobic acid employed. The results obtained on fusing pelopic and tantalic acids with carbonate of soda did not agree. By long-continued fusion of tantalic acid with carbonate of soda, so much carbonic acid is expelled that its amount of oxygen was equal to that of the tantalic acid employed, and finally exceeded it. But nevertheless this basic salt does not dissolve undecomposed in water, but leaves a considerable residue of acid tantalate of soda. Something similar takes place with pelopic acid, only the basic pelopate of soda formed dissolves entirely in water.

When the three acids are fused with carbonate of potash, they exhibit similar properties; but the potash salts are as soluble in the excess of carbonate of potash as in hydrate of potash. In this way we obtain compounds which are soluble in water and crystallize; but they contain carbonate of potash, which cannot be separated in any manner.

The combinations of tantalic acid with the alkalies are characterized by their passing on all occasions into insoluble acid salts, especially on boiling and evaporating their solutions. The solutions of the alkaline pelopates exhibit this property in a far less degree, those of the niobates not at all. Insoluble acid niobates of potash or soda can only be produced by not fusing the acid a sufficient time with the carbonates.

Tantalic acid is soon and entirely precipitated from its alkaline solutions by carbonic acid as an acid salt; the same is the case with pelopic acid, but with greater difficulty and far more slowly. It is owing to this that the neutral solution of tantalite of soda becomes turbid even by exposure to the air, while that of the pelopate of soda does not become turbid even after long exposure, which is characteristic of it. Carbonic acid produces a precipitate in the solution of alkaline niobate only after a considerable length of time, which however is again dissolved by much water.

When the solutions of the alkaline tantalates and pelopates are treated with an excess of hydrochloric acid, the eliminated acids dissolve to faintly opaline liquids. Sulphuric acid produces in these solutions precipitates, and separates the acids on boiling; however, only the pelopic acid entirely, and not so the tantalic acid. Hydrochloric acid precipitates the acid from the solutions of the alkaline niobates in the cold, and still more so on boiling; an excess of hydrochloric acid merely dissolves slight traces. This behaviour is in so far interesting, as we have seen that under other circumstances niobic acid may be wholly soluble in hydrochloric acid. Sulphuric acid precipitates niobic acid from its alkaline solutions even in the cold.

From the solutions of the alkaline tantalates the acid is entirely precipitated, without the assistance of heat, by chloride of ammonium, pelopic acid less perfectly, and niobic acid still less.

When the solutions of the alkaline tantalates are rendered acid with hydrochloric or sulphuric acid, a pale yellow precipitate is produced in them by tincture of galls. An orange-yellow precipitate is formed, under similar circumstances, in solutions of the pelopates, and a dark orange-red in those of the niobates.

Ferrocyanide of potassium produces in solutions of the tantalates of the alkalies, when they have been rendered slightly acid, a yellow precipitate; in those of the pelopates, a brownish-red; and in those of the niobates, a red one.

When the three acids are fused with bisulphate of potash, they dissolve in it. Niobic acid alone solidifies with it to a crystalline mass. Water removes sulphate of potash from the fused masses, and leaves compounds of sulphuric acid with the metallic acids, from which however the sulphuric acid can be removed by very long treatment with water.

When hydrochloric or sulphuric acid is added to the solution of the niobate of potash or soda, and then a bar of zinc immersed in it, the separated niobic acid soon assumes a very beautiful pure blue colour. It gradually becomes dirtier, and finally brown. The blue colour is produced, in the solutions of the alkaline pelopates, only on the addition of sulphuric acid; but not even then is a blue colour produced in the alkaline tantalates, which however takes place when the solution of the chloride of tantalium in sulphuric acid is treated with water and zinc.

Tantalic acid yields before the blowpipe colourless pearls with the fluxes even in the inner flame; pelopic acid gives with the microcosmic salt in the outer flame a colourless, in the inner one a brown pearl. Niobic acid colours the microcosmic salt in the inner flame of a beautiful blue; the pearl can be easily blown colourless in the outer flame.

These are the most important differences between pelopic acid and tantalic acid on the one hand, and niobic acid on the other. To ascertain accurately the behaviour of these acids and their combinations is one of the most difficult tasks, as all three acids frequently exhibit highly anomalous properties. We have seen, for instance, that the niobic acid is readily dissolved, under certain circumstances, by hydrochloric acid, when separated from its combinations, while under not very dissimilar circumstances it is almost entirely precipitated by it. This is owing to the acid assuming different isomeric modifications.

The three acids resemble in this respect silicic acid, the behaviour of which towards reagents is frequently remarkable, and only excites less surprise from our having been long acquainted with this acid, and its properties having been thoroughly examined.

This tendency of the three acids to assume different isomeric modifications is connected with the great variability which they exhibit with respect to their specific gravity. My experiments on this subject have led me to the most unexpected results; although I have not terminated my investigations, I will nevertheless communicate at present some of the most important.

Some time ago I drew attention to the fact, that in the artificially prepared titanic acid the specific gravity gradually increases by long-continued ignition, until it attains that of rutile. In the same way the modifications of titanic acid which occur in nature, anatase and brookite, may be converted by continued ignition into rutile. I thought that the publication of these facts wou[illegible] [illegible]duced chemists to examine the specific gravity of other oxides at different temperatures, since these changes have an important influence on the atomic volume. This however has not happened, with the exception of a very interesting investigation of Count Schafgotsch, on the specific gravity of silicic acid, in which he has shown that opal heated to redness has so low a specific gravity, that it floats on oil of vitriol; but that the specific gravity is so increased by heating to redness, that it equals that of chemically-prepared silicic acid (2·2), but which is still considerably lighter than quartz and rock-crystal (2·6).

The changes which the three metallic acids under consideration experience by heating to redness are far more remarkable. When the hydrate of pelopic acid is deprived of its water by a gentle red heat over a spirit-lamp, just sufficient to produce the phænomenon of incandescence, and then exposed to a strong red heat in a charcoal fire, its specific gravity is considerably increased. If we examine the ignited acid under the microscope, we see that it consists for the greater part of amorphous granules, in which some small crystals are perceptible. The ignited acid was then exposed to the most intense, and at the same time continuous heat that a platinum crucible is capable of bearing, that of the porcelain furnace of the Royal Berlin Manufactory. The acid was not melted by it, but was converted into a coarse sandy powder, which, examined under the microscope, consisted of large perfect crystals. The specific gravity of the acid however was thereby considerably diminished; curious enough, it had become still lower than that which the acid possessed after the hydrate had been exposed to a gentle heat over a spirit-lamp in order to expel its water.

On repeating this experiment, the specific gravity of the crystallized acid, which had been ignited in the porcelain furnace, was found to be constant, while by no other temperature could the acid be brought to a constant specific gravity.

These experiments are in so far remarkable as they prove precisely the contrary of what has hitherto been frequently admitted. Crystalline bodies, such as vesuvian, epidote and garnet, fuse at a high temperature, become amorphous, but of lower specific gravity. It is evident that what applies to these substances cannot be advanced as a general rule.

Niobic acid has a far lower specific gravity than pelopic acid. It exhibits a similar behaviour. The acid, exposed to the temperature of the porcelain furnace, appears under the microscope perfectly crystalline.

Tantalic acid behaves very different to the other two acids. It is the heaviest of all, and, by heating to redness in a charcoal fire, increases considerably in specific gravity, from 7·0 or 7·1 to 8·2. In the fire of the porcelain furnace it is likewise converted into a coarse powder, but which does not appear distinctly crystalline under the microscope. Its specific gravity is thereby only slightly lessened.

In all these experiments no alteration in the absolute weight was perceptible.—*From the Chemical Gazette* for September 15, 1846.

ON ILMENIUM—A NEW METAL.

This new metal was discovered by M. Hermann in a mineral which he supposed at first to be yttro-columbite, which he now proposes to call yttro-ilmenite, since it contains no columbic acid. The ilmenium which occurs in this mineral is in a form that has many characters in common with columbic acid, but is distinguished from it by several others; for example the density of ilmenic acid is much less than that of columbic acid; it becomes very yellow by calcination; its hydrate, moistened with hydrochloric acid, assumes a blue colour when in contact with zinc; and it expels when fused with carbonate of soda a much larger proportion of carbonic acid. In the same way, ilmenic acid is distinguished from niobic acid, by the perfect insolubility of its hydrate in concentrated hydrochloric acid, and by its giving no colour to glass by the blowpipe.

A characteristic property of ilmenic acid is, that a solution of ilmenate of soda gives with a mixture of hydrochloric acid and tincture of galls, or ferrocyanide of potassium, precipitates which are of a much deeper brown colour than hydrate of iron; neither columbic nor niobic acid gives a precipitate of so deep brown a colour. The atomic weight of ilmenium is not so great as that of columbium and niobium. If it be admitted that ilmenic acid contains two atoms of oxygen, the atomic weight of ilmenium will be $753{\cdot}0 = \mathrm{Il}^2$.

The preparation of ilmenic acid from yttro-ilmenite is effected by reducing the mineral to fine powder and fusing it with six times its weight of bisulphate of potash, till a limpid solution is obtained; this is to be treated with boiling water, so as to leave an insoluble subsulphate of ilmenium.

To be sure that none of the mineral remains undecomposed, the subsulphate of ilmenium is to be again fused with bisulphate of potash and treated as before. After having well-washed the subsulphate, it is to be moistened, without drying, with hydrosulphate of ammonia, and digested with it; it is then to be washed, boiled with concentrated hydrochloric acid, again washed and dried. These operations yield subsulphate of ilmenium, which when heated in a forge, gives pure ilmenic acid.

Metallic ilmenium is obtained by calcining in an atmosphere of ammonia, the ammonio-chloride of ilmenium, in the same manner as employed in reducing niobium. It forms porous particles or cohering leaves having the black appearance of soot, or of the charcoal produced by the burning of sugar. Ilmenium does not decompose water; neither nitric nor hydrochloric acid nor aqua regia, even when boiling, acts upon ilmenium; when heated in the air it takes fire, burns and yields white ilmenic acid.

Ilmenic acid is therefore obtainable by three processes,—by the combustion of ilmenium, the calcination of hydrate of ilmenium, and the calcination of subsulphate of ilmenium in a forge. When obtained by the first or third method, it has the form of perfectly white fragments, with an earthy fracture; these fragments possess but slight cohesion, for by the slightest pressure, or by moistening with water, they are reduced to a soft powder. The acid prepared by the second process forms white firm masses which have a conchoidal fracture and no lustre.

During calcination, ilmenic acid assumes a fine golden tint; and the acid obtained from the hydrate becomes, under these circumstances, of a deeper colour than the acid prepared by calcining the subsulphate. On cooling, the ilmenic resumes its white colour perfectly. Its specific gravity is from 4·10 to 4·20.

Hydrate of ilmenium is precipitated when excess of hydrochloric acid is added to a solution of ilmenate of soda; it is a white, diaphanous and gelatinous precipitate, which dries in opake compact pieces.

Ilmenic acid does not dissolve in concentrated sulphuric acid, but at a red heat it fuses readily with bisulphate of potash or soda, yielding a limpid product which is yellow while hot and becomes colourless on cooling. After washing this product, the ilmenic acid remains perfectly insoluble, in combination with sulphuric acid, forming a thick white precipitate, which leaves on drying white pieces of an earthy fracture.

Ilmenic acid has but slight affinity for sulphuric acid; by long-continued washing it separates from it perfectly, and is converted into hydrate.

A portion of sulphate of ilmenium, the last washings of which still retained traces of sulphuric acid, was dried at 122° F.; and was then found to consist of ilmenic acid 77·63, sulphuric acid 7·69, water 14·68.

Subsulphate of ilmenium even while moist is totally insoluble in hot concentrated hydrochloric acid; this property distinctly marks the difference between ilmenic and niobic acid. Subsulphate of ilmenium readily loses the whole of its sulphuric acid at a red heat.

Chloride of ilmenium is prepared like the chloride of columbium; it is deposited in the cold part of the porcelain tube like hoar-frost, composed of yellowish translucent prisms; in a moist atmosphere this salt exhales hydrochloric acid vapours; it then becomes white and opake; when thrown into water it is decomposed, with the evolution of much heat, and with the production of hydrochloric acid and diaphanous colourless flocks of hydrate of ilmenium. It absorbs ammonia with great avidity and the disengagement of considerable heat; a yellow mass is also produced, which by calcination *in vacuo* is converted into hydrochlorate of ammonia and ilmenium.

Equal parts of ilmenic acid and dry carbonate of soda, heated strongly to redness, lost carbonic acid equal to 29·1 per cent. of the weight of the ilmenic acid.

Ilmenate of soda is a gray mass with an earthy fracture; when boiled in water it is decomposed into soluble subilmenate of soda and a superilmenate which is deposited in the state of a white powder.

Excess of either nitric or hydrochloric acid completely precipitates the ilmenic acid from ilmenate of soda; this circumstance distinguishes it essentially from columbic and niobic acids, which remain partly in solution.

A concentrated solution of ilmenate of soda does not yield crystals by spontaneous evaporation; the solution becomes gradually turbid by the action of the carbonic acid of the air, and deposits a white powder of acidulous ilmenate of soda.

When ferrocyanide of potassium, and afterwards hydrochloric acid, are added to a solution of ilmenate of soda, a precipitate of a deeper brown colour than hydrate is produced; tincture of galls acts similarly.

Hydrate and subsulphate of ilmenium do not dissolve in acids; acid solutions of ilmenic acid cannot therefore be prepared for the purpose of acting upon zinc; but if the hydrate or subsulphate recently precipitated be moistened with hydrochloric acid, and then exposed to the action of zinc, a grayish-blue colour is produced.

With borax and with a phosphate, ilmenic acid produces glass both in the interior and exterior flame of the blowpipe, which appears to be yellow, but which becomes colourless on cooling. This metal is so named from the Ilmen mountains near Miask, in which the mineral containing it is found.—*Journ. de Pharm. et de Ch.*, Octobre 1846.

[It is very probable that this new metal may prove to be nothing more than pelopium described in the preceding paper. In a communication with which we have been favoured, Prof. Rose states "that he cannot venture to decide in what relation the ilmenic acid stands to pelopic acid, from the few properties which M. Hermann has described. According to him, ilmenic acid acquires a golden-yellow colour on ignition, while pelopic acid, according to my experiments, only becomes yellowish. The most important character of ilmenic acid, by which it differs essentially from tantalic and niobic acid, is said to be that it is precipitated entirely by hydrochloric acid from the solution of its soda salt; while pelopic acid, according to my experiments, when precipitated by hydrochloric acid from a solution of the pelopate of soda, is dissolved in an excess of the acid to an opalescent liquid, in which no precipitate is formed by boiling. According to Hermann, the solution of the ilmenate of soda is rendered turbid by the influence of the carbonic acid of the atmosphere, which I have not observed to be the case with the pelopate of soda. The quantity of carbonic acid which, according to Hermann, is expelled from the carbonate of soda by fusion with ilmenic acid, is entirely different from that which is expelled under similar circumstances by pelopic acid. Tincture of galls produces, according to Hermann, in a solution of the ilmenate of soda to which some hydrochloric acid has been added, a brown precipitate; that formed under similar circumstances in pelopate of soda, according to my observations, is orange-yellow. The beads produced in the inner and outer flame before the blowpipe, by ilmenic acid with borax and microcosmic salt, are yellow while hot and become white on cooling, which does not agree with the behaviour of pelopic acid towards the same reagents. Lastly, the specific gravity of ilmenic acid, as stated by M. Hermann, does not agree with that which I have found for pelopic acid."—ED. PHIL. MAG.]

ON XANTHIC OXIDE.

MM. Einbrodt and Unger notice an error committed by MM. Wœhler and Liebig respecting the composition of xanthic oxide.

It appears to result from the observations of MM. Einbrodt and Unger, that the substance lately found in guano by the latter is not identical with xanthic oxide; according to them xanthic oxide is $C^5 H^4 N^4 O^2$; and *guanine*, which was mistaken for the same substance, is $C^5 H^5 N^5 O$.

M. Unger also observes that guanine yields, by nitric acid, an acid which crystallizes in small regular prisms, which are but slightly soluble in water, and possess the composition of uric acid, plus oxygen and the elements of water. M. Unger confines himself to indicating $C^{10} H^{10} N^8 O^9$, which probably relates to the anhydrous and hypothetical acid; to this must be added $H^2 O$ in order to designate the real and free acid, that is $C^{10} H^{12} N^8 O^{10}$, or rather $C^5 H^6 N^4 O^5$ in M. Gerhardt's notation.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

TINCTURE OF LOGWOOD AS A TEST FOR BICARBONATE OF LIME.

M. Dupasquier states that he employs spirituous tincture of logwood to discover bicarbonate of lime in spring water; according to this chemist the tincture should be prepared, either cold or with heat, with logwood recently cut and divided. The tincture has a brownish colour, and three or four drops being added, instantly communicates a fine violet colour to water which contains the least trace of bicarbonate of lime. In a water which contains any other salt of lime, it occasions only a pale yellowish colour.

Selenitic waters, containing bicarbonate of lime, give the former appearance when cold, whereas after being boiled so as to decompose the last bicarbonate, the second reaction ensues from the sulphate of lime.

According to M. Dupasquier, the bicarbonate of lime, which is held in solution either in spring or river water, acts upon the colouring principle of the logwood in the same manner as the alkalies or the soluble neutral alkaline carbonates.—*Journ. de Chim. Méd.*, Septembre 1846.

ON THE TREATMENT OF OSMIURET OF IRIDIUM.

Instead of acting upon the osmiuret of iridium by calcination with common salt in a current of chlorine, M. Fritzsche prefers fusing this mineral with a mixture of chlorate and caustic potash.

For this purpose he fuses over a lamp, in a capacious porcelain crucible, equal parts of the chlorate of potash and caustic potash, and introduces the osmiuret of iridium. If the heat be continued, the oxygen which is set free attacks the mineral, which is soon observable by the yellowish-brown colour of the mass; this swells considerably and becomes gradually thicker; as soon as the action is over the product is black and sinks. During this operation no vapour of osmium is disengaged; but some is produced if the heating of the mass is continued; this is, however, useless as soon as the action is complete and the mass has become soluble in water. Sixty parts of osmiuret of iridium require ten parts each of chlorate and hydrate of potash, and the operation may be conveniently performed over a lamp in a porcelain crucible; it requires about half an hour.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

ANHYDROUS ALCOHOL.

M. Casoria proposes to employ perfectly dry sulphate of copper to render alcohol anhydrous, and also to employ it as a test for ascertaining that it is perfectly free from water. For these purposes he saturates alcohol, of 83° by the instrument of Gay-Lussac, with fused chloride of calcium: he distilled one-third of the liquid, and to about 7500 grains he added a small quantity of the anhydrous sulphate, and kept the mixture in a well-closed bottle which was shaken occasionally. The salt, by combining with the water of the alcohol, resumes its blue colour, and fresh portions of the salt are to be added till the blue colour ceases to be produced: the alcohol after distillation may then be considered anhydrous.

To determine the absence of water from alcohol, a small quantity of dry sulphate of copper is to be put into a glass tube and the alcohol poured upon it: if no colour be generated the alcohol may be regarded as anhydrous. In performing this experiment it is requisite to keep the tube closed, as the moisture of the atmosphere would interfere with the accuracy of the result.—*Journ. de Chim. Méd.*, Juillet 1846.

ACTION OF FERROCYANIDE OF POTASSIUM ON THE TARTRATE AND CITRATE OF IRON, ETC.

M. Calloud remarks, that on examining the action of ferrocyanide of potassium on the tartrate and citrate of iron, some remarkable reactions occur, which are not common to all the salts of iron.

The neutral tartrate of iron, like the neutral citrate, is precipitated by the alkaline ferrocyanides, like all the other persalts of iron; but the precipitate formed does not possess all the appearances of common prussian blue; its colour is often much modified, without its being occasioned by an excess of either of the salts employed. Tartrate of iron with excess of tartaric acid, on the contrary, does not precipitate the solution of ferrocyanide of potassium; but the liquor becomes immediately blue and remains clear. After a considerable time has elapsed a slight precipitate is formed, which is not dense, like prussian blue; the liquor remains nevertheless bright and blue.

The properties of the blue liquor, obtained by evaporating to dryness the mixture of tartrate of iron with excess of acid with ferrocyanide, are not altered by evaporation to dryness. It is concentrated by heat without being decomposed: the product of the evaporation remains of a fine deep blue colour, and redissolves in water; ammonia however destroys its colour; this effect is explained by the immediate formation of the double tartrate of iron and ammonia, which, like the other double tartrates of iron and alkalies, does not act upon the ferrocyanide of potassium.

Citrate of peroxide of iron with excess of citric acid and ferrocyanide of potassium form the soluble blue, also decomposable by ammonia. The author did not examine the action of ferrocyanide of potassium on any other salts, but thinks it probable that it would be similar upon all the series of salts of iron with organic bases.

The prussian blue precipitated from a persalt of iron, with the sulphate, nitrate or chloride, is not dissolved by tartaric acid alone, nor is it at all modified by it; but on adding ammonia to the mixture, it is converted from blue to white, and presents phases of colour according to the quantity of ammonia added. Thus it becomes violet, purple, amethystine, rose-red, pale red, and then white. This white precipitate becomes blue again by the addition of a fresh quantity of tartaric acid, and passes again to white by ammonia; when exposed to the air and under water, it remains white, and becomes blue by drying.

The interesting point in this reaction is, that it differs entirely according as a solution of tartrate of ammonia, ready prepared, is poured on the prussian blue, or tartaric acid first and ammonia afterwards. In the first case, the prussian blue dissolves, and the solution is of a magnificent violet colour; in the second, the precipitated prussian blue is not dissolved, and from blue it soon becomes white.

The vegetable salt modifies the prussian blue also, but it does not entirely change the colour unless employed in excess.

Tartrate of ammonia, as first mentioned by M. Monthiers, possesses the property of dissolving prussian blue. M. Calloud states that he had also observed this fact, and had noted it for future examination.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

PREPARATION OF VALERIANATE OF ZINC.

M. Lefort remarks that it is perfectly demonstrated, by the numerous experiments which have been undertaken to discover an œconomical mode of preparing valerianate of zinc, that valerianic acid results from the oxidizement of the oil of valerian.

M. Gerhardt has also found that oil of valerian exposed to the air yields valerianic acid, and it is upon this observation that the new process indicated by M. Brun-Buisson is founded, who remarked that, by exposing the distilled water of valerian for a month to the air, he obtained about 230 grains of valerianate of zinc with about 2 pounds of valerian root; and M. Lefort states that he has repeatedly obtained nearly the same proportion.

M. Lefort having however examined whether it was not possible to achieve a much more rapid oxidizement than that effected by the air, gives the following as the best process attained:—

Take of Valerian root..........	2 pounds [av.]
Water	10 pounds [av.]
Sulphuric acid	1540 grains
Bichromate of potash ..	930 grains

Powder the root coarsely and macerate it for about twenty-four hours with the sulphuric acid and bichromate in a distillatory apparatus and then distil; the first quarter of water distilled, as it still contains a considerable quantity of oil of valerian, is to be returned to the retort; the distillation is then to be continued as long as the product acts upon litmus paper.

The distilled product is then put into a porcelain vessel with excess of hydrated carbonate of zinc, and digested on a sand-bath for two or three hours at a temperature of 194° Fahr.; the liquor, filtered while hot, is evaporated over the naked fire until there remains about a pint of it; and the evaporation is to be continued to dryness on plates in a stove.

By this process M. Lefort obtained at three different times 262, 277, and 284 grains of valerianate of zinc from about two pounds of valerian root.

When it is intended to employ valerian root for the preparation of valerianate of zinc, it is requisite to have it fresh dried; the author obtained considerable difference of product when using the recently and long-dried root.

It is even probable that valerian root does not always contain the same proportion of oil at different times of the year, it being well known that the leaves of the lauro-cerasus yield different quantities of hydrocyanic acid at different seasons.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1846.

Chiswick.—September 1. Cloudy. 2, 3. Cloudless and hot. 4. Foggy: fine. 5. Very fine. 6. Cloudy. 7. Foggy: dusky haze. 8. Overcast. 9. Very fine. 10. Cloudy. 11—14. Very fine. 15. Slight fog: very fine. 16, 17. Exceedingly fine. 18. Cloudy. 19. Foggy: very fine. 20. Clear and dry air. 21, 22. Cloudy. 23. Heavy rain. 24. Uniformly overcast: cloudy: overcast. 25. Fine: cloudy: clear and fine. 26. Overcast: slight drizzle. 27. Cloudy and fine: rain. 28. Rain. 29. Clear and cool: rain. 30. Slight fog: cloudy; clear.

Mean temperature of the month .... 60°·79
Mean temperature of Sept. 1845 .... 52 ·60
Mean temperature of Sept. for the last twenty years ... 57 ·22
Average amount of rain in Sept. .... 2·73 inches.

Boston.—Sept. 1—3. Fine. 4, 5. Cloudy. 6. Cloudy: rain, with thunder and lightning P.M. 7. Cloudy: 2 o'clock, thermometer 77°. 8. Cloudy. 9. Cloudy: rain P.M. 10. Cloudy. 11. Fine. 12. Cloudy. 13—15. Fine. 16, 17. Foggy. 18, 19. Fine. 20, 21. Cloudy. 22. Fine. 23. Cloudy: rain early A.M.: rain A.M. 24. Foggy. 25, 26. Cloudy. 27. Fine: rain P.M. 28. Cloudy. 29, 30. Fine.

Sandwick Manse, Orkney.—Sept. 1. Cloudy: rain. 2, 3. Cloudy. 4. Hazy: cloudy. 5. Cloudy: fog. 6. Bright: fine. 7. Bright: fog. 8. Bright: clear: aurora. 9. Bright: clear. 10. Bright: cloudy. 11. Drizzle: damp. 12. Fine: cloudy. 13. Fog: drizzle. 14. Drizzle: damp. 15. Cloudy: drizzle. 16. Rain: cloudy. 17. Clear: cloudy. 18. Bright: clear. 19. Bright: cloudy. 20. Bright: cloudy: clear. 21. Bright: clear: aurora. 22. Bright: hoar frost: clear: aurora S. 23. Bright: cloudy. 24. Rain: cloudy: fog. 25. Clear: cloudy. 26. Cloudy. 27. Clear: cloudy: aurora. 28. Clear. 29. Cloudy: rain. 30. Bright: cloudy.

Applegarth Manse, Dumfries-shire.—Sept. 1, 2. Very fine harvest day. 3. Very fine harvest day, but threatening. 4. Very fine harvest day: still cloudy. 5. Very fine harvest day. 6. Very fine harvest day: thunder. 7. Very fine harvest day. 8. Rain A.M.: cleared P.M. 9—11. Fair and fine. 12. Fair and fine: thunder. 13. Fair and fine. 14, 15. Fair and fine: fog. 16. Dull day: fog. 17. Dull and threatening: fog. 18. Still fair, but cloudy. 19. Gentle rain P.M. 20. Rain A.M. 21. Fair. 22. Fair, but dull. 23. Wet A.M.: lightning. 24. Rain. 25, 26. Showers. 27. Rain all day. 28. Showers: thunder. 29. Wet all day. 30. Fair and clear.

Mean temperature of the month .... 59°·6
Mean temperature of Sept. 1845 .... 52 ·4
Mean temperature of Sept. for 23 years .... 53 ·0
Mean rain in Sept. for 18 years .... 3·25 inches.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. Sept.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
1.	30·261	30·180	29·65	30·23	30·22	30·17	30·15	67	46	60·5	64½	42½	58	57	n.	calm	sse.	sw.	......	......	......	......
2.	30·287	30·269	29·77	30·20	30·19	30·00	30·02	74	41	57·5	64	52	60	64	ne.	calm	sse.	ssw.	......	......	......	·04
3.	30·280	30·227	29·75	30·20	30·19	30·15	30·16	75	42	57	67	54	61	58	ne.	calm	sw.	sw.	......	......	......	·05
4.	30·233	30·163	29·72	30·19	30·15	30·16	30·14	74	54	62	66½	58½	62	60	e.	calm	ne.	se.	......	......	......	·01
○ 5.	30·136	30·054	29·61	30·12	30·02	30·16	30·08	79	58	66	70	56½	62	57	e.	calm	e.	se.	......	......	......	·02
6.	29·954	29·898	29·30	29·92	29·82	29·96	29·89	82	50	66	74½	56	60½	55	e.	calm	e.	calm	·04	......	......	......
7.	29·909	29·859	29·33	29·81	29·75	29·87	29·75	83	58	64	71½	59	62½	58	e.	calm	w.	se.	·03	·37	......	......
8.	29·985	29·901	29·20	29·66	29·79	29·66	29·77	74	52	65	67½	59½	60	56	sw.	w.	sw.	w.	......	......	0·05	......
9.	30·072	29·999	29·43	29·89	30·14	29·85	30·08	80	54	62·5	65	55½	57	52½	sw.	calm	nne.	sw.	......	......	......	·03
10.	30·304	30·262	29·67	30·28	30·30	30·15	30·15	77	60	64	65	40½	57	59½	nw.	calm	w.	sw.	......	·10	......	·01
11.	30·372	30·353	29·80	30·34	30·34	30·23	30·34	74	52	64	70	44	59½	57½	ne.	calm	ne.	w.	......	......	......	......
☾ 12.	30·382	30·375	29·79	30·40	30·45	30·50	30·55	77	50	64	73½	58	56½	54½	ne.	calm	ne.	s.	......	......	......	·01
13.	30·360	30·281	29·82	30·40	30·29	30·47	30·35	70	55	61	72½	52½	56½	54½	ne.	calm	ne.	w.	......	......	......	......
14.	30·257	30·236	29·69	30·24	30·18	30·25	30·21	71	48	62	72½	50	57	55	ne.	calm	sw.	wnw.	......	......	......	·02
15.	30·235	30·204	29·63	30·18	30·14	30·21	30·20	76	49	64	72	50½	56	55	w.	calm	sw.	wnw.	......	......	0·00	......
16.	30·210	30·115	29·58	30·13	30·08	30·20	30·14	79	45	61	66	50	57½	55	w.	calm	sw.	w.	......	......	......	·10
17.	30·050	29·909	29·46	29·98	29·91	30·07	30·08	79	52	62	67	53½	56	50	sw.	calm	se.	ne.	......	......	......	......
18.	29·889	29·859	29·37	29·90	29·82	30·05	29·88	70	39	60	64½	51½	55	48	ne.	calm	ene.	se.	......	......	......	......
19.	29·790	29·614	29·27	29·69	29·54	29·77	29·70	73	40	58·5	59½	52	53½	50	s.	calm	e.	e.	......	......	......	......
● 20.	29·508	29·478	29·00	29·46	29·54	29·71	29·83	70	52	60·5	64	51	54	46	ne.	calm	e.	ne.	......	......	......	......
21.	29·637	29·556	29·13	29·65	29·68	29·91	29·88	71	55	62	63	53½	54	43	e.	e.	e.	calm	......	......	......	......
22.	29·657	29·583	29·16	29·65	29·51	29·87	29·80	70	56	61·5	64	44	50	47½	e.	w.	e.	e.	·08	......	......	......
23.	29·444	29·350	28·86	29·30	29·31	29·67	29·54	71	52	64	64½	53	53½	55	s.	calm	e–sw.	e.	1·21	·21	0·23	......
24.	29·581	29·403	28·95	29·29	29·40	29·46	29·52	67	52	56	64½	52½	58	56	w.	calm	e.	se.	·01	·06	......	·14
25.	29·813	29·682	29·10	29·59	29·61	29·63	29·66	66	48	60	64	54½	57	54	sw.	w.	sw.	se.	......	......	......	......
26.	29·736	29·700	29·16	29·46	29·37	29·58	29·40	65	52	59	62½	50	56½	56½	sw.	calm	sw.	se.	·05	......	......	......
☽ 27.	29·763	29·723	29·10	29·48	29·51	29·50	29·62	66	49	60	61	52	56	52	sw.	w.	sw.	ese.	·24	......	......	......
28.	29·557	29·513	28·30	29·43	29·34	29·69	29·66	66	38	57·5	58	45	54½	48½	n.	s.	wne.	e.	·09	·06	......	......
29.	29·447	29·391	28·90	29·22	29·30	29·52	29·47	65	36	50	53½	47½	53½	49½	sw.	sw.	se.	nne.	·01	......	......	·03
30.	29·800	29·618	29·17	29·58	29·68	29·66	29·67	63	39	53	62	47	53½	53½	w.	calm	sw.	sse.	......	......	0·97	·14

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LXI. *Remarks on some Points of the Reasoning in the recent Discussions on the Theory of the Aberration of Light.* By *the* Rev. BADEN POWELL, *M.A., F.R.S., F.G.S., F.R.A.S., Savilian Professor of Geometry in the University of Oxford*[*].

THE explanation of the phænomenon of the aberration of light, as given in most of the established treatises on astronomy, seems for at least a long time past, to have been generally acquiesced in; until at the meeting of the British Association, 1845[†], the subject was stirred anew by the announcement of the investigations of Prof. Challis and Mr. Stokes, the discussion of which has been continued in so many numbers of the Philosophical Magazine (vols. xxvii. xxviii.), and which seemed to open a new epoch in the history of the question.

The first impressions taken, and the reports which obtain currency in such a case, are not always to be trusted. But we were given to understand that we had hitherto been all along under an entire mistake with regard to aberration; that all the existing interpretations of it were unsatisfactory; that it in fact remained to this day wholly unexplained; that all previous writers had failed in showing the *modus operandi*; and that (in the nomenclature of Dr. Whewell) "the appropriate idea" was wholly wanting, until now for the first time supplied. Some again understood it to have been asserted, that we were now to believe no object really to lie in the direction which the ray coming from it seemed to indicate; that all objects, terrestrial as well as celestial, are subject to aberration; and that the astronomer, referring the star to the cross wire of his telescope, sees the wire out of its true place and not the star.

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[*] Communicated by the Author.

[†] See Report, Sectional Proceedings, p. 9.

Again, the question was mixed up with the undulatory theory. It seemed to be considered, at least by some, that a lengthy and abstruse analytical investigation was now necessary for anything like a complete explanation of what our predecessors thought sufficiently made out by a short popular illustration, or at most a simple geometrical construction.

On some of these points the two very able mathematicians who first started the question were themselves at issue: and though some other points, at first involved in paradox and mystery, were in some degree cleared up in the course of the discussion, yet it cannot be denied that much still remained which called for further elucidation; and even yet it cannot be said that the minds of the scientific world in general have been brought to any agreement, or perhaps any such clear and definite view of the real state of the case as to be able to come to a final decision, or to render useless an attempt to divest the question of some of the ambiguity and difficulty in which it has been involved.

I trust then it will not be deemed superfluous if, at a period when the immediate controversy seems to have arrived at a termination, I offer a few remarks which may assist in forming a dispassionate judgement on the merits of the discussion; and in doing so I will premise that the chief point really at issue seems to me to lie rather in the general nature of the reasoning and the principles of philosophical logic involved, than in those details to which the controversialists have more immediately addressed themselves.

I will then first advert to the question respecting the existing explanations of the *facts*, and afterwards to that respecting *theories* of light.

If we look to past times, from the date of the discovery of aberration, there certainly appears to have prevailed some little hesitation and difference of opinion as to the precise mode of viewing and explaining it.

Bradley himself seems to have felt an anxiety to dwell on any circumstance capable of aiding the better conception of his idea. Indeed, in the manner of his whole discussion we may acknowledge the justice of a remark made by Prof. Rigaud[*] on one portion of it, viz. that it "conveys the strong impression of its being the result of an inquiry which was new to the writer of it; and shows an evident unwillingness to omit anything which might bear upon the subject." Bradley's original idea[†] (as is well known) was suggested by the chance

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[*] Prof. Rigaud's Memoir and Miscellaneous Works of Bradley, p. xxxiv. Oxford, 1832.

[†] Rigaud's Memoir, p. xxx. Thomson's History of Royal Society, 346.

observation that the vane at the mast-head took a direction intermediate to that of the wind and of the boat: and the analogy was obvious between the direction in which the wind *seemed by this index* to come to the boat *in motion*, and that in which the light from a star *seems* to come to the earth in motion; the illustration however is but vague and general. And in the explanations thus adopted generally, that there was still something wanting to give a firm grasp to the apprehension, appears clearly, as well from the loose and wavering conceptions betrayed in some of the speculations of that time connected with the subject, as in the partial endeavours made by others to fix the view of it more precisely.

It is perhaps a proof of the defective ideas prevalent on the subject, that on the occasion of the transit of Venus, 1769, Bliss and Hornsby should both have calculated the effect of aberration as *accelerating* the phases of the transit; while Prof. Winthorp of Cambridge, United States*, contended by means of a detailed illustration (imagining balls fired at a ship in motion), that the effect ought to be that of *retardation*, in which he was supported by Dr. Price†.

A question somewhat of the same kind was also debated among the French astronomers; Monnier, Bailly and Cassini, contending that in a transit of a planet it was only necessary to consider the aberration of the sun; while Lalande introduced the *difference* of the aberrations of the sun and planet: and the former principle was defended by M. Jeaurat, on the ground that the planet was dark, and its existence only shown by the light of the sun surrounding it‡.

The elaborate discussion of Euler§ contributes little perhaps to the precise explanation. He treats the whole subject in a geometrical form; considering the various cases of a luminous body seen by an observer in motion later in time than the truth, even if the observer be at rest, and different in direction if one or both be in motion: but in constructing for the composition of motions he makes no reference to any further essential conditions.

Even at a much later period, Dr. Young observed‖, "some mistakes appear to have been made respecting this subject; they may be avoided by attending to one general principle; that is, when a body moves uniformly forwards, the relative situation of another body, whether quiescent or in motion, appears at all times to be such as it really was at the moment of the emission of the light of the second body; in other words,

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* Philosophical Transactions, 1770, p. 359.

† Ibid, p. 536.

‡ *Mém. Acad. Paris*, 1786, p. 572.

§ *Comment. Petrop.* xi. 150. 1739.

‖ Natural Philosophy, ii. 294.

neglecting the changes in the earth's motion during the passage of light, the apparent place of the sun, or of any star or planet, is its true geocentric place for the instant at which the light was emitted by the sun or star, or reflected by the planet."

This explanation appears to me, I must confess, itself involved in no inconsiderable obscurity. It may however indicate the kind of notions which were commonly prevalent, and which called for such an answer; and at any rate, the precise point of view in which the distinguished author thus placed the subject, will be at once seen to be quite distinct from that taken in the recent investigations.

Other writers seem to have felt in like manner the defective notions which prevailed, and sought to aid the more correct conception by various analogies. Lalande* illustrated the case by supposing a carriage with a front window open while rain falls vertically; when at rest no rain will enter, but when in motion it will. Maupertuis referred to the instance of hitting a bird flying, by aiming at a point in advance of its true place; and others have imagined various like cases.

But these popular illustrations fail in conveying precisely the idea of which we are in quest. The light from the star after all comes *directly* to us in the position where the earth is for the *instant* of observation; and that it is not the *same* ray which would have come to us at a previous instant, though it is one parallel to it (or in the case of a nearer body, differing only by its parallax), is indeed evident, but does not explain the fact, that the point indicated by the telescope differs from the real place of the star; and where and how, it would still be asked, does the deviation take place? It is probable that the more discerning students would have been led to some answer to this question, from considering attentively the necessity which obviously exists, and indeed on which the whole turns, for *referring the star to some fixed point*, that is to a point fixed relatively to the observer, though really in motion with him and the earth, such as the cross wire of the telescope.

Now in some even of the earliest explanations offered, it is, I think, clear that at least a near approach to a more precise statement of the case, including this essential idea, is distinctly made. In Bradley's original paper† it is worthy of notice that he specially introduces the consideration of the direction of the *tube* of the telescope, and the light passing along its axis, as essential to the conception of the case (p. 647). But besides this, in his lectures at Oxford (which commenced in

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* *Astron.* § 2831.

† Philosophical Transactions, 1728.

1729), he employed a small model (still preserved among the apparatus of the experimental lecture-room), which consists of a board or ruler made to move parallel to itself, while, by means of the same motion, a mark on a thread moves parallel to the ruler over pulleys attached to the fixed part, and consequently describes the diagonal while the ruler moves along the base*.

That this contrivance may be spoken of as showing in general the apparent direction in which the light would enter the eye, is indeed obvious. But the question still remains, *what* light is it which thus seems to come out of its real direction? and it would seem impossible to consider Bradley's model without perceiving that the mark passing down the diagonal must represent the ray passing down *the tube of the telescope*, which is itself in motion with the observer; and it is *essentially this* light which is the medium of the deception, since by *composition* of motions the diagonal actually coincides with another direction in space, viz. the direct course of the ray from the star: or to recur to the case of the *boat*, what is it which corresponds to the misleading *vane*? the answer could only be—*the telescope.*

The fuller discussions given in the most approved elementary treatises, in pointing more precisely to the difference between the real place of the star at the instant, and that shown

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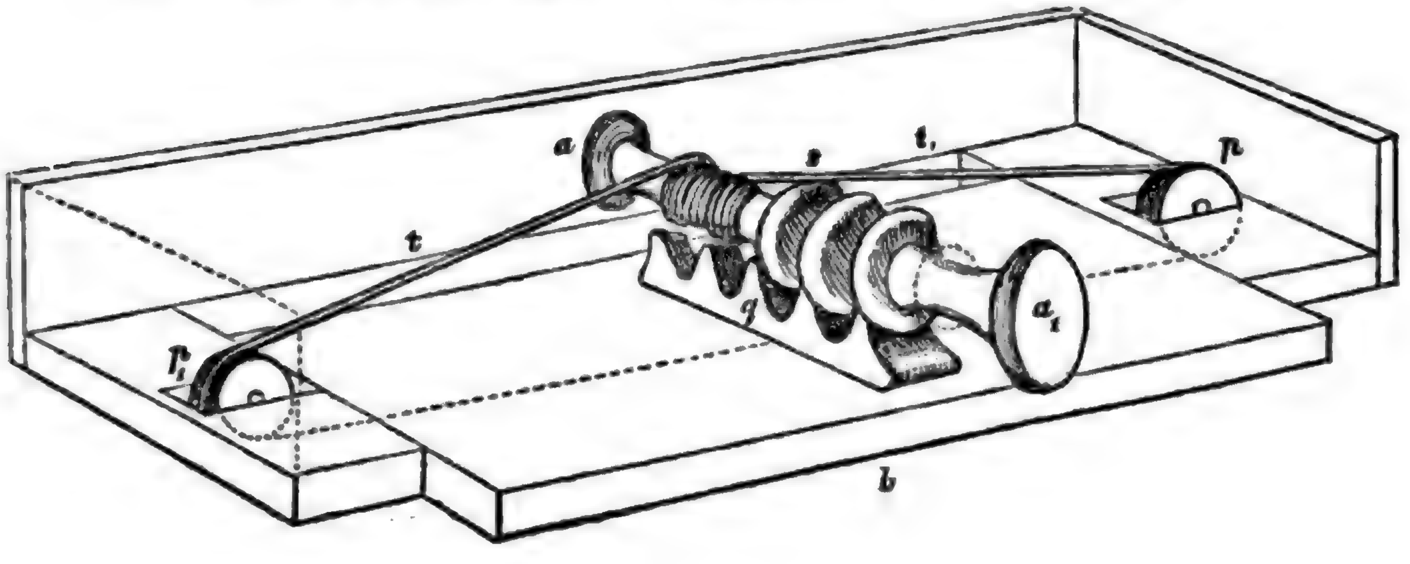
* As it may not be uninteresting to preserve some notice, even of the slightest ideas, of so great a man as Bradley, I subjoin a rough sketch exhibiting the essential part of the little instrument referred to above. The top and side of the box are omitted to give a view of the interior; and it will be seen at a glance that the axis $(a\, a_{,})$, which at once carries the screw (s) (working in the teeth $(q q_{,})$ fixed to b), and on which the two ends of the thread $(t\, t_{,})$ wind in opposite directions, will by the same motion cause that thread to move over the pulleys $(p\, p_{,})$, and give the sliding-board (b) a lateral motion. On its lower side (which is the part to be exhibited to the spectator) is drawn a parallelogram, and its diagonal divided into equal parts, so that the longitudinal motion of a mark upon the thread may be clearly traced upon the diagonal as the parallelogram moves laterally.

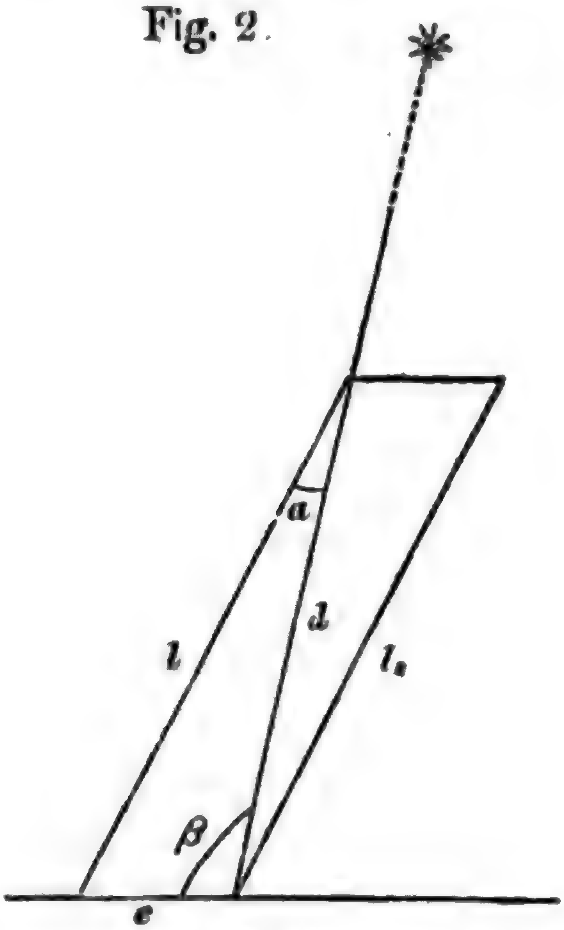


by the direction of the *telescope*, manifestly implying that the light which comes down the *telescope in motion* coincides with that from the *star at rest*, it must be allowed, approach much nearer to a complete explanation, though they do not *explicitly* give it. It is true, several of these writers refer to speculations of another kind, which would maintain the same conclusion with regard to *the naked eye*, by supposing two impacts upon it, respectively proportional to the velocities of light and the earth, the resultant of which coincides with the direction of the ray. But they give such reasoning as entirely separate from the former: and Vince (p. 199, 8vo.) in particular censures those writers who would confound the two. Such reasonings are obviously insufficient as explanations of the aberration, as well from the uncertainty of the analogy assumed between the effects of light and those of mechanical impact, as on other grounds.

