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

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L.&E. &c.
RICHARD TAYLOR, F.L.S. G.S. Astr.S. Nat.H.Mosc. &c.
SIR ROBERT KANE, M.D. M.R.I.A.
WILLIAM FRANCIS, Ph.D. F.L.S. F.R.A.S. F.C.S.

"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. II.—FOURTH SERIES.

JULY—DECEMBER, 1851.

LONDON.

RICHARD TAYLOR, RED LION COURT, FLEET STREET,
Printer and Publisher to the University of London;

SOLD BY LONGMAN, BROWN, GREEN, AND LONGMANS; SIMPKIN, MARSHALL AND CO.; S. HIGHLEY; WHITTAKER AND CO.; AND SHERWOOD, GILBERT, AND PIPER, LONDON: — BY ADAM AND CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH AND SON, GLASGOW; HODGES AND SMITH, DUBLIN; AND WILEY AND PUTNAM, NEW YORK.

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

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

J. B. Pinelli ad Mazonium.

BRITISH MUSEUM NATURAL HISTORY BRANCH

CONTENTS OF VOL. II.

(FOURTH SERIES.)

NUMBER VIII.—JULY 1851.

NUMBER IX.—AUGUST.

NUMBER X.—SEPTEMBER.

NUMBER XI.—OCTOBER.

NUMBER XII.—NOVEMBER.

NUMBER XIII.—DECEMBER.

NUMBER XIV.—SUPPLEMENT TO VOL. II.

ERRATA IN VOL. I.

Page 516, line 11 from bottom, *for* The first were extinguished; *read* The first being extinguished,

— 516, note, line 5 from bottom, *for* from that planet *read* from that body.

ERRATA IN VOL II.

Page 144, line 17 from top, *for*

$$a_{\phi p+\eta} . \beta_{\theta p+\eta} \times a_{\psi p+\eta} . \beta_{\theta p-\zeta}.$$

read

$$a_{\phi p+\eta} . \beta_{\theta p+\eta} \times a_{\phi p+\eta} . \beta_{\theta p-\zeta}.$$

Page 270, line 23 from top, *for* zinc *read* hydrogen.

— 420, — 14 from bottom, *for* level-edged *read* bevel-edged.

— 421, — 6 from bottom, *for* observing media *read* absorbing media.

PLATE.

Illustrative of Mr. H. E. Strickland's Paper on the Elevatory Forces which raised the Malvern Hills.

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JULY 1851.

I. *On the Moving Force of Heat, and the Laws regarding the Nature of Heat itself which are deducible therefrom.* By R. Clausius*.

THE steam-engine having furnished us with a means of converting heat into a motive power, and our thoughts being thereby led to regard a certain quantity of work as an equivalent for the amount of heat expended in its production, the idea of establishing theoretically some fixed relation between a quantity of heat and the quantity of work which it can possibly produce, from which relation conclusions regarding the nature of heat itself might be deduced, naturally presents itself. Already, indeed, have many instructive experiments been made with this view; I believe, however, that they have not exhausted the subject, but that, on the contrary, it merits the continued attention of physicists; partly because weighty objections lie in the way of the conclusions already drawn, and partly because other conclusions, which might render efficient aid towards establishing and completing the theory of heat, remain either entirely unnoticed, or have not as yet found sufficiently distinct expression.

The most important investigation in connexion with this subject is that of S. Carnot†. Later still, the ideas of this author have been represented analytically in a very able manner by Clapeyron‡. Carnot proves that whenever work is produced by heat, and a permanent alteration of the body in action does not at the same time take place, a certain quantity of heat passes

* Translated from Poggendorff's *Annalen*, vol. lxxix. p. 368.

† *Reflexions sur la puissance motrice du feu, et sur les Machines propres à déveloper cette puissance*, par S. Carnot. Paris, 1824.

‡ *Journ. de l'École Polytechnique*, vol. xix. (1834); and Taylor's Scientific Memoirs, Part III. p. 347.

6+

from a warm body to a cold one; for example, the vapour which is generated in the boiler of a steam-engine, and passes thence to the condenser where it is precipitated, carries heat from the fireplace to the condenser. This *transmission* Carnot regards as the change of heat corresponding to the work produced. He says expressly, that *no heat is lost* in the process, that the quantity remains unchanged; and he adds, "This is a fact which has never been disputed; it is first assumed without investigation, and then confirmed by various calorimetric experiments. To deny it, would be to reject the entire theory of heat, of which it forms the principal foundation."

I am not, however, sure that the assertion, that in the production of work a loss of heat never occurs, is sufficiently established by experiment. Perhaps the contrary might be asserted with greater justice; that although no such loss may have been directly proved, still other facts render it exceedingly probable that a loss occurs. If we assume that heat, like matter, cannot be lessened in quantity, we must also assume that it cannot be increased; but it is almost impossible to explain the ascension of temperature brought about by friction otherwise than by assuming an actual increase of heat. The careful experiments of Joule, who developed heat in various ways by the application of mechanical force, establish almost to a certainty, not only the possibility of increasing the quantity of heat, but also the fact that the newly-produced heat is proportional to the work expended in its production. It may be remarked further, that many facts have lately transpired which tend to overthrow the hypothesis that heat is itself a body, and to prove that it consists in a motion of the ultimate particles of bodies. If this be so, the general principles of mechanics may be applied to heat; this motion may be converted into work, the loss of *vis viva* in each particular case being proportional to the quantity of work produced.

These circumstances, of which Carnot was also well aware, and the importance of which he expressly admitted, pressingly demand a comparison between heat and work, to be undertaken with reference to the divergent assumption that the production of work is not only due to an alteration in the *distribution* of heat, but to an actual *consumption* thereof; and inversely, that by the consumption of work heat may be *produced*.

In a recent memoir by Holtzmann*, it seemed at first as if the author intended to regard the subject from this latter point of view. He says (p. 7), "the effect of the heat which has been communicated to the gas is either an increase of temperature

* *Ueber die Wärme und Elasticität der Gase und Dämpfe*, von C. Holtzmann. Manheim, 1845. Also Taylor's Scientific Memoirs, Part XIV. p. 189.

combined with an increase of elasticity, or a mechanical work, or a combination of both; a mechanical work being the equivalent for an increase of temperature. Heat can only be measured by its effects; and of the two effects mentioned, mechanical work is peculiarly applicable here, and shall therefore be chosen as a standard in the following investigation. I name a unit of heat, the quantity which, on being communicated to any gas, is able to produce the quantity of work *a*; or to speak more definitely, which is able to raise *a* kilogrammes to a height of one metre." Afterwards, at page 12, he determines the numerical value of the constant *a*, according to the method of Meyer*, and obtains a number which completely agrees with that obtained in a manner totally different by Joule. In carrying out the theory, however, that is, in developing the equations by means of which his conclusions are arrived at, he proceeds in a manner similar to Clapeyron, so that the assumption that the quantity of heat is constant is still tacitly retained.

The difference between both ways of regarding the subject has been laid hold of with much greater clearness by W. Thomson, who has applied the recent discoveries of Regnault on the tension and latent heat of steam to the completing of the memoir of Carnot†. Thomson mentions distinctly the obstacles which lie in the way of an unconditional acceptance of Carnot's theory, referring particularly to the investigations of Joule, and dwelling on one principal objection to which the theory is liable. If it be even granted that the production of work, where the body in action remains in the same state after the production as before, is in all cases accompanied by a transmission of heat from a warm body to a cold one, it does not follow that by every such transmission work is produced, for the heat may be carried over by simple conduction; and in all such cases, if the transmission alone were the true equivalent of the work performed, an absolute loss of mechanical force must take place in nature, which is hardly conceivable. Notwithstanding this, however, he arrives at the conclusion, that in the present state of science the principle assumed by Carnot is the most probable foundation for an investigation on the moving force of heat. He says, "If we forsake this principle, we stumble immediately on innumerable other difficulties, which, without further experimental investigations, and an entirely new erection of the theory of heat, are altogether insurmountable."

I believe, nevertheless, that we ought not to suffer ourselves to be daunted by these difficulties; but that, on the contrary, we must look steadfastly into this theory which calls heat a motion, as in this way alone can we arrive at the means of establishing

* *Ann. der Chim. und Pharm.*, vol. xlii. p. 239.

† Transactions of the Royal Society of Edinburgh, vol. xvi.

it or refuting it. Besides this, I do not imagine that the difficulties are so great as Thomson considers them to be; for although a certain alteration in our way of regarding the subject is necessary, still I find that this is in no case contradicted by *proved facts.* It is not even requisite to cast the theory of Carnot overboard; a thing difficult to be resolved upon, inasmuch as experience to a certain extent has shown a surprising coincidence therewith. On a nearer view of the case, we find that the new theory is opposed, not to the real fundamental principle of Carnot, but to the addition "no heat is lost;" for it is quite possible that in the production of work both may take place at the same time; a certain portion of heat may be consumed, and a further portion transmitted from a warm body to a cold one; and both portions may stand in a certain definite relation to the quantity of work produced. This will be made plainer as we proceed; and it will be moreover shown, that the inferences to be drawn from both assumptions may not only exist together, but that they mutually support each other.

1. *Deductions from the principle of the equivalence of heat and work.*

We shall forbear entering at present on the nature of the motion which may be supposed to exist within a body, and shall assume generally that a motion of the particles does exist, and that heat is the measure of their *vis viva.* Or yet more general, we shall merely lay down one maxim which is founded on the above assumption:—

In all cases where work is produced by heat, a quantity of heat proportional to the work done is expended; and inversely, by the expenditure of a like quantity of work, the same amount of heat may be produced.

Before passing on to the mathematical treatment of this maxim, a few of its more immediate consequences may be noticed, which have an influence on our entire notions as to heat, and which are capable of being understood, without entering upon the more definite proofs by calculation which are introduced further on.

We often hear of the *total heat* of bodies, and of gases and vapours in particular, this term being meant to express the sum of the sensible and latent heat. It is assumed that this depends solely upon the present condition of the body under consideration; so that when all other physical properties thereof, its temperature, density, &c. are known, the total quantity of heat which the body contains may also be accurately determined. According to the above maxim, however, this assumption cannot be admitted. If a body in a certain state, for instance a quantity of gas at the temperature t_0 and volume v_0, be subjected to various alterations as regards temperature and volume, and brought at the conclusion into its original state, the sum of its sensible and latent heats must, according to the above assumption, be the same as before; hence, if during any portion of the process heat be communicated from without, the quantity thus received must be given off again during some other portion of the process. With every alteration of volume, however, a certain quantity of work is either produced or expended by the gas; for by its expansion an outward pressure is forced back, and on the other hand, compression can only be effected by the advance of an outward pressure. If, therefore, alteration of volume be among the changes which the gas has undergone, work must be produced and expended. It is not, however, necessary that at the conclusion, when the original condition of the gas is again established, the entire amount of work produced should be exactly equal to the amount expended, the one thus balancing the other; an excess of one or the other will be present if the compression has taken place at a lower or a higher temperature than the expansion, as shall be proved more strictly further on. This excess of produced or expended work must, according to the maxim, correspond to a proportionate excess of expended or produced heat, and hence the amount of heat refunded by the gas cannot be the same as that which it has received.

There is still another way of exhibiting this divergence of our maxim from the common assumption as to the *total heat* of bodies. When a gas at t_0 and v_0 is to be brought to the higher temperature t_1 and the greater volume v_1, the quantity of heat necessary to effect this would, according to the usual hypothesis, be quite independent of the manner in which it is communicated. By the above maxim, however, this quantity would be different according as the gas is first heated at the constant volume v_0 and then permitted to expand at the constant temperature t_1, or first expanded at the temperature t_0 and afterwards heated to t_1; the quantity of heat varying in all cases with the manner in which the alterations succeed each other.

In like manner, when a quantity of water at the temperature t_0 is to be converted into vapour of the temperature t_1 and the volume v_1, it will make a difference in the amount of heat necessary if the water be heated first to t_1 and then suffered to evaporate, or if it be suffered to evaporate by t_0 and the vapour heated afterwards to t_1; or finally, if the evaporation take place at any intermediate temperature.

From this and from the immediate consideration of the maxim, we can form a notion as to the light in which *latent* heat must be regarded. Referring again to the last example, we distinguish in the quantity of heat imparted to the water during the change the *sensible* heat and the *latent* heat. Only the former of these, however, must we regard as present in the produced steam; the second is, not only as its name imports, hidden from our perceptions, but has actually *no existence*; during the alteration it has been *converted into work*.

We must introduce another distinction still as regards the heat expended. The work produced is of a twofold nature. In the first place, a certain quantity of work is necessary to overcome the mutual attraction of the particles, and to separate them to the distance which they occupy in a state of vapour. Secondly, the vapour during its development must, in order to procure room for itself, force back an outer pressure. We shall name the former of these *interior work*, and the latter *exterior work*, and shall distribute the latent heat also under the same two heads.

With regard to the *interior* work, it can make no difference whether the evaporation takes place at t_0 or at t_1, or at any other intermediate temperature, inasmuch as the attraction of the particles must be regarded as invariable*. The *exterior* work, on the contrary, is regulated by the pressure, and therefore by the temperature also. These remarks are not restricted to the example we have given, but are of general application; and when it was stated above, that the quantity of heat necessary to bring a body from one condition into another depended, not upon the state of the body at the beginning and the end alone, but upon the manner in which the alterations had been carried on throughout, this statement had reference to that portion only of the *latent heat* which corresponds to the *exterior* work. The remainder of the latent heat and the entire amount of sensible heat are independent of the manner in which the alteration is effected.

When the vapour of water at t_1 and v_1 is reconverted into water at t_0, the reverse occurs. Work is here *expended*, inasmuch as the particles again yield to their attraction, and the outer pressure once more advances. In this case, therefore, heat must be produced; and the *sensible heat* which here exhibits itself does not come from any retreat in which it was previously concealed, but is *newly produced*. It is not necessary that the heat developed by this reverse process should be equal to that consumed by the other; that portion which corresponds to the *exterior* work may be greater or less according to circumstances.

We shall now turn to the mathematical treatment of the subject, confining ourselves, however, to the consideration of per-

* It must not be objected here that the cohesion of the water at t_1 is less than at t_0, and hence requires a less amount of work to overcome it. The lessening of the cohesion implies a certain work performed by the warming of the water as water, and this must be added to that produced by evaporation. From this it follows, that of the heat which the water receives from without, only one portion must be regarded as sensible, while the other portion goes to loosen the cohesion. This view is in harmony with the fact, that water possesses a so much greater specific heat than ice, and probably than steam also.

manent gases, and of vapours at their maximum density; as besides possessing the greatest interest, our superior knowledge of these recommends them as best suited to the calculus. It will, however, be easy to see how the maxim may be applied to other cases also.

Let a certain quantity of *permanent gas*, say a unit of weight, be given. To determine its present condition, three quantities are necessary; the pressure under which it exists, its volume, and its temperature. These quantities stand to each other in a relation of mutual dependence, which, by a union of the laws of Mariotte and Gay-Lussac*, is expressed in the following equation:

$$pv = \mathrm{R}(a+t), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{I.})$$

where p, v, *and* t express the pressure, volume, and temperature of the gas in its present state, a a constant equal for all gases, and R also a constant, which is fully expressed thus, $\frac{p_0 v_0}{a+t_0}$, where p_0, v_0, and t_0 express contemporaneous values of the above three quantities for any other condition of the gas. This last constant is therefore different for different gases, being inversely proportional to the specific weight of each.

It must be remarked, that Regnault has recently proved, by a series of very careful experiments, that this law is not in all strictness correct. The deviations, however, for the permanent gases are very small, and exhibit themselves principally in those cases where the gas admits of condensation. From this it would seem to follow, that the more distant, as regards pressure and temperature, a gas is from its point of condensation, the more correct will be the law. Its accuracy for permanent gases in their common state is so great, that it may be regarded as perfect; for every gas a limit may be imagined, up to which the law is also perfectly true; and in the following pages, where the permanent gases are treated as such, we shall assume the existence of this ideal condition.

The value $\frac{1}{a}$ for atmospheric air is found by the experiments both of Magnus and Regnault to be $=0{\cdot}003665$, the temperature being expressed by the centesimal scale reckoned from the freezing-point upwards. The gases, however, as already mentioned, not following strictly the law of M. and G., we do not always obtain the same value for $\frac{1}{a}$ when the experiment is repeated under different circumstances. The number given above is true for the case when the air is taken at a temperature of 0° under the pressure of *one* atmosphere, heated to a temperature

* This shall be expressed in future briefly thus—the law of M. and G.; and the law of Mariotte alone thus—the law of M.

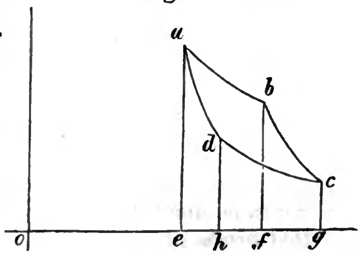
of 100°, and the increase of expansive force observed. If, however, the pressure be allowed to remain constant, and the increase of volume observed, we obtain the somewhat higher value 0·003670. Further, the values increase when the experiments are made under a pressure exceeding that of the atmosphere, and decrease when the pressure is less. It is clear from this, that the exact value for the ideal condition, where the differences pointed out would of course disappear, cannot be ascertained. It is certain, however, that the number 0·003665 is not far from the truth, especially as it very nearly agrees with the value found for hydrogen, which, perhaps of all gases, approaches nearest the ideal condition. Retaining, therefore, the above value for $\frac{1}{a}$, we have

$$a=273.$$

One of the quantities in equation (I.), for instance p, may be regarded as a function of the two others; the latter will then be the independent variables which determine the condition of the gas. We will now endeavour to ascertain in what manner the quantities which relate to the *amount of heat* depend upon v and t.

When any body whatever changes its volume, the change is always accompanied by a mechanical work produced or expended. In most cases, however, it is impossible to determine this with accuracy, because an unknown *interior* work usually goes on at the same time with the *exterior*. To avoid this difficulty, Carnot adopted the ingenious contrivance before alluded to: he allowed the body to undergo various changes, and finally brought it into its primitive state; hence if by any of the changes *interior* work was produced, this was sure to be exactly nullified by some other change; and it was certain that the quantity of *exterior* work which remained over and above was the total quantity produced. Clapeyron has made this very evident by means of a diagram: we propose following his method with permanent gases in the first instance, introducing, however, some slight modifications rendered necessary by our maxim.

Fig. 1.

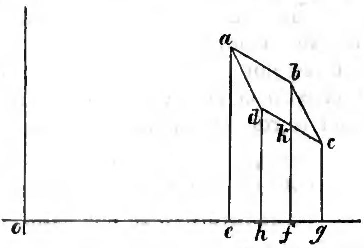


In the annexed figure let oe represent the volume, and ea the pressure of the unit weight of gas when its temperature is t; let us suppose the gas to be contained in an expansible bag, with which, however, no exchange of heat is possible. If the gas be permitted to expand, no new heat being added, the temperature will fall. To avoid this, let the bag during the expansion be brought into contact with a body A of the temperature t, from which it shall receive heat sufficient to preserve it constant at the same temperature. While this expansion by constant temperature proceeds, the pressure decreases according to the law of M., and may be represented by the ordinate of a curve ab, which is a portion of an equilateral hyperbola. When the gas has increased in volume from oe to of, let the body A be taken away, and the expansion allowed to proceed still further without the addition of heat; the temperature will now sink, and the pressure consequently grow less as before. Let the law according to which this proceeds be represented by the curve bc. When the volume of the gas has increased from of to og, and its temperature is lowered from t to τ, let a pressure be commenced to bring it back to its original condition. Were the gas left to itself, its temperature would now rise; this, however, must be avoided by bringing it into contact with the body B at the temperature τ, to which any excess of heat will be immediately imparted, the gas being thus preserved constantly at τ. Let the compression continue till the volume has receded to h, it being so arranged that the decrease of volume indicated by the remaining portion he shall be just sufficient to raise the gas from τ to t, if during this decrease it gives out no heat. By the first compression the pressure increases according to the law of M., and may be represented by a portion cd of another equilateral hyperbola. At the end the increase is quicker, and may be represented by the curve da. This curve must terminate exactly in a; for as the volume and temperature at the end of the operation have again attained their original values, this must also be the case with the pressure, which is a function of both. The gas will therefore be found in precisely the same condition as at the commencement.

In seeking to determine the amount of work performed by these alterations, it will be necessary, for the reasons before assigned, to direct our attention to the *exterior* work alone. During the expansion, the gas *produces* a work expressed by the integral of the product of the differential of the volume into the corresponding pressure, which product is represented geometrically by the quadrilaterals ea, bf and $fbcg$. During the compression, however, work will be *expended*, which is represented by the quadrilaterals $gcdh$ and $hdae$. The excess of the former work above the latter is to be regarded as the entire work produced by the alterations, and this is represented by the quadrilateral $abcd$.

If the foregoing process be reversed, we obtain at the conclusion the same quantity $abcd$ as the excess of the work *expended* over that *produced*.

Fig. 2.



In applying the foregoing considerations analytically, we will assume that the various alterations which the gas has undergone have been *infinitely small.* We can then consider the curves before mentioned to be straight lines, as shown in the accompanying figure. In determining its superficial content, the quadrilateral *abcd* may be regarded as a parallelogram, for the error in this case can only amount to a differential of the *third* order, while the area itself is a differential of the *second* order. The latter may therefore be expressed by the product *ef.bk*, where *k* marks the point at which the ordinate *bf* cuts the lower side of the parallelogram. The quantity *bk* is the increase of pressure due to the raising of the constant volume *of* from τ to t, that is to say, due to the differential $t-\tau=dt$. This quantity can be expressed in terms of v and t by means of equation (I.), as follows:

$$dp=\frac{\mathrm{R}dt}{v}.$$

If the increase of volume *ef* be denoted by dv, we obtain the content of the quadrilateral, and with it

$$\textit{The work produced}=\frac{\mathrm{R}\,dv\,dt}{v}. \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

We must now determine the quantity of heat consumed during those alterations. Let the amount of heat which must be imparted to change the gas by a definite process from any given state to another, in which its volume is $=v$ and its temperature $=t$, be called Q; and let the changes of volume occurring in the process above described, which are now to be regarded separately, be denoted as follows: *ef* by dv, *hg* by $d'v$, *eh* by δv, and *fg* by $\delta' v$. During an expansion from the volume $oe=v$ to $of=v+dv$, at the constant temperature t, the gas must receive the quantity of heat expressed by

$$\left(\frac{d\mathrm{Q}}{dv}\right)dv;$$

and in accordance with this, during an expansion from $vh=v+\delta v$ to $og=v+\delta v+d'v$ at the temperature $t-dt$, the quantity

$$\left[\frac{d\mathrm{Q}}{dv}+\frac{d}{dv}\left(\frac{d\mathrm{Q}}{dv}\right)\delta v-\frac{d}{dt}\left(\frac{d\mathrm{Q}}{dv}\right)dt\right]d'v.$$

In our case, however, instead of an expansion, a compression has taken place; hence this last expression must be introduced with the negative sign. During the expansion from *of* to *og*, and the compression from *oh* to *oe*, heat has been neither received nor given away; the amount of heat which the gas has received over and above that which it has communicated, or, in other words, *the quantity of heat consumed*, will therefore be

$$\left(\frac{dQ}{dv}\right)dv-\left[\left(\frac{dQ}{dv}\right)+\frac{d}{dv}\left(\frac{dQ}{dv}\right)\delta v-\frac{d}{dt}\left(\frac{dQ}{dv}\right)dt\right]d'v. \quad (2.)$$

The quantities δv and $d'v$ must now be eliminated; a consideration of the figure furnishes us with the following equation:

$$dv+\delta' v=\delta v+d'v.$$

During its compression from *oh* to *oe*, consequently during its expansion under the same circumstances from *oe* to *oh*, and during the expansion from *of* to *og*, both of which cause a decrease of temperature *dt*, the gas neither receives nor communicates heat: from this we derive the equations

$$\left(\frac{dQ}{dv}\right)\delta v-\left(\frac{dQ}{dt}\right)dt=0$$

$$\left[\left(\frac{dQ}{dv}\right)+\frac{d}{dv}\left(\frac{dQ}{dv}\right)dv\right]\delta' v-\left[\left(\frac{dQ}{dt}\right)+\frac{d}{dv}\left(\frac{dQ}{dt}\right)dv\right]dt=0.$$

From these three equations and equation (2.) the quantities $d'v$, δv and $\delta' v$, may be eliminated; neglecting during the process all differentials of a higher order than the second, we obtain

$$\textit{The heat expended}=\left[\frac{d}{dt}\left(\frac{dQ}{dv}\right)-\frac{d}{dv}\left(\frac{dQ}{dt}\right)\right]dv\,dt. \quad . \quad (3.)$$

Turning now to our maxim, which asserts that the production of a certain quantity of work necessitates the expenditure of a proportionate amount of heat, we may express this in the form of an equation, thus:

$$\frac{\text{The heat expended}}{\text{The work produced}}=A, \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

where A denotes a constant which expresses the equivalent of heat for the unit of work. The expressions (1.) and (3.) being introduced into this equation, we obtain

$$\frac{\left[\frac{d}{dt}\left(\frac{dQ}{dv}\right)-\frac{d}{dv}\left(\frac{dQ}{dt}\right)\right]dv\,dt}{\frac{R.dv\,dt}{v}}=A,$$

or

$$\frac{d}{dt}\left(\frac{dQ}{dv}\right)-\frac{d}{dv}\left(\frac{dQ}{dt}\right)=\frac{A.R}{v}. \quad . \quad . \quad . \quad . \quad . \quad (II.)$$

This equation may be regarded as the analytical expression of the above maxim applicable to the case of permanent gases. It shows that Q cannot be a function of v and t as long as the two latter are independent of each other. For otherwise, according to the known principle of the differential calculus, that when a function of two variables is differentiated according to both, the order in which this takes place is matter of indifference, the right side of the equation must be equal 0.

The equation can be brought under the form of a *complete differential,* thus:

$$dQ = dU + A.R\frac{a+t}{v}dv, \quad . \quad . \quad . \quad . \quad \text{(II}a.\text{)}$$

where U denotes an arbitrary function of v and t. This differential equation is of course unintegrable until we find a second condition between the variables, by means of which t may be expressed as a function of v. This is due, however, to the last member alone, and this it is which corresponds to the *exterior* work effected by the alteration; for the differential of this work is pdv, which, when p is eliminated by means of (I.), becomes

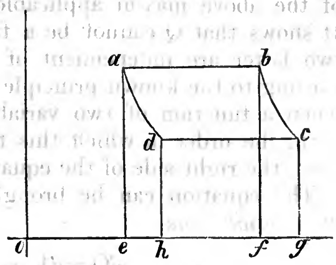
$$\frac{R(a+t)}{v}dv.$$

It follows, therefore, in the first place, from (IIa.), that the entire quantity of heat, Q, absorbed by the gas during a change of volume and temperature may be decomposed into two portions. One of these, U, which comprises the *sensible* heat and the heat necessary for *interior* work, if such be present, fulfils the usual assumption, it is a function of v and t, and is therefore determined by the state of the gas at the beginning and at the end of the alteration; while the other portion, which comprises the heat expended on *exterior* work, depends, not only upon the state of the gas at these two limits, but also upon the manner in which the alterations have been effected throughout. It is shown above that the same conclusion flows directly from the maxim itself.

Before attempting to make this equation suited to the deduction of further inferences, we will develope the analytical expression of the maxim applicable to *vapours at their maximum density*.

In this case we are not at liberty to assume the correctness of the law of M. and G., and must therefore confine ourselves to the maxim alone. To obtain an equation from this, we will again pursue the course indicated by Carnot, and reduced to a diagram by Clapeyron. Let a vessel impervious to heat be partially filled with water, leaving a space above for steam of the maximum density corresponding to the temperature t. Let the volume of both together be represented in the annexed figure by the abscissa *oe*, and the pressure of the steam by the ordinate *ea*. Let the vessel be now supposed to expand, while both fluid and steam are kept in contact with a body A of the constant temperature *t*. As the space increases, more fluid is evaporated, the necessary amount of latent heat being supplied by the body A; so that the temperature, and consequently the pressure of the steam, may remain unchanged. When the entire volume is increased in this manner from *oe* to *of*, an exterior work is produced which is represented by the rectangle *ea bf*. Let the body A be now taken away, and let the vessel continue to expand without heat being either given or received. Partly by the expansion of the steam already present, and partly by the formation of new steam, the temperature will be lowered and the pressure become less. Let the expansion be suffered to continue until the temperature passes from t to τ, and let *og* represent the volume at this temperature. If the decrease of pressure during this expansion be represented by the curve *bc*, the exterior work produced by it will be represented by *fbcg*.

Fig. 3.

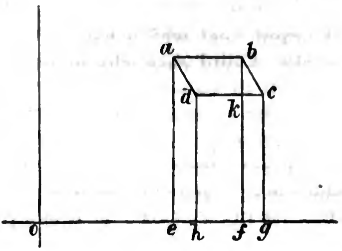


Let the vessel be now pressed together so as to bring the fluid and vapour to their original volume *oe*, and during a portion of the process let the vessel be in contact with a body, B, of the temperature τ, to which any excess of heat shall be immediately imparted, and the temperature of the fluid and vapour kept constant at τ. During the other portion of the process, let the body B be withdrawn so that the temperature may rise; let the first compression continue till the volume has been reduced to *oh*, it being so arranged that the remaining space *he* shall be just sufficient to raise the temperature from τ to t. During the first decrease of volume the pressure remains constant at *gc*, and the quantity of exterior work expended is equal to the rectangle *gc dh*. During the last decrease of volume the pressure increases, and may be represented by the curve *da*, which must terminate exactly in the point *a*, as the original temperature t must again correspond to the original pressure *ea*. The exterior work expended in this case is $=$*hdae*.

At the end of the operation both fluid and vapour are in the same state as at the commencement, so that the excess of the *exterior* work produced over the amount expended expresses the *total amount* of work accomplished. This excess is represented by the quadrilateral *abcd*, the content of which must therefore be compared with the *heat expended* at the same time.

For this purpose let it be assumed, as before, that the described alterations are infinitely small, and under this view let the process be represented by the annexed figure, in which the curves *ad* and *bc* shown in fig. 3 have passed into straight lines. With regard to the content of the quadrilateral *abcd*, it may be again regarded as a parallelogram, the area of which is expressed by the product *ef.bk*. Now if, when the temperature is t, the pressure of the vapour at its maximum tension be equal to p, and the difference of temperature $t-\tau$ be expressed by dt, we have

Fig. 4.



$$bk=\frac{dp}{dt}dt\,;$$

ef is the increase of volume caused by the passing of a certain quantity of fluid represented by dm into a state of vapour. Let the volume of the unit of steam at its maximum density for the temperature t be called s, and the volume of the same quantity of fluid at the temperature t be called σ; then is

$$ef=(s-\sigma)dm\,;$$

and hence the content of the rectangle, or

$$\textit{The work produced}=(s-\sigma)\frac{dp}{dt}\,dm\,dt. \quad . \quad . \quad (5.)$$

To express the amount of heat, we will introduce the following notation:—Let the quantity of heat rendered latent by the passage of a unit weight of fluid at the temperature t, and under a corresponding pressure into a state of vapour, be called r, and the specific heat of the fluid c; both of these quantities, as also the foregoing s, σ, and $\frac{dp}{dt}$, being functions of t. Finally, let the quantity of heat which must be communicated to a unit weight of vapour of water to raise it from the temperature t to $t+dt$,—the vapour being preserved by pressure at the maximum density due to the latter temperature without precipitation,—be called hdt, where h likewise represents a function of t. We shall refer the question as to whether its value is positive or negative to future consideration.

If we name the mass of fluid originally present in the vessel μ, and the mass of the vapour m; further, the mass evaporated during the expansion from *oe* to *of*, dm, and the mass precipitated by the compression from *og* to *oh*, $d'm$, we obtain in the first case the quantity

$$rdm$$

of latent heat which has been extracted from the body A; and in the second case, the quantity

$$\left(r-\frac{dr}{dt}dt\right)d'm$$

of sensible heat which has been imparted to the body B. By the other expansion and contraction heat is neither gained nor lost; hence at the end of the process we have

$$\textit{The heat expended} = rdm-\left(r-\frac{dr}{dt}dt\right)d'm. \quad . \quad (6.)$$

In this equation the differential $d'm$ must be expressed through dm and dt; the conditions under which the second expansion and the second contraction have been carried out enables us to do this. Let the mass of vapour precipitated by the compression from *oh* to *oe*, and which therefore would develope itself by expansion from *oe* to *oh*, be represented by δm, and the mass developed by the expansion from *of* to *og* by $\delta' m$; then, as at the conclusion of the experiment the original mass of fluid and of vapour must be present, we obtain in the first place the equation

$$dm+\delta' m=d'm+\delta m.$$

Further, for the expansion from *oe* to *oh*, as the temperature of the fluid mass μ and the mass of vapour m must thereby be lessened the quantity dt without heat escaping, we obtain the equation

$$r\delta m-\mu . cdt-m . hdt=0;$$

and in like manner for the expansion from *of* to *og*, as here we have only to set $\mu-dm$ and $m+dm$ in the place of μ and m, and $\delta' m$ in the place of δm, we obtain

$$r\delta' m-(\mu-dm)cdt-(m+dm)hdt=0.$$

If from these three equations and equation (6.) the quantities $d'm$, δm and $\delta' m$, be eliminated, and all differentials of a higher order than the second be neglected, we have

$$\textit{The heat expended} = \left(\frac{dr}{dt}+c-h\right)dm\,dt. \quad . \quad . \quad (7.)$$

The formulæ (7.) and (5.) must now be united, as in the case of permanent gases, thus:

$$\frac{\left(\frac{dr}{dt}+c-h\right)dm\,dt}{(s-\sigma)\frac{dp}{dm}dm\,dt}=\text{A};$$

and hence we obtain, as the analytical expression of the maxim, applicable to vapours at their maximum density, the equation

$$\frac{dr}{dt}+c-h=\mathrm{A}(s-\sigma)\frac{dp}{dt}. \quad . \quad . \quad . \quad . \qquad \text{(III.)}$$

If, instead of the above maxim, the assumption that the quantity of heat is *constant* be retained, then, according to (7.), instead of equation (III.) we must set

$$\frac{dr}{dt}+c-h=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8.)$$

And this equation, although not exactly in the same form, has been virtually used heretofore to determine the value of the quantity h. As long as the law of Watt is regarded as true, that the sum of the latent and sensible heat of a quantity of steam at its maximum density is the same for all temperatures, and consequently that

$$\frac{dr}{dt}+c=0,$$

it must be inferred that for this fluid h also is equal 0; this, indeed, has been often asserted, by saying that when a quantity of vapour at its maximum density is compressed in a vessel impervious to heat, or suffered to expand in the same, it will remain at its maximum density. As, however, Regnault* has corrected the law of Watt so that we can set with tolerable accuracy

$$\frac{dr}{dt}+c=0{\cdot}305,$$

the equation (8.) gives for h also the value 0·305. It follows from this, that a portion of the steam in the impermeable vessel must be precipitated by compression, and that it cannot retain its maximum density after it has been suffered to expand, as its temperature does not decrease in a ratio corresponding to the decrease of density.

Quite otherwise is it if, instead of equation (8.), we make use of equation (III.). The expression on the right-hand side is from its nature always positive, and from this follows in the first place that h is less than 0·305. It will be shown further on that the value of the said expression is so great that h becomes even negative. Hence we must conclude that the above quantity of vapour will be partially precipitated, not by the *compression*, but by the *expansion*; when compressed, its temperature rises in a quicker ratio than that corresponding to the increase of density, so that it does not continue at its maximum density.

This result is indeed directly opposed to the notions generally

* *Mém. de l'Acad.*, vol. xxi. 9th and 10th Memoirs.

entertained on this subject; I believe, however, that no experiment can be found which contradicts it. On the contrary, it harmonizes with the observations of Pambour better than the common notion. Pambour found* that the steam issuing from a locomotive after a journey always possesses the temperature for which the tension observed at the same time is a maximum. From this it follows that h is either 0, as was then supposed, because this agreed with the law of Watt, which was considered correct at the time, or that h is *negative*. If h were positive, then the temperature of the issuing steam must have been too high in comparison with its tension, and this could not have escaped Pambour. If, on the contrary, in agreement with the above, h be negative, too low a temperature cannot occur, but a portion of the vapour will be converted into water so as to preserve the remainder at its proper temperature. This portion is not necessarily large, as a small quantity of vapour imparts a comparatively large quantity of heat by its precipitation; the water thus formed is probably carried forward mechanically by the steam, and might remain unregarded; the more so, as, even if observed, it might have been imagined to proceed from the boiler.

So far the consequences have been deduced from the above maxim alone, without any new assumption whatever being made. Nevertheless, by availing ourselves of a very natural incidental assumption, the equation for permanent gases (II*a*.) may be rendered considerably more productive. Gases exhibit in their deportment, particularly as regards the relations of volume, temperature and pressure, expressed by the laws of M. and G., so much regularity as to lead us to the notion that the mutual attraction of the particles which takes place in solid and fluid bodies is in their case annulled; so that while with solids and fluids the heat necessary to effect an expansion has to contend with both an inner and an outer resistance, the latter only is effective in the case of gases. If this be the case, then, by the expansion of a gas, only so much heat can be rendered *latent* as is necessary to *exterior* work. Further, there is no reason to suppose that a gas, after it has expanded at a constant temperature, contains more sensible heat than before. If this also be admitted, we obtain the proposition, *when a permanent gas expands at a constant temperature, it absorbs only as much heat as is necessary to the exterior work produced by the expansion*; a proposition which is probably true for all gases in the same degree as the law of M. and G.

From this immediately follows

$$\left(\frac{dQ}{dv}\right)=A.R\,\frac{a+t}{v}; \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

* *Traité des locomotives*, 2nd edit., and *Théorie des machines à vapeur*, 2nd edit.

for, as already mentioned, $R\frac{a+t}{v}dv$ represents the quantity of exterior work produced by the expansion dv. According to this, the function U, which appears in equation (II*a*.), cannot contain v, and hence the equation changes to

$$dQ=cdt+AR\frac{a+t}{v}dv, \quad . \quad . \quad . \quad . \quad (IIb.)$$

wherein c can only be a function of t; and it is even probable that the quantity c, which denotes the specific heat of the gas at a constant volume, is itself a constant.

To apply this equation to particular cases, the peculiar conditions of each case must be brought into connexion therewith, so as to render it integrable. We shall here introduce only a few simple examples, which possess either an intrinsic interest, or obtain an interest by comparison with other results connected with this subject.

In the first place, if we set in equation (II*b*.) $v=$ const. and $p=$ const., we obtain the specific heat of the gas at a constant volume, and its specific heat under a constant pressure. In the former case $dv=0$, and (II*b*.) becomes

$$\frac{dQ}{dt}=c. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

In the latter case, from the condition $p=$ const., we obtain with help of equation (I.),

$$dv=\frac{Rdt}{p},$$

or

$$\frac{dv}{v}=\frac{dt}{a+t};$$

which placed in (II*b*.), the specific heat under a constant pressure being denoted by c', gives us

$$\frac{dQ}{dt}=c'=c+AR. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10a.)$$

From this it may be inferred *that the difference of both specific heats for every gas is a constant quantity* AR. But this quantity expresses a simple relation for different gases also. The complete expression for R is $\frac{p_0 v_0}{a+t_0}$, where p_0, v_0, and t_0 denote the contemporaneous values of p, v, and t for a unit of weight of the gas in question; and from this follows, as already mentioned in expressing equation (I.), that R is inversely proportional to the specific heat of the gas; the same must be true of the difference $c'-c=AR$, as A is for all gases the same.

If it be desired to calculate the specific heat of the gas, not by the unit of weight, but by the method more in use, the unit of volume, say at the temperature t_0 and the pressure p_0, it is only necessary to divide c and c' by v_0. Let these quotients be expressed by γ and γ', and we obtain

$$\gamma' - \gamma = \frac{A.R}{v_0} = A \frac{p_0}{a + t_0}. \quad . \quad . \quad . \quad . \quad (11.)$$

In this last expression nothing appears which is dependent on the peculiar nature of the gas; *the difference of the specific heats reckoned according to the unit of volume is therefore the same for all gases.* This proposition has been deduced by Clapeyron from the theory of Carnot; but the constant found above is not given by the difference $c'-c$, the expression found for it having still the form of a function of the temperature.

Dividing both sides of equation (11.) by γ, we obtain

$$k - 1 = \frac{A}{\gamma} \cdot \frac{p_0}{a + t_0}, \quad . \quad . \quad . \quad . \quad . \quad (12.$$

wherein k is set for shortness' sake in the place of $\frac{\gamma'}{\gamma}$. This is equal to the quotient $\frac{c'}{c}$; and through the theoretic labours of Laplace on the transmission of sound through air, has attained a peculiar interest in science. *The excess of this quotient above unity in the case of different gases is therefore inversely proportional to their specific heats, reckoned according to the unit of volume when the latter is constant.* This proposition has been proved experimentally by Dulong* to be so nearly correct, that its theoretic probability induced him to assume its entire truth, and to use it in an inverse manner in calculating the specific heat of various gases, the value of k being first deduced from observation. It must, however, be remarked, that the proposition is theoretically safe only so far as the law of M. and G. holds good; which, as regards the various gases examined by Dulong, was not always the case to a sufficient degree of accuracy.

Let us suppose that the specific heat c of the gases by constant volume is constant, which we have already stated to be very probable; this will also be the case when the pressure is constant, and hence *the quotient of both specific heats* $\frac{c'}{c} = k$ *must be also constant.* This proposition, which Poisson, in agreement with the experiments of Gay-Lussac and Welter, has assumed to be correct, and made the basis of his investigations on the tension

* *Ann. de Chim. et de Phys.*, xli.; and Pogg. *Ann.*, xvi.

and heat of gases*, harmonizes very well with our present theory, while it is not possible to reconcile it with the theory of Carnot as heretofore treated.

In equation (II*b*.) let Q= const., we then obtain the following equation between v and t:

$$c\,dt+\text{A.R}\frac{a+t}{v}dv=0; \quad . \quad . \quad . \quad . \quad (13.)$$

from which, when c is regarded as constant, we derive

$$v^{\frac{\text{AR}}{c}}\cdot(a+t)=\text{const.};$$

or, since according to equation (10*a*.), $\frac{\text{AR}}{c}=\frac{c'}{c}-1=k-1$,

$$v^{k-1}(a+t)=\text{const.}$$

Let three corresponding values of v, t and p, be denoted by v_0, t_0 and p_0; we obtain from this

$$\frac{a+t}{a+t_0}=\left(\frac{v_0}{v}\right)^{k-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

By means of equation (I.) let the pressure p, first for v and then for t, be introduced here, we thus obtain

$$\left(\frac{a+t}{a+t_0}\right)^k=\left(\frac{p}{p_0}\right)^{k-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

$$\frac{p}{p_0}=\left(\frac{v_0}{v}\right)^k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

These are the relations which subsist between volume, temperature and pressure, when a quantity of gas is compressed, or is suffered to expand in a holder impervious to heat. These equations agree completely with those developed by Poisson for the same case†, the reason being that he also regarded k as constant.

Finally, in equation (II*b*.) let t=const., the first member at the right-hand side disappears, and we have remaining

$$d\text{Q}=\text{AR}\frac{a+t}{v}dv; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

from which follows

$$\text{Q}=\text{AR}(a+t)\log v+\text{const.};$$

or when the values of v, p, t and Q, at the commencement of the experiment, are denoted by v_0, p_0, t_0 and Q_0,

$$\text{Q}-\text{Q}_0=\text{AR}(a+t_0)\log\frac{v}{v_0}. \quad . \quad . \quad . \quad (18.)$$

* *Traité de Mécanique*, 2nd edit. vol. ii. p. 646.
† *Traité de Mécanique*, vol. ii. p. 647.

From this, in the first place, we derive the proposition developed also by Carnot; *when a gas, without alteration of temperature, changes its volume, the quantities of heat developed or absorbed are in arithmetical progression, while the volumes are in geometrical progression.*

Further, let the complete expression for $R=\frac{p_0v_0}{a+t}$ be set in equation (18.), and we obtain

$$Q-Q_0=Ap_0v_0\log\frac{v}{v_0}. \quad . \quad . \quad . \quad . \quad (19.)$$

If we apply this equation to different gases, not directing our attention to equal weights of the same, but to such quantities as at the beginning embrace a common volume v_0, the equation will in all its parts be independent of the peculiar nature of the gas, and agrees with the known proposition to which Dulong, led by the above simple relation of the quantity $k-1$, has given expression: *that when equal volumes of different gases at the same pressure and temperature are compressed or expanded an equal fractional part of the volume, the same absolute amount of heat is in all cases developed or absorbed.* The equation (19.) is however much more general. It says besides this, *that the quantity of heat is independent of the temperature at which the alteration of volume takes place,* if only the quantity of gas applied be always so determined that the original volumes v_0 at the different temperatures shall be equal; *further, that when the original pressure is in the different cases different, the quantities of heat are thereto proportional.*

[To be continued.]

II. *On the Beudantite of* Levy. *By* H. J. Brooke, *F.R.S.**

HAVING had the pleasure last week of a personal communication with M. Des Cloizeaux, and having shown him Levy's specimen of this mineral, he at once stated that it differed entirely from that examined by himself and M. Damour as Beudantite, as well as from every other specimen under the same name which he had seen. He said that he was not aware of the existence of any mineral resembling Levy's in any collection on the Continent, and that he was inclined with Levy to regard his specimen as belonging to a separate species. The mineral known as Beudantite on the continent appears to be only an impure variety of cube ore of the usual form.

June 9, 1851.

* Communicated by the Author.

III. *On the joint Influence exerted by Light and the Oxidability of certain substances upon common Oxygen.* *By* C. F. SCHŒNBEIN*.

MY DEAR FARADAY,

SINCE I wrote to you last, I have been engaged in making researches on the different ways of exalting the chemical affinities of oxygen at the common temperature, and trust that the labour bestowed upon the subject will have been not entirely lost.

You know that I was inclined to consider phosphorus as the type of all the substances that are capable of undergoing oxidation in atmospheric air or oxygen at the common temperature, *i. e.* that I thought common oxygen unfit to unite chemically to any body; or to speak still more distinctly, I was disposed to conjecture that the slow oxidation of any substance is always preceded by a change of condition, or, if you like, an allotropic modification of the oxygen causing that oxidation. To be able to test the correctness of that conjecture, I wanted a substance which was not affected by common oxygen, but readily oxidized by ozonized or excited oxygen, and at the same time such as to allow perceiving easily and surely its oxidation. Such a matter I think is indigo dissolved in sulphuric acid, *i. e.* common indigo solution, whose colour cannot be discharged by common oxygen, but very easily by means of oxygen in the ozonic condition.

Now I reasoned thus: if any matter (undergoing oxidation in atmospheric air at the common temperature) should have the power of effecting, previously to its oxidation, an allotropic modification of the common oxygen, indigo solution being mixed with that matter and brought in contact with atmospheric air ought to be oxidized *conjointly* with the oxidable substance, just in the same way as the colour of indigo solution placed in contact with phosphorus and atmospheric air, is discharged whilst phosphorus is undergoing oxidation.

My anticipations have, I think, been confirmed by the results of numerous experiments; for I have ascertained that a series of organic and inorganic matters, capable of oxidation at the common temperature, have indeed the power of discharging the colour of indigo solution, and exhibit in this respect *a behaviour exactly like that of phosphorus*. But, before I proceed further, I must not omit to mention that that power is very small in the dark, and, comparatively speaking, strong in direct solar light.

The organic matters as yet tested are—spirit of wine, spirit of wood, linseed oil, tartaric acid, nitric acid, formic acid, acetic

* Communicated by Prof. Faraday.

acid, wine and beer; the inorganic ones are—sulphuretted, arseniuretted, antimoniuretted hydrogen, sulphurous acid.

The most distinguished of the first series is tartaric acid, of the second series, sulphurous acid.

I put into a spacious bottle (charged with atmospheric air) 100 grammes of water, 10 grammes of tartaric acid, and one gramme of my standard indigo solution, continually shaking the whole exposed to strong sunlight, and within forty minutes the colour of the liquid appeared to be discharged. In this way I have already destroyed 50 grammes of indigo solution by the 10 grammes of the acid, without having as yet exhausted its discharging power.

A strip of linen cloth rather strongly dyed by indigo solution, drenched with a solution of tartaric acid, continually kept moist by water and exposed to the joint action of a strong sun and atmospheric air, was completely bleached within five hours. In the dark, under the same circumstances, the bleaching of the dyed cloth, or the discharge of the colour of the indigoferous tartaric acid solution, takes place very slowly. I have kept these last four weeks a bit of moist blue linen impregnated with a solution of tartaric acid in a dark room, and now it appears certainly to be a shade lighter than it was in the beginning; but it is still very blue.

To test the discharging power of the other organic matters named, I put 10 grammes of the one or the other of them into a spacious white bottle filled with air, add to it 100 grammes of water, and 1 gramme of the standard indigo solution, expose the mixture to the action of light, and shake the whole as often as I can, taking care to renew now and then the air of the bottle. Experimenting in this way, the discharging power of the substances mentioned and that of others is easily ascertained.

As to the hydrogenated compounds of sulphur, selenium, arsenic and antimony, I mix them with atmospheric air, putting some water on the bottom of the vessel holding the mixture, suspend within it a moist strip of linen dyed with dilute indigo solution, and expose the whole to the action of solar light. The bleaching of the cloth does not take place very rapidly in those gaseous mixtures; for instance, in that of sulphuretted hydrogen the strip had to remain for a week before its colour was entirely discharged. In the mixture of arseniuretted or antimoniuretted hydrogen and atmospheric air, the cloth becomes brown in consequence of metallic arsenic or antimony being deposited upon the strip.

By far the most interesting oxidable inorganic substance, as to its indirect bleaching power, is sulphurous acid. This acid, as well as the other oxidable matters mentioned, when deprived of any free oxygen, has no appreciable action upon the indigo solution; for you may keep them together any length of time (even in sunlight) without being able to perceive any diminution or change of colour. So soon, however, as you permit oxygen or atmospheric air to enter, an action will ensue, very slow in the dark; comparatively speaking, very rapid in solar light.

The simplest way of exhibiting the action is this: suspend moist strips of linen dyed with indigo solution in a mixture of gaseous sulphurous acid and oxygen gas or atmospheric air. When placed in the dark, such strips require many weeks to become entirely white, whilst a strong noon's insolation of one and a half, or at most two hours' duration, will completely bleach them.

As a matter of course, the colour of aqueous sulphurous acid mixed up with some indigo solution and shaken with oxygen or atmospheric air, will also be much more rapidly discharged in solar light than it is in the dark. 100 grammes of a weak sulphurous acid, coloured by 1 gramme of the standard indigo solution, on being continually shaken with atmospheric air and exposed to a strong sun, lost the colour within twenty minutes. It is hardly necessary to mention, that the bleaching power is exerted as long as there is free oxygen and sulphurous acid; for no sooner has the latter disappeared, *i. e.* been transformed into sulphuric acid, than the action ceases to take place in a perceptible degree.

I was curious to see how much of my standard indigo solution could be destroyed by a given weight of pure sulphurous acid. Five grammes of the latter, mixed up with 200 grammes of water, were therefore put into a bottle holding about two litres and filled with atmospheric air,. then 50 grammes of the standard indigo solution added, the whole exposed to the action of solar light, repeatedly shaken, and the atmospheric air now and then renewed. There being little sunshine, the colour of the liquid was discharged within a couple of days, and in the course of six weeks (very deficient in sunshine) I have been able to destroy 600 grammes of the standard indigo solution, and find that there is still a very perceptible quantity of sulphurous acid in the mixture. The discharging power is therefore not yet exhausted.

Now to discharge the colour of 600 grammes of my indigo solution, I require nearly 11 grammes of the strongest nitric acid (the monohydrate), or fully 18 grammes of good chloride of lime; and 5 grammes of pure sulphurous acid having already done the same work, and being able to do still more, we see that sulphurous acid enjoys a most extraordinary indirect oxidizing power. I say "indirect," because the acid itself has nothing to do with the oxidation of indigo; the only part it performs conjointly with light is, to exalt the chemical affinities of free common oxygen to such an extent as to render that element capable of destroying indigo just as well as ozone does. I have reason to believe that indigo is not the only organic substance which is indirectly oxidized by sulphurous acid, for I have succeeded in destroying some very strongly coloured organic matters by the joint agency of light, sulphurous acid and common oxygen.

It is worthy of remark, that the amount of the exalting effects produced by light conjointly with sulphurous or tartaric acids, &c. upon oxygen far surpasses the sum of the effects brought about singly by those agents. Insolated oxygen or air of itself certainly is capable of discharging the colour of indigo solution, and so is oxygen placed under the exciting influence of sulphurous acid, tartaric acid, &c. unassisted by light; but in both cases the action is very slow, whilst it is rapid if both causes be working together. The facts, that linseed oil, on being shaken with copper filings and atmospheric air in sunlight, soon turns green, brass in contact with fatty matters assumes the same colour, mercury contained in a divided state in the unguentum mercuriale is gradually transformed into the protoxide, fibrous matters impregnated with oils are now and then spontaneously set on fire, &c., seem to be connected with the exciting influence exerted by oxidable matters upon ordinary oxygen.

If, according to Berzelius and others, ozone be but an allotropic modification of common oxygen, we must admit that phosphorus, like electricity, has the power of causing that extraordinary change of condition in common oxygen, and are led to suppose that phosphorus stands not alone in this respect. Indeed, my late researches on oil of turpentine, &c. show that many other substances make oxygen act (even independently of light) as phosphorus does.

Now these facts seem to give room to the conjecture, that many oxidable matters and oxygen, on being put in contact with one another, exert a peculiar influence upon each other previous to their chemically uniting with one another. As to phosphorus, oil of turpentine, &c., it seems as if the first action produced upon common oxygen be the allotropification of that body, and the oxidation of phosphorus the sequel, and not the cause of the formation of ozone. I therefore think it not impossible, that at least some of those strange chemical phænomena, called catalytic, may be connected with an action similar to that produced by phosphorus, oil of turpentine, &c. upon oxygen, *i. e.* depend upon allotropic modifications of elementary bodies brought about by the mere contact of certain substances. Certainly we do not know as yet in what the allotropic modifications consist, and how they are effected; but whatever they may depend upon they are facts; and facts, too, which in my opinion will, if once better understood than they are now, throw floods of light upon the thick darkness with which the chemical world is as yet covered. And I am inclined to think so, because it is very likely that the so called catalytical phænomena reveal the very elementary, and on that account the most important, actions or functions of matter.

I am, my dear Faraday,
Yours most truly,
Bâle, May 1, 1851. C. F. SCHŒNBEIN.

IV. *Reports on the Progress of the Physical Sciences. By* JOHN TYNDALL, *Ph.D., Marburg.*

1. *The Reversion-prism, and its application as ocular to the Terrestrial or Day-Telescope*, by H. W. Dove.
2. *Description of several Prism-stereoscopes, and of a simple Mirror-stereoscope*, by H. W. Dove.
3. *On the deportment of Crystalline bodies between the electric poles*, by H. Knoblauch.

FOR the manuscript of the first two papers I am indebted to the kindness of Professor Dove. The reversion-prism will probably come into practical use both in England and Germany. In leveling instruments, for example, the inconvenience of the common telescope led to Gravatt's invention of the dumpy-level; but the inversion of the figures upon the leveling-staves constitutes an objection in the eyes of many. M. Dove's invention removes this objection; the reversion-prism sets the figures again erect without rendering a lengthening of the instrument necessary. The application of the invention will render the day-telescope in general a more convenient instrument.

The stereoscopic apparatus and phænomena described in the second paper are strikingly simple and beautiful. I would recommend the reader to furnish himself with a pair of prisms and a few stereoscopic drawings; with their aid the paper will be much more intelligible.

The paper by Professor Knoblauch has also been handed to me in manuscript. It is highly interesting to observe the parallelism between electricity and magnetism in the production of phænomena. The author has demonstrated the action of electricity on crystalline substances in a very convincing manner, and seems to have succeeded in tracing the phænomena to the same cause as that to which magneto-optic action has been referred by him and the writer, namely, to peculiarity of aggregation. In the 'Report' the translation of the manuscript is slightly abbreviated.

1. *The Reversion-prism, and its application as ocular to the Terrestrial or Day-Telescope, and to the measurement of angles.*

When rays fall upon the side surface of a prism whose ends are right-angled isosceles triangles, and parallel to the hypothenuse surface of the same, they issue from the other side, after having endured two refractions and one total reflexion.

An object observed through such a prism appears unaltered in shape and magnitude, but it has changed sides in the same manner as the image of an object in a looking-glass. The conditions of achromatism are strictly fulfilled, for all rays which were parallel before their entrance remain so after their exit. If the hypothenuse surface lie horizontal, so that its production shall intersect the object in a horizontal line, then the image is obtained by letting fall from all points of the object perpendiculars upon this line, and producing them until their parts above and below the horizontal line are equal to each other. The ends of the productions taken all together form the image of the object.

If therefore a vertical line be intersected in the centre by the above horizontal line, the former line and its image will coincide, its position being reversed. If the line make an angle of 45° with the produced hypothenuse surface, then the image will be perpendicular to the object. Now as an inclination of 45° on the part of the line which was first considered vertical, the prism remaining fixed, has the same effect as if the line remained fixed and the prism were turned 45° in the opposite direction, the image must rotate with double the velocity of the plane of refraction of the rotating prism.

As the rays which emerge from this first prism with regard to a second similar one may be considered as proceeding direct from an object which occupies the position of the image, it follows,—

If the hypothenuse surfaces of two equal prisms lie in the same plane, their corresponding edges being parallel, then an object viewed through both will appear altogether unchanged; for the second prism reverses the image delivered by the first prism, or in other words, it undoes the work of the latter and restores things to their primitive condition. If, on the contrary, the first prism remain fixed and the second be turned, so that the planes of refraction of both prisms are perpendicular to each other, the object then appears completely reversed. The first prism reverses it with regard to right and left, and the second with regard to top and bottom. As, however, a reversion is equivalent to a turning of the object through an angle of 180°, it follows,—

Through two prisms situated so that the hypothenuse edges form a straight line, an object appears unchanged in size and shape, but inclined at an angle which is twice as large as that inclosed by the planes of refraction of the two prisms. For it is clear that the double reversion takes place in the same manner, whether the two lines in which the productions of the hypothenuse surfaces of the prism cut the object form a right or an acute angle with each other. Everybody will admit that when a plane is turned 180° round an arbitrary line which lies in it, and then 180° round another arbitrary line which also lies in it, the appearance of the plane as regards shape and size is the same; but if the lines do not cross each other at a right angle, the position of the plane will be oblique.

At whatever stage of the rotation the prisms may stand, if they be both turned together, that is to say, if the case which incloses them be caused to rotate, the image continues fixed. For, as the image of the first prism moves with twice the velocity of the second prism, the motion, however, being equivalent to a motion of the prism in the opposite direction, the second prism will therefore carry the image back with exactly the same velocity as the first prism carries it forward.

A system of two such prisms is named by the inventor a *reversion-prism*. If the reversion-prism be screwed before the ocular of an astronomical telescope, then if the planes of refraction of the prisms stand perpendicular to each other, the telescope is converted into a terrestrial one. An ocular so constructed is named by the inventor a *terrestrial prism-ocular*. The prisms are made fast in a cylindrical collar, the second being capable of rotating while the first remains fixed. The periphery of the piece which turns is divided into degrees like the head of a micrometer-screw; and upon the cylindrical collar, two strokes which stand opposite to each other denote the position of the plane of refraction of the fixed prism. The zero of the graduation corresponds to the plane of refraction of the moveable prism. When the planes of refraction inclose the angle 0, the telescope is astronomical; when they inclose an angle of 90°, the telescope is terrestrial. If the planes inclose an acute angle, the observed object will appear turned through twice the angle formed by the planes of refraction, and will remain thus inclined when the telescope is turned round its axis.

Owing to the shortness of the prism-ocular, a terrestrial telescope thus constructed is much shorter than the common one, and may therefore be used with advantage in the naval or military service. It is to be screwed like a shade before the ocular of the astronomical telescope. If it be applied as a terrestrial ocular only, it is best to fasten both prisms together, so that their planes of refraction shall be constantly perpendicular to each other. In the reading of numbers we thus avoid the unpleasantness of an inversion.

The application to the measurement of angles of inclination is as follows:—The prisms being set at an angle of 90° in the telescope, so that objects appear in their natural position, the telescope is turned round its axis until the vertical cross-wire covers the line, the inclination of which is to be determined. The forward prism is now turned until the vertical wire and the line which it covers coincide with a plummet-line suspended before the telescope. The angle of rotation is equal to half the complement of the angle of inclination sought. The reversion-prism can also be placed within the telescope or before the object-glass of the instrument, its size being so chosen that an object can be seen through the prism and through the uncovered portion of the object-glass at the same time. The two lines whose inclosed angle is to be determined are brought to coincidence; the one seen through the uncovered object-glass, and the other through the prism. If the reversion-prism be set in the interior of the telescope, then the terrestrial instrument is of the same length as the astronomical, the entire length of the terrestrial ocular being thus spared. If in this case it be required that the prism should be capable of being turned round, the tube of the telescope may be composed of two parts, one of which fits into the other; in one part the fixed prism is to be set, and in the other part the rotating prism.

In all isosceles triangles the condition of total reflexion is fulfilled for those rays which fall parallel to the base and near it. That which has been heretofore affirmed regarding right-angled triangles, is true of isosceles triangles generally. The number of rays, however, which after their first refraction reach the base, decreases as the angle at the vertex becomes more acute. In each particular case, therefore, where another than the right-angled triangle is applied, the angle must be determined which permits of the whole of the rays falling upon the first surface being totally reflected. With an acute angle, the length of the ocular decreases and the light intensity increases. To adjust the prisms, the common method of turning the telescope round its axis, and observing that the position of any chosen point remains fixed, is to be applied.

2. *Description of several Prism-stereoscopes, and of a simple Mirror-stereoscope.*

When stereoscopic drawings are executed in white lines upon a black ground, a weak image arising from the reflexion which takes place at the uncovered forward surface of the glass often mixes itself with the principal image reflected from the silvered surface behind. To prevent the appearance of such images, total or metallic reflexion is necessary. In stereoscopic investigations, where the contours of the images do not exactly coincide, a doubt may arise whether this be not due to the imperfection of the drawings made use of. The desire to set aside this possible source of error has led to the construction of the following stereoscopes.

a. *Prism-stereoscope, consisting of one Prism and one Drawing intended for a single eye.*

The condition of complete identity can only be fulfilled when one object, and not two, is observed. An object seen in a looking-glass appears reversed as regards right and left. In all stereoscopic drawings, which differ only in the circumstance that one is a reflected image of the other (and to this class belong the greatest number, even the most complicated of those hitherto executed), the reflected image of the object can take the place of the second drawing. Such drawings may be named *simple reversions,* in contradistinction to those in which a plane perpendicular to the line which joins both eyes and passing through the centre of the body does not divide the latter symmetrically. If an erect object be viewed through a prism whose ends are right-angled isosceles triangles, the prism being so placed that its plane of refraction is horizontal, that is to say, with its hypothenuse surface vertical, the object will remain erect; but with regard to right and left, it will be found to have changed sides. Through such a prism, therefore, the arranged type in a printer's workshop may be read as the page of a common book; the letters change places right and left, and thus appear as when they are printed. In the same way a profile is reversed. If a common stereoscopic drawing intended for the left eye be viewed by the right eye through such a prism, then if the projections be simple reversions of each other, the image will appear as if designed for the right eye. To the left, or naked eye, the drawing appears unchanged. It is easy, by turning the prism round one of the edges which stands perpendicular to the plane of refraction, to cause both images to coincide; the moment this takes place, the relief starts into existence with surprising sharpness.

If the position of the drawing remain unchanged, and the prism be held before the left eye, then if the object be conceived to be transparent, so that both its exterior and interior surfaces are visible, the forward surface will appear converted into the surface behind, and *vice versâ*. When, on the contrary, a drawing intended for the right eye is viewed through the prism by the left eye, it will appear exactly as in the first case, always provided that it is viewed at the same time by the naked right eye. If the drawing represents a body, which, like a pyramid or the frustum of a pyramid, appears with regard to the surface of the paper either as convex or concave relief, then the said drawing viewed through the prism with one eye, and with the other eye naked, will appear in convex relief; when the prism is held before the other eye, the stereoscopic combination of both images gives rise to a concave relief. If the drawing be turned in its own plane while the prism remains before the same eye, a rotation of 90° causes the coincidence of the images to appear as a plane projection. A rotation of 180°, on the contrary, changes it from concave to convex. During the rotation, the hypothenuse surface must stand perpendicular, or nearly so, to the observed drawing.

With regard to the dimensions of the prism for holding freely in the hand, an inch in length for the two equal sides of the isosceles triangle at the end, and three-quarters of an inch in width, will be found convenient proportions. When fastened in a cylindrical tube attached to a stand, which permits of its being raised and lowered, a prism will be found sufficiently large in which the height of the right-angled triangle does not quite amount to two lines.

b. *Prism-stereoscope, consisting of one Prism and two Drawings.*

The stereoscope just described fulfils the condition of the complete identity of both projections, and besides this possesses the advantage of a total reflexion, by which incidental images are avoided; but it is applicable to simple reversions only. The following instrument is free from this limitation. In simple reversions, two copies of the same drawing are laid side by side. One of them is viewed through the prism, and the image thus obtained is projected upon the other which is viewed with the naked eye. When the drawings are unsymmetrical, that intended for the right eye is placed right, and viewed through the prism held before the right eye; the image thus obtained is projected upon the second drawing, which is observed by the naked left eye, and which must be a copy of the looking-glass image of the drawing intended for the left eye in the common stereoscope.

c. *Prism-stereoscope, consisting of a Reversion-prism and two Drawings.*

The reversion-prism reverses an object completely, as well in respect to right and left as to top and bottom. The unsymmetrical projections intended for the common stereoscope are placed in reversed positions beside each other, and the image seen through the reversion-prism is projected upon the other drawing which is observed with the naked eye.

d. *Prism-stereoscope, consisting of two Prisms and two Drawings.*

Two equal isosceles right-angled prisms are held one before each eye, so that the hypothenuse surfaces are vertical—best so that the hypothenuse surfaces face each other—and by inclining the prisms, the images of the two drawings placed side by side are brought to coincidence. This stereoscope, like the eye, is applicable to all drawings; when the prisms are fixed in cylindrical tubes and attached to a stand, the arrangement is exceedingly convenient.

e. *Mirror-stereoscope with two Drawings and a plane metallic mirror or reflecting prism.*

The drawing intended for the left eye is laid horizontal, and is viewed with the naked left eye. Before the right eye is held a small metallic mirror or a reflecting prism; and in the case of simple figures, another copy of the same drawing is observed in a vertical plane. For an unsymmetrical figure, a drawing which is the reflected image of that intended for the right eye must be used.

f. *Double-seeing, as Stereoscope.*

Those who have accustomed themselves to double-seeing, when two stereoscopic drawings are laid side by side, can obtain four images in a direction parallel to a line joining both eyes. The two central images can be brought to coincidence, the relief being thus obtained between its two projections. This experiment is, however, so wearying to the eyes, that its frequent repetition is not to be recommended. The author has made the experiment purely on account of its physiological interest. A most peculiar impression is caused by the union of the two images in the relief. When brought near, they seem to rush together with an accelerated velocity as if they more strongly attracted each other. In a similar manner, when two persons lay their foreheads together and look into each other's eyes, each observes the eyes of the other to run together into one large eye in the centre of the forehead.

g. *Why does the depth of the concave Relief appear greater than the height of the Convex?*

In the application of the stereoscopes above described, it is observed in a striking manner, particularly when the drawings are viewed from some distance, that by exchanging the projections the height of the convex relief appears less than the depth of the concave. The plane of the paper on which, as ground surface, the drawing is executed, is seen at the same distance in both cases. That this is the cause of the phænomenon is proved by the fact, that the side surfaces of the frustum of a pyramid, stereoscopic observed, seem to be less steeply inclined towards the base when the top of the frustum is nearest the eye, than when it falls at the other side of the base, and is viewed through the hollow prism. The top surface of the frustum is seen in both cases under the same angle; but in the second case we imagine that it lies further away. Hence the idea of a larger surface seen at a greater distance, and the apparent diminution of the inclination.

This explanation is corroborated by the following experiment: A small gypsum bust was placed before a polished hollow mirror, so that the reversed image seen with both eyes fell beside the bust. Without changing the position of the right eye, the left eye was closed. The image receded immediately to the surface of the mirror, and appeared much larger; it was observed under the same angle, but fancied to be at a greater distance.

In comparative stereoscopic investigations the drawings must always stand at equal distances. It is a proof that the combination is good if, when the head is moved slowly to and fro, the relief is set in slow oscillatory motion.

3. *On the Deportment of Crystalline Bodies between the Electric Poles.*

It is well known that crystalline bodies suspended between the poles of a magnet take up positions which distinguish them from uncrystalline. It will be of interest to examine whether a similar deportment is exhibited between the electric poles.

As in the case of bodies suspended between the poles of a magnet, it is here absolutely necessary to annul all action which might arise from a mere peculiarity of shape. This is done by converting the substance into circular discs, which are to be suspended horizontally.

A plate of *heavy spar* cloven parallel to the plane of most eminent cleavage had the short diagonal of the rhombic base marked upon it. Suspended between the poles of a dry pile consisting of 400 pairs of zinc and gold-paper, or of 2000 pairs of silver-paper and manganese, it set itself so that the short diagonal stood *perpendicular to the line joining the poles.*

For the sake of brevity, as in the case of experiments with the magnet, we shall call this position the *equatorial.*

A circular disc taken from a *gypsum* crystal parallel to the plane of most eminent cleavage, set itself so that a line, *which slightly deviated from the short diagonal* of the rhombus inclosed by the other cleavages, stood equatorial.

These experiments require the greatest caution; only such crystals should be used as are entirely free from all traces of fracture. Seven examples coincided in the deportment just described. The action is stronger in the following crystals:—

Saltpetre was so cut that the crystallographic axis lay in the plane of the disc; between the electric poles the axis stood equatorial.

Discs were taken from *Iceland spar, carbonate of lime and iron,* and *carbonate of iron,* so that the axis in each instance lay in the plane of the disc. The axis in every case turned slowly into the equatorial position and finally remained there.

A disc of *arragonite* exhibited the same deportment. The axis stood equatorial. In this experiment particular precautions are necessary, which shall be immediately pointed out.

Beryl cut into the form of a shallow cylinder set its axis from pole to pole. The plane of most eminent cleavage stood consequently equatorial.

A disc of *tourmaline* set itself so that the line perpendicular to the axis stood equatorial.

In all non-conducting substances it is known that the induction which takes place on the approach of an electrified body continues for some time after the said body is removed. This fixation of the fluid may be readily demonstrated with a disc of glass.

In *rock-crystal* and *topaz* this polarization is exhibited so strongly, that if a disc be held for an instant between the two poles, it will continue to assert this position against an intentional torsion of the suspending fibre.

Although similar phænomena of polarity were exhibited more or less by all the crystals examined, still, if we except *arragonite,* they never attained to such a degree of intensity as to prevent the assumption of the positions above described.

If the crystal be a conductor, a continuance of the electric state is not observed after the exciting cause has been removed. A cylinder of bismuth, the axis of which was perpendicular to the plane of most eminent cleavage, always set itself so that the said plane formed an angle of 90° with the line joining the poles.

In the investigation on the magneto-optic properties of crystals* carried out by Dr. Tyndall and myself, the position of crystals between the poles of a magnet is referred to the peculiar aggregation of the material particles. The question occurs, Do not the phænomena above described permit of being referred to a similar origin?

A fine powder of sulphate of barytes was mixed to a paste with gum-water and pressed together in one direction. From the mass when dried a circular disc was taken, so that the line of compression was parallel to the plane of the disc. The direction of greatest compression takes up between the poles a position similar to that assumed by the short diagonal of heavy spar —it sets equatorial.

* Philosophical Magazine, July 1850.

A disc of carbonate of lime, in which the particles were brought by pressure more closely together in one direction, set itself with the line of compression equatorial. This line therefore corresponds with the axes of arragonite and calcareous spar.

The same coincidence was exhibited between a disc of powdered carbonate of iron and the crystal of the same substance. The direction of compression, like the axis of the crystal, set equatorial.

Besides those bodies whose chemical composition is the same as that of the crystals examined, others of powdered glass, chromate of lead, and phosphate of lime were also submitted to examination. Of conducting bodies, oxide of manganese, oxide of iron, antimony and bismuth, were examined; the direction of compression in all these substances set itself between the electric poles equatorial.

The decided manner in which these phænomena are exhibited, prove to a certainty that those bodies in which the material particles are not the same distance apart in all directions set between the poles (when the directing influence of mere shape is annulled), so that the line in which the particles stand most closely together stands equatorial.

The coincidence of position of diamagnetic crystals between the magnetic and the electric poles, and the difference of position assumed by magnetic crystals, stand in immediate connexion with the results arrived at by Mr. Tyndall and myself. We have shown that, in bodies whose particles are unequally separated in different directions, that direction in which the particles lie most closely together sets in the magnetic field when the substance is *magnetic* from pole to pole; when the substance is *diamagnetic*, equatorial.

Of the crystals mentioned the following are magnetic:—carbonate of lime and iron, beryl and tourmaline. In all these cases, *that* direction, which, by the action of the magnetic poles, sets itself axial, between the electric poles sets equatorial.

Of the crystals examined the following are diamagnetic:—heavy spar, sulphate of lime, saltpetre, Iceland spar, arragonite and bismuth; in all these cases, the direction which between the magnetic pole stands equatorial, takes up the same position between the electric poles.

The principal results of the inquiry may be expressed as follows:—1. Crystals, conductors and non-conductors, under the influence of the electric poles obey a directive force which is independent of the form of the mass. 2. The same is the case with bodies the material particles of which are brought by pressure into unequal distances from each other; that line in which the particles lie most closely together being caused to recede from the poles. 3. When the crystal is magnetic, that direction which between the magnetic poles stands axial, between the electric poles stands equatorial. When the crystal is diamagnetic, the positions between the magnetic poles and electric poles are coincident. The same holds true for substances artificially compressed.

V. *On the Theory of Sound.* *By* W. J. Macquorn Rankine, *F.R.S.E. &c.**

I TRUST that the following brief remarks may remove the objections still entertained by Professor Potter† to my explanation of Poisson's investigation of the velocity of sound in air‡.

Professor Potter objects that I have asserted, without sufficient grounds, that the variation of pressure is developable in terms of the variation of density by means of Taylor's theorem, the coefficient of the first term being a finite quantity,

$$\frac{\rho_0}{p_0} . \phi' \rho_0 = 1 + \beta .$$

My grounds for this assertion are, that if the variation of pressure is not so developable in terms of the variation of density, or if $1+\beta$ is not a finite quantity, then the variation of pressure corresponding to an indefinitely small variation of density must be either null or infinite; that is to say, either the pressure must be a maximum or a minimum with respect to the density, or the density a maximum or a minimum with respect to the pressure. But this is not the case, for the pressure varies continuously with the density; therefore the variation of pressure is developable in terms of the variation of density by Taylor's theorem, and the coefficient $1+\beta$ is a finite quantity.

Professor Potter misconceives my meaning when he supposes that I deny the existence of unsymmetrical waves of sound. My remarks were intended to apply to waves, which, having been originally symmetrical, become unsymmetrical as they advance, like those on the surface of shallow water.

London, June 2, 1851.

* Communicated by the Author.
† Fourth Series, vol. i. p. 476. ‡ Ibid. p. 410.

VI. *Pendulum Experiments.* *By* Thomas G. Bunt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Bristol, June 11, 1851.

THE series of pendulum experiments detailed in my last letter I have now somewhat extended, and arranged more systematically than before. The result of the whole is shown in the following summary:—

Part of azimuth circle observed.	Time occupied in the experiments.	Motion of plane in azimuth.		Rate per hour.
	min.			
0° to 20°	224·9	41°·1		10°·96
20 ... 40	323·8	63·9		11·84
40 ... 60	220·0	44·4		12·11
60 ... 80	177·5	35·7		12·07
80 ... 100	182·6	36·9		12·13
100 ... 120	144·8	27·5		11·39
120 ... 140	209·1	45·1		12·94
140 ... 160	200·7	40·2		12·02
160 ... 180	167·1	33·6		12·06
	1850·5	368·4	=	11·945 per hour.

The arc of vibration usually given to the pendulum on starting it, was about nine feet, which in half an hour decreased nearly one-third. The mean rate of apsidal motion of these arcs, on their becoming elliptical (as they generally did in the course of a few minutes), I had found to be about $\frac{7}{10}$ths of a degree per hour to $\frac{1}{10}$th of an inch of ellipticity, or length of half minor axis. I had assumed that this rate would hold good as the arcs become shorter, provided both axes diminished in the same ratio, and consequently that it would increase when the arc shortened and the ellipticity continued the same. A few experiments on short arcs (which I had begun to prefer to long ones) soon convinced me that this assumption was directly contrary to the fact; and that with a given length of minor axis, the progression of the apsides was slower as the arc of vibration decreased. This discovery, together with other manifest advantages attending short arcs, determined me on confining myself entirely to them in future; and I accordingly substituted instead of my old circle, a new one of only half the diameter. My next improvement was in the mode of suspension; a suggestion for which I am indebted to a scientific friend, who made the apparatus for me.

A brass screw, about $1\frac{1}{2}$ inch long and $\frac{3}{4}$ inch in diameter, was sawn through the greater part of its length into four sections or quadrants, meeting in the axis. The screw was firmly centred into a brass disc, and before having the thread cut upon it, was tapered so as to be larger towards the disc. The wire being passed up the middle of this screw, is tightly held between the four quadrants, which are compressed by a nut. The disc was then screwed down to the floor and leveled by a spirit-level.

Nearly the whole of the following series of experiments were made with these new arrangements, and they appear to be in every respect much superior to those of the former series. The tendency to elliptic motion in the pendulum-bob was very considerably lessened; and sometimes it would vibrate for nearly three-quarters of an hour without any ellipticity at all. After making and tabulating a mass of experiments, I found the correction for $\frac{1}{10}$th of an inch ellipticity, in a mean arc of about three feet, to be only $0^\circ{\cdot}43$ per hour; and from several experiments in which the pendulum was left to vibrate several hours, without receiving a new impulse, I found the apsidal motion of short elliptic arcs to be much below this proportion, though their precise law I do not pretend to determine.

In the following experiments, occupying rather more than thirty-seven hours, an impulse was usually given to the pendulum about once in an hour. The degree of the azimuth circle cut by the plane of vibration, and the amount of the ellipticity, were usually observed and written down at the end of every quarter of an hour. Those experiments in which the pendulum was left unobserved for a whole hour or more are excluded from this series, and their results given separately. The ellipticities were seldom permitted to exceed 0·2 inch, and a correction for each is introduced into the observed motion in azimuth. Care was, however, taken to make the opposite ellipticities as nearly equal as possible; and it will be seen below, that, in taking their sum (regard being had to the sign of each), they amounted to only $-0^\circ{\cdot}32$, although they were about 150 in number.

I find that I was in error in saying that the zero of my circle was in the meridian; it is not zero, but the division 16° nearly.

In Table I. I have given my experiments in the order in which they were made, adding just so many together (usually about four) as would make about the interval of one hour for each group. With the number of minutes are given the degrees of motion in azimuth, the correction for ellipticity, the part of the circle observed, and the rate of motion per hour, for each of the thirty-seven hours through which the experiments extended. Table II. is deduced from Table I., and shows the mean rate of the motion in azimuth for every 20°, from 0° to 180°. Table III. gives the result of experiments with intervals longer than any contained in Table I., varying from one to four hours, at the beginning and end of which intervals only the pendulum was observed.

Table I.—Thirty-seven hours' motion in Azimuth of Plane of Vibration, showing the rate of motion for each hour.

Time.	Observed motion.	Elliptic correction.	Part of circle observed.	Rate per hour.	Date.
min. 72·2	14°·84	°·0	20° to 33°	12°·33	May 27.
50·3	10·50	−0·48	57 ... 69	12·00	28.
70·7	13·92	−0·29	69 ... 76 120 ... 126	11·57	30.
60·0	11·42	−0·31	128 ... 139	11·11	...
47·8	8·65	+0·24	140 ... 145 122 ... 126	10·93	...
61·4	12·42	−0·66	127 ... 135 50 ... 54	11·49	...
61·7	11·90	+0·01	54 ... 62 145 ... 148	11·58	...
65·9	13·01	−0·29	148 ... 161	11·59	31.
51·0	9·57	+0·37	161 ... 171	11·70	...
66·5	12·45	+0·38	171 ... 180	11·57	...
53·9	10·41	+0·26	180 ... 9	11·89	...
57·8	11·14	+0·23	9 ... 20	11·80	...
64·7	12·80	+0·02	20 ... 32	11·89	June 2.
58·0	12·05	−0·17	33 ... 45	12·29	...
60·1	12·46	+0·25	45 ... 51 74 ... 81	12·69	...
63·3	13·18	+0·08	81 ... 94	12·57	...
59·9	12·05	+0·05	95 ... 107	12·12	3.
59·3	11·45	−0·13	107 ... 119	11·45	...
71·6	13·69	+0·05	119 ... 133	11·52	...
53·3	9·66	+0·26	132 ... 142	11·17	...
60·3	11·10	−0·04	142 ... 152	11·01	...
69·1	12·60	+0·09	152 ... 165	11·02	4.
66·7	11·95	+0·18	165 ... 177	10·90	...
61·8	11·56	−0·04	177 ... 6	11·18	...
65·5	12·26	+0·04	6 ... 19	11·27	4. 5.
52·9	10·13	−0·01	19 ... 28	11·48	...
61·2	11·98	+0·0	30 ... 42	11·75	...
56·2	11·06	+0·23	41 ... 52	12·03	6.
63·9	12·95	−0·23	51 ... 64	11·94	...
54·4	11·16	−0·27	65 ... 75	12·02	...
63·6	12·59	·0	75 ... 88	11·89	...
63·7	13·09	−0·15	88 ... 101	12·19	6. 7.
62·9	12·34	−0·36	101 ... 114	11·42	...
62·7	11·77	+0·15	114 ... 126	11·41	...
61·2	11·83	+0·13	126 ... 139	11·73	...
46·8	9·23	−0·5	139 ... 148	11·77	...
39·8	7·56	+0·14	148 ... 156	11·61	...
2222·1	432·73 −·32	−0·32			
	432·41 =	11°·677 mean rate per hour.			

Table II.—Mean hourly rate for every 20° of Azimuth.

0° to 20°	11°·56 per hour.
20 ... 40	11·92 ...
40 ... 60	12·09 ...
60 ... 80	12·08 ...
80 ... 100	12·24 ...
100 ... 120	11·63 ...
120 ... 140	11·29 ...
140 ... 160	11·42 ...
160 ... 180	11·33 ...
	11·729 mean of these per hour.

Table III.—Eleven Experiments with Long Intervals. No correction introduced.

	min.	
May 23.	245·7	49°·45 motion.
28.	130·2	23·90 ...
30.	118·9	23·00 ...
31.	127·4	25·75 ...
June 2.	110·8	21·43 ...
3.	121·5	24·80 ...
4.	102·8	17·02 ...
5.	282·9	58·13 ...
6.	165·7	32·45 ...
...	70·1	14·52 ...
7.	162·5	32·18 ...
	1638·5	322·63 = 11°·814 per hour.

All the experiments which I have as yet detailed were made with my leaden ball, of $53\frac{1}{2}$ lbs. weight. I had previously been experimenting with an iron half cwt., of a form nearly cylindrical; but finding its motion irregular, I suspected the influence of magnetism and laid it aside. Two days ago I determined to suspend it again, and try whether its irregular motion might not have arisen from ellipticity, which in my first experiments with this weight I had not recorded, or whether it must be assigned to some other cause. The rates of motion I obtained, in intervals of about ten or fifteen minutes each, were the following, viz.

Part of circle observed.	Rate per hour.
359° to 0°	6°·30
0 ... 1	6·18
1 ... 3	6·88
3 ... 9	6·70
6 ... 18	8·58
56 ... 57	9·75
57 ... 60	10·64
80 ... 85	12·14

About four hours afterwards on the same day I tried again on the same part of the circle, viz. from 1° to 4°, for fifteen minutes, and obtained a rate of 12°·4 per hour. The next day, from 5° to 6°, for twenty minutes, the rate was only 4°·38 per hour. Several other similar results were obtained, with which I shall not trouble you; but these, contrasted with the experiments made with the leaden ball, in which no such irregularities were ever observed, render it evident that the iron weight was deflected by magnetic currents, and that it is utterly impossible to obtain correct results in these experiments when the pendulum-bob is made of that metal*.

I am, Gentlemen,

Yours very respectfully,

THOMAS G. BUNT.

VII. *On the Anticlinal Line of the London and Hampshire Basins. By* P. J. MARTIN, *Esq.*

To Richard Taylor, Esq.

MY DEAR SIR, Pulborough, May 26, 1851.

THE renewed interest which geologists take in the *modus operandi* of the great chalk denudation of the *Weald* since the advancement of Sir Roderick Murchison's paper, read to the Geological Society on the 14th instant, inclines me to request that you would lend me your assistance for the promulgation of some thoughts on the same subject, and for the description of some additional natural appearances strongly illustrative, as I think, of that phænomenon.

So long ago as the year 1828 you did me the honour to review my first publication (see vol. iv. New Series, Phil. Mag.), in which, as an appendage to a memoir descriptive of Western Sussex, and taking the geological structure of that district as a type of the whole, I ventured to bring forward a "*Theory of Disruption and Denudation*" as a corollary to Dr. Buckland's paper on "Valleys of Elevation," published in the Geological Transactions of the foregoing year. The year following (in 1829) you also published a paper in which, as further illustrative of the subject, I made an attempt to restore the lost beds on the dome of the Weald; showing that, but for the instrumentality of the concomitant flood, the *upburst of the Wealden* would have produced an elevation of at least four or five thousand feet, instead of an excavation of as many hundred, above the sea-level. In a manner, therefore, by an approximate synthesis as well as analysis, we were led to the conclusion that the idea first broached by Dr. Buckland in the before-mentioned paper was the correct one;

* A Postscript to this paper will be found at p. 81.

namely, that the basins, as they are called, of London and Hampshire were once united; or more properly, as I showed in my "Memoir," that these great synclinals had no existence till the convulsion we then contemplated raised the barrier between them; and that they could not therefore be the areas of a marine deposit posterior to the epoch of their formation.

At the time these speculations were given to the world, men's minds were fully engaged in the investigation of the formation and succession of strata, their age and organic contents, and less to geological structure; and there was a disposition to repress opinions founded on any appeal to periods of extraordinary activity. The doctrines of uniformity in geological causation had then the ascendency, and Sir Charles Lyell advanced his theory of the gradual erosion of the Weald and the quiet transport of the materials into the adjoining basins*. For several years after this I was myself otherwise and better employed, and did nothing in the prosecution of my research; but from time to time other observers ventured timidly to differ from Mr. Lyell, especially when in contemplation of the accidents of water-shed, and as appearances of violent disruption were occasionally developed†. Still, having full confidence in the truthfulness of my early interpretation of the structural phænomena ever under my observation, and intending some day to satisfy myself with giving the world a history of the Weald denudation, I made occasional excursions, as my leisure would permit, into Hampshire and Wiltshire, believing that the same parallel lines of fracture and concomitant aqueous denudation would there exhibit (in the great chalk dome of those counties), *mutatis mutandis,* the same or similar features. I say "with a difference," because it could not be supposed that a country composed of so ductile a material as the chalk, ever reluctant to disclose the secrets of its disposition except when great disruptive violence has been used, would offer the broad and unmistakeable marks of disturbance to be found in the variable beds of the Wealden,—its flexible clays and its frangible sandstones.

I felt the propriety of resuming the task I had assigned myself in this direction on two accounts. First, because I have always found that, in discussing the affairs of the Weald, even

* This hypothesis took such entire possession of the public mind, and my opinions fell so much into abeyance, that two or three years after, when I ventured to repeat them before the Philosophical and Literary Society of Chichester, I was told that I was "all wrong,"—"that Mr. Lyell had given the true explanation, and Dr. Mantell had confirmed it." I thought that this hypothesis of Sir C. Lyell had been entirely withdrawn; but I have been informed that it appears in the latest edition of his Elements.

† See Dr. Fitton's "Geology of Hastings," "History of the Beds below the Chalk," Geol. Trans.; Dr. Mantell's Geology of S.E. of England, &c.

persons generally well-informed on the subject found it difficult, or could never be persuaded to abstract themselves from the notion, that the Weald denudation was a piece by itself, that it was to be viewed and spoken of as a district from which certain removals had been effected, by whatever means, and where changes had taken place, in which the neighbouring countries did not share. It was therefore most desirable that Dr. Buckland's original views of the elevation of the great chalk district should be restored in the public mind; and that the idea of a general denudation of all the south-east part of our island should take the place of the denudation of the Weald, of which general denudation the latter was only a part. In my Memoir of 1828 the case is so put, and in that sense it was my object to revive the discussion. Secondly, I was the more inclined to take this course, because I found that, whilst I was preparing myself for bringing the elevation and denudation of the Hampshire district into relation with the other parts of the same parallel, Mr. Hopkins was engaged in a review of the Weald in illustration of his theory of fracture and displacement; and it was highly desirable that any agreement in our views, or any discrepancy, if any such should exist, should stand in juxtaposition. On this account I put myself in communication with Mr. Hopkins, and brought forward a paper "On the probable Connexion of the Eastern and Western Chalk Denudations." This paper was read before the Geological Society in the early part of the session of 1840–41, and, as I was told, was ordered to be printed; but when inquiry was made for it in order to its correction before publication, I was told *"it was lost."* This was the more to be regretted, because it ought to have appeared contemporaneously with Mr. Hopkins's essay, to which it would have been ancillary; and because that gentleman did me the honour to borrow it, in order to bring his own in relation with it, as much as the difference of our practical or theoretical views would allow. And to myself it was more a matter of regret, because I lost the opportunity of explaining some of the discrepancies to which Mr. Hopkins refers*. But the *oubliettes* of the council chamber of the Geological Society are not bottomless; and two years after the publication of Mr. Hopkins's paper, or about three years ago, my MSS. were returned to me without explanation†. I now hand

* Vide Hopkins "On the Geological Structure of the Wealden District," *passim*, Trans. of Geol. Soc., vol. vii. 1845.

† I am willing to believe that the temporary disappearance of my MS. was the effect of accident, notwithstanding that a large roll of papers, with six sections of the Ordnance Map along with it, must have occupied some space; and notwithstanding that any, the most trivial matter, concerning a question which has been so long lying at the very doors of the Society undecided, and about which its two greatest celebrities are now openly at issue might have challenged more care.

them to you to be published, if you please, in your Magazine. And, with only two corrections, which I had proposed to make before their appearance in the Society's publications, I pledge myself to the general fidelity of the details both of fact and theoretical inference. In the last paragraph of my unfortunate paper, you will observe that I propose to myself and to the Geological Society a continuance of the investigation into other parts of the great anticlinal line. This I shall now do with your permission; and I shall unreservedly and without hesitation finally reproduce my original proposition of the contemporaneity of upburst and denuding flood over an area of at least four degrees of longitude, from the chalk of the *Pas de Calais* to where the line of elevation at Devizes is met by that of the general line extending from S.W. to N.E. from the Dorsetshire to the Yorkshire coasts. I should also say, of as many degrees of latitude, but, except as a speculation of the highest order of probability, I am not prepared to include the parallel lines of elevation of the Isle of Wight in this disquisition; and it would be an area inconveniently large for the discussion of matters of a practical nature.

I am, dear Sir,

Yours very truly,

P. J. Martin.

[Read before the Geological Society, December 16, 1840.]

A paper was read before the Geological Society in 1827, afterwards enlarged into a small quarto volume, published the following year, under the title of a "Memoir on a part of Western Sussex, with some observations on the Weald Denudation," &c. In this paper some new facts and some speculations were advanced on the construction of the Weald,—its cross fractures, drainage and other phænomena, illustrative of the simultaneous operations of upheaving force, and violent aqueous abrasion.

In pursuance of the subject, some additional remarks were offered to the public in the Philosophical Magazine for February 1829, on the extent and magnitude of the abraded materials; and to show also, that although the word "denudation" was usually restricted to the Weald valley, the chalk country, especially that of Hampshire and Wiltshire, ought to be included in the same category; and, in fine, that the upburst of the Wealden, and the area comprised in the chalk-boundaries of what was called "the great denudation of the Weald," did not present any features attributable to disturbing forces, distinct and separable from those of the surrounding districts.

The object of the present inquiry is to trace the lines of disturbance from and into the Weald, through the great expanse of chalk which separates the western parts of the basins of London and Hampshire, and their probable connexion with the corresponding lines of the vales of Pewsey, Warminster and Wardour. The construction and arrangement of the chalk country will follow; and the whole will serve, when the drainage and other geological and geographical features come to be considered, as a suitable introduction to a reconsideration of the phænomena of disturbance and denudation. The district in question, moreover, forms with the Boulonnais a convenient geological whole, whether considered apart, or in conjunction with other "lines of elevation," or denuded countries of suspected contemporaneity.

In reference to the connexion of the Weald with the Wiltshire valleys above mentioned, the sum of our present knowledge may be gathered from the following passage in Professor Phillips's "Treatise on Geology*." "In England, two lines of subterranean movement have long been known, by which the tertiary and secondary strata have been raised into anticlinal ridges and sunk into synclinal hollows. They both range east and west, or nearly so; one line, viz. from the Vale of Pewsey, by Kingsclere, Farnham, Guildford, and through the Weald of Sussex to Boulogne, is somewhat parallel to the vale of the Thames, &c." And in reference to the Vale of Wardour, and its probable connexion with the western extremity of the Weald, the latest information is to be derived from Dr. Fitton's History of the Strata below the Chalk†. Dr. Fitton says, "The beds at Harnham Hill, immediately on the south of Salisbury, are inclined to the north (this should be *dip* to the north, the *inclination* southerly), and about a mile to the west of that hill a curved ridge or horeshoe, formed of the upper chalk, seems to be the first divarication of the strata which bound the Vale of Wardour. It therefore deserves inquiry, whether the continuation of the fissure produced by an upheaving on the east of this point may not be discoverable in the space between Salisbury and the head of the Wealden denudation."

These two quotations will serve for the starting-points of our inquiry; and I will anticipate the result so far as to state, that, as regards the Pewsey line of elevation, a vertical disruption is traceable from Folkstone along the whole range of the North Downs (and is mainly instrumental in prescribing their southern limits), which, entering the chalk near Farnham, passes by, and does *not* unite with the Pewsey anticlinal line, as continued on in the Burghclere Hills and Vale of Kingsclere. And secondly, in regard to the anticlinal line of Wardour; that it is continued across the Avon and the Test, and like the before-mentioned northern line, passes by and dies out, but does *not* unite or inos-

* Vol. i. p. 260 (Lardner's Cyclopædia).
† Geol. Trans., vol. iv. 2nd series, p. 245.

culate with another which is projected westward from the "head of the Wealden denudation."

On inspecting Mr. Greenough's map or Dr. Fitton's appended to the above-mentioned memoir, it will be observed that a direct point is made by the Vale of Wardour toward the south west corner of the head of the Weald. And in like manner, an inclination is shown by the Pewsey line of valleys at Kingsclere to bend toward the north-west corner at Farnham. The axes of elevation appear to run in these two directions, and the geographical features of the country are in accordance with them, as indicated by the high grounds. Nevertheless, the anticlinal lines which constitute these axes of elevation are distinct and separate, whether they be contemporaneous or not. The Burghclere Hills sink down and do not unite with the North Downs; and the high grounds south of Salisbury, although continued eastward to the Test, in the neighbourhood of Romsey, decline in a synclinal hollow to rise again in the high hills about Winchester, which are the proper continuation of the South Downs.

To make out this in detail, I begin at the upper or western end of the Weald denudation. This district, which Dr. Fitton has called the "head of the Weald denudation," is a valley ranging almost directly north and south, about sixteen or seventeen miles in length and (east and west) five or six in breadth. We may for the sake of brevity call it the Wolmar Valley, Wolmar Forest being one of its most prominent features. It is bounded on the north by the Hogsback, on the south by the bold eminences of Butser Hill and the South Down range, on the west by the Alton Hills—these three sides all being of chalk—and on the east by an elevated platform of the lower greensand, comprising the Hindhead and Blackdown Hills, with the intervening high grounds of Haslemere. This valley is wholly composed of galt and lower greensand, except a narrow slip of Weald clay in Hartingcombe, as described by Mr. Murchison*. Of the cause of this intrusion of the Weald clay I shall have to speak by and by. Three remarkable anticlinal lines traverse the Wolmar Valley, entering eastward from the greater expansion of the Weald. The middle and most important of them is the great central line of the Weald, which enters the platform above mentioned by a well-marked "valley of elevation" and erosion, and cuts through the lower beds of the lower greensand with much appearance of disruptive violence at Haslemere. The valley thus formed is based on Weald clay, and at an elevation of five or six hundred feet above the level of the sea runs directly west toward Liphook, and is lost as it opens into the greater expanse of the Wolmar Valley. In its further progress this cen-

* Geol. Trans., vol. ii. 2nd series, p. 102.

tral line heaves in succession the higher beds of the lower greensand and the galt; and tilting up the malm or upper greensand beds, it gives them a prominence which they do not elsewhere assume, and enters the chalk between the salient angles of Hawksley and Worldham. The central rent is at Selborne, and the highest point in the chalk hills of this part of the Alton range is a little south and west of that village.

South of the central anticlinal line another enters the Wolmar Valley by Trotton and Rogate, and passing by Petersfield enters the upper greensand and chalk about a mile north of East-meon*. As this line runs the whole length of Sussex, and then traverses the Hampshire chalk and crosses the Test, it will merit a more particular description.

The third anticlinal line which traverses the Wolmar Valley runs north of the other two, and is more strongly marked in this part of its course than the one last mentioned, though elsewhere its features are by no means so prominent. As this line is to carry us on towards Pewsey, and is otherwise so remarkable, I shall give a detailed description of its course from the neighbourhood of Guildford into the chalk hills west of Farnham. But before we quit the Wolmar Valley I must be allowed to say a few words more on the Haslemere and Hindhead platform, particularly as its position is strongly illustrative of the range and operation of these three principal anticlinal lines. Viewed from any good central position in the western Weald, as say Loxwood, Billingshurst or Five Oaks, the platform in question looks like a high table-land ranging north and south. It swells a little at each end to form the hills of Hindhead and Blackdown, and thence declines very gently each way toward the Haslemere gap in the centre. Its appearance when viewed in this position is doubly interesting to the geological observer, when he understands that it is sustained on the central line of elevation, with its two extremities resting on the two lateral parallel lines†.

Turning our attention now to the north side of the Wolmar Valley, we are arrested by the appearance of a remarkable con-

* I have since seen reason to correct this statement. The anticlinal line of Winchester, after passing up the Valley of Chilcomb, takes its progress eastward in an intumescence of the chalk, forming a range of high grounds terminating in the Vale of East and West Meon. The synclinal of this elevation appears to be the Vale of Bramdean, a remarkable longitudinal valley, containing, as to be afterwards noticed, an immense accumulation of angular flints. Its synclinal character is the more probable, as it gives rise to an affluent of the Itchin.

† The copious springs that burst out in the fissure west of Haslemere can only be supplied by the rains that fall on this platform of green sandstone; and the manner in which it is tapped and the water drawn off westward, in the direction of the decline of the Weald clay toward the Hampshire chalk, is as curious as it is instructive.

tortion or irregular saddle, part of an anticlinal line, which I shall call the Peasemarsh line, Dr. Fitton having first pointed out the rise of the Weald clay at that place. The sudden dip of the chalk at the west end of the Hogsback, and the remarkable pre-eminence of the tertiary beds in Farnham Beacon Hill, are produced by this contortion. The Peasemarsh line is here screwed up, as it were, hard to the chalk. In its progress eastward into the broad vale from which it takes its name, it recedes from the chalk and attains its culminating point, or point of greatest intensity, at Peasemarsh, and brings the Weald clay to the surface. And here also, as might be expected, it opens up the transverse fissure by which the river Wey is discharged through the chalk at Guildford. Further east this anticlinal valley is prolonged toward Albury and Shire, bringing down one of the tributaries of the Wey; the lower greensand hills on either side showing the anticlinal disposition, as Dr. Fitton has already pointed out*.

Returning westward to Farnham, the line of disturbance is found to have changed its character, and it begins to heave the galt at Wracklesham†. Here the anticlinal disposition may be tested by examination of the sand-hollow on the north side of the village by the parsonage house, and in the sand-pits a little further up the stream. A galt saddle succeeds, which, with its synclinal replication toward the central ridge, produces the broad expanse of Alice-holt. Further west the galt saddle is continued through Bentley Green, the malm or upper greensand lying on each side,—southerly in the high grounds of Binstead, and northerly in those of Bentley Church and Berry Court. As the synclinal line brings out a great exposure of galt in Alice-holt, so also it produces a great body of malm in the Binstead country. Holybourne Froyle and Bentley form the confluence of the malm or upper greensand, and the line of elevation enters the chalk by Peacombe and Lower Froyle. The chalk succeeds and attains its highest altitude near where Shaldon Copse is to be found in the Ordnance map, and is continued westerly in a broad expanse of highlands, marked in the Ordnance Map by the names of Lipscomb, Ellisfield, Dummer and Popham. From this line, the country declines gently northward by South Warnborough, Uptongrey and Hackwood Park; and southward into a well-marked synclinal valley running westward from Alton to Axford and Woodmancote. At this part of the line Dummer Farm and Popham Farm are on the top of the ridge‡. At Popham Beacon it is inter-

* Trans. Geol. Soc. *loc. cit.* p. 142.

† If this is not the same line of contortion as Peasemarsh, losing much of its intensity west of Farnham, it is a new and independent anticlinal. But I prefer the former view of the case.

‡ This intumescence of the chalk answers very much to that which follows the Winchester line east of Chilcomb. *Vide infra.*

sected by the Southampton Railway at the height of 454 feet above the sea-level; but at this point it has lost much of its intensity. Westward from Popham I cannot say that I have made a very satisfactory exploration. Denudations and river-courses, and the absence of satisfactory sections just where they are wanted, make it very puzzling. My impression is, that, keeping the infant Test in its northern synclinal line, it passes the great gap in the Burghclere Hills north of Whitchurch, and takes on in the vicinity of Andover towards Wayhill. The stratification of the chalk-pit on the south side of the town of Andover exhibits too much dip to have come from the Burghclere line without a reduplication; and the occurrence of a considerable tract of country of a tertiary character in Harewood Forest, favours the supposition of a synclinal arrangement. However this may be, the superior swell of the Pewsey line of upheaval now throws all other into shade, and the drainage is sufficient evidence of the constant slope southward of all the country west of the Popham heights, and the almost entire extinction of this line of elevation.

At this stage of the inquiry, finding that there was now no chance of the Peasemarsh running into the Pewsey anticlinal, and that I was moving in a line parallel with the Burghclere Hills, eight miles north of Popham, it became a point of much interest to ascertain the progress of the Pewsey line, and how it stood in relation to the one I have been describing.

As all the relations of the Vale of Pewsey are well known, and the history of its anticlinal line has been carried on by Dr. Buckland* through the vales of Ham and Kingsclere, it would be superfluous to repeat them. The great peculiarity in the features of this line, after leaving the Pewsey denudation, is its extreme irregularity.

In the Vale of Pewsey the axis of elevation appears to run nearly in the centre of the denudation, the strata sloping off with a gentle declination north and south. But in its progress eastward the northerly dip is much sharper than the southerly†. The effect of this disposition is to bring the tertiary beds often almost to the foot of the Burghclere Hills. This is particularly perceptible after the line emerges from the Vale of Kingsclere. At the eastern extremity of this valley the chalk is confluent at Wolverton Farm. East of this confluence a well-marked anticlinal chalk valley runs out towards Monk-Sherbourne; and evidence may be obtained of the northerly dip, in Wolverton chalk-

* Geol. Trans., vol. xi. 2nd series.
† Dr. Buckland, *loc. cit.*

pit, in a copse on the left of the hollow way that leads from Wolverton Park to Hannington. Here the dip being 30 or 40 degrees, the chalk is soon lost beneath the tertiary beds; but south of this locality it still preserves the character of the Burghclere Hills, sloping away southerly into the broad synclinal hollow which intervenes between the high grounds of Hannington and those of North Waltham, Popham and Dummer, which belong to the Peasemarsh line.

From the narrow anticlinal chalk valley before spoken of, between Wolverton Street and Hannington, the elevation is continued south-easterly and then easterly, till it dies away under the plastic clay at Old Basing; and in a great part of that course it presents a chalk escarpment to the north, with the tertiary beds, as at Ewhurst and Ramsdell, lying almost at its foot*.

The country west of Basingstoke, between Hannington on the expiring Pewsey line, and Dummer on the Peasemarsh, as before said, lies a broad synclinal hollow, in which, and apparently produced by that disposition of the beds, runs the little stream that supplies the head of the Basingstoke Canal.

Vale of Wardour Line.

To trace out the course of this line from where it was left by Dr. Fitton at Harnham Hill, south of Salisbury, was the next object of research. The chalk beds of Harnham Hill dip northward, and those of the country north of Salisbury southward; consequently that town stands in a synclinal depression, which is in part occupied by the tertiary beds. The sexton of Salisbury Cathedral tells you that the cathedral is founded on a bed of stiff clay. This has all the character of plastic clay, of which I had satisfactory evidence from an open grave in the north transept at my late visit. The river Nadder and its alluvium occupying the western extremity of the valley, I do not know exactly where it begins to exhibit signs of the existence of the tertiary beds, but I found them fully developed at and near Bemerton, within a mile of Salisbury. Crossing the Avon east of that place on the Romsey road, we find that the hollow, south of Ashley Hill, where the words dog-kennel occur in the Ordnance Map, is occupied by

* If this line of elevation is renewed east of Basing, it is most probably in the Isle of Thanet, a chalk "Outlier-by-protrusion" from the Kentish chalk. Of the manner in which these parallel lines may pass interruptedly, and sometimes silently through a country, we have a good example in the less questionable case of Portsdown Hill in Hampshire, High Down near Worthing in Sussex, and the cliffs at Seaford in the same county, all elevations of the same character lying in the same line east and west, with a dip opposed to the prevailing one of the intervening country. An elevatory force, acting with greater intensity at these points, can alone explain the coincidence.

drift; but the tertiary beds appear at the brick-kilns and Clarendon Lodge, and advance in importance as we proceed eastward, so as to occupy all the country in which the words Alderbury, Whitmarsh-bottom, Bentley Wood, Berrywell Wood, and French Moor are to be found. The chalk on which these tertiary beds repose emerges at East Grinstead and West Dean, in a low ridge dipping sharply north; whilst the opposite side of the anticlinal consists in the strongly-marked feature of Dean Hill* dipping southerly. Grimstead Fields is a chalk saddle between these two ridges. Still proceeding eastward, the northern ridge is gradually intruded upon by the plastic clay, and is lost under it at Lockerly. The southern, in the line of Dean Hill, still maintains its importance; but that also slopes away to the south-east, and is soon covered up by tertiary formations: Mount Farm, Butler's Wood, Uphill, Roke, are tertiary or shingle beds of the Eocene period. Thus the anticlinal line of the Vale of Wardour, after a course of about six miles east of the Avon, sinks under a saddle of the tertiary beds at, or close upon, the Test. A cursory examination of the country from Michelmarsh and Timsbury on the last-mentioned river, by Anfield toward Otterbourne on the Itchin, gave me some idea of a continuation of this saddle of tertiary sands and clays, with an escarpment in the high grounds of Toothill and Chilworth on the south. But of this I cannot speak with confidence. I am, however, quite assured, that although it may be possible to discover marks of a continuation of this line of elevation eastward, it is not connected with that which I am about to describe, and which issues westward from the south-west corner of the Weald denudation. These two, like the Pewsey and Peasemarsh lines, pass by and do not inosculate with each other.

[To be continued.]

VIII. *Historical Sketch of the progress of improvement in the application of Electro-Magnetism to Geodetical and Astronomical purposes*†. *By* G. P. Bond‡.

THROUGH the kindness of Dr. Bache and Prof. Walker of the United States Coast Survey, I am enabled to give from

* If Dean Hill had the chalk colour, which it ought to have had, in Mr. Greenough's map, as ought also the line of high ground running west in the course of the words "proposed canal," then the tertiary colour would have been seen projected north of it, over the localities above specified, toward Salisbury; the synclinal line being a trough of tertiary beds, and the *cause* of this projection.

† Communicated by the Author.

‡ The article consists, mainly, of extracts from an official communication from Prof. Walker to the Superintendent of the United States Coast Survey, dated April 24th, 1851.

the records of that department, the following abstract of the history of an invention recently brought into use in America, by which electro-magnetism is introduced as an agent in the determination of differences of terrestrial longitude, and for various astronomical purposes in which the exact noting of time enters as an important element.

On the 9th of June 1844, Capt. Charles Wilkes, U.S.N., made the first experiment for determining longitudes by means of the electric telegraph, between Washington and Baltimore, with chronometers rated at each place. All subsequent experiments for determining longitudes by the electric telegraph in the United States, have been made at the expense of the Coast Survey, and by its officers, or by their request, and under their immediate supervision.

On the 10th of October 1846, star-signals were first exchanged between the Washington Observatory and that of the Central High School of Philadelphia. The outfit of telegraph junction lines and apparatus was made by the Coast Survey. The use of the astronomical instruments for the occasion at the Washington Observatory, had been offered by Lieut. Matthew F. Maury, U.S.N., superintendent.

The experiment was made under the charge of Sears C. Walker, Esq., one of the assistants of the Coast Survey, who from that time to the present, under an appointment from Prof. A. D. Bache, LL.D., superintendent, has had uninterrupted charge of this work. The apparatus used this evening was devised and constructed by Joseph Saxton, Esq. The star-signals, or taps on a make-circuit finger-key at the instant of the passage of a star over a wire of a transit instrument, were made that night by Lieut. J. J. Almy, U.S.N., and were recorded by the ear by Mr. Walker and Lieut. J. M. Gillin, U.S.N., at Washington, and Prof. E. O. Kendall, Director of the Philadelphia High School Observatory at Philadelphia.

The longitude between the two stations by this night's work agrees within 0·2 second with the average of all the work done since.

On the 27th of July 1847, coincidence of beats of solar and sidereal chronometers were for the first time tried between Philadelphia and Jersey city. These coincidences were noted at each place by comparison of a solar and sidereal time-keeper. The circuit of the telegraph line was closed temporarily every ten seconds by the astronomer at one of the stations, and the receiving magnet beats were heard sensibly at the same instant of absolute time at both stations. The date of coincidences of these magnet beats with the stationary clock beats (the one being at solar the other at sidereal time), were recorded at both stations. This experiment was repeatedly performed that year by Mr. Walker, assisted at Philadelphia by Prof. E. O. Kendall, Director, and at Jersey city by Prof. E. Loomis.

In July and August 1848, an extensive series of star-signals and clock-signals, by coincidences, were exchanged between the Harvard Observatory at Cambridge, Mass. and the observatory in the garden of the late Peter Stuyvesant in New York city. The work was under the charge of Mr. Walker, assisted respectively by William Crouch Bond, Esq., Director of the Observatory at Cambridge, and Prof. E. Loomis at New York city. During these experiments, Mr. Bond conceived the idea of using an automatic circuit interrupter, and on the recommendation of Mr. Walker, received in July 1848, an order from Prof. Bache, superintendent, for the construction of a clock for this purpose in conformity with Mr. Bond's drawings, then before the superintendent.

This clock was completed in 1850, and forms part of the apparatus in use at Cambridge in 1850 and 1851. The work of 1848, in July and August, forms the date of the first connexion of Mr. Bond and his two sons, Messrs. George P. and Richard F. Bond, with the use of the magnetic telegraph line for longitude, and with the machinery and apparatus for the same. It preceded by two months the work between Philadelphia and Cincinnati of the year 1848, when in the month of October the attention of Prof. O. M. Mitchell, and afterwards of Dr. John Locke, was turned to the subject. The fact that Prof. Bache had ordered an automatic circuit interrupter of Mr. Bond in the preceding August, was communicated both to Prof. Mitchell and Dr. Locke previous to their undertaking similar experiments.

On the 26th of October 1848, Prof. O. M. Mitchell, at the suggestion of Mr. Walker, prepared a circuit interrupter with an ordinary eight-day clock, and used it to graduate the running fillets of paper for several days.

It was not used in the work with Philadelphia, clouds having prevented work on the 27th, proposed for the purpose. The same mode which Prof. Mitchell used had been proposed by Joseph Saxton, Esq. in 1846, but has not been adopted by Prof. Bache and Mr. Walker, from apprehension of injury to the performance of the astronomical clock which must be used for the purpose. This apprehension we know by experience to have been groundless.

On the 26th of October 1848, Dr. J. Locke having stated his objection to Mr. Bond's contrivance of a circuit interrupter, was requested by Mr. Walker, on behalf of the superintendent, to undertake experiments to obviate them.

On the 17th of November 1848, Mr. Walker receiving notice from Dr. Locke that he and his sons had completed an automatic circuit interrupter, extended a junction-wire from the Cincinnati Telegraph Office, so as to embrace Dr. Locke's clock at his house, fitted up as a circuit breaker, with a tilt hammer struck by the teeth of the escapement wheel. Mr. Walker also, acting for the Coast Survey, engaged the use of the line from Louisville to Pittsburg, to try the experiment with Dr. Locke's contrivance. No astronomical nor clock-signals were exchanged this evening, and no attempt was made to determine longitudes. In this experiment Dr. Locke's clock graduated a fillet of paper as delivered by the Morse register.

In 1849, January 19th, the first actual experiment of the automatic imprint of star-signals on a time scale was made between Philadelphia and Cincinnati. The telegraph line from Philadelphia to Cincinnati was engaged for use of the Coast Survey by Mr. Walker. The automatic clock interrupter was furnished by Dr. Locke at Cincinnati. The star-signals were given by Prof. Kendall at Philadelphia, and recorded at both places. The Cincinnati Observatory, in the absence of Prof. Mitchell, could not be used for the purpose of longitudes.

The longitudes of Cambridge, New York and Philadelphia, were determined on the 23rd of January 1849 by star-transit signals, given for the same star as it passed the meridian of these three stations. These signals were recorded at Washington, Philadelphia and Cambridge. The managements were under the charge of Mr. Walker. The circuit-breaking clock was prepared by Mr. Walker on Dr. Locke's plan, and located at Philadelphia. The same clock contained a tilt-hammer interrupter for making signals by the teeth of the hour-wheel every two minutes. This instrument was invented in the year 1847 by J. J. Speed, Esq., President of the Telegraph Company in Detroit, Michigan.

The detection of a delay in the transmission of the galvanic inducing wave proportionate to the space traversed, was made by Mr. Walker immediately after examining and comparing together the registers of the four stations above mentioned.

The consideration of this phænomenon led Prof. Walker to the discovery of the velocity of the galvanic wave. His articles on the subject have been published in the Proceedings of the American Philosophical Society for March 1849, in Silliman's Journal of Science, and in the Astronomische Nachrichten. A velocity of 15,400 miles per second is given by him as the most probable result.

In the summer of 1849, Prof. Mitchell proposed the use of a revolving disc of type-metal to receive the records. Mr. Saxton's plan of receiving the records upon a sheet of paper rolled upon a cylinder, seems to be that which combines the most practical advantages. Mr. Saxton proposed to break the circuit by a tilt-hammer struck by a projecting piece of glass from the middle of the pendulum, which acts as a circuit-breaker; he also contrived an apparatus for making on the sheet the 0, 5, 10, &c. millims. by the omission of one or two breaks respectively. Mr. Saxton's apparatus has been in use ever since at the Seaton station; its only defect is the want of uniformity in the time of revolution of the cylinder.

On the 12th of April, Mr. Bond submitted to Prof. Bache a model of an invention made with a view to remedy this remaining defect. This instrument has been named the Spring Governor. A perfect working instrument was ordered for the use of the Coast Survey at that time. The model was completed and reported upon in November 1850. The cylinder, covered with a paper, revolves once in a minute, and measures time with the precision of an astronomical clock. The sheet, when taken off after being graduated by the clock, has the minute columns vertical. The seconds are marked off horizontally on each minute scale. The eye seizes on the appropriate hour, minute, and whole second, as in an ordinary astronomical table of double-entry; the fraction of a second may be estimated to a tenth by the eye, or read to a hundredth by a graduated scale. A year's work of an ordinary observatory may be bound up in a volume of a few hundred pages, and forms a permanent and legible record of the actual dates of the imprinted transit signals.

By means of the line connecting the observatory at Cambridge with Boston, the time for the use of shipping and for the railroads throughout New England is now regularly transmitted by merely passing the circuit through the clock at Cambridge. Its beats are thus given through a distance of one or two hundred miles. One o'clock has been adopted as the hour for these signals.

The courtesy with which the Telegraph Companies in different parts of the United States have met applications for the use of their lines for scientific purposes, deserves particular acknowledgement, as having contributed most effectually to the success of these operations.

IX. *On certain Questions relating to the Theory of Probabilities.*—Part III. *By* W. F. Donkin, *M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy in the University of Oxford.*

I PROPOSE in this third and last communication, to offer a few remarks on the method of least squares, chiefly with reference to Mr. Ellis's paper on that subject in the Philosophical Magazine for November 1850.

If we are asked what is the method of obtaining the most probable result from a system of observations not numerous enough to justify, as an approximation, the supposition that they are *infinite* in number, it is plain that no answer can be given till we are told whether it is to be assumed that the law or laws of facility of errors in the individual observations are known, or unknown; or, to speak more accurately, until we are told what is to be assumed as the state of information of the observer concerning the laws in question. For the probability of every hypothesis depends upon the state of information presupposed concerning it.

If the law of facility of errors (which we will suppose, for simplicity, the same in all the observations) be assumed as known, the problem involves no difficulty of principle, though for most laws the required integrations would be impracticable.

But if the law be wholly or partially unknown, though it is still easy to indicate the way in which the problem ought, theoretically, to be treated, the processes required are, in all actual cases, entirely beyond the present powers of analysis.

To illustrate this, consider the case in which all the observations refer directly to a single unknown quantity x. If a, a', a'', ... be the observed values, and ϕ were known to be the function expressing the law of facility of errors, then the probability that the true value lies between x and $x+dx$ would be

$$\mathrm{C}.\phi(x-a)\phi(x-a')\phi(x-a'')\ldots dx, \quad . \quad . \quad . \quad (1.)$$

where C is determined by the condition that the integral of this expression, extended to all admissible values of x, shall be equal to 1.

Now suppose that the function ϕ is not known, but may be of any of the forms ϕ_1, ϕ_2, ϕ_3, ... and let p_i be the probability that it is ϕ_i. Then instead of the expression (1.) we should have

$$\Sigma\{\mathrm{C}_i p_i \phi_i(x-a)\phi_i(x-a')\ldots dx\},$$

the summation extending to all the actual values of i.

In the ordinary cases occurring in practice, nothing is known of the form of ϕ, except that it must satisfy some very general conditions, such as that smaller errors are more probable than larger, &c.; the number of supposable forms is therefore infinite, and the summation indicated in the preceding expression would depend upon a calculus bearing the same relation to continuous variation of *form*, that the integral calculus does to continuous variation of *value*. Such a method, it is needless to say, does not at present exist; the calculus of Variations being, with reference to functional *form*, the imperfect analogue, not of the integral, but of the differential calculus.

The proposed problem, therefore, as applying to ordinary cases, has never been, and at present cannot be, solved.

But it is to be observed, that *if* it were solved, that is, if the summation just mentioned were actually performed, it cannot be assumed beforehand that the result would not turn out to be of the form $C\psi(x-a)\psi(x-a')\ldots dx$, giving *the same relative probability* for any value of x as would be obtained *if it were known that* $\psi(x)$ *were the function actually expressing the law of facility of errors in the individual observations.* Such a result would involve no *primâ facie* absurdity or difficulty, and it would not be a valid objection to it to say, that it professed to establish an independent external reality by *à priori* mathematical reasoning. For to prove that a required probability is to be calculated *as if* a certain hypothesis were known to be true, is a perfectly different thing from proving that that hypothesis *is* true, or from proving anything about the probability of its truth at all. To take a simple analogous case, suppose a bag contains an unknown number of balls, of unknown colours; a ball is drawn and replaced n times, and is white each time; now if a person professes to prove that the probability of drawing a white ball at the next trial is $\frac{n+1}{n+2}$, we may object to his proof on other grounds, but certainly not on the ground that he thereby assumes this to be the *actual ratio* of the number of white balls to the whole number of balls. Of course his answer would be, that he assumes no such thing, but only asserts that the probability relative to a certain state of information is the same as it would be if a certain hypothesis were known to be true. The fallacy consists in assuming, that because two probabilities are equal, the states of information to which they refer must be identical.

To return to the subject of observations. If the law of facility of errors were *known*, the mean of the observed values would not be the most probable result, unless the law were expressed by the well-known exponential function assigned by Gauss in his first investigation. But the law of facility *not being known*, although it has never been proved that the mean *is* the most probable result, relative to this state of information, it has certainly never been proved that it *is not*: the question is perfectly open; and whoever professes to prove the affirmative, ought not to be charged with pretending to prove that the law of facility is actually expressed by the function above mentioned. For anything that has yet been shown to the contrary, that function *may* truly express our *expectation* of the unknown law, and the true solution of the problem *may* be obtained by employing this "provisional" law, as if it were a known or "definitive" law. (See an analogous case discussed in the first paper, Phil. Mag.

for May, §§ 17, 18.) The reader who is acquainted with Mr. Ellis's paper, will see that I have been here referring to some parts of his reasoning at pp. 324, 325; and is requested also to observe, that in pointing out the invalidity of a particular objection against the Edinburgh Reviewer's result, I am not defending his argument, about which I shall say something hereafter.

If it be now asked what positive grounds there are for using the method of least squares in the case of a moderate number of observations, beyond motives of mere convenience, I think it may be answered that the method has been proved by Gauss (in the *Theoria Combinationis Observationum, &c.*) to be a very *good* method, though it has not been proved to be the *best* method. He has not shown that it gives the most probable result; but he has shown that it gives a result such, that if the whole system of observations were repeated an infinite number of times, the average value of the square of the error would be a minimum. I presume that Mr. Ellis does not mean to imply more than this when he says (p. 321) that "Gauss afterwards gave another demonstration which is perfectly rigorous." In fact, Gauss himself expressly points out that there is something arbitrary in assuming the *square* of the error as the function whose average value is to be a minimum. (*Theor. Comb.* § 6.) Perhaps he might have added, that the assumption is *less* arbitrary than any other which could have been made; but I shall not attempt to discuss the question how far this fact, supposing it admitted, would tend to give a demonstrative character to the reasoning, considered as an attempt to establish the method of least squares as the *best* method. The point to be observed is, that though Gauss rigorously demonstrates what he professes to demonstrate, he does not profess to *demonstrate the method of least squares*, in the sense in which these words would be commonly understood without explanation.

I shall conclude with a few general remarks on the other proofs which have been, or may be offered, of this remarkable method. And I must remind the reader, that everything which is here said applies only to the case in which the actual law of facility of errors is not known.

Since the rigorous solution of the problem is unattainable, every professed solution which puts on an appearance of demonstration must involve an *assumption*, leading more or less directly to the employment of a particular law of facility as if it were known to be the actual law. And it would appear natural to prefer that solution in which the assumed condition should be most simple, least arbitrary, and most in accordance with common notions and experience. That all solutions which have any pretensions to these qualifications agree in the same result is certainly a very remarkable circumstance, and one which can hardly fail to excite some degree of expectation that this result will turn out to be the true one, if the problem should ever be really solved. I shall not go through an examination of instances in illustration of the above remark; but there are two which I must mention briefly.

The first is the Edinburgh Reviewer's proof, commented upon by Mr. Ellis. Of course it was easy to annihilate it, considered as a professed *demonstration.* But if it had only pretended to be what it really is, a proof founded upon an assumption (of the independence of errors in directions at right angles to one another) which is simple and not more arbitrary than the assumptions made in other proofs, while it leads to the result with remarkable ease and directness, it would, I think, have deserved to be treated with respect. It is to be regretted that the Reviewer should have failed to see, or at least to point out, its real character.

The second instance is a proof proposed by myself some years ago in an Essay published by the Ashmolean Society, and since abridged in Liouville's Journal, vol. xv. This proof depended upon a more complete and systematic development of the analogy between the balance of evidence and the balance of forces, than had been before attempted, and was published chiefly on account of the interest which belongs (at least in my estimation) to all such analogies. I was therefore not concerned to point out, and indeed did not till lately clearly apprehend, what was the assumption really involved in it. This assumption is, that *the knowledge gained from a number of observations is the same in kind as that gained from a single observation.* It is easy to make this the foundation of the theory, treated according to the ordinary method; to begin, namely, by assuming that the function expressing the law of facility of error of the mean of two observations, is of the same form as that which expresses the law for the individual observations. I am inclined to think this assumption in itself more simple and natural than any other; but this is a matter of opinion.

I may add, that in the first paragraph of the preface to the English edition of the Essay just mentioned, I committed the fallacy which I have endeavoured to explain in the former part of this paper, of confounding the case in which the actual law of facility is unknown, with the case in which it is known.

Oxford, May 23, 1851.

P.S. Since Part II. of these remarks was printed, I have, through the kindness of Professor De Morgan, received his second memoir "On the Symbols of Logic," &c. (Camb. Phil. Trans., vol. ix.), with which I was not before acquainted. It contains some investigations on certain applications of the theory of probabilities, to which I ought to have referred. On the subject of the credibility of testimony, my remarks appear to be entirely consistent, so far as they go, with those of Professor De Morgan. But from what he has said in p. 46 of his Memoir, I am uncertain how far he would agree with my account of the surprise excited by the accidental occurrence of a symmetrical event. I shall not, however, enter further into that subject, as enough has been said to make it intelligible what the question is; and any reader who shall have taken the trouble to follow the reasoning of these papers and of Professor De Morgan's, will be in a position to form his own judgement upon it.

X. *On the Principles of Hydrodynamics.* *By* Professor Stokes.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As I do not see the remotest prospect of agreement between Professor Challis and myself respecting the principles of hydrodynamics, I think it time to fulfill the promise which I have already made you of discontinuing the controversy.

As, however, I have seen nothing to shake the firmness of my conviction, which I have already expressed, that the new equation is both unnecessary and untrue, I request that you will have the goodness to record my protest against it.

As I do not mean to continue the controversy, it would not become me to discuss the contents of Professor Challis's last communication. There is, however, one point in a former article which I will briefly notice. In alluding to the experiments by which it is (as I conceive) shown that compression *does directly* raise the temperature of air, Professor Challis speaks of the heat developed by compression as "being in the first moment of its generation in the state of radiant heat." (Phil. Mag., S. 4. vol. i. p. 407.) I do not know what Professor Challis's notions respecting the nature of radiant heat may be; but according to my own, I cannot understand how the heat developed by compression can be in the first instance in the state of radiant heat, or if it were, how the observed effects could be produced.

I remain, Gentlemen,

Yours sincerely,

G. G. Stokes.

Pembroke College, Cambridge,
June 12, 1851.

XI. *On the Mechanical Theory of Heat.* *By* W. J. Macquorn Rankine, *F.R.S.E. &c.*.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As you have announced your intention to publish a translation of the memoir of M. Clausius on the mechanical agency of heat, which appeared last summer in Poggendorff's *Annalen*, I beg leave to offer to you the English version of a letter on the subject of the *First Part* of M. Clausius's paper, which was addressed by me to the editor of that journal, and published in the 9th Number for 1850.

I am, Gentlemen,

Your most obedient Servant,

W. J. Macquorn Rankine.

30 Great George Street, Westminster, February 11, 1851.

Letter from Mr. Macquorn Rankine *to* Dr. J. C. Poggendorff*.

Having observed in your *Annalen der Physik und Chemie* for April 1850 a very able and interesting paper on the motive power of heat, by M. Clausius, I beg leave to call your attention to a paper which was presented by me to the Royal Society of Edinburgh in October 1849, read on the 4th of February 1850, and published in the 20th volume of their Transactions, Part First; in which paper, among other results, I have arrived at conclusions respecting the specific heat of gases and vapours, and the latent heat of evaporation, agreeing with those of M. Clausius, and deduced from principles, and by means of a method, which, though differing in some details from those employed by him, are the same in their essential points. In doing so, I have no wish to detract from the merit of M. Clausius, or to establish any rivalry between him and myself; on the contrary, I am gratified to find the results of my investigations confirmed by so eminent an authority.

It is probable that you have had, or will soon have, an opportunity of seeing the part of the Edinburgh Transactions to which I refer; should that not be case, however, I shall be glad to send you a copy of the paper as soon as I am aware of a convenient and secure means of conveyance. In the meanwhile I shall give you a summary of its contents.

It consists of an introduction and four sections.

The introduction explains the general principles of a conjecture

* From Poggendorff's *Annalen der Physik und Chemie* for 1850, No. 9.

as to the constitution of matter, called the *Hypothesis of Molecular Vortices.* Its chief suppositions are the following:—

1. That each atom of matter consists of a nucleus or central physical point enveloped by an elastic atmosphere, which is retained round it by attraction; so that the elasticity of bodies is made up of two parts,—one arising from the diffused portion of the atmospheres, and resisting change of *volume* only; the other arising from the mutual actions of the nuclei, and of the portions of atmosphere condensed round them, and resisting not only change of *volume,* but change of *figure* also.

2. That the changes of elasticity produced by heat arise from the centrifugal force of revolutions or oscillations amongst the particles of the atomic atmospheres, diffusing them to a greater distance from their nuclei, and thus increasing the elasticity which resists change of volume only, at the expense of that which resists change of figure also.

3. That the medium which transmits light and radiant heat consists of the *nuclei* of the atoms vibrating independently, or almost independently, of their atmospheres; *absorption* being the transference of motion from the nuclei to the atmospheres, and radiation its transference from the atmospheres to the nuclei.

This last supposition is peculiar to my own researches, the first two having more or less resemblance to ideas previously entertained by others.

The elasticity of the atomic atmosphere is supposed to vary according to the law of Marriotte.

The principal results arrived at in this introduction are the following:—

I. The specific gravities of the atomic atmospheres of all substances in the state of perfect gas are inversely proportional to the coefficients of elasticity of those atmospheres.

II. *Quantity of heat* is the *vis viva* of the motions of the particles of the atomic atmospheres, whether *rotations* or rectilinear oscillations.

III. *Temperature* is proportional to

$$\frac{(\text{the velocity of the rotations})^2}{\text{the coefficient of elasticity of the atmosphere}} + \text{constant},$$

the constant added being the same for all substances in nature.

IV. *The maximum pressure of vapour in contact with its liquid* is given by the following formula—

$$\log P = \alpha - \frac{\beta}{\tau} - \frac{\gamma}{\tau^2},$$

where P is the pressure, τ the temperature measured from a point 274°·6 Centigrade below the freezing-point of water, and

α, β, γ three constants, to be determined empirically for each fluid.

[The comparison of this formula with the experiments of Regnault and Ure on the vapours of water, alcohol, æther, turpentine, petroleum and mercury, was published in the Edinburgh New Philosophical Journal for July 1849.]

The first section of the paper contains the general theory of the mutual conversion of heat and expansive power in all substances. The most important of the principles laid down is the following, which is identical with that of M. Clausius:—

If unity of weight of any substance pass through a variety of changes of temperature and volume, and at length return to its primitive volume and temperature, the algebraical sum of the vis viva *expended and produced, whether in the shape of expansion and compression, or in that of heat, must be equal to zero.*

The expression obtained for the quantity of heat produced by a given compression, or consumed by a given expansion, consists of three terms.

The first depends on the mere change of volume.

The second depends on certain changes of molecular arrangement connected with change of volume only.

The sum of those two terms is equivalent, in most cases, to what M. Clausius calls "*aüssere Arbeit.*"

The third depends on other changes of molecular arrangement, and corresponds to the "*innere Arbeit*" of M. Clausius.

The second section relates to real and apparent specific heat, especially in perfect gases, that is to say gases which follow the laws of Marriotte and Gay-Lussac. Real specific heat is the increase of the *vis viva* of the atomic atmospheres for a rise of one degree of temperature in unity of weight, and is equivalent, for each substance, to a certain depth of fall. Apparent specific heat is found by adding to the real specific heat that additional heat which is consumed in producing changes of volume and molecular arrangement.

The apparent specific heat of a perfect gas at *constant volume* is sensibly equal to its real specific heat.

The apparent specific heat of a perfect gas under constant pressure exceeds the real specific heat in a certain ratio, $1+N:1$; the fraction N being inversely proportional to the real specific heat of *unity of volume* of the gas, and probably a function of its chemical constitution. The *difference* of those two specific heats, for *unity* of *volume*, is the same for all gases (as M. Clausius also has shown).

The value of $1+N$, as deduced from the velocity of sound, lies between 1·4 and 1·410 for atmospheric air; for oxygen and hydrogen, and probably for all simple gases, it is 1·426.

The mechanical value of *one Centigrade degree* in atmospheric air, as deduced from N, is

$$238{\cdot}66 \text{ English feet} = 72{\cdot}74 \text{ metres.}$$

The mechanical value of one Centigrade degree in liquid water, as determined by Mr. Joule from experiments on friction, being

$$1389{\cdot}6 \text{ English feet} = 423{\cdot}54 \text{ metres,}$$

it follows that the real specific heat of unity of weight of atmospheric air is

$$\frac{238{\cdot}66}{1389{\cdot}6} = 0{\cdot}1717,$$

and the apparent specific heat under constant pressure

$$0{\cdot}1717 \times 1{\cdot}4 = 0{\cdot}2404;$$

according to De la Roche and Berard it is 0·2669.

I was at first disposed to ascribe this difference to some unknown loss of power in Mr. Joule's apparatus; but now that I am better acquainted with his experiments, I am inclined rather to believe that the error lies chiefly in the experiments of De la Roche and Berard.

The apparent specific heat of *vapour maintained at its maximum pressure* is

$$\text{Real specific heat} \times \left\{1 + \mathrm{N}\left(1 - \frac{d \log \mathrm{P}}{d \log \tau}\right)\right\},$$

and is a *negative* quantity, as M. Clausius has concluded.

The third section applies the principles of the first to the latent and total heat of evaporation, and it is shown—

That the total heat of evaporation, where the vapour is sensibly a perfect gas, increases at a sensibly uniform rate with the temperature; and that the coefficient of its increase with temperature is sensibly equal to the apparent specific heat of the vapour as a gas under constant pressure.

The value of this coefficient for steam, as determined by Regnault, is

$$\text{Specific heat of liquid water} \times 0{\cdot}305.$$

This then is also the apparent specific heat of steam as a permanent gas under constant pressure. The real specific heat of steam is

$$= 0{\cdot}194, \text{ and the ratio } 1 + \mathrm{N} = 1{\cdot}57.$$

These values differ slightly from those given in my original paper, being calculated from Joule's equivalent, instead of the experiments of De la Roche and Berard on atmospheric air.

The fourth section applies the principles of the second and third to the theory of the steam-engine, and shows the modifications required by the practical formulæ of Pambour to suit them to the true mechanical theory of heat. It is proved, that, from the nature of the steam-engine, we cannot expect to convert more than about *one-sixth* of the heat expended in evaporation into available power, the remainder escaping into the condenser or the atmosphere. The actual amount so converted is in many ordinary engines less than one-*twenty-fourth* part. The paper concludes with two tables for practical use,—the first for calculating the pressure of steam from the volume, and *vice versâ*; and the second for computing the effect of expansive working in steam-engines*.

Glasgow, September 14, 1850.

XII. *Description and Analysis of Loganite, a new Mineral Species.* *By* T. S. Hunt, *Chemist to the Geological Commission of Canada*†.

THIS mineral occurs at Calumet Island on the Ottawa, in a white crystalline limestone mixed with pale green serpentine, phlogopite, pyrites, and rarely crystals of apatite.

Form very imperfect, but has the appearance of a prism replaced

* A comparison of the results of those formulæ and tables with Mr. Wicksteed's experiments on the Cornish engine at Old Ford is given in the Edinburgh Transactions, vol. xx. part 2, together with a method of determining the proportions of an expansive engine which shall perform a given amount of work at the least possible pecuniary cost.

Subsequently to the publication of the above letter, I became acquainted with the second part of M. Clausius's paper, the object of which is to adapt the principle known as *Carnot's law* to the mechanical theory of heat. That law, as modified by M. Clausius and Prof. W. Thomson of Glasgow, is as follows:—

When a machine converts heat into expansive power by communicating heat to a substance at a higher temperature (τ_1), and abstracting heat from it at a lower (τ_0), the maximum proportion of the heat converted into expansive power to the whole heat received is a function of the two temperatures only, and independent of the nature of the substance.

I have since succeeded in proving, that Carnot's law is not an independent principle, but is deducible from the equations given in my original paper; and that the function of the temperatures of receiving and emitting heat, which expresses the maximum value of the fraction of the whole heat converted into expansive power, is the following:

$$\frac{\tau_1-\tau_0}{\tau_1-\kappa},$$

κ being a constant, which is the same for all substances in nature. (Trans. Roy. Soc. Edin., vol. xx. part 2.)

W. J. M. R.

London, June 7, 1851.

† Communicated by the Author.

on the acute and obtuse lateral edges, also on the acute solid angles. The edges are generally rounded, and the secondary planes not well defined. Cleavage with the sides and base of the prism distinct, with the macrodiagonal imperfect.

Hardness 3. Specific gravity 2·60 to 2·64. Lustre of the cleavages vitreous, shining, the surfaces of the crystals generally dull. Colour clove-brown to chocolate-brown, streak and powder grayish-white, sub-translucent, brittle, fracture uneven. The crystals, which are short and thick, are generally small, and so penetrated with the calcareous gangue, that great care was necessary in selecting specimens for analysis.

The powdered mineral exposed to heat in a tube gives off a large quantity of water with an empyreumatic odour. Before the blowpipe it loses its colour, becoming grayish-white, but does not fuse; moistened with cobalt solution and ignited, it becomes blue. Acids take up magnesia, alumina and peroxide of iron with a small but variable trace of lime, which exists as a carbonate derived from the gangue, and leave pulverulent silica; the decomposition by this means is not, however, complete. Qualitative analysis showed the presence of no other ingredients than those already indicated, with the exception of a feeble trace of manganese. Regard was had in the examination to the detection of the rarer earths, the alkalies, titanic and phosphoric acids.

The finely pulverized mineral was heated to whiteness, and the loss thus sustained regarded as water, with the trace of carbonic acid, which was so small as to be difficult to determine directly upon the portions of the mineral which my specimens afforded me. The further decomposition was effected by fusion with carbonate of soda, and the silica and bases were separated by the usual methods. In analyses upon three different specimens were obtained—

	I.	II.	III.
Silica	32·84	32·14	33·17
Magnesia	35·12	36·43	
Alumina	13·37	13·00	
Peroxide of iron . .	2·00	2·28	
Water and CO^2 . .	17·02	16·83	16·50
Lime	·96	·93	
	101·31	101·61	

If we subtract from the loss by ignition the amount of the carbonic acid required to form a carbonate with the lime, we have respectively 16·36 of water and 1·70 of carbonate, and 16·12 of water and 1·64 of carbonate. Calculating the oxygen ratio between the silica and the bases, we have for the first analysis 17·515 : 34·990, and for the second 17·140 : 35·198. As it appears from the third analysis that the amount of silica in the second is rather too low, we may regard the first as expressing more exactly the ratio, which is just 1 : 2, and which makes it a protosilicate in the nomenclature of M. Gerhardt, pertaining to the type $Si\,O^3(M^4)$*.

The composition is very closely expressed according to the Berzelian notation by $5Si\,O^3$, $12MgO$, $1\frac{3}{4}Al^2\,O^3$, $\frac{1}{4}Fe^2\,O^3$, $12HO$, which may be represented among others by the formula

$$2(Al^2\,O^3\ Fe^2\,O^3),\ Si\,O^3 + 4(3MgO,\ SiO^3) + 12HO.$$

This affords by calculation the following numbers, which are compared with the first of the analyses above given, from which the carbonate of lime has been deducted :—

	Calculated.	Found.
Silica	33·29	32·84
Magnesia	35·50	35·12
Alumina	13·31	13·37
Peroxide of iron . .	1·92	2·00
Water	16·00	16·36
	100·02	99·69

The peroxide of iron is to be regarded as replacing a portion of the alumina, so that the mineral is essentially a hydrated silicate of magnesia and alumina, which, denoting $Al\frac{2}{3}$ by $Al\beta$, will in the notation of Gerhardt be represented by

$$Si\,O^3(Al\beta\tfrac{4}{5}\ Mg\tfrac{8}{5}\ H\tfrac{8}{5}).$$

The only mineral hitherto described which approximates to this in constitution is chlorite, which belongs to the same type, and is represented* by $Si\,O^3(Al\beta\frac{3}{3}\ Mg\frac{5}{3}\ H\frac{4}{3})$; the principal difference in chemical composition being in the proportion of water. The hardness and other physical characters of this mineral are, however, such as to distinguish it completely from chlorite, and seem to warrant its erection into a new species, for which I propose the name of *Loganite*, in honour of the distinguished geologist who is at the head of the Geological Commission of this province.

Montreal, C.E., Feb. 1, 1851.

* *Introduction à l'étude de la Chimie par le Système Unitaire*, par Ch. Gerhardt, p. 349. He takes for the equivalent of silicon 87·5 on the oxygen scale, which will equal 262·5 if we regard silica as $Si\,O^3$. It is this number which I have used in the calculation of the analysis. M. Gerhardt represents silica by SiO, and hence the formula $Si\,O^3(M^4)$=SiO, $2M^2\,O$, corresponds in the Berzelian notation to $Si\,O^3$, 6MO.

XIII. *Notices respecting New Books.*

Six Lectures on Astronomy delivered at the Meetings of the Friends of the Ipswich Museum. By George Biddell Airy, *Astronomer Royal.* London: Simpkin and Marshall. 8vo. pp. 247.

We wish to call attention to this work, on account of its intrinsic merits, and the circumstances under which it was produced. About four years ago, some of the leading inhabitants of Ipswich, feeling a deep interest in the welfare of the working classes, and believing that the cultivation of the intellect would tend greatly to promote their physical and moral improvement, resolved to establish an institution for this especial object. A museum was formed containing books, scientific instruments, specimens of the fine arts, natural history, geology, &c., to which admittance twice a week, free of charge, has been granted to all. From time to time, as opportunity served, intelligible and untechnical lectures in various branches of science have been delivered, which have been very numerously attended. To show the *quality* of the instruction afforded, it is sufficient to say that the Astronomer Royal, Professors Sedgwick, Henslow, Owen, E. Forbes, Ansted, Playfair, &c. have been the lecturers. The museum has been most handsomely supported by the inhabitants of the town and the neighbouring nobility and gentry, and is now very rich in several departments. It is not the least pleasing characteristic of this admirable institution, that it has been founded and supported by a union of persons of different ranks, politics and religion, who seem to have felt no difficulty in discovering a wide neutral space in which all good citizens may agree. Members of the Society of Friends have been most zealous in this good work; indeed it is, we believe, to Mr. George Ransome, one of the honorary secretaries, that a very large portion of the merit is due; but two successive Bishops of Norwich have presided at the harmonious anniversary meetings, and the names we have just cited as coadjutors show that nothing narrow or exclusive has entered into the management. It is equally agreeable to state, that the general good conduct of the persons for whom this institution was mainly formed has fully equalled the expectations of their well-wishers. At Ipswich, as in most places where the experiment has been fairly and judiciously tried, confidence in the people has been met on their side by a strong sense of self-respect and responsibility.

To assist this excellent institution, the Astronomer Royal, who had long been intimately connected with some of its principal promoters, offered to deliver a course of lectures on astronomy, which was gladly accepted. The nature of the lectures was announced in the following words:—

"To point out simple methods of coarsely observing the fundamental phænomena of astronomy—to describe some of the methods of an astronomical observatory—to indicate the degrees and kinds of evidence of the different parts of the received astronomical system—

and to explain the ways of measuring the principal dimensions of the solar and sidereal systems."—P. iv.

The lectures were given on six successive nights, from March 13 to March 18, 1848, at the Temperance Hall, Ipswich, to between 600 and 700 hearers, a large proportion of whom were working men. Shorthand-writers were engaged to take down the lectures; and the fair copy, revised and corrected by the author, forms the present work. As might be expected from Mr. Airy, these lectures are quite original, and very dissimilar from the greater part of treatises professing to be popular. He has carefully avoided the introduction of theorems unknown to the majority of his audience, and those commonplaces which too many persons think proper food for uneducated listeners. Any one who has learned to solve a plane triangle by *construction*, and a spherical triangle by the *globe*, and who is able to give sustained attention to a chain of reasoning, will find no difficulty in comprehending the whole book. But it must not be considered to be light reading. The truths of astronomy, like most things worth knowing, demand considerable mental exertion for their acquisition. Skill in the teacher may make the steps more easy, and to some extent supply the want of preliminary training; but it would be unreasonable to expect that a science, deduced from a few indisputable phænomena by strict mathematical reasoning, can be as easily mastered by a man of plain common sense as by a geometer. The scope and results of astronomy, a good notion of its methods, its difficulties and its triumphs, may be obtained from this book and from another volume* by the same author, even by those who have not had the advantage of a mathematical education: and we are of opinion, that such a course of study would not only convey a large amount of sound information on an interesting subject, but would be singularly well adapted to strengthen and chastise the powers of a student irregularly and imperfectly educated.

It is indeed to persons of this class that the lectures were more particularly addresed. "I wish," says the author, "to invite especially the attention of those who are commonly called working-men, to the few lectures I propose to deliver. The subjects upon which I have to treat are commonly regarded as rather beyond their reach; I take this opportunity of saying that the subjects of the lectures will not be beyond any working-man's comprehension. Everybody who has examined the history of persons concerned in the various branches of science, has been enabled to learn that, whereas on the one hand those who are commonly called philosophers may be as narrow-minded as any other class, and as little informed; so on the other hand, those who have to gain their daily livelihood by handicraft, may associate their trades or businesses, whatever they may be, with accomplishments of the most perfect and the most elevated kind. I think, then, it is right I should repeat, that these lectures will be directed in some measure with the object of being perfectly compre-

* *Gravitation*, an article in the Penny Cyclopædia, also published in a separate volume.

hended by that class of people. It is not my object, however, to deal with what may be called the picturesque in astronomy. I have proposed it to myself as a special object, to show what may be comprehended, by persons possessing common understandings and ordinary education, in the more elevated operations of astronomical science. The lectures will be, therefore, of what I may call a mathematical kind. But in speaking of this, I beg that the ladies present will not be startled. I do not mean to use algebra or any other science, such as must be commonly of an unintelligible character to a mixed meeting. When I use the word *mathematical*, I mean that it will be my object to show how the measure of great things may be referred to the measure of smaller things; or to sum up in few words, it will be my object, in an intelligible way, to show the great leading steps of the process, by which the distance of the sun and the stars is ascertained by a yard measure—the process by which the weight of the sun and the planets is measured by the pound weight avoirdupois. Occasionally I shall be prepared to go into details; but my principal business will be to show the great steps upon which those who wish to study astronomy may enter, and by which they may attain a general comprehension of the rules which will lead them from one step to another."—Pp. 3, 4.

This design has been kept steadily in view throughout the course, but it is not possible to convey any idea of the author's method or success by extracts. We would however call attention to the masterly analysis of one problem of considerable intricacy, viz. the determination of the parallax of the sun, and consequently of his distance, by observations of the transit of Venus. As this is the connecting link between measures upon the earth's surface and the dimensions of our system, great pains have been taken to make the process intelligible. The further step of investigating the parallax, and consequently the distance of the fixed stars (where that is practicable), is also elucidated; and the author, with evident satisfaction, thus sums up the several steps. "By means of a yard measure, a base-line in a survey was measured; from this, by the triangulations and computations of a survey, an arc of meridian on the earth was measured; from this, with proper observations with the zenith sector, the surveys being also repeated on different parts of the earth, the earth's form and dimensions were ascertained; from these, and a previous independent knowledge of the proportions of the distances of the earth and other planets from the sun, with observations of the transit of Venus, the sun's distance is determined; and from this, with observations leading to the parallax of the stars, the distance of the stars is determined. And every step in the process can be distinctly referred to its basis, that is, the yard measure."—P. 191.