But by far the most lucid and satisfactory was the view of the subject taken by Clairaut*. In his memoir he gave perhaps the first complete investigation of the astronomical theory, leading to formulas for the aberration, not only in declination but in right ascension also, Bradley having originally considered only the former†: and it is as the foundation of this investigation that he introduces his well-known illustration, so deservedly adopted by most subsequent writers, and which is peculiarly worthy of consideration in relation to our present object.

Fig. 1.

Fig. 2.



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* *Mém. Acad. Paris*, 1737, p. 205.

† Rigaud's Memoir, p. xxxiii.

"In what direction must a tube be held by a person in rapid motion so as to catch *at its bottom* a drop of rain falling vertically?" (See fig. 1.) The answer to this question, if put into more geometrical language, would be simply the construction of a parallelogram whose diagonal is the vertical direction of the drop, and whose side and base are respectively proportional to the velocities of the drop and the tube, which consequently give its inclination; *and the drop which was at the top of the tube at the beginning of the motion will be at the bottom of it at the end.* It is but to translate this into the language of the actual case, to say that the light which comes down the tube of the telescope in the time in which the earth moves through the proportional space, will by composition of motions in the same time come down the vertical or diagonal, in which the light from the star comes directly, and with which it will thus coincide.

Clairaut, it is true, speaks *explicitly* only of the light from the *star*; but the essential reference to the *tube*, which he expressly points out[*] as representing the direction of the telescope, is surely equivalent to the consideration of the light which comes along the telescope, or more precisely *from its wire, to the eye.* And in Sir J. Herschel's description of Clairaut's method[†] I conceive this is the interpretation implied; and hardly less distinctly, I think, in the same author's account of aberration in his treatise on Light (§ 10), as well as in the more elaborate discussion of Professor Woodhouse[‡]. Still that these writers fail in giving the idea its due prominence and full import, by *explicit* and *formal* statements, and the degree in which this affects the strict character of the investigation, will be best seen by a comparison with the professedly exact explanation as put forth by Prof. Challis.

That explanation, in the form in which it was stated in the course of the discussion referred to, was misunderstood; but it appeared to me that it readily admitted of being put under a slightly different form, by which means the essential principle (divested of all irrelevant and extraneous considerations with which it had been sometimes mixed up) might be at once rendered more perspicuous, and guarded against the possibility of misconception. As the shortest mode of stating it so as to obviate all objections, I would propose the following:—

1. Let l and e (see fig. 2) respectively be proportional to the known velocities of light, and of the earth in its orbit; let d be the diagonal of a parallelogram of which l and e are

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[*] *Mém. Acad. Paris*, 1737, p. 208.

[†] *Astron.* pp. 177, 178.

[‡] Ibid. i. 253.

respectively the side and base; and let α β be respectively the angles which d forms with l and e; so that we have

$$\frac{l}{e}=\frac{\sin\beta_{,}}{\sin\alpha};$$

or if α be very small, $\alpha=\frac{e}{l}\sin\beta$.

Also let l_2 be the side opposite and parallel to l.

2. Then conceiving l l_2 to be the successive positions of a telescope moving parallel to itself along with the earth through e, light coming from the *top of* l to the observer's eye *at the bottom*, (or, more precisely, from some determinate point as the cross-wire,) *relatively to the tube and the observer*, will in the same time come down the diagonal d *relatively to space*, by composition of motions.

3. If at the same time light from a star come directly in the direction d, this will coincide with the former; it will pass down the telescope as it moves, and the two objects will be referred by the eye to the same direction, and will appear to coincide though really separated by the angle α, which is called the aberration.

4. Since the same inferences would apply also if another parallelogram be similarly constructed on the same diagonal, in like manner it will be seen that *two* objects moving with the observer would be referred to the same direction (in which they really are), though seen by light which really moves in the diagonal by composition of motions.

In this sense *the aberration of terrestrial objects* is spoken of; though it cannot be determined by terrestrial observations.

5. In this investigation it is assumed that the light from the star comes in its original rectilinear course, and with the same velocity, equally whether the observer be at rest or in motion. It is also assumed that the velocity of light is that given by independent observations, and comparable in a known ratio with that of the earth in its orbit.

6. Thus, referring *essentially* to the light coming with the same velocity from the wire to the eye, it is shown that the aberration is explained by a *vera causa*; and from the *extreme accuracy*[*] with which the *amount* of aberration thus calculated agrees with that directly observed, it follows that the aberration is *completely* accounted for, and that there is *no residual phænomenon*. If there were any, then indeed recourse

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[*] The mean of the direct observations, including the latest, given in Capt. Smyth's Cycle (ii. 401), is $\alpha = 20''\cdot43$. The result of calculation, taking the velocity of light from observation of Jupiter's satellites at 8·2 min. in traversing the radius of the earth's orbit, gives $\alpha = 20''\cdot2$.

must be had to other principles on which to find a complete explanation.

7. On this view the assumption of the rectilinear course is justified; and the explanation being complete, it is wholly superfluous to refer to the undulatory or any other theory of light, for an explanation of the phænomenon.

Upon this review of the principles of the explanation, it must, I think, be fully admitted that its most essential and characteristic feature is prominently brought out by Prof. Challis with far more distinct and systematic precision than by any of his predecessors in the inquiry: and from what has been remarked on their explanations, it will be judged to what extent we can consider them to have really implied the more precise principle, though they confessedly did not explicitly or systematically enunciate it. I may also here add, that perhaps it would render the matter still plainer to many apprehensions, if, instead of the telescope and its wire, we were to substitute the idea of a *lamp* elevated so as to appear against the sky to an observer below, and in coincidence with a star. Here then would be distinctly *two rays*; one from the star at rest, another from the lamp moving with the observer, apparently coinciding but really differing in direction.

There is perhaps one point on which it may be necessary to add a remark. In the above explanation it appears that the course of a ray propagated directly (as from the star) coincides with that *given* to another (propagated directly in a different course, as from the wire to the eye) by the motion of the object from which it originates along with that to which it comes; and it is hence inferred, that the two objects will be referred to the same direction as if the light from both were *naturally* propagated in the same direction. In other words, the course by which the star's light actually comes to the eye, *relatively to the observer in motion*, can be no other than the axis of the telescope, in which direction, the eye moving along with the light from the wire, at every instant receives an impression from it, along with one from the star; the one as truly comes down the tube as the other. But then, it may be asked, when we consider the very different *modus operandi* by which they each respectively take this direction, can we be sure that the effect ought to be the same?

This question may, I conceive, be answered by the consideration (quite independent of any theory of light), that as the light from the star comes down the tube only by virtue of the tube's motion, the whole effect of the ray still continues to be in its own direction; but the actual result must be estimated by resolving it, only that part of it which is in the direction of the tube being really effective on the eye.

But this question may appear to some perhaps superfluous; the above explanation, it may be said, only requires to have it shown that the two rays do actually assume *the same direction in space by whatever means*; and this is all that is necessary to support the inference that the two objects must be referred to the same direction. Those who agree in this view will then merely consider any further discussion of this particular question as irrelevant to the complete explanation of the fact, though they may admit it as a question of curiosity.

Others however may not be satisfied without a further explanation; and whether that just offered be considered fully satisfactory or not, I confess it appears to me that some considerations of the kind are necessary for showing that the star and wire ought to be referred to the *same* direction.

The question raised as to *what* that direction is, also seems to be answered at the same time, if the foregoing view be admitted.

The impression produced is clearly that which results from the concurrence of two impressions, the one of the ray from the wire, the other of that from the star; the one simple, the other arising from its motion of propagation together with a motion of translation in the eye, but whose effect coincides with the former: and a series of such compound impressions come in succession continuously in *the direction of the axis* of the tube to the eye. This circumstance appears to me decisive of the question, that both objects must be seen in the direction of the axis.

The explanation however was held to be independent of this question. It was said it would be equally valid should it be contended (as, for example, on any physiological or other grounds) that the eye received the impression *not* in the direction of the axis but in some other, as for instance that of the diagonal.

And when such a *doubt* was started, and when it was contended that the above explanation *was independent* of any such question, or that the introduction of it was superfluous, a difficulty was raised in many minds; it was construed as if it tended to overset all the received notions as to the direction of vision, and to substitute something of an abstruse and theoretical kind; and the question raised, whether the star or the wire is seen displaced, led to serious objection, especially as coupled with the use of the term aberration applied to terrestrial objects.

But on the above view of the matter, the direction of vision does not appear an irrelevant consideration; and the question seems clearly answered. Thus I conceive we arrive at a precise statement of the case, and at the same time cut off a considerable source of cavil. The point determined by the position of the telescope and its wire is that to which the eye refers the *star*, which is consequently seen *out of its true place* by the amount of aberration, while the *wire* is seen in its true direction. The precise explanation then may be stated in all its essential particulars, divested of the mystery and obscurity which was sometimes found in it, and rendered at once simple, exact, and relieved of all perplexity from appearing at variance with received notions as to the direction of vision.

But a material part of the question before us relates to the investigation of the aberration in connexion with *theories of the nature of light.* A considerable portion of the existing controversy (we may observe) has arisen not only on the question of the details of such theoretical analysis, but also on the more general logical question of the grounds on which any such investigation should be undertaken at all.

As to the actual application of theory to the explanation of the phænomenon, Euler long since, adopting for simplicity the emission theory, admitted that some difference in the view taken of the case would result according to the hypothesis of light adopted[*]. He more particularly notices (§ 7) the objection which might arise on the emission theory, that light would partake in the velocity of the body from which it was projected, which would not be the case on the undulatory. This is further discussed (§ 24, 25, 28).

In the present state of the question it seems on all hands to be admitted that the point at issue is a very simple one. It in fact reduces itself ultimately to this: whatever theory we adopt, will it account for the uninterrupted maintenance of the law of the rectilinear propagation of light from a star to the earth in motion, at *all* parts of its course, but especially near the earth, with the same velocity?

The emission theory obviously fulfills this condition; as does also the undulatory, if the æther be at rest. But one of the greatest difficulties attending that theory, as formerly delivered, was the conception of the æther pervading all bodies and the interior of the earth yet remaining at rest, a passage being afforded to it through the interstices of the densest matter, in the words of Dr. Young[†], "as freely as the wind passes through a grove of trees," while the earth was in rapid mo-

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[*] *Comm. Petrop.* xi. 150.

[†] Phil. Trans., 1802, Experiments and Calculations on Physical Optics, § iv. It is remarkable that the author says he was led to this idea chiefly from considering the aberration of the stars.

tion. This and other attendant objections, have at the present day led to the general admission of the æther being set in motion by the passage of the earth through it.

On this hypothesis the conclusion does not *obviously* or directly follow, but it may be shown to do so by an analytical investigation. That the theoretical investigations on the undulatory hypothesis, supposing the æther in motion, given both by Mr. Stokes and Prof. Challis, are of the highest value and importance, as evincing the capability of the undulatory theory to surmount the difficulty here presented, must be on all hands allowed, whatever may be thought of the points on which the two distinguished mathematicians differ.

The principle of Mr. Stokes's investigation is, in a word, the analogy between sound and light; and the object is to show that the luminiferous waves are propagated through *æther in motion*, just as those of air constituting sound are propagated through a mass of air in motion, and that in both cases alike the impression reaches the organ just in the same manner as if the medium were at rest. Hence some limitations are introduced in the analysis; the very natural supposition that the æther near the earth acquires the same velocity as the earth, and thus remains, relatively to it, at rest, involving the condition that a certain expression $(u\,dx + v\,dy + w\,dz)$ is an exact differential.

Prof. Challis, on the other hand, discards these restrictions, allowing this expression to be an exact differential or not, and the motion of the æther any whatever: thus giving a greater degree of generality to the investigation.

But on the question whether we can legitimately infer that the motion acquired by the æther must be precisely *equal* to that of the earth, and whether, consequently, the more general assumption be necessary for a completely satisfactory conclusion,—or, on the other hand, whether the restrictions in the analysis are such as, without impairing its full applicability to the case, are more simple and natural, as well as on other minor points, I do not here enter: the main question with which we are at present concerned, is that which refers to the *necessity* for such theoretical investigation at all, as regards the complete explanation of the phænomenon: on which point also the two eminent disputants are at variance.

If what has been before advanced be assented to, I conceive while we deny the *necessity* for *such* an investigation as regards the explanation of the *facts*, we must at least fully admit its importance as regards the credit of the *theory*.

We may illustrate the subject by taking a parallel case: granting the laws of reflexion and refraction, and the unequal refrangibility of light, the *rainbow* is *perfectly* explained without reference to the undulatory or any other theory of the nature of light. Yet it is material to the truth of that theory that it should explain the dispersion; and if it did so, as it confessedly explains the laws of reflexion and refraction, it would explain the rainbow, which is a mere consequence from them.

In other words, the aberration is not an independent *property* of light, like refraction or polarization, which require a theory to explain them, but the *result* (if the above principles be admitted) of simpler known causes. Any theory which explains these causes explains the aberration. But it is a matter of importance for the credit of any theory, that it should be able to explain them. Thus, more precisely, the question is not whether any theory explains the aberration, but whether it accounts for the facts from which the aberration is a consequence.

On the other hand, there are some, who, not entirely falling in with the above explanation, may view the whole matter under a different aspect, and may ask, before coming to any explanations, does not the naked fact of the aberration stand out as a *primâ facie* exception to the strict universality of the law of rectilinear propagation? and they may argue, as we are quite ignorant of the cause of the rectilinear motion of light, or the *modus operandi* which produces it, we cannot assume that as light approaches the earth in motion, it may not in some way be influenced, or a deviation caused: and it is only so far as we assume *some* theory of the nature of light that we can form any conception which may guide us to a conclusion on this point: and assuredly a theoretical investigation would in this point of view be not only desirable for the theory, but necessary for the explanation of the fact.

Those then who contend for the *necessity* for a *theoretical* investigation to explain the aberration, appear implicitly to *assume* that we cannot infer the absolute *universality* of the law of rectilinear propagation of light *at all parts of its course* to the earth in motion; though the *result* of the investigation on the undulatory hypothesis, with the æther in motion, is to establish it.

The question then is, whether this assumption be really called for in the nature of the reasoning: Is our ignorance of the nature and propagation of light so entire, that, for anything we know to the contrary, the mere circumstance of the rapid motion of the observer may in some unknown way act upon the ray of light as it approaches him and divert its course

> .... "ten thousand leagues away
> Into the devious air"?

or is such an idea so wholly improbable and unsupported by analogy that all consideration of it may be discarded, and the question rather put, What should lead us to imagine such an exception? or the *onus probandi* be thrown on the side of those who would assert it?

On this question I would only observe that every inductive law is essentially open to exceptions which may call for some modification of it, but any such exceptions must be substantiated on the most unequivocal evidence, before they can be admitted as influencing the universality of the law. If this be accepted as a fair rule of inductive logic, it will follow that we need not trouble ourselves with imagined or possible exceptions to the general law of rectilinear propagation, which may conceivably be occasioned by causes wholly unknown. If indeed we had any reasonable ground for suspecting that any *known* cause, such as *e. g.* terrestrial magnetism or electricity or heat, might act upon light as it approached the earth, then it would be a perfectly reasonable demand to see whether those causes produced any deviation, before we assumed the perfectly rectilinear course of the rays.

But without insisting on such considerations as those just referred to, it will I think be agreed on all hands, that, admitting the aberration to be wholly accounted for by the known facts relative to the motion of light and the earth, still the explanation cannot be called a perfect or philosophical one until those facts themselves are completely explained by a theory connecting them, and consequently the aberration, with the whole assemblage of laws and phænomena of light: and therefore that an appeal to the undulatory (or whatever is the best-established) theory is in this sense a *necessary* part of the investigation. Yet we should bear in mind that it is not more peculiarly *essential* in this case than in many others; such as *e. g.* the case of the rainbow before adverted to.

As another parallel case we might refer to the polarized rings. Granting the *facts* of polarization, and of the interferences of polarized light, the formation of the rings is *completely* explained without reference to any theory of the nature of light. It is a question for the credit of any theory, whether it will account for the laws of polarization and the interferences of polarized light: if it does so, it explains the rings; and the undulatory theory alone has been shown to do so. At the same time had we no theory explaining and connecting the laws of polarization and interference, we should certainly feel our views of the whole subject very unsatisfactory.

In the case of aberration, the only difference perhaps is that the facts which explain the aberration are so much more simple and familiar that we feel satisfied almost without any theory of them at all.

Or again:—the retardation owing to light entering a denser medium, as the humours of the eye, and the question of its influence on the aberration, were discussed at an early period by Melvill (1753) and Wilson (1782) on the theory of emission and attraction; who for a long time did not perceive the compensation effected by the greater refraction*: this is in itself independent of theory. Yet the theoretical investigation was not unimportant.

Boscovich† also had maintained that an object viewed through a refracting medium in motion with the observer, will change its apparent place; and both he and Prof. Wilson had proposed to try it by means of a telescope filled with water. But Mr. Robison‡, besides pointing out the impracticability of this project, showed on theoretical grounds an oversight in the reasoning (though he had himself for a time embraced the idea), and concluded by establishing, on the emission theory, this proposition:—"If a ray of light moving in any direction, and with any velocity, meet with the surface of a refracting medium while it is in motion, its final relative motion will be the same as if the medium had been at rest, and the light had approached it with the same initial relative motion." (P. 106.) And questions somewhat akin to these have occupied the attention of more recent inquirers in connexion with the principles of undulations; and especially the conclusion that the laws of reflexion and refraction in general are uninfluenced by the motion of the æther along with the earth, has formed the subject of one portion of Mr. Stokes's investigations. And that such investigations are requisite for a complete and satisfactory theoretical view of the case, must on all hands be admitted.

Or to take a different illustration;—when the composition of forces and the experimental law of falling bodies were known, the explanation of the pendulum was *complete*, without any reference to the theory of universal gravitation. But it was incumbent on that theory to explain the law of falling bodies: and when it did so, it explained the pendulum. No one however doubts that it is highly valuable and satisfactory thus to connect the pendulum with universal gravitation. Here, indeed, the case is far stronger than in those of light, since we

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* See Rigaud's Memoir of Bradley, p. xxxiii. and 483.

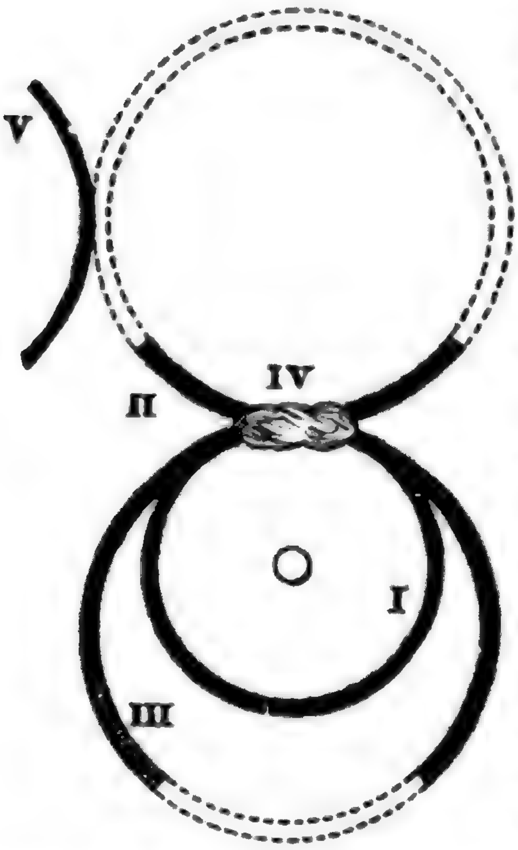
† Miscell. Works, vols. ii. and iv. 1785.

‡ Ed. Trans. ii. 83, 1788.

can here appeal to a *vera causa* of the highest and most comprehensive kind, whilst, however exactly the undulatory theory may explain any or all phænomena, it still does not carry us up to any *vera causa*, the existence of the æther having no independent proof, and being thus altogether hypothetical, as there is no more proof that the medium which resists Encke's comet is the same with the luminiferous æther, than there would be that atmospheric air is the same with inflammable gas.

LXII. *Remarkable Solar Halos seen on the 19th of October 1846.* *By* Edward Joseph Lowe[*].

On Monday, October 19, 1846, remarkable solar halos were observed at Highfield House, Nottinghamshire. The morning was fine with linear-cirri at a great altitude and cumuli floating beneath. The upper current drove the clouds from south, the lower from south-west.



At 22^h the phænomenon might be said to have commenced, for the sky had assumed a hazy appearance, and a halo (I.) of 22° 30′ radius had become visible: this was of a pale straw colour and soon became bright. The sky within the halo was considerably darker than that without.

22^h 10^m. An inverted arc of another halo (II.), of (apparently) 33° radius, also of a pale straw colour, formed at the vertex of the other halo, appeared.

At 22^h 15^m the halo (II.) had assumed a brilliant yellow colour, and the portion (IV.) which joined the halo (I.) a flame-like appearance, having widened considerably, being now about 3° in width and 6° in length. This brilliant parhelion, as I shall call it, became in a few minutes intense in brilliancy, vieing with the sun, for that luminary had been deprived of his usual brilliancy by haze, and had its rays carried out considerably. No change took place until 23^h 30^m, when a third

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[*] Communicated by the Author.

halo (III.), of 33° radius, and also of a straw colour, was visible. This circle touched the halos (1 and 2 at IV.), and its centre was 11° below the true sun. The upper half of the halo (III.) was bright; but the lower portion, which is marked by the dotted lines, was not visible, owing to cumuli being very abundant on the south horizon. The lower portion of the halo (I.) was also at this time nearly invisible, but the parhelion (IV.) was even more brilliant than before.

This appearance continued until $0^h\ 10^m$, then the halo (III.), together with the lower portion of the halo (I.), vanished. The parhelion (IV.), with the halo (II.) and upper half of the halo (I.), was still very bright.

At $0^h\ 40^m$ the whole of the halo (I.) was again very brilliant, as also was the halo (II.) and parhelion (IV.). The upper half of the halo (III.) had again become visible, and a new feature to the phænomenon occurred, to describe which it will be necessary to continue the arc of a halo (II.) so as to describe a circle, which we will do with dotted lines, to show that that portion was imaginary.

In a horizontal line with the centre of the circle (II.) on the north-east side, and at a distance of 34° from the centre, was formed an arc of another circle (V.), inverted with regard to the halo (II.), of (apparently) 33° radius. This was of a pale silvery hue. The arc of a halo (V.) was formed touching the imaginary part of the halo (II.) on a horizontal level with the centre of the halo (II.). This was very apparent, though not very brilliant.

At $0^h\ 45^m$ halo (V.) disappeared, together with halo (III.) and the lower part of the halo (I.).

$1^h\ 0^m$. All the circles had disappeared, and the sky was scattered over with cumuli.

$1^h\ 5^m$. The halo (II.) and the upper portion of the halo (I.) and parhelion (IV.) were again visible and brilliant.

$1^h\ 7^m$. All had again disappeared.

$1^h\ 11^m$. The halo (I.) again visible, but the parhelion (IV.) was not formed, although the halo was visible where it was before formed.

$1^h\ 12^m$. Halo (II.) faintly visible.

$1^h\ 13^m$. All had again disappeared, and cumuli had become very abundant.

$1^h\ 30^m$. The halo (II.) and upper portion of the halo (I.) together with the parhelion (IV.) were once more formed. The parhelion was very brilliant. Much linear cirri was visible, with cumuli beneath moving rather rapidly. A cumulostratus also was moving on the south-east horizon and another on the east horizon.

$1^h\ 45^m$. The phænomenon had finally disappeared, and the haze in which these halos were formed had passed away and the sun again shone brightly.

From 2^h till 3^h a rain-cloud passed along the south-east horizon, but it never rose higher than 15°.

At 4^h much cumuli passed over together with some scud, and a few drops of rain fell. In a few minutes the sky was clear with the exception of few cumuli. Starlight night until 11^h, then overcast. A near resemblance to this phænomenon was noticed at Derby on the 16th of June 1843. (See page 32 of Lowe's Atmospheric Phænomena.)

The 18th had been a thorough rainy day, and $1\cdot\frac{300}{1000}$ inch fell; the wind was from the south-east, with the barometer stationary at 29·296 inches[*] until 8^h when it began to rise; at 9^h it had reached 29·307 inches; it continued to rise all night, and at 21^h of the 19th was 29·426 inches. At 0^h it had fallen to 29·424 inches; it remained then stationary until 8^h. At 9^h it had again fallen to 29·401 inches.

The wind on the 19th was south-west all day, and the minimum temperature (seven feet from the ground, N.N.E. aspect) was 48°; on grass 41°, the maximum temperature (seven feet N.N.E.) 58°·5. The mean temperature during the time the phænomenon was visible was 57°·5, and the hygrometer 53°·5. The halos were all of the same width, viz. 1°.

This phænomenon was formed in a haze decreasing in density: fine weather therefore followed this appearance.

LXIII. *A Simple Demonstration of a remarkable Proposition relative to Polyhedrons*[†].

THE proposition expresses the relation between the number of faces, the number of corners, and the number of edges of a polyhedron.

It was first demonstrated by Euler in the Transactions of the Petersburg Society for the year 1758. Legendre has given a demonstration of it in his Elements of Geometry (English translation, p. 180). His demonstration is quite satisfactory, but it is founded on the rule for finding the area of a spherical polygon, which is not very easily reached; whereas the demonstration which is now submitted to the reader, rests entirely on the definition of a polyhedron, and can be followed with the greatest ease.

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[*] The barometric observations are corrected and reduced to 32°.

[†] Communicated by the Author.

Proposition.

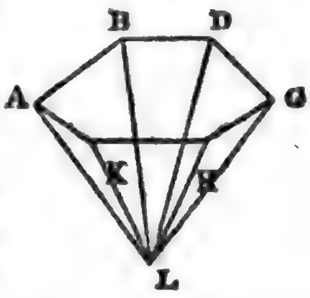
In any polyhedron, the number of corners, together with the number of faces, exceeds the number of edges by two.

Or, symbolically, let

$c =$ the number of corners,
$f =$ the number of faces,
and $e =$ the number of edges,
then $c+f=e+2$.

Demonstration.

1. We shall demonstrate that when you increase c by 1, then the increase upon $c+f$ is equal to the increase upon e.



Let A B D G H K be a face in the original polyhedron, and L the additional corner, which, by drawing L A, L B, &c., forms the new polyhedron, in which c is greater by 1.

Suppose that L is not in the plane produced of any of the faces that meet A D in the edges A B, B D, &c. Let $n =$ the number of sides in the polygon A D.

Then it is evident that n faces, with their common vertex L, are added, while A D ceases to be a face, or 1 face is subtracted, so that the real increase in the number of faces or in f is $=n-1$, and since there is the additional corner L, the total increase on $c+f$ is $=1+n-1=n$.

Again, all the lines L A, L B, &c. form new edges, so that the total increase in the number of edges or upon e is $= n$.

Hence the increase upon $c+f$ is $=$ the increase upon e.

Suppose now that L is in the plane of the face produced that intersects A D in K H.

Then L K H ceases to be an additional face, and is only part of a face of the original polyhedron; hence the number of faces added, instead of n, is only $n-1$, while, as before, A D ceases to be a face, or 1 face is subtracted, so that the real increase in the number of faces is $=n-2$. Hence the increase on $c+f$ is $=1+n-2=n-1$.

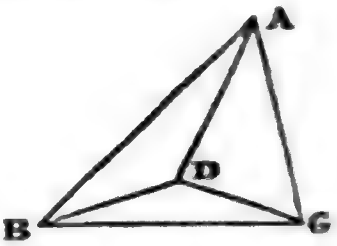
Again, the number of edges added is, as before, $= n$; but K H, which was an edge in the original polyhedron, ceases to be an edge in the new polyhedron, or 1 edge is subtracted, hence the actual increase on e is $=n-1$.

Hence, as before, the increase on $c+f$ is $=$ the increase on e.

2. It follows that, if the proposition be true, or if $c+f=e+2$ in the original polyhedron, it must also be true in the new polyhedron.

3. The proposition evidently holds when the solid is a tetrahedron, or has 4 corners, the smallest possible number.

Thus let A B D G be the solid.



By supposition, $c=4$,
it is evident that $f=4$,
and $e=6$.
Now $4+4=8=6+2$,
or $c+f=e+2$.

4. The proposition being true when $c=4$, it must be true, as above demonstrated, when $c=5$.

In the same manner, the proposition being true for 5 corners, must be true for 6.

Being true for 6, it must be true for 7, and so on for any number of corners.

ST. ANDREWS.

LXIV. *On the Constitution of Aqueous Solutions of Acids and Alkalies.* *By* JOHN JOSEPH GRIFFIN, Esq.

[Continued from p. 310.]

TABLE X.—*Anhydrous Potash.*

KO=589·916 grs. Temperature 62° F.

Specific gravity of the solution.	Atoms of KO in 1000 septems.	Atoms of water to 1 KO.	1 KO		Septems of solution containing 1 KO.
			Occupies: septems.	Condenses: septems.	
2·656 K.	31·516	0·0	31·73	0·0	31·730
2·1 D.	20·928	1·0	31·73	0·0	47·782
1·5607	8·10	6·75	15·05	16·68	123·46
1·5594	8·00	6·89	14·35	17·38	125·00
1·4909	7·00	8·01	14·14	17·59	142·86
1·4374	6·00	9·66	11·37	20·36	166·67
1·371	5·00	11·82	10·07	21·66	200·00
1·3161	4·00	15·23	5·25	26·48	250·00
1·2508	3·0835	20·00	2·94	28·79	324·31
1·2446	3·00	20·15	2·74	28·99	333·33
1·1815	2·0862	30·00	− 2·73	34·46	479·33
1·176	2·00	31·35	− 3·73	35·46	500·00
1·0929	1·00	62·77	− 8·63	40·36	1000·0
1·0471	·50	125·1	− 9·92	41·65	2000·0
1·026	·25	250·1	−19·72	51·45	4000·0
1·0114	·10	624·2	−30·72	62·45	10000·
1·0116	·10	624·2	−31·73	63·46	10000·
1·0078	·06	1040·	−45·49	77·22	16667·
1·007	·05	1248·	−55·72	87·45	20000·
1·00486	·04	1556·	−37·15	68·88	25000·
1·00403	·03	2077·	−50·01	81·74	33333·
1·00293	·02	3155·	−62·16	93·89	50000·
1·00146	·01	6227·	−61·44	93·17	100000·
2	3	4	5	6	7

AUTHORITIES.—D = Dalton. The substance is solid caustic potash. K = Karsten. The substance is anhydrous potash. The solutions are all calculated from my experiments.

Observations on the Table of Anhydrous Potash.

The strongest solution of potash that I could prepare at the temperature of 62° F. had the sp. gr. of 1·5607. Its chemical strength was 810°. This was determined by neutralization with pure sulphuric acid of 100°. Its formula is $KO, 6\frac{3}{4}H^2O$. According to Karsten the specific gravity of anhydrous potash is *about* 2·656, which gives 31·73 septems for the volume of the test atom; but Dalton's specific gravity of the hydrate of potash gives the very same number for the volume of anhydrous potash. As it is improbable that potash and water would combine to form the first hydrate without condensation, I conclude that Karsten has merely calculated the specific gravity of anhydrous potash from the specific gravity of the hydrate.

The condensation effected by potash is considerable, nearly approaching to that effected by sulphuric acid. In a solution of 1° the condensation per atom is 93 septems, being three times the measure of the anhydrous potash which causes it.

The fraction which + 1 indicates the mean specific gravity of 100° of potash is (1·656÷31·516=) ·052545.

TABLE XI. Showing the increase of Specific Gravity occasioned in Solutions of KO by condensation.

Atoms of KO.	Increase in spec. grav.	Atoms of KO.	Increase in spec. grav.	Atoms of KO.	Increase in spec. grav.
8·10	·135086	6·00	·122130	3·00	·086965
8·00	·139040	5·00	·108275	2·00	·070910
7·00	·123085	4·00	·106520	1·00	·040355

The greatest chemical strength, the greatest density, and the greatest condensation in solutions of potash, all coincide. In this particular potash agrees with soda, and with the two fixed alkaline carbonates, but disagrees with the four acids. The above table shows indeed rather more condensation in a solution of 800° than in a solution of 810°, but the difference falls within the limits of errors of experiment. It is difficult to determine the specific gravity of a solution of potash saturated at 62° F., because in cooling down a hot saturated solution an abundant crystallization occurs at about 64° F., which raises the temperature to above 70° F. If the solution is not well-shaken, an excess of potash remains in it at 62° F. If the temperature is allowed to descend a little lower than 62° F., too much potash precipitates. The saturated solutions of caustic soda, carbonate of soda, and carbonate of potash possess similar properties. All these substances crystallize with facility when in saturated solutions at 62° F. It is therefore very difficult to determine the point of saturation.

TABLE XII.—*Carbonate of Potash.*

$KO,CO^2 = 886{\cdot}354$ grs. Temperature 62° F.

Specific gravity of the solution.	Atoms of KO,CO² in 1000 septems.	Atoms of water to 1 KO,CO².	1 KO,CO²		Septems of solution containing 1 KO,CO².
			Occupies: septems.	Condenses: septems.	
2·2643	18·295	0·0	54·66	0·0	54·660
1·5739	6·65	7·03	37·46	17·20	150·48
1·5248	6·00	8·	36·30	18·36	166·67
1·459	5·1292	10·	34·28	20·38	194·96
1·4489	5·00	10·33	33·99	20·67	200·00
1·3681	4·00	13·62	31·02	23·64	250·00
1·2871	3·00	19·	28·07	26·59	333·33
1·2753	2·865	20·	27·67	26·99	349·04
1·2426	2·50	23·23	26·72	27·94	400·00
1·2011	2·00	29·67	23·21	31·45	500·00
1·1995	1·98	30·	23·00	31·66	505·06
1·1047	1·00	61·05	19·06	35·60	1000·0
1·05509	·50	123·4	13·59	41·07	2000·0
1·05517	·50	123·4	13·42	41·24	2000·0
1·05529	·50	123·6	13·19	41·47	2000·0
1·04466	·40	154·8	12·12	42·54	2500·0
1·0284	·25	248·3	10·17	44·49	4000·0
1·02829	·25	248·2	10·62	44·04	4000·0
1·02203	·20	310·3	13·62	41·04	5000·0
1·023	·20	310·4	8·77	45·89	5000·0
1·02386	·20	310·8	4·48	50·18	5000·0
1·01251	·10	622·3	− 1·38	56·04	10000·
1·01257	·10	622·4	− 2·52	57·18	10000·
1·01271	·10	622·5	− 3·38	58·04	10000·
1·00749	·06	1037·	− 1·00	55·66	16667·
1·00631	·05	1245·	− 2·57	57·23	20000·
1·005	·04	1556·	− 1·24	55·90	25000·
1·00417	·03	2075·	−15·28	69·94	33333·
1·00297	·02	3113·	−24·81	79·47	50000·
1·00143	·01	6224·	−19·09	73·75	100000·
1·00146	·01	6224·	−21·95	76·61	100000·
1·00151	·01	6224·	−27·66	82·32	100000·
2	3	4	5	6	7

AUTHORITIES.—The specific gravity of the anhydrous salt is quoted from Karsten. The rest of this Table is from my own experiments.

Observations on the Table of Carbonate of Potash.

The strongest solution of carbonate of potash that can be formed at 62° F. has a specific gravity of 1·5739 and a chemical strength of 665°. Its formula is $KO,CO^2 + 7H^2O$. The greatest amount of condensation occurs in this first solution, where the measure of the test atom of carbonate of potash decreases at once from 54·66 septems to 37·46 septems, effecting a condensation of 17·20 septems. In a solution of

° the atomic measure is diminished to $-27·66$ septems, the ondensation effected by 1 test atom being 82·32 septems.

According to Messrs. Playfair and Joule (Phil. Magazine, ol. xxviii. p. 506), the atomic measure of carbonate of potsh in solution is equal to that of water [namely, 16·07 eptems]. This is correct when the strength of the solution s about 70°, but not under any other circumstances; since t 400° the measure is twice as much, at 10° the measure is othing, at 3° it is minus one volume, and at 1° it is minus wo volumes.

The fraction which + 1 indicates the mean specific gravity f 100° of carbonate of potash is $(1·2643 \div 18·295 =)$ ·069106.

Table XIII.—*Anhydrous Soda.*

$NaO = 390·897$ grs. Temperature 62° F.

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of NaO in 1000 septems.	Atoms of water to 1 NaO.	1 NaO.		Septems of solution containing 1 NaO.
				Occupies: septems.	Condenses: septems.	
K	2·805	50·231	0·0	19·91	0 0	19·908
D	2·0	27·812	1·0	19·89	0·02	35·955
G	1·4	7·00	8·972	− 1·30	21·21	142·86
CT	1·36	6·2809	10·	− 1·47	21·38	159·21
CT	1·3508	6·00	10·53	− 2·62	22·53	166·67
CT	1·3076	5·00	12·8	− 5·68	25·59	200·00
CT	1·2655	4·00	16·21	−10·53	30·44	250·00
CT	1·2335	3·27	20·	−15·56	35·47	305·81
CT	1·2153	3·00	21·73	−15·92	35·83	333·33
CT	1·1546	2·1465	30·	−16·18	36·09	465·88
CT	1·1443	2·00	32·13	−16·31	36·22	500·00
G	1·0727	1·00	63·28	−16·86	36·77	1000·0
	1·0554	·70	90·06	−18·64	38·55	1428·6
	1·0478	·60	105·2	−23·82	43·73	1666·7
	1·0482	·60	105·2	−24·54	44 45	1666·7
	1·039	·50	125·8	−22·16	42·07	2000·0
	1·0412	·50	126·0	−25·56	45·47	2000·0
	1·0207	·25	250·6	−26·96	46·87	4000·0
	1·009	·10	624·4	−34·16	54·07	10000·
	1·0047	·06	1039·	−22·73	42·64	16667·
	1·0052	·06	1039·	−31·30	51·21	16667·
	1·004	·05	1246·	−24·16	44·07	20000·
	1·0043	·05	1247·	−31·02	50·93	20000·
	1·0033	·04	1557·	−26·30	46·21	25000·
	1·0022	·03	2075·	−15·59	35·50	33333·
	1·0018	·02	3114·	−37·02	56·93	50000·
	1·00203	·02	3114·	−45·58	65·49	50000·
	1·0009	·01	6226·	−29·90	49·81	100000·
	1·00106	·01	6226·	−49·87	69·78	100000·
1	2	3	4	5	6	7

Authorities.—D = Dalton. G = Griffin. K = Karsten. CT = calculated after the nearest numbers in Tünnerman's table.

Observations on the Table of Anhydrous Soda.

The strongest solution of anhydrous soda, prepared at 62° F., has a chemical strength of 700° and a density of 1·4. Its formula is $NaO + 9H^2O$. The greatest condensation occurs in this solution, where the test atom of soda effects at once a condensation exceeding its own volume. The atomic measure of the uncombined soda is 19·91 septems, and that of the soda in the strongest solution is −1·30 septem. Hence a decigallon of this solution contains seven times 391 grs. of anhydrous soda, in addition to more water than would fill the vessel if no soda was present. It is impossible to obtain any solution of soda at 62° F., a decigallon of which does not contain more than a decigallon of water. I have already noticed the fact, that 1000 septems of water, when saturated with anhydrous sulphuric acid, produce 5227 septems of solution. By way of contrast, I may state that 1000 septems of water, when saturated with 140 septems of anhydrous soda, produce only 991 septems of solution; and yet the condensation effected by 1 test atom of soda in a solution of this kind, is only one-third part of that which it effects in a solution of 1°. At that stage of dilution the condensation effected by 140 septems of anhydrous soda amounts to about 400 septems.

The fraction which +1 indicates the mean specific gravity of 100° of soda is (1·805÷50·231=) ·035934.

The specific gravity of the saturated solution of 700° or specific gravity 1·4 may be explained thus:—

1·000000=specific gravity of water.
·251538=·035934 × 7 atoms, for mean specific gravity.
·148470=21·21 × 7 atoms, for effects of condensation.

1·400008=real specific gravity of solution of 700°.

In a solution of 300° the density caused by condensation *equals* that which is produced by the replacement of water by the anhydrous alkali, and in weaker solutions the density induced by condensation is *much greater* than that occasioned by the replacement of water by alkali, although the uncombined alkali possesses nearly three times the density of water.

When a test atom of caustic soda dissolves in a minimum of water (say 9 test atoms), the condensation it effects is 21 septems; but when it dissolves in a maximum of water (say 6226 test atoms), the condensation effected is three times as much. Those chemists who consider that hydrate of soda dissolves in water without combining with it chemically, will, it appears to me, not easily account for this condensation.

TABLE XIV.—*Carbonate of Soda.*

NaO, CO^2 = 667·335 grs. Temperature 60° F.

Specific gravity of the solution.	Atoms of NaO, CO_2 in 1000 septems.	Atoms of water to 1 NaO,CO^2.	1 NaO, CO^2		Septems of solution containing 1 NaO, CO_2.
			Occupies: septems.	Condenses: septems.	
2·4659	25·866	0·0	38·66	0·0	38·661
1·51	6·7446	8·0	19·72	18·94	148·27
1·423	5·5582	10·0	19·23	19·43	179·91
1·166	1·85	33·29	5·60	33·06	540·54
1·15643	1·80	34·05	8·43	30·23	555·55
1·156	1·79	34·25	8·18	30·48	558·66
1·13971	1·60	38·39	8·01	30·65	625·00
1·141	1·60	38·44	7·20	31·46	625·00
1·13571	1·50	41·18	4·86	33·80	666·67
1·12229	1·40	43·95	7·98	30·68	714·29
1·10971	1·20	51·61	3·90	34·76	833·33
1·11029	1·20	51·64	3·40	35·26	833·33
1·0939	1·00	62·14	1·45	37·21	1000·0
1·094	1·00	62·15	1·33	37·33	1000·0
1·07729	·80	77·86	− 1·27	39·93	1250·0
1·05806	·60	103·8	− 1·43	40·09	1666·7
1·05817	·60	103·8	− 1·62	40·28	1666·7
1·04929	·50	124·6	− 3·24	41·90	2000·0
1·0494	·50	124·7	− 3·75	42·41	2000·0
1·0384	·40	155·6	− 0·67	39·33	2500·0
1·03843	·40	155·6	− 0·74	39·40	2500·0
1·03066	·30	207·8	− 6·86	45·52	3333·3
1·02589	·25	249·4	− 8·21	46·87	4000·0
1·02117	·20	311·8	−10·52	49·18	5000·0
1·01157	·10	623·5	−20·38	59·04	10000·0
1·01163	·10	623·6	−20·95	59·61	10000·0
1·0119	·10	623·7	−23·52	62·18	10000·
1·00671	·06	1038·	−16·57	55·23	16667·
1·00674	·06	1038·	−17·05	55·71	16667·
1·00526	·05	1245·	− 9·81	48·47	20000·
1·00571	·05	1246·	−18·95	57·61	20000·
1·00409	·04	1556·	− 6·81	45·47	25000·
1·00457	·04	1557·	−18·95	57·61	25000·
1·00343	·03	2075·	−18·97	57·63	33333·
1·00349	·03	2076·	−20·86	59·52	33333·
1·00274	·02	3114·	−41·80	80·46	50000·
1·0028	·02	3114·	−44·66	83·32	50000·
1·0016	·01	6227·	−64·67	103·33	100000·
1·00166	·01	6227·	−70·38	109·04	100000·
2	3	4	5	6	7

The three substances at the head of the list are solids. 2·4659 is the specific gravity of anhydrous carbonate of soda, as given by Karsten. 1·51 is the specific gravity of the carbonate with 8 atoms of water of crystallization, as given by Thomson. 1·423 is the specific gravity of the common crystallized carbonate with 10 atoms of water, as given by Haidinger. The specific gravities of the 36 solutions are all from my experiments. The discrepancies in the results of these experiments are very great. Carbonate of soda is a troublesome substance to examine, in consequence of the extreme facility with which it crystallizes. Finding it impossible to obtain concurrent results, I have quoted duplicate experiments to give an idea of the general range of the reactions that take place.

Observations on the Table of Carbonate of Soda.

The strongest solution of carbonate of soda prepared at 62° F. has the sp. gr. of 1·166, and a chemical strength of 185°. Its formula is nearly $NaO, CO^2 + 33H^2O$. A solution of sp. gr. 1·16657 deposited crystals while weighing at 62° F. The solution of sp. gr. 1·166, if cooled a little below 62° F., also deposits crystals. The exact point of saturation is difficult to hit. I made many trials with perfectly pure materials, and always weighed a quantity that filled a bottle of the capacity of 3000 grs. of water. I believe that the sp. gr. 1·166 is very near the truth. It differs considerably however from the usual quotations. Dr. Thomson states the specific gravity of a saturated solution at 58¼° F. to be 1·07516. This is no doubt a misprint for 1·17516. Dr. Henry quotes the specific gravity at the temperature of the atmosphere to be 1·26. This also seems to be a misprint for 1·16.

The greatest amount of condensation occurs in the solution of sp. gr. 1·166, coincident with the greatest chemical strength and greatest density, as is the case with solutions of all the fixed alkalies described in this memoir.

The measure of the test atom of anhydrous carbonate of soda is 38·66 septems, or a little above that of anhydrous sulphuric acid. The measure diminishes rapidly as the salt combines with water, and in a solution of 1° the atomic volume is reduced to −70·38 septems, showing a difference of 109 septems. The condensation produced by the combination of carbonate of soda with water *exceeds* that produced by the hydration of sulphuric acid. Thus, 1 test atom of each substance, combined with different proportions of water, effects the following condensations, expressed in septems:—

Atoms of water.	1 NaO, CO^2 condenses:	1 SO^3 condenses:
8	19 septems.	15 septems.
33	33 ...	19 ...
62	37 ...	22 ...
1038	55 ...	37 ...
6227	109 ...	105 ...

The fraction which +1 indicates the mean specific gravity of 100° of carbonate of soda is (1·4659÷25·866=) ·056673.

There is a particular hydrate of carbonate of soda, the chemical degree of which is about 90, where the condensation is equal to the volume of the test atom of the anhydrous salt. If a solution be made at this stage of dilution, of course the substance will dissolve without altering the bulk of the water.

If, to the same quantity of salt, less water be taken, there will be an expansion of the volume of the water; but if more water be taken, there will be a visible condensation.

Messrs. Playfair and Joule place carbonate of soda among the substances that dissolve in water without changing the volume of the water, and they give an experiment to prove its possession of this property. (Phil. Magazine, xxviii. 506.) But the experiment proves nothing beyond the fact mentioned in the last paragraph. It shows a property characteristic of a particular solution, but not a property manifested by the salt in all its solutions. Instead of being a general character it is only a speciality.

I may perhaps be permitted to make here a passing remark on the theory of the aqueous solution of hydrates advanced by M. Gay-Lussac (*Ann. Chim. Phys.* t. lxx. p. 426):—

"As it is of interest to know whether a salt, susceptible of forming a hydrate, dissolves in water hydrated or anhydrous, I will cite a fact which appears to me sufficient to destroy the uncertainty. It is, that whenever an anhydrous salt, or any other body not having the property to form a hydrate, dissolves in water, there is always a production of cold, and that, on the contrary, whenever the salt can form a hydrate, there is a production of heat. When the hydrate is completed, previous to the solution in water, the case is the same as when the salt cannot form a hydrate. It is conceivable that it might sometimes happen, that the heat produced by hydration was less than the cold produced by the change of state, but I have not yet met with an exception."

The remark I wish to make on this passage is, that it is fallacious in ascribing the production of heat and cold (meaning thereby such as can be detected in the solutions by the thermometer), each to a single reaction. There is, according to my observations, always a double reaction, and it is for want of noticing the double reaction that the "exceptions" alluded to in the last sentence of the extract were never detected. It is due to this eminent philosopher to offer sufficient reasons for my objections to his opinions.

When a test atom of anhydrous carbonate of soda is dissolved in water much heat is disengaged. 38·66 septems of the salt combine with 160·7 septems of water, and produce 179·91 septems of hydrated carbonate of soda. Here there is a loss of volume equal to 19·45 septems, and this condensation is the source of the heat produced. But there is another reaction also to be taken into consideration: the 38·66 septems of dry salt, in combining with the 160·7 septems of water, pass from the solid state to the liquid, and must absorb as much heat as is necessary to give them fluidity. The thermometric heat is the *difference* between the heat produced by condensation and the cold produced by change of state, and in the present example the heat predominates.

But when 179·91 septems of hydrated or crystallized carbonate of soda are dissolved in water, much cold is produced, though not in consequence of the occurrence of the single phænomenon of change of state from solidity to fluidity. It is true, that the liquefaction of 179·91 septems of crystals, and their dispersion through 23·29 atoms (=374·27 septems) of additional water, producing a liquid compound of the bulk of 540·54 septems, occasions the production of much cold, yet it must be observed that the crystallized hydrate, in forming this second hydrate, causes a condensation equal to 13·64 septems, which must unquestionably produce heat. But since in this experiment the mass of matter to be liquefied is almost five times greater than in the former example, while the amount of condensation is one-third less, the single thermometric result of the solution is a fall in temperature. Here then we have evidently that "exception" which M. Gay-Lussac was unable to find, that, namely, where "the heat produced by hydration is less than the cold produced by change of state." And this exception, so far from being a rarity, must occur in every case where the atomic measure of the substance to be liquefied is large, and its power of condensation but small. Without multiplying examples, I think I may safely assume, that in all cases of solution the thermometric heat or cold produced is not the effect of a single reaction, but is the *difference* between the heat disengaged by hydration and condensation, and the heat absorbed by liquefaction or expansion.

Upon this principle, it is easy to explain the heat produced when strong sulphuric acid and potash-ley are diluted with water, for as in these cases the substances are already liquid, the effect caused by condensation is predominant. We can also easily comprehend the cause of the great degree of cold produced by dissolving sal-ammoniac in water. I shall show, in a subsequent section, that 62·23 septems of anhydrous sal-ammoniac, on being dissolved in 256·70 septems of water, equal together to 318·93 septems, actually produce 326·80 septems of solution. Here the cold produced by the liquefaction of 62·23 septems of the solid salt is added to the cold produced by the expansion of 318·93 septems of solution to 326·80 septems, being an expansion of 7·87 septems—equal to one-eighth part of the bulk of the salt. And as in this ase no heat is disengaged, the whole effects of the cold are hermometric.

Gay-Lussac's idea, that the production of cold on solution evidence that the substance dissolved cannot form a hyrate, or become any further hydrated, appears to me to be rroneous. How is it possible to account for the expansion hich occurs on dissolving sal-ammoniac in a minimum of ater, except by admitting the exercise of chemical action? r how account for the condensation it effects with a maximum f water, except by admitting the same agency? It seems to ne, that *hydration* is evident in both cases, notwithstanding he production of cold. What other explanation also shall e give of the phænomena attending the solution of crystalized carbonate of soda? While in the very act of dissolving, nder production of considerable cold, this salt simultaneusly combines with an additional quantity of water, and auses a large amount of condensation. The heat disengaged by this condensation, being spent in liquefying the rystallized hydrate, is not sensible to the thermometer, which nly detects the cold that remains in excess. But since the ondensation certainly takes place and can be measured, it roves the thermometer to be a very fallible and incompetent uide in this field of research.

TABLE XV.—*Ammonia.*

$N^2H^6 = 214{\cdot}47$ grs. Temperature 62° F.

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of N^2H^6 in 1000 septems.	Atoms of water to 1 N^2H^6.	1 N^2H^6		Septems containing 1 atom of N^2H^6.
				Occupies: septems.	Expands: septems.	
F	·76	24·805	0·0	40·31	0·0	40·314
Dv	·875	9·2816	3·96	44·11	3·80	107·74
CD	·8794	9·00	4·17	44·04	3·73	111·11
CD	·8962	8·00	5·06	43·62	3·31	125·00
U	·9	7·7843	5·29	43·48	3·17	128·46
Dv	·9	7·6374	5·43	43·73	3·42	130·93
Dv	·9054	7·4971	5·61	43·26	2·95	133·39
CD	·9116	7·00	6·20	43·27	2·96	142·86
CU	·9225	6·00	7·66	43·56	3·25	166·67
CU	·9347	5·00	9·73	43·70	3·39	200·00
CU	·9468	4·00	12·82	43·94	3·63	250·00
CU	·9603	3·00	18·01	43·87	3·56	333·33
CU	·97288	2·00	28·36	44·20	3·89	500·00
CU	·98666	1·00	59·50	43·98	3·67	1000·0
CU	·9936	·50	121·8	43·44	3·13	2000·0
			Mean	43·73	3·42	

AUTHORITIES.—Dv = Davy. F = Faraday. Ammonia gas condensed by the pressure of 6½ atmospheres at 50° Fahr. U = Ure. CD = Calculated after Davy's numbers. CU = Calculated after Ure's numbers.

Observations on Table XV.—*Ammonia.*

The measure of the test atom of ammonia, as existing in various solutions, varies from 44·20 septems to 43·26 septems. The mean of 14 examples is 43·73 septems. The differences among the determinations are all so small and so irregular as to be apparently mere errors of experiment. Ammonia therefore differs from the fixed alkalies, and from the acids, in having *a fixed atomic volume in solution.* If we compare this volume with the measure of the test atom of anhydrous liquid ammonia condensed by 6½ atmospheres, we find an apparently constant expansion of about 3½ septems; but of this expansion we have no corroborative evidence. It may occur, or it may not.

Reflecting upon the causes that could give to ammonia this peculiar property of a fixed atomic volume in solution, it occurred to me that there might be some relation between the corpuscular constitution of ammonia and that of water. The number of ultimate atoms in an equivalent of ammonia being 8, and those in an equivalent of water being 3, these numbers bear a near proportion to the apparent atomic measure of ammonia, 43·73, and the atomic measure of water, 16·07, the last of these two numbers being very nearly three-eighths of the preceding number. I thought it possible that solutions of ammonia might consist of compounds in which water and ammonia were united in the proportions indicated by those numbers. I tried to construct a table of solutions of ammonia on that principle, using the atomic numbers of Berzelius both for water and ammonia, but I did not succeed by this means in obtaining consistent results, and I considered the hypothesis to be untenable. Yet, previous to abandoning it, I thought it proper to try whether better results would ensue if the same principle was applied to the atomic numbers for water and ammonia that have been proposed by Thomson and Dumas. It gratified me to find that this threw a new light on the difficulty, and enabled me to calculate the following Table:—

TABLE XVI.—*Ammonia.*

$N^2 H^6 = 212{\cdot}5$ grs. Temperature 62° F.

Specific gravity of the solution.	Atoms of N^2H^6 in 1000 septems.	Atoms of H^2O to 1 atom N^2H^6.	Ratio of N^2H^6		1000 septems of solution contain		Septems containing 1 atom of N^2H^6.
			By weight.	By volume.	Grains of N^2H^6.	Grains of H^2O.	
·708333	23·33	0·0	1·00	1·00	4958·3	0000	42·857
·787878	16·97	1·	·65385	·727273	3606·1	1909	58·929
·833333	13·33	2·	·48571	·571429	2833·3	3000	75·000
·862747	10·98	3·	·38636	·470588	2333·3	3706	91·071
·875000	10·00	3·556	·34694	·428571	2125·0	4000	100·00
·883333	9·333	4·	·32075	·40	1983·3	4200	107·14
·887500	9·00	4·247	·30785	·385714	1912·5	4300	111·11
·900000	8·00	5·111	·26984	·342857	1700·	4600	125·00
·912500	7·00	6·222	·23288	·30	1487·5	4900	142·86
·925000	6·00	7·704	·19691	·257143	1275·	5200	166·67
·937500	5·00	9·778	·16195	·214286	1062·5	5500	200·00
·950000	4·00	12·89	·12782	·171429	850·	5800	250·00
·962500	3·00	18·07	·09462	·128571	637·5	6100	333·33
·975000	2·00	28·44	·062271	·085714	425·	6400	500·00
·987500	1·00	59·55	·030741	·042857	212·5	6700	1000·0
·988750	·90	66·47	·027632	·038571	191·25	6730	1111·1
·990000	·80	75·11	·024531	·034286	170·	6760	1250·0
·991250	·70	86·22	·021438	·03	148·75	6790	1428·6
·992500	·60	101·4	·018352	·025714	127·5	6820	1666·7
·993750	·50	121·8	·015274	·021429	106·25	6850	2000·0
·995000	·40	152·9	·012204	·017143	85·	6880	2500·0
·996250	·30	204·7	·009141	·012857	63·75	6910	3333·3
·997500	·20	308·4	·006101	·008571	42·5	6940	5000·0
·998750	·10	619·5	·003040	·004286	21·25	6970	10000·
·998875	·09	688·7	·002735	·003857	19·125	6973	11111·
·999000	·08	775·1	·002431	·003429	17·	6976	12500·
·999125	·07	886·2	·002127	·003	14·875	6979	14286·
·999250	·06	1034·	·001823	·002571	12·75	6982	16667·
·999375	·05	1242·	·001519	·002143	10·625	6985	20000·
·999500	·04	1553·	·001215	·001714	8·5	6988	25000·
·999625	·03	2071·	·000911	·001286	6·375	6991	33333·
·999750	·02	3108·	·000607	·000857	4·25	6994	50000·
·999875	·01	6220·	·000304	·000429	2·125	6997	100000·
1·00000	62·22	Water, test atom = 112·5 grs.				7000	16·0714

Observations on Table XVI.—*Ammonia.*

The table of ammonia is calculated on the following principles:—The atom of water is assumed to weigh 112·5 grs., and to measure 16·071429 septems. The atom of ammonia is assumed to weigh 212·5 grs., and to measure 42·857143 septems. The relation of these measures is as follows:—

$$16{\cdot}071429 \times \frac{8}{3} = 42{\cdot}857143.$$

The ammonia and water are assumed to combine together in all cases without any alteration of measure. The composition of solution of 100°, or 1 test atom of ammonia per decigallon, is calculated thus :—

42·857143 septems = bulk of 1 test-atom of $N^2 H^6$.
957·142857 ... = water required to complete 1000 sept.
6700· grs. = weight of that measure of water.
212·5 ... = weight of 1 test atom of $N^2 H^6$.
6912·5 ... = weight of 1000 septems of solution.

Corresponding specific gravity $\frac{6912 \cdot 5}{7000} = \cdot 987500$.

The ratio of $N^2 H^6$ by weight is $\frac{212 \cdot 5}{6912 \cdot 5} = \cdot 030741$.

We have thus the weight and the volume of the water, the weight and the volume of the ammonia, and the weight and volume of the resulting solution : in short, all the details necessary to give a complete knowledge of the physical constitution of the compound.

The other solutions were all calculated in the same way.

The weight of 1°, the hundredth part of a test atom, of ammonia, is 2·125 grs.; its volume at 62° F. is precisely equal to that of 3 grs. of water. Every degree of ammonia, added to an aqueous solution, displaces 3 grs. of water, reduces the weight of a decigallon of solution to the extent of $3 - 2 \cdot 125 = \cdot 875$ grs., and lessens the specific gravity by ·000125. From this principle proceed two important practical rules :—

I. *Given, the specific gravity of a solution of ammonia, to find its chemical strength.*

Deduct the specific gravity from 1·000000, and divide the remainder by ·000125. The product is the chemical strength expressed in degrees.

II. *Given, the chemical strength of a solution of ammonia, to find its specific gravity.*

Express the chemical strength in degrees; multiply it by ·000125, and deduct the product from 1·000000. The remainder is the required specific gravity.

The four solutions at the head of the list, separated from the others by a horizontal line, are such as do not exist at the temperature of 62° F. The first line is anhydrous ammonia, assumed to retain its usual volume at 62° F. though free from water. The second line is the supposititious oxide of ammonium $= N^2H^8O$. The third line is the hydrate of that assumed oxide $= N^2H^8O + H^2O$, corresponding to caustic potash. The fourth line represents the last compound with an additional atom of water $= N^2H^8O + 2H^2O$. The chemical strength of this third hydrate is 1098°. Now I have procured t 42° F. a solution of the chemical strength of 1120°, so hat this third hydrate is easily obtainable at a temperature etween 42° F. and 62° F.; and the preparation of even the econd hydrate ($N^2H^8O+H^2O$) is perhaps possible at a lower emperature. At 62° F. however, the solution of 1000° was he strongest I could prepare.

An examination of this table leads naturally to the ques- ions, how far is it to be relied on? how nearly does it gree with the results of experiment? I cannot answer these uestions better than by giving an abstract of a series of ex- eriments made expressly to test the validity of the hypo- hesis. These experiments are embraced in Table XVII.

TABLE XVII.—*Ammonia.*

Experiments on the Specific Gravity of Solutions of Ammonia, of known chemical strength. Temperature 62° F.

Chemical strength in atoms.	Specific gravity:		Difference.	Chemical strength in atoms.	Specific gravity:		Difference.
	By theory.	By trial.			By theory.	By trial.	
10·20	·872500	·875943	− 27½	·90	·988750	·988672	+ ½
10·00	·875000	·875175	− 1½	·90	·988750	·988657	+ ¾
9·975	·875313	·875175	+ 1	·80	·990000	·990057	− ½
9·95	·875625	·880629	− 40	·80	·990000	·990086	− ⅔
9·80	·877500	·880629	− 25	·70	·991250	·991086	+ 1¼
9·00	·887500	·894229	− 54	·60	·992500	·992629	− 1
9·00	·887500	·887943	− 3½	·60	·992500	·992857	− 3
8·10	·898750	·898572	+ 1½	·50	·993750	·993714	+ ⅓
8·00	·900000	·900286	− 2¼	·40	·995000	·995429	− 3½
8·00	·900000	·901714	− 13¾	·40	·995000	·995315	− 2¼
7·90	·901250	·901567	− 2½	·40	·995000	·995200	− 1½
7·63	·904625	·905143	− 4	·30	·996250	·996143	+ ¾
7·55	·905625	·906500	− 7	·30	·996250	·996171	+ ½
7·00	·912500	·912829	− 2⅓	·20	·997500	·997886	− 3
6·85	·914375	·913857	+ 4	·20	·997500	·997800	− 2½
6·80	·915000	·915333	− 2½	·10	·998750	·998714	+ ¼
6·00	·925000	·925086	− ¾	·10	·998750	·998771	− ¼
5·00	·937500	·937514	− } 0	·10	·998750	·998857	− ¾
5·00	·937500	·937486	+	·09	·998875	·998971	− ¾
4·05	·949385	·949267	+ 1	·08	·999000	·999029	− ¼
4·00	·950000	·949686	+ 2½	·07	·999125	·999114	+
4·00	·950000	·949600	+ 3¼	·06	·999250	·999257	−
3·00	·962500	·962143	+ 3	·05	·999375	·999400	− ¼
2·00	·975000	·974429	+ 4½	·04	·999500	·999514	− } 0
2·00	·975000	·974629	+ 3	·04	·999500	·999486	+
1·00	·987500	·987229	+ 2	·03	·999625	·999600	+
1·00	·987500	·987257	+ 2	·02	·999750	·999743	+
1·00	·987500	·987514		·01	·999875	·999857	+ } 0
1·00	·987500	·987543	− ⅓	·01	·999875	·999886	−

The chemical strength of the solutions that were examined was determined by neutralization with pure muriatic acid, the strength of which was ascertained by means of carbonate of soda and nitrate of silver. The specific gravities were determined by weighing the liquors in stoppered glass bottles at the temperature of 62° F. I have placed side by side the theoretical densities and those determined by experiment, and I have added a column of differences, to show in what cases and to what extent the experimental specific gravity indicated a greater or lesser chemical strength than the theoretical specific gravity. If in any of these solutions there had occurred either condensation or expansion, it would be shown in this column, where however are also shown all the errors of experiment. These errors are very great in the experiments on concentrated solutions, and they are unavoidable. A solution of ammonia of 1000° *effervesces* when a warm finger is applied to the bottle containing it. In general the specific gravity determined by experiment is too high, for this reason, that when a concentrated solution is prepared and adjusted to the required temperature it takes but little time to transfer a graduated pipetteful of it into a stoppered bottle containing water to dilute it for chemical examination; whereas, the transferring of a quantity of the undiluted solution to a weighing bottle, the adjustment of the measure, and the accurate weighing, occupy much time, occasion considerable exposure to the air, and permit a large escape of gas. Below 800° this difficulty is much less, and the differences are probably errors of experiment. There is however one part of the Table in which the differences between the computed and experimental specific gravities seem to indicate the occurrence of expansion. This is between 500° and 100°, and it is among solutions of this strength that Dr. Ure found similar differences to exist between experimental and computed densities; from which differences he drew the conclusion, that solutions of ammonia expand when they are diluted. I subjoin his experiments :—

Computed or mean specific gravity.	Specific gravity by experiment.	Computed or mean specific gravity.	Specific gravity by experiment.
·90452	·9045	·94737	·9455
·90909	·909	·95744	·9564
·91838	·9177	·96774	·9662
·9278	·9275	·97826	·9768
·9375	·9363	·989	·9887

—*Tilloch's Philosophical Magazine*, vol. lvii.

The approximate coincidence of my experiments with Dr. Ure's will perhaps be held to prove the correctness of his opinion; and I must admit that there is some appearance of the occurrence of a slight expansion at about 300°. Yet this apparent expansion is so small, and the figures that indicate it are so likely to be merely the expression of errors of experiment, that I consider the expansion, though possible, not by any means proved. The evidence tending to prove that ammonia has a fixed atomic volume in solution, seems to me greatly to preponderate over that which indicates expansion. My doubts respecting the occurrence of expansion were further strengthened by the results of the following experiment.

Into a long narrow graduated bottle I put 500 septems of strong commercial ammonia, the degree of which was 810°. Its specific gravity was 0·898572 by trial. By means of a bottle so graduated as to *deliver* 500 septems of water, I added that quantity to the ammonia. The bottle was stopped, wrapped in a thick towel, and briskly shaken for about a minute. Each liquor previous to the mixture was brought to the temperature of 51° F. Immediately after the shaking, the temperature was found to be 54° F. Here was a rise of 3°, indicating, not expansion, but condensation. When the liquor had returned to 51° F. its volume was examined. There certainly was no expansion. I think there was a slight condensation, but it was scarcely measurable. The chemical strength of the diluted liquor was found to be 406°. This indicated condensation. Its specific gravity was ·950600, and since (·898572 + 1) ÷ 2 = ·949286, this also indicated condensation.

The results which in this experiment indicate condensation are, I believe, as remote from the truth as those which in other experiments seemed to indicate expansion. One of these results is remarkable, namely, the rise in temperature which apparently results from the combination of the two liquids. If water and liquid ammonia combine without change of volume, there should be no change of temperature. To what cause then shall we attribute the rise of 3° F.? To agitation alone? That, I think, is sufficient to account for it.

I cleaned the bottle from ammonia, put into it a decigallon of distilled water, and shook it for one minute. The temperature rose from 49° to 49½° F. Half a gallon of water shaken in a larger bottle rose from 43½° to 44¼°. 3000 septems rose from 49¾° to 50¼°. A solution of pure carbonate of potash rose from 50½° to 51½°. A solution of pearlash from 50½° to 51½°. A solution of bicarbonate of potash from 50½° to 51¾°. Oil of vitriol from 51½° to 52¼°.

These experiments show how difficult it is to determine the amount of heat that is disengaged when liquors are mixed, since the agitation necessary to effect the mixture can of itself produce part of the rise of temperature which is attributed wholly to the chemical action of the substances mixed together.

I shall conclude this notice of ammonia by referring to a fact of considerable importance which is disclosed in Table XVI. This is, that it is impossible to state accurately the specific gravity of solutions of ammonia with less than six places of decimals, since the effect of every chemical degree on the specific gravity is ·000125. Thus while a solution of 800° is accurately expressed by ·9 or ·900000, a solution of 799° requires ·900125, and a solution of 801° requires ·899875. This peculiarity ought to be attended to in future researches on aqueous solutions.

This observation leads me to make another which may be interesting to practical men, who are accustomed to estimate the value of ammonia and similar liquors after the indications afforded by hydrometers that have scales with large divisions —Twaddell's, Baumé's, and the like. Now 40 chemical degrees of ammonia, or ⅖ of a test atom, affect the density of the liquor only so much as answers to 1 degree of the scale of Twaddell's hydrometer. Such an instrument therefore, if perfect in construction, and used with unerring skill, is unfit to tell the strength of solutions of slight density, such as those of ammonia and acetic acid: methods of chemical testing are greatly preferable.

Since the preceding section on ammonia was written, I have seen an abstract of Dr. Faraday's recent researches concerning condensed gases. In that it appears that his present estimate of the specific gravity of condensed liquid ammonia at 60° F. is 0·731. His former estimate was 0·76. The specific gravity which theoretical considerations led me to adopt for anhydrous liquid ammonia at 62° F. is 0·708333. This differs from Dr. Faraday's new experimental determination by only 0·022667. The number of test atoms of ammonia contained in a decigallon is, according to Dr. Faraday's specific gravity, 24·08, according to mine 23·33. The difference is only ¾ of a test atom per decigallon. The atomic measure of a test atom of ammonia, according to Dr. Faraday's specific gravity, is 41·5282, according to mine 42·8571 septems. The difference is 1·3289 septems. Hence my assumptions are almost confirmed by Dr. Faraday's experiments.

TABLE XVIII.—*Sal-Ammoniac.*

$H^2Cl^2+N^2H^6=667{\cdot}63$ grs. Temperature 62° F.

$[H^2Cl^2=455{\cdot}13+N^2H^6=212{\cdot}5.]$

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of $H^2Cl^2N^2H^6$ in 1000 septems.	Atoms of water to 1 $H^2Cl^2+N^2H^6$.	1 H^2Cl^2, N^2H^6		Septems of solution containing 1 H^2Cl^2, N^2 H^6.
				Occupies: septems.	Condenses: septems.	
Kp	1·5	15·727	0·0	63·58	0·0	63·584
Hk	1·525	15·992	0·0	62·53	0·0	62·531
Ms	1·528	16·021	0·0	62·42	0·0	62·419
PJ	1·578	16·545	0·0	60·44	0·0	60·441
Mean	1·53275	16·071	0·0	62·23	0·0	62·225
G	1·077333	3·06	15·97	70·10	−7·87	326·80
G	1·075829	3·00	16·38	70·10	−7·87	333·33
G	1·054114	2·00	26·86	68·32	−6·09	500·00
Hk	1·032300	1·2076	47·26	68·63	−6·40	828·11
G	1·028658	1·00	58·07	66·72	−4·49	1000·0
G	1·028686	1·00	58·08	66·69	−4·46	1000·0
G	1·026086	·90	65·01	66·39	−4·16	1111·1
G	1·023029	·80	73·64	66·59	−4·36	1250·0
G	1·020200	·70	84·76	66·52	−4·29	1428·6
G	1·017571	·60	99·60	66·09	−3·86	1666·7
G	1·014743	·50	120·4	65·89	−3·66	2000·0
G	1·011800	·40	151·5	65·88	−3·65	2500·0
G	1·009143	·30	203·4	64·90	−2·67	3333·3
PJ	1·008800	·27881	219·2	63·81	−1·58	3586·6
G	1·006257	·20	307·1	64·09	−1·86	5000·0
G	1·004657	·15	410·8	64·33	−2·10	6666·7
G	1·004000	·125	493·9	63·38	−1·15	8000·0
G	1·003514	·10	618·5	60·23	2·00	10000·0
G	1·003486	·10	618·5	60·52	1·71	10000·0
G	1·003143	·09	687·6	60·46	1·77	11111·
G	1·002886	·08	774·1	59·30	2·93	12500·
G	1·002657	·07	885·4	57·42	4·81	14286·
G	1·002371	·06	1034·	55·85	6·38	16667·
G	1·001971	·05	1241·	55·95	6·28	20000·
G	1·001543	·04	1552·	56·80	5·43	25000·
G	1·001714	·04	1552·	52·52	9·71	25000·
G	1·001114	·03	2071·	58·23	4·00	33333·
G	1·000800	·02	3108·	55·37	6·86	50000·
G	1·000457	·01	6219·	49·66	12·57	100000·
1	2	3	4	5	6	7

AUTHORITIES.—Kp = Kopp. Hk = Holker. Ms = Mohs. PJ = Playfair and Joule. All these relate to the specific gravity of anhydrous sal-ammoniac. I have adopted the mean as the true number. The solutions are all described from my experiments, except the two marked Holker and Playfair and Joule.

Observations on the Table of Sal-Ammoniac.

I have adopted for the weight of the test atom of sal-ammoniac Berzelius's number for muriatic acid, added to Thomson's number for ammonia; my object being to afford a means of comparing solutions of sal-ammoniac at given strengths with those of muriatic acid and ammonia at the same strengths, quoted in other sections of this memoir.

The strongest solution of sal-ammoniac at 62° F. has the specific gravity of 1·077333 and the chemical strength of 306°. Its formula is probably $H^2Cl^2, N^2H^6 + 16H^2O$. This solution gives a considerable deposit of crystals when cooled to 59° F. The measure of the test atom of the anhydrous salt is 62·23 septems. When existing in the solution of 306° its measure is increased to 70·10 septems. When in a solution of 1° its measure is diminished to about 50 septems. At an intermediate point, somewhere about 12°, the measure of the atom in solution is the same as that of the anhydrous atom. Hence sal-ammoniac has no fixed volume in solution, but possesses the property of causing *expansion* in strong solutions and *condensation* in weak solutions.

The atomic measure of sal-ammoniac is never the same as the joint measure of muriatic acid and ammonia. The differences at different stages of dilution are as follow:—

Chemical strength of the solutions.	Measure of H^2Cl^2 added to that of N^2H^6.	Measure of Sal-ammoniac.	Difference or condensation.
°	Septems.	Septems.	Septems.
300	79	70	9
200	78	68	10
100	77	67	10
50	76	66	10
10	76	60	16
5	76	56	20
1	78	50	28

Messrs. Playfair and Joule seem to be of opinion that when we know the atomic measure of a compound, A B, and that of one of its components, A, then, by taking the difference, we can find the true measure of the other component, B. It is clear, however, that this is not the case with sal-ammoniac; for this salt, when at its state of greatest expansion, has a smaller atomic measure than ammonia *plus* muriatic acid when the latter is at its point of greatest condensation. Hence any attempt to determine the atomic measure of ammonia from a knowledge of the atomic measures of muriatic acid and of sal-ammoniac must lead only to bewilderment.

The fraction which + 1 indicates the mean specific gravity of 100° of sal-ammoniac is (·53257÷16·071=) ·033150.

It follows from what has been stated, that the real specific gravity of solutions of sal-ammoniac must be *less* than the mean specific gravity when the chemical strength is above 12°, and *greater* than the mean specific gravity when the chemical strength is below 12°.

Examples:—

	100°		10°
Mean sp. gr. .	1·033150	Mean sp. gr. .	1·003315
Deduct . . .	0·004490	Add	0·000200
Real sp. gr. .	1·028660	Real sp. gr. .	1·003515

The following experiments were made to determine the correctness of the number assumed for the atomic weight of sal-ammoniac.

1. A solution of 66·763 grs. of sal-ammoniac mixed with a solution of 212·864 grs. of crystallized nitrate of silver, required half a septem of solution of nitrate of silver of 10° to complete the precipitation.

2. A solution of 66·96 grs. of sal-ammoniac (Berzelius's number) mixed with a solution of 212·864 grs. of nitrate of silver, required 3 septems of solution of nitrate of silver of 10° to complete the precipitation.

These experiments tend to prove that, taking for granted the accuracy of Berzelius's number for nitrate of silver, his number for sal-ammoniac is too high. But if, as the above experiments seem to show, 667·63 is the correct number for sal-ammoniac, then 212·5 is the correct number for ammonia, as I have assumed it to be in Table XVI.

TABLE XIX.—*Sugar.*

$C^{12} H^{22} O^{11}$ = 2154 grs. Temperature 62° F.

Authorities for the specific gravity.	Specific gravity of the solutions.	Atoms of $C^{12} H^{22} O^{11}$ in 1000 septems.	Atoms of water to 1 $C^{12} H^{22} O^{11}$.	1 $C^{12} H^{22} O^{11}$		Septems of solution containing 1 $C^{12} H^{22} O^{11}$.
				Occupies: septems.	Condenses: septems.	
F	1·6	5·1996	0·0	192·32	0·0	192·32
PJ	1·596	5·1866	0·0	192·80	0·0	192·80
PJ	1·3866	3·3796	6·383	193·32	−1·00	295·89
U	1·326	2·8728	9·574	194·24	−1·92	348·10
PJ	1·2329	2·0034	19·15	191·46	·86	499·16
U	1·231	2·0002	19·15	192·23	·09	499·94
U	1·1777	1·5309	28·72	191·64	·68	653·21
U	1·144	1·2392	38·30	191·51	·81	806·95
U	1·134	1·1516	42·13	191·35	·97	868·33
U	1·125	1·0753	45·95	191·47	·85	929·97
U	1·111	·95014	53·62	190·89	1·43	1052·5
U	1·1045	·89734	57·44	191·26	1·06	1114·4
U	1·0905	·77044	68·93	190·25	2·07	1298·0
U	1·082	·70325	76·60	191·11	1·21	1422·0
U	1·0685	·57871	95·75	189·34	2·98	1728·0
U	1·05	·42653	134·0	190·49	1·83	2344·5
U	1·0395	·33781	172·3	190·79	1·53	2960·2
PJ	1·0367	·30627	191·5	187·89	4·43	3265·1
PJ	1·00566	·04405	1401·	179·22	13·10	22702·
PJ	1·00352	·02696	2298·	177·11	15·21	37103·
PJ	1·0035	·02658	2330·	176·02	16·30	37627·
1	2	3	4	5	6	7

AUTHORITIES.—F = Fahrenheit. PJ = Playfair and Joule, Phil. Magazine, xxviii. 459 and 468. U = Ure, Dict. of Arts, article Sugar. The first two lines represent anhydrous sugar. The others are all solutions.

Observations on the Table of Sugar.

The experiments of Messrs. Playfair and Joule corroborate those of Dr. Ure, though made in a different manner and for a different purpose. According to the experiments of both parties, when sugar is dissolved in water so as to form a strong solution, there is an *expansion* of volume. The volume of the solution exceeds the volume of the two ingredients. When more water is added *condensation* ensues, and is carried by continued dilution to such an extent, that, according to the experiments of Messrs. Playfair and Joule, in a solution of sp. gr. 1·0035, the condensation effected by 1 test atom of sugar is equal to 16·30 septems, or more than the measure of an atom of water.

The volume of 1 test atom of water being 16·07 septems, that of 11 test atoms is 176·77 septems, and that of 12 test atoms is 192·84 septems. The atomic measure of anhydrous sugar is 192·32 septems, or equal to that of 12 test atoms of water. Its measure in solution varies from 194·24 septems to 176·02 septems. Hence, in one very dilute solution of sugar, its atomic volume, as stated by Dalton and by Messrs. Playfair and Joule (Phil. Magazine, vol. xxviii. page 470), is equal to that of 11 atoms of water, but, in highly concentrated solutions, its atomic volume, according to the experiments both of Dr. Ure and of Messrs. Playfair and Joule, is equal to more than that of 12 atoms of water. Sugar, therefore, has no fixed atomic volume in solution. It does not uniformly add to the bulk of the water of solution, a volume equivalent to its own oxygen and hydrogen estimated as water. Nor does its carbon cease to occupy space in solution. In short, Dalton's experiments on this subject are incorrect, his hypothesis is untenable, and the application of this method of analysis to organic bodies is impossible. Witness, acetic acid.

TABLE XX.—*Sulphate of Magnesia.*

$MgO, SO^3 = 759·518$ grs. Temperature 62° F.
$MgO, SO^3 + 7H^2O = 1546·878$ grs.

Authorities for the specific gravity.	Specific gravity of the solution.	Atoms of the salt in 1000 septems.	$MgO, SO^3 + 7H_2O$ occupies: septems.	Anhydrous MgO, SO^3 occupies: septems.	MgO, SO^3 condenses: septems.
Kn	2·6066	24·023	......	41·63	......
Kp	1·674	7·5753	132·01	19·53	22·10
Hk	1·665	7·5345	132·72	20·24	21·39
D	1·30	3·5944	......	25·04	16·59
D	1·05	·55298	......	18·08	23·55
	......	......	......	......	......
A	1·270	2·7218	121·78	9·30	32·33
A	1·150	1·4868	120·10	7·62	34·01
Hk	1·1284	1·2734	120·15	7·67	33·96
A	1·128	1·2373	117·53	5·05	36·58
A	1·107	1·0334	117·44	4·96	36·67
A	1·055	·51137	113·41	0·93	40·70
A	1·010	·08958	109·35	− 3·13	44·76
A	1·006	·04507	87·84	−24·64	66·27

AUTHORITIES.—A = Anthon (temperature of solutions 59° F.). D = Dalton. Hk = Holker. Kn = Karsten. Kp = Kopp.

Observations on the Table of Sulphate of Magnesia.

Sulphate of magnesia is a substance regarding whose peculiar properties Messrs. Playfair and Joule are in strict accordance with Dr. Dalton. They find, with him, that when it dissolves in water it occupies no space in solution (Phil. Magazine, vol. xxviii. p. 470).

As I also find that this fact is capable of experimental proof, it is necessary to inquire how it happens that the fact is true while the hypothesis that has been founded upon it is fallacious. The matter is not mysterious.

One test atom of anhydrous sulphate of magnesia measures, in round numbers, 42 septems. When this combines with 7 atoms of water to form the crystallized salt, it condenses 22 septems, and retains 20 septems as its measure in combination. These facts result from the experiments of Karsten, Kopp, and Holker, cited in the Table.

In the year 1810 Dalton published some experiments on solutions of this salt, of which, as they are inaccurate, I have only quoted two, the first and the last. According to these, when the solution has a specific gravity of 1·30, the measure of the test atom of anhydrous salt is 25 septems; but when the specific gravity is 1·05, the measure of the test atom is only 18 septems. These experiments, if correct, would prove three interesting points:—1. That the salt expands 5 septems per test atom on being liquefied by solution. 2. That it possesses a considerable volume in solution. 3. That the volume in solution is variable. These results are in direct opposition to Dalton's recent speculations.

Anthon's experiments show that the test atom of anhydrous sulphate of magnesia occupies about 9 septems in a solution of sp. gr. 1·27; that it gradually diminishes in bulk with the progress of the dilution, becomes nothing in a solution of sp. gr. 1·05, and in still weaker solutions effects great condensation, which in the last solution of sp. gr. 1·006, amounts to no less than 66 septems, or $\frac{3}{2}$ of the volume of the anhydrous salt. Anthon's experiments were made with the crystallized salt; but since the measure of 7 atoms of water is 112·48 septems, the reduction to the anhydrous salt is easy and obvious. Anthon's Table is contained in Gmelin's *Handbuch der Chemie, vierte Auflage*, t. ii. p. 235[*].

Holker's experiment corroborates the experiments of Anthon, as far as a single experiment can serve as evidence in a case of this nature.

These researches show that anhydrous sulphate of magnesia, contrary to Dalton's late assumption, *does* occupy space when in strong solutions; that in weak solutions it occasions a condensation greater than its own volume; and that, at a certain intermediate point, it effects no condensation and oc-

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[*] It is due to Professor Gmelin to state, that I am indebted for many facts and authorities quoted in this memoir to the new edition of his *Handbuch der Chemie*, the most comprehensive, accurate, and convenient systematic work on Chemistry that ever issued from the press.

cupies no space in solution. The solution which corresponds to this turning-point has a chemical strength of about 50°, and a specific gravity of nearly 1·05. Were a solution of sulphate of magnesia to be carefully made with proportions of salt and water corresponding to this stage of dilution, the result would appear to corroborate the late experiments of Dalton and of Messrs. Playfair and Joule, and thus a very accurate experiment might be made to support a very fallacious theory.

This last remark applies equally to many other substances whose examination is described in this memoir, and it shows the danger of drawing a sweeping conclusion from the results of any single experiment, however accurately performed. Thus, in the case of sulphuric acid, if we were to adopt the rash principle of deciding upon the atomic measure from the results of a single experiment, we could prove the atomic measure of that acid to be 2 volumes, or 1 volume, or nothing, or less than nothing; in short, anything we pleased between *plus* 34 septems and *minus* 68 septems; for we could pick out an accurate experiment to yield any numerical product that best suited a given purpose within those limits.

LXV. *Observations on a Paper entitled "Some Points in the Meteorology of Bombay," read by* Colonel Sabine *to the British Association in* 1845, *and published in the Philosophical Magazine for January* 1846. *By* THOMAS HOPKINS, *Esq.*[*]

WITH our imperfect knowledge of the influences which determine the general movements of the atmosphere, the semi-diurnal fluctuations of the barometer become interesting objects of study, as it appears not improbable that the causes which produce the daily alterations of that instrument may determine its casual and irregular fluctuations, as well as the general movements of the atmosphere itself.

That the diurnal fluctuations of the barometer are in some way connected with alterations of temperature, is apparent from their relation to the daily variation of solar influences; but the precise way in which these influences operate is not equally clear. A rise of temperature, such as that which takes place in the morning, there can be no doubt tends in some degree to lighten the atmosphere, and to produce a fall of the barometer in the part; but at the same time it increases evaporation of aqueous vapour from wet surfaces, the effect of which is to add to the weight of the whole atmosphere in the locality, to the extent of the addition of the vapour that is

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[*] Communicated by the Author.

thereby produced. Thus through solar influence two causes are brought into operation at the same time, which produce opposite effects on the pressure of the atmosphere,—the one tending to make it lighter, the other heavier.

By a careful registration of the hourly alterations of both the wet and the dry-bulb thermometers, we may obtain,—first, a knowledge of the daily changes of temperature near the surface of the earth; and secondly, of the extent to which evaporation keeps down the temperature in the wet-bulb thermometer: and from the extent to which temperature is thus kept down in the wet-bulb thermometer, we may infer the energy with which vapour is produced from wet surfaces, and thrown into the air to increase the aggregate atmospheric pressure. If the daily fluctuations of the barometer were produced by these two influences alone, those influences might, without much difficulty, be traced to their results, and their separate as well as their joint effects might be exhibited in the movements of the barometer.

But in most of the cases where registrations of the two instruments have been furnished, the barometric fluctuations have not been found to accord with the increase of vapour produced by evaporation. From sunrise until about ten o'clock in the morning evaporation generally proceeds with considerable energy, and a consequent increase of vapour pressure may be traced in a rise of the barometer; but after that time any increase of vapour pressure on the barometer ceases: yet that evaporation continues active afterwards is shown by the effect that is produced by it on the wet-bulb thermometer. What then becomes of this additional vapour? Or why does it not further raise the mercury of the barometer?

About ten o'clock in the morning the barometer ceases to rise, and shortly afterwards begins to fall, and it continues falling until four in the afternoon, although the cooling of the wet-bulb thermometer by evaporation shows that from ten until one or two o'clock, the time of the highest temperature, vapour must be passing into the atmosphere in increasing quantities! and the vapour must be presumed to accumulate in the atmosphere until four o'clock, as the evaporation generally continues active up to that hour, and no decline of the dew-point indicates the condensation of any portion of the vapour thus produced. Now if increase of vapour had raised the barometer up to ten o'clock, why should not a further accumulation of vapour after that hour raise the barometer still higher? The fall of the barometer after ten o'clock, it is evident, cannot be accounted for from the operation of the causes named.

That mere alterations of thermometric temperature and of vapour pressure, either jointly or separately, do not produce the fall of the barometer from ten in the morning to four in the afternoon, may be proved by facts when the registration of the wet-bulb thermometer is given; but unfortunately this is seldom done. In the Plymouth report by Mr. S. Harris that registration is to be found; and there it may be seen that the temperature theory does not harmonize with the facts. But in the account of the meteorology of Bombay, as given by Colonel Sabine, there is no notice of that instrument. Vapour pressure, *as ascertained by the dew-point*, is there exhibited as one of the two causes which determine barometric fluctuation,—thermometric temperature being considered the other; and these two causes are by him represented as sufficient to account for the facts and form the basis of his theory, which for convenient reference I have called "the temperature theory."

In remarking at present on the Bombay report, I propose to confine myself principally to objections to the theory advanced to account for the phænomena, and do not intend to show in a detailed form what were the real causes in operation, seeing that all the facts required are not furnished. Had an account of the evaporation that took place been given, I would have attempted to trace the vapour that was produced after ten o'clock, until it was condensed in the higher part of the atmosphere and converted into cloud, with the effects of that conversion on the barometer; but without the registration of the wet-bulb thermometer this cannot be satisfactorily done.

In the report the facts contained in the following table are given for Bombay in 1843,—of mean temperature, mean barometric pressure, mean tension of vapour, and mean gaseous pressure at every second hour.

Hour of mean Bombay time. Astronomical reckoning.	Temperature.	Barometer.	Dew-point, or Tension of Vapour.	Gaseous Pressure.
	°	in.	in.	in.
16	78·9	29·778	0·761	29·017
18	78·4	29·805	0·750	29·055
20	79·6	29·840	0·766	29·074
22	81·8	29·852	0·771	29·081
0	83·2	29·817	0·768	29·049
2	84·1	29·776	0·795	28·981
4	83·9	29·755	0·800	28·955
6	82·3	29·774	0·802	28·972
8	81·2	29·806	0·801	29·005
10	80·3	29·825	0·780	29·045
12	79·8	29·809	0·775	29·034
14	79·4	29·786	0·766	29·020

This table commences at four o'clock in the morning, that hour being generally one of the turning-points in the semi-diurnal movements of the barometer. From four to ten that instrument rises from 29·778 to 29·852 inches. Now this rise, on the temperature theory, must be considered to have been produced either by a reduction of temperature, as measured by the thermometer near the surface of the earth, or by an increase of the pressure of aqueous vapour, as ascertained by the dew-point; or by both those influences acting together, and producing the rise of the barometer, as a general result.