In a similar manner, through the Schehallien and Cavendish experiments, the density of the earth is ascertained, *i. e.* that it is between five and six times as heavy as a corresponding bulk of water; and as its dimensions are known, the weight of the earth in pounds avoirdupois can be readily assigned. Having the dimensions of the moon's orbit, the space through which the earth draws the moon in a certain time is easily calculated; and again, from the dimensions of the earth's orbit, the space through which the sun draws the earth in the same time is also found. These data and the well-known law of gravitation, that the attractive power varies as the mass of the attracting body divided by the square of the distance, assigns the proportion of the sun's weight to the earth's weight. A similar method applies to all the planets which have satellites; and those which have none are determined, though more imperfectly, by the effects they produce in disturbing other bodies, planets or comets. The satellites of Jupiter are weighed by their mutual disturbances. The mass or weight of the moon is approximated to by several independent methods which agree well together. The author concludes in the following words:—"I shall now repeat what I said in commencing this course of lectures, that I fully believe there is no part whatever of these subjects of which the *principle* cannot be well understood, by persons of fair intelligence, giving reasonable attention to them; but more especially by persons whose usual occupations lead them to consider measures and forces; not without the exercise of thought, but by the application only of so much thought as is necessary for the understanding of practical problems of measures and forces."—P. 247.

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

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

April 10, 1851.—Sir Philip de Malpas Grey Egerton, Bart., V.P., in the Chair.

THE following communication was read:—Extract of a letter from Professor Kämtz to Lieut. Colonel Sabine, on "Corrections of the Constants in the general theory of Terrestrial Magnetism." Received April 3, 1851.

Translation.

Dorpat, $\frac{4}{16}$ January 1851.

From the active zeal with which you pursue the phenomena of terrestrial magnetism, and collect all the facts which can conduce to the elucidation of this difficult subject, I think that some researches with which I have occupied myself will not be wholly uninteresting to you; and I therefore address you the following lines, which I have also permitted myself to write in my own language.

Some years ago I employed myself in endeavouring to correct the constants which Gauss has given for the earth's magnetism. The process I adopted was by considering the horizontal and vertical components separately; but when I learned that Erman had the same work in hand, I left mine unfinished. I did not then possess the Reports of the British Association, as it was not until this last summer (1850) that they were obtained here, and when I had seen

Erman's results, I at once decided on taking up my work afresh. I have made use of all the data I could procure, and have thus been able to determine the component Z at above 1400 places, including a series of observations which I had myself made from 1847 to 1849 in Liefland, Esthonia, Finland, Norway, and on the route from Archangel to Petersburg. I have as far as possible reduced all determinations to the epoch of 1830. A calculation of the several observations by the method of least squares would have required an entire life; I therefore preferred following the same path as Gauss; in doing this, however, I soon discovered that the 5th order could not be neglected; and I then obtained the following values:—

$g^{1,0} = 927{\cdot}9$	$g^{1,1} = 89{\cdot}8$	$h^{1,1} = -163{\cdot}7$	$g^{2,2} = 2{\cdot}5$	$h^{2,2} = -37{\cdot}3$
$g^{2,0} = -6{\cdot}4$	$g^{2,1} = -140{\cdot}6$	$h^{2,1} = -14{\cdot}1$	$g^{3,2} = -86{\cdot}9$	$h^{3,2} = -17{\cdot}2$
$g^{3,0} = -51{\cdot}8$	$g^{3,1} = 112{\cdot}3$	$h^{3,1} = 48{\cdot}5$	$g^{4,2} = -41{\cdot}3$	$h^{4,2} = 43{\cdot}4$
$g^{4,0} = -83{\cdot}2$	$g^{4,1} = -103{\cdot}2$	$h^{4,1} = -18{\cdot}2$	$g^{5,2} = -96{\cdot}5$	$h^{5,2} = -10{\cdot}0$
$g^{5,0} = 14{\cdot}3$	$g^{5,1} = -115{\cdot}1$	$h^{5,1} = 72{\cdot}8$		

$g^{3,3} = -4{\cdot}4$	$h^{3,3} = -25{\cdot}1$	$g^{4,4} = 3{\cdot}9$	$h^{4,4} = 4{\cdot}3$
$g^{4,3} = 18{\cdot}8$	$h^{4,3} = 18{\cdot}6$	$g^{5,4} = {\cdot}0$	$h^{5,4} = 2{\cdot}8$
$g^{5,3} = 3{\cdot}3$	$h^{5,3} = -1{\cdot}6$		
		$g^{5,5} = {\cdot}0$	$h^{5,5} = {\cdot}0$

A comparison will show you that these quantities agree much better with Gauss's than Erman's do; and this is also true in respect to the agreement with the observations, especially in the high south latitudes. Thus there was found—

	Latitude.	Longitude.	Inclination.	Force.
	° ′	° ′	° ′	
	−69 54	179 55	−84 30	1999
	−69 52	180 04	−83 34	1994
Means	−69 53	180 0	−84 02	1996·5

Z=−1985·8 for −70° and 180°; Gauss found −2193·5; Erman −1781·1; my calculation gives −2009·3. My constants also still require a small correction. I do not however mean to examine this at present, but propose first to consider the horizontal component, in order to satisfy myself previously whether both components depend upon the same constants or not. The probable error of a single determination is nearly 19; and to show the degree of agreement, I subjoin the following table. As in forming it I merely took from my large table every 10th observation in the order of succession, you will not be surprised at finding unimportant places, whilst others of greater note in their vicinity are omitted: it may suffice however for the present purpose. The quantities given are the differences between the observed and calculated vertical intensity.

Stations.	Lat. N.	Long. E.	Δ Z.	Observers.
	° ′	° ′		
1. Fairhaven, Spitzbergen	79 40	11 40	− 24·1	Sabine.
11. Tromsöe	69 39	18 55	+ 31·2	Keilhau.
21. Tukansk. Isl.	68 4	39 35	− 39·1	Reinicke.
31. Grundsät	60 56	11 35	+ 39·8	Hansteen.
41. Sundsvall	62 22	17 16	+ 2·5	Hansteen.
51. Abo	60 27	22 18	− 15·7	Hansteen.
61. Danzig	54 21	18 38	+ 31·0	Ericksen.
71. Doskino	56 9	43 34	− 32·8	Erman and Hansteen.
81. Perm	58 1	56 14	− 30·0	Erman and Hansteen.
91. Tiumen	57 10	65 27	− 13·4	Erman and Hansteen.
101. Wandiask	66 16	65 10	− 27·7	Erman.
111. Tschuluim	55 6	81 14	− 24·6	Erman.
121. Botowsk	55 10	105 22	− 3·7	Erman.
131. Monachanowa	50 58	106 29	+ 6·8	Hansteen.
141. Nowaja River	72 7	95 25	+ 22·3	Middendorf.
151. Progromnoi	52 30	111 3	+ 11·8	Fuss.
161. Nalaicha	47 47	107 18	+ 9·8	Fuss.
171. Chapchaktu	46 2	108 35	− 13·0	Fuss.
181. Zackildack	42 48	114 17	+ 13·5	Fuss.
191. Gaschun	44 23	111 19	− 9·8	Fuss.
201. Arki	60 6	142 20	+ 1·2	Erman.
211. Sitka	57 3	224 34	− 2·2	Lütke, Erman, Belcher.
221. F. Dunvegan	55 56	241 26	+ 22·7	Lefroy.
231. Frog Portage	55 28	256 30	+ 9·1	Lefroy.
241. York Factory	57 0	267 34	+ 4·9	Lefroy.
251. Fort Alexander	50 37	263 39	+ 13·4	Lefroy.
261. Devil's Drum Island	53 19	259 20	− 22·7	Lefroy.
271. Cape Disappointment	46 16	236 4	− 34·2	Douglas.
281. Lac à la Pluie	48 32	267 4	+ 19·9	Lefroy.
291. Fort à la Cloche	46 7	277 35	− 19·7	Lefroy.
301. Portage Ecarté	48 25	270 15	+ 4·8	Lefroy.
311. Chat Falls	45 26	283 28	+ 22·8	Lefroy.
321. Pointe aux Chênes	45 37	285 5	+ 7·7	Lefroy.
331. Lake Nipissing	46 13	280 1	+ 16·7	Lefroy.
341. Waterville	44 33	293 23	+ 11·9	Keely.
351. Dubuque's Town	42 29	269 37	+ 20·8	Locke.
361. St. Mary's	40 32	275 41	+ 31·9	Locke.
371. Detroit	42 25	277 0	+ 24·0	Loomis, Younghusband, Locke, Lefroy.
381. Alleghany Summit	40 27	281 50	+ 29·3	Locke.
391. Utica	43 7	284 47	+ 27·2	Loomis, Locke.
401. Portland	43 41	289 40	+ 14·5	Locke.
411. St. Louis	38 38	269 56	+ 35·8	Locke, Loomis, Nicollet.
421. Paoli	38 5	273 35	+ 30·7	Locke.
431. Columbus	39 57	276 57	+ 11·9	Locke.
441. Lerwick	60 9	358 53	+ 46·8	Ross.
451. Loch Slapin	57 14	353 58	+ 30·4	Sabine.
461. Braemar	57 1	356 35	+ 16·2	Sabine.
Edinburgh	....	....	+ 8·8	
471. Valencia	51 56	349 43	+ 8·5	Sabine, Ross.
481. Enniskillen	54 21	352 22	+ 25·6	Lloyd.
491. York	53 58	358 54	+ 22·5	Phillips, Ross.
501. Calderstone	53 23	357 7	+ 17·7	Phillips.
511. Castleton	54 4	355 20	+ 22·7	Phillips.
Dublin	....	....	+ 15·2	All Observers.
521. Fermoy	52 7	351 44	+ 13·5	Sabine.
531. Clifton	51 27	357 25	+ 16·4	Lloyd, Ross.

Stations.	Lat. N. ° ′	Long. E. ° ′	Δ Z.	Observers.
541. London	51 31	359 53	+ 21·9	All Observers.
551. Salisbury	51 4	358 12	+ 18·3	Lloyd, Ross.
561. Dover	51 8	1 19	+ 19·7	Sabine.
571. Fontainebleau	48 24	2 38	+ 11·0	Fox.
581. Nimes	43 50	4 20	− 3·8	Fox, Humboldt.
591. Malaga	36 44	355 36	− 13·4	Norwegian Officers.
601. Prague	50 5	14 27	+ 15·5	Keilhau, Kreil.
611. Berne	46 57	7 25	+ 12·0	Fox.
621. Seelau	49 32	15 17	+ 21·6	Kreil.
631. Rome	41 54	12 26	+ 5·0	Humboldt, d'Abadie, Quetelet.
641. Milo	36 43	24 27	+ 19·6	Norwegian Officers.
651. San Diego	32 41	242 27	− 70·6	Belcher.
661. At sea	47 7	346 54	− 0·4	Sulivan.
671. At sea	44 22	330 54	− 18·3	Erman.
681. At sea	30 0	318 5	− 4·3	Sulivan.
Teneriffe	28 27	343 43	− 0·4	Humboldt, Freycinet, Duperrey, Sabine, Bethune, Wickham, Sulivan.
691. At sea	21 32	316 43	+ 21·3	Sulivan.
701. At sea	23 12	238 9	− 61·2	Erman.
711. Socorro Island	18 43	249 6	+ 28·0	Belcher.
721. Ulean	7 22	143 57	− 10·8	Lütke.
731. At sea	8 55	235 48	− 27·8	Erman.
741. La Guayra	10 36	292 54	+ 2·6	Humboldt.
751. Morales	8 15	286 0	− 7·0	Humboldt.
761. At sea	10 7	319 51	+ 48·9	Sulivan.
771. St. Thomas, Fernando Po, Isla das Rolhas	1 23	7 20	− 35·8	The secular change at this station is uncertain; I take the mean of the inclinations by Sabine and Allen; the force at St. Thomas, from Sabine.
781. At sea	3 47	162 59	+ 8·2	Lütke.
791. At sea	− 2 2	236 4	+ 31·7	Lütke.
801. Pasto	1 13	282 39	− 13·0	Humboldt and Bousingault.
811. At sea	5 45	331 9	+ 8·4	Erman.
821. At sea	5 37	341 3	+ 10·8	Dunlop.
831. Pulo Kumpal	− 2 44	110 7	− 16·8	Belcher.
841. Shell Rock	− 1 57	136 21	+ 1·4	Belcher.
851. Gonzanama	− 4 13	280 27	− 4·5	Humboldt.
861. At sea	− 1 10	223 32	− 19·9	Erman.
871. Tomependa	− 5 31	281 24	+ 10·1	Humboldt.
881. Huaura	− 11 3	282 14	+ 12·9	Humboldt.
891. At sea	− 0 27	324 44	+ 52·2	Sulivan.
901. At sea	− 8 10	339 50	− 21·1	Dunlop.
911. At sea	− 11 54	214 37	− 9·4	Erman.
921. At sea	− 13 9	251 20	+ 57·0	Lütke.
931. Bow Island	− 18 5	219 7	+ 7·5	Belcher.
941. At sea	− 19 56	325 5	− 20·7	Sulivan.
St. Helena	− 15 55	354 17	+ 14·7	All Observers.
951. At sea	− 26 25	49 12	− 17·1	Moore and Clerk.
961. At sea	− 21 54	53 0	− 20·0	Moore and Clerk.
Mauritius	− 20 9	57 31	− 0·5	Duperrey, Fitzroy, Moore and Clerk.
971. At sea	− 22 41	69 54	− 26·7	Dayman.
981. At sea	− 22 38	76 10	− 17·4	Dayman.
991. At sea	− 22 34	80 10	− 22·1	Dayman.

Stations.	Lat. S.	Long. E.	Δ Z.	Observers.
	° ′	° ′		
1001. At sea	24 17	94 6	− 7·5	Moore and Clerk.
1011. At sea	21 51	268 5	+ 19·0	Lütke.
1021. At sea	29 53	313 43	− 19·3	Erman.
1031. At sea	38 44	0 16	− 7·7	Dunlop.
1041. At sea	35 48	18 47	− 25·8	Erebus and Terror.
1051. At sea	32 17	29 34	+ 0·6	Dayman.
1061. At sea	38 11	22 0	− 29·9	Erebus.
1071. At sea	39 16	30 27	− 10·7	Dunlop.
1081. At sea	33 47	111 4	+ 31·3	Dayman.
1091. At sea	35 5	117 56	− 7·1	Moore and Clerk.
1101. At sea	42 35	125 40	+ 51·3	Smith.
Sydney	....	....	+ 39·0	All Observers.
			+ 17·7	(British only).
1111. At sea	33 38	163 42	− 33·2	Erebus.
1121. Bay of Islands	35 16	174 0	− 8·4	Duperrey, FitzRoy, Erebus.
1131. Valdivia	39 53	286 31	+ 41·3	FitzRoy.
1141. At sea	44 4	312 1	+ 9·1	Sulivan.
1151. At sea	37 37	353 36	− 36·3	Dunlop.
1161. At sea	41 47	26 38	− 16·7	Erebus.
1171. At sea	46 28	52 31	− 4·0	Erebus. Maximum of probable error. There are great anomalies in this meridian.
1181. At sea	48 40	68 58	− 55·5	Erebus.
Kerguelen Island	48 41	68 54	− 11·9	Erebus.
1191. At sea	47 39	103 42	− 23·4	Erebus.
1201. At sea	47 34	124 43	− 109·6	Erebus.
Hobart Town	42 53	147 24	+ 41·8	All Observers.
1211. Bass's Strait	40 28	151 35	+ 11·5	Wickham.
1221. At sea	41 49	183 41	− 18·3	Erebus.
1231. At sea	49 23	188 29	·0	Erebus.
1241. At sea	53 57	6 5	− 4·1	Moore and Clerk.
1251. At sea	54 55	132 50	− 53·8	Erebus.
1261. At sea	57 54	170 25	+ 40·4	Erebus.
1271. At sea	53 1	205 8	− 18·3	Erebus.
1281. At sea	58 39	213 17	− 6·7	Erebus.
1291. At sea	60 21	237 54	− 19·0	Erebus.
1301. At sea	58 25	279 44	+ 10·8	Erebus.
Port Famine	53 38	289 2	+ 4·6	King and FitzRoy.
1311. At sea	46 0	299 50	− 49·2	Sulivan.
Falkland Islands	51 33	301 55	+ 41·3	All Observers.
1321. At sea	61 10	9 5	+ 15·9	Moore and Clerk.
1331. At sea	66 33	36 48	+ 17·5	Moore and Clerk.
1341. At sea	66 24	40 30	+ 8·1	Moore and Clerk.
1351. At sea	60 50	87 41	+ 10·1	Moore and Clerk.
1361. At sea	65 9	143 7	− 52·5	Erebus.
1371. At sea	64 41	162 34	− 10·5	Erebus.
1381. At sea	61 34	170 40	+ 26·7	Erebus.
1391. At sea	67 14	188 6	+ 18·4	Erebus.
1401. At sea	65 18	191 39	+ 28·5	Erebus.
1410. At sea	67 16	202 13	+ 30·4	Erebus.
1411. At sea	61 15	213 54	+ 22·2	Erebus.
1412. At sea	62 38	212 44	+ 3·4	Erebus.
1421. At sea	70 23	174 50	− 10·2	Erebus.
1431. At sea	72 58	189 50	+ 21·0	Erebus.
1441. At sea	77 6	192 31	− 8·0	Erebus.
1444. At sea	77 47	197 25	+ 23·2	Erebus.

I think the agreement pretty good for a calculation which I still expect to correct in some degree; it is also to be remarked that I have taken the results of all observers, and that their determinations often differ considerably from each other at the same place. Unfortunately I could not make use of the two important determinations of the Euphrates Expedition for want of the Inclination.

As you collect everything that can serve towards a final determination of the elements, I permit myself to subjoin the following data which are still partly unpublished.

Stations.	Lat. N.	Long. E.	Date.	Inclination. Observed.	Inclination. Reduced to 1830.	Horizontal Force.	Total Force.	Vertical Force. Observed.	Vertical Force. Calculated.	Vertical Force. Difference.
Uellenorm	58° 19′	26° 43′	1847.	70° 9′·8	70° 38′·0	473·7	1396·0	1317·0	1317·6	− 0·6
Dorpat*	58 23	26 44	1847 to 1850.	70 50·7	71 19·9	465·4	1421·9	1347·1	1318·0	+29·1
Kardis	58 51	26 17	1847.	70 17·1	....	471·6				
			1849.	17·5	....	467·1				
				70 17·3	70 48·3	469·3	1388·4	1311·2	1323·2	−12·0
Revel	59 35	24 43	1847.	70 54·4						
			1849.	45·8						
				70 50·1	71 21·1	454·4	1384·2	1311·5	1330·7	−19·2
Nawast	58 35	25 34	1848.	70 41·0	71 12·0	454·7	1374·5	1301·2	1318·6	−17·4
Werder†	58 35	23 40	1848.	69 31·6	70 2·6	484·6	1385·4	1302·2	1315·9	−13·7
Arensburg	58 15	22 25	1848.	70 51·1	71 22·1	455·5	1388·8	1316·0	1309·1	+ 6·9
Kabbil	58 20	22 40	1848.	71 9·3	71 40·3	437·6	1354·8	1286·1	1310·4	−24·3
Pernaw	58 22	24 32	1848.	70 36·3	71 7·3	458·4	1380·1	1305·9	1313·9	− 8·0
Tammiss	58 21	24 33	1848.	70 24·5	70 55·5	459·0	1368·8	1293·6	1313·7	−20·1
Kurkundt	58 8	24 59	1848.	69 47·9	70 18·9	476·2	1378·7	1298·0	1311·8	−13·8
Helsingfors	60 10	24 57	1847.	71 21·7	....	444·3				
			1849.	19·7	....	7·6				
				71 20·7	71 51·7	446·0	1394·3	1325·0	1339·0	−14·0
Bollstad	60 9	24 13	1847.	71 30·2	71 59·4	441·4	1391·4	1323·2	1338·0	−14·8
Kyrkstad	60 10	24 5	1847.	71 21·9	71 51·1	442·6	1385·1	1316·2	1338·0	−21·8
Lambola	60 15	23 10	1847.	71 28·9	71 58·1	442·7	1393·8	1325·4	1335·8	−10·4
Nukari	60 22	24 55	1847.	71 40·3	72 9·5	440·8	1401·8	1334·3	1341·2	− 6·9
Abborfors	60 30	26 30	1847.	71 19·8	71 49·0	450·0	1406·2	1335·9	1346·0	−10·1
Grönwick.........	60 33	27 30	1847.	71 32·3	72 1·5	441·1	1393·1	1325·0	1348·5	−23·5
Wiborg	60 44	28 50	1847.	70 51·6	71 20·8	446·2	1360·9	1289·4	1353·0	−63·6
Turkhauta	60 50	24 47	1847.	72 14·6	72 43·8	425·5	1395·1	1332·2	1346·5	−14·3
Tavastehus	61 0	24 28	1847.	72 8·4	72 37·6	427·6	1394·7	1331·1	1348·0	−16·9
Wilmanstrand ...	61 4	28 16	1847.	71 51·8	72 21·0	439·0	1410·2	1344·0	1356·1	−12·1
Imatra Fall	61 11	28 55	1847.	71 51·0	72 20·2	433·6	1411·3	1344·8	1357·6	−12·8
Huutjarwi	61 28	24 2	1847.	72 2·3	72 31·5	433·5	1405·8	1340·9	1352·9	−12·0
Pumala...........	61 32	28 15	1847.	72 7·9	72 37·1	431·7	1406·9	1342·6	1361·7	−19·1
Wehuwarpe... ..	61 46	22 49	1847.	72 6·4	72 35·6	433·0	1409·3	1344·9	1353·8	− 8·9
Nyslott...........	61 52	29 0	1847.	71 59·9	72 29·1	437·2	1414·6	1349·1	1367·3	−18·2
Tjök‡	62 18	21 23	1847.	72 43·0	73 12·2	419·4	1411·7	1351·3	1357·9	− 6·6
Warkauss-Sluss..	62 20	27 58	1847.	72 32·4	73 1·6	420·1	1400·1	1339·1	1371·0	−31·9
Johannisdal	62 21	21 21	1847.	73 27·3	73 56·5	399·4	1402·6	1347·9	1358·6	−10·7
Kuopio	62 55	27 33	1847.	72 54·3	73 23·5	415·5	1413·6	1354·6	1377·2	−22·6
Wasa §	63 5	21 35	1847.	73 0·8	73 30·0	411·7	1442·0	1351·0	1367·6	−16·6

* In the garden near my house, and at different parts of the town and its environs; including differences of inclination of more than 1° 15′.

† H. F. very anomalous. ‡ Hansteen, 1825, $\Delta Z = -12{\cdot}4$.

§ Hansteen, 1825, $\Delta Z = -13{\cdot}3$.

Stations.	Lat. N.	Long. E.	Date.	Inclination. Observed.	Inclination. Reduced to 1830.	Horizontal Force.	Total Force.	Vertical Force. Observed.	Vertical Force. Calculated.	Vertical Force. Difference.
	° ′	° ′		° ′	° ′					
Sawojarwi	63 22	27 13	1847.	72 53·1	73 22·3	434·6	1511·1	1415·0	1383·0	+32·0
Sundby	63 36	22 40	1847.	73 18·9	73 48·1	401·8	1399·4	1343·9	1375·0	−31·1
Aho	64 2	26 27	1847.	73 24·9	73 54·1	407·4	1427·4	1371·5	1388·0	−16·5
Wirda	63 37	27 3	1847.	73 9·4	73 38·6	411·5	1420·1	1362·6	1384·6	−22·0
Salahmi	63 47	27 0	1847.	73 14·2	73 43·4	408·8	1417·2	1360·6	1386·6	−26·0
Kyrola	64 5	23 30	1847.	73 24·8	73 54·0	409·0	1432·7	1376·5	1382·6	− 6·1
Tuomala	64 25	26 0	1847.	73 30·7	73 59·9	410·7	1446·7	1390·7	1389·8	+ 0·9
Lassila	64 45	24 38	1847.	73 50·5	74 19·7	408·2	1466·9	1412·4	1393·0	+19·4
Uleaborg*	65 3	25 27	1847.	74 6·0	74 35·2	393·2	1435·2	1383·6	1398·6	−15·0
Wuornos	65 36	25 26	1847.	74 4·8	74 30·0	393·5	1434·7	1382·4	1405·3	−22·9
Rautiola	65 47	24 40	1847.	74 49·9	75 19·1	377·5	1437·9	1390·9	1405·2	−14·3
Tornea	65 52	23 30	1847.	74 52·3		380·2				
			1849.	48·4		2·6				
				74 50·3	75 19·5	381·4	1458·4	1410·9	1404·1	+ 6·8
Haaparanda† ...	65 52	23 30	1849.	74 28·1	74 57·3	382·1	1427·0	1378·1	1404·1	−26·0
Alkula ‡	66 20	23 49	1847.	74 21·1		392·2				
			1849.	15·2		3·8				
				74 18·2	74 47·4	393·0	1452·8	1401·9	1410·0	− 8·1
Toluanen ‡	66 36	23 52	1847.	74 31·9	75 1·1	386·9	1450·3	1401·1	1413·3	−12·2
Turtola‡	66 42	23 40	1847.	74 47·7	75 16·9	381·3	1453·9	1406·1	1414·5	− 8·4
Kardis Lappl.‡...	67 0	23 39	1847.	75 4·4	75 33·6	374·3	1452·9	1407·1	1418·0	−10·9
Kexiswaara ‡ ...	67 15	23 27	1847	75 45·2	76 14·4	366·1	1487·8	1445·1	1420·6	+24·5
Muonionisca ‡ ...	68 0	23 42	1847.	75 32·0		364·7				
			1849.	31·0		5·6				
				75 31·5	75 59·7	365·2	1459·1	1415·7	1430·2	−14·5
Kätkesuando ‡...	68 7	23 22	1849.	75 32·1	76 1·3	359·8	1440·6	1397·9	1430·7	−32·8
Palajoensu	68 18	22 45	1849.	76 5·7	76 34·9	350·0	1456·7	1416·9	1432·5	−16·6
Kaaressuando ...	68 24	22 8	1849.	75 37·1	76 6·3	359·3	1446·6	1404·3	1433·7	−29·4
Kielli-jarwi	69 5	20 40	1849.	75 52·4	76 21·6	355·1	1455·0	1414·0	1439·0	−25·0
Tromsöe§.........	69 39	18 56	1849.	76 11·4	76 40·6	348·1	1458·4	1419·2	1444·4	−25·2
Hammerfest ‖ ...	70 40	23 45	1849.	76 43·8	77 13·0	344·3	1500·2	1463·0	1464·0	− 1·0
Havösund¶	71 0	24 45	1849.	76 46·1	77 15·3	336·7	1471·0	1434·8	1466·1	−31·3
Kielwig Mageroe	70 57	26 15	1849.	76 54·6	77 23·8	333·7	1473·5	1438·0	1467·5	−29·5
Kitai-Insel** ...	68 28	38 30	1849.	75 50·6	76 9·6	358·2	1464·6	1422·0	1476·7	−54·7
Archangel†† ...	64 30	40 33	1849.	73 58·8	74 8·3	405·4	1468·9	1413·0	1439·1	−26·1
Bobrowsk	64 28	41 0	1849.	74 1·5	74 11·0	404·5	1469·6	1414·0	1440·8	−26·8
Kaduish	62 55	41 30	1849.	73 19·6	73 29·1	420·2	1464·5	1404·1	1422·8	−18·7
Plesskaja	62 35	40 55	1849.	72 46·7	72 57·2	429·8	1451·7	1387·9	1408·3	−20·4
Krassnowskaja...	62 10	40 10	1849.	72 33·5	72 43·0	432·8	1443·8	1378·7	1407·3	−28·6
Ustwelskoi	61 55	39 12	1849.	72 15·3	72 25·3	442·1	1450·6	1382·8	1400·4	−17·6
Kargopol	61 43	38 57	1849.	72 8·2	72 19·2	444·4	1448·6	1380·2	1395·7	−15·5
Badoshkaja	60 48	37 30	1849.	71 25·3	71 28·6	459·0	1440·8	1366·1	1381·1	−15·0
Wytegra	61 1	36 28	1849.	71 34·2	71 48·2	457·1	1445·9	1373·6	1380·5	− 6·9
Gomorowitschi...	60 55	34 35	1849.	71 34·4	71 53·0	450·6	1425·5	1354·8	1371·0	−16·2
Petersburg ‡‡ ...	59 56	30 18	1849.	70 33·2	70 59·0	473·1	1420·8	1343·2	1347·0	− 3·8

* Hansteen 1825, Δ Z = −12·0. † Hansteen 1825, Δ Z = −12·1.

‡ Hansteen 1825, Δ Z + 0·1; many iron mines in the vicinity; quantities of magnetic ironsand on the banks of Tornea river. § Keilhau, ΔV + 31·2.

‖ Sabine, +2·8; Keilhau, −30·9. ¶ Keilhau, −27·4. ** Keilhau, −3·2.

†† Reinicke and Mailander, −62·5.

‡‡ Inclination observed by me; force by Kupffer; earlier observations gave Δ Z = −9·3.

In the above table, the horizontal force was obtained by vibrations, and reduced to 0° Reaumur. Before and after my journey in 1847, the force was determined at Dorpat by Gauss's method, and the needle employed compared therewith and reduced to the intensity in London=1372. Subsequently I preferred for trying the needles, Poisson's method, at least for traveling purposes; but some alterations require to be introduced in Poisson's formula, as he has overlooked some things. With the same needle which I employed in both my journeys, I have made more than 60 determinations of absolute force at Dorpat, partly in a room and partly in the open air, and in temperatures varying from −13° R. to + 25° R., and have found a very good accordance. I also made several such determinations in the journeys of 1848 and 1849.

As I do not possess an observatory, and cannot employ a Bifilar in my dwelling-house, it has not been possible for me to compare the variations of the force with my determinations; I have however made use of the following method:— If X be the magnetism of the earth and *m* that of the needle, I seek not X but *m*; this latter quantity depends on the temperature *t* and the time T, as the needle is not constant; but if I combine all the values of *m* by an equation of the form

$$m = \mathrm{A} + \mathrm{B}\, e^{-a\mathrm{T}} + e\,.\,t$$

and calculate the constants, the error is about $\frac{1}{600}m$. Besides this, several simultaneous observations with Gauss's apparatus have shown that the value of *m* was itself correct.

The Inclinations have in part been determined by two needles which agreed very well with each other; they are so balanced that I can always take the mean of the eight arcs. On the other hand they are subject to the error of the axle, which I cannot exactly correct, but which does not however exceed 5′. It was only last summer, when I examined the subject more closely, that I became aware you had likewise the idea of loading the needle, and observing in different azimuths. In our latitudes the best loading is such as will cause the north pole to be in one set about 10° above, and in a second set 10° below the horizontal line. Three series which I made with one needle were calculated by my friend Claussen, who in doing so was led to a method of entirely eliminating the form of the axle. Take a well-balanced needle, the axles of which are not cylindrical; different degrees of magnetic force can be given to it without reversing the poles. Taking the strongest force as unity, it is not practically advantageous to go to lower ratios than $\frac{1}{4}$ or $\frac{1}{5}$. Though vibration experiments with dipping-needles are not generally advantageous, yet they suffice in this case, as an approximately correct proportion of the intensities is all that is wanted. It is sufficient to make, with each degree of intensity, the two observations with the face east and face west, without reversing the needle on its supports; if the latter is done, it gives a second determination, affording a check upon the first. You will then find that the mean of the two observations in one position of the axles is less than the true inclination, and in the other position greater; the difference in both cases being more considerable as the intensity of the needle is weaker. Let I_0, I_1, I_2, &c. be the inclination observed with different intensities; T_0, T_1, T_2, &c. be the times of vibration, which increase as the index increases; a small correction is required, which can be determined in the following manner.—Take either I_0 or a somewhat less value (in round minutes) as being nearly correct, and let

$$I_0 - I_1 = \Delta I_1\,;\ I_0 - I_2 = \Delta I_2,\ \&c.,$$

then $$\Delta I = x + T^2 y\,;$$

x being the correction; thus I found

Az. 0°; I=70° 23′·8. Az. 180°; I=71° 26′·5. Mean 70° 55′·1. T=1s·167.
Az. 0; I=70 48·7. Az. 180; I=71 44·7. Mean 71 15·2. T=1·738.
Az. 0; I=66 16·0. Az. 180; I=84 16·5. Mean 75 36·3. T=4·25.

If I take 70° 55′·0 as nearly correct, I obtain the three following equations;

$$0'{\cdot}1 = x + (1{\cdot}167)^2 y\ ;\ 20'{\cdot}2 = x + (1{\cdot}738)^2 y\ ;\ 281{\cdot}3 = x + (4{\cdot}25)^2 y.$$

The three equations have not however the same weight, as the directive force is less in proportion as T is larger; in order to give them all the same weight I divide each by the coefficient of y, and thus obtain in logarithms

$$8{\cdot}86586 = 9{\cdot}86586\,x + y\ ;\ 0{\cdot}82525 = 9{\cdot}51990\,x + y\ ;$$
$$1{\cdot}19239 = 8{\cdot}74322\,x + y.$$

and hence $x = 21'{\cdot}8$; and the true dip=70° 33′·2.

I have here taken an imperfect needle, which I also observed in Azimuths of 30° to 30°; in one position of the axles I obtained 70° 39′·5; ±5′·9; and in a second 70° 42′·5; ±5′·4; mean 70° 41′·0. On a subsequent day I observed with a second needle and obtained 70° 43′·4; but an independent needle gave a dip 2′·6 greater, so that the two determinations are 70° 42′·1, 70° 42′·3, if we add to each the half difference.

In this method, in which no reversal is needed, the differences of the partial determinations will appear somewhat large, but you must not forget that instead of the ordinary eight observations only two have been taken.

I permit myself one additional remark. In observations on different azimuths, it is usual to take simply cot I.=cot I_1 cos α; in latitudes where the dips are so high as here and in England, this equation may be employed without much error, as the force in azimuths perpendicular to the meridian is little less than in the meridian; but it is quite otherwise in small dips. With the decrease of force the possibility of error increases, and hence when the observations made in different azimuths are combined as by Kupffer, they have not the same weight. In more exact determinations I employ the following method.

Let K be the total, H the horizontal, V the vertical force, and α the nearly known azimuth; then

$$K \cos I = H \cos \alpha\,;\ K \sin I = V\,;\ \tan I = \frac{V}{H} \cdot \frac{1}{\cos \alpha}\,;$$

whence $$d\,\mathrm{I}=\frac{\cos^2\mathrm{I}}{\cos a}\,d\left(\frac{\mathrm{V}}{\mathrm{H}}\right)+\frac{\mathrm{HV}}{\mathrm{K}^2}\sin a\,.\,d\,a.$$

If on the right we substitute for cos ²I its value, then

$$d\,\mathrm{I}=\frac{\mathrm{H}^2\cos a}{\mathrm{K}^2}\,d\left(\frac{\mathrm{V}}{\mathrm{H}}\right)+\frac{\mathrm{HV}}{\mathrm{K}^2}\sin a\,d\,a.$$

As the possibility of error is inversely as the force, I multiply the equation by K, to give to the different determinations equal weight, thus

$$\mathrm{K}\,d\,\mathrm{I}=\frac{\mathrm{H}^2\cos a}{\mathrm{K}}\,d\left(\frac{\mathrm{V}}{\mathrm{H}}\right)+\frac{\mathrm{HV}}{\mathrm{K}}\sin a\,d\,a:$$

having determined the dips in the customary manner with the approximately known values of a, I obtain the values d I, which serve to find $d\left(\frac{\mathrm{V}}{\mathrm{H}}\right)$; *i. e.* the correction of I. I possess now with my instrument six needles, which I hope to compare very accurately with each other in the course of this year; but some months must first elapse, as I make all these determinations in the open air, and the bad autumn we have had has interrupted me in the work. I have had two of my needles fitted according to Fox's method, with wheels on their axles; two others have brass indexes, as was formerly proposed by Bernoulli and Euler (Berlin Trans. 1755), and I can now determine the absolute intensity with the inclinatorium. I know Fox's method only from a short notice in the London and Edinburgh Phil. Mag.; if I do not mistake, he proposed also to determine the declination by the same apparatus. With ordinary needles there remains an uncertainty. If we load the S. end of the needle so that the N. end is about 10° above the horizon, the S. end sinks down; and if we seek the azimuth in which the needle is perpendicular and then observe at about half a degree of azimuth on either side, the inclination alters so rapidly with the azimuth, that I have thus been even able to follow the diurnal variations of the declination; and the magnetic meridian may thus be determined for the observations of absolute declination whilst travelling.

I will not trouble you further as my letter is already so long, and will only add one request. The Phil. Trans. arrive here rather late, and the last communications which I have seen of yours contain Keely's determinations. All the observations of the Erebus and Terror have not yet appeared; in the Atlantic I know only the total intensities but without inclinations or declinations, and yet I am very anxious for some determinations that have been made between 10° and 20° of longitude in the higher latitudes to compare my calculations with them. If your time permits, I should be very much obliged to you if you could communicate to me the inclination and force at some points. In the mean time I will occupy myself with the discussion of the two horizontal forces; unfortunately the number of determinations serving for this purpose is much smaller. For North America those recorded by Lamont in Dove's 'Repertorium' are for the most part in comparatively low latitudes.

XV. *Intelligence and Miscellaneous Articles.*

POSTSCRIPT TO MR. T. G. BUNT'S PENDULUM EXPERIMENTS.

June 23. DURING the last two or three days I have been making some further experiments, with a view to ascertain more nearly the rate of the apsidal motion, when the arc of vibration becomes very small. Twelve experiments, averaging about 20 minutes each, gave 11°·60 for the azimuthal horary motion, when the mean length of the arc was 11 inches and the ellipticity +0·19 inch; and seventeen similar experiments gave 11°·39 per hour, when the ellipticity was −0·17 inch. The mean length of arc, ellipticity, and motion in azimuth, for each hour, are as follows:—

Length of Arc.	Mean Ellipticity.	Part of Circle observed.	Motion in Azimuth per hour.
in. in.	inch.	° °	°
12 to 9	+0·18	49 ... 62	11·64
13 ... 9	+0·19	62 ... 74	11·88
14 ... 9	+0·24	74 ...113	11·90 11·90 11·90
14 ... 11	+0·22	113 ...127	11·00
14 ... 9	+0·27	141 ...156	12·66
14 ... 11	−0·16	50 ... 63	11·81
14 ... 9	−0·07	65 ... 89	11·50 11·50
15 ... 11	−0·23	90 ...102	11·55
16 ... 11	−0·13	102 ...113	12·03
14 ... 10	−0·02	113 ...133	11·35 11·35
14 ... 10	−0·13	134 ...144	10·78
15 ... 11	−0·08	127 ...140	11·67
		Mean...	11·651

On leaving the pendulum yesterday evening I gave it an impulse, intending to see what would be the motion of the plane during the night. At 7^h 34^m P.M. it was vibrating towards the division 170°·53, the arc being 22 inches and the ellipticity −0·04 inch. This morning, at 9^h 30^m, the plane of vibration cut the circle at 331°·80, the ellipticity was about −0·01 inch, and the arc 1·6 inch. The mean hourly motion during the night had therefore been 11°·576.

I stated in my former letter, that the motion of the plane of vibration, when in proportion to the sine of the latitude of St. Nicholas Tower, is 11°·7309 per hour. This is true for an hour of *sidereal* time only; for an hour of mean time, the amount will be 11°·763 nearly.

ON THE TOTAL ECLIPSE OF THE APPROACHING 28TH OF JULY.

BY M. FAYE.

A few days ago, through the politeness of Mr. Airy, I received a remarkable tract published by a committee of the British Association for the Advancement of Science, in conjunction with the astronomers of Russia, for the purpose of making known the arrangements which will be requisite for the complete observation of the approaching eclipse. Since the Committee has done me the honour of directing the attention of astronomers to one of my memoirs upon instrumental errors, in which I have incidentally treated of eclipses*, it may perhaps not be considered amiss for me again to dwell upon the notions which I published last year.

In accordance with these notions, it would be of importance to determine the temperature of the atmosphere, and especially its variations, by means of very delicate thermometers, and at different elevations above the surface of the earth. If even aeronauts would consent to make an aerial ascent on the 28th of July, they would undoubtedly enjoy during the complete eclipse, a very beautiful spectacle; they would be certain, at all events, to render their undertaking of great scientific interest, by carefully determining the variations of the temperature at an elevation of some thousands of metres. Probably some very useful information upon the temporary constitution of the atmosphere during the eclipse might be obtained by measuring several very exact zenithal distances of the sun or the moon, before, *during*, and after complete obscuration. M. Otto Struve has informed me that arrangements will be made, at least at one station, in conformity with the plan suggested by me, and this good news diminishes my regret at not being able to render my feeble cooperation in this noble astronomical undertaking. I may, however, be permitted to recall to the attention of observers a phænomenon which the Report of the British Association has omitted to notice, undoubtedly because the phænomenon has appeared too doubtful or completely exceptional. But as numerous stations ought to be arranged in several lines perpendicular to the progressive motion of the shadow, and as the observers ought to occupy every possible position within the cone of the shadow, it is probable that no phænomenon, however rare and exceptional, will escape attention when so scientifically directed. I must say a few words here upon the brilliant points observed by Ulloa and M. Valz upon the disc of the moon. In consequence of an inherent tendency of the mind, which at first always attributes substance and reality to appearances by which it is struck, the luminous points have been explained as material apertures existing in the substance of the moon. But in my opinion, one and the same theory is sufficient to explain both the external protuberances and the internal apertures; this consists in viewing them as two distinct effects of mirage produced temporarily in the atmosphere; and I have pointed out the probable connexion of these phænomena, which are so dissimilar in appearance, in the excellent Astronomical Journal of my learned friend Mr. Gould, published in the United States†. They both depend upon the distribution of the temperatures of the layers of air parallel to the visual rays; except that in the second case, and in consequence of the position of the observer, the trajectory presents a point of inflection which does not exist in the case of the external protuberances. These statements are very reservedly made; as regards the fact itself, it is based upon evidence, the entire value of which is known to astronomers. M. Valz must himself have been forcibly struck by it, for during the occurrence of a partial eclipse here, I have seen him carefully seek for the brilliant aperture which he remarked in 1842.

* *Comptes Rendus*, 1850, Nov. 4.

† Gould's Astronomical Journal, No. 20, p. 157.

As the altitude of the sun will be very different at the various stations, from Norway to the Black or the Caspian sea, the thickness of the layers of the atmosphere which must exert some influence upon the phænomenon, will vary in a very marked degree. Now, the height and the brilliancy of the red mountains will depend essentially upon this thickness; hence it must be expected that the mountains will be larger and more marked to observers situated towards the eastern extremity of the band traversed by the shadow, unless a greater depression of temperature compensates, towards the west, for the effect of a less thickness of the refracting layers of air. It is here again evident how far the measure of these variations may prove of interest in regard to the proofs to which it may be condescended to submit the preceding ideas. The eclipse of the 28th of July will, I hope, be completely decisive, thanks to the admirable understanding of the astronomers of the two great countries.—*Comptes Rendus*, May 19, 1851.

METEOROLOGICAL OBSERVATIONS FOR MAY 1851.

Chiswick.—May 1. Very fine. 2. Clear: fine: slight frost at night. 3. Fine: rain at noon: cloudy. 4. Cloudy and fine: frosty at night. 5. Cloudy and cold. 6. Slight rain: cloudy and cold. 7. Fine, but cold. 8. Fine. 9. Fine: clear. 10, 11. Very fine. 12. Cloudy and fine. 13. Fine: clear. 14. Cloudy: clear and frosty. 15. Very clear: fine: frosty at night. 16. Very fine: densely clouded: rain. 17. Densely clouded. 18. Overcast: clear. 19. Cloudy: fine: clear. 20. Clear and cold: fine. 21. Overcast. 22. Cloudy and warm. 23. Hazy: fine: clear. 24. Very fine. 25. Cloudy: rain. 26—28. Fine. 29—31. Very fine.

Mean temperature of the month	51°·16
Mean temperature of May 1850	51 ·14
Mean temperature of May for the last twenty-five years .	54 ·13
Average amount of rain in May	1·89 inch.

Boston.—May 1. Fine. 2. Cloudy: rain P.M. 3. Cloudy: rain A.M. and P.M. 4. Cloudy: rain and hail A.M. and P.M. 5. Cloudy: rain A.M. and P.M. 6. Cloudy: rain A.M. 7, 8. Cloudy. 9. Fine. 10. Cloudy. 11. Cloudy: rain A.M. 12, 13. Cloudy. 14—16. Fine. 17. Cloudy. 18. Cloudy: rain P.M. 19. Cloudy: rain A.M. 20—22. Cloudy. 23, 24. Fine. 25. Cloudy. 26. Cloudy: rain and hail A.M. 27—31. Fine.

Applegarth Manse, Dumfries-shire.—May 1. Frost keen: hail-shower: rain-shower. 2. No frost, but cold: fair all day. 3. Cold: hail-showers: wind keen. 4. Frost: hail: rain P.M. 5. Cold: dull: quiet. 6. Milder. 7. Mild and slight showers. 8. Dull and cloudy: rain P.M. 9. Heavy showers. 10. Dry and parching. 11. Wind high, but fair. 12. Fine: cloudy P.M. 13. Fine day. 14. Very fine all day. 15. Fine: cloudy P.M. 16. Dull: slight showers. 17. Fine: dull P.M. 18. Wet morning: cleared and fine. 19. Hail-showers frequent. 20. Dull and showery. 21. Dull, but fair. 22. Cloudy: cold wind. 23. Fine clear day and fair. 24, 25. Fine A.M.: slight shower P.M. 26. Fair and clear. 27. Fair, but chilly. 28. Fair and fine: wind strong. 29. Fair and fine: wind keen. 30. Fair and fine: very droughty. 31. Fair and fine: very warm.

Mean temperature of the month	48°·9
Mean temperature of May 1850	49 ·1
Mean temperature of May for the last twenty-nine years ...	50 ·9
Average rain in May for twenty-four years	1·89 inch.

Sandwick Manse, Orkney.—May 1. Damp: cloudy. 2. Damp: drizzle: showers. 3, 4. Snow-showers. 5. Bright: drops. 6. Damp: drops. 7. Clear: fine: clear. 8. Bright: cloudy: aurora. 9. Bright: clear. 10. Bright: fine. 11, 12. Cloudy: clear. 13. Fine: hazy. 14. Bright: hazy. 15. Clear: rain. 16. Fine: clear. 17. Cloudy. 18. Bright: clear. 19. Cloudy: showers. 20. Bright: drizzle. 21. Hazy. 22. Drizzle: showers. 23. Showers. 24. Fine: rain. 25. Showers: clear. 26. Bright: fine. 27. Damp: showers. 28. Hazy: drizzle. 29. Hazy: damp. 30. Showers: hazy. 31. Hazy.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month. 1851. May.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¾ a.m.	Barometer. Orkney, Sandwick. 8¾ p.m.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Thermometer. Orkney, Sandwick. 9¾ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.
● 1.	29·760	29·672	29·40	29·58	29·52	29·67	29·81	58	32	43	52	31	46	43	sw.	w.	nw.	ne.	......	·03	......	·18
2.	29·835	29·659	29·33	29·69	29·73	29·86	29·89	57	29	49	50	40½	42½	37	n.	nnw.	e.	nnw.	......	......	......	·02
3.	29·826	29·732	29·45	29·72	29·74	29·99	29·95	54	32	45	47	39	41	36	nw.	nnw.	ne.	n.	·09	·04	......	·06
4.	29·702	29·665	29·33	29·70	29·72	29·95	29·97	50	30	41	48½	32½	42½	40	n.	nnw.	nne.	n.	·02	·06	......	·14
5.	29·709	29·677	29·27	29·71	29·73	29·95	29·97	54	38	46·5	47	36½	42½	40	n.	n.	n.	n.	·02	·03	......	·01
6.	29·846	29·775	29·36	29·78	29·71	29·91	29·83	52	36	45	52	36	43½	40	nw.	n.	nw.	nw.	·01	·06	......	·04
7.	29·868	29·830	29·44	29·62	29·58	29·71	29·65	53	38	45·5	53	40	48½	41½	w.	nw.	sw.	ese.	......	......	......	·02
☽ 8.	29·770	29·674	29·30	29·49	29·42	29·58	29·65	61	38	53	54	44	47½	47	s.	s.	se.	ese.	......	......	0·20	......
9.	29·676	29·639	29·26	29·50	29·52	29·72	29·80	59	43	55·5	47	45	49	46	s.	se.	se.	ese.	......	......	......	......
10.	29·613	29·598	29·26	29·61	29·62	29·82	30·01	70	37	58	55	40	51½	46½	s.	ese.	se.	se.	·03	......	......	......
11.	29·775	29·715	29·30	29·73	29·88	30·08	30·14	66	47	61	50	44	48	43	n.	ene.	e.	ne.	......	·08	......	......
12.	30·070	29·875	29·48	30·00	30·11	30·16	30·02	63	43	63	58	41	48	47	n.	e.	e.	se.	·29	......	......	......
13.	30·299	30·242	29·80	30·22	30·25	30·28	30·28	58	31	53	58	37	54	47½	ne.	ne.	se.	se.	......	......	......	......
14.	30·335	30·291	29·95	30·30	30·21	30·26	30·16	57	27	52	51	35	56½	54	ne.	ene.	s.	sw.	......	......	......	......
○ 15.	30·255	30·152	29·82	30·25	30·20	30·04	30·00	64	29	51	53	50	59½	51	ne.	e.	s.	......	......	......	0·17	......
16.	30·099	30·041	29·60	29·90	29·83	29·96	29·94	68	46	51	55	48	49½	47	sw.	wnw.	se.	sw.	·05	......	......	·35
17.	29·962	29·945	29·50	29·80	29·75	29·90	29·72	69	48	61	58	46	52	49	sw.	sw.	s.	s.	......	......	......	......
18.	29·881	29·849	29·34	29·60	29·65	29·50	29·54	63	40	61	52	47	48	44½	sw.	wsw.	sw.	w.	·10	......	......	·25
19.	30.071	29·834	29·38	29·58	29·80	29·50	29·76	63	37	51	51	39	45	45	sw.	w.	wsw.	nw.	·04	·06	......	·03
20.	30·238	30·016	29·73	29·98	30·02	29·94	29·96	63	48	52·5	55	46	48½	48½	nw.	nw.	sw.	se.	......	·04	......	·12
21.	30·216	30·238	29·75	30·07	30·11	30·00	29·98	65	44	59	57	48	51½	51½	sw.	wnw.	w.	w.	......	......	......	·05
22.	30·259	30·221	29·77	30·08	30·12	29·86	30·00	70	42	65·5	58	47	49½	47	sw.	wnw.	w.	nw.	......	......	0·23	......
☾ 23.	30·268	30·254	29·83	30·19	30·30	30·08	30·20	66	38	55	61	39	49	46½	nw.	nw.	nw.	w.	......	......	......	·19
24.	30·342	30·286	29·90	30·32	30·06	30·20	29·88	71	47	55	57	37½	51	51	se.	w.	wnw.	sw.	......	......	......	·02
25.	30·208	29·921	29·60	29·99	29·82	29·84	29·82	72	45	62	54	49	49	45	sw.	wnw.	wsw.	nw.	·08	......	......	·16
26.	29·965	29·902	29·49	29·87	29·93	30·00	30·04	62	34	53·5	58	46	47	44½	ne.	n.	wnw.	w.	·01	......	......	·07
27.	30·093	30·066	29·68	29·92	30·00	29·90	29·98	63	46	52·5	59	46½	48½	47	nw.	wnw.	nnw.	w.	......	·04	......	......
28.	30·254	30·203	29·74	30·10	30·18	30·02	30·14	68	39	54	54½	43	51½	49	ne.	wnw.	nw.	w.	......	......	......	·03
29.	30·367	30·349	29·90	30·29	30·31	30·26	30·22	75	47	58	63½	50	53	51	n.	wnw.	nw.	sw.	......	......	......	......
● 30.	30·411	30·402	29·94	30·35	30·38	30·32	30·42	74	45	63	64	50½	49½	47½	e.	nw.	w by n	w.	......	......	0·05	·02
31.	30·470	30·407	30·05	30·40	30·33	30·36	30·30	68	35	60	66	44	50	50	e.	n.	sw.-w.	w.	......	......	......	·01
Mean.	30·046	29·971	29·54	29·914	29·791	29·952	29·968	63·26	39·06	54·0	54·7	42·5	48·80	45·91					0·74	0·44	0·65	1·77

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1851.

XVI. *On the Measurement of Chemical Affinity.* *By* Isaac B. Cooke*.

THE controversy in reference to the source of voltaic electricity, appears to be decided, at least in England, in favour of the chemical theory. A voltaic current seems generally admitted to be nothing else than the circulation, in another form, of the sum of the chemical affinities developed in the circuit. Is it not therefore time, since electric currents are susceptible of minutely accurate measurement, that an attempt was made to analyse the forces circulating in our batteries, and to assign to the different substances present the value of the respective affinities they mutually exert, from the united action of which results that balance of forces constituting the effective power of the battery?

Accurately to measure and tabulate the combining *force* of the constituents of compound substances, as we now do their combining *quantities*, specific gravities, &c., would be an important step in chemical science, and a great addition to our knowledge of the natural history of the materials of the globe.

Many attempts have been made roughly to estimate these forces by purely chemical experiment; and tables have been constructed of the strength of affinities, in which, without numerical values, substances have been arranged, with more or less accuracy, in the order in which they expel each other from their combinations. Estimates have also been formed, founded on other considerations, but with little pretensions to numerical exactness. Accurate measurements, again, have been made by several methods, and by various observers, of the intensities of current developed in different voltaic arrangements, and with

* Communicated by the Author.

different substances in action; but, as far as the writer is aware, no attempt has yet been made to analyse these intensities, and to attribute to any two substances the exact amount of force which their act of combination contributes to the general result.

The inconstancy of the current developed in the simple voltaic cell,—the reduction of its strength arising from the continuance of its own action,—if it do not furnish an argument against the chemical theory, at least seems unfavourable to the attempt to fix any specific constant force, as due to the mutual affinity of any two elements of the series. And the apparent complexity of the more constant forms of electromotors,—the number of elementary substances concerned in their circuits,—appears to forbid the hope of analysing the mere balance of such a multitude of forces, so as to render evident their individual values.

Let us, however, investigate the sources of the inconstancy of the zinc and copper pair, both immersed in the same fluid. For this purpose we shall require to measure, first, the initial intensity of the current of the cell before it becomes in the least degree weakened by its own action; and again, to measure the remainder of constant force which may be left after all the sources of inconstancy have expended their action, and before any increase can be regained by an instant of repose.

The elegant method of compensation devised by M. Poggendorff for the measurement of initial intensities, seems only calculated for the most skilful manipulators. To make a perfect and conclusive experiment by this method, requires beforehand a knowledge of the fact which the experiment is intended to determine. And though the exact compensation may be arrived at after a few preliminary trials, the time consumed in these trials, if many cases have to be examined, is inconveniently great; since if the elements have been subjected to even a slight action, they are no longer admissible for a second experiment, and the whole arrangement must be prepared anew.

Intensities may also be measured by the use of Professor Wheatstone's rheostat, and by Ohm's "limit multiplier;" but these instruments act only by virtue of a continuous current, and cannot therefore be trusted to determine either initial or variable intensities. Galvanometers, however, may be constructed sufficiently sensitive to indicate currents incomparably smaller than those for which they are usually employed; and as it seemed probable that the causes of inconstancy in the forces would diminish more rapidly than the forces themselves, so that the former would be wholly imperceptible to an instrument which might yet be sufficiently sensitive to reveal correctly the relative magnitudes of the latter, an attempt was made in this direction.

So excellent is the conducting quality of metals, that a sufficient resistance could not be obtained for this purpose through their use, except by a most inconvenient length of wire, even of the utmost practicable tenuity, and though used as an independent resistance, without directive action on the needle. Fluids could easily be made to oppose the required resistance, but would introduce new tensions into the circuit, and perhaps additional sources of inconstancy in their electrolysis.

Theoretically, the electrolysis of a salt, between electrodes of its metal base, should introduce only a balance of affinities into the circuit; inasmuch as for every equivalent of the metal thrown down at the cathode, an equivalent is absorbed at the anode. But practically, it is almost impossible to place two pieces of the same metal, in even a solution of their own salt, without a current of electricity being generated between them when put in contact; and if a current be passed between them from an independent source, a reactionary force is generally created. These evils we might also perhaps hope to overcome by such a considerable reduction of the quantity of the current, as would leave merely sufficient to be measured by a galvanometer of the most sensitive construction.

A glass tube 4 inches long, and about $\frac{1}{10}$th of an inch bore, was fitted with a copper cap at each end. It was then by means of a small opening drilled through each cap, filled with a solution of sulphate of copper, formed of one part of the saturated solution with ten of water. The openings were then closed tight by small copper wires screwed in.

When this tube was connected in a vertical position with one wire of a very sensitive galvanometer, the current from a pair of zinc and copper plates in rain-water, exposing a surface of 1 square inch and $\frac{1}{4}$ inch apart, if sent through so as to deposit copper upon the lower cap, caused a deflection of the needle of 8°, which was *perfectly constant through long-continued action.*

The two caps of the tube afterwards showed no tendency to reaction through the galvanometer. All pairs of zinc and copper in rain-water, without reference to size, if only not smaller than the above, gave exactly the same indication through the tube and galvanometer of 8°; and in all these cases it was perfectly constant.

If the copper plate of the electromotive pair was retained at the standard size of 1 inch, the zinc plate might be considerably reduced below that limit, without diminishing the maximum deflection of 8°; but however large the zinc plate was made, the copper plate could not be materially reduced below the standard size of 1 inch, without reducing the deflection and rendering the current inconstant.

The resistance furnished by the tube was thus evidently incomparably greater than the sum of all the other resistances of any circuit of ordinary dimensions and conducting capacity into which it could be introduced; and as by its use the sources of inconstancy could be eliminated from all such circuits, and their resistances reduced to one common standard, the currents they respectively developed would be obtained at the galvanometer in direct proportion to their initial unweakened intensities; or, in other words, to the balance of the affinities exerted between the elements present, before those affinities can in any degree be satisfied by combination.

If, while a cell was thus connected with the resistance tube and galvanometer, the circuit was additionally completed by a short thick wire joining the anode and cathode, the deflection of the needle was of course destroyed; and after a certain lapse of time, if the wire was suddenly removed, the first swing of the needle revealed the amount of diminution of current affinity caused by the continued unobstructed action of the elements during that space of time.

In the case of a pair of zinc and copper in rain-water, the diminution of force was found to vary with the time of continuance of the connexion, with the resistance of the circuit, and with the excess in size of the zinc plate over that of the copper. The current was never reduced to zero, but each individual cell attained to a different minimum.

On removal of the short connecting wire, the pair immediately commenced slowly to recover their original force, which, however, could almost instantly be fully restored by taking out the copper plate, and after waving it in the air, replacing it in the cell. No such result was produced by a similar treatment of the zinc plate, if effected without disturbance of the copper, or of the medium in which it was placed.

If the copper plate was much larger than the zinc, and the pair was placed in a stream of water, the maximum current was maintained, even after a connexion by a thick wire of some moments' duration.

If a single cell of Smee's battery was joined in series with a pair of zinc and copper in water, the inconstancy of the latter could be extended to the zero-point; and indeed, as soon as bubbles of hydrogen covered the surface of the copper plate, the current became reversed, the copper being then slightly positive to the zinc.

In considering the sources of the current in these simple circuits, it appears fair to assume that no affinity is exerted, when, under favourable circumstances, no combination is effected. Thus the development of nascent hydrogen upon the copper plate is favourable to the combination of the hydrogen and copper.

As no such product is formed, it may legitimately be assumed that the mutual affinity of these two substances has no part in causing the current of the cell. In the same manner, when water is decomposed between two zinc plates, as the hydrogen is given off without combining with the zinc, we may conclude that no affinity between these two elements is concerned in modifying the strength of the current. The only possible affinities, therefore, whose balance of forces can constitute the current of a zinc and copper pair in water, may be thus enumerated:—

1st. Zinc for the oxygen of the water.

2nd. Copper for the oxygen of the water.

3rd. The hydrogen and oxygen of the water for each other.

4th. The oxygen dissolved in the water, for the other elements of the circuit.

The only apparent chemical product formed by the action of this simple circuit is the oxide of zinc. But as no hydrogen is evolved from the copper, it must either be absorbed in some way, or the water is not decomposed by the zinc. On the latter supposition, the action would be merely local, the zinc combining directly with the dissolved oxygen in its own neighbourhood, and no current would be circulated. The action must therefore be as follows:—First, the zinc attacks the oxygen of the water, liberating its hydrogen to combine with the oxygen of the next particle of water; and thus by successive combinations and decompositions, hydrogen travels on to the copper plate, when it finally combines with the oxygen held in solution; and in addition to the oxide of zinc, water also is a product of the action.

The most perfect type of electromotive apparatus would probably be produced, if two solid conductors, having a powerful mutual affinity, could be arranged in a fluid electrolyte of which they should be themselves the constituent ions. The two conductors would be equally active as electromotors, and the current resulting would be the measure of their combining energy. The nearest approximation to this type at present known, is probably the gas battery of Grove, where the two ions, oxygen and hydrogen, unite to form the electrolyte water; the platinum plates being merely requisite to serve as odes or doorways for the combination of the elements, and for the passage of the current affinity. All batteries have more or less resemblance to this type. The water battery of zinc and copper differs from it principally in the substitution of the metal zinc for one of the ions of the electrolyte, viz. hydrogen, for which it has no affinity, but to the affinities of which its own have much resemblance. The copper serves as the ode by which the other ion, viz. the dissolved oxygen, contributes its action, and by which the resulting current is transmitted. The copper and dissolved oxygen tend also to produce an opposing current, for which the zinc plate would serve as ode; but this current is overcome and masked by the much stronger one of the other pair, and is only manifested by diminishing the force in comparison with what it would have been, if a plate wholly indifferent to the oxygen was substituted for the copper. The formula of the force may be thus stated,

$$z \text{ for } O - O \text{ for } H + H \text{ for } O - (c \text{ for } O - O \text{ for } H + H \text{ for } O);$$

leaving a balance of affinities, after the elimination of equal and contrary forces, of

$$(z - c) \text{ for } O.$$

If this view be correct, the inconstancy of the water battery must arise from the exhaustion of the dissolved oxygen on the surface of the copper plate, this being the only element of which the supply is deficient in quantity; a conclusion fully warranted by the above experiments with the resistance tube. And it is to the abundant supply of this element in the Daniell's battery that its superior constancy is owing.

The substitution for the copper plate of any other conducting substance having no affinity for either oxygen or hydrogen, would destroy the negative portion of the formula $(z - c)$ for O; and the deflection of the needle by a current passed through the resistance tube would be then proportional simply to the affinity of the zinc for oxygen. And thus the relative affinity for oxygen of all conducting substances, having no affinity for hydrogen, might be ascertained if such a substitute for the copper plate could be procured.

Now platinum forms no combination with, and consequently exercises no affinity sensible to the galvanometer for, either oxygen or hydrogen when developed in the nascent state on its surface by electrolysis. Platinum might therefore be supposed to furnish the required substitute for the copper plate. But here a difficulty arises, in consequence of the peculiar conditions of surface which platinum ordinarily assumes. After an electric current has passed through water, between two plates of platinum, they are no longer similar in their electric relations, but are capable, when closed in circuit, of creating a reactionary current, until they gradually return to their ordinary state of inertness. Though platinum, therefore, has no affinity for oxygen or hydrogen, it is in some way susceptible of different relations to them, and is in fact extremely inconstant in its value as an electro-negative substance.

This property of platinum, which is equally shared by gold, and partially by silver and other electro-negative bodies, has been usually classed, without any very precise definition, as a result of polarization; and though it is perhaps thoroughly understood by many philosophers, has not, as far as the writer is aware, been clearly explained in any published treatise.

A few experiments with the resistance tube sufficed to demonstrate the real nature of the phænomenon. But first, the construction of the tube was improved in accordance with the facts above deduced, and a form adopted which permitted the measurement of intensities with greater minuteness. A glass tube 12 inches long, and $\frac{1}{8}$th of an inch in internal diameter, of perfectly even bore, was graduated through its length on the glass into twentieths of an inch, and these divisions numbered from 1 to 240. It was placed in a tall glass jar about 2 inches in diameter. A coil of copper riband covered the bottom of the jar, and one end of the riband rose to the surface, and was connected with one wire of the galvanometer. A piece of copper wire, rather longer than the tube and thin enough to move easily in it, was inserted into the tube, and being slightly bent, would remain at any height at which it might be placed. To its upper end was attached a long fine copper wire, which could be connected by means of a mercury cup on the table with one of the metals of any cell, the intensity of which was to be tested; the other metal being connected with the second wire of the galvanometer. The connexion was always made so as to deposit copper upon the copper coil. The jar was filled with rain-water, to which a small quantity of a solution of sulphate of copper was added; when it was found that the current of a pair of zinc and copper, of ordinary dimensions in rain-water, produced a momentary deflection of 5° in the needle of the galvanometer, the end of the wire in the tube being raised to the forty-sixth division. When the wire was placed at a lower elevation, a greater deflection was of course obtained. And a permanent deflection of 10° could be maintained for a considerable time without variation, by any pair of zinc and copper exposing more than a square inch of surface.

As the intensity of the current is equal to the product of the quantity into the resistance, the relative intensities of different circuits would therefore be measured by the product of the deflection of the needle into the number of divisions contained between the end of the wire and the bottom of the tube, provided the resistance of the tube could be safely taken as the total resistance of the circuit; or

$$I=5\times 46=230.$$

But inasmuch as the coil of the galvanometer is formed of a very fine long wire, it may be supposed to oppose a sensible resistance, which can be easily measured and allowed for. In fact, we should put

$$I=5\times(46+r),$$

where r is the resistance of the galvanometer coil. The wire was pushed down the tube until an elevation was attained, at which the momentary deflection of the needle by a zinc and copper circuit was 10°, which was found to require a resistance of twenty-one divisions of the tube, or

$$I=10\times(21+r)=5\times(46+r),$$

whence

$$r=4.$$

Consequently, in all measurements of intensity, four has been added to the number of the divisions of the tube, to allow for the resistance of the coil.

Measured thus, the intensity of the current from zinc and copper plates in water, or the affinity of zinc for oxygen, less the affinity of copper for oxygen,

$$=5(46+4)=250;$$

while the intensity produced by zinc and platinum plates in the same fluid

$$=5(49+4)=265;$$

leaving, if the platinum be really inert, only 15 for the affinity of copper for oxygen, or only $\frac{1}{17}$th part of that of zinc for oxygen.

Considering the easy oxidation of copper and its fierce decomposition of nitric acid, this small estimate of its force of affinity for oxygen does not seem a probable result, and the platinum may be fairly suspected of exercising some counteracting influence. When oxygen is nascent upon the surface of platinum, even though it be urged in addition to its own affinity by the current affinity of a thousand cells, no combination is effected. Can it be that there is a tendency to combine which is sensible to the galvanometer, but by some hidden influence which is not sensible to the galvanometer the combination is prevented? The supposition is inadmissible until no other explanation can be found.

The attempt was made to ascertain the limit, if there be one, to which the polarization of platinum can be carried, and whether both anode and cathode are equally affected. Two platinum plates and one of copper were arranged in a glass of rain-water without contact with each other. The copper plate was connected with the galvanometer. The pair of platinum plates were joined in series with a battery by which the water between them was electrolyzed. The wires were so adjusted, that it was easy instantaneously to disconnect the battery, and to join either platinum plate to the resistance tube. As soon as bubbles of oxygen began to form on the anode platinum, its junction with the instrument and copper plate was effected. The swing of the needle was now 5° when the current was passed through fifty-nine divisions of the tube, giving an intensity $=5(59+4)=315$. The swing could be reproduced several times undiminished without reconnexion with the battery. The total secondary current between the two platinum plates $=5(114+4)=590$; but this current was more inconstant than the former, and began immediately to diminish. When the cathode platinum plate was connected with the galvanometer, and the copper plate with the resistance tube, the intensity was $5(51+4)=275$, which equals the difference of the two former numbers; but was very inconstant, and could instantly be reduced almost to zero, by shaking the platinum plate so as to dislodge all the bubbles of hydrogen.

The anode platinum plate was found to receive its maximum polarization from the current of two cells of Smee's battery. The current of six cells did not increase it. The cathode received its maximum polarization from three cells, and after being thoroughly covered with hydrogen, could receive no increase of force.

The polarization of the cathode plate therefore clearly resulted from the coating of hydrogen with which it became covered, but the anode received its greatest charge before a single bubble of oxygen was formed; and indeed, since oxygen was already present in solution in the water, its additional production by electrolysis was not likely to produce an increased effect.

This definite amount of polarization of the anode plate would be accounted for, if its surface could be supposed to contract from the atmosphere, in its ordinary condition, a covering of matter possessing an affinity for oxygen. The current would of course be affected by this covering at its commencement, as by an electro-positive element; but as the covering became eaten away by the oxygen determined to its surface by the force of the battery, the purified platinum would begin to act simply as conductor, and wholly indifferent as to affinity.

That platinum and other substances do contract such a covering, Dr. Faraday has proved in his experiments on the catalytic deflagration of explosive gases. He has also shown, that exposure at the anode of a battery to the action of nascent oxygen, is identical in its results with a mechanical purification of the surface of platinum.

This covering is instantly destroyed by immersion in strong nitric acid; and it is to this fact probably, more than to the excessive facility with which nitric acid yields one out of its five equivalents of oxygen, that Grove's nitric acid battery owes its superiority in energy over Daniell's sulphate of copper battery.

Since the coating actually exists on the surface of the negative metal, and since the erosion of such a coating by oxygen suffices to explain the polarization of the anode plate, it may be accepted as the true explanation. If a platinum plate, therefore, previously purified from all adhering matter, be taken to serve as cathode in combination with an electro-positive substance in water, *the quantity of current urged by them through the galvanometer, multiplied by the number of divisions of the resistance tube through which it is passed, may be taken as the measure of the affinity of the electro-positive substance for oxygen.*

The measurements given in the table were made on this principle in the following manner:—Two plates of platinum were placed in a glass of clean rain-water, and were connected with the poles of a two-celled Grove's battery. A portion of the substance whose affinity for oxygen was to be tested was placed in the same glass, out of contact with the platinum plates. The anode platinum plate was also permanently connected with the resistance tube and the wires from the cathode platinum, and from the substance to be tested, were so adjusted in mercury cells on the table, that the connexion of the one with the battery could be destroyed and the other be instantly joined to the free wire of the galvanometer. The first swing of the needle, multiplied into the number of divisions +4 of the tube below the end of the wire, was recorded as the force of affinity. Each experiment was repeated several times, and the result confirmed by placing the wire of the tube at different altitudes, and comparing the deflection produced with that calculated.

The measurements are far from the limit of minute accuracy, of which the method is susceptible with superior instruments, but are the best which the means and leisure of the writer enable him to obtain. The current of zinc and copper in water is taken as the standard unit of force, because, by simple arrangements on the table, it is capable, at an instant's notice, of easy verification. Frequent verification is indeed essential, in consequence of changes to which the resistance tube is liable. After long-continued action, the copper electrodes of the tube take on a very slight amount of polarization. The homogeneity of the solution in the jar and tube is apt to be slightly disturbed. Changes of temperature, again, somewhat modify the conducting capacity of the solution. Any of these sources of error is detected instantly by appeal to the standard electromotor, and easily remedied or allowed for. The wire of the tube should, when out of use, be thrust down into contact with the copper riband, and also connected with it by its wire through the galvanometer. The copper solution should be thoroughly stirred up by the tube previous to experiment, and the room be kept during experiment as nearly as possible at a uniform temperature. With these precautions, measurements may be taken again and again without the slightest variation. Any great changes in the temperature will of course necessitate a correction in the allowance for the resistance of the galvanometer coil.

Table of the Affinity of various Substances for Oxygen.

Zinc—copper (standard) .	=1
Hydrogen	=2·36
Zinc	=2·23
Potassium	=3·13
Sodium	=2·91
Iron	=1·85
Tin	=1·75
Lead	=1·7
Bismuth	=1·29
Antimony	=1·29
Copper	=1·25
Silver	= ·85

These affinities do not appear to be affected by changes of temperature between the range of 50° and 212° F.

It is almost impossible to obtain mercury perfectly free from traces of impurity; and however small may be the quantity of any substance more electro-positive than itself which may be dissolved in it, the mercury usurps to the full the affinities of that substance. This property renders its own affinity for oxygen difficult to determine, but has been made use of to ascertain those of potassium and sodium, the action of which metals upon water is otherwise too violent to admit of satisfactory experiment. A fluid amalgam of either of these metals decomposes water slowly, especially if the surface exposed be small compared with the bulk of the amalgam, and a deliberate experiment is thus permitted.

It will be seen from the table, that hydrogen has a higher number assigned to it than is given to zinc. Why then, it may be asked, does zinc so easily decompose acidulated water? The truth is, that the zinc is retained in its salts generally by a force fully as strong as that which combines it with oxygen alone, while hydrogen is held in acidulated water with a considerably slighter affinity than in pure water. Pure zinc will not decompose pure water if atmospheric air or oxygen be not present.

The principle developed in this paper is probably applicable to many classes of salts, to sulphurets and other compounds. Attempts to apply it to the chlorides have hitherto proved unsuccessful, chiefly owing to the want of a conducting substance perfectly indifferent to chlorine, which even plumbago can scarcely be supposed to be.

The galvanometer may perhaps, by this method, shortly become a useful instrument in qualitative analyses.

XVII. *Further Observations on the Theory of Probabilities.* *By* George Boole.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Some communications which I have received since the publication of my letter on the Theory of Probabilities in the last Number of your Journal, have led me to think that a little further explanation of certain points involved in it may be desirable. This explanation I the more readily offer, because it appears to me that upon one of the points in question, viz. the prevalent doctrine among mathematicians concerning the investigation of the probabilities of causes, I have made a statement which a more careful survey of authorities does not fully warrant. As the question lies at the foundation of some of the most interesting applications of the theory of probabilities, I am desirous of stating how it has really been viewed by eminent writers; and I shall subsequently notice certain other points suggested to me in the correspondence above referred to.

The problem under discussion was the following:—Given the probability p of the truth of the proposition. If the condition A has been satisfied, the event B has not happened. Required the probability P of the truth of the proposition. If the event B has happened, the condition A has not been satisfied. And its correct solution, as given in my letter, is

$$\mathrm{P}=\frac{c(1-a)}{c(1-a)+a(1-p)}, \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

c and a being arbitrary constants whose interpretation is assigned. I have remarked that it has generally been erroneously held, that the solution of the above question is $\mathrm{P}=p$. It is to this point that I desire first to refer.

The doctrine that $\mathrm{P}=p$ is expressly taught in the Edinburgh Review (Quetelet on Probabilities). Speaking of a certain combination of phænomena observed in rock-crystal, the Reviewer says, "The chances against such a coincidence happening thirteen times in succession by mere accident are more than 8000 to 1; and this therefore was the probability that some law of nature, some cause was concerned."

The same doctrine seems to me to be strongly implied by Laplace in the Introduction to his great work on Probabilities. Discussing the question of a primitive cause, fixing the direction of rotation of the planets in their orbits, he introduces the object of his inquiry in the words "pour avoir la probabilité avec laquelle cette cause est indiquée." And then having determined, on the hypothesis of the absence of such determining cause, the probability *against* the phænomenon of rotation in one uniform direction, he says, "Nous devons donc croire au moins avec la même confiance qu'une cause primitive a dirigé les mouvements planétaires, surtout si nous considérons que l'inclinaison du plus grand nombre de ces mouvements à l'équateur solaire est fort petite." Laplace does not indeed expressly affirm the principle under consideration, but it appears to me that his language does in some degree give it sanction.

Mr. De Morgan, in investigating the probability that there is a cause for the observed phænomenon that the sum of the inclinations of 10 of the planetary orbits is less than 92°, reasons in the following manner. Having found a calculated probability ·00000012, say q, that the sum of the inclinations would be less than 92° on the assumption that all inclinations are equally possible in each orbit, he says, "If there be a reason for the inclinations being as described, the probability of the event is 1. Consequently it is 1 : ·00000012 (*i. e.* $1:q$) that there was a necessary cause in the formation of the solar system for the inclinations being what they are." The probability of the existence of such a cause is thus expressed by the fraction

$$\frac{1}{1+q}.$$

I at one time thought that this reasoning involved an error very nearly equivalent to that which I have adverted to in the previous remarks. But upon examination it appears that Mr. De Morgan's result is really a limitation of the general formula (1.) obtained by assigning particular values to the constants a and c. For in order to apply that formula to the case considered by Mr. De Morgan, let us assume A to represent the absence of any determining cause of the phænomenon B, viz. of the phænomenon that the sum of the planetary inclinations is less than 92°, then will a represent the *à priori* probability of the absence of a determining cause, and c the probability that on the assumption of its existence the phænomenon B would result. Mr. De Morgan's reasoning then involves the hypothesis that $a=\frac{1}{2}$ and that $c=1$.

Also $p=1-q$. If we make these substitutions in the general value of P, we find

$$P=\frac{\frac{1}{2}}{\frac{1}{2}+\frac{1}{2}q}=\frac{1}{1+q}.$$

There is therefore, I conceive, no error in the *reasoning* adopted; although there may be, as it seems to me (but I state this merely as an opinion), a serious doubt as to the determination of the constant a. We are not, I think, at liberty to assume that it is *à priori* as likely as not that a sufficient ground for a determinate phænomenon should exist in nature. All that we can infer from the general solution is, that unless the existence of such a ground is *à priori* highly improbable, then, after frequent experience of the phænomenon, there exists a high probability in favour of the existence of that ground.

I have not at present the opportunity of making further references; but I think the most just inference from what has been adduced, to be, that while the doctrine objected to has really been put forth, it has not been held uniformly or universally. I would suggest also the consideration, that even the passage quoted from the Edinburgh Review, although certainly conveying the erroneous notion adverted to, might by the omission of the word *therefore* be understood as expressing the result of a train of reasoning similar to that which Mr. De Morgan has adopted. For if we granted in that case Mr. De Morgan's determination of the constants, the numerical result obtained would be extremely near to that which the Reviewer has assigned. It seems to me to be the part of justice, to give to such considerations as these their full share in estimating the opinions which a writer has expressed. While on the one hand we ought to bring every statement into comparison with the standard of what is absolutely true and right, we ought on the other hand to be willing to take into account those possible hypotheses upon which there may be reason to think that an author has proceeded, even though no mention of them be retained in his conclusions.

Upon the whole, I conceive that the following is the true theory of that class of questions which has been under consideration:—

1st. That it is not in any case a question whether a particular phænomenon which has come under our notice is an effect of causation or not, but whether or not it is an effect of some single predominant cause, or simple combination of causes, the consequences of which are in some measure within the reach of our intelligence.

2nd. That upon the supposition of the absence of such cause, or simple combination of causes, certain results appearing to us equally probable, the probability of that definite combination of those results which constitutes the effect observed may be definitely calculated.

3rd. That if the value thus obtained be expressed by p, then the formula (1.) will represent the probability of the existence of such predominant cause or combination of causes. That in that formula we may, following Mr. De Morgan, justly assume $c=1$, but that there appear to be no grounds further than the analogy of Nature for determining a. [The difficulty here is not that we are choosing among causes equally probable, but that we are attempting to assign the *à priori* probability of the existence of a condition of things, or in other words, to compare the probabilities of its presence and its absence. Now this is a question, the conjectural solution of which will vary with our varying knowledge of the constitution of Nature. Unless, however, we have reason to suppose that the value in question is very small, the general formula will still be available for our general guidance, if not for definite numerical evaluation.]

Quitting this problem, I shall now notice two others, of which solutions have been given, that appear to me to be defective in generality from the same cause, viz. the non-recognition of the requisite arbitrary constants.

1st. Given p the probability of an event X, and q the probability of the joint concurrence of the events X and Y: required the probability of the event Y.

The solution of this problem afforded by the general method described in my last letter is

$$\text{Prob. of } Y=q+c(1-p),$$

where c represents the unknown probability, that if the event X does not take place the event Y will take place. Hence it appears that the limiting probabilities of the event Y are q and $1+q-p$. The result is easily verified.

The only published solution of this problem with which I am acquainted is

$$\text{Prob. of } Y=\frac{q}{p},$$

a result which involves the supposition that the events X and Y are independent. This supposition is, however, only legitimate when the distinct probabilities of X and Y are afforded in the data of the question.

Given the probabilities p and q of the two premises of the syllogism,

All Ys are Xs
All Zs are Ys.

Required the probability P of the conclusion

All Zs are Xs.

Here, by the probability p of the premiss all Ys are Xs, is meant the probability that any individual of the class represented by Y, taken at random, is a member of the class Z, and so in the other cases. The resulting probability of the conclusion afforded by the general method is then

$$P = pq + c(1-q),$$

where c is an arbitrary constant expressing the unknown probability, that if the minor premiss is false the conclusion is true. The limiting probabilities of the conclusion are thus

$$pq \text{ and } pq + 1 - q.$$

The only published solution of the above problem with which I am acquainted is $P = pq$, a result which manifestly involves the hypothesis that the conclusion cannot be true on any other grounds than are supplied by the premises.

There are also, I have reason to think, other cases than the above in which definite numerical results have been assigned, either by neglecting the arbitrary constants, or by determining them upon grounds not sufficiently explained. I do not, however, purpose to enter into the further consideration of this subject here, nor do I offer the above remarks with any view to depreciate the eminent labours of those from whose writings my illustrations have been drawn. Indeed the results which I have deduced from the new method might all have been obtained by the principles of the received theory, with this principal difference, that the constants, which with their interpretations are given by the one method, would require to be assumed in the other. While I think it right to make this acknowledgement, I feel it to be just also to say, that it is only to the simpler kind of problems that the remark appears to me to be applicable. Granting even a proper assumption of the arbitrary constants, I do not see how a solution is to be obtained by the received methods when the data are much involved; not to mention those cases in which the number of the data exceeds or falls short of the number of simple events combined in them, and in the solution of which cases nevertheless arbitrary constants may not be required. Restricting our attention to the ordinary theory, it appears to me to be certain that the problems which fall under our notice may be resolved into two great classes; viz. 1st, those in which definite numerical solution is attainable from the data alone, without any determination of arbitrary constants; 2nd, those in which the data do not suffice to this end, but in which we must either introduce arbitrary constants, as has been done in this paper, or implicitly determine them as Mr. De Morgan has done. And I can conceive of nothing as more likely to inspire a rational confidence in the theory of probabilities, than a clear and well-marked distinction between these cases, accompanied by a distinct statement of the grounds upon which, whenever constants are determined, their determination is effected.

The question has been suggested to me by a correspondent*, to whom I am indebted for some valuable remarks, whether the general method described in my last paper involves any fundamentally different idea of probability from that which is commonly accepted. He observes, that the results which I have given are in accordance with the principles of the established theory. As the same question may present itself to other minds, I would remark that the theory of probabilities has, in the view which I have been led to take of it, two distinct but accordant sources. From whichsoever of these it may be derived, it will be found to involve the idea of numerical magnitude; but in the one case that idea will have reference simply to the relative frequency of the occurrence of events, being in fact the received ground of the theory; in the other, to the persistency of certain forms of thought, which are manifested equally in the operations of the science of number, and in the reasonings and discourses of common life. Setting out from either of these grounds, we may, I conceive, without difficulty attain to a knowledge of the other. Now it appears to me to be perfectly in accordance with the nature of probability that this should be the case; for its relation to number is not more essential than its relation to the manner in which events are combined. But while the expression of the former relation belongs to arithmetic, or more generally to algebra, that of the latter belongs to logic.

I design, as soon as leisure and opportunity shall permit, to publish the general theory to which reference has been made in this and the previous paper. Had it been possible for me to offer in the space which they have afforded a satisfactory statement of its principles, I should have gladly availed myself of the opportunity of doing so. But for the particular ends here in view this has been the less necessary to be done, as the results actually exhibited admit of verification by known methods. Still I trust that the collateral discussions into which I have entered have not been altogether without interest or profit, even with reference to established doctrines.

I remain, Gentlemen,

Your obedient Servant,

George Boole.

Lincoln, June 17, 1851.

* W. F. Donkin, M.A., Savilian Professor of Astronomy, Oxford.

XVIII. *On the Moving Force of Heat, and the Laws regarding the Nature of Heat itself which are deducible therefrom.* By R. Clausius.

[Concluded from p. 21.]

CARNOT, as already mentioned, has regarded *the production of work as the equivalent of a mere transmission of heat from a warm body to a cold one, the quantity of heat being thereby undiminished.*

The latter portion of this assumption, that the quantity of heat is undiminished, contradicts our maxim, and must therefore, if the latter be retained, be rejected. The former portion, however, may remain substantially as it is. For although we have no need of a peculiar equivalent for the produced work, after we have assumed as such an actual *consumption* of heat, it is nevertheless possible that the said transmission may take place *contemporaneously* with the consumption, and may likewise stand in a certain definite relation to the produced work. It remains therefore to be investigated whether this assumption, besides being possible, has a sufficient degree of probability to recommend it.

A transmission of heat from a warm body to a cold one certainly takes place in those cases where work is produced by heat, and the condition fulfilled that the body in action is in the same state at the end of the operation as at the commencement. In the processes described above, and represented geometrically in figs. 1 and 3, we have seen that the gas and the evaporating water, while the volume was increasing, received heat from the body A, and during the diminution of the volume yielded up heat to the body B, a certain quantity of heat being thus transmitted from A to B; and this quantity was so great in comparison with that which we assumed to be expended, that, in the infinitely small alterations represented in figs. 2 and 4, the latter was a differential of the second order, while the former was a differential of the first order. In order, however, to bring the transmitted heat into proper relation with the work, one limitation is still necessary. As a transmission of heat may take place by conduction without producing any mechanical effect when a warm body is in contact with a cold one, if we wish to obtain the greatest possible amount of work from the passage of heat between two bodies, say of the temperatures t and τ, the matter must be so arranged that two substances of different temperatures shall never come in contact with each other.

It is this *maximum* of work that must be compared with the transmission of the heat; and we hereby find that it may reasonably be assumed, with Carnot, that the work depends solely upon the quantity of heat transmitted, on the temperatures t and τ of both bodies A and B, and not upon the nature of the substance which transmits it. This maximum has the property, that, by its *consumption*, a quantity of heat may be carried from the cold body B to the warm one A equal to that which passed from A to B during its *production*. We can easily convince ourselves of this by conceiving the processes above described to be conducted in a reverse manner; for example, that in the first case the gas shall be permitted to expand of itself until its temperature is lowered from t to τ, the expansion being then continued in connexion with B; afterwards compressed by itself until its temperature is again t, and the final compression effected in connexion with A. The amount of work expended during the compression will be thus greater than that produced by the expansion, so that on the whole a loss of work will take place exactly equal to the gain which accrued from the former process. Further, the same quantity of heat will be here taken away from the body B as in the former case was imparted to it, and to the body A the same amount will be imparted as by the former proceeding was taken away from it; from which we may infer, both that the quantity of heat formerly consumed is here produced, and also that the quantity which formerly passed from A to B now passes from B to A.

Let us suppose that there are two substances, one of which is able to produce more work by the transmission of a certain amount of heat, or what is the same, that in the performance of a certain work requires a less amount of heat to be carried from A to B than the other; both these substances might be applied alternately; by the first work might be produced according to the process above described, and then the second might be applied to consume this work by a reversal of the process. At the end both bodies would be again in their original state; further, the work expended and the work produced would exactly annul each other, and thus, in agreement with our maxim also, the quantity of heat would neither be increased nor diminished. Only with regard to the *distribution* of the heat would a difference occur, as more heat would be brought from B to A than from A to B, and thus on the whole a transmission from B to A would take place. Hence by repeating both these alternating processes, without expenditure of force or other alteration whatever, any quantity of heat might be transmitted from a *cold* body to a *warm* one; and this contradicts the general deportment of heat, which everywhere exhibits the tendency to annul differences of temperature, and therefore to pass from a *warmer* body to a *colder* one.

From this it would appear that we are *theoretically* justified in retaining the first and really essential portion of the assumption of Carnot, and to apply it as a second maxim in connexion with the former. It will be immediately seen that this procedure receives manifold corroboration from its *consequences*.

This assumption being made, we may regard the maximum work which can be effected by the transmission of a unit of heat from the body A at the temperature t to the body B at the temperature τ, as a function of t and τ. The value of this function must of course be so much smaller the smaller the difference $t-\tau$ is; and must, when the latter becomes infinitely small ($=dt$), pass into the product of dt with a function of t alone. This latter being our case at present, we may represent the work under the form

$$\frac{1}{C} \cdot dt,$$

wherein C denotes a function of t only.

To apply this result to the case of permanent gases, let us once more turn to the process represented by fig. 2. During the first expansion in that case the amount of heat,

$$\left(\frac{dQ}{dv}\right) . dv,$$

passed from A to the gas; and during the first compression, the following portion thereof was yielded to the body B,

$$\left[\left(\frac{dQ}{dv}\right) + \frac{d}{dv}\left(\frac{dQ}{dv}\right)\delta v - \frac{d}{dt}\left(\frac{dQ}{dv}\right)dt\right] d'v,$$

or

$$\left(\frac{dQ}{dv}\right)dv - \left[\frac{d}{dt}\left(\frac{dQ}{dv}\right) - \frac{d}{dv}\left(\frac{dQ}{dt}\right)\right] dv\, dt.$$

The latter quantity is therefore the amount of heat transmitted. As, however, we can neglect the differential of the second order in comparison with that of the first, we retain simply

$$\left(\frac{dQ}{dv}\right)dv.$$

The quantity of work produced at the same time was

$$\frac{R\, dv.dt}{v},$$

and from this we can construct the equation

$$\frac{R\,\dfrac{dv\,.\,dt}{v}}{\left(\dfrac{dQ}{dv}\right)dv} = \frac{1}{C} \cdot dt,$$

or

$$\left(\frac{dQ}{dv}\right) = \frac{R.C}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad (IV.)$$

Let us now make a corresponding application to the process of evaporation represented by fig. 4. The quantity of heat in that case transmitted from A to B was

$$\left(r - \frac{dr}{dt}dt\right)d'm,$$

or

$$rdm - \left(\frac{dr}{dt} + c - h\right)dm\, dt;$$

for which, neglecting the differentials of the second order, we may set simply

$$rdm.$$

The quantity of work thereby produced was

$$(s-\sigma)\frac{dp}{dt}dm\, dt,$$

and hence we obtain the equation

$$\frac{(s-\sigma)\frac{dp}{dt} \cdot dm \,.\, dt}{rdm} = \frac{1}{C} \cdot dt,$$

or

$$r = C.(s-\sigma)\frac{dp}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (V.)$$

These, although not in the same form, are the two analytical expressions of the principle of Carnot as given by Clapeyron. In the case of vapours, the latter adheres to equation (V.), and contents himself with some immediate applications thereof. For gases, on the contrary, he makes equation (IV.) the basis of a further development; and in this development alone does the partial divergence of his result from ours make its appearance.

We will now bring both these equations into connexion with the results furnished by the original maxim, commencing with those which have reference to permanent gases.

Confining ourselves to that deduction which has the maxim alone for basis, that is to equation (II*a*.), the quantity U which stands therein as an arbitrary function of v and t may be more nearly determined by (IV.); the equation thus becomes

$$dQ = \left[B + R\left(\frac{dC}{dt} - A\right)\log v\right]dt + \frac{R.C}{v} \cdot dv, \quad (IIc.)$$

in which B remains as an arbitrary function of t alone.

If, on the contrary, we regard the incidental assumption also as correct, the equation (IV.) will thereby be rendered unnecessary for the nearer determination of (II*a*.), inasmuch as the same object is arrived at in a much more complete manner by equation (9.), which flowed immediately from the combination of the said assumption with the original maxim. The equation (IV.), however, furnishes us with a means of submitting both principles to a reciprocal trial. The equation (9.) was thus expressed,

$$\left(\frac{dQ}{dv}\right)=\frac{R.A(a+t)}{v};$$

and when we compare this with equation (IV.), we find that both of them express the same thing; with this difference only, that one of them expresses it more definitely than the other. In (IV.) the function of the temperature is expressed in a general manner merely, whereas in (9.) we have instead of C the more definite expression $A(a+t)$.

To this surprising coincidence the equation (V.) adds its testimony, and confirms the result that $R(a+t)$ is the true expression for the function C. This equation is used by Clapeyron and Thomson in determining the values of C for single temperatures. The temperatures chosen by Clapeyron were the boiling-points of æther, of alcohol, of water, and of oil of turpentine. He determined by experiment the values of $\frac{dp}{dt}$, s and r, for these fluids at their boiling-points; and setting these values in equation (V.), he obtained for C the numbers contained in the second column of the following table. Thomson, on the contrary, limited himself to the vapour of *water*; but has observed it at various temperatures, and in this way calculated the value of C for every single degree from 0° to 230° Cent. The observations of Regnault had furnished him with a secure basis as regards the quantities $\frac{dp}{dt}$ and r; but for other temperatures than the boiling-point, the value of s is known with less certainty. In this case, therefore, he felt compelled to make an assumption which he himself regarded as only approximately correct, using it merely as a preliminary help until the discovery of more exact data. The assumption was, that the vapour of water at its maximum density follows the law of M. and G. The numbers thus found for the temperatures used by Clapeyron, as reduced to the French standard, are exhibited in the third column of the following table:—

Table I.

1. t in Cent. degrees.	2. C according to Clapeyron.	3. C according to Thomson.
35°5	0·733	0·728
78·8	0·828	0·814
100	0·897	0·855
156·8	0·930	0·952

We see that the values of C found in both cases increase, like those of $A(a+t)$, slowly with the temperature. They bear the same ratio to each other as the numbers of the following series:

$$1;\ 1{\cdot}13;\ 1{\cdot}22;\ 1{\cdot}27;$$
$$1;\ 1{\cdot}12;\ 1{\cdot}17;\ 1{\cdot}31;$$

and when the ratio of the values of $A(a+t)$ (obtained by setting $a=273$) corresponding to the same temperatures are calculated, we obtain

$$1;\ 1{\cdot}14;\ 1{\cdot}21;\ 1{\cdot}39.$$

This series of *relative* values deviates from the former only so far as might be expected from the insecurity of the data from which those are derived: the same will also exhibit itself further on in the determination of the *absolute* value of the constant A.

Such a coincidence of results derived from two entirely different bases cannot be accidental. Rather does it furnish an important corroboration of both, and also of the additional incidental assumption.

Let us now turn again to the application of equations (IV.) and (V.); the former, as regards *permanent gases*, has merely served to substantiate conclusions already known. For *vapours*, however, and for other substances to which the principle of Carnot may be applicable, the said equation furnishes the important advantage, that by it we are justified in substituting everywhere for the function C the definite expression $A(a+t)$.

The equation (V.) changes by this into

$$r=A(a+t).(s-\sigma)\frac{dp}{dt}; \quad . \quad . \quad . \quad . \quad (Va.)$$

we thus obtain for the vapour a simple relation between the temperature at which it is formed, the pressure, the volume, and the latent heat, and can make use of it in drawing still further conclusions.

Were the law of M. and G. true for vapours at their maximum density, we should have

$$ps=R(a+t). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20.)$$

By means of this equation let s be eliminated from (Va.); neglecting the quantity σ, which, when the temperature is not very high, disappears in comparison with s, we obtain

$$\frac{1}{p}\frac{dp}{dt}=\frac{r}{\mathrm{AR}(a+t)^2}.$$

If the second assumption that r is constant be made here, we obtain by integration

$$\log\frac{p}{p_1}=\frac{r(t-100)}{\mathrm{A.R}(a+100)(a+t)},$$

where p_1 denotes the tension of the vapour at 100°. Let

$$t-100=\tau,\quad a+100=\alpha,\text{ and }\frac{r}{\mathrm{AR}(a+100)}=\beta;$$

we have then

$$\log\frac{p}{p_1}=\frac{\beta.\tau}{\alpha+\tau}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21.)$$

This equation cannot of course be strictly correct, because the two assumptions made during its development are not so. As however the latter approximate at least in some measure to the truth, the formula $\frac{\beta.\tau}{\alpha+\tau}$ expresses in a rough manner, so to speak, the route of the quantity $\log\frac{p}{p_1}$; and from this it may be perceived how it is, when the constants α and β are regarded as arbitrary, instead of representing the definite values which their meaning assigns to them, that the above may be used as an empirical formula for the calculation of the tension of vapours, without however considering it, as some have done, to be *completely* true theoretically.

Our next application of equation (Va.) shall be to ascertain how far the vapour of water, concerning which we possess the most numerous data, *diverges in its state of maximum density from the law of M. and G.* This divergence cannot be small, as carbonic acid and sulphurous acid gas, long before they reach their points of condensation, exhibit considerable deviations.

The equation (Va.) can be brought to the following form:

$$\mathrm{A}p(s-\sigma)\frac{a}{a+t}=\frac{ar}{(a+t)^2\frac{1}{p}\frac{dp}{dt}}. \quad . \quad . \quad . \quad (22.)$$

Were the law of M. and G. strictly true, the expression at the left-hand side must be very nearly constant, as the said law would according to (20.) immediately give

$$\mathrm{A}.ps\frac{a}{a+t}=\mathrm{A}.\mathrm{R}a,$$

where instead of s we can, with a near approach to accuracy, set the quantity $s-\sigma$. By a comparison with its true values calculated from the formula at the right-hand side of (22.), this equation becomes peculiarly suited to exhibit every divergence from the law of M. and G. I have carried out this calculation for a series of temperatures, using for r and p the numbers given by Regnault*.

With regard to the *latent heat*, moreover, according to Regnault† the quantity of heat λ necessary to raise a unit of weight of water from 0° to $t°$, and then to evaporate it at this temperature, may be represented with tolerable accuracy by the following formula:

$$\lambda=606{\cdot}5+0{\cdot}305t. \quad . \quad . \quad . \quad . \quad . \quad (23.)$$

In accordance, however, with the meaning of λ, we have

$$\lambda=r+\int_0^t cdt. \quad . \quad . \quad . \quad . \quad . \quad . \quad (23a.)$$

For the quantity c, which is here introduced to express the specific heat of the water, Regnault‡ has given in another investigation the following formula:

$$c=1+0{\cdot}00004.t+0{\cdot}0000009.t^2. \quad . \quad . \quad (23b.)$$

By means of these two equations we obtain from (23.) the following expression for the latent heat:

$$r=606{\cdot}5-0{\cdot}695.t-0{\cdot}00002.t^2-0{\cdot}000000.t^3§. \quad . \quad (24.)$$

Further, with regard to the pressure, Regnault has had recourse to a diagram to obtain the most probable value out of his nume-

* *Mém. de l'Acad. de l'Inst. de France*, vol. xxi. (1847).

† Ibid. Mem. IX.; also Pogg. *Ann.*, vol. lxxviii.

‡ *Mém. de l'Acad. de l'Inst. de France*, Mem. X.

§ In the greater number of his experiments Regnault has observed, not so much the heat which becomes *latent* during evaporation, as that which becomes *sensible* by the precipitation of the vapour. Since, therefore, it has been shown, that if the maxim regarding the equivalence of heat and work be correct, the heat developed by the precipitation of a quantity of vapour is not necessarily equal to that which it had absorbed during evaporation, the question may occur whether such differences may not have occurred in Regnault's experiments also, the given formula for r being thus rendered useless. I believe, however, that a negative may be returned to this question; the matter being so arranged by Regnault, that the precipitation of the vapour took place at the same pressure as its development, that is, nearly under the pressure corresponding to the maximum density of the vapour at the observed temperature; and in this case the same quantity of heat must be produced during condensation as was absorbed by evaporation.

rous experiments. He has constructed curves in which the abscissæ represent the temperature, and the ordinates the pressure p, taken at different intervals from -33° to 230°. From 100° to 230° he has drawn another curve, the ordinates of which represent, not p itself, but the logarithms of p. From this diagram the following values are obtained; these ought to be regarded as the most immediate results of his observations, while the other and more complete tables which the memoir contains are calculated from formulæ, the choice and determination of which depend in the first place upon these values.

Table II.

t in Cent. degrees of the air-thermometer.	p in millimetres.	t in Cent. degrees of the air-thermometer.	p in millimetres, according to the curve of the numbers.	p in millimetres, according to the curve of the logarithms*.
-20°	0·91	110°	1073·7	1073·3
-10	2·08	120	1489·0	1490·7
0	4·60	130	2029·0	2030·5
10	9·16	140	2713·0	2711·5
20	17·39	150	3572·0	3578·5
30	31·55	160	4647·0	4651·6
40	54·91	170	5960·0	5956·7
50	91·98	180	7545·0	7537·0
60	148·79	190	9428·0	9425·4
70	233·09	200	11660·0	11679·0
80	354·64	210	14308·0	14325·0
90	525·45	220	17390·0	17390·0
100	760·00	230	20915·0	20927·0

To carry out the intended calculations from these data, I have first obtained from the table the values of $\frac{1}{p}\cdot\frac{dp}{dt}$ for the temperatures -15°, -5°, 5°, 15°, &c. in the following manner. As the quantity $\frac{1}{p}\cdot\frac{dp}{dt}$ decreases but slowly with the increase of temperature, I have regarded the said decrease for intervals of 10°, that is, from -20° to -10°, from -10° to 0°, &c. as uniform, so that the value due to 25° might be considered as a mean between that of 20° and that of 30°. As $\frac{1}{p}\cdot\frac{dp}{dt}=\frac{d(\log p)}{dt}$, I was by this means enabled to use the following formula:

$$\left(\frac{1}{p}\cdot\frac{dp}{dt}\right)_{25^\circ}=\frac{\log p_{30^\circ}-\log p_{20^\circ}}{10},$$

* This column contains, instead of the logarithms derived immediately from the curve and given by Regnault, the corresponding numbers, so that they may be more readily compared with the values in the column preceding.

or

$$\left(\frac{1}{p}\cdot\frac{dp}{dt}\right)_{25^\circ}=\frac{\log p_{30^\circ}-\log p_{20^\circ}}{10.\mathrm{M}}, \quad . \quad . \quad . \quad (25.)$$

wherein log is the sign of Briggs's logarithms, and M the modulus of his system. With the assistance of these values of $\frac{1}{p}\cdot\frac{dp}{dt}$, and those of r given by equation (24.), as also the value 273 of a, the values assumed by the formula at the right-hand side of (22.) are calculated, and will be found in the second column of the following table. For temperatures above 100°, the two series of numbers given above for p are made use of singly, and the results thus obtained are placed side by side. The signification of the third and fourth columns will be more particularly explained hereafter.

Table III.

1. t in Cent. degrees of the air-thermometer.	$\mathrm{A}p(s-\sigma)\frac{a}{a+t}$. 2. According to the values observed.		$\mathrm{A}p(s-\sigma)\frac{a}{a+t}$. 3. According to equation (27.).	4. Differences.	
−15°	30·61		30·61	0·00	
− 5	29·21		30·54	+1·33	
5	30·93		30·46	−0·47	
15	30·60		30·38	−0·22	
25	30·40		30·30	−0·10	
35	30·23		30·20	−0·03	
45	30·10		30·10	0·00	
55	29·98		30·00	+0·02	
65	29·88		29·88	0·00	
75	29·76		29·76	0·00	
85	29·65		29·63	−0·02	
95	29·49		29·48	−0·01	
105	29·47	29·50	29·33	−0·14	−0·17
115	29·16	29·02	29·17	+0·01	+0·15
125	28·89	28·93	28·99	+0·10	+0·06
135	28·88	29·01	28·80	−0·08	−0·21
145	28·65	28·40	28·60	−0·05	+0·20
155	28·16	28·25	28·38	+0·22	+0·13
165	28·02	28·19	28·14	+0·12	−0·05
175	27·84	27·90	27·89	+0·05	−0·01
185	27·76	27·67	27·62	−0·14	−0·05
195	27·45	27·20	27·33	−0·12	+0·13
205	26·89	26·94	27·02	+0·13	+0·08
215	26·56	26·79	26·68	+0·12	−0·11
225	26·64	26·50	26·32	−0·32	−0·18

We see directly from this table that $\mathrm{A}p(s-\sigma)\frac{a}{a+t}$ is not constant, as it must be if the law of M. and G. were valid, but that it decidedly decreases with the temperature. Between 35° and 90° this decrease is very uniform. Before 35°, particularly in the neighbourhood of 0°, considerable irregularities take place; which, however, are simply explained by the fact, that here the pressure p and its differential quotient $\frac{dp}{dt}$ are very small, and hence the trifling inaccuracies which might attach themselves to the observations can become *comparatively* important. It may be added, further, that the curve by means of which, as mentioned above, the single values of p have been obtained, was not drawn continuously from $-33°$ to 100°, but to save room was broken off at 0°, so that the route of the curve at this point cannot be so accurately determined as within the separate portions above and below 0°. From the manner in which the divergences show themselves in the above table, it would appear that the value assumed for p at 0° is a little too great, as this would cause the values of $Ap(s-\sigma)\frac{a}{a+t}$ to be too small for the temperatures immediately under 0°, and too large for those above it. From 100° upwards the values of this expression do not decrease with the same regularity as between 35° and 95°. They show, however, a *general* correspondence; and particularly when a diagram is made, it is found that the curve, which almost exactly connects the points within these limits, as determined from the numbers contained in the foregoing table, may be carried forward to 230°, the points being at the same time equally distributed on both sides of it.

Taking the entire table into account, the route of this curve may be expressed with tolerable accuracy by the equation

$$Ap(s-\sigma)\frac{a}{a+t}=m-ne^{kt}; \quad . \quad . \quad . \quad (26.)$$

in which e denotes the base of the Napierian logarithms, and m, n, and k are constants. When the latter are determined from the values given by the curve for 45°, 125° and 205°, we obtain

$$m=31\cdot549;\ n=1\cdot0486;\ k=0\cdot007138; \quad . \quad (26a.)$$

and when for the sake of convenience we introduce the logarithms of Briggs, we have

$$\log\left[31\cdot549-Ap(s-\sigma)\frac{a}{a+t}\right]=0\cdot0206+0\cdot003100t. \quad (27.)$$

From this equation the numbers contained in the third column are calculated, and the fourth column contains the differences between these numbers and those contained in the second.

From the data before us we can readily deduce a formula which will enable us more definitely to recognize the manner in which the deportment of the vapour diverges from the law of M. and G. Assuming the correctness of the law, if ps_0 denote the value of ps for 0°, we must set in agreement with (20.),

$$\frac{ps}{ps_0}=\frac{a+t}{a},$$

and would therefore obtain for the differential quotients $\frac{d}{dt}\cdot\left(\frac{ps}{ps_0}\right)$ a constant quantity, that is to say, the known coefficient of expansion $\frac{1}{a}=0\cdot003665$. Instead of this we derive from (26.), when in the place of $s-\sigma$ we set s itself simply, the equation

$$\frac{ps}{ps_0}=\frac{m-n\,.\,e^{kt}}{m-n}\cdot\frac{a+t}{a}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (28.)$$

and from this follows

$$\frac{d}{dt}\left(\frac{ps}{ps_0}\right)=\frac{1}{a}\cdot\frac{m-n[1+k(a+t)]e^{kt}}{m-n}. \quad . \quad . \quad (29.)$$

The differential quotient is therefore not a constant, but a function which decreases with the increase of temperature, and which, when the numbers given by (26*a*.) for m, n and k, are introduced, assumes among others the following values:—

Table IV.

$t.$	$\frac{d}{dt}\left(\frac{ps}{ps_0}\right)$.	$t.$	$\frac{d}{dt}\left(\frac{ps}{ps_0}\right)$.	$t.$	$\frac{d}{dt}\left(\frac{ps}{ps_0}\right)$.
0°	0·00342	70°	0·00307	140°	0·00244
10	0·00338	80	0·00300	150	0·00231
20	0·00334	90	0·00293	160	0·00217
30	0·00329	100	0·00285	170	0·00203
40	0·00325	110	0·00276	180	0·00187
50	0·00319	120	0·00266	190	0·00168
60	0·00314	130	0·00256	200	0·00149

We see from this that the deviations from the law of M. and G. are small at low temperatures; at high temperatures, however, for example at 100° and upwards, they are no longer to be neglected.

It may perhaps at first sight appear strange that the values found for $\frac{d}{dt}\left(\frac{ps}{ps_0}\right)$ are *less* than 0·003665, as it is known that for those gases which deviate most from the law of M. and G., as carbonic acid and sulphurous acid, the coefficient of expansion is not *smaller* but *greater*. The differential quotients before calculated must not however be regarded as expressing *literally* the same thing as the coefficient of expansion, which latter is obtained either by suffering the volume to expand under a *constant pressure,* or by heating a *constant volume,* and then observing the increase of expansive force; but we are here dealing with a third particular case of the general differential quotients $\frac{d}{dt}\left(\frac{ps}{ps_0}\right)$, where the pressure increases with the temperature in the ratio due to the vapour of water which retains its maximum density. To establish a comparison with carbonic acid, the same case must be taken into consideration.

At 108° steam possesses a tension of 1 metre, and at $129\frac{1}{2}°$ a tension of 2 metres. We will therefore inquire how carbonic acid acts when heated to $21\frac{1}{2}°$, and the pressure thus increased from 1 to 2 metres. According to Regnault*, the coefficient of expansion for carbonic acid at a constant pressure of 760 millims. is 0·003710, and at a pressure of 2520 millims. it is 0·003846. For a pressure of 1500 millims. (the mean between 1 metre and 2 metres) we obtain, when we regard the increase of the coefficient of expansion as proportional to the increase of pressure, the value 0·003767. If therefore carbonic acid were heated under this mean pressure from 0 to $21\frac{1}{2}°$, the quantity $\frac{pv}{pv_0}$ would be thus increased from 1 to $1+0·003767\times 21·5 = 1·08099$. Further, it is known from other experiments of Regnault†, that when carbonic acid at a temperature of nearly 0°, and a pressure of 1 metre, is loaded with a pressure of 1·98292 metre, the quantity pv decreases at the same time in the ratio of 1 : 0·99146; according to which, for an increase of pressure from 1 to 2 metres, the ratio of the decrease would be 1 : 0·99131. If now both take place at the same time, the increase of temperature from 0 to $21\frac{1}{2}$, and the increase of pressure from 1 metre to 2 metres, the quantity $\frac{pv}{pv_0}$ must thereby increase very nearly from 1 to $1·08099\times 0·99131 = 1·071596$; and from this we obtain, as the mean value of the differential quotients $\frac{d}{dt}\left(\frac{pv}{pv_0}\right)$,

$$\frac{0·071596}{21·5} = 0·00333.$$

We see, therefore, that for the case under contemplation a value is obtained for carbonic acid also which is less than 0·003665;

* *Mém. de l'Acad.*, vol. xxi. Mem. I. † Ibid. Mem. VI.

and it is less to be wondered at if the same result should occur with the vapour *at its maximum density*.

If, on the contrary, the real coefficient of expansion for the vapour were sought, that is to say, the number which expresses the expansion of a certain quantity of vapour taken at a definite temperature and in a state of maximum density, and heated under a constant pressure, we should certainly obtain a value *greater*, and perhaps *considerably* greater, than 0·003665.

From the equation (26.) the *relative* volumes of a unit weight of steam at its maximum density for the different temperatures, as referred to the volume at a fixed temperature, is readily estimated. To calculate from these the *absolute* volumes with sufficient exactitude, the value of the constant A must be established with greater certainty than is at present the case.

The question now occurs, whether a single volume may not be accurately estimated in some other manner, so as to enable us to infer the absolute values of the remaining volumes from their relative values. Already, indeed, have various attempts been made to determine the specific weight of water vapour; but I believe for the case in hand, where the vapour is at its maximum density, the results are not yet decisive. The numbers usually given, particularly that found by Gay-Lussac, 0·6235, agree pretty well with the theoretic value obtained from the assumption, that two measures of hydrogen and one of oxygen give by their combination two measures of vapour, that is to say, with the value

$$\frac{2\times 0{\cdot}06926+1{\cdot}10563}{2}=0{\cdot}622.$$

These numbers, however, refer to observations made, not at those temperatures where the pressure used was equal to the maximum expansive force, but at higher ones. In this state the vapour might nearly agree with the law of M. and G., and hence may be explained the coincidence of experiment with the theoretic values. To make this, however, the basis from which, by application of the above law, the condition of the vapour at its maximum density might be inferred, would contradict the results before obtained; as in Table IV. it is shown that the divergence at the temperatures to which these determinations refer are too considerable. It is also a fact, that those experiments where the vapour at its maximum density was observed have in most cases given larger numbers; and Regnault* has convinced himself, that even at a temperature a little above 30°, when the vapour was developed *in vacuo*, a satisfactory coincidence was first observed when the tension of the vapour was 0·8 of that which corresponded to the maximum density due to the temperature

* *Ann. de Chim. et de Phys.*, 3 ser. vol. xv. p. 148.

existing at the time; with proportionately greater tension, the numbers were too large. The case, however, is not finally set at rest by these experiments; for, as remarked by Regnault, it is doubtful whether the divergence is due to the too great specific heat of the developed vapour, or to a quantity of water condensed upon the sides of the glass balloon. Other experiments, wherein the vapour was not developed *in vacuo* but saturated a current of air, gave results which were tolerably free* from these irregularities; but neither from these, however important they may be in other respects, can a safe conclusion be drawn as to the deportment of the vapour *in vacuo*.

The following considerations will perhaps serve to fill up to some extent the gap caused by this uncertainty. The table (IV.) shows that the lower the temperature of the vapour at its maximum density, the more nearly it agrees with the law of M. and G.; and hence we must conclude, that the specific weight for low temperatures approaches more nearly the theoretic value than for high ones. If therefore, for example, the value of 0·622 for 0° be assumed to be correct, and the corresponding values d for higher temperatures be calculated from the following equation deduced from (26.),

$$d=0{\cdot}622\,\frac{m-n}{m-ne^{kt}}, \quad \ldots \quad (30.)$$

we shall obtain far more probable values than if we had made use of 0·622 for all temperatures. The following table gives some of these.

Table V.

t.	0°.	50°.	100°.	150°.	200°.
d.	0·622	0·631	0·645	0·666	0·698

Strictly speaking, however, we must proceed still further. In Table III. it is seen that the values of $Ap(s-\sigma)\,\frac{a}{a+t}$, as the temperature decreases, approach a limit which is not attained even by the lowest temperatures in the table; and not until this limit be reached can we really admit the validity of the law of M. and G., or assume the specific weight to be 0·622. The question now occurs, what is this limit? Could we regard the formula (26.) to be true for temperatures under −15° also, it would only be necessary to take that value to which it approaches as an asymptote, $m=31{\cdot}549$, and we could then set in the place

* *Ann. de Chim. et de Phys.*, 3 ser. vol. xv. p. 148.

of (30.) the equation

$$d=0{\cdot}622 \cdot \frac{m}{m-ne^{kt}} \cdot \quad . \quad . \quad . \quad . \quad (31.)$$

From this we should derive for 0° the specific weight 0·643 instead of 0·622, and the other numbers of the above table would have to be increased proportionately. But we are not yet justified in making so wide an application of the formula (26.), as it has been merely derived empirically from the values contained in Table III.; and among these, the values belonging to the lowest temperatures are insecure. We must therefore for the present regard the limit of $A(s-\sigma)\frac{a}{a+t}$ as unknown, and content ourselves with an approximation similar to that furnished by the numbers in the foregoing table; so much however we may conclude, that these numbers are rather too small than too large.

By combining (V*a*.) with the equation (III.), which was immediately derived from the original maxim, we can eliminate $A(s-\sigma)$, and we have remaining

$$\frac{dr}{dt}+c-h=\frac{r}{a+t}. \quad . \quad . \quad . \quad . \quad . \quad (32.)$$

By means of this equation, the quantity h, described above as negative, can be more nearly determined. For c and r let the expressions in (23*b*.) and (24.) be substituted, and for a the number 273; we then obtain

$$h=0{\cdot}305-\frac{606{\cdot}5-0{\cdot}695t-0{\cdot}0000t^2-0{\cdot}0000003t^3}{273+t}; \quad (33.)$$

and from this we derive among others the following values for h:

Table VI.

t.	0°.	50°.	100°.	150°.	200°.
h.	−1·916	−1·465	−1·133	−0·879	−0·676

In a manner similar to that already pursued in the case of water-vapour, the equation (V*a*.) might be applied to the vapours of other fluids, and the results thus obtained compared with each other, as is done in Table I., with the numbers calculated by Clapeyron. We will not, however, enter further upon this application.

We must now endeavour to determine, at least approximately, the numerical value of the constant A, or, what is more useful, the value of the fraction $\frac{1}{A}$; in other words, to determine the *equivalent of work for the unit of heat.*

Pursuing the same course as that of Meyer and Holtzmann, we can in the first place make use of equation (10*a*.) developed for permanent gases. This equation was

$$c' = c + AR;$$

and when for c the equivalent expression $\frac{c'}{k}$ is introduced, we have

$$\frac{1}{A} = \frac{k.R}{(k-1).c'}. \quad . \quad . \quad . \quad . \quad . \quad (34.)$$

For atmospheric air, the number 0·267, as given by De Laroche and Bérard, is generally assumed for c'; and for k, as given by Dulong, 1·421. For the determination of $R = \frac{p_0 v_0}{a + t_0}$, we know that the pressure of one atmosphere (760 millims.) on a square metre amounts to 10333 kils.; and the volume of 1 kil. atmospheric air under the said pressure and at the temperature of the freezing-point is $=0·7733$ cubic metres. From this follows

$$R = \frac{10333 . 0·7733}{273} = 29·26,$$

and hence

$$\frac{1}{A} = \frac{1·421 \times 29·26}{0·421 \times 0·267} = 370;$$

that is to say, by the expenditure of one unit of heat (the quantity which raises 1 kil. of water from 0° to 1°) a weight of 370 kils. can be raised to a height of 1 metre. This value, however, on account of the uncertainty of the numbers 0·267 and 1·421, is deserving of little confidence. Holtzmann gives as the limits between which he is in doubt the numbers 343 and 429.

The equation (V*a*.) developed for *vapours* can be made use of for the same purpose. If we apply it to the vapour of *water*, the foregoing determinations, whose result is expressed in equation (26.), may be used. If, for example, the temperature 100° be chosen, and for p the corresponding pressure of one atmosphere $=10333$ kils. be substituted in the above equation, we obtain

$$\frac{1}{A} = 257.(s-\sigma). \quad . \quad . \quad . \quad . \quad (35.)$$

If it now be assumed with Gay-Lussac that the specific weight of the water-vapour is 0·6235, we obtain $s=1·699$, and hence

$$\frac{1}{A} = 437.$$

Similar results are obtained from the values of C contained in Table I., which Clapeyron and Thomson have calculated from equation (V.). If these be regarded as the values of $A(a+t)$

corresponding to the adjacent temperatures, a series of numbers are obtained for $\frac{1}{A}$, all of which lie between 416 and 462.

It has been mentioned above, that the specific weight of the vapour of water at its maximum density given by Gay-Lussac is probably a little too small, and the same may be said of the specific weights of vapours generally. Hence the value of $\frac{1}{A}$ derived from these must be considered a little too large. If the number 0·645 given in Table V. for the vapour of water, and from which we find $s=1·638$, be assumed, we obtain

$$\frac{1}{A}=421\,;$$

which value is perhaps still too great, though probably not much. As this result is preferable to that obtained from the atmospheric air, we may conclude *that the equivalent of work for the unit of heat is the raising of something over* 400 *kils. to a height of* 1 *metre.*

With this theoretic result, we can compare those obtained by Joule from direct observation. From the heat produced by magneto-electricity he found

$$\frac{1}{A}=460*.$$

From the quantity of heat absorbed by atmospheric air during its expansion,

$$\frac{1}{A}=438\dagger;$$

and as mean of a great number of experiments in which the heat developed by the friction of water, of mercury, and of cast iron was observed,

$$\frac{1}{A}=425\ddagger.$$

The coincidence of these three numbers with each other, notwithstanding the difficulty of the experiments, dispels all doubt as to the correctness of the principle which asserts the equivalence of heat and work; and the agreement of the same with the number 421 corroborates in like manner the truth of Carnot's principle in the form which it assumes when combined with our original maxim.

* Phil. Mag., vol. xxiii. p. 441. The English measure has been reduced to the French standard.

† Ibid. vol. xxvi. p. 381. ‡ Ibid. vol. xxxv. p. 534.

XIX. *A Description of Matlockite, a new Oxychloride of Lead. By* R. P. GREG, *Jun., Esq.**

MR. WRIGHT of Liverpool has recently obtained from the old heaps of the level mine at Cromford, near Matlock, a small number of specimens of the murio-carbonate of lead, or phosgenite of Haidinger; and he has also found a few specimens of another ore of lead, differing in appearance from any of the known salts of that metal.

At my request this mineral has been examined by Dr. Robert Angus Smith of Manchester, and his analysis of it has afforded the following results:—

Chloride of lead	55·177
Oxide of lead	44·300
Moisture	·072
	99·549

The proportions by theory would be—

Chloride of lead	55·46
Oxide of lead	44·53
	99·99

This gives a definite chemical composition of 1 atom of chloride of lead combined with 1 atom of oxide of lead; differing in this respect from Mendipite, in which the proportion of chloride to that of oxide is as 1 to 2.

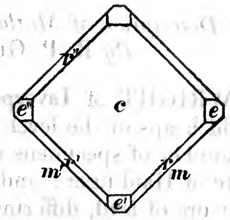
The specific gravity of Matlockite is 7·21, and its hardness 2·5 to 3. Colour yellowish, with sometimes a slight greenish tinge; its lustre is adamantine, and occasionally pearly, and it is transparent and translucent. It cleaves, but not readily, parallel to P. Its fracture is uneven and slightly conchoidal. It decrepitates in the flame of the blowpipe, but with care is reduced to a grayish-yellow globule.

It occurs in tabular crystals, generally thin and superimposed on each other, and occasionally slightly curved; but my friend Mr. W. G. Lettsom has a perfect transparent crystal an inch square and an eighth of an inch thick.

The primitive form is a right square prism; and the following figure, drawn by Professor Miller of Cambridge, who has also corrected the measured angles, represents all the modifications hitherto observed:—

* Communicated by the Author.

mc	=	90° 0′
mm'	=	90 0
ec	=	119 34
ee''	=	59 8
er	=	138 59
ed'	=	104 6
rc	=	111 50
rr''	=	43 41
rr'	=	97 58



Professor Miller adds, "A slice parallel to the plane c, 0·0204 inch thick, being placed in a polarizing instrument having the planes of polarization of the polarizer and analyser at right angles to each other, the angular radius of the first blue ring in air was found to be 22° 81′."

Dr. Smith has also analysed a crystal of the murio-carbonate of lead, and has obtained—

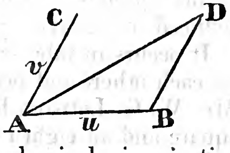
Chloride of lead	51·784
Carbonate of lead	48·215
	99·999

corresponding to 1 atom of chloride combined with 1 atom of carbonate, which agrees with the proportions given by Klaproth and Berzelius.

Norcliffe Hall, near Wilmslow, Cheshire.

XX. *On Symbolical Mechanics. By the Rev.* M. O'Brien, *M.A., Professor of Natural Philosophy and Astronomy, King's College, London, late Fellow of Caius College, Cambridge**.

IN the previous paper I pointed out the distinction between the *geometrical* and *mechanical addition*; the former consisting in the *successive* tracing of lines, the latter in the *simultaneous* action of forces. When + is used in its geometrical sense, $u+v$, or AB+AC, denotes the change of position produced in a tracing point by causing it to describe the lines AB and BD in immediate succession, BD being parallel and equal to AC; but when + has its mechanical signification, U+V denotes the mechanical effect produced by the simultaneous action of the two forces represented by U and V. In this way it is that AB+AC denotes the line AD, while U+V denotes the resultant of U and V.



* Communicated by the Author.

In *ordinary* mechanics, where addition is in all cases merely *numerical*, it is immediately obvious that lines may be assumed to represent forces in magnitude and direction; but whether the same mode of representation can be adopted in *symbolical* mechanics, where + is used in the two different senses just alluded to, is a point to be determined. For, if we suppose AB and AC to represent the forces U and V respectively, AB + AC ought to represent the force U + V; that is, AD ought to represent the resultant of the two forces represented by AB and AC: otherwise lines cannot be assumed as proper representatives of forces. Now this immediately leads us to the *parallelogram of forces*, and shows that the *general* representation of forces by lines *assumes* the truth of that theorem. In fact, the parallelogram of forces is a principle which identifies geometrical and mechanical addition, and shows, that, if the lines u and v represent the forces U and V respectively, in magnitude and direction, then the geometrical sum $u+v$ will also represent the mechanical sum or resultant $\mathrm{U}+\mathrm{V}$. That $u+v$ repesents $\mathrm{U}+\mathrm{V}$ admits of remarkably simple proof by means of the symbolization explained in the former papers, as I shall now briefly show.

Let α and β denote units of length, and A and B units of force parallel respectively to α and β. Let $\mathrm{U}=\mathrm{X}\mathrm{A}$, $\mathrm{V}=\mathrm{Y}\mathrm{B}$, X and Y being the numerical magnitudes of the forces; then, if u and v represent U and V, we must have, $u=\mathrm{X}\alpha$, $v=\mathrm{Y}\beta$. Hence

$$(u+v)(\mathrm{U}+\mathrm{V})=\mathrm{X}^2\alpha\mathrm{A}+\mathrm{XY}(\alpha\mathrm{B}+\beta\mathrm{A})+\mathrm{Y}^2\beta\mathrm{B};$$

but we have shown that $\alpha\mathrm{A}$, $\alpha\mathrm{B}+\beta\mathrm{A}$, and $\beta\mathrm{B}$ are each equal to zero; consequently

$$(u+v)(\mathrm{U}+\mathrm{V})=0,$$

and therefore the force $\mathrm{U}+\mathrm{V}$ is parallel to the line $u+v$; that is, the latter represents the former *in direction*.

Again, let ϵ and E be units of length and force in the common direction of $u+v$ and $\mathrm{U}+\mathrm{V}$, and let $u+v=r\epsilon$, $\mathrm{U}+\mathrm{V}=\mathrm{RE}$, r and R being the magnitudes of $u+v$ and $\mathrm{U}+\mathrm{V}$ respectively. Then we have

$$u\mathrm{U}=(r\epsilon-v)(\mathrm{RE}-\mathrm{V}),$$

or

$$\mathrm{X}^2\alpha\mathrm{A}=r\mathrm{R}\epsilon\mathrm{E}-r\mathrm{Y}\epsilon\mathrm{B}-\mathrm{YR}\beta\mathrm{E}-\mathrm{Y}^2\beta\mathrm{B};$$

but $\alpha\mathrm{A}$, $\epsilon\mathrm{E}$, and $\beta\mathrm{B}$ are each zero, and $\epsilon\mathrm{B}=-\beta\mathrm{E}$; hence

$$0=r\mathrm{Y}-\mathrm{RY}, \text{ or } r=\mathrm{R}.$$

It appears, therefore, that $u+v$ represents $\mathrm{U}+\mathrm{V}$ *in magnitude* as well as in direction.

I shall now always use lines to represent forces, and, therefore, there will no longer be any occasion to distinguish between units of force and units of length. I shall employ the Greek letters α, β, and γ to represent both, and substitute them everywhere in place of A, B and C. Hence, instead of the relations $\alpha A=0$, &c., $\beta A=-\alpha B$, &c., we have the following, viz.

$$\left.\begin{array}{l}\alpha\alpha=\beta\beta=\gamma\gamma=0,\\ \beta\alpha=-\alpha\beta,\quad \gamma\beta=-\beta\gamma,\quad \alpha\gamma=-\gamma\alpha.\end{array}\right\} \quad . \quad . \quad (1.)$$

And generally, supposing U and V to be forces parallel and equal to the lines u and v respectively, I shall substitute the latter for the former. Now, since $vU=-uV$*, this substitution leads to the important result, that

$$vu=-uv; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

that is, the factors in the symbolical product uv may be interchanged if we change the sign of the product.

If u and V be parallel, $uV=0$; hence we have another result of importance, namely, that the condition of parallelism of two lines u and v is

$$uv=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

It will be remembered that uv denotes the effect produced by the translation of the line v *along* the line u, that is, by the parallel removal of the force represented by v from A to B, AB being the line u. It will also be remembered that A and B are supposed to be points *in the same rigid body*, and that uv is, in fact, the couple consisting of the forces $-v$ and v acting at A and B respectively.

All that is here said respecting forces applies equally well to *impressed velocities*.

Units of Translation.—Representation by perpendicular lines.

If θ be the angle between u and v, α and β two units drawn at right angles to each other anywhere in the plane of u and v; and if x and y denote the numerical magnitudes of u and v; then it may be shown, that

$$uv=(xy\sin\theta)\alpha\beta.$$

For, let ϕ and ψ be the angles which u and v respectively make with the direction α; then, by geometrical addition,

$$u=(x\cos\phi)\alpha+(x\sin\phi)\beta$$
$$v=(y\cos\psi)\alpha+(y\sin\psi)\beta.$$

Hence, observing that $\alpha\alpha=\beta\beta=0$, and $\beta\alpha=-\alpha\beta$, we find

$$\begin{aligned}uv&=xy(\cos\phi\sin\psi-\sin\phi\cos\psi)\alpha\beta\\ &=(xy\sin\theta)\alpha\beta.\end{aligned}$$

* For $vU+uV=X^2(\beta\alpha+\alpha\beta)=0$.

I shall call $\alpha\beta$ a *unit of translation* *; consequently the numerical magnitude of uv is to be found by multiplying the numerical magnitude of u by that of v, and by the sine of the angle which v makes with u.

From the result just obtained, it appears that all units of translation in the same plane (or in parallel planes, by former papers) are equivalent to each other; for, if we suppose $x=1$, $y=1$, and $\theta=90^\circ$, uv becomes a unit of translation *anywhere* in the same plane as the unit $\alpha\beta$: since, therefore, these suppositions reduce the equation just obtained to $uv=\alpha\beta$, it follows that all units of translation in the same plane are equivalent to each other. The method employed in statics of representing couples by their axes, suggests a similar sort of representation here; I shall therefore assume a unit of length drawn at right angles to the plane of α and β to represent the unit of translation $\alpha\beta$, which it will properly do, since it completely defines $\alpha\beta$ as regards magnitude and plane of translation; and this is all that need be defined.

Let γ be the unit of length thus drawn; then I shall put γ for $\alpha\beta$, or $\alpha\beta$ for γ, as the case may require, in any investigation.

Since $\beta\alpha=-\alpha\beta$, it follows that $\beta\alpha$ is represented by $-\gamma$. To determine generally the direction of the unit of length which represents a unit of translation, I shall adopt the following rule, viz. Conceive a man to be so placed that his head is in the direction of the translated line (β) and his feet in the opposite direction, and let him turn round till the direction of translation (α) points to the right; then I shall assume the direction in which he looks to be that of the unit (γ) which represents the translation $\alpha\beta$. According to this rule, it is easy to see that $\beta\alpha$ is represented by $-\gamma$; and generally, supposing α, β, γ to be any three units of length at right angles to each other, we have the following equations, viz.

$$\left.\begin{array}{lll}\alpha\beta=\gamma, & \beta\gamma=\alpha, & \gamma\alpha=\beta, \\ \beta\alpha=-\gamma, & \gamma\beta=-\alpha, & \alpha\gamma=-\beta.\end{array}\right\} \quad . \quad . \quad (4.)$$

The equivalence here implied may be called *equivalence of symbolical definition*; it simply implies that the symbols equated define the same thing, and may therefore be substituted for each other in any symbolical equations.

It has been shown that $uv=(xy\sin\theta)\alpha\beta$; whence it follows, that

$$uv=(xy\sin\theta)\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

Now $xy\sin\theta$ is the area of the parallelogram formed upon u

* The translation of a unit along a perpendicular unit may properly be called a *Unit of Translation*.

and v as sides, and γ is at right angles to the plane of that parallelogram; hence it follows, that *the line which symbolically represents* uv *is equal numerically to the area of the parallelogram* uv, *and is drawn at right angles to its plane.*

These principles are sufficient to enable me to apply the symbolical system here proposed to a variety of cases of considerable importance. Among others the following may be mentioned as interesting, because of its connexion with the problem of the pendulum as a means of exhibiting the earth's rotation. It is as follows.

If we calculate the motion of a particle relatively to the earth, forgetting to allow for the earth's rotation, we may completely correct the error by supposing the accelerating force,

$$-\omega\left(2\frac{du}{dt}+\omega u\right),$$

to act on the particle; ω denoting a line equal numerically to the earth's angular velocity and parallel to the polar axis, and u the distance of the particle from the earth's centre; $\frac{du}{dt}$ being taken on the supposition that the earth is fixed. This is the true *centrifugal force*; $-\omega(\omega u)$ represents the ordinary statical centrifugal force in magnitude and direction, and the additional term $-2\omega\frac{du}{dt}$ arises from the motion of the particle relatively to the earth.

By means of this result the true equations of motion of a pendulum are obtained with great facility; they are as follows:—

$$\frac{d^2x}{dt^2}+\frac{g}{a}x=2n\sin\lambda\frac{dy}{dt}$$

$$\frac{d^2y}{dt^2}+\frac{g}{a}y=-2n\sin\lambda\frac{dx}{dt}.$$

Here x and y are the coordinates of the projection of the vibrating particle on the horizontal plane referred to two horizontal axes, one of which always lies in the meridian plane. n denotes the earth's angular velocity, and λ the latitude.

It is obvious from these equations, that the effect produced by the earth's rotation on the pendulum is proportional in every respect to $\sin\lambda$.

[To be continued.]

XXI. *On the Anticlinal Line of the London and Hampshire Basins.* By P. J. MARTIN, *Esq.*, *F.G.S.*

[Continued from p. 51.]

Anticlinal Line of the Vale of Greenhurst.

ALTHOUGH a description of this line of elevation forms a conspicuous feature in my former disquisition on the Weald*, in resuming this part of my subject I may be supposed to be invading a province which Mr. Hopkins has in some measure made his own, by the labour he has lately bestowed on it, with which, however, I was till very recently unacquainted. Of the result of Mr. Hopkins's investigation into the construction of the body of the Weald I am entirely ignorant. I have long been familiar with most of its phænomena; and have, since the publication of my former essays, been waiting for a favourable opportunity of publishing the results of my observations in the shape of a "History of the Weald Denudation." But as I am not yet prepared to fill up all the details of such a history, I am better pleased that an exposition of the construction of the Weald and the Boulonnais should come from the author of a "Theory of Elevation."

On looking at the escarpment of the South Downs in No. 9 of the Ordnance Map, it will be observed that there is a remarkable recession of that escarpment southward, between the salient angles of Duncton Hill in the west of Sussex, and Wolfstanbury in the east. In a line parallel with this receded chalk, and at an average distance of about a mile, lie the Weald-clay valleys of *Greenhurst* and *Henfield*—"valleys of elevation,"—with their anticlinal escarpments of lower greensand, and their synclinal reduplications of the same, with the occasional addition of a trough of galt; the whole occupying a length and breadth of country of about eighteen miles by from one to two.

Greenhurst lies on the road between Storrington and Thakeham. Mary Hill on the south and Jacquet's Hill on the north form its scarped anticlinal boundaries of lower greensand, showing a dip which varies from 30 to 60 degrees. Here the synclinal line runs in the valley in which Abbingsworth House and Champion's Farm are situate; and the beds which dip north at Jacquet's Hill rise north again in the hollow way to Thakeham. East of Greenhurst the northern escarpment breaks off at the high grounds of Warminghurst, and the valley opens into the great expanse of the Weald. The anticlinal line is then carried on in a Weald-clay saddle through Ashington, Guesses' Farm and Horsebridge Common. It next crosses the Adur, and is

* Memoir on Western Sussex, &c.

again bordered by anticlinal scarps of greensand, at New Hall on the south and Henfield on the north. The synclinal disposition is well-characterized north of Henfield. From Henfield, or from the Adur, the line is carried on by a broad Weald-clay valley as far as Homebush, where the escarpments of lower greensand again become confluent. A saddle of galt, another of malm rock, and then the chalk of Poynings and Wolfstanbury succeed, and appear to preclude all further progress to this upheaval. Indeed I formerly thought that the line either terminated here, or ran out through the chalk at Saddlescombe. But when Mr. Lyell published his figure of the upheaved chalk at Southerham near Lewes, with his speculations thereon*, I saw distinctly that the longitudinal fissure of Greenhurst did enter the chalk, and running out again eastward from Lewes, the probability was that it would be found in its place, and be again instrumental in carrying back the chalk southward, as it had done west of Wolfstanbury.

These suspicions have since been verified by observation. The line of elevation as it enters the chalk is the Valley of Piecomb, Pangdean, and the north side of Stanmere Park. As it approaches the Ouse below Lewes, a deep denudation marks its further progress, bounded by the strongly-marked chalk escarpment south-west of Lewes, over Falmer, Kingstone and Iford. Crossing the Ouse, the quarry in the northern escarpment of this denudation, before spoken of, at Southerham corner†, presents a northerly dip of 30 or 40 degrees. The southern escarpment becomes the line of the South Downs as they are carried on towards East Bourne. Under Mount Caburn, and about a quarter of a mile further east, the line is carried on in the lower or gray chalk, which is there quarried. To this succeeds a saddle of upper greensand stretching over from Glynde to Firle; then another of galt; and then obscure indications of the outcrop of the lower greensand. In this part of Sussex it is well known that the sand in question thins out, or is in some way so lost to view, that it might be passed over in a cursory survey of the country, were it not known to be still certainly present‡. Its course appears to be this: it crops out in the usual order at

* Principles of Geology. First edition.
† So called at Lewes.
‡ Mantell's Geology of Sussex and Geology of S.E. of England.
I strongly suspect that the obscure outcrop of the greensands in this part of our island, or rather their immersion, is mainly caused by the operation of this line of fissure. These beds form so prominent a feature in all the country west of Lewes, and appear again in such force on the opposite side of the Channel, in the Boulonnais, that it can hardly be supposed that their obscurity in this intermediate space is the effect of a proper thinning out.

Ringmer north of Lewes, at Ripe it is synclinal, and at Selmston and Berwick Common anticlinal. Here it appears to be lost; for a broad saddle of Weald clay succeeds, and its anticlinal and synclinal arrangement produces several miles of expansion from Swineshill Gate through Hailsham and Hellingly, where the first Wealden sand crops out.

The country becomes now so flat, that I have not been able to determine the exact part of the coast at which the anticlinal line runs out. But the sharp dip of the chalk by the roadside, and in the chalk-pit at Willingdon, shows that it is not far off; and as still more satisfactory evidence, a fine swell of the upper greensand is seen rising out of its synclinal line from under the Bourne Level in the cliff at Sea Houses, near East Bourne, and waving away beautifully southward to sink under the chalk towards Beachey Head*.

We must now return to the west of Sussex. From Greenhurst westward, still following the course of the Downs, the disruption throws back in succession the three members of the lower greensand, forming an irregular but well-defined country of each, with its anticlinal and synclinal lines. A trough of galt is exhibited in the latter of these at Wiggonholt, Hardham, Watersfield and Tripp Hill†. The line then passes through a saddle of galt between the last-mentioned place and the Bury New Woods, and then, bearing north-westerly, and bringing the Chalk Downs with it, it points towards Midhurst. From Duncton and Lavington Commons it enters by Selham into the course of the Rother (the western branch of the Avon), leaves the galt behind it, and by its synclinal line projects the argillaceous beds of the lower greensand northward as far as Lodsworth and the south part of Cowdry Park‡. Then taking its course north of Midhurst, still following the backward course of the Rother, it emerges from it and appears in great force at Woolbeding. In a hollow way a few hundred yards north of Woolbeding Farm, a good section is to be seen of its central and sharpest upshot; but to gain a just notion of its importance here, it is necessary to traverse the hollow ways round about this locality, and observe the escarpments and tilted sections of the sandhills at Midhurst and Trotton. At this most northerly part of its course, at Woolbeding, it enters the lowest or Fuller's earth beds of the lower

* I am not sure that this wave of the upper greensand has not been noticed somewhere by Dr. Fitton, although I do not find it in his "Strata below the Chalk," Geol. Trans. *loc. cit.*

† This trough of galt is five miles long, and from a quarter to half a mile wide.

‡ There is much obscurity in this part of the line, and but for its reappearance at Woolbeding it might be supposed to be worn out.

greensand country, and runs so hard up to the great central fissure of the Weald, as to conspire with it in producing, and is indeed, in this way, the cause of the exposure of the Weald clay in Hartingcombe—that projection into the Wolmar Valley, west of Hindhead and Blackdown, spoken of in the former part of this memoir. The transverse fissures between Telegraph Hill or Holder Hill, Stubs Hill and Vining Common, produce prolongations of that exposure of the Weald clay quite down into the synclinal line, and indeed almost into the anticlinal at Iping*.

Although in its further progress westward the Greenhurst line has still an elevation of its own in a saddle of sand-hills, through Chithurst, Trotton and Rogate, it is very much incorporated in its upheaving effect, and thus acts in conjunction with the central anticlinal line; or, in other words, its synclinal reduplication is immediately lost in the superior heave of that line towards Haslemere. And it is a curious fact, that the chalk, taking advantage, as it were, of this aberration northward, losing the ordinary dip of the South Down range, pushes its escarpment forward in a broad high talus at South Harting.

From Rogate westward no satisfactory section can be obtained; and in its passage on the north side of the town of Petersfield, little is seen of this line but a broken saddle of sand-hills till it strikes the Malm Rock or upper greensand at Langrish†.

The anticlinal disposition is to be seen in an imperfect section on both sides of the East Meon road at Langrish; and to an unpractised observer it would seem insignificant, if the general aspect of a broken chalk saddle in the country west of it had not caused it to be sought for as a nucleus of elevation.

Although trifling of itself, the sweep of the rocks north and south away from this point marks its character; and on proceeding up the valley on the road to Winchester, a chalk-pit in the gray chalk shows the northerly dip and westward strike of

* I am not sure that there is not another and minor contortion of the Weald group projected into this curious offset of the Weald-clay valley. It shows itself in the river bank at Baybridge near Knepp Castle. I have detected it again in the escarpment of the second Wealden sand course at Andrews Hill south of Billingshurst, and again in a sand-pit at Ebernoe or Eberknoll, west of Kirdford; all in a direct line, and pointing toward the gorge of the valley in question, between Blackdown and Bexley Hill. But at all these exposures the wave of elevation is very slight. These minor or local disturbances will be afterwards considered as belonging to the lesser contortions, or puckerings of the Wealden strata.

† Subsequent observations incline me to believe that this fault or upcast at Langrish is not a continuation of the Greenhurst line; and that there is no true inosculation of that anticlinal with the Winchester line, *unless by the Meon Valley*. It is probable that, with the upcast at Woolbeding, and the flexure at Midhurst mentioned by Mr. Hopkins (Geol. Trans., vol. vii. p. 16), the influence of this line of elevation on the escarpment of the South Downs ceases.—P. J. M. May 1851.

that side of the saddle. This little pass is the entrance of the long valley of Bramdean, which is in the synclinal line of the upheaval. The character of this valley is sufficiently well marked, and it corresponds very curiously with that on the south side of the Peasemarsh anticlinal line west of Alton, on the road to Lassam. For several miles the bottom of the Bramdean Valley is covered by a thick bed of washed but angular flint. In its progress westward it soon shows signs of moisture, and a tributary of the Itchin rises in it and runs by Titchborne northward.

On the south side of the saddle runs the denudation of East and West Meon, till it is closed in by the Beacon and Kilmeston Downs. Bierly, Old Down, Kilmeston, Hinton Ampner, are in the anticlinal line. From Hinton the saddle spreads wider, and rises into greater importance; and the northern synclinal line falls back into the course of the Itchin, from Alresford to Winchester. The elevation increases now in a series of heights to Easton High Down, where the saddle bursts suddenly open to form the anticlinal denudation of Chilcomb, at the north-west corner of which Winchester is situated, and where it is intersected by the Itchin. St. Giles' and St. Catherine's Hills are anticlinal. The same disposition might have been observed in the railway cutting when it was fresh, west of the city. The upper chalk becomes confluent again at Cromwell's Battery. From this point the same high and saddle-shaped elevation is continued on in Pitt and Farley Downs to the Test. In this part of its course the line is accompanied on its northern side by a continuation of the synclinal valley which carries the Itchin from Alresford*, till it is lost at Kings Sombourne in the Valley of the Test. Viewed from the country north of Winchester, all this line of elevation gives the idea of a "chalk-bladder," especially as the denudation of Chilcomb is not there visible. Crossing the Test, the line of elevation seems to be taken up again by the Broughton Hills at Bossington. But such a labyrinth of hill and dale succeeds, owing to the deep denudations (many of them water-courses) which occupy the country north-east of Salisbury, that in the absence of sections I have not attempted to follow it further. If it has not died out, and if it still continue its usual westerly course, it points directly to, and perhaps unites with, the Warminster anticlinal line. Where it becomes obscure on the banks of the Test, it passes by the Wardour line, as that line sinks under the tertiary beds at Timsbury near Michelmarsh.

* A stricter examination of this valley would probably prove it to be a trough occupied by tertiary deposit. There is a patch of this kind on the northern slope of Easton Down, and the agricultural character of the country of "the Worthies" (villages so called), and other parishes in the line of the Itchin, favour the supposition.

Anticlinal Line of Warminster.

This line enters south of Warminster, and heaves the galt and upper greensand into a dome-like elevation denuded of its chalk, except the remarkable outlier of Cley Hill, which rests on the north side of it. The line then runs through Crockerton and Sutton in a ridge of greensand, as far east as Cortington. At this point the chalk boundaries approximate so much, that little else is to be seen but the alluvium of the Wiley, which takes its course in the line of the rent towards Salisbury. The anticlinal disposition of the valley in its progress eastward is now little more than presumptive; but the presumption is of the strongest kind. For although no decisive evidence is to be obtained by section, the scarped aspect of the Downs on each side of the valley, and its undeviating course a little to the south of east, as far as Wishford, leave little doubt of its true character. From this last-mentioned place the further course of this line appears to be along the valley of Stoford Bottom, the river-course taking off south towards the Nadder at Wilton*. Stoford Bottom points directly toward the Broughton Hills, and the declining line of the Greenhurst and Winchester denudations. But I doubt much if a more minute search than I have been able to make could produce satisfactory proof of the inosculation of these two lines, as before stated.

Central Line of Elevation.

It remains now to say something of the central line of the Weald and the Wolmar Valley.

Although its broad expanse and superior importance are very much enhanced by the reduplication into it of the synclinal returns of the lateral lines, it no doubt brings with itself many subordinate contortions of powerful agency†. Assisted by these, although they no longer make their appearance on the surface, it heaves the upper greensand between the synclinal valleys of Bramdean and Alton into a broken but distinctly arched escarpment, forming at least half the elevation of this western boundary of the Wolmar Valley. These beds, and in some places the chalk in the rear of them, afford many opportunities of observing the tilting or sudden upward deflection of the truncated edges

* Another instance of drainage transversely to the line of elevation.

† These minor flexures or puckerings, as they may be called, often running up into sharp anticlinal faults, are frequently met with in the greensand and Wealden districts. Of this kind is the flexure in Greyshot Down, mentioned by Dr. Fitton, p. 147 of his memoir on the Strata below the Chalk. The effect of these flexures, like that of the greater anticlinal and synclinal lines, is always to retard more or less the outcrop of the strata in which they occur.

of upheaved strata, which characterizes the entrance of a saddle, or the extremity of an escarpment of any notable elevation, where the materials are not of the most friable nature. This disposition, with the gradual decline of force in the line itself, give a strike to the beds that throws all the water, except a little surface drainage from the chalk marl country, westward towards Alresford and the Candover Valley. By this test, rather than by any sections or surface arrangement, we are able to judge of the prevailing dip.

The highest points of elevation on the central line are in a nameless ridge a little north-east of Var Down, and the high grounds about East Fisted and Bentworth, overlooking the synclinal line south of Alton. The importance of this line of elevation is maintained even beyond the valley of transverse drainage in which the three Candover villages are situate (the synclinal, most probably, of the Alton range), and a long succession of waving hills and high plains (of which the engravings of the Ordnance Map give no adequate conception) carry it on between the longitudinal valleys of the Itchin on the south, and the Mitcheldever River on the north towards the Test, where its presence is marked by the prominent features of the Stockbridge Common Down and Longstock Hills. In the middle of this course, about midway between Mitcheldever and Worthy Down, it is cut through by the second tunnel of the Southampton Railway at an elevation of 350 feet.