We find, however, that during the first two hours, that is from four to six o'clock, the thermometer fell from 78°·9 to 78°·4, being a fall of ·5; and the *tendency* of this fall most undoubtedly was to produce a rise of the barometer; but at the same time the pressure of vapour, as indicated by the dew-point, declined so much as from ·761 to ·750, or ·011. Now this comparatively large reduction of vapour pressure is evidently the greater of the two disturbing forces, and the result of the action of both the forces, according to the theory, ought to be a fall of the barometer. But instead of falling the barometer rose during the two hours from 29·778 to 29·805, or ·027; showing that the movement of that instrument in this time was the reverse of what would have occurred if the temperature theory was true.

From six to ten o'clock, the remainder of the first period of six hours, the barometer continued rising, and passed from 29·805 to 29·852, making a rise in these four hours of ·047, whilst the thermometer rose from 78°·4 to 81°·8, or 3°·4, and the dew-point increased from ·750 to ·771, or ·021. Thus of the whole six hours we find that, during the first two,

$$\begin{array}{l}\text{a fall of temperature of . . } \cdot 5 \\ \text{and a fall of the dew-point of } \cdot 011\end{array}\left\{\begin{array}{c}\text{produced a rise of}\\ \text{the barometer of}\end{array}\right\}\cdot 027$$

which would appear to prove, and as far as the facts given go, do prove, that the small fall of temperature had the greater influence on atmospheric pressure, as the barometer rose ·027 while both influences were in operation.

But in the following four hours, from six to ten o'clock, a rise of the thermometer of 3°·4 and a rise of the dew-point of ·021, were accompanied by a rise of the barometer of ·047; or put into tabular form, say that

$$\begin{array}{l}\text{a rise of temperature of } 3^\circ{\cdot}4 \\ \text{and of the dew-point of } \cdot 021\end{array}\left\{\begin{array}{c}\text{produced a rise of}\\ \text{the barometer of}\end{array}\right\}\cdot 047$$

that is between four and six o'clock, when the fall of the dew-point is great compared with the fall of temperature, and the two acting together ought to produce a fall of the barometer, that instrument rises! But between six and ten the temperature increases greatly, as much as nearly seven times the amount that it previously fell, whilst the dew-point, although it also rises, does not rise to quite double the extent that it fell in the two hours; yet notwithstanding these great alterations, amounting to complete reversals of the two influences that are in operation, the barometer continues to rise, and attains an increase of ·047. These facts, however, taken together, show that temperature, as measured by the thermometer near the surface of the earth, produces little comparative effect on the amount of atmospheric pressure; and that the alteration of vapour pressure must have had much greater influence than alteration of temperature as measured by the thermometer, and must have produced the rise of the barometer from six to ten o'clock.

Proceeding to the next six-hour stage of the daily fluctuations of the barometer, we find that from ten to two o'clock, the first four of these six hours, temperature advances 2°·3, and reaches its highest point for the day at the last-named hour; and the dew-point, indicating vapour pressure, also rises as much as ·024. Here it appears that vapour pressure increases so much as ·024, whilst temperature advances only 2°·3; alteration of vapour pressure therefore should, in accordance with what had occurred previously, be now more powerful than alteration of temperature, and should further increase the whole atmospheric pressure, and cause the barometer to rise higher than the point it had previously reached, as the theory we are examining teaches it ought to do; but instead of rising the barometer falls no less than ·076; so that from six to ten o'clock, whilst

a rise of temperature of 3°·4 } jointly produced a rise of the barometer of } ·047
and a rise of the dew-point of . . . . ·021

in the following four hours, from ten to two o'clock,

a rise of temperature of . ·2°·3 } produced a fall of the barometer of } ·076
and a rise of the dew-point of ·024

Thus, where the vapour pressure is relatively weak, we see that the barometer rises; and where it is relatively strong the barometer sinks,—being the reverse of that which must have taken place if the temperature theory were correct.

Again, from two to four o'clock P.M. the temperature sank ·2 and the dew-point rose ·005. Thus during these two hours the alterations which took place in both the temperature and the dew-point were such as from the operation of each of their influences ought, on the temperature theory, to have caused the barometer to rise. But it did not rise; on the contrary it fell,—no less than ·021. These facts prove most conclusively, that the fluctuations of the barometer in the times named were not produced by alterations of temperature and of vapour pressure, as they are exhibited to us by the thermometer and the dew-point.

It should be borne in mind, that it is the fall of the barometer from ten in the morning to four in the afternoon while evaporation, as shown by the wet-bulb thermometer, is active, for which we have particularly to account. Temperature rises while vapour augments up to ten o'clock in the morning, and at the same time the barometer rises; but after ten, though the same two causes continue in operation, yet the barometer falls, and continues falling until four in the afternoon. Now why should the same causes be supposed to produce one effect up to ten o'clock, and another, of an opposite character, for six hours after that time? This is the question that has to be answered. The temperature theory, so far from accounting for the occurrence of these phænomena, is directly opposed to them, and utterly fails to account for the fall of the barometer from ten to four o'clock.

The alterations which took place in the subsequent twelve hours present additional evidence of the inadequacy of the temperature theory to account for the semi-diurnal fluctuations of the barometer; but as they are not equally palpable with those already examined, it is unnecessary to go into a consideration of them.

In furnishing such accounts as those of the meteorology of Bombay, that which is required in order more fully to elucidate the subject is, that in addition to the usual registrations the temperature of the wet-bulb thermometer should be given. This would enable us to trace the vapour that is thrown by evaporation into the atmosphere in the hottest part of the day. Could we do this, there seems no doubt that it would be found ascending in the atmosphere until it became condensed and formed cloud; and the heat liberated by this condensation, as I have elsewhere explained, is the real cause of the diminished pressure of the atmosphere in the locality during the six hours that the barometer falls.

In a note appended to the paper published in the Philosophical Magazine for January 1846, Colonel Sabine gives an extract from a paper received from M. Dove, which is stated to have been read in the Academy at Berlin, in which it is said that "at Catherinenbourg and Nertchinsk, on the mean of several years, and at Barnaoul, in the years 1838 and 1840, the mean diurnal barometric curve itself exhibits but one maximum and one minimum in the twenty-four hours; the maximum coinciding nearly with the coldest and the minimum with the hottest hours of the day." But the Colonel afterwards shows from other accounts, that there is a very small morning maximum in the locality named, which occurs between the hours of eight and eleven A.M. This he attributes to the pressure of additional aqueous vapour, and he seems to think such a fact confirmatory of the correctness of his views. If however an increase of vapour up to eleven o'clock could raise the barometer, why should not that instrument rise further after that hour? Had we a registration of the wet and dry-bulb thermometers and of the dew-point, it is to be presumed that proof would be furnished that evaporation was more active after eleven than it had been before that hour, and that there was no decline of the dew-point until a much later period of the day. The vapour that must be presumed to have passed into the atmosphere in the hottest part of the day, must either have added to the previous pressure on the barometer and raised it, or it must have been condensed, and warmed the higher part of the atmosphere. As it did not produce the former effect, in the absence of information we may assume that it produced the latter.

In the extract given from M. Dove's communication we have no account of the daily range of the barometer, or of the extent of its fall during the day, though that is the most interesting fact to be ascertained; this is to be regretted. If a sun-heated surface is the cause of the atmosphere becoming so much lighter in the day than it is in the night, the atmosphere at the places named by M. Dove ought to be rendered much lighter every day than it was in the previous night, seeing that the temperature of the day is raised greatly above that of the night; the daily range of the barometer should consequently, on the Colonel's theory, be extensive. Is it so? If it is not, it is so far evidence against the theory.

It is stated in the Russian account that in the places pointed out there is one maximum and one minimum, the maximum nearly coinciding with the coldest, and the minimum with the hottest hours of the day. And the conclusion drawn seems to be, that alteration of surface temperature is the sole cause of the movement of the barometer: hence the desirableness of showing the extent of that movement. If it be great, it will so far show that the heating of the surface of the earth by the sun could materially diminish atmospheric pressure. But if the daily movement of the barometer is found to be small, it will present presumptive evidence that daily surface-heating has no important local effect on the weight of the atmosphere. A full account of the registrations in this part of the world might furnish the means of proving either the correctness or the fallacious nature of the temperature theory, to which objections have been advanced in this paper.

LXVI. *Note on the Bands formed by Partial Interception in the Prismatic Spectrum. By* Professor Powell.*

IN a communication to the Physical Section of the British Association at its last meeting, I offered some remarks on the question relative to the above-named singular phænomena; in particular stating *one* experimental result, originally announced by Sir D. Brewster, which I had fully verified, and which had appeared to him at variance with the theory of Mr. Airy, along with *another* fact of my own detection, viz. that when the aperture was contracted the bands became more vivid. No one, I trust, was led, from the manner in which my remarks were reported, to imagine that I brought this forward in any spirit of hostility to the theory. On the contrary, I never entertained a doubt (and I believe I expressed this at the meeting) that upon every ground of analogy and probability the theory would still be found adequate to the solution of the difficulty: I fully expected that it wanted only the introduction of some further condition or modification, though, as it appeared, no such condition had yet been pointed out. It seems that this essential condition, now first brought to notice by the Astronomer Royal in his paper in the last Number of this Journal, had in some way escaped the attention of all those who had read his previous papers in the Philosophical Transactions, ever since the first announcement of Sir David Brewster's objection at the British Association at Manchester, 1842†, and notwithstanding that his detailed repetition of that objection was fully discussed at the Cambridge meeting in 1845. And I am anxious to seize this opportunity of acknowledging and recording my conviction, that it entirely supplies what was wanting to relieve the theory of the objection which seemed to beset it; and that this has proved but one more of those already numerous and striking instances, in which what seemed a most formidable difficulty in the undulatory theory of light has been converted into one of its strongest points of support.

Oxford, November 2, 1846.

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* Communicated by the Author.

† See Report, Sectional Proceedings, p. 12.

LXVII. *On Palmic Acid, a Fat Acid related to the Margaryle Series.* *By* LYON PLAYFAIR, *Esq., Ph.D.*[*]

BOUDET[†] has shown that castor oil is solidified by nitric acid containing peroxide of nitrogen (NO_4), and that the latter is itself sufficient for this purpose. He described the solid fat thus procured under the name of *palmine*, and the fat acid obtained by the decomposition of its soap as *palmic acid*.

Palmic acid is stated to have a fusing-point of 50° C., although described as usually having it a few degrees lower; but I have never succeeded in procuring it of a higher melting-point than 45° to 46° C. (113° to 114° F.). Boudet did not analyse this acid, nor has it, as far as I am aware, engaged the attention of any other chemist. The singular nature of the compound formed merits a careful examination; so without assigning to myself the task of determining at present the reason of the transformation, I propose to examine the composition of palmine and palmic acid.

Palmine, as well as castor oil, contains glyceryle, as has previously been asserted by Bussy, Lecanu and Boudet, but the solidification of the oil is not due to the presence of this compound. In order therefore to be more assured of the purity of the acid formed, I operated principally upon the acids of castor oil. For this purpose, castor oil is saponified by caustic soda and the soap separated by common salt. The soap removed from the liquor is redissolved in pure water and again separated by salt; and this operation is repeated until the excess of caustic soda is removed. The soap is now quite white and readily soluble in hot water, from which solution the fatty acids are separated by the addition of muriatic acid. They are of a pale-olive colour, and are purified from adhering muriatic acid by repeated washings with water.

The acids thus procured are mixed with diluted nitric acid, through which is passed a stream of peroxide of nitrogen, made by the action of nitric acid on starch. The acids, which are previously fluid, soon become solid, and finally acquire the colour and consistence of pale yellow bees'-wax. This solid substance is washed with hot water till all the nitric acid is removed, the washing being performed by melting it in hot water, and drawing off the latter by a siphon. This fat is not *palmine*, but the acid contained in that substance; it differs from palmine by not containing glycerine. Thus prepared, it is

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[*] Communicated by the Chemical Society; having been read May 18, 1846.

[†] *Annales de Chimie et de Physique*, t. l. p. 411.

readily soluble in alcohol, which on evaporation yields palmic acid of a yellow colour and still very impure. It is purified by repeated crystallizations and pressure, and when quite pure has a melting-point between 113° to 114° F. There remains in the alcohol an uncrystallizable yellow oil, which becomes red on being saponified with caustic potash.

Palmic acid, procured as described, is readily soluble in alcohol, from which it crystallizes, the crystals generally being grouped together into clumps. From a fused state it solidifies into stars, consisting of small crystals radiating from a centre. The acid was burned with oxide of copper, and gave the following results:—

I. 0·074 grm. gave 0·2005 grm. carbonic acid, and 0·079 water.

II. 0·128 grm. gave 0·3455 grm. carbonic acid, and 0·1365 grm. water.

	I.	II.	Mean.
Carbon	73·89	73·61	73·75
Hydrogen . .	11·86	11·84	11·85
Oxygen . . .	14·25	14·55	14·28
	100·00	100·00	100·00

In order to determine the atomic weight of the acid, the silver salt was prepared in the following manner:—The pure acid was boiled with a strong solution of pure carbonate of potash, which it readily decomposes: on cooling, the soap separated and was removed; it was then dried and dissolved in alcohol; the solution of soap, after being separated from the carbonate of potash, was filtered and evaporated to dryness in a water-bath; it was then dissolved in warm water and precipitated by an excess of nitrate of silver, the precipitate being washed in the dark.

Thus prepared, palmate of silver is a light white powder, insoluble in alcohol and æther, but readily soluble in ammonia. The following determinations of the amount of silver give the data for estimating the atomic weight.

I. 0·994 grm. salt left on ignition 0·271 silver.
II. 0·315 grm. salt left 0·0855 grm. silver.
III. 0·3195 grm. salt left 0·0885 grm. silver.

I.	Silver =	27·26	per cent.
II.	...	27·14	...
III.	...	27·69	...
Mean . . .		27·36	...

This makes for the atomic weight of the silver salt 4937, and for that of the acid contained in it, 3486.

The silver salt burned with oxide of copper gave the following results:—

I. 0·395 grm. gave 0·751 carbonic acid, and 0·303 water.

II. 0·2590 grm. gave 0·5005 carbonic acid, and 0·192 water.

III. 0·2990 grm. gave 0·5770 carbonic acid, and 0·2190 water.

	I.	II.	III.
Carbon	51·64	52·66	52·64
Hydrogen . .	8·52	8·23	8·12
Oxygen . . .	12·48	11·75	11·88
Silver	27·36	27·36	27·36

From these data the following theoretical composition is calculated:—

		In 100 Parts.
34 Carbon	2550	52·05
32 Hydrogen . .	400	8·16
6 Oxygen . . .	600	12·26
1 Silver	1349	27·53
	4899	

Hence the theoretical composition of the anhydrous acid is,

		In 100 Parts.
34 Carbon	2550	73·91
32 Hydrogen . .	400	11·59
5 Oxygen . . .	500	14·50
	3450	

This agrees closely with the numbers found in the analysis of the acid, showing that it is anhydrous, and possesses the formula $C_{34}\,H_{32}\,O_5$.

I remarked that the acid obtained by saponifying palmine had a melting-point which never exceeded 112°, while the acid prepared directly by crystallizing the solidified fatty acids without saponification possessed a melting-point 2° higher. To ascertain whether any change took place upon saponification, a portion of pure palmic acid, procured as formerly described, was saponified by potash, liberated by hydrochloric acid, crystallized out of alcohol and analysed. Its melting-point was 111°·5 F.

I. 0·1575 grm. acid gave 0·414 carbonic acid, and 0·1635 water.

II. 0·166 grm. substance gave 0·4345 carbonic acid, and 0·171 water.

III. 0·0805 grm. substance gave 0·2115 carbonic acid, and 0·0835 water.

	I.	II.	III.
Carbon. . . .	71·68	71·26	71·65
Hydrogen . .	11·53	11·44	11·52
Oxygen . . .	16·79	17·30	16·83
	100·00	100·00	100·00

Hence the acid has altered in composition during saponification. A silver salt was now prepared in the manner previously described.

0·3195 grm. substance left on ignition 0·0885 silver.

0·1785 grm. substance, burned with oxide of copper, gave 0·3425 carbonic acid, and 0·133 water.

				In 100 Parts.
Carbon. . . .	52·33	34	2550	52·05
Hydrogen . .	8·28	32	400	8·16
Oxygen . . .	11·70	6	600	12·26
Silver	27·69	1	1349	27·53
	100·00		4899	100·00

The silver salt is therefore of the same composition as that made from the acid analysed before saponification. The only alteration is in the assumption of an atom of water during the separation from the soap, as will be seen from the following theoretical composition:—

		In 100 Parts.
34 Carbon. . . .	2550	71·57
33 Hydrogen . .	412·5	11·57
6 Oxygen . . .	600·0	16·96
	3562·5	100·00

Hence the palmic acid procured by decomposing palmate of potash differs only in its assumption of an atom of water. Its formula is $HO, C_{34} H_{32} O_5$, the water being displaced by oxide of silver during the formation of the silver salt.

Palmate of Lead.—A solution of palmate of potash poured into a solution of acetate of lead causes the precipitation of a basic salt, which was analysed, but appears to be of no definite composition.

Palmate of Barytes.—This salt was made by precipitating palmate of potash with chloride of barium, and washing the precipitate first with water, then with alcohol. It is a white powder with a soapy feeling to the touch; it is insoluble in water.

0·391 grm. of the salt yielded 0·1275 sulphate of barytes.

0·547 grm. salt, burned with chromate of lead, gave 1·164 carbonic acid, and 0·448 water.

				In 100 Parts.
Carbon	58·04	34	2550	57·86
Hydrogen . .	9·09	33	400	9·08
Oxygen . . .	11·42	5	500	11·35
Barytes . . .	21·45	1	957	21·71
	100·00		4407	100·00

Palmic Æther.—This æther was prepared by dissolving hydrated palmic acid in hot alcohol and then passing through the solution a stream of hydrochloric acid gas. Water is then added, and the æther washed with hot water. When the æther is thoroughly washed, the water is allowed to cool, and the æther solidifies into a crystalline mass which is easily removed. It fuses at 61° F., and is very soluble in hot alcohol, but sparingly so in cold. Burned with oxide of copper it gave the following results:—

I. 0·126 grm. substance gave 0·335 carbonic acid, and 0·1385 water.

II. 0·070 grm. substance gave 0·1855 carbonic acid, and 0·076 water.

III. 0·129 grm. gave 0·345 carbonic acid, and 0·141 water. The specimen analysed in this case was further purified by crystallization in alcohol.

	I.	II.	III.			In 100 Parts.
Carbon	72·51	72·27	72·93	38	2850·0	72·85
Hydrogen . .	12·22	12·05	12·13	37	462·5	11·82
Oxygen . . .	15·37	15·68	14·94	6	600·0	15·33
	100·00	100·00	100·00		3912·5	

The acid is therefore united with one equivalent of æther, its formula being $C_4 H_5 O, C_{34} H_{32} O_5$.

Palmine.—It now only remained to determine the composition of *palmine* itself. For this purpose palmine was prepared by allowing castor oil to stand in contact with dilute nitric acid containing peroxide of nitrogen until it solidified. The solid fat was purified by repeated solutions in æther, in which it is very soluble, although only sparingly so in cold alcohol. It deposits itself from the æthereal solution in opaline grains without the appearance of crystallization, and has the melting-point at 43° C. (109° F.).

I. 0·0865 grm. substance burned with oxide of copper gave 0·231 carbonic acid, and 0·089 water.

II. 0·123 grm. substance gave 0·3295 carbonic acid, and 0·128 water.

	I.	II.			In 100 Parts.
Carbon	72·84	73·06	37	2775	73·02
Hydrogen . .	11·43	11·56	34	425	11·18
Oxygen . . .	15·73	15·38	6	600	15·80
	100·00	100·00		3800	100·00

This empirical formula leads us to the following rational one:—

	C.	H.	O.
Palmic acid	34	32	5
Oxide of lipyle . . .	3	2	1
Palmine	37	34	6

Had this acid possessed one atom more hydrogen, that is, if the analyses warranted us in assuming 33 atoms of hydrogen instead of 32, the acid would have become one of the members of the margaryle series, assuming as correct the formula given to it by Redtenbacher.

Margaric acid	$C_{34}\,H_{33}\,O_3$
Palmic acid (assumed formula)	$C_{34}\,H_{33}\,O_5$
Stearic acid	$2(C_{34}\,H_{33})\,O_5$

But the analyses do not warrant us in this assumption; and we are therefore compelled to adopt $C_{34}\,H_{32}\,O_5$ as the formula of palmic acid.

Now Berzelius, Dumas and Mulder argue that margaryle, the radical of margaric and stearic acids, possesses another equivalent of hydrogen, being in fact $C_{34}\,H_{34}$. In this case, palmic acid connects itself with margaric acid in a very interesting way. Palmic acid, $C_{34}\,H_{32}\,O_5$, may in fact be viewed as margaric acid, $C_{34}\,H_{34}\,O_3$, in which two atoms of hydrogen have become replaced by two atoms of oxygen.

Margaric acid . . .	$C_{34}\,H_{32}\,H_2\,O_3$
Palmic acid	$C_{34}\,H_{32}\,O_2\,O_3$

I previously pointed out a similar case of substitution in œnanthic and myristic acids*.

Myristic acid . . .	$C_{28}\,H_{27}\,O_3$
Œnanthic acid . . .	$C_{28}\,H_{26}\,O_4$

The substitution in this case of an equivalent of oxygen for one of hydrogen is exactly the same as with palmic and margaric acids, and seems to show that such relations are not merely fortuitous.

LXVIII. *Researches upon Cumarine.*

By HERMANN BLEIBTREU†.

THE attention of chemists has long been attracted to the *Asperula odorata*, a plant well-known in Germany under the name of 'Waldmeister,' ('Woodruff') and which in many districts, particularly on the Rhine, is employed in the preparation of the favourite 'Maitrank' or 'Maiwein.'

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* *Annalen der Chemie*, vol. xxxvii. p. 155.

† Communicated by the Chemical Society; having been read May 4, 1846.

Many experiments have been made for the purpose of separating and investigating the volatile substance contained in the 'Woodruff,' to which the 'Maiwein' owed its agreeable odour and flavour.

Many years ago Voget * occupied himself with this subject, and obtained from the 'Woodruff' a crystalline substance, which he described as possessing the same odour as the Tonka bean and *Melilotus*, and which his experiments proved to be benzoic acid.

More lately Kosmann † has published some experiments, which led him to the conclusion that the principle contained in this plant was cumarine, the stearoptine of Tonka beans. By treating the dried plant with alcohol and æther, driving off these solvents and boiling the syrupy residue in water, and evaporating, he obtained a crystalline mass, whose agreeable odour and piquant taste were exactly similar to cumarine prepared from the Tonka beans.

The experiments of Kosmann were however purely qualitative. No analysis was made of the crystals obtained from the Woodruff, which alone could determine the point; nor was the experiment tried as to the ready transformation of cumarine into salicylic acid, so easily recognised by its action with the salts of the sesquioxide of iron, and from which a tolerable conclusion could have been drawn.

It hence appeared to me desirable to establish by a more exact investigation the observations of Kosmann.

The following experiments instituted for this purpose, and including a revision of the investigation of cumarine by Delalande, were commenced in the laboratory of my friend Professor A. W. Hofmann in Bonn, and finished in that of the Royal College of Chemistry in London, to which I accompanied him in the capacity of assistant on his election as Professor to the latter.

I have much pleasure in embracing this opportunity to express my thanks to Dr. Hofmann for his instructions in organic investigations generally, and more particularly for his assistance in the present memoir.

Investigation of the Crystals from Asperula odorata.

For the preparation of this substance I proceeded in the following manner:—

A quantity of Woodruff, collected shortly before and

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* *Generalbericht des König. Preus. Rhein. Medicinalcol.* Uber das Jahr 1835. Coblenz bei Kehr, page 240.

† *Journ. de Pharm.*, 3 ser. t. v. p. 393. *Ann. der Chem. u. Pharm.*, vol. lii. p. 387.

during flowering, was dried in the air and digested for some time with spirits of wine; the alcohol was distilled from the dark greenish-brown solution in a water-bath, and the dark brown residue evaporated to the consistency of syrup. The distilled alcohol had an aromatic odour, and from the last more watery portion small crystalline plates were gradually deposited.

The syrup, boiled with water, and the brown solution separated by filtration from the insoluble matters, consisting of chlorophylle, &c., were agitated with æther. On distilling off the æther there remained a yellow substance, possessing the appearance and odour of honey, from which, after some time, a considerable quantity of yellow crystalline needles separated, which on heating with water to 50° C. collected at the bottom of the vessel into a melted mass, and were dissolved on raising the temperature to the boiling-point. From this solution, which on cooling became milky, crystals of an inch in length were gradually deposited, which were purified by repeated crystallizations. After being thrice recrystallized the crystals were of a pure white colour, but deposited some brownish flakes after melting at a gentle heat. On repeating the recrystallizations a few times more, the substance remained quite unchanged in colour after fusing, and could now be considered as quite pure and proper for analysis.

Before proceeding to the analysis I endeavoured to convert a small portion of the substance into salicylic acid. A few crystals were dissolved in strong potassa ley, evaporated till the yellow colour at first produced had disappeared, and then heated somewhat stronger. The solid residue was saturated with hydrochloric acid and agitated with æther. The æthereal solution, evaporated to drive off the æther, gave with sesquichloride of iron the beautiful violet colour so characteristic of salicylic acid.

Analysis of the Crystals from Asperula odorata.

The analysis of the fused crystals, in one case with oxide of copper, in the other with chromate of lead, gave the following results:—

I. 0·3276 grm. gave 0·8821 carbonic acid and 0·1267 water.

II. 0·2607 grm. substance gave 0·7105 carbonic acid and 0·1043 water.

From which the following per-centage is calculated:—

	I.	II.
Carbon . . .	73·63	74·33
Hydrogen . .	4·31	4·44

According to the experiments of Delalande the formula of cumarine is $C_{18} H_7 O_4$, giving the following per-centage, which I compare with the mean of his analyses calculated with the new atomic weight of carbon:—

	Theory.		Mean of Delalande's experiments.
C_{18} =	1350·00	73·47	72·91
H_7 =	87·50	4·76	4·73
O_4 =	400·00	21·77	
	1837·50	100·00	

The results of my analyses approach near enough to those of Delalande to prove the identity of cumarine and the substance obtained from the *Asperula odorata.* A comparison however of the amount of carbon and hydrogen shows, particularly in the former, a sufficient deviation to throw a doubt on the absolute number of equivalents. The circumstance likewise of the near agreement, nay even of the excess of the quantity of hydrogen calculated over that obtained in the mean of Delalande's experiments, whilst in analysis the reverse is always the case, gave me grounds to doubt the correctness of his formula, and induced me to repeat the investigation of cumarine obtained from the Tonka beans.

Preparation of Cumarine from Tonka Beans.

The following appeared to be the best method of preparing cumarine from the Tonka beans.

The beans, finely cut or pounded, were digested for some time with strong alcohol. The syrupy mass remaining after distilling off the alcohol, becomes on cooling for the most part a solid magma of crystals of cumarine, which can be easily purified by treatment with animal charcoal and repeated crystallizations. By proceeding in this manner it is more easy to separate a fatty oil, which is contained in considerable quantity in the Tonka bean. This body is colourless, and without any characteristic odour, soluble in æther, insoluble in alcohol and water, and possesses the property of dissolving cumarine in considerable quantity.

The process of treating the beans with æther, or directly extracting the cumarine by boiling in water, is much inferior, as the greater part of the oil is also removed with it, and can afterwards only be separated with difficulty. In the fresh bean the cumarine appears to be contained in solution in this oil, from which it gradually separates in the form of pure white crystals, and is found collected particularly between the cotyledons and on their surfaces under the skin.

Composition of Cumarine.

Two combustions of cumarine from the Tonka beans gave the following results :—

I. 0·4870 grm. substance gave 1·3209 carbonic acid and 0·1882 water.

II. 0·6423 grm. substance gave 1·7361 carbonic acid and 0·2416 water.

These numbers correspond in per-centage to

	I.	II.
Carbon . . .	73·97	73·72
Hydrogen . .	4·29	4·18

These two analyses correspond exactly with that of the cumarine obtained from the *Asperula*. In all there is a larger amount of carbon and a smaller quantity of hydrogen than in the analyses of Delalande.

The expression corresponding to the mean of my experiments is $C_{18}H_6O_4$, which contains 1 equivalent of hydrogen less than in Delalande's formula, as is seen in the following scheme, in which the experimental and theoretical numbers are compared.

	Theory.		Bleibtreu.	Delalande.
18 Carbon .	1350	73·97	73·91	72·91
6 Hydrogen	75	4·11	4·30	4·73
4 Oxygen .	400	21·97		
	1837	100·00		

These experiments leave no doubt as to the identity of the crystalline principle of the *Asperula* and the stearoptine of the Tonka bean.

The delightful aroma which characterizes 'Maiwein' is entirely due to this substance. A liquor artificially prepared with the stearoptine of the Tonka bean met with the warmest approbation from a number of the younger teachers in the University of Bonn.

Products of Decomposition of Cumarine.

The new formula for cumarine I endeavoured to control by investigating its products of decomposition.

Delalande obtained on heating cumarine with concentrated potassa a peculiar acid, $HO, C_{18}H_7O_5$, which he described under the name of cumaric acid. According to him, by the continued action of the potassa a further transformation of the latter is effected into that remarkable acid first obtained by Piria as a product of the decomposition of salicine, and to which the attention of chemists has more lately been in the most lively manner attracted by the investigations of Cahours on the "Oil of the *Gualtheria procumbens.*"

Delalande expresses these transformations by the following formula:—

$$\underbrace{C_{18}H_7O_4+2HO}_{\text{Cumarine.}} = \underbrace{HO,C_{18}H_7O_5+H}_{\text{Cumaric acid.}}$$

$$\underbrace{HO,C_{18}H_7O_5}_{\text{Cumaric acid.}} = \underbrace{HO,C_{14}H_5O_5}_{\text{Salicylic acid.}} + C_4H_2.$$

On repeating the experiments of Delalande I arrived at the following results.

Cumarine dissolves readily in potassa and forms with it a yellowish solution, from which acids precipitate cumarine again unchanged. It is only attacked by heating with concentrated ley, to which pieces of solid caustic potassa have been added. From this solution hydrochloric acid precipitates a substance totally differing in its character from cumarine, and which is evidently the acid described by Delalande.

By careful treatment of cumarine however by potassa, I have never been able to observe a disengagement of hydrogen.

The acid prepared in the above manner may contain traces of salicylic acid. After removal of the chloride of potassium and the excess of hydrochloric acid, and dissolving in water, it gave indeed, with salts of the sesquioxide of iron, the violet colour so characteristic of salicylic acid. Delalande however states expressly in his memoir, that this reaction is not only characteristic of salicylic but also of cumaric acid. This remark is evidently erroneous, for on washing with water the acid once crystallized, this reaction became weaker and weaker, and finally totally disappeared on a second crystallization. The violet colour produced by the sesquichloride of iron was consequently due to the presence of a small quantity of salicylic acid, the formation of which cannot easily be prevented.

The acid prepared in the above manner, and freed from salicylic acid as much as possible by repeated recrystallizations, may still contain traces of undecomposed cumarine, from which it must be separated; for this purpose it was dissolved in ammonia. From the yellow solution the excess of ammonia was expelled by boiling, and the yellowish-white silver salt produced by the addition of nitrate of silver was collected on a filter and washed with water, and then with alcohol, and finally æther. After such a treatment the silver salt cannot contain any trace of cumarine, and the acid now separated by hydrochloric acid, extracted by æther, and recrystallized from boiling water after distilling off the æther, may be regarded as *pure cumaric acid.*

With salts of sesquioxide of iron it now produces no colour whatever, and possesses the following properties. It is readily soluble in alcohol and æther, and crystallizes from boiling water in white brittle crystals. It possesses a decidedly acid reaction, and expels carbonic acid from its salts. Its point of fusion is near 190° C. If more strongly heated, it is partly decomposed into white shining crystals, which sublime, and a brownish residue.

The cumarate of ammonia is not precipitated by a salt of baryta; but a white powdery insoluble precipitate is obtained by the addition of acetate of lead.

A combustion with chromate of lead of cumaric acid dried at 100° C. gave the following results:—

0·3415 grm. of substance gave 0·8215 carbonic acid and 0·1532 water.

In per-centage,—

Carbon	65·61
Hydrogen	4·98

These numbers correspond exactly to the formula of Delalande for this acid, HO, $C_{18}H_7O_5$.

The following are the experimental and theoretical numbers:—

	Theory.		Bleibtreu.	Delalande.
18 Carbon .	1350·00	65·85	65·61	65·34
8 Hydrogen	100·00	4·88	4·98	5·05
6 Oxygen .	600·00	29·27		
	2050·00	100·00		

To control this formula I also analysed the same silver salt as Delalande. It was obtained, as above stated, by dissolving cumaric acid in ammonia and precipitating with nitrate of silver. There appeared however to exist two compounds of cumaric acid with oxide of silver; for on precipitating a solution of the acid in excess of ammonia a flocculent orange-yellow precipitate was obtained, whilst that from the neutral salt was light yellow, almost white, quickly settling to the bottom of the vessel. The two compounds are further essentially distinguished by the orange-coloured substance becoming quickly brown on exposure to the air, and being immediately decomposed on heating to 100° C., whilst under the same circumstances the lighter remains quite unchanged. In the following analyses the latter compound was employed.

I. 0·5850 grm. cumarate of silver gave 0·8580 carbonic acid and 0·1392 water.

In per-centage,—

Carbon	40·00
Hydrogen	2·64

II. 0·8237 grm. of the salt gave 0·3287 silver.
III. 0·5181 grm. salt gave 0·2049 silver.
In per-centage,—

II. Silver	39·90
III. Silver	39·55
Mean number .	39·72

These results correspond exactly with those of Delalande and give the following formula, $AgO, C_{18} H_7 O_5$. The calculated and experimental numbers are as follows:—

	Theory.		Experimental.	
			Bleibtreu.	Delalande.
18 C .	1350·00	39·86	40·00	39·82
7 H .	87·50	2·58	2·64	2·62
6 O .	600·00	17·72		
1 Ag .	1349·00	39·84	39·72	40·00
	3386·51	100·00		

Formation of Cumaric and Salicylic Acid from Cumarine.

From the above experiments it is evident that the formula given by Delalande to cumaric acid must be regarded as the correct one. The representation however of the formation of this acid, to which this chemist was led by the incorrect formula of cumarine, requires correction. The formation of cumaric acid depends upon no decomposition of water, the oxygen of which is added to the elements of cumarine, but simply on the assumption of 2 equivalents of this body, exactly analogous to the formation of benzilic acid from benzile, or isatinic acid from isatine.

$$\underbrace{C_{28} H_{10} O_4 + 2HO}_{\text{Benzile.}} = \underbrace{HO, C_{28} H_{11} O_5}_{\text{Benzilic acid.}}$$

$$\underbrace{C_{16} H_5 NO_4 + 2HO}_{\text{Isatine.}} = \underbrace{HO, C_{16} H_6 NO_5}_{\text{Isatinic acid.}}$$

$$\underbrace{C_{18} H_6 O_4}_{\text{Cumarine.}} + 2HO = \underbrace{HO, C_{18} H_7 O_5}_{\text{Cumaric acid.}}$$

It is not requisite even to fuse the cumarine with caustic potash: by the employment of an alcoholic solution of potassa it is also transformed, though very slowly, into cumaric acid. There however in this latter case always remains, even after long treatment, a large portion of cumarine undecomposed.

The evolution of hydrogen mentioned by Delalande, and which, as already stated, I never observed in a well-conducted experiment, arose evidently from the formation of salicylic acid by the further action of the potassa on the cumaric acid. This view is supported by the violet-coloured reaction of sesquichloride of iron described by him, and which, as I have shown, does not belong to cumaric acid. His cumaric acid evidently contained a small quantity of salicylic acid.

From the aforesaid grounds, the view taken by Delalande on the transformation of cumaric into salicylic acid must also be modified. According to him, no evolution of hydrogen takes place in this formation, but an aromatic gas is disengaged burning with a coloured flame, and which he regarded as a carbo-hydrogen $C_4 H_2$, representing the difference of constitution between the two acids—

$$\underbrace{HO, C_{18} H_7 O_5}_{\text{Hydrated cumaric acid.}} - \underbrace{C_4 H_2}_{\text{Aromatic gas.}} = \underbrace{HO, C_{14} H_5 O_5}_{\text{Hydrated salicylic acid.}}.$$

This manner of viewing the reaction is evidently incorrect. If pure cumarine is fused with hydrate of potassa, the latter is almost entirely transformed into carbonate after the formation of the salicylic acid. The carbon therefore separates in the form of an oxygenated compound.

I have made no quantitative experiment on this point; but all my observations tend to prove that the formation of the salicylic acid may be represented by the following equations:—

$$\underbrace{HO, C_{18} H_7 O_5}_{\text{Cumaric acid.}} + 8HO = \underbrace{HO, C_{14} H_5 O_5 + 4CO_2}_{\text{Salicylic acid.}} + 10H.$$

As to the aromatic gas burning with a brilliant flame, which is evolved in the latter stages, it is no carbo-hydrogen, but the oxide of a carbo-hydrogen, viz. hydrate of phenyle, arising from the further loss of carbonic acid from the salicylic acid already formed,—

$$\underbrace{HO, C_{14} H_5 O_5}_{\text{Salicylic acid.}} - 2CO_2 = \underbrace{HO, C_{12} H_5 O}_{\text{Hydrate of phenyle.}}.$$

Nitrocumarine.

Delalande has studied the action of nitric acid on cumarine. The correction of the formula of the latter body made it desirable to repeat this experiment also, as it was probable that a corresponding change would likewise require to be made in that of the product of decomposition.

Cumarine dissolves without almost any evolution of red fumes in cold fuming nitric acid, and with considerable elevation of temperature. On addition of water a white cheesy precipitate is obtained, which quickly settles to the bottom. The fluid possesses a deep yellow colour. After washing with cold water, the precipitate was dissolved in boiling alcohol, from which, on cooling, white shining silky crystals separated. In this treatment with nitric acid a strong heat is to be particularly avoided, as the whole of the cumarine is converted into nitropicric acid if the fluid is allowed to boil. The formation of a small quantity of the latter acid can never be completely avoided, and from this cause arises the yellow colour of the mother-liquor.

To the above product of decomposition Delalande gave the name nitrocumarine, and the following formula derived from his view of the composition of cumarine:—

$$C_{18}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}O_4.$$

It is scarcely necessary to remark that this formula must be changed into

$$C_{18}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}O_4,$$

as is shown by the analysis of the fused crystals with oxide of copper.

0·5675 grm. of substance gave 1·1657 carbonic acid and 0·1348 water.

In per-centage,—

Carbon	56·02
Hydrogen	2·64

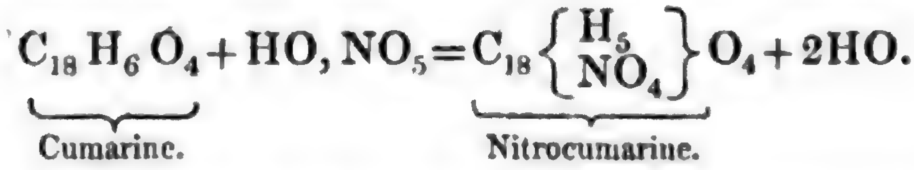
which, compared with the per-centage calculated from the new formula, is

	Theory.		Experiment.
18 Carbon .	1350·00	56·54	56·02
5 Hydrogen	62·50	2·62	2·64
1 Nitrogen .	175·25	7·34	
8 Oxygen .	800·00	33·50	
	2387·75	100·00	

Delalande's formula requires the following per-centage:—

Carbon	56·24
Hydrogen	3·12

The determination of the hydrogen, on which the formula more directly depended, leaves no doubt as to the choice of the two. The formation of nitrocumarine is analogous to that of nitrobenzole, nitrostyrole, &c.:—



$$\underbrace{C_{18}H_6O_4}_{\text{Cumarine.}} + HO, NO_5 = \underbrace{C_{18}\left\{\begin{matrix}H_5\\ NO_4\end{matrix}\right\}}_{\text{Nitrocumarine.}} O_4 + 2HO.$$

I have but little to add to Delalande's description of the properties of this body. Nitrocumarine dissolves with difficulty in water, alcohol and æther, and crystallizes in fine needles from these solvents as soon as the temperature falls a little below their boiling-point. Its point of fusion is about 170° C.; heated beyond this it sublimes without decomposition in the form of large white crystals of the lustre of mother-of-pearl.

Nitrocumarine forms with alkalies a solution of a deep yellow, almost red colour, which disappears on the addition of an acid, with separation at the same time of a white crystalline matter. This reaction was further examined in the hopes of obtaining an acid corresponding to nitrocumarine, viz. the nitrocumaric acid.

For this purpose nitrocumarine was boiled for some time in an alcoholic solution of potassa; the potassa compound formed, decomposed by hydrochloric acid, and the precipitate after removal by washing of the chloride of potassium, recrystallized from boiling alcohol.

To free the crystals (which I regarded as nitrocumaric acid) from any nitrocumarine which might still be mixed with them, they were dissolved in ammonia, and on boiling, to remove any excess of the latter, a portion of the substance was deposited in the form of white crystalline flakes. That these crystals were only cumarine was proved by the absence of any trace of ammonia on heating with potassa.

The filtered solution, which was neutral, gave with nitrate of silver a beautiful orange-yellow colour, with acetate of lead an orange precipitate. Both compounds were soluble in a considerable quantity of water, forming a lightish yellow solution, but less so in alcohol and æther; so that they could be purified by boiling in these menstrua. I have analysed these compounds as well as the substance separated from them by acids.

The lead and silver precipitates are direct compounds of nitrocumarine with the oxides of the metals, and the electronegative bodies obtained from them is *pure nitrocumarine.*

The combustion of this substance with oxide of copper gave the following results:—

0·2567 grm. of substance gave 0·5338 carbonic acid and 0·0672 water.

In per-centage,—

Carbon	56·71
Hydrogen . . .	2·90

Nitrocumarine, $C_{18}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}O_4$, contains, as before stated,

Carbon	56·54
Hydrogen . . .	2·62

whilst nitrocumaric acid, HO, $C_{18}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}O_5$, corresponding to cumaric acid, ought to contain the following widely differing per-centage,—

Carbon	51·67
Hydrogen . . .	3·35

The lead compound was dried at 100°, decomposed by nitric, and precipitated by dilute sulphuric acid.

0·6557 grm. substance gave 0·5548 sulphate of lead.

In per-centage,—oxide of lead 62·27, which would give a formula approaching nearly to

$$3PbO, C_{18}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}O_4,$$

of which the calculated per-centage of lead is 63·66.

The silver compound being decomposed by the temperature of the water-bath was dried *in vacuo.*

A determination of the silver made by burning the compound, in which however a slight loss was incurred through deflagration, gave in 0·3037 grm. of substance 0·1526 silver.

In per-centage,—oxide of silver 53·97, which corresponds to a compound for nitrocumarine and 2 equivalents of oxide of silver,

$$2AgO, C_{18}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}O_4,$$

of which the per-centage of oxide of silver calculated would be 54·83.

From these experiments it is evident that nitrocumarine undergoes no decomposition on boiling in an alcoholic solution of potassa. The same lead and silver compounds are also obtained by adding a lead or silver salt to nitrocumarine simply dissolved in ammonia. Further experiments must decide whether nitrocumaric acid can be obtained by careful fusion with caustic alkalies, or directly from cumaric acid, by proper treatment with nitric acid.

The formation of this acid would have been of some interest. The late investigations of Zinin have shown us that acids of this class, under the influence of reducing agents, as hydrosulphuric acid, &c., exchange 4 equivalents of oxygen for 2 of hydrogen, or, to express it otherwise, 1 equivalent of hyponitric acid for 1 of amidogen.

Whilst bases are thus formed from nitro-carbohydrogens, the electro-negative character of the nitro-acids remains unchanged. Benzoic acid, for example, treated in the above manner, gives the following series:—

Benzoic acid . .	$HO, C_{14} H_5 O_3$
Nitrobenzoic acid .	$HO, C_{14}\left\{\begin{matrix} H_4 \\ NO_4 \end{matrix}\right\}O_3$
Amidobenzoic acid Benzamidic acid	$HO, C_{14}\left\{\begin{matrix} H_4 \\ Ad \end{matrix}\right\}O_3$

Were it possible to form from cumaric acid a similar series:

Cumaric acid . .	$HO, C_{18} H_7 O_5$
Nitrocumaric acid .	$HO, C_{18}\left\{\begin{matrix} H_6 \\ NO_4 \end{matrix}\right\}O_5$
Amidocumaric acid	$HO, C_{18}\left\{\begin{matrix} H_6 \\ Ad \end{matrix}\right\}O_5.$

it gave a hope of producing artificially hippuric acid, for amidocumaric acid is, according to its formula, nothing but hippuric acid:

$$\underbrace{HO, C_{18}\left\{\begin{matrix} H_6 \\ Ad \end{matrix}\right\}O_5}_{\text{Amidocumaric acid.}} = \underbrace{HO, C_{18} H_8 NO_5.}_{\text{Hippuric acid.}}$$

Whether or not such a relation exists must be decided by further experiments, which I intend still to make in this direction. In the following I will briefly state some experiments made partly with the same intention, and which as yet have not led to decided results.