Westward of the Test, the progress and full effect of this line of elevation becomes very obscure. The central hills which bound the remarkable transverse valley* that strikes across, and forms with the Valley of the Wiltshire Avon the natural limits of Salisbury Plains, lie in its course. Beacon Hill near Amesbury is the culminating point of these high grounds; and in all probability the strong central line of the Weald denudation is continued onwards to assist in the support of the high platform of Salisbury Plains.

The arrangement of the Wiltshire country west of the natural boundary of the Avon is much more simple than that which has formed the principal subject of the foregoing paper. By the joint operation of the Pewsey and Warminster lines, assisted by the faded influence of those projected from the Weald, this country seems to be maintained almost in horizontal equilibrium; the superior energy of the first mentioned giving to the whole a southerly bearing, as indicated by the drainage.

The northern limb of the Wardour elevation tilting the south

* See in the Ordnance Map the valley in which the names of Collingbourne, Kingston, North and South Tidworth, Newton Toney, and Winterbourne-gunner occur.

side of the long chalk triangle bounded by the Wiley and Nadder, and the southern limb of the Warminster anticlinal line doing the same for the north side, a synclinal trough is formed in which I suspect there are relics of tertiary beds.

Of the valleys themselves, a stricter examination would probably show that groups of fissures, puckerings, or subordinate contortions, accompany the great central upheaval. That these phænomena exist in greater force in the Weald Valley is most probably owing to the great thickness of the Wealden formation. If a section of the whole Wealden in this great exposure could be obtained, it would exhibit the appearances we observe in some ancient schistose formations, and which obtain most probably in argillaceous deposits of all ages (except the most recent) that afford the *requisite thickness.*

Of the epoch of these parallel lines of disruption, and of their contemporaneity, I propose to treat when the phænomena of transverse fissure, drainage, lacerated escarpments, drift, and other circumstances bearing on the subject of denudation come to be considered. For the present it is sufficient to observe, that the presence of tertiary beds of the Eocene period in great force in some parts of the synclinal lines here reviewed, as, for instance, in that of Salisbury, and the protrusion of the chalk through them in the anticlinal, bespeak a date posterior to the æra of those deposits. Of this, stronger evidence is yet to be adduced from districts of similar structure not now under review; as, for instance, in the case of Portsdown and its synclinal line of the Forest of Bere*. Indeed the proposition with which I started, the contemporaneity of the acts of elevation and denudation, necessarily implies an epoch posterior to all the regularly stratified beds of these districts.

I have already pointed out the obvious connexion of the long line of the Greenhurst elevation with the escarpment of the South Downs. And I may briefly call attention to the variable force of these longitudinal fractures in the different parts of their course. This is a matter of great importance as bearing on the subject of transverse fissure. I have already spoken of it in the Pewsey line. At one point it scarcely heaves the tertiary beds; at another it is carried on in the chalk; and in a third it brings up the greensand on a level with the plastic clay. In the Peasemarsh line we have seen that the Weald clay rises, allowing something for denudation, within a few feet of the chalk marl at Alton, and a transverse fissure shows itself at the point

* See also Dr. Buckland's memoir on "Valleys of Elevation," Geol. Trans., vol. ii. 2nd series, p. 125.

of greatest tension at Guildford. Then in the Greenhurst line, we see that where it is exhibited in greatest force, and its features are best displayed almost in the axis of the Weald, it brings up a long line of Weald-clay exposure from Greenhurst to Poynings. And at this part of its course two notable lines of transverse fissure show themselves*. It heaves the galt in a saddle at Waltham Park and the New Woods, and throws it back into a synclinal at Hardham and Wiggonholt. At Bramdean it is a chalk saddle, and at Winchester an open anticlinal valley of the lower chalk. All this necessarily implies change of strike, and of angle of inclination, which cannot take place beyond a certain point without transverse fracture at the surface.

Having now cleared the way for a recognition of the strict relations of the chalk dome of Hampshire and Wiltshire with the Weald denudation, I hope to be able in my next communication to proceed to the subject of transverse fissure and the phænomena of drainage; recapitulating and carrying forward my former disquisitions on the simultaneous and tumultuous operations of upheaval and aqueous abrasion.

[To be continued.]

XXII. *On the Apsidal Motion of a freely suspended Pendulum. By the Rev.* JOSEPH A. GALBRAITH *and the Rev.* SAMUEL HAUGHTON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following investigation of the apsidal motion of a freely suspended pendulum may be interesting to those of your readers who have been engaged in verifying M. Foucault's experimental demonstration of the earth's rotation. Some time ago we undertook a course of experiments with that view; and although we arrived as a general result at a complete verification of this remarkable experiment, we found considerable deviations from the law of uniform angular motion. This led us to consider the different disturbing forces, and if possible calculate their effects, and thus eliminate them from our observations.

The motion of a pendulum may be compared with that of a point moving in a plane round a centre of force, whose intensity is directly as the distance, if the amplitude of vibration be *indefinitely small*; but if this be not the case, we must consider the motion as taking place in a spherical ellipse and disturbed by a small force directed from the centre, and varying as the third power of the distance. As the influence of this disturbing force

* Viz. the river course of the Adur, and the Vale of Findon, the line of the Worthing road.

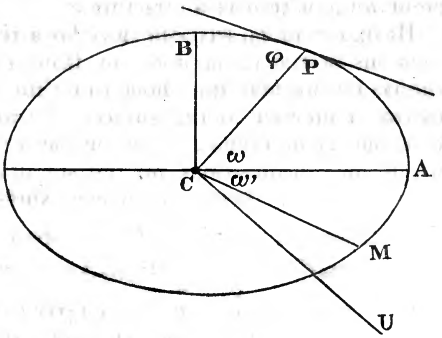
is very considerable in modifying the angular motion, we send you a complete investigation of its effects, requesting the favour of its insertion in your valuable Journal.

We remain, Gentlemen,

Yours, &c.,

JOSEPH A. GALBRAITH,
SAMUEL HAUGHTON.

Trinity College, Dublin,
July 14, 1851.



If a point P move on the surface of a sphere under the influence of a force F, which acts in the tangent to the great circle joining P with a fixed point C on the sphere, it will describe a spherical ellipse round C as centre; if the force F act from P towards C, and be equal to $g \tan r \sec^2 r$, r being the angle at the centre subtended by the arc CP, and g the accelerating force of gravity*.

Let ω be the angle which CP makes with the axis, m the angle which the semidiameter CM, conjugate to CP, subtends at the centre, ω' the angle which CM makes with the axis: let also a and b be the semiaxes major and minor, α and β the tangents of the angles which they subtend at the centre, v the velocity of P, and p the perpendicular arc, drawn from C to the tangent. The following fundamental equations connect the motion of P with the elements of the ellipse:

$$\left.\begin{aligned} F &= g \tan r(1+\tan^2 r) \\ \sin^2 r\,d\omega &= \sqrt{g}\,\alpha\beta\,dt \\ v &= \frac{\sqrt{g}\,\alpha\beta}{\sin p} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Let a small force R acting in the direction of the tangent to CP at P, and outwards, at each instant disturb this elliptic motion, we may still suppose the point P to move in an ellipse, the mag-

* This elegant theorem is due to Professor Graves, who communicated it, together with some others connected with the motion of a point on a sphere, to the Royal Irish Academy, January 24, 1842.

nitude and direction of whose axes are continually varying. In order to see how R produces these effects, suppose it resolved into two components, $R\cos\phi$ and $R\sin\phi$: the former, acting along the tangent, either accelerates or retards the elliptic velocity; whilst the latter, acting in the direction of the normal, increases or diminishes the angle ϕ which the tangent makes with the radius vector. These variations of ϕ and v are the immediate effects of the disturbing force. The momentary variations of the elements of the orbit depend on and may therefore be expressed in terms of them, so that by the application of the rules of the integral calculus we can calculate the total variation of these elements produced in a given time.

To fix our ideas, suppose that P is in the first quadrant moving from the apse A towards B, the effect of the tangential component is to diminish the velocity in the ellipse, and that of the normal component to increase the angle which the tangent makes with the radius vector; these variations are expressed as follows:

$$\delta v = -R\cos\phi dt \quad \delta\phi = \frac{R\sin\phi dt}{v}. \quad . \quad . \quad (2.)$$

From these and equations (1.), we obtain

$$\left.\begin{aligned}\delta v &= -\frac{R\cos\phi\sin^2 r d\omega}{\sqrt{g}\,\alpha\beta}\\ \delta\phi &= \frac{R\sin^2\phi\sin^3 r d\omega}{g\alpha^2\beta^2}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

It may be easily shown that

$$\tan^2 r\sin 2\omega = \tan^2 m\sin 2\omega'. \quad . \quad . \quad . \quad . \quad (4.)$$

Differentiate this, considering r constant, and eliminating $\delta\omega'$ by means of the relation

$$\cos r\tan\phi = \tan(\omega+\omega'), \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

we obtain after some reduction

$$\left.\begin{aligned}(\alpha^2-\beta^2)\delta\omega &= \sin 2\omega'\tan m\delta\tan m\\ &+ \cos 2\omega'\tan^2 m\frac{\cos^2(\omega+\omega')}{\cos^2\phi}\cos r\delta\phi\end{aligned}\right\} \quad . \quad . \quad (6.)$$

It may be easily proved from equations (1.) that

$$\tan^2 m = \frac{v^2}{g}\cos^2 p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

By means of this and equations (3.), we may eliminate $\delta\tan m$ and $\delta\phi$ from (6.), and obtain, finally,

$$(\alpha^2-\beta^2)\delta\varpi = \frac{R}{g}\tan r d\omega\frac{\sin(\omega'-\omega)}{\sin(\omega'+\omega)}\cos^2 r, \quad . \quad . \quad (8.)$$

in which ϖ is the longitude of the apse measured from a fixed line CU.

From this equation we can obtain the motion of the apse which results from any radial disturbing force. In order to apply it to the case of the elliptic vibration of a freely suspended pendulum, we suppose the pendulum to be a point moving on a sphere, and urged towards its lowest point by a force tangential to the surface $=g\sin r=\frac{g\tan r}{\sqrt{1+\tan^2 r}}$. We may therefore suppose that the motion takes place in a *moveable* spherical ellipse, the disturbing force being radial, acting outwards from the centre, and equal to the difference of $g\tan r(1+\tan^2 r)$ and $\frac{g\tan r}{\sqrt{1+\tan^2 r}}$, or to $\frac{3}{2}g\tan^3 r$, if the arc r be so small that all powers higher than the third may be neglected. Substituting, therefore, for $\frac{\mathrm{R}}{g}$ its value $\frac{3}{2}\tan^3 r$, and for $\cos^2 r$ its approximate value unity, we obtain

$$(\alpha^2-\beta^2)\delta\varpi=\frac{3}{2}\tan^4 r\frac{\sin(\omega'-\omega)}{\sin(\omega'+\omega)}d\omega. \quad . \quad . \quad . \qquad (9.)$$

Let ψ be an angle which satisfies the equations

$$\cos\omega=\frac{\alpha\cos\psi}{\tan r},$$

$$\sin\omega=\frac{\beta\sin\psi}{\tan r};$$

and therefore

$$\cos\omega'=\frac{\alpha\sin\psi}{\tan m},$$

$$\sin\omega'=\frac{\beta\cos\psi}{\tan m}.$$

The last equation may be reduced to

$$\delta\varpi=\frac{3\alpha\beta}{8}\left(1+2\frac{\alpha^2+\beta^2}{\alpha^2-\beta^2}\cos 2\psi+\cos 4\psi\right)d\psi. \quad . \qquad (10.)$$

Let $\Delta\varpi$ be the total variation in the value of ϖ, while the angle ψ, which varies uniformly with the time, increases from cipher to any finite value, we have by integrating equation (10.), and substituting for α and β their approximate values $\frac{a}{l}$ and $\frac{b}{l}$, l being the length of the pendulum,

$$\Delta\varpi=\frac{3ab}{8l^2}\left(\psi+\frac{a^2+b^2}{a^2-b^2}\sin 2\psi+\frac{1}{4}\sin 4\psi\right). \quad . \qquad (11.)$$

As the last two terms of this expression are periodic, it is evident that the progression of the apse during one complete vibration of the pendulum is equal to

$$\frac{3\pi ab}{4l^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

and that for any other period it is equal to

$$\frac{3}{4} \times \frac{\text{area described by central radius vector}}{(\text{length of pendulum})^2} \quad . \quad . \quad (13.)$$

Let N be the number of degrees described in one hour, then

$$N = \frac{135 \times 1800}{\pi} \sqrt{\frac{g}{l}} \frac{ab}{l^2}, \quad . \quad . \quad . \quad (14.)$$

In this equation, g, l, a, b are supposed to be expressed in feet.

The length of the pendulum used in our experiments was 35·4 feet; consequently, assuming gravity to be 32·19 feet, equation (14.) will become for the pendulum used by us

$$N = 58{\cdot}86 \times ab; \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

At the commencement of the experiments, $a=24$ inches, $b=0$; at the end of first hour, $a=13$ inches, $b=\cdot 134$ inch.

The above figures are taken from ten experiments. Taking the means of the semiaxes at the beginning and end of the hour, and converting them into feet, we obtain $ab=\cdot 009$ square feet. Hence

$$N = 0^\circ{\cdot}53. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

The progression of the apse is consequently a little more than half a degree in the first hour, and of course in the succeeding hours should be considerably less in consequence of the small value of the product ab.

The observed deviation from 12° per hour (due to the rotation of the earth, at the latitude of Dublin,) in the ten experiments is contained in the following table:—

Right-handed motion.	Left-handed motion.
0·60	0·85
1·10	1·00
0·00	1·00
0·20	0·50
2·00	
0·50	
0·73 mean.	0·83 mean.

A comparison of the foregoing table with (16.) shows, that although apsidal motion, of the kind here considered, accounts for the greater part of the observed deviation, it leaves about three-tenths unaccounted for.

In the account of Mr. Bunt's experiments, contained in the June Number of the Magazine, the observed deviation of the apse for every tenth of an inch of semiaxis minor is given for the pendulum used by him, which was 53 feet in length.

The observed deviation per tenth of inch semiaxis minor is 0·7 of a degree per hour.

On calculating (14.) for this pendulum, we find

$$N=21{\cdot}46\times ab. \quad . \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

Substituting in this expression the values 3·5 feet and ·1 inch for a and b (as stated in Mr. Bunt's communication), we obtain

$$N=0^{\circ}{\cdot}626. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

In this case the formula agrees more nearly with experiment.

In Mr. Bunt's communication in the last Number of the Magazine, he states as the result of a new set of experiments, that he found the correction for $\frac{1}{10}$th of an inch ellipticity or semiaxis minor, in a mean arc of about three feet, to be $0^{\circ}{\cdot}43$ per hour. If we apply equation (17.) to this case, the result is $0^{\circ}{\cdot}27$, which differs much more from observation than Mr. Bunt's former determination of the correction. From all this it would appear, that other causes beside the apsidal motion here considered operate in disturbing the angular motion due to the earth's rotation.

XXIII. *Reply to a Note from* Mr. W. Thomson *on the Effect of Fluid Friction, &c., which appears in the June Number of the Philosophical Magazine. By* R. Clausius*.

THE above-mentioned note of Mr. Thomson refers to an investigation of mine on the deportment of steam during its expansion under various circumstances†; and it is stated in the note, that although I determine the *work executed* by the steam issuing from a vessel, I have overlooked the *mechanical effect*, which consists in the circumstance that a certain velocity must be imparted to the steam, and which must be measured by the *vis viva* of this motion, and that on this account my objections against his reasoning are *groundless*. I believe, however, that I shall be able fully to establish the views which I have expressed.

The above mechanical effect was both known to me and taken into account; and it is only with regard to the force which causes the velocity of the steam at a small distance behind the

* Communicated by the Author.

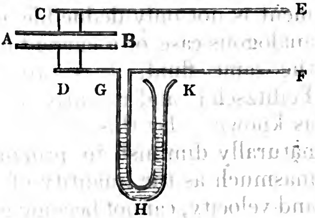
† Pogg. *Ann.*, vol. lxxxii. p. 263; and Phil. Mag. 4th ser. vol. i. p. 398.

orifice to be much less than in the orifice itself, and which therefore destroys the greater part of the said effect, that our views differ from each other.

Mr. Thomson explains this fact by referring it to "the friction of the steam as it rushes through the orifice*." In order rightly to estimate the possible influence exerted by this friction, its action must in the first place be more clearly characterized. This action does not consist in the loss of a velocity which the steam had already attained, but in the circumstance that the steam from the commencement never attains the velocity which it would have done had friction been entirely absent. That velocity, on the contrary, which it possesses in the orifice, and which it loses *further on*, cannot be lost by friction. It is in no way difficult to demonstrate the actual ground of this loss.

For this purpose we will retain the assumption, which, for the sake of simplifying the matter, I have introduced in my investigation, that the orifice is furnished with a widening neck (see fig. vol. i. p. 403†), and that up to the point where we suppose the loss of velocity to have already taken place, the steam has remained unmixed with atmospheric air.

What takes place in the neck may be represented somewhat more clearly by means of an apparatus of the shape shown in the figure. AB is a narrow tube fastened by means of a closely-fitting cork in the wider tube CDEF, which latter is furnished with a siphon-shaped tube partially filled with fluid, by means of which the pressure within may be observed. If we blow through the narrow tube from A towards B, so that the current of air can expand itself in the wide tube before it reaches the open atmosphere, it is well known that the fluid immediately rises in the leg HG and sinks in HK.



In the vicinity of B a smaller pressure exists than that of the atmosphere which acts at K and EF. It is this difference of pressure which destroys so much of the original velocity of the current of air on its way from B to EF, that the same quantity of air which passed the orifice B during the unit of time can during the unit of time fill the cross section EF. Let it be imagined that a current of steam from a high-pressure boiler passes through the tube AB instead of the current of air, we have then in the interior of the wide tube a retarding force,

* Phil. Mag., vol. xxxvii. p. 388.

† In this figure, as already stated in the errata to Poggendorff's *Annalen*, the surface GHI must be moved somewhat further from the orifice towards the centre of the vessel.

which is evidently independent of the small friction which there takes place.

Inasmuch as the steam between B and EF loses the greater part of the *vis viva* which it possessed at B, a quantity of heat equal to that formerly expended in the production of this portion of the *vis viva* must be again produced; this portion of the *vis viva* does not therefore at all enter into the calculation of the entire quantity of heat consumed up to EF; so that, without *overlooking it,* I might leave it altogether unmentioned.

We must now consider those cases where the orifice is unfurnished with either a widening neck or a tube such as we have described, but where the orifice opens immediately into the atmosphere. And here we will choose an extreme case; that is, where the orifice is at a tube-end which opens into the free atmosphere, and the issuing steam has not only the air in front of it, but is encompassed by it *on all sides*.

The current immediately after its exit sets a large mass of air in motion; not only the air before the opening, but also that behind the same. For inasmuch as a portion of the air before the opening is carried forward with the steam, a decrease of density takes place in the immediate neighbourhood of the orifice, and thus the air behind will be drawn forwards. This deportment is not only deducible on theoretic principles, but for the analogous case of a current of water streaming into a reservoir of the same fluid, it is proved experimentally by Venturi*, Von Feilitzsch†, and recently by Magnus‡, so that I may assume it as known. By this communication of motion the velocity must naturally diminish in proportion as the mass moved increases; inasmuch as the quantity of motion, that is, the product of mass and velocity, cannot become greater. But if *this* product remains constant, the product of the mass and the *square* of the velocity, that is, *the vis viva,* must become smaller; and we must assume that the *vis viva* which thus disappears again makes its appearance as heat.

All other possible cases with respect to the position of the orifice, for example, that generally considered where the orifice is situate in a wide plate, so that the steam after its issue has the air all around it in front and the plate behind, lie between those two already considered. These form, so to say, the two limits; and by comparison with these we can always obtain an approximate idea of the phænomena under consideration. If it be even granted that in all other cases the *vis viva* lost by the

* *Recherches Expérimentales sur le Principe de la Communication Latérale du Mouvement dans les Fluides.*

† Pogg. *Ann.*, vol. lxiii. p. 216.

‡ Pogg. *Ann.*, vol. lxxx. p. 1; and Phil. Mag. 4th ser. vol. i. p. 1.

steam is not *so completely* compensated by heat as in the first case, I could notwithstanding affirm in my investigation, in which it was not my design specially to discuss all incidental circumstances, but which was directed to the establishment of a principle, that the widening neck was not necessary to the validity of the reasoning.

I believe I have thus justified the views to which I have given utterance.

With regard to the friction of the steam as it rushes through the orifice, I have arrived at the conclusion that it is *not necessary* to the explanation of the fact adduced by Mr. Thomson. At the same time, its action, which, according to my view, would be the reverse of that imagined by Mr. Thomson, and must be introduced as a *loss of heat* into the calculation, is by no means excluded. In the case of a very *small* orifice, its influence may be even considerable. If, however, in such cases as the issuing of the steam through the safety-valve of a high-pressure engine I have regarded it as playing a less important part than that attributed to it by Mr. Thomson, this opinion will not be considered as *groundless* by those who understand the subject.

XXIV. *On a certain Fundamental Theorem of Determinants. By* J. J. Sylvester, *M.A., Dub.**

THE subjoined theorem, which is one susceptible of great extension and generalization, appears to me, and indeed from use and acquaintance (it having been long in my possession) I know to be so important and fundamental, as to induce me to extract it from a mass of memoranda on the same subject; and as an act of duty to my fellow-labourers in the theory of determinants, more or less forestall time (the sure discoverer of all truth) by placing it without further delay on record in the pages of this Magazine. Its developments and applications must be reserved for a more convenient occasion, when the interest in the New Algebra (for such, truly, it is the office of the theory of determinants to establish), and the number of its disciples in this country, shall have received its destined augmentation. In a recent letter to me, M. Hermite well alludes to the theory of determinants as "That vast theory, transcendental in point of difficulty, elementary in regard to its being the basis of researches in the higher arithmetic and in analytical geometry."

The theorem is as follows:—Suppose that there are two determinants of the ordinary kind, each expressed by a square array of terms made up of n lines and n columns, so that in each

* Communicated by the Author.

square there are n^2 terms. Now let n be broken up in any given manner into two parts p and q, so that $p+q=n$. Let 1°, one of the two given squares be divided in a given *definite* manner into two parts, one containing p of the n given lines, and the other part q of the same; and 2°, let the other of the two given squares be divided *in every possible way* into two parts, consisting of q and p lines respectively, so that on tacking on the part containing q lines of the second square to the part containing p lines of the first square, and the part containing p lines of the second square to the part containing q of the first, we get back a new couple of squares, each denoting a determinant different from the two given determinants; the number of such new couples will evidently be

$$\frac{n.(n-1)\ldots(n-p+1)}{1.2 \quad \ldots \quad p};$$

and my theorem is, that *the product of the given couple of determinants is equal to the sum of the products (affected with the proper algebraical sign) of each of the new couples formed as above described.* Analytically the theorem may be stated as follows.

Let

$$\left\{\begin{matrix} a_1 & a_2 \ldots a_n \\ b_1 & b_2 \ldots b_n \end{matrix}\right\} \quad \left\{\begin{matrix} a_1 & a_2 \ldots a_n \\ \beta_1 & \beta_2 \ldots \beta_n \end{matrix}\right\},$$

according to the notation heretofore employed by me in the preceding Numbers of this Magazine, denote any two common determinants, each of the nth order, and let the numbers $\theta_1, \theta_2 \ldots \theta_n$ be disjunctively equal to the numbers $1, 2, \ldots n$ and $p+q=n$; then will

$$\left\{\begin{matrix} a_1 & a_2 \ldots a_n \\ b_1 & b_2 \ldots b_n \end{matrix}\right\} \times \left\{\begin{matrix} a_1 & a_2 \ldots a_n \\ \beta_1 & \beta_2 \ldots \beta_n \end{matrix}\right\}$$

$$=\Sigma \pm \left\{\begin{matrix} a_1 \; a_2 & \ldots & a_n \\ b_1 \; b_2 \ldots b_p & \beta\theta_{p+1} & \beta\theta_{p+2} \ldots \beta\theta_n \end{matrix}\right\} \times \left\{\begin{matrix} a_1 \quad a_2 & \ldots & a_n \\ \beta\theta_1 \; \beta\theta_2 \ldots \beta\theta_p & b_{p+1} & b_{p+2} \ldots b_n \end{matrix}\right\}$$

The general term under the sign of summation may be represented by aid of the disjunctive equations

$$\phi_1 \quad \phi_2 \ldots \phi_n = 1, \quad 2, \ldots n$$
$$\psi_1 \quad \psi_2 \ldots \psi_n = 1, \quad 2, \ldots n,$$

under the form of

$$(a_{\phi_1}.b_1 \times a_{\phi_2}.b_2 \times \ldots a_{\phi_p} b_p)(a_{\psi_{p+1}}.b_{p+1} \times a_{\psi_{p+2}}.b_{p+2} \times \ldots a_{\psi_n}.b_n)$$

$$\times (a_{\phi_{p+1}}.\beta\theta_{p+1} \times a_{\phi p+2}.\beta\theta_{p+2} \times \ldots a_{\phi_n}.\beta\theta_n)(a_{\psi_1}.\beta\theta_1 \times a_{\psi_2}.\beta\theta_2 \times \ldots \times a_{\psi_p})\beta\theta$$

1st. When $\phi_1 \; \phi_2 \ldots \phi_r = \psi_1 \; \psi_2 \ldots \psi_r$, it will readily be seen, that for given values of $\phi_1, \phi_2 \ldots \phi_r$, the product of the third and fourth factors becomes *substantially* identical with the general term of the determinant

$$\left\{\begin{matrix} a_1 & a_2 \ldots a_n \\ \beta_1 & \beta_2 \ldots \beta_n \end{matrix}\right\};$$

and consequently, making the system $\phi_1, \phi_2 \ldots \phi_p$ (or, which is the same thing, its equivalent $\psi_1, \psi_2 \ldots \psi_r$) go through all its values, we get back for the sum of the terms corresponding to the equation $\phi_1\,\phi_2 \ldots \phi_p = \psi_1\,\psi_2 \ldots \psi_p$, the product of the determinant

$$\left\{\begin{matrix} a_1 & a_2 \ldots & a_n \\ b_1 & b_2 \ldots & b_n \end{matrix}\right\} \text{ and } \left\{\begin{matrix} a_1 & a_2 \ldots a_n \\ \beta_1 & \beta_2 \ldots \beta_n \end{matrix}\right\}.$$

2nd. When we have not the equality above supposed between the ϕ's and the ψ's, let

$$\phi_{p-h} = \psi_{p+k} \text{ and } \phi_{p+\eta} = \psi_{p-\zeta};$$

the corresponding term included under the Σ will contain the factor

$$a_{\phi_{p+\eta}} \cdot \beta_{\theta_{p+\eta}} \times a_{\psi_{p+\eta}} \cdot \beta_{\theta_{p-\zeta}}.$$

Now leaving $\phi_1, \phi_2 \ldots \phi_p$, and $\psi_1, \psi_2 \ldots \psi_p$ unaltered, we may take a system of values $\theta_1', \theta_2' \ldots \theta_n'$, such that

$$\theta'_{p+\eta} = \theta_{p-\zeta},$$

and

$$\theta'_{p-\zeta} = \theta_{p+\eta},$$

and for all other values of q except $p+\eta$, or $p-\zeta$, $\theta'_q = \theta_q$.

The corresponding new value of the general term so formed by the substitution of the θ' for the θ series, will be identical with that of the term first spoken of, but will have the contrary algebraical sign, because the θ' arrangement of the figures $1, 2, 3 \ldots p$ is deducible by a single interchange from the θ arrangement of the same, the rule for the imposition of the algebraical sign plus or minus being understood to be, that the term in which

$$\beta_{\theta_{p+1}}\ \beta_{\theta_{p+2}} \ldots \beta_{\theta_n};\quad \beta_{\theta_1}\ \beta_{\theta_2} \ldots \beta_{\theta_p}$$

enter into the symbolical forms of the respective derived couples of determinants, has the same sign as, or the contrary sign to, that in which

$$\beta_{\theta'_{p+1}}\ \beta_{\theta'_{p+2}}\ \beta_{\theta'_n},\quad \beta_{\theta'_1}\ \beta_{\theta'_2} \ldots \beta_{\theta'_p}$$

so enter, according as an odd or an even number of interchanges is required to transform the arrangement

$$\theta_{p+1}\ \theta_{p+2} \ldots \theta_n,\quad \theta_1\ \theta_2 \ldots \theta_p$$

into the arrangement

$$\theta'_{p+1}\ \theta'_{p+2} \dots \theta'_{n},\quad \theta'_1\ \theta'_2 \dots \theta'_p.$$

[In applying the theorem thus analytically formulized, it is of course to be understood that, under the sign Σ, *permutations* within the separate parts of a given arrangement,

$$\theta_{p+1}\ \theta_{p+2} \dots \theta_{p+n},\quad \theta_1\ \theta_2 \dots \theta_p,$$

are inadmissible, the total number of terms so included being restricted to $\frac{n.(n-1) \dots (n-p+1)}{1.2 \quad \dots \quad p}$].

I have therefore shown that all the terms arising from the expansion of the products included under the sign of summation, for which the disjunctive identity $\phi_1\ \phi_2 \dots \phi_p = \psi_1\ \psi_2 \dots \psi_p$ does not exist, enter into the final sum in pairs, equal in quantity and differing in sign, which consequently mutually destroy, and that the terms for which the said identity does exist together make up the sum

$$\begin{Bmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \end{Bmatrix} \times \begin{Bmatrix} \alpha_1 & \alpha_2 & \dots & \alpha_n \\ \beta_1 & \beta_2 & \dots & \beta_n \end{Bmatrix};$$

which proves, upon first principles drawn direct from that notion of polar dichotomy of permutation systems which rests at the bottom of the whole theory of the subject, the fundamental, and, as I believe, perfectly new theorem, which it is the object of this communication to establish.

The theorem may be extended so as to become a theorem for the expansion of the product of any number of determinants, and adapted so as to take in that far more general class of functions known to Mr. Cayley and myself under the new name of commutants, of which determinants present only a particular, and that the most limited instance.

26 Lincoln's-Inn-Fields,
July 22, 1851.

XXV. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

April 11, 1851. — ON the Measurements of Azimuths on a Spheroid. By Lieut. A. R. Clarke, R.E.

The author commences his paper with the following words:—

"It is generally assumed in geodetical calculations, that the sum of the reciprocal azimuths of two stations on a spheroid is the same as if the stations were on a sphere and had the same latitudes and difference of longitude. This is based on Dalby's geometrical proof, that the difference between the two sums in question is very small if the stations be equally elevated above the surface. It is not, however (nor can be geometrically), shown that this difference is not greater than the probable error of observation, and therefore it may be useful to find an expression for this small difference in terms of the latitudes and longitudes of the stations, in order to see whether it may be in any case greater than the probable errors of observation, and large enough to be worth taking into account."

The author then investigates by accurate formulæ of analytical geometry, as applied to the co-ordinates of points which satisfy the spheroidal equation, the expressions for the tangents of the angles of reciprocal azimuths of two stations, and forms the accurate expression for the tangent of the sum of azimuths, and for the tangent of the excess of this sum above the sum of corresponding spherical azimuths. The expression is then cautiously reduced, and it is found, at length, that the value of this excess is insensibly small; amounting only to $0''{\cdot}000003 \times m^2 n$, where m is the number of degrees in the distances of the stations, and n the number of degrees in the difference of latitude. Then the influence of difference of heights is computed; and it is shown that, though (in cases which may arise in practice) it is greater than what has just been found, yet that it also will be insensible.

At the close of the meeting, Mr. De Morgan made some remarks upon the Gregorian Calendar, as an instrument for determining the moon's phases with sufficient accuracy to settle the question of *moonlight*. Having been led to examine it in this point of view, for the purposes of a collection of almanacs which he is preparing for publication (and which has since been published), he found that it may be made to give the day of new moon or of full moon right in three cases out of five, and with an error of only one day in almost all the other cases; the error of two days occurring only about once in 120 results. In order to obtain this amount of accuracy, the rule is:—Use the Gregorian epact to determine full moons, and that epact increased by 1 to determine new moons; both with the well-known epact-table which appears in all extensive works or articles on the calendar.

The reason of this rule is as follows:—Clavius constructed the Gregorian Calendar expressly in such manner that the moon of his calendar should be always, as well as it could be managed, one day younger than the moon of the heavens; the object being, that the fourteenth day, by which Easter is determined, should follow the day on which the Jews keep the Passover. And as this was done with good success, it follows that one day added to the age of the calendar moon at the beginning of the year (that is, to the Gregorian epact), gives the same degree of success to the calendar, as a means of determining the day of astronomical new moon.

If the chronological full moon had been correctly laid down, this same addition of 1 would have been equally successful as to the full moon. But the *chronological* full moon is on the *fifteenth* day of the moon. Now, half a lunation being, on the average, $14\frac{3}{4}$ days, it follows that, unless the mean new moon happen in the first quarter of its day, the mean full moon is on the *sixteenth* day; so that, in the long run, the sixteenth is the proper day three times out of four. Hence there is no occasion to increase the epact by 1, in order to determine the astronomical full moon; which is as correctly determined as the calendar will do it, by applying the existing epact to the existing hypothesis of the fifteenth day.

The preceding conclusions as to the probability of truth and error were obtained from the nineteen years 1828–1846; the following are the results for 1851, 1852, and 1853:—

New Moon.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1851	2	1	2+	1·30+	30	28+	28	26	25	24	23	22
1852	21	19+	21−	19	19	17	17	15	14−	13	12−	11
1853	10−	8	10−	8	8	6	6	4+	3	2	1·30	30

Full Moon.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1851	17	16	17	16−	15	14−	13	12−	10	10	8	8
1852	6+	5	6	5−	4−	3−	2−	1−30−	29−	28−	27−	26
1853	25	23	25	23	23−	21	21−	19−	18−	17	16−	15

Here are exhibited the days of new and full moon by the calendar: when + or — follows the date, the real day is the day after or the day before. And though in this period of three years the errors of the full moon much exceed in number those of the new moon, there is no such excess in the long run. The nineteen years 1828–1846 gave 140 cases of new moon true to the day, and 141 cases of full moon.

May 9.—On the Vibration of a Free Pendulum in an Oval differing little from a Straight Line. By G. B. Airy, Esq., Astronomer Royal.

"In a paper communicated to this Society several years since, and printed in the eleventh volume of their Memoirs, I investigated the motion of a pendulum in the case in which it describes an oval differing little from a circle; and I showed that, if the investigation is limited to the first power of ellipticity, and if α is the mean value of the angle made by the pendulum rod with the vertical, then the proportion of the time occupied in passing from one distant apse to the next distant apse, to the mean time of a revolution, is the proportion of 1 to the square root of $4-3\sin^2\alpha$. When α is small, this proportion is nearly the same as the proportion of $\frac{1}{2}$ to $1-\frac{3}{8}\sin^2\alpha$; or the time of moving from one distant apse to another distant apse is equal to the time of half a revolution divided by $1-\frac{3}{8}\sin^2\alpha$. This shows that the major axis of the oval is not stationary, but that its line of apses progresses, and that, while the ellipticity is small, the velocity of progress of the apses is sensibly independent of the ellipticity, and may be assigned in finite terms for any value of the mean inclination of the pendulum-rod.

"This theorem, however, fails totally when the minor axis of the oval is small. It is then found that the velocity of progress of the apses is nearly proportional to the minor axis. But, although the movement of the pendulum in this case may be defined to any degree of accuracy by infinite series, it does not appear that it can be expressed in finite terms of any ordinary function of the time. This is to be expected, inasmuch as, when the problem is reduced to its utmost state of simplicity by making the minor axis $=0$, the motion of the pendulum can be expressed only by series. The utmost, therefore, for which we can hope is, to determine the general form of the curve and the rate of progress of its apses, on the supposition that the minor axis is small, in series proceeding by powers of the major axis. This might be so extended as to include higher powers of the minor axis, if it were judged desirable.

"I have thought that an exhibition of the first steps of solution (carried so far as to include the principal multiplier of the first power of the minor axis) might be acceptable to this Society, not purely as a mechanical problem, but more particularly because it bears upon every astronomical or cosmical experiment in which the movement of a pendulum is concerned. The difficulty of starting a free pendulum so as to make it vibrate at first in a plane is extremely great; and every experimenter ought to be prepared to judge how much of the apparent torsion of its plane of vibration is really a progression of apses due to its oval motion."

After a careful analysis of the problem, when the pendulum describes an extremely elongated ellipse, the Astronomer Royal arrives at the following conclusion, which is the principal object of his present investigation. If the length of the pendulum be a, the semi-major axis of the ellipse described by the pendulum-bob be b, and the semi-minor axis be c, then the line of the apses of the ellipse will perform a complete revolution in the time of a complete double vibration (*i. e.* the time of describing the ellipse) multiplied by $\frac{8}{3}\frac{a^2}{bc}$.

"Thus if a pendulum, 52 feet long (which performs its double vibration in 8 seconds), vibrates in an ellipse whose major axis is 52 inches and minor axis 6 inches, the line of apses will perform a complete revolution *from this cause* in 30 hours nearly.

"If a common seconds pendulum (which performs its double vibration in 2 seconds) vibrates in an ellipse whose major axis is 4 inches and minor axis $\frac{1}{13}$ inch, the line of apses will perform a complete revolution *from this cause* in 30 hours nearly.

"The direction of rotation of the line of apses is the same as the direction of revolution in the ellipse.

"It is worthy of remark, that the expression which is thus found for the progression of the apse on the supposition that the minor axis is much smaller than the major, will, if we make in it c very nearly equal to b, correspond exactly to the formula cited in the beginning of this paper, as found by an accurate investigation when the ellipse approaches very near to a circle. It appears, therefore, very probable that, while b is moderately small, the expression for the progression of the apses is true for all values of c up to b.

"Although the principal object of this paper, as mentioned in the beginning, was to point out how far an apparent rotation of the plane of a pendulum's vibration may depend on causes which would exist if the suspension were perfect, and if the point of suspension were unmoved and the direction of gravity invariable, still it may not be uninteresting to point out how an effect, in some respects similar, may be produced by a fault in the suspension. If a pendulum be suspended by a wire passing through a hole in a solid plate of metal, the orifice of that hole may be oval. If the wire be part of a thicker rod tapering to the size of the wire, it may taper unequally on different sides. In either case there will be two planes of vibration, at right angles to each other, in which, if the pendulum is vibrating, it will continue to vibrate, and in one of which the time of vibration is greater, and in the other less, than in any other plane; and, the amplitude of vibration being very small, the complete motion may be found by compounding the vibrations corresponding to these two planes."

After investigating the effect of these causes of error, the Astronomer Royal arrives at the following conclusion:—"It appears, therefore, that the effect of faulty suspension may be sensibly eliminated between two experiments in which the azimuths of the first vibration differ by 45°; and it may be prudent, in making any important experiment, thus to change the commencement-azimuth in successive trials."

ROYAL SOCIETY.

[Continued from p. 80.]

June 19, 1851.—The Earl of Rosse, President, in the Chair.

The following papers were read:—

1. "Researches in Symbolical Physics. On the Translation of a Directed Magnitude as Symbolised by a Product. The Principles of Statics established symbolically." By the Rev. M. O'Brien, M.A., late Fellow of Caius College, Cambridge, and Professor of Natural Philosophy and Astronomy in King's College, London. Communicated by W. A. Miller, M.D., F.R.S. &c. Received April 10, 1851.

In this communication the author (starting from the well-known theorem, that two sides of a triangle are equivalent to the third, when *direction*, as well as magnitude, is taken into account) proposes an elementary step in symbolization which consists in representing the *Translation of a Directed Magnitude* by a *Product*. Any magnitude which is drawn or points in a particular direction, such as a force, a velocity, a displacement, or any of those geometrical or physical quantities which we exhibit on paper by *arrows*, he calls a *directed magnitude*. By the *translation* of such a magnitude he means the removal of it from one position in space to another *without change of direction*.

U representing any directed magnitude and u any distance, the translation of U to any parallel position in space, in such wise that every point or element of U is caused to describe the distance u, is termed the *translation of* U *along* u.

This translation consists generally of two distinct changes, one the *lateral* shifting of the line of direction of U, and the other the motion of U *along* its line of direction. The former is called the *transverse effect*, the latter the *longitudinal effect* of the translation of U along u.

Both these effects are shown to be *products* of U and u; the transverse effect is represented by uU, and the longitudinal by u.U, inserting a dot between the factors in the latter for the sake of distinction.

The author then goes on to apply the principles established to the proof of the *Parallelogram of Forces*, and the determination of the effect of any set of forces on a rigid body. In doing this a remarkable symbolization of the *point of application*, as well as the direction and magnitude of a force, is obtained, namely, that the expression $(1+u)$U represents a force U acting at a distance u from the origin.

The principles of statics are deduced with remarkable facility from the symbolical representation of the translation of a force along a given distance.

2. "On an Air-Engine." By James Prescott Joule, F.R.S. &c. Received May 13, 1851.

The air-engine described in this paper consists of a pump by which air is compressed into a heated receiver; and a cylinder, through which the air passes again into the atmosphere. The difference between the work evolved by the cylinder and that absorbed by the pump, constitutes the work evolved by the engine on the whole. Two tables are given; the first of which contains the pressure, temperature and work absorbed, for various stages of the compression of a given volume of air. The second table gives the theoretical duty of the air-engine described, worked at various pressures and temperatures. The temperature recommended to be adopted in practice is as little below the red heat as possible, which would involve the consumption of only about one-third the amount of fuel consumed by the best steam-engines at present constructed.

3. "Experiments made at York (Lat. 53° 58′ N.) on the Deviation of the Plane of Vibration of a Pendulum from the meridional and other vertical planes." By John Phillips, Esq., F.R.S. Received June 3, 1851.

The following is the author's account of these experiments.

The experiments, of which the following is a notice, were made partly in the north-western Tower of the Minster, and partly in a room of my residence. The latter attempts have only within a few days acquired sufficient method and consistency to deserve reporting; nor have the trials in the Minster been uniformly successful.

Mr. Thomas Cooke, an able optician of York, began the experiments in the Minster. On the 30th of April, Mr. Gray and myself observed the vibration of his pendulum, and found it so accurate as to justify the belief that it might not only indicate the direction, but measure the angular value of the deviation of the pendulum plane from the meridian. Computing this value for an hour to be 12°+, we watched the result and found the arc passed over to be 13°. When this observation was recorded, the pendulum was supposed to have commenced its vibrations on a north and south line; but that was an error; it was really swung from east to west.

In repeating this experiment, I have been more anxious to vary the conditions, in a few arranged observations, than to accumulate many similar results. We have observed in four azimuthal planes; one of our balls weighed eight pounds, the other twenty pounds: one was an oblate, the other a prolate spheroid; suspension was effected at first by thoroughly softened catgut, afterwards by untwisted silk: we have compared small and large arcs, counted the periodical times of vibration in three planes, noted the direction of motion in the elliptic path of the pendulum, and estimated the length of its minor axis. We have recorded results when no ellipticity was remarked, and others in which its injurious effect was manifest.

The pendulum performs one complete vibration in 8″: from which its length is deduced =52·+ feet. The chord of the arc of vibration was usually taken at 14 feet, but was on some trials reduced to 7 feet. The graduated circle was 12 feet in diameter. Great care was used in starting the ball, which did not rotate, but presented the same face to the same quarter of the room, in whatever direction it was swung. The apartment was subject to air currents; the floor from which the suspension was effected though strong was large; and there was no method of securing exact verticality in the iron tube which carried the flexible catgut or silk.

From one or all of these causes it happened that ellipticity in the path was noticed in almost every experiment, and it might exist in all, and be unobserved if the minor axis did not exceed one-fourth of an inch. After abandoning several trials in which the minor axis was observed to increase rapidly, it was thought desirable to determine by experiment the effect of this elliptical swing on the angular movement of the pendulum plane (see exp. 5).

First Set. Deviation observed after given intervals of Time. Ball a prolate spheroid, weighing eight pounds. Suspension softened catgut.

Direction at origin.	Minutes of time.	Deviation.		Chord at origin.	Ellipticity.	
		Left.	Right.		Direction.	Minor axis.
			°	ft.		
1. E. and W....	60	......	13	14	None ob	served.
2. N. and S. ...	45	......	11½	14	None ob	served.

Supposing no ellipticity to have *existed* in these experiments, the results are—

Deviation in one hour from E. and W. line to right 13°·0
„ „ N. and S. „ 15°·3

Second Set. Time observed when deviation amounted to given angles. Ball an oblate spheroid, weighing 20 pounds. Suspension catgut.

Direction at origin.	Minutes of time.	Deviation.		Chord at origin.	Ellipticity.	
		Left.	Right.		Direction.	Minor axis.
			°	ft.		
3. E. and W.	*a.* 15·38	......	3	14	None observed.	
	b. 23·40	......	5	...	None observed.	
4. N. and S.	*a.* 15·0	......	3	14	Very minute.	
	b. 24·44	......	5	...	Left.	½ inch.

Neglecting the ellipticity in these experiments, the results are—
Deviation in one hour from

E. and W. line...... *a.* 11·5, *b.* 12·9 } mean 12°·2+.

N. and S. line ...... *a.* 12·0, *b.* 12·1 } mean 12 ·0+.

For obvious reasons—the force maintaining the pendulum plane being greatest in the beginning of the sweep (when the versed sine of the arc is greatest), and the action of the elliptical swing then least—the deviations in the first portions of the hour appear likely to be more correct than the average of the whole hour.

The suspension was now changed from softened catgut to untwisted silk. From some disarrangement connected with this change it happened that ellipticities were generated in every experiment, and nearly all were abandoned as useless on account of the great dimensions of the ellipse, which, growing as the arc of vibration lessened, sometimes acquired a minor axis of above three inches. Whatever the direction of the movement in the ellipse, its effect was to rotate the pendulum plane in the same direction; thus augmenting the deviation when the motion in the ellipse was to the right, and diminishing it when it was to the left. In most azimuths the elliptical motion was to the *left*. In the following example its effect was followed for an hour to the *right*.

Direction at origin.	Time.	Deviation.		Chord.	Ellipse.		Time to 1° from origin.	Successive times of 3°.	Accelerating effect of ellipse.
		Left.	Right.		Direction.	Minor axis.			
	m s		°			inches.	m. s.	m. s.	m. s.
	0	......	......	14	none.	none.	......	14 6	
5. E. and W.	7 3	......	1½	Not observed.	small.	small.	4 42	11 23	2 43
	11 23	......	3		......	1¼	3 47		
	14 28	......	4½		......	......	3 13		
	16 56	......	5		......	2	3 39	8 35	5 31
	19 58	......	6		......	2½	3 19		
	24 16	......	8		......	3	3 2	7 40	6 26
	27 38	......	9		......	3+	3 4	6 50	7 16
	34 18	......	12	7	......	3¼	2 51	7 50	6 16
	42 8	......	15	Not obs.	......	3¼−	2 48	8 50	5 36
	50 38	......	18		......	3⅛	2 48		
	60 0	......	26½*		......	Not obs.			

* Doubtful.

When this experiment was recorded I had but slight expectation of being able to apply a correction to results which were so largely influenced by elliptical motion. While making it my attention was mainly directed to the rather difficult task of correctly estimating the minor axis of the ellipse (the most important of the elements for determining its rotatory effect), and I only twice recorded the length of the major axis, viz. at its origin, 14 feet, and, after the expiration of rather more than half an hour, 7 feet.

The Astronomer Royal, to whom the experiment in the state here set down was communicated, having kindly furnished me with an appropriate formula, I have resumed the consideration of what had appeared to me an unmanageable result. In this formula $\left(\frac{8}{3}\times\frac{a^2}{bc}=n\right)$ a is the length of the pendulum, b and c the semiaxes of the ellipse, n the number of complete double vibrations of the pendulum during the period of one rotation due to the ellipse. In this case eight such vibrations being performed in one minute, $\frac{a^2}{3bc}$ = minutes of time to one rotation of the ellipse.

The first 12° of deviation were performed in $34^{m}\ 18^{s}$
or at the rate of 360° . . . . in 1029
or 17·15 hours,

the ellipse having its major axis varying from 14 to 7 feet, and its minor axis from 0 to $3\frac{1}{4}$ inches. Taking b and c at arithmetical means of their extreme values (in the case of the major axis this mean is something too great, and in the case of the minor axis something too small), we have

$$\frac{a^2}{3bc}=\frac{52^2}{3\times 5{\cdot}25\times{\cdot}0674}=2547^{m},$$

and

$$\frac{1}{1029}-\frac{1}{2547}=\frac{1}{1726};\ \text{whence}$$

360° are performed by the pendulum without ellipticity in 28·75 hours, and 12°·5 in . . . . 1 hour.

We have thus from the Minster experiments,—

a. Uncorrected for Ellipse.

Exp. 2. 4. On N. and S. line $\frac{15{\cdot}3+12{\cdot}0}{2}=13^{\circ}{\cdot}65$ in one hour.

1. 3. On E. and W. line $\frac{13{\cdot}0+12{\cdot}2}{2}=12^{\circ}{\cdot}60$.

β. Corrected for Ellipse.

Exp. 5. On E. and W. line . . . . 12·50.

Experiments in my House.

After many failures the apparatus became tolerably efficient, with a pendulum of 68·7 inches, as deduced from the vibrations, 22·64 in a minute. The balls used were a prolate spheroid weighing $6\frac{1}{4}$ lbs., and a globe weighing only $1\frac{1}{12}$ lb. I have obtained the best results with the smaller weight. The suspensions have been silk, gutta percha, and various contrivances of points and sockets of agate, brass and steel. The best results have been obtained with gutta percha, and sockets of agate and steel. The arc mostly used was from 16 to 20 inches. The experiments were seldom continued beyond half an hour. By that time the chord of vibration was reduced to about 7 or 8 inches, and the errors of experiment were thought likely to be too great, with so short a radius, if longer continued. The ball, in whatever direction swung, presents the same face to the same side of the room.

Direction at origin.	Time.	Deviation.		Chord.	Ellipse.	
		Left.	Right.		Direction.	Minor axis.
	m			in.		
(6). N.E. and S.W.	0	......	0	18	None.	
	15	......	$3\frac{1}{2}$	....	None observed.	
	30	......	7	....	None observed.	
(7). N.W. and S.E.	0	......	....	18	None.	
	45	......	8	....	None observed.	
(8.). N.E. and S.W.	0	......	....	18	None.	
	15	......	3	....	None observed.	
	30	......	6	4	Left.	$\frac{1}{18}$ inch.
(9). N.W. and S.E.	9	......	....	18	None.	
	15	......	3	....	None observed.	
	30	......	5·55	....	Left.	Minute.

From these experiments uncorrected for ellipse, we have,—

Exp. 6. 8. On N.E. and S.W. line $\frac{14+12}{2}=13^{\circ}\cdot 0$ in one hour.

7. 9. On N.W. and S.E. line $\frac{10\cdot 66+11\cdot 10}{2}=10^{\circ}\cdot 88$.

I have since made a great variety of experiments with this apparatus, which, notwithstanding the theoretical and practical disadvantage of working with so short a pendulum, I hope to render accurately effective, so that the angular deviation of the pendulum-plane may become an ordinary and easy experiment. It should, however, be tried in a glass case, and probably *in vacuo*.

4. "Note on instantaneous Photographic Images." By H. F. Talbot, Esq., F.R.S. &c.

Having recently met with a photographic process of great sensibility, I was desirous of trying whether it were possible to obtain a truly instantaneous representation of an object in motion. The experiment was conducted in the following manner. A printed paper was fixed upon a circular disc, which was then made to revolve on its axis as rapidly as possible. When it had attained its greatest velocity, an electric battery, kindly placed at my disposal by Mr. Faraday, was discharged in front of the disc, lighting it up with a momentary flash. A camera containing a very sensitive plate of glass had been placed in a suitable position, and on opening this after the discharge, an image was found of a portion of the words printed on the paper. They were perfectly well-defined and wholly unaffected by the motion of the disc.

As I am not aware that this experiment has ever succeeded, or indeed been tried, previously, I have thought it incumbent on me to lay an early account of it before the Royal Society.

5. "The Human Iris; its Structure and Physiology." By Bernard E. Brodhurst, M.R.C.S. Communicated by Thomas Bell, Esq., Sec.R.S. &c. Received May 22, 1851.

The author commences by stating that the iris is an active fibro-cellular tissue, or that it may be considered to be a transition tissue from the ordinary fibro-cellular to the organic muscular: that it is a tissue differing from every other in the body; being possessed of a motor power exceeding that of any other tissue, yet differing in construction and appearance of fibre from those other tissues, the types of motion.

He remarks that the microscope shows that the fibres of the iris differ essentially from muscular fibre, whether striped or of organic life: they are pale, easily separable and readily torn; but they resemble in no essential particular muscular fibre; indeed, the effect of galvanism on the iris is totally opposed to that produced on muscular fibre.

He observes that the nerves that pass to the iris are derived from both motor, sensitive and negative nerves; but voluntary motion is not supplied, neither sensation. The motions of the iris are wholly independent of the powers usually deemed motor; they are influenced primarily by the sympathetic system of nerves, through which motion is accorded without sensation, motion without design.

In death, the author observes, the iris assumes a median state, the pupil being neither dilated nor contracted. In health, it is contracted. During sleep it is contracted. During the presence of disease, the pupil is dilated, and so much dilated beyond its usual state, as the tonicity of the vegetative system is removed, as the presence of disease operates on the nutritive system to diminish not only the power of nutrition, but, in a like degree, tension of the visceral system; nutrition and tension being as cause and effect of the healthy operation of this basic system of the animal œconomy. And as it is not essential to the motions of the iris, either to their performance or that they be understood, that they partake of many of those peculiarities, the distinguishing features of muscular tissue, and as we find that this membrane is obedient to those laws which are applicable to each organ under immediate sympathetic influence, and opposed to those phenomena which result from spinal and cerebral influence, it may be asserted that the contractility of the iris is, *primo loco*, the motor power of the sympathetic. For the iris is an irritable membrane with power alone of involuntary motion and tension, its active condition agreeing in these respects with vegetative life in general. And as animal death may be said to ensue when deep sleep takes possession of the senses, when those systems under spinal and cerebral influence are rendered inactive, to be fitted for renewed exertion on waking, it follows, that those organs which still remain active cannot be governed on the same principle, but must necessarily be subject to the sole remaining power, through which is accorded involuntary motion, motion which never tires, and tension its active condition.

The fimbriated edge of the ciliary body floats loosely in the posterior chamber around the lens, to produce, through the to and fro motion of each process (their aggregate number representing a circle), a current forwards or towards the iris. The force of this current is in a ratio to the pupillary opening, being increased as this is contracted, to produce, in proportion to its contraction, convexity of the iris. On the escape of the aqueous humour from the chambers, these processes fall down to form a serrated border upon the lens.

6. "On the Automatic Temperature-compensation of the Force Magnetometers." By C. Brooke, M.B., F.R.S.

After explaining the necessity of automatic temperature-compensation in these instruments in order to give the highest degree of accuracy to results deduced from the ordinates of the magnetic curves, the author infers from a reference to the formula expressing the conditions of equilibrium of the bifilar magnet, that the interval between the lower extremity of the suspension lines will be most advantageously submitted to some mechanical agency governed by change of temperature.

The object in view has been attained by attaching the lower ends of the suspension skein to the adjacent ends of two zinc tubes that are clamped to a glass rod which is attached by its middle point to the middle of the bar-magnet. When the temperature rises, the ends of the skein will evidently be approximated to each other by a quantity that is equal to the difference of expansion of the lengths of zinc and glass intervening between the clamps. The interval between the clamps is to be approximately determined by calculation, and corrected by experiment, so that the ratio of the expansion to the distance between the threads may be equal to the first term of the temperature coefficient.

In the balanced magnetometer the compensation is effected by means of a small thermometer attached to the magnet, the stem of which is parallel to the axis of the bar. In this thermometer, the size of the bulb, its distance from the freezing-point and length of the scale, may be so proportioned to each other, that the second as well as the first term of the temperature coefficient will be represented in the correction.

7. "On the Reproduction of the *Ascaris Mystax*." By Henry Nelson, M.D. Communicated by Allen Thomson, M.D., F.R.S. Received May 22, 1851.

The author commences with a brief anatomical description of the *Ascaris Mystax*, found in the intestinal canal of the Domestic Cat; with more especial reference to the organs of generation in the two sexes. He traces the gradual formation of the semen; originally thrown off as seminal particles by the cæcal extremity of the tubular testicle, the exterior of each solid particle enlarges to constitute a cell, while the interior retains its consistency and forms a nucleus. The cell then acquires a granular protecting envelope, and in this state is introduced into the female. The solution of the protective envelope and the great enlargement of the seminal cell follow, and its nucleus is now seen to present a granular structure. The external granules of the nucleus coalesce to form a membrane, at first exactly resembling a watch-glass in shape, but by the contraction of its margin ultimately forming a curved cæcal tube. This is the true spermatic particle or spermatozoon, and is set free by the rupture of the seminal cell.

The generative apparatus of the female, commencing also in cæcal extremities, is next treated of, and the author draws particular attention to a transparent, narrow contractile portion, the oviduct, intervening between the ovary and uterus, as the part in which the ovule encounters the spermatic particles, and is by them fecundated. The cæcal end of the ovary likewise throws off a solid particle, which enlarging forms a germinal vesicle and spot. As the germinal vesicle travels slowly down the tubular ovary, it acquires a thick granular investment or yolk, secreted by the ovarian walls. The ovules now present a flattened triangular shape, are placed side by side, and form one solid mass. At the commencement of the oviduct however they become detached, separated from each other, and propelled singly along its interior. Here the gelatinous ovule meets the tubular spermatic particles, and is surrounded on all sides by them. They are at first seen to be merely applied against the ovule, but by degrees the margin of the latter presents a rupture, some of the vitelline granules are displaced, and the spermatic particles become imbedded in the substance of the yolk itself.

While the penetration of the spermatic particles is going on, a chorion, secreted by the oviduct, surrounds the ovule, forming a spherical envelope, within which the germinal vesicle, the granular yolk, and the imbedded spermatozoa, are enclosed. The spermatic particles after penetration are seen to swell, become transparent, and ultimately to dissolve. The vitelline granules likewise either disappear altogether, or are transformed into others of a different colour; and, lastly, the germinal vesicle is destroyed.

By tracing the changes of the ovule in unfecundated females of the same species, the author finds the disappearance of the vitelline granules to be dependent upon, while the formation of the chorion is wholly independent of, the influence exerted by the spermatic particles on the ovule.

As soon as the vitelline granules and germinal vesicle have disappeared, the whole interior of the chorion is filled with a clear fluid, in which a few granules and the germinal spot are seen to remain. By swelling up this constitutes the embryonic vesicle and spot. A membrane separates from the interior of the chorion, and contracting on the granules forms a spherical yolk, in the centre of which is the embryonic vesicle. This is the perfect ovum. The subsequent divisions of the embryonic spot, vesicle and yolk are described; the author particularly pointing out the gyrations of the embryonic vesicle immediately after division. As soon as the whole interior of the egg has been filled by the subdivisions of the yolk, the external granules coalesce and form a continuous membrane internal to the chorion, which by gradual depression on one of its sides forms first a fleshy cup, and then, as the membrane of its concavity touches that of its convex surface, acquires the form of a ring. The ring divides at some point of its circumference, the extremities become pointed, and thus the young Ascaris receives its characteristic shape. The author has frequently repeated his observations with a view to their verification, and has employed the camera lucida to render the illustrative figures as accurate as possible.

XXVI. *Intelligence and Miscellaneous Articles.*

PENDULUM EXPERIMENTS. BY THOMAS G. BUNT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Bristol, July 24, 1851.

IN my last letter I gave the particulars of eleven experiments, each performed with a single impulse of the pendulum, and of from one to four hours' duration, in which the effect of ellipticity was disregarded, and the mean arc of vibration for the most part less than eighteen inches. This series of experiments I have since considerably extended; their average duration has been increased to nearly twelve hours, and the initial arc reduced to about twelve inches. I have already stated, that I find a reduction of the arc of vibration to be accompanied by a diminution of the elliptic error; one-tenth of an inch of ellipticity causing an apsidal motion of $0^{\circ}\cdot7$ per hour on a mean arc of about seven feet, and only $0^{\circ}\cdot06$ per hour on an arc of eleven inches. I have also found, that, in the case of my pendulum, the direction which the elliptic motion assumes in the first quadrant of the circle is changed, in the second quadrant, into its opposite; and that it is almost invariably the same in the same part of the circle. Thus, between 15° and 70° the elliptic motion is retrograde; between 70° and 90° it almost disappears; from 90° to 150° it is direct; and from 150° to 0° (or 180°) it again becomes nearly imperceptible. Hence it appears, that if the pendulum be set in motion with a very small arc, and left to vibrate for a period of fourteen or fifteen hours, the elliptic errors will not only be everywhere inconsiderable, but will also tend in a great measure to neutralize each other. The advantage of using very small arcs in performing these experiments is therefore sufficiently apparent.

In the following summary I have brought forward the total of the eleven experiments given in my last letter, and added to them thirty others which have been performed since. Four of these extend through a period of nearly twenty-four hours each.

Date.	Time.	Motion.	Number of impulses.	Motion per hour.
	h m	°		°
May 23 to June 7. (brought forward).	27 18·5	322·63	11	11·819
June 14 to 17.	22 27·3	265·52	5	
21 ... 26.	50 30·3	584·23	7	11·568
26 ... 28.	42 38·7	502·30	3	11·778
June 30 to July 2.	45 14·1	535·40	3	11·836
July 2 to 7.	46 13·3	545·61	6	11·804
8 ... 9.	45 16·4	523·05	3	11·553
14 ... 16.	46 52·8	557·97	3	11·902
	326 31·4	3836·71	=	11°·750 per hour.
		Theory.. ...		11°·763 ...

A few days ago I received an obliging and most interesting letter from the Rev. J. A. Galbraith, of Trinity College, Dublin, containing a formula which he has discovered for calculating the apsidal motion of the ellipses which a pendulum-ball describes. He says, "The value you gave for the correction for each $\frac{1}{10}$th of an inch, in the June Number, viz. 0°·7, agrees very well with it, the formula giving 0°·626." I had there stated, that the mean length of arc in those experiments was "*about* 7 feet." This rough estimation I afterwards examined more carefully, and altered it long ago in my minute-book into 7·4 feet. This gives a still better agreement, viz. 0°·66 formula, 0°·70 experiment. It does not agree so well (as Mr. Galbraith observes) with what I gave in the July Number, viz. 0°·43 per hour for a mean arc of 3 feet, the formula giving only 0°·27. The agreement with what I gave in the postscript to that letter is much closer. 11°·60 per hour with + ·19 inch ellipticity, and 11°·39 per hour with —·17 inch, gives 0°·058 per hour for each $\frac{1}{10}$th of an inch ellipticity; the formula gives 0°·082 per hour. This important formula I believe Mr. Galbraith intends communicating to your Magazine, together with the calculations from which it is derived.

I am, Gentlemen,

Yours very respectfully,

Thomas G. Bunt.

PENDULUM EXPERIMENTS:—FORMULA FOR CALCULATING THE APSIDAL MOTION.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The following formula may be of some use in observations like those of Mr. Bunt on the motion of a pendulum.

If l be the length of the string, and a and b the apsidal distances of the orbit described by the ball, then the apsidal angle

$$=\frac{\pi}{2}\left\{1+\frac{3}{8}\cdot\frac{ab}{l^2}+\frac{27}{256}\cdot\frac{ab(a^2+b^2)}{l^2}\right\},$$

and consequently the progression of the apse in one revolution

$$=135^\circ\times\frac{ab}{l^2}\left(1+\frac{9}{32}\cdot\frac{a^2+b^2}{l^4}\right).$$

I have been informed that a result, not differing much from this, was given by the Astronomer Royal in a communication read a short time since before the Astronomical Society. His paper, I believe, has not yet been printed *.

I am, Gentlemen,
Your obedient Servant,
A. Thacker.

Trinity College, Cambridge,
July 8, 1851.

ON ATMOSPHERIC SHADOWS. BY PROFESSOR E. WARTMANN.

In a former Number of this Journal (June 1849), I described an observation made at Nyon, by M. Thury, relative to a blue ray which was seen before sunrise. In my opinion it was only an effect of shadow. Analogous appearances are frequently observed at sunset, when the atmosphere is charged with vapours or dust. But it is easier to study them on heights, from the greater transparency of the atmosphere and the less absorption which it exercises on the light: the vicinity of large surfaces of water and of glaciers is particularly favourable to the development and the study of these optical illusions. Every one has been able, in the morning, to follow at considerable distances, in the humid and diaphanous air of the still dark valleys, the course of the solar rays which border the surrounding crests.

A similar phænomenon was manifested on the 31st of last July, toward three o'clock in the afternoon. I was on the signal-station of the Dole, 1680 metres above the sea. The temperature was high. A thin band of mist extended horizontally on the mass of Mont Blanc and on the heights of that chain, at a mean height of 2400 metres. Only one cloud was perceptible in the sky, displaced slowly by the south-east wind, and the image of which was distinctly projected on the slopes of the mountains of Faucigny. The whole space, deprived of light by the interposition of this cloud, was depicted in transparent grayish-black with great clearness. Thus, the floating vesicles of vapour, which, reflecting the rays of the sun, whitened the blue of the sky, existed abundantly at 1900 metres above the lake.

The same day, and at the same hour, I remarked much more curious shadows. On examining the strata of the air comprised between my eye and the bottom of the lake, towards the east, I saw four nearly parallel and equidistant bands, which, inclined toward the sun, appeared to rise from the surface of the water to a height of about 30 degrees. These sombre but indistinct bands had the

* An abstract of the paper will be found at p. 147.—Ed.

same aspect as the obscure train produced by the cloud in a very different region of the sky. They were distinguished from it however by their size, which was much less; each extended only a degree in diameter. They were displaced in a perceptible manner toward the south-east, in proportion as the sun declined toward the horizon. They continued to be visible at least two hours, to my companions as to me. But we knew not to what cause to attribute them. No perceptible obstacle in the immense panorama which we commanded could produce shadows in their direction, and serve thus to explain their presence.

The disposition of the atmospheric vapours in layers, may sometimes engender appearances which are confounded with those of shadows. Long observation, and the variations of tint of the dark bands, serve to avoid the error. An example of these false shadows was presented to me two days after from the plateau of the Barillette. A few minutes before sunset, the ground of the sky assumed a very marked gray colour, trenched upon by three horizontal bands tolerably long, of a more leaden colour, and which converged toward the east. These bands, which would have presented an inverse distribution if they had been parts of the space destitute of light, became gradually of a bright rose colour a few minutes after the twilight came on. They were therefore only formed by a slight haze, suspended at a great height in the atmosphere.—*Bibliothèque Universelle*, September 1849.

ON THE ARTIFICIAL FORMATION OF CORUNDUM AND DIASPORE BY THE WET METHOD. BY M. H. DE SENARMONT.

Not long since I laid before the Academy some experiments upon the artificial production of several kinds of minerals by the wet method, under the combined influences of heat and great pressure ; and I endeavoured to explain in this manner the formation of a peculiar class of metalliferous deposits, principally formed from liquid agents.

Various minerals belong to another class of deposits where the influence of gaseous agents appears to have predominated, where water, which has played an important part in these phænomena, must have acted principally in the state of vapour. It must not, however, be expected that we should find a well-marked line of demarcation between these two kinds of formations. Those substances which mineralize thermal waters, frequently perhaps spring from depths below the earth's surface in the form of volatile compounds; the liquid and gaseous agents have been present in very variable proportions, and the phænomena must have presented numerous intermediate stages between their two extreme limits. It is moreover very difficult, even in a chemical point of view, to imagine the action of water to be very different when filling the same space in the state of a very strongly heated liquid, or when saturating it in the state of vapour under enormous pressure.

Numerous kinds of minerals must therefore be formed, almost indifferently, under one or the other of these conditions.

If a hydrochloric solution of an oxide of the formula $R^2 O^3$ or RO^2 be strongly heated, the acid, even when in excess, becomes free in the solution, and the oxide is separated. The complete precipitation moreover corresponds to a temperature which appears to depend upon the state of dilution and the excess of acid present. In this manner I have obtained the sesquioxides of iron and chromium and titanic and stannic acids, in a pulverulent, amorphous and anhydrous state; the latter alone presenting traces of crystallization. Alumina, on the other hand, crystallizes, under favourable circumstances, in the anhydrous state in the form of *corundum*, and in the hydrated state in the form of *diaspore*. The corundum obtained by heating a dilute solution of hydrochlorate of alumina to a temperature which must exceed 662° F., is a white crystalline sand, which scratches the emerald when pressed between two polished plates. It is insoluble in acids, unalterable by heat, and under the microscope is seen to be uniformly composed of minute, very distinct, almost cubic rhombohedra; they are usually perfect, and more rarely modified by truncatures tangential to the culminating angles, perfectly transparent, and acting regularly upon polarized light. These rhombohedric crystals are often accompanied by others, elongated, and in thin layers, the lateral boundary of which is terminated by two parallel right lines, and towards each extremity by two obtuse symmetrical beveled lines. Like corundum, they are insoluble in acids, but when heated they become changed, and then some of the alumina is dissolved by sulphuric acid; which singular property M. Damour detected in diaspore. In the natural state they are transparent, act strongly upon polarized light; and their neutral lines are, one parallel and the other perpendicular to their greatest length. We may convince ourselves, by the aid of the camera lucida, that the plane obtuse angle of the terminal slopes is nearly equal to 115 degrees. All these characters are those of diaspore, in layers parallel to the plane of cleavage, and there can be no doubt that the prismatic crystals belong to this species.

It is worthy of remark, that the natural diaspore almost always accompanies corundum in its various repositories; and this association is also one of the geological proofs which every means of artificial production aiming at imitating the processes of nature must satisfy.

The oxides of iron, chromium, tin and of titanium, are too rapidly precipitated to assume a regular structure. This separation may be retarded by rendering the liquid very acid; but then it strongly corrodes the glass.—*Comptes Rendus*, May 19, 1851.

THE THEORY OF SOUND.

A further communication on this subject has been addressed to us by Professor Potter, from which we extract the material portions, and with which we shall consider the controversy in this Magazine as closed, all parties having had the fullest latitude in bringing their views fairly before the public. Mr. Potter observes, "The expressions used by Poisson in his solution of the problem are

$$p=gmh(1+s+\sigma)$$

and

$$\sigma=\beta s;$$

therefore we have

$$p=gmh(1+(1+\beta)s).$$

"I have shown in the Philosophical Magazine for April, page 318, and June, page 476, that no reason has been given which proves that β is finite in value.

"Mr. Rankine asserts in the last Magazine, that I have stated this of $(1+\beta)$, and then proceeds to argue upon it. He has evidently considered the arguments applied with respect to the value of β to have been applied with respect to that of $1+\beta$."

"In his concluding remark he says, with respect to the popular view of Laplace's proposal to account for the discrepancy shown by the Newtonian formula, 'My remarks were intended to apply to waves, which, having been originally symmetrical, become unsymmetrical as they advance, like those on the surface of shallow water,'" which Mr. Potter considers inconsistent with Mr. Rankine's statement in the Magazine for March, page 266, where he says, "as every wave must consist of a compressed and a dilated part, the different parts of a wave would travel with different velocities, the compression and dilatation existing from the beginning."

METEOROLOGICAL OBSERVATIONS FOR JUNE 1851*.

Chiswick.—June 1, 2. Very fine. 3. Fine: cloudy. 4. Cloudy: fine: clear: cold at night. 5. Densely clouded: rain. 6. Boisterous: cloudy and fine. 7. Densely overcast: slight rain. 8. Boisterous. 9. Drizzly. 10. Uniformly overcast: rain. 11. Very fine. 12. Densely clouded: showers. 13. Overcast: densely clouded: rain. 14. Fine: heavy clouds: slight rain. 15. Cloudy: rain. 16. Boisterous. 17. Cloudy and fine. 18. Very fine: boisterous. 19—21. Very fine. 22. Cloudy: clear. 23. Fine: clear and cold at night. 24, 25. Very fine. 26, 27. Hot and very dry. 28, 29. Hot and dry. 30. Slightly clouded.

Mean temperature of the month	59°·21
Mean temperature of June 1850	59 ·26
Mean temperature of June for the last twenty-five years .	60 ·72
Average amount of rain in June	1·80 inch.

Boston.—June 1, 2. Fine. 3. Fine: rain P.M. 4. Fine. 5—7. Cloudy: rain A.M. and P.M. 8. Cloudy. 9, 10. Cloudy: rain P.M. 11. Fine. 12. Cloudy: rain A.M. 13. Cloudy: rain P.M. 14. Fine. 15. Fine: rain P.M. 16. Cloudy: stormy. 17. Fine: stormy. 18—20. Cloudy. 21. Fine: thunder and lightning, with rain and hail P.M. 22—25. Cloudy. 26—30. Fine.

Sandwick Manse, Orkney.—June 1. Bright: showers. 2. Bright: rain. 3. Clear. 4. Showers: fine. 5. Fine: showers. 6. Fine: clear. 7. Bright: fine. 8. Rain: hazy. 9. Showers: clear. 10. Showers: damp. 11. Showers. 12. Clear: fine. 13. Bright: fine. 14. Fine: hazy. 15. Rain. 16. Rain: drizzle. 17. Showers: clear. 18. Fine: drizzle. 19. Showers: hazy. 20. Fine: clear. 21. Bright: showers. 22. Cloudy. 23. Bright: drizzle. 24. Cloudy. 25. Bright: damp. 26. Cloudy: clear. 27, 28. Clear: fine. 29, 30. Hot: fine.

---

* The observations from the Rev. W. Dunbar of Applegarth Manse have not reached us.

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.
1851. June.	Max.	Min.		9 a.m.	9 p.m.	9¾ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·363	30·195	29·87			30·18	29·94	76	43	57			53½	51	ne.	wsw.		w.	......	......		......
2.	30·181	30·042	29·68			29·90	29·72	72	41	58			50	44	nw.	wnw.		nw.	......	......		·06
3.	29·879	29·672	29·36			29·58	29·68	74	39	64·5			45½	42½	sw.	w.		nw.	·08	......		·65
4.	29·904	29·819	29·40			29·64	29·70	66	36	50			44½	44	nw.	nw.		sw.	·01	·04		·02
5.	29·833	29·773	29·37			29·62	29·48	62	45	53			51	45½	sw.	s.		se.	·14	·04		·01
☽ 6.	29·950	29·822	29·27			29·48	29·60	61	52	60			54½	45	sw.	sw.		sw.	......	·42		·11
7.	29·965	29·937	29·44			29·72	29·68	66	53	61·5			51	49	sw.	ssw.		se.	·01	·20		·08
8.	29·960	29·936	29·38			29·32	29·28	71	56	63			52	49	sw.	w.		ne.	......	·18		·36
9.	29·963	29·551	29·46			29·72	29·70	61	48	57			47	45	se.	wnw.		n.	·02	......		·12
10.	29·720	29·642	29·22			29·60	29·68	55	43	50			44	43	ne.	n.		w.	·32	·82		·09
11.	29·980	29·965	29·53			29·74	29·74	67	48	51			48	49	sw.	wnw.		se.	·04	·08		·05
12.	29·737	29·650	29·23			29·66	29·66	61	56	57			53	49½	sw.	s.		e.	·23	......		·07
○ 13.	29·913	29·722	29·23			29·70	29·86	67	47	63·5			54½	57	sw.	w.		n.	·04	·02		......
14.	30·061	30·036	29·57			29·92	29·90	67	41	57			55½	49	sw.	w.		se.	·01	......		......
15.	30·056	29·904	29·55			29·68	29·36	64	52	64			52	51	sw.	sw.		se.	·38	......		·25
16.	30·031	29·832	29·33			29·33	29·46	67	45	61			50	50½	w.	w.		nw.	......	·21		·25
17.	30·383	30·209	29·67			29·94	30·20	66	38	57			47	48½	nw.	wnw.		w.	......	......		·07
18.	30·402	29·231	29·90			30·10	29·80	71	55	57			53½	50	w.	wnw.		w.	·05	......		......
19.	30·160	30·108	29·55			29·54	29·84	75	49	67			51	47½	sw.	wnw.		nw.	......	......		·11
20.	30·098	30·035	29·58			29·96	29·94	81	47	67			50	48½	sw.	w.		se.	......	......		......
☾ 21.	29·883	29·689	29·38			29·70	29·60	87	55	70			59	53	se.	s.		ne.	......	......		·04
22.	30·014	29·813	29·26			29·57	29·84	67	41	60			52	47½	nw.	nw.		wnw.	......	·08		·15
23.	30·248	30·163	29·68			30·09	30·05	67	35	59			49	48½	nw.	w.		nw.	......	......		·02
24.	30·267	30·242	29·76			30·05	29·94	73	45	59			56	53	w.	w.		se.	......	......		·04
25.	30·260	30·246	29·68			29·94	29·88	77	47	67			53½	53½	w.	w.		s.	......	......		·28
26.	30·267	30·200	29·70			29·84	30·09	85	46	65			62	52	s.	sw.		w.	......	......		·03
27.	30·169	30·125	29·63			30·17	30·17	91	52	69			58	58	e.	calm		e.	......	......		......
28.	30·147	30·137	29·63			30·13	30·11	84	51	69			64½	60½	e.	se.		e.	......	......		......
29.	30·148	30·125	29·62			30·11	30·11	82	50	67·5			69	67	e.	e.		......	......	......		......
● 30.	30·124	30·061	29·60			30·09	30·09	82	55	67			69	60	e.	e.		......	......	......		......
Mean.	30·069	29·929	29·51			29·797	29·803	71·40	47·03	60·9			53·31	50·56					1·33	2·09		2·86

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1851.

XXVII. *On Diamagnetism and Magnecrystallic Action.* *By* JOHN TYNDALL, *Ph.D.**

§ 1. *On Diamagnetism.*

FIVE years ago Faraday established the existence of the force called diamagnetism, and from that time to the present some of the first minds in Germany, France and England have been devoted to the investigation of this subject. One of the most important aspects of the inquiry is the relation which subsists between magnetism and diamagnetism. Are the laws which govern both forces identical? Will the mathematical expression of the attraction in the one case be converted into the expression of the repulsion in the other by a change of sign from positive to negative?

The conclusions arrived at by Plücker in this field of inquiry are exceedingly remarkable and deserving of attention. His first paper, "On the relation of Magnetism and Diamagnetism," is dated from Bonn, September 8, 1847, and will be found in Poggendorff's *Annalen* and in Taylor's Scientific Memoirs. He sets out with the question, "Is it possible, by mixing a magnetic substance with a diamagnetic, so to balance the opposing forces that an indifferent body will be the result?" This question he answers in the negative. "The experiments," he writes, "which I am about to describe, render it necessary that every thought of the kind should be abandoned."

One of these experiments will serve as a type of the whole, and will show the foundation on which the negative reply of M. Plücker rests. A piece of cherry-tree bark, 15 millims. long and 7 millims. wide, was suspended freely between the two moveable poles of an electro-magnet; on bringing the points of the

* Communicated by the Author, having been read before the Physical Section at the Meeting of the British Association at Ipswich, July 2, 1851.

poles so near each other that the bark had barely room to swing between them, it set itself, like a diamagnetic substance, with its length *perpendicular* to the line which united the two poles. On removing the poles to a distance, or on raising the bark to a certain height above them, it turned round and set its length *parallel* to the line joining the poles. As is usual, we shall call the former position the *equatorial*, and the latter position the *axial*. Thus when the poles were near, diamagnetism was predominant, and caused the mass to set equatorial; when the poles were distant, magnetism, according to the notion of M. Plücker, was predominant, and caused the mass to set axial. From this he concludes, "*That in the cherry-tree bark two distinct forces are perpetually active; and that one of them, the magnetic, decreases more slowly with the distance than the other, the diamagnetic.*

In a later memoir* this predominance of the diamagnetic force at a short distance is affirmed by M. Plücker to be due to the more general law, that when a magnet operates upon a substance made up of magnetic and diamagnetic constituents, if the power of the magnet be increased, the diamagnetism of the substance increases in a much quicker ratio than the magnetism; so that without altering the distance between it and the magnet, the same substance might at one time be attracted and at another time repelled by merely varying the strength of the exciting current.

This assertion is supported by a number of experiments, in which a watch-glass containing mercury was suspended from one end of a balance. The watch-glass was magnetic, the mercury was diamagnetic. When the glass was suspended at a height of 3·5 millims. above the pole of the magnet, and the latter was excited by a battery of four cells, an attraction of one millegramme was observed; when the magnet was excited by eight cells, the attraction passed over into a repulsion of the same amount.

It is to be regretted that M. Plücker, instead of giving us the actual strength of the exciting current, has thought proper to mention merely the number of cells employed. From this we can get no definite notion as to the amount of magnetic force evolved in the respective cases. It depends of course upon the nature of the circuit whether the current increases with the number of cells or not. If the exterior resistance be small, an advance from four to eight cells will make very little difference; if the said resistance be a vanishing quantity, one cell is as good as a million†.

* Poggendorff's *Annalen*, vol. lxxv. p. 413.

† The usual arrangement of the cells is here assumed; that is, where the negative component of one cell is connected with the positive component of the next.

During an investigation on the magneto-optic properties of crystals*, which I had the pleasure of conducting in connexion with Professor Knoblauch, I had repeated opportunities of observing phænomena exactly similar to those observed by M. Plücker with the cherry-tree bark; but a close study of the subject convinced me that the explanation of these phænomena by no means necessitated the hypothesis of two forces acting in the manner described. Experiment further convinced me, that a more delicate apparatus than the balance used by M. Plücker would be better suited to the measurement of such feeble manifestations of force.

An exact acquaintance with electro-magnetic attractions appeared to be a necessary discipline for the successful investigation of diamagnetic phænomena; and pursuing this idea, an inquiry was commenced last November into the action of an electro-magnet upon masses of soft iron. I was finally led to devote my entire attention to the attraction of soft iron spheres, and the results obtained were so remarkable as to induce me to devote a special memoir to them alone†.

In this investigation it was proved, that a ball of soft iron, separated by a small fixed distance from the pole of an electro-magnet, was attracted with a force exactly proportional to the square of the exciting current. Now this attraction is in each case the product of two factors, one of which represents the magnetism of the magnet, and the other the magnetism of the ball. For example, if the magnetism of the magnet of any given moment be represented by the number 4, and that of the ball by 3, the attraction, which is a consequence of their reciprocal action, is represented by the number 12. If we now suppose the magnetism of the magnet to be doubled by a current of double strength, the ball will have its magnetism also doubled, and the attraction resulting will be expressed by the number 48. Thus we see that a doubling of the power of the magnet causes four times the attraction; and that while the attraction increases as the *square* of the current, *the magnetism of the ball increases in the simple ratio of the current itself.*

Our way to a comparison of magnetism and diamagnetism is thus cleared. We know the law according to which the magnetism of an iron ball increases, and we have simply to ascertain whether the diamagnetism of a bismuth ball follows the same law. For the investigation of this question I constructed the following apparatus.

In two opposite sides of a square wooden box two circular holes were sawed about four inches in diameter. The holes were

* Philosophical Magazine, July 1850.

† Phil. Mag., April 1851. Poggendorff's *Annalen,* May 1851.

diagonally opposite to each other, and through each a helix of copper wire was introduced and wedged fast. Each helix contained a core of soft iron, which was pushed so far forward that a line parallel to the sides of the box through which the helices entered, and bisecting the other two sides, was a quarter of an inch distant from the interior end of each core. The distance between the two interior ends was six inches, and in this space a little beam of light wood was suspended. At the ends of the beam two spoon-shaped hollows were worked out, in which a pair of small balls could be conveniently laid. The beam rested in a paper loop, which was attached to one end of a fine silver wire. The wire passed upward through a glass tube nearly three feet in length, and was connected at the top with a torsion head. The tube was made fast in a stout plate of glass, which was laid upon the box like a lid, and thus protected the beam from currents of air. A floor of Bristol board was fixed a little below the level of the axes of the cores, the 'board' being so cut as to fit close to the helices: the two corners of the floor adjacent to the respective cores and diagonally opposite to each other bore each a graduated quadrant. When the instrument was to be used, two balls of the substance to be experimented with were placed upon the spoon-shaped hollows of the beam and there exactly balanced. The balance was established by pushing the beam a little in the required direction through the paper loop in which it loosely rested; and to accomplish this with greater ease, two square pieces were sawed out of the sides of the box, and two others were exactly fitted into the space thus opened; these pieces could be taken out at pleasure, and the hand introduced without raising the lid. The torsion-head was arranged so that when the beam bearing the balls came to rest, a thin glass fibre attached to the beam pointed to zero on the graduated quadrant underneath, while the index of the head pointed also to the zero of the graduated circle above. A current was sent through the helices in such a direction, that the poles which operated on the balls were of opposite names—the balls were repelled. Preserving the current constant, the index above was turned in a direction opposed to the repulsion until the beam stood again at zero. The torsion necessary to effect this is evidently the expression of the repulsive force exerted at this particular distance.

Fig. 1. represents the appearance of the beam and helices when looked down upon through the glass lid. Fig. 2. represents the beam and balls attached to the suspending wire.

When the fibre pointed to zero, an interval of about $\frac{1}{12}$th of an inch usually separated the diamagnetic balls from the core ends. The intensity of the current was measured by a galvanometer of tangents, and was varied by means of a rheostat. Always before commencing a series of experiments, the little beam was proved. With very strong currents it was found to be slightly diamag-

Fig. 1.

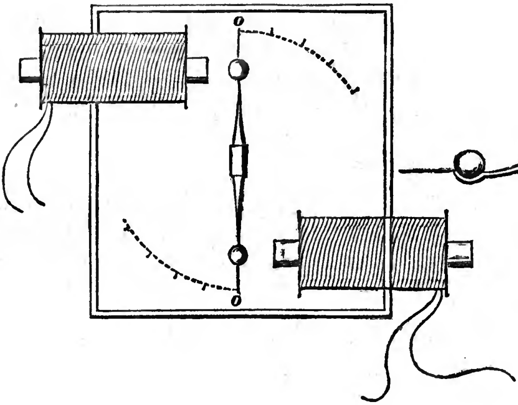
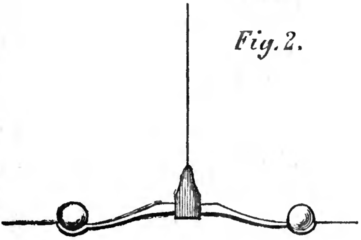


Fig. 2.



netic; but so feeble, that its action, even supposing it not to follow the same law of increase as the ball (which, however, it certainly does), could cause no measurable disturbance.

I neglected no precaution to secure the perfect purity of the substances examined. The entire investigation was conducted in the private cabinet of Professor Magnus in Berlin; and at the same time a gentleman, Dr. Schneider, happened to be engaged in the Professor's laboratory in determining the chemical equivalent of bismuth. From him I obtained a portion of this substance prepared in the following way:—The metal of commerce was dissolved in nitric acid and precipitated with distilled water; whatever iron was present remained in the solution. The precipitate was filtered, washed for six days successively, and afterwards reduced by means of black flux. The metal thus obtained was again melted in a Hessian crucible, and saltpetre was gradually added, the mass at the same time being briskly stirred. Every remaining trace of foreign ingredient was thus oxidized and rose to the surface, from which it was carefully skimmed. The metal thus purified was cast into a bullet-mould, the interior surface of which was coated by a thin layer of oil; the outer surface of each bullet was carefully scraped away with glass, the ball was then scoured with sea-sand, and finally boiled in hydrochloric acid. I have already described the method of experiment. The bismuth balls were placed upon the hollows of the beam, and their repulsions by various currents determined in the manner indicated. The series of repulsions thus obtained are exactly analogous to the series of attractions in the case of the ball of soft iron. The square roots of these attractions give a series of numbers exactly proportional to the currents employed; the question therefore is,—"Will the square roots of the repulsions give a similar series, or will they not?"

Calling the angle which the needle of the galvanometer, under the influence of the current, makes with the magnetic meridian α, then if the attraction of the iron ball and the repulsion of the bismuth ball follow one and the same law, we shall have the equation

$$\sqrt{T}=n\tan\alpha,$$

where T represents the torsion necessary to bring the beam back to zero, and n is a constant depending on the nature of the experiment. The following tables will show the fulfilment or non-fulfilment of this equation.

Table I.—Bismuth spheres, 8 millims. diameter.

$n=11\cdot7.$

α.	tan α.	T.	$\sqrt{T}$.	n tan α.
10	0·176	5	2·23	2·06
20	0·364	16·3	4·04	4·25
30	0·577	42·3	6·50	6·74
35	0·700	64	8	8·19
40	0·839	100	10	9·81
45	1·000	136	11·66	11·7
50	1·192	195	13·96	13·95

A second series was made with a pair of spheres of the bismuth of commerce with the same result.

Sulphur is also a diamagnetic substance, but much weaker in this respect than bismuth. The next series of experiments were made with two balls of this substance.

Table II.—Sulphur spheres, 8 millims. diameter.

$n=3\cdot3.$

α.	tan α.	T.	$\sqrt{T}$.	n tan α.
20° 0′	0·364	1·2	1·10	1·20
30 45	0·595	3·0	1·73	1·96
41 20	0·880	8·0	2·83	2·90
54 0	1·376	21·0	4·58	4·54

A pair of sulphur balls were next taken of nearly twice the diameter of the preceding.

Table III.—Sulphur spheres, 13·4 millims. diameter.
$n=6·7$.

α.	tan α.	T.	$\sqrt{T}$.	n tan α.
20° 0′	0·364	6·2	2·45	2·44
30 45	0·595	15·0	3·87	3·98
41 20	0·880	34·5	5·90	5·89
54 0	1·376	89·0	9·43	9·22

The sulphur from which these balls were made was the material of commerce. After the experiments one of the balls was placed in a clean porcelain crucible and brought over the flame of a spirit-lamp; the sulphur melted, ignited, and disappeared in sulphurous acid vapour. A portion of solid substance remained in the crucible unvolatilized. This was dissolved in hydrochloric acid, and ferrocyanide of potassium was added; the solution turned immediately blue; iron was present. The other ball was submitted to a similar examination, and with the same result; both balls contained a slight admixture of iron.

In this case, therefore, the two opposing forces, magnetism and diamagnetism, were present, but we find the equation $\sqrt{T}=n \tan \alpha$ fulfilled notwithstanding. Did one of the forces increase with the ascending magnetic power more quickly than the other, this result would be impossible.

Flowers of sulphur were next tried, but found to contain a considerable quantity of iron. I have to thank Prof. Magnus for a portion of a native crystal of the substance obtained in Sicily, which upon trial was found to be perfectly pure. From this, two small pellets were formed and laid upon the torsion-balance; they gave the following results:—

Table IV.—Spheres of Native Sulphur.
$n=2·65$.

α.	tan α.	T.	$\sqrt{T}$.	n tan α.
20	0·364	0·9	0·95	0·96
30	0·577	2·5	1·58	1·53
40	0·839	5·0	2·24	2·22
45	1·000	7·0	2·64	2·65
50	1·192	10·0	3·16	3·16

The next substance chosen was calcareous spar. The corners of the crystalline rhomb were first filed away, and the mass thus rendered tolerably round; it was then placed between two pieces of soft sandstone, in each of which a hollow, like the cavity of a bullet-mould, had been worked out. By turning the stones, one right and the other left, and adding a little water, and a little patience, the crystal was at length reduced to a spherical form. The ball was then washed, and its surface carefully cleansed in dilute hydrochloric acid. The first pair of balls were from the neighbourhood of Clitheroe in Lancashire.

Table V.—Spheres of Calcareous Spar, 9·2 millims. diameter. $n=3·7$.

α.	tan α.	T.	$\sqrt{}$T.	n tan α.
20	0·364	1·8	1·34	1·34
25	0·466	3·0	1·73	1·72
30	0·577	4·5	2·12	2·13
35	0·700	7·0	2·64	2·59
40	0·839	9·7	3·11	3·10
45	1·000	14·0	3·74	3·70

The spar from which these balls were taken was not quite transparent; to ascertain whether its dullness was due to the presence of iron, a crystal which weighed about 3 grammes was dissolved in hydrochloric acid; the solution was exposed in a flat basin to the air, and the iron, if present, suffered to oxidize; ferrocyanide of potassium was added, but not the slightest tinge indicative of iron was perceptible.

A series of experiments were next made with a pair of spheres of calcareous spar from Andreasberg in the Harz Mountains.

Table VI.—Spheres of Calcareous Spar, 10·8 millims. diameter. $n=5$.

α.	tan α.	T.	$\sqrt{}$T.	n tan α.
20·0	0·364	2·8	1·68	1·82
25·0	0·466	5·0	2·21	2·33
30·0	0·577	8·0	2·83	2·83
35·0	0·700	11·2	3·35	3·50
37·30	0·767	14·5	3·81	3·83
57·0	1·540	60·0	7·75	7·70

The spar from which these balls were taken was perfectly transparent. After the experiment, the balls were partially dissolved in hydrochloric acid, and the solution tested for iron, as in the former case—no trace of iron was present.

The conclusion to be drawn from all these experiments, and from many others which I forbear citing, is, that the law of increase for a diamagnetic body is exactly the same as for a magnetic—a result irreconcilable with that arrived at by M. Plücker. I had proceeded further with this investigation than the point which I have already reached, when I learned that a memoir on diamagnetism by M. Edmond Becquerel had appeared in the May number of the *Annales de Chimie et de Physique.* In this memoir the views of Plücker are also controverted, and a number of experiments are adduced to prove the identity of the laws which regulate magnetic attraction and diamagnetic repulsion. The argument employed by M. Becquerel is the same in principle as that furnished by the foregoing experiments. He proves that the repulsion of *bars* of bismuth, sulphur and wax, increases as the square of the exciting current, and that the attraction of a little bar of iron follows the same law. We have both been guided in our inquiries by the same fundamental thought, though our modes of carrying out the thought are different.

I have observed many phænomena, which, without due consideration, would lead us directly to Plücker's conclusions; a few of these I will here describe. The bismuth balls were placed upon the beam, and one core was excited; on the top of the ball opposite, a particle of iron, not the twentieth part of a common pin-head in size, was fixed. A current of 10° circulated in the helix, and the beam came to rest at the distance of 4° from the zero of the under graduation. I then permitted the current to increase gradually. The magnetism of the iron particle and the diamagnetism of the bismuth rose of course along with it, but the latter triumphed; the beam was repelled, and finally came to rest against a stop which was placed 9° distant.

The particle of iron was removed, and a small crystal of carbonate of iron put in its place; a current of 15° circulated in the helix, and the beam came to rest at about 3° distant from zero. The current was raised gradually, but before it had reached 30° diamagnetism conquered, and the beam receded to the stop as before.

Thinking that this apparent triumph of diamagnetism might be due to the fact, that the crystal of carbonate of iron had become saturated with magnetism, and that it no longer followed the law of increase true for a larger piece of the substance, I tested the crystal with currents up to 49°; the attractions were exactly proportional to the square of the exciting currents.

Thinking also that a certain reciprocal action between the bismuth and the crystal, when both were placed together in the magnetic field, might so modify the latter as to produce the observed result, I removed the crystal, and placed a cube of the zinc of commerce upon the opposite end of the beam. The zinc was slightly magnetic. Bismuth and zinc were thus separated by an interval of 6 inches; both cores were excited by a current of 10°, and the beam, after some oscillations, came to rest at 4°

distant from zero. The current was now gradually raised, but when it reached 35° the beam receded and was held firmly against the stop. When the circuit was broken it left the stop, and, after some oscillations, came to rest at zero.

These experiments seem fully to bear out the notion of Plücker. In each case we waited till both forces were in equilibrium; and it might be thought that if the forces followed the same law, the beam ought not to move. Let us, however, clear the experiment of all mystery; when the beam was in equilibrium with a current of 10°, let us ask what forces were opposed to the repulsion of the bismuth? There was first of all the attraction of the zinc; but besides this, there was a torsion of 4°, for the position of equilibrium for the beam when the magnet was unexcited was at zero. Let us suppose the magnetism of the zinc at the distance of 4°, and with the current 10°, to be equal to 8° of torsion; this, added to the 4° already present, will give the force opposed to the bismuth; the repulsion of the latter is therefore equal to 12. Let us now conceive the current raised from 10° to 35°, that is quadrupled. Supposing the magnetism of the zinc to be increased in proportion to the strength of the current, its attraction will now be 32°; this, added to 4° of torsion, which remains constant, makes 36, which is therefore the force brought to bear against the bismuth by a current of 35° under the present circumstances. But the repulsion of the bismuth is also quadrupled; it is now 48. This, opposed to a force of 36, necessarily conquers, and the beam is repelled.

We thus see, that although the magnetic force on one side, and the diamagnetic on the other side, follow precisely the same law, the introduction of the small constant 4° entirely destroys the balance of action, so that to all appearance diamagnetism increases in a much greater ratio than magnetism. Such a constant has probably crept into the experiments of Plücker; an inadvertency not to be wondered at, when we remember that the force was new at the time, and our knowledge of the precautions necessary to its accurate investigation very imperfect.

§ 2. *On Magnecrystallic Action.*

Plücker has discovered, that when a crystal of pure carbonate of lime is suspended in the magnetic field with its optic axis horizontal, the said axis always sets itself equatorial. He attributed this action of the spar to a repulsion of the optic axis by the magnet, wholly independent of the magnetism or diamagnetism of the mass of the crystal. It was the product of a new force, which Mr. Faraday has named the optic axis force.

In the memoirs published by Knoblauch and me, this view is dissented from, and it is there proved that the action of the crystal, so far from being independent of the magnetism or diamagnetism of its mass, is totally changed by the substitution of a magnetic constituent for a diamagnetic. Our experiments led us to the conclusion, that the position of the crystal of carbonate of lime was due to the superior repulsion of the mass of the crystal in the direction of the optic axis. This view, though supported by the strongest presumptive facts, has remained up to the present time without direct proof; if, however, a difference of repulsion, such as that we have supposed, actually exists, it may be expected to manifest itself upon the torsion-balance.

But the entire repulsion of calcareous spar is so feeble, that to discover a differential action of this kind requires great nicety of experiment. I returned to this subject three different times; twice I failed, and despaired of being able to establish a difference with the apparatus at my command. But the thought clung to me, and after an interval of some weeks I resolved to try again.

The spheres of calcareous spar were placed upon the beam, and the latter was exactly balanced. The index above was so placed, that when the beam came to rest, the attached glass fibre exactly coincided with a fine black line drawn upon the Bristol board underneath. Two dots were placed upon the glass cover, about the fiftieth of an inch asunder, and the fibre was observed through the interval between them. The beam was about 4 inches below the cover, and parallax was thus avoided. On exciting both cores the balls receded, the index above was softly turned against the recession, till the fibre was brought once more into exact coincidence with the fine black line, and the torsion necessary to effect this was read off upon the graduated circle above.

The repulsion of the spheres was measured in four different directions:—

1. The optic axes were parallel to the axes of the iron cores.

2. The spheres were turned through an arc of 90°, so that the optic axes were at right angles to the cores.

3. The spheres were turned 90° in the same direction, so that the other ends of the axes faced the cores.

4. The spheres were turned 90° further, so that their axes were again at right angles to the cores, but with the opposite surface to that in (2) facing the latter.

The following are the respective repulsions :—

	Repulsion.
1st position	28·5
2nd position	26·5
3rd position	27·0
4th position	24·5

Each of the helices which surrounded the cores was composed of two isolated wires; the four ends of these could be so combined that the current could pass through both at the same time, as if they were a single wire, or it could be caused to traverse one wire after the other. The first arrangement was advantageous when a small exterior resistance was an object to be secured, the second when the force of the battery was such as to render exterior resistance to a certain extent a matter of indifference. In the above experiments the first arrangement was adopted. Before commencing, however, I had taken fresh acid and freshly amalgamated zinc cylinders, so that the battery was in good condition. The second arrangement was adopted, that is, the current was allowed to traverse one wire after the other, and the following repulsions were observed; the numbers refer to the positions already indicated:—

1st position	57
2nd position	51
3rd position	53
4th position	48

These experiments furnish the direct proof that calcareous spar is repelled most strongly in the direction of the optic axis. That Mr. Faraday has not succeeded in establishing a difference here is explained by reference to his mode of experiment. He observed the distance to which the spar was repelled, and found this the same for all positions of the crystal. The magnetic force at this distance is too weak to show a difference. In the above experiments, on the contrary, the crystal was forced back into a portion of the magnetic field where the excitement was intense, and here for the first time the difference rises to a measurable quantity.

Carbonate of iron is a crystal of exactly the same form as calcareous spar, the iron filling up, so to speak, the exact space vacated by the calcium. This crystal is strongly magnetic; suspended in the magnetic field, that line which in calcareous spar sets equatorial, sets here axial, but with an energy far surpassing the spar; a greater differential action may therefore be anticipated.

A pair of spheres were formed from this crystal, but their attraction was so strong, that to separate them from the magnet would strain the wire beyond its limits of elasticity; one sphere only could therefore be used, the other being used as a balance-weight merely. The core opposite to the latter was removed, and the current sent round that helix only which surrounded the former. A piece of Bristol board was placed against the end of the core, and the torsion-head was so turned that when the index above pointed to zero the little sphere was on the verge of contact. The magnet was then excited and the sphere attracted. The index was then turned in a direction opposed to the attraction until the ball gave way; the torsion necessary to effect this expresses the attraction. The crystal was first placed so that its axis was parallel to the magnet, and afterwards so that it was perpendicular to the same. The following tables exhibit the results in both cases respectively:—

Table VII.—Carbonate of Iron. Axis of Crystal parallel to axis of Magnet. $n=25{\cdot}5$.

α.	tan α.	T.	$\sqrt{}$T.	n tan α.
15	0·268	43	6·56	6·57
20	0·364	80	8·94	8·91
25	0·466	129	11·36	11·42
30	0·577	200	14·14	14·14

Table VIII.—Carbonate of Iron. Axis of Crystal perpendicular to axis of Magnet. $n=20{\cdot}7$.

α.	tan α.	T.	$\sqrt{}$T.	n tan α.
15	0·268	30·5	5·52	5·55
20	0·364	56·0	7·48	7·53
25	0·466	92·5	9·62	9·64
30	0·577	142·5	11·44	11·44

We learn from these experiments that the law according to which the attraction of carbonate of iron increases, is exactly the same as that according to which the repulsion of the calcareous spar increases, and that the respective forces manifest themselves in both cases with the greatest energy in the direction of the optic axis.

Let us observe for an instant the perfect antithesis which exists between carbonate of lime and carbonate of iron. The former is a diamagnetic crystal; suspended before the single pole of a magnet the entire mass is repelled, but the mass in one direction is repelled with peculiar force, and this direction, when the crystal is suspended in the magnetic field, recedes as far as possible from the poles, and finally sets equatorial. The crystal of carbonate of iron is, on the contrary, strongly magnetic; suspended before a single pole the entire mass is attracted, but in one direction the mass is attracted with peculiar energy, and this direction, when the crystal is suspended in the magnetic field, will approach the poles and finally set axial.

Sulphate of iron in the magnetic field displays a directive action considerably inferior to that of carbonate of iron. Some large crystals were obtained from a chemical manufactory, and from these I cut two clean cubes. Each was suspended by a cocoon fibre in the magnetic field, and the line which stood axial was marked upon it. The white powder which collects by efflorescence around these crystals was washed away, and two transparent cubes remained. These were laid upon the torsion-balance, and instead of the Bristol board two plates of glass were placed against the core ends; the adhesion of the cubes, which in delicate experiments of this nature sometimes enters as a disturbing element, was thus reduced to a minimum. As in the case of carbonate of iron, one core only was excited. The cube opposite to this core was first so placed that the line which stood axial in the magnetic field was parallel to the core; preserving this line horizontal, the three remaining faces were presented successively to the core and the attraction measured in each particular case; these attractions were as follows:—

Cube of sulphate of iron, edges 10 *millims.*

	Attraction.
1st position	43·0
2nd position	36·3
3rd position	40·0
4th position	34·5

Hence the attraction of this crystal in the direction of the line which sets axial in the magnetic field, is to the attraction in a direction perpendicular to the same in the ratio of 7 : 6 nearly.

In an article translated from Poggendorf's *Annalen*, which appears in the June Number of the Philosophical Magazine, it will be seen that Prof. Plücker has experimented with a cube of sulphate of iron, and has arrived at results which he adduces against the theory of magnecrystallic action advanced by Knoblauch and myself. He rightly concluded that if the position of the crystal, suspended between two poles, were due to the superior attraction exerted in a certain direction, this peculiarity ought to exhibit itself in the attraction of the entire mass of the crystal by the single pole of a magnet. He brings this conclusion to the test of experiment, suspends the crystal from one end of a balance, weighs the attraction in different directions, but finds no such difference as that implied by the conclusion. This result I believe is entirely due to the imperfection of his apparatus; I have tried a very fine balance with even worse success than M. Plücker. Although the torsion-balance furnishes a means of experiment immeasurably finer, still, with it, great delicacy of manipulation and a considerable exercise of patience are necessary to ensure invariable success. It is gratifying to find M. Plücker's deduction so strictly fulfilled, and I doubt not that he will, with his usual frankness, grant the theory the full benefit of the corroboration.

Faraday has discovered, that if a bismuth crystal be suspended in the magnetic field, it will set itself so that a line perpendicular to the plane of most eminent cleavage will be axial; this line he calls the magnecrystallic axis of the crystal. In the memoir before alluded to, the position of the magnecrystallic axis is affirmed to be a secondary result, depending on the fact that the mass in the direction of the planes of cleavage is most strongly repelled.

Here again the torsion-balance furnishes us with the direct proof of this affirmation. Two cubes of bismuth were prepared, in each of which the plane of most eminent cleavage formed two of the opposite sides. Suspended by a fibre of cocoon-silk in the magnetic field, the line perpendicular to the cleavage turned into the axial position, or what amounts to the same as far as the eye is concerned, the cleavage itself receded from the poles and stood equatorial. These cubes were placed one on each end of the torsion-balance; first, so that the plane of most eminent cleavage was parallel to the axes of the cores, and afterwards perpendicular to these axes. The respective repulsions are stated in the following tables.

Table IX.—Cubes of bismuth, crystal edges 6 millims. Plane of most eminent cleavage parallel to axes of cores.

α.	T.
20	11·7
30	34·8
40	78
45	111
50	153

Table X.—The same cubes. Plane of most eminent cleavage perpendicular to axes of cores.

α.	T.
20	8
30	23
40	53
45	76·5
50	110

A comparison of these two tables shows us that the repulsion of the cubes, when the plane of most eminent cleavage was parallel to the magnetic axis, is to the repulsion when the said plane was perpendicular thereto in the ratio of 15 : 11 nearly. The general fact of superior repulsion in the direction of the cleavages has been already demonstrated by Mr. Faraday.

What is it, then, which causes this superior manifestation of force in a certain direction? To this question experiment returns the following reply:—"If the arrangement of the component particles of any body be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that of strongest attraction in magnetic bodies and of strongest repulsion in diamagnetic bodies."

The torsion-balance enables us to test this theory. A quantity of bismuth was ground to dust in an agate mortar, gum-water was added, and the mass was kneaded to a stiff paste. This was placed between two glasses and pressed together; from the mass when dried two cubes were taken, the line of compression being perpendicular to two of the faces of each cube and parallel to the other four. Suspended by a silk fibre in the magnetic field, upon closing the circuit the line of compression turned strongly into the equatorial position, exactly as the plane of most eminent cleavage in the case of the crystal. The cubes were placed one upon each end of the torsion-balance; first with the line of compression parallel to the cores, and secondly with the said line perpendicular to the cores. The following are the repulsions exhibited in both cases respectively.

Table XI.—Cubes of powdered bismuth, edges 7 millims. Line of compression parallel to axes of cores.

α.	tan α.	T.	$\sqrt{T}$.	$8{\cdot}3\times$ tan α.
30	0·577	22	4·69	4·78
40	0·839	46	6·78	6·96
45	1·000	67	8·19	8·30
50	1·192	98	9·89	9·89

From this table we see that the law of increase for the artificial cube is the same as that for diamagnetic substances generally.

Table XII.—The same cubes. Line of compression perpendicular to cores.

α.	T.
30	13
40	31
45	46
50	67

A comparison of both tables shows us that the line which stands equatorial in the magnetic field is most strongly repelled upon the torsion-balance, exactly as in the case of the crystal; the repulsion in the direction of this line and in a direction perpendicular to the same being nearly in the ratio of 3 : 2. Similar experiments were made with cubes of powdered carbonate of iron. The line of compression in the magnetic field stood axial; and when laid upon the torsion-balance, the mass in the direction of this line was attracted most strongly.

At the last meeting of the British Association, an objection, which will probably suggest itself to all who study the subject as profoundly as he has done, was urged against this mode of experiment by Professor William Thomson. "You have," he said, "reduced the mass to powder, but you have not thereby destroyed the crystalline property; your powder is a collection of smaller crystals, and the pressing of the mass together gives rise to a predominance of axes in a certain direction; so that the repulsion and attraction of the line of compression which you refer to the mere closeness of aggregation is, after all, a product of crystalline action."

I know that this objection, which was specially directed against the experiment made with powdered bismuth and carbonate of lime, floats in the minds of many both in Germany and England, and I am therefore anxious to give it a full and fair reply. I might urge, that in the case of the bismuth powder at least, the tendency of compression would be to place the little component crystals in such a position, that a deportment precisely the reverse of that actually observed might be anticipated. If we pound the crystal to the finest dust, the particles of this dust, to render Mr. Thomson's hypothesis intelligible, must have a certain predominant shape, otherwise there is no reason in the world to suppose that pressure will *always* cause the axes of the little crystals to take up the same predominant direction. Now what shape is most likely here? The crystal cleaves in one direction more easily than in any other; is it not then probable that the powder will be chiefly composed of minute scales, whose opposite flat surfaces are the surfaces of principal cleavage? And what is the most probable effect of compression? Will it not be to place these little scales with their flat surfaces perpendicular to the line in which the pressure is exerted? In the crystal, the line perpendicular to the principal cleavage sets axial, and hence it might be expected that the line of compression in the model would set axial also; it does not, however,—it sets equatorial.

This, however, though a strong presumptive argument, is not yet convincing; and it is no easy matter to find one that shall be so. Bismuth powder will remain crystalline, and carbonate of lime is never free from suspicion. I thought I had found an unexceptionable substance in chalk, inasmuch as Ehrenberg has proved it to be a mere collection of microscopic shells; but Prof. Ehrenberg himself informs me, that even these shells, which require a high magnifying power to render them visible, are in their turn composed of infinitesimal crystals of calcareous spar. In this dilemma one way remains open to us: we will allow the objection to stand, and follow it out to its inevitable consequences; if these are opposed to fact, the objection necessarily falls.

Let us suppose the bismuth powder to be rearranged, so that the perfect crystal from which it was obtained is restored. In this case the axes of all the little component crystals are parallel, they work all together, and hence their action must be greater than if only a majority of them were parallel. In a bismuth *crystal*, therefore, the difference of action in the line of the magnecrystallic axis, and in a line perpendicular thereto, must be a maximum. It must, for example, be greater than any difference which the model of bismuth powder can exhibit; for a portion of the force attributed to the axes must in this case be annulled by the confused grouping of the little component crystals. In the words of Professor Thomson, it is merely a balance of action brought about by predominance, which can make itself manifest here. Hence if we measure the repulsion of the crystal in a direction parallel to the principal cleavage, and in a direction perpendicular to it, and also measure the repulsion of the model in the line of compression and in a line perpendicular to it, the ratio of the two former repulsions, that is, of the first to the second, must be greater than the ratio of the two latter, that is, of the third to the fourth.

Turning to Tables IX. and X., we see that the ratio of the repulsion of the crystal in the direction of principal cleavage to the repulsion in a direction perpendicular to the same is expressed by the fraction $\frac{15}{11}=1{\cdot}36$. Turning to Tables XI. and XII., we find that the ratio of the repulsion of the model in the line of compression to the repulsion in a line perpendicular to it is expressed by the fraction $\frac{3}{2}=1{\cdot}5$. In the latter case, therefore, we have the greatest differential effect; which result, were the repulsion due to the mere predominance of axes, as urged by Mr. Thomson, would certainly bear a suspicious resemblance to the conclusion that a part is greater than the whole. This result has been entirely unsought. The models were constructed with the view of establishing the general fact, that the repulsion in the line of compression is greatest. That this has fallen out in the manner described is a pure accident. I have no doubt whatever that models might be made in which this difference of action would be double of that exhibited by the crystal.

The case, however, is not yet free from suspicion; the gum-water with which it is necessary to bind the powder may possibly exert some secret influence. When isinglass or jelly is compressed, we know that it exhibits optical phænomena similar to those exhibited by crystals; and the squeezing of the metallic dough may induce a kind of crystalline structure on the part of the gum sufficient to produce the phænomena observed.

An experiment to which I was conducted by the following accident will set this doubt, and I believe all other doubts regarding the influence of compression, completely at rest. Having repeated occasion to refer to the deportment of crystals in the magnetic field, so as to be able to compare this deportment with the attraction or repulsion of the entire mass upon the torsion balance, through the kindness of Professor Magnus, the great electro-magnet of the University of Berlin* was placed in the room where I experimented. One morning a cube of bismuth was suspended between the moveable poles, and not knowing the peculiarities of the instrument, I chanced to bring the poles too near each other. On closing the circuit, the principal cleavage of the crystal receded to the equator. Scarcely however was this attained, when the poles were observed moving towards each other, and before I had time to break the circuit, they had rushed together and caught the crystal between them. The pressure exerted squeezed the cube to about three-fourths of its former thickness, and it immediately occurred to me that the theory of proximity, if it were true, ought to tell here. The pressure brought the particles of the crystal in the line of compression more closely together, and hence a modification, if not an entire reversion of the previous action, was to be expected. Having liberated the crystal, I boiled it in hydrochloric acid, so as to remove any impurity it might have contracted by contact with the iron. It was again suspended between the poles, and completely verified the foregoing anticipation. The line of compression, that is, the magnecrystallic axis of the crystal, which formerly set from pole to pole, now set strongly equatorial. I then brought the poles intentionally near each other, and allowed them to close once more upon the already compressed cube; its original deportment was thereby completely restored.

* A notion of the power of this instrument may be derived from the fact, that the copper helices alone which surrounded the pillars of soft iron weighed 243 pounds.

This I repeated several times with several different crystals, and with the same unvarying result; the line of compression always stood equatorial, and it was a matter of perfect indifference whether this line was the magnecrystallic axis or not. The experiment was then repeated with a common vice. I rubbed the letters from two copper coins with sandstone, and polished the surfaces; between the plates thus obtained various pieces of bismuth were placed and squeezed forcibly together; in this way plates of bismuth were procured about as thick as a shilling, and from half an inch to an inch in length. Although the diamagnetism of the substance tended strongly to cause such a plate, suspended from its edge between the poles, to take up the equatorial position, although the force attributed to the magnecrystallic axis worked in each case in unison with the diamagnetism of the mass, every plate set nevertheless with its length from pole to pole, and its magnecrystallic axis equatorial.

This superior repulsion of the line of compression manifests itself upon the torsion balance also. The cubes of bismuth crystal already made use of were squeezed in a vice to about four-fifths of their former thickness; the line of compression in each case being perpendicular to the principal cleavage, and consequently parallel to the magnecrystallic axis. From the masses which were thus rendered oblong, two new cubes were formed; these, laid upon the torsion-balance in the positions indicated in the tables, gave the following results:—

Table XIII.—Bismuth crystals, compressed cubes. Plane of most eminent cleavage parallel to axes of magnets.

α.	T.
20	7·8
30	21
40	47
45	67
50	101

Table XIV.—The same cubes. Plane of most eminent cleavage perpendicular to axes of magnets.

α.	T.
20	9
30	25 5
40	57·3
45	79
50	113

Looking back to Tables IX. and X., we see that the line which was there most strongly repelled is here repelled most feebly, and *vice versâ*, the change being due to compression.

I have been careful to make similar experiments with substances concerning whose amorphism there can be but little doubt. A very convenient substance for showing the influence of compression is the white wax used in candles. The substance is diamagnetic. A little cylinder of the wax suspended in the magnetic field set with its axis equatorial. It was then placed between two stout pieces of glass and squeezed as thin as a sixpence; suspended from its edge, the plate thus formed set its length, which coincided with the axis of the previous cylinder, axial, and its shortest dimension equatorial.

The plate was then cut into little squares, these were laid one upon the other and then pressed together to a compact cubical mass. Two such cubes were placed upon the torsion-balance, and the repulsions in the line of compression, and in a line perpendicular to the same, were determined—the former was considerably the greater.

The pith was scooped from a fresh roll, placed between the glass plates, and squeezed closely together; after remaining in the vice for half an hour, an oblong was taken from the plate thus formed, and suspended from its edge in the magnetic field; it set like a magnetic body, with its length from pole to pole. The mass was diamagnetic, its line of compression was repelled, and an apparent attraction of the plate was the consequence.

Fine wheat-flour was mixed with distilled water into a stiff paste, and the diamagnetic mass was squeezed into thin cakes. The cakes when suspended from the edges set always with their longest dimension from pole to pole, the line of compression being equatorial.

Rye-flour, from which the Germans make their black bread, was treated in the same manner and with the same result.

I have an oblong plate of shale from the neighbourhood of Blackburn in Lancashire, which imitates Plücker's first experiment with tourmaline with perfect exactitude. The mass is magnetic, like the tourmaline. Suspended from the centre of one of its edges, it sets *axial*; this corresponds to the position of the tourmaline when the optic axis is vertical. Suspended from the centre of the adjacent edge, it sets even more strongly *equatorial*; this corresponds with the tourmaline when the optic axis is horizontal. If the eyes be closed, and the respective positions of the plate of shale ascertained by means of touch, and if the same be done with Plücker's plate of tourmaline, it will be impossible to distinguish the one deportment from the other.

Whoever denies the influence of proximity must be prepared to answer the following questions:—How is it possible that a greater differential action can be exhibited by a cube of powdered bismuth than by the crystal itself? What is it that causes the magnecrystallic axis of the crystal to forsake its usual position and to set equatorial when the mass is compressed in the direction of the said axis? He must further assume a crystalline structure on the part of wax, flour, shale, and the pith of fresh rolls.

With regard to the experiment with the cherry-tree bark, I have a bar of chemically pure bismuth which does not contain a trace of magnetism, and which exhibits the precise phænomena observed with the bark. These phænomena do not therefore necessitate the hypothesis of two conflicting forces, the one or the other of which predominates according as the poles of the magnet are more or less distant. I have already commenced an investigation in which the deportment of the bark and other phænomena of an analogous nature will be more fully discussed.

Every physicist who has occupied himself experimentally with electro-magnetic attractions must have been struck with the great and speedy diminution of the force by which soft iron is attracted in the immediate neighbourhood of the poles. In experiments with spheres of soft iron, I have usually found that a distance of $\frac{1}{100}$th of an inch between the sphere and the magnet is sufficient to reduce the force with which the former is attracted to $\frac{1}{10}$th of the attraction exerted when the sphere is in contact. To any one acquainted with this fact, and aware, at the same time, of the comparative sluggishness with which a bismuth ball moves in obedience to the repulsive force even when close to the poles, a law the exact reverse of that affirmed by Plücker must appear exceedingly probable.

The bismuth balls were placed upon the torsion-balance; on the top of one of them a particle of an iron filing was fixed, and with this compound mass the space opposite to a core excited by a current of 50° was sounded. The beam was brought by gentle pushing into various positions, sometimes close to the magnet, sometimes distant. The position of equilibrium for the beam when the core was unexcited was always zero. When the beam was pushed to a distance of 4° (about $\frac{3}{10}$ths of an inch) from the core end, on exciting the magnet it receded still further and rested against a stop at 9° distant. When the current was interrupted the beam left the stop and approached the magnet; but if, before it had attained the third or fourth degree, the circuit was closed, the beam was driven back and rested against the stop as before.

Preserving the current constant at 50°, the index of the torsion-head was turned gently against the repulsion, and in this way the ball was caused slowly to approach the magnet. The repulsion continued until the glass fibre of the beam pointed to 2°; here an *attractive* force suddenly manifested itself, the ball passed speedily on to contact with the core end, to separate it from which a torsion of 50° was requisite.

The circuit was broken and the beam allowed to come to rest at zero, a space of about $\frac{1}{12}$th of an inch intervening between the ball and the end of the magnet; on closing the circuit the beam was *attracted.* The current was once more interrupted, and the torsion-head so arranged, that the beam came to rest at 3° distant; on establishing the current again the beam was *repelled.* Between 0° and 3° there was a position of unstable equilibrium for the beam; from this place to the end of the magnet the attraction was triumphant, beyond this place repulsion prevailed.

Here we see, that on approaching the pole, the attraction of the magnetic particle mounts much more speedily than the repulsion of the diamagnetic ball; a result the reverse of that arrived at by M. Plücker, but most certainly coincident with that which everybody who has studied electro-magnetic attractions would expect. Shall we therefore conclude that 'magnetism' increases more quickly than 'diamagnetism?' The experiment by no means justifies so wide a generalization. If magnetism be limited to the attraction of soft iron, then the above conclusion would be correct; but it is not so limited. Plücker calls the attraction of his watch-glass magnetism, the attraction of a salt of iron bears the same name, and it so happens that the attraction of a salt of iron on approaching the poles increases incomparably more slowly than the attraction of iron itself. The proof of this remarkable fact I will now proceed to furnish.

From one end of a very fine balance a sphere of soft iron, $\frac{1}{4}$th of an inch in diameter, was suspended. Underneath, and about $\frac{1}{8}$th of an inch distant from the ball when the balance stood horizontal, was the flat end of a straight electro-magnet. On sending a current of 30° through the surrounding helix, the ball was attracted, and the force necessary to effect a separation was measured: it amounted to 90 grammes. A plate of thin window-glass was then placed upon the end of the magnet, and the ball allowed to rest upon it. The weight necessary to effect a separation, when the magnet was excited by the same current, amounted to 1 gramme. Here an interval of about $\frac{1}{15}$th of an inch was sufficient to reduce the attractive force to $\frac{1}{90}$th of that exerted in the case of contact.

A sphere of sulphate of iron, of somewhat greater diameter than the iron ball, was laid upon one end of the torsion-balance; the opposite core was excited by a current of 30°, and the force necessary to effect a separation of the core and the sphere was determined: it amounted to 20° of torsion. The same plate of glass used in the last experiment was placed against the core end, and the force necessary to effect a separation from it with a current of 30° was also determined. The difference, which in the case of the soft iron amounted to $\frac{89}{90}$ths of the primitive attraction, was here scarcely appreciable. At a distance of $\frac{1}{15}$th of an inch the sphere of sulphate of iron was almost as strongly attracted as when in immediate contact.

Similar experiments were made with a pellet of carbonate of iron, and with the same result. At a distance of $\frac{1}{7}$th of an inch the attraction was two-thirds of that exerted in the case of contact. An interval of $\frac{1}{1000}$th of an inch is more than sufficient to effect a proportionate diminution in the case of soft iron.

A salt of iron in the immediate neighbourhood of the poles behaves like iron itself at a considerable distance, and the deportment of bismuth is exactly similar. A slight change of position will make no great difference of attraction in the one case or of repulsion in the other. To make the antithesis between magnetism and diamagnetism perfect, we require a yet undiscovered metal, which shall bear the same relation to bismuth, antimony, sulphur, &c., which iron does to a salt of iron. Whether nature has such a metal in store for the enterprizing physicist, is a problem on which I will hazard no conjecture.

Principal Results of the foregoing Investigation.

1. *The repulsion of a diamagnetic substance placed at a fixed distance from the pole of a magnet is governed by the same law as the attraction of a magnetic substance.*

2. *The entire mass of a magnetic substance is most strongly attracted when the attracting force acts parallel to that line which sets axial when the substance is suspended in the magnetic field; and the entire mass of a diamagnetic substance is most strongly repelled when the repulsion acts parallel to the line which sets equatorial in the magnetic field.*

3. *The superior attraction and repulsion of the mass in a particular direction is due to the fact, that in this direction the material particles are ranged more closely together than in other directions; the force exerted being attractive or repulsive according as the particles are magnetic or diamagnetic. This is a law applicable to matter in general, the phænomena exhibited by crystals in the magnetic field being particular manifestations of the same.*

Berlin, June 1851.

XXVIII. *On the Anticlinal Line of the London and Hampshire Basins.* By P. J. Martin, *Esq., F.G.S.*

[Continued from p. 134.]

THE concluding words of the foregoing Memoir* on the western part of this line might serve for an introduction to what follows. But as many of my readers may not have seen Dr. Buckland's dissertation, before mentioned, and many more not know anything of my former publications on this subject, I will quote, in addition, the following passage from the latter, as the key to further discussion; and as the proposition now before us for elucidation.

"The strata which compose these basins, then, previously in a horizontal position, suffered disruption; and in the act of basining (whether by the elevation of the sides, or the subsidence of the central parts, is not now material) all their parts were deeply and extensively fissured, in an order correspondent with that act, producing, with the help of diluvian action, a system of longitudinal and transverse valleys answering to the double inclination (the dip and lateral bearings, or strike) of their fractured masses, and a consequent removal of the broken materials, brought within the range of the denuding force. The effect of raising from the horizontal position, or in any other way stretching a ponderous and frangible body, is to produce a division of its parts, in such order and direction as its varying strength and tenacity dictate; the fractured parts taking their places according to their magnitude or gravity, or the disposition of those which support them. This irregular fracture, alternate elevation and subsidence, and settling of parts thus disturbed, are well exemplified in the familiar operation of the heaving of the spade in digging. If the earth be tenacious and the action steady, it tears with such a divergence of the principal rents as will be here described, and the more friable parts are seen dropping in, in such a way, and in such proportion as the moving power dictates and their structure allows. If another illustration were necessary, it might be found in what we observe in the elevation and cracking of the flour which covers the fermenting nucleus in a baker's trough†."

The evidence in support of this proposition,—the elevation of the great anticlinal of the London and Hampshire basins, and concomitant abrasion, on and around that line of disturbance,—may be classed under four heads.

* It is to be remembered that that paper was read to the Geological Society in 1840; what follows is of present date.

† Geol. Mem. of Western Sussex, p. 59.

1. The general arrangement or geographical aspect of the country.

2. Its valleys, and lines of drainage.

3. The lacerated state of some of its escarpments, still to be discerned behind the detrital materials of age, and atmospheric agencies.

4. The nature and disposition of the diluvium on every part of its surface.

The first of these is so familiar, in a general sense, and has been so often described, that it would seem superfluous to take it into consideration;—and yet it will be well to cast a geological eye over it. If we take our stand on the lowest beds of the upheaval, say at the well-known point of Crowborough, on the Ashburnham limestone (supposed by Dr. Mantell to be the lowest in the series), or on the sand rocks of Hastings, we find all around us a *quaquaversal* dip; a succession of escarpments or basset edges, confluent at either end;—westward in Sussex and Hampshire, eastward in the Boulonnais. The successional courses of clays, sands and limestones of the Hastings sands, after skirting the "Forest ridge," form saddles in the west of Sussex. To these succeed the lower greensands, the galt, the malm-rock or upper green, and the chalk; afterwards the tertiary beds, still confluent above the Hampshire chalk, in the shape of patches of plastic clay and sand; and the gray-weathers of the Hampshire and Wiltshire downs*. Turning to the east, we find that this confluence is maintained in like manner in all the beds of the *Bas Boulonnais*; substituting the more ancient formations, which take the place of the Wealden (there reduced to a very small compass). And this confluence is maintained there also in the tertiary beds, as in Hampshire, in the shape of relics spread over the chalk hills of the surrounding *Haut Boulonnais*. These facts are pretty well known; but for the satisfaction of those who have not turned their attention especially to this subject, we may cite the authority of the French geologist, M. Rozet, who in 1828 followed Dr. Fitton in a description of this part of France. "On rencontre des lambeaux de terrain tertiaire sur les montagnes qui limitent le Bas Boulonnais. Au dessus de Tingry, de Niembourg, près d'Hubersent, de Courset, &c., on exploite des lambeaux d'un grès siliceux, très-semblable à celui de Fontainebleau†," &c. M. Rozet also speaks of the same sort of remains found dispersed in the diluvium of the Boulogne denudation; to which we may refer

* Vide Dr. Buckland's paper (Geological Transactions) and my foregoing Memoir.

† Description Géognostique du Bas Boulonnais. Par M. Rozet. Paris, 1828, p. 31–36.

when speaking of the drift on this side of the Channel. This gentleman ventures also to use the word "débâcle," and speaks constantly of the denudation as a "great catastrophe." With all this evidence before us, it seems then to be no great stretch of the imagination to suppose that all the tertiaries follow the secondary in the same order of denudation. From the nature of their materials, we do not wonder that they do not present the bold and prominent escarpments of the chalk and greensands; they lie beveled off in succession, as they crop out within the borders of the so-called chalk-basins. At the back of the Surrey Hills and South Downs we find the plastic clay and sands thinned out on the chalk. More remotely from the chalk hills, succeed the beds of London clay and tertiary limestones;—at Bracklesham and Bognor on one side, and in the bed of the Thames and at Sheppy on the other. Where the materials of these tertiaries are of firmer texture and have afforded more resistance, and where their synclinal position has given them protection, we still find some signs of escarpment in them, as, east of Croydon, in the Addington and Keston Hills, and north of Farnham, at Farnham Beacon, and in the line of country north of the Hogsback. The only escarpment exhibited by the tertiaries south of the South Downs, and that is synclinal, is the cliff at Castle Hill, described by Dr. Mantell*. From this point westward great ravages have been made; but I can say with confidence that considerable relics of these beds exist in the synclinal of what I have called the chalk "Outlier-by-protusion" at Highdown Hill near Worthing†; again in the eminences between Arundel and Angmering. Shingle beds of this æra show strongly also at Box-grove. And all the tract of country called the "Manwood," between Chichester and Bracklesham, is plastic clay, with an occasional sprinkling of diluvium. West of Chichester, and north of Emsworth again, the plastic clay emerges from beneath the thick beds of drift that abound in this line of country. And the forest of Bere, which is the synclinal of Portsdown, is wholly tertiary. With this comprehensive view before us of the general denudation of all the beds on and about the great line of elevation, and looking on it as the last great change that has come over the S.E. of England, we discard all notions of marine deposit of a more recent date, or in other words, as asserted in my memoir of 1828 before alluded to, "the chalk basins so often spoken of, never could have been areas of deposit for beds not to be found also on the denuded surfaces, at the same level."

2. We pass now to the consideration of the second order of phænomena,—the valleys and lines of drainage. Although it is

* Mantell's Geology of S.E. of England, p. 55.
† Geol. Mem. of Western Sussex, pp. 95, 96.

abundantly apparent that the same causes have produced the same effects in the whole course of our line, as regards these surface-phænomena, two circumstances have conspired to give a broader aspect and a more decided character to them in that part of it which is generally called the "denudation of the Weald."

First, the greater violence of disruptive force in that part of it; and secondly, the greater variety in the strata there exposed, and their greater tenacity and durability, as compared with the more ductile and friable chalk.

A description of the cross-fractures and drainage of the Weald was begun by myself in 1828, continued in the foregoing memoir in 1840, and prolonged by Mr. Hopkins, in illustration of his "Theory of Elevation" (Geol. Trans. vol. vii.). I accept Mr. Hopkins's description of the structure of the Weald, as a faithful representation of some of its most prominent features; and if I am able to add anything to that gentleman's exposition of these surface-changes, it is because my long residence on the spot has made me familiar with many minor details, of which a cursory observer would not be cognizant. If I differ from him in my interpretation of the phænomena in question, it may be because of my imperfect knowledge of the data on which he proceeds. It is probable that his "Theory of Elevation" may be founded in nature, and every one will rejoice that the exact sciences can be brought to bear in this branch of geological research; and that a theory should be propounded as convincing as a "theory of glacier motion," or of "wave of transport," or of any other object of geological dynamics. Without calling in question the general propositions advanced with so much mathematical precision by Mr. Hopkins, but as he allows only with approximate results, we may be allowed to doubt if they meet all the requirements of the case, or explain fully all the appearances exhibited in the structure of the Weald. I have said that if we could obtain a section of that district, it would exhibit all the contortions of the older schists*. Subordinate anticlinal lines assist in making up the great anticlinal, and valleys of elevation exist all over its surface, subordinate to the great valley of which they form parts. I consider these minor anticlinals *as so many foldings* of the strata, produced not by lateral pressure, but by *lateral resistance* in their struggle upwards; and all the lesser flexures and faults, as so many puckerings and rents to be included in the same category. Mr. Hopkins considers the principal anticlinals in the light of fissures. I am not prepared altogether to dispute the proposition; lines of disturbance may perhaps be changed in their character as they are propagated through masses of various densities, and various degrees

* Phil. Mag., p. 133 of the present volume.

of tenacity. Fissures and faults abound in limestones and sandstones, flexures in clays. Both Dr. Mantell and Mr. Hopkins have observed perpendicular faults of great magnitude in the Hastings sands. There are many minor ones in my own neighbourhood,—in the lower greensand; and there are some remarkable downcasts along the Surrey Hills. I will here describe one of them. Where the traces of the Peasemarsh anticlinal are lost near Albury, a line of disturbance takes off in a north-easterly direction, through the chalk between Shere and Horseley. The farms in the Ordnance Map of Pobley, Green-dean and Potsdean, between these two villages, mark its direction, till it runs into the transverse fissure of the Mole near Mickleham. Hereabout are the "swallow-holes" of the Mole, in which that river is lost in the summer season; and they are probably caused by the joint operation of these two lines of disturbance. Taking the direct footpath from Horseley to Shere, after crossing the chalk downs of the former place, you find yourself on a distinct though low escarpment of chalk descending on Netley Heath, which consists of sand of the plastic clay formation. Crossing Netley Heath, you come again on the chalk, and the descent of the deep escarpment to Shere, makes you sensible of having traversed the whole thickness of the latter formation. East of the Vale of Mickleham there are patches of tertiary, as at Headley, with signs of much disturbance thereabout, as at Pebble Hill,—one of those deep fissures filled with loam and shingle so often met with in the chalk.

To return to the structure of the Weald. Mr. Hopkins has distinguished flexures from anticlinals: why should they differ, except as in degree or shape? The flexure, which by its sudden dip north gives rise to the Hogsback, is an anticlinal, twisted to one side, that is, of unequal dip*. As it recedes from the chalk, it becomes a perfect anticlinal at Peasemarsh; and the Vale of Peasemarsh is a true "Valley of Elevation;" as distinctly so as that of Kingsclere, first described by Dr. Buckland. Again, Mr. Hopkins has noted a flexure at Pulborough north of the line of the Greenhurst anticlinal. This flexure is no more than the commencement or northern edge of the synclinal of the last-mentioned line, and the trough of galt at Hardham; the river Arun taking its course along the same depression. These flexures, when not seeming to have any relation to distinct anticlinals, very much modify the surface arrangements; and are instrumental also in the production of springs and water-courses; and, moreover, like faults and anticlinals, they prolong the outcrop of the strata in which they are found. There is a remarkable one of this sort crossed by the lane leading from West

* Phil. Mag. p. 48.

Chiltington to Wood's Hill. In the Boughton quarries, near Maidstone, there is a flexure of this kind; and the springs which run on the south side of these quarries, and come down from Langley, are most probably thrown out by this flexure. And to all appearance (although I have not been able to find a section to enable me to speak positively) the course of the Medway after it enters the greensand country is for some miles determined east and west (by Wateringbury) by a flexure or minor contortion. This flexure also assists in prolonging the extent of the lower greensand country in the Maidstone district*. In short, the contortions and flexures, and smaller anticlinals superadded to the larger ones, over all the Weald denudation, are almost innumerable. I will undertake in the drives of two mornings to show any person, competent to judge of these things, beside the great line of Greenhurst, at least six well-marked smaller ones, and as many flexures and faults giving shape more or less to the neighbouring lands.

We have hitherto confined ourselves to the consideration of the longitudinal anticlinal folds and contortions. But before we take a general view of the manner in which these folds on coming to the surface yielded to the tensive power, and opened to form fissures, and give admission to the denuding floods, and so eventually became the valleys and water-courses we now see; we must advert to another modification of the disruptive action, not so potent for the production of surface-changes, because not so extensive, but still of much influence, and inseparable from the consideration of the one grand and total act of upheaval. I mean the frequent occurrence of transverse anticlinals, opening up transverse valleys *distinct from those which appear to be the result of the cross fracture of the longitudinal ones, at their points of greatest tension.* I can best convey the idea of these transverse flexures by reference to a case or two in point, and eminent examples, on both sides of the denudation. On looking at the

* Of the manner in which anticlinals, or flexures of any kind, prolong the extent of exposure of a particular stratum, we may cite the following. The transverse fissure of the Mole changes the dip in the line of the Leith Hill country, and the escarpment of the lower greensand falls back northward to Brockham, Betchworth and Reigate; and between the latter place and Crawley there is a very broad expanse of Weald clay. One flexure, at least, was wanted to account for this broad expanse; and one was pointed out to me by an intelligent observer, who caught sight of it in a road cutting at Norwood Place, between Leigh and Horley. It is there a small valley of elevation, and conveys an affluent of the Mole, and is probably a continuation of Mr. Hopkins's Bidborough line. I am told also by Dr. Fitton that there is another notable flexure in the Maidstone district, which brings up the Weald clay through the greensand south of Pennenden Heath, a circumstance which might be predicated of the broad expanse of the greensand country east of the Medway.

Ordnance Map, it is to be observed that there is a very remarkable valley separating the broad expanse of lower greensand country in the neighbourhoods of Maidstone and of Sevenoaks, into two groups: I shall call it the valley of Plaxtole, from the village of that name. Here all the thick and tenacious beds of the Kentish-rag, as well as the upper beds of the greensand, have been swept clean away, so that the drainage from the country up to the foot of the chalk hills, above Ightham and Rotham (through which this fissure does not appear to extend), is brought down by a rivulet running in the bottom of the valley, due south toward the Medway, near Tunbridge. The escarpments of this valley are anticlinal; and there is, unless it has been lately quarried away, a remarkable group of rocks on the road from Plaxtole to Crouch, tilted westward, and giving undeniable testimony of the extraordinary swell of the Weald clay below. Another remarkable instance of this sort of anticlinal may be seen in a ridge running north and south between Wotton and Portnail, and crossed by the road from the former place to Dorking. This anticlinal ridge throws the watershed of the country westward into the Wey by Albury and Shalford, and eastward into the Mole by Dorking. And there can be no doubt that the copious springs which arise in that part of the Leith Hill country, each side of this anticlinal, are thrown out by the same disturbance. If we turn to the south side of the Weald again, we find examples of the same sort of transverse anticlinal disposition. The affluent of the western Rother, which in my early publication I have called "the Lod," cuts the high grounds of lower greensand at Lodsworth transversely; and the anticlinal disposition is to be seen at Halfway-bridge on the Petworth and Midhurst road. Again, the same disruption is to be observed where another affluent of the river before-mentioned runs by Petworth. The tilting of the beds east and west is visible in the hollow ways near the bridge, on each side of the stream at Haslingbourne*.

Of the transverse valleys, and the fissures in which they originated, which properly belong to the more prominent longitudinal flexures, and which Mr. Hopkins has made use of in illustration of his theory of elevation, I will cite two remarkable examples. They have already been cursorily mentioned in my former memoir (pp. 48 and 134), but it will be well to pay more particular

* I quote from memory, but I think that there appeared in the "Proceedings of the Geological Society" some time since, a description of a transverse upheaval like these here spoken of, which could be traced all across the Weald from the neighbourhood of Bletchingley to the South Downs, controlling and directing the watershed east and west. I have not examined the ground, but I have no doubt of the fact, as there described.

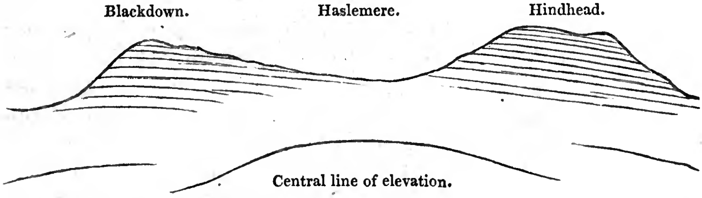
attention to them now, as offering in themselves an epitome of the very extensive disruptive operation, to which, in my earliest dissertation on this subject, I gave the name of "the cross-fractures of the Weald denudation." The anticlinal of Peasemarsh or great flexure of the Hogsback shows its greatest intensity at the first-mentioned place, that is, the greatest elevation of the subjacents of the greensand are there; and there the transverse fracture shoots off northward to transmit the Wey through the chalk at Guildford, and southward, to bring down a tributary to the same river from the Weald. If we turn to the great anticlinal of Greenhurst, under the South Downs, we find a still more remarkable example of the same arrangement. This flexure acts most forcibly between Warminghurst and Henfield; there, the Weald clay rises highest, the greensand is entirely swept away, and a saddle of Weald clay left, with a small outlier of the sand at Ashurst. In this part of the "Valley of Elevation" thus formed, two transverse valley fissures present themselves; one to convey the Adur through the South Downs in a straight line to Shoreham. The other, a little further west, is the Vale of Findon, through which runs the Worthing road.

Over and above these various larger lines of contortion and fracture, it may be observed, as before adverted to, that change of dip has had some influence in fissuring and breaking up these masses. Every such change, if sudden, would produce fracture; if more gradual, it would produce contortion and crumbling. And many of the minor valleys and slopes and lines of drainage are evidently the result of the crackings and twistings of the minor disturbances before spoken of. So that, on a general review of these phænomena, one is led to the conclusion, that, although some order is to be observed, answering to the tensive influence,—greatest in the shorter axis of the upheaval, and less in the longer axis,—yet the result answers to my earliest proposition,—"the effect of raising from the horizontal position, or in any other way stretching ponderous and frangible bodies, is to produce a division of their parts in such order and in such direction as their varying strength and tenacity dictate,"—and that all the surface-changes of the Weald answer to this predicament.

There is yet one remarkable feature to be noticed before we quit this important branch of our subject. It is the broad and expanded surface and unvarying course of the central anticlinal line, from which we see all the principal subordinate ones rolled back, as it were, on either side.

If we take our stand on that part of it called the "Forest Ridge,"—the ground made familiar to all who take interest in the vestiges of extinct organisms, by the labours of one in whom palæontology is a passion, and whose indomitable energy triumphs over the difficulties of position and infirmities of health, and makes himself the object of our own "especial *wonder*"—from the commanding points of this elevation, we look north and south over the long ranges of longitudinal flexures, and observe that the most strongly marked and most influential of these are the most distant; they lift the chalk downs on either side and regulate their position. The strong flexure of the Hogsback is propagated from Farnham eastwards in a strong line of elevation, at least as far as Sevenoaks*; and it is more than matched by the Greenhurst line, which, with little intermission, regulates the escarpment of the South Downs from Beachy Head to the borders of Hampshire. If we advance further west and take our stand at Itchingfield or Five Oaks, we find that we are still within the range of the same disruptive courses; and looking west from thence, we see in the profile of the country before us, and at twelve miles distance, the passage of the central line of elevation through the lower greensand, in the shape of a valley of elevation at Haslemere, flanked by the bold eminences of Black Down on the south and Hindhead on the north.

Profile of the Haslemere country, seen from high grounds at Itchingfield.

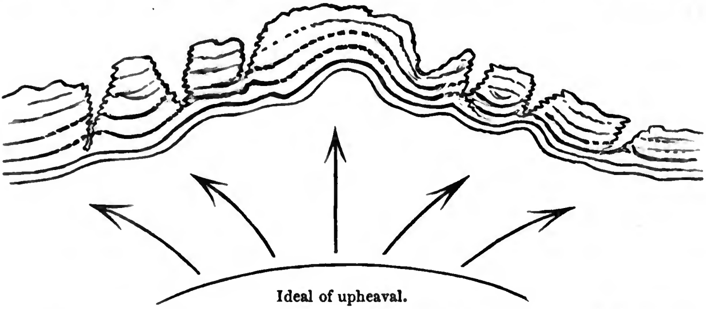


Let us advance again to Var Down in the Alton Hills, and thence along the chalk dome of Hampshire, and we find our position again flanked by similar disruptions; the Winchester, Warminster and Wardour lines of flexure on the south, and the Burghclere Hills, and the anticlinal vales of Pewsey and Kingsclere on the north. In all this long range of country the same arrangement of hill and valley, the joint operation of fracture and aqueous erosion, and the same transverse river-courses obtain; a structural arrangement that cannot but compel the belief of a unity of cause.

The superior breadth and volume of this central line of elevation within the Weald denudation, its uniform and almost unbroken course, make it the body of which the lesser anticlinals are the wings. And from the greater prominence, and more notable character of the distant flexures under the chalk downs

* Hopkins, Geol. Trans. *loc. cit.*; and Dr. Fitton, *vide supra.*

on each side, we infer that it is there the antagonistic resistance more effectually overpowers the momentum of the central mass*.



Ideal of upheaval.

Entertaining the idea that all these contortions are superficial and contemporaneous, and the result of a leverage from below more uniform and of much wider extent, we hasten to the conclusion, that the protrusion of the Wealden beds through the chalk, and of the chalk through the tertiaries in this line of elevation, extensive as it is, is only a part of a much greater whole. We shall revert again to this consideration in the sequel.

[To be continued.]

XXIX. *Experiments on the Conducting Powers of Wires for Voltaic Electricity.* *By* C. L. Dresser, *Esq.*†

THE instrument used in these experiments was the glass thread galvanometer of Ritchie, described in the Philosophical Transactions‡. This instrument, though one of the most perfect kind, easy of construction, well adapted for the measurement of electro-magnetic forces, and extremely accurate, has not received that attention from scientific men to which the facility of its use entitles it. Requiring no calculation, a vast number of experiments may be read off in rapid succession.

A few alterations were made in its construction.

1. The graduated card placed under the needles was discarded as being no measure of the forces exerted, and a plain card with a black mark under the centre of influence of the conducting wires substituted. To this mark the needles were carefully adjusted at every experiment.

* It is not to be supposed that we mean by this, that the Downs *quoad* Downs had any share in this resistance, but only that the central impulse gradually fading in the distance is there more successfully resisted. The long synclinal that extends with but little interruption northward from Hindhead, terminating in the sudden flexure of Peasemarsh and the Hogsback, is strongly illustrative of the antagonism here spoken of.

† Communicated by the Author.

‡ Philosophical Transactions, 1830, p. 218.

2. The graduated card at the top was enlarged to five inches diameter, and carefully graduated to degrees; and by an index traversing this card, the degrees of torsion necessary to bring the deflected needle vertical to the black mark on the lower card was read off easily to a fraction of a degree.

3. The graduated plate turned on its own axis independently of the axis of the glass thread, rendering the adjustment of the needles easy and perfect.

4. The needles were considerably increased in size and highly magnetized

With these alterations the action of the galvanometer was certain and delicate, returning after even a deflection of a thousand degrees, or three times round the card, with certainty to the index mark on the lower card; and the same experiment repeated corresponding to the fraction of a degree.

The battery used was my gas-carbon battery, and the following means were adopted to keep it constant.

1. The nitric acid cell was filled with the acid of commerce, but the zinc cell only half-filled with dilute sulphuric acid.

2. The prism of carbon was suspended at its top to a rack-work, by which its immersion to a greater or less depth was regulated; consequently any required amount of electricity obtained.

With these precautions a constant current of electricity was maintained for hours; rarely varying, after effecting a torsion of three or four hundred degrees, one degree for hours. By this means also, at all times the same amount of current could be obtained, rendering it easy to recommence the experiments.

Table I.—Battery power 400. Each wire was No. 20.

Feet.	Copper wire.	Differences.	Feet.	Iron wire.	Differences.
1	398		1	330	
2	380	18	2	280	50
3	365	15	3	240	40
4	352	13	4	210	30
5	340	12	5	190	20
6	330	10	6	172	18
7	320	10	7	158	14
9	296	24	9	135	22
10	286	10	10	128	7
12	269	17	12	112	12
14	254	15	14	100	12
16	240	14	16	92	8
18	230	10	18	85	7
20	220	10	20	78	7
			22	73	5
			24	68	5
			26	64	4
			28	60	4

From the above table of experiments, it is evident that the often quoted law of the conducting power of wire being inversely as the length does not obtain in short lengths. But there is an evident intimation of some other law, and probably different for different metals.

Broke glass thread. New thread gives 300 without altering battery power.

Table II.—One cell. No. 16 wire.

Feet.	Copper.	Differences.	Iron.	Differences.
1	282		256	
2	275	7	235	21
3	268	7	217	18
4	262	6	200	17
5	256	6	187	13
6	148	5	175	12
7	138	5	164	9
8	133	5	157	7

From this table, compared with Table I., it does not appear that with a thicker wire there is any nearer approach to the old law, but also that some other law obtains.

Table III.—No. 16 wire. Battery power 400. Intensity two cells.

Feet.	Copper.	Differences.	Iron.	Differences.
1	400		355	
2	391	9	320	33
3	382	9	294	26
4	476	6	270	24
5	370	6	252	18

Increase of intensity does not appear to approach near to the supposed law.

Table IV.—Wire measured with a micrometer in hundredths of an inch. Battery power 207. One foot of wire. Diameter of galvanometer wire 740 of an inch.

Measure.	Copper. Current conducted.	Measure.	Iron. Current conducted.
370	190	360	129
480	195	510	165
700	206½	640	182
740	207	720	188

The wires of iron and copper on parallel lines were said to be of the same gauge, but the micrometer showed them to be of very different diameters. This table does not coincide with the law of the conduction of wires of different diameters being as the squares of their diameters.

Power of hydrogen to abstract the heat produced by the passage of electricity.

Battery power 410. Current through steel wire 175 hundredths of an inch.

Quantity conducted.

220	.	wire red-hot in air.
310	.	in hydrogen, and invisible in the dark.

In this experiment battery power not observed.

Same wire as above.

In air red-hot	220
In current of air, quite cold	270

It would appear that the heating power of a current of electricity diminishes the power of conduction; also that hydrogen, by absorbing the heat, has the same effect as a current of cold air.

Table V.—No. 20 wire placed in the air-bulb of sulphuric acid thermometer.

Battery power.	Differences.	Current conducted.	Differences.	Degrees of heat.	Differences.
67		58		26	
92	25	73	15	44	18
107	15	84	11	75	31
124	17	91	7	90	15
145	21	100	9	124	34
170	25	102	2	143	19
202	32	108	6	160	17

These experiments, very tedious and difficult to conduct, do not appear to indicate any particular law.

Much in this department of electricity appears yet to be done before we are able to define the laws of conduction, and there are many difficulties to be encountered. It is almost impossible to get wire of any length of equal thickness and texture. It is also not easy always to obtain the same connexions, and the least variation in this respect vitiates the experiments. Some of the anomalies in the tables are to be traced to these causes.

A difference of temperature also, it appears, will affect conduction. Even bending the wire with so delicate an instrument as the torsion galvanometer will affect the experiment; and twisting will alter its powers permanently.

XXX. *Suggestions for the Preparation of Phosphorus.* *By* M. Donovan, *Esq., M.R.I.A.**

PHOSPHORUS, a substance abundantly diffused throughout the animal, vegetable, and mineral kingdom, is obtained by processes which, although they have undergone many alterations and improvements during nearly two centuries, are still troublesome, expensive and difficult. According to M. Hellot's method, the description of which occupies many pages in the Memoirs of the Academy of Sciences, 1737, in order to obtain one ounce of phosphorus we must submit to the tedious and disgusting process of evaporating nearly three hogsheads of putrid urine. Dolfuss, who was more successful, obtained but 54 grains from 100 pints. Henckel first and Marggraff afterwards, by adding plumbum corneum to urine evaporated to thickness after being allowed to putrify for two months, procured two ounces and a half of phosphorus from nine or ten pounds of the inspissated matter: by this improvement the product was doubled, provided that the persons from whom the urine was obtained indulged in drinking malt-liquors in preference to vinous. But still its price was enormous. Mr. Boyle induced a chemist named Bilgar to extract it from very old night-soil. By some unknown addition Bilgar produced it so abundantly from this source, that he lowered the price of it to six guineas per ounce, yet made a large fortune. It could now be purchased for about half as many shillings. Giobert, by precipitating *fresh* urine with acetate or nitrate of lead, obtained a powder, from 100 parts of which he distilled from 14 to 18 of phosphorus.

But when Gahn discovered that the earthy part of bone consists of phosphate of lime, a more abundant and available source of phosphorus was made known to chemists. Crell accordingly decomposed bone-ashes by caustic alkali, dissolved the mass in water, precipitated it by nitrate of mercury, and distilled the phosphate of mercury with charcoal in the usual manner. But the phosphorus so obtained contains mercury; just as it contains zinc when sulphate of zinc is used for the separation of phosphoric acid from bone-ashes.

At length the present processes were contrived: bone-ashes were mixed with dilute sulphuric acid; a solution was thus obtained consisting of phosphoric acid holding some lime dissolved. This was either evaporated to dryness and distilled with charcoal, or it was precipitated with a salt of lead, and the precipitate distilled with charcoal; or it was neutralized with ammonia, filtered, and evaporated to dryness; the ammonia was expelled by heat, and the glassy residue finally distilled with charcoal.

* Communicated by the Author.

By the distillation of the phosphoric acid with charcoal, Wiegleb obtained ten drachms and a half of phosphorus from two pounds of bone-ashes. In the hands of Dolfuss, the same quantity of bone-ashes furnished rather less than five drachms. Pelletier sometimes obtained so much as $3\frac{1}{3}$ ounces, and sometimes but half that quantity from two pounds of bone-ashes.

Although these methods may be thus epitomized, the practical details are very troublesome. It is not always easy to obtain an adequate supply of bone-ashes. So truly did Lagrange appreciate this difficulty, that in his instructions for burning bones, he directs as the first step that a brickwork one yard in diameter and nine inches high shall be erected. Others order a furnace for the purpose, the chimney of which must necessarily be high, in order to carry off the truly abominable fumes. When the burnt bones are procured, the task of pulverizing and sifting them, so that their substance shall be permeated and acted upon by the sulphuric acid, is of no small labour, as the middle portions of the cylindrical bones are exceedingly hard. The washing out of the phosphoric acid from the voluminous, pasty, and somewhat tenacious sulphate of lime, is troublesome; for much of the acid obstinately adheres. If the sequel of the process be to obtain the acid in the solid state, the evaporation of the various washings to dryness is exceedingly tedious. But when lead is employed to engage the phosphoric acid from the washings, the resulting phosphate of lead is so bulky, so retentive of water, and by the ordinary means so difficult to dry, that the increased quantity of phosphorus procurable in this way scarcely compensates the trouble and loss of time. Beside all this there is another defect; the precipitate, whether obtained from acetate or nitrate of lead, contains lead in some state different from the phosphate, more in the case of the former salt than of the latter, and thus, during the distillation, the retort is partly occupied with an unproductive material. The acid liquor, beside phosphate of lime, always contains a little sulphate of lime, great in proportion to the quantity of water used in the washing; and this gives origin to sulphate of lead, which not only uselessly occupies the retort, but by suffering decomposition, during the distillation with charcoal, evolves sulphur; and this inquinates the phosphorus produced to a certain extent.

The precipitation of acetate or nitrate of lead by fresh urine, and the distillation of the precipitate with charcoal, might to some persons appear a convenient and simple process when large supplies of urine can be procured at once, as in barracks, hospitals, or prisons. But the advantage is far from being so great as it appears; for there are other substances present besides phosphates, which will afford precipitates with salts of lead. In an imperial pint measure of ordinary urine, according to the analysis of Berzelius, there are but 41 grains of phosphates, while there are nearly three times as much of other salts, all capable of precipitating acetate or nitrate of lead. Thus but a small part of the precipitate is available for the purpose, and the retort is uselessly occupied with substances which contribute nothing.

From bones, it is true, we procure phosphorus more easily, and in greater quantity; but so long as we follow the process given in chemical works, the details, as already shown, are extremely troublesome. On this account I have sought for a more simple method.

Bones are procurable in various commercial states; we have them solid; ground to a coarse powder between crushing rollers, for manure; distilled in close vessels for carbonate of ammonia, and the residual charred bone afterwards ground to a powder, well known under the name of bone-black; or in small particles obtainable from the lathe of the bone-turner. In all these states, bones afford phosphate of lime; but there are other sources, one of the most abundant of which is the horn of certain animals. The horns of the Stag (*Cervus elaphus*) and of the Fallow-deer (*Cervus dama*), and perhaps those of the whole class of Cervidæ, furnish phosphate of lime abundantly; but those of the Ox, Ram, Goat and Chamois, scarcely contain any. The commercial representative of the horn of the Stag or Fallow-deer is known under the name of shavings of hartshorn, and may be procured in great abundance.

Many analyses of bone have been published by chemists; but their results are so utterly discordant, owing to the different states in which the bones were examined, that I could not collect from them the ratio of salts which constitute the earthy basis. To arrive at the required information, I made many trials of recent bones, containing their natural quantity of fat and moisture, and obtained the following average results. Recent ox-ribs, from which the flesh had been carefully scraped, when calcined to whiteness, afforded 37·14 per cent. of earthy matter. Recent sheeps' bones (from the leg), when similarly treated, returned 38·71 per cent. It may be concluded then that a mixture of dense, recent bones, with which no pains have been taken to remove fat or moisture, will afford about 38 per cent. of earthy salts by incineration. A very porous ox-bone gave but 21 per cent. Neumann obtained 40·6 per cent. Fourcroy and Vauquelin give 49 as their result. Berzelius states his product to be 61 per cent. Von Bibra quotes 66·78 per cent. These great differences arise from the variable ratio of fat and moisture in the bones, which however I took no pains to remove.

With regard to the quantity of earthy salts contained in hartshorn, few analyses have been made. Dr. Pearson calcined his hartshorn-shavings to a brown colour, and obtained $54\frac{1}{2}$ per cent. Neumann, who only tried the tops of the horns, recovered 60 pounds of black caput mortuum from 100 pounds. Many trials gave me an average of 62 per cent. when the shavings were burnt to whiteness. These white ashes consist almost entirely of phosphate of lime; and it appears that hartshorn by calcination returns at least twice as much phosphate of lime as fresh bones. The horns of a fallow-deer will weigh about $1\frac{1}{2}$ pound.

These different forms of bones and horns present us with phosphate of lime in states which possess different advantages: some hold out the inducement of cheapness; some of facility in employing them: all of them answer the purpose. Ground bones may be procured in Dublin at so low a price as 3*s*. per bushel, weighing about 42 pounds. Bone-black, the caput mortuum remaining after the distillation of carbonate of ammonia from bones, may be had in quantity at 8*s*. per cwt. This black, by exposure to a red heat in the open air, becomes white bone-ashes; but the process is not necessary. Hartshorn-shavings are expensive, being so high, even in quantity, as 8*d*. per pound; but as the ratio of phosphate of lime contained in them is nearly double, we may estimate them at $4\frac{1}{2}$*d*.; and if their cleanliness and facility of employment be taken into account, they become still more eligible. But what renders their claims to preference paramount, is that even after having been kept a very long time, they contain an ingredient in perfectly good condition, which, when dissolved in water and properly seasoned, constitutes a light, highly nutritious, and most agreeable jelly, which has found its way to the kitchen, the nursery, and the sick room. A legally-authorized process for preparing it is given in many of the Pharmacopœias of Europe. Bone also possesses an analogous substance, which, although it cannot be extracted with the same facility, and after a lapse of time is not in good condition, has been recommended in its recent state as an article of food. Every-one is aware of the employment of the Digester for the purpose of its extraction.

In order to remove the animal matter from the earthy portion of bones, the process of calcination is resorted to; but it is unœconomical, and very troublesome. Instead of this, it will be better and much easier to withdraw the earthy portion from the animal matter, and thus preserve both for use. The separation is easily effected by digesting the bones either in muriatic or very dilute nitric acid: the earthy salts will thus be dissolved away, and the cartilage, retaining the shape of the bone, will remain unaltered. It will presently be shown that diluted commercial nitrous acid is better adapted for the purpose.

After the nitrous solution of the earthy salts has been obtained, the next step is to detach from it the phosphoric acid. This can be done in the usual manner by a salt of lead. Chloride of lead, although successful in the process of Henckel and Marggraff, does not answer here; for a boiling solution of it added to the nitrous solution does not produce any precipitation; and chloride of lead crystallizes as the mixture cools. Nor will nitrate of lead occasion a precipitate; for the phosphate of lead, if it be formed at all in this case, remains in solution in the free nitric acid. Acetate of lead answers the purpose perfectly: if a solution of it be poured into the nitrous solution of phosphate of lime, phosphate of lead precipitates instantly.

It has been stated by Giobert that when acetate of lead is poured into a solution obtained by acting on bone-ashes with dilute sulphuric acid, the acetate is decomposed, not only by the phosphoric acid, but, as he believes, by the water; and he adds, that the precipitate not only contains phosphate of lead, but calx of lead, which adds materially to its quantity. He further states that nitrate of lead comports itself in the same manner but in a less degree; and hence he recommends the employment of nitrate of lead for detaching the phosphoric acid. In the process recommended by me, as the precipitation takes place in a liquid which contains a considerable quantity of uncombined nitric acid, the objection of Giobert to acetate of lead cannot apply.

As to the choice of one amongst these different sources of phosphate of lime, circumstances must decide. If the process be conducted in the large way, bones ground between crushing rollers, or even broken into moderately small bits, will be found to answer, and to be the cheapest form: the cartilage that remains may be converted to many œconomical uses, for instance, the making of glue, or of an excellent size. If bone-black be employed, according to the process already described, the residuum is animal charcoal, which, after being washed and heated, is valuable as a decolorizing and antiseptic agent. If powder of burnt bones must be used, there is no incidental advantage, but the difficulty and trouble of minute division are to be encountered. The bones in burning ought not to be exposed to a very violent heat; for they lose a little phosphorus, as appears by the white light which issues from them; and besides this, they suffer a certain degree of vitrification, which renders them refractory under the pestle and to the action of acids. When the quantity of phosphorus to be made is small, and a little additional cost is no object, the unburnt shavings of hartshorn will perhaps be preferred. The phosphate of lime may be dissolved out of them by means of dilute commercial nitrous acid with facility, and in a very short time; the process is cleanly; does not require large vessels; and the jelly which may be obtained from the residuum is delicate and nutritious.

When unburnt, crushed or unburnt broken bones, whether of beef or mutton, are to be used, provided they be of the dense kind, I found that the quantity of "commercial nitrous acid" requisite to dissolve the earthy matter is nine ounces and two-fifths avoirdupois to one pound avoirdupois of bone. When the same kind of bones have been burnt to whiteness, the ratio must be $26\frac{1}{2}$ ounces of acid to one pound of bone-ashes. The acid must in all cases be diluted with ten times its weight of water.

If unburnt shavings of hartshorn be employed, each pound avoirdupois will require about 17 ounces of commercial nitrous acid. This acid is procurable at the price of about 6*d.* per pound wholesale.

With regard to the quantity of commercial sugar of lead required for the precipitation of the phosphoric acid contained in bones, the following ratio will be found adequate. For unburnt, crushed or broken bones, if not old, 1 pound avoird. will require 13 ounces. If bone-ash be used, 1 pound will require $41\frac{1}{2}$ ounces. For 1 pound of unburnt shavings of hartshorn, $1\frac{1}{2}$ pound of sugar of lead will be necessary.

The cost of sugar of lead, wholesale, is about 5*d.* per pound: but much of its expense may be saved in the following manner. When nitrous solution of bone or hartshorn is precipitated by means of sugar of lead, the phosphoric acid seizes on the oxide of lead, and the nitric acid combines with the lime. If the solution be filtered and evaporated, nitrate of lime is obtained. But the solution contains the acetic acid of the sugar of lead employed; and if it be boiled on carbonate of lead, which may be purchased at 5*d.* per pound, a solution of sugar of lead will be regenerated which will answer for a new precipitation. At length the liquor becomes so rich in acetic acid, that on a large scale of manufacture it will be well worth while to distil it for a product of pure acetic acid.

When the phosphate of lead is first separated, it contains nitrate of lime; from this it must be freed by washing, otherwise some phosphoric acid will be regenerated at the expense of phosphorus during the subsequent distillation.

The common process of drying the precipitated phosphate of lead on the filter is tedious, troublesome, and inconvenient; so tenacious is it of water that it will long remain a thin paste. I find the best mode of drying it is to transfer both filter and precipitate to an iron pot, and to heat it until the matter fall to powder. Any part of the paper that escapes burning may then be picked out. The drying is thus easily and quickly accomplished.

The phosphate of lead, when dry, is a voluminous powder: a retort of ordinary size would not hold a sufficiency of it to return a remunerative product of phosphorus. This inconvenience is not peculiar to the processes here given, but affects all others equally in which a salt of lead is employed. There is however an easy remedy. Let the phosphate of lead be transferred to the kind of crucible called a skittle-pot, and let the crucible covered be heated red-hot in a common coal fire: it will during the heating shrink at least to one-half its former bulk; but the weight will be scarcely diminished. It will be still a loosely aggregated pulverulent mass; if the heat be increased, it will shrink more and melt, but it then becomes rather difficult to powder.

The last step in the process is the distillation of the phosphate of lead with charcoal. The phosphate and charcoal, both in fine powder, and well mixed, are to be introduced into an earthenware retort, the pores of which have been closed with a glaze consisting of borax and lime as directed by Higgins.

On account of all the foregoing facts, I conclude with recommending the following as the easiest and cheapest processes for obtaining phosphorus:—

Take of dense bones, crushed or broken into small pieces, as many pounds as may be deemed sufficient, say ten avoirdupois pounds. Digest them in a mixture of 6 pounds of commercial nitrous acid and five gallons of water for a few days. When the bones feel perfectly soft and flexible, strain off the liquor, and add to it 8 pounds of sugar of lead dissolved in a sufficiency of water. An abundant precipitate will appear; wash and dry it by heat in the manner already directed. Its bulk will be reduced to one-half if it be heated red-hot in a crucible. Mix it well with one-sixth of its weight of fine charcoal powder or lampblack, and distil out of large earthen retorts properly prepared.

The phosphate of lead resulting from the above process would, according to my trial, amount to $91\frac{1}{2}$ ounces avoird. Giobert states that 100 parts of phosphate of lead precipitated from urine by acetate of lead afforded from 14 to 18 parts of phosphorus. If this be a correct estimate, the $91\frac{1}{2}$ ounces should return from 12 ounces to 1 pound of phosphorus. A large quantity of cartilage is also obtained, which is well calculated for making size, glue, and for many other purposes.

The following is a shorter, neater, and less troublesome, although a little more expensive process for preparing phosphorus, which may be employed when the quantity required is not very large. Take of unburnt shavings of hartshorn 1 avoirdupois pound; digest it for four hours in a mixture of 17 ounces weight of commercial nitrous acid and one gallon of water. Strain the liquor, and add to it $1\frac{1}{2}$ pound of sugar of lead, previously dissolved in a sufficiency of water: mix, and let the precipitate subside. Pour off the supernatant liquor; dry and wash the precipitate as already directed: mix it with one-sixth of charcoal powder or lampblack, and distil as before.

The charcoal powder or lampblack will in all cases afford a better product if previously well calcined in a crucible covered with sand, or in any close vessel. The waste of phosphorus, by solution in the gas evolved during the subsequent distillation, will thus be much lessened; and the same end will be further promoted by a previous exposure of the phosphate of lead to an obscure red heat, which will also cause a reduction of bulk to one-half.

These processes appear to œconomise time, trouble, fuel, and cost of large vessels. On the whole, they are probably the best when bones or horns are to be the source. In the Province of Estremadura, the hills contain considerable quantities of phosphate of lime, and houses are built of it in the district of Truxillo. This stone, when thrown on the fire, emits a beautiful green light. I know not whether phosphorus can be profitably extracted from it.

XXXI. *An Account of a remarkable Flood at Chipping in Lancashire. By the Rev.* Alfred Weld, *B.A., F.R.A.S., M.B.M.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I TAKE the liberty of presenting you with some details of a very remarkable flood with which this neighbourhood was visited during the afternoon of July the 1st. The accounts which reached us of its effects, as well as the subsequent appearance of the river into which the waters were poured, were of so unusual a character, that I thought it worth while to visit the site, and satisfy myself as far as possible, from my own observation and the facts I could collect from eye-witnesses, as to the nature and extent of a storm that had committed such terrible ravages, and spread such alarm amongst the unsuspecting inhabitants of Chipping.

It would be useless here to enter into all the particulars of the devastation; such details would possess no general interest, and are to be found at length in all the local journals; but the traces which I witnessed, relating principally to the origin and extraordinary force of the inundation, and the information I gained regarding the distribution of the storm in the neighbourhood, will, I think, afford points of scientific interest; and it is with this view I have determined to offer this little account to you, hoping that it may be found worthy to be laid before your readers.

The little village of Chipping lies near the bottom of the valley contained by the parallel ridges of Bowland Hills and Longridge Fell, which bound it on the north-west and south-east, leaving it open to the sea in the south-west, while in the east and north-east the horizon is terminated by Pendle Hill and the Newton and Waddington Fells. It is watered by a brook to which it gives its name, whose waters flow in general through a deep rocky channel lined with wood, but in some parts of their course emerge among meadows and pasture lands. In ordinary circumstances it is a shallow stream, barely covering the stones which form its bed; but occasionally swollen into a torrent, for which, however, its natural channel is amply sufficient. This brook is formed by the junction of two little streams, the extreme distance between whose sources can scarcely be more than two miles. It is along the course of these brooks that traces are exhibited which show the character of the flood, and in my opinion render the details of all further ravages easily credible.

I was assured that the smaller of these brooks, called Dobson's brook, does not drain at most above 150 acres of land; and yet even near its source it bears signs of having been washed by a furious torrent, such as no ordinary thunder-storm can account for. Its waters enter the main brook a little more than a quarter of a mile above Chipping. A little above this place a weir had been constructed of strong masonry to turn a portion of the waters to the village mill. This was entirely demolished; and one of the stones, which I found to measure 5 feet in length and $1\frac{1}{2}$ in breadth, carried to the distance of about forty yards and imbedded in the mud of the brook. In places where the channel widened, the bed was strewn with large stones piled up in great heaps, every stone bearing evident traces in its chipped and bruised surface of having been rolled and dashed along by the current. Some blocks of limestone of considerable size had been freshly broken in two by the force with which they had been thrown. In order to satisfy myself that these effects were produced by the rolling of the stones themselves, and not merely by the passage of smaller stones over them, I caused some of the larger of them to be turned over, and found exactly the same bruised and chipped appearance on every side. A large stone, which was estimated to weigh about 7 cwt., and which had been used for a stepping-stone where a path crosses the brook, was carried about a quarter of a mile down the stream. Another, which could not have weighed less than 12 cwt., showed signs of having been borne along by the torrent.

Smaller stones appear to have literally floated, as they may be found strewn upon the ground several feet above the steep bank of the stream; and trees stripped of their bark, four or five feet above the bed of the brook, by the passage of stones, bear testimony to the force of the current. Where the channel is narrow, the wreck is still left hanging at the height of seven, and in one place of nine feet, above the usual surface of the water. In some places the bed has been lowered to the depth of a yard; in others a new channel has been worked out in the clay, and the old one filled up with stones and gravel.

At intervals throughout its course trees of considerable size have been rooted up and carried down till some immoveable obstacle arrested them, or left dry upon the fields on the retiring of the waters. I learned from a farmer living by the side of the brook, that the flood reached its height in less than twenty minutes, and that for the space of about half an hour the rain fell with extreme violence. At a small farm a little to the west of this, and situated on an exposed declivity on the southern side of Saddle Fell, I was told that the rain fell with great violence for above two hours. It was described to me as having the appearance of flakes of snow, and it was said that "every drop seemed of the size of a half-crown." At this place the water streamed down the road to the depth of from one to two feet; and yet upon examination I found that the whole extent of ground drained by this road could scarcely be half an acre. This will aid in conveying an idea of the extraordinary and even tropical violence of the rain which could have produced such a flood in so short a period. The summit of this road is the extreme limit of the lands drained by the little brook of which we have been speaking.

An observer standing at this point and facing west, has before him a spacious basin, open on the left, bounded by Parlick in front, and by the main chain of hills on the right. Beneath is the principal brook which terminates in two gorges among the hills, from which its waters are supplied. The easternmost of these, called Greenlough Clough, presented a sight more astonishing than anything I had ever seen. At the point where its bed opens out from the hills, scarcely a mile from the watershed, many hundreds of tons of large stones are thrown together in a great heap, covering an area of about forty yards in extreme width and at least eighty in length. These stones had been carried down the steep ravine by the impetuosity of the torrent and heaped up at this place, where the waters, being spread over a larger surface, had no longer the force to bear them along. Still, on ascending the ravine and examining the sides of the hill above the mark which the waters had reached, I found the heath and fern growing fresh and unsullied, and presenting no appearance of having been washed by any great flow of water.

It now remains to describe the scene to which my attention was principally directed, and which alone suffices to distinguish this flood from all others that I have ever witnessed, or which have ever been known in this part of the country.

The eastern side of Parlick, which rises to the height of about 1400 feet above the sea, presents an exceedingly steep ascent richly clothed with fern. On this face of the hill seven huge scars have been hollowed out by the water, varying from five to fifteen yards in width, all cut abruptly from the face of the hill; and with the exception of two of the most southern, which are somewhat lower, all commencing nearly in the same horizontal line. There is in no case any appearance of a water-course opening into them, but in every instance the upper limit is formed by a definite line, like the edge of a cliff. The depth to which the ground has been carried away varies from one or two to five or six feet. Between these slips the fern is still green and flourishing, and I am quite sure that no great body of water can have passed over it. A little above one of the slips whose upper limit is lower than the others, the fern is beaten down, and appears to have been washed by a torrent; but there is no channel opening into the hollow produced by the slip, the commencement of which is no less abrupt than the rest. Hence it appears that several distinct discharges of water must have taken place, of such tremendous violence as to be able to carry away hundreds of tons of earth and stones, whilst the land between, in narrow strips of ten or twelve yards in width, remained untouched.

The volume of water which flowed down the seven channels must have been enormous, since the stones and earth are spread over acres of land below. In some places the stones are thrown up at the edges of the stream into a sort of mound, one or two feet in height, by the force of the torrent. Towards the bottom of the steep declivity the several streams seem to have united and formed one great river about 150 yards in breadth. This must have been in some parts at least five feet in depth, as appears from the remnants of a wall which ran nearly parallel to the course of the water. The greater part of this wall is thrown down; but the portions of it that remain, about five feet in height, are still covered with mud, showing that they were buried beneath the torrent. Along the foundations of the wall, a channel four feet deep was hollowed out where previously no water-course had existed. This great stream had to make its way across the land, working out for itself a deep and broad bed, till it mingled with the waters of the main brook coming from Greenlough and Whitestone Cloughs.

The devastation which the passage of so great a body of water must have produced will easily be understood. In one or two places where I measured the channel, I found its area to be about twelve square yards, and yet the water rose above its banks and spread over the land on each side. At a place called Wolfenhall Mill on the banks of the brook, I was told that when the flood first came down, it presented a perpendicular breast of two yards in height. Considering the great body of water which suddenly descended Parlick, and bearing in mind what I was everywhere told, that scarcely half an hour elapsed between the first rise of the flood and its entire subsidence, I am convinced that this is no great exaggeration. Great numbers of trees along the banks were washed away; all the weirs constructed to turn the water to the various mills situated near the stream destroyed; walls and fences overthrown; and all the meadows and pastures along the banks covered with stones and sand.

The greatest loss was suffered by the owner of a little cotton-factory, which stands about ten feet above the bed of the stream. The water rose to the height of four feet six inches on the ground floor of the factory, ruining part of the machinery, and covering everything with mud and gravel. The garden, which was on the same level as the factory, and was laid out with the greatest taste, and adorned with many varieties of beautiful shrubs and flowers, is entirely destroyed. It now presents to the view nothing but great heaps of stones and rubbish, while every shrub has been either carried away or buried beneath sand and gravel. A strong wall which had been built to protect the garden from the brook was entirely carried away, while many tons of large stones have been raised to the height of ten or twelve feet above the natural bed, and thrown up on what were formerly flower-beds and gravel-walks. In Chipping, the water entered the houses to the depth of nearly six feet, committing great havoc in the shops and cellars. The large stones which formed the parapet of the bridge were washed from their places and carried some distance up the street; part of the foundation and floor of a house carried away; a wooden bridge washed down the stream, and a little lower down, a stone bridge almost entirely destroyed; and many acres of land covered with mud and gravel, and strewn with wreck of every description.

Leagram Hall is situated about half a mile from the nearest point of the brook, and on an elevation of about 200 feet above it. The roar of the torrent created a sudden alarm; everyone ran to the windows, which overlook the valley, and beheld the water spread out like a lake before them; the waves, which were visible from that distance, gave it the appearance of an arm of the sea; while the rapidity with which it spread over field after field conveyed the impression of the bursting of a great lake, whose waters were about to inundate the whole valley. The rain fell here with great fury for about an hour. A rain-gauge exposed in the garden became choked with sand; not, however, until it had collected rain to the depth of 2·2 inches during that single shower. The hour at which the flood passed through Chipping was, as far as I could ascertain, about 4^{h} P.M.

From the account which has been given, it appears that the overflowing of Chipping brook was mainly attributable to the sudden discharge of a large volume of water on the side of Parlick; that this discharge must have partaken somewhat of the nature of a water-spout, and must have consisted of several distinct discharges, all contained within the space of about 150 yards measured in a horizontal line along the side of the hill. Besides this, rain must have fallen with extraordinary fury along the whole extent of hills, as appears from the degree to which the ordinary sources of Chipping brook were swollen, as also the other brooks which take their rise in the same range of hills*. Leagram brook, which descends from the same range but more to the eastward, was also flooded, but not to such a degree as to be worthy of any very special notice. Graystoneley brook, which rises among the same hills but still further to the east, rose suddenly, as I was assured, to the depth of a yard. Still, a farmer who saw it about an hour and a half afterwards, told me there was no flood, and that the water was merely muddied. I do not look upon these accounts as altogether contradictory, since all agree that the subsidence of the waters was as rapid as their rise. Still the Hodder was not even coloured above the confluence of these brooks, showing that at Whitewell there was no rain of any consequence, but that the watershed of the Graystoneley brook was the extreme eastern limit of the storm; whereas the Loud, which rises on the western side of Parlick, was a great flood even before it received the waters of the Chipping and Leagram brooks. So great an effect had the water of the Loud upon the Hodder, that even at Lower Hodder Bridge, where the river flows near Stonyhurst, the muddy waters came

* Some persons who were on the top of Parlick at the time describe the rain as having fallen in streams or sheets of water, and affirm that they experienced difficulty in breathing whilst exposed to it. A gentleman who was exposed to it in Chipping, told me that he experienced the sensation of warmth as it fell upon his person, although the day was sultry, and he was heated with walking at the time.

I have been able to discover no traces of marine deposits, which would probably have been found if the water had been raised up in a body from the sea, as is the case in a water-spout. I am inclined to look upon the discharges as produced by the sudden condensation of a cloud loaded with an enormous amount of vapour, accumulated doubtless during the previous extremely hot weather, and thus poured down in a volume on the side of the hill: the fact of several discharges having taken place so near to each other is not so easy to explain.

down in a perpendicular breast of about a yard in height, driving the fresh water before it. This point is about $9\frac{1}{4}$ miles from Chipping, following all the windings of the streams, and about 220 feet lower; and as I find that the flood reached it at about $6^h\ 30^m$ P.M., it would seem that it took $2^h\ 30^m$ or thereabouts to travel that distance, which corresponds to the rate of 3·7 miles an hour. For several days afterwards the water continued muddy, and a thick deposit is left all along the banks of the Hodder and Ribble.

It is worth remarking, that on the south-east side of the valley bounded by Longridge Fell there was no rain; and in several places at the bottom of the valley, within two miles of Leagram, I found on inquiry that there had been nothing but a few drops. Everywhere I was told that the lightning was terrific. At Stonyhurst there was no rain, but the lightning was very brilliant, and the thunder almost incessant. The storm appeared to be raging in the direction of Chipping. Still about two miles to the east, at Higher Hodder Bridge, the rain fell with great violence for about fifteen minutes. From a point in this neighbourhood, the whole range of Bowland Hills appeared enveloped in a dense black cloud, from some points of which the rain seemed to be descending in torrents. This feature, and the lurid aspect of the sky, joined to the frequent displays of lightning of the most vivid description, rendered the whole scene one of the most imposing I remember to have witnessed.

XXXII. *On the Growth of Plants in various Gases.* *By* Dr. J. H. Gladstone *and* G. Gladstone, *Esq.**

THAT both plants and animals are very dependent upon the chemical composition of the atmosphere in which they live, and that the constituents of it play important functions in their œconomy, are matters of every-day observation. These constituents are oxygen, nitrogen, aqueous vapour, a small amount of carbonic acid, a still smaller quantity of ammonia, and occasional traces of other gases.

Since the plant stores up oxygen within its cells at certain periods, it can scarcely be doubted that this gas fulfils some important office; but, excepting in the case of the germination of seeds, we have little information upon this subject.

It has been doubted whether any of those vegetable principles which contain nitrogen derive that element by direct assimilation of the gas existing in the atmosphere. Ammonia is looked upon

* Communicated by the Authors, being the substance of two papers read before the British Association at the meetings of 1850 and 1851.

as the source whence it is obtained; but when the minuteness of the quantity is taken into account, the assimilation of nitrogen by plants, though improbable, may certainly still be held an open question.

It is to the decomposition of the carbonic acid in the atmosphere that vegetables are mainly indebted for the carbon which forms the basis of their structure. The proportion of carbonic acid in the air in which different plants will thrive has engaged the attention of many experimenters, and is still the subject of investigation. This at least is clearly ascertained, that, unless in strong sunshine, plants are destroyed by being placed in an atmosphere of pure carbonic acid.

The aqueous vapour in the air not only supplies hydrogen and oxygen to the organized vegetable structure, but serves many other purposes of a more physiological character.

We proposed examining the growth of plants when exposed to the action of gases that do not occur in the normal atmosphere, or of the ordinary gases in very unusual proportions.

Our first experiment was made with hydrogen gas. A pansy was placed in an atmosphere consisting of 95 per cent. of hydrogen and 5 per cent. of carbonic acid. The glass vessel in which it was confined had a capacity of 53 cubic inches. It was inverted over water, so that connexion with the outer air was entirely precluded, and yet there was very little surface of water exposed to the artificial atmosphere within. A little additional carbonic acid was introduced from time to time to replace that absorbed by the liquid. The plant when first experimented upon had one blue flower in full bloom and one in the bud; for the first ten days the bud was gradually opening; but a slight mouldiness then came on, which increased during the next fortnight, by which time the plant having drooped decidedly, it was thought unnecessary to prolong the experiment. For the sake of comparison another pansy was placed in a glass vessel filled with atmospheric air, all other circumstances being equal: the plant became mouldy at about the same period as the other; but the mould did not advance beyond the lower leaves, and the plant continued healthy for weeks.

Two flowering grass-plants (*Poa annua*) were placed in vessels of the same character and capacity as in the preceding experiment, and protected from the external air in the same manner. The one vessel was filled with pure hydrogen gas, the other with common air. The plants grew and appeared healthy for about a fortnight, when mouldiness supervened in each instance. Thus the substitution of unmixed hydrogen gas for the normal atmosphere produced no visible alteration.

The next experiment was with a mixture of hydrogen and oxygen gases, the vegetable being supplied, as in nature, with the carbonic acid necessary for its growth through the agency of animal life. A large glass receiver capable of containing 177 cubic inches was filled with oxygen and hydrogen in the proportion of two measures of the latter to one of the former gas, in fact, in exactly the proportions requisite to form water. Into this atmosphere was introduced a pansy in flower, together with a few common house-flies and some sugar, and it was kept from communicating with the external atmosphere by being placed over water. A precisely similar arrangement was made in another receiver of similar capacity, but with the normal atmosphere in place of the mixed gases. It was anticipated that the plant would derive its nutriment from the aqueous vapour, and the carbonic acid produced by the respiration of the flies, while these again would feed upon the sugar; and we should thus have the same balance between the vegetable and animal kingdoms as obtains in nature, except that the animal would not feed upon the very plant which its breath nourished, a circumstance which it was obviously necessary for us to avoid. Besides ascertaining whether the pansy would flourish in mixed hydrogen and oxygen gases—the direct object of this experiment—it would afford an opportunity of observing any effects which the unnatural atmosphere might have upon the insects themselves. The pansy continued in a healthy condition for some time. As to the flies, it did not appear that the substitution of hydrogen for nitrogen in the atmosphere had any marked immediate effect upon their breathing; and thus the observations of M. Regnault upon other living creatures were confirmed by an instance drawn from the Articulata. But it was curious to observe the effects that resulted from the low specific gravity of the gaseous mixture. The larger flies when first introduced found themselves unable to walk up the glass, nor when they shook their wings did it assist them in mounting into the air; if they launched themselves from any prominent object, it was only by a great effort that they were able to fly an inch or two before falling to the bottom. While the comparative experiment made with atmospheric air presented a scene of animation by the rapid evolutions of the winged insects, and their buzzing against the glass which confined them, the flies walked slowly and in perfect silence about the interior of the vessel containing mixed hydrogen and oxygen.

Experiments were made in nitrogen gas. A pansy in flower, a young stock, and a grass-plant (*Poa annua*) in flower, were placed in atmospheres of this gas with the same arrangements as in the preceding instances. Another young stock was placed in air under similar circumstances for the sake of comparison: it was thought unnecessary to repeat the comparative experiment with the others. The pansy and the stock dried up and died in the course of a day or two; but the grass-plant grew and seemed healthy for several weeks, mouldiness only appearing on partially decayed portions. We surmised that the deleterious effects of this gas, which we as well as other experimenters had observed, might be attributed to the phosphorus vapour which it always contains when prepared by the usual method. Accordingly, we have subsequently employed pure nitrogen gas evolved by the mutual action of nitrite of potash and chloride of ammonium at a high temperature. A pansy placed in this atmosphere under a glass vessel of 54 inches capacity inverted over water, remained flowering and vigorous for between two and three weeks.

It is curious to remark the readiness with which mouldiness grows in nitrogen gas. A receiver half-full of nitrogen happened to be left standing over the pneumatic trough, and a bung was floating on the surface of the water inside. In a few days' time a white growth was observed upon the bung; and not on it only, but also in patches over the surface of the water. A bottle also partially filled with the gas was standing inverted in ordinary spring-water; the surface of the liquid inside was soon found covered with small patches of mould, which continued to increase.

A pansy and grass plant were placed in a receiver containing 180 cubic inches of pure oxygen gas, inverted as usual over water. Both plants were in flower at the time of their introduction; they grew considerably taller, and the *Poa* showed extraordinary luxuriance in fructification. After about twenty-four days the grass became mouldy, and the pansy drooped a short time afterwards.

Two similar plants were placed under a receiver of similar capacity, but filled with nitrous oxide. In order to exclude the external air, the glass vessel was inverted over water saturated at the ordinary temperature with the same gas. The water however gradually absorbed the nitrous oxide within, allowing it to diffuse into the atmosphere without; thus the liquid rose in the receiver, but it allowed of the continuation of the experiment for two months, during which time no extraordinary effect upon either plant was observable. Davy records a trifling experiment upon a sprig of mint, which, so far as it went, indicated the innocuous character of his laughing gas; and Drs. Turner and Christison * found that 72 cubic inches of this, mixed with air in a vessel of 509 inches capacity, produced no visible effect upon a mignonette plant in forty-eight hours. They do not appear to have pursued the investigation further.

The same experimenters concluded that carbonic oxide is probably of the same class of gaseous poisons, in respect to plants,

* Brewster's Journal, January, 1828.

as sulphurous acid or cyanogen, but that "its power is much inferior." They found that 23 cubic inches of carbonic oxide, with five times the volume of air, had no apparent effect upon a mignonette plant in twenty-four hours, but that it drooped when removed from the abnormal atmosphere. We imagine that the death of this plant must be ascribed to some other cause, since we have kept a pansy in a healthy condition for four weeks in 53 inches of pure carbonic oxide gas.

It order to ascertain what effect different gases might have in accelerating or retarding germination, and what compounds of carbon were capable of affording nourishment to the young plant, four onions just commencing to sprout were taken, weighed and placed in vessels containing respectively carbonic acid, carbonic oxide, coal-gas collected by displacement, and atmospheric air containing eight per cent. of light carburetted hydrogen prepared by the decomposition by heat of an alkaline acetate. The four vessels containing these bulbs were each capable of holding 53 to 54 cubic inches of gas, and were inverted over water as in the previous instances. The water employed to prevent access of air to the vessel containing the carbonic acid was itself saturated with the same gas; that employed for the hydrocarbons was an alkaline solution, so as to absorb any carbonic acid which might happen to be present in the gas, or which might be generated by the growing root itself. A comparative experiment was made with another onion placed under a glass shade under similar circumstances as to light, heat, &c., but open to the atmosphere. The onions selected had each a plumule of about half an inch in length, and the experiments extended through the months of April, May, and June. The onions in the atmospheres containing hydrocarbons grew rapidly, and put forth fresh roots. After thirty days the plants were taken out of the coal-gas and carburetted hydrogen, as they had grown to the top of the vessels, being at the time in a very healthy condition. The onion in air grew, but not so fast. Those placed in carbonic acid and carbonic oxide stopped growing, and eventually became rotten. In each instance they lost weight.

In the early part of the year a crocus commencing to sprout was placed in a jar containing 70 cubic inches of carbonic oxide mixed with 100 cubic inches of atmospheric air, inverted over alkaline water so as to absorb any carbonic acid that might be formed. The rootlets of the bulb dipped into clear water. The crocus grew and put forth abundance of long leaves, but it never flowered. After remaining in this state for fifteen weeks, the experiment was discontinued, and the bulb and plant were found to weigh 285 grains, which was one grain less than at the commencement of the experiment. The gas within the receiver decreased very much in volume, but was beginning to increase again towards the close, when the plant was becoming unhealthy in appearance.

A precisely similar experiment was made with light carburetted hydrogen prepared from the acetates in the place of the carbonic oxide: the growth of the plant was similarly affected, but it proved that this gas (at least in the proportion of seven parts of carburetted hydrogen to ten of air) is not poisonous to the bulbous-rooted plant in question. We have not performed any experiments with unmixed carburetted hydrogen or olefiant gas; it is to be expected, however, that they would be found equally innocuous with carbonic oxide, hydrogen, and others which have been examined. Drs. Turner and Christison found no deleterious effect in twenty-four hours from a mixture of $4\frac{1}{2}$ inches of olefiant gas, with 100 times as much air. The action of gaseous hydrocarbons upon plants is a separate branch of inquiry, as it involves the interesting question as to whether these gases are capable of being assimilated or decomposed by the living organism of the vegetable, and thus of contributing to its support.

There is a peculiar circumstance attending the growth of the plants in most of the experiments above detailed, as also in the comparative ones made with atmospheric air,—a circumstance which may be constantly observed in "Ward's Cases," though perhaps not in so striking a manner. It is this:—they increase at first somewhat in height, and the leaves or flowers may open a little further than when first placed in the confined atmosphere, but after a day or two their growth appears retarded without any signs of decay. Thus in the experiment with the crocus in mixed light carburetted hydrogen and air, the bulb was placed in the inverted vessel on Dec. 27th; rootlets and leaves of about 5 inches in length speedily shot forth, but then the functions of the vegetable seemed suspended, and it remained in the month of July just as it was in February, a crocus with delicate green leaves opening for the protrusion of the flower-stalk.

These results indicate that gases may be divided into two great classes in respect to their action upon vegetable life; namely, those which are decidedly poisonous, and those which exert no deleterious influence. The poisonous gases have been investigated by Drs. Christison and Turner in the memoir already adverted to more than once; they are sulphurous acid, sulphuretted hydrogen, hydrochloric acid, chlorine, and cyanogen; and a very minute quantity of any of these is found to destroy plants immersed in them for only a few hours; indeed some of them, sulphurous acid for instance, are decidedly more injurious to vegetable than to animal life. In respect to hydrogen, Davy came to the conclusion that it was injurious to some plants, but not to others; Saussure found that a plant of *Lythrum Salicaria* flourished for five weeks in an atmosphere of this gas. Is it not possible that some of the compound gases which frequently contaminate hydrogen, and which are known to be poisonous even in very small proportion, may have led to the destruction of those plants which died apparently through the influence of hydrogen gas? As far as our own experiments are concerned, we find hydrogen, nitrogen, oxygen, carbonic oxide, nitrous oxide, and perhaps gaseous hydrocarbons, to be perfectly innocuous to vegetable life in any proportion.

The earth's atmosphere is common to all the tribes of organized existence which inhabit the land, whether fixed to one locality or endowed with voluntary motion; but its component gases perform different functions in respect to the two great classes into which we are in the habit of dividing them. No animal, as far as we are aware, can exist for any length of time in an atmosphere devoid of oxygen, whilst on the other hand all those which are usually included under the appellation of the "vegetable kingdom" are dependent for their food upon those gases which contain carbon. We know from Regnault's experiments that the amount of oxygen in the air may vary largely, and that the nitrogen may be replaced by hydrogen gas without any marked effect upon animal life; and we now find even more strikingly in regard to plants, that either of the great constituents of the atmosphere may prevail to the exclusion of the other, or that they may be replaced by totally different gases, without involving the destruction of the living organism; of course they cannot increase in substance without carbonaceous food, yet the deprivation of this appears only to lead to an indefinite suspension of their functions. Doubtless the actual constitution of the atmosphere is that which is most suited to the permanent well-being of the whole of the organized creation, and perhaps it is equally requisite both for plants and animals; yet it is evident that great deviations from its normal constitution may take place without producing serious injury.

XXXIII. *On Extensions of the Dialytic Method of Elimination.* *By* J. J. Sylvester, *M.A., F.R.S.**

THE theory about to be described is a natural extension of the method of elimination presented by me ten years ago (in June 1841) in the pages of this Magazine, which I have been induced to review in consequence of the flattering interest recently expressed in the subject by my friend M. Terquem, and some

* Communicated by the Author.

other continental mathematicians, and because of the importance of the geometrical and other applications of which it admits, and of the inquiries to which it indirectly gives rise. We shall be concerned in the following discussion with systems of homogeneous rational integral functions of a peculiar form, to which for present purposes I propose to give the name of aggregative functions, consisting of ordinary homogeneous functions of the same variables but of different degrees, brought together into one sum made homogeneous by means of powers of new variables entering factorially.

Thus if F, G, H ... L be any number of functions of any number of letters $x, y \ldots t$ of the degrees $m, m-\iota, m-\iota' \ldots m-(\iota)$ respectively, $F+G\lambda^{\iota}+H\mu^{\iota'}+\ldots L\theta^{(\iota)}$ will be an aggregative function of the variables entering into F, G, &c., and of $\lambda, \mu \ldots t$. I shall further call such a function binary, ternary, quaternary, and so forth, according to the number of variables contained in the functions (F, G, H, &c.) thus brought into coalition.

It will be convenient to recall the attention of the reader to the meaning of some of the terms employed by me in the paper above referred to.

If F be any homogeneous function of $x, y, z \ldots t$, the term augmentative of F denotes any function obtained from F of the form

$$x^{a}.y^{\beta}.x^{\gamma}\ldots t^{\delta}\times F.$$

Again, if we have any number of such functions F, G, H ... K of as many variables $x, y, z \ldots t$, and we decompose F, G, H ... K in any manner so as to obtain the equations

$$F=x^{a}.P_1+y^{b}.P_2+z^{c}.P_3+\&c.\ldots+t^{d}.(P)$$
$$G=x^{a}.Q_1+y^{b}.Q_2+z^{c}.Q_3+\&c.\ldots+t^{d}.(Q)$$
$$H=x^{a}.R_1+y^{b}.R_2+z^{c}.R_3+\&c.\ldots+t^{d}.(R)$$
$$\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot$$
$$K=x^{a}.S_1+y^{b}.S_2+z^{c}.S_3+\&c.\ldots+t^{d}.(S),$$

and then form the determinant

$$\begin{matrix} P_1 & P_2 & P_3 \ldots (P) \\ Q_1 & Q_2 & Q_3 \ldots (Q) \\ R_1 & R_2 & R_3 \ldots (R) \\ \cdot & \cdot & \cdot \quad \cdot \\ S_1 & S_2 & S_3 \ldots (S) \end{matrix}$$

this determinant, expressed as a function of $x, y, z \ldots t$, is what, in the paper referred to, I called a secondary derivee, but which for the future I shall cite by the more concise and expressive name of a *connective* of the system of functions F, G, H ... K, from which it is obtained. One prevailing principle regulates all the cases treated of in this and the antecedent memoir, viz. that of forming linearly independent systems of augmentatives or connectives, or both, of the given system whose resultant is to be found, of the same degree one with the other, and equal in number (when this admits of being done) to the number of distinct terms in the functions thus formed. The resultant of these functions, treated as linear functions of the several combinations of powers of the variables in each term, will then be the resultant of the given system clear of all irrelevant factors. If the number of terms to be eliminated exceed the number of the functions, the elimination of course cannot be executed. If the contrary be the case, but the equality is restored by the rejection of a certain number of the equations, the resultant so obtained will vary according to the choice of the equations retained for the purpose of the elimination. The true resultant will not then coincide with any of the resultants so obtained, but will enter as a common factor into them all.

The following simple arithmetical principles will be found applicable and useful for quotation in the sequel:—

(*a.*) The number of terms in a homogeneous function of p letters of the mth degree is

$$\frac{m.(m+1)\,.\,.\;\;(m+p-1)}{1.2\;\;.\,.\,.\;\;\;p}.$$

(*b.*) The number of augmentatives of the $(m+n)$th degree belonging to a function of p letters of the mth degree is

$$\frac{(n+1)(n+2)\ldots(n+p-1)}{1\,.\,2\;\;\;\;.\,.\,.\;\;\;\;p}.$$

(*c.*) The number of solutions in integers (excluding zeros) of the equation $a_1+a_2+\ldots+a_p=k$ is

$$\frac{(k-1)(k-2)\ldots(k-p+1)}{1\,.\,2\;\;\;\;.\,.\,.\;\;\;\;(p-1)}.$$

To begin with the case of binary aggregatives. Let

$$\left.\begin{array}{l} F_m(x,y)+F_{m-\iota}(x,y)\lambda^{\iota}+F_{m-\iota'}(x,y)\mu^{\iota}+\&c.\ldots+F_{m-(\iota)}(x,y)\theta^{(\iota)} \\ G_n(x,y)+G_{n-\iota}(x,y)\lambda^{\iota}+G_{n-\iota'}(x,y)\mu^{\iota}+\&c.\ldots+G_{n-(\iota)}(x,y)\theta^{(\iota)} \\ \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\ K_p(x,y)+K_{p-\iota}(x,y)\lambda^{\iota}+K_{p-\iota'}(x,y)\mu^{\iota}+\&c.\ldots+K_{p-(\iota)}(x,y)\theta^{(\iota)} \end{array}\right\}\ (\text{A.})$$

be a system of functions (whose Resultant it is proposed to determine) equal in number to the variables x, y, λ, $\mu\ldots\theta$, and similarly aggregative, *i. e.* having only the same powers of λ, μ,

&c. entering into them, but of any degrees equal or unequal $m, n \ldots p$. Let the number of the functions be r. Raise each of the given functions by augmentation to the degree s, where

$$s=\{m+n+\ \ldots\ +p\}-\big(\iota+\iota'+\ \ldots\ +(\iota)\big)-1,$$

the number of augmentatives of the several functions will be

$$\begin{gathered}(s+1)-m\\(s+1)-n\\ \cdot\quad\cdot\quad\cdot\quad\cdot\\(s+1)-p,\end{gathered}$$

and the total number will therefore be

$$r(s+1)-(m+n+\&\text{c}.+p),$$

which

$$=(r-1)(m+n+\ \ldots\ +p)-r\big(\iota+\iota'+\ \ldots\ +(\iota)\big).$$

Again, the number of terms to be eliminated will be the sum of the numbers of terms in functions respectively of the sth, $(s-\iota)$th, $(s-\iota')$th ... $\big(s-(\iota)\text{th}\big)$ degrees, which are respectively

$$\begin{gathered}s+1\\s+1-\iota\\s+1-\iota'\\ \cdot\quad\cdot\quad\cdot\\s+1-(\iota),\end{gathered}$$

and the number of these partial functions is $r-1$. Hence the number of terms to be eliminated is

$$(r-1)\big(m+n+\&\text{c}.+p-\iota+\iota'+\&\text{c}.+(\iota)\big)-\big(\iota+\iota'+\&\text{c}.+(\iota)\big)$$
$$=(r-1)(m+n+\&\text{c}.+p)-r\big(\iota+\iota'+\ \ldots\ +(\iota)\big),$$

which is exactly equal to the number of the augmentative functions. Hence the Resultant* of the given functions can be found dialytically by linear elimination, and the exponent of its dimensions in respect to the coefficients of the given functions will be the number

$$(r-1)\Sigma m-r\Sigma\iota,$$

as above found.

The method above given may be replaced by another more compendious, and analogous to that known by the name of Bezout's abridged method for ordinary functions of two letters. As the method is precisely the same whatever the number of the

* The Resultant of a system of functions means in general the same thing as the left-hand side of the final equation (clear of extraneous factors) resulting from the elimination of the variables between the equations formed by equating the said functions severally to zero.

functions employed may be, I shall for the sake of greater simplicity restrict the demonstration to the case of three functions, U, V, W, whose degrees (if unequal, written in ascending order of magnitude) are m, n, p respectively. Let

$$U = F_m(x, y) + F_{m-\iota}(x, y)z^{\iota}$$
$$V = G_n(x, y) + G_{n-\iota}(x, y)z^{\iota}$$
$$W = H_p(x, y) + H_{p-\iota}(x, y)z^{\iota}.$$

Let p, q be taken any two numbers which satisfy in integers greater than zero the equation $\theta+\omega=m+1$, and let

$$F_m(x, y) = \phi_{m-\theta}.x^{\theta} + \phi_{m-\omega}.y^{\omega}$$
$$G_n(x, y) = \gamma_{n-\theta}.x^{\theta} + \gamma_{n-\omega}.y^{\omega}$$
$$H_p(x, y) = \eta_{p-\theta}.x^{\theta} + \eta_{p-\omega}.y^{\omega},$$

where the ϕ's, γ's, η's may be always considered rational integer functions of x and y; for every term in each of the functions F, G, H must either contain x^{θ} or y^{ω}, since, if not, its dimensions in x and y would not exceed $(\theta-1)+(\omega-1)$, *i. e.* $m-1$, whereas each term is of m conjoined dimensions, at least in x and y. Hence from the equations

$$U=0$$
$$V=0$$
$$W=0,$$

by eliminating x^{ω}, y^{θ} we obtain the connective determinant

$$\begin{array}{lll} \phi_{m-\theta}; & \phi_{m-\omega}; & F_{m-\iota} \\ \gamma_{n-\theta}; & \gamma_{n-\omega}; & G_{n-\iota} \\ \eta_{p-\theta}; & \eta_{p-\omega}; & H_{p-\iota}, \end{array}$$

which will be of the degree

$$\big(m+n+p-(\theta+\omega+\iota)\big),$$

i. e. of the degree $(n+p-\iota-1)$ in x and y; and the number of such connectives by principle (c) is p.

Again, by augmentation we can raise each of the functions U, V, W to the same degree as the connectives, and by principle b the number of such will be

$$n+p-m-\iota$$
$$p-\iota$$
$$n-\iota$$

from U, V, W respectively, together making up the number

$$2n+2p-m-3\iota.$$

Hence in all we have $2n+2p-3\iota$ equations; and the number of terms to be eliminated will be $n+p-\iota$, arising from F_m, G_n, H_p; and $n+p-2\iota$ from $F_{m-\iota}$, $G_{n-\iota}$, $H_{p-\iota}$; together making up the proper number $2n+2p-3\iota$.

Each Connective contains ternary combinations of the coefficients, viz. one of the coefficients belonging to that part of U, V, W which contains z^ι, and two coefficients from the other part: the dimensions of the resultant in respect of the coefficients of the former will hence be readily seen to be equal to the number of connectives + the number of terms in the augmentatives into which z^ι enters, *i. e.* will equal $m+n+p-2\iota$; the total dimensions of the resultant in respect to all the coefficients of U, V, W will be $3m+(2n+2p-m-3\iota)$, *i. e.* $2m+2n+2p-3\iota$; and consequently, in respect to the coefficients of F_m; G_n; H_p, will be of

$$(2m+2n+2p-3\iota)-(m+n+p-2\iota),$$

i. e. of $m+n+p-\iota$ dimensions. This result, which is of considerable importance, may be generalized as follows.

Returning to the general system (A.), (for which we have proved that the total dimensions of the resultant are

$$(r-1)(m+n+\ldots p)-r(\iota+\iota'+\ldots+(\iota)),$$

let the coefficients of the column of partial functions

$$\begin{array}{c} F_m \\ G_n \\ \vdots \\ K_p \end{array}$$

be called the first set; the coefficients of the column

$$\begin{array}{c} F_{m-\iota} \\ G_{n-\iota} \\ \vdots \\ K_{p-\iota} \end{array}$$

the second set, and so forth; then the dimensions in respect of the 1st, 2nd ... $(r-1)$th sets respectively are s, $s-\iota$, $s-\iota'$ ... $s-(\iota)$, where

$$s=m+n+\&c.+p-\big(\iota+\iota'+\&c.+(\iota)\big).$$

The important observation remains to be made, that all the above results remain good although any one or more of the indices of dimension of the partial functions in the system (A.), as $m-\iota$, $m-\iota'$, $n-\iota$, &c., should become negative, provided that the terms in which such negative indices occur be taken zero, as will be apparent on reviewing the processes already indicated upon this supposition. If we take $m=n=p=q$, and $\iota=\iota'=\&c. =(\iota)=m-\epsilon$, the exponent of the total dimensions of the resultant becomes

$$(r-1)rm-r(r-2)(m-\epsilon)$$
$$=rm-r(r-2)\epsilon,$$

when $\epsilon=0$, this becomes mr, which is made up of $2m$ units of dimension belonging to the coefficients of the first column, and of m belonging to each of the $(r-2)$ remaining columns. Consequently, if we have

$$\mathrm{F}_m(x, y)+\xi\lambda+\xi'\lambda'=0$$
$$\mathrm{G}_m(x, y)+\eta\lambda+\eta'\lambda=0$$
$$\mathrm{H}_m(x,y)+\zeta\lambda+\zeta'\lambda'=0$$
$$\mathrm{K}_m(x,y)+\theta\lambda+\theta'\lambda'=0,$$

or any other number of equations similarly formed, the result of the elimination is always of m dimensions only in respect of ξ, η, ζ, θ, or of $\xi', \eta', \zeta', \theta'$, and of $2m$ in respect of the coefficients in F, G, H, K.

I now proceed to state and to explain some seeming paradoxes connected with the degree of the resultant of such systems of defective functions as have been previously treated of in this memoir, as compared with the degree of the general resultant of a corresponding system of *complete* functions of the same number of variables.

In order to fix our ideas, let us take a system of only three equations of the form

$$\left.\begin{array}{l}\mathrm{F}_m(x, y)+\mathrm{F}_{m-\iota}(x, y)z^{\iota}=0\\ \mathrm{G}_n(x, y)+\mathrm{G}_{n-\iota}(x, y)z^{\iota}=0\\ \mathrm{H}_p(x,y)+\mathrm{H}_{p-\iota}(x, y)z^{\iota}=0\end{array}\right\} \quad . \quad . \quad . \quad . \qquad (\mathrm{B}.)$$

The resultant of this system found by the preceding method is in all of $2m+2n+2p-3\iota$ dimensions. But in general, the resultant of three equations of the degrees m, n, p is of $mn+mp+np$ dimensions.

Now in order to reason firmly and validly upon the doctrine of elimination, nothing is so necessary as to have a clear and precise notion (never to be let go from the mind's grasp) of the proposition that every system of (n) homogeneous functions of (n) variables has a single and invariable Resultant. The meaning of this proposition is, that a function of the coefficients of the given functions can be found, such that, *whenever* it becomes zero, and *never except* when it becomes zero, the given functions may be simultaneously made zero for some certain system of ratios between the variables. The function so found, which is sufficient and necessary to condition the possibility of the coexistence of the equality to zero of each of the given functions, is their resultant, and by analogy they may be termed its components. It follows that if R be a resultant of a given system of functions, any numerical multiple of any power of R or of any root of R when (upon certain relations being supposed to be instituted between the coefficients of its components) R breaks up into equal factors, will also be a resultant. This is just what happens in system (B.) when $m=n=p=\iota$; the resultant found by the method in the text is of the degree $3m$; the general resultant of the system of three equations to which it belongs is of the degree $3m^2$; the fact being, that the latter resultant becomes a perfect mth power for the particular values of the coefficients which cause its components to take the form of the functions in system (B.).

Suppose, however, that we have still $m=n=p$, but ι less than (m), $6m-3\iota$ will express the degree of the resultant of system (B.); but this is no longer in general an aliquot part of $3m^2$, and consequently the resultant of system (B.) that we have found is no longer capable in general of being a root of the general resultant. The truth is, that on this supposition the general resultant is zero; as it evidently should be, because the values $\frac{x}{z}=0, \frac{y}{z}=0$ satisfy the equations in system (B.), except for the case of $m=\iota$; consequently the resultant furnished in the text, although found by the same process, is something of a different nature from an ordinary resultant; it expresses, not that the system of equations (B.) may be capable of coexisting, but that they may be capable of coexisting for values of $\frac{x}{z}, \frac{y}{z}$ other than 0 and 0. This is what I have elsewhere termed a sub-resultant. But there is yet a further case, to which neither of the above considerations will apply. This is when m, n, p are not equal, but $p-\iota=0$.

On this supposition the degree of the resultant of B becomes $2m+2n-p$, which in general will not be a factor of $mn+mp+np$; and in this case it will no longer be true that the values $\frac{x}{z}=0$, $\frac{y}{z}=0$ will satisfy the system B, inasmuch as the last equation therein cannot so be satisfied. Now if we call the general resultant R and the particular resultant R′, if R′ should break up into factors so as to become equal to $(r')^a \times (s')^b \dots (t')^c$, it might be the case that R should equal $(r')^\alpha . (s')^\beta \dots (t')^\gamma$, and there would be nothing in this fact which would be inconsistent with the theory of the resultant as above set forth; but suppose that R′ is indecomposable into factors, then it is evident that we must have $R=R'.R''$, and consequently that the existence of such a particular resultant as R' will argue the necessity of the existence of another resultant R''; in other words, the resultant so found cannot be in a strict sense the true and complete resultant for the particular case assumed, and yet the process employed appears to give the complete resultant, or at least it is difficult to see how the wanting factor escapes detection. To make this matter more clear, take a particular and very simple case, where $m=2$ $n=2$ $p=2=0$, so as to form the system of equations

$$\left.\begin{array}{rl} Ax^2+Bxy+Cy^2+(Dx+Ey)z & =0 \\ A'x^2+B'xy+C'y^2+(D'x+E'y)z & =0 \\ lx+my+nz & =0 \end{array}\right\} \text{(C.)}$$

By virtue of my theorem, the degree of the resultant R' is $2(2+2+1)-3.1=7$, but the resultant R of the system

$$\left.\begin{array}{rl} Ax^2+Bxy+Cy^2+(Dx+Ey)z+Fz^2 & =0 \\ A'x^2+B'xy+C'y^2+(D'x+E'y)z+F'z^2 & =0 \\ lx+my+nz & =0 \end{array}\right\} \text{(D.)}$$

which becomes identical with the former when $F=0$, $F'=0$ is of $2\times 2+2\times 1+2\times 1$, *i. e.* of 8 dimensions. Hence it is evident that when $F=0$, $F'=0$, R must become $R'\times R''$.

It will be found in fact, that on the supposition of $F=0$, $F'=0$, R becomes equal to $n\times R''$; and accordingly, besides the portion R' of the resultant of system (C.), found by the method in the text, there is another portion n which has dropped through; but it may be asked, is n truly a relevant factor? were it not so, the theory of the resultant would be completely invalidated; but in truth *it is*; for $n=0$ will make the equations in system (C.), *considered as a particular case* of system (D.), capable of coexisting; the peculiarity, which at first sight prevents this from being obvious, consisting in the fact that the values of $\frac{x}{z}$, $\frac{y}{z}$ which satisfy the three equations when $n=0$ become *infinite*.

Thus, finally, we have arrived at a clear and complete view of the relation of the particular to the general resultant.

The general resultant may be zero, in which case the particular resultant is something altogether different from an ordinary resultant; or the particular resultant may be a root of the general resultant, or it may be more generally the product of powers of the simple factors, which enter into the composition of the general resultant; or lastly, it may be an incomplete resultant, the factors wanting to make it complete being such as when equated to zero, will enable the components of the resultant to coexist, but not for other than infinite values of certain of the ratios existing between the variables.

Without for the present further enlarging on the hitherto unexplored and highly interesting theory of Particular Resultants, I will content myself with stating one beautiful and general theorem relating to them; to wit, "if $F=0$, $G=0$, &c. be a given system of equations with the coefficients left general, and R be the resultant of F, G, &c., and if now the coefficients in F, G be so taken that R comes to contain as a factor or be coincident with R'^m, then will $R'=0$ indicate that (when the coefficients are so taken as above supposed) $F=0$, $G=0$, &c. will be capable of being satisfied, not, as in general, by one only, but by (m) distinct systems of values of the variables in F, G, &c., subject of course to the possibility, in special cases, of certain of the systems becoming multiple coincident systems."

I pass on now to the more recondite and interesting theory of the resultant of Ternary Aggregative Functions, that is to say functions of the form

$$F_m(x, y, z) + F_{m-\iota}(x, y, z)\, t^{\iota} + \&c. \ldots + F_{m-(\iota)}(x, y, z) t^{(\iota)},$$

which will be seen to admit of some remarkable applications to the theory of reciprocal polars.

[To be continued.]

XXXIV. *On the Magnetism of Pewter Coils.* *By* Reuben Phillips.

[Continued from vol. xxxvii. p. 288.]

149. IT will be in the recollection of the readers of the former papers, that I thought I had discovered that a jet of steam escaping into the air is magnetic, which I attributed to thermo-electric currents passing between the hotter and colder particles; thence, in endeavouring to discover the equivalent static effect, I found instead, that mode of electrical development consisting of the friction of gaseous matter on water, and which in my opinion completely solves the question of the source of atmospheric electricity. I have found since then I was wrong in ascribing magnetic properties to the jet of steam, the magnetic disturbance being situated in the metallic steam-passages, and it should seem directly related to terrestrial magnetism.

150. A straight glass tube, about ·3 inch diameter and 17 inches long, connected the glass coil with the condenser. All effect on the magnetic needle now ceased, even when the coil was kept cool by being partly immersed in water and covered with wet cloth.

151. The brass jet (9.) was united to the end of a glass tube

8 inches long, the other end of the tube communicating with the condenser. The only effect now produced by the steam on the magnetic needle was a slight tremor, owing to the concussions it produced in the air shaking the apparatus: the magnetism of the steam (11.) had completely disappeared. The boiler was now moved until the condenser stood with regard to the galvanoscope just as it did before (11.); opening the cock of the boiler gave a strong swing to C, and a strong start to A when the cock was closed.

152. It is, I think, now quite certain, that in such experiments as (9, 145.) the real place of magnetic excitation is in the apparatus through which the steam passes before it enters the atmosphere or the glass tube.

153. The pewter coil (29.) was united to the condenser, and the other end held the brass jet (9.); some of the pewter coil had been opened out, so that there was a distance of 11 inches between the nearest part of the condenser and the coil. The steam was at 6 lbs. on the inch. The coil being cool, when the steam was turned on the needle moved about $\frac{3}{4}$ the length of the scale to C. The coil was removed, and some steam was passed through the condenser to warm it and to blow the water out of the steam passages. A piece of cane, of the size before described (116.), and which had been soaked for some hours in a similar solution of soda, was now put in the pipe of the condenser, and then the coil was quickly restored as before. On opening the cock of the boiler the swing was unaltered in direction, and, as far as I could judge from many experiments, in force also. The fluid that escaped from the brass jet felt strongly alkaline. Considering the ready solubility of the oxides of tin and lead in a solution of caustic alkali, it appeared to me fair to infer that this magnetism was not produced by a chemical action. The following experiment is, I think, conclusive on this point.

154. A stop-cock was united to the cock of the boiler in the place of the condenser. From this stop-cock proceeded horizontally a glass tube which continued straight for 14 inches; it then descended vertically for $1\frac{1}{4}$ inch, and then again horizontally in the same direction as before for a distance of 1 inch; at this place the glass tube joined a straight platinum tube 5 inches long, lying in the direction of the glass tube at the place where the glass and platinum tubes were united; the other end of the platinum tube received a glass tube, which, at a short distance from the platinum tube, ascended nearly perpendicularly, and then went in the first direction of the glass tube of the stop-cock: the tube proceeding from the platinum was contracted at the end, which finally discharged the steam to an orifice $\frac{3}{40}$ inch diameter. The steam as it issued from this apparatus was received into a

glass catch-tube. A copper pan was brought under the platinum tube, so that the tube could easily be put under water by filling the copper pan. The pan contained stout wire supports, on which rested a square piece of iron 4·5 inches in each side and $\frac{1}{20}$ inch thick, which had been made red-hot and slowly cooled; a good bearing for the iron was obtained on three points, and it lay horizontally at a vertical distance of about $\frac{3}{16}$ inch from the under side of the platinum tube: supposing a perpendicular plane to have been raised from the iron to the nearest line on the platinum tube parallel with its axis, it would have been found that the iron extended $\frac{3}{8}$ inch in an easterly direction from this plane towards the galvanoscope, at which distance the iron came in contact with one of the flat sides of the pan. The platinum tube was united to the glass tube by India-rubber, oiled silk and thread only; and the needle of the galvanoscope lay about parallel with, and in the same horizontal plane as, the axis of the platinum tube.

155. With the steam at about 25 lbs. per inch, five puffs sent the edge of the needle about the whole length of the scale; and the swing was to A when the steam was turned on. In this way I made many experiments, having the platinum tube sometimes partially and sometimes entirely covered with water; but I could not observe any decided difference, although I think on the whole the magnetism was perhaps rather stronger when the tube was about $\frac{1}{3}$rd immersed; the tube was always parallel with the surface of the water. The internal diameter of the platinum tube was $\frac{1}{4}$ inch, and it was about $\frac{1}{40}$ inch thick.

156. A pewter tube 5 inches long, $\frac{9}{40}$ inch internal diameter, and $\frac{1}{20}$ inch thick was substituted for the platinum tube. When the tube was about one-third covered with water, the swing was just as with the platinum tube; but when the tube lay entirely under water, the swing was about one-half less; with this exception everything was as with the platinum tube.

157. I could produce no effect on the galvanoscope by substituting a glass tube, nor with the metal tubes when the iron was away, nor when the iron was in its place unless the tubes were kept cool by water.

158. The variation in the magnetic intensity of the pewter tube led me to suppose that the direction of the magnetism of a coil would vary according as cold was applied to it. A pewter pipe 3 feet 8 inches long, $\frac{1}{4}$ inch internal diameter, and $\frac{1}{20}$ inch thick, was wound up into a helix; the convolutions lay regularly side by side, but without overlapping; the interior of the coil measured 1·3 inch diameter, and there were six convolutions. A piece 7 inches long was left unwound at each end of the pewter pipe, and the ends were each furnished with a stop-cock having

a steam-way $\frac{3}{20}$ inch diameter; these stop-cocks are denoted respectively by N and O. There was also a thick wad of loosely spun cotton, which could either be wrapped about the coil or stuffed into it. In these experiments it was found necessary to interpose a sheet of lead-foil between the zinc screen and the coil; otherwise the steam produced from the wad, striking on the shield, considerably interfered with the purity of the result.

159. The wad was placed on the outside of the coil and thoroughly drenched with water. N was now united by a brass connecting piece to the cock of the boiler; O was partly and sometimes fully opened and N shut. On opening N the swing was to C; five puffs gave a swing about three-quarters the length of the scale. In these experiments the cock of the boiler is always to be understood as open.

160. N was partly opened; on alternately opening and shutting O, the swing was to A when O was opened, and to C when O was shut. When N was fully opened, the swing was much less.

161. The wad was now removed from the outside and thrust inside the coil and saturated with water. O being partly open, on fully opening N the swing was to A, and rather less than the swing to C when the wad was outside.

162. The stop-cocks N and O were used as described in (160.). When O was shut the needle moved to A, and to C when O was opened. In experiments where either N or O was partly opened, the area of the steam-way was probably about equal to a circle $\frac{1}{12}$ inch diameter.

163. Many other experiments were made with this coil, which it is not considered necessary to mention, as they only showed, in addition to what I have just narrated, that which is abundantly proved by former experiments; namely, that the two ends of the axis of the coil are in opposite magnetic states, and that the direction of the magnetism is independent of the direction of the motion of the steam.

164. One end of the pipe of the coil (29.) was united to the cock of the boiler, and there was a distance of ten inches of pewter pipe between the coil and the connecting pieces; the other end of the pipe of the coil held the brass jet (9.). The axis of the coil was placed perpendicular to the horizon. The needle of the galvanoscope lay in a horizontal plane about ·4 inch lower than a horizontal plane resting upon the upper end of the axis of the coil. When the steam passed, the swing was to A, and one puff sent the needle nearly the length of the scale. The apparatus was now adjusted so that the needle stood ·6 inch lower with regard to the coil; the swing was still to A, and nearly as strong as before. The needle was now placed about equidistant from either end of the coil; the swing was still to A,

but not above half as strong. The galvanoscope was again lowered with respect to the coil; the swing was now to C, and powerful. Finally, the galvanoscope was again lowered until the needle stood in the same position with respect to the lower end of the axis of the coil as it did at the commencement with regard to the upper end; the swing was to C, and equally powerful with the corresponding swing to A.

165. The coil was now arranged as usual with its axis horizontal, having that end which before pointed to the zenith now directed to the needle of the galvanoscope. The swing was to A when the steam passed.

166. The pewter coil (29.) was supported in the same position as in (165.), and so arranged that the fountain (77.) could easily be connected with or removed from the coil. The zinc screen and galvanoscope were used as in the foregoing experiments with steam; and the water-way of the cock of the fountain, which was the passage of the least diameter through which the water passed, was $\frac{3}{20}$ inch across. Water was placed in the fountain and made to boil, the air was then pumped in, and the fountain was united with the coil. On opening the cock of the fountain, and so allowing the water to flow through the coil, the needle started towards A, and the swing produced was about one-third the length of the scale. The fountain was immediately removed, rinsed out with cold water, charged again with cold water, and then reunited with the coil which was still hot. The cock of the fountain now being opened, the swing was to C, and nearly as strong as before.

167. The iron core (131.) was placed in the coil. Hot water now being sent through the coil produced a violent swing to A; and then, while the coil was hot, sending cold water through it produced nearly as strong a swing to C; in both instances the needle vibrated across and considerably outside the field of view. Sending cold water through, the coil also being cool, produced no magnetic effect.

168. These experiments show that the magnetism of the coils is an effect of heat independent of condensation.

169. A piece of stout copper wire, 4 feet 2 inches long and $\frac{1}{8}$ inch diameter, was covered by winding tape about it; some copper wire, $\frac{1}{40}$ inch diameter, and covered, was now wound outside the tape, and the rounds lay nearly close together without overlapping. The stout copper wire, having been thus covered throughout its whole length, was next wound up into a helix of eleven convolutions, which did not overlap each other, and the internal diameter of the coil was 1·3 inch. The current from a galvanic battery being transmitted through the fine copper wire, caused this compound helix to possess similar magnetic properties to those of the pewter coils.

170. In the experiment with the pewter coil (159.), there is a difference of temperature between the exterior and interior surfaces of the tube, this difference of temperature being greater at those parts of the tube forming the exterior of the coil than at those portions internally situated. Now the difference of temperature will, from a thermo-electric action, throw the surfaces into opposite electrical states, the intensity of which will vary with the difference of temperature; consequently those parts of the tube which have the greatest difference of temperature will produce electricity, which will circulate by overcoming the resistance opposed by those parts where the difference of temperature is less. Suppose a small portion of pipe cut out from the coil in the shape of a right cylinder. Now looking at one end, which gives an annulus, and bisecting this annular space, not the metal itself, by a straight line, and supposing that in one of the halves so obtained the difference of temperature is greater than in the other half; then a chain of the metallic particles producing electricity will cause the current to flow along the curved boundary of the metal until it comes to a part where the difference of temperature is less, and at this point it will more or less force its way across; thus forming a circuit, the contour of which is some segment of the annulus, and the tube may be regarded as built up of many such annuli. Such a system of currents, it can easily be shown by direct experiments, produces a magnetism parallel in direction with the axis of this bit of tube; consequently, the system of currents existing in the pewter coils in fact resembles that of the copper helix.

171. But to account for the regular magnetism of the pewter coils, it is necessary to suppose that the direction of the currents in each part of the pipe is, on the whole, the same with regard to the coil; and this requires the admission of some force or property in the coil capable of giving uniform direction to the currents. This view involves the least assumption, nevertheless its application to many of the experiments is exceedingly difficult. If an experiment could be devised in which this magnetism could be developed under circumstances where thermo-electric currents could not exist, it would perhaps be necessary to look upon the effect as the direct conversion of heat into magnetism.

172. The magnetism of these pewter coils affords, as far as it goes, a sufficient explanation of the cause of terrestrial magnetism; for the internal heat of the earth continually passing outwards produces electric currents; then, assuming the existence of a structural force, and which can hardly be unconnected with stratification, capable, as with the pewter coils, of giving regularity to the arrangement of these currents, and the product will be a regular magnetic force, such as is terrestrial magnetism. That Humboldt's isothermal bands should coincide with Sabine's

lines of equal magnetic intensity, follows at once from referring terrestrial magnetism to the internal heat of the earth; for where the climate is coolest, the difference of temperature will be the greatest, and there the electric developments will be more powerful. This explanation of the cause of terrestrial magnetism is so far independent of the hypothesis used to account for the magnetism of the coils, that if the magnetism of the coils proceeds from the direct conversion of heat into magnetic force, then terrestrial magnetism results from a similar transformation.

---

173. *On the specific inductive capacity of cloud.*—The tin pipe was arranged before the brass jet of the boiler as in (83.), but with the longer arm pointing to the zenith; and in the longer arm of the tin pipe, and concentric with it, stood a copper tube having a cork inserted at each end; the upper one of these corks received the end of a glass tube, which soon after leaving the cork turned off at a right angle, and this horizontal portion of the glass tube was put in a tube-holder and thus supported and insulated the copper tube; finally, a copper wire passed through the whole length of the glass tube to the copper tube; and consequently, on bringing the knob of a charged Leyden jar into contact with one end of the wire, the copper tube became electrified inductrically, and the tin pipe inducteously. The copper tube was externally $\frac{11}{20}$ inch diameter, and it extended a distance of 7 inches into the tin pipe.

174. A large Leyden jar was now charged positively by a plate machine, and then the knob of the jar was supported against the end of the copper wire. The tin pipe was presently connected with an electrometer, and the electricity was carefully drawn off from the copper pipe by means of a point which I held in my hand, until the electricity passed but very slowly between the copper and the tin pipes; the tin pipe was now put in communication with the single-leaf electrometer, and the electric intensity of the Leyden jar still further reduced if the electricity passed too rapidly. The intensity with which I preferred working being that at which the positive electricity nearly ceased to pass to the tin pipe; consequently, the electrometer connected with the tin pipe, after being discharged, would soon indicate a feeble positive charge. The cock of the boiler was now opened so that the issuing steam might leave the boiler positive; the only effect of which on the electrometer was a feeble negative action, produced by the negative steam-cloud acting inductrically on the tin pipe.

175. It occasionally happened, from circumstances which I could not succeed in determining, that the steam-cloud re-established a communication with the copper and tin pipes; and so

much positive electricity would at these times pass to the electrometer as was apt to destroy the gold-leaf.

176. The foregoing experiment (174.) is decisive in showing that the increase of electricity in such experiments as (95.) does not result from the specific inductive capacity of cloud, which can differ but little if at all from air. The following experiment is a proof that this effect of the steam in increasing the electricity of a jet of water does not depend on the cold water condensing the steam-cloud.

177. The fountain, tin pipe, &c. were arranged as in (94.), except that the distance between the lower end of the brass jet of the fountain and the upper end of the tin pipe was diminished to 5·5 inches. Water was placed in the fountain and caused to boil, the air was pumped in, and the glass tube and jet screwed into its place. The fountain was connected with the ground, and the tin pipe with the single-leaf electrometer, and then the cock of the fountain was opened a little to give a stream of sufficient force. The bulb of a thermometer was now held in the stream of water just inside the upper end of the tin pipe; the mercury soon reached 142°; the thermometer was removed, and the electrometer was observed to be scarcely affected; but on now opening the cock of the boiler a little, and so filling the tin pipe with cloud, the quantity of electricity produced by the stream of water was greatly increased, the gold-leaf rapidly striking the conducting plate. The thermometer was again brought into the stream of water as before; the steam still passing, it marked 149°; the thermometer was now placed in the steam-cloud in the tin pipe and soon fell to 125°.

178. Of course these numbers given by the thermometer, depending as they do upon the extent to which the cocks were opened, varied in every experiment; for example, another experiment conducted as the foregoing gave the first temperature of the water 154°, the second temperature 156°, and the temperature of the steam-cloud 123°. The increase of electricity produced by the steam-cloud appeared to be quite as great when hot water was discharged from the fountain as when the water was cold.

179. It should seem there can be now only one property of the steam-cloud to which this increase of electricity can be ascribed, namely, some species of conduction, which, by lowering the electrical intensity of the jet of water, would account for the phænomenon. Something of this power appears in the experiments (43, 52.) in the connexion existing between the tube and the boiler. The following experiment is still less ambiguous.

180. A large Leyden jar was charged positively and inverted on a proper support, so that the distance between the end of the brass jet of the boiler and the centre of the ball of the jar was

$3\frac{1}{2}$ inches. When the cock of the boiler was opened, the steam struck on the lower side of the ball and passed thence to a wire-gauze screen, placed at a distance of ten inches from the ball; this screen was connected with the two-leaved electrometer. Positive electricity passed very slowly to the screen; but on now allowing the steam to escape negatively electrified, the leaves of the electrometer almost immediately diverged to their full extent; on shutting off the steam, positive electricity began to pass about as slowly as before; again allowing the steam to play on the ball, the leaves diverged with positive electricity as previously, and these actions could be repeated a great many times.

181. The account of the jet of steam (62.), when the rough noise is being produced, is not sufficiently full and accurate. The main distinction in the appearance of the jet of steam with the hiss and the roar is, that with the hiss there is a transparent space between the orifice and the steam-cloud of about half an inch; but with the roar this interval is of a dense white; and the mass of white can be seen to extend two or three inches into the steam-cloud, as it does not diverge from the axis of the projected jet so rapidly as the steam. When there is only a smooth hiss, one or more transparent lines of water proceed from the edge of the orifice, and which I think are never longer than the one-eighth of an inch, and certainly shorter if more than one fibre appears. If when the roar is set up, the jet of steam be viewed near to the orifice, the whiteness is seen not to be uniform, but to be made up of a number of little white cones, having their vertices stuck on the margin, and I think also on the interior of the orifice, which cones coalesce at a short distance from the orifice. The cones are apt to proceed from the same places which the fibres of water frequent; perhaps they are produced from the fibres by a rotary motion resulting from two rectangular vibrations.

7 Prospect Place, Ball's Pond Road.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 158.]

May 8, 1851.—The Earl of Rosse, President, in the Chair.

A PAPER was read, entitled "Memoir on the Megatherium. Part II." By Prof. Owen, F.R.S. Received May 6, 1851.

The author premised a brief sketch of the successive steps which had led to the knowledge of the Megatherium acquired at the date of his researches, and of the different hypotheses which had been broached of its affinities, habits and food. He then recounted the mode of the acquisition of the complete skeleton, and of its articulation, at the British Museum, and commenced its description by the vertebræ of the trunk. These consist of 7 cervical, 16 dorsal, 3 lumbar, 5 sacral, and 18 caudal vertebræ. The first to the fifth dorsal vertebræ are characterized by having the ordinary number of articular processes (zygapophyses), two before and two behind; and by having three articular surfaces for the ribs on each side, one on the centrum, one on the neurapophysis, and one on the diapophysis. The sixth dorsal vertebra has an accessory zygapophysis between the posterior pair; the thirteenth dorsal has one between the anterior pair; the seventh to the twelfth inclusive have the accessory median zygapophysis between both the anterior and posterior pairs of the ordinary zygapophyses. The fourteenth and succeeding dorsals have no costal surface on the diapophysis or centrum. The fifteenth has both metapophysis and anapophysis—the latter with an articular surface: the sixteenth superadds the parapophysis with an articular facet.

The lumbar vertebræ lose the costal surface on the centrum, and retain the metapophyses, anapophyses and parapophyses. The nature of these accessory processes was explained by reference to the descriptions and figures of the exogenous processes of vertebræ in Part I. of the present Memoir.

The characteristics of the cervical vertebræ were next detailed.

Of the five anchylosed sacral vertebræ, three are confluent with the iliac bones, and two with the ischia.

The fourteen anterior caudals are characterized by articular surfaces for hæmapophyses. These elements are separate from each other in the first caudal, and confluent as usual at their distal ends, forming a 'chevron-bone' in the others. The posterior zygapophyses lose their articular surfaces in the eleventh caudal; the anterior ones disappear in the twelfth: the metapophyses have subsided in the fifteenth. The neural canal is unclosed above in the sixteenth; and the vertebra is reduced to its central element in the last two caudals.

The skull is remarkable for its small proportional size, for its long and slender cranial portion, its large and complex zygomatic arches, its broad truncate facial part, with the slender produced premaxillaries, and for the great depth of the middle of the lower jaw.

The mastoid element developes a large tuberous process and a deep semicircular articular cavity for the stylohyal. The malar bone sends down a long process outside the lower jaw. The number of teeth is $\frac{5-5}{4-4}=18$, the fifth in the upper jaw being the smallest. They are alike in structure, and differ but little in shape: the grinding surface in most is crossed by two transverse ridges; the summits of which are formed by hard dentine; the rest of the tooth being composed of a central body of vaso-dentine and a peripheral mass of vascular cement. The microscopic characters of these several constituents of the teeth were then described. Each tooth is deeply implanted in the jaw, where it terminates without dividing into fangs, by a widely open pulp-cavity for a persistent matrix, ensuring perpetual growth. The stylohyal bone has the form of a hammer, with a long, slightly bent handle; one part of the head being thickened and rounded for articulation with the cavity in the mastoid.

The scapula presents almost the form of a trapezium, with the inferior angle bent outwards, increasing the depth of the subspinal fossa: there is a rudiment of a second spine, below the normal one: the acromion is expanded, produced and confluent with the coracoid; and the supraspinal fossa is perforated by a circular aperture. The clavicle has a well-marked sigmoid flexure, equally-developed obtuse extremities, without any articular surface. The humerus is remarkable for the enormous development of ridges for the attachment of the muscles, especially at its distal end: the inner condyle is not perforated as in the *Megalonyx*; it is devoid of a medullary cavity.

The ulna and radius are next described. The carpus consists of seven bones, three of which are proper to the first row, three to the second, and one is common to both: the latter answers to the 'scaphoides' and 'trapezium' in the human wrist, and articulates with the radius above, and the rudiment of the metacarpal of the pollex below.

Only four digits are developed, the first or 'pollex' being obsolete. The 'index' or second digit has three phalanges, the last supporting a large claw, and being twice as long as the two preceding phalanges. The proximal and middle phalanges of the 'digitus medius' are confluent. The ungual phalanx is shorter than that of the index, but has twice its vertical breadth. The metacarpals progressively increase in length from the first to the fifth. The fourth digit or 'annularis' has three phalanges, the last being unguiculate and longer than that of the 'medius.' The fifth digit has only two very short rounded phalanges, which were doubtless buried in a thick callous outer border of the foot, on which the Megatherium rested when applying the foot to the ground.

The pelvis shows the conversion of the ischiadic notch into a foramen by the anchylosis of the ischia with the posterior sacral vertebræ, and the union of the ossa pubis at a short anteriorly produced symphysis. The ilia are extraordinary for their vast breadth, and the thickness of the rugged labrum; indicative of the enormous muscular forces, of which this conspicuous part of the skeleton was the centre.

The femur is hardly less remarkable for its breadth and strength. The head is devoid of an impression for the ligamentum teres: but from the dimensions of the hemispheroid cavity receiving it, the author calculates that the muscles are aided in retaining the head of the femur in its place by an atmospheric pressure, with the barometer at 30 in., of not less than 660 pounds. At the distal end of the femur there is a great angular projection above the outer condyle. The rotular surface is continuous with that upon the outer condyle, but not with the inner one. The tibia and fibula are anchylosed together at both their extremities. Besides the patella in front of the knee-joint, there is a sesamoid 'poplitella' behind, wedged between the outer condyle and the tibia; which was doubtless imbedded at its base in the femoro-tibial articular capsule, and gave insertion to the tendon of the *popliteus* muscle. This sesamoid is not to be confounded with the 'fabella,' developed in many quadrupeds in the origin of the gastrocnemius, behind one or both condyles of the femur. The most peculiar feature in the tibia of the Megatherium is the form of the distal articular surface: especially the large and deep hemispherical excavation on the inner part of that surface for an unusually secure interlocking of the foot to the leg.

The bones of the tarsus are six in number in the Megatherium, and the astragalus offers corresponding peculiarities with those of the tibia with which it is articulated, and also remarkable modifications for the articulation of the naviculare and calcaneum. In the calcaneum, the length and strength of the hinder prominence forming the great lever for the extension of the foot, are amongst its most striking characteristics. These, with those of the other bones of the tarsus, are minutely detailed. There is no digit answering to the great toe or 'hallux,' nor any trace of the 'os cuneiforme' for that toe. The innermost of the 'ossa cuneiformia' answers to the middle one, and if any rudiment of the second toe ever existed independently, it has coalesced with that cuneiform bone: but this cannot be supposed to represent both middle and internal cuneiform bones and their digits blended together, as Cuvier supposed. There are no little bones missing from the inner side of the middle cuneiforme, as Pander and D'Alton conjectured. The first or innermost distinct metatarsal bone is that of the toe answering to the third, or digitus medius, in the pentadactyle foot: it is a short thick irregular wedge-shaped bone, with a large triangular concave base for the 'ecto-cuneiforme'; a semicircular flattened surface on the outer side for the fourth metatarsal, and a small semi-elliptic flat surface on the inner side for the 'meso-cuneiforme'. The distal end of the bone presents a strong median vertical obtuse ridge, dividing two vertically elongated slightly concave surfaces, to which the anchylosed proximal and middle phalanges of the strong claw-bearing digit articulate. The ungual phalanx is shorter in proportion to its depth, than in the digitus medius of the fore-foot, and differs in the greater breadth of the upper part of the claw-sheath, and in the straighter cone, or bony core, which supported the claw. The metatarsals of the fourth and fifth toes are much larger than that of the third; but they support mere rudiments of digits reduced in each to two stunted phalanges, which were doubtless buried like those of the outer digit in the fore-foot in a kind of callous hoof.

Having completed the description of the skeleton, which is illustrated by an extensive series of accurate and highly finished drawings, the author proceeds to the comparison of the modifications of the osseous structure of the gigantic extinct animal with that in other known existing and extinct species of the class Mammalia.

The teeth agree in number, kind, mode of implantation and growth, with those of the Sloth, and their structure is a modification of that peculiar to the Sloth-tribe. All the modifications of the skull relating to the act of mastication, especially the large and complex malar bone, repeat the peculiarities presented by the existing Sloths. There are the same hemispheric depressions for the hyoid bone in the Megatherium as in the Sloth. In the number of cervical vertebræ the Megatherium, like the two-toed Sloth, agrees with the Mammalia generally. In the accessory articular surfaces afforded by the anapophyses and parapophyses of the hinder dorsal and lumbar vertebræ, the Megatherium resembles the Ant-eaters

(*Myrmecophagæ*): but it does not resemble the Armadillos (*Dasypus*) in having long metapophyses, the peculiar development of which in those loricated *Bruta* has a direct relation to the support of their bony dermal armour. In the mesozygapophyses of the middle dorsal vertebræ the Megatherium is peculiar. In the small extent of the produced and pointed symphysis pubis it resembles the Sloths; and in the junction of both ilium and ischium with the sacrum, it manifests a character common to the Edentate order; but in the expanse and massiveness of the iliac bones, it can only be compared with other extinct members of its own peculiar family of Phyllophagous Edentata. Its habits necessitating a strong and powerful tail, we find this resembling in its bony structure that of other Edentata with a similar appendage, especially in the independency of the two hæmapophyses of the first caudal, a character which obtains in the Great Ant-eater and in some Armadillos; but this is no evidence of direct affinity to either of these families; the habits of the small arboreal Sloths render their eminently prehensile limbs sufficient for their required movements, and the tail is wanting. Had that appendage been proportionally as large as in the Megatherium, we cannot suppose that the caudal vertebræ would have materially differed from those of other Edentata.

In the coalescence of the anterior vertebral ribs with the bony sternal ribs, the Megatherium resembles the Sloths. This essential affinity is still more marked in the peculiarities of the scapula and of the carpus. In the *Myrmecophaga jubata*, the scaphoid is distinct: in the *Manis* it coalesces with the lunare: in the *Dasypus gigas* the trapezoides is anchylosed to the second metacarpal: in the *Das. sexcinctus* it has coalesced with the trapezium. Not any of these characteristics are manifested by the Megatherium: its carpus repeats the peculiarities of that in the Sloths, viz. the reduction of the number of carpal bones to seven by the coalescence of the scaphoid with the trapezium. The first digit (pollex), which is retained in the Anteaters and Armadillos, is obsolete in the Megatherium as in the Sloths and Orycteropus: three digits are fully developed and armed with claws, as in the *Bradypus tridactylus*; and the fifth, though incomplete in the Megatherium, is better developed, because it was required in the ponderous terrestrial Sloth for its progression on level ground. In no existing ground-dwelling Edentate is the fifth digit deprived of its ungual phalanx, as in the Megatherium. The bones of the fore-foot of that extinct animal are thus seen to be modified mainly after the type of the *Bradypodidæ*.

The long bones of all the limbs are devoid of medullary cavities, as in the Sloths. The femur lacks the ligamentum teres as in the Sloths. The fibula is anchylosed to the tibia at both ends in Megatherium, as in *Dasypus*; but this is not the case in the closely-allied extinct Megatherioids called *Mylodon*, *Megalonyx* and *Scelidotherium*, a fact which diminishes the force of the argument which Cuvier deduced from the coalesced condition of the bones in the Megatherium in favour of its affinities to the Armadillos. The semi-inverted but firm interlocking articulation of the hind-foot to the leg shows the peculiarities of that joint in the Sloths exaggerated, and departs further from its characteristics in other Edentata. In all the existing *Edentata*, save the Sloths, the hind-foot is pentadactyle, and four of the toes have a long claw, even in the little arboreal *Myrmecophaga didactyla*: the departure by degradation from the pentadactyle type is a peculiar characteristic of the Sloth-tribe in the order. It is carried further in the same direction in the great extinct terrestrial Sloths. In these the mutilation of the foot has commenced on the outer side by the removal of the ungual phalanx from the fifth and fourth toes; but this accompanied by modifications which adapt these toes to the important office of support and progression of the body on level ground. In the scansorial Sloths, the three middle digits being equally developed for prehension, one toe on the outer and one on the inner side of the foot, are reduced to their metatarsal basis. In the Megatherium the mutilation of the foot on the inner side is carried to a greater extent; the innermost toe or hallux, with its entocuneiform bone, is wholly removed: the second toe is represented, like the first in the Sloths, by its cuneiform bone and a coalesced rudiment of the metatarse: and it is only the third toe or medius that repeats the condition of the claw-bearing toes in the climbing Sloths.

Finally, the author enters upon the question of the habits and food of the Megatherium. Guided by the general rule that animals having the same kind of dentition have the same kind of food, he concludes that the Megatherium must have subsisted, like the Sloths, on the foliage of trees; but that the greater size and strength of the jaws and teeth, and the double-ridged grinding surface of the molars in the Megatherium, adapted it to bruise the smaller branches as well as the leaves, and thus to approximate its food to that of the Elephants and Mastodons. The existing Elephants and the Giraffe are specially modified to obtain their leafy food; the one being provided with a proboscis, and the entire frame of the lofty Giraffe adapting it to browse on branches above the reach of its largest ruminant congeners. If the Megatherium possessed, as Cuvier conjectured, a proboscis, it cannot, judging from the suborbital foramina, have exceeded in size that of the Tapir, and could only have operated upon branches brought near its mouth. Of the use of such a proboscis in obtaining nutritious roots, on the prevalent hypothesis that such formed the sustenance of the Megatherium, it is not easy to speculate: the hog's snout might be supposed to be more serviceable in obtaining those parts of vegetables; but no trace of the prænasal bone exists in the skull. A short proboscis would be very useful in rending off the branches of a tree prostrated and within reach of the low and broad-bodied Megatherium, and it would be aided in this act by the tongue, of which, both the hyoid skeleton, by its strength and articulation, and the foramina for the muscular nerves by their unusual area, attest the great size and power.

As regards the limbs, the Megatherium differs from the Giraffe and Elephant in the unguiculate character of certain of its toes, in the power of rotating the bones of the fore-arm, in the corresponding development of supinator and entocondyloid ridges in the humerus, and in the possession of complete clavicles. These bones are requisite to give due strength and stability to the shoulder-joint for varied actions of the fore-arm, as in grasping, climbing and burrowing: but they are not essential to scansorial or fossorial quadrupeds; the Bear and the Badger have not a trace of clavicles, and the mere rudiments of these bones exist in the Rabbit and the Fox. We must seek, therefore, in the other parts of the organization of the Megatherium, for a clew to the nature of the actions by which it obtained its food. In habitual burrowers the claws can be extended in the same plane as the palm, and they are broader than they are deep. In the Megatherium the depth of the claw-phalanx exceeds its breadth, especially in the large one of the middle finger; and they cannot be extended into a line with the metacarpus, but are more or less bent. Thus, although they might be used for occasional acts of scratching up the soil, they are better adapted for grasping; and the whole structure of the fore-foot militates against the hypothesis of Pander and D'Alton, that the Megatherium was a burrowing animal. The same structure equally shows that it was not, as Dr. Lund supposes, a scansorial quadruped; for, in the degree in which the foot departs from the structure of that of the existing Sloths, it is unfitted for climbing; and the outer digit is modified, after the ungulate type, for the exclusive office of supporting the body in ordinary terrestrial progression. It may be inferred from the diminished curvature and length, and from the increased strength and the inequality of the claws, especially the disproportionately large size of that weapon of the middle digit, that the fore-foot of the Megatherium was occasionally applied by the short and strong fore-limb in the act of digging; but its analogy to that of the Ant-eaters teaches that the fossorial actions were limited to the removal of the surface-soil, in order to expose something there concealed, and not for the purpose of burrowing. Such an instrument would be equally effective in the disturbance of roots and ants; it is, however, still better adapted for grasping than for delving. But to whatever task the partially unguiculate hand of the Megatherium might have been applied, the bones of the wrist, fore-arm, arm and shoulder, attest the prodigious force which would be brought to bear upon its execution. The general organization of the anterior extremity of the Megatherium is incompatible with its being a strictly scansorial or exclusively fossorial animal, and its teeth and jaws decidedly negative the idea of its having fed upon insects; the two extremes in regard to the length of the jaws are presented by the phyllophagous and myrmecophagous members of the Edentate order, and the Megatherium in the shortness of its face agrees with the Sloths.

Proceeding then to other parts of the skeleton for the solution of the question as to how the Megatherium obtained its leafy food, the author remarks that the pelvis and hind limbs of the strictly burrowing animals, *e.g.* the Mole, are remarkably slender and feeble, and that they offer no notable development in the Rabbit, the Orycterope, or other less powerful excavators. In the climbing animals, as *e.g.* the Sloth and Orang, the hind-legs are much shorter than the fore-legs, and even in those Quadrumana in which the prehensile tail is superadded to the sacrum, the pelvis is not remarkable for its size or the expansion of the iliac bones. But in the Megatherium the extraordinary size and massive proportions of the pelvis and hind limbs arrest the attention of the least curious beholder, and become eminently suggestive to the physiologist of the peculiar powers and actions of the animal. The enormous pelvis was the centre whence muscular masses of unwonted force diverged to act upon the trunk, the tail, and the hind legs, and also by the 'latissimus dorsi' on the fore-limbs. The fore-foot being adapted for scratching as well as for grasping, may have been employed in removing the earth from the roots of the tree and detaching them from the soil. The fore-limbs being well adapted for grasping the trunk of a tree, the forces concentrated upon them from the broad posterior basis of the body may have co-operated with them in the labour, to which they are so amply adapted, of uprooting and prostrating the tree. To give due resistance and stability to the pelvis, the bones of the hind-legs are as extraordinarily developed, and the strong and powerful tail must have concurred with the two hind-legs in forming a tripod as a firm foundation for the massive pelvis, and affording adequate resistance to the forces acting from and upon that great osseous centre. The large processes and capacious spinal canal indicate the strength of the muscles which surrounded the tail, and the vast mass of nervous fibre from which those muscles derived their energy. The natural co-adaptation of the articular surfaces shows that the ordinary inflection of the end of the tail was backwards as in a *cauda fulciens*, not forwards as in a *cauda prehensilis*. Dr. Lund's hypothesis, therefore, that the Megatherium was a climber and had a prehensile tail, is destroyed by the now known structure of that part.

But viewing, as the author conceives, the pelvis of the Megatherium as being the fixed centre towards which the fore-legs and fore-part of the body were drawn in the gigantic leaf-eater's efforts to uprend the tree that bore its sustenance, the colossal proportions of its hind extremities and tail lose all their anomaly, and appear in just harmony with the robust claviculate and unguiculate fore-limbs with which they combined their forces in the Herculean labour.

The author then referred to the *Mylodon robustus*, a smaller extinct species of the same natural family of phyllophagous *Bruta*, and to the additional arguments derivable from the skeleton of that animal in favour of the essential affinity of the Megatherium to the Sloths; and the light which the remarkable healed fractures of the skull of a specimen in the Museum of the College of Surgeons threw upon the habits and mode of life of the species.

Finally, with reference to the hypothesis of the German authors and artists of the degeneration of the ancient Megatherioids of South America into the modern Sloths, the author remarked that the general results of the labours of the anatomist in the restoration of extinct species, viewed in relation to their existing representatives of the different continents and islands, commonly suggested the idea that the races of animals had deteriorated in point of size. Thus the palmated Megaceros is contrasted with the Fallow-deer, and the great Cave-bear with the actual Brown Bear of Europe. The huge

Diprotodon and Nototherium afford a similar contrast with the Kangaroos of Australia, and the towering Dinornis and Palapteryx with the small Apteryx of New Zealand. But the comparatively diminutive aboriginal animals of South America, Australia and New Zealand, which are the nearest allies of the gigantic extinct species respectively characteristic of such tracts of dry land, are specifically distinct, and usually by characters so well marked as to require a subgeneric division, and such as no known or conceivable outward influences could have progressively transmuted. Moreover, as in England, for example, our Moles, Water-voles, Weasels, Foxes and Badgers, are of the same species as those that co-existed with the Mammoth, Tichorrine Rhinoceros, Cave Hyæna, Bear, &c.; so likewise the remains of small Sloths and Armadillos are found associated with the Megatherium and Glyptodon in South America; the fossil remains of ordinary Kangaroos and Wombats occur together with those of gigantic herbivorous marsupials; and there is similar evidence that the Apteryx existed with the Dinornis: and the author offered the following suggestions as more applicable to or explanatory of the phenomena than the theory of transmutation and degradation. He observed, that in proportion to the bulk of an animal is the difficulty of the contest which, as a living being, it has to maintain against the surrounding influences which are ever tending to dissolve the vital bond and subjugate the organised matter to the ordinary chemical and physical forces. Any changes, therefore, in the external circumstances in which a species may have been created to exist, will militate against that existence in probably a geometrical ratio to the bulk of such species. If a dry season be gradually prolonged, the large mammal will suffer from the drought sooner than the small one; if such alteration of climate affect the quantity of vegetable food, the bulky Herbivore will first feel the effects of the stinted nourishment; if new enemies are introduced, the large and conspicuous quadruped or bird will fall a prey, whilst the smaller species might conceal themselves and escape. Smaller quadrupeds are usually, also, more prolific than larger ones. The actual presence therefore of small species of animals in countries where the larger species of the same natural families formerly existed, is not to be ascribed to any gradual diminution of the size of such larger animals, but is the result of circumstances which may be illustrated by the fable of the 'oak and the reed'; the small animals have bent and accommodated themselves to changes under which the larger species have succumbed.

XXXVI. *Intelligence and Miscellaneous Articles.*

ON THE ARTIFICIAL PRODUCTION OF CRYSTALLIZED MINERALS. BY M. EBELMEN*.

THE author has continued his experiments upon the artificial production of minerals. In his recent experiments, instead of the porcelain furnace he made use of one of Bapterosse's furnaces, the

* An abstract of the author's former experiments was given in the April Number for 1848.

temperature of which is somewhat lower than that of the porcelain furnace. When large quantities of alumina, magnesia and silica, were exposed for several days uninterruptedly to the constant temperature of this furnace, he obtained spinelle in octohedra of such size, that they could be readily distinguished with the naked eye, and their angles measured. They all consisted of perfectly transparent octohedra, the twelve edges of which were truncated. The facets of some of the octohedra were between three and four millimetres in breadth.

M. Ebelmen has also produced zinc-spinelle or Gahnite. This mineral, as found in nature, is always coloured brown or green by peroxide of iron. M. Ebelmen obtained artificial Gahnite in a perfectly transparent and colourless state. When oxide of chrome was added to it, beautiful ruby-red octohedra with rhombic dodecahedral facets of from two to three millimetres were obtained.

The specific gravity of pure artificial Gahnite is 4·58, that of the native mineral 4·23 to 4·70. The hardness of the former is the same as that of the latter; both scratch quartz readily. On comparing the density and the atomic weight of the aluminates of zinc and magnesia, their atomic volume is found to be exactly the same; thus, that of the magnesian spinelle is 25·2, and that of pure Gahnite 25·1.

M. Ebelmen also obtained chromites of zinc and magnesia, *i. e.* compounds of sesquioxide of chromium $Cr^2 O^3$ with bases RO, which belong to the spinelle series. These, with the protochromite of iron, which has been already described by the author in his first memoir, show clearly that the native chromate of iron belongs to the same family.

The author also procured the ferrite of zinc, $Fe^2 O^3, ZnO$; it crystallizes in black strongly sparkling octohedra, which yield a black powder. They were not attacked by dilute acids, but dissolved in concentrated hydrochloric acid. Their density is 5·132. The author concludes from the existence of this compound, that Franklinite is identical with it.

The two following compounds are new, and of very great interest: 1, *magneso-borate of chrome*; and 2, *per-magneso-borate of iron*, which the author regards as compounds of oxide of chrome and peroxide of iron, with tribasic borate of magnesia $BO^3, 3MgO$. This borate $BO^3 + 3MgO$ is produced by the long-continued action of a very high temperature upon the borate of magnesia with excess of acid, and forms to a certain extent the mother-ley, out of which the two above-described compounds crystallized.

By the aid of boracic acid as a solvent, Ebelmen has also obtained some silicates which were infusible *per se* at the furnace heat. In this way he procured the silicate of magnesia MgO, SiO in perfectly formed crystals, the angles of which could be measured, and the measurements of which showed that they were identical with the transparent Peridote of mineralogists. The bisilicate MgO, 2(SiO) was obtained in long, beautifully white, pearly prisms, which exhibited the cleavage of pyroxene. The corresponding zinc compounds were also obtained in crystals.

With borax, M. Ebelmen formerly obtained microscopic crystals of alumina; the addition of a substance which gives a somewhat more difficult fusibility to the flux, as carbonate of baryta or silica, caused the production of beautiful crystals of alumina of the most splendid lustre. These crystals had the form of a six-sided double pyramid, appearing very considerably truncated on both summits, so that they resembled the strongly flattened plates of iron-glance of the volcanoes. The measurement of the angle between the lateral facets and the base showed that it was identical with those of corundum. This artificial corundum scratched quartz and topaz easily.

By using the phosphates as solvent, tantalic, niobic, and titanic acids were obtained in crystals. Titanic acid crystallizes from the phosphate in long needles, the specific gravity of which is $=4\cdot283$, hence identical with rutile.

All the artificial crystals which the author obtained have been optically examined by him, and found, with the exception of those belonging to the regular system, to exhibit the action upon polarized light.—*Comptes Rendus*, vol. xxxii. p. 330–333.

FURTHER RESEARCHES UPON CRYSTALLIZATION BY THE DRY METHOD. BY M. EBELMEN.

In my former investigations, I employed as solvents for the elements various fluxes which are volatile at high temperatures, such as boracic acid, borax, and acid and alkaline phosphates. It struck me that some new series of compounds might be obtained by using alkaline instead of acid fluxes, such as the carbonates of potash and soda, which are so frequently employed in mineral analyses for dissolving by the dry method those substances which are not acted upon by acids. These substances, like boracic acid, possess the double property of assuming the liquid state at temperatures easily produced in our furnaces, of dissolving a large number of metallic oxides, and of becoming completely volatilized in open vessels at a temperature slightly above that at which they undergo fusion.

When a mixture of silica and magnesia, in such proportions as to constitute the bisilicate, is exposed to a high temperature with bicarbonate of potash, after the lapse of some days we obtain a perfectly liquid vitreous mass, at the bottom of which very transparent colourless crystals are formed. These are easily separated from the fused mass, by treating it with very weak acids and solution of potash, which dissolves the glass without acting upon the crystals. They are easily recognized as peridote. I have succeeded in measuring their angles. The facet g' is well-marked; the other facets which I have detected are those of $e^{\frac{1}{2}}$, e', g^3, h', and a'; the measured angles differ scarcely a few minutes from those which have been obtained with the natural crystals.

It is evident that in this reaction half the silica separates to form an infusible combination with the magnesia; the vitreous matter acted upon by acids also contains magnesia; a great part of the potash was volatilized. Titanate of lime, TiO CaO, may be obtained in the same manner crystallized in cubes, the edges of which are slightly truncated; the compound is slowly separated, by the action of weak acids, from the vitreous matter in which it is formed; its density is $=4\cdot10$; it is identical with the mineral to which M. G. Rose has given the name of *Perowskite*, which was first found in the Ural, and more recently in the volcanic districts of the Kaiserstuhl.

The silicate of glucina, when fused with excess of alkaline carbonate, yields a semi-vitreous mass, from which microscopic crystals may be separated by the action of acids; the crystals consist of perfectly pure glucina; their specific gravity is 3·02; they are not acted upon by acids, excepting hot and concentrated sulphuric acid.

I have also obtained various accessory products in these experiments, as platinum crystallized in very brilliant octohedra and cubo-octohedra. The results which have just been described, differ very clearly from the ordinary phænomena of vitrification. This, it is well known, occurs in a mass of glass which is slightly softened, but not fused, and the ill-defined crystals which have been obtained, differ but little in chemical composition from the vitreous mass which surrounds them. In the present case, on the other hand, the crystals are formed in the midst of a perfectly liquid mass of glass, and their properties and chemical composition are completely different from those of the vitreous portion.

In conclusion, I must point out another method of crystallization, which, like the method by evaporation, has its analogue in the operations of the wet method. We might expect that metallic oxides, either simply or combined with each other, would separate in the crystalline state from the fused masses, when these are acted upon by more powerful bases than those primarily dissolved. In this manner, by causing fragments of lime to act upon borate of magnesia, we obtain *magnesia* in diaphanous crystals, the form of some of which is readily determinable by a lens, and which may be isolated from the mass in which they are disseminated by the action of weak acids, which do not attack them. These crystals appear identical in form and composition with the native magnesia discovered in the blocks of the *Somma*, and to which M. Sacchi has given the name of *Periclase*.

I shall soon lay before the Academy a more detailed memoir upon this subject, and show the consequences deducible from these experiments in explaining the formation of a large number of minerals belonging to the alkaliferous rocks.—*Comptes Rendus*, May 12, 1851.

NEW STATIC AND DYNAMIC THEORY OF ULTIMATE PARTICLES. BY M. ZANTEDESCHI.

As the ancient hypotheses of the constitution of matter and bodies, and physical and chemical phænomena, do not completely respond to the requirements of science in its present state, it appears to me, that by viewing the formation of bodies and the production of phænomena in the following light, a clear and evident explanation of them will result.

A body is composed of contiguous but not continuous parts, and these, instead of being rigid and hard, are eminently compressible and elastic. These contiguous parts form molecular groups and systems, which may be arranged differently in regard to each other, in virtue of the attractive force acting from molecule to molecule, from system to system. In accordance with this view, liquids would be formed by strongly compressed and but slightly adherent molecular groups; solids, by less strongly compressed but not adherent groups; and lastly, aëriform fluids would be constituted by still less compressed molecular groups, and these much less adherent. If the arrangement of the systems be destroyed, the equilibrium between the attractive and the elastic force disappears; the internal molecular movement increases, the vibrations augment, the elastic force gains the ascendant, and the matter expands, becomes dissipated and attenuated on assuming the elastic state, which state precedes every chemical phænomenon.

I have endeavoured on these principles to explain all the phænomena of physics and chemistry, in the same manner as the general laws of mechanics.

In accordance with this hypothesis, the phænomena of capillarity are merely a necessary consequence of the expansion of matter at its edges, of the adhesion of the expanded layer to the adjacent wall, and of the force of cohesion exerted between the upper parts of the liquid prism and its base. The limits of this phænomenon are determined by the equilibrium of the force of adhesion and cohesion with the excess of pressure of the internal upon the external level. The expansive force may be greater or less, according to the nature of the liquid; it may give rise to the formation of new prisms, which cease to remain suspended when the pressure of the external layer becomes less than the pressure of the liquid outside, which pressure does not correspond to the weight of the entire column of liquid raised, considering that a portion of it is supported by the adherence of the liquid to the adjacent solid walls.

All electrical, magnetic, thermotic and luminous phænomena, are finally nothing more than currents, projections of more finely divided, rarer and more elastic matter, which by their encounters give rise to new solutions or new combinations, which we call *physical, chemical* and *organoleptic* properties. Beyond the animal sphere, we find merely the motion of matter which becomes disaggregated or recomposed. Those bodies, which have hitherto been called *imponderable, dynamides* or *material forces*, are nothing more than the matter itself in the elastic state, which striking against the masses, or penetrating between the different molecular systems, breaks up or alters the primitive arrangement, augments their internal and vibratory motion, and gives rise to new systems and other arrangements.

In nature there is always motion, which is at the same time both the cause and the effect of other motions, by causing the relations between the elastic force and the attraction of the molecules to change at every moment. When nature is contemplated on these principles, it appears simple in its manifestations, grand and sublime in its results, always consistent with itself.

We next have, in the memoir itself, the application of these theoretical views to the explanation ;—1, of combustion ; 2, of the expansions and the contractions of bodies, and the changes in their condition; 3, of the capacity of bodies for heat, specific heat and latent heat ; 4, of the spheroidal state of liquids ; 5, of irradiation ; and lastly, 6, of electro-magnetic currents. The more I study, the more I meditate profoundly upon the phænomena of nature, the more I feel convinced that nothing is so simple and fertile as this dynamic system.—*Comptes Rendus*, May 19, 1851.

METEOROLOGICAL OBSERVATIONS FOR JULY 1851*.

Chiswick.—July 1. Hazy and mild : rain : cloudy and fine : thunder and lightning, with very heavy rain. 2. Fine : very fine : clear. 3. Uniformly overcast : cloudy and fine : densely clouded. 4. Overcast : very fine : clear. 5—7. Very fine. 8. Cloudy : rain. 9. Cloudy and fine. 10. Rain. 11, 12. Very fine. 13. Cloudy and fine : overcast : rain. 14. Cloudy : windy. 15. Fine : windy : slight rain. 16, 17. Very fine. 18. Cloudy. 19. Fine : rain : constant heavy rain in the evening. 20. Cloudy and fine. 21. Very fine. 22. Dry haze : very fine. 23. Rain. 24. Heavy rain. 25, 26. Very fine. 27. Cloudy and fine. 28. Cloudy : rain. 29. Very fine. 30. Foggy : very fine. 31. Hazy : overcast.

Mean temperature of the month	60°·71
Mean temperature of July 1850	61 ·91
Mean temperature of July for the last twenty-five years .	63 ·13
Average amount of rain in July	2·30 inches.

Boston.—July 1, 2. Fine. 3. Cloudy. 4, 5. Fine. 6, 7. Cloudy. 8. Cloudy : rain A.M. and P.M. 9. Rain : rain A.M. 10—12. Cloudy. 13. Fine : rain P.M. 14. Cloudy : rain A.M. and P.M. 15, 16. Cloudy. 17. Cloudy : rain with thunder A.M. 18. Fine. 19. Fine : rain P.M. 20. Cloudy : rain A.M. 21. Fine : rain P.M. 22. Fine. 23. Cloudy : rain A.M. and P.M. 24. Rain : rain A.M. and P.M. 25. Cloudy : rain A.M. and P.M. 26. Cloudy : rain P.M. 27. Fine. 28. Rain : rain early A.M. 29. Cloudy : rain P.M. 30. Cloudy. 31. Cloudy : rain P.M.

Sandwick Manse, Orkney.—July 1. Fog. 2. Cloudy : clear. 3. Clear. 4. Cloudy : drizzle. 5. Damp : clear. 6. Damp : drizzle. 7. Drizzle : rain. 8. Bright : clear. 9. Bright : clear : fine. 10. Drops. 11. Showers : fog. 12. Rain. 13. Cloudy : rain. 14. Damp. 15. Drizzle : rain. 16. Cloudy. 17. Damp : drizzle. 18. Bright : fine. 19. Fine. 20. Bright : rain. 21. Drizzle : rain : cloudy. 22. Bright : clear : fine. 23. Fine : clear : fine. 24. Cloudy : fine. 25. Cloudy : drizzle. 26. Cloudy : rain. 27. Drizzle : fine. 28. Rain : cloudy. 29, 30. Cloudy. 31. Rain : drizzle.

---

* The observations from the Rev. W. Dunbar of Applegarth Manse have not reached us.

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.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1851. July.	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	Max.	Min.	9½ a.m.	8½ p.m.	1 p.m.							
1.	30·005	29·899	29·48			30·08	30·10	78	59	69			57	53	e.	e.		n.	1·18	......		......
2.	29·988	29·851	29·37			30·26	30·33	82	54	67			53	53	ne.	ne.		nne.	......	......		......
3.	30·021	30·013	29·56			30·30	30·19	66	52	58			52	53	ne.	n.		wnw.	......	......		......
4.	30·027	30·011	29·57			30·03	29·94	67	37	58·5			53	51	ne.	n.		wnw.	......	......		......
☽ 5.	30·021	29·975	29·54			29·94	29·97	75	45	55			53½	49	n.	sw.		calm	......	......		·04
6.	30·058	30·050	29·54			29·93	29·86	74	50	63			53	54	nw.	sw.		w.	......	......		·05
7.	30·051	29·903	29·53			29·83	29·73	75	55	65			54	53	nw.	w.		nw.	......	......		·04
8.	29·798	29·714	29·20			29·79	29·85	71	50	63·5			52	45	nw.	w.		nnw.	·08	·14		·28
9.	29·811	29·696	29·30			29·73	29·70	67	43	58			51½	46	sw.	wsw.		n.	·16	·34		......
10.	29·864	29·677	29·20			29·74	29·84	69	38	60			46	48	nw.	wnw.		wnw.	·24	·05		·09
11.	30·140	30·089	29·63			29·84	29·73	70	56	57·5			51	53	w.	w.		w.	......	......		·08
12.	30·036	29·919	29·50			29·66	29·45	75	48	65			56½	52	w.	wnw.		wnw.	......	......		·19
○ 13.	29·866	29·571	29·27			29·45	28·94	73	51	66			56	56	sw.	sw.		sse.	·20	......		·30
14.	29·577	29·452	28·85			28·84	29·00	71	50	63			60	51½	w.	w.		wnw.	......	·17		·40
15.	29·730	29·727	29·18			29·28	29·47	70	41	59			49	47	w.	w.		nw.	·02	·01		·05
16.	29·884	29·763	29·30			29·62	29·74	67	46	56			48	47	nw.	wnw.		nw.	......	......		·18
17.	29·851	29·826	29·48			29·76	29·77	74	41	63			50	49	nw.	calm		w.	......	......		......
18.	29·950	29·893	29·44			29·79	29·83	72	39	57			53	52	nw.	nw.		calm	......	·41		·03
19.	29·970	29·806	29·48			29·84	29·82	71	52	61			61	53	sw.	w.		calm	·31	......		......
20.	29·775	29·708	29·16			29·73	29·68	72	50	67			58	51	sw.	sw.		nne.	......	·10		......
☾ 21.	29·979	29·960	29·44			29·65	29·87	72	42	58			51	49½	w.	w.		nw.	......	·63		·19
22.	30·036	29·806	29·57			29·96	29·96	76	58	59			53	51	e.	calm		calm	·08	......		·30
23.	29·737	29·550	29·32			29·86	29·80	71	57	64			56½	55	e.	ese.		calm	·70	......		......
24.	29·553	29·473	29·03			29·74	29·73	60	53	54			58	51	sw.	n.		calm	·28	2·65		......
25.	29·888	29·469	29·05			29·66	29·61	71	51	59			52½	51	ne.	nw.		n.	......	·40		......
26.	29·749	29·656	29·20			29·56	29·60	69	44	57·5			54	49	w.	nw.		nw.	·01	·30		·18
27.	29·914	29·898	29·40			29·70	29·75	72	57	58			54	55	sw.	w.		e.	·02	·02		·18
● 28.	29·895	29·803	29·33			29·60	29·62	69	57	62			53	52	sw.	s.		nnw.	·06	·26		·27
29.	29·790	29·768	29·30			29·75	29·97	75	49	67			56	50½	sw.	w.		n.	·03	·18		·06
30.	29·973	29·837	29·43			30·03	29·93	75	55	64			57½	52	nw.	n.		se.	·52	·07		......
31.	30·000	29·886	29·50			29·58	29·58	73	62	63			54	55	sw.	calm		w.	·01	......		·41
Mean.	29·901	29·795	29·36			29·759	29·753	71·68	49·74	61·2			53·77	51·20					3·90	5·73		3·32

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

OCTOBER 1851.

XXXVII. *On a Class of Ammoniacal Compounds of Cobalt.* *By* FREDERIC CLAUDET*.

WHEN ammonia is added in excess to a solution of protochloride of cobalt mixed with four times its weight of chloride of ammonium, the solution becomes of a dark brown colour without any appearance of a precipitate. In this state the solution rapidly absorbs oxygen from the air; and on frequently agitating a bottle half-filled with this solution, removing the stopper from time to time to renew the air, the absorption is much facilitated, and is complete in the space of three or four days, the colour of the liquid changing at the same time from dark brown to an intense violet-red. If the air be replaced in this experiment by pure oxygen gas, the oxidation is still more rapid, and may be completed (if the quantity of solution be not too large) without requiring the removal of the stopper. By boiling this oxidized ammoniacal solution, strongly acidified with hydrochloric acid, a heavy crimson powder is deposited. A slight effervescence takes place at the same time, due to the evolution of a certain quantity of oxygen, and the liquid becomes nearly colourless owing to the precipitation of the whole of the cobalt in the form of the new compound. The liquid when cold is drawn off from the red powder, which is washed several times by decantation with distilled water, thrown on a filter and allowed to dry in a warm chamber. The precipitated powder thus obtained is nearly pure. Before examination it is, however, necessary that it should be crystallized. The powder for this purpose is dissolved in boiling water to which a few drops of hydrochloric acid have been added; and on cooling, the salt is deposited in

* Communicated by the Author.

the form of regular octohedrons, small, sparkling, and of a ruby-red colour, very much resembling small crystals of chrome-alum.

This salt, which is an intense colouring matter, is sparingly soluble in cold water, 1 part requiring at 60° F. 244 parts of water; it is soluble to a much larger extent in water at the boiling-point, to which it imparts a very deep red colour; it is however slightly decomposed, and altogether so on boiling the solution; but this may be prevented by keeping the solution slightly acid with hydrochloric acid.

Hydrochloric acid, saturated solutions of chloride of ammonium and sodium, completely precipitate the new salt from its solution; alcohol acts in the same way. The salt is not decomposed by boiling hydrochloric acid. Sulphuric acid evolves hydrochloric acid, a corresponding sulphuric salt being formed; the reaction, however, is not complete, for at the end of the operation chlorine comes off from some decomposition. Nitric acid partially transforms the salt into the nitrate of the base. Potash and soda decompose the solution of the salt, a hydrated peroxide of cobalt being thrown down and ammonia evolved in considerable quantity. Hydrate of baryta decomposes the salt in the same way with the aid of heat, but not in the cold. Carbonate of potash or soda has no effect. Yellow prussiate of potash gives with a solution of the salt a dirty brown precipitate, red prussiate none; but on standing, bright yellow needles crystallize from the solution.

Sulphuretted hydrogen precipitates the whole of the cobalt as a bisulphide of that metal, ammonia being liberated at the same time. The analysis of three different preparations of this sulphide gave—

	Calculated.		Found. I.	II.	III.
Cobalt . .	29·5	47·96	48·9	49·5	48·2
Sulphur . .	32	52·04	51·1	50·5	51·8
	61·5	100·00			

On boiling a solution of the new salt, it is decomposed into ammonia, which escapes, and a superior hydrated oxide of cobalt, containing a certain amount of a nitride of cobalt which is precipitated, nothing but chloride of ammonium remaining in solution. The composition of the precipitated oxide of cobalt appears to be $Co^3 O^4 + 3HO$.

Dried in the air, the salt contains no water of crystallization, neither does it contain oxygen. When heated to low redness in a glass tube, a large quantity of ammonia is disengaged, a certain quantity of chloride of ammonium sublimed, and a residue of common protochloride of cobalt remains. In this reaction no moisture is produced, which would necessarily be formed if any oxygen existed in the compound.

The analysis of this salt was effected in the following manner. The chlorine was estimated from the chloride of silver, obtained on boiling the solution with an excess of nitrate of silver and nitric acid. In the cold the precipitation by nitrate of silver is not complete. The cobalt was determined by reducing a certain quantity of the substance introduced into a tube with a bulb, by pure hydrogen and heat. The nitrogen was estimated as ammonia, by distilling the salt with caustic soda, receiving the ammonia into hydrochloric acid, and determining the weight of the double chloride of platinum and ammonium. The ammonia was also obtained by heating the salt with soda-lime, according to the method of Will and Varrentrapp. This last process, however, gave less accurate results, a deficiency of about 1 per cent. in the nitrogen being found. The hydrogen was determined by combustion of the salt with a mixture of oxide of copper and chromate of lead, and copper turnings.

The following are some of the results obtained:—

				Per cent.
20 grains of	salt gave	34·14 Ag Cl	=8·445 Cl	=42·22
12	...	20·56 ...	=5·086	=42·38
10	...	17·08 ...	=4·225	=42·25
19	...	4·49 of cobalt		=23·63
8·68	...	2·04 ...		=23·50
9·48	...	2·25 ...		=23·73
12·51	...	2·96		=23·66
14·81	...	65·54 $PtCl^2+NH^4Cl$	=4·116 N	=27·79
8	...	34·64	=2·175	=27·20
13	...	7·38 HO	= ·82 H	= 6·31
13·5	...	7·70 ...	= ·855	= 6·34
11·655	...	6·775 ...	= ·753	= 6·46

The number of equivalents of chlorine, cobalt, nitrogen and hydrogen deduced from these results, are 3Cl, 2Co, 5N and 16H, as may be seen from the calculated numbers:—

	Calculated.		Found.		
			I.	II.	III.
3Cl=	106·5	42·34	42·22	42·38	42·25
2Co=	59·0	23·46	23·63	23·50	23·66
5N=	70·0	27·83	27·20	27·79	
16H=	16	6·36	6·31	6·34	6·46
	251·5	100·00			

The salt containing a large quantity of chlorine, it might be expected that the volatilization of minute quantities of chloride of copper or chloride of lead in the combustion would give an increase in the results for the hydrogen, one equivalent of the latter making a difference only of 0·37 per cent. The results obtained, however, agree pretty well together; and as they do not differ much from the calculated numbers, I am much inclined to believe sixteen the true number of equivalents of hydrogen in the salt; and I am further confirmed in this view by the manner in which the salt is decomposed by heat. A combustion-tube about two feet long was closed at one end and bent at right angles within about half an inch of the closed end, so as to form a kind of retort. A certain quantity of the salt was rubbed into a paste with a little water and rolled up into the size of a pea. When quite dry, this was dropt into the tube and made to enter the small retort; mercury was then gently poured into the tube, which was gradually filled and then inverted in a mercurial trough. The mercury descended about a quarter of an inch in the tube, on account of a small quantity of air which remained in that portion containing the salt. The retort part of the tube was now slowly heated by means of a spirit-lamp until the salt was entirely decomposed. The gas produced occupied nearly the whole of the tube, which was two feet in height. On allowing the tube to cool, and introducing a small quantity of hydrochloric acid, the whole of the gas was absorbed with the exception of a column of about three-quarters of an inch in height, showing that the space above the mercury was entirely composed of ammoniacal gas. Now the decomposition of this salt into no other gas than ammonia, and no other solid products than chloride of ammonium and protochloride of cobalt, is only compatible with a certain number of atoms of hydrogen, which is sixteen, for—

$$3Cl, 2Co, 5N, 16H = 2Co\,Cl + NH^4\,Cl + 4NH^3.$$

Had there been one or two equivalents less of hydrogen, one equivalent of ammonia would have been broken up, giving hydrogen and nitrogen not condensed by the hydrochloric acid.

Assuming, then, the above number of atoms to be correct, and applying Berzelius's theory of the copulated compounds, the formula of this salt may be written—

$$3(NH^4\,Cl) + 2(NH^2\,Co);$$

that is, a compound of 3 equivalents of chloride of ammonium with 2 equivalents of an ammonia, in which 1 atom of hydrogen is replaced by cobalt. In fact the salt has the characters of such conjugate compounds. It has the properties of chloride of ammonium with regard to form and taste; while on the other hand the basic property of the 2 equivalents of ammonia have totally disappeared, the salt being quite neutral to test-paper. This compound is analogous to the remarkable platinum compounds discovered by Gros and Reiset; but with this difference, that it is a sesqui-conjugated compound, if it may be so called, being composed of 3 equivalents of the salt united with 2 equivalents of the adjunct.

Another way of grouping the atoms of this compound is the following, proposed by Mr. Graham:—

$$Cl^3\begin{cases} NH^2\,Co^2 \\ NH^3,\,NH^4 \\ NH^3,\,NH^4 \end{cases}$$

Here $NH^2\,Co^2$ represents an ammonium in which 2 equivalents of hydrogen are replaced by 2 equivalents of cobalt; while NH^3 NH^4 represents an ammonium in which 1 equivalent of hydrogen is replaced by ammonium itself, as the hydrogen of ammonia is replaced by ethyle, methyle, &c. in Wurtz's and Hofmann's bases. Or

$$Cl\,N\begin{cases} H^2 \\ Co^2 \end{cases} + 2Cl\,N\begin{cases} H^3 \\ NH^4 \end{cases}$$

The compound would then be viewed as a double salt, composed of 1 equivalent of a chloride of cobalt-ammonium and 2 equivalents of a chloride of ammonium, in which the fourth atom of hydrogen is replaced by ammonium.

This peculiar compound has the property of forming double salts with bichloride of platinum and bichloride of mercury.

Double salt with bichloride of platinum.—On adding a warm solution of the salt to bichloride of platinum in excess, a silky crystalline buff-coloured precipitate falls down, much less soluble than the salt itself; it may therefore be well washed with water, thrown on a filter and dried.

12 grains of this double salt were fused with carbonate of soda, dissolved in hot water and filtered, to separate the platinum and oxide of cobalt. The solution neutralized with nitric acid and precipitated with nitrate of silver gave 20·11 grs. Ag Cl= 4·975 Cl=41·6 per cent. The filtrate of platinum and oxide of cobalt ignited was treated with boiling hydrochloric acid, which dissolved out the cobalt, and left 4·05 platinum =33·75 per cent.

18·59 grains of double salt reduced by hydrogen gave 8·06 mixed metals =43·35 per cent., giving 9·60 per cent. for the cobalt. The double salt is consequently composed of 1 equivalent of the new compound and 2 equivalents of bichloride of platinum.

	Calculated.		Found.
5Cl =	248·5	42·12	41·60
2Pt =	256·2	33·43	33·75
2Co =	59	10	9·60
5N =	70		
16H =	16		

the formula of which is—

$$Cl^3\begin{cases}NH^2\,Co^2\\ 2(NH^3\,NH^4)\end{cases}+2Pt\,Cl^2.$$

When this salt is decomposed by heat, treated with nitro-hydrochloric acid, and the excess of acid driven off by heat, the solution crystallizes in large orange-brown prismatic tables, no mother-liquor remaining. This salt proves to be a double chloride of platinum and cobalt, the 2 equivalents of bichloride of platinum combining with 2 equivalents of protochloride of cobalt from the new compound.

Double salt with bichloride of mercury.—Prepared in the same way as the preceding double salt, by adding a warm solution of the cobalt salt to an excess of bichloride of mercury, a bulky silky precipitate is formed composed of small red needles. This may be collected on a filter, slightly washed with cold water and recrystallized from a warm solution, this double salt being tolerably soluble in hot water.

15 grains fused with carbonate of soda in the same way as the double platinum salt, gave 18·10 grs. Ag Cl=4·477 Cl=29·84 per cent.

14·16 grs. reduced by hydrogen gave 0·80 cobalt =5·65 per cent.

	Calculated.		Found.
9Cl	=319·5	30·00	29·84
6Hg	=600		
2Co	= 59	5·54	5·65
5N	= 70		
16H	= 16		

This double salt contains, therefore, for 1 equivalent of the cobalt compound, 6 equivalents of bichloride of mercury.

$$Cl^3\begin{cases}NH^2\,Co^2\\ 2(NH^3\,NH^4)\end{cases}+6Hg\,Cl.$$

Recently prepared oxide of silver throws down the chlorine from the new ammoniacal compound, a highly alkaline red solution remaining, not having the slightest odour of ammonia. On standing a few hours it decomposes, ammonia is evolved, and hydrated peroxide of cobalt precipitated. The compound in solution represents before changing, the base of the present class of salts. It is an oxide, of which the composition is the same as that of the chloride already described, with the substitution of 3 equivalents of oxygen for 3 equivalents of chlorine:—

$$\text{Formula of new cobalt base} \ldots\ O^3\begin{cases}NH^2\,Co^2\\ NH^3\,NH^4.\\ NH^3\,NH^4\end{cases}$$

The study of this and other allied compounds of cobalt which exist will no doubt greatly extend our views respecting the compound ammonias.

The chlorine of the original chloride may also be eliminated by any silver salt, an analogous cobalt salt containing the acid of the silver salt being formed and remaining in solution. In this way I have been enabled to obtain a sulphate, nitrate, oxalate, acetate, and carbonate of the new base. From the carbonate I have prepared the bromide and iodide which have the octohedral form of the chloride, are equally sparingly soluble in water, and of a still darker ruby colour. The bromide was found to contain 61·15 per cent. bromine, the calculated amount being 61·8 per cent.

The insolubility of this ammoniacal compound of cobalt in boiling hydrochloric acid may be advantageously turned to account in the preparation of chemically pure cobalt, and also in the qualitative examination of substances containing small quantities of cobalt. The pulverized ore or its oxide to be purified is dissolved in nitro-hydrochloric acid, diluted with water, and filtered in order to separate any gangue or insoluble residue. Chloride of ammonium is now added in large excess, and the liquid saturated with ammonia; it is then poured into a glass bottle, and oxidated in the way I have already described in the preparation of the new salts. During the oxidation a certain quantity of the new compound is deposited, especially when the solutions are rather concentrated, on account of its insolubility in a strong solution of chloride of ammonium. The solution still retains a certain quantity of cobalt salt; it is therefore boiled with a considerable excess of hydrochloric acid, which causes the total precipitation of the new compound, dissolving at the same time any oxide of iron or other oxides thrown down by the ammonia. When cold the clear liquid is decanted off, and the deposit well washed with acidulated water and then dried. By heating this compound to low redness it is decomposed, leaving for residue protochloride of cobalt slightly decomposed, but absolutely free from any other metal. This may be reduced by hydrogen gas giving pure metallic cobalt.

By these means I have been able to prepare perfectly pure cobalt directly from the grey cobalt ore of Tunaberg, which is an arsenio-sulphuret of cobalt, and also to detect small quantities of cobalt in different samples of oxide of nickel.

The preceding results embody the most definite conclusions of an investigation of the ammoniacal salts of cobalt which I have had in hand for the last two or three years. M. Fremy has also lately announced that he is occupied with an extended inquiry into the same class of compounds, respecting which he has published some important general results*. Dr. A. Genth appears also to have formed several of the salts of the new base described in this paper, but his analytical results differ entirely from mine†. It is in such circumstances that the present contribution is offered towards the advancement of our knowledge respecting the salts of cobalt.

University College,
August 29, 1851.

XXXVIII. *A Summary of recent Nilotic Discovery.* *By* Charles T. Beke, *Ph.D., F.S.A. &c.*‡

At the Meeting of the British Association at Southampton, in September 1846, I had the honour of explaining to the Section of Geology and Physical Geography my views respecting the physical configuration of the Table-land of Abessinia§; and at the Meeting at Swansea, in August 1848, I enunciated before the same Section my hypothesis as to the sources of the Nile in the Mountains of the Moon‖. I may be allowed here briefly to recapitulate the main results of those two communications.

The table-land of Eastern Africa, instead of consisting, as was generally supposed, of a succession of terraces rising one above the other, the lowest being towards the Red Sea and the highest in Enárea, is an elevated region of irregular surface, having its line of greatest elevation towards the sea-coast, whence the general level gradually falls westward towards the valley of the Nile; the water-parting between the streams tributary to that great river and those flowing towards the Red Sea and the Indian Ocean, being along the extreme eastern limit of the table-land.

The eastern flank of this table-land is abrupt and precipitous, the greater portion of the ascent to the height of 8000 or 9000 feet (the average elevation of its eastern edge) being within the horizontal distance of a very few miles; so that persons approaching it from the coast can only regard it as a lofty range

* *Comptes Rendus*, April 7, 1851, and May 26, 1851.

† Chemical Gazette, 1851, p. 286. [The priority of discovery of this new class of salts belongs, not to M. Fremy, but to Dr. Genth, whose researches were published early in 1850; but unfortunately in a journal, the circulation of which appears to be confined to the German physicians of the United States.—W. F.]

‡ Communicated by the Author, having been read before the Section of Geography and Ethnology of the British Association for the Advancement of Science, at the Meeting at Ipswich, on the 4th of July 1851.

§ See Report of the British Association for 1846, Report of the Sections, pp. 70–72; and Journal of the Royal Geographical Society, vol. xvii. p. 76 *et seq.*

‖ See Report of the British Association for 1848, Report of the Sections, pp. 63, 64; and Edinb. New Phil. Journ., vol. xlv. p. 221 *et seq.*

of mountains running along the eastern side of Africa from north to south.

To the southward of about the 2nd parallel of south latitude, and between the 29th and 34th meridians of east longitude, is the country of Mono-Moézi or Uniamézi—names which may be respectively interpreted "the king of the moon" and "the possession of the moon;"—and in this country, which forms a portion of the table-land, various considerations induced me to place the sources of the Bahr-el-Abyad or White River, the direct stream of the Nile. And I expressed the opinion that the "Mountains of the Moon" of the geographer Ptolemy, in which he places the sources of the Nile, consist of the mountain range of Eastern Africa, which flanks the country of Mono-Moézi to the east, instead of being, as we see them usually marked in the maps, a range stretching across the continent from east to west.

The direct stream of the Nile, which I thus conceive to have its sources in the mountains of Mono-Moézi, was in 1840 and 1841 ascended beyond the 5th parallel of north latitude by the second of the expeditions sent by Mohammed Ali, Pasha of Egypt, to explore its course, and was found to be joined in about 9° 20′ N. lat. by two principal arms, viz. the Keïlak or Bahr-el-Ghazal, and the Sobat, Telfi, or River of Habesh. The former, which joins the main stream from the west, and of which the course is yet unexplored, is apparently the Nile of Herodotus and other writers anterior to Ptolemy. The latter, namely the Sobat, which falls into the Nile from the east, is the lower course of the Godjeb, the principal river of Kaffa, which in its upper course is joined by three other streams, bearing in common the name of Gibbe, and draining the extensive elevated districts in the south of Abessinia Proper now occupied by numerous and powerful Galla tribes. Further, the Bahr-el-Abyad or true Nile, and the Sobat or Godjeb, appear to be the two principal arms of the Nile described by Ptolemy as having their sources in the Mountains of the Moon, or the Alpine regions of Eastern Africa; while the Bahr-el-Azrek, Blue River or Abai, and the Atbara or Takkazie, which both rise in the more northerly extension of the same elevated regions, are respectively the Astapus and the Astaboras of the same geographer.

The foregoing is a brief summary of my views respecting the orography and hydrography of Eastern Africa, from the 18th degree of north latitude to probably the 3rd or 4th parallel south of the Equator, as submitted to the British Association down to the year 1848. I now propose to take a rapid survey of the principal additions since made to our knowledge on the subject.

At the date of my last communication, it was not known in Europe that the members of the Church Mission in Eastern Africa, stationed at Rabbai 'Mpia, near Mombas, in about 4° south latitude, had already begun exploring the interior of the continent. In the month of October 1847, Mr. Rebmann penetrated westwards to Teita, "a country whose mountains rise to such a height out of the vast surrounding plains, that on some eminences near Rabbai 'Mpia they are to be seen at a distance of 90 miles;" and in the April following (1848), the same missionary performed a journey further into the interior, to the still more elevated country of Djagga, where, at a distance of rather more than 200 geographical miles from the coast, in a direction about W.N.W. from Mombas, he made the remarkable discovery of a lofty mountain, named Kilimandjáro, of which the summit is covered with perpetual snow. The existence of snow on Kilimandjáro has been disputed in Europe, though it is difficult to say on what reasonable ground. However, on subsequent journeys, both Mr. Rebmann and his colleague Dr. Krapf have satisfied themselves of the fact; and unless it be intended absolutely to impugn their veracity, their evidence cannot be rejected.

In Djagga Mr. Rebmann obtained information respecting the country of Uniamézi—or Mono-Moézi, as it is designated by the early Portuguese, by whom it was first mentioned—situated considerably further inland; and towards the end of the year 1848 the same missionary returned to Djagga, for the purpose of ascertaining the practicability of reaching Uniamézi. Having been assured by the king of the former country of his readiness to assist him on the journey, he returned to the coast, whence, on the 5th of April 1849, he again set out on his way into the interior; his intention being to proceed, if possible, as far as the large lake in Uniamézi, respecting which also he had obtained information in Djagga, and which, from the name of Usámbiro attributed to it, appears to be the Lake Zambre of the early Portuguese maps; and when there he purposed making inquiries as to the road beyond the lake to the west coast of Africa. On this journey, however, Mr. Rebmann was unable to proceed beyond Djagga; the king of that country, Mamkinga, having "by tormenting beggary taken all his things from him, and leaving him at last no means whereby to travel any further."

Dr. Krapf had in the interval been occupied in exploring the districts to the south-west of Mombas, nearer the coast; but after the unsuccessful issue of Mr. Rebmann's last expedition, he himself determined on undertaking the difficult and perilous journey to Ukambáni, a country situated northwards of Djagga. This undertaking was successfully accomplished in the months of November and December 1849. The distance performed by Dr. Krapf, as measured on the map published in the Church Missionary Intelligencer for September 1850, is in a direct line 240 geographical miles north-west from Mombas. Towards the extreme point of his journey Dr. Krapf crossed the river Adi, supposed by him to be the upper course of the Sabaki, which falls into the Indian Ocean close to Melinda. The absolute height of the bed of the river where thus crossed is not given; but it must be considerable, inasmuch as the river thence runs upwards of 200 geographical miles through a mountainous country before reaching the ocean. From the valley of the Adi Dr. Krapf ascended about 1800 feet to "the plain of Yata," on reaching which he says, "We had a majestic view of the whole region around. We viewed the serpentine course of the Adi towards the west and north-west; we saw the hills and plains of the wild Wakuāfi; we noticed the mountains Noka, Djulu, Engolia, Théuka, in whose vicinity lay the road we had taken to Kikúmbŭliu. Eastward we saw the mountains of Mudumóni, which separate the Galla country from Ukambáni. To the north, Ukambáni Proper lay before our view. Had I been a mere traveller pursuing only geographical objects, I would, *standing on the plain of Yata,* have considered myself amply compensated for the troubles I had sustained on the road; for a great many geographical problems were solved in an instant on the height of Yata*." From this it is evident that Dr. Krapf must have attained an elevation of several thousand feet, even if he had not already reached the summit level of the table-land of Eastern Africa.

Of the geographical results of this journey, one of the most important is the discovery of another snowy mountain, named Kénia, of larger size, if not of greater elevation than Kilimandjáro. Kénia is thus described by Dr. Krapf:—"The sky being clear, I got a full sight of the snow mountain. . . . . It appeared to be like a gigantic wall, on whose summit I observed two immense towers, or horns as you may call them. These horns or towers, which are at a short distance from each other, give the mountain a grand and majestic appearance, which raised in my mind overwhelming feelings. Kilimandjáro in Djagga has a dome-like summit; but Kénia has the form of a gigantic roof, over which its two horns rise like two mighty pillars, which I have no doubt are seen by the inhabitants of the countries bordering on the northern latitudes of the Equator. Still less do I doubt that the volume of water which Kénia issues to the north runs towards the basin of the White Nile†."

Though this conclusion of the worthy missionary is only conjectural, it appears to be founded on substantial reasons; and it can scarcely be doubted, that, through the discoveries thus made, we may arrive at a close approximation to the southern limits of the basin of the Nile. In Mr. Rebmann's map, already adverted to, Kénia is placed in 1° S. lat. and 35° 10′ E.

* Church Missionary Intelligencer, vol. i. p. 417. † Ibid. p. 470.

long., at a distance of 320 geographical miles N. 55° W. from Mombas; while the northern limit of the great lake in Uniamézi is, in the same map, laid down conjecturally in about 1° 20′ S. lat. and 29° E. long., at a distance of 650 geographical miles N. 75° W. from Mombas; and beyond these two points we can scarcely look for the continuation of the river, unless indeed it should actually be found to flow out of the lake itself.

It is proper to remark here, that, according to Dr. Krapf's explicit declaration*, this lake in Uniamézi is not identical with Nyassi,—or Niassa, as Dr. Krapf spells the name,—the great lake, respecting which some years back Mr. Cooley made an elaborate communication to the Royal Geographical Society of London, which is printed in the fifteenth volume of the Society's Journal. It should be added, that, when in Ukambáni, Dr. Krapf heard of the existence of a volcano in actual activity, at some distance beyond Kénia to the north-west, but he did not go far enough to see it. According to my hypothesis as to the physical character of the "Mountains of the Moon," they may in the most general manner be likened to the Andes of South America; and these particular coincidences of snowy peaks and active volcanoes serve further to complete the resemblance †.

Turning now to the exploration of the upper stream of the Nile itself, we may proceed to see how far these conjectural opinions with respect to the position of its sources are borne out by facts.

At the period when my opinions on the subject were placed on record, the course of the river was known only as far as 4° 42′ 42″ N. lat., that being the extreme point reached in January 1841 by the second Egyptian expedition ‡. In this expedition M. d'Arnaud and M. Werne took part, and the particulars furnished by those two travellers, from native information, respecting the river above the point attained by them, differed materially; the former stating that it came from the east, while the latter asserted that it continued a month's journey further south.

It is only recently that the question has been decided by Dr. Ignatius Knoblecher, the Pope's Vicar-General in Central Africa, who in January 1850, accompanied by two missionaries, Don Angelo Vinco and Don Emanuel Pedemonte, having surmounted the rapids which had stopped MM. d'Arnaud and

* Church Missionary Intelligencer, vol. i. p. 128.

† See Athenæum of December 1st, 1849, No. 1153, p. 1209.

‡ It is quite a mistake to suppose that the first expedition penetrated up the river as far as 3° 30′ N. lat. The extreme point reached by it on the 27th of January 1840 was 6° 35′ N., which point was passed by the second expedition, as is expressly stated by M. Werne in his *Expedition zur Entdeckung der Quellen des Weissen Nil*, p. 9.