It was not impossible that by the direct action of reducing agents on nitrocumarine an amidocumarine could have been formed, which by treatment with alkalies might have been converted into the desired acid. For this purpose a quantity of nitrocumarine was mixed with alcohol, into which ammoniacal gas was passed till the whole was dissolved, and then submitted for a considerable time to a current of hydrosulphuric acid.[*]

The fluid, from which, even after several days, no sulphur separated, was distilled in a retort till no more sulphide of ammonia passed over. On exposure to the air in a flat vessel it now deposited a yellow, slimy, tenacious mass, the characteristic garlic smell of which led to the suspicion of its containing sulphur.

By longer exposure to the air the slimy mass assumed the consistence of wax, and finally hardened into a resinous-like body, insoluble in water, but tolerably soluble in alcohol and æther.

Ammonia also dissolves this substance with separation of a little sulphur. On boiling this solution a portion of the substance is again deposited as the ammonia is driven off.

The filtered solution, which is nearly neutral, gave on the addition of hydrochloric acid a brown-coloured amorphous precipitate, and with lead and silver salts light yellow compounds.

Want of substance has as yet prevented me from making a thorough investigation of these bodies. I will however here only add, that the yellow lead compound gave 34·21 per cent. of lead, and that the organic substance separated from it contained 9·55 per cent. of sulphur.

Cumarine in Anthoxanthum odoratum.

The detection of cumarine in the *Asperula odorata* led to the suspicion that this principle is probably also contained in other plants in which benzoic acid has been hitherto supposed to have been found.

Some time ago Guillemette [*] showed that the crystalline principle of the *Melilotus officinalis*, which was formerly believed to be benzoic acid, was identical with cumarine.

This observation was completely confirmed by some experiments made by me with the crystalline principle of this plant, particularly by completely transforming it into salicylic acid.

Of the other plants said to contain benzoic acid I have as yet been able to obtain only *Anthoxanthum odoratum*, a grass to which is attributed the agreeable smell of new hay, and which, under the name of 'Sweet Vernal Grass,' is to be found in most meadows.

To detect the cumarine I proceeded in the same manner as with the *Asperula odorata*, and obtained white crystals, which in odour and taste, as well as in all other properties, were not

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[*] *Ann. der Chem. u. Pharm.*, vol. xiv. p. 324. *Journ. de Pharm.*, 1835, p. 172.

to be distinguished from cumarine. On fusing these crystals with hydrate of potassa and testing the product of decomposition with sesquichloride of iron, the intense violet colour produced by the salicylic acid which had been formed showed with certainty the presence of cumarine in the original substance.

The hippuric acid found in the urine of graminivorous animals has in various ways been supposed to be derived from the benzoic acid imagined to exist in the *Anthoxanthum odoratum.* The transformation experienced by that acid in the animal œconomy appeared in the highest degree to give support to this view.

It would be desirable to try if the same change takes place with cumarine. The narcotic action of this substance on the animal organism however prevented the making of any direct experiments.

LXIX. *On the Integration and Transformation of certain Differential Equations. By the* Rev. Brice Bronwin.*

I HAVE lately discovered a very simple method of integrating and transforming certain differential equations, which I believe to be quite new. A similar method may be applied to the solution of equations in finite differences. Brief illustrations of it have been transmitted to the Mathematician and the Cambridge Mathematical Journal, but not yet published. The present paper contains an extension of the method as applied to differential equations. The following subsidiary formulæ will be required to facilitate the reductions. And it must be observed that D always stands for $\frac{d}{dx}$, and operates upon the whole of the quantity which follows it.

$$\left.\begin{aligned}
&x\,\mathrm{D}^n y = \mathrm{D}^n x y - n\,\mathrm{D}^{n-1} y\\
&x^2\,\mathrm{D}^n y = \mathrm{D}^n x^2 y - 2n\,\mathrm{D}^{n-1} x y + n(n-1)\,\mathrm{D}^{n-2} y\\
&x^3\,\mathrm{D}^n y = \mathrm{D}^n x^3 y - 3n\,\mathrm{D}^{n-1} x^2 y + 3n(n-1)\,\mathrm{D}^{n-2} x y - n\\
&\qquad\qquad (n-1)(n-2)\,\mathrm{D}^{n-3} y
\end{aligned}\right\}. \qquad (a.)$$

These will be verified by actually performing the differentiations.

$$\left.\begin{aligned}
&x\,\mathrm{D}^{-n} y = \mathrm{D}^{-n} x y + n\,\mathrm{D}^{-n-1} y\\
&x^2\,\mathrm{D}^{-n} y = \mathrm{D}^{-n} x^2 y + 2n\,\mathrm{D}^{-n-1} x y + n(n+1)\,\mathrm{D}^{-n-2} y
\end{aligned}\right\} \qquad (b.)$$

&c., which may be verified by successive differentiation.

We now proceed to give some examples.

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* Communicated by the Author.

Let $$x\left(\frac{d^2 y}{d x^2}+k^2 y\right)+2p\frac{d y}{d x}=0,$$

where p is a positive integer. Make

$$y=\mathrm{D}^2 u+k^2 u=(\mathrm{D}^2+k^2)\,u.$$

Then, reducing by (*a.*), we have

$$x\frac{d^2 y}{d x^2}=x\,\mathrm{D}^4 u+k^2 x\mathrm{D}^2 u=\mathrm{D}^4 x u-4\mathrm{D}^3 u+k^2\mathrm{D}^2 x u-2k^2\mathrm{D}u$$
$$=(\mathrm{D}^2+k^2)\,(\mathrm{D}^2 x\,u-4\,\mathrm{D}\,u)+2\,k^2\,\mathrm{D}\,u.$$

In like manner

$$2p\frac{d y}{d x}=2p\,\mathrm{D}^3 u+2p\,k^2\,\mathrm{D}\,u=(\mathrm{D}^2+k^2)\,2p\,\mathrm{D}\,u.$$

Also

$$k^2 x\,y=k^2 x\,\mathrm{D}^2 u+k^4 x u=k^2\,\mathrm{D}^2 x\,u-2\,k^2\,\mathrm{D}\,u+k^4\,x\,u$$
$$=(\mathrm{D}^2+k^2)\,k^2\,x\,u-2\,k^2\,\mathrm{D}\,u.$$

Substituting these values in the given equation, it becomes

$$(\mathrm{D}^2+k^2)\,\{\mathrm{D}^2\,x\,u+(2\,p-4)\,\mathrm{D}\,u+k^2\,x\,u\}=0,$$

or $$\mathrm{D}^2\,x\,u+(2\,p-4)\mathrm{D}\,u+k^2\,x\,u=0,$$

which is equivalent to

$$x\left(\frac{d^2 u}{d x^2}+k^2 u\right)+(2p-2)\frac{d u}{d x}=0,$$

an equation similar to the given one, p being changed into $p-1$. Hence, by continuing to repeat the same operations, the last term may be taken away. If therefore $y=(\mathrm{D}^2+k^2)^p\,u$, we shall have $\frac{d^2 u}{d x^2}+k^2 u=0$, the integral of which is known.

Again, let $$\frac{d^2 y}{d x^2}+k\,x\frac{d y}{d x}-p\,k\,y=0.$$

Here make $y=\mathrm{D}\,u+k\,x\,u=(\mathrm{D}+k\,x)\,u$. As before, reducing by (*a.*), we have

$$\frac{d^2 y}{d x^2}=\mathrm{D}^3\,u+k\,\mathrm{D}^2 x u=\mathrm{D}^3\,u+k\,x\,\mathrm{D}^2 u+2\,k\,\mathrm{D}\,u=(\mathrm{D}+k\,x)\mathrm{D}^2 u$$
$$+\,2\,k\,\mathrm{D}\,u.$$

And

$$k\,x\frac{d y}{d x}=k\,x\,\mathrm{D}^2\,u+k^2\,x\mathrm{D}\,x\,u=k\,\mathrm{D}^2\,x\,u-2\,k\,\mathrm{D}u+k^2\,x\,\mathrm{D}x u$$
$$=(\mathrm{D}+k\,x)\,k\,\mathrm{D}\,x\,u-2\,k\,\mathrm{D}\,u.$$

Substituting these values in the given equation, it becomes

$$(\mathrm{D}+k\,x)\,(\mathrm{D}^2 u+k\,\mathrm{D}\,x\,u-p\,k\,u)=0.$$

Or $$\mathrm{D}^2 u+k\,\mathrm{D}\,x\,u-p\,k\,u=0,$$

which is equivalent to

$$\frac{d^2 u}{d x^2} + k x \frac{d u}{d x} - (p-1) k u = 0.$$

This is similar to the proposed, p being diminished of unity. By p operations, therefore, the last term will be taken away. Hence if

$$y = (\mathrm{D} + k x)^p u,$$

then

$$\frac{d^2 u}{d x^2} + k x \frac{d u}{d x} = 0,$$

an integrable equation.

To take a third example, let

$$x \left(\frac{d^3 y}{d x^3} + k^3 y \right) + 3 p \frac{d^2 y}{d x^2} = 0.$$

Make $\quad y = \mathrm{D}^3 u + k^3 u = (\mathrm{D}^3 + k^3) u.$

We have

$$x \frac{d^3 y}{d x^3} = x \mathrm{D}^6 u + k^3 x \mathrm{D}^3 u = \mathrm{D}^6 x u - 6 \mathrm{D}^5 u + k^3 \mathrm{D}^3 x u - 3 k^3 \mathrm{D}^2 u$$

$$= (\mathrm{D}^3 + k^3)(\mathrm{D}^3 x u - 6 \mathrm{D}^2 u) + 3 k^3 \mathrm{D}^2 u,$$

and

$$k^3 x y = k^3 x \mathrm{D}^3 u + k^6 x u = k^3 \mathrm{D}^3 x u - 3 k^3 \mathrm{D}^2 u + k^6 x u$$

$$= (\mathrm{D}^3 + k^3) k^3 x u - 3 k^3 \mathrm{D}^2 u.$$

With these values the given equation becomes

$$(\mathrm{D}^3 + k^3) \{ \mathrm{D}^3 x u + (3 p - 6) \mathrm{D}^2 u + k^3 x u \} = 0;$$

or $\quad \mathrm{D}^3 x u + (3 p - 6) \mathrm{D}^2 u + k^3 x u = 0;$

or $\quad x \left(\frac{d^3 u}{d x^3} + k^3 u \right) + 3 (p - 1) \frac{d^2 u}{d x^2} = 0.$

Therefore if $y = (\mathrm{D}^3 + k^3)^p u$, the proposed reduces to

$$\frac{d^3 u}{d x^3} + k^3 u = 0,$$

which we know how to integrate.

Next, let $\quad x \frac{d^2 y}{d x^2} + (2 p + 2 m x) \left(\frac{d y}{d x} + m y \right) = 0.$

Here we make $\quad y = (\mathrm{D}^2 + 2 m \mathrm{D} + 2 m^2) u.$

Then $\quad x \frac{d^2 y}{d x^2} = x \mathrm{D}^4 u + 2 m x \mathrm{D}^3 u + 2 m^2 x \mathrm{D}^2 u$

$$= \mathrm{D}^4 x u - 4 \mathrm{D}^3 u + 2 m \mathrm{D}^3 x u - 6 m \mathrm{D}^2 u + 2 m^2 \mathrm{D}^2 x u - 4 m^2 \mathrm{D} u$$

$$= (\mathrm{D}^2 + 2 m \mathrm{D} + 2 m^2)(\mathrm{D}^2 x u - 4 \mathrm{D} u) + 2 m \mathrm{D}^2 u + 4 m^2 \mathrm{D} u$$

$$2 m x \frac{d y}{d x} = 2 m x \mathrm{D}^3 u + 4 m^2 x \mathrm{D}^2 u + 4 m^3 x \mathrm{D} u$$

$$= 2 m \mathrm{D}^3 x u - 6 m \mathrm{D}^2 u + 4 m^2 \mathrm{D}^2 x u - 8 m^2 \mathrm{D} u + 4 m^3 \mathrm{D} x u - 4 m^3 u$$

$$= (\mathrm{D}^2 + 2 m \mathrm{D} + 2 m^2)(2 m \mathrm{D} x u - 6 m u) + 4 m^2 \mathrm{D} u + 8 m^3 u$$

$$2\,m^2\,x\,y = 2\,m^2\,x\,D^2\,u + 4\,m^3\,x\,D\,u + 4\,m^4\,x\,u$$
$$= (D^2 + 2\,m\,D + 2\,m^2)\;2\,m^2\,x\,u - 4\,m^2\,D\,u - 4\,m^3\,u.$$

Substituting these values in the given equation, and dividing the result by $D^2 + 2\,m\,D + 2\,m^2$, a factor common to all the terms, we have

$$D^2\,x\,u + 2\,m\,D\,x\,u + (2\,p-4)\,D\,u + 2\,m^2\,x\,u + (2\,p-4)\,m\,u = 0;$$

or
$$x\frac{d^2u}{d\,x^2} + (2\,p - 2 + 2\,m\,x)\left(\frac{d\,u}{d\,x} + m\,u\right) = 0,$$

which is similar to the given equation, p being changed into $p-1$. If therefore

$$y = (D^2 + 2\,m\,D + 2\,m^2)^p\,u,$$

we shall have for the determination of u the integrable equation

$$\frac{d^2u}{d\,x^2} + 2\,m\frac{d\,u}{d\,x} + 2\,m^2 u = 0.$$

We will now apply the transformation $y = (D+m)\,u$ to a few examples at once. This gives

$$x\frac{d^2y}{d\,x^2} = x\,D^3\,u + m\,x\,D^2 u = D^3\,x\,u - 3\,D^2 u + m\,D^2\,x\,u - 2m\,D u$$
$$= (D+m)(D^2\,x\,u - 3\,D\,u) + m\,D\,u$$
$$x\frac{d\,y}{d\,x} = x\,D^2\,u + m\,x\,D\,u = D^2\,x\,u - 2\,D\,u + m\,D\,x\,u - m\,u$$
$$= (D+m)(D\,x\,u - 2\,u) + m\,u$$
$$x\,y = x\,D\,u + m\,x\,u = D\,x\,u - u + m\,x\,u = (D+m)\,x\,u - u.$$

Let
$$x\left\{\frac{d^2y}{d\,x^2} + (m+r)\frac{d\,y}{d\,x} + m\,r\,y\right\} + p\left(\frac{d\,y}{d\,x} + r\,y\right) = 0.$$

If the above values be substituted in this, the result will have the factor $D+m$ common to all the terms, and will reduce to

$$x\left\{\frac{d^2u}{d\,x^2} + (m+r)\frac{d\,u}{d\,x} + m\,r\,u\right\} + (p-1)\left(\frac{d\,u}{d\,x} + r\,u\right) = 0.$$

Consequently, if $y = (D+m)^p\,u$, the proposed will reduce to

$$\frac{d^2u}{d\,x^2} + (m+r)\frac{d\,u}{d\,x} + m\,r\,u = 0.$$

Again, let $x\left(\dfrac{d^2y}{d\,x^2} + m\dfrac{d\,y}{d\,x}\right) + p\dfrac{d\,y}{d\,x} + n\,y = 0.$

The same values substituted in this will give in like manner

$$x\left(\frac{d^2u}{d\,x^2} + m\frac{d\,u}{d\,x}\right) + (p-1)\frac{d\,u}{d\,x} + n\,u = 0.$$

Therefore $y = (D+m)^p\,u$ will reduce it to

$$x\left(\frac{d^2u}{dx^2}+m\frac{du}{dx}\right)+nu=0,$$

which is more simple than the given equation.

The equation

$$\frac{d^2y}{dx^2}+rx\left(\frac{dy}{dx}+my\right)+ry=0,$$

reduces to

$$\frac{d^2u}{dx^2}+rx\left(\frac{du}{dx}+mu\right)=0,$$

where $y=(\mathrm{D}+m)u$.

With the same transformation we have

$$x^2\frac{dy}{dx}=(\mathrm{D}+m)(\mathrm{D}x^2u-4xu)+2mxu+2u$$

$$x^2y=(\mathrm{D}+m)x^2u-2xu;$$

and we shall find

$$\frac{d^2y}{dx^2}+rx^2\left(\frac{dy}{dx}+my\right)+2rxy=0$$

to transform immediately into

$$\frac{d^2u}{dx^2}+rx^2\left(\frac{du}{dx}+mu\right)=0.$$

Or to take a more general example,

$$\frac{d^2y}{dx^2}+(rx^2+nx+k)\left(\frac{dy}{dx}+my\right)+(2rx+n)y=0$$

will transform to

$$\frac{d^2u}{dx^2}+(rx^2+nx+k)\left(\frac{du}{dx}+mu\right)=0.$$

In the two last examples, where $y=(\mathrm{D}+m)u$, the transformed are more simple than the given equations.

To give one more example of this sort of transformation,

let $$\frac{d^3y}{dx^3}+mx^2\frac{d^2y}{dx^2}-2my=0.$$

Here we shall make $y=\mathrm{D}u+mx^2u$. Then

$$\frac{d^3y}{dx^3}=\mathrm{D}^4u+m\mathrm{D}^3x^2u=\mathrm{D}^4u+mx^2\mathrm{D}^3u+6mx\mathrm{D}^2u+6m\mathrm{D}u$$
$$=(\mathrm{D}+mx^2)\mathrm{D}^3u+6mx\mathrm{D}^2u+6m\mathrm{D}u=(\mathrm{D}+mx^2)\mathrm{D}^3u$$
$$+6m\mathrm{D}^2xu-6m\mathrm{D}u.$$

Also

$$x^2\frac{d^2y}{dx^2}=x^2\mathrm{D}^3u+mx^2\mathrm{D}^2x^2u=\mathrm{D}^3x^2u-6\mathrm{D}^2xu+6\mathrm{D}u$$
$$+mx^2\mathrm{D}^2x^2u=(\mathrm{D}+mx^2)\mathrm{D}^2x^2u-6\mathrm{D}^2xu+6\mathrm{D}u.$$

Substituting these values, the proposed equation becomes

$$(\mathrm{D}+m\,x^2)\,(\mathrm{D}^3\,u+m\,\mathrm{D}^2\,x^2\,u-2\,m\,u)=0;$$

or $$\mathrm{D}^3\,u+m\,\mathrm{D}^2\,x^2\,u-2\,m\,u=0;$$

or $$\frac{d^3\,u}{d\,x^3}+m\,x^2\,\frac{d^2\,u}{d\,x^2}+4\,m\,x\,\frac{d\,u}{d\,x}=0.$$

Make $\frac{d\,u}{d\,x}=z$, and the preceding becomes

$$\frac{d^2\,z}{d\,x^2}+m\,x^2\,\frac{d\,z}{d\,x}+4\,m\,x\,z=0,$$

which is only of the second order. The integration of it will give z. Then $y=(\mathrm{D}+m\,x^2)\,\mathrm{D}^{-1}\,z$.

Positive powers only of D have been used in these transformations; but it is obvious that we may employ negative ones also, and may make such assumptions as $y=\mathrm{D}^{-1}\,u+m\,u$, &c. We may also use higher powers of D, and we may express y by more terms than two or three. And it is plain that we may apply a similar method to the transformation and integration of equations in partial differentials, and also to equations in finite differences. I shall close this paper with some examples of a more simple transformation.

Let $$(1-x^3)\,\frac{d^3\,y}{d\,x^3}-p\,(p-1)\,(p-2)\,y=0.$$

Make $$y=\mathrm{D}^{p-3}u.$$

Then $$\mathrm{D}^p\,u-x^3\,\mathrm{D}^p\,u-p(p-1)(p-2)\mathrm{D}^{p-3}u=0,$$

which by (*a.*) reduces to

$$\mathrm{D}^p\,u-\mathrm{D}^p\,x^3\,u+3\,p\,\mathrm{D}^{p-1}\,x^2\,u-3\,p\,(p-1)\,\mathrm{D}^{p-2}\,x\,u=0;$$

or

$$\mathrm{D}^2u-\mathrm{D}^2x^3u+3\,p\,\mathrm{D}x^2u-3\,p\,(p-1)\,x\,u=\mathrm{D}^{-p+2}\,0=\mathrm{X}$$

suppose, which is equivalent to

$$(1-x^3)\,\frac{d^2\,u}{d\,x^2}+(3p-6)x^2\,\frac{d\,u}{d\,x}-3\,(p-1)(p-2)\,x\,u=\mathrm{X},$$

which is only of the second order.

Let $$(1-x^3)\,\frac{d^3\,y}{d\,x^3}+p\,(p+1)\,(p+2)\,y=0.$$

Make $$y=\mathrm{D}^{-p-3}\,u.$$

Then $$\mathrm{D}^{-p}\,u-x^3\,\mathrm{D}^{-p}u+p\,(p+1)\,(p+2)\,\mathrm{D}^{-p-3}\,u=0$$

by (*b.*) will reduce to

$$\mathrm{D}^{-p}u-\mathrm{D}^{-p}x^3\,u-3\,p\,\mathrm{D}^{-p-1}x^2\,u-3\,p\,(p+1)\,\mathrm{D}^{-p-2}x\,u=0;$$

or $$\mathrm{D}^2\,u-\mathrm{D}^2\,x^3\,u-3\,p\,\mathrm{D}\,x^2\,u-3\,p\,(p+1)\,x\,u=0;$$

or

$$(1-x3)\frac{d^2u}{dx^2}-(3p+6)x^2\frac{du}{dx}-3(p+1)(p+2)xu=0,$$

an equation of the second order only.

If
$$(1+x^3)\frac{d^3y}{dx^3}+p(p-1)(p-2)y=0,$$

$y=D^{p-3}u$ will transform it to

$$(1+x^3)\frac{d^2u}{dx^2}-(3p-6)x^2\frac{du}{dx}+3(p-1)(p-2)xu=D^{-p+2}0=X.$$

And if
$$(1+x^3)\frac{d^3y}{dx^3}-p(p+1)(p+2)y=0,$$

$y=D^{-p-3}u$ will transform it to

$$(1+x^3)\frac{d^2u}{dx^2}+(3p+6)x^2\frac{du}{dx}+3(p+1)(p+2)xu=0.$$

Gunthwaite Hall, October 20, 1846,
Near Barnsley, Yorkshire.

LXX. *On the Electricity of Gun-Cotton.* *By* Mr. J. E. Bowman, *Demonstrator of Chemistry, King's College.*

To Richard Phillips, Esq.

Dear Sir,

SINCE every one appears to be more or less engaged in the manufacture of the so-called "gun-cotton," it may be interesting to some of your readers to know that it is capable of being applied to a purpose different from any hitherto described, viz. that of insulating an electrically charged body. I was yesterday engaged in unravelling some cotton which had matted together while in the acid, when I was struck with the tenacity with which it adhered to my fingers; and on lightly holding a small flock of it, and approaching a finger of the other hand, or any foreign body, found that it was strongly attracted towards it, thus differing essentially from the unprepared cotton. I then subjected a little sewing cotton to the action of the acid, with the view of obtaining it in a more convenient form for testing its insulating power, and suspended from a brass rod two equally charged copper balls, one with white silk and the other with the prepared cotton. By examining the two balls at short intervals of time by means of a delicate gold-leaf electrometer, I found that the one suspended by the cotton retained its charge considerably longer than the other; thus proving the cotton to be a more perfect insulator than the silk, which has hitherto been chosen as best adapted for the purpose of insulation.

The acid employed was a mixture of equal parts of nitric acid, sp. gr. 1·46, and sulphuric acid, sp. gr. 1·83, and the cotton was immersed for about five minutes. It was not highly explosive, detonated only partially when struck with a hammer, and required to be heated considerably to cause it to explode[*].

King's College, London, Yours truly,
November 13, 1846. J. E. Bowman.

LXXI. *On a Common Origin of the Acids* $(CH)_n O_4$ *with a Boiling-point under* 300° *Centigrade. By* Dr Joseph Redtenbacher. (*Translated by* E. F. Teschemacher.)[†]

UNDER this head I include not merely the volatile acids of butter, but also acetic acid, metacetonic acid, valerianic acid, &c.; and, in short, all those acids, according to the above formula, beginning at $n=4$ to $n=20$, and including all the intervening members whose values are even numbers of n. These are acetic acid, metacetonic acid, butyric acid, valerianic acid, caproic acid, œnanthylic acid, caprylic acid, pelargonic acid (a new acid, $n=18$), and capric acid.

By former examinations, it is known that they occur and are formed under the most varying circumstances. They are found partly in plants and in animals, in small quantities, as mixtures of solid and fluid fats; their more immediate origin in these cases has not yet been clearly shown. They may be produced from bodies resembling aldehyd, $(CH)_n O_2$, by simply withdrawing the oxygen. The similar theoretical origin of acetic acid, metacetonic, butyric, valerianic and œnanthylic acids has been already proved. That the same has not been accomplished with capric, caprylic and pelargonic acids, is owing to the want of a sufficient quantity for examination. It is not probable that the origin would be found in the organism of the corresponding aldehydes. The different aldehydes by decomposition have been heretofore represented by complex atoms; they have been produced from organic bodies, but not found in them, although the possibility cannot be denied that they may be formed by organic bodies. Another

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[*] [In agreement with the above statement, I may mention that Mr. Reeks, of the Museum of Economic Geology, when drying some of the substance in question, and drawing it out, heard a crackling noise, which induced him to present it to the gold-leaf electrometer, when it instantly caused strong divergence of the leaves.—R.P.]

[†] Communicated by the Chemical Society; having been read May 18, 1846.

mode of origin has been made known by Pelouze, namely the production of butyric acid by the decomposition of the hydrate of carbon, of decayed cheese, and of similar bodies; and the production of butyric acid has been detected by Marchand in sour-krout and in cucumber-water; and also by Wurz in tainted meat.

Iljenko and Laskowsky, in Liebig's laboratory, have found a considerable quantity of valerianic acid in half-decayed cheese. Cheese which consists of the ingredients of the milk contains only fat and caseine; the valerianic acid could only be formed out of these two ingredients. Fresh butter contains no valerianic acid; Liebig has however obtained by the action of caustic alkali on cheese valerianic and butyric acid. But these last two modes cannot be the ordinary way in which these acids exist in organic bodies. It is not especially to be referred to putrefaction or to cheese.

It is probable that nature follows a more general course in producing so great a variety of the easily volatile fatty acids. They are combined with the base oxide of lipyle, the constant companion of all the fixed and volatile fatty bodies of the vegetable and animal kingdom. The different fatty bodies may be submitted to the same process by which the volatile acids are separated from butter, but in every case a very small quantity of these acids is found.

It is very possible that they proceed either from the fixed acids or from the oily acids of the fat.

The solid acids, stearic, margaric, palmitic, cocinic, myristic, contain carbon and hydrogen in the same proportions; they may merely combine with oxygen in order to be converted, in several equivalents, into acids volatile under 300° Centig., $(CH)_n O_4$.

But these solid acids are in themselves permanent, and do not undergo change by exposure to the air; on the contrary, oleic acid is by the action of the oxygen of the atmosphere, and also by other oxidizable agents, a body subject to changes. According to the formula $C_{38} H_{34} O_4$ of Gottlieb, two atoms of carbon must be given off in order that it may be converted into the acid $(CH)_n O_4$.

Laurent and Bromeis having examined the oxidizing action of nitric acid on the oily acids, the one considered he had found in them œnanthic acid, the other that he had found butyric acid. Tilley, by the action of nitric acid on castor oil, also produced an acid of this class; and still later, Gottlieb, by dry distillation, which is partly an oxidizing process, obtained capric and caprylic acids from oleic acid.

The foregoing considerations have induced me to study the oxidizable products arising from the action of nitric acid upon oleic acid, but not with the view of repeating or controlling the experiments of Laurent and Bromeis: I have not occupied myself with the products which these two chemists have obtained, but only examined the volatile substances given off on oxidation by the action of nitric acid; I have discovered the before-mentioned series of acids volatile under 300° Centigrade. The oleic acid which served for my experiments was obtained from the stearine factory of this place; it is exclusively obtained from the fat of oxen. It contained some tallow and margaric acid, which did not essentially interfere with the following experiments. When pure stearine is acted upon by soda, and the solution very much concentrated, the decomposition, which is subsequently effected by an acid, gives off the smell of butyric acid; the separated oleic acid melts under 70° C., so that there remains no doubt that even the fixed acids by oxidation give off acids volatile at 300° C., $(CH)_n O_4$.

I made use of a very spacious tubulated retort to effect the oxidation of about 10 gallons contents, the neck of which was furnished with a cooling apparatus. 5 lbs. of the strongest brown fuming acid were poured in, and a funnel provided, whose lower opening dipped into the nitric acid. The acid was slightly warmed, and the oil carefully introduced in small portions at a time. Upon each addition of oil a violent action took place; it was necessary to wait until the previous action had subsided before any fresh addition of oil was made. The thick brown fumes condensed partly in the water of the receiver through which it passed. Sometimes drops of oil appeared on it, and the passing nitrous gas smelt strongly of volatile fatty acids. After 2 or 3 lbs. of oleic acid had been added, the further oxidation was assisted by heat. When, after long heating, a quantity of oil remains on the surface, a fresh portion of nitric acid may be added, using the same precautions as when the oil was added. In this way I have digested 3 or 4 lbs. of oleic acid for several days with nitric acid. At last I obtained, by the addition of water and redistillation, all the volatile products contained in the retort. The products of the distillation were partly saturated with potash and subjected to a fresh distillation. In the retort there remained nitrate of potash; strata of volatile fatty acids floated upon the surface of the colourless distilled product, and the easily soluble acids were dissolved in the water. The oily portion was drawn off by a siphon and neutralized by baryta water, the different acids separated by crystallization. It contained all the acids, from the valerianic to the capric. The solution of the easily soluble acids was neutralized by carbonate of soda and evaporated to a syrup. Acetate of soda first crystallized out. The mother-liquor was decomposed by sulphuric acid, the floating oily stratum drawn off, and the acetic, metacetonic and butyric acids were partly separated from one another by their boiling-points; the acetic acid and the metacetonic were determined by a silver salt, and the butyric acid by a barytic salt. It may be conceived that the separation of these nine acids, possessing such similar properties, was effected with much difficulty. It is already troublesome to separate the four volatile acids of butter by means of the crystallization of the barytic salts; nearly one-third of the barytic salts remain as an inseparable mixture. The volatile acids of butter differ in the amount of the factor *n*, but not more than 4, while the series with which I was occupied differed only 2. The similarity of the properties of the barytic salt, or, what is the same thing, the difficulty of their separation, was therefore twice as great. I had therefore a large quantity of the mixed barytic salts which was unfit for examination; it would have been impossible to exhibit separately all the acids unless I had had 4 oz. of the mixed volatile acids.

Before I made use of the barytic salts I tried the separation of the hydrated acids by means of their boiling-point. According to recent experiments of Kopp and Fehling, it appears that the hydrated acids $(CH_n)\,O_4$ increase in their boiling-point about 20° C. when the factor *n* is augmented by 2. But when I undertook the distillation, the thermometer rose so regularly from degree to degree that I soon discovered the futility of this mode of separation. A portion of the acid hydrates which came over at the boiling-points of butyric acid, 164° C. and about 4° lower, were converted into silver salts; they proved to be upon examination a mixture of three acids,—the butyric acid, the next lower, the metacetonic, and the next higher, the valerianic acid. Therefore, as a complete separation could not be effected by distillation of the hydrated acids, I did not attempt the separation by means of the ethyle compounds, which would in all probability have acted in the same manner.

For the less volatile acids, from valerianic to capric acids, I had recourse to the barytic salts. By gentle evaporation and crystallization I obtained a series of about twenty crystallizations, and by means of an examination according to their known atomic weights, I was enabled to refer the separate crystallizations to their different acids, and by subsequent recrystallization of the different portions to separate them perfectly from one another, taking only the middle portion of each.

I had originally intended only to undertake the examination of the volatile acids of butter, but the form of the barytic salts led me to the expectation and to the discovery of the intervening acids. If the crystallization of the four volatile acids of butter be compared, it is found that they all crystallize in forms which are only of different sizes and differently grouped. The prisms of caprate of baryta are microscopical, and appear like fine powder. Those of caprylate of baryta are united in small grains; the groups of the caproate of baryta are much larger; and the baryta salts of the butyric acid crystallize in groups as large as hazel-nuts. The other three intervening acids, whose factor *n* is not divisible by 4 but by 2, crystallize in scaly clusters of a pearly lustre, as is already known to be the case with valerianic acid and œnanthylic acid.

I will now give the analytical results of the series, beginning with the lowest. It is to be observed, that in the analysis of bodies presenting so many difficulties, the results cannot be expected to agree perfectly with the theory.

1. *Acetic Acid.*—The silver salts were formed from crystallized acetate of soda; it gave 69·19 per cent. of oxide of silver; theory requires 69·46 per cent.

2. *Metacetonic Acid.*—As before mentioned, the mother-liquor, from which the acetate of soda was crystallized, was decomposed by sulphuric acid. An oily stratum was produced, which was distilled over. It began to boil at 110° C.; what came over from 120° to 140° was neutralized by ammonia and precipitated with nitrate of silver, then boiled until completely dissolved. On cooling, the silver salt was deposited in small hard grains and druses, which blackened at 100° C., and at a higher temperature fused and decomposed. It had all the other properties of metacetonate of silver. 0·242 grm. dried in vacuum gave 0·144 grm. silver; and further, 0·203 grm. gave 0·1525 grm. carbonic acid and 0·057 grm. water.

	Theory.		Experiment.
6 eqs. Carbonic acid	450·0	19·89	20·49
5 ... Hydrogen	62·5	2·76	3·12
3 ... Oxygen.	300·0	13·26	12·47
1 ... Oxide of silver . . .	1450·0	64·09	63·92
1 ... of Metacetonate of silver	2262·5	100·00	100·00

3. *Butyric Acid.*—The oily acid just mentioned under metacetonic acid as passing over at 140° to 164° C. was saturated with baryta and crystallized. The first two crystal-

lizations still contained valerianic acid; the third and fourth crystallizations had all the properties of butyric acid, and were again crystallized; upon analysis it gave the following result. This, as well as all the following analyses, was made with chromate of lead.

0·415 grm. butyrate of baryta gave 0·261 grm. carbonate of baryta; 0·4697 grm. of the same salt gave 0·5265 grm. carbonic acid and 0·1955 grm. water.

		Calculated.		Found.
8 eqs.	Carbonic acid	600·0	30·86	30·57
7 ...	Hydrogen	87·5	4·50	4·62
3 ...	Oxygen	300·0	15·43	15·95
1 ...	Baryta	956·9	49·21	48·86
1 ...	Butyrate of barytes	1944·4	100·00	100·00

As already stated, the oily stratum which floated on the liquid in the receiver, and which contained the less soluble acids, was saturated with caustic baryta, and a series of crystallizations of the baryta salts thus produced.

4. *Valerianic Acid.*—The soluble salts below crystallized in clusters of large plates, with the known properties of valerianate of barytes.

0·268 grm. valerianate of barytes gave 0·154 grm. carbonate of barytes; 0·394 grm. gave 0·506 grm. carbonic acid and 0·1935 grm. water.

		Calculated.		Found.
10 eqs.	Carbonic acid	750·0	35·38	35·03
9 ...	Hydrogen	112·5	5·31	5·46
3 ...	Oxygen	300·0	14·15	14·87
1 ...	Baryta	956·9	45·16	44·64
1 ...	Valerianate of baryta	2119·4	100·00	100·00

5. *Caproic Acid.*—The foregoing second and third crystallizations of the valerianate of baryta appeared in small semiglobular druses, which were made up of small prismatic crystals. These were now recrystallized, and had then all the appearance and properties of caproate of barytes.

0·282 grm. gave 0·149 carbonate of barytes; 0·5085 grm. gave 0·7255 carbonic acid and 0·270 grm. water.

		Calculated.		Found.
12 eqs.	Carbonic acid	900·0	39·22	38·92
11 ...	Hydrogen	137·5	5·99	5·90
3 ...	Oxygen	300·0	13·08	14·14
1 ...	Baryta	956·6	41·71	41·04
1 ...	Caproate of barytes	2294·4	100·00	100·00

6. *Œnanthylic Acid.*—Between the crystallization of ca-

proate of baryta and the foregoing fine granular crystals of caprylate of baryta, there appeared crystals in large plates, with a mother-of-pearl lustre, which recrystallized several times in the same form. It appeared from analogy that it could only contain an acid whose composition was between the caproic and caprylic acids, or it rather was presumed to be with certainty the œnanthylic acid discovered by Tilley. The quantity of the salt obtained was very small, only a few grammes, nevertheless it was necessary to recrystallize it in order to take only the middle portions. I obtained only about 400 milligrammes of the pure salt for examination, of which a part was used for qualitative analysis. The properties however agreed in every respect with the œnanthylate of baryta of Tilley. There remained only sufficient for the determination of the atomic weight of the salt.

0·3273 grm. œnanthylate of baryta gave 0·162 grm. carbonate of baryta; this gave œnanthylate of baryta:—

	Calculated.	Found.
Atomic weight	2469	2489
Anhydrous acid	1413	1413
In 100 parts,—Baryta . .	38·73	38·45

7. *Caprylic Acid.*—The before-mentioned very small fine granular crystals from œnanthylate of baryta were decomposed by sulphuric acid; they gave out the odour of sweat, and formed the known properties of caprylic acid. I have obtained crystals of caprylate of baryta in other forms. I left a cold saturated solution of this salt for eight months in a glass vessel covered with blotting-paper to spontaneous evaporation. During this time the solution had evaporated to three-quarters of its former bulk, and at the bottom were deposited prismatic crystals, above one-quarter of an inch in length, very hard, with a waxy lustre. The atomic weight showed me that they were pure caprylate of baryta.

Of the above fine granular crystals, 0·493 grm. gave 0·229 grm. carbonate of baryta; 0·3265 grm. gave 0·545 grm. carbonic acid and 0·212 grm. water.

	Calculated.		Found.
16 eqs. Carbon	1200·0	45·38	45·51
15 ... Hydrogen	187·5	7·09	7·22
3 ... Oxygen	300·0	11·35	11·19
7 ... Baryta	956·9	36·18	36·08
1 ... Caprylate of baryta	2644·4	100·00	100·00

8. *Pelargonic Acid.*—Between capric and caprylic acids no

other acid was previously known to exist. When the crystallizations of the mixed barytic salts were made, crystals of caprylate of baryta formed on the powder of the caprate. I have already mentioned in my work upon the products of the oxidation of choloidic acid, that a barytic salt in large plates, with a mother-of-pearl lustre, was formed between the caprate and caprylate of baryta, which, from its variable composition, I considered as a mixture of the two salts. But during my proposed examination I obtained between caprate and caprylate of baryta, in the third and fourth crystallizations, a salt of barytes in large plates, which remained the same after repeated crystallization; they were purified in the same manner as the other salts, by taking only the middle portions of the crystals. I consider I am not hasty, even with so imperfect a knowledge, to admit the existence of a new acid, and which analysis confirms to contain the factor $n=18$. I name it, upon grounds which I shall subsequently state, pelargonic acid.

Pelargonate of baryta crystallizes, as I have already stated, similarly to valerianate and œnanthylate of baryta, but is much less soluble in water and in alcohol; it is considerably more soluble than caprate of baryta, and, as it crystallizes out before caprylate of baryta, it is therefore less soluble than this latter salt. It has properties between these two salts. In its other properties it naturally corresponds with the barytic salts of all the volatile fatty acids. It gave out at 100° C. a very small quantity of water, so that its crystals are anhydrous.

0·3395 grm. gave 0·148 grm. carbonate of baryta; 0·316 grm. gave 0·5485 grm. carbonic acid and 0·212 grm. water.

		Calculated.		Found.
18 eqs.	Carbon	1350·0	47·88	47·34
17 ...	Hydrogen	212·5	7·54	7·46
3 ...	Oxygen	300·0	10·64	11·34
1 ...	Baryta	946·9	33·94	33·86
1 ...	Pelargonate of baryta	2809·4	100·00	100·00

The mother-liquor from which the above barytic salts were crystallized, and which doubtless still contained caprylic acid, as a trial of it proved, was precipitated by nitrate of silver. A white curdy precipitate fell, which was with difficulty soluble in hot water. The precipitate was dried at 100° C.

0·198 grm. pelargonate of silver gave 0·0815 grm. silver; 0·3845 grm. of the salt gave 0·5635 grm. carbonic acid and 0·219 water.

	Calculated.		Found.	Pless*.
18 eqs. Carbon	1350·0	40·75	39·98	40·88
17 ... Hydrogen	212·5	6·42	6·33	6·46
3 ... Oxygen	300·0	9·06	9·48	8·89
1 ... Oxide of silver . . .	1450·0	43·77	44·21	43·77
1 ... Pelargonate of silver	3312·5	100·00	100·00	100·00

A small quantity of caprylic acid contained in it decreased the carbon and the water, but increased the quantity of oxide of silver.

9. *Capric Acid.*—Of all the foregoing acids, the action of nitric acid upon the oily acids produces the smallest quantity of this acid, it being the strongest combination, and containing the smallest quantity of oxygen; and also from possessing the highest boiling-points, during the distillation with the nitric acid and water, the smallest quantity is distilled over. Having dissolved all the barytic salt in water, the first crystallization had all the appearance of caprate of baryta. The atomic weight was considerably higher than that of the pelargonate of baryta, but not so high as that of the caprate of baryta. By means of crystallization, redissolving the first-formed crystals and recrystallizing, I at last raised the atomic weight to 2950; but pure caprate of baryta possesses an atomic weight of 2990. Although I had not enough of the substance to submit it to combustion, still there is no doubt that capric acid had been formed. With this is closed the series of the volatile fatty acids, which are obtained by the action of nitric acid upon the oily acids. The largest in quantity formed are the caproic, valerianic and acetic acids; the next in quantity are caprylic, butyric and metacetonic acids; and the least in quantity formed are the capric, pelargonic

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* H. Pless undertook some time ago the examination of the volatile ingredients of the plant *Pelargonium roseum*, (so called from the known scent of the rose), but could not complete it for want of sufficient material. He distilled the plant with water; the product had the scent of the plant, and an oil swam at the top. The liquid had a sour reaction. It was neutralized by baryta water, and the neutral, strongly-scented oil was distilled over. The solution evaporated to dryness contained fatty acids. It was boiled in alcohol, and the first product precipitated by nitrate of silver.

0·284 grm. gave 0·1157 grm. silver; another portion of 0·354 grm. gave 0·5305 grm. carbonic acid and 0·2058 grm. water.

The calculation of this analysis is given above under the head of Pless. This is the reason, for want of a better, that I gave it the name pelargonic acid. Respecting the properties of the hydrates of pelargonic acid, I have little to notice, as the quantity of it I possessed was too small. It is an oily, greasy acid, slightly soluble in water, but easily soluble in alcohol and æther. With regard to its other properties, as far as experiments lead, it may be considered to hold a middle place in the hydrates between the capric and caprylic acids.

and œnanthylic acids. Before I make any further observations in connection with these acids, I will refer to a non-volatile body, which is also obtained by the oxidation of the oily acids, and which appears as a middle member that first originates from oily acids and then changes, on the one hand, into suberic and pimelic acids, and, on the other, by the presence of water into the volatile fatty acids. If the oxidation is not carried so far that the last trace of the fatty stratum in the retort does not disappear, there floats on the surface of the water on cooling an oily stratum of an unguent consistency, which, after washing with water, possesses a strong odour of sweat. This unctuous body is heavier than water, easily soluble in alcohol and æther, but not capable of being distilled over. If heated to above 100° C., a kind of combustion takes place of its elements, giving out an odour of cinnamon and nitrous acid; volatile fatty acids distil over, and the remainder becomes quite black and tough; suberic acid may be detected in it; when acted upon by an alkali it is dissolved with a blood-red colour, and can be again separated unchanged by an acid. When boiled with water in a retort it undergoes decomposition, and volatile fatty acids distil over into the receiver; suberic and pimelic acid remain dissolved in the water in the retort, and the first acid may be crystallized out. These bodies therefore appear to consist of that portion of the atom of the oily acids combined with nitrous acid which produce suberic and pimelic acids, $(CH)_n O_4$.

As it was not possible to obtain it free from mixture, I did not submit it to any further examination. The common origin of the volatile fatty acids from the oily acids clearly accounts for the wide-spread occurrence of these with the oily acids in all fat.

As before stated, I have for many years examined, partly myself and partly by means of others, the volatile acids contained in animal fat, namely, the fat of fowls, of geese, snakes, badgers, hares, particularly human fat, and several others, and in every one, without exception, I have discovered small portions of volatile fatty acids. The most common appear to be the caprylic, caproic and valerianic acids.

Human fat, which M. Lerch very carefully examined, and in large quantities, gave, with the known margaric and oleic acids, a considerable quantity of volatile fatty acids besides caprylic acid, so that the barytic salt of the latter could be clearly established. As before stated, the weakened odour of caprylic acid brings to my mind the ordinary odour of the perspiration of a healthy man; and it is therefore probable that this acid passes off with the perspiration of the body after strong exercise and heating.