Werne, penetrated up the stream of the Bahr-el-Abyad as far as 4° 9′ N. lat. Here, on ascending a mountain called Logwek, he saw the Nile trending away in a south-westerly direction till it vanished between two mountains named Rego and Kidi; and he was informed there by the Bari negroes, the last natives he met with, that beyond those mountains the river comes *straight from the south.* From the summit of Logwek Dr. Knoblecher observed, in the extreme distance of the southern horizon, a lofty mountain-chain, the outlines of which were barely discernible through the haze of the atmosphere, and which, from its distance, must be considered as lying nearly in the third parallel of north latitude.

According to Dr. Knoblecher, the Nile as far as the fourth parallel of north latitude continues to be a considerable stream, of the average breadth of 200 mètres, with a depth of from 2 to 3 mètres; which proves, beyond all question, that the river must come from a considerable distance, and most probably from beyond the Equator, in order to allow of the collection of a volume of water sufficient to form so large a stream. Dr. Knoblecher was confirmed in the opinion that the source of the Nile is to the south of the Equator, "by the fact that the river was rising on the 16th of January, which he considered as a consequence of the rainy season having set in in districts much further south*."

The longitude of the river at the extreme point reached by M. d'Arnaud in 1841, is, according to him, 31° 38′ east of Greenwich. If, now, Dr. Knoblecher's "furthest" in 4° 9′ N. lat. be conjecturally placed in the same longitude of 31° 38′ E.—which cannot be very far from the truth,—we shall have a distance between that point and Kénia of 370 geographical miles, on a bearing of S. 33° E.; while from the same point to the northern extremity of the lake in Uniamézi the distance is 360 geographical miles, on a bearing of S. 25° W. Within these limits therefore we may reasonably look for the southern boundary of the basin of the Nile; and it is not at all unlikely that Kénia itself is the "high mountain, the top of which is quite white," of which Baron von Müller, a recent traveller in Sennár, heard from the report of a native of the country of Bari, who was said to have travelled a great way to the south, and to have there seen the origin of the Bahr-el-Abyad in "the White Mountain" in question†.

In the present state of our knowledge on the subject, it would, of course, be wrong to pretend to establish any absolute identification. It is most probable that in the Alpine region of which

* See Athenæum of February 22nd and March 29th, 1851, Nos. 1217, 1222, pp. 217, 353.

† See Journal of the Royal Geographical Society, vol. xx. p. 287.

Kilimandjáro and Kénia form parts, other snowy peaks of at least equal altitude will be discovered. And even if it should be ascertained that one of the head-streams of the Nile has its origin on the northern flank of Kénia (as Dr. Krapf conjectures), we may be satisfied that others of those head-streams take their rise in other mountains further to the west. At all events, having reached this Alpine region, we have every reason to conclude that we shall here find the southern limits of the basin of the Nile; and we shall consequently have arrived at the solution, in general accordance with the statements of Ptolemy as now elucidated and explained, of the greatest problem of geography—the discovery of the mysterious sources of the giant stream of the African continent, the largest river of the Old World, perhaps even of the entire globe. One important consideration must however be constantly borne in mind, namely, that it is not by arbitrarily fixing on this or that particular head-stream that the question will be finally set at rest. As I have already observed in a communication made to the Syro-Egyptian Society of London on the 9th of January 1849*, "our object must be in the first place to determine the entire limits of the basin of the river; we have next to ascertain what principal arms unite to form the main stream; we must then trace to their heads the several smaller branches which form those arms; and when we have succeeded in all these points, we shall then—but not before—be competent to decide which of these numerous ramifications has the fairest claim to be regarded as the true Source of the Nile."

London, May 5th, 1851.

Appendix to the foregoing Paper.

The rise of the Nile in 4° 9′ N. lat., observed by Dr. Knoblecher on January 16th, 1850, cannot have been caused by the setting in of the regular rainy season, either north or south of the Equator.

It is well known that on the Abessinian plateau, north of the ninth parallel of north latitude, the rains begin about the middle of June and last till the middle of September:—"cominciando il verno generale nell' Ethiopia alla metà di Giugno fino a mezzo Settembre," as was recorded by Alvarez† three centuries ago.

Within five degrees north of the Line the rains set in nearly three months earlier than in Abessinia. M. Werne, who was in the country of Bari, in about 4° 40′ N. lat., at the end of Janu-

* "On the Sources of the Nile, being an attempt to assign the limits of the Basin of that River," printed in the Philosophical Magazine for August 1849, vol. xxxv. p. 98 *et seq.*

† Viaggio, &c. cap. 159.

ary 1841, was informed there that "the rainy season would not commence for two months, that is to say, not till the end of March or beginning of April*."

Crossing the Line, we learn from the experience of Dr. Krapf and Mr. Rebmann, that in the mountainous regions west of Mombas, within four degrees south of the Equator, the rainy season sets in towards the end of March or the beginning of April†,—that is to say, at the same time precisely as it commences within the like distance north of the Line; and it continues till the end of June or the beginning of July‡. And seeing that the commencement of the rains is the same within five degrees north as it is within five degrees south of the Line, it may reasonably be inferred, in the absence of direct evidence on the subject, that their duration is likewise in both cases the same; whence it will result that throughout the equatorial regions the regular rainy season lasts, as in Abessinia, about three months, only it takes place there at a period nearly three months in advance of the time of its occurrence in the latter country.

But, in addition to the regular rains, there is generally within the tropics a second rainy season. In Abessinia the two are distinguished by the names of "the rain of covenant" and "the rain of bounty;" the former being fixed and constant in its commencement and duration, while the latter is more uncertain and irregular. The ordinary occurrence of "the rain of bounty" in the southern portion of that country is during the entire month of February, or thereabouts.

We have not any direct evidence as to the period of this second rainy season within five degrees north of the Equator. But within the same distance south, according to Dr. Krapf and Mr. Rebmann§, it commences towards the end of November,—"in the middle of the dry season ||,"—and continues through the month of December; and, by analogy, the same is most probably the case to the north of the Line likewise.

Taking all these circumstances into consideration, it would seem to result that the increase of the Nile, observed by Dr. Knoblecher on the 16th of January 1850 in 4° 9′ N. lat., could only have been caused by "the latter rain" in the equatorial regions of Eastern Africa, whether north or south of the Line. And if, as it is reasonable to suppose, the commencement, amount,

* *Expedition zur Entdeckung der Quellen der Weissen Nil*, pp. 326, 333.

† Church Missionary Intelligencer, vol. i. pp. 21, 107, 329, 377, &c.

‡ Ibid. pp. 329, 376, 379, &c.; Church Missionary Record, 1847, p. 3.

§ Church Missionary Intelligencer, vol. i. pp. 416, 417, 454, 469-471, 474, &c.

|| Ibid. pp. 151, 273.

and duration of this "rain of bounty" are, like as on the Abessinian plateau, irregular and uncertain, we may fairly infer that it has at times no sensible effect on the volume of water in the Nile. Hence we may understand how it happened that in the year 1841 the river, so far from rising in the middle of January, as it did in 1850, continued falling till the end of that month*.

In Lower Egypt, precisely at the period of the regular equatorial rains, namely, "during the months of April, May and June, the waters of the Nile are at their lowest level. Towards the end of June the river at Cairo begins to rise, without the occurrence there of any rainy season, and without the existence of the slightest apparent cause. The increase of the Nile usually continues three months, from the summer solstice to the autumnal equinox, when its waters again begin gradually to fall†."

I refrain from discussing here the effect of the flooding of the various head-streams of the Nile on the inundation of that river in Egypt; merely remarking that its occasional abnormal and momentary increase appears to be solely attributable to the fall of rain in the eastern mountains of Egypt and Nubia: for instance, the extraordinary rise of the river observed at Cairo in May 1843 was caused by the rain-waters collected and brought down by Wady Ollaky in about 23° N. lat.‡

August 19th, 1851.

XXXIX. *On the Heat of Chemical Combination.* *By* Thomas Woods, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Parsonstown, July 1851.

In the course of some investigations respecting the cause of *the heat of chemical combination,* I found that some facts hitherto unnoticed, or not sufficiently attended to, required to be proved. This I have endeavoured to do; and will, if you allow me, publish a few papers in the Philosophical Magazine illustrative of these facts, which I intend to employ as data in establishing a theory I have formed on the subject.

The first of these propositions is, that *the decomposition of a compound body gives rise to as much cold as the combination of its elements produces heat.*

* Werne, pp. 330, 334.

† Ehrenberg, in *Monatsberichte d. Akad. d. Wissensch. in Berlin* (May) 1851, p. 334.

‡ Journ. Roy. Geogr. Soc. vol. xx. p. 292; and see Burckhardt's Travels in Nubia, p. 10.

To me this is a new idea, and one with which I have not met in any work on chemical research. I believe it will be found important, and in the present paper I will confine myself to a proof of its truth.

(2.) That decomposition generally requires a certain amount of heat is admitted, for as a general rule heat causes decomposition; that is, to decompose a substance a source for the supply of heat is necessary, or some body capable of giving up heat to the compound to be decomposed must be brought into its vicinity before decomposition takes place.

(3.) That *decomposition* absorbs as much heat as combination produces, might be proved by the fact, that in double decompositions no heat is given off. We know, for instance, that carbonic acid and magnesia in combining produce a certain amount of heat, and that sulphuric acid and potash likewise give rise to a definite quantity; and yet when sulphate of magnesia and carbonate of potash are mixed in solution, although such combinations take place, no rise of temperature (except that of solidification) occurs. Does not the decomposition neutralize the effect of the combination?

(4.) In cases of simple decompositions the same result does not obtain. We know from the researches of Andrews and others, that certain bases produce on combination with acids certain amounts of heat, and that each base gives rise to a different quantity. Andrews has also proved, that if one base displace another a definite rise of temperature is the consequence. Now if the rise of temperature be examined, it will be found that it is the *difference of the amount produced by the combining and separating body.* For instance, an equivalent of potash by combining with an acid produces $6\frac{1}{2}$ units of heat; ammonia, $5\frac{1}{2}$ units. If the potash displace the ammonia from any of its combinations, one unit of heat is the result; the difference between $6\frac{1}{2}$ units of heat produced by the formation of one salt and $5\frac{1}{2}$ units of cold by the decomposition of the other.

(5.) It occurred to me, however, that the fact might be proved more satisfactorily by finding what amount of heat is given off by the combustion of zinc, then ascertaining what quantity is evolved by its oxidizement in water; for, as the water must be decomposed, we should have the amount of heat in the second instance less than that in the first, and less by the quantity of heat produced when hydrogen is burnt. And such I find to be precisely the case.

Grassi shows that 1 lb. of oxygen uniting with hydrogen raises the temperature of 43·4 lbs. of water 180°.

The amount of heat produced, according to Despretz, when zinc is burnt, is sufficient to raise the temperature of 53 lbs. of water 180°.

I find that 1 gr. of zinc, by dissolving in dilute sulphuric acid, raises the temperature of 60 grs. of water 18° F., making the necessary allowance for specific heats, containing vessel, &c.; but 1 gr. of zinc is equivalent to $1\frac{1}{4}$ gr. of oxide of zinc, and this would, by its combination with the acid, raise the temperature of 60 grs. of water $10\frac{1}{2}$° (Graham, Andrews). This amount must therefore be taken from the 18° in order to find what is due to the *oxidizement* of the zinc alone. One grain of zinc, therefore, by being oxidized, or $\frac{1}{4}$ gr. of oxygen uniting with zinc with decomposition of water, raises the temperature of 60 grs. of water $7\frac{1}{2}$° F. This is the same as 1 lb. of oxygen uniting with zinc, *with* decomposition of water raising the temperature of 10 lbs. of water 180°, whereas *without* the decomposition it raises the temperature of 53 lbs. of water 180°; therefore the decomposition absorbs as much heat as would raise 43 lbs. of water 180°—the same amount which we saw the combination of oxygen and hydrogen was capable of producing.

(6.) If zinc be dissolved in *muriatic acid,* I find that 1 gr. causes a rise of temperature equal to 21° F. in 60 grs. of water; or 1 lb. of chlorine uniting with zinc, with decomposition of chloride of hydrogen, raises the temperature of 6 lbs. of water 180°. Abria (*L'Institut,* vol. xiv. p. 635) found that 1 lb. of chlorine uniting directly with zinc would raise the temperature of 36 lbs. of water 180°; that is, $\frac{8}{10}$ths of what oxygen produces. If chlorine uniting with zinc produces likewise $\frac{8}{10}$ths of the heat oxygen does under similar circumstances, 42 lbs. of water would be raised 180° by 1 lb. uniting with the metal. In other words, zinc uniting with chlorine *with* decomposition of chloride of hydrogen produces 6 units of heat, *without* decomposition 42 units. The difference, or 36 units, is what the combination of chlorine and hydrogen produce, and consequently what is lost by the decomposition.

(7.) When 1 lb. of zinc is dissolved in *nitric acid,* it raises the temperature of 43 lbs. of water 180° F., more than twice as much heat being produced than when it is dissolved in sulphuric acid. This combination is accompanied by decomposition of nitric acid.

The last two instances are brought forward for the present only to prove, that it is not because zinc is oxidized under different circumstances, as in air and water, that different amounts of heat are produced, but that an absorption of heat always accompanies decomposition, and varies with the substances decomposed. The first instance, the solution of zinc in sulphuric acid, proves that this absorption is equal to the quantity liberated when the same elements combine.

(8.) When I had satisfied myself so far, I thought that if a compound body could be decomposed directly, or without combination taking place at the same time, a positive loss of temperature ought to result; and it occurred to me that the decomposition of water by the galvanic battery might be an *experimentum crucis.* I forgot, however, the heat that is produced by the resistance offered to the current in passing through the water. Still, on consideration, I saw that the experiment would prove whether decomposition occasioned loss of heat or not; for although heat is given out by the resistance, might not the amount of that heat be greater if no decomposition accompanied it? If the idea I adopted were correct, not only should such be the case, but the difference should be the amount of heat the liberated gases would give if again chemically combined. And now the problem was to find, whether the heat produced by a galvanic current passing through water was what was due to the resistance offered, or whether decomposition made it less, and how much less?

(9.) The manner in which I endeavoured to solve this question was as follows:—

Having set a battery, consisting of twelve Daniel's cells, copper cylinders 5 inches high, 3 inches in diameter, with brown paper diaphragms and amalgamated zinc, into action as one series, I noted to what distance the needle of a tangent galvanometer moved when the current passed through the copper ring, which was $12\frac{1}{2}$ inches in diameter. I then included in the circuit a volta-electrometer holding acidulated water, and again noted to what division of the scale the needle moved, the current having passed through the fluid in the electrometer with the decomposition of the water. The difference of the tangents of the angles, when the electrometer was included and when it was not, showed the resistance the water offered to the current. I now removed the electrometer, and in its place introduced a fine platina wire of such a length as offered the same resistance to the current that the water did, which I knew by the needle of the tangent instrument being equally deflected by both. I had therefore two substances offering the same amount of resistance, and consequently developing the same amount of heat; but in one case decomposition was present, in the other it was not. What, then, was the result? Exactly what I anticipated—*the heat liberated by the passage of the current through the water was less than that produced by its passage through the wire, and to the same amount that the gases given off would develope if again chemically combined.*

(10.) I tested the truth of this result by many experiments varied in different ways, both as to the quantity and intensity of the current, the amount of resistance and the duration of the experiment, and in every case met with a similar result. The details, therefore, of one or two experiments will suffice.

I filled a glass tube with an ounce of acidulated water (one part by measure of sulphuric acid spec. grav. 1·84 to six parts of water); I put two platina leaves into the fluid, and sending a galvanic current through it by means of the twelve-cylinder battery, I decomposed the water. $1\frac{1}{2}$ cubic inch of gas was given off in the minute; and the tangent instrument having been included in the circuit, the needle rose to 21 degrees on the scale. The current passed through the water for two minutes, and the temperature rose from 75° to 83° F. I now removed the platina leaves from the fluid, and substituted a platina wire of such a length as kept the needle, the current having passed through the wire, at 21 degrees. The same fluid into which the electrometer leaves had been immersed now surrounded the wire; the current in the former case passing through the water and consequently *with* decomposition, in the latter through the wire *without* it; and after traversing the wire for the same length of time, the temperature of the fluid was raised from 75° to 90°, that is 15 degrees, or 7° more than in the case where decomposition accompanied it; although in both cases the same resistance was offered to the current, and of course the same amount of heat generated; therefore the 7° were absorbed by the decomposition. In the two minutes that the experiment lasted, 3 cubic inches of gas were given off: now if these gases were made to combine chemically, what amount of heat would they produce? The same that their separation caused a loss of, viz. about 7° F. to an ounce of water acidulated as above; for 3 cubic inches of the mixed gases in proper proportion to form water weigh ·387 gr.; and as oxygen is $\frac{8}{9}$ths of the mixture, it must weigh ·344 gr. Now as 1 lb. of oxygen combining with hydrogen would cause the temperature of 43 lbs. of water to rise 180°, 1 grain would raise the temperature of 43 grs. 180° F.; then ·344 gr. would raise the temperature of 43 grs. 62° F., or of 1 oz. very nearly 5°·6 F. But the specific heat of the acidulated water to that of distilled water is as 8·3 to 10; therefore the 5°·6 must be raised inversely in that proportion, or to 6°·7. In our experiment very nearly the same amount was lost by their separation.

(11.) I tried the experiment in another way. Through the bottom of a glass tube I passed two short pieces of thick platina wire, and attached to each piece a fine wire which reached to the top of the vessel, as at AB. I could join the wires at the bottom so as to make the two thin wires into one, or separate them by disuniting the thick pieces outside the bottom of the tube. In the former case the current passed through the wire, in the latter through the water. Each offered the same degree of opposition to the passage of the current. Here the same wires that acted as the leaves of the electrometer conducted the galvanic current; the same fluid was used and the same vessel; in fact everything was the same, except that in one case decomposition accompanied the resistance, in the other it did not; and in the former the temperature did not rise to the same degree as it did in the latter, and it was as much less as the combination of the gases given off would have produced if combined.

I also placed two glass tubes, each containing the same quantity of acidulated water, in one of which were the leaves of the electrometer, in the other a platina wire offering the same resistance as the fluid, in the circuit; so that the galvanic current passed through both at the same time, and with the same result as in the other experiments.

(12.) I might bring forward many such experiments; but although they vary in details they are all the same in principle, and prove the same fact. Enough I think has been said to establish the truth of my proposition. If admitted, some interesting difficulties may be removed by its application; for instance, it explains why some compounds, such as alcohol, turpentine, &c., do not give out as much heat when burnt as their elements do when separately ignited. It may also be made the means of determining the amount of heat produced by the combination of bodies, as the loss occasioned by their decomposition shows the gain by their combination; and in many other ways the principle may be turned to advantage. For my present purpose, I only ask that the simple fact I have endeavoured to prove be allowed, viz. *that decomposition of a compound body occasions as much cold as the combination of its elements originally produced heat*.

XL. *Second Note on the Effect of Fluid Friction in drying Steam which issues from a High-pressure Boiler into the open Air.* *By* Prof. W. Thomson*.

In the August Number of this Magazine, M. Clausius has replied to a Note, published in the June Number, in which I endeavoured to show that the objections he had made to my reasoning regarding the condition of steam issuing from a high-pressure boiler, were groundless. I cannot perceive that this reply at all invalidates any of the statements made in my two former communications†, to which I refer the reader who desires to ascertain what my views are, and to judge as to the correctness of the reasoning by which they are supported. An analytical investigation, according to the principles discovered by Mr. Joule, of the thermo-dynamical circumstances of the rushing

* Communicated by the Author.

† Phil. Mag., vol. xxxvii. p. 387 (Nov. 1850), and vol. i. 4th Ser., p. 474 (June 1851).

of any fluid through a small orifice, is given in a paper communicated last April to the Royal Society of Edinburgh, and since published in the Transactions (vol. xx. part II.) under the title "On a Method of discovering Experimentally the Relation between the Mechanical Work spent and the Heat produced by the compression of a gaseous fluid."

I take the present opportunity of correcting a mistaken expression in my first communication regarding steam issuing from a high-pressure boiler, by which I gave a false, or an inadequate, representation of the connexion of that application of Mr. Joule's general principles which I was bringing forward, with one which he had himself made in one of his published papers. The following is the passage of my communication (addressed as a letter to Mr. Joule), which requires correction:—

"The pretended explanation of a corresponding circumstance connected with the rushing of air from one vessel to another in Gay-Lussac's experiment, on which you have commented, is certainly not applicable in this case, since, instead of receiving heat from without, the steam must lose a little in passing through the stop-cock or steam-pipe by external radiation and convection*." I wrote this under the impression that Mr. Joule had, in his paper "On the Changes of Temperature produced by the Condensation and Rarefaction of Air†," pointed out the incorrectness of an explanation often given of Gay-Lussac's experiment‡, and shown that the phænomenon could be truly explained only by taking into account the heat developed in the air by friction in its passage from one vessel to the other through the stop-cock. I find, however, on looking to the paper, which I had not by me when I wrote, that it contains no reference to Gay-Lussac's experiment, but the following passage, referring to Mr. Joule's own experiments on the heat developed by the compression of air, and the heat absorbed by air allowed to expand from a vessel into which it has been compressed, through a small orifice, into the atmosphere, from which I obtained the idea of considering the heat developed by the friction of steam issuing from a high-pressure boiler.

"It is quite evident that the reason why the cold in the experiments of Table IV. was so much inferior in quantity to the heat evolved in those of Table I., is, that all the force of the air, over and above that employed in lifting the atmosphere, was applied in overcoming the resistance of the stop-cock, and was there converted back again into its equivalent of heat§."

Ardmillan, Ayrshire, Sept. 4, 1851.

* Phil. Mag. S. 3. vol. xxxvii. p. 388.
† Ibid. vol. xxvi. p. 369 (May 1845).
‡ See Lamé, *Cours de Physique*, vol. i. § 352.
§ Phil. Mag., S. 3. vol. xxvi. p. 381.

XLI. *On the Motion of a Free Pendulum.* By the Rev. A. Thacker, *Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The rotation of the plane of vibration, in M. Foucault's pendulum experiment, admits of being deduced from the equations of motion; and as some of your readers may wish to see the problem solved on dynamical principles, I venture to offer the following investigation for insertion in your Journal.

Let a be the radius of the earth,

ω its angular velocity,

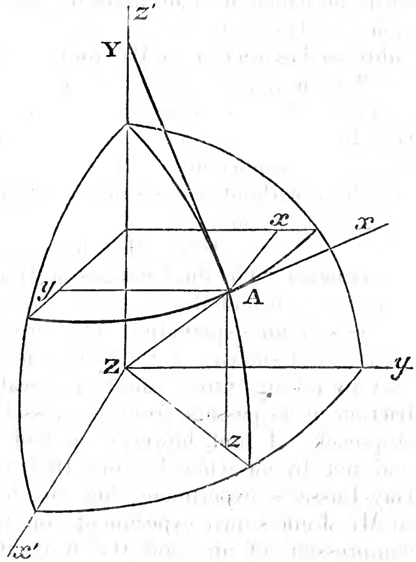
λ the latitude of the place,

l the length of the pendulum,

R the tension of the string,

x', y', z' the coordinates of the ball measured along axes fixed in space, the axis of z' coinciding with that of the earth,

x, y, z the coordinates of the ball measured from the point of suspension in directions opposite to those of x', y', z'.



The equations of motion are

$$\frac{d^2x'}{dt^2} = -g\cos\lambda\cos\omega t + \frac{R}{m}\cdot\frac{x}{l}$$

$$\frac{d^2y'}{dt^2} = -g\cos\lambda\sin\omega t + \frac{R}{m}\cdot\frac{y}{l}$$

$$\frac{d^2z'}{dt^2} = -g\sin\lambda + \frac{R}{m}\cdot\frac{z}{l},$$

where

$$x' = a\cos\lambda\cos\omega t - x$$

$$y' = a\cos\lambda\sin\omega t - y$$

$$z' = a\sin\lambda - z.$$

Hence

$$\left.\begin{aligned}&\frac{d^2x}{dt^2}-(g-\omega^2a)\cos\lambda\cos\omega t+\frac{R}{m}\cdot\frac{x}{l}=0\\&\frac{d^2y}{dt^2}-(g-\omega^2a)\cos\lambda\sin\omega t+\frac{R}{m}\cdot\frac{y}{l}=0\\&\frac{d^2z}{dt^2}-g\sin\lambda\qquad\qquad\qquad\quad+\frac{R}{m}\cdot\frac{z}{l}=0\end{aligned}\right\}\quad .\quad .\quad (1.)$$

Let the position of the ball be now referred to other coordinates X, Y, Z, the axis of X being taken due east, that of Y due north, and that of Z towards the centre of the earth. We then have

$$\begin{aligned}&X=x\sin\omega t-y\cos\omega t\\&Y=x\sin\lambda\cos\omega t+y\sin\lambda\sin\omega t-z\cos\lambda\\&Z=x\cos\lambda\cos\omega t+y\cos\lambda\sin\omega t+z\sin\lambda\\&x=X\sin\omega t+Y\sin\lambda\cos\omega t+Z\cos\lambda\cos\omega t\\&y=-X\cos\omega t+Y\sin\lambda\sin\omega t+Z\cos\lambda\sin\omega t\\&z=-Y\cos\lambda+Z\sin\lambda.\end{aligned}$$

Eliminating x, y, and z from equations (1.), we shall find

$$\left.\begin{aligned}&\frac{d^2X}{dt^2}-2\omega\sin\lambda\frac{dY}{dt}-2\omega\cos\lambda\frac{dZ}{dt}-\omega^2X+\frac{R}{m}\cdot\frac{X}{l}=0\\&\frac{d^2Y}{dt^2}+2\omega\sin\lambda\cdot\frac{dX}{dt}-\omega^2Y\sin^2\lambda+\omega^2\sin\lambda\cos\lambda.(a-z)\\&\qquad+\frac{R}{m}\cdot\frac{Y}{l}=0\\&\frac{d^2Z}{dt^2}+2\omega\cos\lambda.\frac{dX}{dt}-\omega^2Y\sin\lambda\cos\lambda+\omega^2\cos^2\lambda(a-z)\\&\qquad+\frac{R}{m}\cdot\frac{Z}{l}-g=0.\end{aligned}\right\}\quad (2.)$$

Finally, we will suppose the horizontal motion of the ball referred to axes which revolve about the vertical with an uniform angular velocity $\omega\sin\lambda$. If $\bar{x}$, $\bar{y}$ be the coordinates of the ball, we have

$$\begin{aligned}&\bar{x}=X\cos(\omega t\sin\lambda)-Y\sin(\omega t\sin\lambda)\\&\bar{y}=X\sin(\omega t\sin\lambda)+Y\cos(\omega t\sin\lambda)\\&z=\bar{z}\\&X=\bar{x}\cos(\omega t\sin\lambda)+\bar{y}\sin(\omega t\sin\lambda)\\&Y=-\bar{x}\sin(\omega t\sin\lambda)+\bar{y}\cos(\omega t\sin\lambda).\end{aligned}$$

Eliminating X, Y, Z, the equations (2.) become

$$\left.\begin{aligned}
&\frac{d^2\bar{x}}{dt^2}+\frac{\mathrm{R}}{m}\cdot\frac{\bar{x}}{l}-2\omega\cos\lambda\cos(\omega t\sin\lambda)\cdot\frac{d\bar{z}}{dt}\\
&\quad-\omega^2\cos^2\lambda\cos(\omega t\sin\lambda)\{\bar{x}\cos(\omega t\sin\lambda)+\bar{y}\sin(\omega t\sin\lambda)\}\\
&\quad-\omega^2\sin\lambda\cos\lambda\sin(\omega t\sin\lambda).(a-z)=0\\
&\frac{d^2\bar{y}}{dt^2}+\frac{\mathrm{R}}{m}\cdot\frac{\bar{y}}{l}-2\omega\cos\lambda\sin(\omega t\sin\lambda)\frac{d\bar{z}}{dt}\\
&\quad-\omega^2\cos^2\lambda\sin(\omega t\sin\lambda)\{\bar{x}\cos(\omega t\sin\lambda)+\bar{y}\sin(\omega t\sin\lambda)\}\\
&\quad-\omega^2\sin\lambda\cos\lambda\cos(\omega t\sin\lambda).(a-z)=0\\
&\frac{d^2\bar{z}}{dt^2}+\frac{\mathrm{R}}{m}\cdot\frac{\bar{z}}{l}-g+2\omega\cos\lambda\left\{\cos(\omega t\sin\lambda).\frac{d\bar{x}}{dt}+\sin(\omega t\sin\lambda).\frac{d\bar{y}}{dt}\right\}\\
&\quad-\omega^2\sin\lambda\cos\lambda\{\bar{x}\sin(\omega t\sin\lambda)-\bar{y}\cos(\omega t\sin\lambda)\}\\
&\quad+\omega^2\cos^2\lambda.(a-z)=0.
\end{aligned}\right\}\quad(3.)$$

These equations hold for any value of ω. In the case we are considering ω is small, namely $\frac{\pi}{12\times60\times60}$; the terms multiplied by ω and ω^2 are small and periodical; and if these be neglected, we have

$$\left.\begin{aligned}
&\frac{d^2\bar{x}}{dt^2}+\frac{\mathrm{R}}{m}\cdot\frac{\bar{x}}{l}=0\\
&\frac{d^2\bar{y}}{dt^2}+\frac{\mathrm{R}}{m}\cdot\frac{\bar{y}}{l}=0\\
&\frac{d^2\bar{z}}{dt^2}+\frac{\mathrm{R}}{m}\cdot\frac{\bar{z}}{l}-g=0
\end{aligned}\right\}\quad\ldots\ldots\quad(4.)$$

which are equations of exactly the same form as those which apply to the motion of a pendulum suspended from a fixed point in space; the motion, therefore, is the same with regard to the revolving axes as it would be with regard to fixed axes, if the earth had no rotation. The angular velocity of the horizontal axes being $\omega\sin\lambda$, it follows that the orbit will appear to revolve at that rate round the vertical.

I am, Gentlemen,

Your obedient Servant,

A. THACKER.

Trinity College, Cambridge,
June 1, 1851.

[It is much to be desired that the approximation should be carried on one step further, and that at least the general effect should be made out of such of the neglected terms in the above equations as contain the first power of ω. If the oscillations are as considerable as they have been hitherto usually taken in practice, in comparison with the length of the string, by those who have busied themselves in verifying M. Foucault's law, there is no doubt, as is apparent from the equations, that the accuracy of the law, and probably of the period of the revolution of the apsides as dependent on the rotation of the earth, may be appreciably affected. Mr. Thacker's valuable and interesting contribution is confirmed by precisely identical results similarly worked out, and shown to us in MS. some considerable time back from the able pen of a well-known young English analyst; and, since this article has been in type, we have received a communication, not essentially differing, from our esteemed correspondent the Rev. J. A. Coombe.—Eds.]

XLII. *On the Anticlinal Line of the London and Hampshire Basins. By* P. J. Martin, *Esq., F.G.S.*

[Continued from p. 198.]

BEFORE we quit the subject of the drainage of the Wealden area, and of its connexion with the phænomena of upheaval, it will be well to take a review of it in its totality. Much has been said already of the rivers taking their courses north and south through the transverse fissures, enlarged into valleys by denudation. But it is of much importance for the maintenance of the opinion we entertain of the unity of the act of upheaval, and of its suddenness, to take a comprehensive view of this great feature.

The first notice we find of it is, I believe, in Conybeare and Phillips's Outlines of the Geology of England and Wales. The passage is so remarkable, that I cannot refrain from introducing it here. "A very interesting geological phænomenon is presented by the course of the rivers watering this district, and the arrangement of the valleys which convey them. We have already noticed that the two grand valleys of this district,—that of Holmsdale and that of the Weald clay,—are parallel to the direction of the strata; but these do not form the channels through which any of the more important streams seek the sea, for these generally have their source in the central ridge of ironsand (Wealden); and flowing thence both to the north and to the south, in directions nearly at right angles both to these valleys and the strata, traverse the ranges of greensand and chalk through gorges opened across them, in their way to join the Thames on one side, and the Channel on the other; instead of being turned by their escarpments into the great subjacent valleys, as they would be if the fractures in those escarpments were repaired, and forced to empty themselves into Romney Marsh and Pevensey Level. In no place perhaps is the important fact of a double system of valleys crossing each other transversely (a fact which we shall hereafter see to be of the greatest consequence with reference to theories on the origin of the present inequalities of the earth's surface), more strikingly displayed*."

Since the early publication of my "Theory of the Weald Denudation," and the explanation there given of the nature and cause of these transverse river-courses, the subject has become familiar to a great majority of geologists; all that remains now to be insisted on is the collective view of this phænomenon:—the watershed diverging by different channels from a common centre, the curious opposition of the river valleys as they traverse the North and South Downs (betokening the original stretch of the deepest rents across the whole breadth of the area), and the uniform character of the whole arrangement, from the coast at Hastings to the Alton Hills,—all bespeaking a general, simultaneous, and sudden upburst of the whole.

Westward from the Alton range, the Meon Valley and the chalk denudation at Winchester maintain the same character of transverse drainage in the courses of the Itchin and the Test. But a change takes place as we approach the extremity of our anticlinal line. The greater part of the Vale of Pewsey, up to the foot of the Marlborough Downs, sheds its waters across the whole breadth of the chalk by the Avon; and the Vales of Warminster and Wardour send theirs also eastward and southward into the Avon at Salisbury, and so through the New Forest into the Channel at Christchurch. This is still transverse drainage; but it seems to be influenced, if not altogether directed, by the well-known general rise of the secondary strata in a direction N.E. and S.W. all across the kingdom, the line of which elevation passes through that part of Wiltshire. The remarkable transverse valleys of the Avon, and of the stream called the Bourne in Mr. Greenough's map—like the winter-bournes of the chalk, very generally dry half the year—seem to answer to this inflection. This requires, and is worthy of further investigation.

In the foregoing sketch I have confined myself chiefly to the phænomena of elevation and disruption, the basis of the surface-changes we are contemplating. I come now to the third and fourth classes of phænomena before spoken of, viz. lacerated escarpments and drift. And as the first two related to upheaval, so the latter have reference mainly to the concomitant action of denuding flood;—always keeping in mind, that the operations of elevation and denudation have gone hand in hand.

3. By *lacerated escarpments* I mean those appearances in the

* Conybeare and Phillips's Outlines, &c., p. 145.

outcrop of all the strata concerned; and particularly of those stony strata in which we should be most likely to find the conjoint signs of disruptive violence and of aqueous erosion, distinguishable from the detrital operations of time and weathering. Passing by the well-known fact, that where denudation has been active, the prominence, or the want of it, of any given stratum or order of strata is in exact proportion to their induration, or their resisting power,—if soft and destructible, the surface being receding and low, if hard and stony, hilly and high,—we fix our attention first on the chalk.

The soft and destructable nature of the material, whilst it produced the smooth outline of the chalk hills, has so determined the form and constitution of their escarpments, that they exhibit no signs of laceration beyond their coved and scooped surfaces. The sharp angles and fracture edges which convulsion had left, atmospheric agencies have obliterated. The homogeneous structure of the rock-masses of the chalk has also determined the straight and even course of the North and South Downs, as well as the gentle undulations of the saddle or dome-like elevation of the western part of our anticlinal line, for the most part denuded of its "tertiary" covering. But although all the signs of abrupt fracture have disappeared, we still see how the fissures of this stratum have had their edges eroded and spread out,—the deepest into river-valleys, the more superficial into dry transverse valleys and mountain passes on a small scale. A very cursory view of the river-courses through the North and South Downs, as they are delineated in the Ordnance Map*, will explain what is here meant, and show how transverse fissures, whether of independent formation, or as the necessary accompaniments of longitudinal fractures, have been enlarged into valleys by aqueous abrasion. From the chalk we pass to the next rocky stratum, the lower greensand. Here we have more decided evidence of the violence of the denuding operation. There is nothing in the surface arrangement of the chalk that might not be accounted for on the principle of a gradual and gentle removal by sea-currents, or by atmospheric erosion. But a close inspection of the lower greensand escarpment will soon convince us, that water in a state of violent and tumultuous agitation has been at work immediately consequent on, or in conjunction with, the act of upheaval and the fracture of the rock-masses.

Of the three groups into which the lower greensand is divided, and each of which has a distinct country, as faithfully and mi-

* In all matters of local detail the reader is referred to the Ordnance Map; and a comprehensive notion of the act of denudation will be mainly assisted by the study of the arrangements of its high grounds and escarpments.

nutely described by Dr. Fitton*, the lower one only presents an abrupt and rocky escarpment. With some interruptions, this kind of outcrop is continued for many miles together round the west end, and along the north and part of the south sides of the Wealden area. It is to these parts of its course I now direct the observer's attention. Wherever sections in these rocky escarpments offer themselves, a tumultuous and tortuous disposition is to be seen penetrating deeply into them, behind their ordinary coating of alluvial and diluvial rubble. Railway cuttings have sometimes brought these into view; but better examples may often be found in the stone quarries, and would be still more frequent, if it were not so often found more convenient and more profitable to go further back to extract the stone, out of the way of the "débris" above mentioned. There are no better examples of the tumultuary and contorted appearances of which I am now speaking than are to be observed in the outskirts and approaches to the stone pits by the Medway in the Maidstone† country, where much broken material and unprofitable detritus (in which large rock-masses lie loose, and are crumpled and tossed about) have to be removed before the undisturbed rock can be got at. It is on this account also that the "Fire-stone" (the *plateau* of the upper greensand) is generally quarried by shafts sunk near or even through the chalk; as was anciently the case at the Merstham‡ quarries, and as is now done at Reigate Hill. But I remember inspecting a quarry many years ago opened by Alderman Waithman at Ray Common, near the latter place, and worked by an open adit, in the entrance to which, to the extent of twenty or thirty feet or more, the rock-masses lay in great disorder, broken up and contorted *in situ*, and not in the manner of the blocks and broken materials of the talus of a sea-cliff. Such cases as these might be multiplied from all around the escarpments of the area under review.

There is another appearance, and one much more conclusive as to the violence of the diluvial action to which these escarpments owe their existence, which is to be found under favourable circumstances at the angles of the cross fractures described in the foregoing pages. In the imperfect description given in my earliest memoir on this subject, of the course of the river Arun through the greensand escarpment at Pithingden near Pulborough§, I spoke of a remarkable slide of the stony strata on the east side of the gorge-like valley down toward the river, which at that place takes its course in the Weald clay beneath. It is to this extraordinary appearance Mr. Hopkins alludes, p. 17 of his Memoir on the Structure of the Weald, published,

* Trans. of Geol. Soc., vol. iv. 2nd series. † Kent. ‡ Surrey.
§ Geol. Memoir on Western Sussex, pp. 66, 67.

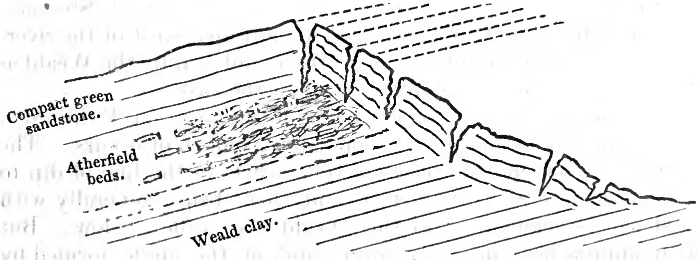
as before said, in the Geological Transactions of 1845. Mr. Hopkins calls it an "anomalous dislocation," and thought with me that it was connected with the river fissure. Although I described and gave a rude figure of this dislocation in the memoir above mentioned, I did not, up to the time of showing it to Mr. Hopkins, thoroughly understand its true nature and the manner of its production. I will endeavour to make it better understood. In this part of Sussex the river Arun takes its course in a direct line from the older strata in the central line of elevation, through the newer strata to the sea; and the gaps in the escarpments of the lower greensand and of the chalk are directly opposed to each other, although ten miles apart, and constitute a remarkable feature in this part of the country. Much study of this long line of transverse fracture has convinced me that it is the result of the compound operation of a slight anticlinal divergence, and also of a slight change of the general southerly dip. The first is shown in a sand-bank at Stopham Bridge; the second by the immediate advance, west of the river, of the greensand country two or three miles into the Wealden area, beyond the greensand country on the east.

It is on the east side or left bank of the river at Pithingden Farm that the extraordinary slope above spoken of occurs. The surface line of the country rises very nearly in the line of dip to about 250 feet above the river, and then drops, generally with a sharp escarpment, into the Weald clay valley below. But as it approaches the river-gorge, and at the angle formed by the intersection of the two lines of longitudinal and transverse escarpment, the hill slopes gently down over a space of about twenty or thirty acres of ground; and by the disclosures of a hollow way on the side of the hill and of the stone quarry at the top (seen by Mr. Hopkins, as above mentioned), we learn that this slope is formed by the breaking down of the great tabular masses of sandstone, of which all the plateau of the lowest bed of greensand in this line of country is composed. The rents which answer to this uncommon deflection from the ordinary line of dip are to be seen in the stone quarry, in the hollow road aforesaid, and at the top of the hill where the fall commences. For a long time I was inclined to think that this phænomenon was the result of a sliding down of these stony strata at the moment of the disruption of the river fissure. But this explanation was not satisfactory, and gave place to a better, and I doubt not, the true one.

In consequence of some inquiries of Dr. Fitton, when he was engaged in his exposition of the extension of the Atherfield beds along the greensand escarpment of the Weald, I was led to discover that in all this line of country the representatives of the

Atherfield beds consisted of a series of loose porous sands and clays of no great thickness, interposed between the green sandstone and the top of the Weald clay; and that it was by the removal of these loose materials, during the act of denudation, that these large masses of stone had been let down as we now see them.

To produce this effect to so remarkable a degree at this point, two circumstances have combined,—the sharpness of the angle when the denudation was brought down to this part of the river fissure, and the projection of the greensand country as before-mentioned into the Bedham Hills; which rise at least 500 feet immediately west of the river, and would cause the flood of denudation in the flux and reflux of its wave to impinge violently on this particular spot. I here repeat with a little variation my original sketch of the downcast I have endeavoured to describe*.



Possessed with the justness of this interpretation of these appearances, I have for comparison examined some of the salient points of the greensand escarpment on the other side of the Weald, and have not been disappointed. I pass over many minor indications of the like kind of dislocation and dilaceration in the hollow ways and small stone pits of the Hartingcombe and Haslemere country, to mention the highly illustrative stone quarry now open at Nore Farm, at the eastern extremity of the Hasscomb Hills†. The Bargate stone beds (as the corresponding greensand stone of this side of the Weald is called) are not quite so low in the series as the Pulborough stone, or the Kentish-rag, and do not lie so close on the Atherfield beds. Nevertheless they have been extensively warped and tossed about by the removal of the looser and more destructible materials, and

* The dislocation has none of the character of an "under cliff," or of the "Hawksley slip" described in White's 'Selborne,' Letter 45, but sweeps round the angle of the eminence, and is evidently produced by the subsidence of the mass of stone, as the loose materials were removed from below.

† Surrey.

present, though in a lesser degree, the same dislocation as the Pithingden Hill.

The transverse fissure-valleys which transmit the affluents of that branch of the Arun called the Western Rother by Petworth and Lodsworth*, present the same downcast of stony beds. A quarry opened in the Petworth Rectory grounds some years ago was just the counterpart of the one at Pithingden. In other less angular parts of the greensand escarpment, wide fissures, open joints filled with rubble, and other signs of violence, abound where the intersections of hollow ways or stone quarries expose their basset edges to observation. On the north side of the Weald, again, the Kentish-rag country is not without these signs of violent disruption,—the escarpments of Boughton-Malerbe, Boughton-Monchelsea, and Sutton-Vallance, for instance; and I strongly suspect that the dislocation at Tilburstow or Tilvester Hill, described by Dr. Mantell†, is a slide of this kind.

Much more might be said, and many more localities pointed out, to show the effects of violent watery abrasion in these escarpments; but I pass on to the equally important subject of *diluvium* or *drift*.

4. I should have preferred the use of the former word to designate the transported materials of the area under review; because I abjure for them every idea of other means of transport than a diluvial action,—be it short and transient, or be it longer, and frequently repeated till the denudation was complete. But since the repudiation of the theoretical views on which that term was founded by its author, and the general adoption of the word drift, gives latitude to the use of the phrase, I shall use both words indifferently, with the understanding that if diluvium is used, I mean (as I believe is everywhere meant now) the drift of *a* deluge, and not of *the* deluge.

To those who are familiar only with the confused masses of gravel, loam, sand, rounded and angular flints and rolled clays of the London district, or indeed of the greater part of the south of England, it will seem rather surprising to be told that the drift of the district under review admits of a division into four distinct natural zones, which occupy as many lines of country,—mantling round the nucleus of the Weald. It is not pretended that the lines of demarcation of these zones are so hard and well-defined as some other boundary lines in this branch of natural history. But they are strict enough to be very remarkable; and such as I could not myself have predicated, and could hardly have believed to be in existence, if repeated and extended observations had not convinced me of the truth.

* Sussex. † Geol. of S.E. of England, p. 177.

The first and uppermost we may call the *Tertiary Zone.* The beds and sprinklings of this division consist mainly of shingle of the eocene æra, with some angular flints and sandy or argillaceous loam, abounding everywhere with rounded pebbles.

The next, the *Cretaceous Zone,* consisting entirely of angular or unbroken flints, in which we very rarely find a pebble except when entangled in a more than usual invasion of the clays of the lowest tertiary formations.

The *Subcretaceous,* which consists of angular flints with a large admixture of the ironstone, sandstone, and chert of the greensand beds.

And fourthly, the *Wealden Zone,* in which the flints, except in a few points on the margin of this formation, have disappeared along with the pebbles and other materials characteristic of the strata higher in the order of geological superposition. It will be convenient, perhaps, to put this in a tabular form:—

Drift.	1. Tertiary zone.	Pebbles and broken shingle beds; slight admixture of angular flints; sand and loam, and some chalk-rubble.
	2. Cretaceous zone.	Angular flints. Pebbles very rare. Very little loam, but sometimes much chalk-rubble.
	3. Subcretaceous zone.	Angular flint with chert, ironstone and sandstone, much sand and little loam.
	4. Wealden zone.	Iron-rag (a conglomerate of the *débris* of the various beds above and below the Weald clay). Beds of diluvial loam, sometimes of great depth.

Of these, the two first contain the bones of mammals, and the usual organic remains of what is called the Pleistocene. In the third, these are very rare; and in the fourth there are none at all.

All the mineral substances of these several zones belong to the strata which are found in and around the great anticlinal line, if it be determined that the "grey wethers" are the production of any of the tertiary beds. And no substances foreign to these strata are to be found in the drifts; unless it be that some intrusions from the older beds west of the area in review may by accident be found in that part of the line; of which, whether or no, I am not critically certain.

Moreover, if by pleistocene is meant a marine deposit made in the ordinary way we usually understand the formation of sedimentary beds, from a sea of the range and extent of which we cannot now have any possible conception, then I unhesitatingly affirm that no such deposit, or relic of such deposit, is to be found in any part of this area.

The first, or *Tertiary Zone*, ranges all along the edges of the plastic clay as they basset on the chalk, and contain everywhere a large admixture of the shingle which abounds so much in that stratum. As the bare chalk begins to appear, angular flint is largely added; and finally angular flint alone is found on the chalk, and the rounded pebbles disappear, except here and there a few stragglers.

This change from pebbly drift to broken flint may be conveniently studied on the verge of the North Downs, on Walton Heath*, between the Addington Hills† and the chalk escarpment, and in all the outcrop of the tertiary beds, to the Darent. Similar observations may be made along the same outcrop to the Basingstoke‡ and Odiham country. I have not examined this outcrop to the north of the Pewsey line, but I do not doubt of finding a similar interchange of rounded pebbles for broken flints there also.

Crossing the Hampshire and Wiltshire chalk, we find the same pebbly drift from Salisbury to Michelmarsh on the Test and Romsey§; and we meet it again on the northern borders of the Forest of Bere, mixed with a very large proportion of flints in the gravel pits at Horn-dean and Rowland's Castle‖. Eastward from Bere Forest, where we enter on the line of country south of the South Downs, so great has been the destruction of the chalk, that the cretaceous or flint zone falls in and almost excludes the pebble beds. Nevertheless they appear in force at Boxgrove¶, more sparingly along the flat country towards Arundel, but again strongly at Clapham and Patching, north of Highdown Hill, and still prevail along the Worthing and Shoreham vicinities, till they are cut off by the Brighton cliffs.

The section of the diluvial beds given by the Brighton cliffs, from Copperas Gap to Rottingdean, possesses much interest, because it includes deep masses of drift, composed of chalk-rubble, angular flints, and sand and shingle of the eocene epoch, so commingled as to have led the historian of the "Geology of the South-east of England" into the belief that they were stratified beds of the æra of the crag. To these he has given the name of "Elephant beds**," because they yielded the bones of elephants and other mammals. But Dr. Mantell seems to have had some

* Surrey. † Surrey. ‡ Hants.
§ Hants. ‖ Hants. ¶ Sussex.
** Geol. of the S.E. of England, by Gideon Mantell, F.R.S., 1833.

misgivings on this head, for he has allowed the elephant beds their proper place and prominence in the chapter on diluvium.

Dr. Mantell's description of these cliffs is perfectly faithful; and I have only to add, that they form an excellent type of the passage of the tertiary into the cretaceous zone of drift. In the composition of that portion of them which lies to the east of Kemp Town, I imagine the sands to be derived from the plastic clay; the angular flints and chalk-rubble from the eroded chalk-rock in the vicinity; and the shingle bed or ancient beach at the bottom belongs to the pre-eocene epoch, and is the beach of a sea of which we now know nothing more, than that it beat upon the chalk before the deposit of the tertiary formations; and was most probably the parent of the great shingle beds of the plastic clay, of which so much has been said.

The second, or *Cretaceous Zone.*—As before said, on drawing nearer to the bare and abraded chalk-hills, the rounded gravel of the tertiaries ceases to be a feature in the composition of drift; and although a few are to be found in the "vents" (? rents) and fissures of the chalk, and some stragglers adhering to the rolled clays and clay loams which still linger amongst the drifts of the verge of the Downs, they give place to a plentiful coating, and in some cases large accumulations of entire and broken flints; and here and there beds of loam and patches of chalk-rubble. In the line of country to which these belong also, we find the "grey wethers" or Druid sandstones, and the flint conglomerates of the eocene æra,—witnesses of the demolition of the strata which lay on the chalk before the catastrophe of elevation.

It is scarcely necessary to say, that all the Down country affords an ample display of this sort of drift. The largest accumulations of flint are to be found where there appears to have been the greatest amount of denudation, at the bottom of valleys, both longitudinal and transverse. On the broad expanse of the Hampshire chalk, the tops of some of the highest eminences, which have escaped perfect denudation, afford loams with a few remaining round pebbles. The Burghclere Hills, for instance, above the sources of the Test, and the high grounds between Andover and Micheldever*; and a few round pebbles are also to be found in the flint gravel beds of the Candover Valley, and in the loams round the northern borders of the Vale of Meon†. To this zone also I refer two remarkable accumulations of drift; —the one consisting almost entirely of angular flint and strong loam, at and in the neighbourhood of Farnham‡; and the other an equally important one of the same sort of flint, with chalk-rubble and a very slight sprinkling of rounded pebbles, near Dorking§, at the entrance of the Vale of Leatherhead. A

* Hants. † Hants. ‡ Surrey. § Surrey.

conveniently deep and illustrative section of this bed of cretaceous diluvium, long worked for road materials, is to be found near Burford Bridge. The materials here are firmly impacted and loosely cemented together, like the Brighton cliffs, by their own carbonate of lime; which gives an appearance, as at Brighton, approaching to regular stratification. But there is nothing here to remove this accumulation from the category of common cretaceous drift. Beds of this kind show themselves on the banks of the Mole in the hollow way near the bridge on the Reigate road; and also on the verge of the gault in digging foundations of houses at the foot of Reigate Hill. Shifting to the South Downs country, the same appearances are to be found on the abraded surface of the chalk near Chichester*. The Port-field there has long been worked for angular flint mixed with chalk-rubble; and between that place and the Union house at West Hampnet, there was some time since, and perhaps is still, a flint-gravel pit the exact counterpart of the Dorking one, and composed of the same materials as the diluvial beds there, and as those on which Brighton is built. I may here observe, that it was the great denudation of the chalk, and the spread of these drifts along the flat country south of the Downs from Chichester to Brighton, and the raised beaches of eocene shingles, which gave early observers the idea of its being an ancient sea-bed, of which the chalk downs were the border;—a position perfectly untenable.

Before we quit the chalk downs and enter the denudation below, we may observe that, although the escarpment of these downs, north and south, seems to have been swept clean of flint, it presents here and there some beds of rubble. The same may be said of the terraces of the malm or upper greensand. But the gault has a large sprinkling of flints, which sometimes lie in considerable hollows, as recorded many years ago by Sir R. Murchison in his account of Alice Holt under the Alton Hills†. These belong properly to the cretaceous zone, and are often agglomerated by oxide of iron derived from the stratum in which they have been imbedded. The gault country forms but a narrow strip at the foot of the Downs. So narrow is its outcrop in the western part of Surrey, that it is sometimes intruded on by the subcretaceous drift. But except in this line of country and in that east of Lewes, where it ceases to be bounded by the high grounds of the lower greensand, it exhibits very little more than angular flints.

[To be continued.]

* Sussex.

† Geol. Trans., vol. ii. 2nd series, p. 100.

XLIII. *On the Solution of certain Systems of Equations. By* JAMES COCKLE, *M.A., Barrister-at-Law, of the Middle Temple**.

(1.) THE following investigations were suggested to me by the perusal of Mr. Sylvester's paper at pp. 370–373 of the last November Number of this Journal; but they do not involve the theory of determinants. In no spirit of disparagement of that theory, nor of the splendid scientific achievements of Mr. Cayley with reference to it, I venture to intimate an opinion that, as the theory of determinants (in its explicit form at least) is in no degree indispensable to the progress of the theory of algebraic equations, so also that its processes have no decided superiority over others that enter into the algebraic theory. This introductory remark must be considered in exclusive reference to the theory of equations, otherwise it would be indicative of impertinence on my part, as well as of inaccuracy.

(2.) Let there be given for solution m simultaneous equations. And, further, suppose that, by some of the known artifices of algebra, those m equations can be put under the respective forms

$$u_1+\Omega=0,\ u_2+\Omega=0, \ldots, u_m+\Omega=0, \quad . \quad . \quad (a.)$$

then the solution of the system (a.) involves that of the given equations.

(3.) Let $$u_r-u_{r+1}=v_r,$$

then, if we can satisfy the m relations

$$v_1=0,\ v_2=0, \ldots, v_{m-1}=0,\ u_m+\Omega=0, \quad . \quad . \quad (b.)$$

we can satisfy the system (a.). We might give various forms to v_r, and consequently to (b.), but I have selected that which appears to be the most convenient. These forms may however be departed from as individual examples may render it desirable.

(4.) Let x be one of the unknowns involved in the given equations. Then, if we assume that

$$v_r=A_rX_r,$$

where A_r either is free from x or capable of being made to vanish without determining x, we obtain a very remarkable form of v_r; for, in this case, the solution of (b.) reduces itself to that of the system

$$A_1=0,\ A_2=0, \ldots, A_{m-1}=0,\ u_m+\Omega=0. \quad . \quad (c.)$$

* Communicated by the Author, who adds the following note:—

["There are one or two observations which I should have been glad to have included in the above paper. But, as it has already extended to the limits within which it is perhaps desirable that I should confine myself, I shall seek another opportunity of laying them before the readers of this Journal.—JAMES COCKLE."]

It is immaterial whether X_r contains x, or not, but it is a condition, essential to the solution of (*c.*), that $u_m+\Omega$ should involve x. There are of course other conditions, but I shall not here examine them in detail.

(5.) Let $u_m+\Omega$ be of the nth degree in x; we will now proceed to consider a few instances in which m given equations admit of what I have (Phil. Mag. S. 3. vol. xxxvii. p. 502, art. 17, and p. 503, art. 19) denoted by the expression a 'determination' of the nth degree. And, first, let us proceed to the system

$$U=aP^2,\ V=bP^2$$

already (Ibid. pp. 372, 373) treated of by Mr. Sylvester.

(6.) Let

$$-P^2=\Omega,$$

then, in the present case*, we have

$$v_1=v_{m-1}=a^{-1}U-b^{-1}V;$$

and, if we make

$$X_1=X_{m-1}=a^{-1}b^{-1}x^2,$$

we have

$$A_1=A_{m-1}=bU'-aV',$$

where

$$U=U'x^2 \text{ and } V=V'x^2.$$

Now, U and V, being homogeneous quadratic functions of x and y, U' and V', are quadratic functions of $x^{-1}y$ and involve no other undetermined quantity. Hence we may satisfy

$$A_1=0$$

by means of a relation of the form

$$y=px,$$

p being known and x left wholly undetermined. Consequently, the relation

$$b^{-1}V-P^2=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d.)$$

being the only one remaining to be satisfied, the problem admits of an n^{ic} determination capable in the present instance of being reduced still lower.

(7.) For, since V' is a known quantity of the form

$$\alpha p^2+\beta p+\gamma,$$

the equation (*d.*) is equivalent to

$$\pm x\sqrt{b^{-1}(\alpha p^2+\beta p+\gamma)}=P,$$

an equation of $\frac{1}{2}n$ dimensions; n being, in this particular instance, even.

* In the present case we might, perhaps advantageously, have made $\Omega=-abP^2$. So in art. (9.) we might have employed $ABC\omega$ in place of ω.

(8.) It is worthy of remark that the same determination may be effected when U and V are both homogeneous *cubic*, or both homogeneous *biquadratic* functions. The only difference is that in the former case p is determined by means of a *cubic*, and in the latter by means of a *biquadratic* equation.

(9.) Let us now proceed to the first system given (Ibid. p. 370) by Mr. Sylvester, viz.

$$U=A.\omega, \; V=B.\omega, \; W=C.\omega.$$

We here have

$$v_1=A^{-1}U-B^{-1}V, \; v_2=B^{-1}V-C^{-1}W;$$

and, if we make

$$X_1=A^{-1}B^{-1}x^2, \; X_2=B^{-1}C^{-1}x^2, \text{ and } W=W'x^2,$$

we also have

$$A_1=BU'-AV', \text{ and } A_2=CV'-BW';$$

and, if we assume that $z=qx$, the relations

$$A_1=0, \text{ and } A_2=0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (e.)$$

will be ordinary simultaneous quadratics in p and q.

(10.) The solution of these two quadratics would at first sight seem to entail upon us the necessity of solving a biquadratic. This however may be avoided by means of the general theory of linear transformations. For, since by linear transformation the system (e.) may, without the occurrence of any equation higher than a cubic, be transformed into two pure quadratics in which the unknowns are linear functions of p and q, we see that those quantities may be determined (after the transformation) by reduction and quadratic evolution only. But the above is not the only method of avoiding the occurrence of a biquadratic. The following algebraic artifice enables us to arrive at the same result with perhaps greater ease, simplicity and directness. Valuable and interesting as is the general theory of linear transformation, it may be questionable whether the sphere of its *practical* usefulness extends over the pure theory of algebraic equations.

(11.) Either of the quadratics (e.) may be put under the form*

$$xy+a=0, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (f.)$$

* For, adopting the notation of my Method of Vanishing Groups (as to which see paragraph XV. *et seq.* of p. 177 of the last [May] number of the Cambridge and Dublin Mathematical Journal), we have

$$\gamma^2{}_2(A_1)=h^2{}_1+h^2{}_2+a=xy+a,$$

provided that

$$x=h_1+h_2\sqrt{-1} \text{ and } y=h_1-h_2\sqrt{-1}. \quad . \; . \; . \quad (f'.)$$

If, by means of (f'.), we determine p and q in terms of x and y, we pass to (g.) by *substitution* only, and without recourse to any general theory of linear transformation. When A_2 is better adapted for our purpose, we may form the function $\gamma^2{}_2(A_2)$ instead of $\gamma^2{}_2(A_1)$.

and the other may then, after a linear substitution, be denoted by

$$x^2+bxy+c^2y^2+dx+ey+f=0; \quad . \quad . \quad . \quad (g.)$$

let us now examine the relation $(g.)+\lambda(f.)$.

(12.) First, let $\lambda=2c-b$, and we have

$$(x+cy)^2+dx+ey+2ac-ab+f=0. \quad . \quad . \quad (h.)$$

Next, let $\lambda=-2c-b$, and we have

$$(x-cy)^2+dx+ey-2ac-ab+f=0. \quad . \quad . \quad (i.)$$

(13.) Now let

$$x+cy+\mu=\mathrm{X}$$

and

$$2ac-ab+f-\mu^2=\mathrm{M},$$

then we may give $(h.)$ the form

$$\mathrm{X}^2+\mathrm{M}+(d-2\mu)x+(e-2c\mu)y=0. \quad . \quad . \quad (j.)$$

So, if we make

$$x-cy+\nu=\mathrm{Y},$$

and

$$-2ac-ab+f-\nu^2=\mathrm{N},$$

we may represent $(i.)$ by

$$\mathrm{Y}^2+\mathrm{N}+(d-2\nu)x+(e+2c\nu)y=0. \quad . \quad . \quad . \quad (k.)$$

(14.) If we assume that

$$\frac{e-2c\mu}{d-2\mu}=-c=-\frac{e+2c\nu}{d-2\nu},$$

and, consequently, that

$$\mu=(4c)^{-1}(cd+e), \quad \nu=(4c)^{-1}(cd-e),$$

and

$$d-2\mu=(2c)^{-1}(cd-e), \quad d-2\nu=(2c)^{-1}(cd+e);$$

and if we also make

$$\mathrm{M}'=\mathrm{M}-(8c^2)^{-1}(cd-e)^2, \quad \mathrm{N}'=\mathrm{N}-(8c^2)^{-1}(cd+e)^2,$$

then $(j.)$ and $(k.)$ may be put under the respective forms

$$\mathrm{X}^2+(2c)^{-1}(cd-e)\mathrm{Y}+\mathrm{M}'=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (l.)$$

$$\mathrm{Y}^2+(2c)^{-1}(cd+e)\mathrm{X}+\mathrm{N}'=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (m.)$$

(15.) The equations $(l.)$ and $(m.)$ may be still further simplified as follows; assume that

$$\mathrm{X}=\alpha x, \qquad \mathrm{Y}=\beta y,$$

$$\mathrm{M}'=\alpha^2 m, \text{ and } \mathrm{N}'=\beta^2 n,$$

and determine α and β so as to satisfy the relations

$$\alpha^{-2}\beta(2c)^{-1}(cd-e)=1=\beta^{-2}\alpha(2c)^{-1}(cd+e),$$

then (*l.*) and (*m.*) will take the respective forms

$$x^2+y+m=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (n.)$$

$$y^2+x+n=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (o.)$$

(16.) The two last equations may be solved thus:—
Let

$$y=-2r(x-r)+(4r)^{-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (p.)$$

then (*n.*) and (*o.*) become, respectively,

$$(x-r)^2+r^2+(4r)^{-1}+m=0 \quad . \quad . \quad . \quad . \quad . \quad (q.)$$

and

$$4r^2(x-r)^2+r+(4r)^{-2}+n=0. \quad . \quad . \quad . \quad . \quad . \quad (r.)$$

Form the equation $4r^2\times(q.)-(r.)$, and the result is

$$4r^4+r+4mr^2-r-(4r)^{-2}-n=0,$$

which last equation, being multiplied into $2^{-2}r^2$, gives

$$r^6+mr^4-2^{-2}nr^2-2^{-6}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (s.)$$

a cubic in r^2, whence r may be determined. And x may then be obtained from (*q.*) and y from (*n.*).

2 Pump Court, Temple,
August 9, 1851.

XLIV. *On Carmufellic Acid**. *By* Dr. Sheridan Muspratt, *F.R.S.E., Professor of the College of Chemistry, Liverpool, and* Joseph Danson, *F.C.S., late Assistant in the College*†.

ALTHOUGH numerous researches connected with eminent names in chemistry prove that the importance of a correct history of the Clove and its derivatives has not escaped consideration, still the meagre and indefinite details upon them which are to be found in a few scientific manuals indicate that a very great deal remains yet to be accomplished. The principal results thus far upon cloves are by Dumas‡ and Ettling§; the former upon caryophylline, an indifferent substance abstracted from them by alcohol; the latter upon caryophyllic or eugenic acid, an oily acid obtained from the undeveloped buds of *Caryophyllus aromaticus*, oil of cloves being a mixture of the acid and a neutral oil. Dumas found the purification of caryophylline extremely difficult, owing to a resinous body which tenaciously adheres to it, and which after months of investigation he could not remove. It was recently, however, that the following for-

* From the Arabic, Karmufel, *the clove-tree.*

† Communicated by the Authors, having been read before the Royal Society of Edinburgh.

‡ Liebig's *Annalen*, vol. ix. p. 73. § Ibid. vol. ix. p. 68.

mula for this substance was established :—

$$C^{10} H^8 O^*.$$

Chemists well know the numerous difficulties presenting themselves at every step in organic chemistry; and as we have now worked for more than twelve months upon cloves, we feel convinced that others who ventured upon this department left it impatiently on account of the numerous obstacles daily appearing, and the small quantities of some of the substances derivable from this spice; *e. g.* it would be utterly impossible to obtain enough of eugenine from the aqueous extract of ten or twelve pounds of cloves; and the acid we are about to describe, and which is produced by the action of nitric acid upon the aqueous extract of cloves, could not be obtained in sufficient quantity for examination from less than twenty pounds. One pound of cloves yields two grains of impure acid.

Preparation of the Acid.

About twenty pounds of cloves were introduced into a copper boiler and well-macerated with four gallons of water, and then boiled briskly for an hour; the dark brown liquid syphoned off, and the pulpy mass boiled with successive portions of water until the solution withdrawn was nearly colourless. These several decantations amounted to about thirty gallons of liquor, which were evaporated to about six gallons. We found upon treating a small portion of the brownish decoction with nitric acid that the action was most violent.

The mixture frothed up to about twelve times its volume, consequently this was a warning for future operations. We divided the six gallons into twenty or thirty portions, treating each with nitric acid in the cold. When the intumescence had subsided, the vessels were ranged on a covered sand-bath and allowed to digest for several days; the liquid became of a pale yellow colour, and large quantities of a whitish precipitate floated through the menstruum. The gas evolved during the brisk effervescence was most irritating, producing extreme lacrymation; in fact the eyes were quite swollen when subjected for a short time to its influence. A small quantity of nitric oxide was evolved collaterally with the suffocating vapours, and a large quantity of oxalic acid was found in the solution. Carbonic acid also escapes during the action. If the above extract be concentrated to the consistence of syrup, the action is very energetic even in the cold, and the supernatant liquid cannot be obtained except of a blood-red colour. All the irritating vapours were completely expelled before separating through bibulous paper

* Dr. Muspratt on Caryophylline, Lancet, November 2, 1850; Pharmaceutical Journal, vol. vii. p. 343.

the deposit from the canary-coloured fluid. The filtration was extremely tedious; it occupied upwards of a month: the precipitate was edulcorated completely with boiling water, until the liquid percolating did not redden litmus paper. When cold water was employed, the filtrates were always milky, which, however, disappeared on boiling. The filtrate was evaporated to a small bulk, when fine yellow micaceous scales deposited. They were collected and redissolved in boiling water, then precipitated by acetate of lead, and the lead salt washed by decantation to separate the last traces of acetic acid; an excess of sulphide of hydrogen was next passed through the lead salt suspended in water, the whole boiled and then filtered through pure animal charcoal, which gave a colourless solution that on evaporation yielded to our satisfaction splendent white crystals of the acid. The crystals were insoluble in alcohol, æther and cold water, soluble in hot ammonia, in potassa, and in large quantities of boiling water; concentrated sulphuric acid leaves it intact in the cold, but in the heat carbonizes it, giving off sulphurous acid. We have not enumerated a tithe of the difficulties encountered, as they would only occupy space, and not prove interesting to the reader; but those wishing to prepare the acid will find the preceding directions sufficiently explicit.

Behaviour of the Acid with Metallic Oxides.

On mixing a moderately strong solution of the acid with any soluble salt of baryta, strontia or lime, the whole becomes a perfect transparent jelly of such spissitude as to allow the vessel containing it to be inverted.

Soluble salts of lead give with the acid a white transparent jelly.

...	copper	...	pea-green flakes.	
...	silver	...	white	...
...	oxide of iron	...	white	...
...	sesquioxide of iron		pale yellow	...

When the precipitates are dried they occupy a very small space, and much resemble mica in their feel and crispness. All these precipitates are soluble to a certain extent in nitric and hydrochloric acids. The lead salt is perfectly dissolved, the liquid becoming clear, while with the others it remains opake.

The acid, heated *per se*, fuses and gives off fumes resembling those from burnt sugar, together with a yellow oil which condenses on the sides of the tubes.

Analysis of the Acid and its Salts.

The acid, dried at 100° C., and burned with chromate of lead, gave the following numerical results:—

I. 0·456 grm. of substance gave 0·575 grm. of carbonic acid =0·156 grm. of carbon = 34·210 per cent., and 0·198 grm. of water =·022 grm. of hydrogen = 4·824 per cent.

II. 0·420 grm. of substance gave 0·528 grm. of carbonic acid =0·1440 grm. of carbon = 34·285 per cent., and 0·182 grm. of water =·0202 grm. of hydrogen = 4·809 per cent., which corresponds with the formula

$$C^{24} H^{20} O^{32}.$$

		Centesimally represented.		
			Found.	
		Theory.	I.	II.
24 equivs. of Carbon .	144	34·285	34·210	34·285
20 ... Hydrogen	20	4·761	4·824	4·809
32 ... Oxygen .	256	60·954	60·966	60·906
1 ... Acid . .	420	100·000	100·000	100·000

We found the greatest difficulty at first in preparing the salts of this acid; *e. g.* if we added the acid to nitrate of baryta, a jelly was the result; but this could not be dried on bibulous paper, as it adhered so persistently that it was impossible to scrape the salt from it. Our only resource was to prepare the salt from the acetates, taking atomic proportions.

Carmufellate of Baryta.

Dissolved equivalent proportions of acid and acetate of baryta in water, mixed the solutions and evaporated to dryness in a water-bath; washed the residue with water, and dried on a porous tile over sulphuric acid under a bell-jar. It is slightly soluble in water, but dissolves copiously in nitric and hydrochloric acids.

Analysis of the Salt.

0·2950 grm. of salt gave 0·0710 grm. of sulphate of baryta =0·0466 grm. of baryta =15·796 per cent.

		Centesimally represented.	
		Theory.	Found.
1 equiv. of Carmufellic acid . .	420	84·592	
1 ... Baryta	76·5	15·408	15·796
1 ... Carmufellate of baryta	496·5	100·000	

Formula $BaO, C^{24} H^{20} O^{32}$.

Carmufellate of Lead.

This salt was similarly prepared to the baryta one. It is slightly soluble in water, soluble in nitric acid.

Analysis of the Salt.

0·1320 grm. of salt gave 0·0375 grm. of sulphate of lead =0·0272 grm. of oxide of lead = 20·606 per cent.

			Centesimally represented.	
			Theory.	Found.
1 equiv.	Carmufellic acid .	420	78·947	
1 ...	Oxide of lead . .	112	21·053	20·606
1 ...	Carmufellate of lead	532	100·000	

Formula PbO, $C^{24} H^{20} O^{32}$.

Atomic Weight of the Acid.

	Theory.	Found.	Mean.
Baryta salt . . .	420	407	419
Lead salt	420	432	

Having finished the investigation of the acid, we concluded that it might have been formed from the oxidation of eugenine by nitric acid; but this we found not to be the case. We obtained eugenine in transparent pearly laminæ from the distilled water of cloves, but in very small quantities; it is only very slightly affected by strong nitric acid. Dumas assigned it the subjoined formula, which we deem the correct one:—

$$C^{20} H^{12} O^{4}.$$

The following are the formulæ of the substances already derived from cloves:—

Caryophylline . .	$C^{10} H^{8} O$	Muspratt.
Carmufellic acid .	$C^{24} H^{20} O^{32}$	Muspratt and Danson.
Eugenine . . .	$C^{20} H^{12} O^{4}$	Dumas.
Eugenic acid . .	$C^{24} H^{15} O^{5}$	Liebig and Ettling.

We are at present engaged upon eugenic acid and the neutral oil of cloves; the subject is extremely difficult, but well worthy of research.

XLV. *On the deduction of* Fresnel's *construction from the formulæ of* Cauchy *for the Motion of Light. By* Dr. Beer *of Bonn**.

THE method generally pursued in deducing the optical relations of crystalline media from the formulæ of Cauchy for the motion of light, leads to the result, that in such a medium plane waves, whose direction of oscillation is perpendicular to a principal axis, proceed with one and the same velocity, however

* Communicated by the Author.

otherwise they may be situated with regard to the axes of the crystal; that, on the contrary, the velocity of oscillations which are parallel to a principal axis are dependent on the position of the plane of the waves. From this it would follow, that the vibrations of the extraordinary ray in a crystal of one axis are perpendicular to the said axis; that those of the ordinary refracted ray, on the contrary, lie in a principal section. One might be disposed to believe that the proof is hereby furnished, that in the case of plane-polarized light the plane of oscillation and the plane of polarization coincide, and thus to regard the other view, according to which these planes stand perpendicular to each other, as refuted. This apparent proof is, however, illusory. Proceeding from the formulæ of Cauchy, the last-mentioned view, which is known to be that to which Fresnel adhered, may be established, if other quantities be neglected than those usually fixed upon, but which we are equally entitled to neglect. I shall show this in the following; and by reference to figures in space, will endeavour to make this subject plainer than would be possible if we confined ourselves to the geometrical expression of the final result alone. I am induced to publish the present paper by the reading of a memoir from W. J. M. Rankine in the June Number of this Magazine, in which the question as to the direction of vibration in plane-polarized light is handled, and to which the following in some measure attaches itself.

In his *Mémoire sur la Dispersion de la Lumière*, Cauchy has shown that in every homogeneous medium, to every plane P and definite length of wave λ an ellipsoid E belongs, which he has named the ellipsoid of polarization, with the aid of which it is easy to give a simple expression to the laws according to which waves of the length λ, whose planes are parallel with P, proceed through the said medium. In general only three plane-polarized waves proceed parallel with P; their directions of vibration run parallel with the three axes of E; and if we express the halves of these axes by A, B and C, then the velocities of these three waves respectively are

$$\frac{\lambda}{2\pi}\cdot\frac{1}{A},\ \frac{\lambda}{2\pi}\cdot\frac{1}{B}\ \text{and}\ \frac{\lambda}{2\pi}\cdot\frac{1}{C}.$$

Supposing the centre of the ellipsoid E to be the origin of a system of orthogonal coordinates, the said origin being conceived to coincide with any molecule of æther, it will be expressed by an equation of the following form:

$$Lx^2+My^2+Nz^2+2Pyz+2Qxz+2Ryz=1.$$

Between the coefficients of this equation on the one side, and the constitution of the æther, as well as the direction of the plane P, the following relations subsist:

$$L=2m\Sigma\left\{\frac{f}{\Delta r}+\frac{\Delta x^2.\phi}{\Delta r^3}\right\}\sin\frac{\pi}{\lambda}\{u\Delta x+v\Delta y+w\Delta z\}^2.$$

M= analogon mut. mut., N= analogon mut. mut.

$$P=2m\Sigma\frac{\phi}{\Delta r^3}.\Delta y\Delta z\sin\frac{\pi}{\lambda}\{u\Delta x+v\Delta y+w\Delta z\}^2.$$

Q= anal. mut. mut., R= anal. mut. mut.,

where the symbols denote as follows:

1. m the mass of a particle of æther.
2. Δr the distance of any æther particle whatever from the origin of coordinates; the projections of the said distance upon the three axes, that is to say, the coordinates of the particle, being denoted by Δx, Δy, Δz.
3. f and ϕ certain functions of Δr not to be further characterized here.
4. u, v and w, the cosines of the angles which the normal to the plane of undulation encloses with the three axes. The summation expressed by the sign Σ is, strictly speaking, to be extended to all the æther particles.

As the motion of the light is due to the play of molecular forces, by far the greater portion of the accelerating force of any particle is derived from the action of those particles which lie near it; so that in our formulæ those members only are to be retained in which Δx, Δy, Δz refer to those particles which immediately surround the origin of coordinates. And further, since the proximity of the particles is very great, in a case where an approximate result only is required, the members may be neglected in which any one of the quantities Δx ... appears involved higher than the first power. True, the constants which enter into the resulting laws will be thus rendered independent of the wave length λ, and hence we must give up the expression of the dispersion; we obtain only an approximation to the phænomena displayed by homogeneous light. In order, then, to bring the analysis into harmony with the results of experience, let us express the constants which enter into the formulæ; that is to say, the principal indices of refraction for a certain colour, by the values obtained from measurement. With this procedure we must rest satisfied; the results, indeed, almost completely correspond to the exactitude of our observations.

We develope, therefore, in the expressions for the coefficients of the ellipsoid of polarization, the sinus function in its equivalent series; and neglecting the members which, in respect to Δx ... &c., are of a higher order than the first, we obtain

$$L=2m\frac{\pi^2}{\lambda^2}\Sigma\left\{\frac{f}{\Delta r}+\frac{\Delta x^2\phi}{\Delta r^3}\right\}\{u\Delta x+v\Delta y+w\Delta z\}^2 \text{ \&c.}$$

$$P=2m\frac{\pi^2}{\lambda^2}\Sigma\frac{\phi}{\Delta r^3}.\Delta y.\Delta z\{u\Delta x+v\Delta y+w\Delta z\}^2 \text{ \&c.}$$

The most natural way of presenting a symmetrical medium possessing two axes to the mind, that is, a medium which is built symmetrically as regards three principal sections which stand perpendicular to each other, is that in it the particles in three groups of parallel lines, which stand perpendicular to the three sections respectively, are at equal distances from each other. If we suffer the axes of our system of coordinates to run parallel with the normals to the principal sections, with the so-called principal axes of the medium, and denote the distance between two neighbouring particles in the direction of these axes by δx, δy, δz, then the coordinates of a particle, according to this method of representation, will be

$$\Delta x=m.\delta x,\quad \Delta y=n.\delta y,\quad \Delta z=p.\delta z,$$

where m, n, and p denote whole numbers.

In a medium characterized as we have supposed, the particles whose coordinates possess the same absolute value arrange themselves by eights which lie in the corners of a parallelopiped, the centre of which coincides with the origin of coordinates, and the edges of which run parallel with the axes of coordinates. For every such eight particles the sum of the members

$$\frac{f}{\Delta r}.\Delta x^a.\Delta y^b.\Delta z^c \text{ and } \frac{\phi}{\Delta r^3}.\Delta x^a.\Delta y^b.\Delta z^c$$

is evidently equal to zero when one of the exponents, a, b or c, is an odd number; thus in this case we obtain generally

$$\Sigma\frac{f}{\Delta r}.\Delta x^a.\Delta y^b.\Delta z^c=0,\quad \Sigma\frac{\phi}{\Delta r^3}.\Delta x^a.\Delta y^b.\Delta z\ =0\,;$$

and according to this, the coefficients for the ellipsoid of polarization passes into the following:

$$L=2m.\frac{\pi^2}{\lambda^2}\Sigma\left\{\frac{f}{\Delta r}+\frac{\phi}{\Delta r^3}.\Delta x^2\right\}\{u^2\Delta x^2+v^2\Delta y^2+w^2\Delta z^2\}\text{\&c.}$$

$$P=2.2m.\frac{\pi^2}{\lambda^2}\Sigma\frac{\phi}{\Delta r^3}.\Delta y^2.\Delta z^2.vw \text{ \&c.}$$

For the sake of brevity let us set

$$\frac{1}{2}m\Sigma\frac{f}{\Delta r}.\Delta x^2=\xi^2,\quad \frac{1}{2}m\Sigma\frac{f}{\Delta r}.\Delta y^2=\eta^2,\quad \frac{1}{2}m\Sigma\frac{f}{\Delta r}.\Delta z^2=\zeta^2$$

$$\frac{1}{2}m\Sigma\frac{\phi}{\Delta r^3}.\Delta y^2\Delta z^2=\alpha^2,\ \frac{1}{2}m\Sigma\frac{\phi}{\Delta r^3}.\Delta x^2.\Delta z^2=\beta^2,\ \frac{1}{2}m\Sigma\frac{\phi}{\Delta r^3}.\Delta x^2\Delta y^2=\gamma^2$$

$$\frac{1}{2}m\Sigma\frac{\phi}{\Delta r^3}.\Delta x^4=p^2,\quad \frac{1}{2}m\Sigma\frac{\phi}{\Delta r^3}.\Delta y^4=q^2,\quad \frac{1}{2}m\Sigma\frac{\phi}{\Delta r^3}.\Delta z^4=r^2\,;$$

we thus obtain for the equation of the ellipsoid of polarization the following:

$$
\begin{aligned}
&x^2\{u^2(\xi^2+p^2)+v^2(\eta^2+\gamma^2)+w^2(\zeta^2+\beta^2)\}\\
&+y^2\{v^2(\eta^2+q^2)+u^2(\xi^2+\gamma^2)+w^2(\zeta^2+\alpha^2)\}\\
&+z^2\{w^2(\zeta^2+r^2)+u^2(\xi^2+\beta^2)+v^2(\eta^2+\alpha^2)\}\\
&+4.yz.\alpha^2.vw+4.xz.\beta^2.uw+4.xy.\gamma^2.uv=\left(\frac{\lambda}{2\pi}\right)^2.
\end{aligned}
$$

If, in the first place, the normal to the wave coincides with the x axis, that is, if $v=0$, $w=0$, $u=1$, the equation of the ellipsoid of polarization will be

$$x^2(\xi^2+p^2)+y^2(\xi^2+\gamma^2)+z^2(\xi^2+\beta^2)=\left(\frac{\lambda}{2\pi}\right)^2.$$

The velocities of the vibrations which proceed parallel to the y axis and the z axis respectively will therefore be

$$\sqrt{\xi^2+\gamma^2} \text{ and } \sqrt{\xi^2+\beta^2}.$$

If, secondly, the plane of the waves stand perpendicular to the y axis, we obtain in a perfectly similar manner for the velocity of the vibrations which are parallel to the x axis and to the z axis respectively,

$$\sqrt{\eta^2+\gamma^2} \text{ and } \sqrt{\eta^2+\alpha^2}.$$

If, finally, the plane of the waves stand perpendicular to the z axis, the velocities of the oscillations which are parallel to the axes x and y respectively are

$$\sqrt{\zeta^2+\beta^2} \text{ and } \sqrt{\xi^2+\alpha^2}.$$

Experiment teaches, however, that a ray whose plane of polarization coincides with a principal section possesses one and the same velocity, whatever its direction may be in other respects; or, *if we assume that the plane of oscillation is perpendicular to the plane of polarization,* that oscillations which are parallel to a principal axis are propagated with equal velocities, whatever the direction of the plane of the waves in other respects may be. According to this, we are justified in assuming that we have

$$\eta^2+\gamma^2=\zeta^2+\beta^2, \quad \xi^2+\gamma^2=\zeta^2+\alpha^2, \quad \xi^2+\beta^2=\eta^2+\alpha^2 \text{ nearly.}$$

These relations, to the assumption of which we are equally entitled as to the assumptions regarding the connexion between the planes of polarization and vibration required by the opposite notion, reduce the equation of the ellipsoid of polarization to the following:

$$x^2\{u^2(\xi^2+p^2-\eta^2-\gamma^2)+\eta^2+\gamma^2\}+y^2\{v^2(\eta^2+q^2-\zeta^2-\alpha^2)+\zeta^2+\alpha^2\}$$
$$+z^2\{w^2(\zeta^2+r^2-\xi^2-\beta^2)+\xi^2+\beta^2\}$$
$$+4.\alpha^2vwyz+4\beta^2.uwxz+4\gamma^2.uvyx=\left(\frac{\lambda}{2\pi}\right)^2.$$

In the place of this ellipsoid of polarization let us substitute another, concentric, similar, and similarly situated to the former, but whose dimensions are to those of the first in the ratio of $\frac{\lambda}{2\pi}:1$. We thus obtain at once, in the reciprocal values of its semiaxes, the velocities of the conjugate plane waves. The equation of the second ellipsoid differs from that of the first only in the circumstance, that in the second the place of $\frac{\lambda}{2\pi}$ is taken by the unit. For the equation thus modified let us set, for the sake of shortness,

$$x^2\{Au^2+a\}+y^2\{Bv^2+b\}+z^2\{Cw^2+c\}$$
$$+2dvwyz+2euwxz+2fuvxy=1.$$

On experimental grounds it is to be assumed, that, of the three species of oscillations, which, according to the above, belong to a plane of undulation, there are two always parallel to the said plane, these two being within the limits of observation; while the third, which corresponds to an imperceptible motion of the æther, coincides with the normal to the wave. According to the theory, this is exactly the case in isotropic media; also in homogeneous media generally, when the wave falls in a principal section; and for vibrations parallel to an axis, when the plane of the waves is parallel to the said axis. From this we conclude, that we incur an imperceptible error, if, instead of the axes of the ellipsoid of polarization, which are nearly parallel with the plane of the waves, and which alone correspond to the motion which produces light, we make use of the axis of the diametral section D, which is parallel with the plane of the waves P. From the equation of the plane of this section, that is, from

$$ux+vy+wz=0,$$

and from the equation of the ellipsoid, we obtain for the projection of the section upon the plane of yz the equation

$$y^2\{(A+B-2f)u^2v^2+av^2+bu^2\}+z^2\{A+C-2e)u^2w^2+aw^2+cu^2\}$$
$$2yz\{(A+d-e-f)u^2+a\}vw=1.$$

The plane of the diametral section D intersects an ellipsoid E′, whose equation is

$$x^2a+y^2b+z^2c=1,$$

in an ellipse D′, for the projection of which on the plane yz we have

$$y^2\{av^2+bu^2\}+z^2\{aw^2+cu^2\}+2yz.avw=1.$$

Now the ellipses D and D′ coincide as often as the normal to the plane of the waves coincides with a principal axis. When the plane of the waves runs parallel with a principal axis, both these sections in the direction of the said axis possess axes of equal length. It seems, in fine, that for homogeneous media these two sections differ from each other only in an inappreciable degree; that is to say, for these media the following relations appear to be very nearly correct:

$$A+B-2f=0, \quad A+C-2e=0, \quad A+d-e-f=0.$$

In order to obtain the directions of vibration and the velocities for a given plane P, let the diametral section D′ of the ellipsoid E′ parallel with P be constructed. The directions of its axes are the directions of vibration, the planes of which are parallel to P, and the reciprocal values of its semiaxes furnish the corresponding velocities.

A surface whose rays are equal to the reciprocal values of the rays of the ellipsoid E′ will be represented by the equation

$$x^2.a+y^2.b+z^2.c=(x^2+y^2+z^2)^2.$$

This is the surface of construction of Fresnel, the axes of which coincide with the principal axes of the medium, and are equal to double the principal velocities,

$$\sqrt{a}=\sqrt{\eta^2+\gamma^2}=\sqrt{\zeta^2+\beta^2}, \quad \sqrt{b}=\sqrt{\zeta^2+\gamma^2}=\sqrt{\xi^2+\alpha^2}$$

and

$$\sqrt{c}=\sqrt{\xi^2+\beta^2}=\sqrt{\eta^2+\alpha^2}.$$

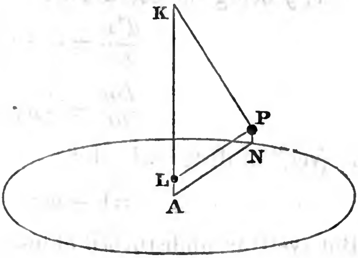
A diametral section thereof, the plane of which is parallel with P, furnishes in the directions of its semiaxes and the direct values of their lengths, the directions and velocities of the vibrations which belong to P; and this is the principle which forms the basis of the construction of Fresnel.

XLVI. *On the Motion of the Apse-Line in the Pendulum Oval. By the* Rev. J. A. Coombe, *M.A., late Fellow of St. John's College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The following method of investigating the motion of the apse-line in the pendulum experiment presents some peculiarities which may be interesting to some of your readers. It depends upon the principle of the Variations of Elements so beautifully applied by Lagrange to the planetary perturbations.



Let P be the ball of the pendulum; K the point of suspension; PN perpendicular to the table; PL parallel to AN.

Let $KP=l$, $KL=z$, $AN=r$, T the tension of the string KP. Then the force upon P resolved in the projection AN

$$=\mathrm{T}\cdot\frac{r}{l}\left\{g\cdot\frac{z}{l}+\frac{(\mathrm{vel})^2}{l}\right\},$$

and

$$(\mathrm{vel})^2=2gz+\mathrm{C} \text{ and } z=\sqrt{l^2-r^2};$$

∴ force to the centre A

$$=\left(3g+\frac{\mathrm{C}}{l}\right)\cdot\frac{r}{l}-\frac{3g}{2}\cdot\frac{r^3}{l^3},$$

omitting higher powers of r than the third; or the accelerating force to centre

$$\mathrm{A}=\mu r-\mu' r^3.$$

The second term may be looked upon as expressing a small disturbing force upon the first, which alone would cause the projection of P to move in an elliptic orbit about centre A. Hence by Lagrange's principle we may consider the motion as taking place in an ellipse *with variable elements*; so that if the disturbing force were at any moment to cease, the body would go on describing an ellipse about A as centre with elements corresponding to their value at that instant.

Let the equation to the instantaneous ellipse be

$$\frac{\cos^2(\theta-\varpi)}{a^2}+\frac{\sin^2(\theta-\varpi)}{b^2}=\frac{1}{r^2}.$$

Then we have, by the theory,

$$0=\frac{dr}{da}\cdot\frac{da}{dt}+\frac{dr}{db}\cdot\frac{db}{dt}+\frac{dr}{d\varpi}\cdot\frac{d\varpi}{dt}\cdot\quad\cdot\quad\cdot\quad\cdot\qquad(1.)$$

But

$$\frac{1}{r^3}\cdot\frac{dr}{da}=\frac{\cos^2\overline{\theta-\varpi}}{a^3};\quad\frac{1}{r^3}\cdot\frac{dr}{db}=\frac{\sin^2\overline{\theta-\varpi}}{b^3}$$

and

$$\frac{1}{r^3}\cdot\frac{dr}{d\varpi}=\sin\overline{\theta-\varpi}.\cos\overline{\theta-\varpi}.\left(\frac{1}{b^2}-\frac{1}{a^2}\right).$$

Also to find $\frac{da}{dt}$ and $\frac{db}{dt}$, we have the equations of motion about A, x, y being the coordinates of N,

$$\left.\begin{aligned}\frac{d^2x}{dt^2}&=-\mu x+\mu' r^3.\frac{x}{r}\\ \frac{d^2y}{dt^2}&=-\mu y+\mu' r^3.\frac{y}{r}\end{aligned}\right\};$$

$\therefore$ (vel)^2 in disturbed orbit

$$=\text{C}-\mu r^2+\frac{\mu' r^4}{2}.$$

But (vel)^2 in undisturbed ellipse

$$=\mu(a^2+b^2-r^2).$$

And these by the theory are equal;

$$\therefore \mu(a^2+b^2)=\text{C}+\frac{\mu' r^4}{2};$$

$$\therefore \mu\left(a\frac{da}{dt}+b\frac{db}{dt}\right)=\mu' r^3.\frac{dr}{dt}. \quad . \quad . \quad . \quad (2.)$$

Also, from the equations of motion,

$$\frac{d.\left(x\frac{dy}{dt}-y.\frac{dx}{dt}\right)}{dt}=0;$$

or

$$\frac{d.(ab)}{dt}=0,$$

$$\therefore a\frac{db}{dt}+b\frac{da}{dt}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

Hence from (2.) and (3.) we have

$$\frac{da}{dt}=\frac{\mu' ar^3}{\mu(a^2-b^2)}.\frac{dr}{dt}: \quad \frac{db}{dt}=-\frac{\mu' br^3}{\mu(a^2-b^2)}.\frac{dr}{dt}.$$

Hence substituting in (1.), and calling $\theta-\varpi=\phi$, we have, remembering that

$$\frac{dr}{dt}=-r^3\sin\phi\cos\phi.\left(\frac{1}{a^2}-\frac{1}{b^2}\right).\frac{d\phi}{dt},$$

$$\delta\varpi=\frac{\mu'}{\mu(a^2-b^2)}.r^6\left\{\frac{\cos^2\phi}{a^2}-\frac{\sin^2\phi}{b^2}\right\}.\delta\phi;$$

$$\therefore \Delta\varpi=\frac{\mu'}{\mu(a^2-b^2)}\,{}^{0}\!\!\int_{\phi}^{\frac{\pi}{2}} r^6\left\{\frac{\cos^2\phi}{a^2}-\frac{\sin^2\phi}{b^2}\right\}.$$

This is at once integrable by making the usual assumptions,

$$r\cos\phi=a\cos\psi; \quad r\sin\phi=b\sin\psi,$$

and

$$\Delta\varpi=\frac{\mu' ab}{\mu(a^2-b^2)}\int_{\psi}^{0}{}^{\frac{\pi}{2}}(\cos^2\psi-\sin^2\psi)(a^2\cos^2\psi+b^2\sin^2\psi)$$

$$=\frac{\mu' ab}{\mu(a^2-b^2)}\left\{(a^2-b^2)\cdot\frac{3\,.\,1}{4\,.\,2}\cdot\frac{\pi}{2}-(a^2-b^2)\left(\frac{1}{2}\cdot\frac{\pi}{2}-\frac{3}{4.2}\cdot\frac{\pi}{2}\right)\right\},$$

or

$$\Delta\varpi=\frac{\mu' ab}{\mu}\cdot\frac{\pi}{8}.$$

Hence in one complete revolution in the oval, or in a double vibration of the pendulum, the progress of the apse-line

$$=\frac{\mu' ab}{\mu}\cdot\frac{\pi}{2}.$$

But it is evident from the equations for the motion of the pendulum, that

$$\mu=\frac{g}{l}\text{ (approximately)},$$

and

$$\mu'=\frac{3g}{2l^3}.$$

Hence we arrive at the final result, that the apse-line will make a complete revolution in time $\frac{8}{3}\cdot\frac{l^2}{a\,.\,b}$ multiplied by the time of a double vibration.

This is the same conclusion as that arrived at by the Astronomer Royal in a paper read before the Astronomical Society.

Believe me, Gentlemen,

Yours faithfully,

J. A. Coombe.

Alburgh Rectory, Norfolk,
Sept. 24, 1851.

XLVII. *Account of Experiments demonstrating a limit to the Magnetizability of Iron.* *By* J. P. Joule, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SEVERAL years ago I published some experiments on electro-magnetic attraction, by which I confirmed the law of Lenz and Jacobi, discovered about the same time, viz. that the magnetism induced in a soft iron bar is proportional to the electro-magnetic force of the exciting coils; but I was led to the conclusion that this law fails when very high degrees of magnetization are attained, and that, indeed, there is a limit beyond which it is impossible to increase the intensity of the magnetic virtue in a bar of soft iron. This fact, which, although at variance with the views of Poisson, has been arrived at *à priori* by Prof. Thomson from theoretical considerations*, has been confirmed by the important researches of Feilitzsch†, Gartenhauser, and especially of Müller‡, whose numerous experiments appear to have been made with very great care, and to have dispelled all doubt as to the existence of such a limit. The interest which these comparatively recent researches have so justly excited has encouraged me to hope that you will consent to reprint a few short papers, in which the discoveries of the above-named philosophers are confirmed, but which have hitherto only received publication in a periodical which seems not to have been widely circulated. I have made a few alterations, but with a view to explain, not in the smallest degree to alter the meaning of the original.

I have the honour to remain, Gentlemen,

Yours very respectfully,

J. P. Joule.

On the Use of Electro-magnets made of Iron Wire for the Electro-magnetic Engine. By J. P. Joule, *Esq. Communicated in a Letter to the late* Mr. Sturgeon§.

Dear Sir, Salford, March 27, 1839.

In my last letter I gave you an account of some experiments which were intended to prove that electro-magnets made of iron wire are the most suitable for the electro-magnetic engine. In those experiments round wire was used; and it was my opinion that the wire magnets were put in a disadvantageous position, in consequence of the interstices between the wires. I have since confirmed my views on this subject by the following experiment:—

I constructed two magnets. The first consisted of sixteen pieces of square iron wire, each $\frac{1}{11}$th of an inch square and 7 inches long, bound very tightly together so as to form a solid mass, whose transverse section was $\frac{4}{11}$ths of an inch square; it was enveloped by a ribbon of cotton, and wound with sixteen feet of covered copper wire, of $\frac{1}{16}$th inch diameter. The second was made of solid iron, but was in every other respect precisely like the first. These magnets were fitted to the apparatus used in my former experiments, and care was taken to make the friction of the pivots equal in each. The mean of several experi-

* Phil. Mag., vol. xxxvii. p. 252.

† "Electromagnetismus in weiches Eisen und über den Sättigungspunkt derselben."—Poggendorff's *Annalen*, 1850.

‡ Ueber den Sättigungspunkt der Electromagnetismus von J. Müller in Freiburg.—Pogg. *Ann.*, Feb. 1851, vol. lxxxii. p. 181.

§ Annals of Electricity, vol. iv. p. 58.

ments gave 162 revolutions per minute with the first, and 130 with the second magnet.

In the further prosecution of my inquiries, I took six pieces of round iron of different diameters and lengths, and also a piece of hollow round iron, half an inch in diameter, and $\frac{1}{13}$th of an inch thick in metal; these were bent into the U-form, so that the shortest distance between the poles of each was half an inch; each was then wound (with the usual precautions to ensure insulation) with ten feet of covered copper wire of $\frac{1}{40}$th inch diameter. The lengths and diameters are given in the following table. No. 1 is the hollow magnet. The attraction was ascertained by suspending a straight steel magnet, $1\frac{1}{2}$ inch in length, horizontally to the beam of a balance, and bringing the several electro-magnets directly underneath at the distance of half an inch, which was preserved by the interposition of a piece of wood half an inch thick. Care was taken that the battery remained constant during the experiments.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.
Length in inches.........	6	$5\frac{1}{2}$	$2\frac{2}{3}$	$5\frac{1}{4}$	$2\frac{1}{2}$	$5\frac{1}{4}$	$2\frac{1}{4}$
Diameter in inches......	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{3}{8}$	$\frac{1}{4}$	$\frac{1}{4}$
Weight lifted in ounces	36	52	92	36	52	20	28
Attraction for steel magnet in grains...	7·5	6·3	5·1	5·0	4·1	4·8	3·6

A steel magnet of such dimensions as enabled me to compare it fairly with the electro-magnets, was found to exert an attraction of 23 grains for the small steel magnet, though its lifting power was only 60 oz.

These results will not appear surprising if we consider, first, the resistance which iron presents to the induction of magnetism; and secondly, how very much the power of iron to conduct magnetism is exalted merely by the completion of the ferruginous circuit. In order, however, to explain why the long electro-magnets have a *greater* attracting power at a distance, though they lift *less* weight, than the short magnets of the same diameter, it will be necessary to observe that it was impossible to wrap the whole ten feet of wire on the smaller magnets, without disposing it in two or three layers (according to the size of the magnets). This was a great disadvantage; and one might have anticipated in consequence, that the power of the long magnets would be greater than that of the short ones for lifting, as well as distant attraction, which is contrary to the results of the table; this may however be explained, if we admit that the comparative resistance of the iron of the electro-magnet increases to a very great amount, when its magnetism is so greatly excited as by the contact of the armature.

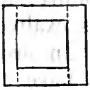
Nothing can be more striking than the difference between the ratios of lifting to distant attractive power, in the different magnets; whilst the steel magnet attracts with a force of 23 grains and lifts 60 oz., No. 3 attracts 5·1 grains and lifts 92 oz.

The following are some general directions for making electro-magnets for lifting:—1st. The magnet, if of considerable bulk, should be compound, and the iron used of good quality and well annealed. 2nd. The bulk of the iron should bear a much greater ratio to its length than is generally the case. 3rd. The poles should be ground quite true, and fit flatly and accurately to the armature. And 4th. The armature should be equal in thickness to the iron of the magnet.

I shall now proceed to consider with greater care what form of electro-magnet is best for distant attraction, as that is the only force of any use in the electro-magnetic engine. Here two things must be considered; the length of the iron, and its sectional area.

Now with regard to the length of the iron, I have found that its increase is always accompanied with disadvantage, unless the wire is (by using a shorter length) forced to too great a distance from the iron. In making magnets for an engine, it will be proper to use a length less than that which gives the maximum of attraction, on several accounts.

The next thing to be considered is the sectional area. You have shown* that, on placing a hollow and solid cylinder of iron successively within the same electro-magnetic coil, the hollow piece exerted the greatest influence on the needle. I wished to ascertain whether a hollow magnet could be represented by a solid one, of which the sectional area and circumference are the same, and the thickness of which is twice that of the hollow magnet. The accompanying figures represent sections of hollow and solid rectangular magnets; and it will be seen, that if either of them is divided at the dotted lines, the separate pieces, when put properly together, will make up the other. Two electro-magnets were constructed, each 7 inches long, and wound with twenty-two feet of insulated copper wire; the sections were similar to, but twice the size of the figures. Their attractions at half an inch distance for the contrary pole of a straight steel magnet were as follow:—



	Hollow magnet.	Solid magnet.
Attraction in grains	1·9	1·7
Do. with a more powerful battery .	4·5	4·0

The above results show that the hollow magnet has the greater

* Annals of Electricity, vol. i. p. 470.

attractive force; but I do not think that the difference between the two is so great as to counterbalance the practical advantages which solid bars would give if used in the engine. I shall now therefore attempt to determine the sectional area of solid iron most proper for various galvanic powers.

I made five straight electro-magnets of square iron wire $\frac{1}{11}$th of an inch thick; each was 7 inches long, and wound with twenty-two feet of insulated copper wire of $\frac{1}{16}$th of an inch diameter. No. 1 consisted of nine, No. 2 of sixteen, No. 3 of twenty-five, No. 4 of thirty-six, and No. 5 of forty-nine square iron wires, arranged in the form of square prisms. Five other electro-magnets were made of square iron rod, but in every other respect were exactly similar to the first. The following are the attractions (at half an inch distance) for a straight steel magnet, with three different voltaic forces.

		No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
1st experiment.	Attraction of iron bar magnet in grains...	1·5	1·9	1·6	2·1	2·0
	Ditto of wire magnet...	2·1	2·1	1·7	2·0	1·9
2nd experiment.	Iron bar magnet	2·0	2·5	2·35	2·45	2·2
	Wire magnet	2·6	2·8	2·1	2·2	2·05
3rd experiment.	Iron bar magnet.........	2·7	3·6	3·4	3·2	3·1
	Wire magnet	3·3	3·8	3·0	2·9	2·65

The square iron wire of which the wire magnets were constructed, was taken at the same degree of temper that it possessed when it came from the manufacturer. It was in consequence not so well annealed as the iron bars. On this account the numbers opposite the wire magnets are less than they would have been with better annealed wire: still the results of the table seem anomalous; for it will be remarked, that whilst the wire magnets are the most powerful of the smaller electro-magnets, the bar magnets are most powerful of the larger ones.

* * * * *

I remain, &c.,

J. P. Joule.

Investigations in Magnetism and Electro-Magnetism. By James P. Joule. *In two Letters to the late* Mr. Sturgeon*.

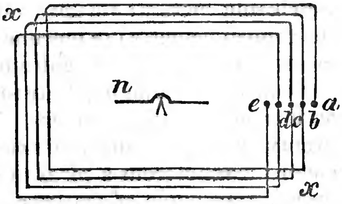
Broom Hill, near Manchester,
May 28, 1839.

Dear Sir,

I am now able to send you an account of my further investigations on electro-magnetic attraction. It was judged to be a matter of the first importance, in a research like the following,

* Annals of Electricity, vol. iv. p. 131.

to use a galvanometer the indications of which might be depended upon.



The accompanying figure represents the form of galvanometer I have employed. The needle *n* is 2 inches long. The wire is 10 feet long, and $\frac{1}{16}$th of an inch in diameter: it is disposed in four circumvolutions, mercury cups being placed at the parts marked *a, b, c, d, e*. The coils cross one another at *x, x*, but in every other part they are in the same plane. By this contrivance the forces of the several coils are made equal to one another, or, as it would seem, not perceptibly different.

The process of graduation was conducted in the following manner:—The electricity of a constant battery was first passed through each of the coils in succession, and the deflection of the needle was observed to be the same in each case. A current of a certain intensity was then passed from *a* to *b*, *a* to *c*, *a* to *d*, and *a* to *e*, taking care to decrease the resistance of the battery wires in proportion as the length of that part of the galvanometer wire through which the current passed was increased, and I marked the several deflections of the needle on the card of the instrument 1, 2, 3, 4. I then increased the power of the battery until the needle stood at the mark 2, when the current passed from *a* to *b*; the former process was then repeated, and I thus observed the quantities 2, 4, 6 and 8; and going on in the same manner, I had the card of the galvanometer marked with the numbers 1, 2, 3, 4, 6, 8, 9, 12, 16, &c. In using the galvanometer the current is passed from *a* to *b*, and the above numbers indicate absolute quantities of current electricity.

In order to obtain a definite idea of the quantities indicated by my galvanometer, I took a diluted acid, consisting of ten parts water and one of sulphuric acid, spec. grav. 1·8, and passed through it a current which deflected the needle to the mark 1. In seven minutes 0·62 of a cubic inch of the mixed gases was produced. The electrodes employed in the above experiment were pointed platina wires 1·1 inch asunder.

The electro-magnets used first were those described in my last communication. They are straight and square, 7 inches long, and wound with twenty-two feet of copper wire $\frac{1}{16}$th of an inch in diameter. Five of them were constructed of bar iron, and five corresponding ones of square iron wire. The sections of those marked No. I. are $\frac{3}{11}$ths of an inch square; a dimension which is successively increased in Nos. II. III. and IV. until No. V., which are $\frac{7}{11}$ths of an inch square.

The bar electro-magnets were suspended in succession, vertically, from the beam of a balance; the corresponding wire electro-magnets being brought vertically underneath, so that $\frac{1}{8}$th of an inch intervened between the poles of the two magnets. Electrical currents of the quantities exhibited in the table were passed through the continuous line of conductors presented by the galvanometer and electro-magnetic coils. The attraction was measured in grains by ascertaining the weight required to raise the suspended electro-magnet from a piece of wood $\frac{1}{8}$th of an inch thick, placed on the upper pole of the lower electro-magnet.

Table I.

Quantities of current electricity.	No. I.	No. II.	No. III.	No. IV.	No. V.
6	76	65	88	62	42
8	133	100	180	103	98
12	258	296	300	286	206
16	500	548	530	550	410
24	1080	1280	1190	1210	1050

In order to vary the above experiments, and with a view to ascertain what effect an increase of length would produce, I constructed ten more electro-magnets of the same sectional areas, but 14 inches long, or double the former length, and wound with twenty-two yards, or three times the length of similar insulated wire to that previously used. Nos. VI. and VII. were made of square iron wire; the rest of bar iron.

Table II.

Quantities of current electricity.	No. VI.	No. VII.	No. VIII.	No. IX.	No. X.
8	410	667	1150	1205	1175
12	690	1170	2150	3025	2625
16	1000	1920	4575	5687	4675
24	1460	3500	9625	11812	10500

Every one of the magnets used in the above series of experiments, except Nos. VI. and VII., was wound to two thicknesses by the wire; and in the large ones the iron was left uncovered at equal intervals. I must mention, however, that Nos. VI. and VII. had to be wound to three thicknesses in some parts on account of their small size. On this account the magnetic attractions of these two pairs were somewhat deteriorated.

It does not appear from the experiments that any great loss of power arises from an increase of the length of the magnets. It is plain, that, as the magnets in Table II. are wound with three times the length of wire, twenty-four of current electricity in the first table should have the same effect as eight in the second table. The difference, if any, should be due to the increased length of the iron. I do not think myself justified in assigning any amount to this difference, which, however, seems to increase in value as the section of the magnets decreases. In order to determine this and many other circumstances of great interest, it would be necessary to conduct experiments in a much more comprehensive manner, and to examine more minutely into the various powers of hard and soft iron and iron wire.

I think, however, that I have by these experiments discovered a most important law, namely, *The attractive force of the electro-magnet is directly as the square of the electric force to which its iron is exposed; or if* E *denote the quantity of electricity,* M *the magnetic attraction, and* W *the length of wire,* $M = E^2W^2$*.

It must be confessed that there are many instances in the above tables which seem to form exceptions to this law. I consider, however, that the effects of magnetic inertia, and sources of error which I have found it impossible to avoid, are sufficient to account for these. Perhaps the fairest way of comparing the law with experiment is, to take the mean of the magnetic attractions of all the magnets in the first table, and the mean of Nos. VIII., IX. and X. in the second table, omitting Nos. VI. and VII., because it is clear that they are at last becoming saturated with magnetism. The means of the attractions observed, and the estimated results, are tabulated below.

From the 1st table.			From the 2nd table.	
Electric current.	Experiment.	Calculation.	Experiment.	Calculation.
6	66·4	66·4		
8	123	118	1177	1177
12	269	265	2600	2648
16	508	472	4979	4708
24	1163	1063	10646	10593

Anxious to ascertain whether the law obtained in lifting as well as in distant attraction, I made the following rough experiment with a horse-shoe electro-magnet made of a cylinder of iron, 7 inches long, and $\frac{5}{8}$ths of an inch in diameter, and wound with five yards of thick copper wire. The law seems in this case to fail principally because the iron is sooner saturated with magnetism; hence the propriety of making electro-magnets for lifting of considerable bulk.

* Jacobi and Lenz communicated their report on magnetic attraction to the Academy of St. Petersburgh in March 1839, or two months previously to the date of this paper. In it they announced a law similar to the above. —Note, May 1851.

Quantities of current electricity.	Lifting power in lbs.	Calculated power.
4	3·5	3·5
6	6·5	8·0
8	11·5	14·0
12	21·0	31·5

* * * * *

I remain, dear Sir,

Yours truly,

J. P. Joule.

Broom Hill, near Manchester,
July 10, 1839.

Dear Sir,

The following experiments were designed to test still further the law given in my last letter. Two pairs of electro-magnets were constructed; each of the first pair was made of a bar of iron, 30 inches long and 1 inch square; each of the second pair was made of a bar of iron 30 inches long, 2 inches broad, and 1 inch thick. The sharp edges were ground down to prevent inconvenience in the winding of the wire. Each magnet was properly insulated, and wound with eighty-eight yards of covered copper wire $\frac{1}{16}$th of an inch in diameter.

The attractions were measured in precisely the same manner as before, saving the substitution of copper for wood, to keep the magnets at the proper distance apart. The attraction of the suspended magnet for the fixed one was measured in ounces avoirdupois.

	Quantities of current electricity.		6.	8.	12.	16.	24.	32.
First pair.	Attraction at $\frac{1}{8}$ inch.	Experiment	18	33	72	124	260	
		Theory......	18	32	72	128	288	
	Attraction at $\frac{1}{4}$ inch.	Experiment	7	13	28	47	96	
		Theory......	7	12·44	28	49·7	112	
	Attraction at $\frac{1}{2}$ inch.	Experiment	3	5·25	12	18	38	62
		Theory......	3	5·33	12	21·3	48	85·3
Second pair.	Attraction at $\frac{1}{8}$ inch.	Experiment	14	27	60	100	240	
		Theory......	14	25	56	100	224	
	Attraction at $\frac{1}{4}$ inch.	Experiment	6·25	12	25	40	96	
		Theory......	6·25	11·1	25	44·4	100	
	Attraction at $\frac{1}{2}$ inch.	Experiment	2·5	5·0	9·5	17·5	36	
		Theory......	2·5	4·44	10	17·7	40	

The experimental results are quite as near to the theoretical as could be expected, considering the several sources of error. Those belonging to the first pair are particularly satisfactory, especially if, with regard to the numbers under 16, 24 and 32, we make some allowance for the approaching saturation of the iron.

I inferred from the experiments detailed in my last letter, that little difference of attractive power would result from the mere alteration of the shape of the sectional area of the iron of an electro-magnet; that view is confirmed by the experiments just related, in which it will be seen that little difference exists between the magnetic powers of the first and second pairs; and even that difference may be partly accounted for by taking into account the difficulty of winding the covered wire closely to the surface of broad rectangular iron bars.

The above magnets were wound to two thicknesses by the covered wire, and in other respects were similar to those I before used. The effect arising from increase of length may therefore be estimated. These magnets, which were 30 inches long, wound with eighty-eight yards of wire, and excited by a current of 6, sustained a weight of 7000 grains at the mean distance of $\frac{1}{8}$th of an inch; whilst the attractive power of the pairs marked VIII., IX. and X., in my last, with the same electro-magnetic force, or twenty-two yards of wire, and a current of 24, was 10646 grains.

* * * * *

I remain, dear Sir,

Yours most respectfully,

J. P. Joule.

XLVIII. *Notices respecting New Books.*

L'Algèbre d'Omar Alkhayyâmî. Par F. Woepcke. Paris, 1851. 8vo.

THERE is an old tradition that among the Arabic manuscripts bequeathed by Walter Warner to the University of Leyden, was one which treated of the algebraical solution of *cubic equations*. In 1834 M. Sédillot discovered a manuscript fragment in the Royal Library at Paris, which, it seemed probable, was part of the same work; of this he published some account (*N. Jo. Asiat.*, May 1834; *Not. et ext. des MSS. de la Bibl. R.*, vol. xiii. pp. 130–136). M. Libri afterwards found a complete manuscript in the Royal Library (*Histoire*, &c., vol. i. p. 300). M. Woepcke has now published this work, with the assistance of the fragment and the complete manuscript just noted, and also the manuscript of the Leyden library itself.

As noted by M. Libri, the work does not contain the solution of cubic equations, but only their geometrical construction, by aid of the conic sections. But though the tradition which we have mentioned imposes upon us this depreciatory kind of description, it is not the less to be noted that what we really have obliges us to form a much higher idea of the Arabian algebra than could have been gathered from our own historians. Up to the time (1831) when Dr. Rosen published his edition and translation of Mohammed Ben Musa, we knew nothing of the Arab writers except in fragments. Dr. Rosen's contribution (which is also that of Mr. Warburton) showed us the manner in which, and the writer by whom, the Indian science was made Arabian. M. Woepcke lets us into the secret of the progress made by the Arabs themselves; and a comparison of the writings of the introducer and of the improver will show that the science had made remarkable advances. This comparison, however, we must leave to the interested reader, on account of the space it would require.

M. Woepcke gives the Arabic text, a French version, a running paraphrase in modern language, and various additions and notes. He has earned a right to the gratitude of all who take interest in watching the progress of science, by the able manner in which he has performed every part of the task of which we can judge. We say this that we may not be supposed to give an opinion of the translation; this we are compelled to leave to those who know how to decipher the methodical scratches and scrawls which we fully believe, upon testimony, to represent Arabic words.

Omar Alkayyâmî was alive in A.D. 1079: Mohammed Ben Musa lived two centuries before him.

Photogenic Manipulation. *By* ROBERT J. BINGHAM. Eighth Edition. Knight, Foster Lane.

This little work contains a short account of the theory of the chemical action of light, with simple directions for producing photographic pictures by different agents, bringing down the descriptions to the most recent improvements at the time of publication. Among these the production of images upon glass plates naturally assumes a very prominent place, forming, as it does, in practice a marked step in advance, from the extreme beauty and minuteness of detail that can be attained by the employment of the transparent medium, rendering photography still further available for scientific purposes.

XLIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 246.]

May 1, 1851.—The Earl of Rosse, President, in the Chair.

A PAPER was read, entitled "An account of two cases in which an Ovule, or its remains, was discovered after death in the Fallopian tube of the unimpregnated human female, during the period of Menstruation." By H. Letheby, M.B., Lecturer on Chemistry and Medical Jurisprudence in the Medical School of the London Hospital. Received Feb. 20, 1851.

At the commencement of the paper the author refers to the opinions of Drs. Power, Lee, Paterson, Barry, Girdwood, and Wharton

Jones of this country, and also to those of MM. Valentin, Negrier, Pouchet, Gendrin, Raciborski, and Bischoff on the continent, respecting the supposed nature of the physiological phenomena manifested during the period of menstruation; and he mentions the law of Bischoff, namely, that "the ova formed in the ovaries of the females of all mammiferous animals, including the human female, undergo a periodical maturation and exclusion quite independently of the influence of the male seminal fluid. At these periods, known as those of 'heat' or 'the rut' in quadrupeds, and 'menstruation' in the human female, the ova which have become mature, disengage themselves from the ovary and are extruded. If the union of the sexes takes place at this period, the ovum is fecundated by the direct action of the semen upon it, but if no union of the sexes occurs, the ovum is nevertheless evolved from the ovary, and enters the Fallopian tube where it perishes." He states, however, that the arguments which have been advanced in support of this opinion, in respect of the human female, are entirely of an analogical character; and that although the ovaries of women who have died during the menstrual period have been frequently examined, and Graafian follicles found in a recently ruptured state, yet the discovery of the liberated ovule had not, so far as the author was aware, ever been detected. The importance of his cases rests upon three grounds, namely,—1st, the circumstances under which the women had died; 2ndly, the finding of recently ruptured Graafian follicles; and 3rdly, the discovery of the ovule and its remains in the fluid matter of the Fallopian tubes.

In the first of the cases recorded, the woman died during a menstrual period. She had been an inmate of the London Hospital for twenty-four days before her death, where she was closely watched day and night by a nurse, in consequence of her having attempted self-destruction by cutting her throat twenty-nine days before her death.

An examination of the body showed that the pelvic viscera were much congested; that the uterus was considerably enlarged; that the vagina contained a sero-sanguineous fluid; and that the hymen was unruptured. The ovaries were covered with stellate fissures, or cicatrices; and at one part of the left organ there was a purple spot having a ragged hole in its centre. By means of an incision into the gland through this spot, it was found that the opening led into a small cavity which was surrounded at its lower part by a dense tissue, infiltrated with dark coagulated blood (reference was here made to the preparation which shows the cavity and its coagulum). After macerating in spirit for a short time, it was noticed that the clot consisted of four parts, which the author described.

In other parts of the ovary several false *corpora lutea*, in different stages of decline, were found. The Fallopian tubes were highly congested, and the cavities of the tubes were filled with a bloody mucus. The left one contained at about one inch from its fimbriated end, a small vesicular body, which was, in the author's opinion, an ovule; for it consisted of nucleated cells and oil-globules. The fluid matters of the uterus and Fallopian tubes were made up of blood-discs, cylindrical epithelium, granular corpuscles, and a few spindle-shaped bodies.

The second case was that of a girl who had died at St. Luke's Hospital, where the supervision of the patient was quite as strict as that in the last case. In this instance the anatomical features were precisely like the preceding. The right Fallopian tube contained a globular body similar to that found in the left on the former occasion. This globular body, on being crushed between two pieces of glass and examined under the microscope, was found to consist externally of a mass of nucleated cells, the remains of the *tunica granulosa*, and of a transparent ring, enclosing an opake granular mass, and a highly pellucid spot. The author considered that this body was the liberated ovule, and the influence of chemical reagents served to support his opinion.

An examination of the *corpora lutea* found in both cases, showed that they consisted of large granular corpuscles and oil-globules.

The conclusions arrived at by the author were as follows:—

1. That ovules escape from the ovaries of women during the period of menstruation; and that their escape is a spontaneous act, taking place quite independently of sexual intercourse.

2. That immediately before, or else consentaneous with, the escape of an ovule, the whole substance of the Graafian follicle becomes charged with effused blood; and that a sort of fatty degeneration of the effused matter soon afterwards takes place.

3. That the mere presence of a yellow body containing a clot in the ovary, is not by any means a certain sign of recent impregnation.

4. That a sanguineous fluid is poured out over the whole mucous tract of the generative system during the catamenial period.

5. That the results of the observation tend to confirm the opinions entertained by Wagner, Bischoff, Barry, and Wharton Jones, concerning the membranous nature of that portion of the ovule known as the *zona pellucida*.

6. That the oil-globules of the yelk are either enclosed in a distinct membrane, or else that a structureless solid material pervades the entire substance of the vitelline body, and so binds the several component elements of it together.

7. That the recognition of the germinal vesicle removes some doubts concerning its appearance and position in the germ-mass.

May 15.—The Earl of Rosse, President, in the Chair.

"Report of further Observations made upon the Tidal Streams of the English Channel and German Ocean, under the authority of the Admiralty, in 1849 and 1850." By Captain F. W. Beechey, R.N. Communicated by G. B. Airy, Esq., F.R.S. &c., Astronomer Royal. Received March 27, 1851.

This is the continuation of a report which the author made upon the tidal phenomena of the Irish Sea and English Channel in 1848. After detailing the manner in which the investigation had been conducted, and the great care which had been bestowed upon the observations, which are numerous, the author enters into an explanation of the whole system of tidal streams in the English Channel and North Sea, as deduced from these observations, and also as to what he considers to be the cause of the peculiar movement of the streams in these channels. He supposes, in conformity with Dr. Whewell's theory, a tide-wave to pass along the western shores of Europe, and to enter the English Channel and North Sea by opposite routes, and to arrive off the Texel and Lynn at the same tidal hour as the tide-wave in the English Channel arrives off the Start and Jersey. From these points there are thrown off branch or derivative waves, which differ materially both in dimensions and rate of travelling from the parent wave. These waves roll on towards the strait of Dover and there merge into each other and form a combined wave.

The effect of this wave upon the tidal establishments of the Channel had long been known; but its influence upon the streams of the Channel had never before been considered, nor had any observations upon them been systematically undertaken.

In arranging the plan of observation the author considered that, as the combined wave was common to both the English Channel and North Sea, the tidal streams of both these channels would be found to correspond in every important particular, and that the movement of the streams throughout the strait would be materially influenced, if not wholly governed, by the motion of the combined wave; that the time of this wave attaining its greatest altitude would thus afford a standard to which the turn of the streams throughout the Channel might be advantageously referred; and that there would be found in this Channel, as in the Irish Sea, which is equally under the influence of a combined wave, a stream which would turn nearly simultaneously throughout the strait with the times of high and low water on the shore at the point of combination or virtual head of the tide.

Accordingly the observations were conducted upon this plan, and all the movements of the stream were referred to the time of high water at Dover, which had been determined upon as the standard from its being situated nearly at the point where the combined wave is formed. It appeared from the intervals which this mode of comparison afforded, that whilst the water was *rising* at Dover, the stream of the channels on both sides ran *towards* that place; and on the contrary, in the *opposite* direction whilst the water was *falling* there; and that these streams pursued a steady course throughout the tide, and extended from a line joining the Texel and Lynn, in the North Sea, to a line joining the Start and Jersey in the English Channel. Beyond these limits the streams of the Channel were found to encounter those of the offing or parent wave, and to occasion the tides in those localities to partake of a rotatory character, revolving for the most part with the sun, and having scarcely any interval of slack water.

The line of meeting of these streams was found not to be a stationary line, neither in those parts where the Channel-stream encounters the offing stream, nor where the streams meet in the strait of Dover, but was found to shift from west to east as the tide rises and falls at Dover, beginning at Beachy Head and ending at the North Foreland; so that the space occupied by the Channel-stream always preserves the same dimensions, notwithstanding its limits extend over a distance of 360 miles. The strait of Dover was found never to have slack water throughout its whole extent at any time, as was the case in the other ports of the Channel, from which it differs in this respect; and the streams in this locality have in consequence been designated as those of the "Intermediate tide."

As the simultaneous turn of the stream throughout the Channel is a point of considerable interest and entirely new, the author takes considerable pains to point out the methods by which this important fact was ascertained, and refers to the observations kept on board the light vessels along the coast, and to others made at various important stations; and whenever any contradictory evidence appears, the cause of the discrepancy is inquired into and explained. It was found, for instance, that in a port of the North Sea, near the node referred to by Dr. Whewell, that there was a retardation of an hour in the turn of the stream; and, upon an investigation as to the cause of this delay, it is seen to be owing to the stream running round the Texel and entering the North Sea at a time when the Channel-stream had ceased; but as soon as the Channel-stream acquired sufficient strength, it speedily drove the Texel stream back and confined it to its proper limits. In the English Channel also a similar discrepancy is observable near the coast of France; but this also the author considers to be fully accounted for by causes incidental to that part of the Channel, and not to be of sufficient consequence to derogate from the character ascribed to the general motion of the water throughout the strait.

A reference is made to the erroneous opinions which have hitherto been entertained with respect to the motion of the streams of our channels; and the author concludes his paper by explaining his views as to the manner in which the turn of the stream is rendered simultaneous by the rapid rise of the combined wave in the centre of the strait, and expresses a hope that he has satisfactorily shown from the observations, that throughout the English Channel and North Sea the movement of the stream may safely be referred to a common standard. This, it is considered, will be of great importance to navigation; as thus the seaman's progress through these moving waters will be freed from the numerous and perplexing references he was before obliged to make, and which too often—and, it is to be feared, in many instances too fatally—caused the tides to be wholly disregarded. All uncertainty as to the effect of the stream will henceforward, it is expected, be obviated by a simple reference to a tide table.

The paper, which is accompanied by numerous plans and charts, forms a practical illustration of the tidal streams of straits, under the influence of a combined wave.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 149.]

May 9, 1851.—Some Views respecting the Source of Light, &c. By James Nasmyth, Esq., F.R.A.S.

"Impressed with the conviction that the progress of science has often been most importantly advanced by the setting forth of hypothetical views as to the nature of those causes which result in great phænomena, I am, for these reasons, induced to hazard and venture forth with some views on the subject of the nature of solar light, more especially in reference to the well-known but most remarkable phænomena, occurring in the case of stars of variable and transitory brightness, as also in reference to those wonderful results of geological research, namely, the unquestionable evidence of the existence of an arctic or glacial climate in regions where such cannot now naturally exist; thus giving evidence of the existence of a condition of climate, for the explanation of which we look in vain to any, at present, known cause.

"I must plead the fact of the existence of such wonderful phænomena as these alluded to as my apology for thus attempting to come forth with what, although they may appear crude, theoretical notions, yet may, as tending to direct increased attention to important phænomena, so lead in due time to the development of truth, and extend the present bounds of our knowledge of those mighty laws which are so mysteriously indicated by the existence of the phænomena in question, and with the evidences of which we are yet surrounded.

"A course of observations on the solar spots, and on the remarkable features which from time to time appear on the sun's surface, which I have examined with considerable assiduity for several years, had in the first place led me to entertain the following conclusion; namely, that whatever be the nature of solar light, its main source appears to result from an action induced on the *exterior surface* of the solar sphere,—a conclusion in which I doubt not all who have attentively pursued observations on the structure of the sun's surface will agree.

"Impressed with the correctness of this conclusion, I was led to consider whether we might not reasonably consider the true source of the latent element of light to reside, *not in the solar orb*, but in space itself; and that the grand function and duty of the sun was to act as an agent for the bringing forth into vivid existence its due portion of the illuminating or luciferous element, which element I suppose to be diffused throughout the boundless regions of space, and which in that case must be perfectly exhaustless.

"Assuming, therefore, that the sun's light is the result of some peculiar action by which it brings forth into *visible* existence the element of light, which I conceive to be latent in, and diffused throughout, space, we have but to imagine the existence of a very probable condition, namely, the *unequal* diffusion of this light-yielding element, to catch a glimpse of a reason why our sun may, in common with his solar brotherhood, in some portions of his vast stellar orbit, have passed, and may yet have to pass, through regions of space, in which the light-yielding element may either abound or be deficient, and so cause him to beam forth with increased splendour, or fade in brilliancy, just in proportion to the richness or poverty of this supposed light-yielding element as may occur in those regions of space through which our sun, in common with every stellar orb, has passed, is now passing, or is destined to pass, in following up their mighty orbits.

"Once admit that this light-yielding element resides in *space*, and that it is *not* equally diffused, we may then catch a glimpse of the cause of the variable and transitory brightness of stars, and more especially of those which have been known to beam forth with such extraordinary splendour, and have again so mysteriously faded away; many instances of which abound in historical record.

"Finally, in reference to such a state of change having come over our sun, as indicated by the existence of a glacial period, as is now placed beyond doubt by geological research, it appears to me no very wild stretch of analogy to suppose that in such former periods of the earth's history our sun may have passed through portions of his stellar orbit in which the light-yielding element was deficient, and in which case his brilliancy would have suffered the while, and an arctic climate in consequence spread from the poles towards the equator, and leave the record of such a condition in glacial handwriting on the everlasting walls of our mountain ravines, of which there is such abundant and unquestionable evidence. As before said, it is the existence of such facts as we have in stars of transitory brightness, and the above-named evidence of an arctic climate existing in what are now genial climates, that renders some adequate cause to be looked for. I have accordingly hazarded the preceding remarks as suggestive of a cause, in the hope that the subject may receive that attention which its deep interest entitles it to obtain.

"This view of the source of light, as respects the existence of the luciferous element throughout space, accords with the Mosaic account of creation, insofar as that light is described as having been created in the first instance *before the sun* was called forth."

Note by the Astronomer Royal.

"In an oral address to the Society, on 1849, December 14, an abstract of which is printed in the Monthly Notices, vol. x. No. 2, in describing the method of recording transits by the agency of a galvanic current, I ascribed certain steps of the invention to Dr. Locke and Professor Mitchell. I have lately been informed that the invention was also shared by Mr. Bond, Mr. Walker, and perhaps by other persons. I am desirous of explaining to the Society that the history, such as I gave it at that time, was founded upon the printed papers which had then reached me, and upon my correspondence with American friends; both necessarily imperfect sources of information; and that I had no wish to assert the claims of Messrs. Locke and Mitchell further than as they seemed to be implied in those documents, nor to express any opinion on the claims of others, either to the first idea or to the subsequent steps of the invention."

Description of the Apparatus for observing Transits, by means of a Galvanic Current, now used at the Observatory of Cambridge, U.S. By Mr. G. P. Bond *.

The apparatus exhibited to the Society, is the same which has been for some time past in use at the Harvard Observatory, U.S., and is the property of the United States Coast Survey. It consists of an electric break-circuit clock, a galvanic battery of a single Grove's cup, and the spring governor, by which a uniform motion is given to the cylinder carrying the paper.

The electric clock is of the form proposed by Mr. Bond. Though different in its object and construction, the effect produced is the same with that of the clock proposed by Professor Wheatstone, namely, the interruption of the galvanic circuit at intervals of a second. The pallets and the escapement wheel are insulated, both from the pendulum and from the other wheels. When the battery is in connexion, the circuit is broken by the pallet leaving the tooth of the wheel, and is restored at the instant of the beat of the clock, which is in fact the sound produced by the completion of the contact restoring the circuit; the passage of the current being through the pallet and the escapement wheel alone. With the exception of the connecting wires, and the insulation of some parts, the clock is like those in common use for astronomical purposes.

Two wires pass from the clock, one direct to the battery, and the other, through the break-circuit-key used by the observer, and through the recording magnet, back to the battery. The length of wire is of course immaterial.

The magnet, with a slight difference in the form of the armature, is the same with those used on Morse's telegraph lines in the United States. The armature carries a glass pen, supplied with ink from a small reservoir. Under this pen the paper revolves on which the records are made. The breaking of the circuit by the clock, every second, is marked by an offset made by the pen, and the breaking of the circuit by the observer, is similarly recorded between the second marks of the clock. The paper is wound upon a cylinder, as suggested by Mr. Saxton of Washington. Unless a motion perfectly uniform is given to the cylinder, the second marks at the end of an hour, instead of being arranged in regular straight lines upon the paper, will change their relative positions, and the record become so confused as to make it a most serious undertaking to read off the observations after they have been taken.

To give a uniform motion to this cylinder has been the chief obstacle in the way of the application of electro-magnetism to practical astronomy, so that it should be of general utility; for although very rude contrivances will illustrate the process, and even afford accurate results, the time required to interpret the record may be greater than that required to make the observations throughout by

* This is the substance of a lecture delivered by Mr. Bond, in which the whole *modus operandi* was clearly shown.

the old method, and the liability to errors in the minutes and seconds is increased. A saving in the quantity of recording surface was also requisite.

The apparatus invented at Cambridge for this object is called the spring governor. The train of wheels which communicates the motive power to the cylinder connects with a small fly-wheel. This fly is for supplying momentum, and holds no part in the regulation. Beyond this fly, reckoning from the cylinder, is a half-seconds pendulum, with a dead-beat escapement. The connexion between the escapement-wheel and the fly is through a short spring. The elasticity of this spring allows the motion of the escapement-wheel to be completely arrested at each vibration of the pendulum, while the momentum of the fly, acting for a small fraction of a second only on the spring, keeps up the motion of the cylinder. The machinery is thus completely under the control of the pendulum. No accumulation of irregularity can take place beyond the limits of the bending and unbending of the connecting spring. After this is adjusted to its minimum, the continuous rotary motion will be performed with all the accuracy of the beats of the pendulum for any length of time. It is, in fact, a complete solution of the difficulty of producing exact uniform motion. An advantageous application of the same principle might be made to the clock-work for the equatoreal motion of telescopes.

The cylinder makes a single rotation in a minute. The second marks and the observations succeed each other in a continuous spiral. When a sheet is filled, and it is taken from the cylinder, the second marks and observations appear in parallel columns, as in a table of double entry, the minutes and seconds being the two arguments at the head and side of the sheet.

The observer, with the break-circuit-key in his hand or at his side, at the instant of the transit of a star over the wire of a telescope, touches the key with his finger. The record is made at the same instant on the paper. The operation may be repeated easily, at intervals between the successive transits, of one or two seconds each.

The experience we have now had places beyond doubt the fact, that, for convenience and accuracy of individual results, this new mode of observing is in advance of the old. The number of comparisons for differences of right ascension may be increased to an extent which distinguishes it, equally with its superior accuracy, as a real improvement in the science of practical astronomy. The extension of the method to the registration of differences of declination, simultaneously with differences of right ascension, promises great facility in taking zones of small stars.

Owing to the difficulty of obtaining precise information respecting scientific matters in America, considerable inaccuracies have crept into the *historical* part of the lecture given by the Astronomer Royal on the American method of observing by the electro-magnetic circuit (Phil. Mag. S. 3. vol. xxxvi. p. 142). The preceding note from the Astronomer Royal will prevent misconception on this point. But, setting aside the claims of individuals in this matter, so far as this is an American discovery, it is only under the auspices of the Department of the Coast Survey of the United States, and with the facilities and means furnished by its present enlightened superintendent, Dr. A. D. Bache, that the application of electro-magnetism to the purposes of geodesy and of astronomy has been successfully accomplished.

Extract of a Letter from Mr. Lassell.

"I have been very busy, and have brought to a most successful issue my efforts to support my two-foot speculum free from sensible flexure. All has gone on well and come right at once; and the speculum having been once placed in the tube, I have neither reason nor inclination to take it out again. I was pretty sanguine, yet must acknowledge the result has gone beyond my hopes. I announced the details of the plan to the British Association at Edinburgh *, and there is a clear and sufficient description of it in the Report just about publishing, or perhaps already out. I have scarcely varied at all in carrying it out. I have found 27 or 28 levers sufficient: and these are about as many as can be conveniently applied without interfering with the 18 discs and levers for zenithal support. Moreover, I have found cementing fulcral blocks of speculum metal upon the back with plaster of Paris quite efficient—firm enough to bear twice the requisite strain. Each lever, in a horizontal position of the tube, supports 15 lbs. of the speculum's weight; diminishing, of course, as the telescope approaches the zenith, where they are inactive. The superiority of action of the telescope since the application of this apparatus, I think none but myself who have seen it in both states can yet appreciate, and the atmosphere now alone remains my formidable and unconquerable foe, as it is indeed of all large apertures. So tenderly is the metal sustained in all positions, that no part of it can ever come into contact, with more than the pressure of a few pounds, against the tube or box in which it is placed. The plan seems to me applicable to specula of two or three times the diameter of mine with equal success. I was scarcely prepared to believe beforehand that the bending would follow so regular a law, as that it should be completely eliminated by a regularly devised system of counteracting support. I believe the application of the apparatus does not add more than 40 lbs. to that end of the tube which contains the speculum.

"Did I mention to you that I had (some time ago now) made an addition and improvement to the polishing machine by communicating a regular slow motion to the polisher? It has given me some trouble and looks complex, but it is efficient, and tends, I think, to greater uniformity of curve. But when I have leisure, I must describe it more fully."

Occultation of a Fixed Star by Jupiter. By the Rev. W. R. Dawes.

1851, May 8, 9^h G.M.T. Having turned my $8\frac{1}{2}$-foot refractor

* Reports of the Twentieth Meeting of the British Association, 1850; Notices and Abstracts, p. 180, &c. On a method of supporting a large speculum, free from sensible flexure, by Mr. Lassell, &c.

upon Jupiter, I instantly perceived a small star near his western edge, and observed its occultation with power 188. The disappearance occurred at $9^h\ 20^m\ 48^s \pm$ G.M.T.; the angle on the limb, measured from the planet's northern pole round by the eastern or following side, being about 250°. Jupiter was obscured by clouds at the time of the reappearance of the star, which is Bessel (Weisse) xii. 966. Mean place for 1825, R.A. $12^h\ 54^m\ 49^s{\cdot}21$, δ—4° 12′ 33″·2. It is of the 8th magnitude, according to Bessel.

The air was unfavourable, and the time noted is therefore uncertain to a few seconds.

On the evening of March 12, 1851, as the sun was setting in the midst of a thick haze, Mr. Weld observed a spot on the sun's disc with the naked eye. On pointing it out to one or two other persons, they saw it with facility. Next day he observed the sun with the equatoreal, and found a single large spot nearly round but somewhat angular. Its greatest measured diameter parallel to the equator was $4^s{\cdot}05$, that of the nucleus $1^s{\cdot}60$. Its diameter measured along the meridian circle was 52″·53.

L. *Intelligence and Miscellaneous Articles.*

ON THE PRODUCTION OF SUGAR IN THE LIVER OF MAN AND ANIMALS. BY CLAUDE BERNARD.

ALTHOUGH it has long been known that, under certain conditions, sugar may be found in the blood and other animal fluids, yet hitherto the presence of saccharine matters has always been considered as accidental, and dependent exclusively on the nature of the food. In the present note I shall demonstrate by the result of my experiments—

1. That the presence of sugar in the animal organism is a constant fact, and is indispensable for the regular accomplishment of the phænomena of nutrition.

2. I shall prove that the presence of sugar in the animal body is not dependent on the kind of food, but that sugar is formed in the liver by a special function of that organ.

3. I shall finally point out the principal characters of the production of sugar in the liver, showing that it is in immediate dependence on the nervous system.

1st. Of the presence of sugar in the organism. *During the period of digestion, the blood which issues from the liver by the hepatic veins (veines sus-hépatiques) invariably contains sugar, both in man and animals, whatever the nature of their food may be.*

The liver in most animals, and particularly in mammifera, is placed intermediately between the abdominal and the general circulation, so that the blood of the ventral vena portæ, returning from the spleen and intestines, must pass through the tissue of the liver before arriving at the heart. Now without attending for the present to the source of the sugar, I first establish the general facts:—1st, that it is by the hepatic veins, and them alone, that the sugar is conveyed into the general circulation; and 2nd, that when the hepatic veins carry sugar, the tissue of the liver is also saturated with it in a high proportion. No other organ of the body is in the same condition; so that the constant presence of the saccharine principle is distinctive of the tissue of the liver during digestion. These facts have been proved by a very great number of direct experiments, and confirmed in a variety of animals belonging to nearly every order of the zoological series. Without entering on details, I shall enumerate the species on which my investigations have been made.

On man, in the state of health, I have three times had the opportunity of ascertaining the presence of sugar in the liver; first on the body of an executed criminal; next in an individual killed accidentally by a gun-shot; and lastly, in a case of sudden death.

In the class Mammalia:—*Quadrumana*—the cynocephalous ape. *Carnivora*—dog, cat, hedgehog, mole. *Rodentia*—squirrel, guinea-pig, rabbit, hare, black rat, and brown rat. *Ruminantia*—goat, sheep, ox. *Pachydermata*—horse, ass, pig.

Birds. *Rapaces*—hawk, owl, strix and ulula. *Passeres*—swallow, sparrow, raven, lark. *Gallinaceæ*—pigeon, turkey, cock. *Grallæ*—snipe. *Palmipedes*—goose, duck.

Reptiles. *Chelonian*—land-tortoise. *Saurian*—green lizard and gray lizard. *Ophidian*—anguis fragilis, coluber, and common viper. *Batrachian*—common frog, grenouille rousse, red frog, brown toad, aquatic salamander or triton.

Fishes.—Osseous. *Acanthopterygian*—common perch, labrax, thunny. *Abdominal Malacopterygian*—common carp, barbel, leuciscus, common trout. *Sub-branchial Malacopterygian*—cod, turbot. *Apodous Malacopterygian*—common eel, conger-eel.

Chondropterygian or Cartilaginous Fishes. *Sturiones*—sturgeon, *Selacian*—sea-dog.

Mollusca. *Gasteropodous—pulmonary*—the red slug, gray slug, common snail, fresh-water snail. *Pectinibranchiata*—paludina vivipara. *Acephalous Mollusca* or *Lamellibranchiata*—the edible oyster, pecten, mussel, anodont and unio.

Among the Articulata, I have detected sugar in the liver of some *decapodous Crustacea*, as the crawfish and lobster; but in this branch of the animal kingdom the organs of nutrition undergo such profound modifications that the determination of the liver would lead to unnecessary discussion.

The numerous researches cited above have all been made on animals in full health, and during digestion, or shortly after it. They are, I think, sufficient to prove that the presence of sugar in the liver is a general fact, both in man and in all animals evidently provided with that organ.

In proportion as the act of digestion draws to a close, the quantity of sugar poured by the hepatic veins into the general circulation gradually diminishes; and at the same time the tissue of the liver eliminates by degrees all the saccharine matter which it contained. But in warm-blooded animals which are in good health, and in the usual conditions of supply of food, there is never complete absence of sugar from the liver, because the digestion of another meal commences before the quantity of saccharine material already formed is exhausted. If, however, we subject animals to forced abstinence, the sugar after a time completely disappears, and the liver exhibits no more trace of it than any other organ of the body.

Accordingly, as it is a fact that in all animals during digestion the hepatic tissue and the blood which issues from it constantly contain sugar, so it is equally true inversely, that in all animals subjected to abstinence prolonged sufficiently, the liver and hepatic blood are entirely deprived of saccharine matter, which, however, immediately reappears as soon as digestion and nutrition resume their activity.

The duration of abstinence required for the complete elimination of sugar from the liver presents many variations according to species, age, health, &c. I shall merely state that in birds the disappearance is very rapid, occurring at the end of two or three days; while in dogs it is complete only at the end of seven or eight days of starvation. In cold-blooded animals a much longer time is required.

We shall afterwards observe, that in cases where the function of digestion is disturbed or disordered, one of the first results is the disappearance of sugar from the liver and from the blood of the hepatic veins. Hence the presence of saccharine matter there must be regarded in animals in their ordinary condition as the indication of the normal performance of digestion.

2nd. *Sugar is produced in the liver independently of the nature of the food.*

The experiments above cited might serve to show that the sugar is formed in animals without the intervention of saccharine or amylaceous principles in the food, since the presence of sugar was detected in carnivora, as well as in omnivora and herbivora in the animal series. However, as the fact of the production of sugar in the liver is still new, and has most important bearings on physiology, I shall support the above proposition on the evidence of special and direct experiments.

To demonstrate that the saccharine matter originates in the liver, and is not introduced with the food, animals such as dogs, cats, or even rabbits, must be subjected to a diet exclusively animal, and containing no substance which can by the process of digestion give rise to saccharine principles in the alimentary canal. Thus I have fed dogs during three, four, five, and even eight months exclusively on flesh; and on examination at the end of that period, I have constantly found that, while the intestines and blood of the vena portæ at its entrance into the liver contained no sugar, the blood of the hepatic veins was always abundantly charged with it. But an experiment less prolonged proves the production of sugar in the liver. In fact, as before stated, the dog's liver may be completely deprived of sugar by an abstinence of seven or eight days' duration. If at the end of that period the animal be fed on flesh only, the sugar will nevertheless reappear in the liver as soon as the process of digestion determines increased activity in the circulation of the organ. When, therefore, in animals fed exclusively upon flesh, it is constantly ascertained that the blood brought to the liver by the vena portæ contains no sugar, and that the blood which leaves the organ by the hepatic veins is always charged with it, we must admit that the blood acquires the saccharine principle in passing through the hepatic texture, or in other words, that the liver is endowed with a peculiar function in virtue of which sugar is produced.

The liver, therefore, performs two functions at the same time, namely, the secretion of bile and the production of sugar; and the latter function commences even before birth, for I have detected sugar in the liver of the young of mammalia and birds at different periods of fœtal life. It is remarkable, that while the bile, like other intestinal secretions, is poured out into the alimentary canal, the sugar, on the contrary, mixed with the portal blood returned from the intestines and spleen, is carried out into the general circulation, and disappears in contributing to the phænomena of nutrition. This separation of the bile and sugar, however, occurs only in vertebrata, for in mollusca I have found the biliary fluid highly charged with saccharine matter.

The sugar produced in the liver presents the chemical characters of glucose. Along with M. Barreswil, I have ascertained the following properties:—

1. The saccharine principle of the liver ferments when put in contact with yeast, and yields alcohol and carbonic acid.

2. Its solution is rendered brown by the caustic alkalies, and reduces the tartrate of copper dissolved in potash*.

I ought to add, that the hepatic sugar undergoes spontaneous destruction in contact with blood and animal textures much more rapidly than ordinary glucose; a circumstance indicating that, to operate in favourable conditions, the search for sugar in the liver should be made on animals recently dead.

3. Influence of the nervous system on the formation of sugar in the liver.

The formation of sugar in the liver is a function placed under the immediate influence of the nervous system.

In vertebrata, the liver receives two kinds of nerves supplied from the pneumogastric and the solar plexus. In this, as in other functions, it is difficult to determine the kind of participation which the nervous system has in the chemical acts of nutrition. It is, however, incontestable that some of the phænomena of nutrition cannot be produced external to the living individual, and are connected in an immediate manner with the integrity of the nervous system; so that we can extinguish, exalt, or disturb these chemical phænomena simply by modifying the nervous organs which influence them. In particular, those functions, generally periodical, designated in physiology as secretions, are so placed; and I shall show that the production of sugar in the liver belongs to the same category.

Thus, for example, whatever be the kind of food, we can cause the complete disappearance, in a few hours, of the saccharine matter of the liver in dogs or rabbits by the section of the pneumogastric nerves in the middle region of the neck. The same result occurs

* The latter test is the one commonly used by M. Bernard to detect the presence of sugar.

whenever, by any means, a violent commotion of the nervous system is produced.

In the whole extent of the nervous system, I have found only one limited spot of the medulla oblongata where a lesion occasions the opposite effect. Thus, when in dogs or rabbits we succeed in pricking the medulla oblongata with a sharp-pointed instrument within the narrow space, limited below by the origin of the pneumogastric, and above by the emergence of the acoustic nerves, we ascertain after a short lapse of time that the saccharine principle has been formed so abundantly as to spread throughout all the organism. The blood and other liquids of the body are surcharged with it; the urine eliminates the excess; the animal is diabetic.

Thus we can cause the excess or disappearance of sugar solely by modifying the phænomena of innervation.

In conclusion, from the results presented in this paper, I think I have proved beyond doubt the production of sugar in the liver of man and animals; and have established the existence of a function which, up to the present time, had remained entirely unknown.—*Monthly Journal of Medical Science*, September 1851.

ON THE CRYSTALLIZATION OF CYMOPHANE. BY M. EBELMEN.

In 1847, I presented to the Academy the description of the method by which I was enabled to prepare cymophane, or aluminate of glucina, in the crystalline state. The crystals were microscopic. Their specific gravity and their chemical composition agreed with the form as determinable with the microscope, so as to allow of their identification with the natural crystals.

Perfect crystals of cymophane are very rare in mineralogical collections. It struck me that it would be a matter of great interest to mineralogists, to prepare this species in such crystals as might be easily determined and measured. I easily succeeded in this, by prolonging the duration of the evaporation and modifying the composition of the flux, in such a manner as to render it more liquid. The crystals which I obtained are from 5 to 6 millimetres in length. Sometimes they are simple and present the facets m m of the primitive prism, the facet g' well-developed, the base P and the modification e' upon the edge of intersection of the facet g' by the base. The angles which I measured are identical with those obtained by M. Descloiseaux in the crystals of M. De Drée's collection. The specific gravity of the artificial crystals is 3·759; that of natural cymophane is comprised between 3·70 and 3·80. A large number of *macled* crystals are found among the artificial crystals of cymophane; the macles are identical either with those of the crystals from Brazil and Haddam, or those of the crystals from the Ural. Hence the artificial crystals not only present the same primitive form and the same angles as the natural crystals, but even their ordinary *facets* and the principal accidental crystallizations of the latter. On the addition to the flux of 1 per cent. of bichromate of potash, crystals of cymophane are obtained, which are green by day light, like those from the Ural. By candlelight they appear violet.—*Comptes Rendus*, May 12, 1851.