According to the knowledge we possess concerning the fat of plants and of animals, the combinations of the volatile and non-volatile acids are with oxide of lipyle. A person the least accustomed can with ease distinguish by their smell the difference between the fat of oxen, of pigs, or of fowls, particularly when the fat is heated to the melting-point, but still the composition of them all is the same. If the fat is washed and the acids separated, it is no longer possible to distinguish between them (butter is an exception). During the washing the substances disappear which are characteristic of the different fats; they appear to have volatilized or to be decomposed. The substance to which the peculiar smell of the fat is due has not yet been isolated. It is possible that it is the corresponding oxide of lipyle combination of the volatile fatty acids.

LXXII. *Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus. By* G. B. Airy, *Esq., Astronomer Royal*[*].

IT has not been usual to admit into the Memoirs of this Society mere historical statements of circumstances which have occurred in our own times. I am not aware that this is a matter of positive regulation: it is, I believe, merely a rule of practice, of which the application in every particular instance has been determined by the discretion of those officers of the Society with whom the arrangement of our Memoirs has principally rested. And there can be no doubt that the ordinary rule must be a rule for the exclusion of papers of this character; and that if a positive regulation is to be made, it must absolutely forbid the presentation of such histories. Yet it is conceivable that events may occur in which this rule ought to be relaxed; and such, I am persuaded, are the circumstances attending the discovery of the planet exterior to Uranus. In the whole history of astronomy, I had almost said in the whole history of science, there is nothing comparable to this. The history of the discoveries of new planets in the latter part of the last century, and in the present century, offers nothing analogous to it. Uranus, Ceres and Pallas were discovered in the course of researches which did not contemplate the possible discovery of planets. Juno and Vesta were discovered in following up a series of observations

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[*] From the Proceedings of the Royal Astronomical Society for November 13, 1846.

suggested by a theory, which, fruitful as it has been, we may almost venture to call fanciful. Astræa was found, in the course of a well-conducted re-examination of the heavens, apparently contemplating the discovery of a new planet as only one of many possible results. But the motions of Uranus, examined by philosophers who were fully impressed with the universality of the law of gravitation, have long exhibited the effects of some disturbing body: mathematicians have at length ventured on the task of ascertaining where such a body could be; they have pointed out that the supposition of a disturbing body moving in a certain orbit, precisely indicated by them, would entirely explain the observed disturbances of Uranus: they have expressed their conviction, with a firmness which I must characterise as wonderful, that the disturbing planet would be found exactly in a certain spot, and presenting exactly a certain appearance; and in that spot, and with that appearance, the planet has been found. Nothing in the whole history of astronomy can be compared with this.

The principal steps in the theoretical investigations have been made by one individual, and the published discovery of the planet was necessarily made by one individual. To these persons the public attention has been principally directed; and well do they deserve the honours which they have received, and which they will continue to receive. Yet we should do wrong if we considered that these two persons alone are to be regarded as the authors of the discovery of this planet. I am confident that it will be found that the discovery is a consequence of what may properly be called a movement of the age; that it has been urged by the feeling of the scientific world in general, and has been nearly perfected by the collateral, but independent labours, of various persons possessing the talents or powers best suited to the different parts of the researches.

With this conviction, it has appeared to me very desirable that the authentic history of this discovery should be published as soon as possible; not only because it will prove a valuable contribution to the history of science, but also because it may tend to do justice to some persons, who otherwise would not receive in future times the credit which they deserve. And as a portion of the history, I venture to offer to this Society a statement of the circumstances which have come to my own knowledge. I have thought that I could with propriety do this, not because I can pretend to know all the history of the discovery, but because I know a considerable part of it; and because I can lay claim to the character of impartiality to this extent, that, though partaking of the general movement of the age, I have not directly contributed either to the theoretical or to the observing parts of the discovery. In a matter of this delicacy I have thought it best to act on my own judgement, without consulting any other person; I have, however, solicited the permission of my English correspondents for the publication of letters.

Without pretending to fix upon a time when the conviction of the irreconcilability of the motions of Uranus with the law of gravitation first fixed itself in the minds of some individuals, we may without hesitation date the general belief in this irreconcilability from the publication of M. Alexis Bouvard's Tables of Uranus in 1821. It was fully shown in the introduction to the tables, that when every correction for perturbation indicated by the best existing theories was applied, it was still impossible to reconcile the observations of Flamsteed, Lemonnier, Bradley and Mayer, with the orbit required by the observations made after 1781, and the elements of the orbit were adopted from the latter observations, leaving the discordances with the former (amounting sometimes to three minutes of arc) for future explanation.

The orbit thus adopted represented pretty well the observations made in the years immediately following the publication of the tables. But in five or six years the discordance again growing up became so great that it could not escape notice. A small error was shown by the Kremsmünster Observations of 1825 and 1826: but perhaps I am not in error in stating that the discordance was first prominently exhibited in the Cambridge Observations, the publication of which from 1828 was conducted under my superintendence.

While still residing at Cambridge, I received from the Rev. T. J. Hussey (now Dr. Hussey) a letter, of which the following is an extract. It will be considered, I think, as honourable to that gentleman's acuteness and zeal. I must premise that the writer had lately passed through Paris.

No. 1. *The Rev. T. J. Hussey to G. B. Airy.*

[Extract.]

"Hayes, Kent, 17 November 1834.

"With M. Alexis Bouvard I had some conversation upon a subject I had often meditated, which will probably interest you, and your opinion may determine mine. Having taken great pains last year with some observations of Uranus, I was led to examine closely Bouvard's tables of that planet. The apparently inexplicable discrepancies between the ancient and modern observations suggested to me the possibility of some disturbing body beyond Uranus, not taken into account because unknown. My first idea was to ascertain some approximate place of this supposed body empirically, and then with my large reflector set to work to examine all the minute stars thereabouts: but I found myself totally inadequate to the former part of the task. If I could have done it formerly, it was beyond me now, even supposing I had the time, which was not the case. I therefore relinquished the matter altogether, but subsequently, in conversation with Bouvard, I inquired if the above might not be the case: his answer was that, as might have been expected, it had occurred to him, and some correspondence had taken place between Hansen and himself respecting it. Hansen's opinion was, that one disturbing body would not satisfy the phænomena, but that he conjectured there were two planets beyond Uranus. Upon my speaking of obtaining the places empirically, and then sweeping closely for the bodies, he fully acquiesced in the propriety of it, intimating that the previous calculations would be more laborious than difficult; that if he had leisure he would undertake them and transmit the results to me, as the basis of a very close and accurate sweep. I have not heard from him since on the subject, and have been too ill to write. What is your opinion on the subject? If you consider the idea as possible, can you give me the limits roughly between which this body or those bodies may probably be found during the ensuing winter? As we might expect an eccentricity [inclination?] approaching rather to that of the old planets than of the new, the breadth of the zone to be examined will be comparatively inconsiderable. I may be wrong, but I am disposed to think that, such is the perfection of my equatorial's object-glass, I could distinguish almost at once the difference of light of a small planet and a star. My plan of proceeding, however, would be very different: I should accurately map the whole space within the required limits, down to the minutest star I could discern; the interval of a single week would then enable me to ascertain any change. If the whole of this matter do not appear to you a chimera, which, until my conversation with Bouvard, I was afraid it might, I shall be very glad of any sort of hint respecting it."

My answer was in the following terms:—

No. 2. *G. B. Airy to the Rev. T. J. Hussey.*

[EXTRACT.]

"Observatory, Cambridge, 1834, Nov. 23.

"I have often thought of the irregularity of Uranus, and since the receipt of your letter have looked more carefully to it. It is a puzzling subject, but I give it as my opinion, without hesitation, that it is not yet in such a state as to give the smallest hope of making out the nature of any external action on the planet. Flamsteed's observations I reject (for the present) without ceremony; but the two observations by Bradley and Mayer cannot be rejected. Thus the state of things is this,—the mean motion and other elements derived from the observations between 1781 and 1825 give considerable errors in 1750, and give *nearly the same errors* in 1834, *when the planet is at nearly the same part of its orbit.* If the mean motion had been determined by 1750 and 1834, this would have indicated nothing; but the fact is, that the mean motions were determined (as I have said) independently. This does not look like irregular perturbation. The observations would be well reconciled if we could from theory bring in two terms, one a small error in Bouvard's excentricity and perihelion, the other a term depending on twice the longitude. The former, of course, we could do; of the latter there are two, viz. a term in the equation of the centre, and a term in the perturbations by Saturn. The first I have verified completely (formula and numbers); the second I have verified generally, but not completely: I shall, when I have an opportunity, look at it thoroughly. So much for my doubts as to the certainty of any extraneous action. But if it were certain that there were any extraneous action, I doubt much the possibility of determining the place of a planet which produced it. I am sure it could not be done till the nature of the irregularity was well determined from several successive revolutions."

It will readily be understood that I do not quote this letter as a testimony to my own sagacity; but I think it deserving of production, as showing the struggle which was made twelve years ago to explain the motions of Uranus, and the difficulty which seemed to envelope the subject. With regard to my last sentence, I think it likely that the same difficulty would still have been felt, if the theorists who entered seriously upon the explanation of the perturbations had not trusted more confidently to Bode's law of distances than I did myself.

In the year 1836, having quitted the Observatory of Cambridge, I completed the reduction of the planetary observations made there during the years 1833, 1834, 1835, in such a form as to exhibit the heliocentric errors of the tabular places of Uranus, together with the effect of errors of the tabular radius vector. The memoir containing these reductions was subsequently printed in the Memoirs of this Society. The progress of the errors of the tables of Uranus was here clearly marked.

In 1837, I received from M. Eugène Bouvard a letter, from which I trust I may be permitted to make an extract. It will, I am certain, be received as creditable to the intelligence and industry of the astronomers of the Observatory of Paris.

No. 3. *M. Eugène Bouvard to G. B. Airy.*

[EXTRACT.]

"Paris, ce 6 Octobre, 1837.

"Dans le peu de moments de loisir que me laissent mes fonctions, je m'occupe d'un travail que je crois n'être pas sans importance. Mon oncle [M. Alexis Bouvard] travaille à réfaire ses tables de Jupiter et de Saturne, en se servant des corrections apportées recemment aux élémens astronomiques. Il m'a cedé les tables d'Uranus a réconstruire. En consultant les comparaisons que vous avez fait des observations de cette planète avec les calculs des tables, on voit que les différences en latitude sont très-grandes et qu'elles vont toujours en augmentant. Cela tient-il à une perturbation inconnue apportée dans les mouvemens de cet astre par un corps situé au-delà? Je ne sais, mais c'est du moins l'idée de mon oncle. Je regarde la solution de cette question comme fort importante. Mais, pour réussir, j'ai besoin de réduire les observations avec la plus grande précision, et souvent les moyens me manquent."

The remainder of this letter relates principally to the reduction of observations.

The following are extracts from my answer:—

No. 4. *G. B. Airy to M. Eugène Bouvard.*

[EXTRACT.]

"Royal Observatory, Greenwich, 1837, Oct. 12.

"I think that, probably, you would gain much in the accuracy of the reduced observations by waiting a short time before you proceed with that part of your labour. Some time ago, I presented to the Astronomical Society of London a very complete reduction of the observations of all the planets made at Cambridge in the years 1833, 1834, 1835. This paper will, as I expect, very shortly be printed. I have reduced the observations made at Greenwich in 1836 in the same manner: the volume containing these reductions will very soon be published. * * * You may also know that

I am engaged upon a general reduction of the observations of planets made at Greenwich, from the commencement of Bradley's observations to the present time. It may perhaps be a year before I can furnish you with the places deduced from these observations. * * * With respect to the errors of the tables of Uranus, I think you will find that it is the *longitude* which is most defective, and that the errors in *latitude* are not at present increasing. To show this, I set down a few of my results. * * * You will see by this statement that the errors of longitude are increasing with fearful rapidity, while those of latitude are nearly stationary. * * * I cannot conjecture what is the cause of these errors, but I am inclined, in the first instance, to ascribe them to some error in the perturbations. There is no error in the pure elliptic theory (as I found by examination some time ago). If it be the effect of any unseen body, it will be nearly impossible ever to find out its place."

On the 24th of February, 1838, I addressed a letter to M. Schumacher, which is printed in the *Astronomische Nachrichten*, No. 349. In this letter it is shown, by treatment of the results of the reduced observations of 1833, 1834, 1835, 1836 (to which allusion was made in my letter to M. Eugène Bouvard), that the tabular radius vector of Uranus was considerably too small. This deduction (which has been confirmed by the observations of all the subsequent years) has always appeared to me to be very important. It is perhaps worth while here to point out that the detection of this error arose, in the first place, from the circumstance that my observations of Uranus had not been confined to the mere opposition (as had too often been done), but had been extended, as far as possible, to quadratures; and, in the next place, from my having so reduced the observations as to exhibit the effect of error of the radius vector.

On the 14th of May, 1838, I transmitted to M. Eugène Bouvard the reduced observations of 1833, 1834, 1835, 1836; and referred him to the paper in the *Astronomische Nachrichten*, which I have cited.

The following letter from M. Eugène Bouvard will show how vigorously the attention of the astronomers of Paris was still directed to Uranus:—

No. 5. *M. Eugène Bouvard to G. B. Airy.*

[EXTRACT.]

"Paris, ce 21 Mai, 1844.

"* * * Je viens aujourd'hui vous prier de me communiquer, si c'est possible, les ascensions droites et les déclinaisons d'Uranus depuis 1781 jusqu'en 1800. * * * J'ai réduit moi-même toutes ces observations en m'en tenant aux élémens imprimés, mais je crains qu'il n'y ait quelques erreurs. Il y a surtout une telle incertitude sur les erreurs de collimation du quart de cercle depuis 1785 jusqu'en 1800, qu'il est presque impossible d'avoir une grande confiance dans les observations. * * * Mon travail est fort avancé. Je suis arrivé à des resultats fort bons déjà, puisque je satisfais aux observations actuelles et aux premières de 1781, 1782, &c., à 15″ de dégré près en longitude: tandisque d'après les tables de mon oncle les erreurs sont de près de 2′ de dégré actuellement. Si je mettais de côté les observations de Maskelyne faites depuis 1785 jusqu'à 1796, mes tables pourraient satisfaire aux observations à 7″ ou 8″ près. Mais je crains que cette période ne m'empêche d'y parvenir; et malheureusement c'est dans cette intervalle que les observations sont le plus défectueuses. * * * D'après mes calculs, il faut changer considérablement les élémens elliptiques d'Herschel, surtout le moyen mouvement et le périhélie. J'ai determiné aussi la masse de Saturne, et je la trouve très différente de celle que l'on admet; il faut l'augmenter beaucoup. Mais j'attendrai une nouvelle approximation pour être tout à fait sûr de ma détermination."

After some further correspondence, I transmitted to M. Eugène Bouvard, on June 27, 1844, the proof sheets of the Planetary Reductions, containing the Right Ascensions and North Polar distances of Uranus; and M. Bouvard, in acknowledging the receipt of them, on July 1, 1844, pointed out an error in the refraction for June 15, 1819. I mention this to show the extreme care with which M. Bouvard's collateral calculations had been conducted.

Although no allusion is made in the last letter to the possible disturbing planet, it would be wrong to suppose that there was no thought of it. In fact, during the whole of these efforts for reforming the tables of Uranus, the dominant thought was, "Is it possible to explain the motions of Uranus, without admitting either a departure from the received law of attraction, or the existence of a disturbing planet?" I know not how far the extensive and accurate calculations of M. Eugène Bouvard may have been used in the subsequent French calculations, but I have no doubt whatever that the knowledge of the efforts of M. Bouvard, the confidence in the accuracy of his calculations, and the perception of his failure to reconcile in a satisfactory way the theory and the observations, have tended greatly to impress upon astronomers, both French and English, the absolute necessity of seeking some external cause of disturbance.

I have departed from a strictly chronological order for the sake of keeping in connexion the papers which relate to the same trains of investigation. Several months before the date of the last letter quoted, I had received the first intimation of these calculations which have led to a distinct indication of the place where the disturbing planet ought to be sought. The date of the following letter is Feb. 13, 1844:—

No. 6. *Professor Challis to G. B. Airy.*

[EXTRACT.]

"Cambridge Observatory, Feb. 13, 1844.

"A young friend of mine, Mr. Adams, of St. John's College, is working at the theory of Uranus, and is desirous of obtaining errors of the tabular geocentric longitudes of this planet when near opposition in the years 1818–26, with the factors for reducing them to errors of heliocentric longitude. Are your reductions of the planetary observations so far advanced that you could furnish these data? and is the request one which you have any objection to comply with? If Mr. Adams may be favoured in this respect, he is further desirous of knowing, whether in the calculation of the tabular errors any alterations have been made in Bouvard's Tables of Uranus besides that of Jupiter's mass."

My answer was as follows:—

No. 7. *G. B. Airy to Professor Challis.*

[EXTRACT.]

"Royal Observatory, Greenwich, 1844, Feb. 15.

"I send all the results of the observations of Uranus made with both instruments [that is, the heliocentric errors of Uranus in longitude and latitude from 1754 to 1830, for all those days on which there were observations, both of right ascension and of polar distance]. No alteration is made in Bouvard's Tables of Uranus, except increasing the two equations which depend on Jupiter by $\frac{1}{50}$ part. As constants have been added (in the printed tables) to make the equations positive, and as $\frac{1}{50}$ part of the numbers in the tables has been added, $\frac{1}{50}$ part of the constants has been subtracted from the final results."

Professor Challis, in acknowledging the receipt of these, used the following expressions:—

No. 8. *Professor Challis to G. B. Airy.*

[EXTRACT.]

"Cambridge Observatory, Feb. 16, 1844.

"I am exceedingly obliged by your sending so complete a series of tabular errors of Uranus. * * * The list you have sent will give Mr. Adams the means of carrying on in the most effective manner the inquiry in which he is engaged."

The next letter shows that Mr. Adams had derived results from these errors.

No. 9. *Professor Challis to G. B. Airy.*

"Cambridge Observatory, Sept. 22, 1845.

"My friend Mr. Adams (who will probably deliver this note to you) has completed his calculations respecting the perturbation of the orbit of Uranus by a supposed ulterior planet, and has arrived at results which he would be glad to communicate to you personally, if you could spare him a few moments of your valuable time. His calculations are founded on the observations you were so good as to furnish him with some time ago; and from his character as a mathematician, and his practice in calculation, I should consider the deductions from his premises to be made in a trustworthy manner. If he should not have the good fortune to see you at Greenwich, he hopes to be allowed to write to you on this subject."

On the day on which this letter was dated, I was present at a meeting of the French Institute. I acknowledged it by the following letter:—

No. 10. *G. B. Airy to Professor Challis.*

"Royal Observatory, Greenwich, 1845. Sept. 29.

"I was, I suppose, on my way from France, when Mr. Adams called here: at all events, I had not reached home, and therefore, to my regret, I have not seen him. Would you mention to Mr. Adams that I am very much interested with the subject of his investigations, and that I should be delighted to hear of them by letter from him?"

On one of the last days of October, 1845, Mr. Adams called at the Royal Observatory, Greenwich, in my absence, left the following important paper:—

No. 11. *J. C. Adams, Esq. to G. B. Airy.*

"According to my calculations, the observed irregularities in the motion of Uranus may be accounted for by supposing the existence of an exterior planet, the mass and orbit of which are as follows:—

Mean distance (assumed nearly in accordance with Bode's law)	38·4
Mean sidereal motion in 365·25 days	1°30′·9
Mean longitude, 1st October, 1845	323 34
Longitude of perihelion	315 55
Excentricity	0·1610
Mass (that of the sun being unity)	0·0001656.

For the modern observations I have used the method of normal places, taking the mean of the tabular errors, as given by observations near three consecutive oppositions, to correspond with the mean of the times; and the Greenwich observations have been used down to 1830: since which the Cambridge and Greenwich observations, and those given in the *Astronomische Nachrichten*, have been made use of. The following are the remaining errors of mean longitude:—

Observation—Theory.

1780	+0″·27	1801	−0″·04	1822	+0″·30
1783	−0·23	1804	+1·76	1825	+1·92
1786	−0·96	1807	−0·21	1828	+2·25
1789	+1·82	1810	+0·56	1831	−1·06
1792	−0·91	1813	−0·94	1834	−1·44
1795	+0·09	1816	−0·31	1837	−1·62
1798	−0·99	1819	−2·00	1840	+1·73

The error for 1780 is concluded from that for 1781, given by observation compared with those of four or five following years, and also with Lemonnier's observations in 1769 and 1771.

"For the ancient observations, the following are the remaining errors:—

Observation—Theory.

1690	+44″·4	1750	−1″·6	1763	− 5″·1
1712	+ 6·7	1753	+5·7	1769	+ 0·6
1715	− 6·8	1756	−4·0	1771	+11·8

The errors are small, except for Flamsteed's observation of 1690. This being an isolated observation, very distant from the rest, I thought it best not to use it in forming the equations of condition. It is not improbable, however, that this error might be destroyed by a small change in the assumed mean motion of the planet."

I acknowledged the receipt of this paper in the following terms:—

No. 12. *G. B. Airy to J. C. Adams, Esq.*

"Royal Observatory, Greenwich, 1845. Nov. 5.

"I am very much obliged by the paper of results which you left here a few days since, showing the perturbations on the place of Uranus produced by a planet with certain assumed elements. The latter numbers are all extremely satisfactory: I am not enough acquainted with Flamsteed's observations about 1690 to say whether they bear such an error, but I think it extremely probable.

"But I should be very glad to know whether this assumed perturbation will explain the error of the radius vector of

Uranus. This error is now very considerable, as you will be able to ascertain by comparing the normal equations, given in the Greenwich observations for each year, for the times *before* opposition with the times *after* opposition."

I have before stated, that I considered the establishment of this error of the radius vector of Uranus to be a very important determination. I therefore considered that the trial, whether the error of radius vector would be explained by the same theory which explained the error of longitude, would be truly an *experimentum crucis*. And I waited with much anxiety for Mr. Adams's answer to my query. Had it been in the affirmative, I should at once have exerted all the influence which I might possess, either directly or indirectly, through my friend Professor Challis, to procure the publication of Mr. Adams's theory*.

From some cause with which I am unacquainted, probably an accidental one, I received no immediate answer to this inquiry. I regret this deeply, for many reasons.

While I was expecting more complete information on Mr. Adams's theory, the results of a new and most important investigation reached me from another quarter. In the *Comptes Rendus* of the French Academy for the 10th of November, 1845, which arrived in this country in December, there is a paper by M. Le Verrier on the perturbations of Uranus produced by Jupiter and Saturn, and on the errors in the elliptic elements of Uranus, consequent on the use of erroneous perturbations in the treatment of the observations. It is impossible for me here to enter into details as to the conclusions of this valuable memoir: I shall only say that, while the correctness of the former theories, as far as they went, was generally established, many small terms were added; that the accuracy of the calculations was established by duplicate investigations, following different courses, and executed with extraordinary labour; that the corrections to the elements, produced by treating the former observations with these corrected perturbations, were obtained; and that the correction to the ephemeris for the present time, produced by the introduction of the new perturbations and the new elements, was investigated, and found to be incapable of explaining the observed irregularity of Uranus. Perhaps it may be truly said that the theory of Uranus was now, for the first time, placed on a satisfactory foundation. This important labour, as M. Le Verrier states, was undertaken at the urgent request of M. Arago.

In the *Comptes Rendus* for June 1, 1846, M. Le Verrier gave

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* Here the Astronomer Royal explained to the meeting, by means of a diagram, the nature of the errors of the tabular radius vector.

his second memoir on the theory of Uranus. The first part contains the results of a new reduction of nearly all the existing observations of Uranus, and their treatment with reference to the theory of perturbations, as amended in the former memoir. After concluding from this reduction that the observations are absolutely irreconcilable with the theory, M. Le Verrier considers in the second part all the possible explanations of the discordance, and concludes that none is admissible except that of a disturbing planet exterior to Uranus. He then proceeds to investigate the elements of the orbit of such a planet, assuming that its mean distance is double that of Uranus, and that its orbit is in the plane of the ecliptic. The value of the mean distance, it is to be remarked, is not fixed entirely by Bode's law, although suggested by it: several considerations are stated which compel us to take a mean distance, not *very* greatly differing from that suggested by the law, but which, nevertheless, without the suggestions of that law, would leave the mean distance in a most troublesome uncertainty. The peculiarity of the form which the investigation takes is then explained. Finally, M. Le Verrier gives as the most probable result of his investigations, that the true longitude of the disturbing planet for the beginning of 1847 must be about 325°, and that an error of 10° in this place is not probable. No elements of the orbit or mass of the planet are given.

This memoir reached me about the 23rd or 24th of June. I cannot sufficiently express the feeling of delight and satisfaction which I received from it. The place which it assigned to the disturbing planet was the same to one degree as that given by Mr. Adams's calculations, which I had perused seven months earlier. To this time I had considered that there was still room for doubt of the accuracy of Mr. Adams's investigations; for I think that the results of algebraic and numerical computations, so long and so complicated as those of an inverse problem of perturbations, are liable to many risks of error in the details of the process: I know that there are important numerical errors in the *Mécanique Céleste* of Laplace; in the *Théorie de la Lune* of Plana; above all, in Bouvard's first tables of Jupiter and Saturn; and to express it in a word, I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence. But now I felt no doubt of the accuracy of both calculations, as applied to the perturbation in longitude. I was however still desirous, as before, of learning whether the perturbation in radius vector was fully explained. I therefore addressed to M. Le Verrier the following letter:—

No. 13. *G. B. Airy to M. Le Verrier.*

"Royal Observatory, Greenwich, 1846, June 26.

"I have read, with very great interest, the account of your investigations on the probable place of a planet disturbing the motions of Uranus, which is contained in the *Comptes Rendus de l'Académie* of June 1; and I now beg leave to trouble you with the following question. It appears from all the later observations of Uranus made at Greenwich (which are most completely reduced in the Greenwich Observations of each year, so as to exhibit the effect of an error either in the tabular heliocentric longitude, or the tabular radius vector), that the tabular radius vector is considerably too small. And I wish to inquire of you whether this would be a consequence of the disturbance produced by an exterior planet, now in the position which you have indicated.

"I imagine that it would not be so, because the principal term of the inequality would probably be analogous to the moon's variation, or would depend on sin 2 $(v-v')$; and in that case the perturbation in radius vector would have the sign $-$ for the present relative position of the planet and Uranus. But this analogy is worth little, until it is supported by proper symbolical computations.

"By the earliest opportunity I shall have the honour of transmitting to you a copy of the Planetary Reductions, in which you will find all the observations made at Greenwich to 1830 carefully reduced and compared with the tables."

Before I could receive M. Le Verrier's answer, a transaction occurred which had some influence on the conduct of English astronomers.

On the 29th of June a meeting of the Board of Visitors of the Royal Observatory of Greenwich was held, for the consideration of special business. At this meeting Sir J. Herschel and Professor Challis (among other members of the Board) were present; I was also present by invitation of the Board. The discussion led, incidentally, to the general question of the advantage of distributing subjects of observation among different observatories. I spoke strongly in favour of such distribution; and I produced, as an instance, the extreme probability of now discovering a new planet in a very short time, provided the powers of one observatory could be directed to the search for it. I gave, as the reason upon which this probability was based, the very close coincidence between the results of Mr. Adams's and M. Le Verrier's investigations of the place of the supposed planet disturbing Uranus. I am authorized by Sir J. Herschel's printed statement in the

Athenæum of October 3, to ascribe to the strong expressions which I then used the remarkable sentence in Sir J. Herschel's address, on September 10, to the British Association assembled at Southampton. "We see it [the probable new planet] as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to that of ocular demonstration*." And I am authorized by Professor Challis, in oral conversation, to state that the same expressions of mine induced him to contemplate the search for the suspected planet.

M. Le Verrier's answer reached me, I believe, on the 1st of July. The following are extracts from it:—

No. 14. *M. Le Verrier to G. B. Airy.*

[EXTRACT.]

"Paris, 28 Juin, 1846.

" * * * Il a toujours été dans mon désir de vous en écrire, aussi qu'à votre savante Société. Mais j'attendais, pour cela, que mes recherches fussent complètes, et ainsi moins indignes de vous être offertes. Je compte avoir terminé la rectification des éléments de la planète troublante avant l'opposition qui va arriver; et parvenir à connaître ainsi les positions du nouvel astre avec une grande précision. Si je pouvais espérer que vous aurez assez de confiance dans mon travail pour chercher cette planète dans le ciel, je m'empresserais, Monsieur, de vous envoyer sa position exacte, dès que je l'aurai obtenue.

"La comparaison des positions d'Uranus, observées dans ces dernières années, dans les oppositions et dans les quadratures, montre que le rayon de la planète, calculé par les tables en usage, est effectivement très-inexact. Cela n'a pas lieu dans mon orbite, telle que je l'ai déterminée; il n'y a pas plus d'erreur dans les quadratures que dans les oppositions.

"Le rayon est donc bien calculé dans mon orbite; et, si je ne me trompe, M. Airy désirerait savoir quelle est la nature de la correction que j'ai fait subir à cet égard aux tables en usage.

"Vous avez raison, Monsieur, de penser que cette correction n'est pas due à la perturbation du rayon vecteur produite *actuellement* par la planète troublante. Pour s'en rendre un compte exact, il faut remarquer que l'orbite d'Uranus a été calculée par M. Bouvard sur des positions de la planète qui n'étaient pas *les positions elliptiques*, puisqu'on n'avait pas pu

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* This sentence is copied from the written draft of the speech. Sir J. Herschel appeared to suppose that the sentence had not been reported in the public journals as spoken. I did however see it so reported in an English newspaper, to which I had access on the continent.

avoir égard aux perturbations produites par la planète inconnue. Cette circonstance a nécessairement rendu les éléments de l'ellipse faux, et c'est à l'erreur de l'excentricité et à l'erreur de la longitude du périhélie qu'il faut attribuer l'erreur actuelle du rayon vecteur d'Uranus.

"Il résulte de ma théorie que l'excentricité donnée par M. Bouvard doit être augmentée, et qu'il en est de même de la longitude du périhélie; deux causes qui contribuent, à cause de la position actuelle de la planète dans son orbite, à augmenter le rayon vecteur. Je ne transcris pas ici les valeurs de ces accroissements, parceque je ne les ai pas encore avec toute la rigueur précise, mais je les aurai rectifié avant un mois, et je me ferai un devoir, Monsieur, de vous les transmettre aussitôt, si cela vous est agréable.

"Je me bornerai à ajouter que la position en quadrature, déduite en 1844 des deux oppositions qui la comprennent, au moyen de mes formules, ne diffère de la position observée que de $0''\cdot6$; ce qui prouve que l'erreur du rayon vecteur est entièrement disparue.

"C'est même une des considérations qui devront donner plus de probabilité à la vérité de mes résultats, qu'ils rendent un compte scrupuleux de toutes les circonstances du problème. Ainsi, bien que je n'aye fait usage dans mes premières recherches que des oppositions, les quadratures n'ont pas laissé de se trouver calculées avec toute l'exactitude possible. Le rayon vecteur s'est trouvé rectifié de lui-même, sans que l'on l'eut pris en considération d'une manière directe. Excusez-moi, Monsieur, d'insister sur ce point. C'est une suite du désir que j'ai d'obtenir votre suffrage.

"Je recevrai avec bien du plaisir les observations que vous voulez bien m'annoncer. Malheureusement le temps presse; l'opposition approche; il faut de toute nécessité que j'aye fini pour cette époque. Je ne pourrai donc pas comprendre ces observations dans mon travail. Mais elles me seront très-utiles pour me servir de vérifications; et c'est ce à quoi je les employerai certainement."

It is impossible, I think, to read this letter without being struck with its clearness of explanation, with the writer's extraordinary command, not only of the physical theories of perturbation, but also of the geometrical theories of the deduction of orbits from observation, and with his perception that his theory *ought* to explain all the phænomena, and his firm belief that it *had* done so. I had now no longer any doubt upon the reality and general exactness of the prediction of the planet's place. My approaching departure for the Continent made it useless for me to trouble M. Le Verrier with a request for the more accurate numbers to which he alludes; but the following correspondence will show how deeply his remarks had penetrated my mind.

About a week after the receipt of M. Le Verrier's letter, while on a visit to my friend, the Dean of Ely, I wrote to Professor Challis as follows:—

No. 15. *G. B. Airy to Professor Challis.*

"The Deanery, Ely, 1846, July 9.

"You know that I attach importance to the examination of that part of the heavens in which there is * * * * reason for suspecting the existence of a planet exterior to Uranus. I have thought about the way of making such examination, but I am convinced that (for various reasons, of declination, latitude of place, feebleness of light and regularity of superintendence) there is no prospect whatever of its being made with any chance of success, except with the Northumberland telescope.

"Now I should be glad to ask you, in the first place, whether you could make such an examination?

"Presuming that your answer would be in the negative, I would ask, secondly, whether, supposing that an assistant were supplied to you for this purpose, you would superintend the examination?

"You will readily perceive that all this is in a most unformed state at present, and that I am asking these questions almost at a venture, in the hope of rescuing the matter from a state which is, without the assistance that you and your instruments can give, almost desperate. Therefore I should be glad to have your answer, not only responding simply to my questions, but also entering into any other considerations which you think likely to bear on the matter.

"The time for the said examination is approaching near."

In explanation of this letter, it may be necessary to state that, in common, I believe, with other astronomers at that time, I thought it likely that the planet would be visible only in large telescopes. I knew that the observatory of Cambridge was at this time oppressed with work, and I thought that the undertaking—a survey of such an extent as this seemed likely to prove—would be entirely beyond the powers of its personal establishment. Had Professor Challis assented to my proposal of assistance, I was prepared immediately to place at his disposal the services of an efficient assistant; and for approval of such a step, and for liquidation of the expense which must thus be thrown on the Royal Observatory, I should have referred to a Government which I have never known to be illiberal when demands for the benefit of science were made by persons whose character and position offered a guarantee that the assistance was fairly asked for science, and that the money would be managed with fair frugality. In the very improbable event of the Government refusing such indemnity, I was prepared to take all consequences on myself.

On the 13th of July, I transmitted to Professor Challis "Suggestions for the Examination of a Portion of the Heavens in search of the external Planet which is presumed to exist and to produce disturbance in the motion of Uranus," and I accompanied them with the following letter:—

No. 16. *G. B. Airy to Professor Challis.*

"Royal Observatory, Greenwich, 1846, July 13.

"I have drawn up the inclosed paper, in order to give you a notion of the extent of work incidental to a sweep for the possible planet.

"I only add at present that, in my opinion, the importance of this inquiry exceeds that of any current work, which is of such a nature as not to be totally lost by delay."

My "Suggestions" contemplated the examination of a part of the heavens 30° long, in the direction of the ecliptic, and 10° broad. They entered into considerable details as to the method which I proposed; details which were necessary, in order to form an estimate of the number of hours' work likely to be employed in the sweep.

I received, in a few days, the following answer:—

No. 17. *Professor Challis to G. B. Airy.*

[EXTRACTS.]

"Cambridge Observatory, July 18th, 1846.

"I have only just returned from my excursion. * * * I have determined on sweeping for this hypothetical planet. * * * With respect to your proposal of supplying an assistant I need not say anything, as I understand it to be made on the supposition that I decline undertaking the search myself. * * * I purpose to carry the sweep to the extent you recommend."

The remainder of the letter was principally occupied with the details of a plan of observing different from mine, and of which the advantage was fully proved in the practical observation.

On August 7, Professor Challis, writing to my confidential assistant (Mr. Main) in my supposed absence, said,—

No. 18. *Professor Challis to the Rev. R. Main.*

[EXTRACT.]

"Cambridge Observatory, August 7, 1846.

"I have undertaken to search for the supposed new planet more distant than Uranus. Already I have made trial of two different methods of observing. In one method, recommended by Mr. Airy * * * I met with a difficulty which I had anticipated. * * * I adopted a second method."

From a subsequent letter (to be cited hereafter), it appears that Professor Challis had commenced the search on July 29, and had actually observed the planet on August 4, 1846.

Mr. Main's answer to the other parts of this letter, written by my direction, is dated August 8.

At Wiesbaden (which place I left on September 7), I received the following letter from Professor Challis:—

No. 19. *Professor Challis to G. B. Airy.*

[EXTRACT.]

"Cambridge Observatory, Sept. 2, 1846.

"I have lost no opportunity of searching for the planet; and the nights having been generally pretty good, I have taken a considerable number of observations: but I get over the ground very slowly, thinking it right to include all stars to 10–11 magnitude; and I find that to scrutinize thoroughly in this way the proposed portion of the heavens will require many more observations than I can take this year."

On the same day on which Professor Challis wrote this letter, Mr. Adams, who was not aware of my absence from England, addressed the following very important letter to Greenwich:—

No. 20. *J. C. Adams, Esq. to G. B. Airy.*

"St. John's College, Cambridge, Sept. 2, 1846.

"In the investigation, the results of which I communicated to you last October, the mean distance of the supposed disturbing planet is assumed to be twice that of Uranus. Some assumption is necessary in the first instance, and Bode's law renders it probable that the above distance is not very remote from the truth: but the investigation could scarcely be considered satisfactory while based on anything arbitrary; and I therefore determined to repeat the calculation, making a different hypothesis as to the mean distance. The excentricity also resulting from my former calculations was far too large to be probable; and I found that, although the agreement between theory and observation continued very satisfactory down to 1840, the difference in subsequent years was becoming very sensible, and I hoped that these errors, as well as the excentricity, might be diminished by taking a different mean distance. Not to make too violent a change, I assumed this distance to be less than the former value by about $\frac{1}{30}$th part of the whole. The result is very satisfactory, and appears to show that, by still further diminishing the distance, the agreement between the theory and the later observations may be rendered complete, and the excentricity reduced at the same time to a very small quantity. The mass and the elements of the orbit of the supposed planet, which result from the two hypotheses, are as follows:—

	Hypothesis I. $\left(\frac{a}{a^1}=0\cdot5\right)$	Hypothesis II. $\left(\frac{a}{a^1}=0\cdot515\right)$
Mean longitude of planet, 1st Oct. 1846	325° 8′	323° 2′
Longitude of perihelion	315·57	299·11
Excentricity	0·16103	0·12062
Mass (that of sun being 1)	0·00016563	0·00015003

"The investigation has been conducted in the same manner in both cases, so that the differences between the two sets of elements may be considered as wholly due to the variation of the fundamental hypothesis. The following table exhibits the differences between the theory and the observations which were used as the basis of calculation. The quantities given are the errors of *mean* longitude, which I found it more convenient to employ in my investigations than those of the true longitude.

Ancient Observations.

Date.	(Obs.—Theory.)		Date.	(Obs.—Theory.)	
	Hypoth. I.	Hypoth. II.		Hypoth. I.	Hypoth. II.
1712	+6″·7	+6″·3	1756	− 4″·0	− 4″·0
1715	−6·8	−6·6	1764	− 5·1	− 4·1
1750	−1·6	−2·6	1769	+ 0·6	+ 1·8
1753	+5·7	+5·2	1771	+11·8	+12·8

Modern Observations.

1780	+0″·27	+0″·54	1810	+0″·56	+0″·61
1783	−0·23	−0·21	1813	−0·94	−1·00
1786	−0·96	−1·10	1816	−0·31	−0·46
1789	+1·82	+1·63	1819	−2·00	−2·19
1792	−0·91	−1·06	1822	+0·30	+0·14
1795	+0·09	+0·04	1825	+1·92	+1·87
1798	−0·99	−0·93	1828	+2·25	+2·35
1801	−0·04	+0·11	1831	−1·06	−0·82
1804	+1·76	+1·94	1834	−1·44	−1·17
1807	−0·21	−0·08	1837	−1·62	−1·53
1810	+0·56	+0·61	1840	+1·73	+1·31

"The greatest difference in the above table, viz. that for 1771, is deduced from a single observation, whereas the difference immediately preceding, which is deduced from the mean of several observations, is much smaller. The error of the tables for 1780 is found by interpolating between the errors given by the observations of 1781, 1782, and 1783, and those of 1769 and 1771. The differences between the results of the two hypotheses are exceedingly small till we come to the last years of the series, and become sensible precisely at the point where both sets of results begin to diverge from the observations; the errors corresponding to the second hypothesis being, however, uniformly smaller. The errors given by the Greenwich Observations of 1843 are very sensible, being for the first hypothesis $+6''\cdot84$, and for the second $+5''\cdot50$. By comparing these errors, it may be inferred that the agreement of theory and observation would be rendered very close by assuming $\frac{a}{a^1}=0\cdot57$, and the corresponding mean longitude on the 1st of October, 1846, would be about 315° 20′, which I am inclined to think is not far from the truth. It is plain also that the excentricity corresponding to this value of $\frac{a}{a^1}$ would be very small. In consequence of the divergence of the results of the two hypotheses, still later observations would be most valuable for correcting the distances, and I should feel exceedingly obliged if you would kindly communicate to me two normal places near the oppositions of 1844 and 1845.

"As Flamsteed's first observation of Uranus (in 1690) is a single one, and the interval between it and the rest is so large, I thought it unsafe to employ this observation in forming the equations of condition. On comparing it with the theory, I find the difference to be rather large and greater for the second hypothesis than for the first, the errors being $+44''\cdot5$ and $+50''\cdot0$ respectively. If the error be supposed to change in proportion to the change of mean distance, its value corresponding to $\frac{a}{a^1}=0\cdot57$, will be about $+70''$, and the error in the time of transit will be between 4^s and 5^s. It would be desirable to ascertain whether Flamsteed's manuscripts throw any light on this point.

"The corrections of the tabular radius vector of Uranus, given by the theory for some late years, are as follows:—

Date.	Hypoth. I.	Hypoth. II.
1834	+0·005051	+0·004923
1840	+0·007219	+0·006962
1846	+0·008676	+0·008250

"The correction for 1834 is very nearly the same as that which you have deduced from observation, in the *Astronomische Nachrichten*; but the increase in later years is more rapid than the observations appear to give it; the second hypothesis, however, still having the advantage.

"I am at present employed in discussing the errors in latitude, with the view of obtaining an approximate value of the inclination and position of the node of the new planet's orbit; but the perturbations in latitude are so very small that I am afraid the result will not have great weight. According to a rough calculation made some time since, the inclination appeared to be rather large, and the longitude of the ascending node to be about 300°; but I am now treating the subject much more completely, and hope to obtain the result in a few days.

"I have been thinking of drawing up a brief account of my investigation to present to the British Association."

Mr. Main, acting for the Astronomer Royal in his absence, answered this letter as follows:—

No. 21. *The Rev. R. Main to J. C. Adams, Esq.*

"Royal Observatory, Greenwich, 1846, Sept. 5.

"The Astronomer Royal is not at home, and he will be absent for some time; but it appears to me of so much importance that you should have immediately the normal errors of Uranus for 1844 and 1845, that I herewith send you the former (the volume for 1844 has been published for some time), and I shall probably be able to send you those for 1845 on Tuesday next, as I have given directions to have the computations finished immediately. If a place (geocentric) for the present year should be of value to you, I could probably send one in a few days."

In acknowledging this letter, Mr. Adams used the following expression:—

No. 22. *J. C. Adams, Esq. to the Rev. R. Main.*

[EXTRACT.]

"St. John's College, Cambridge, 7th Sept., 1846.

"I hope by tomorrow to have obtained approximate values of the inclination and longitude of the node."

On the same day, Sept. 7, Mr. Main transmitted to Mr. Adams the normal places for 1845, to which allusion was made in the letter of Sept. 5.

On the 31st of August, M. Le Verrier's second paper on the place of the disturbing planet (the third paper on the motion of Uranus) was communicated to the French Academy. I place the notice of this paper after those of Sept. 2, &c. because, in the usual course of transmission to this country, the No. of the *Comptes Rendus* containing this paper would not arrive here, at the earliest, before the third or fourth week in September; and it does not appear that any earlier notice of its contents was received in England.

It is not my design here to give a complete analysis of this remarkable paper; but I may advert to some of its principal points. M. Le Verrier states that, considering the extreme difficulty of attempting to solve the problem in all its generality, and considering that the mean distance and the epoch of the disturbing planet were determined approximately by his former investigations, he adopted the corrections to these elements as two of the unknown quantities to be investigated. Besides these, there are the planet's mass, and two quantities from which the excentricity and the longitude of perihelion may be inferred; making in all, five unknown quantities depending solely on the orbit and mass of the disturbing planet. Then there are the possible corrections to the mean distance of Uranus, to its epoch of longitude, to its longitude of perihelion, and to its excentricity; making, in all, nine unknown quantities. To obtain these, M. Le Verrier groups all the observations into thirty-three equations. He then explains the peculiar method by which he derives the values of the unknown quantities from these equations. The elements obtained are,—

Semi-axis major	36·154 $\left(\text{or } \frac{a}{a'} = 0{\cdot}531.\right)$
Periodic time	217^{y}·387
Excentricity	0·10761
Longitude of perihelion	284° 45′
Mean longitude, 1 Jan. 1847 ...	318 47

$$\text{Mass } \frac{1}{9300} = 0{\cdot}0001075$$

True heliocentric longitude, 1 Jan. 1847	326° 32′
Distance from the sun	33·06

It is interesting to compare these elements with those obtained by Mr. Adams. The difference between each of these and the corresponding element obtained by Mr. Adams in his second hypothesis is, in every instance, of that kind which corresponds to the further change in the assumed mean distance recommended by Mr. Adams. The agreement with observations does not appear to be better than that obtained from Mr. Adams's elements, with the exception of Flamsteed's first observation of 1690, for which (contrary to Mr. Adams's expectation) the discordance is considerably diminished.