ON THE PRESENCE OF AMMONIA IN HAIL-STONES. BY M. MÈNE.

On Monday the 5th of May, there was a somewhat severe hail-storm at Paris and its environs. Being at the time in my laboratory, the idea occurred to me of collecting some of this hail and submitting it to analysis. For this purpose I placed a piece of linen upon some tressles and collected about 800 grammes. I immediately melted it in a porcelain capsule with the addition of a little muriatic acid, and evaporated it to dryness. When this operation was on the point of completion, I was much astonished at perceiving the occurrence of crystallization at the bottom of the vessel. I tested some of these crystals, which in all weighed 2·78 grammes, and was satisfied that they consisted of muriate of ammonia.

I must not omit to mention another circumstance, viz. when the evaporation was almost completed, a black carbonaceous matter was deposited in rings upon the glaze of the capsule: it resembled the charcoal of organic matters. These spots were very numerous, and I believe they were produced by particles suspended in the air, for I took every precaution to exclude foreign matters.—*Comptes Rendus*, May 19, 1851.

ON THE APPLICATION OF RECTIFIED OIL OF COAL-TAR TO THE PRESERVATION OF MEAT AND VEGETABLES. BY M. ROBIN.

When the flesh of animals, entire birds with the feathers, vegetables, fruits, &c. are placed in air-tight vessels filled with water, at the bottom of which there is a little oil of coal-tar, so that the substances to be preserved are covered by the water, which becomes charged with the vapour of the oil evaporating at the ordinary temperature, they are perfectly preserved from decomposition.—*Comptes Rendus*, vol. xxxii. p. 650.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1851.

Chiswick.—August 1. Cloudy and warm: slight rain. 2—5. Very fine. 6. Fine: densely clouded. 7. Overcast: fine: clear: lightning at night. 8. Very fine. 9. Overcast: cloudy. 10. Cloudy. 13. Sultry. 14. Fine: lightning at night. 15. Cloudy and fine. 16. Very fine. 17. Showery. 18. Cloudy and fine: clear. 19. Very fine: slight haze: clear. 20, 21. Very fine. 22. Very hot. 23. Overcast. 24. Heavy showers, with sunny intervals. 25. Very fine. 26. Slight rain. 27. Fine: constant and very heavy rain at night. 28. Fine: densely clouded. 29. Clear and cold: heavy showers, with hail in afternoon: overcast. 30, 31. Cloudy.

Mean temperature of the month	62°·84
Mean temperature of Aug. 1850	59 ·38
Mean temperature of Aug. for the last twenty-five years	62 ·21
Average amount of rain in Aug.	2·41 inches.

Boston.—Aug. 1. Cloudy: rain P.M. 2—4. Fine. 5—7. Cloudy. 8. Fine. 9—11. Cloudy. 12. Fine. 13. Cloudy: rain early A.M., and lightning P.M. 14. Fine: rain, thunder and lightning P.M. 15. Fine. 16. Fine: rain P.M. 17. Fine. 18. Cloudy. 19. Fine. 20. Cloudy. 21, 22. Fine. 23. Cloudy: rain A.M. 24. Cloudy: rain P.M. 25. Fine. 26. Cloudy: rain P.M. 27. Cloudy. 28. Fine: rain early A.M. 29. Cloudy: rain A.M. and P.M. 30, 31. Cloudy.

Sandwick Manse, Orkney.—Aug. 1. Bright: showers. 2. Cloudy. 3. Bright: clear. 4. Bright: very clear: fine. 5. Clear: fine: very clear: fine. 6. Clear: fine: very clear: fine: aurora. 7. Clear: fine: haze. 8. Cloudy. 9, 10. Cloudy: bright. 11. Cloudy: drops. 12. Drizzle: damp. 13. Rain: damp. 14. Rain: drops: fine. 15. Drops: damp. 16. Clear: fine. 17. Cloudy: clear: fine. 18. Clear: cloudy. 19. Rain: cloudy. 20. Hazy: fine. 21. Rain. 22. Damp: cloudy. 23. Bright: cloudy: thunder. 24. Clear: cloudy. 25. Bright: clear. 26—28. Showers. 29. Showers: drizzle: showers. 30. Bright: clear: aurora. 31. Drizzle: clear: aurora.

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

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	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.
1851. Aug.	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.	29·847	29·830	29·30			29·56	29·60	78	62	68			55	52	nw.	w.		wsw.	......	·23		·03
2.	30·020	29·989	29·50			29·76	29·77	79	60	59			54½	55	sw.	calm		sse.	·02	·40		·04
3.	30·066	30·063	29·47			29·67	29·85	78	54	69			57	53	sw.	sw.		w.	......	......		......
☽ 4.	30·173	30·102	29·55			30·06	30·01	81	57	70			54	53	e.	sw.		calm	......	......		·03
5.	30·297	30·226	29·80			30·33	30·36	71	53	64			58	54	e.	e.		calm	......	......		......
6.	30·234	30·173	29·80			30·35	30·36	67	53	63			61½	53	e.	e.		se.	......	......		......
7.	30·056	30·014	29·68			30·34	30·30	75	55	63			59	53	ne.	e.		se.	......	......		......
8.	30·018	29·988	29·60			30·24	30·22	80	52	67			56½	54½	ne.	ne.		calm	......	......		......
9.	30·034	29·964	29·57			30·17	30·17	69	55	62·5			56½	52½	ne.	ne.		e.	......	......		......
10.	30·085	30·075	29·62			30·20	30·20	71	53	59			56	50½	ne.	calm		ese.	......	......		......
○ 11.	30·106	30·098	29·65			30·10	30·04	79	52	64			53	52	s.	calm		ese.	......	......		......
12.	30·074	30·010	29·59			29·97	29·98	83	59	64			55	55½	s.	s.		calm	......	......		·06
13.	29·944	29·841	29·45			29·82	29·72	82	52	68			58	56	sw.	calm		w.	·02	·02		·10
14.	29·916	29·824	29·33			29·68	29·67	74	55	67			59	56½	sw.	s.		calm	......	......		·50
15.	29·933	29·893	29·33			29·72	29·88	79	48	64			57	52	w.	wnw.		nw.	......	·25		......
16.	29·999	29·931	29·48			29·97	30·02	77	58	62			52	48	sw.	w.		e.	·02	......		......
17.	29·893	29·882	29·30			30·02	30·10	76	54	66·5			52	46½	sw.	w.		calm	·28	·05		......
18.	30·240	30·056	29·55			30·20	30·18	70	38	61			55½	50	n.	n.		se.	......	......		·06
19.	30·355	30·317	29·90			29·96	29·79	72	45	53			56½	52	e.	s.		ssw.	......	......		·41
☾ 20.	30·294	30·153	29·75			30·00	30·00	82	50	65			58	51½	sw.	wsw.		sw.	......	......		·09
21.	30·119	30·003	29·52			29·68	29·58	79	54	65			55	55	sw.	w.		w.	......	......		·07
22.	30·012	29·936	29·35			29·66	29·40	82	57	69			56	56	sw.	sw.		w.	·01	......		·10
23.	29·878	29·849	29·29			29·47	29·44	76	59	66·5			58½	52	sw.	w.		sw.	......	......		·05
24.	29·879	29·781	29·30			29·52	29·64	72	46	63			55	52	sw.	sw.		nw.	·18	·03		·05
25.	30·160	30·044	29·50			29·78	29·86	71	41	58			54	52	nw.	nw.		wsw.	......	·02		·04
● 26.	30·104	29·887	29·54			29·50	29·36	64	55	59			54	52½	sw.	s.		s.	·11	......		·19
27.	29·993	29·889	29·42			29·41	29·49	68	52	60			55	51½	sw.	w.		w.	1·32	·26		·25
28.	29·739	29·669	29·20			29·52	29·57	64	44	57			51	48	nw.	nnw.		wnw.	......	·24		·08
29.	29·902	29·707	29·22			29·80	30·06	59	45	51			48½	50½	n.	wnw.		n.	·07	·06		·06
30.	30·180	30·024	29·62			30·16	30·12	63	35	55			50	45	n.	nnw.		calm	......	·08		......
31.	30·240	30·178	29·78			29·91	30·02	68	51	53			56	51	w.	wnw.		w.	......	......		·02
Mean.	30·058	29·980	29·51			29·888	29·895	73·84	51·74	62·4			55·38	52·12					2·03	1·64		2·23

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

NOVEMBER 1851.

LI. *On the Polarity of Bismuth, including an Examination of the Magnetic Field.* *By* JOHN TYNDALL, *Ph.D.**

1. THE polarity of bismuth is a subject on which philosophers have differed and continue to differ. On the one side we have Weber, Poggendorff, and Plücker, each affirming that he has established this polarity; on the other side we have Faraday, not affirming the opposite, but appealing to an investigation which is certainly calculated to modify whatever conviction the results of the above-named experimenters might have created. It will probably have occurred to everybody who has occupied himself experimentally with diamagnetic action, that whenever the simple mode of permitting the body experimented with to rotate round an axis passing through its own centre of gravity, can be applied, it is preferable in point of delicacy to all others. A crystal of calcareous spar, for example, when suspended from a fine fibre between the poles, readily exhibits its directive action, even in a field of weak power; while to establish that peculiar repulsion of the mass which is the cause of the directive action, even with high power and with the finest torsion balance, is a matter of considerable difficulty†. These considera-

* Communicated by the Author.

† Phil. Mag. 4th series, vol. ii. p. 175. I have much pleasure in referring here to the following remark of Professor W. Thomson in his paper "On the Theory of Magnetic Induction," which appears in the Phil. Mag. for March last. "Thus," he writes, "a ball cut out of a crystal of pure calcareous spar which tends to turn with its optic axis perpendicular to the lines of force, and which tends as a whole from places of stronger to places of weaker force, would experience this latter tendency more strongly when the optic axis is perpendicular to the lines of force than when it is parallel to them; since, according to § 8 of the text, the crystal must have the greatest inductive capacity, or (the language in the text being strictly

tions, together with the fact of having in my possession a piece of bismuth, whose peculiar structure suggested the possibility of submitting the question of diamagnetic polarity to an additional test, are the inducements in which the present brief inquiry originated.

2. In December 1847 a paper on 'Diamagnetic Polarity' was read before the Academy of Sciences in Berlin by Professor Poggendorff, the result arrived at by the writer being, that a bismuth bar, suspended horizontally and occupying the equatorial position between two excited magnetic poles, was transversely magnetic,—that side of the bar which faced the north pole possessing north polarity, and that side which faced the south pole possessing south polarity; the excitation being thus the opposite of that of iron, and in harmony with the original conjecture of Faraday.

3. The method adopted by the writer was as follows:—the bismuth bar was suspended within a helix of copper wire, the coils of which were perpendicular to the axis of the bar. The helix was placed between the opposite poles of a magnet, so that the axis of the helix was perpendicular to the line joining the poles. The bismuth took up the usual equatorial position, its length thus coinciding with the axis of the helix. On sending

algebraic when negative quantities are concerned) least capacity for diamagnetic induction perpendicular to the optic axis. I am not aware that this particular conclusion has been verified by any experimenter," &c. Since the above was written the differential action of calcareous spar has been established; and up to a day or two ago, when I subjected Mr. Thomson's paper to a more careful perusal, my impression was that his theory and my experiments perfectly harmonized. I now fear that there must be some misapprehension on my part as regards his meaning, for not only do the results of my investigation seem to be at variance with his conclusion, but the conclusion appears to be at variance with the experiment of Professor Faraday quoted by himself. This is written with some diffidence, as the manner in which Mr. Thomson has stated Mr. Faraday's experiment proves that he clearly comprehends the particular result obtained. The repulsion which a sphere of calcareous spar endures at any particular point may, I think, be taken as a correct measure of its 'tendency' to move from that point; but it has been proved that the repulsion of a sphere of calcareous spar when the optic axis is *parallel* to the lines of force (parallel to the axis of the soft iron core which repels it) being represented by the number 57, the repulsion experienced when the optic axis is *perpendicular* to the lines of force will be represented by the number 51 (see Phil. Mag. for Sept. p. 176). The 'tendency' to pass from stronger to weaker places of force is therefore stronger when the optic axis is parallel to the lines of force than when it is perpendicular to them, which is a conclusion precisely the reverse of that expressed by Mr. Thomson. I would here, however, repeat my conviction, either that I misunderstand Mr. Thomson, or that through some excusable inadvertence, perhaps through a typographical error, each of the words 'parallel' and 'perpendicular' occupies the place which should be occupied by the other.—J. T.

an electric current through the latter the bar was weakly deflected in a certain direction, and on reversing the current, a feeble deflection in the opposite direction was observed. The deflection was such as must follow from the supposition, that the north pole of the magnet had excited a north pole in the bismuth, and the south pole of the magnet a south pole.

4. It will be at once seen that a considerable mechanical disadvantage was connected with the fact that the distance from pole to pole of the transverse magnet was very short, being merely the diameter of the bar. If a piece of bismuth, instead of setting equatorial, could be caused to set axial, a mechanical couple of far greater power would be presented to the action of the surrounding current. Now it is well known that bismuth sets in the magnetic field with the plane of most eminent cleavage equatorial; hence the possibility, that if a bar of bismuth could be obtained with the said plane of cleavage perpendicular to its length, the directive power of such a bar might be sufficient to overcome the tendency of its ends to proceed from stronger to weaker places of magnetic action and to set the bar axial. After repeated trials of melting and cooling in the laboratory of Professor Magnus in Berlin, I succeeded in obtaining a plate of this metal in which the plane of most eminent cleavage was perpendicular to the flat surface of the plate, and perfectly parallel to itself throughout. From this plate a little cylinder, an inch long and 0·2 of an inch in diameter, was cut, which being suspended horizontally between the excited poles turned strongly into the axial position, thus deporting itself to all appearance as a bar of iron.

5. About 100 feet of copper wire overspun with silk were wound into a helix of such a dimension that the cylinder was able to swing freely within it; through a little gap in the side of the helix a fine silk fibre descended, to which the bar was attached; to prevent the action of the bar from being disturbed by casual contact with the little fibrous ends protruding from the silk, a coating of thin paper was gummed to the interior.

6. The helix was placed between the flat poles of an electro-magnet, so that the direction of its coils was from pole to pole. It being first ascertained that the bar moved without impediment, and that it hung perfectly horizontal, the magnet was excited by two of Bunsen's cells; the bar was immediately pulled into the axial line, being in this position parallel to the surrounding coils. A current from a battery of six cells was sent through the helix, so that the direction of the current, *in the upper half of the helix,* was from the south pole to the north pole of the magnet. The cylinder, which an instant before was motionless, was deflected, forming at the limit of its swing an angle of 70° with its former position; the final position of equi-

librium for the bar was at an angle of 35°, or thereabouts, with the axial line.

7. Looking from the south pole towards the north pole of the magnet, or in the direction of the current as it passed *over* the bar, that end of the bar which faced the south pole swung *to the left.*

8. The current through the helix being interrupted and the bar brought once more to rest in the axial position (which of course is greatly facilitated by the proper opening and closing of the circuit), a current was sent through in the opposite direction, that is from the north pole to the south; the end of the bar, which in the former experiment was deflected to the left, was now deflected an equal quantity to the right. I have repeated this experiment a great number of times and on many different days with the same result.

9. In this case the direction of the current by which the magnet was excited was constant, that passing through the helix which surrounded the bismuth cylinder being variable. The same phænomena are exhibited if we preserve the latter constant and reverse the former.

10. A polar action seems undoubtedly to be indicated here; but if a polarity be inferred, it must be assumed that the north pole of the magnet excites a south pole in the bismuth, and the south pole of the magnet a north pole in the bismuth; for by reference to the direction of the current and the concomitant deflection, it will be seen that the deportment of the bismuth is exactly the same as that which a magnetized needle freely suspended between the poles must exhibit under the same circumstances.

11. The bar of bismuth was then removed, and a little bar of magnetic shale was suspended in its stead; it set axial. On sending a current through the surrounding helix, it was deflected in the same manner as the bismuth. The piece of shale was then removed and a little bar of iron was suspended within the helix; the residual magnetism which remained in the cores after the cessation of the exciting current was sufficient to set the bar axial; a very feeble current was sent through the helix and the deflection observed,—it was exactly the same as that of the bismuth and the shale.

12. These results being different from those obtained by M. Poggendorff, I repeated his experiment with all possible care. A bar of ordinary bismuth, an inch in length and about 0·2 of an inch in diameter, was suspended within the helix; on exciting the magnet, it receded to the equator and became finally steady there. The axis of the bar thus coincided with the axis of the helix. A current being sent through the latter, the bar was distinctly deflected. Supposing an observer to stand before the

magnet, with the north pole to his right and the south pole to his left, then when a current passed through the upper half of the coil from the north to the south pole, that end of the bismuth which was turned towards the observer was deflected towards the north pole; and on reversing the current, the same end was deflected towards the south pole. This seems entirely to agree with the former experiment. When the bar hung equatorial between the excited poles, on the supposition of polarity the opposite ends of all its horizontal diameters were oppositely polarized. Fixing our attention on one of these diameters, and supposing that end which faced the north pole of the magnet to be gifted with south polarity, and the end which faced the south pole endowed with north polarity, we see that the deportment to be inferred from this assumption is the same as that actually exhibited; for the deflection of *a polarized diameter* in the same sense as a magnetic needle, is equivalent to the motion of *the end of the bar* observed in the experiment.

13. The following test, however, appears to be more refined than any heretofore applied. Hitherto we have supposed the helix so placed between the poles that the direction of its coils was parallel to the line which united them; let us now suppose it turned 90° round, so that the axis of the helix and the line joining the poles may coincide. In this position the planes of the coils are parallel to the planes in which, according to the theory of Ampère, the molecular currents of the magnet must be supposed to move; and we have it in our power to send a current through the helix in the same direction as these molecular currents, or in a direction opposed to them. Supposing the bar first experimented with suspended within the coil and occupying the axial position between the excited poles, a current in the helix opposed to the molecular currents of the magnet will, according to the views of the German philosophers before named, be in the same direction as the currents evoked in the bismuth: hence such a current ought to exert no deflecting influence upon the bar; its tendency, on the contrary, must be to make the bar more rigid in the axial position. A current, on the contrary, whose direction is the same as that of the molecular currents in the magnet, will be opposed to those evoked in the bismuth; and hence, under the influence of such a current, the bar ought to be deflected.

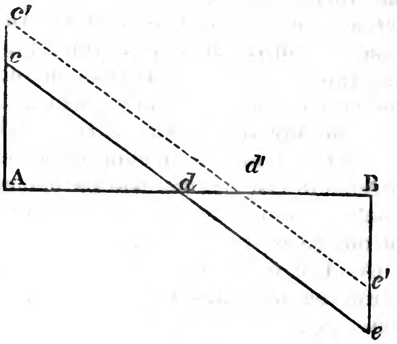
14. The bar at first experimented with was suspended freely within the helix, and permitted to come to rest in the axial position. A current was sent through the helix in the same direction as the molecular currents of the magnet, but not the slightest deflection of the bar was perceptible; when, however, the current was sent through in the opposite direction, a very distinct deflection was the consequence: by interrupting the current

whenever the bar reached the limit of its swing, and closing it when the bar crossed the axial line, the action could be increased to such a degree as to cause the bar to make an entire rotation round the axis of suspension. This result is diametrically opposed to the above conclusion—here again the bismuth bar behaves like a bar of iron.

15. These experiments seem fully to bear out the theory advanced by M. von Feilitzsch in his letter to Mr. Faraday*. He endeavours to account for diamagnetic action on the hypothesis that its polarity is the same as that of iron; "only with this difference, that in a bar of magnetic substance the intensity of the distribution over the molecules *increases* from the ends to the middle, while in a bar of diamagnetic substance it *decreases* from the ends to the middle." So far as I can see, however, the reasoning of M. von Feilitzsch necessitates the assumption, that in the self-same molecule the poles are of unequal values, that the intensity of the one is greater than that of the other, an assumption which will find some difficulty of access into the speculations of most physicists. A peculiar *directive* action might be readily brought about by the distribution of magnetism assumed by M. von Feilitzsch; but up to the present time I see no way of reconciling the repulsion of the total mass of a piece of bismuth with the idea of a polarity similar to that of iron.

16. During these inquiries, an observation of Mr. Faraday perpetually recurred to me. "It appeared to me," he writes†, "that many of the results which had been supposed to indicate a polar condition were only consequences of the law that diamagnetic bodies tend to go from stronger towards weaker places of action." The question here arose, whether the various actions observed might not be explained by reference to the change effected in the magnetic field when it is intersected by an electric current. The distribution of magnetic intensity between the poles will perhaps be rendered most clear by means of a diagram. Let AB represent the distance between the polar faces; plotting the intensity at every point in AB as an ordinate from that point, the line which unites the ends of all these ordinates will express the magnetic distribution.

Fig. 1.

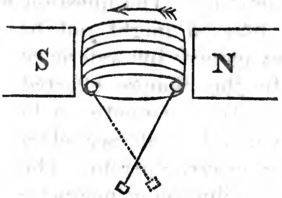


* Phil. Mag., S. 4. vol. i. p. 46. † Ibid., S. 3. vol. xxxvii. p. 89.

Suppose this line to be *cde*. Commencing at A, the intensity of attraction towards this face decreases as we approach the centre *d*, and at this point it is equilibrated by the equal and opposite attraction towards B. Beyond *d* the attraction towards A becomes negative, that is, it is now in the direction *d*B. The point *d* will be a position of stable equilibrium for a diamagnetic sphere, and of unstable equilibrium for a magnetic sphere. But if, through the introduction of some extraneous agency, the line of distribution be shifted, say to $c'd'e'$, the point *d* will be no longer a position of equilibrium; the diamagnetic sphere will move from this point to d', and the magnetic sphere will move to the pole A.

17. For the purpose of investigating whether any change of this nature takes place in the magnetic field when an electric current passes through it, I attached a small sphere of carbonate of iron to the end of a slender beam of light wood; and balancing it by a little copper weight fixed to the other end, the beam was suspended horizontally from a silk fibre. Attaching the fibre to a moveable point of suspension, the little sphere could be caused to dip into the interior of the helix as it stood between the poles, and to traverse the magnetic field as a kind of feeler. The law of its action being that it passes from weaker to stronger places of force, we have in it a ready and simple means of testing the relative force of various points of action. The point of the beam to which the fibre was attached being cut by the axis of the helix produced, and the sphere being also on the same level with the axis, when the magnet was excited* it passed into the position occupied by the *hard* line in fig. 2, thus resting against the interior of the helix a little within its edge. On sending a current through the helix, which in the upper half thereof had the direction of the arrow, the sphere loosed from its position, sailed gently across the field, and came to rest in the position of the dotted line. If, while thus sailing, the direction of the current in the helix, or of the current by which the magnet was excited, became reversed, the sphere was arrested in its course and brought back to its original position. In like manner, when the position of the sphere between the poles was that of the dotted line, a current sent through the helix in a direction opposed to the arrow, caused the sphere to pass over into the position of the hard line.

Fig. 2.

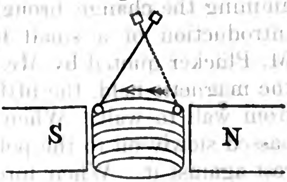


18. The sphere was next introduced within the opposite edge

* One of Bunsen's cells was found sufficient; when the magnetic power was high, the change caused by the current was not sufficient to deflect the beam.

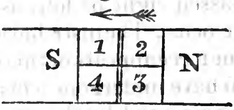
of the helix (fig. 3). On exciting the magnet, the beam came to rest in the position of the hard line; on sending a current through the helix in the direction of the arrow, the sphere loosed, moved towards the north pole, and came to rest in the dotted position. If while in this position either the current of the magnet or the current of the helix was reversed, the sphere went back; if both were reversed simultaneously, the sphere stood still.

Fig. 3.



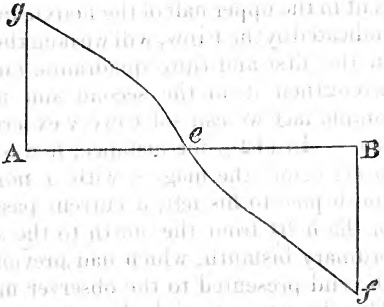
19. From these facts we learn, that if the magnetic field be divided into four compartments, as in fig. 4, the passage of an electric current through a helix placed therein, the direction of the current in the upper half of the helix being that indicated by the arrow, will weaken the force in the first and third quadrants, but will strengthen it in the second and fourth. With the aid of this simple fact we can solve every experiment made with the bismuth bars. In (12.), for instance, it was found that when an observer stood before the magnet with a north pole to his right and a south pole to his left, a current passing through the upper half of the helix from the north to the south pole deflected a bar of ordinary bismuth, which had previously stood equatorial, so that the end presented to the observer moved towards the north pole. This deportment might be inferred from the constitution of the magnetic field; the bar places its ends in quadrants 1 and 3, that is, in the positions of weakest force.

Fig. 4.



20. The experiments (7, 8.) with the other bar are capable of an explanation just as easy. Preserving the arrangement as in the last figure, the bismuth bar, which previously stood axial, would be deflected by the surrounding current, so that its two ends would occupy the quadrants 2 and 4, that is, the positions of strongest force. Now this is exactly what they did in the magnetic field before the passage of any current, for the bar set axial. It was first proved by Mr. Faraday, that the mass of a bismuth crystal was most strongly repelled when the repulsive force acted parallel to the planes of most eminent cleavage; and in the magnetic field the superior repulsion of these planes causes them always to take up that position where the force is a minimum. It is the equatorial setting of these planes which causes the bar at present under consideration to set axial. The planes of cleavage being thus the true indicators, we see that when these set from the first to the third quadrant, or in the line of weakest action, the ends of the bar must necessarily occupy the second and fourth, which is the deportment observed.

21. The little test-sphere can also be made available for examining the change brought about in the magnetic field by the introduction of a small bar of iron, as in the experiment of M. Plücker quoted by Mr. Faraday*. Removing the helix from the magnetic field, the little sphere was at liberty to traverse it from wall to wall. When the magnet was excited, the sphere passed slowly on to the pole to which it was nearest and came to rest against it. When forcibly brought into the centre of the magnetic field, after a moment's apparent hesitation it passed to one pole or the other with a certain speed; but when a bar of iron was brought underneath while it was central, this speed was considerably increased. Over the centre of the bar there was a position of unstable equilibrium for the sphere, from which it passed right or left, as the case might be, with greatly increased velocity. The distribution of the force appears in this case to have undergone a change represented by the line *gef* in the diagram. From the centre towards the poles the tension steepens suddenly, the quicker recession of a bismuth bar towards the equator, as observed by M. Plücker, being the natural consequence.



22. Assuming the law of action for a small magnetic sphere to be that it proceeds from weaker to stronger places of force, we find that the passage of an electric current in the manner described so modifies the 'field,' that the positions of its two diagonals are of unequal values as regards the distribution of the force, the portion of the field intersected by the diagonal which bisects 1 and 3, fig. 4, being weaker than the portion intersected by the diagonal which bisects 2 and 4. But here the believer in diamagnetic polarity may enter his protest against the use which we have made of the assumption. "I grant you," he may urge, "that in a simple magnetic field, consisting of the space before and around a single pole, what you assume is correct, that a magnetic sphere will pass from weaker to stronger places of action; but for a field into which several distinct poles throw their forces, the law by no means sufficiently expresses the state of things. If we place together two poles of equal strengths but of opposite qualities close to a mass of iron, it is an experimental fact that there is almost no attraction; and if they operate upon a mass of bismuth, there is no repulsion. Why? Do the magnetic rays, to

* Phil. Mag., S. 3. vol. xxxvii. p. 104.

express the thing popularly, annul each other by a species of interference *before they reach the body*; or does one pole induce in the body a certain condition upon which the second pole acts in a sense contrary to the former, both poles thus exactly destroying each other? If the former, then I grant you that the magnetic field is rendered weaker, nay deprived of all force if you will, by the introduction of the second pole; but if the latter, then we must regard the field as possessing two systems of forces; and it is to the peculiar inductive property of the body, in virtue of which one system neutralizes the other, that we must attribute the absence of attraction or repulsion. Once grant this, however, and the question of diamagnetic polarity, so far as you are concerned, is settled in the affirmative."

23. Our hypothetical friend mentions it as 'an experimental fact,' that if dissimilar poles of equal strengths operate upon a mass of bismuth there is no repulsion. This was Reich's result—a result which I have carefully tested and corroborated. I shall now proceed to show the grounds which the believer in diamagnetic polarity might urge in support of his last assertion. A twelve-pound copper helix was removed from the limb of an electro-magnet and set upright. A magnetized sewing-needle being suspended from one end, the other end was caused to dip into the hollow of the spiral, and to rest against its interior surface. When a current was sent through the helix in a certain direction, the needle was repelled towards the axis of the coil; the same end of the needle, when suspended at half an inch distance from the *exterior* surface of the coil, was drawn strongly up against it. When the current was reversed, the end of the needle was attracted to the interior surface of the coil, but repelled from its exterior surface. If we suppose a little mannikin swimming along in the direction of the current, with his face towards the axis of the helix, the exterior surface of that end towards which his *left arm* would point *repels* the north pole of a magnetic needle, while the interior surface of the same end *attracts* the north pole of a magnetic needle. The complementary phænomena were exhibited at the other end of the helix. Thus if we imagine two observers placed, the one within and the other without the coil, the same end thereof would be a north pole to the one and a south pole to the other.

24. If we apply these facts to the case of the helix within the magnetic field, we see that each pole of the magnet had two contrary poles of the helix in contact with it; and we moreover find that the quadrants which we have denominated the strongest are those in which the poles of magnet and helix were in conjunction; while the quadrants which we have called weakest are those in which the poles of magnet and helix were in opposition.

25. "Which will you choose?" demands our hypothetical

friend; "either you must refer the weakening of a quadrant to magnetic interference, or you must conclude, that that induced state, whatever it be, which causes the bismuth to be repelled by the magnet, causes it to be *attracted* by the coil, the resultant being the difference of both forces. In the same manner the strengthening of a quadrant is accounted for by the fact, that here the induced state which causes the bismuth to be repelled by the magnet causes it to be repelled by the coil also, the resultant being the sum of both forces. The matter may be stated still more distinctly by reference to Reich's experiments*. He found that when a bundle of magnet-bars was brought to bear upon a diamagnetic ball suspended to the end of a torsion balance, when similar poles were presented to the body, there was a very distinct repulsion; but if one half of the poles were north and the other half south, there was no repulsion. Let us imagine the two halves to be brought to bear upon the ball consecutively; the first half will cause it to recede to a certain distance; if the second unlike half be now brought near, the ball will approach again, and take up its original position. The question therefore appears to concentrate itself into the following:—Is this 'approach' due to the fact that the magnetic forces of the two halves annul each other before they reach the ball, or is it the result of a compensation of inductions in the diamagnetic body itself? If a sphere of soft iron be suspended from a thread, the north pole of a magnet will draw it from the plumb-line; if the south pole of an exactly equal magnet be brought close to the said north pole, the sphere will recede to the plumb-line. Is this recession due to a compensation of inductions in the sphere itself, or is it not? If the former, then, by all parity of reasoning, we must assume a similar compensation on the part of the bismuth."

26. That bismuth, and diamagnetic bodies generally, suffer induction, will, I think, appear evident from the following considerations. The power of a magnet is practically ascertained by the mechanical effect which it is able to produce upon a body possessing a constant amount of magnetism,—a hard steel needle, for instance. The action of a magnet in pulling such a needle from the magnetic meridian may be expressed by a weight which acts at the end of a lever of a certain length. By easy practical rules we can ascertain when the pull of one magnet is twice or half the pull of another, and in such a case we should say that the former possesses twice or half the strength of the latter. If, however, these two magnets, with their powers thus fixed, be brought to bear upon a sphere of soft iron, the attraction of the one will be four times or a quarter that of the other. The strengths of the magnets being, however, in the ratio of 1 : 2, this attraction of 1 : 4 can only be explained by taking into account

* Phil. Mag., S. 3. vol. xxxiv. p. 127.

the part played by the sphere itself. We are compelled to regard the sphere as an induced magnet, whose power is directly proportional to the inducing one. Were the magnetism of the sphere a constant quantity, a magnet of double power could only produce a double attraction; but the fact of the magnetism of the sphere varying directly as the source of induction leads us inevitably to the law of squares; and conversely, the law of squares leads us to the conclusion that the sphere has been induced.

27. These sound like truisms; but if they be granted, there is no escape from the conclusion that diamagnetic bodies are induced; for it has been proved by M. E. Becquerel and myself*, that the repulsion of diamagnetic bodies follows precisely the same law as the attraction of magnetic bodies; the law of squares being true for both. Now were the repulsion of bismuth the result of a force applied to the mass alone, without induction, then, with a constant mass, the repulsion must be necessarily proportional to the strength of the magnet. But it is proportional to the square of the strength, and hence must be the product of induction.

28. In order to present magnetic phænomena intelligibly to the mind, a material imagery has been resorted to by philosophers. Thus we have the 'magnetic fluids' of Poisson and the 'lines of force' of Mr. Faraday. For the former of these Professor W. Thomson has recently substituted an 'imaginary magnetic matter.' The distribution of this 'matter' in a mass of soft iron, when operated on by a magnet, has attraction for its result. We have the same necessity for an image in the case of bismuth. If we imagine the two magnetic matters which are distributed by induction on a piece of iron to change places, we have a distribution which will cause the phænomena of bismuth. Hence it is unnecessary to assume the existence of any *new* matter in the case of diamagnetic bodies, the deportment being accounted for by reference to a peculiarity of distribution. Further, the experiments of Reich, which prove that the matter evoked by one pole will *not* be repelled by an unlike pole, compel us to assume the existence of *two kinds* of matter, and this, if I understand the term aright, is polarity.

---

29. During this inquiry I changed my mind too often to be over-confident now in the conclusion at which I have arrived. Part of the time I was a hearty subscriber to the opinion of Mr. Faraday, that there existed no proof of diamagnetic polarity; and if I now differ from that great man, it is with the honest wish to be set right, if through any unconscious bias of my own I have been led either into errors of reasoning or misstatements of facts.

Queenwood College,
Oct. 1851.

* Phil. Mag., Sept. 1851.

LII. *On the Combination of Arsenious Acid with Albumen.* *By* THORNTON J. HERAPATH, *Esq.**

NOT a little controversy has recently occurred amongst chemists with regard to the nature of the combination which is stated to take place between arsenious acid and albumen. According to Prof. Liebig†, these two bodies combine in atomic proportions, and it is owing, he says, to this circumstance that the vitality of the organs is destroyed in cases of arsenical poisoning. The same opinion, it seems, is entertained by Dr. Sheridan Muspratt of Liverpool, who has lately published some results ‡ which certainly appear to establish the truth of the hypothesis. Other chemists, on the contrary, say that this statement of Liebig's is founded on error—that arsenious acid does not, under any circumstances, form a true chemical combination with albumen, and is only separated by the latter substance from its solutions by a species of mechanical action; that is to say, somewhat in the same manner, perhaps, as iodine and many vegeto-alkaloids are well known to be absorbed and extracted from their solutions by animal charcoal. In evidence, they adduce the results of Mr. Edwards's experiments, an account of which was published some few months back in the Transactions of the Chemical Society of London §. According to this latter experimentalist, the whole of the arsenious acid may be readily extracted from the compound by means of boiling water, provided the operator takes care "thoroughly to break up the mechanical network of the coagulate" (I adopt his own expression), by trituration with a pestle and mortar.

A short time ago, my attention was drawn to the subject by my father, Prof. W. Herapath, who desired me to repeat the experiments above referred to, with the view, if possible, of deciding the question, as it was one which he considered of much interest, in consequence of its bearings on Toxicology. I accordingly did so. My results may be summed up as follows.

Experiment I.—499 grains of the glairy albumen of eggs were taken and intimately mixed, by long-continued trituration, with 3·0 grs. of arsenious acid; the latter having been previously dissolved in a quantity of water sufficient to effect a perfect solution. The mixture was then coagulated by heat, and afterwards carefully evaporated to dryness in a water-bath. The yellowish residue thus obtained was reduced to a very fine powder in a mortar, and repeatedly digested, for several hours together, in

* Communicated by the Author.
† Organic Chemistry, part 2. chap. xiv. p. 358.
‡ Quart. Journ. of the Chem. Soc. of London, No. xiv. p. 178.
§ Ibid. No. ix. p. 14.

boiling water; care being taken to reduce the compound to a still more minute state of division, by patient trituration in a mortar, after each digestion, &c. The washings, on being treated by Reinsch's process, were found to contain a large proportion of arsenious acid.

The insoluble residue having been digested in water for about twelve or fifteen hours, as before described, was dried and weighed. It amounted to about 29 grs.* It was then boiled in strong hydrochloric acid, when it dissolved with the characteristic colour of the proteine compounds. Upon testing this solution in the usual manner, only exceedingly minute traces of arsenic could be detected; a quantity insufficient to yield, on oxidation, a perceptible sublimate of arsenious acid.

Experiment II.—According to the authorities above referred to, 61·07 grms. of albumen will combine with 0·386 grm. of arsenious acid =0·632 per cent. If this statement be correct, I argued, of course if I make a mixture of these two substances, so arranging my experiment that the arsenious acid shall not be present in such large quantity, or in other words, if I employ an excess of albumen, and evaporate the compound to dryness, no trace of arsenious acid ought to be extracted from the mass upon subsequent digestion in boiling water. 438·5 grs. of glairy albumen were therefore mixed with an aqueous solution of arsenious acid, containing 0·25 gr. of AsO^3. The mixture was evaporated to dryness in a gas-oven, and the brittle residue thus obtained treated as in the preceding experiment. The washings, when tested by Reinsch's process, gave evident tokens of the presence of a large quantity of arsenic. The insoluble residue, however, when decomposed in the usual manner by sulphuric acid and heat, and afterwards introduced into Marsh's apparatus, gave but very faint stains of metallic arsenic.

Experiments III., IV. and V.—The same experiments repeated, different proportions of arsenious acid and albumen only being employed. Similar results were obtained.

Experiment VI.—Experiment II. repeated, the same quantities of arsenious acid and albumen being used. Instead of evaporating the mixture to dryness, however, as before, it was merely heated to the boiling-point and then filtered, in order to separate the coagulum which was formed. This latter was then divided into two portions; one of these was treated by Reinsch's process, and found to contain sensible quantities of arsenic, as did also the liquid from which the coagulate had been separated, in either case bright steel-gray stains being formed upon the

* It must be understood, however, that this quantity does not represent the whole of the albumen contained in the white of egg employed; a portion was undoubtedly lost during the long-continued washing.

copper; the other half, on the contrary, was triturated for several minutes in a mortar with about twice its weight of pure quartzose sand, afterwards boiled in water, and treated as in the former experiments. Upon subsequently testing it by Reinsch's process, only very faint and doubtful traces of arsenic were discovered.

Experiment VII.—483 grs. of albumen were mixed with 3·0 grs. of dissolved arsenious acid. The mixture was then coagulated by heat and treated as before. The washings having been carefully collected and evaporated to dryness left a yellowish residue, which was redissolved in boiling water. The latter solution was filtered, in order to remove some few flocculi of albumen which had passed through the pores of the muslin strainer; it was then acidulated with hydrochloric acid, and treated with sulphuretted hydrogen, a stream of which was passed through it for some time, until the liquid smelt strongly of the gas. The solution was boiled in order to dispel the excess of hydrosulphuric acid, and the precipitate of tersulphide of arsenic formed separated by filtration. When dried at 212° F. it was found to weigh 3·63 grs. $=$ 2·921 grs. AsO^3.

Experiment VIII.—This was undertaken with the view of ascertaining whether arsenious acid really loses its poisonous properties on admixture with albumen, as has been asserted.

The whites of three eggs, weighing altogether 1624 grs., were mixed with water and 6·5 grs. of dissolved arsenious acid. The mixture, having been evaporated to dryness, was finally comminuted, mixed with food, and administered to a cat. Having eaten about a fifth or a quarter of the powder, the animal refused to take the remainder; in a short time it exhibited considerable uneasiness, vomited repeatedly, and was soon afterwards attacked with all the symptoms of arsenical poisoning. It lingered on, however, in a state of extreme torment, for two or three days, and then died, refusing food to the last. No *post-mortem* examination of the body was made; my time being then much occupied with other matters of importance.

As regards the above results, it is in my opinion quite unnecessary to offer any observations upon them, as they may be said to speak for themselves. I shall therefore content myself with relating the facts I have observed, and leave my readers to draw their own conclusions.

Mansion House, Old Park, Bristol,
May 16th, 1851.

LIII. *An Account of the late* John Walsh *of Cork. In a letter from* Professor Boole *to* Professor De Morgan*.

My dear Sir, Cork, April 12, 1851.

After an interval which you will, I fear, think to have been needlessly protracted, I am at length able to transmit to you some of those particulars which you have desired me to collect respecting the life of the late John Walsh of Cork. What I have to relate to you will constitute a remarkable, and in some respects a melancholy story. This I say, not because I think that there is evidence that the subject of my letter was on the whole an unhappy individual; on the contrary, he appears to have been a man of cheerful habits and hopeful temperament; but because upon any serious view of life and of human concerns, it must ever be a mournful spectacle to see earnestness and perseverance and many of the nobler elements of character wasted upon pursuits altogether void of any useful result. That Mr. Walsh's labours were of this nature you will have already learnt from the communications which he was in the habit of addressing to you, and of which I have heard you speak; but with what unwearied ardour these labours were pursued, and of how abiding a passion they were the fruit, you will only learn to estimate from the facts which I have now to communicate to you. Whatever may be thought of Mr. Walsh's abilities, you will feel it impossible not to admire his singular application, and not to regret that it was not directed to some more profitable if less ambitious end.

I think it proper to premise that, for the information contained in this letter, I am mainly indebted to Mr. K., now a scholar of Queen's College, Cork, who was for some years a pupil of Mr. Walsh, and to whom his instructor's books and manuscripts were bequeathed. Mr. K. has been so good as to submit the latter to my inspection, and has given me full permission to make such use of them and of his own communications as I think proper. I have also had the opportunity of conversing with the physicians who attended Mr. Walsh during his last illness while a patient in one of the infirmaries, and finally in the Union workhouse of this city, and from them I have received much interesting information.

John Walsh was born at Shandrum, on the border of the County of Limerick, probably about the year 1786. His parents were small farmers; and the only education which he appears to have received was from itinerant schoolmasters, a class of teachers of humble rank, who resided, while imparting their little stock of knowledge, with the parents of their pupils, and thus

* Communicated by Professor De Morgan.

may have contributed to foster that respect for learning which still characterizes the Irish peasant. Of his mother, Mr. Walsh always spoke with great affection, attributing to her influence his first love of letters. He also held in kind remembrance one of his early school-fellows, John Harding, to whom in later life he dedicated a little tract on "The General Principles of the Theory of Sound."

When about 28 years of age, John Walsh, in company with Harding, removed to Cork. Necessity, however, compelled the friends to separate. Walsh, who wrote a fine hand, an accomplishment which he stated that he owed to his mother's instruction, obtained employment as a teacher of writing in ladies' schools. He also received private pupils, and at a subsequent period was engaged as writing-master in two respectable boys' schools in the city. The teaching of writing and arithmetic appears to have been his chief source of subsistence; for although he sometimes obtained pupils in the higher mathematics, this was not a frequent occurrence. Mr. Walsh is said to have been a careful and diligent writing-master, and to have succeeded in making his pupils in arithmetic understand and like the subject. The few testimonies which I have heard of his abilities as a teacher of the higher mathematics would not lead me to think that he was equally successful there. He is stated to have been too intent on enforcing his own peculiar views. Indeed there can be little doubt, from an examination of his papers, that upon this subject he laboured under a peculiar mental hallucination.

At what time Mr. Walsh began to write on mathematical topics I am not able to determine. By degrees, however, this class of speculations appears to have absorbed his entire interest. He became convinced that the differential calculus was a delusion; that Sir Isaac Newton was a shallow sciolist, if not an impostor; and that the universities and academies of Europe were engaged in the interested support of a system of error. Whether this was a sudden conviction, or whether it was the gradual result of the successive disappointments which he was destined to endure in his attempts to convince the world how misplaced its confidence had been, it is not easy to determine; but the latter is the more probable view. However this may have been, Mr. Walsh was for a series of years engaged in a constant endeavour to induce the principal learned societies of Europe to print his communications. His posthumous papers show that he was thus in frequent correspondence with the French Academy, the Royal Societies of London and Edinburgh, the Royal Irish Academy, and other similar bodies.

Failing in every effort of this nature, he published at his own expense a large number of tracts, in which he endeavoured to establish his views, and denounced in no measured terms the unjust and selfish opposition which he thought that he had met with. Of a considerable number of these tracts, and also of the original manuscripts of them, I have found copies among his papers, and a brief account of them may be interesting.

The printed tracts and papers are for the most part occupied with the announcement of some discovery which was designed to supersede the differential calculus in its application to problems respecting curves. The method in question consisted in transferring the origin of coordinates to a point upon the curve, developing the ordinate y in terms of the abscissa x, and making use of the coefficients of the expansion just in the same way as the ordinary principles of the differential calculus would direct us to do. The titles of some of Mr. Walsh's papers will serve to throw light on the particular objects which he had in view. The equation of a curve transformed as above Mr. Walsh calls its "partial equation."

"Memoir on the invention of Partial Equations."

"The Theory of Partial Functions. Letter to the Right Honourable Lord Brougham."

"Memoir on the Theory of Partial Functions."

"Irish Manufactures. A new method of Tangents."

"An Introduction to the Geometry of the Sphere, Pyramid and Solid Angles."

"General Principles of the Theory of Sound."

"The Normal Diameter in Curves."

"The Problem of Double Tangency."

"The Geometric Base."

"Letters to S. F. Lacroix, the Editor of the Edinburgh Review, Rev. F. Sadleir, &c."

"Dublin University. Notes on a Mathematical Controversy between Dr. Lloyd the Provost, the Rev. Mr. Luby and Dr. O'Brien, Fellows of the College, and Mr. Walsh, author of the Geometric Base."

"The Theoretic Solution of Algebraic Equations of the Higher Orders."

"Metalogia, &c."

The mere list of titles above given, and it is far from being complete, affords evidence of considerable industry, and Mr. Walsh's unpublished papers confirm this testimony. The following is an account of the principal ones:—

"The Elements of Geometry, by John Walsh." (Folio.) This merely contains a series of definitions and axioms, &c., beginning with the "doctrine of ratio."

"On the Measurement of Infinite Space, and the Theory of

Parallel Lines." (Folio.) A series of definitions, axioms, and enunciations of propositions without proof annexed.

The definitions are headed by the motto "Space is Space, Time is Time, Truth is Truth," and the first of the so-called definitions is "Space and Time are infinite, coeternal, and cannot be increased or diminished." For the rest, the propositions appear to be those of Euclid expressed in another form, the word "angular plane" being used for angle.

"Memoir on the Calculus of Variations, showing its total unreality."

"The Principles of Geometry." This consists of two books; the first, on the "Measurement of Infinite Space," apparently the same as the second manuscript, but with demonstrations appended; the second on the "Measurement of Bounded Space."

A manuscript in a brown paper cover, apparently a note-book containing a series of mathematical speculations on the "measurement of infinite space," the solution of equations of the higher orders, the trisection of an angle, physical astronomy, &c.

In these, and in nearly all of Mr. Walsh's speculations which I have taken the trouble to examine, one peculiarity of his mental procedure is very observable. He takes up some known method or formula of analysis, makes in it a slight and quite unimportant change (for every theorem admits of some variety in the mode of its expression), and views the result to which he is led as an original discovery. Thus, in a page headed "Cubic Equations," he writes the name of Cardan opposite to a well-known algebraic solution, that of Walsh opposite to the same result put under another and less convenient form, and below these he gives a formula headed "For a complete Cubic by Walsh only." It is related of the dramatic poet Wycherley, that in his old age the functions of memory and of genius were so strangely mingled and confused, that if verses were read to him in the evening he would reproduce them the following morning with all the effort of original composition, quite unconscious of the source of his borrowed inspiration. Mr. Walsh committed similar errors without the intervention of a sleep.

What importance Mr. Walsh attached to his supposed discoveries will appear from the following extract which I make from the MS. note-book above referred to. It is not a solitary example.

"Discovered the general solution of numerical equations of the fifth degree at 114 Evergreen Street, at the Cross of Evergreen, Cork, at nine o'clock in the forenoon of July 7th, 1844; exactly twenty-two years after the invention of the Geometry of Partial Equations, and the expulsion of the differential calculus from Mathematical Science."

Besides Mr. Walsh's own papers, there remain a large number of letters which had been received by him, in reply to his applications, from different learned societies. The most interesting of these conveys a report by Poisson and Cauchy on one of his papers submitted to the Academy of Sciences. That report points out clearly what I have already had occasion to remark in other instances, that Mr. Walsh's supposed discovery, in so far as it was true, was not original. In a subsequent report by Poisson upon another communication, that great analyst, referring to the former one, stated explicitly that Mr. Walsh's papers did not merit the attention of the Academy.

Certainly Mr. Walsh had no right to complain of the treatment which he received from the French Academy. Alluding, however, to their rejection of his first paper, he observes in his MS. memoir on the Calculus of Variations,—

"Such was the commencement of a controversy, or rather persecution, scarcely yet known to exist, but which will hereafter be recorded as one of the most memorable æras in the history of human knowledge. . . . . It [the paper] merited a more profound consideration of its contents than M. Poisson thought well to bestow on them; an error of which M. Poisson was not aware of the consequences, as affecting in the future history of science not only his own character, and the character of the institution of which he is a member, but that of the age in which he lives."

From the scientific societies of his own country and of the United Kingdom, Mr. Walsh received less attention than from the French Academy. The latter stated the grounds upon which his communications were declined; the former simply declined them. To establish the rule of propriety in such cases is not easy, but I am disposed to think that it would favour the course adopted by the French Institute rather than our own. It would seem in the case of societies, as of individuals, to be the right course to speak the truth in its simplicity and integrity. To do this would not entail the further obligation to answer unmeaning objections, or engage in controversy, nor would occasion often arise to exercise the right of declining further discussion; for it is not to be presumed that all who are mistaken in opinion are therefore captious and unreasonable. Probably there are many cases in which a simple and candid statement of the nature of the error into which an author has fallen would at once produce conviction. In such cases it would be kind as well as just to convey the information required. There is indeed too much reason to fear that Mr. Walsh's case was not one of this nature; still it is possible that the sense of neglect and injustice under which he laboured might by such little attentions have been mitigated, and that a more wholesome feeling might have arisen in his breast than that which he appears to have indulged.

Mr. Walsh continued to pursue his avocation as a writing-master in Cork until the year 1845, when a paralytic seizure threw him almost helpless upon the charity of those who had known him in better days. Among his papers is a subscription-list, testifying that the appeal made for him to the benevolence of his fellow-citizens was not unheard. I have however been informed upon credible authority, that the first use which Mr. Walsh made of the sum put into his hands was to rush into print. It will not be surprising to learn that about this period he was for some time confined in the city jail for debt, and that shortly after he was an inmate of the Union. For the particulars of this part of Mr. Walsh's life I am indebted chiefly to Mr. K., who, with a zeal and fidelity of which there are not many examples, continued to retain his former relation to his old, and one would think, helpless instructor. In the solitary prison-cell, or surrounded by paupers in the crowded Union, poor Walsh might still enjoy the satisfaction of descanting upon his favourite topics to his one remaining pupil. It is a happy circumstance, that, never having married, he had no family cares to weigh upon his spirits. What time poor Walsh spent in the Union in this his first visit to it I have not ascertained; but before long he was removed, chiefly through the benevolent intercession of Dr. Finn, one of the physicians of the North Infirmary, to that Institution, where he remained for some months. It is not improbable that at this period his disease may have been accompanied by cerebral excitement, for he is described as having been a rather intractable patient. Peculiar notions which he had formed on the subject of religion led him to attempt to convert some of his fellow-patients to the same views. I have been informed by one of the physicians who was then in attendance at the infirmary, that he would rise at night from his bed, and addressing the other patients, declaim in the most earnest manner against the belief in the immortality of the soul. The particular argument upon which he relied is stated in a paper which a short time before he had printed under the title of Metalogia. It is, in his own words, as follows:—

"The Deity is coeternal with Time and Space, and has all his attributes infinite. He cannot confer any of these attributes on thinking beings; for if the Divine Being could confer any one of his attributes, viz. immortality, for example, therefore inductively he could confer all his attributes on mankind, and make them coequal to himself in every respect, which would be contradictory and absurd. Therefore, &c." In the same paper, which is interesting as being probably his last performance, he thus defines the science of Metalogia, and describes its claims: "Metalogia, which signifies beyond reason, is the name I have given to a new branch of knowledge which inquires into the causes of such phænomena as ignorance would persuade us had been beyond the power of human reason to investigate. Already it has opened the way for three great movements in human affairs." These movements he describes with a simplicity which would excite a smile, if the whole history did not too deeply draw upon the sources of pity, as, First—"The falsehood of the Greek method of exhausted quantities, so celebrated throughout all ages, even in our own times, by the mathematicians, astronomers and philosophers of the world, as an admirable and refined invention. And the falsehood of the offspring of that method, namely, the no less celebrated doctrine of fluxions, differentials, limits, &c., the boast and glory of England, France, and Germany, demonstrated by the great invention of the geometry of partial equations which has superseded them, at least in my hands, and indefinitely surpassed the old system in power."

"The second great movement in human affairs is in physical science, viz. the falsehood of Newton's law of gravity." "The third of these great movements" is the above argument against immortality, which, he says, "because it is based upon demonstrated truth will ultimately overspread the earth, and banish superstition from its surface." Observe the admirable candour of the admission "*at least in my hands*" with which poor Walsh is forced to qualify his harmless boast of the triumphs of his system. "Whether," he confesses in another part of the same paper, "it is owing to the prejudices of the philosophers or to the actual irrational bearing of the human species," his most important discoveries had been "completely sent to Coventry."

The remainder of poor Walsh's story is soon told. After remaining without benefit for some time in the North Infirmary, he was received into the house of a brother, the Rev. M. Walsh, parish priest of Sneem in the county of Kerry. There, however, he did not remain long. Restless and unhappy, he returned, at his own desire, to Cork, and resided on Patrick's Quay, where he endeavoured again, but vainly, to obtain pupils in his favourite science. The paralysis from which he suffered had moreover destroyed the beauty of his hand-writing, which from one specimen that I have seen of it appears to have been once remarkable, and thus cut off all hopes of subsistence from his former employment. Doubtless it was by the aid of benevolent friends (and in generous sympathy for misfortune, Cork is not wanting) that he was able to subsist. I have seen a letter addressed by him while under these afflicting circumstances to Dr. Finn, who, as already mentioned, had shown him kindness on a former occasion. In that letter he complained that, notwithstanding all his discoveries, he obtained no pupils, and expressed a desire to be removed into the Union at Kanturk. Shortly after he was again admitted into the Cork Union. Dr. O'Connor, physician to the Union, has thus described to me his appearance on that occasion. "I remember Mr. Walsh when brought to me for examination," Dr. O'Connor says, "as a little neat-looking man, with a very thoughtful and pleasing expression of countenance, and apparently not at all depressed by the unhappy circumstances in which he was placed. He had a slate, a black board, and a little roll of paper under his arm. I said to him, 'I am sorry, Mr. Walsh, to see you reduced to your present necessity.' 'Oh, by no means,' replied Walsh, 'it is the turn of the wheel of life. I must bear it like a philosopher.' 'Well, Mr. Walsh,' replied the good doctor, 'is there anything that I can do to make you more comfortable here?' 'Oh,' said Walsh, 'if you could get me a quiet place to put up my board, and allow a pupil of mine to visit me occasionally, that is all that I shall desire.'" Thinking to procure for his patient a greater measure of indulgence than could otherwise be conceded to him consistently with the rules of the house, Dr. O'Connor ordered his admission into the hospital, although medical treatment did not, from the nature of his malady, appear to be required. There poor Walsh spent the remainder of his days. After a time, his debility having greatly increased, he was entirely confined to his bed, but even then his faithful pupil K. continued to visit him, indulgently listened to his projects for the reformation of science, and consoled him by the tribute of a generous sympathy for the loss of health, the loss of home and station, and for that which, to the poor dying enthusiast must have been far harder to bear, the world's imagined neglect and ingratitude.

Since the period when Mr. Walsh was an inmate of the North Infirmary some change for the better had passed over his mind. He was now more docile and tractable, and attended to the wishes of his physician and of those who were appointed to take charge of him, nor did he again endeavour to engage his fellow-patients in religious disputes. In reference to this improvement of character, the consequence perhaps of a remission of the activity of disease, or perhaps also of self-reflection under the sobering influence of adversity, Dr. O'Connor has told me the following anecdote:—"On one of my visits to Walsh's bedside he inquired of me if he had ever since his admission into the Union endeavoured to disturb the religious opinions of those around him. The doctor admitting that he had not, and commending him for his moderation, Walsh replied, 'And yet, doctor, I could say a few words, a very few words, that would make you and the chaplain and everybody here abandon your present convictions for ever.' 'Well,' said Dr. O'Connor, 'say them, and then see whether they produce the effect on me.' 'No,' replied Walsh, 'I know well that they would, but I forbear to utter them.'" Some time after this Walsh consented to see a Roman Catholic clergyman. To him he revealed the potent spell. It was the argument of the Metalogia.

It was at the commencement of an awful period that John Walsh sought an asylum in the Cork Union. The autumn of 1846 and the whole of the following winter and summer will long be remembered in Ireland. The food of a nation had perished, and a desolation unexampled in modern times came down upon the land. At the time of Mr. Walsh's admission, the Union house built for the accommodation of 2000 persons was already crowded. Ere long the number of its inmates exceeded 7000, and despite of all endeavours to provide accommodation for the continually increasing throng by the erection of sheds and temporary hospitals, all the avenues of approach were thronged with the dying and the dead. Amid this scene of national woe and calamity in the famine year of 1847 poor Walsh breathed his last. He had been for some time before his death insensible and unable to recognize his pupil. I have been informed by Dr. O'Connor that he did not die of the fever which was carrying off the inmates of the Union house at the rate of two or three hundred weekly, but of the paralytic affection under which he had for some time laboured.

Mr. Walsh was a man of agreeable address, and, when treated with the respect which he thought due to himself, of friendly and courteous manners. In the affairs of the world he was a child, and was apt to become the dupe of interested persons. With proper œconomy he might have saved sufficient to support himself in old age; but the easiness of his temper, and, I fear, during the latter years of his life, a too great fondness for social enjoyments kept him poor. The freedom of his opinions upon religion operated also unfavourably upon his temporal interests. I have reason to think, from an examination of his papers, that the looseness of his sentiments upon this subject was not the result of any desire to release himself from the restraints of moral obligation, but of an exaggerated self-esteem, and a too great confidence in his own not very exalted powers of intellect, the source probably of nearly all his errors and misfortunes. To this cause we may attribute the intemperate tone of his remarks whenever he is discussing the merits of those whom the world has consented to make its guides in science. Upon his favourite topic of discourse it is said that he was quite unable to bear contradiction.

Mr. Walsh in his day attracted some attention even in high quarters. The Edinburgh Review, No. 143, p. 192, referring apparently to Mr. Walsh's pertinacious obtrusion of his views upon the public, says, "Let us hope that the person who in our day occupies himself with printing his mathematical reveries against the method of fluxions and the first section of the *Principia,* and who insults the public taste by publishing the foulest, most vulgar abuse of the 'Saxon Philosopher,' may not succeed in making his reflecting countrymen believe that the name which all mankind have consecrated to receive only veneration represents only a driveller and a knave."

I find this passage and another from the same journal copied in Mr. Walsh's hand-writing among his papers. He there denies that he made use of the language imputed to him, and addresses a letter to the editor of the Edinburgh Review upon the subject. Of this letter, or of a similar one, there is also a printed copy. It is to be feared that, whether Walsh used the particular terms in question or not, he had laid himself fully open to the charge of employing violent and abusive language.

Mr. Walsh is an extreme instance of a class of persons, who, without having mastered the very elements of received science, spend their lives in attempting its subversion, and in the vain endeavour to substitute in its place some visionary creation of their own fancy. Whether such persons would not in the earlier stages at least of their career be accessible to the conviction of their error is worthy of consideration. A little judicious kindness at that period might in some cases prevent the misspending of a life. But when that which was originally but a fond and foolish notion has been fostered into a disease of the mind, the cure is generally hopeless. Trisectors of an angle, squarers of the circle, discoverers of perpetual motion, constitute a class of mankind whose peculiarities deserve the attention of the student of human nature, and whose personal history is often calculated to awaken the deepest commiseration. Providence seems to have in some measure vindicated the equality of its dispensations by assigning to them a double measure of hope, which serves them in the stead both of ability and of success.

But there is a class superior to these whose history is far more affecting; men who with both genius and competent knowledge devote themselves, perhaps in the over hours of labour, to the improvement of some mechanical invention, and either through want of means, or through legal impediments, or because they have miscalculated the requirements of the age, find themselves doomed to ceaseless disappointment. If they are unburdened with family ties, the case is not so distressing. Amid the greater sorrows of the times we may permit ourselves to forget theirs. But if they have wife and children looking up to them for support, yet destined to see their comforts depart and their hopes grow less; if, in addition to this, sickness follows in the train of toil and disappointment, and unstrings the skilful hand and quenches the fire of the inventive mind, then I confess that, guilt and its consequences apart, I know of few sadder spectacles in the varied drama of human life.

A history of some of the cases of this nature which have come under your knowledge would, I think, be a valuable record—valuable from its intrinsic interest, valuable as a beacon and a warning. I presume that you are acquainted with a greater number of such cases than any other person. That inventors such as I have spoken of do really constitute a class apart, is, I think, very evident. Generally it would perhaps be found that they are men of innocent and blameless lives, of great simplicity of character, ignorant of the world, and perhaps for this very reason imbued with a too great self-esteem, and an unwarranted confidence in their own powers. We should probably discover in them as a class the peculiar effects which a life too special in its pursuits tends to produce, and which in those who are more favourably circumstanced are mitigated by intercourse with other minds, by self-reflection, and by a knowledge of the peculiar dangers to which they are exposed.

I remain, my dear Sir,

Ever sincerely yours,

Professor De Morgan. GEORGE BOOLE.

LIV. *On the Elevatory Forces which raised the Malvern Hills.* *By* H. E. STRICKLAND, *F.G.S.**

[With a Plate.]

PROFESSOR PHILLIPS has already pointed out (Mem. Geol. Survey, vol. ii. p. 5) that the syenitic ridge of the Malvern Hills forms a part of a great line of dislocation, extending for at least 120 miles from Flintshire on the north to Somersetshire on the south. He shows that this line of disturbance forms the eastern boundary of that vast region of elevation which includes the whole of Wales and part of Southern Ireland, and that the principal movement which caused this elevation took place between the Carboniferous and Triassic epochs †. He

* Communicated by the Author.

† We cannot speak more precisely as to the date of a convulsive movement which perhaps extended over a considerable period. According to the researches of Sir R. Murchison in other regions, an entire geological epoch,—that of the "Permian System"—intervened between the Carboniferous and the Triassic systems. But deposits of this age are scarcely, if at all, traceable in the region here described; and we cannot therefore assert whether the Malvern ridge was elevated at the beginning, the middle,

further shows that this line of fracture, bounding the elevated region on the east, partakes throughout the greater part of its course of the nature of a fault; that this fault is on an enormous scale in its vertical and horizontal dimensions, and that it is much concealed by the thick deposits of new red sandstone which have covered it up on the downcast side, and followed the sinuosities of its course.

The demonstration of so vast a line of disturbance, evidently due to one set of operations acting at a very remote epoch, enormous in dynamic amount, yet comparatively limited in their duration, is one of the grandest generalizations at which British geologists have arrived. The nature of the movement which has produced these results seems consequently to deserve a fuller investigation than it has yet received.

These disturbing forces appear to have been partially continued during, and even after, the deposition of the New Red Sandstone. Both that and the incumbent Lias show proofs of elevation and of dislocation, which may be regarded as the expiring efforts of those vast forces which raised the mountains of Wales above the plains of England. Indeed the general south-easterly inclination of the whole secondary series of Southern England is a further proof of the continuation of these elevating movements down to a late geological date. But all these more recent changes of level were so feeble in amount compared to the vast convulsions of the pre-triassic period, that we may eliminate them altogether from our present inquiry. We shall gain clearer notions by supposing the New Red Sandstone and all the superior formations entirely removed, and by endeavouring to decipher the state of things which immediately preceded the deposition of those strata.

Of the whole line of dislocation above mentioned, the ten or fifteen miles which include the Malvern and Abberley Hills probably afford the best information on this subject. The syenitic axis of Malvern, eight miles long, about half a mile wide, and almost perfectly straight, naturally suggests the idea of a vast dyke of injected trap rock. But Prof. Phillips has successfully shown, from the absence of lateral ramifications of syenite, from the rare and slight indications of metamorphic action, and from other phænomena, that this plutonic ridge must have been elevated in a solid state. Indeed the fact that it occurs, not on a line of simple fissure, but on a line of fault, is conclusive of its

or the end of the Permian epoch. From the conformability, however, of the "Lower New Red Sandstone" to the Coal-measures in Staffordshire and Shropshire, and its unconformability to the Triassic or Upper New Red Sandstone, we may consider the conclusion of the Permian epoch as the probable date of this event. (See Murchison's Silur. Syst. p. 131.)

having been elevated as a solid; for the downcast side being lower by several thousand feet than the upcast, the syenite, if fluid, could not have been raised to its present position, but would have overflowed the downcast side to a great distance.

Admitting this wall-like mass of syenite to have been forced up from below in a solid state, we at once obtain a clue to the vertical or highly inclined (sometimes reversed) position of the sedimentary strata on the west, or upcast side of the Malvern ridge.

It appears, then, that the Malvern district, though forming part of a great line of fault, yet exhibits the phænomena of a fault under a very complicated aspect. To explain this I must refer for a moment to a few elementary principles.

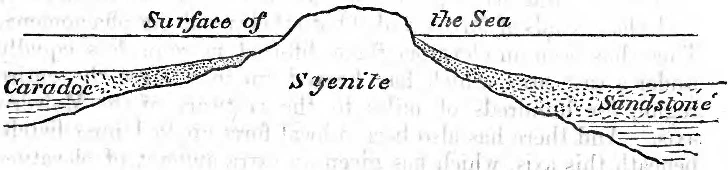
In the simplest form of a fault, when one portion of a horizontal stratum is elevated by an equally diffused pressure from below, while the other portion remains at rest, the stratum preserves its horizontality up to the very plane of separation; or, more frequently, the friction of the two masses causes the strata to bend slightly *towards* each other on the opposite surfaces of the fault. Again, if the upward pressure be confined to a *line* instead of being spread over a *surface*, the strata are thrown in opposite directions, and an anticlinal is the result.

But the Malvern region presents us with a combination of both these kinds of forces, and of both their resulting phænomena. There has been an elevatory force diffused more or less equally under a vast area, which has heaved up in a mass the entire region for hundreds of miles to the westward of the Malvern axis. And there has also been a local force applied immediately beneath this axis, which has given an extra amount of elevation to the marginal portion of the upcast area.

It is this excessive development of motive force *at the very margin* of an elevated region, and in immediate contact with a non-elevated tract, that renders the phænomena of the Malvern Hills peculiarly anomalous. Under ordinary circumstances, when an upward force is applied locally along a line, it acts equally on both sides of that line, elevating the strata, as already shown, into an anticlinal position. If, however, the resistance be greater on one side of the axis than the other, a certain amount of displacement ensues, and the anticlinal arrangement is combined with that of a fault. The Malvern elevation is probably an extreme and unusually exaggerated instance of the last class of phænomena. If we could strip off the thick mantle of New Red Sandstone which conceals the eastern side of this axis, we should probably find the strata from the Caradoc sandstone up to the Coal-measures more or less upturned at their edges. (See Plate I.) So vast a force as was required to elevate the syenitic axis could hardly have failed to shatter and twist up the margin of the deposits on its eastern or downcast side, although their amount of statical resistance was such as to forbid any general elevation of them *en masse.*

Assuming that such was the condition of things in this region before the deposition of the New Red Sandstone, let us endeavour to trace the mode of action of the forces which produced it.

There is evidence that elevatory movements have taken place along the axis of the Malvern chain before, as well as since, that great and transient outburst which dates between the Carboniferous and Triassic epochs. A mass of syenitic rock had been elaborated by igneous agency beneath this tract in very remote geological times. It had become solidified, and had been elevated above the oceanic surface before the Upper Silurian formations were deposited. The sections on the west side of the Malvern Hills show that the Mollusca and Corals of the Caradoc sandstone lived and flourished in immediate contact with the plutonic rock, and that pebbles of the latter were rolled into the sea of that period, and were there imbedded in company with the animal remains. (See Mem. Geol. Surv. vol. ii. p. 33.) We may therefore suppose that at this period a state of things prevailed such as is here represented.



In other portions of the Welsh region we find similar proofs of elevations having taken place in remote palæozoic times. Thus at Bishop's Castle, and in the country to the north-west of it, the Caradoc sandstone is found to lie unconformably to the subjacent rocks; and the Wenlock shale in the same way overlaps the Caradoc sandstone near Bishop's Castle and Builth. (See Ramsay in Journ. Geol. Soc., vol. iv. p. 296.) Some of these ancient disturbances were probably connected with those referred to in the Malvern district. But this most ancient elevation of the syenite seems to have been comparatively small in amount, and was wholly covered up by the formations which succeeded the Caradoc sandstone, and which contain no fragments of syenitic rocks. In order to explain the changes which now took place, it may be legitimately assumed that the floor of solidified syenite on which the sedimentary deposits rested was itself underlaid by igneous rock in a fluid and active state. Let it be further granted, that the present *breadth* of the Malvern syenite (averaging half a mile) approximately represents the *thickness* of the upper or solid portion of the plutonic rock. Such I assume to have been the condition of things when that great elevatory movement commenced which upheaved the westernmost side of our island. It is irrelevant here to inquire whether this general upheaval was effected by the mere expansion caused by increased temperature, or by the introduction from other quarters of vast masses of fluid matter beneath the elevated area. It will be sufficient to admit that a special volcanic focus existed beneath the syenitic axis of Malvern, and that its energies were called into action simultaneously with the more general movement which elevated the area of Herefordshire and Wales.

We may now suppose that the elevatory forces beneath the Cambrian region had accumulated so as to overcome the superincumbent weight; while the region to the eastward, either from its greater rigidity, or from the less amount of subjacent force, remained in a quiescent state. A separation would now take place between these two areas; a long and sinuous line of fracture would divide them; and the region where Force had overcome Resistance would begin to rise higher and higher above the area which remained unmoved.

The previous elevatory movement which has been shown to have existed along the Malvern axis probably rendered this a weak point in the earth's crust, and caused the line of fracture to coincide with that axis. As soon as one side of this line began to rise and a fault to be produced, the volcanic forces which had been pent up beneath the syenitic axis would now find, or endeavour to find, a vent. Struggling to escape along the line of fault, they would thrust up the solid syenite above them, raising it into a lofty cliff above the downcast area, and elevating, overturning, or crumpling up the edges of the Silurian, Devonian, and Carboniferous strata which rested upon it. (See Plate I.)

In the above diagram I have taken as a basis the section across the Worcestershire Beacon published by the Geological Survey, and have endeavoured to supply conjecturally those portions of the strata which have been removed by denudation, or which lie too deep to be visible. I have supposed that a vast mass of Devonian and Carboniferous rocks has been upheaved bodily, while the lower strata nearer the syenite are more or less fractured, crushed and contorted. The thickness of the strata which have been since denuded may appear enormous; but it is founded on the careful measurements of the Geological Survey, which give about 5500 feet for the Old Red Sandstone of Herefordshire, and 3500 for the incumbent Carboniferous series at the nearest point (Dean Forest), where the undulations of the beds have saved them from denudation. As, however, the coal-fields of Wyre Forest and the Clee Hills on the north present a less development of the series than is seen in Dean Forest, I have reduced the thickness of the Coal-measures and Carboniferous limestone which once existed on the west of the Malvern Hills to about 2300 feet. Adding these amounts to the thickness of the Upper Silurian, Caradoc sandstone, and syenite, we obtain a total of at least 13,000 or 14,000 feet for the amount of dislocation between the two sides of the Great Fault; an amount greater, perhaps, than can be paralleled in any other instance of a single fault which the world can produce. Nearly one-half of this amount may, however, be assigned to the more local forces which elevated the Malvern syenite; so that about 7000 or 8000 feet would represent the difference of level between the strata in the less disturbed parts of Herefordshire west of Malvern, and their equivalents now buried beneath the New Red Sandstone of Worcestershire, allowing about 1000 feet for the thickness of the latter down to the subjacent Coal-measures.

The fluid matter which I suppose to have thus forced up the solid syenite may itself have never reached the surface. The plutonic axis of Malvern seems only to exhibit its upheaving effects, and shows no signs of fluid ejections contemporaneous with the elevation. It is possible, however, that volcanic matter may have poured out over the downcast area, where it is now concealed by the New Red Sandstone. And the laterally injected dyke of Brockhill, as well as the trappean masses in the black shales on the west of Ragged Stone and Midsummer Hills, are not improbably connected with the volcanic forces which thrust up the syenite. This supposition appears to me at least equally probable with that of Professor Phillips (Mem. Geol. Surv., vol. ii. p. 56), that these greenstone eruptions were contemporaneous with, and overlaid the black Caradoc shales with which they are in contact. In the arrangement of the strata around Eastnor Park, we seem to have indications of a crater of elevation, caused by an incipient volcanic eruption whose focus never reached the surface. The great expansion and crumpled condition of the Silurian rocks at Ledbury, and their general semicircular arrangement round a central point, indicate a local and special development of volcanic energy beneath. But the syenitic axis itself affords no more signs of eruptive force at this point than at any other. The efficient force seems to have acted not in, but at the west side of this axis. A mass of basaltic matter ejected beneath the Caradoc sandstone will explain these phænomena. Its ramifications would be likely to select the black shales as being less resisting than the sandstones above and below them, and would produce that series of trappean dykes which Professor Phillips was the first to describe. By penetrating the shales (as trap-dykes often do) in the planes of stratification, they would produce an appearance of contemporaneity, though their real dates might be long subsequent.

The district here referred to seems to be exactly analogous to the well-known *elevation crater* of Woolhope, distant only seven or eight miles to the westward, in which we also see the ineffectual struggles of a focus of volcanic energy to burst through the incumbent strata. Here also the concealed volcano has left a collateral proof of its existence in the single basaltic dyke of Bartestree Chapel.

These detached indications seem to show that the volcanic matter which underlies, and which has elevated this region, is different in mineral character from the more ancient syenite of Malvern, and is probably more allied to greenstone or basalt. The trap-rocks of Wyre Forest, north of Abberley, further corroborate this view.

In tracing to the north or south that long line of dislocation of which the Malvern Hills form a part, we find a continuation of analogous phænomena more or less modified by local circumstances. The Abberley range of hills is, as is ably shown by Professor Phillips (Mem. Geol. Surv., vol. ii. p. 145), completely analogous to the Malvern district; the chief difference being, that the syenitic axis which upheaved the Silurian rocks is here almost wholly concealed from view, and (with one small exception) is only known by its effects. The Silurian and Old Red formations are here, as on the west of Malvern, *overturned* for a distance of several miles. This remarkable phænomenon may, I think, be explained in a simpler mode than either of those proposed by Professor Phillips. All that is requisite is to resolve a certain portion of the vertical uplifting force into a lateral direction. Now it is certain that an enormous fault-line runs along the eastern side of all the disturbed and elevated district, and that the downcast region on the east has remained relatively rigid and unmoved. In accordance with the well-known law that the plane of a fault (almost invariably) dips *towards the downcast side*, it is evident that this oblique surface would act mechanically as an inclined plane or wedge, in reference to a vertical uplifting of the strata on the west, and would force them over to a certain distance in a lateral direction. (See Plate I.)*

The same lateral force would explain the sharp anticlinal

* A very analogous case occurs at Hohnstein in Saxony, where a mass of granite, upheaved in a solid state, has not only elevated but *overturned* the contiguous strata, causing beds of the Jurassic series to repose upon Cretaceous ones. (See Cotta, *Geognostische Wanderungen*. Dresden, 1838.)

curves into which some parts of the Ridge Hill near Abberley are compressed. (Mem. Geol. Surv., vol. ii. p. 151.)

At numerous other points, as we proceed northwards along the eastern limit of the elevated district, or southwards by May Hill to Tortworth, we find indications of the same great line of fault. Sometimes, as at Oswestry and Higley, these faults have affected the Lower New Red Sandstone as well as the Carboniferous rocks, proving that here at least the elevatory movement was subsequent to the commencement of the Permian epoch. Generally the great marginal fault seems to have formed a nearly vertical cliff, against which the Upper or Triassic portion of the New Red Sandstone was deposited, as in the Shropshire coal-field, at Bewdley, Abberley, Malvern, May Hill and Pyrton Passage.

The Cambrian and Herefordshire area having now become elevated many thousand feet above the eastern region, and the volcanic forces having spent their energy in thrusting up and overturning the syenite and incumbent strata of Malvern, a period of comparative tranquillity ensued. The elevated region had become dry land, while the downcast area remained beneath the sea. The sands and marls of the Triassic series filled up the bed of this sea, while its littoral waves, beating against the syenitic cliffs of Malvern, formed accumulations of conglomerate such as those of Rosemary Rock and the Berrow and Woodbury Hills. The oolitic, cretaceous, and tertiary formations were successively piled upon the triassic rocks, and may possibly have raised this downcast area to the same level as the upcast portion, though there is no evidence that they ever overlaid the latter in the region west of the Severn.

The elevated area meanwhile was undergoing a vast amount of denudation. During the long ages of the Triassic and Oolitic systems, it was doubtless exposed to atmospheric degradation, and supplied the adjacent ocean with much of its sedimentary matter, as has been ably shown by Prof. Ramsay (Mem. Geol. Surv., vol. i. p. 297). The denuding forces which were so active in the Pliocene period terminated these vast operations, and gave to this rugged and dislocated area those smooth undulating outlines which it now generally presents.

I trust that I have now in some degree confirmed and extended the proofs adduced by the geological surveyors of the elevation and subsequent denudation of the Cambrian region, and that I have shown how the peculiar phænomena of the Malvern district may be explained by the supposition of a local development of plutonic energy superadded to a more general upheaving force.

LV. *On the Anticlinal Line of the London and Hampshire Basins.* *By* P. J. Martin, *Esq., F.G.S.*

[Continued from p. 288.]

I HASTEN now to a review of the next in order, namely, the *Subcretaceous Zone.* Immediately that the lower greensand emerges from below the gault, we are presented with a great variety of the subcretaceous diluvium. Here it still consists of a large share of angular flint, mixed up with fragmentary iron-stone (carr-stone?) and sandstone derived from its own rocks, and bearing slight marks of being drifted or rolled. Amongst these *débris,* in two places only I have detected the presence of a very few rounded pebbles; at Hurston Warren near Storrington* on the south side of the Weald, with small chalk pebbles, and near Sevenoaks in Kent;—in both cases, I presume, strays from the lost tertiaries.

The most notable fact in regard to these gravel beds is, that they lie, when in most force, in hollows scooped out of the soft sand rock. Elsewhere they are widely sprinkled over the surface of the country, where the grosser materials seem to have been retained by entanglement in the loose sand. Indeed the only important accumulations of the angular gravels are to be found on the soft or shanklin sands, or upper ferruginous beds of the lower greensand; where, as I have just said, they seem to have been retained mainly by their involvement in the broken sand, and their lodgement in the hollows of the soft rock, which, when cleared out, have all the appearance of being originally scooped out by water moving with great violence. One of the most remarkable beds of this kind is being worked for road material in and about Peasemarsh near Guildford. In its composition it is an exact counterpart to the beds on the hill tops at Fittleworth, Lavington Common, and the ferruginous sand country south of Midhurst†. The prevailing materials are broken flint, chert and sandstone, derived from the lower beds of greensand, and perhaps here and there a stray pebble.

These drifts prevail all round the subcretaceous zone; on the south side as far east as the country north of Lewes‡, and on the north from Peasemarsh to Ashford§; and I doubt not are continued on to Hythe, although I have not followed them so far. But it is a notable fact, that, except in a few instances, these gravels do not lie on the high bold platforms of the lower beds of the formation in question, although they may be found in small quantities on the Weald clay below. In the same manner

* Sussex. † Sussex.
‡ Mantell's Geol. of S.E. of England, p. 29. § Kent.

as the flints have been swept from the outcrop of the malm, but lie on the gault, so they seem not to have taken any hold on the rocky beds of the lower greensand, but are sparingly scattered along the verge of the Weald clay country. I have found a thin coat of broken flints and ironstone in Hartingcombe*, and have traced this into the iron conglomerate of the Weald surface, up to the banks of the Arun. Again, after passing over the high grounds that range from Wolmar Forest† to Warminghurst‡, which are destitute of flints, we find a thin sprinkling, sometimes associated with the hard "clinker" ironstones or "carr-stone" (which are plentifully distributed in the ferruginous shanklin sands), along the Weald clay valley below. On the south side of the Wealden area I find these drifts intruding to the very verge of the Hastings sand country near Shipley§ and West Grinstead Churches; and into the valley of the Adur, from thence toward Ashurst. And drift of this sort has been found by Dr. Mantell at Barcombe in the same line of country‖. On the north side of the Weald, again, the Godalming Hills, Ewhurst Hills and Leith Hills¶, show no flint drift, but it is found in the loose sands of Betchworth and Reigate; and in that line of country obtruding, as it does at Shipley, into the Weald clay at Flanchford, and along the course of the Mole, where it is crossed by the Brighton road. The *plateaux* of the Sevenoaks and Maidstone districts very rarely exhibit angular flint gravel; but the country of the upper beds between these high grounds and the outcrop of the gault abounds, as usual, in the *débris* of all the surrounding strata.

Leaving the subcretaceous line of drift, a few observations will suffice for the consideration of the—

4th, or *Wealden Zone.*—There is nothing in the history of that part of the anticlinal line of the "chalk basins" called the "Weald Denudation" more conclusive as to the agency of strong water currents and the flux and reflux of waves of immeasurable force (immeasurable, I believe, but by our ideas of the removal and transport which we suppose they have effected) than the bare state of the central parts of the Weald.

The country of the Hastings sands rises geographically higher than the sandstone hills and chalk downs that surround it; and it is filled and fortified by strong and tough stone-courses, which gave it that prominence and stability, whilst the Weald clay was yielding to erosion. But that the Weald generally is destitute of diluvium is an opinion which has been too hastily embraced. Both members of the "Wealden" have their appropriate drift; and it is just of that kind which might be expected to be left

* W. Sussex. † Hants. ‡ Sussex. § Sussex.
‖ Geol. of S.E. of England, *loc. cit.* ¶ Surrey.

behind by the retiring waves of the denuding flood, after the previous removal of the thousands of feet of upper coatings.

Soon after I began to turn my attention to these surface-changes, I was attracted to large masses of a ferruginous *breccia* which were frequently ploughed up and brought "to bank" by the labourers, who gave it the name of "Iron-rag." I found afterwards that it was anciently extensively sought for and taken out of the hollows in which it lay, and smelted like bog iron-ore (which it sometimes resembles), when the fields of Sussex were filled with "iron furnaces." On closer inspection too, I found that many hill slopes were enriched with a thin coating of diluvial loam, and especially on the borders of the river-courses, high above the reach of modern alluvium. For the truth of this I may cite the border slopes of the river Arun from Stopham to Rudgwick*, and onward over half the parish of Slinfold towards Horsham,—of the Adur from Henfield into the "forest-ridge"—and of the Medway in the greater part of its course through the Weald clay. Dr. Mantell has observed a modification of the ironrag at Barcombe, Wellingham and Horsted†. And chance some time since threw in my way one of the best opportunities that could occur of observing an instructive exposure of the ferruginous drift. In digging the ditches and fencing an enclosure at Lowfield Heath near Crawley‡, large quantities of the rag were collected and may now be seen mouldering in a heap near the White Lion public-house; and if the sides of the ditches are inspected, numerous sections of the hollows in which this iron conglomerate lay may be seen; corresponding very much with the water-worn depressions filled with gravel on the sand-hills, in the subretaceous zone as before described. Indeed this brecciated drift is to be found all over the Weald. On the Weald-clay country of the west of Sussex it is filled with fragments of chert, with now and then a stray flint. At West Grinstead and near Knepp Castle it is full of flint, mixed with fragments of the Wealden sandy-courses; and on the borders of the forest-ridge it is composed of fragmentary Hastings sandstone with the septaria of the upper parts of the Weald clay, or of the superincumbent Atherfield beds.

But the most important evidences of drift are to be found where they might be best expected,—on the beautiful and fertile slopes of the eastern part of Sussex and the south-east of Kent, where the Rother and its affluents take their courses through the longitudinal fissure valleys of the central line of upheaval. On these slopes, and in these valleys, beds of diluvial loam exist, made up of the washings of the surrounding ridges, and give fertility to localities which would be otherwise of comparatively

* Sussex. † Geol. of S.E. of England, *loc. cit.* ‡ Sussex.

little value. The cuttings of the railway now traversing this line of country from Tunbridge Wells toward Hastings, come conveniently to our aid in identifying the existence of these loams; and I particularly recommend the inspection of a section near Etchingham Church*, where a luxuriant hop-garden is seen standing on a bed of loam at least twenty feet in thickness; and from thence for several miles across the valley of the Rother by Rotherbridge, loam-beds of various thickness are traversed. To any one having leisure and patience for the task, I think it not unlikely that amongst these loams minute fragments of many if not all of the upper beds might be discovered. On a cursory view, their principal materials seem to be derived from the Wealden beds,—the sweepings, as I before observed, of the retiring waters of denudation.

I have said that diluvial bones have not been discovered in any of the drifts below the cretaceous zone. Of this I have a word or two more to say, and an exception to make. That the remains of the animals that perished in the catastrophe we contemplate, should be found most numerous in the ruins of the uppermost strata, was a thing to be expected. But there is another reason why they should be rare in the arenaceous drifts of the subcretaceous group, and that is, the bad preservative quality of these soils. I am in possession of mammal bones from the chalk-rubble of West Burton and Bury; and I have one bone, which I am told is elephant's, from a gravel bed of the ferruginous sand-drift at Cold Waltham†. I am moreover informed by the gravel-digger, who has had much experience in the pits at Peasemarsh‡, that he has taken out bones; but they were invariably found in the clay at the bottom of the bed.

In taking a general view of the arrangement of these drifts, and the constancy of their character throughout so wide an area, we cannot but be struck by a unity of design, and a totality so much in consonance with the other phænomena of the anticlinal line generally, and of the Weald denudation in particular.

Before we finish with the subject of drifts, it will be well to give a little consideration to the question,—Do they exhibit any certain signs of the prevailing direction of the currents which excavated the valleys and carried off the broken materials? If I have succeeded in enabling my readers to realize in their minds the picture of the conjoint action of earthquake and flood which I have in my own, they will be able to understand the confused flux and reflux, and the clash of opposing torrents which must necessarily follow in the train of so extensive a displacement of solid matter; whether the convulsion took place at the bottom of a sea or in the open air; and whether or no it was prolonged

* Sussex. † Sussex. ‡ Surrey.

by a continued heaving and falling (terrene undulations of incalculable violence) of some continuance*. We have not the means of following the great bulk of the displaced materials. Much of it was doubtless cast off over the great synclinals on each side. We have sufficient evidence of this in the loams and the extensive gravel beds in the mixed diluvium of the London basin†. And we have a striking proof of it in the enormous accumulations of the less destructible parts of them in the Brighton "elephant beds;" in the inexhaustible sources of flint-gravel, sometimes of unknown depth, from thence to Chichester, under Portsdown Hill, and further west on the northern slope of the Hampshire basin. But leaving this out of consideration, and supposing these accumulations to be only the tithe part of the lost beds, and the great bulk of them to be lying at the bottom of the German Ocean, and rising out of it in the Cromer Cliffs, or spread over the plains of Westphalia, we are soon convinced that the drift I have attempted to describe, is only the remnants of those materials, and the last leavings of the retiring waters.

The motions of these retiring waters, then, would now, after the great business of excavation was effected, be determined by the arrangement of the surface so left. Whether the centre of the upheaval be in the high grounds of the Weald or in the English Channel nearer the Boulonnais, it is not material to inquire; nor whether the Channel as we now see it had previously to this convulsion any existence or not: most probably it had not. In whatever direction the central movement lay, every wave would have its recoil, and the flood would have so wide a range as to take whatever courses the great boundary lines would dictate. As these boundary lines run for the most part east and west, in the long axis of the upheaval, we expect to see signs of the movement of drift in those directions. Nevertheless there is sufficient proof of cross and contrary movements, and of the frequent deflection of currents in opposite courses. The large accumulation of flints in the transverse vale of Findon‡, is matched by a similar deposit in the long longitudinal one of Bramdene§. The long transverse valleys of Leatherhead and Smitham Bottom|| have at their lower extremities, the one a great accumulation of

* I have not been able to detect any appearance of friction like "slickensides." But such appearances were observed by Buckland and De la Beche in Dorsetshire (Geol. Trans. vol. iv. new series). I attribute the absence of such appearances in the Wealden area to the friable and loose nature of the rocks and the flexibility of the clays.

† This does not militate against the opinion that all the beds to be found there have also suffered denudation, crag and all.

‡ Sussex. § Hants. || Surrey.

flint and rubble near Dorking (spoken of in a former part of this paper), and the other an immense deposit of stiff loam traversed in part by the Merstham-tunnel. Again, much *débris* from all the surrounding beds is lodged in the Peasemarsh valley, apparently favoured by the position of that valley after its earlier excavation; so the great accumulation of strong and fertile loams which make the hop-gardens of Farnham*, might have been brought from all directions,—out of the gullies at Alton†, along the valley of the gault at Bentley, or down from the rear of the Hogsback; perhaps from all of these sources. Cross-currents have mixed the sandstones of the lower greensand beds with the upper at Fittleworth‡ and at Peasemarsh; and much rubble from the former of these has rushed out on the latter through the transverse gullies (north and south) between Petworth and Thakcham§. These instances might be multiplied, and much more might be said about the range and the other phænomena of drift; but it all comes to the conclusion which I drew from these appearances in my earliest essay on this subject, that "to the eye of the practised observer the Weald valley presents the appearance of a great water-channel after a flood:—some parts of it clean and clear of all incumbrance, others loaded with drift; the banks in some parts torn clean away, in others heaped up with rubbish ||;" and, to make the parallel more complete, in the drift of both are to be found the bones or the bodies of animals that have perished in their several catastrophes.

It is proper for the completion of this sketch to say a few words about the "Bassin du Bas Boulonnais," as the French call the eastern extremity of the Weald denudation. Mr. Hopkins has described the signs of upheaval it exhibits correspondingly with the phænomena of like kind on this side of the channel¶. I have enjoyed two opportunities of a cursory inspection of the country, but cannot speak of it critically. M. Rozet has described its diluvium**, which only differs from that of the Weald in having a larger admixture of materials derived from the wreck of the tertiaries, and particularly the fragments of the millstones or "burrstones" of those beds, which we may suppose correspond in position with our Druid stones or grey-wethers. And M. Rozet considers the denudation of the Boulogne country as the work of the same "*débâcle*" that excavated the Weald.

* Surrey. † Hants. ‡ Sussex. § Sussex.

|| Geol. Memoir of Western Sussex, p. 84. London, 1828.

¶ Vide Geol. Trans. vol. vii.

** Description Géognostique du Bas Boulonnais. Paris, 1828.

Unfortunately for the progress of theoretical geology, it is constantly being given up to the domination of a prevailing opinion. I think I speak the sentiments of many faithful observers, and men whose ambition it is to be thought good practical geologists, when I say, that the persevering attempts that have been made (doubtless actuated by the firmest convictions) for the last twenty years, to reduce all theoretical notions of surface-changes to one standard, and that the lowest of constantly existing agencies, have been carried too far; and that however necessary it might have been in the infancy of the science to clip the wings and curb the fancy of its votaries, extreme caution has overdone its work; and that it is become necessary, if we are to make any great advances toward a more perfect knowledge of the nature of these and some other phænomena that pertain to geological dynamics, to return, in part at least, to the doctrines that held sway some quarter of a century ago; to reinvoke the assistance of some long-neglected agencies, and familiarize ourselves with scenes of greater activity than the uniform causation of the present times will afford. The transporting power of icebergs, the glacial abrasion of rock surfaces, the accumulation of moraines, and the "oser" banks of the Scandinavian peninsula have had their share of attention. But the subject of denudation, in its larger sense, has long been held in abeyance. Whether it is that the magnitude of the fields it embraces, or its inexplicable confusion, deters men from entering into the investigation of its causes, the fact is unquestionable, that whilst everybody is speaking of it, or recording instances of its operation on the scenes they describe, nobody makes any attempt to trace these effects to their causes. Or if any such attempt is made, they are carelessly referred to the feeble agencies with which we are surrounded, without due consideration of their adequacy, or the propriety of their application. For in many cases, as for instance in such phænomena as have been lately descanted on by Sir Roderick Murchison in his lectures on the alpine regions of Savoy, agencies have been invoked, which, as he shows, could not have effected the purposes assigned them, in all time; agencies, in fact, which are no agents at all, inapplicable, and to appeal to which is to fall into the "vulgar error" of *non causa pro causa*. Put the case, as of the gradual elevation of the Wealden, and the quiet removal of the materials into the adjoining basins; —then, where are the beaches that should attest such gradual elevations and gentle retirement of the sea, from the newly-elevated lands? Again, could we imagine the extensive excavations under review effected by the slow operation of sea-currents at the bottom of an ocean, be it shallow or profound;—then, whence the smashings and poundings of such mountains of angular flints, without a solitary pebble amongst them to bespeak the scene of a patient attrition? But it is not so much my wish to set aside the hypotheses of others as to establish my own theory. I argue the unity of design and the totality of the phænomena of upheaval in all the long line (a line of 200 miles extent) of the great anticlinal of which the denudation of the Weald is a part, as proof of a sudden and uniform upward movement, to which the water-shed, a systematic arrangement of valleys, begun in fracture and enlarged by aqueous erosion, lacerated escarpments and drift faithfully respond. I have elsewhere said that "an act like the elevation of the anticlinal line which formed the basins of London and Hampshire, or the subsidence of these basins, would be alone sufficient to raise a wave that would drown the habitable parts of half a hemisphere. A few such actions coming into play contemporaneously, or in quick succession, are cause sufficient for a deluge*." And this I venture to reassert; and that the flux and reflux of the waters of such an inundation would be sufficient to remove all the materials here supposed to be excavated,—all the calculations of the power of "waves of transport," or of denuding water-currents, to the contrary notwithstanding.

To those who are startled at the magnitude of such operations, and who are unwilling to admit such a *Deus ex machina* into the great scheme of nature, I recommend an inspection of Plate XL. of De la Beche's "Sections and Views,"—representing the insignificance of mountain elevations, and of a depth of a hundred miles, compared with the diameter of the earth,—and his observation, "How insignificant do our *tremendous* dislocations, *stupendous* mountains, and the like, become, when we contemplate such a figure as that before us†!" At the same time, it is not unlikely that the very persons who advance such objections, grounded on their observation of what is now passing under their eyes, will indulge freely in a speculation on the bursting of a planet, and the distribution of the asteroids so created! There is another class of objectors, who have more show of reason, who would split the difference between the extremes of uniformity, or the slow working-hand of time and catastrophic action, who think that nature has no need, and does not afford evidence of operations of such magnitude, and so would have done that which we see has been done, by a succession of minor convulsions, piecemeal. Then what becomes, in a case like this of the Weald, of a widely-extended and uniform class of phænomena, combined operations bespeaking unity of cause, acting toward and for a perfect and consistent whole? No advance in our knowledge of the

* Phil. Mag. vol. v. p. 119 (1829).

† Sections and Views illustrative of Geological Phænomena, by Henry De la Beche. London, 1830.

denudations of the south of England or investigations into their causes, have been made since the publication of the description of the Weymouth country, and of its phænomena of disturbance, the joint production of Dr. Buckland and Sir H. De la Beche, in the Geological Transactions of 1830*. This country lies at the western extremity of the other great parallel line of upheaval on the south side of the Hampshire basin, which, as Conybeare and Phillips say†, extends at least sixty miles, from the eastern extremity of the Isle of Wight to Abbotsbury in Dorsetshire. This line there is good reason to believe takes its course also in the opposite direction across the Channel, like the foregoing, toward and probably into the French coast.

It would be a task of no great difficulty to bring the phænomena there described into harmonious relation with those of the Weald, and to show their great family likeness and their *synchronism*. There, great perpendicular faults and fissures seem to have been the subordinate agencies and to have done the part of the numerous anticlinals of the Weald. There, *mutatis mutandis*, the same kind of valleys, the joint operation of fracture and aqueous erosion, are to be found, and the same sort of diluvium. But the same orderly arrangement of the drift that we find in the Wealden area is hardly to be expected, from the greater irregularity of the denuded surface; and perhaps also the greater variety of the strata, or formations, concerned in the structure of the country.

After speaking of the inadequacy of existing causes for the production of these surface-changes, the authors of the above-mentioned description say, "The only satisfactory solution we can find is in the waters of a violent inundation; and in these we think we see a cause that bears a due ratio to the effects that have been produced. How far the causes of this inundation may be connected with the elevation of the strata in the immediate neighbourhood or in distant regions, is a subject which at present we conceive it premature to enter into, further than to suggest that the relation of the one to the other may possibly be nearer than has been hitherto apprehended‡." This was written a year after the publication of my "Theory of the Denudation of the Weald," in which I had shown the relation of these phænomena to each other, the arrangement of the fissures of the upheaval, and their enlargement into a system of longitudinal and transverse valleys by aqueous abrasion, and the drainage of the country by their means. If Dr. Buckland had followed out his original exposition of the phænomena of "Valleys of Elevation"

* Geol. Trans. vol. iv. new series.

† Conybeare and Phillips's Outlines of the Geol. of England and Wales, 1822.

‡ Buckland and De la Beche, *loc. cit.*

to its legitimate conclusion, as I think he might have done, with the felicity that usually attended his speculations and researches, it is not saying too much to suppose that we might have been spared all the unprofitable labour that has been bestowed on the supposed operations of the pleistocene sea; which covered a great part of the continent of Europe, and all the south of England:—a sea teeming with icebergs, depositing here and there the materials they held in suspension, with the remains of animals of the higher orders that floated in from the adjoining countries! A fallacy that has produced more fruitless speculation, and the exposition of more false facts and false observations than are otherwise to be found in the recent records of geology. To conclude: the obvious inferences to be drawn from what we have seen, are these:—

Since the deposition of the tertiary beds a great and sudden upheaval of some parts, and perhaps contemporaneous subsidence of others, took place over a widely extended area; perhaps over the greater part of the south of England.

That the phænomena of the arrangement of valleys, and of watershed, over all the length and breadth of the anticlinal line of the London and Hampshire basins, respond to this convulsion.

That this convulsion was attended or immediately followed by a devastating flood, which excavated and carried off the broken materials, and only left a small quantity of drift to attest its agency; and that this inundation subsiding, the waters withdrew at once, a period of tranquillity succeeding, which has continued up to the present time. Or, in other words, that this is the most modern change of any magnitude that has come over this part of the world: it would be hardly proper to say our island, for in all probability this country did not previously exist in that form.

That although these convulsions may have been synchronous with, or in part the effect of changes "in distant regions," as hinted at by Dr. Buckland and Sir Henry De la Beche, yet to overlook these evidences of local disturbance, and not to consider them the proximate cause of inundation and denudation, appears to be a gratuitous dereliction of the proof before us. A part of the truth at least, and that of the greatest importance, is at hand; the rest remains as yet at a distance*.

* Since the publication of the greater part of this memoir, the writer's attention has been directed to Mr. Prestwich's "Geological Inquiry on the Water-bearing Strata of the country around London." He agrees most cordially with all that Mr. Prestwich has advanced respecting that part of the country which enters into the area under review. On this and some other explanatory matters he proposes to make a few observations, which will be the subject of a postscript to appear in the next Number of the Philosophical Magazine.

LVI. *On the Motion of a Pendulum affected by the Earth's Rotation.* By Septimus Tebay, *Mathematical Master, Bruce's Academy, Newcastle-upon-Tyne.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SHOULD you deem the following brief solution of this interesting problem worthy of notice, its insertion in an early Number of your valuable periodical will much oblige

Your obedient Servant,

Septimus Tebay.

Let the centre of the earth be the origin, and its axis the axis of x, and at the commencement of the motion let the plane xz coincide with the meridian. Let r be the radius of the earth, l the length of the pendulum, β the angular velocity of the earth, θ the inclination of the plane of the pendulum to the plane of the meridian, $\epsilon, \epsilon', \epsilon''$ the directing angles of the vertical line, xyz the coordinates of the centre of oscillation, ρ the line from this point to the origin, η, η', η'' the directing angles of ρ, and χ the inclination of ρ to the vertical.

The dynamical conditions of the problem are represented by the equation

$$\left(\frac{d^2x}{dt^2}+\mathrm{G}\cos\epsilon\right)\delta x+\left(\frac{d^2y}{dt^2}+\mathrm{G}\cos\epsilon'\right)\delta y+\left(\frac{d^2z}{dt^2}+\mathrm{G}\cos\epsilon''\right)\delta z=0 \quad (1.)$$

(Poisson, *Traité de Mécanique*, No. 531), G being the whole attraction of the earth, supposed at rest, on a particle at its surface.

By the geometry we have

$$x=\rho\cos\eta,\quad y=\rho\cos\eta',\quad z=\rho\cos\eta''.$$

And ϵ being equal to the colatitude of the place,

$$\cos\epsilon'=\sin\epsilon\sin\beta t,\quad \cos\epsilon''=\sin\epsilon\cos\beta t.$$

Let $\epsilon+\phi$ be the polar distance of the ball of the pendulum, and ψ its longitude measured from the meridian of the place, ϕ and ψ being necessarily small. We have

$$\begin{aligned}\cos\eta&=\cos(\epsilon+\phi),\\ \cos\eta'&=\sin(\epsilon+\phi)\sin(\beta t+\psi),\\ \cos\eta''&=\sin(\epsilon+\phi)\cos(\beta t+\psi).\end{aligned}$$

Also, putting $r+l=\mathrm{R}$, we have

$$\mathrm{R}^2+\rho^2-2\mathrm{R}\rho\cos\chi=l^2.$$

Hence, as far as small quantities of the second order,

$$\rho=\mathrm{R}-\sqrt{l^2-\mathrm{R}^2\chi^2}=r+\frac{\mathrm{R}^2}{2l}\chi^2.$$

But

$$\phi=\chi\cos\theta,\ \ \psi\sin\epsilon=\chi\sin\theta,$$

retaining only infinitesimals of the first order. Whence

$$\chi^2=\phi^2+\psi^2\sin^2\epsilon,$$

$$\frac{\psi}{\phi}\sin\epsilon=\tan\theta.$$

Consequently

$$\rho=r+\frac{R^2}{2l}(\phi^2+\psi^2\sin^2\epsilon);$$

and therefore,

$$x=r\cos(\epsilon+\phi)+\frac{R^2}{2l}(\phi^2+\psi^2\sin^2\epsilon)\cos\epsilon,$$

$$y=r\sin(\epsilon+\phi)\sin(\beta t+\psi)+\frac{R^2}{2l}(\phi^2+\psi^2\sin^2\epsilon)\sin\epsilon\sin\beta t,$$

$$z=r\sin(\epsilon+\phi)\cos(\beta t+\psi)+\frac{R^2}{2l}(\phi^2+\psi^2\sin^2\epsilon)\sin\epsilon\cos\beta t.$$

Substituting therefore in (1.), observing that the *virtual velocities* $\delta\phi$, $\delta\psi$ are geometrically independent, and neglecting small quantities of orders higher than the first, we obtain

$$\frac{d^2\phi}{dt^2}+A\frac{d\psi}{dt}+B\phi+C=0, \quad \dots \quad (2.)$$

$$\frac{d^2\psi}{dt^2}+A'\frac{d\phi}{dt}+B'\psi\ \ =0, \quad \dots \quad (3.)$$

in which

$$A=-\beta\sin 2\epsilon,\ B=\frac{g}{l}-\beta^2\cos 2\epsilon,\ C=-\frac{\beta^2}{2}\sin 2\epsilon,$$

$$A'=\beta\cos\epsilon,\ B'=\frac{g}{l}.$$

Write $\Omega-\frac{C}{B}$ for ϕ, and equations (2.), (3.) become

$$\frac{d^2\Omega}{dt^2}+A\frac{d\psi}{dt}+B\Omega=0. \quad \dots \quad (4.)$$

$$\frac{d^2\psi}{dt^2}+A'\frac{d\Omega}{dt}+B'\psi=0. \quad \dots \quad (5.)$$

These equations are linear and of the second order. We shall obtain a particular solution of them by assuming

$$\Omega=Re^{mt},\ \ \psi=Rke^{mt}.$$

Substituting in (4.) and (5.), we have for determining k, m,

$$m^2+Akm+B=0,$$

$$km^2+A'm+B'k=0.$$

Eliminating k,

$$m^4+(B+B'-AA')m^2+BB'=0.$$

Denote the roots of this equation by

$$m'\sqrt{-1},\ -m'\sqrt{-1},\ m''\sqrt{-1},\ -m''\sqrt{-1};$$

and let the corresponding values of k be

$$k',\ -k',\ k'',\ -k''.$$

The complete solution is

$$\Omega=R_1e^{tm'\sqrt{-1}}+R_2e^{-tm'\sqrt{-1}}+R_3e^{tm''\sqrt{-1}}+R_4e^{-tm''\sqrt{-1}},$$

$$\psi=R_1k'e^{tm'\sqrt{-1}}-R_2k'e^{-tm'\sqrt{-1}}+R_3k''e^{tm''\sqrt{-1}}-R_4k''e^{-tm''\sqrt{-1}}.$$

The constants R_1, R_2, R_3, R_4 may be determined from the initial circumstances of the motion, namely when $t=0$, in which case we shall also have

$$\frac{d\Omega}{dt}=0,\ \frac{d\psi}{dt}=0.$$

Let Ω_1, ψ_1 be the initial values of Ω, ψ. We shall have

$$\Omega_1=R_1+R_2+R_3+R_4,$$
$$\psi_1=R_1k'-R_2k'+R_3k''-R_4k'',$$
$$0=R_1m'-R_2m'+R_3m''-R_4m'',$$
$$0=R_1k'm'+R_2k'm'+R_3k''m''+R_4k''m''.$$

Whence

$$R_1=-\frac{m''}{2}\left\{\frac{\Omega_1k''}{k'm'-k''m''}+\frac{\psi_1}{k''m'-k'm''}\right\},$$

$$R_2=-\frac{m''}{2}\left\{\frac{\Omega_1k''}{k'm'-k''m''}-\frac{\psi_1}{k''m'-k'm''}\right\},$$

$$R_3=\frac{m'}{2}\left\{\frac{\Omega_1k'}{k'm'-k''m''}+\frac{\psi_1}{k''m'-k'm''}\right\},$$

$$R_4=\frac{m'}{2}\left\{\frac{\Omega_1k'}{k'm'-k''m''}-\frac{\psi_1}{k''m'-k'm''}\right\}.$$

And therefore,

$$\Omega=\frac{\psi_1}{2(k''m'-k'm'')}\{-m''(e^{tm'\sqrt{-1}}-e^{-tm'\sqrt{-1}})+m'(e^{tm''\sqrt{-1}}-e^{-tm''\sqrt{-1}})\}+$$
$$\frac{\Omega_1}{2(k'm'-k''m'')}\{-k''m''(e^{tm'\sqrt{-1}}+e^{-tm'\sqrt{-1}})+km'(e^{tm''\sqrt{-1}}+e^{-tm''\sqrt{-1}})\},$$

$$\psi=\frac{\psi_1}{2(k''m'-k'm'')}\{-k'm''(e^{tm'\sqrt{-1}}+e^{-tm'\sqrt{-1}})+k''m'(e^{tm''\sqrt{-1}}+e^{-tm''\sqrt{-1}})\}+$$
$$\frac{\Omega_1}{2(k'm'-k''m'')}\{-m''(e^{lm'\sqrt{-1}}-e^{-tm'\sqrt{-1}})+m'(e^{tm''\sqrt{-1}}-e^{-tm''\sqrt{-1}})\}.$$

These equations may be also written as follows:—

$$\Omega=\frac{\psi_1\sqrt{-1}}{k''m'-k'm''}\{-m''\sin m't+m'\sin m''t\}+$$
$$\frac{\Omega_1}{k'm'-k''m''}\{-k''m''\cos m't+k'm'\cos m''t\}\quad . \; . \; . \quad (6.)$$

$$\psi=\frac{\psi_1}{k''m'-k'm''}\{-k'm''\cos m't+k''m'\cos m''t\}+$$
$$\frac{\Omega_1\sqrt{-1}}{k'm'-k''m''}\{-m''\sin m't+m'\sin m''t\}.\quad . \; . \; . \; . \quad (7.)$$

The principal object of the problem appears to be the determination of θ when χ is a maximum for a given number of vibrations. For maximum values of χ we shall have

$$\phi\frac{d\phi}{dt}+\sin^2\epsilon\frac{d\psi}{dt}=0.\quad . \; . \; . \; . \; . \; . \; . \; . \quad (8.)$$

The preceding equations give all the circumstances of the motion for small oscillations, but I have not yet attempted their solution in any particular case.

Newcastle-upon-Tyne,
September 5, 1851.

LVII. *On the Motion of a Free Pendulum.* *By the* Rev. R. R. Anstice, *M.A.**

I. A PLANE is rigidly connected with an axis, which axis rotates with an uniform angular velocity $=b$, carrying the plane along with it. A material particle is constrained to move in the said plane, and also acted upon by a central attractive force varying directly as the distance, and situated in the intersection of the axis and plane. To determine the motion.

This I shall afterwards prove will be the same as the *small* oscillations of a simple pendulum at the earth's surface, free to move in any azimuth: b will then be the angular velocity of the earth's rotation. The axis will correspond in direction with that of the earth, and the plane with the horizontal plane at the place of observation.

Refer the motion to three axes mutually at right angles. Take origin at intersection of axis of motion and plane; make axis of motion axis of z. Then the plane of xy will correspond in direction with the earth's equator.

Let l, m, n be the cosines of inclination to the axes of x, y and z of the normal to the rotating plane at any time t. Then n will be constant and $=$ sine latitude. l and m will be func-

* Communicated by the Author.

tions of t; and their values, as I shall show hereafter, must be such as to verify the equations

$$\frac{d_t^2 l}{l} = \frac{d_t^2 m}{m} = -b^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Let a^2 be the central force at the unit of distance, N the normal (accelerative) force of reaction of the rotating plane. We have then the following equations:

$$\frac{d_t^2 l}{l} = \frac{d_t^2 m}{m} = -b^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\left.\begin{array}{l} n = \text{constant} = \text{sine latitude} \\ l^2 + m^2 + n^2 = 1 \end{array}\right\} \quad . \quad . \quad (2.)$$

$$lx + my + nz = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$\left.\begin{array}{l} d_t^2 x = -a^2 x + \mathrm{N}l \\ d_t^2 y = -a^2 y + \mathrm{N}m \\ d_t^2 z = -a^2 z + \mathrm{N}n \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

Multiply the first of equations (4.) by l, the second by m, the third by n, and add; and we find (attending to equations (2.) and (3.)),

$$\mathrm{N} = l d_t^2 x + m d_t^2 y + n d_t^2 z. \quad . \quad . \quad . \quad . \quad (5.)$$

Again, multiply the first by $d_t l$, the second by $d_t m$, and add; and we have, attending to equation (2.),

$$d_t l . d_t^2 x + d_t m . d_t^2 y + a^2 (x d_t l + y d_t m) = 0. \quad . \quad . \quad : \quad (6.)$$

Now u and v being any functions of t, we have

$$d_t^2 (uv) = u d_t^2 v + 2 d_t u . d_t v + v d_t^2 u$$
$$= u d_t^2 v - v d_t^2 u + 2 d_t (v d_t u);$$
$$\therefore\ u d_t^2 v = d_t^2 (uv) - 2 d_t (v d_t u) + v d_t^2 u. \quad . \quad . \quad . \quad (7.)$$

In this formula write in succession,

$$\left.\begin{array}{llll} \text{in place of } u, & l, & \text{and in place of } v, & x \\ \quad\ldots & m, & \quad\ldots & y \\ \quad\ldots & n, & \quad\ldots & z \end{array}\right\}, \text{ and add;}$$

$$\therefore \left.\begin{array}{l} l d_t^2 x \\ + m d_t^2 y \\ + n d_t^2 z \end{array}\right\} = d_t^2 \left\{\begin{array}{l} lx \\ + my \\ + nz \end{array}\right\} - 2 d_t \left\{\begin{array}{l} x d_t l \\ + y d_t m \\ + z d_t n \end{array}\right\} + \left\{\begin{array}{l} x d_t^2 l \\ + y d_t^2 m \\ + z d_t^2 n \end{array}\right\}.$$

That is, by help of equations (1.), (3.) and (5.),

$$\mathrm{N} = -2 d_t (x d_t l + y d_t m) - b^2 (lx + my);$$

and again by (3.),

$$\mathrm{N} = -2 d_t (x d_t l + y d_t m) + b^2 nz. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

Again, in formula (7.) write in succession,

$$\left.\begin{array}{l}\text{in place of } u,\ d_t l, \text{ and in place of } v, x\\ \quad\ldots\qquad d_t m, \qquad\ldots\qquad\qquad y\end{array}\right\}, \text{ and add;}$$

$$\therefore \left(\begin{array}{c}d_t l\, d_t^2 x\\ +d_t m d_t^2 y\end{array}\right)=d_t^2\left(\begin{array}{c}x d_t l\\ +y d_t m\end{array}\right)-2d_t\left(\begin{array}{c}x d_t^2 l\\ +y d_t^2 m\end{array}\right)+\left(\begin{array}{c}x d_t^3 l\\ +y d_t^3 m\end{array}\right);$$

or, by help of (1.),

$$=d_t^2\left(\begin{array}{c}x d_t l\\ +y d_t m\end{array}\right)+2b^2 d_t\left(\begin{array}{c}lx\\ +my\end{array}\right)-b^2\left(\begin{array}{c}x d_t l\\ +y d_t m\end{array}\right);$$

and again by (3.),

$$=d_t^2\left(\begin{array}{c}x d_t l\\ +y d_t m\end{array}\right)-2b^2 n d_t z-b^2\left(\begin{array}{c}x d_t l\\ +y d_t m\end{array}\right).$$

Therefore, substituting this value in equation (6.),

$$d_t^2(x d_t l+y d_t m)+(a^2-b^2)(x d_t l+y d_t m)-2b^2 n d_t z=0. \quad . \quad (9.)$$

Also, substituting in the last of equations (4.) the value of N given by (8.),

$$d_t^2 z+(a^2-b^2n^2)z+2nd_t(x d_t l+y d_t m)=0. \quad . \quad . \quad (10.)$$

Now if, retaining the same origin, we refer the particle to rectangular coordinates X and Y in the rotating plane itself, and make the line of nodes the axis of X, I shall presently show that we must have

$$\left.\begin{array}{r}z=\sqrt{1-n^2}\,\mathrm{Y}\\ x d_t l+y d_t m=b\sqrt{1-n^2}.\mathrm{X}\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

Substituting these values, equations (9.) and (10.) become

$$\left.\begin{array}{l}d_t^2\mathrm{X}+(a^2-b^2)\mathrm{X}-2bnd_t\mathrm{Y}=0\\ d_t^2\mathrm{Y}+(a^2-b^2n^2)\mathrm{Y}+2bnd_t\mathrm{X}=0\end{array}\right\}; \quad . \quad . \quad (12.)$$

which are the equations of motion in their simplest form. It remains to establish equations (1.) and (11.).

Let, then, i be the inclination of the rotating plane to the plane of xy, θ the inclination of line of nodes of said plane to axis of x. Then of course i = colatitude, $\cos i=n$, and also

$$d_t\theta=b. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

Then

$$\left.\begin{array}{l}x=\mathrm{X}\cos\theta-\mathrm{Y}\sin\theta\cos i\\ y=\mathrm{X}\sin\theta+\mathrm{Y}\cos\theta\cos i\\ z=\mathrm{Y}\sin i=\sqrt{1-n^2}\,\mathrm{Y}\end{array}\right\}, \quad . \quad . \quad . \quad . \quad (14.)$$

which also is the first of equations (11.) Multiply the first by $\sin\theta\sin i$, the second by $-\cos\theta\sin i$, the third by $\cos i$, and add;

$$\therefore\ x\sin\theta\sin i-y\cos\theta\sin i+z\cos i=0.$$

Comparing this with (3.), we get

$$l=\sin\theta\sin i \quad m=-\cos\theta\sin i \quad n=\cos i.$$

From these we find, by help of (13.),

$$\left.\begin{array}{l} d_t l=b\sin i\cos\theta \\ d_t^2 l=-b\sin i\sin\theta \end{array}\right\} \quad \begin{array}{l} d_t m=b\sin i\sin\theta \\ d_t^2 m=b^2\sin i\cos\theta. \end{array}$$

Therefore

$$\frac{d_t^2 l}{l}=\frac{d_t^2 m}{m}=-b^2,$$

which are equations (1.) And also

$$xd_t l+yd_t m=b\sin i(x\cos\theta+y\sin\theta);$$

that is, by help of (14.),

$$=b\sqrt{1-n^2}\,X,$$

which is the second of equations (11.).

II. Solution of the equations of motion.

We have then

$$\left.\begin{array}{l} d_t^2 X+(a^2-b^2)X-2bnd_t Y=0 \\ d_t^2 Y+(a^2-b^2n^2)Y+2bnd_t X=0 \end{array}\right\} \quad . \quad . \quad . \quad (1.)$$

Let

$$\left.\begin{array}{l} X=A\sin(kt+\alpha) \\ Y=A'\cos(kt+\alpha) \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

be a particular integral; A, A′, k, and α being constants. Therefore substituting in the equations of motion, we have

$$\left.\begin{array}{l} A(a^2-b^2-k^2)+2bnkA'=0 \\ A'(a^2-b^2n^2-k^2)+2bnkA=0 \end{array}\right\}; \quad . \quad . \quad (3.)$$

$$\therefore\ (a^2-b^2-k^2)(a^2-b^2n^2-k^2)-4b^2n^2k^2=0. \quad . \quad (4.)$$

Therefore if k', k'' are the two *positive* values of k which verify equation (4.), the general solution of (1.) will be

$$\left.\begin{array}{l} X=A\sin(k't+\alpha)+B\sin(k''t+\beta) \\ Y=A'\cos(k't+\alpha)+B'\cos(k''t+\beta) \end{array}\right\} \quad . \quad . \quad . \quad (5.)$$

Here k' and k'' are definite constants, determinable by equation (4.); A, B, α, β indefinite, being the arbitraries of the problem.

Also A′ and B′ are given in terms of A and B by the equations

$$\left.\begin{array}{l} \dfrac{A'}{A}=\dfrac{k'^2-a^2+b^2}{2bnk'}=\dfrac{2bnk'}{k_{\prime}^2-a^2+n^2b^2}=\pm\sqrt{\dfrac{k_{\prime}^2-a^2+b^2}{k_{\prime}^2-a^2+n^2b^2}} \\ \dfrac{B'}{B}=\dfrac{k_{\prime\prime}^2-a^2+b^2}{2bnk_{\prime\prime}}=\dfrac{2bnk_{\prime\prime}}{k_{\prime\prime}^2-a^2+n\,b^2}=\pm\sqrt{\dfrac{k_{\prime\prime}^2-a^2+b^2}{k_{\prime\prime}^2-a^2+n^2b^2}} \end{array}\right\} \quad (6.)$$

The problem will be much simplified if we suppose the period of oscillation of the body very small compared with the period of rotation of the plane. Then b will be very small compared with a, so that its square, &c. may be neglected, and our equations become

$$\left.\begin{array}{l} d_t^2\mathrm{X}+a^2\mathrm{X}-2bnd_t\mathrm{Y}=0 \\ d_t^2\mathrm{Y}+a^2\mathrm{Y}+2bnd_t\mathrm{X}=0 \end{array}\right\}$$

$$(k^2-a^2)^2-4b^2n^2k^2=0\,;$$

$$\therefore k'=a+bn \quad k'=a-bn.$$

Also
$$\frac{\mathrm{A}'}{\mathrm{A}}=+1 \quad \frac{\mathrm{B}'}{\mathrm{B}}=-1,$$

$$\left.\begin{array}{l} \therefore \mathrm{X}=\mathrm{A}\sin(at+\alpha+bnt)+\mathrm{B}\sin(at+\beta-bnt) \\ \quad\ \mathrm{Y}=\mathrm{A}\cos(at+\alpha+bnt)-\mathrm{B}\cos(at+\beta-bnt) \end{array}\right\} \quad . \quad . \quad (7.)$$

Now were the term bnt involved in these equations constant instead of a function of the time, the orbit we know would be an ellipse round the centre, or a straight line. That term, however, contains t, and will in process of time become sensible; but as it alters with extreme slowness, we may consider it as sensibly constant during one oscillation of the body, and determine the elements of the ellipse on that hypothesis. To do this, consider for a moment the equations

$$\left.\begin{array}{l} x=\mathrm{C}\sin(at+\epsilon) \\ y=\mathrm{D}\cos(at+\epsilon) \end{array}\right\},$$

C, D, a, and ϵ being constants;

$$\therefore \left(\frac{x}{\mathrm{C}}\right)^2+\left(\frac{y}{\mathrm{D}}\right)^2=1\,;$$

and the orbit is in this case an ellipse, whose axes coincide with those of the coordinates, and $=$ 2C, 2D respectively.

But if these axes, instead of coinciding with, were inclined at an angle ϕ to the coordinate axes, and X and Y are the coordinates in that case, we have

$$\mathrm{X}=x\cos\phi-y\sin\phi=\mathrm{C}\sin(at+\epsilon)\cos\phi-\mathrm{D}\cos(at+\epsilon)\sin\phi$$

$$\mathrm{Y}=x\sin\phi+y\cos\phi=\mathrm{C}\sin(at+\epsilon)\sin\phi+\mathrm{D}\cos(at+\epsilon)\cos\phi.$$

That is,

$$\left.\begin{array}{l} \mathrm{X}=\frac{\mathrm{C}}{2}\big(\sin(at+\epsilon+\phi)+\sin(at+\epsilon-\phi)\big) \\ \quad +\frac{\mathrm{D}}{2}\big(-\sin(at+\epsilon+\phi)+\sin(at+\epsilon-\phi)\big) \end{array}\right\}$$

$$\left.\begin{array}{l} \mathrm{Y}=\frac{\mathrm{C}}{2}\big(-\cos(at+\epsilon+\phi)+\cos(at+\epsilon-\phi)\big) \\ \quad +\frac{\mathrm{D}}{2}\big(\cos(at+\epsilon+\phi)+\cos(at+\epsilon-\phi)\big) \end{array}\right\}$$

Or

$$\left.\begin{array}{l} X=\dfrac{C+D}{2}\sin(at+\epsilon-\phi)+\dfrac{C-D}{2}\sin(at+\epsilon+\phi) \\ Y=\dfrac{C+D}{2}\cos(at+\epsilon-\phi)-\dfrac{C-D}{2}\cos(at+\epsilon+\phi) \end{array}\right\} . \quad (8.)$$

By comparing equations (7.) and (8.), we get at once

$$\left.\begin{array}{ll} \dfrac{C+D}{2}=A & \dfrac{C-D}{2}=B \\ \epsilon-\phi=\alpha+bnt & \epsilon+\phi=\beta-bnt \end{array}\right\};$$

$$\left.\begin{array}{l} \therefore C=A+B \quad D=A-B \\ \phi=\dfrac{\beta-\alpha}{2}-bnt \end{array}\right\} . \quad . \quad . \quad . \quad . \quad (9.)$$

Therefore equations (7.) refer to an ellipse, whose axes are constant, and $=2(A+B)$, $2(A-B)$ respectively; but the direction of which axes have an uniform angular motion of regression (*i. e.* contrary to that of the earth), and which $=bn=$ earth's angular velocity $\times$ sine latitude. If one of the two, $A+B$, $A-B=0$. The motion in that case will be rectilinear.

III. It now only remains to prove (what is in fact self-evident) that the problem already discussed is that of the pendulum at the earth's surface free to move in any azimuth, provided the oscillations thereof are *small.*

Consider, then, the motion of a material particle acted on by gravity, and constrained to move in a spherical surface attached to the earth and rotating with it.

Make the earth's axis the axis of z, and take origin at the point where the vertical of the place of observation cuts the same.

Let l, m, n be the cosines of inclination of the vertical to the axes of x, y and z; $\therefore n$ is constant, and $=$ sine latitude.

Let R $=$ distance of particle when at lowest point from origin; $\therefore$ Rl, Rm, Rn will be coordinates of lowest point.

Let $Rl+x$, $Rm+y$, $Rn+z$ be coordinates of particle at time t; $r=$ radius of spherical surface;

$$\therefore (rl-x)^2+(rm-y)^2+(rn-z)^2=r^2 \quad . \quad . \quad (1.)$$

will be the equation to the surface;

$$l-\frac{x}{r}, \quad m-\frac{y}{r}, \quad n-\frac{z}{r}$$

will be the cosines of inclination of the normal of said surface.

Let g be the force of gravity at the given place; l', m', n' the cosines of inclination of the direction in which it acts.

The normal accelerative force of reaction may be divided into two; one constant, the same as is exercised when there is no oscillation, and the particle remains in (apparent) rest at its lowest point; this we will call K: another variable, produced by the motion, which call N. The whole force therefore $=\text{K}+\text{N}$.

Our equations of motion therefore are

$$\left.\begin{aligned} d_t^2(\text{R}l+x) &= -gl'+(\text{K}+\text{N})\left(l-\frac{x}{r}\right) \\ d_t^2(\text{R}m+y) &= -gm'+(\text{K}+\text{N})\left(m-\frac{y}{r}\right) \\ d_t^2(\text{R}n+z) &= -gn'+(\text{K}+\text{N})\left(n-\frac{z}{r}\right) \end{aligned}\right\} \quad . \quad . \quad (2.)$$

Also, as before,

$$\left.\begin{aligned} \frac{d_t^2 l}{l} = \frac{d_t^2 m}{m} &= -b^2 \\ n = \text{constant} & \end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

b being the angular velocity of the earth; and our equations become

$$\left.\begin{aligned} d_t^2 x &= -gl'+(\text{K}+\text{N}+\text{R}b^2)l-(\text{K}+\text{N})\frac{x}{r} \\ d_t^2 y &= -gm'+(\text{K}+\text{N}+\text{R}b^2)m-(\text{K}+\text{N})\frac{y}{r} \\ d_t^2 z &= -gn'+(\text{K}+\text{N})n-(\text{K}+\text{N})\frac{z}{r} \end{aligned}\right\} \quad . \quad (4.)$$

Now these equations must be satisfied when there is no oscillation, and the particle remains in (apparent) rest at its lowest point. In which case

$$0=\text{N}=x=y=z.$$

Consequently we must have

$$\left.\begin{aligned} 0 &= -gl'+(\text{K}+\text{R}b^2)l \\ 0 &= -gm'+(\text{K}+\text{R}b^2)m \\ 0 &= -gn'+\text{K}n \end{aligned}\right\}. \quad . \quad . \quad . \quad . \quad (5.)$$

If in these equations we bring the term involving g to the other side, square and add, attending to the relations

$$l^2+m^2+n^2=l'^2+m'^2+n'^2=1,$$

we find

$$g^2=(\text{K}+\text{R}b^2)^2(1-n^2)+\text{K}^2n^2, \quad . \quad . \quad . \quad . \quad (6.)$$

from which quadratic K may be determined in terms of known constants.

From the same equations we may also find l', m' and n', in terms of l, m and constants. But this is not necessary for what follows.

Now if we subtract each of equations (5.) from the corresponding one of equations (4.), our equations of motion become

$$\left.\begin{aligned} d_t^2x &= -(\mathrm{K}+\mathrm{N})\frac{x}{r}+\mathrm{N}l \\ d_t^2y &= -(\mathrm{K}+\mathrm{N})\frac{y}{r}+\mathrm{N}m \\ d_t^2z &= -(\mathrm{K}+\mathrm{N})\frac{z}{r}+\mathrm{N}n \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

Now suppose the oscillations very small. Then x, y, z, and N will be small quantities whose squares and products may be neglected. Therefore equation (1.) becomes

$$lx+my+nz=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

Equations (7.) become (calling $\frac{\mathrm{K}}{r}=a^2$)

$$\left.\begin{aligned} d_t^2x &= -a^2x+\mathrm{N}l \\ d_t^2y &= -a^2y+\mathrm{N}m \\ d_t^2z &= -a^2z+\mathrm{N}n \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

But equations (3.), (8.), and (9.) are precisely the equations of motion of the former problem; and the two problems are therefore identical.

LVIII. *On the Effect of the Rotation of the Earth upon the Flight of a Projectile.* *By* Captain E. M. Boxer, *R.A.**

To the Editors of the Philosophical Magazine and Journal.

Mill Hill, Woolwich,
June 10, 1851.

Gentlemen,

Having lately been investigating a curious question with regard to the rotation of the earth, viz. the amount of its effect upon a projectile in causing it, during its flight, to deflect from the object to which it was directed, or more correctly speaking, the object to alter its position with regard to the path of the shot, the data, so far as the range and time of flight are concerned, not being assumed, but taken from actual practice, I have been surprised at the result. Although I do not consider it to be of any practical importance in the present state of gunnery, yet perhaps at some future time such perfection may be obtained in the machine from which the shot is propelled, as well as in the projectile itself, as to make it worth while taking

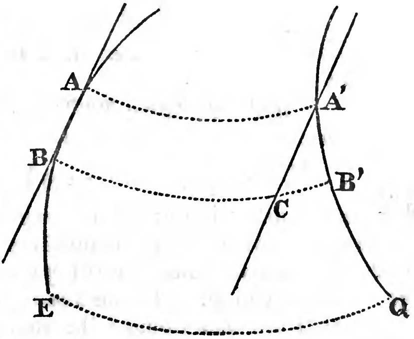
* It is due to the author to state, that this paper was received by us on the 17th of June last; its publication has been delayed owing to great press of matter.—Edits.

into account the rotation of the earth: but independently of this consideration, it becomes a very interesting question on account of the amount of effect that is produced.

When I first heard of M. Foucault's pendulum experiment, I felt perfectly satisfied in my own mind that the principle was correct, by imagining the case of a projectile discharged at an object at some distance in the line of the meridian; and I communicated to Professor Barlow a solution of the question, as to the apparent deviation of the plane of the pendulum in different latitudes, by determining the angular velocity of the tangent to the meridian, previously to any similar demonstration appearing in print. Although this may not be the most elegant solution, I think it is more easily comprehended by the majority of persons. The investigation of the following problem is according to this method.

In the experimental practice of 1839, a 56-pounder of 97 cwt. with a charge of 17 lbs. of powder and an elevation of 35°, projected a ball 5600 yards, the time of flight was 34″. What effect would the rotation of the earth have in causing the shot to fall to the right or left of the object fired at, assuming the latitude of the place as 52°?

Suppose the earth to be a perfect sphere, and a geographical mile equal to 2000 yards, and for the sake of simplicity the gun fired due south.



Let AEQA′ represent a portion of the terrestrial surface between the parallel of latitude AA′ and the equator EQ, and let BB′ represent another parallel of latitude, distant from the former 5600 yards, or 2·8 geographical miles the range of the shot. Let a gun be supposed to be placed at A, and fired at an object at B in the meridian. The time of flight of the shot being 34″, which is equal to 0′·566, therefore during the time of flight of the shot, the earth will have passed through 8′·513 of space. Now suppose at the end of that time the position of the gun to be A′, AA′ being equal to 8′·513; and the object fired at to be at B′, BB′ being also equal to 8′·513. But the ball participating in the motion of the point A will have arrived only at C, BC being equal to AA′. Considering this small portion of the terrestrial surface as a plane, the position of the shot may be found by drawing A′C parallel to AB, and B′C will be the difference of length of the two arcs AA′ and BB′, which will be the deflection of the shot. It will be observed that the two tangents AB, A′C are assumed to be parallel; but this is not strictly correct; for the same reason as in the pendulum experiment, the apparent revolution of the plane of the pendulum at any place upon the surface of the earth will not be $\frac{23 \cdot 934}{\sin \text{lat}}$ hours. And the reason of it is this: the path of the point A when projected upon a horizontal plane will be a *curve*; therefore in the case of the projectile, the shot only receiving an *impulse* due to the earth's rotation at the point A from which position it is fired, the two tangents manifestly will not be perfectly parallel; but the correction from this cause would be so small as not to be of any moment in an approximation of this sort. In the case of the pendulum, the path of the point of suspension when projected upon a horizontal plane being a curve, unless the arc of vibration be infinitely small, the law of inertia will cause the ball to take an elliptical motion, and an apsidal motion will be the result. We will therefore take the two tangents to be parallel, or rather AA′ to be equal to BC,

$$2 \cdot 8 \text{ miles } = 2' \ 48''.$$

The latitude of the place being 52°,

$$51^\circ \ 57' \ 12'' \text{ will be that of B.}$$

The circumference of the earth being 21,600 geographical miles, the arc

$$\left.\begin{aligned} AA' &= 8 \cdot 513 \cos EA \\ \text{arc } BB' &= 8 \cdot 513 \cos EB \end{aligned}\right\} \text{in geographical miles;}$$

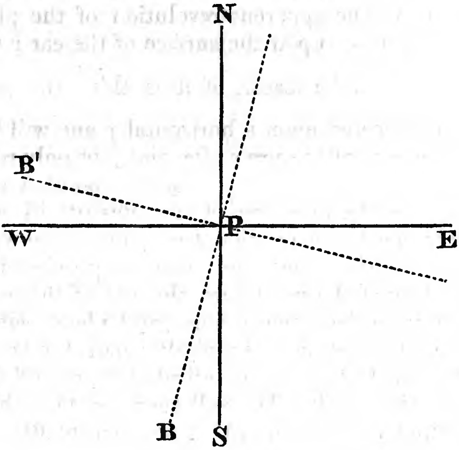
$$\therefore BC = 8 \cdot 513 (\cos EB - \cos EA) = 10 \cdot 914 \text{ yards,}$$

which is the deflection of the shot when fired due south.

The motion of the earth being from west to east, or from A to A′, the ball will fall 10·914 yards to the west, or to the right of its direction. If fired due north from the point A, it is obvious that the shot would fall nearly the same distance to the east of the object, but still to the right of its direction.

The next point to be considered is, whether if the ball be projected due east or west, or in any other direction, the same amount of effect will be produced. I think there is no doubt that there would be, and perhaps the most intelligible manner of showing this is as follows:—

The line joining the north and south points of any place upon the earth's surface is a tangent to a great circle at that point passing through the two poles; and the line joining the east and west points is the tangent to another great circle passing through the place at right angles to the former, and may be represented by the two lines NS, WE; these lines always of course remaining at right angles to each other. We will suppose that



two guns are placed at P, which is 52° lat., the one pointing due south and the other due west, at two objects 5600 yards distant from P in these directions, and that they are both fired at the same moment. Now the directions in which the balls are fired are at right angles to each other, and there is nothing in the rotation of the earth to alter the relative positions *of the two lines drawn from the guns to the shot during any moment of their flight.*

It has been shown that the shot fired due south will be deflected 10·914 yards to the right of the object in 34″. Let the above figure represent the north and south, and east and west lines, at the end of 34″. Now the position of the ball fired due south will be B, SB being equal to 10·914 yards; join PB, and draw B′P at right angles to PB; then if PB and PB′ be equal, WB′ will be 10·914 yards. This perhaps may not be rigorously correct, as the correction necessary to be made on account of the horizontal curved motion of the point where the gun stands would not be so great in the one case as in the other.

By the same reasoning, it can be shown that the deviation of the shot will be the same in amount in the same latitude, or nearly so, whatever may be the direction of the range, and that the deviation will in all cases be to the right of the object.

In different latitudes, it appears, from what has been stated, that the amount of deflection depends upon the length of range, the time of flight, and the cosine of the latitude of the gun; it will therefore be obviously greatest at the pole, and may be said to vanish at the equator.

It may be interesting here to ascertain what the deflection ought to be, solving the question by the apparent deviation of the plane of the pendulum in 34″. The tangent of the arc of vibration may be looked at in the same light as the line drawn from the gun to the shot at any moment of its flight; for there is nothing in the rotation of the earth to cause this tangent to have an angular motion in a horizontal direction.

$8'·513 =$ rotation of the earth in 34″ nat. sin $52° = ·7880108$.

$8·513 \times ·7880108 = 6·708 = 6'\ 42''\ 28'''$ deviation in 52 lat. in 34″.

Length of cir. arc of $6'\ 42''\ 28''' = ·0019512$ of radius rad. $= 5600$ yards.

$5600 \times ·0019512 = 10·926$ yards, which is the deflection.

Taking into account that the amount of deflection of the projectile has been determined, in the one case, by assuming the length of a geographical mile to be 2000 yards, and that in the other, viz. in the pendulum experiment, the amount is independent of the size of the earth, the results are as nearly alike as can be possibly expected.

I am, Gentlemen,

Your obedient Servant,

E. M. Boxer.

LIX. *On a new Mineral named Orangite.* *By* Dr. A. Krantz *of Bonn**.

THIS mineral has been found only at Langesundfjord near Brewig in Norway, and is of very rare occurrence.

It has been analysed by Dr. Bergemann, and is stated by him to contain a new metal which he has named Donarium. [His analysis, together with an account of the properties of the new metal, were given in the Philosophical Magazine for June 1850, p. 583.]

No indications of crystalline form have been observed. The fracture is conchoidal; structure imperfectly foliated; transparent to translucent; colour deep orange-yellow; streak yellowish-white; hardness between fluor and apatite; specific gravity 5·34 to 5·39. It occurs usually imbedded in felspar, associated with mosandrite, black mica, hornblende, thorite, zircon and erdmannite.

* Communicated by W. G. Lettsom, Esq.

LX. *On a remarkable Discovery in the Theory of Canonical Forms and of Hyperdeterminants.* By J. J. Sylvester, *M.A., F.R.S.**

In a recently printed continuation of a paper which appeared in the Cambridge and Dublin Mathematical Journal†, I published a complete solution of the following problem. A homogeneous function of x, y of the degree $2n+1$ being given, required to represent it as the sum of $n+1$ powers of linear functions of x, y. I shall prepare the way for the more remarkable investigations which form the proper object of this paper, by giving a new and more simple solution of this linear transformation.

Let the given function be

$$a_1.x^{2n+1}+(2n+1)a_2.x^{2n}y+(2n+1)\frac{2n+1}{2}a_2.x^{2n-1}.y^2, \text{ \&c.}$$
$$\ldots\ldots+a_{2n+2}.y^{2n+1},$$

and suppose that this is identical with

$$(p_1x+q_1y)^{2n+1}+(p_2x+q_2y)^{2n+1}+\text{ \&c.}$$
$$+(p_{n+1}.x+q_{n+1}.y)^{2n+1}.$$

The problem is evidently possible and definite, there being $2n+2$ equations to be satisfied, and $(2n+2)$ quantities p_1, q_1, &c. for satisfying the same.

In order to effect the solution, let

$$q_1=p_1.\lambda_1$$
$$q_2=p_2.\lambda_2$$
$$\text{\&c.}=\text{\&c.}$$
$$q_{n+1}=p_{n+1}.\lambda^{n+1},$$

we have then

$$\begin{aligned}
&p_1 + p_2 + \ldots + p_{n+1} = a_1\\
&p_1\lambda_1 + p_2\lambda_2 + \ldots + p_{n+1}.\lambda_{n+1} = a_2\\
&p_1\lambda_1{}^2 + p_2\lambda_2{}^2 + \ldots + p_{n+1}\lambda^2{}_{n+1} = a_3\\
&p_1\lambda_1{}^n + p_2\lambda_2{}^n + \ldots + p_{n+1}.\lambda^n_{n+1} = a_n\\
&p_1\lambda_1{}^{n+1} + p_2.\lambda_2{}^{n+1} + \ldots + p_{n+1}.\lambda_{n+1} = a_{n+1}\\
&\qquad\text{\&c.} \qquad = \text{\&c.}\\
&p_1\lambda_1{}^{2n+1} + p_2\lambda_2{}^{2n+1} + \ldots + p_{n+1}.\lambda^{2n+1}_{n+1} = a_{2n+1}.
\end{aligned}$$

Eliminate $p_1, p_2 \ldots p_{n+1}$ between the 1st, 2nd, 3rd...$(n+1)$th equations, and it is easily seen that we obtain

$$a_{n+1}-a_n\Sigma\lambda_1+a_{n-1}\Sigma\lambda_1\lambda_2 \text{ \&c.} \pm a_1\lambda_1\lambda_2\ldots\lambda_{n+1}=0.$$

* Communicated by the Author.

† Published under the title of 'An Essay on Canonical Forms,' by Bell, Fleet Street.

Again, eliminating in like manner $p_1\lambda_1, p_2\lambda_2, \ldots p_{n+1}\lambda_{n+1}$ between the 2nd, 3rd, ... $(n+2)$th equations, we obtain

$$a_{n+2}-a_{n+1}\Sigma\lambda_1+\&c\ldots\mp a_2\lambda_1\lambda_2\ldots\lambda_{n+1};$$

and proceeding in the same way until we come to the combination of the $(n+1)$th ... $(2n+1)$th equations, and writing

$$\begin{aligned}\Sigma\lambda_1 &= s_1\\ \Sigma\lambda_1.\lambda_2 &= s_2\\ &\&c.\\ \Sigma\lambda_1.\lambda_2\ldots\lambda_{n+1} &= s_{n+1},\end{aligned}$$

we find

$$\begin{aligned}&a_{n+1}-a_n s_1+a_{n-1}.s_2\ldots\pm a_1.s_{n+1} &&=0\\ &a_{n+2}-a_{n+1}.s_1+a_n.s_2\ldots\mp a_2.s_{n+1} &&=0\\ &a_{n+3}-a_{n+2}.s_1+a_{n+1}.s_2\ldots\pm a_3.s_{n+1} &&=0\\ &\qquad\&c. \qquad\qquad \&c.\\ &a_{2n+1}-a_{2n}.s_1+a_{2n-1}.s_2\ldots+a_{n+1}.s_{n+1}&&=0^*.\end{aligned}$$

Hence it is obvious that

$$(x+\lambda_1 y)(x+\lambda_2 y)\ldots(x+\lambda_{n+1}.y)$$

is equal to the determinant

$$\left.\begin{matrix} x^{n+1}; & -x^n.y; & x^{n-1}.y^2; & \ldots & \pm y^{n+1}\\ a_{n+1}; & a_n; & a_{n-1}; & \ldots & a_1\\ a_{n+2}; & a_{n-1}; & a_{n-2}; & \ldots & a_2\\ . & . & . & . & .\\ a_{2n+1}; & a_{2n}; & a_{2n-1}; & \ldots & a_{n+1}\end{matrix}\right\}.$$

Hence $\lambda_1, \lambda_2, \ldots \lambda_{n+1}$ are known, and consequently

$$p_1 \quad p_2\ldots p_{n+1}; \quad q_1 \quad q_2\ldots q_{n+1}$$

are known by the solution of an equation of the $(n+1)$th degree.

Thus suppose the given function to be

$$\begin{aligned}\mathrm{F}&=ax^5+5bx^4y+10cx^3y^2+10dx^2y^3+5exy^4+10y^5\\ &=(p_1x+q_1y)^5+(p_2x+q_2y)^5+(p_3x+q_3.y^5),\end{aligned}$$

we shall have, by an easy inference from what has preceded,

$$(p_1x+q_1y)(p_2x+q_2y)(p_3x+q_3y)$$

$=$ a numerical multiple of the determinant

$$\begin{matrix} x^3; & -x^2y; & xy^2; & -y^3\\ d; & c; & b; & a\\ e; & d; & c; & b\\ f; & e; & d; & c.\end{matrix}$$

* These equations in their simplified form arise from the ordinary result of elimination in this case containing as a factor the product of the differences of the quantities $\lambda_1, \lambda_2, \ldots \lambda_{n+1}$.

The solution of the problem given by me in the paper before alluded to presents itself under an *apparently* different and rather less simple form. Thus, in the case in question, we shall find according to that solution,

$$(p_1x+q_1y)(p_2x+q_2y)(p_3x+q_3y)$$

= a numerical multiple of the determinant

$$\begin{array}{l} ax+by\,;\ bx+cy\,;\ cx+dy \\ bx+cy\,;\ cx+dy\,;\ dx+ey \\ cx+dy\,;\ dx+ey\,;\ ex+fy. \end{array}$$

The two determinants, however, are in fact identical, as is easily verified, for the coefficients of x^3 and y^3 are manifestly alike; and the coefficient of x^2y in the second form will be made up of the three determinants,

$$\begin{array}{ccc|ccc|ccc} a & b & d & a & c & c & b & b & c \\ b & c & e & b & d & d & c & c & d \\ c & d & f & c & e & e & d & d & e \end{array}$$

of which the latter two vanish, and the first is identical with the coefficient of x^2y in the first solution. The same thing is obviously true in regard of the coefficients of xy^2 in the two forms, and a like method may be applied to show that in all cases the determinant above given is identical with the determinant of my former paper, viz.

$$\begin{array}{llll} a_1x+a_2y\,; & a_2x+a_3y\,; & \ldots & a_nx+a_{n+1}y \\ a_2x+a_3y\,; & a_3x+a_4y\,; & \ldots & a_{n+1}x+a_{n+2}y \\ \cdot\quad\cdot & \cdot\quad\cdot & \cdot\quad\cdot & \cdot\quad\cdot \\ a_nx+a_{n+1}y\,; & a_{n+1}x+a_{n+2}y\,; & \ldots & a_{2n}x+a_{2n+1}y. \end{array}$$

Thus, then, we see that for odd-degreed functions, the reduction to their canonical form of the sum of $(n+1)$ powers depends upon the solution of one single equation of the $(n+1)$th degree, and can never be effected in more than one way.

This new form of the resolving determinant affords a beautiful criterion for a function of x, y of the degree $2n+1$ being composed of n instead of, as in general, $(n+1)$ powers. In order that this may be the case, it is obvious that two conditions must be satisfied; but I pointed out in my supplemental paper on canonical forms, that all the coefficients of the resolving determinant must vanish, which appears to give far too many conditions. Thus, suppose we have

$$ax^7+7bx^6y+21cx^5y^2+35dx^4y^3+35ex^3y^4+21fx^2y^5+7gxy^6+hx^7.$$

The conditions of catalecticism, *i. e.* of its being expressible under the form of the sum of three (instead of, as in general, four) seventh powers, requires that all the coefficients of the different powers of x and y must vanish in the determinant

$$\begin{array}{ccccc} x^4 & -x^3y & x^2y^2 & -xy^3 & y^4 \\ a & b & c & d & e \\ b & c & d & e & f \\ c & d & e & f & g \\ d & e & f & g & h; \end{array}$$

in other words, we must have five determinants,

$$\begin{array}{cccc} a & b & c & d \\ b & c & d & e \\ c & d & e & f \\ d & e & f & g \end{array} \qquad \begin{array}{cccc} a & c & d & e \\ b & d & e & f \\ c & d & f & g \\ d & e & g & h \end{array} \qquad \begin{array}{cccc} a & b & c & e \\ b & c & d & f \\ c & d & e & g \\ d & e & f & h \end{array}$$

$$\begin{array}{cccc} a & b & c & d \\ b & c & d & e \\ c & d & e & f \\ d & e & f & g \end{array} \qquad \begin{array}{cccc} b & c & d & e \\ c & d & e & f \\ d & e & f & g \\ e & f & g & h, \end{array}$$

all separately zero. But by my homoloidal law, all these five equations amount only $(5-4)(5-3)$, *i. e.* to 2. I may notice here, that a theorem substantially identical with this law, and another absolutely identical with the theorem of compound determinants given by me in this Magazine, and afterwards generalized in a paper also published in this Magazine, entitled "On the Relations between the Minor Determinants of Linearly Equivalent Quadratic Forms," have been subsequently published as original in a recent number of M. Liouville's journal.

The general condition of mere singularity, as distinguished from catalecticism, *i. e.* of the function of the degree $2n+1$, being incapable of being expressed as the sum of $2n+1$ powers, is that the resolving resultant shall have two equal roots; in other words, that its determinant shall be zero, which will be expressed by an equation of $2n(n+1)$ dimensions in respect of the coefficients. Mr. Cayley has pointed out to me a very elegant mode of identifying the two forms of the resolving resultant, which I have much pleasure in subjoining. Take as the example a function of the fifth degree, we have by the multiplication of determinants,

$$\begin{vmatrix} y^3 & -x^2y & xy^2 & -x^3 \\ a & b & c & d \\ b & c & d & e \\ c & d & e & f \end{vmatrix} \times \begin{vmatrix} 1 & 0 & 0 & 0 \\ x & y & 0 & 0 \\ 0 & x & y & 0 \\ 0 & 0 & x & y \end{vmatrix}$$

$$= \begin{vmatrix} y^3 & a & b & c \\ 0 & ax+by & bx+cy & cx+dy \\ 0 & bx+cy & cx+dy & dx+ey \\ 0 & cx+dy & dx+ey & ex+fy \end{vmatrix}$$

which dividing out each side of the equation by y^3, immediately gives the identity required, and the method is obviously general.

Turn we now to consider the mode of reducing a biquadratic function of two letters to its canonical form, *videlicet*

$$(fx+gy)^4+(hx+ky)^4+6m(fx+gy)^2(hx+ky).$$

Let the given function be written

$$ax^4+4bx^3y+6cx^2y^2+4dxy^3+ey^4.$$

Let

$$g=f\lambda_1 \quad k=h\lambda_2 \quad mfh=\mu \quad \lambda_1+\lambda_2=s_1 \quad \lambda_1 . \lambda_2=s_2,$$

then we have

$$f+h+6\mu=a$$
$$4f\lambda_1+4h\lambda_2+6\mu(2s_1)=4b$$
$$6f\lambda_1^2+6h\lambda_2^2+6\mu(s_1^2+2s_2)=6c$$
$$4f\lambda_1^3+4h\lambda_2^3+6\mu(2s_1s_2)=4d$$
$$f\lambda_1^3+h\lambda_2^3+6\mu s_2^2=e.$$

Eliminating f and h between the first, second and third, the second, third and fourth, and the third, fourth and fifth equations successively, we obtain

$$(a-6\mu)s_2-(b-3\mu s_1)s_1+\big(c-\mu(s_1^2+2s_2)\big)=0$$
$$(b-3\mu s_1)s_2-\big(c-\mu(s_1^2+2s_2)\big)s_1+(d-3\mu s_1s_2)=0$$
$$\big(c-\mu(s_1^2+2s_2)\big)s_2+(d-3\mu s_1s_2)s_1+(e-6\mu s_1s_2)=0,$$

i. e.

$$as_2-bs_1+c-\mu(8s_2-4s_1^2)=0$$
$$bs_2-cs_1+d-\mu(4s_1s_2-2s_1^3)=0$$
$$cs_2-ds_1+e-\mu(8s_2^2-4s_1^2s_2)=0.$$

Let now

$$(4s_1^2-8s_2)\mu=\nu,$$

and we shall have

$$as_2-bs_1+(c+\nu)=0$$
$$bs_2-\left(c-\frac{\nu}{2}\right)s_1+d=0$$
$$(c+\nu)s_2-ds_1+e=0.$$

Hence ν will be found from the cubic equation,

$$\left.\begin{matrix} a; & b; & c+\nu \\ 2b; & 2c-\nu; & 2d \\ c+\nu; & d; & e \end{matrix}\right\}=0,$$

$$i.\,e.\ \nu^3-\nu(ae-4bd+3c^2)+\begin{vmatrix} a & b & c \\ b & c & d \\ c & d & e \end{vmatrix}=0,$$

in which equation it will not fail to be noticed that the coefficient of ν^2 is zero, and the remaining coefficients are the two well-known hyperdeterminants, or, as I propose henceforth to call them, the two Invariants of the form

$$ax^4+4bx^3y+6cx^2y^2+4dxy^3+ey^4;$$

be it also further remarked that

$$\nu=8\left(s_2-\frac{1}{2}s_1{}^2\right)\mu,$$

in which equation the coefficient of 8μ is the Determinant or Invariant of

$$x^2+s_1xy+s_2\,.\,y^2$$

ν being thus found, s_1, s_2, and μ being given by the equations in terms of ν are known, and by the solution of a quadratic λ_1, λ_2 become known in terms of s_1, s_2, and f, h in terms of λ_1, λ_2, μ, and the problem is completely determined. The most symmetrical mode of stating this method of solution is to suppose the given function thrown under the form

$$(fx+gy)^4+(f^1x+g^1y)^4$$
$$+6\epsilon(fx+gy)(f^1x+g^1y).$$

Then writing

$$(fx+gy)(f^1x+g^1y)$$
$$=\mathrm{L}x^2+\mathrm{M}xy+\mathrm{N}y^2$$

$-\nu$, the quantity to be found by the solution of the cubic last given becomes

$$8\epsilon\left(\mathrm{LN}-\frac{\mathrm{M}^2}{4}\right).$$

I shall now proceed to apply the same method to the reduction of the function

$$a_0x^8+8dx^7\,.\,y+28a_2x^6y^2+56a_3x^5y^3+70a_4x^4y^4+56a_5x^3y^5$$
$$+28a_6\,.\,x^2y^6+8a_7xy^7+a_8\,.\,y^8,$$

under the form of

$$(p_1x+q_1y)^8+(p_2x+q_2y)^8+(p_3x+q_3y)^8+(p_4x+q_4y)^8$$
$$+70\epsilon(p_1x+q_1y)^2(p_2x+q_2y)^2(p_3x+q_3y)^2(p_4x+q_4y)^2.$$

It will be convenient to begin, as in the last case, by taking

$$q_1=p_1\lambda_1 \quad q_2=p_2\lambda_2 \quad q_3=p_2\lambda_3 \quad q_4=p_4\lambda_4$$
$$\epsilon p_1p_2p_3p_4=m,$$

and

$$(x+\lambda_1 y)(x+\lambda_2 y)(x+\lambda_3 y)(x+\lambda_4 y)$$
$$=x^4+s_1x^3y+s_2x^2y^2+s_3xy^3+s_4y^4=\mathrm{U},$$

we shall then have nine equations for determining the nine unknown quantities of the general form

$$p_1\lambda_1^\iota+p_2\lambda_2^\iota+p_3\lambda_3^\iota+p_4\lambda_4^\iota+\mathrm{M}_\iota m=a_\iota,$$

where ι has all values from 0 to 8 inclusive, and where

$$\mathrm{M}_\iota=70\,.\,\frac{(1\,.\,2\ldots\iota)\big(1\,.\,2\ldots(8-\iota)\big)}{1\,.\,2\,.\,.\,.\,8}$$

multiplied into the coefficient of $y^\iota\,.\,x^{8-\iota}$ in U^2.

Taking these nine equations in consecutive fives, beginning with the first, second, third, fourth, fifth, and ending with the fifth, sixth, seventh, eighth, ninth, we obtain the five equations following:—

$$a_0\,.\,s_4-a_1s_3+a_2s_2-a_3\,.\,s_1+a_4\,.\,s_0-m\mathrm{N}_1=0$$
$$a_1\,.\,s_4-a_2s_3+a_3s_2-a_4\,.\,s_1+a_5\,.\,s_0-m\mathrm{N}_2=0$$
$$a_2\,.\,s_4-a_3s_3+a_4s_2-a_5\,.\,s_1+a_6\,.\,s_0-m\mathrm{N}_3=0$$
$$a_3\,.\,s_4-a_4s_3+a_5s_2-a_6\,.\,s_1+a_7\,.\,s_0-m\mathrm{N}_4=0$$
$$a_4\,.\,s_4-a_5s_3+a_6s_2-a_7\,.\,s_1+a_8\,.\,s_0-m\mathrm{N}_5=0,$$

where

$$\mathrm{N}_1=\mathrm{M}_0s_4-\mathrm{M}_1\,.\,s_3+\mathrm{M}_2s_2-\mathrm{M}_3\,.\,s_1+\mathrm{M}_4$$
$$\mathrm{N}_2=\mathrm{M}_1s_4-\mathrm{M}_2\,.\,s_3+\mathrm{M}_3s_2-\mathrm{M}_4\,.\,s_1+\mathrm{M}_5$$
$$\mathrm{N}_3=\mathrm{M}_2s_4-\mathrm{M}_3\,.\,s_3+\mathrm{M}_4s_2-\mathrm{M}_5\,.\,s_1+\mathrm{M}_6$$
$$\mathrm{N}_4=\mathrm{M}_3s_4-\mathrm{M}_4\,.\,s_3+\mathrm{M}_5s_2-\mathrm{M}_6\,.\,s_1+\mathrm{M}_7$$
$$\mathrm{N}_5=\mathrm{M}_4s_4-\mathrm{M}_5\,.\,s_3+\mathrm{M}_6s_2-\mathrm{M}_7\,.\,s_1+\mathrm{M}_8.$$

Developing now U^2, we obtain

$$\mathrm{M}_0=70\quad \mathrm{M}_1=\frac{35}{2}s_1\quad \mathrm{M}_2=5s_2+\frac{5}{2}s_1^2\quad \mathrm{M}_3=\frac{5}{2}s_3+\frac{5}{2}s_1s_2$$

$$\mathrm{M}_4=2s_4+2s_1s_3+s_2^2\quad \mathrm{M}_5=\frac{5}{2}s_1s_4+\frac{5}{2}s_2.s_3\quad \mathrm{M}_6=5s_2.s_4+\frac{5}{2}s_3^2$$

$$\mathrm{M}_7=\frac{35}{2}s_3\,.\,s_4\quad \mathrm{M}_8=70s_4^2.$$

Hence

$$\mathrm{N}_1=72s_4-18s_1s_3+6s_2^2$$
$$\mathrm{N}_2=18s_1s_4-\frac{9}{2}s_1^2s_3+\frac{3}{2}s_2^2$$
$$\mathrm{N}_3=12s_2s_4-3s_1s_2\,.\,s_3+s_2^3$$
$$\mathrm{N}_4=18s_3s_4-\frac{9}{2}s_1s_3^2+\frac{3}{2}\,.\,s_2^2\,.\,s_3$$
$$\mathrm{N}_5=72s_4^2-18s_1s_3\,.\,s_4+6s_2^2\,.\,s_4.$$

Hence we have

$$N_1=72I \quad N_2=72I\frac{s_1}{4} \quad N_3=72I\frac{s_2}{6} \quad N_4=72I\frac{s_3}{4} \quad N_5=72I.s_4,$$

where it will be observed that I is the quadratic invariant of U.

Making now

$$72mI=\nu,$$

we shall have the five following equations:—

$$a_0s_4-a_1s_3+a_2s_2-a_3s_1+(a_4-\nu)=0$$

$$a_1s_4-a_2s_3+a_3s_2-\left(a_4+\frac{\nu}{4}\right)s_1+a_5=0$$

$$a_2s_4-a_3s_3+\left(a_4-\frac{\nu}{6}\right)s_2-a_5s_1+a_6=0$$

$$a_3s_4-\left(a_4+\frac{\nu}{4}\right)s_3+a_5s_2-a_6s_1+a_7=0$$

$$(a_4-\nu)s_4+a_5s_3-a_6.s_2-a_7.s_1+a_8=0;$$

so that the problem reduces itself to finding ν, which is found from the equation of the fifth degree:—

$$\left.\begin{array}{lllll} a_0; & a; & a_2; & a_3; & a_4-\nu \\ a_1; & a_2; & a_3; & a_4+\frac{\nu}{4}; & a_5 \\ a_2; & a_3; & a_4-\frac{\nu}{6}; & a_5; & a_6 \\ a_3; & a_4+\frac{\nu}{4}; & a_5; & a_6; & a_7 \\ a_4-\nu; & a_5; & a_6; & a_7; & a_8 \end{array}\right\}=0,$$

ν, it will be observed, being $72\times$ the quadratic invariant of

$$(p_1x+qy)(p_2x+q_2y)(p_3x+q_3y)(p_4x+q_4y),$$

when the function is supposed to be thrown under the form of

$$\Sigma(p_1x+q_1y)^8+70\epsilon(p_1x+q_1y)^2\times(p_2x+q_2y)^2(p_3x+q_3y)^2\times(p_4x+q_4y)^2.$$

It is obvious that in the equation for finding ν, all the coefficients being functions of the invariable quantities p_1, q_1, &c., and ϵ must be themselves invariants of the given function; so that the determinant last given will present under one point of view four out of the six invariants belonging to a function of the eighth degree, and these four will be of the degrees 2, 3, 4, 5 respectively*.

* The reasoning in this paragraph seems of doubtful conclusiveness. It may be accepted, however, as a fact of observation confirmed and generalized by the subsequent theorem, that the coefficients are invariants.

I shall now proceed to generalize this remarkable law, and to demonstrate the existence and mode of finding $2n$ consecutively-degreed independent invariants of any homogeneous function of the degree $4n$, and of $n+1$ consecutively-even-degreed independent invariants of any homogeneous function of the degree $4n+2$; a result, whether we look to the fact of such invariants existing, or to the simplicity of the formula for obtaining them, equally unexpected and important, and tending to clear up some of the most obscure, and at the same time interesting points in this great theory of algebraical transformations.

In the first place, let me recall to my readers in the simplest form what is meant by an invariant* of a homogeneous function, say of two variables x and y. If the coefficients of the function $f(x, y)$ be called $a, b, c \dots l$, and if when for x we put $ax+by$, and for y, $cx+dy$, where $ad-bc=1$, the coefficients of the corresponding terms become $a', b', \dots l'$; and if $\mathrm{I}(a, b, \dots l)=\mathrm{I}(a', b', \dots l')$, then I is defined to be an invariant of f.

Let now $f(x, y)$ be a homogeneous function in x, y of the 2ιth degree, and write

$$\left(\xi\frac{d}{dx}+\eta\frac{d}{dy}\right)^{\iota}\cdot f(x, y)+\lambda(\eta x-\xi y)^{\iota}=\mathrm{P}$$

$$\left(\xi\frac{d}{dx}+\eta\frac{d}{dy}\right)^{\iota} f(lx+my\,;\,nx+py)+\lambda(\eta x-\xi y)^{\iota}=\mathrm{P}',$$

where ξ and η are independent of x, y, and $lp-mn=0$.

Let $$x'=lx+my$$
$$y'=nx+py,$$

then $$\xi\frac{d}{dx}+\eta\frac{d}{dy}=\xi\frac{d}{dx'}\cdot\frac{dx'}{dx}+\xi\frac{d}{dy'}\cdot\frac{dy'}{dx}$$
$$+\eta\cdot\frac{d}{dx'}\cdot\frac{dx'}{dy}+\eta\frac{d}{dy'}\cdot\frac{dy'}{dy}.$$

And if we now write
$$l\xi+m\eta=\xi'$$
$$n\xi+p\eta=\eta',$$

we find $$\xi\frac{d}{dx}+\eta\frac{d}{dy}=\xi'\cdot\frac{d}{dx'}+\eta'\frac{d}{dy'}.$$

Again, from the equations between x', y', x, y we find

$$x=\frac{px'-my'}{pl-mn}=px'-my'$$

$$y=\frac{ly'-nx'}{pl-mn}=ly'-nx';$$

* *Olim,* Hyperdeterminant constant derivative.

$$\therefore\ \eta x-\xi y=(p\eta+n\xi)x'-(m\eta+l\xi)y'=\xi'x'-\eta'y'.$$

Hence

$$P'=\left(\xi'\frac{d}{dx'}+\eta'\frac{d}{dy'}\right)^{\iota}f(x',y')+\lambda(\eta'x'-\xi'y')^{\iota}.$$

Again,

$$\frac{d}{d\xi}=l\frac{d}{d\xi'}+n\frac{d}{d\eta'}$$

$$\frac{d}{d\eta}=m\frac{d}{d\xi'}+p\frac{d}{d\eta'}.$$

Hence

$$\left(\frac{d}{d\xi}\right)^{\iota}P'=l^{\iota}.\left(\frac{d}{d\xi'}\right)^{\iota}.P'+\iota.l^{\iota-1}.n\left(\frac{d}{d\xi'}\right)^{\iota-1}.\frac{d}{d\eta'}.P'+\&c.$$

$$+n^{\iota}.\left(\frac{d}{d\eta'}\right)^{\iota}P'$$

$$\left(\frac{d}{d\xi}\right)^{\iota-1}.\frac{d}{d\eta}.P'=l^{\iota-1}.n\left(\frac{d}{d\xi'}\right)^{\iota}.P'+\left(l^{\iota-1}.p+(\iota-1)l^{\iota-2}mn\right)$$

$$.\left(\frac{d}{d\xi'}\right)^{\iota-1}.\frac{d}{d\eta'}P'+\&c.\qquad +n^{\iota-1}.p\left(\frac{d}{d\eta'}\right)^{\iota}.P'$$

$$\&c.\qquad = \qquad \&c.$$

$$\left(\frac{d}{d\eta}\right)^{\iota}.P'=m^{\iota}.\left(\frac{d}{d\xi'}\right).P'+\iota m^{\iota-1}.p\left(\frac{d}{d\xi'}\right)^{\iota-1}.\frac{d}{d\eta'}.P+\&c.$$

$$+p^{\iota}.\left(\frac{d}{d\eta'}\right)^{\iota}.P'.$$

But P' being of ι dimensions in x' and y', and also in x and y, each of the equations above written will be of ι dimensions in x and y, and of no dimensions in x', y'; in fact, the successive terms of the right-hand members of the above $\iota+1$ equations will be multiples of the $(\iota+1)$ quantities

$$(x')^{\iota},\quad (x')^{\iota-1}y',\quad (x')^{\iota-2}.y'^{\iota}\dots(y')^{\iota}.$$

Consequently a linear resultant may be taken of

$$\left(\frac{d}{d\xi}\right)^{\iota}.P',\quad \left(\frac{d}{d\xi}\right)^{\iota-1}.\frac{d}{d\eta}.P',\dots\left(\frac{d}{d\eta}\right).P',$$

treating $x'^{\iota}, x'^{\iota-1}.y', \dots y'^{\iota}$ as independent, and as the quantities to be eliminated; and this, according to a well-known principle of elimination, will prove the linear resultant of the foregoing equations to be equal to the linear resultant of

$$\left(\frac{d}{d\xi'}\right)^{\iota}.P',\ \left(\frac{d}{d\xi'}\right)^{\iota-1}.\left(\frac{d}{d\eta'}\right)P',\dots\left(\frac{d}{d\eta'}\right).P',$$

multiplied by the determinant

$$\begin{matrix} l^\iota; & \iota n l^{\iota-1}; & \iota.\frac{\iota-1}{2} n^\iota l^{\iota-2}; & \ldots; & n^\iota \\ l^{\iota-1} m; & l^{\iota-1}.p+(\iota-1)mnl^{\iota-2}; & & \ldots; & n^{\iota-1}p \\ & \&c. & & \&c. & \\ m^\iota; & \iota m^{\iota-1}.p; & \iota.\frac{\iota-1}{2} m^\iota p^{\iota-2}; & \ldots; & p^\iota. \end{matrix}$$

This last above written determinant may be shown from the method of its formation to be equal to $(lp-mn)^{\frac{\iota(\iota+1)}{2}}$, *i. e.* to unity, because $lp-mn=1$. Again, since

$$x'^\iota = l^\iota x^\iota + \iota l^{\iota-1} m . x^{\iota-1} . y + \&c. \ldots + m^\iota . y^\iota$$

$$x'^{\iota-1} . y = l^{\iota-1} n x^\iota + \left(l^{\iota-1} . n + (\iota-1) l^{\iota-2} mn\right) x^{\iota-1} y + \&c.$$
$$+ m^{\iota-1} . p y^\iota$$

$$\&c. \quad = \quad \&c.$$

$$y'^\iota = n^\iota . x^\iota + \iota n^{\iota-1} . p x^{\iota-1} . y + \ldots + p^\iota . y^\iota,$$

the resultant of $\left(\frac{d}{d\xi}\right)^\iota \mathrm{P}' \ldots \left(\frac{d}{d\eta}\right)^\iota . \mathrm{P}'$, obtained by treating $x^\iota, x^{\iota-1} . y, \ldots y^\iota$ as the eliminables, will be equal to the resultant of the same functions when $x'^\iota, x^{\iota-1} . y, \ldots y'^\iota$ are taken as the eliminables* multiplied by a power of the determinant

$$\begin{matrix} l^\iota; & \ldots; & m^\iota \\ l^{\iota-1} . n; & \ldots; & m^{\iota-1} . p \\ \cdot & \cdot \; \cdot \; \cdot & \cdot \\ n^\iota; & \ldots; & p^\iota; \end{matrix}$$

which determinant, like the last, is unity. Thus, then, we have succeeded in showing that the resultant obtained by eliminating $x^\iota, x^{\iota-1} . y, \ldots y^\iota$ between

$$\left(\frac{d}{d\xi}\right)^\iota \mathrm{P}', \quad \left(\frac{d}{d\xi}\right)^{\iota-1} . \frac{d}{d\eta} \mathrm{P}', \ldots \left(\frac{d}{d\eta}\right)^\iota . \mathrm{P}'$$

is equal to the resultant obtained by eliminating $(x')^\iota$, $x'^{\iota-1} . y \ldots y'^\iota$ between

$$\left(\frac{d}{d\xi'}\right)^\iota \mathrm{P}', \quad \left(\frac{d}{d\xi'}\right)^{\iota-1} . \left(\frac{d}{d\eta'}\right) \mathrm{P}', \ldots \left(\frac{d}{d\eta}\right)^\iota \mathrm{P}';$$

or, which is evidently the same thing, the resultant obtained by eliminating $x^\iota, x^{i-1} . y \ldots y^\iota$ between

* For the statement of the general principle of the change of the variables of elimination, see my paper in the March Number, 1851, of the Cambridge and Dublin Mathematical Journal.

$$\left(\frac{d}{d\xi}\right)^{\iota}\mathrm{P},\quad \left(\frac{d}{d\xi}\right)^{\iota-1}\cdot\frac{d}{d\eta}\cdot\mathrm{P}\ldots\left(\frac{d}{d\eta}\right)^{\iota}\mathrm{P};$$

that is to say, this last resultant remains absolutely unaltered in value when for x, y we write respectively

$$lx+my$$
$$nx+py,$$

provided that $lp-mn=1$.

Hence by definition this resultant is an invariant $f(x, y)$, and λ being arbitrary, all the separate coefficients of the powers of λ in this resultant must also be invariants. I proceed to express this resultant in terms of λ and the coefficients of (x, y). Let

$$\left(\frac{d}{d\xi}\right)^{\iota}\quad .\quad \mathrm{P}=\left(\frac{d}{dx}\right)^{\iota}f+\lambda(-y)^{\iota}=\mathrm{E}_1$$

$$\frac{1}{\iota}\left(\frac{d}{d\xi}\right)^{\iota-1}\cdot\frac{d}{d\eta}\quad .\quad \mathrm{P}=\left(\frac{d}{dx}\right)^{\iota-1}\cdot\frac{d}{dy}f+\lambda(-y)^{\iota-1}\cdot x=\mathrm{E}_2$$

$$\iota\cdot\frac{1}{\frac{\iota-1}{2}}\left(\frac{d}{d\xi}\right)^{\iota-2}\left(\frac{d}{d\eta}\right)^{\iota}.\mathrm{P}=\left(\frac{d}{dx}\right)^{\iota-2}\cdot\left(\frac{d}{dy}\right)^{\iota}.f+\lambda(-y)^{\iota-1}.x^2=\mathrm{E}_3$$

$$\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot$$

$$\left(\frac{d}{d\eta}\right)^{\iota}\quad .\quad \mathrm{P}=\left(\frac{d}{dy}\right)^{\iota}.f+\lambda x^{\iota}=\mathrm{E}_{\iota+1}.$$

Let now

$$f(x,y)=a_0x^{2\iota}+2\iota\,.\,a_1x^{2\iota-1}.y+2\iota\,.\frac{2\iota-1}{2}a_2x^{2\iota-2}.y^2+\&\mathrm{c}.$$
$$+a_{2\iota}.y^{2\iota}.$$

We find

$$\mathrm{E}_1=\frac{2\iota\,.(2\iota-1)\ldots(\iota+1)}{1\,.\,2\ldots\iota}\left\{a_0x^{\iota}+\iota a_1x^{\iota-1}.y+\iota\,.\frac{\iota-2}{2}a_2x^{\iota-2}.y^2\right.$$
$$\left.\ldots+\frac{\iota\,.(\iota-2)}{2}a_{\iota-1}.x^2y^{\iota-2}+\iota a_{\iota}.xy^{\iota-1}+a_{\iota+1}.y^{\iota}+\lambda(-y)^{\iota}\right\}$$

$$\mathrm{E}_2=\frac{2\iota\,.(2\iota-1)\ldots(\iota+1)}{1\,.\,2\ldots\iota}\left\{a_1x^{\iota}+\iota a_2x^{\iota-1}.y+\iota\,.\frac{\iota-1}{2}a_3x^{\iota-2}.y^2\right.$$
$$\left.\ldots+\iota\,.\frac{\iota-1}{2}a_{\iota}x^2y^{\iota-2}+\iota a_{\iota+1}.xy^{\iota-1}+a_{\iota+2}.y^{\iota}-\lambda(-y)^{\iota-1}x\right\}$$

$$\mathrm{E}_3=\frac{2\iota\ldots(\iota+1)}{1\,.\,2\ldots\iota}\left\{a_2.x^{\iota}+\iota a_3x^{\iota-1}y\ldots+\iota\,.\frac{\iota-1}{2}a_{\iota}.x^{\iota}.y^{\iota-2}\right.$$
$$\left.\ldots+\iota a_{\iota+2}.xy^{-1}+a_{\iota+3}.y^{\iota}+\lambda(-y)^{\iota-2}x^{\iota}\right\}$$

$$\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot$$

$$\mathrm{E}_{\iota+1}=\frac{2\iota\ldots(\iota+1)}{1\,.\,2\ldots\iota}\left\{a_{\iota}x^{\iota}+\&\mathrm{c}.+\lambda x^{\iota}.\right\}.$$

Accordingly, by eliminating

$$x^\iota,\quad \iota x^{\iota-1}.y,\quad \iota.\frac{\iota-1}{2}x^{\iota-2}.y^2\ldots y^\iota,$$

we obtain as the required resultant,

$$\begin{array}{lllll}
(a_{\iota+1}+\lambda); & a_\iota; & a_{\iota-1}; & \ldots & a_1 \\
a_{\iota+2}; & \left(a_{\iota+1}-\frac{\lambda}{\iota}\right); & a_\iota; & \ldots & a_2 \\
a_{\iota+3}; & a_{\iota+2}; & a_{\iota+1}+\frac{\lambda}{\iota.\frac{\iota+1}{2}}; & \ldots & a_3 \\
\cdot\quad\cdot & \cdot\quad\cdot & \cdot\quad\cdot & \cdot\quad\cdot & \cdot \\
a_{2\iota+1}; & a_{2\iota}; & \ldots; & & (a_{\iota+1}\pm\lambda)^*
\end{array}$$

Inasmuch as all the coefficients of λ in this expression are invariants of $f(x,y)$, and these are the invariants of the first order, it is clear that the coefficient of λ^ι must be always zero, which is easily verified.

Again, if ι is odd, the determinant remains unaltered if we write $-\lambda$ for λ; hence when $f(x,y)$ is of the degree $4\epsilon+2$, all the coefficients of the odd powers of λ disappear. Thus, then, our theorem at once demonstrates that a function of x,y of the degree 4ϵ has 2ϵ invariants of all degrees from 2 up to $2\epsilon+1$ inclusive, and that a function of x,y of the degree $4\epsilon+2$ has $\epsilon+1$ invariants whose degrees correspond to all the even numbers in the series from 2 to $2\epsilon+2$.

But in order that the proposition, as above stated, may be understood in its full import and value, it is necessary to show that these invariants are independent of one another, which is usually a most troublesome and difficult task in inquiries of this description, but which the peculiar form of our grand determinant enables us to accomplish with extraordinary facility. In order to make the spirit of the demonstration more apparent, take the case of a function of the twelfth degree, whose coefficients, divided by the successive binomial numbers 1, 12, $\frac{12.11}{2}$, &c. may be called

$$a,\ b,\ c,\ d,\ e,\ f,\ g,\ h,\ \iota,\ j,\ k,\ l,\ m.$$

* Mr. Cayley has made the valuable observation, that λ (given by equating to zero the above determinant) may be defined by means of the equation

$$\left(\frac{d}{dx}\cdot\frac{d}{d\eta}-\frac{d}{dy}\cdot\frac{d}{d\xi}\right)^\iota\{f(x,y)\times\phi(\xi,\eta)\}=\lambda\phi(x,y),$$

ϕ being itself a certain rational integral form of a function of the ιth degree, the ratio of whose coefficients would be given by virtue of the above equations as functions of λ and the coefficients of $f(x,y)$.

Our grand determinant then takes the form

$$\begin{matrix} g+\lambda; & f; & e; & d; & c; & b; & a \\ h; & g-\frac{\lambda}{6}; & f; & e; & d; & c; & b \\ \iota; & h; & g+\frac{\lambda}{15}; & f; & e; & d; & c \\ j; & \iota; & h; & g-\frac{\lambda}{20}; & f; & e; & d \\ k; & j; & \iota; & h; & g+\frac{\lambda}{15}; & f; & e \\ l; & k; & j; & \iota; & h; & g-\frac{\lambda}{6}; & f \\ m; & l; & k; & j; & \iota; & h; & g+\lambda. \end{matrix}$$

Here it will be observed that

a and m appear only		1 time.
b and l	...	2 times.
c and k	...	3 ...
d and j	...	4 ...
e and ι	...	5 ...
f and h	...	6 ...
g	...	7 ...

Let now the coefficients be called

$$H_2 \quad H_3 \quad H_4 \quad H_5 \quad H_6 \quad H_7,$$

H_2 and H_3 manifestly are independent.

Again, if possible, let $H_4=pH_2^2$, then a and m would appear twice in H_4, contrary to the rule.

Hence H_4 is independent of H_2, H_3.

For a similar reason H_5 cannot depend on H_2, H_3.

Again, if possible, let

$$H_6=pH_2^3+qH_2 . H_4+rH_3^2,$$

H_2^3 will contain $b^6 . l^6$, which by the rule cannot appear in $H_2 . H_4$, or in $H_3 . H_3$.

Hence $p=0$.

Also H_4 will contain $b^2l^2 \times$ the coefficient of λ^3 in

$$\left(g+\frac{\lambda}{15}\right)\left(g-\frac{\lambda}{20}\right)\left(g+\frac{\lambda}{15}\right),$$

which is not zero. And H_2 also contains bl; hence H_2H_4 will contain $b^3 . l^3$. But H_3 will evidently not contain b^3 or l^3, or b^2l or bl^2, nor can H_6 contain b^3l^3; hence $q=0$. Finally, H_3^2 will contain c^6 and k^6, but H_6 can only contain as to these letters the combination $c^3 . k^3$; hence $r=0$.

Consequently H_6 does not depend on H_2, H_4, H_3. As regards H_2, H_3, H_4, H_5, H_6 not vanishing, this may be made at once apparent by making all the letters but g vanish; the H's then become identical with the coefficients of

$$(g+\lambda)^2\left(g-\frac{\lambda}{6}\right)^2\left(g+\frac{\lambda}{15}\right)^2\left(g-\frac{\lambda}{20}\right),$$

none of which are zero except that of λ^6. The same or a similar demonstration may be extended to H_7 and easily generalized; hence, then, this most unexpected and surprising law is fully made out*.

To return to the subject of canonical forms, I have not found the method so signally successful in its application to the 4th and 8th degree, conduct to the solution of other degrees, such as the 6th, 12th, or 16th, of all of which I have made trial; possibly another canonical form must be substituted to meet the exigency of these cases †; and it may be remarked in general, that if we have a function of the $(2n)$th degree, the canonical form assumed may be taken,

$$\Sigma(p_1x+q_1y)^{2n}+V;$$

where V, in lieu of being the squared product of

$$(p_1x+q_1y)(p_2x+q_2y)\ldots(p_{n+1}.x+q_{n+1}.y)$$

* This demonstration, however, does not extend to show that the coefficients of the powers of λ may not possibly be dependents, *i. e.* explicit functions of one another combined with other invariants not included among their number, or of these latter alone. For example, in the case of the 12th degree, we know by Mr. Cayley's law that there must be two invariants of the 4th order. Our determinant gives only one of these. Call the other one K_4; by the above reasoning it is not disproved but that we may have

$$H_6=p.H_2^3+qH_2.H_4+rH_3^2+sH_2.K_4.$$

I believe, however, that the H's may be demonstrated without much difficulty to be primitive or fundamental invariants. The law of Mr. Cayley here adverted to admits of being stated in the following terms:—The number of independent invariants of the 4th order belonging to a function of x, y of the nth degree is equal to the number of solutions in integers (not less than zero) of the equation $2x+3y=n-3$. Vide his memorable paper (in which several numerical errors occur against which the reader should be cautioned) On Linear Transformations, vol. i. Camb. and Dub. Math. Journ., new series. There is no great difficulty in showing, by aid of the doctrine of symmetrical functions, that there can never be *more* than one quadratic or one cubic invariant, and in what cases there is one or the other, or each, to any given function of two variables. The general law, however, for the number of invariants of any order other than 2, 3, 4, remains to be made out, and is a great desideratum in the theory of linear transformations.

† See the Postscript for a verification of this conjecture.

may be any hyperdeterminant, or (as I shall in future call such functions) co-variant of this product, understanding $P(x, y)$ to be a co-variant of (x, y) when $P(lx+my, nx+py)$ stands in precisely the same relation to $f(lx+my, nx+py)$ as $P(x, y)$ to $f(x, y)$, provided only that $lp-mn=1$. For the relation and distinction between co-variants and contra-variants, see a short article of mine in the forthcoming Number of the Cambridge and Dublin Mathematical Journal for this month. In endeavouring to apply the method of the text to the Sextic Function

$$ax^6+6bx^5y+15cx^4y^2+2dx^3y^3+15ex^2y^4+6xy^5+gy^6,$$

thrown under the form

$$\Sigma(px+qy)^6+20\epsilon U^2,$$

where

$$U=(p_1x+q_1y)(p_2x+q_2y)(p_3x+q_3y)=s_0x^3+s_1x^2y+s_2.xy^2+s_3y^3,$$

I obtain the following equations:

$$as_3-bs_2+cs_1-ds_0=\epsilon(162s_0{}^2s_3-54s_0s_1s_2+12s_1{}^3)$$
$$bs_3-cs_2+ds_1-es_0=\epsilon(54s_0s_1.s_3+6s_1{}^2.s_2-36s_0s_2{}^2)$$
$$cs_3-ds_2+es_1-fs_0=\epsilon(-54s_0s_2.s_3-6s_1.s_2{}^2+36s_3.s_1{}^2)$$
$$ds_3-es_2+fs_1-gs_0=\epsilon(-162s_0s_3{}^2+54s_0s^2+12s_3s_2s).$$

In these equations, if we call the quantities multiplied by ϵ, L, M, N, P, we shall find

$$s_3L-\frac{1}{3}s_2M-\frac{1}{3}s_1N+s_0.P=0,$$

and

$$s_3L-s_2M-s_1N+s_0.P=I;$$

where I denotes the determinant, or, as I shall in future call such function (in order to avoid the obscurity and confusion arising from employing the same word in two different senses), the Discriminant*, which is the biquadratic (and of course sole) invariant of the cubic function

$$s_0x^3+s_1x^2y+s_2xy^2+s_3y^3.$$

* "Discriminant," because it affords the *discrimen* or test for ascertaining whether or not equal factors enter into a function of two variables, or more generally of the existence or otherwise of multiple points in the locus represented or characterized by any algebraical function, the most obvious and first observed species of singularity in such function or locus. Progress in these researches is impossible without the aid of clear expression; and the first condition of a good nomenclature is that different things shall be called by different names. The innovations in mathematical language here and elsewhere (not without high sanction) introduced by the author, have been never adopted except under actual experience of the embarrassment arising from the want of them, and will require no vindication to those who have reached that point where the necessity of some such additions becomes felt.

The reduction of the function of the fourth degree to its canonical form may be effected very easily by means of the properties of the invariants of the canonical form, as I have shown in the paper in the Cambridge and Dublin Mathematical Journal before alluded to. Accordingly I have endeavoured to ascertain whether the reduction of the sixth degree might not be effected by a similar method.

If we start with the form $ax^6+by^6+cz^6+90mx^2y^2z^2$, where $x+y+z=0$, and which is only another mode of representing the canonical form previously given, we shall find that there are four independent invariants of the second, fourth, sixth, and tenth degrees. Calling these H_2, H_4, H_6, H_{10}, and writing s_1, s_2, s_3 for $a+b+c$, $ab+ac+bc$, abc, it will be found, after performing some extremely elaborate computations, that

$$\begin{aligned}
H_2&=s_2-270m^2\\
H_4&=6ms_3+45m^2s_2+216m^3s_1+891m^4\\
H_6&=4s_3{}^2+120s_2s_3m-\{684s_2{}^2+432s_1s_3\}m^2\\
&\quad+(13\,.\,27\,.\,64s_3-64\,.\,81s_1s_2)m^3+8\,.\,81\,.\,169s_2m^4\\
&\quad+7\,.\,128\,.\,729s_1\,.\,m^5+16\,.\,729\,.\,239m^6.
\end{aligned}$$

H_{10} is too enormously long to attempt to compute; but we can easily prove its independent existence by making $m=0$, in which case the (determinant, or, to use the new term proposed, the) discriminant of $ax^6+by^6+cz^6$ becomes the product of the twenty-five forms of the expression

$$(ab)^{\frac{1}{5}}+(ac)^{\frac{1}{5}}\,.\,1^{\frac{1}{5}}+(bc)^{\frac{1}{5}}\,.\,1^{\frac{1}{5}}\text{*}.$$

Now in general the value of such a product for $\alpha^{\frac{1}{5}}+\beta^{\frac{1}{5}}1^{\frac{1}{5}}+\gamma^{\frac{1}{5}}\,.\,1^{\frac{1}{5}}$ is obviously of the form

$$(\alpha+\beta+\gamma)^5+\alpha\beta\gamma(f\,\overline{\alpha+\beta+\gamma}^2+g\,\overline{\alpha\beta+\alpha\gamma+\beta\gamma});$$

for when $\alpha=0$ or $\beta=0$ or $\gamma=0$, the product must become respectively $(\beta+\gamma)^5$, $(\gamma+\alpha)^5$, and $(\alpha+\beta)^5$. Moreover, without

* Such a product in the language of the most modern continental analysis is, I believe, termed a Norme. If we suppose the general function of x, y of the 4th degree thrown under the form $Au^4+Bv^4+Cw^4$, where $u+v+w=0$, and the general function of x, y, z of the 3rd degree thrown under the form $Au^3+Bv^3+Cw^3+D\theta^3$, where $u+v+w+\theta=0$, the theory of normes will afford an instantaneous and, so to speak, intuitive demonstration of the respective related theorems, that the discriminant (*aliter* determinant) of each such function is decomposable into the sum of a square and a cube. Each of these forms is indeterminate, in either case there being but two relations fixed between the coefficients A, B, C; A, B, C, D; and we may easily establish the following singular species of algebraical porism. In the first case

$$(ABC)^2:(AB+AC+BC)^3,$$

and in the second case

$$(ABCD)^3:(\Sigma A^2B^2C^2-2ABCD\Sigma AB)^2$$

are *invariable ratios.*

caring to calculate f, g*, it is enough for our present purpose to satisfy ourselves that g cannot be zero, as then the product would have a factor $(\alpha+\beta+\gamma)^2$. Hence, then, on putting $\alpha=bc$, $\beta=ac$, $\gamma=ab$, we see that the discriminant, when m is 0, will be of the form

$$s_1^5+fs_2^2 \cdot s_3^2+gs_3^3 \cdot s_1.$$

But when m is 0, H_4 vanishes, and there is no term s_1 or s_3 in H_2. Hence evidently the discriminant H_{10} just found cannot be dependent on H_2, H_4, or H_6; nor is it possible to make $H_{10}+pH_2^5+qH_2^2 \cdot H_6$, *i.e.* $(p+1)s_1^5+fs_2^2 \cdot s_3^2+gs_3^3 \cdot s_1$ a perfect square on account of g not vanishing; so there is no H_5 upon which H_{10} can depend. Hence, admitting, as there seems every reason to do, that the number of invariants of a function of x, y of the degree m is $m-2$, we find that the four invariants in the case of the first degree are respectively of the second, fourth, sixth, and tenth dimensions, a determination in itself as a step to the completion of the theory of invariants of no minor importance.

But it seems hopeless by means of these forms to arrive at the desired canonical reduction. The forms, however, of $s_1\ s_2\ s_3$ are very remarkable as not rising above the 1st, 1st and 2nd degrees respectively in s_1, s_2, s_3. Also H_4 vanishes when $m=0$ and H_4 has been obtained by putting

$$a \cdot x^6+by^6+cz^6+mx^2y^2z^2$$

under the form of

$$Hx^6+6Bx^5y+15Cx^4y^2+20Dx^3y^3+15Ex^2y^4+6Fxy^5+Gy^6,$$

and taking the determinant

$$\begin{matrix} A & B & C & D \\ B & C & D & E \\ C & D & E & F \\ D & E & F & G \end{matrix}$$

Consequently *in general* the vanishing of the above-written determinant will express the condition that a function of the sixth degree may be decomposable into three sixth powers. This also is true more generally. If $F(x, y)$ be a function of $2i$ dimensions, the vanishing of the resultant in respect to $x^i, x^{i-1} \cdot y, \ldots, y^i$, (taken dialytically) of

$$\left(\frac{d}{dx}\right)^i \cdot F, \quad \left(\frac{d}{dx}\right)^{i-1} \cdot \frac{d}{dy} F, \ \ldots \left(\frac{d}{dy}\right)^i \cdot F$$

will indicate that F admits of being decomposed into i powers of linear functions of x, y†.

* $f=-625 \quad g=3125$.

† Such a function so decomposable may be termed meio-catalectic. Meio-catalecticism for even-degreed functions is the analogue of singularity for odd-degreed functions.

In consequence of the greater interest, at least to the author, of the preceding investigations, I have delayed the insertion of the promised continuation of my paper on extensions of the dialytic method, which will appear in a subsequent Number. I take this opportunity of correcting a trifling slip of the pen which occurs towards the end of the paper alluded to. The values of $\frac{x}{z}$ and $\frac{y}{z}$ become zero, and not infinite, when $n=0$; and the antepenultimate paragraph should end with the words "an incomplete resultant." The theorem also, in the last paragraph but one, should be stated more distinctly as subject to an important exception as follows.

Whenever the resultant of a system of equations $F=0$, $G=0$, &c. contains a factor R'^m, this will indicate that, on making $R'=0$, the given system of equations will admit of being satisfied by m algebraically distinct systems of values of the variables, except in those cases where there is a singularity in the forms of F, G, &c., taken either separately, or in partial combination with one another. An example will serve to make the meaning of the exception apparent. Let F, G, H denote three quadratic equations in x and y, so that $F=0$, $G=0$, $H=0$ may be conceived as representing three conic sections. Let R be the resultant of F, G, H, and suppose the relations of the coefficients in F, G, H to be such that $R=R'^2$; then $R'=0$ will imply the existence of one or the other of the three following conditions: viz. either that the three conics have a chord in common, which is the most general inference; or, which is less general, that two of the conics touch one another; or, which is the most special case of all, that one of the conics is a pair of right lines.

So, again, if we have two equations in x, and their resultant contains F^2, this may arise either from one of the functions containing a square factor, or from their being susceptible, on instituting one further condition, namely of $F=0$, of having a quadratic factor in common between them.

Lincoln's-Inn-Fields,
October 14, 1851.

P.S. The conjecture made in the preceding pages has been since confirmed by the discovery of a modification in the canonical form applicable to functions of the sixth degree, which simplifies the theory in a remarkable manner. Assume $f(x, y)$ a function of the 6th degree as equal to

$$au^6+bv^6+cw^6\pm muvw.(u-v)(v-w)(w-u),$$

where u, v, w, linear functions of x and y, satisfy the equation

$$u+v+w=0;$$

then will the product of *uvw* be capable of being determined by means of the solution of a quadratic equation, of the square root of whose roots the coefficients of *uvw* will be known linear functions. Thus by an affected quadratic, a pure quadratic, and a cubic equation, the values of *u, v, w* may be completely ascertained. The discussion of this theory, and of a general inverse method for assigning the true (in the sense of the most manageable) Canonical Form for functions of any even degree, will form the subject of a subsequent communication.

LXI. *An Account of Pendulum Experiments made at Ceylon. By* Jones Lamprey, *A.B., M.B., Assistant Surgeon, 15th Regt., and* Lieut. H. Schaw, *R.E.*

To the Editors of the Philosophical Magazine and Journal.

Colombo, Ceylon,
September 15, 1851.

Gentlemen,

As the following pendulum experiment made in this latitude (6° 56′ 6″ N.) may prove of value to those interested in similar experiments in Europe, we beg leave to communicate it to you, together with the results of our observations thereon.

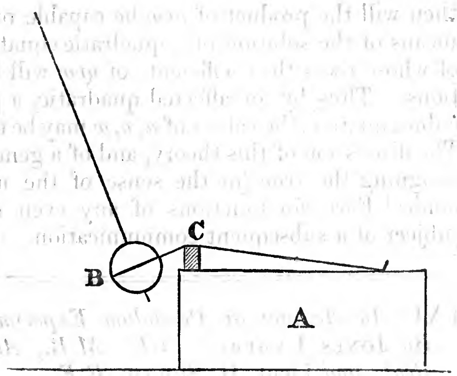
The recent publications on this subject which have appeared in your valuable Magazine afforded us much interest, and induced us to repeat the experiment here; thinking that if the law holds good at a place so near the equator, its truth will be confirmed beyond doubts.

The building in which the experiment was conducted was a large church belonging to the Dutch Presbyterians of Colombo, and from its height was the only one at all applicable for such a purpose. The principal timbers of the roof are sixty-eight feet above the floor; between two of these was secured a small beam, from which our pendulum was suspended.

The ball of the pendulum was of lead, cast in a mould, and afterwards turned true in a lathe, and was nearly a sphere; it weighed $30\frac{1}{2}$ lbs. The wire by which it was suspended was $\frac{1}{25}$th of an inch in diameter, and 66 feet 6 inches long; to its upper extremity were attached a number of fibres of raw silk, which were evenly drawn through a small hole in an iron plate and carefully secured on its other side; this plate was screwed to the under side of the small beam before mentioned.

The circle over which the pendulum was made to oscillate, was in the experiments here recorded only 6 feet in diameter, there being evidently less inclination to ellipticity when small arcs were used, and the ellipticity, when it does occur, having less tendency to cause apsidal motion.

The marginal diagram will explain our method of putting the pendulum in motion, which our observations proved to us must be done with great precision. At the circumference of the circle over which the pendulum was to oscillate, a box A was made stationary by means of weights. On its upper surface, at the extremity nearest the circle, was placed a small block of hard wood, C, which served for adjusting the proper amount of elevation, and for supporting the thread which held the ball B by means of a noose, and was stretched over the block C and secured to the other end of the box A. The ball of the pendulum being brought to a state of perfect rest in a point of suspension vertically over the selected line of oscillation, the thread was severed on the block by the vertical pressure of a sharp knife: this mode of releasing the ball was attended with less chance of extraneous motion than by burning the thread, or by any other means we could devise.



Observations were recorded every hour. When the motion of the pendulum was renewed, or if ellipticity was not observed, it was allowed to continue for another hour.

Previous to the experiment here recorded, many attempts were made with balls of less weight, thicker wires, different modes of suspension and starting, &c.; but the ellipticity of the pendulum's motion was in all of them so considerable, that the results were very unsatisfactory and are not here introduced.

Direction of the initial motion with reference to the magnetic meridian.	Angular variation observed after the lapse of intervals of one hour each.
N. and S.	1·7 1·88
N.E. and S.W.	1·65 1·88 1·9 2·0
E. and W.	1·8 1·9 1·7
N.W. and S.E.	2·15 2·15
Mean variation per hour	1·87
Calculated do.	1·8111

It will be observed, that, rejecting the two results obtained from the oscillation in the direction N.W. and S.E., the mean of the remaining results would be 1·812,—a very close approximation to that calculated from the sine of the angle of latitude. The above, however, are all the experiments that have been made with this apparatus; and as the result comes so near to the mathematical solution, and the means of perfecting our experiments further are not obtainable without great difficulty in this colony, we consider it would serve no end to multiply our observations further.

We are, Gentlemen,

Your obedient Servants,

JONES LAMPREY, *A.B., M.B.,*
Assistant Surgeon, 15*th Regt.*
H. SCHAW, *Lieut. R.E.*

LXII. *Formulæ connected with the Motion of a Free Pendulum.* *By the* Rev. A. THACKER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your Number for August last, you did me the favour of inserting a short letter on the apsidal motion of a pendulum. I observe, in the formulæ there given, two errors of the press, which I trust you will allow me to correct. The expression for the apsidal angle should be

$$\frac{\pi}{2}\left\{1+\frac{3}{8}\cdot\frac{ab}{l^2}+\frac{27}{256}\cdot\frac{ab(a^2+b^2)}{l^4}\right\};$$

and for the progression of the apse in one revolution,

$$135^\circ\times\frac{ab}{l^2}\left(1+\frac{9}{32}\cdot\frac{a^2+b^2}{l^2}\right).$$

If the dimensions of the orbit be small compared with the length of the pendulum, the last term may be neglected; and we then have the formula given by the Astronomer Royal, by Messrs. Galbraith and Haughton, and more recently by Mr. Coombe.

I may add, that by the method I have employed, the approximation may be continued without difficulty; for instance, the next step gives

$$135^\circ\times\frac{ab}{l^2}\left\{1+\frac{9}{32}\cdot\frac{a^2+b^2}{l^2}+\frac{151(a^4+b^4)+58a^2b^2}{1024l^4}\right\}$$

for the progression of the apse. The complete formula, as might be expected, is an infinite series, symmetrical with respect to a and b, and convergent for all possible values of those quantities.

The time of revolution (that is, the interval between two successive arrivals of the pendulum-ball at the same apse) in terms of a, b and l, is equal to

$$2\pi\sqrt{\frac{l}{g}}\left\{1+\frac{a^2+b^2}{16l^2}+\frac{25(a^4+b^4)-26a^2b^2}{1024l^4}+\&c.\right\}.$$

The demonstration of these expressions, although not long, might occupy more space than I can venture to claim.

I am, Gentlemen,

Yours, &c.,

Trinity College, Cambridge,
Oct. 9, 1851.

A. THACKER.

LXIII. *Notices respecting New Books.*

The Ethnology of the British Colonies and Dependencies. By R. G. LATHAM, *M.D., F.R.S. &c.* John Van Voorst, Paternoster Row. 1851.

Man and his Migrations. By R. G. LATHAM, *M.D., F.R.S. &c.* John Van Voorst. 1851.

ETHNOLOGY, or the science of races, has for its objects the consideration of the physical, mental, and moral characteristics of the different races of men, their physiological and philological affinity, and the question of their descent from one or more sources of origin. The mere statement of these objects sufficiently indicates the comprehensive nature of the science, and the difficulties opposed to a satisfactory solution of the problems which it involves. Not only must the living characters of races be faithfully observed and compared, and the influence of intermixture taken into consideration, but the footsteps of each must be tracked along 'the sands of time;' and where history ceases, and the mist of ages obscures their course and movements, the aid of philology and archæology must be invoked, and through these, if possible, their probable affinities and origin be determined. Man must be considered also in relation to the earth which he inhabits, and the question entertained, whether the influences in operation on its surface afford a clue to the different phases under which he exhibits himself in various regions. Hence a moderate acquaintance with physical geography is indispensable. Notwithstanding the varied nature of the qualifying studies, and the faint hope of attaining by scientific research only to any definite conclusion as to the specific unity or diversity of human races, the science has of late years been prosecuted with considerable energy in this country, as well as on the continent and in America. The characteristic features of existing races have been accurately delineated, their affinities in many instances established, and philologists have done something towards a solution of the subtle question of their unity of origin. The general intellectual movement of the age, the buoyant delight with which the philosopher ever explores the more unbeaten tracks, and the interest with which the imagination lingers about those regions of inquiry where facts merge in speculation, may in some degree account for the rapid advancement of the science. Man, too, is at length beginning to feel that the natural history of his species constitutes an object of research, which, whilst it more immediately concerns him, involves questions of equal interest with any that can be agitated in other departments of science.

Unwearied energy, a cautious critical spirit of investigation, and extensive philological research, entitle Dr. Latham to the highest rank among modern ethnologists. None have done so much to fill the void created by the death of Dr. Prichard. Within the short period of a year no less than three works have emanated from his pen. Of these, the first constitutes a tolerably comprehensive text-book of the science, and will prove of infinite service to the ethnological student. Of the other two, whose titles head the present notice, the one is rather practical in purpose and character, the other deals with some of the more abstruse and intricate questions which everywhere beset the inquirer in this department of science. Each work is arranged in the form of lectures, delivered, one series at Manchester, the other at Liverpool; additions having been made by the author prior to publication.

'Man and his Migrations,' though published the last, is anterior in point of subject matter, and first merits consideration. The first three lectures are devoted to a general review of the progress of ethnology, the several branches of the subject, their relative value, and the proper mode and order of investigation. Its history is traced up from the imperfect notices of ancient writers, to the period, scarce half a century back, when principally through the labours of Blumenbach, it first assumed the aspect and fair proportions of a science. A just tribute is paid to the researches of Dr. Prichard, who first associated the philological and zoological series of inquiries, and whose work will ever be invaluable as an encyclopædia of facts for the student. In reference to the influence of climate upon race, our author notices an hypothesis of Dr. Knox, that no race can maintain a permanent footing in any country for which it was not originally destined. In support of the hypothesis, Dr. Knox adduces the American Anglo-Saxons, whose existence in the New World he considers to depend on the continual infusion of fresh blood from the father-land. Notwithstanding this constant renovation, he believes that they are gradually degenerating; a notion, we fancy, which our relatives on the other side of the water will be scarcely inclined to adopt. Dr. Latham hazards no opinion on the matter, and merely observes that "it is forcibly and confidently expressed." We however doubt altogether the truth of the assertion. The frequent migrations from this country are chiefly to settlements in the interior; and there is no reason for supposing that the families inhabiting the larger towns have for years in any way depended on such periodical reinfusion for life and vigour. That climate has produced some effect, as diminished muscular and adipose development, a somewhat lower average longevity, we readily admit. Change of condition, too, has engendered different habits, a peculiar tone of thought, &c.; but for the present let us not talk of degradation. The following suggestions of Dr. Latham throw some light on the subject of acclimatization.

"A European regiment is decimated by being placed on the Gambia, or in Sierra Leone. The American Anglo-Saxon is said to have lost the freshness of the European—to have become brown in colour, and wiry in muscle. Perhaps he has. Yet what does this prove? Merely the effect of *sudden* changes; the results of *distant* transplantation; the imperfect character of those forms of acclimatization which are not *gradual*. It was not in this way that the world was originally peopled. New climates were approached by degrees, step by step, by enlargement and extension of the circumference of a previously acclimated family. Hence the experience of the kind in question, valuable as it is in the way of Medical Police, is comparatively worthless in a theory as to the Migrations of Mankind. Take a man from Caucasus to the Gold Coast, and he either dies or takes a fever. But would he do so if his previous sojourn had been on the Gambia, his grandfather's on the Senegal, his ancestor's in the tenth degree on the Nile, and that ancestor's ancestor's on the Jordan—thus going back till we reached the first remote patriarch of the migration on the Phasis? This is an experiment which no single generation can either make or observe; yet less than this is no experiment at all, no imitation of that particular operation of Nature which we are so curious to investigate."—Pp. 69, 70.

In respect to the phænomena of the present distribution of races, and the probable order of migrations, several circumstances must be considered. Where contiguous races differ materially in character, as in the case of the Majyars and surrounding European tribes, the Hottentots and Kafirs, either encroachment or displacement of the isolated family is implied. Where contiguous islands are peopled by different tribes, the same conclusion is not necessarily forced upon us. "The populations of two islands may agree, whilst that of a whole archipelago lying between them may differ. Yet this is no discontinuity; since the sea is an unbroken chain, and the intervening obstacle can be sailed round instead of crossed. The nearest way from the continent of Asia to the Tahitian archipelago—the nearest part of Polynesia—is viâ New Guinea, New Ireland, and the New Hebrides. All these islands, however, are inhabited by a different division of the oceanic population. Does this indicate displacement? No. It merely suggests the Philippines, the Pelews, the Carolines, the Ralik and Radak groups, and the Navigator's Isles as the route; and such it almost certainly was."

We perfectly agree with Dr. Latham in making so light of the migration from Asia to America. To account for the peopling of the New World, there is no occasion to call up from the deep any fabulous Atlantis, as a resting-place on the high ocean-road for adventurers from Europe and Africa. Neither need we fall back on the theory of a Scandinavian or Welsh colonization, although the Northmen are known to have visited Labrador, and probably the coast more southward; and Madoc, for aught we know, may with his followers have taken the same direction. Everything favours the probability that the migration took place from north-eastern Asia, by Behring's Straits—which were narrower formerly than now—or by the Aleutian chain of islands. Such is the geographical continuity here, that Dr. Pickering asks, 'Where shall Asia end, and America begin?' If we look to the physical characters of the tribes inhabiting these parts, we find the American variety passing by insensible gradations through the Koluschians and Esquimaux into the Mongolian. This view is further favoured by certain traits of resemblance in customs and religious observances, and by such comparison of languages as has hitherto been instituted.

Where physical and other characteristics fail in establishing affinity, philology frequently comes to our aid, and *vice versâ*. The difference between the Hottentots, Kafirs, Negroes, Copts, and other African races is very striking, but the labours of the philologist have succeeded in establishing a complete chain of affinity. On the other hand, if we regard the Chinese in reference to their peculiar monosyllabic language, they appear completely isolated from surrounding races; but then physical character allies them beyond a doubt to other Mongolian families. We cannot accompany our author through the various stages of the argument. It would appear, however, that while the probable course of migration may be readily traced in reference to America, Africa, Polynesia and Australia, when we come to Europe difficulties multiply. The more we know of the ethnology of this quarter, the more are we mystified. The effects of conquest, displacement, intermixture, must be considered; and the value of names, ancient or recent, be determined. Isolated languages, too, the Basque and Albanian, stare us in the face, and archæological research presents us with traces of a race anterior to the Celtic distribution. Our difficulties again would seem to increase tenfold when we reach south-western Asia,—the supposed locality of the cradle of the human race. But if Dr. Latham has brought us no nearer to a satisfactory conclusion, he has at least stated fairly the complicated questions involved in the inquiry, and cleared the road for future progress. His researches, as far as they have gone, lead him to believe that the human family originated somewhere in intratropical Asia from a single pair, although he does not pretend to have arrived at any proof of this.

A knowledge of what we would term practical ethnology, of the living characters of the races composing various nations, is indispensable alike to the historian and the statesman. Through such knowledge only is the former in a position to reason philosophically on, and trace to their true cause the movements that are ever convulsing society; and the government of a dependency will be enlightened and liberal in proportion as it is based on an intimate acquaintance with the genius of the people whom such dependency may embrace. To Dr. Knox belongs the credit of having advocated with force and eloquence this application of the science; and to its ethnological character M. Thierry's History of the Norman Conquest owes not a little of its truthfulness and fascination. 'The Ethnology of the British Colonies' is a pleasing index of the component elements of our 'possessions,' and should be in the hands of every British statesman. The philological affinities of races, as far as they are established, are clearly indicated. In these, indeed, lies the author's strength; and we cannot but think that the usefulness of the work would have been enhanced by a more extended notice of the ethical character, habits, and institutions of the several races. Not that these are altogether overlooked, but that in a practical treatise they should occupy the more prominent place. While the 'Migrations of Man' will probably prove more interesting to the general reader, both works will be indispensable to the student, who will find therein a clear statement of difficulties, and many additions to the accumulating store of ethnological science.

De Morgan's *Elements of Arithmetic and of Algebra translated into the Marathi language by* Colonel George R. Jervis, *Chief Engineer, Bombay Presidency, assisted by* Vishnoo Soonder Chutry, Gungadhur Shastri Phudkay, *and* Govind Gungadhur Phudkay. Bombay. American Mission Press, 1850 and 1851.

If there be two questions of interest in their connexion with each other, upon which the balance of indifference among men of science in this country is most impartially held, they are the questions how arithmetic and algebra were imported into Europe from India, and how they are to be carried back again, with the accessions which they have received. From this kind of indifference it arises that we know nothing of the efforts which a few earnest men, with the help of the local governments, are making for the improvement of more than a hundred million of our fellow subjects.

For some time it has been matter of controversy in India as to whether the *higher* education should be given to the natives in English or in their own languages. The plan of teaching English to selected natives, and thus bringing them into contact with literature and science by aid of all the facilities for instruction which our language affords, tempted many friends of education, and obtained for some time the sanction of the government. But experience showed that the creation, as it were, of an English mind in the Hindu was not the way to make him an effective interpreter to his fellow-countrymen; and experience did no more than confirm the previous belief of almost all the names of celebrity connected with India. Persons who had formed their opinions upon associations and other modes of arrival as different as, for instance, those of Mountstuart Elphinstone and James Mill, or Sir John Malcolm and Professor Wilson, agreed in thinking that the vernacular languages were the proper medium of the higher instruction for the natives of India. And to this conclusion it would now seem that the government has nearly, if not quite, arrived. We need hardly say that no one ever doubted that the rudiments were to be taught in native languages.

Among those who have fought the battle of vernacular instruction, Colonel George Jervis holds the prominent station due to one whose way of proving that a thing can be done is to do it. For five-and-twenty years he has pursued the subject, in the leisure which a laborious profession affords. To use the words of a Bombay newspaper of three years since, "it is to Colonel Jervis entirely we are indebted for the earliest successful efforts made to introduce a correct system of education among the natives. When we were satisfied with schools and schoolmasters, and before the dream of colleges and professors had entered our heads, Captain (now Colonel) Jervis laboured without intermission to obtain translations, and have men taught to read and write, and reason and reflect." The principal languages of the Bombay presidency are the Marathi (or Mahratta) and the Gujerati. In these two languages Colonel Jervis published translations of Hutton's course of mathematics, of a course of practical geometry, and of the preliminary treatise to the library of useful knowledge. He established a lithographic press in the chief engineer's office, and determined to attempt some works which contain more development of the principles of mathematical science. In 1848 he published *in lithograph* a translation of De Morgan's Algebra. Having prepared a translation of the Arithmetic also, and ascertained that the works could be effectively *printed*, he offered both to the government on condition of their immediate publication in type. The offer was accepted, and the publication completed as proposed.

By various testimonies we learn that it is perfectly practicable to translate works which dwell upon principles into languages, which, like the Marathi, are derivations from the Sanscrit. Whether those which, like the Tamul, are not so derived, present the same capabilities, we have no means of knowing. The only criticism which it is in our power to attempt relates to the translation of the language of algebra. In the Nagri type, which is that adopted by Colonel Jervis, the letters of simple formation do not answer to those of the Italic alphabet. The x, for instance, in Nagri has a form which we think a child might hit upon to represent a man in a broad-brimmed hat holding out a snake of ample curl by his single arm. If the native writer, by habit, has managed to forget the difference between this letter and others, in point of complexity, we have no more to say; but if not, it is worth consideration whether it would not be advisable to substitute a more simple letter, of which there are many. In this way the letters of easiest formation might be made those of most frequent occurrence. This, however, is a small matter, and has probably been duly considered.

October 17.—Since the above was printed, we have seen with great regret the announcement of the death of Colonel Jervis, as having taken place at Boulogne on the 14th.

LXIV. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. i. p. 568.]

May 19, 1851.—ON the Colours of Thick Plates. By G. G. Stokes, M.A., Fellow of Pembroke College, and Lucasian Professor of Mathematics in the University of Cambridge.

By the expression "colours of thick plates" is usually understood the system of coloured rings, discovered by Newton, which are formed on a screen when the sun's light is transmitted through a small hole in the screen, and received perpendicularly upon a concave mirror of quicksilvered glass, placed at such a distance from the screen that the image of the hole is at the same distance from the mirror as the hole itself. The brilliancy of the rings, as was afterwards discovered, is greatly increased by tarnishing the surface of the mirror; and it is also advantageous to use a lens to collect the sun's rays, and to place the screen so that the small hole may be situated at the focus of the lens. These rings were first explained on the undulatory theory by Dr. Young, who attributed them to the interference of two streams of light; of which the first is scattered at the tarnished surface of the mirror, and then regularly reflected and refracted, while the second is regularly refracted and reflected, and then scattered in coming out of the glass. The theory has been worked out in detail by Sir John Herschel, who has investigated the case in which the two surfaces of the glass belong to a pair of concentric spheres, and the hole in the screen is situated in the common centre of curvature.

A set of coloured bands has since been observed by Dr. Whewell in a common plane mirror. These bands are seen when a candle is held near the eye, at the distance of several feet from the mirror, and is viewed by reflexion. It is necessary that the first surface of the glass should be a little tarnished. The theory of these bands had not been worked out, and it had even been doubted by some philosophers whether they were of the nature of the coloured rings of thick plates.

In this paper the author gave a general investigation, which includes as particular cases the theory of the rings formed on a screen in Newton's experiment, and that of the bands which Dr. Whewell had observed in a plane mirror, and which are not thrown on a screen, but viewed directly by the eye. He also exhibited to the meeting a variation of Newton's experiment, in which an extremely beautiful system of rings is very easily produced without sunlight. The face of a concave mirror of quicksilvered glass was prepared by pouring on it a mixture consisting of one part of milk to three or four of water, and then holding the mirror vertically in front of a fire to dry. When the flame of a taper, or of an oil-lamp with a small wick, is placed in front of a mirror thus prepared, in such a position as to coincide with its inverted image, a beautful system of rings is seen encompassing the flame. These rings appear to have a definite position in space, like a bodily object. The rings thus formed, which are evidently of the nature of Newton's coloured rings of thick plates, may be made to pass in a perfectly continuous manner into the coloured bands observed by Dr. Whewell.

The author has compared theory and experiment in various particulars, and has found the agreement perfect. It will be sufficient to mention here one result of theory, which is of great generality and of considerable elegance. It applies to the system of rings seen by reflexion in a mirror, either plane or curved, when a luminous point is placed anywhere near the axis, and the eye occupies any other position likewise near the axis. The result is as follows:—Join the eye with the luminous point, and likewise with its image, whether it be real or virtual, and find the points in which the joining lines, produced if necessary, cut the mirror. Describe a circle having for diameter the line joining these two points. This circle will be the middle line of the bright colourless fringe of the order zero, and on each side of it the colours will be arranged in descending order.

June 2.—On a new Elliptic Analyser. By Professor Stokes.

After mentioning some of the inconveniences and inaccuracies attending the use of a Fresnel's rhomb in the analysis of elliptically-polarized light, and alluding to some other methods which had been employed for the purpose, the author proceeded to describe a new instrument which he had invented, and which he exhibited to the meeting. In the construction of this instrument he had aimed at being independent of the instrument-maker in all important points except the graduation. The construction is as follows:—

A brass rim or annulus is mounted so as to stand with its plane vertical when placed on a table. Within this rim turns a brass graduated disc; and the angle through which it turns is read off by means of verniers engraved on the face of the rim, and reading to tenths of a degree. This disc is pierced at the centre, and carries on the side turned towards the incident light a retarding plate of selenite, of such a thickness as to give a difference of retardation in the oppositely polarized pencils amounting to *about* a quarter of an undulation. In front it carries a hollow cylinder, turned on the lathe along with the disc itself. Round this cylinder there turns a collar containing a Nicol's prism, and carrying a pair of level-edged verniers, by which the angle may be read off through which the prism has been turned. Thus the retarding plate moves in azimuth carrying the prism along with it, and the prism has likewise an independent motion in azimuth.

In observing, the light is extinguished by a combination of the two movements, in which case the elliptically-polarized light is converted by the retarding plate into plane polarized, which is then extinguished by the Nicol's prism. On account of chromatic variations, the light is not, strictly speaking, extinguished, unless homogeneous light be employed, but only reduced to a minimum. There are two principal positions of the retarding plate and Nicol's prism in which the light is extinguished, or at least would be extinguished if the incident light were homogeneous; and for each principal position there are four subordinate positions, since either the retarding plate or the Nicol's prism may be reversed by turning it through 180°. The mean of the four subordinate positions may be taken for greater accuracy.

Let R, R′ be the readings of the fixed, r, r' those of the moveable verniers in the two principal positions; I the index error of the fixed verniers, that is, the azimuth of the major axis of the ellipse described, measured from a plane fixed in the disc; i the index error of the moveable verniers, that is, the azimuth of the principal plane of the prism, measured from a fixed plane in the disc; ϖ the angle whose tangent is equal to the ratio of the axes of the ellipse described; ρ the difference of retardation of the oppositely polarized pencils transmitted through the plate, measured as an angle, at the rate of 360° to one undulation. Then the unknown quantities I, i, ϖ, and ρ are given in terms of the known quantities R, R′, r, and r' by the following formulæ, which happen to be extremely convenient for numerical calculation:—

$$\mathrm{I}=\frac{1}{2}(\mathrm{R}'+\mathrm{R})\,; \qquad i=\frac{1}{2}(r'+r)\,;$$

$$\cos 2\varpi=\frac{\sin(r'-r)}{\sin(\mathrm{R}'-\mathrm{R})}\,; \qquad \cos\rho=\frac{\tan(r'-r)}{\tan(\mathrm{R}'-\mathrm{R})}.$$

The author stated that he had already observed with this instrument, and after a little practice had found that it worked in a very satisfactory manner. When the light of the clouds was reflected horizontally by a mirror, and modified so as to produce elliptically-polarized light in which the ratio of the axes was about 3 to 1, it was found that the mean error of single observations amounted to about a quarter of a degree in the determination of the azimuth of the major axis, about three or four thousandths in the determination of the ratio of the minor to the major axis, and little more than the thousandth part of an undulation in the determination of ρ.

Since the magnitude of ρ depends upon the length of wave, or, what comes to the same, the refrangibility of the light, it follows that a knowledge of the former leads to a knowledge of the latter. It may thus be said that the instrument determines the azimuth and excentricity of the ellipse described, and the refrangibility of the light. An error of the thousandth part of an undulation in the determination of ρ would correspond to an error in the place in the spectrum assigned to the light operated on amounting to less than the twentieth part of the interval between the fixed lines D and E. Now by the use of observing media it is possible, without too much reducing the intensity of the light employed, to alter greatly its mean refrangibility; and yet for each medium the refrangibility may be determined very accurately by means of the value of ρ. Accordingly, the instrument is specially adapted for investigations relating to the dispersion of metals, and for other similar researches.

LXV. *Intelligence and Miscellaneous Articles.*

FOUCAULT'S PENDULUM EXPERIMENT.

To the Editors of the Philosophical Magazine and Journal.

Gordon's Hospital, Aberdeen,
July 1851.

Gentlemen,

As M. Foucault's experiment is now an engrossing topic in scientific circles, I beg leave to forward a verification of it for this place, lat. 57° 9′ N., and to offer a few remarks tending to explain some of the phænomena connected with it in a manner somewhat different from that usually adopted.

Every experimenter is struck by a tendency of the pendulum to get into an elliptical orbit, and is disposed to ascribe the ellipticity to the imperfection of the apparatus, the resistance of the air, or some cause accidental and not essential to the experiment. But as a greater or less amount of ellipticity sooner or later makes its appearance in every instance, it would appear reasonable to infer that it must proceed from some cause inseparable from the conditions of the motion; and on consideration, it is obvious that the path of the pendulum from the very first will not be in a straight line across the table, but in a curve approaching to an ellipse; for the body, when steadied at the circumference of the table and let go, is acted upon by two forces, viz. gravity, which would carry it in a straight line over the centre, and a lateral impulse derived from the rotatory motion of the circumference of the table with respect to its own centre. The joint effect of these two forces will be to produce a motion in an elliptical or ellipsoidal orbit passing a little to the right of the centre, and returning a little to the left of it. The point of the circumference, to which the body will return, will depend upon whether the major axis of the ellipse is fixed or moveable.

1. If the axis of the ellipse be fixed, the pendulum will return to a point a little to the left of that from which it set out, and will have an apparent motion contrary and equal to the real motion of the table. This is M. Foucault's experiment when successfully performed.

2. If the axis of the orbit move directly, *i. e.* from west to east at the same rate as the circumference of the table, the body will return to the point from which it was let go, and no deviation will be observed. If the motion of the axis is direct and quicker than that of the table, the body will deviate from west to east with respect to the circumference. Both these effects are observed in conducting the experiment, if the pendulum either by accident or design receive a slight impulse to the right.

3. If the axis or line of apsides of the orbit have a retrograde motion, the pendulum will deviate from east to west, and at a higher rate than that of the real motion of the table. This result is observed in most of the experiments, even those conducted with the greatest care. From the paper contributed by Mr. Bunt of Bristol to a recent Number of your Magazine, it appears that the observed

rate of deviation exceeds the computed rate. This effect seems paradoxical, and has led many to doubt the soundness of the theory, as it seems to exceed the cause to which it is ascribed, or to exhibit a motion in an opposite direction to that of the impulse. A little attention to the circumstances will however remove this difficulty, and show that the contradiction to theory is only apparent.

If a pendulum is swung in a tub of water, drawn to the circumference and let go with a considerable lateral impulse from west to east, the resistance of the dense fluid causes it at each successive vibration to fall visibly short of the height or distance from the centre attained by it at the previous one, and the line of apsides sensibly recedes, *i. e.* moves from east to west, and this retrogression continues until the whole fluid has acquired a rotatory motion).

Now the resistance of the air must produce an effect similar in kind, though not equal in degree, on a body moving in it, and this retrograde motion of the apsides of the orbit arising from the gradual loss of momentum, explains the excess of the actual deviation over that expected from computation. The error arising from this source will obviously be less the slower the loss of momentum, that is, *cæteris paribus,* the smaller the arc of vibration.

A small arc is recommended by another consideration independent of the resistance of the air or of friction, viz. that with a small arc the motion of the line of apsides is slower, and the path of the pendulum approaches nearer to a fixed orbit. I do not know whether the motion of a body in the circumstances of Foucault's pendulum, *i. e.* of a body performing a gyration approaching to a simple oscillation, has been investigated; and the investigation might prove difficult, as the path does not lie in one plane. But that the motion of the line of apsides will be slower the smaller the arc, will appear from considering generally that the periodic time of such a body must lie between the time of two oscillations of the simple pendulum, and the time of one revolution of the conical pendulum moving at the same height or distance from the centre from which the simple pendulum is let go, and must therefore be always less than the time of two oscillations of the simple pendulum. But the body will require the full time of two simple oscillations to visit the opposite circumference and return to its greatest height on the same side from which it set out, and will therefore have completed a revolution in its orbit before this height on the same side is attained, that is, the apsis of the curve will be in advance of the point of starting, but the less so the less the difference between the time of a gyration and that of two simple oscillations, or *cæteris paribus,* the smaller the arc.

To obviate as far as possible errors arising from the retrograde motion of the apsides produced by the loss of momentum, and from the direct motion produced by lateral impulse, a pendulum was employed, not of a spherical form, but of the shape of the pendulum of a clock, and hung with its sharp edge horizontal. The height was about 25 feet, the wire simply passing through a hole in a board, and the weight 20lbs. of lead. The arc of vibration was about four inches on each side of the centre. For a period of 12 hours, without renewing the impulse, the average rate of deviation per hour was 12°·8. Having been permitted through the kindness of J. D. Milne Esq. and the Rev. J. Longmuir to attach the same bob to a wire of 30 feet in the Mechanics' Institution here, with the same extent of arc I found the hourly rate on a motion continued for seven hours to be 12°·6, which is exactly the rate computed from the sine of the latitude 57° 9′. A nearer coincidence of experiment with theory cannot in any case be expected.

From the above-mentioned and other experiments similarly conducted, it appears that the deviation is the same in every point of the compass.

By giving this paper a place in your journal you will honour

Your most obedient humble Servant,

ALEXANDER GERARD.

Oct. 25.—To the above I would now add, that with the small arc the condition of the body approaches nearer to that referred to in Newton's *Principia*, Prop. 47, Book I.; and that the apparent acceleration produced by the resistance of the air may account for the discrepancy between the observed phænomena and some of the formulæ given in your Number for August or September.—A. G.

PENDULUM EXPERIMENTS AT THE PHILOSOPHICAL INSTITUTION, BRISTOL. BY THOMAS G. BUNT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A short time before I had concluded my pendulum experiments in St. Nicholas Spire, I was requested by the Curator of the Bristol Philosophical Institution to try what results I might be able to obtain from a pendulum which had been erected there. The suspending apparatus of this pendulum is precisely the same as that which I last employed at St. Nicholas, and described in your Number for July. The ball is an accurately turned sphere of lead, weighing 35lbs; the suspending wire is of bright iron; and the length of the pendulum, carefully computed from the number of its oscillations in a given time, is 22·73 feet. A pointed wire projects from the bottom of the ball, and reaches to within about $\frac{1}{8}$th of an inch of the azimuth circle, which is of 12 inches diameter, beautifully engraved and printed on a card. Across the circle, fastened down by a pin through its centre, is a moveable index of card, 1 inch wide, divided lengthwise into inches, and laterally by parallel lines, $\frac{1}{10}$th of an inch asunder. By watching the motions of the wire over these divisions the semi-axes major and minor (a and b) may be read off, the former to tenths, and the latter to hundredths, of an inch.

The Curator informed me, that the earlier trials of this pendulum were very unsatisfactory. At that time it was suspended from the floor of the room over the theatre, so as to be liable to disturbance by persons walking upon it; but on removing the suspending screw from the floor, and attaching it to a strong bracket projecting from the wall, the tendency to elliptic motion was greatly diminished, and other sources of error entirely got rid of.

In experimenting with this pendulum, I have again confined myself to very small arcs of vibration. One of the many advantages of this method is, that the ball and lower portion of the pendulum can be easily enclosed, and thus protected against currents of air. This was at first done by placing round it a rude enclosure of boards; but afterwards a square pyramidal frame was constructed, and glazed on the four sides, one of them opening as a door; so that the ball vibrated, during the experiments, within the glass case, the top only being left open to admit the wire.

My experiments with this pendulum are divided into two classes, according to their different duration. Those of the first class have an average duration of about an hour each; and the values of *a* and *b* (or semi-axes major and minor), at the beginning and end of each, have been observed and recorded. These are afterwards subdivided into six groups, according to the value and sign of *b*.

The second class contains experiments of much longer duration; two of them extending through more than 37 hours each, with only a single impulse of the pendulum. The ellipticity of these has not been observed; but as the mean arcs are small and the direction of the elliptic motion is twice reversed while the plane of vibration is passing through 180°, the apsidal motions of the whole mass must almost entirely destroy one another. It is somewhat remarkable that the changes from direct to retrograde (or + to —) motion, fall nearly on the same parts of the azimuth circle here, as they did on that at St. Nicholas; the change from — to + being at about 60° or 70°, and that from — to + at about 140° or 150°, in both instances.

Experiments of short duration, arranged according to values of *b*.

Elliptic Motion Retrograde.

Mean values of				Mean values of				Mean values of			
b.	*a.*			*b.*	*a.*			*b.*	*a.*		
in.	in.	min.	arc.	in.	in.	min.	arc.	in.	in.	min.	arc.
—·05	5·9	33·6	6°·25	—·19	4·7	104·5	18·10	—·23	3·6	87·0	16°·75
—·05	4·5	95·3	17·70	—·19	1·1	73·5	13·85	—·23	2·8	69·5	10·70
—·09	2·7	108·0	20·00	—·20	4·3	118·1	20·55	—·20	2·4	41·0	8·20
—·00	1·9	192·0	37·25	—·16	4·5	56·0	10·00	—·24	4·4	27·0	4·95
—·00	4·5	44·5	9·77	—·15	4·3	45·8	8·90	—·21	4·2	13·0	2·00
—·08	4·5	80·6	15·11	—·16	3·7	69·0	11·95	—·25	4·9	60·7	10·10
—·00	5·3	87·0	16·95	—·13	3·2	33·3	6·10	—·23	4·1	46·5	8·00
—·03	4·2	89·0	15·50	—·19	4·2	41·8	7·20	—·22	3·4	95·0	16·00
—·10	3·5	23·0	4·40	—·13	4·1	65·0	11·90	—·21	2·8	31·5	5·70
—·00	2·0	20·0	3·85	—·19	3·0	35·5	6·65	—·28	2·5	77·3	13·00
				—·15	2·7	41·6	7·50	—·28	4·0	49·0	8·30
—·04	3·60	773·0	146·78					—·24	3·4	59·2	9·75
Mean vals.		or 11°·39 per hr.		—·176	3·80	684·1	122·70				
				Mean vals.		or 10°·77 per hr.		—·235	3·5	656·7	113·45
								Mean vals.		or 10°·36 per hr.	

N.B. In finding the mean values of *b* and *a* for each group, regard has been had to the different durations of the experiments.

Elliptic Motion Direct.

Mean values of *b*.	Mean values of *a*.		
in.	in.	min.	arc.
+·04	56	44·0	$^{\circ}$8·85
·04	18	76·0	13·55
·06	4·2	86·4	17·55
·05	3·7	44·3	8·90
·09	3·9	174·4	35·70
·09	3·0	44·3	9·25
·06	3·6	90·8	18·37
+·07 Mean vals.	3·6	560·2	112·17 or 12°·01 per hr.

Mean values of *b*.	Mean values of *a*.		
in.	in.	min.	arc.
+·17	5·2	69·7	15·00
·17	4·9	21·5	4·55
·17	3·7	69·2	15·40
·15	3·3	45·	9·30
+·167 Mean vals.	4·2	205·4	44·25 or 12°·68 per hr.

Mean values of *b*.	Mean values of *a*.		
in.	in.	min.	arc.
+·23	3·9	42·2	8·87
·21	3·6	55·7	11·10
·23	3·8	58·8	12·05
·24	3·4	153·8	34·40
+·232 Mean vals.	3·56	310·5	66·42 or 12°·83 per hr.

Comparing the mean values of *b* and *a* in the 3rd and 6th group, we have—

b.	*a*.	Motion per hour.
in.	in.	
−·235	3·5	10·36
+·232	3·56	12·83
·467 Diff.	3·53 Mean.	2·47 Diff.

whence, if $a=3·53$ in., the apsidal motion due to $b=0·467$ in. will be 2°·47 per hour, or 0°·53 per hour for $\frac{1}{10}$th inch, *b*.

The 2nd and 5th groups compared give (when *a* is reduced $=3·6$ in.) 0°·50 per hour for $\frac{1}{10}$th inch, *b*.

The 1st and 4th groups give (a being$=3·6$ in.) 0°·56 per hour for $\frac{1}{10}$th inch, *b*.

The mean result of all is, that *a* being$=3·6$ inches, the apsidal motion per hour for every tenth of an inch of ellipticity (*b*) will be 0°·52 nearly. Messrs. Galbraith and Haughton's formula gives 0°·445.

By applying the empirical correction 0°·52 per hour for $\frac{1}{10}$th of an inch, *b*, so as to eliminate the effects of ellipticity, the mean motion per hour for each of the six groups of experiments is as follows, viz.–

1st group	$11^{\circ}·60$	per hour.
2nd	11·73	..
3rd	11·54	..
4th	11·65	..
5th	11·67	..
6th	11·63	..
Mean of all	11·637	..

The experiments of longer duration, and unobserved ellipticity, are the following:—

	Arc.	Rate per hour.
h m	°	°
13 30·2	155·30	11·50
13 30	158·75	11·76
37 2	446·00	12·04
12 40·5	140·50	11·11
3 34·7	43·40	12·13
13 44·5	167·50	12·18
2 54·4	35·70	12·00
2 59·5	35·30	
15 1	169·20	11·27
5 42·3	69·35	12·14
13 4	154·45	11·82
38 4·5	449·50	11·80
4 39	52·73	11·33
16 52	197·30	11·70
5 41	64·80	11·40
11 26·3	137·10	12·00
210 25·9	2476·88	=11·770 per hour.

Mean value of a =1·7 inch.

The latitude of the Philosophical Institution is 51° 27′ 16″, the sine of which × 15° 2′ 28″ (=the angle through which the earth rotates in 1 hour of *mean* time) gives 11°·7638.

The set of long experiments performed at St. Nicholas gave a mean motion of 11°·750 per hour, instead of 11°·7631 as per theory, the latitude being 51° 27′ 0″.

The close agreement thus obtained between theory and experiment, by two very different pendulums, and at two different stations, not only confirms the truth of Foucault's hypothesis, but seems to show that the latitude of a place may be found, by means of this experiment, with a considerable degree of accuracy.

I am, Gentlemen, Yours respectfully,

7 Nugent Place, Bristol, THOMAS G. BUNT.
17th October, 1851.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1851.

Chiswick.—September 1. Cloudy and fine: overcast. 2. Drizzly: cloudy and fine. 3. Hazy: very fine: clear. 4. Foggy: cloudy: fine. 5. Very fine: clear. 6. Fine: cloudy. 7. Clear: very fine. 8. Overcast: cloudy and fine. 9—13. Mornings foggy: days very fine: nights clear. 14. Slight fog: very fine. 15. Slight fog: cloudy and fine. 16. Light clouds and fine: overcast. 17. Overcast: fine: cloudy. 18, 19. Fine: clear. 20—22. Very fine. 23. Slight fog: very fine: rain. 24. Foggy: overcast: foggy at night. 25. Slightly overcast: rain. 26. Partially overcast: cloudy: clear. 27. Cloudy: drizzly. 28. Clear and fine. 29. Foggy: fine. 30. Rain: overcast.

Mean temperature of the month .... 55°·15
Mean temperature of Sept. 1850 .... 54 ·23
Mean temperature of Sept. for the last twenty-five years . 57 ·18
Average amount of rain in Sept. .... 2·61 inches.

Boston.—Sept. 1. Fine. 2. Cloudy: rain early A.M. and P.M. 3—5. Cloudy. 6. Fine. 7. Cloudy. 8—11. Fine. 12, 13. Foggy. 14. Fine. 15—17. Cloudy. 18, 19. Fine. 20. Cloudy. 21—23. Fine. 24. Cloudy. 25. Cloudy: rain P.M. 26. Cloudy: rain A.M. and P.M. 27. Cloudy. 28, 29. Fine. 30. Cloudy.

Sandwick Manse, Orkney.—Sept. 1. Drizzle: damp. 2. Cloudy: rain. 3. Drizzle: cloudy. 4. Clear: aurora. 5. Clear. 6. Bright: cloudy. 7. Cloudy: clear: aurora. 8. Clear. 9. Bright: hazy. 10. Cloudy: drops. 11. Cloudy: fine. 12, 13. Bright: fine: clear. 14. Bright: cloudy. 15. Cloudy: clear: aurora. 16. Bright: clear: aurora. 17. Bright: clear. 18, 19. Fog: cloudy. 20. Showers: clear. 21. Cloudy. 22. Drops: drizzle. 23. Bright: clear: aurora. 24. Clear: cloudy. 25. Showers: sleet-showers. 26. Bright: showers. 27. Clear: showers: aurora. 28. Bright: showers: aurora. 29. Cloudy. 30. Cloudy: showers.

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

Days of Month. 1851. Sept.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8¾ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8½ p.m.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.
1.	30·219	30·136	29·73			29·93	29·96	76	58	62·5			61	55	sw.	nw.		wsw.	·10	......		·03
☽ 2.	30·093	30·067	29·54			29·94	30·01	73	59	66			62	57	nw.	nw.		w.	......	·20		·05
3.	30·127	30·072	29·60			30·07	30·22	72	51	66			57	55	ne.	n.		w.	......	·03		·24
4.	30·049	30·029	29·52			30·21	30·21	69	46	64·5			50	51	n.	nw.		n.	·01	......		......
5.	30·209	30·151	29·70			30·39	30·48	67	49	58·5			53	47	n.	n.		n.	......	......		......
6.	30·373	30·296	29·89			30·52	30·52	63	44	57			54	51	ne.	nne.		n.	......	......		......
7.	30·448	30·430	30·02			30·57	30·58	66	48	58			55	47½	e.	n.		calm	......	......		......
8.	30·498	30·465	30·06			30·55	30·50	62	42	55			53	49½	ne.	ne.		se.	......	......		......
9.	30·465	30·449	30·05			30·42	30·36	67	31	56			55	49	e.	e.		calm	......	......		......
○ 10.	30·497	30·457	30·07			30·33	30·35	70	33	52			58½	56	se.	nw.		wsw.	......	......		......
11.	30·481	30·381	30·05			30·37	30·37	68	36	51			58	56	se.	nw.		w.	......	......		·05
12.	30·401	30·287	29·95			30·35	30·32	74	38	49			56½	52	nw.	nw.		w.	......	......		......
13.	30·319	30·301	29·90			30·31	30·32	72	33	52			55½	51	ne.	nw.		calm	......	......		......
14.	30·393	30·370	29·97			30·36	30·46	73	40	50			57	53½	se.	nw.		calm	......	......		......
15.	30·540	30·491	30·00			30·54	30·58	66	38	59			56	49	e.	ne.		calm	......	......		......
16.	30·572	30·460	30·06			30·57	30·56	66	52	58			57	49	ne.	n.		se.	......	......		......
17.	30·411	30·306	30·04			30·52	30·46	69	45	59			57	52	ne.	ne.		se.	......	......		......
☾ 18.	30·273	30·117	29·91			30·41	30·28	66	47	54			51½	47½	ne.	ne.		ese.	......	......		......
19.	30·063	30·025	29·70			30·14	30·04	66	45	56			57	52	ne.	ne.		sse.	......	......		......
20.	30·065	30·020	30·01			30·07	30·12	67	42	52			52	47	ne.	n.		e.	·02	......		·11
21.	30·026	29·993	29·66			30·07	30·03	65	49	53			57	54½	ne.	ne.		ese.	......	......		......
22.	30·055	30·005	29·63			29·82	29·91	67	39	56			55	56½	ne.	n.		wsw.	......	......		......
23.	30·114	30·106	29·68			30·12	30·15	73	54	59			55	51½	ne.	nw.		s.	·08	......		·13
24.	30·118	29·939	29·70			30·01	29·70	66	47	62			55	56	ne.	ne.		s.	......	......		......
● 25.	29·815	29·647	29·32			29·70	29·85	64	42	59			45	41	sw.	s.		n.	·06	·28		·08
26.	29·694	29·606	29·21			29·84	29·78	56	43	43			46	45	sw.	nw.		ne.	......	1·07		·05
27.	29·764	29·625	29·17			29·67	29·74	57	39	49			46½	44	w.	nw.		wnw.	·01	......		·11
28.	29·790	29·665	29·62			29·68	29·66	62	30	48			51	52	nw.	nw.		sw.	......	......		·04
29.	29·705	29·601	29·50			29·57	29·48	60	39	43			52½	51½	s.	s.		s.	......	......		·02
30.	29·443	29·340	29·12			29·32	29·04	62	46	53			52½	54	s.	s.		se.	·14	......		......
Mean.	30·167	30·094	29·74			30·145	30·134	66·80	43·50	55·3			52·71	51·10					0·42	1·58		0·91

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

DECEMBER 1851.

LXVI. *On the Mechanical Theory of Electrolysis.* *By* Prof. William Thomson*.

1. CERTAIN principles discovered by Mr. Joule, and published for the first time in his various papers in this Magazine, must ultimately become an important part of the foundation of a mechanical theory of chemistry. The object of the present communication is to investigate, according to those principles, the relation in any case of electrolysis between the electro-motive intensity, the electro-chemical equivalents of the substances operated on, and the mechanical equivalent of the chemical effect produced in the consumption of a given amount of the materials; and by means of it to determine in absolute measure the electro-motive intensity of a single cell of Daniell's battery, and the electro-motive intensity required for the electrolysis of water, from experimental data which Mr. Joule has kindly communicated to me.

2. If a galvanic current, produced by means of a magneto-electric machine, be employed in electrolysis, it will generate, in any time, less heat throughout its entire circuit than the equivalent of the work spent, by an amount which may be called the thermal equivalent of the chemical action which has been effected, being the quantity of heat which would be obtained by recombining the elements of the decomposed substance, and reducing the compound to its primitive condition in every respect; or generally, by undoing all the action which has been done in the electro-chemical apparatus. Now the quantity of heat which is equivalent to the work done is obtained by dividing the number which measures the work by the number which measures by the

* Communicated by the Author.

same unit the mechanical equivalent of the unit of heat. Hence if the mechanical equivalent of the thermal unit be denoted by J, the work done in any time by W, the total quantity of heat evolved in the same time throughout the circuit by H, and the thermal equivalent of the chemical effect produced by Θ, we have

$$H=\frac{W}{J}-\Theta; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

an equation which may also be written in the form

$$W=JH+M, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

if M be used to denote the value of $J\Theta$, or, as it may be called, the mechanical equivalent of the chemical effect produced in the stated period of time.

3. To avoid the necessity of considering variable or discontinuous currents, let us suppose the "machine" to consist of a metallic disc, touched at its centre and at its circumference by fixed wires, and made to revolve in its own plane about an axis through its centre, held in any position not at right angles to the direction of the earth's magnetic force*. If these wires be connected by contact between their ends, there will, as is known, be a current produced in them of a strength proportional directly to the angular velocity of the disc, and inversely to the resistance through the whole circuit. Hence there will be between the ends of the wires, if separated by an insulating medium, an electro-motive force the intensity of which will be constant and proportional to the angular velocity of the disc.

4. Let us now suppose the wires to be connected with the electrodes of an electro-chemical apparatus, for instance a galvanic battery of any kind, or an apparatus for the decomposition of water; and let us conceive the electro-motive intensity between them to be sufficient to produce a current in its own direction. The preceding equations, when applied to this case, will have each of their terms proportional to the time, since the action is continuous and uniform, and therefore it will be convenient to consider the unit of time as the period during which the amounts of work and heat denoted by W and H, and the amount of chemical action of which the thermal and the mechanical equivalents are denoted respectively by Θ and M, are produced. If r denote the radius of the disc, ω the angular velocity with which it is moved, F the component of the earth's magnetic force perpendicular to its plane, and γ the strength of the current which is induced; the work done in a unit of time in moving the disc against the resistance which it experiences in virtue

* This is in fact the "new electrical machine" suggested by Faraday in the Bakerian Lecture of 1832. (Experimental Researches, § 154.)

of the earth's magnetic action on the current through it, will be expressed by the integral $\int_0^r \omega z . \mathrm{F} . \gamma dz$; as is easily proved, whether the current be supposed to pass directly between the centre of the disc and the point of its circumference touched by the fixed wire, or to be, as it in reality must be, more or less diffused from the direct line, on account of the lateral extension of the revolving conductor. Hence we have

$$\mathrm{W} = \frac{1}{2} r^2 \mathrm{F} \gamma \omega. \quad \ldots \ldots \quad (3)$$

5. Let E denote the quantity (in units of matter, as grains for instance) of one of the elements concerned in the chemical action, which is electrolysed or combined in the unit of time, and let θ denote the quantity of heat absorbed in the chemical action during the electrolysis or combination of a unit quantity of that element. Then we have

$$\Theta = \theta . \mathrm{E}. \quad \ldots \ldots \quad (4)$$

$$\mathrm{M} = \mathrm{J} . \theta \mathrm{E}. \quad \ldots \ldots \quad (5)$$

Now it has been shown by Faraday, that in electro-chemical action of any known kind, produced by means of a continuous current, the amount of the action in a given time is approximately if not rigorously proportional to the strength of the current; and all subsequent researches on the subject have tended to confirm this conclusion. The only exception to it which, so far as I am aware, has yet been discovered, is the fact established by Faraday, that various electrolytes can conduct a continuous current, when the electro-motive intensity is below certain limits, without experiencing any continued decomposition*; but from it we may infer as probable, that in general the quantity decomposed with high or low electro-motive intensities is not quite rigorously proportional to the strength of the current.

This non-electrolytic conducting power is, however, at least in the case of water, found to be excessively feeble; and it is not probable that when electrolysis is actually going on in any ordinary case, the quantity of electricity conducted by means of

* It is probable that when an electromotor of an intensity below a certain limit is put in connexion with two platinum electrodes immersed in water, there is at the first instant no electrolytic resistance; and a decomposing current passes which gradually falls off in strength, until the electrodes are, by the separated oxygen and hydrogen, put into a certain state, such that with the water between them, they exert a resisting electric force very nearly equal to that of the electromotor; after which a uniform current of excessively reduced strength passes without producing further decomposition. I hope before long to be able to communicate to the Magazine an account of some experiments I have made to illustrate these circumstances.

it is ever considerable compared with that which is electrically conducted; and the normal law of true electrolytic conduction will therefore be assumed as applicable to the conduction through the electro-chemical apparatus, subject to modification in any case in which the deviations from it can be determined. If, then, we denote by ϵ the electro-chemical equivalent of the particular element referred to for measuring the chemical action, that is, the quantity of it which is electrolysed or combined in a unit of time by the operation of a current of unit strength, since the actual strength of the current is γ, we have

$$E=\epsilon\gamma. \quad \ldots \ldots \quad (6)$$

The deviations from the normal law which may exist in any particular case may be represented by giving ϵ a variable value. For instance, if it were true that when the electro-motive intensity in an apparatus for the decomposition of water exceeds a certain limit, there is decomposition at a rate precisely proportional to the strength of the current; and when the intensity is below that limit, a slight current passes without any decomposition; ϵ would be a discontinuous function of the intensity, having a constant value when the intensity is above, and being zero when the intensity is below, the limit for decomposition.

6. According to Joule's law of the generation of heat in the galvanic circuit, the quantity of heat developed in a unit of time would be rigorously proportional to the square of the strength of the current, if the total resistance were constant in all the circumstances considered; and therefore we may conveniently assume

$$H=R\gamma^2; \quad \ldots \ldots \quad (7)$$

but as we are not sure that the whole resistance is independent of the strength of the current when an electrolysed fluid forms part of the circuit, we must not assume that R is constant. In what follows, all that is assumed regarding the value of R is, that it is neither infinitely great nor infinitely small in any of the circumstances considered*.

* Since the present article was put into the Editor's hands, I have become acquainted with a paper by Mr. Joule "On the Heat evolved during the Electrolysis of Water," published by the Literary and Philosophical Society of Manchester in 1843 (vol. vii. part 3, second series), in which it is shown, that in some cases of electro-chemical action (for instance, when hydrogen is evolved at an electrode or battery-plate of a metal possessing a considerable affinity for oxygen) there is a "resistance to electrolysis without chemical change," producing "a reaction on the intensity of the battery," and causing the evolution of heat to an amount exactly equivalent to the loss of heating power, or of external electro-motive force, which the battery thus suffers. In any electro-chemical apparatus in which this kind of resistance occurs, the quantity of heat developed by a current of strength γ will be expressible in the form $A\gamma+B\gamma^2$, where A and B are finite when

7. If we substitute the expressions (3), (4) and (6), (7) for the three terms of the original equation (1), we have

$$R\gamma^2=\frac{\frac{1}{2}r^2F\gamma\omega}{J}-\theta\epsilon\gamma, \quad . \quad . \quad . \quad . \quad (8$$

from which we deduce

$$\gamma=\frac{\frac{1}{2}r^2F\omega-J\theta\epsilon}{JR}. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

8. It appears from this result that the value of γ will be positive or negative according as the angular velocity of the disc exceeds or falls short of a certain value Ω, given by the equation

$$\Omega=\frac{J\theta\epsilon}{\frac{1}{2}r^2F}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and therefore we conclude that, when the angular velocity has exactly this value, the electro-motive intensity of the disc is just equal to the intensity of the reverse electro-motive force exerted on the fixed wires, by the electro-chemical apparatus with which they are connected.

9. If we adopt as the unit of electro-motive intensity that which is produced by a conductor of unit length, carried, in a magnetic field of unit force, with a velocity unity, in a direction which is both perpendicular to its own length and to the lines of force in the magnetic field, it is easily shown that the electro-motive force of the disc, in the circumstances specified above, is given by the equation

$$i=\frac{1}{2}r^2F\omega. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Hence if I denote the electro-motive force of the disc when it just balances that of the chemical apparatus, we have by (10)

$$I=J\theta\epsilon. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

This equation comprehends a general expression of the conclusion long since arrived at by Mr. Joule, that the quantities of heat developed by different chemical combinations are, for quantities of the chemical action electrically equivalent, proportional to the intensities of galvanic arrangements adapted to allow the combi-

γ is infinitely small. Consequently what is denoted in the text by R will be equal to $\frac{A}{\gamma}+B$, and will therefore be infinitely great when γ is infinitely small. The modification required for such cases will be simply to use B in place of R, and to diminish the value of I found in the text (12) by JA; but the assumption that R does not become infinite in any of the circumstances considered is, I believe, quite justifiable in the two special cases which form the subject of the present communication.—W. T. Nov. 1, 1851.

nations to take place without any evolution of heat in their own localities; and it may be stated in general terms thus:—

The intensity of an electro-chemical apparatus is, in absolute measure, equal to the mechanical equivalent of as much of the chemical action as goes on with a current of unit strength during a unit of time.

10. When ω is less than Ω, γ is (§ 8) negative; and hence equations (3), (5) and (6), show W and M to be negative also. In this case the direction of the current is contrary to the electromotive force of the disc; the chemical action is the source of the current instead of being an effect of it; and the disc by its rotation produces mechanical effect as an electro-magnetic engine, instead of requiring work to be spent upon it to keep it moving as a magneto-electric machine. If we assume

$$\gamma=-\gamma', \quad M=-M', \quad W=-W',$$

so that when γ, M, and W are negative their absolute values may be represented by γ', M$'$ and W$'$, we find by (9), (10), (5), (6), (2), (3) the following expressions for these quantities:

$$\gamma'=\frac{\frac{1}{2}r^2F}{JR}(\Omega-\omega) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$M'=J\theta\epsilon\gamma'=\frac{\frac{1}{2}r^2F.\theta\epsilon}{R}(\Omega-\omega) \quad . \quad . \quad . \quad (14)$$

$$W'=M'-JH=\frac{1}{2}r^2F\omega.\gamma'=\frac{\omega}{\Omega}M'. \quad . \quad (15)$$

The first of the three expressions (15) for W$'$ merely shows that the mechanical effect produced by the disc in any period of time is less than M$'$, the full mechanical equivalent of the consumption of materials in the electro-chemical apparatus, by the mechanical equivalent of the heat generated in the whole circuit during that period. From the third we infer, that the fraction of the entire duty of the consumption which is actually performed by the engine is equal to $\frac{\omega}{\Omega}$. If ω were precisely equal to Ω, the electro-motive force of the battery would be precisely balanced, and there could be no current, and hence the performance of the engine cannot be perfect; but if ω be less than Ω by an infinitely small amount, the battery will be allowed to act very slowly; a very slight current, with a very small consumption of materials, will be generated; and the mechanical effect produced from it will be infinitely nearly equal to the whole duty, and infinitely greater than the portion of the effect wasted in the creation of heat throughout the circuit.

11. A condition precisely analogous to that of *reversibility,*

established by Carnot and Clausius as the criterion of perfection for a thermo-dynamic engine*, is applicable to this electro-magnetic engine; and is satisfied by it when the disc revolves with an angular velocity infinitely nearly equal to Ω, since then γ', M', and W' are each of them proportional to $\Omega-\omega$, whether this quantity be positive or negative; and consequently if the motion of the disc relatively to a state of rotation with the angular velocity Ω be reversed, all the physical and mechanical agencies are reversed.

12. From experiments made at Manchester in the year 1845 by Mr. Joule, on the quantity of zinc electrolysed from a solution of sulphate of zinc by means of a galvanic current measured by his tangent galvanometer, I have found the electro-chemical equivalent of zinc to be ·07284†; and I am informed by him, that from other experiments which he has made, he finds that the entire heat developed by the consumption of a grain of zinc in a Daniell's battery is as much as would raise the temperature of 769 grains of water from 0° to 1° Cent.‡ Hence, if we wish to apply the preceding investigations to the case in which the electro-chemical apparatus (§ 4) is a single cell of Daniell's battery, we may consider the consumption of a grain of zinc as the unit of the chemical action which takes place, and therefore we have

$$\epsilon = \cdot 07284, \quad \theta = 769.$$

Again, according to Mr. Joule's last researches on the mechanical equivalent of heat, the work done by a grain of matter in descending through 1390 feet is capable of raising the tem-

* "If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motions are all reversed, it produces as much mechanical effect as can be produced by any thermo-dynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat." (From § 9 of "Dynamical Theory of Heat." Transactions of the Royal Society of Edinburgh, March 17, 1851, vol. xx. part 2.)

† See Note on Electro-chemical Equivalents published at the end of this paper.

‡ By experiments on the friction of fluids, Mr. Joule has found that the quantity of work necessary to raise the temperature of a pound, or 7000 grains, of water from 0° to 1° Cent. is 1390 foot-pounds. Hence the mechanical equivalent of the consumption of a grain of zinc in Daniell's battery is 152·7, or nearly 153 foot-pounds. Messrs. Scoresby and Joule, in their paper "On the Powers of Electro-magnetism, Steam and Horses," (Phil. Mag., vol. xxvi. 1846, p. 451) use 158 as the number expressing this equivalent according to earlier experiments made by Mr. Joule. The experiments from which he deduced the thermal equivalents of chemical action, communicated to me for this paper, are described in a paper communicated to the French Institute, and acknowledged in the *Comptes Rendus* for Feb. 9, 1846, but not yet published.

perature of a grain of water from 0° to 1°. Hence, since the unit of force adopted in the measurement of galvanic strength on which the preceding value of ϵ is founded, is that force which, operating during one second of time upon one grain of matter, would generate a velocity of one foot per second, and is consequently $\frac{1}{32 \cdot 2}$ of the weight of a grain at Manchester, we have

$$J = 1390 \times 32 \cdot 2 = 44758.$$

Substituting these values for ϵ, θ, and J in (12), we have

$$I = 2507100$$

for the "intensity" or "electro-motive force" of a cell of Daniell's battery in absolute measure. To compare this with the electro-motive intensity of a revolving disc such as we have considered (§ 3), let the axis of rotation of the disc be vertical or nearly vertical, and, the vertical component of the terrestrial magnetic force at Manchester being about 9·94, let us suppose that we have $F = 10$ exactly, which would be the case with a disc exactly horizontal in localities a little north of Manchester, and might be made the case in any part of Great Britain by a suitable adjustment of the axis of the disc. Then we have by (11),

$$i = 5\omega r^2;$$

or if n be the number of turns per second,

$$i = 5 \times 2\pi n r^2 = 31 \cdot 416 \times n r^2.$$

Hence

$$\frac{i}{I} = \frac{31 \cdot 416 \times n r^2}{2507100} = \frac{n r^2}{79803}.$$

It appears, therefore, that if the radius of the disc be one foot, it would, when revolving at the rate of one turn per second, produce an intensity $\frac{1}{79803}$ of that of a single cell of Daniell's, and it would consequently have to make more than 79803 turns per second to reverse the action of such a cell in the arrangement described in § 4*. We conclude also, that a disc of one foot radius, touched at its centre and circumference by the electrodes of a single cell of Daniell's, and allowed to turn about a vertical axis by the action of the earth upon the current passing through it, would revolve with a continually accelerated motion approaching to the limiting rate of 79803 turns per second, if it

* Hence in the multiple form of "the new electrical machine" suggested by Faraday, about 800 discs, each one foot in radius, would be required, so that with a rotation at the rate of 100 turns per second about a vertical axis in any part of Great Britain, it might give an intensity equal to that o a single cell of Daniell's.

were subject to no frictional or other resistance; and that if, by resisting forces, it were kept steadily revolving at the rate of n turns per second, it would, in overcoming those forces, be performing $\frac{n}{79803}$ of the whole work due to the consumption of zinc and deposition of copper in the battery.

13. If the electro-chemical apparatus mentioned in § 4 be a vessel of pure water with two plates of platinum immersed in it, we may consider a grain of hydrogen electrolysed as the unit for measuring the chemical action which takes place. Now Mr. Joule finds that, in the electrolysis of one grain of hydrogen from water acidulated with sulphuric acid, as much heat is absorbed as would raise the temperature of 33553 grains of water from 0° to 1°. Hence θ must be less than 33553 by the quantity of heat evolved when as much pure water as contains one grain of hydrogen is mixed with acidulated water, such as that used by Mr. Joule; but, without appreciable error on this account, we may take

$$\theta=33553.$$

I have found also, from results of experiments on the electrolysis of water made by Mr. Joule at Manchester in 1845, that the electro-chemical equivalent of hydrogen is ·002201. Using this value for ϵ, and the values indicated above for θ and J, we have by (12),

$$I=3305400$$

for the electro-motive force, in absolute measure, required for the decomposition of water. This exceeds the electro-motive force of a single cell of Daniell's battery, found above, in the ratio of 1·318 to 1. Hence at least two cells of Daniell's battery are required for the electrolysis of water; but fourteen cells of Daniell's battery connected in one circuit with ten electrolytic vessels of water with platinum electrodes would be sufficient to effect gaseous decomposition in each vessel.

14. In the Bakerian Lecture of 1832, "On Terrestrial Magneto-electric Induction," Faraday, after describing some experiments he had made at Waterloo Bridge, without however arriving at any positive results, to test the existence of an inductive effect of the terrestrial magnetic force upon the flowing water of the Thames, brought forward some very remarkable speculations regarding the possible effects of magneto-electric induction upon large masses in motion relatively to the earth, or upon the earth itself in motion with reference to surrounding space. The preceding investigations enable us to compare the electro-motive forces in such cases with electro-motive forces the effects of which are familiarly known to us, and so to form some estimate,

it may be very vague, of the anticipated effects. Thus let us conceive a mass of air or water, or any other substance moving relatively to the earth with a velocity V, and let A and B be two fixed points in it or at its two sides, at a distance a apart, in a line perpendicular to the direction of motion. Then if F be the component of the terrestrial magnetic force perpendicular to the plane of AB and the lines of motion across it, there will be between A and B, or between any fixed conductors connected with them, and insulated in all other places from the moving mass, an electro-motive force, the intensity of which is given by the equation

$$i = \mathrm{F}.\mathrm{V}.a.$$

15. If, for instance, the velocity be one mile per hour, we should have $\mathrm{V}=1{\cdot}4667$; and if we take $\mathrm{F}=10$, which will be nearly the case for a mass moving horizontally in any part of Great Britain*, we have

$$i = 14{\cdot}667 \times a.$$

If we take $a=960$, we find $i=14080$ for the electro-motive force between two platinum plates immersed, as in Faraday's experiment, below the surface of the Thames, at a distance of 960 feet apart across the stream, when the tide is in such a state that the current is at the rate of a mile an hour. The electro-motive force, varying directly as the rate of the current, must therefore, when there is a current of two miles and a half an hour, be 35200, which is very little more than $\frac{1}{100}$ of that which was found in § 13 for the intensity required to decompose water; and as there is probably in no state of the tide a current of more than three or four miles an hour, it is not to be wondered at that no galvanic current was discovered in a wire connecting the platinum plates.

16. An experiment on a much larger scale might be performed by means of the telegraph wires which have recently been laid between England and France, across the straits of Dover, by simply connecting the ends of one of these wires with platinum plates immersed in the sea on the two sides of the channel. If the distance between the plates be twenty miles, in a direction on the whole at right angles to the direction of the motion of the water through the channel, and if, in a particular state of the tide, there be an average velocity of a mile an hour, there would, as

* In June 1846 the horizontal magnetic force was found to be 3·7284, and the dip 68° 58′, at Woolwich (Philosophical Transactions, 1846, p. 246). Hence the vertical force was $3{\cdot}7284 \times \tan 68° 58'$, or 9·696. At the same period it was 9·94 at Manchester, and it must have been 10 exactly at localities in England or Scotland not far north of Manchester.

we find from the preceding expression, by substituting 20×5280 for a, be an electro-motive force of 1,549,000, or very nearly half of that which is required for the decomposition of water. It is not probable that the current produced by the action of this force alone through the wire connecting the platinum plates would be found to be sensible; since a sensible continuous current with water and platinum electrodes in the circuit can scarcely be obtained by any electro-motive force less than that which is required for the continued gaseous decomposition of water. The existence of the inductive action might, however, I think, be tested by using a galvanic battery of very low intensity, to assist the electro-motive force arising from induction, and by adding a little nitric acid to the liquid till it is found that a sensible current is produced. It might then be observed whether or not, when the tide turns and the water flows in the other direction through the channel, the electrical current becomes insensible, or becomes less than it was; and whether it goes on again as before when the tide turns again, and the water flows as it did at first. There would probably be some difficulty experienced in keeping the electro-motive force of the battery sufficiently constant during twelve hours to make the experiment perfectly satisfactory, and many difficulties that could not be foreseen might occur. If, however, in any state of the tide the mean rate per hour of the stream in the Channel exceeds two miles or two miles and a half, it is probable that the inductive action might produce a sensible electric current in the telegraph wire without such assistance. It is very much to be desired that the experiment should be tried, as it would afford probably the best test that could at present be applied, to find whether electrolysable liquids possess the property of magneto-electric induction discovered by Faraday in metallic conductors.

17. The possible magneto-electric effects of the earth's rotation were also considered by Faraday, and it was conjectured that electricity may, in virtue of them, rise to considerable intensity. The general nature of the effect was shown to be a tendency for electricity to flow through the earth from the equator towards the poles, from whence it would endeavour to return externally to the equatorial regions. If the distribution of terrestrial magnetism were perfectly symmetrical about the axis of rotation, there could be no other kind of effect than this produced by the rotatory motion; and, neglecting at present the currents in complete external circuits, which may exist in virtue of the want of this symmetry, we may endeavour to form a rough estimate of the electro-motive force that would exist between the equatorial regions of the revolving mass and a quadrantal conductor fixed relatively to the earth's centre, with one end near the surface at

the equator and the other touching the surface at one pole. The electrical circumstances would be the same if the earth were at rest, and the conductor were made to revolve once round in $23^h\ 56^m\ 4^s$, with one end always touching at the pole, and the other close to the surface at the equator. In such circumstances there would be an electro-motive force equal to $f.v.ds$ on any infinitely small element ds of the moving conductor, if v denote the velocity of its motion, and f the vertical magnetic force at the part of the earth over which it is passing. Now if θ be the latitude of the element ds, and V the velocity of the surface at the equator, we have

$$v=\mathrm{V}\cos\theta;$$

if the distribution of magnetic force at the surface be, as in making this rough estimate we may assume it to be, of the simplest type, we have

$$f=\mathrm{F}\sin\theta,$$

where F denotes the vertical magnetic force at the pole; and if r denote the earth's radius, we have

$$ds=rd\theta.$$

The intensity of the total electro-motive force between the equatorial end of the moving conductor and the earth, being the sum of the electro-motive forces on all its elements, will consequently be equal to

$$\int_0^{\frac{1}{2}\pi}\mathrm{FV}r\sin\theta\cos\theta\,d\theta;$$

and hence, denoting it by i, we have

$$i=\frac{1}{2}\mathrm{FV}r.$$

Now the earth's diameter being about 7912 miles, we have $r=3956\times5280$; and, by dividing the number of feet in the earth's circumference by 86164, the number of seconds in the sidereal day, we find $\mathrm{V}=1523$. If we take $\mathrm{F}=14$, we find, by substituting these values for the factors of the preceding expression,

$$i=222{,}700{,}000{,}000.$$

This is about 88800 times the intensity of a single cell of Daniell's battery (§ 12), and may therefore be about 50 times that of the battery of two thousand pairs of copper and zinc plates, charged with nitro-sulphuric acid, by which Sir Humphry Davy only obtained sparks half an inch long in the exhausted receiver of an air-pump. Now the electro-motive force we have been considering could in reality only produce galvanic currents by forcing a passage through the whole thickness of the atmosphere, upwards from the surface about the poles, and downwards to the earth in the equatorial regions, and we may conclude *that it does not produce galvanic currents.*

18. From the smallness of the electro-motive intensity in this extreme case, we may infer that no part of the phænomena of atmospheric electricity can be attributed to the inductive action of the terrestrial magnetism on masses of air or water in motion near the surface of the earth.

19. If the space surrounding the earth, beyond the limits of the atmosphere, were capable of conducting electricity, and were affected as a fixed conductor by the motion of a magnet in the neighbourhood of it, there would be electrical currents in complete external circuits, induced both by the earth's rotatory motion, on account of the distribution of magnetism not being symmetrical about the axis of rotation, and by its motion through space; and it is I think far from improbable that the phænomena of aurora borealis and australis are so produced. It is quite impossible, in the present state of science regarding the relative motion of the earth or of the solar system, and the medium filling all space, which by its undulations transmits light and radiant heat, to form any estimate on satisfactory principles of the inductive electro-motive forces which may arise from the motion of translation of the terrestrial magnet through this medium; but we may form some idea of those which its rotatory motion may produce by calculating the total electro-motive force on a closed conductor held externally in a fixed position with reference to the earth's centre. Thus let us conceive a circular conductor, of radius R, to be held with a diameter coincident with the earth's axis of rotation; and let i be the intensity of the total electro-motive force which it would experience if it were made to revolve round the earth once in $23^{\mathrm{h}}\,56^{\mathrm{m}}\,4^{\mathrm{s}}$, and the earth held at rest. Denoting by P the radial component of the terrestrial magnetic force at any element of this conductor, and in other respects using the same notation as before, we have

$$i=\int_{-\pi}^{\pi} \mathrm{P}.\frac{\mathrm{R}}{r}\mathrm{V}\cos\theta\,.\,\mathrm{R}d\theta=\mathrm{V}\frac{\mathrm{R}^2}{r}\int_{-\pi}^{\pi}\mathrm{P}\cos\theta d\theta.$$

If we assume the distribution of magnetic force at the earth's surface to be of the simplest type, the force at either magnetic pole to be 15, and the magnetic axis to be inclined at an angle of 20° to the axis of rotation, we have, at the time when the moving conductor is passing over the earth's magnetic poles,

$$\mathrm{P}=15\frac{r^3}{\mathrm{R}^3}\sin(\theta+20^\circ);$$

and in these circumstances we have consequently

$$i=15\mathrm{V}\frac{r^2}{\mathrm{R}}\int_{-\pi}^{\pi}\sin(\theta+20^\circ)\cos\theta d\theta=15\times1523\,.\frac{r^2}{\mathrm{R}}\pi\sin 20^\circ.$$

If we take $r=\mathrm{R}$, we find, from this,

$$i=512{,}700{,}000{,}000,$$

which is about 204000 times the intensity of a single cell of Daniell's. One-half or one-third of this amount would be the electro-motive force experienced by a fixed circular conductor of twice or three times the earth's diameter, at the time when the earth's magnetic poles are passing under it.

Greypoint, County Down,
October 6, 1851.

Note on Electro-chemical Equivalents.

The electro-chemical equivalents of zinc and hydrogen used in the preceding paper were deduced from experiments made by Mr. Joule on the electrolysis of sulphate of zinc and of water acidulated with sulphuric acid, in which the galvanic currents used were measured by means of a tangent galvanometer consisting of a needle half an inch in length, suspended in the centre of a circular conductor one foot in diameter, fixed in the plane of the magnetic meridian. The electro-chemical equivalent of a substance, being defined as the mass (in grains) electrolysed from any combination in a second of time by the action of a current of unit strength, will be found by dividing the mass of the substance electrolysed per second in any experiment by the strength of the current. One way of combining several experiments so as to obtain a mean result, will be to take the arithmetical mean of the quantities of the substance found to be electrolysed per second in the different experiments, and divide it by the mean of the observed strengths of the currents. In the tangent galvanometer, the tangents of the angles of deflection are proportional to the strengths of the currents, and consequently the arithmetical mean of the tangents of the angles of deflection in different experiments will be the tangent of the angle of deflection corresponding to a current of mean strength. The mean results, taken in this way, of four experiments on the electrolysis of sulphate of zinc, and of four experiments on the electrolysis of acidulated water, made at Manchester on the 8th, 9th, 15th and 16th of September 1845, are as follows:—

Electrolysis of Sulphate of Zinc.

Mean corrected tangent of deflection.	Zinc deposited per second.
·7345	·01508 grain.

Electrolysis of Acidulated Water.

Mean corrected tangent of deflection.	Hydrogen liberated per second.
1·7600	·001092 grain.

To determine the strength of the current (γ) in absolute measure, which produces a deflection (δ) of the needle in the tangent galvanometer, we have the equation

$$\gamma = \frac{r\mathrm{H}}{2\pi} \tan \delta,$$

where r denotes the radius of the circular conductor, and H the horizontal component of the earth's magnetic force, in absolute measure; since the magnetic axis of the needle will be drawn from the magnetic meridian into a vertical plane containing the resultant of the horizontal force H in the magnetic meridian, and the force $\frac{2\pi\gamma}{r}$ perpendicular to the plane of the conductor, and consequently to the magnetic meridian. It is impossible at present to assign with accuracy the values of the horizontal magnetic force at Manchester at the times when the experiments were made; but according to data which Colonel Sabine has kindly communicated to me, it must have been nearly 3·542 in 1846, and cannot probably at any time of observation during that or the preceding year have differed by as much as $\frac{1}{100}$ of its value from that amount. Taking, therefore, 3·542 for H, and taking $\frac{1}{2}$ for r (the diameter of the conductor being one foot), we have $\frac{r\mathrm{H}}{2\pi} = {\cdot}28186$; and consequently, for observations made with Mr. Joule's tangent galvanometer at Manchester in 1846,

$$\gamma = {\cdot}28183 \times \tan \delta.$$

Hence from the preceding experimental results, we find for the electro-chemical equivalent of zinc,

$$\frac{{\cdot}01508}{{\cdot}28183 \times {\cdot}7345}, \text{ or } {\cdot}07284;$$

and for the electro-chemical equivalent of hydrogen,

$$\frac{{\cdot}001092}{{\cdot}28183 \times 1{\cdot}7600}, \text{ or } {\cdot}002201.$$

From the mean results of a series of four experiments on the electrolysis of sulphate of copper, communicated to me by Mr. Joule, I have found for the electro-chemical equivalent of copper,

·07052.

In Dove's *Repertorium* (vol. viii. p. 273), values of the electro-chemical equivalents of water and zinc, determined by Weber, who was the first to give an electro-chemical equivalent in absolute electro-magnetic measure, and by other experimenters, are given in absolute measure according to the French units. To reduce these to British measure, we must multiply by (2·1692), the square root of the fraction obtained by dividing the number (15·438) of grains in a gramme, by the number (3·2809) of feet in a metre. The electro-chemical equivalent of water is obtained by multiplying that of hydrogen by 9; and according to the theory of equivalence in electro-chemistry, it might also be obtained by multiplying the electro-chemical equivalent of zinc by $\frac{9}{32\cdot 3}$, and that of copper by $\frac{9}{31\cdot 7}$. The following table shows the values of the electro-chemical equivalent of water in British absolute measure obtained in these different ways.

Observers.	Galvanometer used.	Electro-chemical action observed.	Deduced electro-chemical equivalent of water.
Weber	The "electro-dynamometer"	Decomposition of water	·02034
Bunsen	Tangent galvanometer	Decomposition of water	·02011
Bunsen	Ditto.	Dissolution of zinc......	·01995
Casselmann ...	Ditto.	Decomposition of water in acid and saline solutions	·02033
Casselmann ...	Ditto.	Zinc [deposited or dissolved ?]	·02021
Joule	Ditto.	Decomposition of water	·01981
Joule	Ditto.	Deposition of zinc from solution of sulphate of zinc	·02030
Joule	Ditto.	Deposition of copper from solution of sulphate of copper	·02002

LXVII. *Geometry and Geometers. Collected by the late* Thomas Stephens Davies, *F.R.S.L. & E. &c.**

No. VIII.

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

ONE or two remarks upon points suggested by Professor Rigaud's letter, apart from all considerations about Pappus, Halley, and Porisms, will not be out of place here; viz. upon the

* Communicated by James Cockle, Esq., M.A., Barrister-at-Law, who adds the following note:—

[" The above manuscript and its accompanying foot-note, both of which references to Professor Leybourn's speculations as a publisher*.

Mr. Leybourn came to London, as most young men do, to make his way in the world. He had served an apprenticeship to a business in Shields; but during that period had contracted a strong propensity to the study of mathematics, and had determined somehow or other (as he should find an opening) to render his inclinations coincident with his "bread-and-cheese" toils. The companion of his pilgrimage was the late Mr. Glendinning, the printer of Hatton Garden.

Mr. Leybourn was fortunate enough to obtain the partiality and friendship of Dr. Hutton; and being a handsome young

(with the exception of the heading, for which I am responsible) are autographs of my deceased friend Professor T. S. Davies, have been confided to me by Mrs. Davies. From the strong internal evidence offered by the opening paragraph of the above paper, I infer that the present are the 'remarks' alluded to by Professor Davies at p. 394 of vol. xxxvi. S. 3, and reserved by him for another occasion. They were apparently not intended by their author to stand alone; but in the difficulty, perhaps impracticability, of realizing the views of one so many of whose scientific aspirations were frustrated by death, I hope to find a sufficient apology for so placing them. They form at all events an interesting fragment of mathematical biography and history, and I considered it better to forward them for publication now, than to delay them until I could communicate with Mrs. Davies, and request her to search among Professor Davies' papers for those intended to accompany them. The result of such a search would moreover be doubtful, and I have delayed these papers too long already.

"The following references may be found useful, my deceased friend having omitted to give back-references at the commencement of his papers:—No. I. of 'Geometry and Geometers' will be found in the Phil. Mag. S. 3. vol. xxxii. p. 419; No. II., Ibid. vol. xxxiii. p. 201; No. III., Ibid. p. 513; No. IV., Ibid. vol. xxxv. p. 497; No. V., Ibid. vol. xxxvi. p. 382. In No. V. will be found Professor Rigaud's letter alluded to in the text.

"James Cockle.

"2 Pump Court, Temple,
Sept. 8, 1851."]

* Mr. Leybourn speculated extensively in the way of publishing mathematical works; and I am convinced with the purest motives for the advance of science, and with very little eye to whether he should lose or gain. All his business operations were conducted through his printer Glendinning, of Hatton Garden (to whom he was greatly attached, and whose press was alternately at the service of mathematics for Leybourn, posting bills, bills for the auctioneer, and programmes of the entertainments at Sadler's Wells); but the indifference of the workmanship and paper, together with the extreme irregularity with which his Repository appeared (varying from a few months to eight or ten years) prevented the possibility of pecuniary success attending that undertaking. The book referred to by Professor Rigaud was one of his "ventures," viz. Strachey's translation of the Beja Ganita. The editorship of the Gentleman's Diary for the Stationers' Company, was probably the only undertaking which ever

man, was a welcome visitor to the family. This was during the feverish feeling created in England by the first French revolution.

Whether the Mathematical Repository or the Gentleman's Mathematical Companion was first *projected* I do not know with perfect certainty, but several trivial circumstances incline me to the belief that the latter was. It is pretty clear, however, that Mr. Leybourn was first in the open field. I cannot give the exact date of its first issue; but from the cover of No. 3 bearing the date March 1, 1797, and the work being published half-yearly (at first only and with tolerable regularity) we may put down its origin as March 1, 1796. The first number of the Mathematical Companion bears date *for* 1798, and was therefore printed in the preceding year, probably about the same time (November) as the almanacs. This will explain the ground of the otherwise unaccountable opposition of the "Diary Editor," Dr. Hutton, to the Mathematical Companion; the consequent assumption of a different form of title; and making it an independent work, trusting to its own merits rather than one of homage to the Stationers' Hall editors of the period.

cleared him a sixpence, and this he held from Dr. Hutton's death as long as he himself lived.

If, however, Mr. Leybourn lost money, he at the same time gained a high reputation from editing his Mathematical Repository. He thus obtained one great object of his early ambition, which he could have gained in no other way; for the most devoted of his friends and admirers (amongst whom I place myself) will not contend for a moment, that either his range of power or his mathematical acquirements could have gained for him that reputation, in whatever other way exerted. The Repository (as well as the Diary) was *edited practically by his friends from its origin to its termination*. Dr. Hutton aided him in the outset of the first series, and subsequently Dr. Gregory and Mr. Lowry. In the earlier part of the new series he was dependent on the judgement of Messrs. Dalby, Lowry, Wallace and Ivory, with one or two others occasionally. In closing the fifth volume, and throughout the sixth, this office devolved partly on Mr. Woolhouse, but mainly on myself. During this latter period, too, the same may be said of the Gentleman's Diary. Dr. Gregory, however, supplied the almanac part of the Diary.

I have felt this distinct statement to be necessary to prevent some misconceptions that might hereafter arise, if they have not (as I am led to think they have) already been formed, as to my connexion with the Repository and Diary. My labours were neither few nor small, but they were wholly gratuitous; for besides what I wrote in those works, I had immense masses of papers (often very jejune and absurd) to read; and from these to select what should be printed—a kind of labour, the disagreeableness and tediousness of which can alone be understood by those who have exercised similar editorial functions in respect of *mathematical papers and solutions*.

I bear my testimony, however, to Professor Leybourn's honourable intentions, and (as long as he was a free agent—which latterly he ceased to be) his high sense of honourable friendship. He was a *good* if he was not a *great* man.—T. S. D.

LXVIII. *Account of Experiments demonstrating a limit to the Magnetizability of Iron.* By J. P. Joule, *F.R.S.*

[Concluded from p. 315.]

On Electro-Magnetic Forces.*

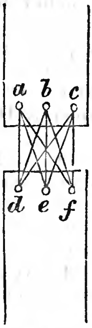
I HAVE shown in a previous paper, that when a current of voltaic electricity is transmitted through the coils of two electro-magnets, their mutual attraction is proportional to the square of the quantity of electrical force, and also that the lifting power of the horseshoe electro-magnet is governed by the same law.

I have recently made experiments which prove that the attraction of an electro-magnet, for a magnet of constant force, varies in the simple direct ratio of the quantity of electricity passing through the coil of the electro-magnet. In order to succeed it was necessary to guard against the effects of induction by a proper arrangement of the apparatus.

Magnetism appears therefore to be excited in soft iron in proportion to the intensity of the magnetizing electrical force; and electro-magnetic attraction, as well as the attraction of steel magnets, may be considered as proportional to the product of the intensities of magnetic force in the bodies attracting one another.

I have recently learned that the Russian philosophers, Jacobi and Lenz, have arrived at some of the same conclusions with regard to the laws of electro-magnetic attraction.

The accompanying figure will perhaps illustrate, with some degree of accuracy, the complex action of the forces which constitute the aggregate attraction which exists between two magnets. The magnetic particles, of which six only, viz. *a b c d e f*, are represented, may be conceived to be of an indefinitely large number spread throughout the region of the poles: the straight lines drawn between the particles represent the directions of the several attractive forces.



If this view be correct, it is obvious that the closer the approximation of the magnetic particles in each magnet, the greater will be the aggregate attraction; for in that case the particle *a*, for instance, will be nearer the particle *f*, and the force exerted between them will be in a less oblique direction.

It was in consequence of the entertainment of a different hypothesis that I was led, by the experiments recorded in the previous paper, to imagine that I had detected a decrease of power due to an increase of the length of the electro-magnets. I gave a comparison of the attractions of electro-magnets 14 inches long and of the several sections $\frac{5}{11}$ths, $\frac{6}{11}$ths, and $\frac{7}{11}$ths of

* Annals of Electricity, vol. iv. p. 474.

an inch square, with the attractions of electro-magnets 30 inches long and of the sections 1 inch square and 2 inches by 1. In these experiments the poles of the attractive magnets were $\frac{1}{8}$th of an inch distant from one another, and it is probable that the deterioration of the attractive energy of the long electro-magnets was principally owing to the greater extent of their polar surfaces.

Hence also corrections ought to be applied to the attractions of the larger electro-magnets in Tables I. and II. of my previous paper, in order fairly to compare their respective powers with those of the smaller magnets.

These corrections would not, however, be of sufficient amount to affect the general conclusion at which I have arrived, with regard to the laws under which magnetic attraction (as applicable to the production of motive force) is developed by electricity, viz. *that the attraction of two electro-magnets towards each other is in every case represented by the formula* $M = W^2E^2$, *where* M *denotes the magnetic attraction,* W *the length of wire, and* E *the quantity of electricity conveyed by the wire in a given period of time;* a formula *modified* merely by the effects of approaching saturation, of the conducting power of the iron, and of the distance of the coils from the surface of the iron.

I have observed that magnetic and electro-magnetic attractions decrease in certain cases in the simple ratio of the distances. This was found to be the case, particularly when the magnets were long and the distance between them small. Mr. Harris has noticed the same phænomenon in his "Experimental Inquiries concerning the laws of Magnetic Forces." It may be almost entirely accounted for by the complex action previously illustrated. It is impossible to doubt, that the law of magnetic attraction is that of the inverse square of the distances.

* * * * * *

Broom Hill, near Manchester,
March 10, 1840.

On Electro-Magnetic Forces.*

In resuming the relation of my researches, I shall consider the laws which govern that peculiar condition which is assumed on the completion of the ferruginous circuit, viz. the lifting or sustaining power of the electro-magnet.

Although this wonderful property is known to all, and a variety of forms have been given to the electro-magnet both as regards the bulk and shape of its iron and the length and number of its spirals, I am not aware that any general rules have been laid down for its manufacture. I shall therefore attempt to supply this want, and in so doing shall describe a construction attended by far greater results than have hitherto been produced.

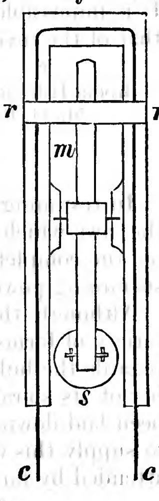
* Annals of Electricity, vol. v. p. 187.

It was my desire to make my experiments as exact as possible; and as I wish the relation of them to be clear and definite, I shall begin with some observations on the *measure* of current electricity indicated by my galvanometer.

* * * * *

The galvanometer of which I made use in the last series of researches was connected with an electrolytic apparatus furnished with very fine platina wires. Voltaic currents of varied intensity were then conducted through the circuit which included the two instruments, the circuit being broken at the end of two or three minutes in each case, and the hydrogen measured in a graduated glass tube. The mean of ten trials gave 0·76 of a grain as the quantity of water decomposed during each hour by the electrical current indicated by the unit of my former quantity numbers. Hence the current indicated by 11·8 of these last would decompose nine grains, or one equivalent of water per hour. This current I propose to call a *degree* in the present paper. The dimensions of the single coil of the above galvanometer are 12 inches by 6, and the deviation of its needle for one *degree*, 34° of the graduated card. From these data we may easily calculate the value of the indications of any similar instrument, bearing in mind that the electro-dynamic force produced by a constant current of electricity is directly as the number of coils and inversely as their linear dimensions.

The quantities of current electricity which were brought into play in the subsequent experiments were frequently so great, that the needle of the above galvanometer would have been brought almost to a right angle with the plane of the coil, if subjected to their influence. I therefore devised a new measure, which I flatter myself will prove of greater service in some cases than the instrument proposed for the same purpose by Mr. Iremonger*. The plan of my instrument is represented by the accompanying figure, in which *cc* is a rod of copper bent double, and fastened firmly to a strong wooden frame; *m* is a magnetized cylindrical bar of steel, one foot long and half an inch in diameter, supported slightly above the centre of gravity (like an ordinary balance-beam) by knife-edges resting on hard concave surfaces of steel. A scale *s* is attached to one end of the magnet for the purpose of receiving the weights by which the electrical currents are measured. Lastly, *rr* is a rest which the magnet just touches when at zero.



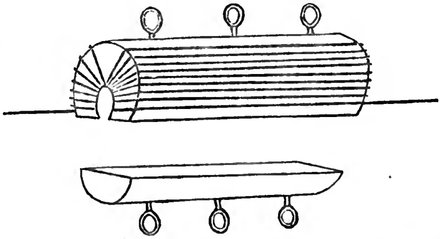
In using this instrument, it is merely necessary to adjust the magnet to zero, either by

* Annals of Electricity, vol. iii. p. 413, 414.

means of screws, weights, or by the attraction or repulsion of a steel magnet kept for the purpose. Then on making the necessary battery communications at *cc*, the scale *s* will rise with a force estimated by the weight, in grains, which is required to bring the magnet again to zero. In my particular instrument, I have found that one degree of current is indicated by 0·69 of a grain.

The value of this new galvanometer (the sensibility of which may be increased at pleasure by multiplying the number of coils), besides its usefulness in measuring copious currents, consists chiefly in its perfect independence of the terrestrial magnetism, as well as of any magnetic influence of surrounding bodies. In every possible situation, provided that the intensity of the balance bar is constant, and that no interference is induced *after* the adjustment to zero, the transmitted current will be exactly proportional to the weight lifted by the scale; and I should have as much confidence in working with it on an iron steam-boat as if every particle of iron were entirely removed from it.

I now proceed to describe my electro-magnets, which I had occasion to construct of very different sizes, in order to develope any curious circumstance which might present itself. A piece of cylindrical wrought iron, 8 inches long, had a hole 1 inch in diameter bored through the entire length of its axis; one side of it was then planed away, until the hole was laid open through its entire length. Another piece of iron, also 8 inches long, was then planed; and having been secured with its face in contact with the other planed surface, the whole was turned into a cylinder 8 inches long, $3\frac{3}{4}$ inches in exterior diameter, and $1\frac{1}{4}$ inch in the diameter of the bore. The larger piece (which was intended for the electro-magnet) was then wound with four copper wires, each of which was 23 feet long and $\frac{1}{11}$th of an inch in diameter, and covered with silk. This electro-magnet*, which I shall designate No. 1, is along with its armature represented by the accompanying figures.



I constructed another electro-magnet (No. 2) of a piece of round iron bar, half an inch in diameter and 2·7 inches long. It was bent into an almost semicircular shape, and covered with 7 feet of well-insulated copper wire $\frac{1}{20}$th of an inch thick. The poles were half an inch asunder.

* This electro-magnet is at present on view in the Exhibition of Industry of all Nations.—May 1851, J. P. J.

A third electro-magnet, No. 3, was made of a piece of iron 0·7 of an inch long, 0·37 of an inch broad, and 0·15 of an inch thick, of which the lateral edges were well-rounded. It was bent into the semicircular shape, and covered with 19 inches of insulated copper wire $\frac{1}{40}$th of an inch in diameter.

Anxious to procure a still greater variety, I made what might, from its extreme minuteness, be almost termed an elementary electro-magnet. It was the smallest, I believe, hitherto made; and was constructed of a piece of iron wire $\frac{1}{4}$ of an inch long and $\frac{1}{25}$th of an inch in diameter. It was bent into a semicircle, and wound with three turns of uninsulated copper wire $\frac{1}{40}$th of an inch in diameter.

In the following tables, in which the experiments with the above electro-magnets are recorded, the first column gives the quantity of electrical current in degrees; the second contains the same multiplied by the length of the coils in feet; and the last contains the lifting power in pounds avoirdupois.

Table I.—Electro-magnet No. 1. Weight of its iron 15 lbs. Length of coils 23 feet.

Quantity of current in degrees.	Electro-magnetic force.	Electro-magnetic force corrected.	Weight lifted.
0°·8	18·4	6·5	2·75
1·8	41·4	14·4	10
2·6	59·8	21·0	23
3·8	87·4	31·0	45
8·1	186·0	65·0	238
10·9	250·0	88·0	540
4·3	99·3	99·3	670
5·7	132·5	132·5	890
8·6	198·7	198·7	1060
14·4	331·0	331·0	1400
21·6	497·0	497·0	1800
36·0	828·0	828·0	2030

Subsequently, with a more powerful battery, the weight necessary to remove the keeper was 2090 lbs., which is, I believe, a greater weight than any magnet has hitherto carried, and is certainly vastly superior to the performance of any other of the same weight; but I can show that this power, great as it is, is not *so much* as is due to the form I have employed.

The latter part of the above table was obtained experimentally before the first part, and in the mean time the proper insulation of the coils from the iron was destroyed by accident; and not having had the opportunity of refitting the electro-magnet, I have been obliged to supply the column of corrected electric forces, calculated from the power obtained when the insulation was good.

Table II.—Electro-magnet No. 2. Weight 1057 grs. Length of coils 7 feet.

Quantity of current in degrees.	Electro-magnetic force.	Weight lifted.
$0\overset{\circ}{\cdot}51$	3·57	20
1·53	10·7	38·5
6·1	42·7	49

Table III.—Electro-magnet No. 3. Weight 65·3 grs. Length of coil 1·58 feet.

Quantity of current in degrees.	Electro-magnetic force.	Weight lifted.
$0\overset{\circ}{\cdot}42$	0·66	5·5
1·0	1·58	9
2·0	3·16	11

With great care No. 3 in one instance supported 12 lbs., or 1286 times its own weight.

Electro-magnet No. 4, which weighed only half a grain, carried in one instance a weight of 1417 grains, or 2834 times its own weight*. There was, however, a good deal of difficulty in experimenting with this minute arrangement, and it is on this account that its greatest lifting power was not observed; the relative power obtained was nevertheless far greater than any that I had heard of before, and is in fact more than eleven times that of the celebrated steel magnet which belonged to Sir Isaac Newton.

It is well known that the length of a steel magnet ought to bear a great proportion relatively to its breadth and thickness, and that a contrary shape occasions the confusion of the poles, and a general diminution of virtue; and Dr. Scoresby has found that if a large number of straight steel magnets are bundled together, the power of each is thereby greatly deteriorated. All this is easily understood, and finds its cause in the attempt of each part of the system to induce upon the other part a contrary magnetic polarity to its own. Still there is no reason why the principle of construction should be extended to the electro-magnet, especially as in its case a great and commanding inductive power is brought into play to sustain what the steel magnet has to support by its own unassisted retentive property. All the preceding experiments confirm this principle; and I give the following table in proof of its obvious and necessary consequence,

* I subsequently had the pleasure of presenting Dr. Roget with a still more minute electro-magnet, which had sustained about 3500 times its own weight of iron.—May 1851, J. P. J.

—that *the maximum power of the electro-magnet is directly proportional to its least transverse sectional area.*

Table IV.

		Least sectional area in square inches.	Maximum lifting power.	Maximum power divided by area.
My own electro-magnets.	No. 1.	10	2090	209
My own electro-magnets.	No. 2.	0·196	49	250
My own electro-magnets.	No. 3.	0·0436	12	275
My own electro-magnets.	No. 4.	0·0012	0·202	162
Electro-magnet made by Mr. Nesbit. Length round the curve 3 feet; diameter of iron $2\frac{3}{4}$ inches; sectional area 5·7 inches; do. of armature 4·5 inches; weight of iron about 50 lbs.		4·5	1423	317
Professor Henry's: of iron 2 inches square, the sharp edges being rounded; length round the curve 20 inches; weight 21 lbs.		3·94	750	190
Mr. Sturgeon's: length about 1 foot; diameter half an inch		0·196	50	255

The results of the table are, I think, sufficient to prove the rule, if we allow for various sources of error. No. 1 is unfortunately made of a piece of unsound iron, and was in all probability not fully saturated, otherwise I have no doubt that its power, per square inch section of the ferruginous circuit, would have approached 300 lbs. Again, the specific power of No. 4 is less than the mean, simply on account of the extreme difficulty of making a good experiment with it. With regard to Mr. Nesbit's electro-magnet, the battery used was so powerful (nineteen of Daniell's two-feet cells) and the quantity of conducting wire so very large (fourteen lengths of wire, each 70 feet long and about $\frac{1}{14}$th of an inch thick), that its magnetism must have been brought to the utmost possible pitch of intensity, which therefore exceeded the mean specific power of the table. On the other hand, Professor Henry's, which was excited only by a single pair, could not have been nearly saturated.

The mean of the specific powers of No. 2, No. 3, and Mr. Nesbit's electro-magnet, may, I think, be fairly taken for the expression of the maximum magnetic force of iron under ordinary circumstances, which may therefore be taken as equal to the least sectional area of the magnetic circuit in square inches, multiplied by 280 lbs*.

* * * * * *

With regard to the magnetizing coils, I may observe that each

* According to this, the maximum magnetic attraction of two iron surfaces, each 1 inch square, for one another will be 140 lbs.—May 1851, J. P. J.

particle of space through which a certain quantity of electricity is propagated, appears to operate in moving the magnetism of the bar with a force proportionate to the inverse square of its distance from the iron; and that when the tension or specific magnetism is the same, the thickness of the iron upon which that particle of conducting space acts, has nothing (apart from resistance and other foreign circumstances) to do with the whole effect. Now it may be mathematically demonstrated, that, such being the law, if each particle induce upon a *large surface*, the resulting magnetic force will not vary much with the distance, but be a very constant quantity for any distance which bears a small ratio to the dimensions of that surface. Hence it is that a coil *within* a hollow piece of iron has no power to magnetize it*. And hence also in the case of my large electro-magnet, of which the surfaces are large, every particle of conducting wire would perform its full extent of duty, even if it were not quite close to the iron.

When the interferences arising from tension are reduced to a minimum by completing the magnetic circuit and making use of a very small electrical force, the resistance from *length* becomes a very sensible quantity, varying probably in the direct ratio of that element. Some idea of its character may be formed from the following table, in which I have compared half the maximum powers of each electro-magnet with the electro-magnetic forces which produced them; and by dividing the former by the latter I have obtained the third column, which, under the title of *specific power*, contains the quantity of lifting power (of that degree of tension) due to an unit of electro-magnetic force.

Table V.

	Electro-magnetic force.	Half the maximum lifting power.	Specific power.
No. 1	200	1060	5·3
No. 2	4·5	25	5·5
No. 3	0·66	5·5	9·2

The electro-magnetic force against No. 2 is rather greater than the truth, on account of the greater relative distance of its coils from the iron; allowing for this, we may observe that the variation of the specific powers is due to the *resistance of length*.

It is well known, that, after the current is cut off from an electro-magnet, the armature is retained in its place with very considerable force. I was anxious to try the capability of my electro-magnet No. 1 in this respect, and have arranged the

* Scientific Memoirs, part 5. p. 14.

results of some experiments in the following table, the first column of which contains the forces to which the electro-magnet had been exposed; the second, the lifting powers due to those forces; and the third, the lifting power left after the circuit was broken.

Table VI.

Electro-magnetic force.	Lifting power.	Lifting power retained.
88	540	33
29	40	16
14·5	10	10

There was considerable difficulty in observing the smaller lifting powers. Nevertheless it is certainly the case, that the *power retained after the circuit was broken was very nearly equal to the lifting power,* with small electric forces.

When the whole current is not entirely cut off, but merely reduced in intensity by the interposition of a bad conductor, a surprising quantity of magnetism may be *supported* by a very small electric force. I subjected No. 1 to an electro-magnetic force of 90, a quantity adequate to bring its power up to 560 lbs., and then reduced the current to different degrees of intensity. In the following table, the first column contains the electro-magnetic force to which the force of 90 was reduced; the second expresses the lifting power which is simply due to those quantities; and the third gives the lifting power which the same quantities could support.

Table VII.

Electro-magnetic force.	Lifting power.	Supported lifting power.
31	45	294
21	23	210
14·5	10	112
6·2	2·6	63
4·1	1·1	56

A battery of the size of a common thimble was quite sufficient to produce an electro-magnetic force of 31 in the coil of No. 1, and consequently to sustain a magnetic attraction of about 300 lbs.; and it is easy to perceive, that by increasing the size of the electro-magnet and the quantity of its coils, the same minute source could support a magnetic virtue of indefinite amount.

* * * * * *

Broom Hill, near Manchester,
August 21, 1840.

*On Electro-Magnetic Forces**.

* * * * * *

Suspecting that the extreme power of the large electro-magnet, No. 1, had not been attained in my last experiments, on account of the imperfect insulation of its coils, I determined to try it again, using every precaution which was calculated to develope its magnetism to the full extent.

The old wire was removed, and a bundle consisting of twenty-one copper wires, each 23 feet long and $\frac{1}{23}$th of an inch in diameter, was formed by binding the wires together with cotton tape. This bundle of wires was coiled on the iron of the electro-magnet, which had been previously insulated by a fold of calico.