M. Le Verrier then enters into a most ingenious computation of the limits between which the planet must be sought. The principle is this: assuming a time of revolution, all the other unknown quantities may be varied in such a manner, that though the observations will not be so well represented as before, yet the errors of observation will be tolerable. At last, on continuing the variation of elements, one error of observation will be intolerably great. Then by varying the elements in another way, we may at length make another error of observation intolerably great; and so on. If we compute, for all these different varieties of elements, the place of the planet for 1847, its *locus* will evidently be a discontinuous curve or curvilinear polygon. If we do the same thing with different periodic times, we shall get different polygons: and the extreme periodic times that can be allowed will be indicated by the polygons becoming points. These extreme periodic times are 207 and 233 years. If now we draw one grand curve, circumscribing all the polygons, it is certain that the planet must be within that curve. In one direction, M. Le Verrier found no difficulty in assigning a limit; in the other he was obliged to restrict it, by assuming a limit to the excentricity. Thus he found that the longitude of the planet was certainly not less than 321°, and not greater than 335° or 345°, according as we limit the excentricity to 0·125 or 0·2. And if we adopt 0·125 as the limit, then the mass will be included between the limits 0·00007 and 0·00021; either of which exceeds that of Uranus. From this circumstance, combined with a probable hypothesis as to the density, M. Le Verrier concluded that the planet would have a visible disc, and sufficient light to make it conspicuous in ordinary telescopes.

M. Le Verrier then remarks, as one of the strong proofs of the correctness of the general theory, that the error of radius vector is explained as accurately as the error of longitude. And finally, he gives his opinion that the latitude of the disturbing planet must be small.

My analysis of this paper has necessarily been exceedingly imperfect, as regards the astronomical and mathematical parts of it: but I am sensible that, in regard to another part, it fails totally. I cannot attempt to convey to you the impression which was made on me by the author's undoubting confidence in the general truth of his theory, by the calmness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, "Look in the place which I have indicated, and you will see the planet well." Since Copernicus* declared that, when means should be discovered for improving the vision, it would be

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* I borrow this history from Smith's Optics, sect. 1050. Since reading this Memoir, I have, however, been informed by Professor De Morgan, that the printed works of Copernicus do not at all support this history, and that Copernicus appears to have believed that the planets are self-luminous. —G. B. A.

found that Venus had phases like the Moon, nothing (in my opinion) so bold, and so justifiably bold, has been uttered in astronomical prediction. It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician; it is here that we see the philosopher. The mathematical investigations will doubtless be published in detail; and they will, as mathematical studies, be highly instructive: but no details published after the planet's discovery can ever have for me the charm which I have found in this abstract which preceded the discovery.

I understand that M. Le Verrier communicated his principal conclusions to the astronomers of the Berlin Observatory on September 23, and that, guided by them, and comparing their observations with a star-map, they found the planet on the same evening. And I am warranted by the verbal assurances of Professor Challis in stating that, having received the paper on September 29, he was so much impressed with the sagacity and clearness of M. Le Verrier's limitations of the field of observation, that he instantly changed his plan of observing, and noted the planet, as an object having a visible disc, on the evening of the same day.

My account, as a documentary history, supported by letters written during the events, is properly terminated: but I think it advisable, for the sake of clearness, to annex extracts from a letter which I have received from Professor Challis since the beginning of October, when I returned to England.

No. 23. *Professor Challis to G. B. Airy.*

[EXTRACT.]

"Cambridge Observatory, October 12, 1846.

"I had heard of the discovery [of the new planet] on October 1. * * * I find that my observations would have shown me the planet in the early part of August, if I had only discussed them. I commenced observing on July 29, attacking first of all, as it was prudent to do, the position which Mr. Adams's calculations assigned as the most probable place of the planet. On July 30, I adopted the method of observing which I spoke of to you * * * In this way I took all the stars to the 11th magnitude in a zone of 9′ in breadth, and was sure that none brighter than the 11th escaped me. My next observations were on August 4. On this day * * * I took stars here and there in a zone of about 70′ in breadth, purposely selecting the brighter, as I intended to make them reference points for the observations in zones of 9′ breadth. Among these stars was the planet. A comparison of this day's observations with a good star-map would most probably have detected it. On account of moonlight I did not observe again till August 12. On that day I went over again the zone of 9′ breadth which I examined on July 30. * * * The space gone over on August 12, exceeded in length that of July 30, but included the whole of it. On comparing [at a later time] the observations of these two days, I found that the zone of July 30 contained *every* star in the corresponding portion of the zone of August 12, *except one star of the 8th magnitude.* This, according to the principle of search, which in the want of a good star-map I had adopted, must have been a planet. It had wandered into the latter zone in the interval between July 30 and August 12. By this statement you will see, that, after four days of observing, the planet was in my grasp, if only I had examined or mapped the observations. I delayed doing this, partly because I thought the probability of discovery was small till a much larger portion of the heavens was scrutinized, but chiefly because I was making a grand effort to reduce the vast number of comet observations which I have accumulated; and this occupied the whole of my time when I was not engaged in observing. I actually compared to a certain extent the observations of July 30 and August 12, soon after taking them, more for the sake of testing the two methods of observing adopted on those days than for any other purpose; and I stopped short within a very few stars of the planet. After August 12, I continued my observations with great diligence, recording the positions of, I believe, some thousands of stars: but I did not again fall in with the planet, as I took positions too early in right ascension. * * * On Sept. 29, however, I saw, for the first time, Le Verrier's last results, and on the evening of that day I observed strictly according to his suggestions, and within the limits he recommended; and I was also on the look-out for a disc. Among 300 stars which I took that night, I singled out one, against which I directed my assistant to note "seems to have a disc," which proved to be the planet. I used on this, as on all other occasions, a power of 160. This was the third time I obtained an approximate place of the planet before I heard of its discovery."

This letter was written to me purely as a private communication, but I have received permission from Professor Challis to publish it with the rest. Before terminating this account, I beg leave to present the following remarks:—

First. It would not be just to institute a comparison between papers which at this time exist only in manuscript, and papers which have been printed by their authors; the latter being in all cases more complete and more elaborately worked out than the former.

Second. I trust that I am amply supported by the documentary history which I have produced in the view which I first took, namely, that the discovery of this new planet is the effect of a movement of the age. It is shown, not merely by the circumstance that different mathematicians have simultaneously but independently been carrying on the same investigations, and that different astronomers, acting without concert, have at the same time been looking for the planet in the same part of the heavens; but also by the circumstance that the minds of these philosophers, and of the persons about them, had long been influenced by the knowledge of what had been done by others, and of what had yet been left untried, and that in all parts of the work the mathematician and the astronomer were supported by the exhortations and the sympathy of those whose opinions they valued most. I do not consider this as detracting in the smallest degree from the merits of the persons who have been actually engaged in these investigations.

Third. This history presents a remarkable instance of the importance, in doubtful cases, of using any received theory as far as it will go, even if that theory can claim no higher merit than that of being plausible. If the mathematicians whose labours I have described had not adopted Bode's law of distances (a law for which no physical theory of the rudest kind has ever been suggested), they would never have arrived at the elements of the orbit. At the same time, this assumption of the law is only an aid to calculation, and does not at all compel the computer to confine himself perpetually to the condition assigned by this law, as will have been remarked in the ultimate change of mean distance made by both the mathematicians, who have used Bode's law to give the first approximation to mean distance.

Fourth. The history of this discovery shows that, in certain cases, it is advantageous for the progress of science that the publication of theories, when so far matured as to leave no doubt of their general accuracy, should not be delayed till they are worked to the highest imaginable perfection. It appears to be quite within probability, that a publication of the elements obtained in October 1845 might have led to the discovery of the planet in November 1845.

I have now only to request the indulgence of my hearers for the apparently egotistical character of the account which I have here given; a character which it is extremely difficult to remove from a history that is almost strictly confined to transactions with which I have myself been concerned.

LXXIII. *On the Luminous Phænomena of the Voltaic Battery.* *By* M. Van Breda. (*Extract of a Letter to* M. Arago*.)

"* * * I SHOULD have preferred deferring the communication which I have now the honour to lay before you, of the results to which I have been led by the study of the phænomena accompanying the transfer of particles between metallic points, spheres or plates, by an energetic galvanic current.

"The investigation of the molecular phænomena, which I have undertaken with the aid of the powerful apparatus in the Teylerian Museum, is still far from being finished; but as I find in the *Comptes Rendus*, tome xxii. p. 690, an extract of a memoir by M. De la Rive on the same phænomena, and that its illustrious author there communicates some results which appear to me not wholly to coincide with what my experiments have indicated, I think it may be useful for science to transmit to you some of the principal facts at which I have arrived, deferring to give them more in detail when I shall have finished several other experiments, the apparatus for which is constructing.

"The battery, on Grove's construction, in the Teylerian Museum, is composed of sixty pairs, the elements of which have a surface of forty-five English square inches. I had first an apparatus made by means of which I could produce the luminous arc at the focus of a microscope. I moved a metallic point under the microscope, one while towards a similar point, then towards a metallic plate. The movement was effected by means of a screw micrometer, which enabled me to appreciate the hundredths of a millimetre.

"I could never obtain the transfer of matter without a previous contact, and in this I am in perfect agreement with the illustrious Genevese professor. I held the two very elongated points at a distance from one another, not exceeding $\frac{1}{400}$th of a millimetre, without ten elements of my large pile being able to produce a commencement of transference. As soon as contact has taken place the current passes; but likewise instantly ceases when the points are very much attenuated [*effilées*], because their ends are fused and repelled along the electrodes,—a phænomenon connected with another, of which I shall speak hereafter, and to which I shall return in a subsequent memoir.

"When however I employed ten elements of ordinary dimension, the current continued after the contact; and I observed very well under the microscope what passes when

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* From the *Comptes Rendus* for August 31, 1846.

the two points are withdrawn from one another, or the point from a plate. The repulsive force being less, I saw that as soon as the contact took place the points fused; that the liquid matter,—the melted metal, followed the receding movement of the point, and remained adhering to the point and to the plate, whilst it was pushed from one of the poles to the other,—a movement which was perceivable enough by means of the microscope; at the same time the metal burnt, producing the well-known light of the galvanic arc.

"This phænomenon is not that of the transfer of the matter between two poles placed at a distance from one another. You may, it is true, by suddenly withdrawing the points, break the thread of melted metal by which the current is conducted in this experiment, and the transfer of the matter will then be made through the air; but it seems to me that there will always be some difficulty in ascertaining which of the two phænomena takes place. It cannot always be decided whether the transport of the fused matter, forming an uninterrupted thread, takes place, or if it is effected through the air without the points being connected by any [*continuous liquid?*] matter whatever.

"The phænomenon which is observed after a previous contact seems to me to be of a complex nature, and to lead necessarily to uncertain results.

"Fortunately I could resort to a contrivance which enabled me to establish the current with transfer of the particles, when the electrodes were placed at a sufficient distance from one another, and without there being the least contact. This contrivance consists in passing an electric discharge from point to point, or from a point to a plate arranged previously at the distance at which the action of the current permits the transfer. As soon as the electric spark passes, either from the positive pole to the negative pole, or in the opposite direction, the current is immediately established, and the transfer of the particles commences*.

"After having thus removed a first cause of error, which the previous contact seemed to me liable to introduce into the results, I observed that there still remained another, namely the combustion of the metallic particles, the transfer of which does not take place without a great development of heat.

"It was necessary for me to render this combustion impossible by making my experiments *in vacuo*; an apparatus constructed for this purpose by one of our most skilful engineers, M. Logeman, and which I shall describe in detail when my experiments are completed, fully answered my wishes.

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* This had been previously done by Sir J. Herschel. See a paper by Mr. Daniell in the Philosophical Transactions.—Ed. *Phil. Mag.*

"By means of this apparatus, I could easily make the electrodes approach and recede under a glass jar nearly void of air; by passing a discharge of a Leyden jar through electrodes terminated by points, spheres or plates, I produced the transfer of particles *without contact and in vacuo.*

"The transfer also took place when a plate of metal, and even of a metal different from that of the spheres, was interposed between two spheres.

"A most beautiful spectacle is seen in the vacuum. I doubt not that it will soon be repeated in all lectures on physics, where apparatus sufficiently energetic can be provided.

"The transfer not only does not take place, as in the air, under the form of a flame or of a very intense light, but if the distance be not too great, the particles are projected in the form of sparks; the matter issues from the electrodes in reddened brilliant globules, which break up into sparks of fire against the plate interposed between the two spheres.

"By this means the question proposed by M. De la Rive in his last memoir may be answered.

"'It is very difficult,' says this eminent philosopher, 'to determine what is the state of the incandescent system of particles which are transported from one pole to the other. Is it a liquid state, or a kind of gaseous state? Is it simply a state of powder? The mere inspection of the phænomenon cannot decide this: the physical constitution of the deposit seems to prove that the particles have passed, at least in some cases, through a liquid or gaseous state.'

"There is no doubt that, in my experiments, the matter was transported in a liquid state. Very considerable particles, discernible by the naked eye, repelled by the surface of the electrodes on which they were projected, may, after the experiment, be gathered in a quantity at the bottom of the apparatus in the form of small globules. They even fly out with such force, that they are thrown, still liquid and melted, on the sides of the jar in which the experiment is made; and in their turn they melt the glass which they touch, and are found incorporated in it.

"These particles are all spherical, they have been melted; the observer's eye can follow them from the electrode from which they issue, towards the other electrode of the form of a plate, which repels them [*sous la forme de plaque qui les réfléchit*], and to the bottom of the jar in which they may be collected.

"When iron is employed, they are very attractable by the magnet.

"Another important consequence appears to me to result from these experiments; namely, that the particles which are *projected* by the poles from which they issue are not at all altered by the opposite poles. If this took place, would the particles passing from a sphere to a plate be repelled and rejected from all sides?

"M. De la Rive, in his memoir, professes the same opinion as his predecessors, with respect to the direction of the transport of the matter. He supposes that this transfer only takes place from the positive pole to the negative pole. I have reason to think that, on repeating the experiments with the precautions indicated, it will be found that this opinion requires a further examination.

"All my experiments *in vacuo* exhibit the transfer taking place from the two sides, from the positive pole to the negative pole, and at the same time from the negative to the positive. The quantity of matter transported depended on the substance and on the form of the electrode.

"With a very delicate balance I weighed the two electrodes before the experiment, and I again ascertained their weight with the same accuracy after it was finished. The loss of weight at the negative pole often exceeded that which took place at the positive pole.

"There moreover appeared to me to be an inaccuracy in saying that the matter is transported from one of the poles to the other. My experiments seem to indicate, as I have just said, that the particles of the electrodes are projected from both sides into space: this does not prevent some of these particles reaching the opposite pole and attaching themselves to its surface, which is in a state of fusion.

"The greater part however projected into space falls, and may be collected at the bottom of the vessel in which the experiment is made. This repulsion is above all visible when a sphere and a plate are employed: the surface of the plate sends the matter towards the opposite pole by reflecting it; it does not retain it, as it should do, if attraction had existed.

"The following is the detail of some of my experiments:—

"1. Two iron spheres terminate the electrodes; the experiment is made *in vacuo* and without previous contact; the sphere at the positive pole is weighed before and after the experiment. It has lost by the effect of the current 309 milligrammes, that of the negative pole 55 milligrammes. They are both hollowed into a bowl.

"2. Two copper spheres terminate the electrodes; a thick lamina of iron, isolated, is interposed between the two. The experiment is conducted, like the preceding one, *in vacuo*. The communication is established, as in that one, by an electric discharge.

"The sphere at the positive pole has gained 63 milligrammes;

it is covered with iron. That at the negative pole has gained 360 milligrammes; it is also covered with metallic iron. The plate of iron interposed has lost 327 milligrammes, and traces of copper are perceived in it.

"3. An iron sphere at the positive pole; a cone of coke, prepared according to Bunsen's manner, at the negative pole. The distance is 3 millimetres at the beginning of the experiment: after it had been continued for 33 seconds the iron had lost 32 milligrammes. The coke had lost 5 milligrammes, and it was nevertheless covered with iron, or rather with steel.

"4. An iron sphere terminated the negative electrode; a Bunsen's cone of coke formed the positive. The iron had lost in 33 seconds 80 milligrammes, nearly the same quantity as in the preceding experiment, of which this is the inverse. The coke at the positive pole had lost 25 milligrammes. The margins of the excavation of the coke were remarkable for their hardness; they blunted the best files.

"It seems to me from all this, that there is no doubt that the matter is projected from the two poles at the same time.

"It remains to be ascertained whether their quantity is the same from both sides. The two following experiments appear to me to render this equality doubtful; but this point must be decided by further experiments.

"5. I terminate the positive pole with a plate of copper, the negative pole with a point of the same metal. The copper point melts and runs; the plate gains 22 milligrammes. As soon as the electric spark has established the current, a superb bluish flame is projected against the plate.

"6. The copper plate forms the negative pole, the point the positive pole: the point melts, and the plate gains 30 milligrammes; but the light is less intense, and the current ceases when the distance between the electrodes is only slightly augmented by the fusion of the point; whilst in the fifth experiment, the current has not ceased to pass until the fusion of the point has increased the distance between the electrodes to two centimetres, a distance which, at the commencement of the experiment, was only as many millimetres*.

"If, between two spheres or a plate and a sphere, I make the distance small enough, I produce, without previous contact, by means of a battery, a metallic wire which joins the two electrodes to each other, and by which the current is continued†.

"Finally, I recommend one more experiment to your attention. It appears to me to demonstrate the repulsion which

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[*] An important point is not stated in this experiment, viz. whether the plate were underneath, above, or laterally placed with regard to the point.—Ed.

[†] This seems not to be consistent with the statement in paragraph 4.—Ed.

exists between the particles of the metal which conducts the current: it is as follows:—

"If we make the current of the battery of sixty elements pass through a platina, iron, copper, or any metallic wire, the metal at first reddens, then it melts, and after some instants this fused metallic wire, in which various movements are perceived, breaks at an undetermined place, and the ends formed by the rupture are projected far along the wire; this wire is, after the experiment, terminated, at the spot where it has been broken by the force of the current, by two globules. This phænomenon is not accompanied by any evolution of light or any combustion. The wire neither gains nor loses in weight in this experiment; there is only the ordinary spark which appears at the moment when the repulsion produces its effect, and when the wire is divided into two fragments."

Teylerian Museum, Haarlem,
August 20, 1846.

Note. On the subject of this Paper see Mr. Grove's Lecture on the Voltaic Arc at the Royal Institution in February 1845, in the Literary Gazette.—Ed.

LXXIV. *On the Formation of Nitric Acid in Eudiometric Combustions of Gases mixed with Nitrogen.* *By* Dr. H. Kolbe*.

In an analysis of mixed gases, which contained 90 parts of light carburetted hydrogen and 10 parts of nitrogen, I have often found, that by the combustion in the eudiometer more oxygen disappeared than in accordance with the calculation ought to have disappeared. The explosion caused always such an elevation of temperature, that the mercury sublimed and covered the inside of the eudiometer with a thin, gray, metallic film. After the caustic potash-ball was introduced into the eudiometer for the purpose of absorbing the carbonic acid formed, and the residual volume of gas dried, the diaphanous film of mercury was seen to be covered by innumerable little white crystals, the behaviour of which with water, muriatic acid and caustic potash, showed clearly that they contained protoxide of mercury for their basis.

If these crystals consist of nitrate of protoxide of mercury, as the well-known experiments by Cavendish on the formation of nitric acid render probable, then the before-mentioned diminution of the volume is easily understood.

Therefore, trying at first whether this error was occasioned only by the presence of nitrogen, I mixed a certain volume of pure hydrogen with an excess of oxygen in an eudiometer†

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* Communicated by the Chemical Society; having been read May 18, 1846.

† In all these experiments I used an eudiometer which was furnished

over mercury, and after having exploded the mixture, I observed the diminution of the volume of gas.

[The volumes found by observation in this and the following examples were saturated with the vapour of water.]

Experiment I.

	Vol. obs.	Temp.	Barom.	Height of mercury in tube over that in trough.	Corr. vol.
Hydrogen	78·8	11·2°C.	764·7$^{\text{mm}}$	366·0$^{\text{mm}}$	29·4
After admission of O.	137·6	11·0	...	305·2	59·5
After combustion .	45·6	11·0	...	400·3	15·5

Experiment II.

	Vol. obs.	Temp.	Barom.	Height of mercury in tube over that in trough.	Corr. vol.
Hydrogen	145·5	12·0°C.	764·7$^{\text{mm}}$	297·9$^{\text{mm}}$	63·6
After admission of O.	281·4	11·5	...	162·1	159·9
After combustion .	146·5	11·3	...	296·5	64·4

When we calculate the quantity of the consumed hydrogen from the whole volume of gas which has disappeared by combustion, we find numbers which correspond exactly with the hydrogen used.

	I.	II.
Hydrogen used	29·4	63·6
... calculated . .	29·4	63·6

By this similarity of the results as found and calculated, it is evident that the mercury cannot become oxidized when pure hydrogen is burnt with an excess of oxygen. Mercury sublimed also in this case; but no trace of the crystals spoken of could be seen, even when the residual gas was dried.

Varying the experiments, I mixed with a similar explosive mixture a small portion of atmospheric air (which is calculated to contain in 100 vols. 20·9 vols. of oxygen); I obtained the following results, very different from the former:—

Experiment III.

	Vol. obs.	Temp.	Barom.	Height of mercury in tube over that in trough.	Corr. vol.
Atmospheric air . .	76·8	8·8°C.	767·9$^{\text{mm}}$	367·2$^{\text{mm}}$	29·2
After admission of H.	246·0	8·8	768·2	197·0	134·1
After admission of O.	402·9	9·5	768·3	45·8	277·8
After combustion .	225·1	9·6	768·4	218·2	117·8

with a correctly graduated millimetre-scale etched upon the glass, and exactly graduated, on which by employing a mirror it was easy to determine the tenth part of a millimetre. I also took care to observe all the precautions, by which Bunsen has excluded everything that can occasion an error. The mercury was previously purified by digesting for some days with nitric acid, and had all the properties of the pure metal.

Experiment IV.

	Vol. obs.	Temp.	Barom.	Height of mercury in tube over that in trough.	Corr. vol.
Atmospheric air . .	79·2	8·4°C.	773·0$^{\text{mm}}$	364·2$^{\text{mm}}$	30·7
After admission of H.	250·4	8·5	773·7	192·4	139·1
After admission of O.	391·7	8·5	773·7	55·8	269·4
After combustion .	200·3	8·6	773·4	241·9	101·5

	III.	IV.
Hydrogen used . . .	104·9	108·4
... calculated .	106·8	111·9

According to these experiments, it appears impossible to determine accurately in the usual way, by combustion with oxygen, the quantity of a combustible gas when nitrogen is present, on account of the simultaneous oxidation of the nitrogen producing from 2 to 3 per cent. more hydrogen, as shown by the last two experiments, and which under more unfavourable circumstances is liable to be still increased. Notwithstanding the dilution with atmospheric air, the elevation of the temperature on combustion was so great, that the inner surface of the eudiometer was covered with sublimed mercury, upon which was deposited a number of microscopic crystals; they made their appearance as soon as the remaining gas was dried by means of chloride of calcium.

One of the conditions which in the above case appears to be absolutely necessary for the formation of nitric acid, is the great elevation of temperature of the gas during combustion. The lowering the temperature to a certain point is sufficient entirely to prevent the oxidation of the nitrogen. This is best accomplished by diluting the combustible gas with 1, 2 or 3 volumes of atmospheric air, by which means the accuracy of the analysis is not in the least affected. The following analysis, where hydrogen was deflagrated with twice its volume of atmospheric air mixed with the requisite quantity of oxygen, will justify this assertion, besides which I have been able to confirm it by a series of other experiments.

	Vol. obs.	Temp.	Barom.	Height of mercury in tube over that in trough.	Corr. vol.
Atmospheric air . .	212·3	14·2°C.	756·9$^{\text{mm}}$	244·8$^{\text{mm}}$	100·9
After admission of H.	282·7	15·1	757·4	175·3	152·5
After admission of O.	321·6	15·4	758·3	137·3	185·1
After combustion .	221·7	14·7	758·5	234·0	107·7

	Used.	Calculated.
Hydrogen . . .	51·6	51·6

Professor Bunsen has been so kind as to communicate to me the following very interesting series of experiments relating to this subject, proving clearly that the formation of nitric acid is diminished in the combustion of explosive mixtures according to the dilution with atmospheric air. He mixed a constant quantity of atmospheric air with a decreasing quantity of pure explosive gas formed by decomposing water by means of electricity, and obtained the following results on combustion:—

Vol.		Vol.		Vol.
100	atmospheric air gave after combustion with	259·70	explosive gas	86·15
100	... ...	226·80	...	88·56
100	... ...	84·98	...	99·19
100	... ...	63·21	...	99·97
100	... ...	48·98	...	99·99
100	... ...	40·	...	100·10
100	... ...	36·39	...	100·36
100	... ...	21·20	...	100·79

100 air mixed with 11 vols. of explosive gas did not ignite.

By this it is proved that the ignition of combustible gases containing nitrogen must not be attempted at too far a limit from the point at which the mixed gases are combustible. This point may be ascertained with an unknown mixture by previous experiment, as the gas before its separation from the carbonic acid, &c. may be mixed with nearly double its volume of oxygen gas (which quantity is sufficient even if it consisted of pure light carburetted hydrogen), and small portions added by degrees to a known volume of atmospheric air in an eudiometer until ignition by means of the electric spark took place. For example, if it is found that 50 volumes of such a mixture with 100 volumes of atmospheric air will just ignite, then, according to the foregoing experiments, treble the quantity of the former mixture may be ignited without the formation of nitric acid. Therefore, in an eudiometrical analysis, the gas to be examined may be mixed with twice its volume of atmospheric air, besides the requisite quantity of oxygen gas. In this manner it is ascertained whether the gas to be analysed requires still a dilution with atmospheric air.

LXXV. *On Tribasic Boracic Æther.*

By J. E. Bowman, *Esq.*[*]

A FEW months ago M. Ebelmen published an account of two compounds of silicic acid and æther, and announced that by subjecting them to the gradual action of moist air, he

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[*] Communicated by the Chemical Society; having been read May 18, 1846.

had succeeded in obtaining specimens of hydrated silicic acid perfectly hard and transparent.

This was the first time silica had been obtained artificially in a state at all resembling quartz; and though it is anything but probable that nature ever manufactured her rock-crystal or hydrophane by the action of moist air on silicic æther, I felt interested in the subject, and prepared a quantity of the æther according to Ebelmen's directions, viz. by mixing together liquid chloride of silicon and absolute alcohol. I then placed a little of the æther in some test-tubes, closed them with corks having a small slit cut down the sides, to allow the access of air, and put them, with a small pan of water, under a bell-glass.

In the course of a couple of months a thin ring of transparent silica was deposited round the inside of the tubes, an inch or two from the surface of the æther, apparently at the point where the æthereal vapour had come in contact with the moist air. This silica, though hard, is extremely brittle, and will scarcely bear removal from the glass on which it is deposited without injury. It appears destitute of crystalline structure, and is wholly without action on polarized light.

It occurred to me as highly probable, from the analogous properties of silicon and boron, that if chloride of boron were carried into absolute alcohol, a boracic æther would be produced by a decomposition similar to that of the chloride of silicon.

I passed pure and perfectly dry chlorine over an ignited mixture of charcoal and boracic acid, and purified the gas thus formed from the free chlorine with which it was mixed, by passing it through two U-shaped tubes filled with copper turnings. To the last of these I connected two of Liebig's bulb-apparatus charged with absolute alcohol.

As soon as the gas began to fill the first bulb, a heavy liquid was deposited above the surface of the alcohol, through which it sank and gradually mixed, forming a clear solution. The temperature of the liquid rose considerably during the formation of this heavy fluid, and to prevent it getting too hot I immersed the bulbs in cold water.

After some time the liquid in the bulbs became turbid, owing to the formation of minute globules of a new fluid immiscible with the first; this gradually increased considerably in quantity, and at length occupied nearly as much space as the other. This new fluid was considerably lighter than the first, and the globules as they separated rose to the surface, forming a distinct layer, similar to oil floating on the surface of water.

During the formation of this second fluid the temperature did not sensibly change, though it had risen considerably while the heavy one was forming. The weight of the liquid in the bulbs increased during the operation about 50 per cent.

From the heavy fluid I have been unable to obtain, after repeated distillations, a perfectly homogeneous compound. It appears to consist chiefly of a chlorinated compound, and gives on analysis—

$$\left.\begin{array}{r}46{\cdot}29\ \mathrm{C}\\ 11{\cdot}86\ \mathrm{H}\\ 32{\cdot}32\ \mathrm{O}\end{array}\right\}\text{nearly in the proportion of alcohol;}$$

$$\begin{array}{rl}9{\cdot}53\ \mathrm{Cl} & \\ & \text{and a trace of boracic acid.}\\ \hline 100{\cdot}00 & \end{array}$$

When first distilled it gave off a large quantity of acid fumes, consisting chiefly of hydrochloric acid and chloride of æthyle. Its boiling-point appears to be about 190°, and its specific gravity 0·901. Its smell is aromatic, its taste acid and pungent. It fumes slightly in the air, and is miscible with alcohol and æther. Ammonia decomposes it, with formation of muriate of ammonia, and solution of potash produces chloride of potassium. It is very combustible, reddens litmus, and turns turmeric brown.

Though the two fluids are apparently so completely immiscible, they nevertheless dissolve each other to a certain extent, and retain that small quantity with very considerable force; so that in almost every rectification of the heavy liquid a few drops of the lighter one distilled over.

The lighter liquid has furnished more satisfactory results. When first heated it gave off fumes of hydrochloric acid and chloride of æthyle. It retained with considerable tenacity a small portion of the heavy fluid, with which it evidently mixes in small and definite quantity, and then refuses to dissolve any more, as is the case with many other liquids, as water and common æther.

In the fourth distillation I obtained a pure product, having a specific gravity of ·871 and boiling at 250°. It is curious that though this is considerably the lighter of the two fluids, its boiling-point is 60° higher than the other.

I first attempted its analysis with oxide of copper, but found it impossible to obtain constant results. Four analyses with chromate of lead gave,—

	I.	II.	III.	IV.	Mean.
C . .	47·76	48·12	47·20	47·68	47·69
H . .	9·85	9·95	9·82	9·98	9·90
BO_3 .	23·81	24·94	23·43	25·01	24·29
Chlorine		(accidental)			·18

It is therefore clearly a tribasic æther, the carbon and hydrogen being in the proportions to form æther, and the boracic acid being in slight excess. The calculated results would be,

Carbon	49·34
Hydrogen	10·27
Oxygen	16·45
BO_3	23·94
	100·00

The density of the vapour confirms this view of its composition:—

Weight of globe filled with air	1550·93 gr.
Temperature of air	58°
Pressure	30·61 inch.
Temperature of bath when sealed	278°
Weight of globe full of the vapour	1573·14
Weight of air in globe (corrected)	8·068 gr.
Weight of vapour in globe at 278°	30·278 gr.
Weight of 100 cubic inches of vapour at 60° and 30 inches	164·81 gr.
Specific gravity of vapour	5·31
Calculated specific gravity of vapour, supposing it to consist of 3 equivalents of æther and 1 of BO_3 condensed into 4 oxygen vols.	5·05

From this it appears that it has suffered the same amount of condensation as most of the monobasic æthers, notwithstanding it contains 3 equivalents of æther instead of 1. The theory of its formation from alcohol and chloride of boron is very simple:

$$BCl_3, 3(C_4H_6O_2) = 3HCl + BO_3, 3(C_4H_5O).$$

Tribasic boracic æther is a colourless liquid, with a pungent, slightly aromatic smell and acrid taste. It fumes when exposed to moist air, owing to the decomposition of its vapour, and round the stopper of the bottle containing it, is generally formed a downy deposit of boracic acid.

When a drop of water is added to it, boracic acid is immediately precipitated, which is redissolved when more water is added. A similar decomposition takes place when mixed with solutions of ammonia and potash, borates of those bases being formed. With alcohol and æther it is miscible in all proportions. It burns with a bright green flame, giving off white fumes of BO_3, and leaving a residue of the fused acid. The colours of dry litmus and turmeric are not changed by it, but when moist the first is reddened and the latter becomes brown, owing to the liberation of boracic acid.

I am now engaged in examining the effects of passing some of the volatile metallic chlorides into absolute alcohol; and I am not without hope that I shall be able in this way to form some metallic æthers.

Since writing the above, I have seen in the last number of the *Annales de Chimie*, which has just reached us, a notice that M. Ebelmen has also succeeded in forming the tribasic æther I have now described, and has also formed a similar compound with oxide of methyle. Our results appear to be almost identical, excepting that his numbers approach the calculated ones somewhat more closely than my own.

LXXVI. *Notices respecting New Books.*

Guide to the Geology of Scotland. By JAMES NICOL. Edinburgh.

SCOTLAND, regarded geologically and in a general view, may be considered as a trough, one side of which consists principally of mountains of primary, the other of mountains of transition rocks. This trough is filled with strata of old red sandstone and coal-measures, through which trap rocks intrude. Its axis is a line passing near Irvine, Bothwell and St. Andrews, from W.S.W. to E.N.E. To this axis its limits both above and below are parallel; the one boundary, viz. that on the north-western side, passing from Greenock to Stonehaven, the other, viz. that on the south-eastern, from Girvan to Dunbar. This trough contains the basins and friths of the Clyde, the Tay, and the Forth, though not the higher valleys of these rivers, as the sources of the Clyde are in the transition mountains on the south-east, and those of the Tay and the Forth are in the primary mountains on the north-west. Its surface is a gently undulating plain, beneath which are the evidences of many long extinct animal and vegetable creations, and which is now occupied by a vast population, whose subsistence depends ultimately upon its peculiar mineral and geological properties.

The primary mountains, which form the north-western side of the trough, are the loftiest and the most rugged and barren in Great Britain. Although they include vast beds of slate, together with large and numerous patches of granite, and some rocks of very inferior extent and importance, yet their most abundant constituent is gneiss. The appearance of the country consisting of this rock, which occupies more than a third part of the entire surface of Scotland, is described by Mr. Nicol in the following graphic terms:—"Occupying large tracts of the central highlands, the characters which gneiss impresses on the scenery are very distinctly seen. It is the least picturesque and most monotonous of the primary rocks, the hills being flat and shapeless, and their sides embossed with small round protuberances, between which the water stagnates and moss accumulates. Most gneiss districts seem but a repetition of these features on the great scale, the hills being seldom serrated in outline or broken into rocky cliffs; whilst the valleys or straths are wide and flat, full of small lakes or pools, and disfigured by brown heaths and dark morasses. It has altogether the aspect of a land newly raised from the ocean, in which the rivers have not had time to hollow out channels for themselves or to complete its drainage. In a word, all who are in quest of the picturesque should avoid the pure gneiss districts, as the few spots worthy of notice will be found separated by long, dreary, uninteresting tracts." (Page 134.)

Another important substance in the formation of the Highlands is quartz rock, and our author's description of the features which it impresses on the scenery is no less felicitous than the preceding. "The regions where it abounds have a very peculiar aspect, the hills being in general conoidal, with a smooth flowing outline and few asperities, though with numerous scattered fragments. The soil that covers them is remarkable for sterility even in this land of barrenness; whilst their summits and declivities, refusing nourishment to the humblest moss, shine with dazzling whiteness. Of this kind is the conical stack Balloch-nan-fey, the last remarkable mountain on the west coast, whose naked ridge of bright quartz shines in the sun like snow, and was described by Pennant as marble." (Pp. 135, 136.)

The above extracts may serve as specimens of Mr. Nicol's talent in describing the general aspect of a country, as resulting from the nature of its mineral constituents. It is greatly to his praise that these large and extended views are introduced in connection with the most minute and accurate descriptions of the rocks themselves.

The other side of the trough consists principally of transition rocks, viz. clay-slate and graywacke, corresponding to the older Silurian of Sir Roderick Murchison. In them fossils are exceedingly rare, but they contain graptolites (p. 28). They are penetrated not only by trap rocks, but also by granite and syenite.

In the work before us, Mr. Nicol, having briefly described the physical geography of Scotland, proceeds to give an account of the three great districts which have now been indicated. Under each head we have first a general view of the geological constitution of the country, and then a descriptive survey of its several localities. The author thus introduces in their proper places all the most interesting particulars, such as collieries, mines, medicinal waters, the ichthyolites of the red sandstone and of the Burdie House limestone; the graphic granite and serpentine of Portsey, the evidences of changes of level and of the elevation and depression of the country, earthquakes, parallel roads, the succession or superposition of the different rocks, the destruction effected by moving sands, the principal arguments in support of the Huttonian or metamorphic theory of the origin of the oldest stratified rocks, and the experiments of Hutton and Maskelyne made at Schiehallien relative to the density of the earth*. Among these more minute details the following are remarkable. In the carboniferous strata the shale is often so bituminous as to be used for fuel, as at Pitfirrane in Fife, and other places; and it finally passes into coal. (P. 64.)

The shores opposite to the Isle of Mull consist partly of a sandstone, which is distinguished by particular qualities. Dunstaffnage Castle stands on it. On the authority of Dr. Macculloch our author states, that the doorway of the castle is "of a calcareous sandstone, exactly resembling the fatal stone on which the Scottish kings were crowned, and which is said to have been carried from it to Scone by Kenneth II., and four centuries after removed to Westminster Abbey by Edward I." (P. 158.)

In speaking of the origin of the Glen More-na-Albin, or Great Glen of Scotland, the author has occasion to remark that the depth of Loch Ness is 810 feet, "which is three times that of the sea between Scotland and Norway." (P. 197.)

The following passage is highly instructive. The author is describing Caithness, which consists in great part of secondary rocks in uniform strata, unbroken by the irruption of igneous formations. "That part of it," says he, "which lies to the north and west of Ulbster forms a monotonous undulating plain, almost devoid of running streams, and deformed by stagnant pools of water and dreary mosses, in many places covered by a black rusty crust of the accumulating bog iron ore. The only dry ground is along the edge of the cliffs which rise precipitously from the ocean. The southern part is more diversified with hills, rocks, and valleys, and is drained by more numerous streams. The peculiar features of this district are closely connected with its rock formations, which dip regularly northwards at a low angle, and have been little disturbed by intruding rocks. Nothing can more forcibly demonstrate the highly beneficial effects of these igneous formations, which have sometimes been described as inconsistent with a scheme of perfect wisdom and beneficence, than the consequences seen to result from their absence. Scarce a sixth of this country is cultivated, and nearly two-thirds are regarded as unprofitable; whilst in Fife, where igneous rocks abound, the cultivated ground is two-thirds and the unprofitable surface about a tenth. In Fife 250 individuals dwell on every square mile; in Caithness they do not amount to fifty." (Pp. 205, 206.)

The style of Mr. Nicol's book is clear and elegant. His facts are supported by numerous authorities with useful references. His combination of general views with particular descriptions is most judicious and philosophical. As a guide to the Scottish traveller his work

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* We have noticed the omission of but one circumstance relative to the geology of particular districts, viz. the recent floods in Morayshire, of which a most distinct and minute, and consequently a very instructive account, was published by Sir T. D. Lauder. The agency of running water, as described by him and exemplified in the case referred to, must be taken into account in all inquiries relative to the origin of sedimentary rocks, of whatever age.

must be of the greatest value. Happy may he be deemed who draws from the study of geology the admirable inferences with which the writer concludes,—"that past revolutions, guided by infinite wisdom and beneficence, have produced a scene, not of confusion but of order, and have been rendered conducive to the welfare of man and of the whole animated creation." "A firm conviction," he adds, "of the perfect wisdom of the scheme of creation is the highest aim of science, and the chief benefit resulting from an enlarged contemplation of the phænomena of nature. In order to attain this, however, right feelings and principles, both moral and philosophical, must be carried to the observation of the external universe." (P. 260.)

LXXVII. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS ON COFFEE—CHLOROGENIC ACID.

M. PAYEN finds the composition of coffee to be, approximately, as follows:—

Cellulose	34·000
Fatty substances	10 to 13·000
Glucose, dextrine, and an undetermined vegetable acid	15·500
Legumine, caseine (gluten)?	10·000
Chlorogenate of potash and caffeine	3·5 to 5·000
Azotized organic matter	3·000
Free caffeine	0·800
Concrete essential oil	0·001
Aromatic essential oil	0·001
Mineral bodies	6·997
Hygrometric moisture	12·000
	100·299

According to M. Payen the formula of caffeine is $C_8 H_{10} N_2 O_3$. A part of the caffeine may be obtained directly, by treating the coffee first with æther and then with absolute alcohol: by afterwards treating it with alcohol of 0·60 several substances are separated, among which is a very important crystalline body, containing the remainder of the caffeine in a state of combination. These crystals are formed of a double salt, resulting from the combination of an organic acid (chlorogenic acid) with two bases; one of which is organic, and is caffeine; the other mineral, is potash.

This natural salt of coffee is then a double chlorogenate of potash and caffeine: if it be rubbed after being dried at 212° on a sheet of paper while hot, it is so electrical as to adhere to the blade of a knife presented to it, and remains attached to it in bulky elongated flocculi. When exposed to heat it suffers no alteration from 212° F. up to about 300°; but when heated to about 365° F. it fuses, becomes of a fine yellow colour, boils and swells to five times its original volume, and it remains spongy, yellowish, solid and friable; when heated to 450° F. it becomes brownish, and is partially decomposed. The vapour which is disengaged yields, when condensed, acicular crystals of caffeine: it is scarcely soluble in anhydrous alcohol, even when heated; but a saturated boiling solution in alcohol of 95 degrees, yields on cooling prismatic crystals radiating from a common centre; it is more soluble in alcohol of 85 degrees, and the crystallization is more abundant on cooling: the solubility always increases with the proportion of water: pure water dissolves still more, and a hot saturated solution becomes a mass on cooling. A cold solution, when slowly evaporated, gradually deposits a crown of very fine crystals in mammillated groups. The aqueous solution while crystallizing suffers more or less alteration by exposure to the air, and becomes yellow first, and afterwards greenish-brown.

The crystals of this double salt, when slightly heated with hydrate of potash, become of a vermilion or orange-red colour; heated more strongly, the mixture melts, becomes yellow, yields abundant ammoniacal vapours, and becomes brown, &c. When this double salt is heated with concentrated sulphuric acid, an intense violet colour is produced, and a bronze-coloured pellicle. Hydrochloric acid produces a similar but less marked effect; nitric acid produces an orange-yellow colour.

When acetate of lead is added to the aqueous or alcoholic solution of the double chlorogenate, a flocculent greenish-yellow precipitate is formed: the trisacetate yields a pure yellow-coloured precipitate. Nitrate of silver, alone, produces no effect; but when previously mixed with a very small quantity of ammonia, it produces a yellow colour which becomes brown; the liquid becomes turbid, and a pellicle of reduced metallic silver floats upon it, and gradually extends over the sides of the glass.

Chlorogenic acid is obtained separately by treating chlorogenate of lead with sulphuretted hydrogen; the solution obtained, when rapidly evaporated, yields a confused crystallization of chlorogenic acid.

This acid, when purified by small quantities of anhydrous alcohol, is white, soluble in anhydrous alcohol, and more so in diluted alcohol; it is very soluble in water and difficultly crystallizable. The aqueous solution, when nearly saturated at a boiling heat, crystallizes very slowly in microscopic crystals, radiating from a common centre, and yielding in twenty or thirty days numerous spherical agglomerations of $\frac{4}{100}$th to $\frac{8}{100}$th of an inch in diameter.

The aqueous solution of chlorogenic acid has a very decided acid reaction; it reddens litmus strongly; it is the active principle of the various colours produced and described in the original salt of coffee. When heated in a tube, it fuses, becomes of a yellow colour, boils and leaves a thin and brilliant stratum of charcoal: its vapour condenses into a brown liquid, which, when rapidly heated, leaves a very thin stratum of charcoal, which is iridescent.

Ultimate analysis gave the following results:—

Chlorogenic acid $C_{14}H_8O_7$.

Carbon	56·0
Hydrogen	5·6
Oxygen	38·4
	100·

The double chlorogenate contains—

Chlorogenic acid	63·5
Potash	7·5
Caffeine..................	29·
	100·

Caffeine therefore acts the part of a base in the composition of the natural double salt. This new compound exists in its original state in the perisperm of the coffee berry, from which it is readily extracted.

Among the curious properties of chlorogenic acid, the remarkable power which it possesses of developing a very intense green colour, adds interest to the discovery of the crystallizable compound, which its rapid and variable transformations have hitherto prevented from being observed.—*Journ. de Pharm.*, Octobre 1846.

ANALYSIS OF TALC AND STEATITE.

M. Delesse observes that talc and steatite are common in nature, but that mineralogists do not agree as to their composition. According to his analysis, the talc of Rhode Island in the United States consists of—

Silica..............	61·75
Magnesia	31·68
Protoxide of iron	1·70
Water	4·83
	99·96

The water exists so combined that it is impossible to expel it by the heat of a spirit-lamp, as found by M. Marignac in operating on the talc of Chamouny; to expel the water perfectly the mineral must be kept at a white heat in a calcining furnace.

The steatite examined by M. Delesse was of a milk-white colour, and came from Nyntsch in Hungary; it is the *Speckstein* of German mineralogists; the water is not expelled from it by the heat of a spirit-lamp. It was found to consist of—

Silica..............	64·85
Magnesia	28·53
Protoxide of iron	1·40
Water	5·22
	100·

Journ. de Pharm. et de Ch., Septembre 1846.

CAUSE OF THE EFFLORESCENCE OF LAUMONITE.

MM. Malaguti and Durocher state, that the efflorescence of laumonite is owing to the loss of a small quantity of water. This mineral did not suffer the least alteration when kept for several months in a moist atmosphere. Crystals of laumonite altered, recovered their original transparency and appearance by being immersed in water; and these same crystals, after drying and exposure to dry air, behaved like crystals recently taken from their locality.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

ANALYSIS OF HEULANDITE. BY M. DAMOUR.

The difference between the analyses of this mineral by MM. Walmstedt, Thomson and Rammelsberg, consists in the presence of a small quantity of soda and potash. The analysis of Damour gave

Silica	59·64
Alumina	16·33
Lime	7·44
Soda	1·16
Potash	0·74
Water	14·33
	99·64

This composition indicates that heulandite should be ranked with zeolites.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

EASY REDUCTION OF CHLORIDE OF SILVER.

M. Levol states that this chloride is decomposed by being boiled in a solution of potash in which a little sugar is previously dissolved; the sugar gradually reduces the metal in a short time, carbonic acid gas being disengaged: after due washing the metal is obtained in the pulverulent state.—*Journ. de Ch. Méd.*, Novembre 1846.

ON THE CHEMICAL PROPERTIES OF RUTHENIUM AND SOME OF ITS COMPOUNDS. BY DR. C. CLAUS.

Ruthenium occurs in the residues of the Russian and also of the American platinum ores, but only to the small extent of 1–1½ per cent. It is a constituent of osmium-iridium; the varieties of this mineral analysed by me contained, besides osmium and iridium, 3–6 per cent. ruthenium, 10 per cent. platinum, 1½–2 per cent. rhodium, with traces of copper, iron and palladium. The mineral described by Herman under the name of irite contains 3 per cent. Ru along with Rh, Pt, Os, Ir, FeO, Cr^2O^3, Al^2O^3, SiO^3 and IrO^2. The ruthenium does not occur in that portion of the platinum ore which is soluble in *aqua regia*; it was discovered in the platinum residues, because these always contain osmium-iridium.

The following is the method which I adopt at present for its preparation:—Osmium-iridium is powdered as finely as possible in a cast iron mortar; the particles of iron removed from the mortar are extracted with muriatic acid, and the powder mixed with chloride of sodium treated at a faint red heat with moist chlorine gas. The decomposed mass is extracted with cold water, and a few drops of ammonia added to the concentrated brownish-red, almost opake solution, which is then heated in a porcelain dish. A voluminous blackish-brown precipitate, consisting of sesquioxide of ruthenium and oxide of osmium, is thrown down. This, after edulcoration, is heated in a retort with a sufficient quantity of nitric acid until the acid has passed over, and the osmium removed as osmic acid. The residue in the retort is removed, ignited with nitre and caustic potash which is free from silica, in a silver crucible, for an hour, and the ignited mass softened and dissolved in cold distilled water. The solution is left to clear for 2 hours in a corked flask, the perfectly transparent, beautiful orange-coloured solution removed with a siphon from the insoluble portion, and the alkaline liquid neutralized with nitric acid; this produces a precipitate of velvety-black sesquioxide of ruthenium, which after edulcoration and drying is reduced in a current of hydrogen. In this way perfectly pure metallic ruthenium is obtained. This mode of separating it from the other platinum metals, with the exception of osmium, is based on the behaviour of the solution of the sesquichloride of ruthenium, which is decomposed by heat into free muriatic acid and sesquioxide. The oxide of osmium is mixed with the oxide of ruthenium, as the solution contains chloride of osmium, which is likewise decomposed. The osmium-iridium is only partially decomposed in the above treatment with chlorine, and must therefore be treated with it three or four times.

Ruthenium thus prepared forms angular pieces, of metallic lustre, which are porous, and very closely resemble iridium. Its specific gravity is very low, viz. 8·6 at 61° F.; but it is highly probable that it is but little inferior in this respect to iridium, for some porous iridium, which was prepared in a similar manner from the blue oxide of iridium IrO^2, had a specific gravity of 9·8. Ruthenium is very brittle, does not fuse in the flame of the oxyhydrogen blowpipe, and is almost insoluble in acids; *aqua regia* dissolves mere traces of it. After osmium, ruthenium has the greatest affinity for oxygen of all the platinum metals, for it is very readily oxidized on heating to redness. There are four oxides of ruthenium:—

I. *The protoxide*, RuO, is obtained when 1 equiv. $Ru\,Cl^2$ is heated strongly with rather more than 1 equiv. $NaO\,CO^2$ in a current of carbonic acid, and then extracted with water. The protoxide is left as a blackish-gray metallic powder, which is insoluble in acids, and is reduced by hydrogen at the ordinary temperature. It is anhydrous, and contains in 100 parts 86·6 ruthenium and 13·4 oxygen. The hydrate is still unknown, and will probably be as difficult to prepare as the hydrate of the protoxide of iron.

II. *Sesquioxide*, $Ru^2\,O^3$. *Anhydrous.*—When the pulverulent metal is exposed to a bright red heat in a platinum crucible over Piclet's glass-blower's lamp, it acquires a black colour, and very quickly absorbs for every 100 parts 18 parts oxygen; subsequently the oxidation slowly proceeds further until the oxide becomes blackish blue, and then contains 23–24 parts oxygen to 100 of ruthenium. This oxide increases still more in weight when exposed to a very long-continued red heat, but it could not be oxidized to RuO^2.

The *hydrate*, $Ru^2\,O^3 + 3aq$, is obtained by precipitating the sesquichloride with alkalies. It must be well-washed, and nevertheless contains a few per cent. of the alkali. It forms a blackish-brown powder, which dissolves with an orange colour in acids, glows suddenly when heated, and is not perfectly reduced at the ordinary temperature by hydrogen. It is insoluble in alkalies.

III. *Oxide of Ruthenium*, RuO^2. *Anhydrous.*—On roasting and igniting RuS^2, a blackish-blue powder, with a greenish play of colours, is obtained, which is insoluble in acids, and contains 30·7 parts oxygen to 100 of metal. It is obtained when $RuO^2, 2SO^3$ is exposed to a strong red heat in small grayish particles of metallic lustre, and with a beautiful blue and green iridescence.

The hydrate, $RuO^2 + 2aq$?, has not yet been analysed, but it will undoubtedly possess the above composition; it is obtained as a gelatinous yellowish-brown precipitate, when the solution of the double chloride $KCl^2 + RuCl^4$ is mixed with $NaO\ CO^2$, and evaporated. It contains much alkali, dissolves with a yellow colour in acids, and these solutions become of a rose-red on evaporation. When heated in a platinum spoon it detonates with a lively incandescence, and is projected in all directions. In the dry state it has the colour of impure oxide of rhodium.

IV. *Ruthenic acid*, RuO^3, is not yet known in its isolated state. It is most probably a very unstable compound, which is readily decomposed into oxide and oxygen. It occurs as basic rutheniate of potash in the solution of the ruthenium which has been ignited with potash and saltpetre. The composition of the acid was ascertained in the same way as H. Rose analysed the ferrate of potash. For the analysis however a rutheniate of potash was employed, which had been prepared by igniting the metal with potash and chlorate of potash. The salt could not be obtained crystallized, as the solution is readily decomposed. The solution is of a beautiful orange colour, is very astringent to the taste, like tannic acid, and is neutral, if too much potash and nitre has not been used in its preparation; it colours organic substances black, owing to a deposition of reduced oxide. Acids immediately precipitate from it a black oxide, which contains potash if the solution had not been perfectly neutralized with acid; with a slight excess of acid however the precipitated oxide contains some per cents. of acid. When sulphuric acid is used, a metal is obtained on reducing the oxide which contains some sulphuret, and which it is very difficult to decompose at a faint red heat in a current of hydrogen. I analysed the oxide containing acid, especially that precipitated by nitric acid, and obtained results which led me to believe that this oxide was $RuO^2 + 2aq$ (I was not then acquainted with the true hydrated oxide); but it is very probable that, on igniting it in a current of carbonic acid in order to determine the water, the sesquioxide is converted by the action of the nitric acid into oxide. At present I consider this black oxide to be a hydrate of the sesquioxide, because it dissolves in muriatic acid, forming sesquichloride*.

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* Before I was acquainted with the true oxide and perchloride, I thought that muriatic acid converted the black oxide with reduction into sesquichloride, especially as some chlorine was disengaged in the experiment.

Chlorides of Ruthenium.

I. *Protochloride of Ruthenium*, RuCl.—In my former experiments it appeared to me as if ruthenium were little acted upon by chlorine. I subsequently observed however that after very long-continued treatment the metal was converted at a faint red heat into protochloride. When ruthenium is heated to redness in the bulb of a reduction-tube over an Argand lamp, and dry chlorine passed over it, yellow vapours (probably the highest and most volatile chloride) are first disengaged and carried away by the current of gas; the metal seems not to change and to increase in bulk; subsequently some sesquichloride sublimes, and the metal becomes black; after 2 hours it is converted into a black partially crystalline protochloride. It is however not always possible to saturate the ruthenium perfectly with chlorine in one operation; it is best to reduce the protochloride formed to a fine powder, and to treat it again with chlorine. In this way a combination is always obtained, yielding on analysis numbers corresponding to the above formula. Water removes a trace of sesquichloride from it; otherwise it is insoluble in it and in acids; a solution of caustic potash has little action upon it even when evaporated with it to dryness.

Soluble Protochloride.—When the solution of the sesquichloride is treated for a long time with sulphuretted hydrogen, a black-brown sulphuret is precipitated, and the liquid acquires the well-known beautiful azure-blue colour. The sulphuretted hydrogen can be removed from this fluid by a current of atmospheric air, and in this way a solution of the blue protochloride with muriatic acid obtained. I regard this blue combination as the protochloride, although I cannot vouch for the correctness of this view; for it is not possible to prepare it in a solid form or as a crystalline double salt, as it is very readily decomposed, and passes into the orange sesquichloride. I therefore attempted to obtain an idea of the composition of this compound by precipitating the solution of the blue protochloride by alkalies; but the oxide thus obtained possessed the composition of sesquioxide Ru^2O^3. This negative result is no direct proof against my view, for I have observed that, in the decomposition of the lower chlorides of iridium by alkalies, the higher oxide IrO^2 is constantly formed. Ruthenium must behave similarly, as it exhibits a greater affinity for oxygen than that metal. The reasons which have led me to regard the blue compounds as the protochloride are,—1st, HS, in its action upon the perchlorides of the other platinum metals, converts them into lower chlorides; 2nd, the sulphuret which separates on the formation of the blue compound does not contain, as it should, 3 atoms of sulphur to 2 of metal, but 2 and more atoms of sulphur to 1 atom of metal; 3rd, other reducing agents, as zinc, percyanide of mercury, &c., colour the sesquichloride blue; 4th, on heating the evaporated solution of the sesquichloride, it becomes green (a mixture of blue and yellow protochloride), and at some spots blue; but it is not possible to prepare in this manner a pure blue protochloride, because a portion of the salt is converted into a basic compound.

II. *Sesquichloride of Ruthenium*, $Ru^2 Cl^3$, is obtained by dissolving the sesquioxide, precipitated from the rutheniate of potash, in muriatic acid, and evaporating to dryness. It is deliquescent, has a very astringent non-metallic taste, like tannic acid; and dissolves, leaving behind the yellow basic compound, with a beautiful orange-red colour in water and spirit. When heated it acquires the above-mentioned green and blue colour. One of its most remarkable properties is that its dilute solution is decomposed by heat into free muriatic acid and hydrated sesquioxide. This decomposition takes place likewise at the ordinary temperature in the course of a few days.

Caustic and carbonated alkalies, as well as the tribasic phosphate of soda, immediately produce in the solution of this salt a blackish-brown precipitate of hydrated sesquioxide, which is not soluble in an excess of the precipitant. Some unprecipitated metal is however left in the solution. Solution of borax at first produces no precipitate, but merely decolorizes the solution, which on the application of heat deposits $Ru^2 O^3 + 3aq$. Formiate of soda does not reduce the metal, but only decolorizes the solution; the same happens with oxalic acid. Ferrocyanide of potassium at first decolorizes the solution, which subsequently becomes green. Percyanide of mercury colours the solution blue, with formation of a blue precipitate. Nitrate of silver yields a black precipitate, which subsequently turns white, while the liquid becomes rose-red. Chloride of potassium and chloride of ammonium produce dark brown crystalline precipitates only in very concentrated solutions. Sulphurous acid decolorizes the sesquichloride only after long-continued action. Sulphuretted hydrogen produces the above-mentioned blue reaction, with deposition of sulphuret. Sulphuret of ammonium throws down most of the ruthenium from the solution as a blackish-brown sulphuret, which is not perceptibly soluble in an excess of the precipitant.

III. *Perchloride of Ruthenium*, $RuCl^2$.—This compound is not known in the isolated state; it exists however in combination with chloride of potassium. In the double salt described below it has the rose colour of the salts of the perchloride of rhodium ($Rh^2 Cl^3$).

Double Salts.

I. *Sesquichloride of Ruthenium and Potassium*, $2KCl + Ru^2 Cl^3$. —From the composition of this salt the atomic weight of ruthenium was ascertained, and found to be identical with that of rhodium. The analyses of the other compounds of ruthenium have fully confirmed this determination. In the crystallized state it is perfectly insoluble in alcohol of 0·863 spec. grav., and possesses in this respect the properties of the double chlorides of all the other platinum metals with potassium and ammonium. When however the salt is in a concentrated solution, alcohol only throws down a portion; when the solution of the salt is mixed with the chloride of any other metal soluble in alcohol, evaporated to dryness, and digested with strong spirit, the salt of ruthenium dissolves in proportion to the quantity of the other soluble chloride. This property is likewise possessed by the sparingly soluble double salts of other platinum metals, among others by the sodiochloride of rhodium. The crystallized sesquichloride of ruthenium and potassium is almost insoluble in a concentrated solution of chloride of ammonium, and I have used this salt in washing the salt of ruthenium, to free it from any admixture of chloride of potassium. This method is far more certain than the employment of spirit, which only removes with difficulty the last traces of that admixture. It is scarcely necessary to observe that the chloride of ammonium must be subsequently extracted with spirit, which however, from the great solubility of the salt, is very readily and quickly effected.

II. *Sesquichloride of Ruthenium and Ammonium*, $2NH^4Cl + Ru^2Cl^3$, is very easily obtained by mixing the concentrated solution of the black sesquioxide in muriatic acid with chloride of ammonium, and concentrating by evaporation with the addition of a little nitric acid. The salt cannot be distinguished in form and properties from the potash salt; it leaves, as required by the formula, 32·7 per cent. metal when ignited in a current of hydrogen. Both these salts, although more soluble than $KCl + IrCl^2$, do not dissolve easily in the crystallized state in water; notwithstanding they crystallize with difficulty from their solutions, and only when these are highly concentrated.

III. *Sesquichloride of Ruthenium and Sodium?*—This compound could not be obtained crystallized and in a state fit for analysis. It formed a semi-crystalline deliquescent mass, readily soluble in spirit, which dried when strongly heated, but then became partly green and blue, and behaved therefore like a mixture of chloride of sodium and sesquichloride of ruthenium. A solution of chloride of barium and Ru^2Cl^3 dried to a deliquescent mass, which resembled the preceding salt, but from which spirit extracted the sesquichloride of ruthenium, leaving behind chloride of barium.

IV. *Perchloride of Ruthenium and Potassium*, $KCl + RuCl^2$.—The conversion of Ru^2Cl^3 into $RuCl^2$ is very difficult. When treated for a long time with nitromuriatic acid, only a very small portion passes over into perchloride, and it is impossible to effect its complete conversion. When heated with muriatic acid and chlorate of potash, some of the salt is formed; but the greater portion of the ruthenium is lost, it being converted into a higher chloride (probably $RuCl^3$), which is volatile and escapes with the aqueous vapours. I unfortunately did not make this observation before I had already consumed most of the ruthenium, and did not possess sufficient to examine more closely this interesting compound.

I once obtained the perchloride accidentally, having added by mistake too much nitric acid in precipitating the oxide from the rutheniate of potash. The solution of the oxide in nitric acid filtered from the precipitated oxide was brown, and yielded, on evaporation with some muriatic acid, at first a considerable quantity of crystallized saltpetre; and on further concentration of the rose-coloured mother-ley a red salt crystallized, which, washed first with chloride of ammonium, then with spirit, was the compound in its pure state. The crystals of this salt are so minute that their form cannot be recognised with the naked eye; with a magnifying power of 300 times diameter they appear as perfectly transparent red prisms with pointed terminal surfaces, probably belonging to the rhombohedral system. The salt is readily soluble in water, insoluble in spirit of 0·889, and scarcely soluble in a concentrated solution of chloride of ammonium; its solution is of a rose colour playing into violet, and cannot be distinguished from that of the chloride of rhodium and sodium; it is scarcely affected by sulphuretted hydrogen, and only deposits after some time a little yellowish-brown sulphuret, without the liquid losing its red colour. If to the solution of this salt an alkali is added, no precipitate results, but on evaporation a yellowish-brown gelatinous hydrated oxide separates, which contains a considerable quantity of alkali, and when heated on platinum explodes slightly with a sudden glowing. A concentrated solution of this salt in water is only partially precipitated by alcohol; the greater portion remains in solution with a red colour, and is not reduced to a lower chloride on evaporation. The blue reaction of the sesquichloride of ruthenium cannot be produced in the solution of this salt even when sulphuretted hydrogen is passed through it for six hours. The metal reduced from this salt is ruthenium, for it yielded on ignition with nitre rutheniate of potash, and this with muriatic acid sesquichloride of ruthenium, in which the blue reaction could be produced by sulphuretted hydrogen.

Sulphurets.

It is highly probable that as many combinations of ruthenium with sulphur exist as this metal forms oxides, but their preparation is accompanied with numerous difficulties. When, for instance, powdered ruthenium is mixed with sulphur and heated in an atmosphere of carbonic acid, there is no appearance of any chemical reaction, the sulphur distils off, and the ruthenium increases but little in weight. The precipitates obtained from the chlorides by sulphuretted hydrogen by no means correspond to those compounds; they always contain too much sulphur, and are very probably mixtures of sulphurets with sulphur; their analysis is very difficult, as they easily become oxidized in drying, and are partially converted into sulphate. When heated, in order to remove the water, they explode faintly with incandescence; and when treated with fuming nitric acid, they are likewise oxidized with a faint explosion and a shower of sparks. They dissolve, it is true, in ordinary nitric acid very easily, but the sulphuric acid formed cannot be precipitated pure from this solution by a salt of baryta; it is coloured yellowish, and always contains some sulphate of ruthenium, which cannot be removed by any solvent. When the sulphuret precipitated from the sesquichloride is heated in a reduction-tube through which carbonic acid is passed, water and sulphur are removed, accompanied by a glowing and explosion, and the blackish-gray metallic powder left yields results which correspond to the formula $Ru^2 S^3$. The sulphuret which is precipitated when sulphuretted hydrogen is passed for a short time through the solution of Ru^2Cl^3, frequently contains 3 atoms of sulphur to 1 of ruthenium; but if the gas has been passed through for several hours, a sulphuret, RuS^2, of yellowish-brown colour, is obtained. Sulphuret of ammonium throws down a blackish-brown sulphuret, Ru^2S^3, from the blue protochloride; however, I place little value on these analyses, as the difficulties render the results very uncertain.

Oxysalts.

I have little experience on this series of compounds, as their preparation is very difficult, and my material was consumed; I only obtained the sulphate of ruthenium, $RuO^2 + 2SO^3$, from the sulphuret, which is formed on treating Ru^2Cl^3 with sulphuretted hydrogen by oxidizing it with ordinary nitric acid. An orange-coloured solution is obtained, yielding on evaporation to dryness a yellowish brown amorphous mass, which attracts moisture, deliquesces, and has an acid astringent taste. The dry compound reduced to powder resembles mosaic gold, dissolves readily in water, and is not precipitated at first by alkalies; but on evaporation the yellowish-brown gelatinous hydrated oxide separates, which has the greatest resemblance to the impure oxide of rhodium, and explodes with incandescence when heated. Sulphuretted hydrogen does not produce the blue reaction in the solution of this salt.—Liebig's *Annalen* for August 1846.

ON PHOSPHAM AND PHOSPHAMIDE.

M. Gerhardt observes that chemists admit of the existence of two compounds of nitrogen and phosphorus; one composed of 1 atom of phosphorus and 2 atoms of nitrogen (PN^2) is produced, according to M. H. Rose, when the protochloride is heated with ammonia; according to MM. Liebig and Wöhler it is also formed by the action of heat on the produce of the reaction of ammonia on perchloride of phosphorus.

The other compound of nitrogen and phosphorus has been described by M. Liebig in his *Annalen*. It is considered by him as a compound of the preceding phosphuret of nitrogen, and the elements of water (PN^2+H^2O); it is therefore a hydrated phosphuret of nitrogen.

The composition which is attributed to these two compounds by the German chemists being different from those defended by MM. Gerhardt and Laurent, the former has subjected them to fresh examination, and states as the result that no such compound as phosphuret of nitrogen exists; and that the two bodies improperly so named are mixtures of three definite compounds, one of which he calls ***phosphamide***, containing phosphorus, hydrogen, nitrogen and oxygen; the second, which he names ***biphosphamide***, contains phosphorus, nitrogen and oxygen; and the third, named ***phospham***, consists of phosphorus, nitrogen and hydrogen.

Phosphamide.—When gaseous ammonia is passed over perchloride of phosphorus contained in a long tube, the chloride becomes considerably heated, fuses, gives out much hydrochloric acid gas, and produces much hydrochlorate of ammonia. The product obtained is a white powder, which is to be mixed with water, in which a portion only of it is dissolved and the water is rendered acid; the insoluble portion is impure phosphamide; it is to be purified by boiling for some hours in dilute solution of potash until it ceases to dissolve any chlorine; then with weak nitric acid, and lastly with water.

When dried at 212° its formula is [$PH^3 N^2O$]. M. Liebig obtained only 24·27 per cent. of water by calcining this substance with oxide of copper, whereas M. Gerhardt states that he has never obtained less than 34 per cent.; he also says that he obtained only 28·5 of nitrogen from 100, whereas M. Gerhardt procured 35·5.

Biphosphamide.—If instead of submitting humid phosphamide to the action of heat, it be first dried by the water-bath, it loses all its hydrogen in the form of ammonia; the residue, instead of fusing as in the preceding experiment, undergoes no change of appearance. When submitted to analysis, it is found to have lost half of its nitrogen and the whole of its hydrogen, so that it consists of equal atoms of phosphorus, nitrogen and oxygen. In order to have this compound entirely free from phosphamide, it must be kept for a considerable time at a low red heat. The external characters of this substance do not serve to distinguish it from the preceding; like it, it is a white powder, insoluble in any solvent: it also acts like phosphamide if moistened with water and exposed to heat. When fused with potash, ammonia is disengaged and common phosphate left. The formula of biphosphamide is PNO.

Phospham.—When the product of the action of ammonia on perchloride of phosphorus free from water is calcined in close vessels, hydrochloric acid and hydrochlorate of ammonia are disengaged and a perfectly white residue is obtained, which, according to MM. Liebig and Wöhler, is phosphuret of nitrogen PN^2. According to M. Gerhardt this statement is incorrect; and it contains three things, if particular precautions are not taken, viz. biphosphamide, a compound formed of phosphorus, nitrogen and hydrogen, which M. Gerhardt calls *phospham*, and a chlorinated compound which he has been unable to isolate, but which is converted into phosphamide and hydrochlorate of ammonia by contact with water.

In order to have a product free from chlorine and oxygen, it is requisite to operate on perfectly dry substances, and to finish the action of the ammonia at a very high temperature. M. Gerhardt, when operating under the same circumstances as the German chemists, could never obtain a product free from chlorine, the presence of which they attributed to hydrochlorate of ammonia; but this is an error, for it is derived from the incomplete action between the ammonia and the perchloride of phosphorus.

Phospham contains 1·5 per cent. of hydrogen. MM. Liebig and Wöhler attributed the water which they obtained to the humidity of the oxide of copper which they employed.

Finally, no phosphuret of nitrogen is formed by the action of ammonia or perchloride of phosphorus. When the action of the alkali is complete, a substance remains containing phosphorus, nitrogen and hydrogen, which M. Gerhardt represents by [PHN^2].

Like the previously described compounds, phospham is a white powder, insipid and insoluble in common solvents; it also yields, when fused with potash, common phosphate and ammonia.

In concluding, M. Gerhardt asserts that no phosphuret of nitrogen is formed, as stated by M. Liebig, by the action of ammonia on perchloride of phosphorus, the result of their action containing 1·5 per cent. of hydrogen; and that no such compound as hydrate of phosphuret of nitrogen exists.—*Journ. de Pharm.*, Septembre 1846.

ON ALOETINE. BY M. EDMOND ROBIQUET.

The author states that M. Chevallier has shown that Socotrine aloes contain a great number of substances foreign to its composition, and which renders its purification a long process. The specimen which M. Chevallier examined contained about, in 100 parts—

Pure aloes	85·00
Ulmate of potash	2·00
Sulphate of lime	2·00
Carbonate of potash Carbonate of lime Phosphate of lime	traces.
Gallic acid	0·25
Albumen	8·00
	97·25

In order to obtain aloetine, or the pure juice of aloes, the aloes reduced to fine powder was exhausted with distilled water, in order that, of all the salts which it contains, the filtered liquor should contain only ulmate of potash, and a small quantity of sulphate of lime and carbonate of potash. This solution was evaporated by the water-bath till it had lost half its weight, and an excess of acetate of lead was then added to it. A light flocculent precipitate was formed, which was a mixture of gallate, ulmate, and albuminate of lead, containing traces of carbonate and sulphate of lead.

The solution, besides the juice of aloes, contained acetate of potash and acetate of lime, formed by double decomposition, with excess of acetate of lead; a sufficient quantity of ammonia was added to it; the oxide of lead precipitated carried with it all the aloes contained in the solution, and thus formed a kind of lake of a rather pure orange colour, becoming of a greenish yellow by a few minutes' exposure to solar light. This lake, quickly separated and washed with boiling water, was decomposed out of the contact of the air by a current of sulphuretted hydrogen. This operation being finished, the sulphuret of lead was covered by a stratum of a perfectly colourless liquid, which, carefully decanted and evaporated *in vacuo*, yielded no trace of crystals, and merely dried into a kind of scaly varnish of a very light yellow colour. The product thus prepared was employed by M. Robiquet to examine whether it contained nitrogen, and for analysis by means of oxide of copper: and in order to distinguish it from the crude article of commerce, M. Robiquet gave it the name of *aloetine*.

The properties of aloetine are that, when prepared out of the contact of the air as much as possible, it is in the form of nearly colourless scales, which are very soluble in water and alcohol, slightly dissolved by æther, and perfectly insoluble in fixed and volatile oils; when calcined in close vessels it leaves merely a bulky and brilliant charcoal, which disappears entirely by calcination. Aloetine is not coloured by the salts either of protoxide or peroxide of iron, and gives no precipitate with acetate of lead; in other respects it acts upon reagents similarly to Socotrine aloes.

If aloetine be dried in contact with the air, the scales obtained, instead of being nearly colourless, have an intense red colour derived from a slight absorption of oxygen. Except this difference of colour, aloetine altered by the air undergoes no change of chemical properties, and analysis scarcely shows any difference between them. The purification of aloes may be much shortened by drying it by the water-bath or exposure to the sun, when it is not required to be of the utmost purity.

M. Robiquet gives the following as the results of his analysis of aloetine, and also a formula, which, however, he states as an approximation only:—

	Theory.	Weight of the equivalent.	
Carbon	27·69	450	C^6
Hydrogen	10·77	175	H^{14}
Oxygen	61·54	1000	O^{10}
	100·	1625	$C^6 H^{14} O^{10}$

M. Robiquet states that aloetine possesses in the highest degree the purgative property and bitter taste of common aloes: medicinally employed, 8 parts of aloetine represent 10 parts of Socotrine and 50 parts of Cape aloes.—*Journ. de Pharm. et de Ch.*, Septembre 1846.

WÖHLER'S PROCESS FOR THE PREPARATION OF PURE HYDROCYANIC ACID.

Cyanide of potassium is prepared by fusing together 8 parts of dry ferrocyanide of potassium and 3 parts of pure carbonate of potash containing carbon (ignited cream of tartar), and 1 part of charcoal in fine powder in a covered crucible; the mass while still warm is powdered, and placed in a stoppered bottle of such a capacity that when 6 parts of water are added the bottle is quite filled*. When the cyanide of potassium is dissolved and the metallic iron is all deposited, the clear solution is poured into the retort *a*. The

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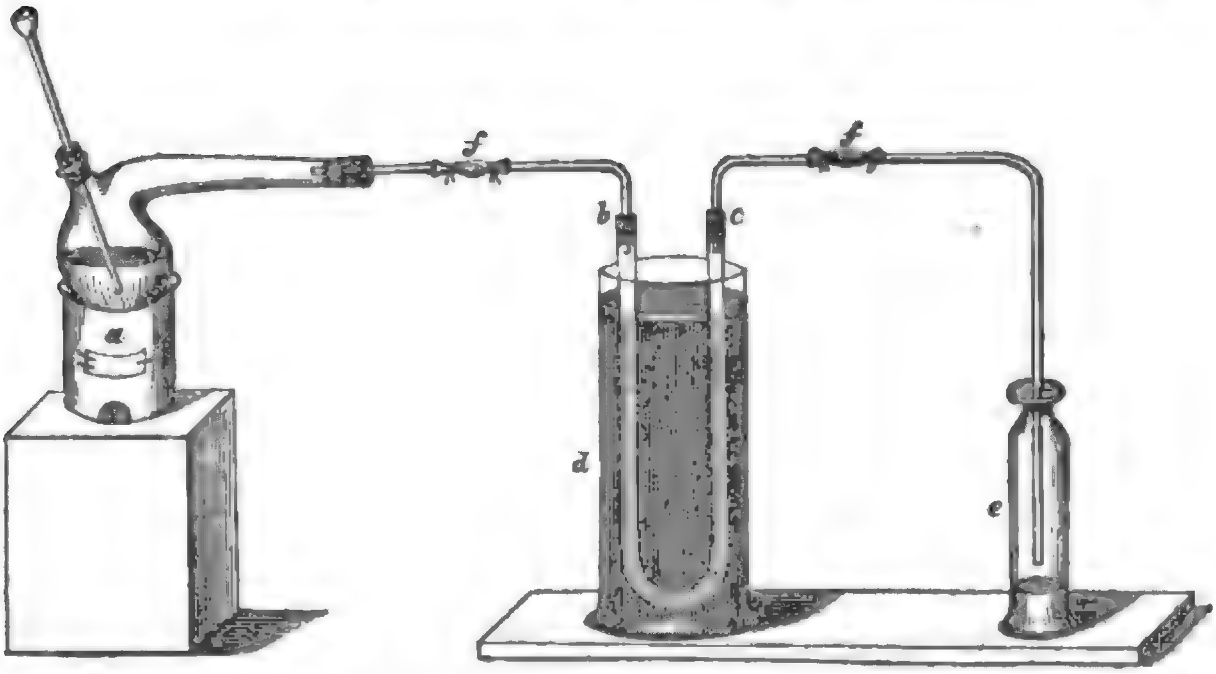
* The cyanide prepared by Liebig's process is less fit for the preparation of prussic acid; on account of the large amount of cyanate of potash it contains, much carbonic acid is disengaged, which renders the condensation of the acid far more difficult.



retort is connected by means of a bent tube with the U-shaped tube *b c*; each arm of this tube is about 18 inches long and $\frac{1}{2}$ to $\frac{3}{4}$ of an inch wide. It is filled with small pieces of fused chloride of calcium, with the exception of the first one-third of *b*, which contains small pieces of the above-mentioned black cyanide of potassium.

This tube stands in a cylindrical vessel *d* of equal height; from the arm *c* proceeds a long and rather narrow conducting tube, bent at a right, or any other convenient angle, into the vessel *e*; by means of the caoutchouc tubes *ff*, the apparatus is made more safe. When the apparatus is thus disposed, and all the junctions and corks found to be perfectly tight, the cylinder *d* is filled with ice-cold water, and the small and narrow flask *e*, destined to receive the acid, is immersed in snow or ice and salt up to the neck.

A cold mixture of equal parts of sulphuric acid and water is now poured through the funnel into the cyanide of potassium in the retort in very small successive portions; for 2 parts of the melted mass of cyanide of potassium 1 part of oil of vitriol is taken; the mass in the retort is so highly heated by this addition of the dilute acid as to begin to boil; the acid must therefore only be added at long intervals, taking care that no air enters with it. It is probable that a concentrated solution of tartaric acid would be preferable to the sulphuric acid. During the addition of the sulphuric acid a considerable quantity of prussic acid is developed, which would be carried out of the apparatus along with air; to avoid this the chloride of calcium tube is placed in cold water and the recipient in ice. When all the acid has been added, and the fluid in the retort no longer boils, the cold water is removed from the cylinder *d* by means of a siphon, and replaced with water at a temperature of 85° to 90° F. By this means the prussic acid previously condensed in the chloride of calcium tube is evaporated, and passes into the recipient *e*; at the same time the contents of the retort are brought to gentle ebullition, which is continued as long as prussic acid is disengaged. The tube *f* may be surrounded with ice also, when conveniently bent, so as to render the condensation of the acid more certain. Without the use of ice the preparation of the anhydrous acid should by no means be attempted; and even then, the operation, and all experiments with this substance, should be conducted with the greatest care and precaution.—*Handwörterbuch der Chemie*, vol. ii. p. 406; and *Chemical Gazette* for Oct. 15, 1846.

PURIFICATION OF ZINC. BY M. DE SMEDT.

The author proposes the following process for obtaining zinc in a state of purity:—Treat the zinc of commerce with concentrated nitric acid, which leaves the tin insoluble in the state of peroxide, and the antimony in that of antimonious acid, while the arsenic becomes arsenic acid, and the zinc, cadmium and iron are dissolved; the solution is to be filtered to separate the oxides of tin and antimony, and great excess of carbonate of ammonia is to be added to it, which precipitates the iron, and the cadmium as a carbonate, while the arsenic acid remains in solution. The solution is to be poured off and evaporated to dryness, and the residue is to be calcined to destroy the ammoniacal salt; the oxide of zinc is to be dissolved in nitric acid, and carbonate of potash added to the solution; the zinc is precipitated and the arsenic acid remains in solution in the state of arseniate. The carbonate of zinc is to be carefully washed, in order to separate very small portions of arseniate which may be mixed with it; it is then to be heated to redness in a crucible to convert it into oxide, which is afterwards reduced to the metallic state by means of hydrogen and heat; for this purpose the oxide is placed in a strong glass or porcelain tube, heated to redness, and is to have a current of pure dry hydrogen gas passed over it.—*Journ. de Chém. Méd.*, Août 1846.

ACTION OF NITRIC ACID ON BRUCINE,—PRODUCTION OF NITROUS ÆTHER, AND CACOTHELINE.

M. Gerhardt states that M. Laurent has confirmed the accuracy of his statement, that nitrous æther is developed when nitric acid is made to act upon brucine. He operated on about 230 grains of brucine, and after having passed the gas generated over lime, he condensed it by means of a mixture of common salt and ice. He obtained about 15 grains of a very fluid liquid, lighter than water, and which possessed the odour of nitrous æther: this liquid was distilled to the last drop, without being made to boil, at a temperature of about 60° Fahr.

When nitric acid has ceased to act, at common temperatures, upon brucine, this alkali is converted into an orange-red substance, which M. Laurent succeeded in causing to crystallize. This new substance he calls *cacotheline*; analysis showed its formula to be $C^{21} H^{22} N^{4} O^{10}$. If from 1 equivalent of brucine and 3 equivalents of nitric acid there be subtracted 1 equivalent of nitrous æther and 1 equivalent of water, the remaining elements represent the composition of cacotheline.

When cacotheline is subjected to the action of ammonia it is converted into various products, among which is an alkaline base possessing several remarkable properties. It contains the elements of the vapour of nitrous æther, and acts, when heated, like substances which have undergone substitution by hyponitrous acid. The small quantity which M. Laurent possessed did not permit an analysis.—*Journ. de Pharm. et de Ch.*, Août 1846.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1846.

Chiswick.—October 1. Foggy: fine: overcast. 2. Hazy: very fine: rain. 3, 4. Very fine. 5. Cloudy. 6. Overcast: rain. 7. Fine: rain. 8. Clear: cloudy: rain. 9. Rain. 10. Boisterous: rain. 11. Fine: rain. 12. Foggy. 13. Slight rain: cloudy: clear. 14. Densely overcast: heavy rain. 15. Rain: cloudy: clear. 16. Clear: fine: rain at night. 17. Slight fog. 18. Heavy and continued rain. 19. Foggy: cloudy and fine: clear. 20. Hazy: very fine: clear. 21. Heavy rain: boisterous. 22. Cloudy: boisterous. 23. Fine: clear: slight frost at night. 24. Rain: fine: rain. 25. Cloudy. 26. Foggy. 27. Hazy: fine: foggy at night. 28. Dense fog. 29. Cloudy: slight rain. 30. Hazy: overcast: clear. 31. Foggy.

Mean temperature of the month	50°·37
Mean temperature of Oct. 1845	49 ·96
Average mean temperature of Oct. for the last twenty years	50 ·43
Average amount of rain in Oct.	2·58 inches.

Boston.—Oct. 1. Fine. 2. Cloudy and foggy: rain A.M. and P.M. 3. Cloudy. 4. Fine. 5. Cloudy: rain P.M. 6. Fine. 7, 8. Cloudy: rain early A.M. 9. Rain: rain early A.M.: rain A.M. 10. Stormy. 11. Fine: rain P.M. 12. Cloudy: rain P.M. 13. Windy. 14. Rain: rain A.M. and P.M. 15. Fine. 16. Fine: rain early A.M. 17. Cloudy: rain early A.M. 18. Cloudy: rain A.M. and P.M. 19. Fine. 20. Rain: rain early A.M. 21. Rain and stormy: rain early A.M.: rain A.M. 22. Cloudy: rain P.M. 23. Cloudy. 24. Rain: rain early A.M. 25. Fine. 26—28. Foggy. 29. Cloudy. 30, 31. Foggy.—The past month has been an extraordinary moist one.

Sandwick Manse, Orkney.—Oct. 1, 2. Cloudy: clear. 3. Bright: clear. 4, 5. Bright: hazy. 6. Cloudy: rain. 7. Clear: hazy: aurora. 8. Bright: clear. 9. Bright: rain. 10. Rain: cloudy. 11. Bright: rain. 12. Cloudy: showers. 13. Cloudy. 14. Rain. 15. Rain: showers: aurora. 16. Bright: clear: aurora. 17. Fine: damp. 18. Bright: cloudy. 19. Rain: cloudy: aurora. 20. Bright: cloudy. 21. Cloudy: rain. 22. Bright: showers: aurora. 23. Showers: cloudy. 24. Cloudy: showers: aurora. 25. Bright: clear: aurora. 26. Frost: cloudy. 27. Frost: bright: clear: aurora. 28. Cloudy. 29, 30. Rain: drops. 31. Showers: cloudy.

Applegarth Manse, Dumfries-shire.—Oct. 1. Rain A.M.: cleared: fine. 2, 3. Fair and fine. 4. Fair, but raw: threatening. 5. Showers. 6. Violent showers P.M. 7. Violent showers P.M.: thunder. 8. Violent showers P.M. 9. Rain all day. 10. Showers. 11. Rain P.M. 12. Showers. 13. Fine and fair. 14. Heavy rain. 15. Fair and fine. 16. Rain P.M.: thunder. 17. Slight showers. 18. Slight showers P.M. 19. Frequent showers. 20. Showers A.M.: cleared. 21. Showers A.M. 22, 23. Fair. 24. Heavy rain. 25. Fair and clear. 26. Frost: clear. 27. Frost: cloudy. 28. Fair and fine. 29. Slight showers. 30. Rain A.M.: cleared. 31. Slight shower P.M.

Mean temperature of the month	49°·5
Mean temperature of Oct. 1845	49 ·6
Mean temperature of Oct. for twenty-three years	46 ·4
Mean rain in Oct. for eighteen years	3¼ inches.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1846. Oct.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	29·937	29·886	29·43	29·70	29·78	29·72	29·72	67	52	53	63½	51	58	53	w.	calm	sw.	sw.	......	......	......	......
2.	29·785	29·610	29·30	29·70	29·47	29·62	29·42	64	55	57	60½	39	55	52	w.	calm	sw.	sw.	·44	......	......	......
3.	29·700	29·657	29·18	29·57	29·62	29·48	29·58	65	36	54	59	41	55	49	w.	calm	sw.	sw.	......	·31	......	......
○ 4.	29·707	29·532	29·28	29·57	29·45	29·63	29·60	67	55	54·5	56½	44	52	51½	sw.	s.	ese.	se.	·08	......	......	......
5.	29·432	29·393	28·95	29·37	29·22	29·59	29·42	65	52	59·5	59½	52	57½	57	s.	e.	ese.	se.	·10	......	......	......
6.	29·544	29·430	28·81	28·99	29·13	29·14	29·10	65	51	61	59	54	58	48½	sw.	w.	sw.	s.	·20	·12	......	......
7.	29·455	29·368	28·87	29·08	28·88	29·08	29·04	63	48	56	55½	48	53	53	sw.	sw.	sw.	se.	·16	·08	......	·24
8.	29·667	29·452	28·87	29·04	29·22	28·98	29·08	61	44	55	54½	47	52½	52	sw.	w.	ese.	nw.	·13	·20	1·20	·19
9.	29·652	29·496	29·17	29·28	29·02	29·33	29·14	62	52	50	58½	48	52½	51½	sw.	calm	ese.	ne.	·20	·07	......	......
10.	29·788	29·447	28·95	28·85	29·40	29·14	29·41	63	46	61	58	55	51½	48	sw.	w.	sw.	n.	·12	·25	......	·35
11.	29·753	29·592	29·20	29·52	29·39	29·72	29·76	63	39	53·5	58	46	48	46	se.	s.	ese.	ne.	·26	......	......	·10
☾ 12.	29·531	29·405	28·96	29·40	29·55	29·90	30·10	59	45	50	57	50½	47	42½	sw.	calm	ese.	nne.	·07	·39	......	·07
13.	29·882	29·803	29·31	29·92	29·78	30·13	29·85	54	36	50	56	40	44½	41	n.	n.	ese.	calm	·01	·11	......	·03
14.	29·501	28·992	29·00	29·05	28·75	29·26	29·00	56	45	45	51½	37	44½	48	s.	calm	sw.	e.	·50	......	......	·05
15.	29·084	28·934	28·56	28·74	28·92	28·98	29·19	56	42	49	55	45	50½	49	s.	sw.	e.	e.	·69	·60	1·90	·45
16.	29·268	29·189	28·80	29·08	29·20	29·31	29·42	64	43	55·5	54½	45	51	46½	sw.	calm	ne.	e.	·40	·05	......	·10
17.	29·470	29·241	28·87	29·32	29·39	29·64	29·67	60	42	56	57	44	51	43½	sw.	calm	nne.	e.	·18	·07	......	......
18.	29·685	29·449	29·11	29·48	29·41	29·66	29·59	51	35	52	56	48	54½	50½	w.	calm	nne.	e.	·85	......	......	......
19.	29·750	29·668	29·24	29·40	29·35	29·33	29·43	63	45	52	58	50	52	52	sw.	w.	se.	se.	·08	1·00	......	·61
● 20.	29·559	29·536	29·06	29·30	29·25	29·38	29·33	62	40	51	54	56	52½	45	sw.	calm	wnw.	calm	·25	·11	......	·08
21.	29·071	28·950	28·65	28·78	28·70	29·13	29·10	57	44	50	53	44	48	50	sw.	s.	se.	e.	·37	·34	......	·02
22.	29·338	29·080	28·52	28·82	29·10	29·13	29·24	50	42	50	52½	44	50½	48½	sw.	s.	nne.	w.	......	·15	......	·56
23.	29·821	29·614	29·15	29·44	29·47	29·42	29·53	56	29	48·5	50	41	44½	43	w.	w.	ne.	s.	·22	·15	0·62	·07
24.	29·523	29·479	29·16	29·28	29·45	29·63	29·89	56	40	47	50	41½	43½	44	sw.	w.	ne.nw	ne.	·12	·28	......	......
25.	29·864	29·687	29·34	29·80	29·89	30·06	30·00	53	41	45·5	53	40½	44	38	nw.	calm	n.	ne.	·08	·10	......	·08
26.	30·149	29·968	29·61	29·80	30·01	29·87	30·09	47	39	40	49½	31½	42	48	n.	calm	ne.	w.	......	......	......	......
☽ 27.	30·260	30·136	29·85	30·12	30·10	30·18	30·06	48	32	37	50	33	48½	46½	n.	calm	ene.	se.	·01	......	......	·04
28.	30·211	30·109	29·88	30·00	29·92	29·95	29·98	52	39	38	52	45	51	50	s.	calm	ne.	calm	......	......	......	......
29.	30·028	29·974	29·72	30·02	30·07	30·12	30·12	50	45	40	52	44	48	47	se.	calm	nw.	calm	·01	......	......	·13
30.	30·130	30·102	29·72	30·03	30·00	29·96	29·85	54	33	47	50	39	48	51	n.	calm	sse.	s.	......	......	0·47	......
31.	30·131	30·071	29·78	29·88	29·80	29·66	29·58	44	39	36	56	41	53	53½	w.	calm	s.	s.	·01	......	......	·38

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