Sixteen cast-iron cells, of the same size as those described in my last, were arranged in a series of four elements, and connected by good conductors with the electro-magnet. The attractive force developed was then found to be so great, that a weight of 2775 lbs. had to be applied to the armature in order to separate it from the electro-magnet.

Now by the formula $x = 280a$ given in my last paper, we obtain 2800 lbs. for the greatest lifting power of this electro-magnet, or only 25 lbs. more than that actually found, which coincidence cannot but be considered as a striking proof of the accuracy of the general principles I have before advanced. That the *saturation* of the iron must have been very nearly effected in the above experiment appears from the fact, that the quantity of electrical current employed was four times as great as that which was competent to make the same electro-magnet sustain 2128 lbs.

Although the battery used above for obtaining *maximum* effects was very powerful, each cell exposing an active surface of two square feet of cast iron, I have found that a very good lifting power may be obtained with this electro-magnet by means of a very small voltaic arrangement. For instance, it can lift 8 cwt. when the current generated by a single pair of 4-inch plates of iron and amalgamated zinc is passed through its coils; and with a single pair of platinized silver and amalgamated zinc plates exposing only two inches of surface, the attraction is such as to require the utmost force I can exert, even to *slide* the armature upon its poles.

Broom Hill, near Manchester,
November 23, 1840.

* Annals of Electricity, vol. v. p. 470.

LXIX. *On the Products of the Destructive Distillation of Animal Substances.*—Part II. *By* Thomas Anderson, *M.D.**

I PROPOSE in the following pages to communicate to the Society the progress of my investigation of the products of the destructive distillation of animal substances, the first part of which was published in the 16th volume of the Transactions. Since that period, partly owing to my numerous avocations, and partly to the inherent difficulties of the subject, less progress has been made than I had hoped or expected, but still I have accumulated some facts of considerable interest, which I think deserving of the attention of the Society.

It may be remembered that, in the paper just referred to, I announced the discovery, among those products, of picoline, which I formerly obtained from coal-tar, and of a new base, to which I gave the name of Petinine; and I entered pretty fully into the method adopted for the preparation of these substances, and of certain other bases, the existence of which I merely indicated, without at the time attempting to characterize them. On proceeding to the more minute investigation of these bases, I soon found that the quantity of material at my disposal was much too small to admit of satisfactory or complete results, although I had employed for their preparation above 300 pounds of bone-oil. I found it necessary, therefore, to begin *ab initio* with the preparation of the bases from another equally large quantity of the oil; and after going through the whole of the tedious processes described in my previous paper, with the expenditure of the labour of some months, I found my object again defeated by deficiency of material. After various experiments, which, though they led to no definite or conclusive results, served to familiarize me with the nature and relations of the products obtained, I made up my mind once more to begin again; and being resolved on this occasion not to be foiled in the same way as before, I used for my new preparation no less than 250 gallons of crude bone-oil, the weight of which was somewhat above a ton. The result of this process, though involving an immense amount of labour, has been satisfactory, not only in supplying me with a large amount of material, but has also enabled me to obtain many substances, some of them possessed of very remarkable properties, which had escaped my observation when operating on a smaller scale.

The employment of so large a quantity of material has, as might be expected, led to some modification of the process described in the first part of this paper, which, though convenient enough on

* From the Transactions of the Royal Society of Edinburgh, vol. xx. part 2; read 21st April 1851. Part I. appeared in the September Number of this Journal for 1848.

the small scale, was too tedious for the large quantities on which I now operated. The preliminary process of rectifying the oil, which was quite beyond the resources of a laboratory, was effected at a manufactory. The whole oil was introduced at once into a cast-iron retort, furnished with a good condenser, kept cool by an abundant current of ice-cold water. A very gentle heat was applied, and the first twenty gallons which passed over were collected apart; they consisted of about equal bulks of a highly volatile oil, and of water charged with sulphide of ammonium, hydrocyanate and carbonate of ammonia, and a small quantity of very volatile bases. The oil which distilled over after this fraction had been separated was collected in a succession of casks, which were numbered as they were filled.

In the after treatment of the oil, a process was employed similar to that which I had formerly made use of, with this exception, that the watery fluid, which had formerly been rejected, was employed for obtaining any bases which might have been dissolved in it along with the ammonia. For this purpose it was separated from the oil, and dilute sulphuric acid gradually added, when carbonic, hydrocyanic, and hydrosulphuric acids escaped with violent effervescence. When acid enough had been added to communicate a powerfully acid reaction to the fluid, it was put into a large copper boiler and boiled for some time, water being added at intervals, so as to keep up the bulk. After the ebullition had been sufficiently prolonged, the fluid was allowed to cool, and slaked lime added in excess. A copper head was then fitted to the boiler and luted down with clay, a condenser attached, and heat applied. The distillate was collected in a large glass receiver, which, in order to prevent the escape of ammonia and any very volatile products which might be carried along with it, was connected by a doubly-bent tube with a second receiver containing water, through which the gaseous products were allowed to stream. The fluid which distilled was coloured blue by the solution of small quantities of copper from the condenser; it had a powerfully ammoniacal and putrid odour, and when treated with sticks of caustic potass, in the manner described in the first part of this paper, ammonia was rapidly evolved with effervescence, and a small quantity of very volatile and pungent bases collected on the surface of the potash. These bases were separated from the potash fluid, which was preserved along with the ammoniacal solution obtained by the absorption of the gaseous products in the second receiver.

The treatment of the oil was conducted in a manner very similar to that already described, and as I desired to have only the more volatile products, I employed the first half of the oil only. It was agitated with dilute sulphuric acid in casks about half-full, and after two or three days, during which the agitation was frequently repeated, more water was added, and the solution of the bases separated from the oil. To this fluid acid was added, so as to have a distinct excess; and it was then boiled for the separation of Runge's pyrrol, to which reference has been made in the first part of this paper. As, however, I observed that a very powerful and pungent odour was evolved when the fluid began to boil, and the vapours presented the characteristic reaction of pyrrol in a very high degree, the head of the boiler was luted on, and the condenser attached, for the purpose of endeavouring to obtain that substance, which in my previous experiments I had not done. The fluid which distilled over carried with it a small quantity of oil, which, at the moment of distillation, was perfectly colourless, but soon acquired a reddish shade, and in the course of a few days became almost black. The greater part of this oil passed over with the first portion of water; but the last traces adhered with great obstinacy to the acid fluid, and could only be separated by very protracted distillation. The substance thus obtained proved to be a mixture of an oil insoluble in acids, and which appeared to be merely a small quantity of the crude oil, mechanically mixed with the fluid, and of a series of bases of very remarkable properties, and obviously related to one another, to which I shall afterwards refer under the provisional name of *pyrrol bases.*

When these substances had entirely distilled, the fluid was allowed to cool, excess of slaked lime added, and the distillation again commenced, in order to obtain the bases which had been retained by the sulphuric acid. The separation of these was conducted in a manner in all respects similar to that employed in the former preparations, solid caustic potash being added in sufficient quantity to cause the separation of the bases held in solution in the water. The potash fluid, however, retained a certain proportion of ammonia, another gaseous base, and of the most volatile bases, which could be separated only by a very large excess of potash. The fluid was therefore distilled in glass vessels, and the product collected in a succession of three receivers, the first of which was kept cold by water, the second by a freezing mixture, and the third contained hydrochloric acid, for the purpose of condensing the gaseous products. The first receiver now contained the bases dissolved in a small quantity of water, from which they were readily separated by potash; the second receiver contained only a drop or two of liquid; but in the third the hydrochloric acid was rapidly saturated, and required repeated renewal during the progress of the distillation.

The hydrochloric solution thus obtained contained a very large quantity of chloride of ammonium, along with a small proportion of another base, in order to obtain which the fluid was slowly evaporated, allowed to cool at intervals, and the sal-ammoniac which deposited was separated by straining through cloth and expression. After the separation of several crops of crystals, a dark-brown mother-liquor was left, which refused to crystallize by evaporation on the water-bath, but on cooling solidified into a mass of long foliated crystals, which soon deliquesced in moist air. These crystals still contained traces of sal-ammoniac, for the separation of which they were evaporated to complete dryness on the water-bath, and dissolved in the smallest possible quantity of absolute alcohol, with the aid of heat. The filtered fluid, on cooling, deposited a few tabular crystals mixed with a little sal-ammoniac, which was got rid of by a second filtration; and the filtrate, when treated with animal charcoal and further concentrated, solidified, on cooling, into a mass of large foliated crystals.

These crystals are long, transparent, and colourless plates, entirely without odour, and with a pungent and bitter taste. In moist air they deliquesce rapidly. Solid potash added to their concentrated solution causes the immediate escape of a gaseous base resembling ammonia, but distinguished by its peculiar putrid odour. This gas dissolves readily in water, and gives a powerfully alkaline solution. It gives with corrosive sublimate a fine white precipitate, soluble in hot water or spirit, and deposited on cooling in fine silvery plates; and its hydrochlorate gives, with bichloride of platinum, a soluble salt, depositing from its hot saturated solutions in beautiful golden-yellow scales. I selected this salt as a means of determining the constitution of its base.

I. 6·885 grs. of the platinochloride, dried at 212°, gave 1·243 carbonic acid and 1·648 water.

II. 6·189 grs. of the salt gave 2·565 platinum.

III. 11·531 grs. of another preparation gave 4·764 platinum.

	Experiment.		Calculation.		
Carbon .	4·92	...	5·06	C^2	12
Hydrogen .	2·67	...	2·52	H^6	6
Nitrogen .	...	...	5·92	N	14
Chlorine .	...	...	44·89	Cl^3	106·5
Platinum .	41·31	41·44	41·61	Pt	98·7
			100·00		237·2

These analyses, then, correspond exactly with the formula $C^2H^5N, HCl, PtCl^2$; and the base is consequently methylamine, with which it and its salts agree in all respects.

The oily bases which had been separated from their solution in water by means of potash, were dried by the addition of successive portions of that substance, as long as it continued to become moist. The dry oil, which was very dark-coloured, was then introduced into a large retort, furnished with a thermometer and a tubulated receiver kept cold by ice, and connected first with a U-tube immersed in a freezing mixture, and then with a large vessel of water, in order to collect the gaseous bases which began to escape with effervescence almost as soon as heat had been applied. At a temperature under 150° Fahr. drops began to condense in the neck of the retort, and the fluid entered into rapid ebullition. At 212° the receiver was changed, and the oil distilling above that temperature was collected in receivers, which were changed at every ten degrees.

The quantity of bases which distilled under 212° was much less than I had anticipated, and proportionably much smaller than that obtained when operating on a much smaller scale before; and I consequently found myself compelled to proceed very carefully, so as to avoid loss in the purification. By distilling the product which boiled under 212°, I collected fractions nearly equal in bulk at every five degrees, all very similar in their general properties. They were all limpid and colourless fluids, with high refractive power, and pungent odour, remarkably similar to that of ammonia in the lower fractions. They fumed strongly when a rod moistened with hydrochloric acid was brought near them, and presented all the properties of powerful bases. Exposed in the anhydrous state to a mixture of snow and salt, they remain perfectly fluid, but if a small quantity of water be added, beautiful white crystals of a hydrate are deposited. I attempted, by several successive distillations, to obtain fixed boiling-points; but the quantity I had to work with was too small for an operation involving so much loss of material, and I therefore converted portions of the fractions which I had reason to suspect corresponded with particular bases into platinum salts. I selected, in the first place, the lowest fraction of all, that, namely, which boiled under 150°. It was dissolved in water, saturated with hydrochloric acid, and evaporated to dryness on the water-bath. The highly crystalline residue obtained was dissolved in water, and mixed with a solution of bichloride of platinum, when a yellow crystalline salt was slowly deposited, which dissolved readily in water even in the cold, and still more abundantly on boiling; and the solution on cooling deposited fine golden scales, scarcely to be distinguished in their appearance from those of methylamine or of petinine. These crystals were separated, and as the salt was highly soluble, and much remained in the mother-liquor, a mixture of alcohol and æther was added, when the fluid rapidly filled with small shining scales. The analysis of this salt, dried at 212°, gave the following results:—

6·970 grs. of plantinochloride gave 3·392 carbonic acid and 2·434 water.

6·475 grains of the salt gave 2·422 grs. platinum.
8·257 ... ... 3·047 ...

	Experiment.	Calculation.		
Carbon . .	13·27	13·57	C^6	36
Hydrogen . .	3·88	3·77	H^{10}	10
Nitrogen . .	...	5·27	N	14
Chlorine . .	...	40·18	Cl^3	106·5
Platinum . .	37·56	37·21	Pt	98·7
		100·00		265·2

From these results we arrive at the formula $C^6 H^9 N, HCl, Pt Cl^2$, which is that of the platinum salt of a base $C^6 H^9 N$. The base is therefore the substance I have before described* as a product of the action of alkalies upon codeine, under the name of Metacetamine, but which I now prefer calling Propylamine, in accordance with the name now usually applied to the acid with which it corresponds. Unfortunately the quantity of propylamine obtained was too small to admit of my examining either its compounds or itself with accuracy. It is, however, a perfectly limpid and colourless fluid, with a strong pungent odour resembling that of petinine, but more ammoniacal. It gives an abundant white cloud when a rod dipped in hydrochloric acid is brought near it, and unites with the concentrated acids, with the evolution of much heat. Its hydrochlorate crystallizes in large plates closely similar to those of methylamine and petinine.

The discovery of methylamine and propylamine among these products naturally directed my attention to the probable presence of ethylamine, the intermediate term of the same series; but as I had not employed any very particular precautions in condensing the more volatile products during the successive rectifications to which I had subjected the crude oil, almost the whole of it appears to have escaped. By collecting, however, the first few drops passing over in the rectification of the portion boiling under 150° in hydrochloric acid, and forming a platinum salt, I obtained the following result:—

6·930 grs. of platinochloride gave 2·649 grs. platinum.

This corresponds to 38·22 per cent. Now the per-centage of platinum in the ethylamine salt is 39·60, and the result

* Edinburgh Philosophical Transactions, vol. xx. p. 82.

obtained, which is much too high for the propylamine salt, shows that I must have had a mixture of the two, which might have been separated had I possessed a sufficient quantity of the salt. It will readily be understood that a result of this kind could not in general be produced as evidence of the existence of ethylamine, but under the particular circumstances of the case, the next term of the same series on either side of it having been detected, it may be considered as sufficiently conclusive of its presence.

The occurrence of these bases enables us to establish, on satisfactory grounds, the constitution of petinine. In the first part of this paper, an analysis of that base is given, which agrees in the most perfect manner with the formula $C^8 H^{10} N$, which was also confirmed by that of its platinum salt. It cannot, however, for a moment be doubted that it is homologous with the bases with which I have now shown it to be associated, that its true formula is $C^8 H^{11} N$, and that it is really butylamine, the corresponding base of the butyric group. The analysis of the platinum salt given in my former paper agrees equally well with this formula, and though that of the base differs from it to some extent, much less reliance is to be placed upon it, as it is scarcely possible, when operating upon so small a scale as that upon which I was compelled to work, to subject the bases to a sufficient number of distillations to effect their complete separation.

I have thus then established the existence, among the products of destructive distillation, of ammonia, and the first four members of the series of bases homologous with it. I have every reason, however, to believe that the series does not end with petinine, for the fraction boiling about 200° yields a platinum salt in fine scales, and having all the characters of the salts of the same series of bases, and in all probability contains valeramine. I am not without hope also of obtaining caprylamine; but this I expect will be the last of the series, for when we reach the temperature of about 240°, the character of the bases changes, and we enter upon an entirely different series.

In the separation of the bases boiling above 240°, I have encountered very great difficulties. After the trial of many different processes, such as converting them into salts, exposing them to cold, partial saturation, and every other plan which appeared likely to answer, I have been compelled to return to fractionated distillation, as the method most likely to answer the end I had in view. But even with this process the difficulties are great, and I have been by no means so successful in obtaining fixed boiling-points as I was when operating on a smaller scale in my former preparations. I subjected the whole of the oils boiling above 212° to a systematic course of fractionation, each fraction being distilled alone, and the product collected in a fresh series of bottles, and the receivers changed at every ten degrees. In the earlier rectifications each fraction spread itself over a very large number of degrees, and showed little tendency towards concentration to fixed points. The distillations were repeated no less than fourteen times, but even after all this the indications of boiling-points were extremely indistinct. Sometimes in one distillation certain fractions appeared larger than others, but their pre-eminence disappeared again in succeeding rectifications. Still a certain improvement was manifest, some of the fractions being confined more nearly to the range of degrees within which they had boiled at the previous rectification. It was obvious, from the whole phænomena of the distillation, that the separation of the different bases was going on, although with extreme slowness; and at this point I endeavoured, by the examination of the platinum salts obtained at different temperatures, to determine the constitution of the bases which these fractions contained; and as I knew from previous experiment that the quantity boiling between 270° and 280° consisted of picoline, I had from this fact indications of the temperatures at which bases were likely to be found, and I have thus been enabled to determine the existence of two substances belonging to the same homologous series with that substance.

Pyridine.

The first of these bases, to which I give the name of pyridine, occurs in the fraction boiling about 240°. This fraction has an odour precisely similar to that of picoline, but more powerful and pungent. It is perfectly transparent and colourless, and does not become coloured by exposure to the air. It dissolves in water in all proportions, and is also readily soluble both in the fixed and volatile oils. It dissolves in the concentrated acids, with the evolution of much heat, and the formation of highly soluble salts. When bichloride of platinum is added to a solution of its hydrochlorate, a double salt is slowly deposited in flattened prisms, which are tolerably soluble in boiling water, less so in alcohol, and entirely insoluble in ether. When these crystals are boiled for a considerable time in water, they appear to undergo decomposition, with the formation of a platinum salt, crystallizing in golden scales. Two analyses of this salt were made, one upon the substance simply precipitated from the hydrochlorate; the other was the same salt redissolved in hot water, so as to leave a considerable proportion undissolved. In the last analysis the salt was mixed with the chromate of lead when still rather hot, and it immediately evolved a strong smell of the base, which accounts for the loss of carbon obtained in the experiment.

I. 8·234 grs. of the platinochloride gave 6·486 carbonic acid and 1·705 water.

II. 5·396 grs. of the platinochloride gave 4·015 carbonic acid and 1·091 water.

8·138 grs. platinochloride gave 2·792 grs. platinum.
4·956 ... ... 1·703 ...

	Experiment.		Calculation.		
Carbon . .	21·48	20·29	21·03	C^{10}	60
Hydrogen .	2·30	2·24	2·10	H^{6}	6
Nitrogen . .	...	...	4·93	N	14
Chlorine . .	...	...	37·34	Cl^{3}	106·5
Platinum .	34·30	34·56	34·60	Pt	98·7
			100·00		285·2

The formula $C^{10} H^{5} N, HCl, Pt Cl^{2}$ agrees very closely with these analyses; and the salt is consequently that of a base having the formula $C^{10} H^{5} N$, which forms a term of the picoline series. I have not as yet directed further attention to this base, as the phænomena observed in the examination of the next base served to show that, notwithstanding the correspondence of the salt with theory, much difficulty would be experienced in obtaining the base itself in a state of purity.

Lutidine.

In the fraction boiling about 310°, a base occurs which possesses precisely the constitution of toluidine, and to which I give the name of Lutidine. When in the distillation of the mixed bases the temperature rises to about 305° to 310°, more distinct indications of a fixed boiling-point are obtained than at any other temperature, and the base which distils presents sufficiently distinct characters from those obtained at lower points. The product is now much less soluble in water; when dropped into a small quantity of that fluid it floats on the surface, and is only slowly dissolved on agitation. It possesses the remarkable property of immediately separating from its solution on the application of a gentle heat, and collecting on the surface in the form of an oily layer which dissolves again as the temperature falls. Its smell is less pungent and more aromatic than that of picoline, and it is also more oily in its characters. It unites with the acids and forms salts, all of which are highly soluble.

Analyses were made of the different portions of oil boiling about the temperature of 310°, with the following results:—

I. 3·840 grs. of the base, boiling between 310° and 315°, gave 11·007 carbonic acid and 3·060 water.

II. 4·012 grs. of the base, boiling between 315° and 320°, gave 11·516 carbonic acid and 3·160 water.

III. 4·319 grs. of the base, boiling between 316° and 320°, gave 12·430 carbonic acid and 3·576 water.

IV. 4·430 grs. of the base, boiling between 320° and 324°, gave 12·812 carbonic acid and 3·405 water.

	I.	II.	III.	IV.
Carbon . .	78·17	78·28	78·48	78·87
Hydrogen .	8·85	8·75	9·10	8·54
Nitrogen . .	12·98	12·97	12·42	12·59
	100·00	100·00	100·00	100·00

These results agree very closely with the formula $C^{14} H^9 N$, as is shown by the following comparison of the mean experimental and calculated numbers.

	Mean.	Calculation.		
Carbon . . .	78·45	78·50	C^{14}	84
Hydrogen . .	8·81	8·41	H^9	9
Nitrogen . .	12·54	13·09	N	14
	100·00	100·00		107

Notwithstanding the close correspondence of these results, however, further experiment showed that some of the fractions, especially those of lower boiling-points, contained appreciable quantities of picoline, the presence of which was established by the analysis of the platinum salts. When, for instance, a portion of any of these fractions was saturated with dilute hydrochloric acid and bichloride of platinum added, fine prismatic crystals were slowly deposited, which, as the result of numerous experiments, were found to contain about 32·8 per cent. of platinum, which is exactly the quantity present in the picoline salt, of which the theoretical per-centage is 32·92. On evaporation of the mother-liquor, crystals were deposited which gave quantities of platinum varying from 32·5 to 32·0 per cent., and which were obviously mixtures of the picoline and lutidine salts. When the last mother-liquor, however, was evaporated to a small bulk, and alcohol and æther added, another salt altogether distinct from that of picoline, and crystallizing in flattened tables, was deposited, which analysis proved to have the constitution of the lutidine salt.

This platinum salt crystallizes from its solutions in square tables, sometimes very distinct, at other times confused and irregular. It dissolves very readily in cold water, and still more abundantly in boiling, and appears also to be very easily soluble in excess of hydrochloric acid. Numerous analyses of this salt were made, of which the following are the results :—

No. 1. This was the analysis of the salt prepared from the oil distilling between 315° and 325° in the seventh rectification.

6·377 grs. of platinochloride gave 6·187 of carbonic acid, and 1·915 of water.

6·810 grs. platinochloride gave 2·146 grs. platinum.
64·76 ... ... 2·051 ...

No. 2. Portion of the oil distilling between 295° and 300° in the fourteenth rectification; the platinum salt of picoline was separated by crystallization, and the salt analysed precipitated by alcohol and æther.

7·906 grs. gave 2·491 grs. platinum.

7·835 grs. of the salt recrystallized gave 2·470 grs. platinum.

No. 3. Another preparation from the same portion of oil.

7·330 grs. of platinochloride gave 7·070 carbonic acid and 2·090 water.

6·830 grs. gave 2·155 grs. platinum.

No. 4. Portion of the oil boiling between 300° and 305° in the thirteenth rectification.

7·401 grs. gave 2·328 grs. platinum.

No. 5. Portion boiling between 325° and 335° in the seventh rectification.

7·194 grs. gave 2·256 grs. platinum.

	I.		II.		III.	IV.	V.
Carbon .	26·41	...	...	...	26·30		
Hydrogen .	3·33	...	...	...	3·16		
Platinum .	31·51	31·67	31·50	31·52	31·55	31·45	31·35

These results correspond very closely with the formula $C^{14} H^9 N, HCl, Pt Cl^2$, of which the following is the calculated result compared with the mean of experiment.

	Mean.	Calculation.		
Carbon . .	26·35	26·81	C^{14}	84
Hydrogen .	3·23	3·19	H^{10}	10
Nitrogen .	...	4·49	N	14
Chlorine .	...	34·00	Cl^3	106·5
Platinum .	31·50	31·51	Pt	98·7
		100·00		313·2

It is clear, from these analyses, that the salt obtained is that of the base of which the analysis is given above; but it is equally evident, from the presence of small quantities of picoline, that the base itself was not obtained in a state of absolute purity, notwithstanding the close approximation of the experimental results with those required by theory. I have been struck throughout the whole course of the investigation by the fact, that when the fraction corresponding to the boiling-point of any particular base has been analysed, results very nearly correct were obtained, even when the substance was very far from being pure. I found, for instance, in the earlier part of the investigation, that the fraction boiling between 270° and 280°, after one or two rectifications, gives precisely the results obtained from pure picoline, although on further rectification the fluid will begin to boil about 250°, and a small portion will still remain in the retort when the thermometer has risen to 300°. It is, however, readily intelligible, that this should be the case when we have to deal with a series of homologous bases, in which the per-centage of carbon goes on increasing as the boiling-point rises, so that, as in this particular case, we have the excess of carbon in the less volatile base exactly counterbalancing the deficiency in the more volatile. Thus lutidine, containing 78·5 per cent. of carbon, and pyridine only 75·9, and each successive rectification removing equal quantities of the more and less volatile substances of which the boiling-points are equidistant from that of the intermediate member of the series, must always leave a substance in which the quantities of the two impurities must be exactly sufficient to counterbalance the error which each will occasion.

Hydrargo-chloride of Lutidine.—I directed my attention to this compound, which is sparingly soluble and crystallizable, in hopes that it might be adapted to the purification of the base itself. I soon, however, abandoned it, as it turned out that it was not possible, in repeating its preparation, to obtain invariably the same substance, each base appearing, like aniline, to form different compounds with corrosive sublimate. When a solution of corrosive sublimate in alcohol is added to an alcoholic solution of lutidine, a curdy white precipitate falls immediately, unless the solutions be highly dilute, in which case it is slowly deposited in groups of radiated crystals. This salt dissolves in boiling water, with partial decomposition; it is still more soluble in spirit, and is deposited unchanged as the solution cools. The following analysis corresponds exactly with the formula $2HgCl + C^{14}H^{9}N$.

7·850 grs. dried *in vacuo* gave 6·373 of carbonic acid, and 1·905 water.

3·112 grs. gave 2·32 grs. of chloride of silver.
7·684 ... gave 4·090 grs. mercury.

	Experiment.	Calculation.		
Carbon	22·14	22·05	C^{14}	84
Hydrogen	2·69	2·36	H^9	9
Nitrogen	...	3·69	N	14
Chlorine	18·43	18·64	Cl^2	71
Mercury	53·22	53·26	Hg^2	202
		100·00		380

On another occasion results were obtained more nearly corresponding with the formula $3HgCl + C^{14}H^9N$; and intermediate results were also obtained, but as the existence of these different compounds appeared to me to be fatal to their employment as a means of purifying the base, I did not attempt to pursue the subject further. The separation of lutidine from the other bases was also attempted by forming other salts, but none were found to answer, all being highly soluble except the carbazotate, which crystallizes in beautiful, long, yellow needles, a property which, however, is unfortunately possessed by the carbazotates of all the other bases.

From all these experiments, it appears that I have been able to substantiate the existence of two bases, pyridine and lutidine, although it has been as yet impossible to obtain the bases themselves in a state of satisfactory purity. I am inclined, however, to think that the platinum salts, from their greater stability, and the ease and regularity with which they crystallize, will afford means of purification, but I have been hitherto deterred from trying this method on the large scale by the enormous quantity of platinum which would be requisite for the purpose.

It appears, then, that Dippel's oil contains two series of bases, one that is homologous with ammonia, the other a series peculiar to that oil, homologous with one another, and remarkable for their isomerism with the series of which aniline is the type. Thus we have—

Pyridine	$C^{10}H^5N$	
Picoline	$C^{12}H^7N$	Aniline.
Lutidine	$C^{14}H^9N$	Toluidine.

And it is probable that the series existing in Dippel's oil does not cease here, as I have found that the bases, with higher boiling-points, give a steadily decreasing per-centage of platinum. It is impossible, in the present state of the investigation, to give any opinion as to the intimate constitution and relations of these two groups of what I may call isohomologous bases. The most obvious explanation, however, would be to suppose the new bases to be imidogen or nitrile bases, which would enable us to understand why they differ from the aniline series, which we know to be amidogen bases. If, however, they belong to either of these classes, they must differ remarkably from any of those hitherto examined, all already formed being extremely unstable, and decomposed even by very feeble affinities, while picoline and its congeners are extremely stable, and resist even the action of nitric acid. Into these points, however, I shall not now enter, but reserve their discussion for a future part of this paper.

Pyrrol Bases.

I have already referred, at the commencement of this paper, to another series of bases, to which I have given the provisional name of pyrrol bases, and which distil away from the acid fluid by which the others are retained. They are obtained in the form of an oil, which is transparent and colourless at the moment of distillation, but rapidly acquires first a rose, then a reddish-brown, and finally an almost black colour, and the mixture gives, with hydrochloric acid and a piece of fir-wood, the purple-red colour which Runge describes as characteristic of pyrrol. In fact, I imagined that I had at length obtained this substance, which had escaped me in my previous experiments, but I soon found that the product was really a mixture of several different bases. When distilled with the thermometer it began to boil at about 212°, and the temperature gradually rose to above 370°, and during the whole of the distillation pretty large fractions were obtained at every ten degrees, but those between 280° and 310° were decidedly larger than the others. These oils were all bases, with a peculiar and disgusting odour, quite different from, and much more disagreeable than, that of the picoline series of bases. They all acquire colour on standing, although more slowly than the crude oil. These substances dissolve easily in a small quantity of hydrochloric acid, and give, with bichloride of platinum, a precipitate which is at first yellow, but is rapidly converted into a black substance. When dissolved in an excess of acid, and heated along with it, they present a very remarkable character; the solution at a certain temperature becomes filled with red flocks, so abundant and bulky, that, if not too dilute, the fluid becomes perfectly solid, and the vessel can be inverted without anything escaping. The same change takes place, though more slowly, in the cold, and the substance deposited is then of a pale orange-colour, but becomes darker by boiling or exposure to the air. When this substance is collected on a filter, washed, and dried, it forms a reddish-brown and very light and porous mass. It is insoluble in water, acids, and alkalies, but soluble in alcohol, and the solution on evaporation leaves a dark resinous mass. When subjected to dry distillation, it leaves a bulky charcoal, while an exceedingly disgusting oil distils.

The acid fluid which has been separated from this substance by filtration, when supersaturated by an alkali, evolves the odour of the bases of the *picoline* series. These pyrrol bases I conceive, therefore, to be substances formed by the coupling of the picoline series with some substance which yields the red matter to which I have alluded. I have not as yet, however, pursued the investigation of these bases, but shall communicate the result of their examination in a future paper.

The Non-basic Constituents of Bone-Oil.

I have as yet directed very little attention to this branch of the subject. I have found, however, that when the most volatile part of the oil, after separation of the bases, is repeatedly rectified, it improves in odour, and at length there is obtained a substance which, when acted upon by nitric acid, and then by sulphide of ammonium, gives the reaction of aniline,—indicative of the presence of benzine in the oil. It is probable, therefore, that this series of homologous carbohydrogens forms a part of the oil, but not the whole of it, for I have found that when the oil is boiled for some time with potash, ammonia is evolved, and on supersaturating the potash solution with sulphuric acid, the odour of butyric acid, or at all events of one of the fatty acids, becomes apparent; from which phænomena I draw the conclusion that it also contains the nitriles of these acids.

LXX. *Postscript to* Mr. P. J. Martin's *Paper On the Anticlinal Line of the London and Hampshire Basins.*

SINCE the publication of the greater part of this memoir, I have read Mr. Prestwich's exposition of the range of the arenaceous and other water-bearing strata round London; and of their probable capacity for the reception and transmission of a supply of water to the metropolis, and especially of their aptitude for giving that supply by means of Artesian wells*.

A small part only of the area comprised in Mr. Prestwich's review belongs to that which I have made the subject of my particular research; although every part of it, of course, is comprehended in the range of the surface-changes brought about by the convulsion which produced the great synclinal called the London basin.

After the chalk, the most important member in this order of

* A Geological Inquiry, &c. London, 1851.

strata as a water-bearing one, as Mr. Prestwich has observed, is the greensand. The upper greensand of the Vale of Pewsey and the subordinate valleys of that line of elevation, is satisfactorily disposed of by Mr. Prestwich. Its rainfall, like that of the chalk of that line of country, is stopped by the gault, and much of it carried off southward by the Wiltshire Avon. But the inflection which brings down the Kennet eastward, and the synclinal which throws out the waters of the Basingstoke Canal, would assist in filling the beds under London, and feeding any Artesian wells carried deep enough there.

Of the greater and more important exposure of the *Lower Greensand* in the great *plateaux* of Wolmar Forest, Hind Head, the Hambledon, Hasscomb and Ewhurst Hills, there is more to be said.

A cursory inspection only of the map will show that the surface-drainage of all this country, including also a part of Leith Hill, is taken off by the Wey. This river draws some water out of the Malm country near Alton, and in its course by Farnham takes in the Bourne and other springs thrown out by the sharp flexure of the Hogsback. It then receives the surplus of the Frensham Ponds, which are maintained on the middle argillaceous beds of the lower greensand of Dr. Fitton, of which there is a considerable exposure from that line of country by Pepper Harrow to Godalming. It then takes in the stream that works the paper-mills at Haslemere, and passing by Elsted drains Hind Head, takes in other subordinate streams from the Godalming and Hasscomb countries, and finally receives the large supply out of the Albury and Shiere Valley at Shalford*. All these waters are thrown out by the Weald clay; and looking at the collective stream as it is constantly flowing under Guildford Bridge, one cannot but suppose that it is *sufficient to account for all the rain* that falls on the above-mentioned superficies, not lost by evaporation.

The sharp flexure at the border of the chalk escarpment from Farnham eastward, and the rise of the Weald clay in all the anticlinal of which the Peasemarsh exposure is a part, extending eastward toward Albury and then resumed in the flexure at Bury Hill, must preclude the thought of much infiltration toward and under the chalk of the London basin. I therefore incline to the opinion, that if the rain which falls on the greensand country north of a line drawn from Leith Hill by Hambledon to Haslemere, and from thence to the gault under the Alton Hills, is to benefit London, *it must be collected from the sources of the Wey, and led there by artificial means.*

Passing eastward toward the next largest greensand exposures,

* None comes down from the Weald except in flood-times.

the Sevenoaks and Maidstone countries, we find that the flexures which rule the line of the escarpment of the North Downs run longitudinally south of the greensand escarpment; so that the infiltration of the Reigate, Nutfield and Westerham Hills, is turned toward London. But further east in the Sevenoaks district, the flexure at Montreal, first noticed by Dr. Fitton and afterwards spoken of by Mr. Hopkins as the "Sevenoaks anticlinal," and then another in the Maidstone district, which, as Dr. Fitton informs me, brings the Weald clay up at Tenterden Heath, must produce a strong diversion, and throw much of the rainfall of these districts severally into the Medway and the Darent.

But if I may venture an opinion on such a subject, neither these nor any of the minor flexures and faults that can be traced through the districts, the subject of Mr. Prestwich's research, should militate against a trial of the efficacy of Artesian wells carried through the superincumbent strata into the greensand under London. The faults and flexures that run east and west within the great synclinal cannot be of much moment as obstructive agencies. In my earliest publication I have spoken of Windsor as a chalk "outlier-by-protrusion," and of the Isle of Thanet as another, and it is probable that the Deptford chalk is intermediate in the same parallel of elevation; but these, and such as these, would not be likely to offer any serious obstruction to the constant infiltration from higher levels. Borings through the chalk to the level of the gault would probably absorb the supply of the springs which now issue from that stratum round London, and especially those which take their rise only during great engorgements of it. Such are the "winter-bournes" of Hampshire and Wiltshire, and such intermitting streams as the Lavant and the Bourne, which do not fill till the ordinary issues are insufficient for the transmission of the superabundant supply. I reside in the synclinal of the great anticlinal flexure of Greenhurst, and I have an Artesian well in my garden, which soon after it began to operate, dried up a perennial spring about two hundred yards off and about twenty feet above me. And so it continues to do, except in long-continued rainy seasons, when the natural spring discharges again for a short time. What I have said of the waters of the chalk, as they are stopped by the gault, may apply to those of the greensand, which is constrained to throw them out because they do not find issue at a lower level; and such issue the Artesian wells would afford them.

Nomenclature.—I share in Mr. Prestwich's objection to the name of "plastic clay," and I only use it in submission to the tyranny of prescription. Mr. Prestwich's "lower tertiary" is a better phrase. The imposition of names is seldom well-considered. At the time I suggested the use of the word "Wealden*," which was adopted by Dr. Fitton and Sir H. De la Beche†, I also advocated the use of the word "Glauconite" for greensand, upper and lower, and to include everything from the chalk to the Wealden. If this also had been adopted, it would have been taken up by the French geologists, who would have found their subdivisions of *Glauconie crayeuse* and *Glauconie sableneuse* conveniently included, and we should not have been incommoded by the introduction of the synonym of "Neocomien." Or if Dr. Fitton (who of all men has the greatest right to be name-father in such a case) had given earlier enunciation to his happy thought of "Vectine," the propriety and convenience of such a collective appellation for all the members of the lower part of the cretaceous system, must have assured its general approval and adoption.

Evidence of upheaval.—As the evidence of the continuation of the same upheaving forces, from the great denudation of the Weald westward into what I have called the great chalk dome of Hampshire, is a matter of so much importance, I have again visited the line of country pointed out by Dr. Fitton as likely to contain signs of disturbance, bringing the eastern and western denudations into direct relation with each other.

I am induced to dwell on this point more particularly, because in his map of the Weald, and adjoining country, Mr. Hopkins has drawn an imaginary line round the "disturbed district‡," which conveys to the cursory observer, more strictly perhaps than the author intended, the notion, that the signs of disturbance are confined within those limits. We are to understand only, I believe, that within the area so described, the best evidence is to be found in exemplification of Mr. Hopkins's "Theory of Elevation."

The last examination, the third I have especially made of this line of country, confirms my opinion of the projection of the Winchester anticlinal westward, as far at least as the banks of the Test, and eastward into the great valley of the Weald by the Vale of Meon; the high grounds between these two points being the true continuation of the elevation of the South Downs. The valley which carries the Itchin from Alresford to Winchester is the synclinal of this elevation, and is, as I suspected, occupied by "lower tertiary."

In the above-mentioned visit, I discovered a flexure and fault in the Malm rock north of the new church at South-Harting, which I suspect to be the expiring point of this line of disturb-

* Geol. Mem. of West Sussex, 1828.
† See "Table of Superposition."
‡ See map in Geol. Trans. vol. vii.

ance. This arrangement in relation to the long and important flexure of Greenhurst on one side, and of the anticlinals of the Vale of Wardour and Portsdown on the other, appears to correspond with that intercurrency, or digitation of fissures, which Mr. Hopkins seems to consider an essential part of his system*.

I have previously spoken of the same arrangement on the north side of the great anticlinal, in the decline respectively of the Peasemarsh line, or more properly of the Froyle and Popham-beacon line, and that of Pewsey, in the Basingstoke country.

Lacerated Escarpments.—Although minor indications of this phænomenon, familiar to the practised eye, occasionally peep out, it is not often that we can get behind the mass of *débris* with which the basset edges of stony strata are encumbered. I have spoken of the entrance to a quarry of the upper green sandstone at Ray Common near Reigate, exhibiting unequivocal marks of the original violence. That quarry is now in disuse, but an open one is being worked a little to the east of the same place, on the outskirts of which obscure indications of contortion and displacement are visible; such as to show, that if a clean section could be obtained, transversely to the plane of stratification, from the chalk above to the gault beneath, a good specimen of lacerated escarpment would be brought into view. There is at this point a very remarkably prominent terrace of the stratum in question, illustrative of the projection given to such indurated layers at the angles of cross fracture, the transverse valley of Smitham-bottom running out southward here by Merstham and Redhill†.

Denudation and Diluvium.—It has been suggested to me that so remarkable a feature as the bare sand-rocks about Tunbridge Wells ought to be noticed in any disquisition on the Weald-area, however summary. No one can look at these rocks, or walk over those of Eridge Park, without being struck with their singular position and prominence, not in any way accountable but on the supposition of the violent abrasion of diluvial flood. It is well known that similar appearances are to be found all the world over, where layers of durable and destructible materials alternate with each other. And no one can view the wilds of Henley Heath and Bexley Hill, and the head of Hartingcombe‡ (covered as they are with boulder stones, and other signs of the

* Greenhurst————————
Winchester————————
Wardour————————
Portsdown————————

† This upper greensand platform is also noticed by Mr. Prestwich,—"Water-bearing Strata," &c., p. 80.

‡ West Sussex.

catastrophe we contemplate, only skimmed over by a scanty herbage), or walk round the slopes of Hind Head, and look down into the combs and gullies below, without having his mind filled with images of the mighty flux and reflux, that not very long ago (speaking in geological epoch) scooped out these valleys and modeled the picturesque and remarkable scene before him.

I have not time to do more than notice the account of Mr. Mackie's bone-bed at Folkstone, in the last Number of the Journal of the Geological Society. It is a specimen, and within the range, of my subcretaceous zone of drift, into which a few pebbles of the lowest tertiary have strayed. Much loam and brick earth seem to enter into the composition of this bed, and make it, as in the bottom of the Peasemarsh gravel, more than usually preservative of diluvial bones.

The same Number of the Journal contains Mr. Prestwich's description of, and speculation on, the cliffs and diluvial beds of Sangatte. That deposits of this sort on the chalk confines of the Boulogne denudation should correspond with those we find at Dover, Folkstone and Brighton, on the flanks of the Weald, on this side of the Channel, is only just what might be expected. It is probable that an examination of the French coast, south and west of the Somme, where the chalk sinks again under the tertiary beds, would afford the same intermixture of drift and the same transition from the angular and fractured flint of the "cretaceous" into the pebbly, sandy and loamy *débris* of the "tertiary drift zone" of the foregoing memoir. It cannot have escaped Mr. Prestwich's notice, that his ancient chalk cliff of the pre-eocene, or earliest tertiary shingle-bed at Sangatte, exactly corresponds with the sectional view given by Dr. Mantell in his Geology of the S.E. of England, of the ancient cliff and beach, with the superincumbent mingled drift materials in the cliffs between Kemp-town and Rottingdean;—all of which appearances are there still visible.

I have spoken cursorily of the diluvial deposits of the London basin and the eastern counties, the rolled clays and the heterogeneous admixtures of the Cromer Cliffs, and other phænomena indicative of the marginal relations of the *massif* of transported materials at the bottom of the German Ocean. It is not necessary to be reminded of the mammalian bones which are occasionally fished up at Harwich, and along all that line of coast, to be assured of the identity of the several bone-beds of Brighton, Folkstone, Cromer, the chesil-bed of Portland*, the elephant-bed at Peppering near Arundel†, the gravel-pits near

* *Vide* Dr. Buckland and Sir H. de la Beche, *loc. cit.*
† Mantell's Geol. of S.E. of England.

Guildford, with all the other diluvial beds of this age in the South of England.

The nomenclature of transported materials (*terrain de transport*) is not yet well determined. *Diluvial drift* will probably serve for the materials under review, in contradistinction to the loose materials of undoubted "moraines," "erratic boulders," "glacial accumulations," &c.

In my *prefatory letter* (p. 41), I have alluded to a paper by Sir Roderick Murchison, on the flint-drift of part of the area I have here had under review, read before the Geological Society on the 14th of May last, and of which an abstract was given in the Athenæum of the week following. I had the pleasure of affording some assistance locally to Sir Roderick in his researches preparatory to the production of that paper, and it was agreed that I should be present at the reading; and it was my intention then and there to have advocated the opinions here advanced, and to have adduced in brief, the facts and geological phænomena in their support, as imperfectly detailed in the foregoing pages; but from some misunderstanding I did not receive the intimation I expected of the day on which the reading and discussion were to take place.

The publication of Sir R. Murchison's paper is promised in the next forthcoming number of the Society's Journal; and although I am a stranger to that gentleman's precise opinions on the subject of the Weald denudation, I do not doubt of meeting with his support and full concurrence in relation to facts, however we may differ as to inferences.

Pulborough, Nov. 7, 1851.

LXXI. *On the Integration of Linear Differential Equations. By the* Rev. Brice Bronwin*.

In this paper D is used for $\frac{d}{dx}$, and the coefficients are supposed to be integer functions of x. The following formula is supposed to be well known:—

$$\left.\begin{array}{ll}\text{If} & \rho^k(\pi+k)u=\pi\rho^k u,\\ \text{then} & \rho^k f(\pi+k)u=f(\pi)\rho^k u,\end{array}\right\} \quad \ldots\ldots \quad (a)$$

where ρ and π denote operative symbols. Any forms of ρ and π therefore which satisfy the first of these will satisfy the second. From this we have the theorem

$$\epsilon^{kx}f(\mathrm{D}+k)u=f(\mathrm{D})\epsilon^{kx}u,$$

* Communicated by the Author.

which is equivalent to

$$x^k f(x\mathrm{D}+k)u=f(x\mathrm{D})x^k u,$$

into which it is changed by changing ϵ^x into x. But this last is only a particular case of the more general theorem,

$$x^k f(\pi+k)u=f(\pi)x^k u, \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{A})$$

where

$$\pi=x\mathrm{D}+\lambda(x).$$

Here $\lambda(x)$ denotes any arbitrary function of x, and may be a constant or nothing. This theorem is a modification of one which I have given in a memoir printed in the second part of the Philosophical Transactions for 1851.

A more general form is

$$\phi(x)^k f(\pi+k)u=f(\pi)\phi(x)^k u, \quad . \quad . \quad . \quad (\mathrm{B})$$

where

$$\pi=\left(\frac{\phi(x)}{\phi'(x)}\right)\mathrm{D}+\lambda(x).$$

The first of (a) is easily verified in both these cases, as also in the two that follow; therefore the theorems (A), (B), &c. are true.

$$\mathrm{D}^k f(\varpi-k)u=f(\pi)\mathrm{D}^k u \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{C})$$

if

$$\varpi=\mathrm{D}x+\lambda(\mathrm{D}), \text{ or } \varpi=x\mathrm{D}+\lambda(\mathrm{D});$$

where, of course, the arbitrary function $\lambda(\mathrm{D})$ may be a constaut or nothing.

A more general form than this is

$$\phi(\mathrm{D})^k f(\varpi-k)u=f(\varpi)\phi(\mathrm{D})^k u, \quad . \quad . \quad . \quad (\mathrm{D})$$

where

$$\varpi=\left(\frac{\phi(\mathrm{D})}{\phi'(\mathrm{D})}\right)x+\lambda(\mathrm{D}), \text{ or } \varpi=x\left(\frac{\phi(\mathrm{D})}{\phi'(\mathrm{D})}\right)+\lambda(\mathrm{D}).$$

These might be derived from (A) and (B) by the commutation of symbols; that is, by changing x into D, and D into $-x$, or by changing x into $-$D and D into x, and by suitably changing the functions f, λ and ϕ, when necessary.

In order to apply these theorems to the integration of linear differential equations with integer functions of x for their coefficients, suppose

$$\lambda(x)=a+a_1x+a_2x^2+\ldots,$$

and $\lambda(\mathrm{D})$ a function of the same form; we can put any equation under the form

$$\mathrm{X}=f(\pi)u+f_1(\pi)xu+f_2(\pi)x^2u+\ldots \quad . \quad . \quad (1)$$

For

$$\pi u=x\mathrm{D}u+\lambda(x)u,$$

and therefore

$$Du = x^{-1}\pi u - x^{-1}\lambda(x)u.$$

Change u into $xD + \lambda(x)$ in the first member, and into πu in the second, and we have

$$DxDu + D\lambda(x)u = x^{-1}\pi^2 u - x^{-1}\lambda(x)\pi u,$$

or

$$xD^2u + \big(1+\lambda(x)\big)Du = x^{-1}\pi^2 u - x^{-1}\lambda(x)\pi u - \lambda'(x)u.$$

Whence by eliminating Du and dividing by x, there results

$$D^2u = x^{-2}\pi^2 u - x^{-2}\big(1+2\lambda(x)\big)\pi u - x^{-2}\big(\lambda(x)^2 + \lambda(x) + x\lambda'(x)\big).$$

In like manner, we should find D^3u, D^4u, &c. We can thus eliminate D, and find a resulting equation containing only π and x, which by (A) can be put under the form required.

Also if

$$\varpi u = Dxu + \lambda(D)u,$$

then

$$xu = D^{-1}\varpi u - D^{-1}\lambda(D)u.$$

Change u into $Dxu + \lambda(D)u$ in the first member, and into ϖu in the second, and we have

$$xDxu + x\lambda(D)u = D^{-1}\varpi^2 u - D^{-1}\lambda(D)\varpi u,$$

or

$$Dx^2u - \big(1-\lambda(D)\big)xu = D^{-1}\varpi^2 u - D^{-1}\lambda(D)\varpi u + \lambda'(D)u,$$

and

$$x^2u = D^{-2}\varpi^2 u + D^{-2}\big(1-\lambda(D)\big)\varpi u + D^{-2}\big(\lambda(D)^2 - \lambda(D) + D\lambda'(D)\big).$$

Similarly, we should find x^3u, x^4u, &c.; and thus eliminating x, we should have a resulting equation in ϖ and D, which by (C) may be put under the form

$$X = f(\varpi)u + f_1(\varpi)Du + f_2(\varpi)D^2u + \ldots \qquad (2)$$

But if

$$\varpi u = xDu + \lambda(D)u,$$

change u into $D^{-1}u$; then

$$\varpi D^{-1}u = xu + \lambda(D)D^{-1}u,$$

and

$$xu = \varpi D^{-1}u - \lambda(D)D^{-1}u.$$

By continuing to change u into xu, and eliminating x from the second member, we should have the values of x^2u, x^3u, &c. in terms of ϖ and D. Thus by eliminating x, we should have, as in the last case, a resulting equation in the form of (2).

We might have proceeded in the second case with

$$xu = D^{-1}\varpi u - D^{-1}\lambda(D)u$$

in the same manner; and similarly in the first case, or

$$Du=x^{-1}\pi u-x^{-1}\lambda(x)u,$$

by changing u into Du and eliminating Du from the second member.

The equations (1) and (2) may be put under the forms

$$X_1=u+\phi(\pi)xu+\phi_1(\pi)x^2u+\ldots.$$
$$X_1=u+\phi(\varpi)Du+\phi_1(\varpi)D^2u+\ldots.$$

Then it will sometimes happen that by (A) and (C) they may further be put under the forms

$$X_1=u+a\phi(\pi)xu+b\phi(\pi)x\phi(\pi)xu+\ldots.$$
$$X_1=u+a\phi(\varpi)Du+b\phi(\varpi)D\phi(\varpi)Du+\ldots.$$

Make $\phi(\pi)x=\theta$, and also $\phi(\varpi)D=\theta$, and both take the form

$$X_1=u+a\theta u+b\theta^2u+\ldots.$$

or

$$X_1=(1-k\theta)(1-k_1\theta)\ldots.u;$$

which may be treated in the manner explained by Mr. Boole.

We can, however, rarely integrate when the second member contains more than two terms. Let then

$$X=f(\pi)u+f_1(\pi)x^ru.$$

We may consider $f(\pi)$ and $f_1(\pi)$ as integral functions of π, and therefore as factorial functions.

Assume

$$u=(\pi+h)(\pi+h_1)\ldots.(\pi+h_n)u_1,$$

or

$$u=(\pi+h)^{-1}(\pi+h_1)^{-1}\ldots.(\pi+h_n)^{-1}u_1,$$

as the case may require. Then by (A) we have

$$x^ru=(\pi+h-r)\ldots.(\pi+h_n-r)x^ru_1,$$

or

$$x^ru=(\pi+h-r)^{-1}\ldots.(\pi+h_n-r)^{-1}x^ru_1.$$

The constants h, h_1, &c. being suitably chosen, after substituting the assumed value of u in the given equation, we must operate on both members with the inverse of all those factors which are common to the two terms of the second member. When the method succeeds, the result will be an equation of an order lower than the given one.

But if the given equation be

$$X=f(\varpi)u+f_1(\varpi)D^ru,$$

make

$$u=(\varpi+h)\ldots.(\varpi+h_n)u_1,$$

or

$$u=(\varpi+h)^{-1}\ldots.(\varpi+h_n)^{-1}u_1.$$

Then by (C),

$$D^r u = (\varpi + h + r) \ldots . (\varpi + h_n + r) D^r u_1,$$

or

$$D^r u = (\varpi + h + r)^{-1} \ldots . (\varpi + h_n + r)^{-1} D^r u_1,$$

and the remainder of the process as before.

We will now illustrate this by two examples in each case. Let

$$X = \pi(\pi + a)u + k(\pi + ir)(\pi + b)x^r u. \quad . \quad . \quad . \quad (3)$$

Assume

$$u = (\pi + r)(\pi + 2r) \ldots . (\pi + ir)u_1,$$

then

$$x^r u = x^r(\pi + r) \ldots . (\pi + ir)u_1 = \pi(\pi + r) \ldots . \big(\pi + (i-1)r\big)x^r u_1$$

by (A). It will be obvious that i is supposed to be an integer number. Substituting these values, and operating with the inverse of the factors common to the second member, we have

$$X_1 = (\pi + a)u_1 + k(\pi + b)x^r u_1,$$

$$X_1 = \pi^{-1}(\pi + r)^{-1} \ldots (\pi + ir)^{-1} X.$$

This being only of the first order is immediately integrable.

The next example is chosen because it cannot be reduced in the same manner, and because it leads to a result of a very different form.

Let $\quad X = \pi(\pi - r)u + k(\pi + a)\big(\pi + a - (2i + 1)r\big)x^{2r} u. \quad (4)$

Make

$$u = (\pi + a)(\pi + a - 2r) \ldots . \big(\pi + a - (2i - 2)r\big)u_1.$$

Then by (A),

$$x^{2r} u = (\pi + a - 2r) \ldots . (\pi + a - 2ir)x^{2r} u_1.$$

By substitution and reduction as before, we have

$$X_1 = \pi(\pi - r)u_1 + k(\pi + a - 2ir)\big(\pi + a - (2i + 1)r\big)x^{2r} u_1,$$

and

$$X_1 = (\pi + a)^{-1} \ldots . \big(\pi + a - (2i - 2)r\big)^{-1} X.$$

If we put the last equation under the form

$$X_2 = u_1 + \frac{k(\pi + a - 2ir)\big(\pi + a - (2i + 1)r\big)}{\pi(\pi - r)} x^{2r} u_1,$$

it will reduce by (A) to

$$X_2 = u_1 + k\left(\frac{\pi + a - 2ir}{\pi}\right)x^r\left(\frac{\pi + a - 2ir}{\pi}\right)x\, u_1.$$

Therefore, making

$$\left(\frac{\pi + a - 2ir}{\pi}\right)x = \rho,$$

it becomes

$$X_2 = u_1 + k\rho^2 u_1,$$

which may be treated in Mr. Boole's way.

These examples may serve to indicate the mode of procedure in other cases; they belong each to a distinct class, but the mode of reduction is the same. We have employed only one of the forms assumed for u; in other cases it may be necessary to employ the other, and cases may perhaps arise in which it will be necessary to use a combination of both forms.

Now let

$$X = \varpi(\varpi + a)u + k(\varpi - ir)(\varpi + b)D^r u. \quad . \quad . \quad (5)$$

Assume

$$u = (\varpi - r)(\varpi - 2r) \ldots . (\varpi - ir)u_1.$$

Then

$$D^r u = D^r(\varpi - r)\ldots(\varpi - ir)u_1 = \varpi(\varpi - r)\ldots\big(\varpi - (i-1)r\big)D^r u_1 \text{by(C)}.$$

Substituting these values in the given equation, and operating with the inverse of the factors common to the terms of the second member, we have

$$X_1 = (\varpi + a)u_1 + k(\varpi + b)D^r u_1.$$

$$X_1 = \varpi^{-1}(\varpi - r)^{-1} \ldots . (\varpi - ir)^{-1}X.$$

The solution of the given equation is therefore made to depend upon that of one an order lower.

In the last place, let

$$X = \varpi(\varpi + r)u + k(\varpi + a)\big(\varpi + a + (2i+1)r\big)D^{2r}u. \quad . \quad (6)$$

Make

$$u = (\varpi + a)(\varpi + a + 2r) \ldots . \big(\varpi + a + (2i-2)r\big)u_1.$$

Then by (C),

$$D^{2r}u = (\varpi + a + 2r) \ldots . (\varpi + a + 2ir)D^{2r}u_1.$$

And proceeding as before, we find

$$X_1 = \varpi(\varpi + r)u_1 + k(\varpi + a + 2ir)\big(\varpi + a + (2i+1)r\big)D^{2r}u_1.$$

$$X_1 = (\varpi + a)^{-1} \ldots . \big(\varpi + a + (2i-2)r\big)^{-1}X.$$

This by (C), in the same manner as in the second example, may be put under the form

$$X_2 = u_1 + k\left(\frac{\varpi + a + 2ir}{\varpi}\right)D^r\left(\frac{\varpi + a + 2ir}{\varpi}\right)D^r u_1.$$

Make

$$\left(\frac{\varpi + a + 2ir}{\varpi}\right)D^r = \rho,$$

then

$$X_2 = u_1 + k\rho^2 u_1.$$

Thus the solution of the proposed is made to depend upon those of equations of a lower order.

Examples might be given to illustrate the use of the theorems (B) and (D), which are a generalization of (A) and (C); but the method must be plain from what has been done, the only difference being that we should have $\phi(x)$ in the room of x, and $\phi(\mathrm{D})$ in the room of D, and the symbols π and ϖ would be more general.

In reducing equations to the forms (1), (2), &c., we shall obtain from the same equation results of different forms by giving different forms to the arbitrary functions $\lambda(x)$, $\lambda(\mathrm{D})$; which is one advantage which these functions give us, and we must give them that form which will render our transformed equation the most convenient for solution. But in some cases, instead of supposing $\lambda(x)$, $\lambda(\mathrm{D})$ integer, or even rational functions, of x and D, we may so determine their form as to take away from the equation functions of them which are neither integer nor rational.

The examples (5) and (6), with their solutions, might be deduced from those of (3) and (4) by the commutation of symbols mentioned further back, and by a suitable change in the functions concerned; and it may be presumed universally, that by making this interchange of symbols in any linear equation and its solution, we shall obtain as the result another equation and its solution. But when the coefficients of a differential equation are integer functions of x, those of the commuted one will likewise be integer functions of x, and in this respect they will be alike. Therefore either of the formulæ (A) and (C) may be employed to effect its solution, but not perhaps with equal facility or equal success. A variety of means, however, is better than one only, as it augments our chances.

When the second member of an equation contains three or more terms, its solution may sometimes be made to depend on the solution of several other equations having only two terms in their second members; but I cannot enter upon that subject here.

October 30, 1851.

LXXII. *On the Theoretic Connexion of two Empirical Laws relating to the Tension and the Latent Heat of different Vapours. By* R. Clausius*.

A SUPERFICIAL contemplation of the tension series, experimentally developed for the vapours of different fluids, suffices to show that a certain uniformity exists therein; and

* From Poggendorff's *Annalen*, vol. lxxxii. p. 274.

hence the various efforts which have been made to ascertain a definite law by means of which the series which holds good for one fluid, water for instance, might be applied to other fluids.

A very simple law of this nature was expressed by Dalton. Calling those temperatures which belong to equal tensions *corresponding* temperatures, the law ran thus:—In the case of any two fluids *the differences between the corresponding temperatures are all equal.*

This law agrees pretty well with experience in the case of those fluids whose boiling-points are not far apart; for those, however, which possess very different degrees of volatility it is inexact. This is shown by a comparison of the vapour of mercury with that of water, according to the observations of Avogrado*. Still more decidedly does the divergence exhibit itself in the investigations of Faraday† on the condensation of gases.

In the "Additional Remarks" to his memoir, Mr. Faraday, after having disproved the applicability of the law of Dalton to gases, expresses himself as follows:—"As far as observations upon the following substances, namely, water, sulphurous acid, cyanogen, ammonia, arseniuretted hydrogen, sulphuretted hydrogen, muriatic acid, carbonic acid, olefiant gas, &c., justify any conclusion respecting a general law, it would appear that the more volatile a body is, the more rapidly does its vapour increase by further addition of heat, commencing at a given point of pressure for all;" and further on, "there seems every reason therefore to expect that the increasing elasticity is directly as the volatility of the substance, and that by further and more correct observation of the forces a general law may be deduced, by the aid of which and only a single observation of the force of any vapour in contact with its fluid, its elasticity at any other temperature may be obtained."

What Faraday here expresses with evident reserve and caution, we find again in the form of an equation in a later memoir by M. Groshans‡. The equation (3.) of the said memoir contains implicitly the following law:—*If all temperatures from* $-273°$ C. *downwards* (that is, downwards from that temperature which is expressed by the inverse value of the coefficient of expansion for atmospheric air) *be reckoned, then for any two fluids the corresponding temperatures are proportional.*

Although this carries with it a great degree of probability, at least as an *approximate* law, and is undoubtedly proved by the

* *Ann. de Chim. et de Phys.* xlix. p. 369. Pogg. *Ann.* vol. xxvii. p. 60. Complete in *Mém. de l'Acad. de Turin*, vol. xxxvi.

† Phil. Trans. of the Roy. Soc. of London for 1845, p. 155.

‡ Pogg. *Ann.* vol. lxxviii. p. 112.

experimental researches of Avogrado and Faraday to be preferable to the law of Dalton, still the manner in which M. Groshans deduces his equations leaves much to be desired. He premises the deduction by two equations which can only be regarded as approximately correct, inasmuch as they contain the expression of the law of Mariotte and Gay-Lussac for vapours at their maximum density. For the further development, however, he makes use of the following proposition:—If in the case of any two vapours the temperatures are so chosen that the tensions of both are equal, then, if the density of each vapour at the temperature in question be measured by its density at the boiling-point, these densities are equal. This proposition is introduced by the author in the memoir alluded to without any proof whatever. In a later memoir*, however, he says that he was led to the above conclusion by observing that in the case of seven different bodies composed of $p\,C+q\,H+r\,O$ the density of the vapour at the boiling-point compared with the density of steam at 100° could be expressed by the formula

$$D=\frac{p+q+r}{3};$$

and immediately afterwards he states, that "there are several bodies to which the formula

$$D=\frac{p+q+r}{3}$$

is inapplicable." From this it appears that the foundation on which the proposition rests cannot be regarded as established. It seems to me, that although the law mentioned above has obtained from M. Groshans a more definite form than in Faraday's expression, its probable validity is in no way augmented thereby.

In this state of uncertainty every new point of view from which a more extended insight as to the deportment of fluids during evaporation may be obtained is deserving of attention; and hence it will not perhaps be without interest, to establish such a connexion between the above law as regards the *tension* and another law regarding the *latent heat*,—the latter being also empirically established in a manner totally independent of the former—that the one shall appear to be a necessary consequence of the other.

I refer to the law, *that the latent heat of a unit of volume of vapour developed at the boiling-point is for all fluids the same.* Although this has not been completely corroborated by the experiments hitherto made, and even if it were perfectly true could not be so corroborated, our knowledge of the volumes of vapours at their maximum density being too scanty, still, an approxima-

* Pogg. *Ann.* vol. lxxviii. p. 292.

tion is observed which it is impossible to regard as accidental. We will therefore for the present assume the law to be correct, and thus make use of it for further deductions.

In the first place, it is clear that if the law be true for the boiling-points of all fluids, it must also be true for every other system of corresponding temperatures; for the boiling-points depend merely upon the accidental pressure of the atmosphere, and hence the law can be immediately expanded thus: *the latent heat calculated for the volume is for all fluids the same function of the tension.* Let r be the latent heat of a unit of weight of vapour at the temperature t, the volume of the unit of weight for the same temperature being $=s$, the latent heat of a unit of volume will then be expressed by the fraction $\frac{r}{s}$; let p be the corresponding tension; the law will then be expressed by the equation

$$\frac{r}{s}=f(p), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(I)}$$

in which f is the symbol of a function which is the same for all fluids.

Let this function be substituted for $\frac{r}{s}$ in the equation (V*a.*) of my memoir "On the Moving Force of Heat*," by neglecting therein the volume σ of a unit of weight of water as compared with that of vapour, we thus obtain

$$f(p)=\mathrm{A}(a+t)\frac{dp}{dt},$$

where A and a are two constants, the latter denoting the number 273, so that $a+t$ is the temperature of the vapour reckoned from $-273°$ downwards. If, for the sake of brevity, we call this quantity T, we have

$$\frac{d\mathrm{T}}{\mathrm{T}}=\frac{\mathrm{A}dp}{f(p)};$$

and from this we obtain by integration

$$c\,.\,\mathrm{T}=\mathrm{F}(p),$$

in which F is the symbol of another function, which is likewise the same for all fluids, and c an arbitrary constant which must be determined for each fluid. Let us suppose this equation solved for p, it will assume the form

$$p=\phi(c.\mathrm{T}), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(II)}$$

* Pogg. *Ann.* vol. lxxix. p. 508; and Phil. Mag. p. 107 of the present volume.

where ϕ is the symbol of a third function, which is the same for all fluids.

This equation is evidently the mathematical expression of the law of tension mentioned above; for to apply the function which in the case of any one fluid determines the tension from the temperature, to any other fluid, it is only necessary to multiply the temperature by a different constant, which constant is easily found when the tension for a single temperature is known.

It is thus shown, that, in so far as the validity of equation (V*a.*) is granted, the two laws expressed by the equations (I) and (II) are so connected with each other that when *one* of them is true, *the other* must necessarily be true also.

But in case both laws are only approximations to the truth, as to me appears most probable, the equation (V*a.*), which by introducing T instead of *t* becomes

$$\frac{r}{s-\sigma}=\text{A}.\text{T}.\frac{dp}{d\text{T}},$$

enables us at least to conclude as to the manner and degree in which two vapours diverge from each other with regard to their latent heat, likewise as to their divergence from the tension series, and the reverse. Thus, for instance, in comparing water with other fluids, it is observed that the tension of the vapour of the former increases more quickly with the temperature than the tension of other vapours. There is a complete coincidence between this fact and that observed by Andrews*, that the vapour of water possesses a greater latent heat than an equal volume of the vapour of any other fluid which Andrews examined, alcohol excepted. From this we perceive that it is by no means advantageous for the application of the above two laws to choose, as is generally done, water as the fluid of comparison; but that, on the contrary, the comparison of water with fluids of lower boiling-points is peculiarly calculated to support the law of Dalton.

LXXIII. *On the Detection of Arsenic. By* Andrew Fyfe, *M.D., F.R.S.E., Professor of Chemistry, University and King's College, Aberdeen*†.

It is well known that some metals unite with chlorine, not only synthetically, but also by decomposition of their compounds, and form volatile chlorides. Arsenic is one of the metals that comes under this class, and hence it can be volatilized as a chloride, and, under certain conditions, can be separated,

* Quarterly Journal of the Chem Soc. of London, No. 1. p. 27.
† Communicated by the Author.

not only from non-volatile chlorides, but also from those metals the chlorides of which are easily volatilized. That arsenic might be volatilized in this way, and be thus detected, occurred to Dr. Clarke, Professor of Chemistry in Marischal College, Aberdeen, some years ago, but, so far as I know, he has not prosecuted the subject. My attention was lately drawn to it more particularly, by the perusal of a paper, in which processes are described for the separation of metals from one another, in analysing their ores and alloys*. The perusal of that paper induced me to take up the consideration of the subject, and the results of the experiments which I have made have confirmed the opinion I had entertained, that the volatilization of arsenic, by the action of chlorine, as suggested by Professor Clarke, would add another to the many tests which we have of the presence of arsenic.

The question at first occurred, How is arsenic, in the state of arsenious acid, to be made to unite with chlorine? This was suggested by the results of the trials recorded in the paper referred to, viz. causing the decomposition of a chloride in solution in the fluid containing the arsenic. For this purpose the fluid is heated with oil of vitriol in a flask, to which a bent tube is adapted, and by which the product of distillation is conveyed into a cool receiver. When the mixture is brought to the boiling-point, a little dried sea salt is thrown in, and the distillation is continued for some time. By this process the hydrochloric acid evolved yields its hydrogen to the oxygen of the arsenious acid, while the liberated arsenic and chlorine unite, and come over as chloride, which is condensed in the receiver.

$$(\mathrm{As\,O^3 + 3SO^3\,HO + 3Cl\,Na = 3SO^3\,NaO + 3HO + As\,Cl^3}).$$

We thus obtain a transparent colourless liquid, in which arsenic is easily detected by the transmission of sulphuretted hydrogen, which gives the yellow sulphuret. By the addition of nitrate of silver in excess to the product of distillation, a copious white deposit of chloride of silver is formed. On filtering this fluid, and then adding ammonia solution, *continuously*, or, which I prefer, by holding over it a rod dipt in the ammoniacal solution, so as to avoid excess, the yellow arsenite of silver is either precipitated or appears on the surface as a yellow film, according to its quantity.

Though this method of detecting arsenic acts easily with pure fluids in which arsenious acid only is dissolved, it was necessary to ascertain whether it would act with mixed fluids, particularly those containing organic matter. A number of these was ac-

* So far as I remember, the paper alluded to appeared in one of the Foreign Journals—I think the *Comptes Rendus*—two or three years ago, but, though I have again and again searched for it, I have not succeeded in discovering it.

cordingly tried, and with all the rest was found to be equally delicate. The following are a few of the results:—

1st. $AsO^3 + SO^3\,HO$ + Water were boiled together for some time, and ClNa was then thrown in and distillation carried on. SH passed through the product of distillation, gave a copious yellow deposit, and $NO^5\,AgO$ + filtration + rod dipt in NH^3, gave a yellow precipitate.

2nd. Strong solution of starch was treated in the same way; SH and the addition of $NO^5\,AgO + NH^3$ gave the same results.

3rd. AsO^3 was boiled in a solution of starch and gelatine, and the product of distillation was collected in the usual way; when treated with SH and $NO^5\,AgO + NH^3$, it gave no indications of the presence of arsenic.

4th. Trials similar to 1 and 2 were made with barley broth and hare soup, with the same results.

5th. To try the delicacy of the test, 0·5—0·1—0·05 gr. of AsO^3 were boiled, each in an ounce of hare soup, with $SO^3\,HO$, to which was afterwards added Cl Na. In the product of distillation arsenic was detected by SH. In the last trial the AsO^3 was dissolved in nearly 10000 of fluid.

6th. A quarter of an ounce of flesh was soaked for 24 hours in a solution consisting of 0·5 gr. of AsO^3 in one ounce of water. The flesh was then boiled for some time in SO^3, slightly diluted and distilled with ClNa. In the distilled liquor arsenic was easily detected by SH.

The only metal likely to cause a source of fallacy in this method of detecting the presence of arsenic is antimony, which, with chlorine, forms a volatile chloride. But the antimonial compound which may be supposed to exist in a suspected fluid, I mean tartar-emetic, does not yield a volatile compound when treated with oil of vitriol and sea-salt, or, if it does, does so with great difficulty. Allowing that it does afford a volatile chloride, as the product of distillation is in general colourless, there is usually no difficulty in distinguishing the sulphuret of arsenic from that of antimony. If we cannot distinguish them, or supposing both metals to be present, the addition of nitrate of silver in excess, filtration and consequent application of ammonia to the filtered liquor, will show the presence or absence of arsenic.

I consider the method which I have now described as a valuable addition in toxicological researches. It is very easily performed; it separates the arsenic from substances which interfere, and render, in other methods, the results fallacious; indeed, in some cases, prevent the arsenic, though in considerable quantity, from being detected; such as the presence of organic matter, which, as in Marsh's process, occasionally causes annoyance by the frothing up of the materials, and consequent rise of the mixture into the tube.

The method which I now follow in conducting the process is to pour the suspected fluid into a flask with a wide mouth, to which is adapted a cork having two apertures in it. To one of them is fitted a bent tube, one end of which terminates in the flask immediately below the cork; the other end terminates in a tube containing distilled water, and placed in a cold fluid. Into the other aperture is placed a cork. Oil of vitriol is now poured in, and the mixture is boiled for some time; after which dried sea-salt is thrown in rapidly, and the cork is quickly replaced. The distillation is then continued for some time, taking care to keep the fluid, in which the tube is placed, as cool as possible. Instead of one, I sometimes employ two or even three tubes, with distilled water, in each of which the product of distillation is condensed, and all of which, if necessary, are tested for arsenic. When any of the tissues is to be examined, it is introduced into the flask with oil of vitriol, and boiled for some time, or till it entirely disappears. Sea-salt is then thrown in, and the process is conducted as described.

As the transmission of SH through the product of distillation in general gives satisfactory evidence of the presence or absence of arsenic, it is scarcely necessary to have recourse to any other test. It may however be more satisfactory to have recourse to others, and of these by far the best is nitrate of silver. It is to be added as long as it causes precipitation. The fluid is then to be shaken and filtered, by which the whole of the chlorine and hydrochloric acid is removed. After this a rod dipt in solution of ammonia is held over the filtered liquor. If arsenic is present the yellow film appears.

In using SH the operator requires to be on his guard, because the presence of uncombined acid causes the decomposition of the gas, and consequent deposit of sulphur, which may, by one not accustomed to observe precipitates, be mistaken for yellow arsenic.

It may be objected to this method of detecting arsenic, that it will not succeed when the arsenical compound is in the state of sulphuret; with King's yellow, for instance, which is sometimes taken as a poison. This must be admitted, when the sulphuret is *pure*. In one trial, in which sulphuret, prepared from an arsenical solution by precipitation, after being well washed, was treated with oil of vitriol and sea-salt, as described, the distilled fluid did not show indications of arsenic by the usual tests.

This, however, is not the case with the commercial sulphuret, which, in all the trials that I have made with it, has afforded a fluid by distillation with oil of vitriol and sea-salt, in which arsenic was easily detected. This is owing to its containing arsenious acid. Accordingly, when King's yellow was washed till it was pure, and was then treated with oil of vitriol and sea-salt, it did not yield arsenic by distillation. In conducting this process, the King's yellow requires to be washed for a long time before the whole of the arsenious acid is removed. In the trial above referred to, boiling water was used repeatedly for one day, and cold water was allowed to flow on it, in a constant stream, for another day, before the water came off pure. The first washings were alkaline, and gave a yellow precipitate on the addition of muriatic acid, and also by the transmission of sulphuretted hydrogen, showing that it contained not only arsenious acid, but also sulphuret of arsenic, held in solution by an alkali.

King's College, Aberdeen, July 1851.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 320.]

May 22, 1851. "On the Annual Variation of the Magnetic Declination, at different periods of the Day." By Lieut.-Col. Sabine, R.A., V.P. and Treas. R.S. &c.

In this communication the author has arranged and presented together the Annual variations which the magnetic declination undergoes at every hour of the day at the four Colonial Observatories established by the British government, at Toronto, Hobarton, the Cape of Good Hope and St. Helena. This has been done by means of a graphical representation, in which the annual variations at every hour are shown by vertical lines varying in length according to the amount of the range of the annual variation at each hour; each line having also small cross lines marking the mean positions of the several months in the annual range. The mean declination in the year at the respective hours is marked by a horizontal line which crosses all the verticals at each station. The hours are those of mean solar time at each station, the day commencing at noon. The annual variations represented in the plate were obtained at Toronto from three years of observation, viz. 1845, 46, 47; at Hobarton from five years, viz. July 2nd, 1843 to July 1, 1848; at the Cape of Good Hope from five years, viz. July 2nd, 1841 to July 1st, 1846; and at St. Helena from three years, viz. July 2nd, 1844 to July 1st, 1847.

The author observes that it is perceived at the first glance at the plate, that the range of variation at all the four stations is considerably greater during the hours of the day than during those of the night; and that there is a great similarity, though not a perfect identity, at all the stations in the relative amount of the range at different hours. Further, that the amount does not progressively enlarge to a maximum at or about noon, when the sun's altitude is greatest; or at the early hours of the afternoon, when the temperature is greatest; but that at all the stations the increase of the range is most rapid in the first or second hour after sunrise; and that its extent at the hours from 7 to 9 A.M. is not exceeded at any subsequent hour at Hobarton, the Cape and St. Helena, whilst at Toronto the great enlargement takes place even earlier, the hours of 6, 7 and 8 A.M. being exceeded by none, though they are equalled by a second increase at noon and the two following hours. This second enlargement is perceptible at the same hours at Hobarton and St. Helena.

With reference to the relative positions of the several months in each of the vertical lines, or at the different hours, it is observed that certain months, which are found congregated at the one extremity of the range during the early hours of the morning, undergo a transfer towards the opposite extremity at a subsequent period of the day; thus the months June, July, August usually occupy one extremity of the range, and November, December, January the other extremity, in the morning hours, and until from 8^h to 10^h A.M., when each of the two groups is respectively transferred towards the opposite extremity to that which it previously occupied. The period at which this transfer takes place is somewhat earlier at Toronto and St. Helena than at the Cape of Good Hope and Hobarton. The comportment of the two equinoctial months, March and September, at the Cape of Good Hope and St. Helena is pointed out as presenting a remarkable contrast to that of the two solstitial groups which have been described, and at the same time a still more remarkable contrast to each other, March being at almost all the hours on the West, and September on the East, of the mean line.

In conclusion the author points out one or two practical considerations suggested by the facts under notice:—

1. That as, in the Annual Variation represented in the plate, the same months occupy positions on opposite sides of the mean line at different parts of the twenty-four hours, the *mean* annual variation, or that which is shown by the mean values in each month taken from *all* the observation hours, must be merely a residual and not an absolute quantity; and that consequently natural features must be more or less masked in deductions in which only mean values are brought into view. In fact, as has been shown in the published volumes of the observations at St. Helena and Hobarton, the mean annual variation at those stations is so small as to be scarcely sensible. But when we resolve these mean results into their respective constituents, viz. the annual variation *at each of the observation hours*, there is then at once disclosed to us an order of natural phenomena, very far from inconsiderable in amount, systematic in general aspect, and apparently well deserving the attention of those who are occupied in the delightful and highly intellectual pursuit of tracing the agencies of nature.

2. We perceive in the variations of the position of the several months in the annual range, the necessity of paying regard to the period of the year, as well as to the period of the day at which ob-

servations have been made which do not include long intervals, and from which, nevertheless, inferences are drawn in respect to secular change. Such observations, when not those of a fixed observatory, are usually made at some hour in the day-time, when it needs only a glance at the plate to perceive that annual as well as diurnal variation-corrections are required, unless the month as well as the hour are the same in the earlier and later observations. A table of corrections for every hour of the day to the mean value in each month—corrections derived, as in the instances now before the Society, from a series of strictly comparable observations continued for several years—should be considered, not merely as a desirable, but as an almost indispensable provision, in countries where magnetic surveys are conducted with the degree of perfection of which they are now susceptible.

"On Induced and other Magnetic Forces." By Sir W. Snow Harris, F.R.S. &c.

The question as to identity in the source of those several and mysterious powers of nature by which masses or particles are moved either toward, or from each other, being a question of deep physical interest, the author of this paper has been led to some further investigation of the nature and laws of magnetic force, in the course of which several new facts have presented themselves which he thinks not altogether unworthy of attention.

Magnetic attraction as commonly observed being found to depend on certain impressions made on the attracting bodies usually designated by the general term induction, it appears essential to the progress of any inquiry into the laws of those forces operating externally to a magnet through space, to commence with a rigid examination of the nature and mode of action of those inductive forces upon which the reciprocal force of attraction between the bodies immediately depends. These forces of induction may be considered as a series of successive or reverberating influences, operating between the near and opposed surfaces of the magnetic bodies. When, for example, a magnet is opposed to a mass of soft iron, a direct impression is first made on the iron by which the iron is rendered temporarily magnetic; this induced force operates in its turn by a species of reverberation or reflexion upon the near pole of the magnet, and calls into play a portion of the magnetic force in the direction of the iron, which was previously operating toward the centre of the magnet; this action being once set up, may continue for a series of waves reverberating between the opposed surfaces, until the action sinks away as it were into rest. The author examines experimentally, by means of instruments, the principles of which he has already detailed in the Transactions of the Royal Society, this peculiar kind of action, and arrives at the following deductions relative to the laws of magnetic induction.

A limit exists in respect of induced magnetic force, different for different magnets, and varying with the magnetic conditions of the experiment, toward which the increments in the force continually approach, as if the opposed bodies were only susceptible of a given amount of induction under the existing circumstances.

Taking the force toward the limit of action, the amount of induction is in some inverse ratio greater than that of the simple distance; it was not however in any case found to exceed the inverse sesquiplicate ratio or $\frac{3}{2}$ power of the distance; as the distance is diminished the induction is as the distance inversely, but may in the mean time be as the $\frac{4}{3}$ or $\frac{5}{4}$ powers of the distances inversely, or near those powers. On further diminishing the distance, the induction was found in certain cases to be as the $\frac{2}{3}$ and $\frac{1}{2}$ powers of the distances, thus causing a series of changes in the law of magnetic attraction as commonly observed, which have hitherto greatly embarrassed the views of philosophers in their inquiries into this species of force. When the convergence is slow the induced force may not for a long series of terms appear to change, but when from any circumstance the convergence is accelerated, then the changes become more marked and successive. As a general result, however, the author is led to conclude, that magnetic induction is as the magnetic intensity directly, and from the $\frac{1}{2}$ to the $\frac{3}{2}$ power of the distance inversely.

In the course of these inquiries, it was found that the inductive action depended, not on the mass, but on the surface of the magnetic substance, and that magnetism, like electricity, exhibits a decrease of intensity when the surface of the iron upon which it is disposed is extended. A hollow cylinder of soft iron was carefully prepared in a lathe, and fitted with a solid interior core capable of being drawn out from within the cylinder; this compound body was exposed to the inductive action of a powerful magnetic bar, and the induced force estimated by the reciprocal force of attraction exerted between the mass and a cylinder of soft iron suspended from the author's magnetic balance, or from one arm of a light beam, set up in the way of a common balance. The degree of force being observed, the solid core was drawn out so as to extend the surface of the mass under induction. The intensity immediately declined, and again increased on replacing the solid within the hollow cylinder, being a result of exactly the same character as that produced by the extension of an electrified surface. When the interior solid core was removed altogether, then the induced force remained unchanged, it being precisely the same whether the body were taken hollow or solid. In accordance with this result, hollow cylindrical magnets were found as susceptible of magnetic power as solid masses of the same temperament and dimensions; an unmagnetized solid and tempered steel cylinder, placed within a hollow tempered steel cylinder, does not become magnetic on touching the external cylinder in the usual way. The magnetism, however, of a hollow cylindrical magnet is partially destroyed by placing within it a cylinder of soft iron, or the reverse poles of another magnet; nor can a hollow cylinder of tempered steel having a solid core of soft iron be rendered magnetic by the usual methods of touch. These results, it is considered, supply the experiments thought by Mr. Barlow so desirable to confirm his deductions relative to the action of iron shells and balls on the compass needle, which he found to be as the $\frac{3}{2}$ power of the surface, whatever the weight and thickness of the iron.

The author now proceeds to notice the investigations of Hawksbee, Brook Taylor, Muschenbroek and others, and thinks the inquiries of these philosophers have not been sufficiently considered or appreciated; that instead of the results exhibiting anomalies and discrepancies, they are really necessary consequences of the more elementary laws of induction, and perfectly explicable upon the fundamental principles of magnetism. He endeavours to show, that by the changes in the law of the induction, as already stated, laws of force will arise perfectly coincident with the results arrived at by Hawksbee, Brook Taylor and others; that is to say, the law of force may appear to be as the $\frac{5}{2}$ power of the distance inversely, as found by Brook Taylor; or as the $\frac{3}{2}$ power inversely, as found by Martin; or in the inverse duplicate ratio of the distance, as observed by Lambert; or as the simple distance inversely, as determined by Muschenbroek in several cases; or it may be as the cubes of the distances inversely, as stated by Newton. Examples are given in which these several laws were found to obtain.

In examining the laws of magnetic repulsion, similar results are arrived at. The inductive forces here, however, are subversive of the existing polar arrangements; hence the apparent repulsion: so long as the existing magnetic polarities remain unchanged, the law of force will be generally as the second power of the distance inversely; when the distances are small, it will be inversely as the simple distance; when the inductive actions subvert the existing polarities, then the law of force appears irregular and subject to no regular variation, as observed in all the early experiments with repellent poles.

The author is led to conclude, that the apparent law of attractive force will be found to depend in certain cases on the distances at which the force operates, as referred to the total distance or limit of action. Taken between $\frac{2}{5}$ths and $\frac{5}{6}$ths of the limit of action, the force may be inversely as the third powers or cubes of the distances; taken between $\frac{2}{5}$ths and $\frac{3}{5}$ths of the limit of action, it may be in the inverse sesquiduplicate ratio, or $\frac{5}{2}$ power of the distances; between $\frac{1}{3}$rd and $\frac{3}{5}$ths as the squares of the distances inversely. From the $\frac{1}{5}$th to $\frac{1}{2}$ of the limit of action it may be as the $\frac{3}{2}$ power of the distance inversely; within less than $\frac{1}{5}$th, it will be generally as the simple distance inversely.

On a further review of these laws of magnetism, it is evident that the immediate effect of an increase or decrease of distance, is an increase or decrease of the effective magnetism on which the total or reciprocal force depends. Thus taking the cases just quoted, it will be seen that the total force is always as the square of the induction, whatever be the resulting law of the attraction. Hence the force may as well be taken as the square of the quantity of effective magnetism directly, as some power of the distance inversely.

The author admits the difficulty in the way of the employment of such terms as quantity of magnetism, magnetic charge, and the like, and therefore only employs them according to the common acceptation of such terms, and not as referring to any particular hypothesis: he thinks there must necessarily be in such inquiries an element fairly enough expressed by the general term quantity as expressive of the relative or absolute magnitude of the cause, whatever it be, upon which the observed effects depend, and thinks it so far essential to obtain exact quantitative measures. In electricity we may estimate the charge conveyed into a battery by means of the unit measure, and we can at pleasure operate with one-half, one-third, &c. the quantity of electricity numerically expressed; but we have as yet no such measure in magnetism, and we are quite uncertain as to the quantity of effective magnetism in operation. The author hence endeavours to verify the law of magnetic charge just mentioned by a direct quantitative experimental process. A cylindrical rod of soft iron being surrounded by three successive coils of covered copper wire, was placed under the trial cylinder of the magnetometer and exposed to the operation of one or more precisely equal and similar batteries; one coil being appropriated to each battery. It is inferred that if one battery and one coil produced one measure of magnetism, two batteries and two coils would develope two measures, and so on; so that we should have only to determine the attractive force under this condition; now the attractive forces were found to be as the square of the number of batteries in action upon this cylinder, that is to say, as the square of the magnetism induced in the iron; hence the quantity of magnetism is as the square roots of the reciprocal forces. If therefore the reciprocal force between a magnet A and a cylinder of soft iron taken at a constant distance were represented by an equivalent of 4 grains, whilst the similar force with a magnet B at the same distance were represented by 9 grains, then the effective quantities of magnetism and operation in each case would be as $\sqrt{4} : \sqrt{9}$, that is as 2 : 3.

Availing himself of this law, the author endeavours to deduce experimentally the magnetic development in different points of a regularly tempered and magnetized bar, taken between the magnetic centre and extremities; and he finds by a very careful manipulation, that the magnetism in these points is directly as the distance from the magnetic centre; the reciprocal force on a small trial cylinder being as the squares of the distances from the centre.

Some striking analogies in the state of a magnetized steel bar and the common Leyden jar are noticed in this communication, from which it would appear that the conditions of electrical and magnetic force are precisely the same, and from which the author concludes that magnetic attraction is reducible, as in electricity, to an action between opposed surfaces; he thinks that a predisposition to identify these forces with that of gravity and other central forces has led many profound mathematicians and philosophers to question unduly the accuracy of every result not in accordance with such a deduction. He observes that Sir Isaac Newton considered "that the virtue of the magnet is contracted to the interposition of an iron plate, and is almost terminated by it, for bodies further off are not attracted by the magnet so much as by the iron plate*;" as also that this power is essentially different from gravity, "and in receding from the magnet decreases not in the duplicate, but almost in the triplicate proportion of the distance*," a result which has been shown to be perfectly consistent with experiments. Newton however has been supposed to have had "very inaccurate ideas of magnetic phenomena†;" it would be very difficult however to show from the little which this great author has advanced upon this subject in his grand work, the Principia, in what his views of magnetic action were defective; they appear on the contrary to be in most perfect accordance with experimental facts. In associating magnetic action with a law of the "centrifugal forces of particles terminating in particles next them," Newton never pretended to offer any theory of magnetism, but says with his usual diffidence, "whether elastic fluids do really consist of particles so repelling each other is a physical question," and "which he leaves philosophers to determine." On the other hand, a large amount of experimental research by Hawksbee, Brook Taylor, Whiston, Muschenbroek, and other eminent men, has been supposed by Dr. Robison as unworthy of confidence, and ill-adapted to the object for which it was designed‡. The same learned writer thinks that magnetic attractions and repulsions are not the "proper phenomena for declaring the precise law of variation." Yet was it by these same attractions and repulsions that Lambert, and more especially Coulomb, deduced what this accomplished author considers as being the true law of force. The author of this communication is led to believe, that all the results of these inquiries, including the deduction of Newton, are not only consistent with, but necessary consequences of, the laws of induced magnetic forces, as he has endeavoured to prove, and that the action of magnetism as commonly observed is something different from what has been usually imagined. That future inquiries may lead to the identity in origin of magnetic and gravitating force he thinks not improbable; there may be some diffuse emanation through space, the source of gravity, and other central forces; and it is not impossible but that the relations of this medium to the particles of common matter may admit of considerable modification or change, and which may be the source of that peculiar power we find displayed in those bodies we consider as being magnetic and call magnets. It has been occasionally supposed that in the reciprocal force between magnets and iron there is a peculiar agency in operation, the law of which is disturbed by the new forces of induction liable to ensue in changing the distances. The author however is of opinion that such a notion is inconsistent with the course of nature; it is induction which constitutes magnetic action, there is no other form of action; when induction is not present there is in fact no action; we must hence look to these very changes for an explanation of variable magnetic force.

* Principia, Books 2 and 3. † Edinb. Ency. vol. xiii. p. 270.
‡ Mechanical Philosophy, vol. iv. p. 217.

"Researches into the Identity of the Existences or Forces, Light, Heat, Electricity and Magnetism." By John Goodman, M.D. Communicated by Thomas Bell, Esq., Sec. R.S. &c.

In this communication the author describes the effects that were produced on a moderately sensitive galvanometer by exposure to the sun's rays, and which were observed by him during a period of four months, commencing on the 14th of November, 1850. The instrument is described as consisting of forty-six turns of covered copper wire, $\frac{1}{23}$th of an inch in diameter. The helix is blackened with ink at its southern extremity, and has a single magnetized sewing-needle suspended by about sixteen inches of silken fibre in its centre. The dial, which is of card-board, and divided into the usual number of degrees, rests upon the upper surface of the helix, and shades it from the ordinary light or sun's rays, except at its extremities, and occasionally some portions of the lower bundle of wires; and when the sun is very low the rays may be seen also to illumine to some extent the surface of the upper bundle. The indicator is formed of a slender filament of light wood in the usual manner, and the whole is enclosed in a glass shade. This instrument was placed for experiment in a window having a southern aspect; and whilst the sun was strongly shining upon it, it was frequently observed that there could not be obtained, either on account of vibrations or the erroneous condition of the instrument, any true indications. On shading the instrument from the sun's rays by a screen, the vibrations ceased, and the needle again adjusted itself north and south.

On removing the screen the needle began again to vibrate, and was soon discovered to become stationary at some distance from zero, indicating the transmission of a current in the helix. This deflection of the needle was soon found to be always, under the same circumstances, in the same direction, and to give indications of a current corresponding to the brightness of the sun.

This action appeared to depend upon the incidence of the sun's rays upon the south extremity, and some of the lower or upper bundle of wires only of the helix; for when they began to illumine the opposite extremity, either very slight indications, or a neutral result, constant vibrations, or the movement of the needle some degrees in the opposite direction, were always observed. The maximum deflection, at any time attainable by the galvanometer, when the sun was quite unclouded, was about 12°, generally only 10°. It may be observed that in all these experiments the power of the rays was probably somewhat diminished, by passing though the glass pane of the window, and through the glass shade of the instrument itself.

In order to show that the effect was not thermo-electric action, the extremities of the helix were removed from their mercury cups and wrapped in paper, so as to exclude the mercurialized portion of the copper from the action of the sun's rays; but no alteration occurred in the ordinary results of the experiments. There is, moreover, the author considers, no evidence on record of any thermo-electric action ensuing from the application of heat to copper wire alone, nor without the formation of a complete electrical circuit. But in these experiments hitherto the completion of the circuit had not been attempted. During the course of the experiments the circuit was established by means of a connecting wire between the mercury cups, and the circuit was again and again completed, and as frequently broken, without any deviation occurring in any of the results, either during the progression, stationary condition, or decline of the needle.

That these phenomena were the result of the action of the sun's rays upon the helix itself, was further shown, from the circumstance that when the sun remained clouded for days together, there was no deflection of the needle; that when the helix was partly shaded by a pillar, or the window-frame, the instrument indicated an amount of current corresponding to the number of coils of wire illumined; and *that the illumination of the whole bundle of wires* at the southern extremity of the helix was necessary to produce the usual results, for when a burning lens of high power was employed to condense the rays and throw them in a focus upon one or two wires only, no deflection of the needle was observed. It was also further shown that the action of the rays upon the helix was attributable to that portion situate chiefly at the southern extremity, for the whole instrument was in a variety of ways and at different periods shaded from the solar rays; but its results were unaffected, unless the south end was obscured, when the needle immediately declined; or the north end was illumined when the deflections were lessened, or the motion of the needle took place in the opposite direction.

A pile of red-hot burning embers held in the vicinity of one extremity of the helix caused a slight deflection of $\frac{1}{2}°$, and when held at the opposite extremity, caused a deflection in the opposite direction.

The author states a remarkable circumstance, viz. that vibrations and neutral action were observed during bright sunshine about the 11th of December, and again on the 23rd of January; that previous to the former period the deflections of the needle were to the *left-hand*; between these two periods they were to the *right-hand*; and after the latter period always to the left, after a given hour of the day. During the early sun, however, they were to the *right-hand*, and as the sun approached a given altitude, they were invariably to the *left-hand*. Deflections observed during the summer season were also to the *left-hand*; but those of the early sun were not submitted to the test.

On testing the instrument with a voltaic pair, it was shown that the current passed from south to north *above the needle* with the *early sun*, or when the indicator deflected to the right-hand, and *beneath the needle* with the rays which proceeded from a *considerable elevation*, or when the needle deflected to the left-hand.

In conclusion, the author states that the results of these experiments evince to his mind more than ever the *unity of force*; and that experimental evidence appears to justify the conclusion at which he has long since arrived, *that there is one, only, universal force* *in nature, which is modified by the accidental and varied conditions to which it is subjected, but that its essential nature and characteristics are at all times unchangeably the same.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 421.]

May 5, 1851.—Of the Transformation of Hypotheses in the History of Science. By W. Whewell, D.D.

The author remarks that new theories supersede old ones, not only by the succession of generations of men, but also by transformations which the previous theories undergo. Thus the Cartesian hypothesis of vortices was modified so that it explained, or was supposed to explain, a central force: and then, the Cartesian philosophers tried to accommodate this explanation of a central force to the phænomena which the Newtonian principles explained; so that in the end, their theory professed to do all that the Newtonian one did. The machinery of vortices was, however, a bad contrivance to produce a central force; and when it was applied to a globe, its defect became glaring. Still however, the doctrine of vortices has in it nothing which is absurd anterior to observation. The "nebular hypothesis" is a hypothesis of vortices with regard to the origin of the system of the universe, and is now held by eminent philosophers. Nor is the doctrine of the universal gravitation of matter at all inconsistent with some mechanical explanation of such a property; for instance, Le Sage's. We cannot say therefore that if the planets are moved by gravitation, they are not moved by vortices. The Cartesians held that they were moved by both: by the one, because by the other.

Like remarks may be made with respect to the theories of magnetism and of light.

LXXV. *Intelligence and Miscellaneous Articles.*

ON THE CONSTITUTION OF THE ATMOSPHERE. BY M. LEWY.

THE memoir which I have the honour to submit to the Academy contains the results of a series of researches on the constitution of the atmosphere, executed between France and New Granada, and from the coast up to 3193 metres above the level of the sea.

The analyses were performed by the new process of MM. Regnault and Reiset, which consists, as is well known, in analysing the air by volumes. To measure the elastic forces of the gas, I employed an excellent cathetometer constructed by M. Perraux; the process thus combined enables us to obtain much greater precision than has hitherto been reached in this class of experiments. To judge of the degree of accuracy attainable, it suffices to examine the numerical details of two analyses; and it will be seen that the greatest difference between two analyses made with the same sample of air never exceeded $\frac{1}{10,000}$th, and most frequently was not more than $\frac{1}{100,000}$th.

The various samples of air which I analysed were collected in bulb-tubes with the two ends drawn out and open; the capacity of these tubes was about 100 cub. centims. The air was collected in the following manner:—One of the extremities of the tube was connected, by means of caoutchouc tubing, with a little pair of bellows, which was moved sufficiently long to be certain that the whole of the air contained in the tube was replaced by the atmospheric air of the locality, taking the necessary precautions to avoid any mixture of the air of respiration; as soon as the air was collected, the tubes were sealed before the lamp.

I have divided the analyses into three series:—1st, analysis of the atmospheric air of France; 2nd, analysis of the atmospheric air of the Atlantic Ocean and Carribean Sea; 3rd, analysis of the air of New Granada.

The comparison of the results obtained in this investigation with previous ones shows that the constitution of the atmosphere is nearly the same in the New and the Old World. Taking the mean of the analyses, executed in eleven different localities of New Granada, we find that 10,000 vols. of normal atmospheric air contain 4·008 carbonic acid, 2101·425 oxygen, and 7894·557 nitrogen, which are nearly the same proportions as those which have been found for normal atmospheric air in various parts of Europe. However, on examining carefully all the experiments hitherto made on the constitution of the atmosphere, it is readily seen that the composition of the air is not absolutely constant. Perceptible differences exist, which vary with the meteorological conditions; thus after a long rain the carbonic acid and oxygen are always in smaller proportion than after a long drought; however, these differences are only appreciable when the analysis has been carried out with very great accuracy.

In the New World, where the seasons are more defined than in Europe, these variations are more easily detected. During the fine season the normal air always contains a little more oxygen and a little more carbonic acid than in the season of the rains. Thus, taking the mean of a large number of analyses, I found that 10,000 vols. of normal atmospheric air of Bogota contain—

	With a cloudy sky, and during the rains.	With a bright sky, and during the fine season.
Carbonic acid	3·822	4·573
Oxygen............	2099·542	2102·195
Nitrogen	7896·636	7893·232

The difference which exists between the atmospheric air of the two seasons is therefore on an average 0·751 for the carbonic acid and 2·653 for the oxygen in 10,000 vols. of air.

Taking the maximum and minimum of the results obtained in the analyses performed during the two seasons, we have—

	During the rainy season.	During the fine season.
Carbonic acid	3·609	5·043
Oxygen	2099·032	2103·199
Nitrogen	7897·359	7891·758

The greatest difference amounts consequently to 1·434 for the carbonic acid and to 4·167 for the oxygen. These differences are nearly the same as those which I found between the atmospheric air of Paris and that of Havre, analysed under meteorological conditions corresponding to the two seasons of South America.

It follows that the composition of the air which we respire is the same in Europe and in the New World as far as regards the oxygen and the nitrogen, whether it be taken at the level of the sea or at an elevation of 3000 metres; the only difference is the amount of carbonic acid, which appears, especially on the high mountains, to be somewhat greater than in the valleys and on the sea-shore.

With respect to the analyses of the air collected on the ocean, they have yielded a very interesting result. In the day-time this air constantly contains a little more oxygen and a little more carbonic acid than during the night. This difference becomes more perceptible as we leave the coasts; and it is probably owing to the solar rays, which, heating the surface of the sea during the day, disengage a portion of the gases which the sea-water holds in solution, and which, as is well known, contains more oxygen and carbonic acid than atmospheric air.

Taking for term of comparison the samples of atmospheric air collected on the Atlantic, on the same day, with the same wind, and at more than 400 leagues distant from land, we find—

	At 3 A.M.	At 3 P.M.
Carbonic acid	3·346	5·420
Oxygen............	2096·139	2106·099
Nitrogen	7900·515	7888·481

The difference is therefore 2·074 for the carbonic acid and 9·960 for the oxygen in 10,000 vols. of air.

The analyses of the *abnormal* air of New Granada present us with results not less interesting. From time to time, once or twice in the year, the atmosphere of New Granada contains an extraordinary proportion of carbonic acid, which coincides with an appreciable decrease of oxygen, and consequently alters the constitution of the atmosphere in a very marked manner.

The great number of volcanoes which exist in the New World, and the clearing of forests which are effected every year in this country, may cause these alterations. It is, in fact, during these clearances that the constitution of the atmosphere experiences the extraordinary changes which I have just mentioned.

These clearings, which are effected by vast conflagrations, called in the country *las quemas*, produce considerable quantities of carbonic acid, which, mixing with the atmosphere, alter its composition. The amount of carbonic acid which I found in this air rose in some analyses to 49 in 10,000 volumes of air. It is consequently from 10 to 11 times greater than in the air in its normal condition. The diminution of oxygen amounted sometimes to 68·350 in 10,000 vols. of air; instead of 2101·425 oxygen, I found only 2033·075. This decrease is therefore readily detected even by less sensitive methods than the one which I employed.

On the other hand, the air of the plain of Bogota sometimes presents an amount of carbonic acid far greater than the atmosphere of the *tierra caliente*. This difference may be explained either by the existence of volcanoes, which are situated not far from Bogota, or by the more or less active influence of the solar light. It will be conceived, in fact, that in the *tierra caliente*, where the temperature is very elevated, the decomposition of the carbonic acid by the green parts of the vegetables must be effected in a far more rapid manner than on the high plain of Bogota, where the temperature is not higher than from 57° to 64° F.

It is perhaps allowable to suppose, on observing this enormous quantity of carbonic acid appear from time to time in the atmosphere of the New World, and considering the large number of volcanoes which exist in the country, that a portion of the carbonic acid of the air is due to them, and that they thus contribute in part to nourish the vast and beautiful vegetation of the tropics.—*Comptes Rendus*, Sept. 29, 1851.

ON THE MAGNETISM OF GASES. BY M. PLÜCKER*.

I introduce the gases to be examined into a thin glass bulb, 45 millimetres in diameter, and which can be closed by a cock, also composed of glass. I attach the bulb to one of the arms of a delicate balance capable of indicating $\frac{1}{10}$th of a millegramme with perfect distinctness. The glass of the bulb is slightly magnetic; its magnetism is exactly compensated by the magnetic action of the surrounding air, so that the action of the magnet upon the bulb, previously exhausted, is absolutely null, whilst the attraction of the bulb when filled with a gas, either compressed or expanded, is exactly that of the same gas. At the ordinary pressure, the weight of the oxygen contained in the bulb is about equal to 57 millegrammes, and the attraction exerted upon this gas by the electro-magnet, when 6 of Grove's elements are used, is equal to 20 millegrammes.

1. On comparing the specific magnetism of oxygen with that of iron, taken as unity, I found the number 0·003500, which differs considerably from that given by M. E. Becquerel, but agrees tolerably well with the valuation made by Prof. Faraday.

2. Oxygen loses its sensible magnetism in almost all those gases with which it enters into chemical combination. Nitric oxide (NO_2) forms an exception, which is unique at present; its magnetism in round numbers is $\frac{2}{5}$th of that of oxygen. The protoxide of nitrogen (NO) did not exhibit the least trace of action, *i.e.* if this action

* Extract from a letter to M. Arago.

exists, it is not equivalent to $\frac{1}{100}$th of that exerted in the case of oxygen.

3. If oxygen gas is introduced in small quantities at a time into the bulb filled with nitric oxide, the magnetism diminishes until the proportion of the two gases becomes sufficient to form hyponitric acid (NO_4). The action is then apparently null. On adding still more oxygen, the magnetism reappears and continues to increase.

4. Hyponitric acid (NO_4), when condensed, is a diamagnetic liquid ; nothing is at present opposed to the view that the gas, in proportion to its volume, is but very feebly magnetic. All my attempts to decide this important question have failed : does nitrous acid gas (NO_3), which is pretty strongly magnetic, retain its specific magnetism when it becomes liquid ?

5. The magnetism of oxygen and of nitric oxide, as also that of the magnetic mixtures, is in proportion to the density of the gases.

6. A magnetic gas, when mechanically mixed with any other indifferent gas, retains its magnetism, whatever the density of the mixture may be ; but in the vicinity of the poles, separation of the gases appears to take place to a certain extent, which must slightly increase the attraction of the entire mass. In certain cases this separation does not appear to take place instantaneously ; it is found, at least in the case of a mixture of oxygen and chlorine, that the magnetic attraction augments uniformly if the bulb remains for some minutes exposed to the magnetic induction of the electro-magnet. The *primitive* attraction is found to be exactly that which corresponds to the quantity of oxygen contained in the mixture.

7. A magnetic gas which has been attracted by the electro-magnet for some minutes, is very distinctly repelled by it, if the polarity of the latter be changed by means of a commutator. I therefore conclude that gases possess what has been called the *coercive force* to a well-marked degree.—*Comptes Rendus*, Sept. 15, 1851.

ON THE FORMATION OF DOLOMITE BY THE ACTION OF MAGNESIUM VAPOURS. BY M. DUROCHER.

Pieces of a porous limestone and anhydrous chloride of magnesium were introduced into a gun-barrel, so that neither substances were in contact. The closed tube was then exposed for three hours to a dull red heat, in order to maintain an atmosphere of chloride of magnesium vapour round the limestone. At the end of that time the pieces of limestone were found to be covered with a crust of fused chloride of calcium and chloride of magnesium, mixed with a little peroxide of iron and the oxides of the two earths. The chlorides were separated by washing with water, and the nuclei were thus found to be partially converted into dolomite. On the addition of hydrochloric acid, the limestone which had not been altered dissolved first with strong effervescence, which subsequently became less energetic, as is the case with dolomite. Transparent groups of crystals were visible under the microscope : the mass had a white colour passing into yellow and grayish-yellow, and was, like dolomite, full of cavities. Durocher is of opinion, that the assumption of some geologists, that dolomite has been formed naturally by aqueous agency, is proved by this experiment not to be absolutely correct, as it may also have been formed by magnesian vapours issuing from the interior of the earth, and gradually converting limestone into dolomite.—*Comptes Rendus*, vol. xxxiii. p. 64.

NEW PHOTOGRAPHIC PROCESS UPON GLASS. BY M. J. R. LE MOYNE.

The process in question is completely practical, and does not merely refer to a few accidental occurrences. Nearly a year ago, I found that the impressions upon glass sometimes presented a positive aspect, and after a short period, my researches in this direction furnished me with tolerably good specimens; but I did not succeed in arriving at a sure and constant method until after making a long series of continuous attempts; these have extended to the present time.

The object of most of my experiments was to overcome the well-known inconveniences of the albuminous plates, and, independently of the positive process, I have made considerable progress in the production of the impressions by the following modifications, which in fact constitute a new method of preparation:—

1. Purification of the albumen of the whites of eggs by keeping them for a long time, and even adding sugar to produce slight fermentation, which clarifies them much better than the beating process which is generally adopted. This first addition of sugar (8 grs. to each white of egg) does not prevent the necessity of subsequently adding the quantity already recommended (38 to 46 grs.) to obtain greater sensibility to light; and with the processes I afterwards employ, the presence of this substance increases the adhesion of the coating, instead of diminishing it, which has been urged as an objection to it.

2. Iodizing the albuminous part, after it has dried, by immersing it in a bath of tincture of iodine to which $\frac{1}{10}$th part of its volume of nitric acid of specific gravity 1·380 has been added.

This method is very simple, and is not attended with any of the defects inherent to the use of albumen containing iodide of potassium in solution. There is but one of the processes known which could be brought into competition with this; it is the employment of the vapour of iodine; but the moist method has the advantage, both in the rapidity of execution and in the simplicity of the apparatus.

3. Omission of the use of acetic acid, and employing a simple solution of nitrate of silver (1 part to 10), for rendering the plates sensible.

I do not know whether acetic acid is really necessary, upon albuminated glass, when gallic acid is used to bring out the image; but, with the sulphate of iron, it is undoubtedly an expensive superfluity; moreover, the volatility of this acid is a cause of spontaneous changes in the solutions, and this is also a serious inconvenience.

4. The use of a second bath of nitrate of silver (1–20) after washing the plates with fluoride of potassium used as an accelerating agent. The object of this operation is not only to increase still more the sensibility, but especially to transform the excess of fluoride of potassium into fluoride of silver, and thus to prevent its acting upon the glass and causing the separation of the albumen; it is also useful, but in regard to the first point only, whatever the accelerating agent used may be.

5. Substitution of a strong bath of sulphate of iron *at the temperature of* 194° *F.* for the gallic acid generally used for bringing out the image. This modification produces an enormous increase of sensibility; moreover, the high temperature furnishes images of a very light shade, and this is the essential point upon which the production of proofs upon glass depends; lastly, the opacity is less than by the other processes; hence, in regard to the productions upon paper, there results a softness which does not exclude delicacy, and the absence of which has hitherto frequently been urged as an objection to the employment of albuminous films.

6. Fixing the proofs in four or five minutes by the *perfect solution of the iodide of silver*, by means of a bath of cyanide of potassium and hyposulphite of soda of proper strength.

This method of fixing is superior in every respect, both to the bromide of potassium, and the hyposulphite generally employed unmixed. It gives in a very short time, and without injuring the proofs, not only unexceptionable fixation, but also complete transparence in the parts which have not taken the impression, and lastly, considerable increase in the adhesion of the entire coating. It may, moreover, be easily applied to all the known processes of photography upon glass, and even without any doubt to the operations upon paper.

The proofs obtained by this process consist of opake, yellowish white images, lying in a diaphanous medium, and presenting, therefore, the positive or negative aspect, according as to whether they are placed upon a darker or lighter ground.

As negative proofs, they resist changes of temperature better, are more transparent (which allows of their being produced by a feeble light), and lastly, as I have stated, they furnish softer drawings upon paper than those prepared by other processes.

As positive proofs, and to allow of their being included under this head, the side of the albumen has only to be coated with black paint; they present a clearness and delicacy comparable to metallic plates, infinitely more beauty of outline, and lastly, a variety of shades, of which many are very artistic.

In regard to the time requisite for exposure to the light, I may add that I have obtained landscapes by the sun in a second (with a plano-convex objective consisting of a combination of glasses, furnished with a diaphragm the aperture in which was $0^{\cdot m}03$), and portraits in the shade out-of-doors in 4 or 5 seconds, and in a room in from 8 to 15 seconds (with the same object-glass without the diaphragm). Moreover, the proofs are naturally erect; and hence, even if the metallic plate sometimes offers a slight advantage in regard to rapidity, it loses it completely when inversion of the images would be inadmissible, and apparatus for reversing it is required.—*Comptes Rendus*, Sept. 15, 1851.

REFLEXION OF LIGHT FROM THE SURFACE OF LIQUIDS.

The following are the conclusions of M. Jamin, from a series of experiments recorded in the *Annales de Chimie et de Physique* for February 1851:—

1st. Liquid surfaces polarize light incompletely and elliptically.

2nd. Liquids having a high index of refraction have a positive anomaly, or difference of phase, between the principal components of the reflected motion.

3rd. This anomaly becomes negative when the index is very small.

4th. There are substances, whose indices are about 1·4, whose polarization is rectilineal.

5th. The laws of the intensities and anomalies are represented by the formulæ of M. Cauchy.

6th. If substances are superposed, the reflexion from their surface of separation follows the same *laws*.

7th. In this case it is impossible to predict the value and the sign of the coefficient of ellipticity.

The memoir moreover includes a table of the constants of reflexion for a number of liquids.—*Journal of the Franklin Institute*, July 1851.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1851.

Chiswick.—October 1. Densely clouded: rain. 2. Overcast: showery. 3. Fine: showery. 4. Rain: very fine: clear. 5. Fine. 6. Rain early: very fine. 7. Rain. 8. Very fine. 9. Foggy: drizzly: rain. 10. Cloudy. 11. Foggy: very fine. 12. Cloudy. 13. Very fine: rain. 14. Very fine. 15. Constant rain. 16, 17. Clear: very fine. 18. Fine: rain: cloudy. 19. Fine: overcast. 20. Slight drizzle: uniformly overcast. 21. Foggy: fine. 22. Slight fog: hazy. 23, 24, 25. Overcast. 26. Fine. 27. Overcast: exceedingly fine. 28. Overcast: rain. 29. Fine: clear. 30. Clear: fine. 31. Very fine.

Mean temperature of the month	51°·25
Mean temperature of Oct. 1850	44 ·32
Mean temperature of Oct. for the last twenty-five years .	50 ·50
Average amount of rain in Oct.	2·66 inches.

Boston.—Oct. 1. Fine: rain P.M. 2. Fine. 3. Rain: rain A.M. and P.M. 4. Cloudy: rain A.M. 5. Fine: rain A.M. and P.M. 6. Fine: rain early A.M. 7, 8. Fine. 9. Cloudy: rain A.M. and P.M. 10, 11. Fine. 12. Cloudy. 13. Cloudy: rain P.M. 14. Fine. 15. Cloudy: rain early A.M. and P.M. 16—19. Fine. 20. Cloudy. 21. Rain. 22. Fine. 23—25. Cloudy. 26, 27. Fine. 28. Cloudy: rain P.M. 29. Fine: rain P.M. 30. Cloudy: rain A.M. and P.M. 31. Fine: rain P.M.

Sandwick Manse, Orkney.—Oct. 1. Bright: clear: aurora. 2. Rain: clear: aurora. 3. Bright: clear. 4. Rain: clear: large lunar halo. 5. Bright: showers. 6. Cloudy: showers. 7. Cloudy: rain. 8. Bright: lunar rainbow. 9. Bright: showers. 10. Showers: cloudy. 11. Clear: showers. 12. Cloudy: damp. 13. Cloudy: showers. 14. Bright: showers. 15. Rain: bright: showers. 16. Bright: clear. 17. Showers: drops. 18. Showers: rain: aurora. 19. Clear: showers: aurora. 20. Bright: cloudy: rain. 21. Bright: cloudy. 22. Bright: clear: aurora. 23. Damp: drops: aurora. 24. Fog: fine. 25. Fog: rain. 26. Drizzle: rain. 27. Fine: cloudy: rain. 28. Rain: showers: aurora. 29. Sleet-showers: aurora. 30. Cloudy. 31. Rain: showers: aurora.

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

Days of Month. 1851. Oct.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9½ a.m.	Barometer. Orkney, Sandwick. 8½ p.m.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Thermometer. Orkney, Sandwick. 9½ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.
1.	29·290	29·012	28·94			29·13	29·11	60	44	55			53	54	s.	s.		ese.	·35	......		·05
☽ 2.	29·388	29·298	28·90			28·95	29·37	58	45	49			55	51	s.	s.		se.	·14	·44		·16
3.	29·577	29·440	29·12			29·39	29·34	61	49	52			53	51½	s.	s.		se.	·06	·04		......
4.	29·485	29·462	29·04			29·16	29·27	64	43	57			54	46	sw.	s.		se.	·25	·11		·09
5.	29·748	29·571	29·10			29·20	29·24	61	43	50			50½	50	nw.	s.		w.	·18	......		·21
6.	29·774	29·677	29·27			29·36	29·37	61	40	48			48	49	w.	w.		wsw.	......	·31		·17
7.	29·785	29·669	29·30			29·20	29·19	60	40	51			47½	47	sw.	ssw.		nw.	·09	......		·15
8.	29·955	29·833	29·43			29·42	29·61	59	32	47			50½	49	w.	w.		wnw.	......	......		·40
9.	29·924	29·802	29·54			29·68	29·54	60	56	42·5			49	50	s.	s.		sw.	·13	......		·03
○ 10.	30·168	29·535	29·54			29·60	29·86	68	46	59			53½	50	w.	wnw.		w.	......	·25		·09
11.	30·224	30·188	29·75			29·62	29·68	68	53	46			57½	52	s.	sw.		w.	......	......		·06
12.	30·283	30·274	29·78			29·89	29·84	68	54	57·5			50	52	s.	sw.		sw.	......	......		·06
13.	30·129	29·989	29·58			29·36	29·62	62	54	60			53½	49	sw.	sw.		sw.	·04	......		·05
14.	29·928	29·841	29·50			29·52	29·55	62	44	51·5			48½	46	w.	w.		calm	·03	·03		·47
15.	29·430	29·336	28·94			28·81	28·88	56	32	56·5			50	44	w.	w.		wnw.	·34	·20		·44
16.	29·606	29·502	29·10			29·11	29·36	57	28	41·5			46	43	sw.	w.		nw.		·06		·21
17.	29·894	29·802	29·43			29·53	29·58	57	30	39			46	45	w.	wnw.		s.	......	......		·07
☾ 18.	29·941	29·742	29·52			29·40	29·41	59	52	47			52	47	sw.	sw.		wnw.	·01	......		·09
19.	30·041	29·930	29·50			29·66	29·81	62	55	57			49½	45	sw.	wsw.		s.	·01	......		·13
20.	30·080	30·045	29·64			29·99	29·90	64	55	58			50	49	w.	ssw.		se.	......	......		·05
21.	30·076	29·993	29·62			29·84	29·83	62	52	58·5			55	51½	e.	se.		se.	......	......		·15
22.	30·137	29·986	29·56			29·94	30·10	56	50	56			50½	46	n.	calm		se.	......	......		·05
23.	30·255	30·219	29·83			30·22	30·30	57	50	47·5			47	50	se.	wnw.		s.	......	......		·01
● 24.	30·361	30·272	29·98			30·31	30·34	57	37	52			48½	51	ne.	calm		se.	......	......		·02
25.	30·386	30·348	30·00			30·30	30·09	55	49	50			51	54½	ne.	wnw.		wsw.	......	......		......
26.	30·214	30·099	29·79			29·90	29·83	57	42	44			53	53	e.	w.		w.	......	......		·05
27.	30·170	30·084	29·66			30·13	30·03	59	37	48			47	48½	nw.	wnw.		w.	......	......		·12
28.	30·100	29·576	29·65			29·56	29·28	54	39	47			47	44	sw.	wsw.		nw.	·33	......		·26
29.	29·419	29·305	29·04			29·46	29·71	48	30	38			42	42½	nw.	nw.		nne.	·02	·26		·10
30.	29·599	29·424	29·24			29·90	29·80	49	36	44			45	44	ne.	n.		w.	·01	·28		·08
☽ 31.	29·612	29·602	29·35			29·46	29·29	48	32	34			46	44	nw.	n.		wnw.	·02	·12		·14
Mean.	29·902	29·769	29·44			29·580	29·617	59·00	43·51	49·8			49·96	48·33					2·01	2·12		3·96

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. II. FOURTH SERIES.

LXXVI. *On the Centrifugal Theory of Elasticity, as applied to Gases and Vapours.* By WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.S.E., F.R.S.S.A. &c.**

(1.) THE following paper is an attempt to show how the laws of the pressure and expansion of gaseous substances may be deduced from that which may be called the *hypothesis of molecular vortices,* being a peculiar mode of conceiving that theory which ascribes the elasticity connected with heat to the centrifugal force of small revolutions of the particles of bodies.

The fundamental equations of this theory were obtained in the year 1842. After having been laid aside for nearly seven years, from the want of experimental data, its investigation was resumed in consequence of the publication of the experiments of M. Regnault on gases and vapours. Its results having been explained to the Royal Society of Edinburgh in February 1850, a summary of them was printed as an introduction to a paper on the Mechanical Action of Heat in the twentieth volume of the Transactions of that body. I now publish the investigation in detail in its original form, with the exception of some intermediate steps of the analysis in the second and third sections, which have been modified in order to meet the objections of Professor William Thomson of Glasgow, to whom the paper was submitted after it had been read, and to whom I feel much indebted for his friendly criticism.

This paper treats exclusively of the relations between the density, heat, temperature, and pressure of gaseous bodies in a statical condition, or when those quantities are constant. The laws of their variation belong to the theory of the mechanical action of heat, and are investigated in the other paper already referred to.

The present paper consists of six sections.

The first section explains the hypothesis.

* Communicated by the Author, having been read to the Royal Society of Edinburgh, February 4, 1850.

The second contains the algebraical investigation of the statical relations between the heat and the elasticity of a gas.

The third relates to temperature and real specific heat.

The fourth treats of the coefficients of elasticity and dilatation of gases, and compares the results of the theory with those of M. Regnault's experiments.

The fifth treats of the laws of the pressure of vapours at saturation.

The sixth relates to the properties of mixtures of gases of different kinds.

I have endeavoured throughout this paper to proceed as directly as possible to results capable of being compared with experiment, and to carry theoretical researches no further than is necessary in order to obtain such results with a degree of approximation sufficient for the purpose of that comparison.

Section I. *On the Hypothesis of Molecular Vortices.*

(2.) The hypothesis of molecular vortices may be defined to be that which assumes—*that each atom of matter consists of a nucleus or central point enveloped by an elastic atmosphere, which is retained in its position by attractive forces, and that the elasticity due to heat arises from the centrifugal force of those atmospheres, revolving or oscillating about their nuclei or central points.*

According to this hypothesis, *quantity of heat* is the *vis viva* of the molecular revolutions or oscillations.

Ideas resembling this have been entertained by many natural philosophers from a very remote period; but so far as I know, Sir Humphry Davy was the first to state the hypothesis I have described in an intelligible form. It appears since then to have attracted little attention, until Mr. Joule, in one of his valuable papers on the production of heat by friction, published in the London and Edinburgh Philosophical Magazine for May 1845, stated it in more distinct terms than Sir Humphry Davy had done. I am not aware, however, that any one has hitherto applied mathematical analysis to its development.

(3.) In the present stage of my researches, there are certain questions connected with the hypothesis as to which I have not found it necessary to make any definite supposition, and which I have therefore left indeterminate. Those questions are the following.

First. Whether the elastic molecular atmospheres are continuous, or consist of discrete particles. This may be considered as including the question, whether elasticity is to a certain extent a primary quality of matter, or is wholly the result of the repulsions of discrete particles.

Secondly. Whether at the centre of each molecule there is a real nucleus having a nature distinct from that of the atmosphere, or a portion of the atmosphere in a highly condensed state, or merely a centre of condensation of the atmosphere, and of resultant attractive and repulsive forces. Therefore, although the word *nucleus* properly signifies a small central body, I shall use it in this paper for want of a better term to signify an *atomic centre,* whether a real nucleus or a centre of condensation and force. I assume, however, that the volume of the nucleus, if any, is inappreciably small as compared with that of the atmosphere.

(4.) I have now to state a supposition, which, so far as I am aware, is peculiar to my own researches. It is this:—*that the vibration, which, according to the undulatory hypothesis, constitutes radiant light and heat, is a motion of the atomic nuclei or centres, and is propagated by means of their mutual attractions and repulsions.*

It will be perceived at once, that from the combination of this supposition with the hypothesis of molecular vortices, it follows that the absorption of light and of radiant heat consists in the transference of motion from the nuclei to their atmospheres, and conversely, that the emission of light and of radiant heat is the transference of motion from the atmospheres to the nuclei.

It appears to me that the supposition I have stated possesses great advantages over the ordinary hypothesis of a luminiferous æther pervading the spaces between ponderable particles, especially in the following respects.

First. The propagation of transverse vibrations requires the operation of forces, which, if not altogether attractive, are of a very different nature from those capable of producing gaseous elasticity, and which it is difficult to ascribe to such a substance as the æther is supposed to be; while attractive forces between the atomic centres are perfectly consistent with their being kept asunder by the elasticity of their atmospheres.

Second. The immense velocity of light and radiant heat is a natural consequence of this supposition, according to which the vibrating masses must be extremely small as compared with the forces exerted by them.

Third. According to the most probable view of the theory of dispersion, the unequal refrangibility of undulations of different lengths is a consequence of the distances between the particles of the vibrating medium having an appreciable magnitude as compared with the lengths of the undulations. This is scarcely conceivable of the æther, but easily conceivable of the atomic nuclei.

Fourth. The manner in which the propagation of light and of radiant heat is affected by the molecular arrangement of crystalline bodies is rendered much more intelligible if the vibrations are supposed to be those of the atomic nuclei, on whose mutual forces and positions the form of crystallization must depend.

[*Note.*—The consequences of this supposition, in the theory of double refraction and polarization, are pointed out and shown to be corroborated by Professor Stokes's experiments on diffraction, in a paper read to the Royal Society of Edinburgh on the 2nd of December 1850, and published in the Philosophical Magazine for June 1851.]

Section II. *Investigation of the General Equations between the Heat and the Elasticity of a Gas.*

(5.) I now proceed to investigate the *statical* relations between the heat and the elasticity of a gaseous body; that is to say, their relations when both are invariable. The *dynamical* relations between those phænomena which involve the principles of the mutual conversion of heat and mechanical power by means of elastic fluids, and of the latent heat of expansion and evaporation, form the subject of another paper.

(6.) It is obvious that, in the condition of perfect fluidity, the forces resulting from attractions and repulsions of the atomic centres or nuclei upon their atmospheres and upon each other, must be considered as being sensibly functions merely of the general density of the body, and as being either wholly independent of the relative positions of the particles, or equal for so many different positions as to be sensibly independent of them; for otherwise a certain degree of viscosity would arise, and constitute an approach to the solid state. For the same reason, in the state of perfect fluidity, each atomic atmosphere must be considered as being sensibly of uniform density in each spherical layer described round the nucleus with a given radius, and the total attractive or repulsive force on each indefinitely small portion of an atmosphere must be considered as acting in a line passing through its nucleus; that force, as well as the density, being either independent of the direction of that line, or equal for so many different and symmetrical directions as to be sensibly independent of the direction.

(7.) An indefinite number of equal and similar atoms, under such conditions, will arrange themselves so that the form of their bounding surfaces will be the rhombic dodecahedron, that being the nearest to a sphere of all figures which can be built together in indefinite numbers.

(8.) I may here explain, that by the term *bounding surfaces of the atoms,* I understand a series of imaginary surfaces lying between and enveloping the atomic centres, and so placed that at every point in these surfaces the resultant of the joint actions of all the atomic centres is null. To secure the permanent existence of each atom, it must be supposed that the force acting on each particle of atomic atmosphere is centripetal towards the nearest nucleus or centre.

The variation of that force in the state of perfect fluidity must be so extremely small in the neighbourhood of those surfaces, that no appreciable error can arise, if, for the purpose of facilitating the calculation of the elasticity of the atmosphere of an atom at its bounding surface, the form of that surface is treated as if it were a sphere, of a capacity equal to that of the rhombic dodecahedron.

(9.) If the several atoms exercised no mutual attractions nor repulsions, the total elasticity of a body would be equal to the elasticity of the atomic atmospheres at their bounding surfaces. Supposing such attractions and repulsions to exist, they will produce an effect, which, in the state of perfect fluidity, will be a function of the mean density of the body; and which, for the gaseous state, will be very small as compared with the total elasticity. Therefore if p be taken to represent the superficial elasticity of the atomic atmospheres, P the actual or total elasticity of the fluid, and D its general density,

$$\mathrm{P}=p+f(\mathrm{D}), \quad \ldots\ldots\ldots \quad (1)$$

where $f(\mathrm{D})$ is a function of the density, which may be positive or negative according to the nature of the forces operating between distinct atoms.

(10.) The following relations must subsist between the masses of the atmosphere and nucleus, and the density and volume of each atom.

Let R represent the radius of the sphere already mentioned, whose capacity is equal to the volume of an atom, that volume being equal to $\frac{4\pi}{3}\mathrm{R}^3$.

Let μ denote the mass of the atmosphere of an atom, m that of the nucleus, and $\mathrm{M}=\mu+m$ the whole mass of the atom (so that if there is no real nucleus, but merely a centre of condensation, $m=0$, and $\mathrm{M}=\mu$).

Then D being the general density of the body, $\frac{\mu}{\mathrm{M}}\mathrm{D}$ is the mean density of the atomic atmosphere, and $\mathrm{M}=\frac{4\pi}{3}\mathrm{R}^3\mathrm{D}$.

If $u\mathrm{R}$ be taken to denote the distance of any spherical layer of the atmosphere from the nucleus, the density of the layer may be represented by

$$\frac{\mu}{\mathrm{M}}\mathrm{D}\psi(u),$$

and the function ψu will be subject to this equation of condition,

$$\mu = \int_{u=0}^{u=1} du \left(4\pi R^3 \frac{\mu}{M} Du^2 \psi u \right),$$

which is equivalent to

$$1 = 3\int_0^1 du(u^2 \psi u). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

(11.) So far as our experimental knowledge goes, the more substances are rarefied, that is to say, the more the forces which interfere with the operation of the elasticity of the atomic atmospheres are weakened, the more nearly do they approach to a condition called that of *perfect gas,* in which the elasticity is simply proportional to the density. I therefore assume the elasticity of the atomic atmosphere at any given point to be represented by multiplying its density at that point by a constant coefficient b, which may vary for different substances, but, as I have already stated, without deciding whether that elasticity is a primary quality or the result of the repulsion of particles. Consequently the superficial atomic elasticity

$$p = b\frac{\mu}{M} D\psi(1), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$\psi(1)$ being the value of ψu which corresponds to the bounding surface of the atom, where $u=1$.

(12.) Let an oscillatory movement have been propagated from the nuclei to every part of their atmospheres, the size of the orbits of oscillation being everywhere very small as compared with the radii of the atoms, and let this movement have attained a permanent state, which will be the case when every part of each atmosphere, as well as each nucleus, moves with the same mean velocity v; *mean velocity* signifying that part of the velocity which is independent of periodic changes. It is necessary to suppose that the propagation of this movement to all parts of a molecular atmosphere is so rapid as to be practically instantaneous.

We shall conceive all the masses and densities referred to, to be measured *by weight.* Then taking g to represent the velocity generated by the force of gravity at the earth's surface in unit of time, the whole mechanical power to which the oscillatory movement in question is equivalent in *one atom* will be represented in terms of gravity by

$$\frac{Mv^2}{2g} = q; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

that is to say, the weight of the atom, M, falling through the height $\frac{v^2}{2g}$ due to the velocity v; and this is the mechanical measure of the quantity of heat in one atom in terms of gravity.

(13.) Any such motion of the particles of a portion of matter confined in a limited space will in general give rise to a centrifugal tendency with respect to that space. In order to obtain definite results with respect to that centrifugal tendency in the case now under consideration, it is necessary to define, to a certain extent, the general character of the supposed movement.

In the first place, it is periodical; secondly, it is similar with respect to so large a number of radii drawn in symmetrical directions from the atomic centre, as to be sensibly similar in its effects with respect to all directions round that centre. This symmetry exists in the densities of the different particles of the atomic atmosphere in a gas, and in the forces which act upon them; and we are therefore justified in assuming it to exist in their motions.

Two kinds of motion possess these characteristics.

First. Radial oscillation, by which a portion of a spherical stratum of atmosphere surrounding an atomic centre, being *in equilibrio* at a certain distance from that centre, oscillates periodically to a greater and a less distance. This forms part of the *vis viva* of the molecular movements; but it can only affect the superficial atomic elasticity by periodic small variations, having no perceptible effect on the external elasticity.

Second. Small rotations and revolutions of particles of the atomic atmosphere round axes in the direction of radii from the atomic centre, by which each spherical layer is made to contain a great number of equal and similar vortices, or equal and similar groups of vortices having their axes at right angles to the layer, and similarly situated with respect to a great many symmetrical directions round the atomic centre.

Let us now consider the condition, as to elasticity, of a small vortex of an atmosphere whose elasticity is proportional to its density, inclosed within a cylindrical space of finite length, and not affected by any force at right angles to the axis except its own elasticity. Let Z denote the external radius of the cylinder, ρ_1 its external density, ρ its mean density, ρ' the density at any distance z from the axis (all the densities being measured by weight), w the uniform velocity of motion of its parts. The condition of equilibrium of any cylindrical layer is, that the difference of the pressures on its two sides shall balance the centrifugal force; consequently (b being the coefficient of elasticity)

$$0=\frac{w^2\rho'}{gz}-b\,\frac{d\rho'}{dz}.$$

The integral of this equation is

$$\rho'=\alpha z^{\frac{w^2}{bg}}.$$

The coefficient α is determined by the following relation, analogous to that of equation (2), between the densities

$$\rho\frac{Z^2}{2}=\int_0^Z dz(\rho' z)=\frac{\alpha}{\frac{w^2}{bg}+2}Z^{\frac{w^2}{bg}+2};$$

whence

$$\alpha=\rho\left(\frac{w^2}{2bg}+1\right)Z^{-\frac{w^2}{bg}}.$$

And the general value of the density is

$$\rho'=\rho\left(\frac{w^2}{2bg}+1\right)\left(\frac{z}{Z}\right)^{\frac{w^2}{bg}}. \quad . \quad . \quad . \quad . \quad (5)$$

Making $z=Z$, and multiplying by the coefficient of elasticity b, we obtain for the elasticity of the atmosphere, at the cylindrical surface of the vortex,

$$b\rho_1=b\rho+\frac{w^2\rho}{2g}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5a)$$

which exceeds the mean elasticity $b\rho$ by a quantity equivalent to the weight of a column of the mean density ρ, and of the height due to the velocity w, and independent of the radius of the vortex.

Supposing a spherical layer, therefore, to contain any number of vortices of any diameter, in which the mean density is equal, it is necessary to a permanent condition of that layer that the velocities in all these vortices should be equal, in order that their lateral elasticities may be equal.

Although the mean elasticity at the plane end, or any plane section at right angles to the axis of a vortex, is simply $=b\rho$, being the same as if there were no motion, yet the elasticity is variable from point to point, and the law of variation depends on the velocity. Therefore if two vortices are placed end to end, it is necessary to a stable condition of the fluid, not only that their terminal planes should coincide, and that their mean elasticities should be *in equilibrio*, but also that their velocities should be equal, or subject only to periodical deviations from a state of equality.

Therefore the mean velocity of vortical motion, independent of small periodic variations, is the same throughout the whole atomic atmosphere; and the mean total velocity, independent of small periodic variations, being uniformly distributed also, the *vis viva* of the former may be expressed as a constant fraction of that of the latter, so that

$$w^2=\frac{v^2}{k}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5b)$$

$\frac{1}{k}$ being the mean value of a coefficient which is subject to small periodical variations only*.

This coefficient, being the ratio of the *vis viva* of motion of a peculiar kind to the whole *vis viva* impressed on the atomic atmospheres by the action of their nuclei, may be conjectured to have a specific value for each substance, depending in a manner as yet unknown on some circumstance in the constitution of its atoms. It will afterwards be seen that this circumstance is the chemical constitution.

Let the entire atmosphere of an atom be conceived to be divided into a great number of very acute pyramids meeting at the centre, and having even numbers of faces, equal and opposite in pairs; and let one of these pyramids, intersecting a spherical layer whose distance from the nucleus is $\mathrm{R}u$ and thickness $\mathrm{R}du$, cut out a frustum, containing and surrounded by vortices. Consider one pair of the faces of that frustum; their length being $\mathrm{R}du$, let their breadth be h and their distance asunder f. Then they make with each other the angle at the apex of the pyramid

$$2 \sin^{-1} . \frac{f}{2\mathrm{R}u};$$

their common area is $h\mathrm{R}du$; and the sum of the volumes of the two triangular frusta of the spherical layer, included by diagonal planes drawn between their radial edges, is

$$\frac{fh\mathrm{R}du}{2},$$

the sum of all such triangular frusta being the whole volume of the spherical layer.

The additional pressure due to the centrifugal force of vortices, viz.

$$\frac{v^2\rho}{2gk},$$

* As it has been represented to me, that I have, without stating sufficient grounds, assumed the velocity of revolution w to be constant throughout each individual vortex, I add this note to assign reasons for that supposition.

First. Unless w, the velocity of revolution of a particle, is independent of z, its radius vector, the atomic atmosphere cannot be in a permanent condition.

For if w is a function of z, the external elasticity of a vortex will be a function of its diameter. If the whole atmosphere is in motion, vortices of different diameters must exist in the same spherical layer; and if their external elasticities are different, their condition cannot be permanent.

Second. Whatsoever may be the nature of the forces by which velocity is communicated throughout the atmosphere, the tendency of those forces must be to equalize that velocity, and thus to bring about a permanent condition.

acts on the two lateral faces, its total amount for each being

$$\frac{v^2\rho}{2gk}h\mathrm{R}du.$$

The transverse components of this pair of forces balance each other. Their radial components, amounting to

$$\frac{f}{\mathrm{R}u}\times\frac{v^2\rho}{2gk}h\mathrm{R}du=\frac{v^2\rho fhdu}{2gku},$$

constitute a centrifugal force relatively to the atomic centre, acting on the pair of triangular frusta whose mass is

$$\frac{\rho fh\mathrm{R}du}{2}.$$

The condition of permanent, or periodical equilibrium of this pair of frusta, requires that this centrifugal force shall be balanced by the variation of the mean elasticity of the atmosphere at the two surfaces of the spherical layer, combined with the attraction of the nucleus. The action of the former of these forces is represented by

$$-b\frac{d\rho}{du}du\times\frac{fh}{2}.$$

Let the accelerating force of attraction towards the nucleus be represented by

$$-\frac{\phi(\mathrm{R}u)}{\mathrm{R}},$$

ϕ being a function, which, by the definition of an atomic bounding surface in article 8, is null at that surface, or when $u=1$. Then the attraction on the pair of frusta is

$$-\frac{\rho fhdu\phi(\mathrm{R}u)}{2}.$$

Add these three forces together; let the sum be divided by

$$\frac{1}{2}\rho fhdu,$$

and let the density ρ be denoted, as in article 10, by

$$\frac{\mu}{\mathrm{M}}\mathrm{D}\psi(u);$$

then the following differential equation is obtained, as the condition of a permanent state of the atomic atmosphere:

$$\frac{v^2}{gku}-\frac{b}{\psi(u)}\cdot\frac{d\psi(u)}{du}-\phi=0. \quad . \quad . \quad . \quad (5c)$$

This equation will be realized for each layer at its mean position, on each side of which its radial oscillations are performed.

The variation of this expression being of opposite sign to the variation of $\frac{d\psi(u)}{du}$, shows that any small disturbance of the density produces a force tending to restore that distribution to the state corresponding to the position of equilibrium of the layers, and therefore that the state indicated by equation (5*c*) is *stable.*

(14.) The integral of equation (5*c*) is

$$\psi u = u^{\frac{v^2}{gkb}} e^{a-\frac{1}{b}\int_1^u du\,.\,\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The arbitrary constant a is determined from the equation of condition (2) in the following manner.

Substituting for ψu in equation (2) its value as given above, we obtain

$$1=3\int_0^1 du\left(u^{\frac{v^2}{gkb}+2} e^{a-\frac{1}{b}\int_1^u du\,.\,\phi}\right),$$

or

$$e^{-a}=3\int_0^1 du\left(u^{\frac{v^2}{gkb}+2} e^{-\frac{1}{b}\int_1^u du\,.\,\phi}\right); \quad . \quad . \quad . \quad (7)$$

which integration having been effected, we shall obtain for the value of the superficial elasticity of the atomic atmospheres,

$$p=b\frac{\mu}{\mathrm{M}}\mathrm{D}\psi(1)=b\frac{\mu}{\mathrm{M}}\mathrm{D}e^{a}. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

To obtain an infinite series for approximating to the value of the integral in equation (7), let the following substitutions be made:

$$\left.\begin{array}{l} \log_e u=\lambda \\ \dfrac{v^2}{gkb}+3=3\theta \\ e^{-\frac{1}{b}\int_1^u du\,.\,\phi}=\omega, \\ \text{and let the values of the successive differential} \\ \text{coefficients of } \omega \text{ with respect to } \lambda\text{, when } \lambda=0, \\ \phi=0\text{, and } \omega=1\text{, be denoted by} \\ \qquad (\omega'),\ (\omega''),\ (\omega'''),\ \&\text{c.} \end{array}\right\} \quad . \quad . \quad . \quad (9)$$

Then

$$e^{-a}=3\int_{-\infty}^{0} d\lambda\,.\,e^{3\theta\lambda}\omega.$$

The value of which (when the function ϕ is such as to admit of its having a finite value) is

$$\left.\begin{aligned} e^{-a} &= \frac{1}{\theta}\left(1 - \frac{(\omega')}{3\theta} + \frac{(\omega'')}{9\theta^2} - \frac{(\omega''')}{27\theta^3} + \&c.\right), \\ &\text{whence} \\ \psi(1) = e^{a} &= \theta\left(1 + \frac{(\omega')}{3\theta} + \frac{(\omega')^2 - (\omega'')}{9\theta^2}\right. \\ &\left. + \frac{(\omega')^3 - 2(\omega')(\omega'') + (\omega''')}{27\theta^3} + \&c.\right) \end{aligned}\right\}. \qquad (10)$$

Now because $(\omega') = -\frac{1}{b}\phi_{(u=1)} = 0$,

$$\left.\begin{aligned} \psi(1) = e^{a} &= \theta\left(1 - \frac{(\omega'')}{9\theta^2} + \&c.\right), \\ &\text{which may be represented by} \\ &\left(\frac{v^2}{3gkb} + 1\right)\left(1 - \mathrm{F}(\mathrm{D}, \theta)\right) \end{aligned}\right\}; \quad . \quad . \quad (10\mathrm{A})$$

$\mathrm{F}(\mathrm{D}, \theta)$ being a quantity which becomes continually less as the density becomes less and the heat greater. The complete expression for the elasticity of a gas is therefore, according to equations (1), (8) and (10A),

$$\mathrm{P} = p + f(\mathrm{D}) = \frac{\mu}{\mathrm{M}}\mathrm{D}\left(\frac{v^2}{3gk} + b\right)\left(1 - \mathrm{F}(\mathrm{D}, \theta)\right) + f(\mathrm{D})\,; \quad . \quad (11)$$

when each atom contains a quantity of heat measured by the mechanical power corresponding to the velocity v in the weight M, or

$$q = \frac{\mathrm{M}v^2}{2g},$$

according to equation (4).

Section III. *Of Temperature, and of real Specific Heat.*

(15.) The definition of temperature consists of two parts:—first, the definition of that condition of two portions of matter when they are said to be *at the same temperature*; and second, the definition of the measure of *differences of temperature.*

Two bodies are said to be *at the same temperature,* when there is no tendency for one to become hotter by abstracting heat from the other; that is to say (calling the two bodies A and B), when there is either no tendency to transmission of heat between them, or when A transmits as much heat to B as B does to A. Now it is known by experiment, that any surface or other thing which affects the transmission of heat being placed between B and A, has exactly the same influence upon the same quantity of heat passing in either direction; therefore to produce equilibrium of temperature between A and B, the powers of their atoms to communicate heat must be equal.

(15*a*.) If we apply to vortices at the surface of contact of the atmospheres of two atoms of the same or different kinds, the conditions of permanency laid down in article 13 for vortices in the same atmosphere, these conditions take the following form:—

First. The superficial atomic mean elasticities must be the same; in other words, the superficial atomic mean densities must be inversely as the coefficients of elasticity of the atmospheres. This is the condition of *equilibrium of pressure*.

Second. The law of variation of the elasticity from the centre to the circumference of a vortex, as expressed in equation (5), must be the same for both atoms; and this law depends on the quantity $\frac{w^2}{b}=\frac{v^2}{bk}$; therefore the condition of *equilibrium of heat* is, that the square of the velocity of vortical motion, divided by the coefficient of atmospheric elasticity, shall be the same for each atom. Of this quantity, therefore, and of constants common to all substances, *temperature* must be a function.

Taking the characteristics (A) and (B) to distinguish the quantities proper to the two atoms, we have the following equation:

$$\left.\begin{aligned} b\frac{\mu}{\mathrm{M}}\mathrm{D}\psi(1)(\mathrm{A})&=b\frac{\mu}{\mathrm{M}}\mathrm{D}\psi(1)(\mathrm{B}) \\ \frac{v^2}{kb}(\mathrm{A})&=\frac{v^2}{kb}(\mathrm{B}) \\ \text{temperature}&=\phi\left(\frac{v^2}{kb},\text{ universal constants}\right) \end{aligned}\right\}. \quad (12)$$

(16.) In a *perfect gas*, equation (11) is reduced to

$$\mathrm{P}=p=b\frac{\mu}{\mathrm{M}}\mathrm{D}\left(\frac{v^2}{3gkb}+1\right), \quad . \quad . \quad . \quad (12a)$$

the pressure being simply proportional to the mean elasticity of the atmospheric part of the gas, multiplied by a function of the heat, which as equation (12) shows, is a function of the *temperature*, from its involving only $\frac{v^2}{kb}$ and universal constants.

Therefore in two perfect gases at the same pressure and temperature, the mean elasticities of the atmospheric parts are the same, and consequently—

The mean specific gravities of the atmospheric parts of all perfect gases are inversely proportional to the coefficients of atmospheric elasticity.

Let n therefore represent the number of atoms of a perfect gas, which fill unity of volume under unity of pressure at the temperature of melting ice, so that $n\mathrm{M}$ is the total specific gravity of the gas, and $n\mu$ that of its atmospheric part; then

$$bn\mu=\text{constant for all gases}, \quad . \quad . \quad . \quad (12b)$$

and consequently

$$\frac{v^2}{kb} \propto \frac{n\mu v^2}{k}. \quad \ldots \ldots \ldots \ldots \quad (12c)$$

Therefore

Temperature is a function of universal constants, and of the vortical vis viva *of the atomic atmospheres of so much of the substance as would, in the condition of perfect gas, fill unity of volume under unity of pressure at some standard temperature.*

The equation (12*a*) further shows, that *in any two perfect gases, the respective values of the quotient of the pressure by the density corresponding to the same temperature, bear to each other a constant ratio for all temperatures,* being that of the values of the coefficient $b\frac{\mu}{M}$.

Therefore the pressure of a perfect gas at a given density, or its volume under a given pressure, is the most convenient *measure* of temperature.

Let P_0 represent the elasticity of a perfect gas of the density D at the temperature of melting ice, P that of the same gas at the same density, at a temperature distant T degrees of the thermometric scale from that of melting ice, and C a constant coefficient depending on the scale employed; then the value of T is given by the equation

$$\left.\begin{array}{l} T=C\dfrac{P-P_0}{P_0} \\ \text{or} \\ T+C=C\dfrac{P}{P_0} \end{array}\right\} \quad \ldots \ldots \ldots \quad (13)$$

The value of the constant C is found experimentally as follows:—Let P_1 represent the elasticity of the gas at the temperature of water boiling under the mean atmospheric pressure, T_1 the number of degrees, on the scale adopted, between the freezing- and boiling-points of water; then

$$\left.\begin{array}{l} T_1=C\dfrac{P_1-P_0}{P_0} \\ \text{and} \\ C=T_1\dfrac{P_0}{P_1-P_0} \end{array}\right\} \quad \ldots \ldots \ldots \quad (14)$$

C is in fact the reciprocal of the coefficient of increase of elasticity with temperature, or the reciprocal of the coefficient of dilatation, of a perfect gas at the temperature of melting ice.

(17.) As it is impossible in practice to obtain gases in the theoretical condition referred to, the value of C can only be obtained by approximation. From a comparison of all M. Regnault's best experiments, I have arrived at the following values, which apply to all gaseous bodies.

For the Centigrade scale, $C=274°\cdot6$, being the reciprocal of $0\cdot00364166$.

For Fahrenheit's scale, if adjusted so that 180° are equal to 100° Centigrade,—

C for temperatures measured from the freezing-point of water

$$=494°\cdot28.$$

C for temperatures measured from the ordinary zero

$$=494°\cdot28-32°=462°\cdot28.$$

The point C degrees below the ordinary zero of thermometric scales may be called the *absolute zero of temperature*; for temperatures measured from that point are proportional to the elasticities of a theoretically perfect gas of constant density.

Temperatures so measured may be called *absolute temperatures.* Throughout this paper I shall represent them by the Greek letter τ, so that

$$\tau=T+C. \quad \ldots \ldots \ldots \quad (15)$$

It is to be observed, that the *absolute zero of temperature* is not the *absolute zero of heat.*

(18.) If we now substitute for P in equation (13) its value according to equation (12*a*), we obtain the following result:—

$$\tau=T+C=C\frac{\mu}{M}\cdot\frac{D}{P_0}\left(\frac{v^2}{3gk}+b\right).$$

Let n represent, as before, the theoretical number of atoms in unity of volume, under unity of pressure, at the temperature of melting ice, of the gas in question, supposing the disturbing forces represented by $-F(D, \theta)$ and $f(D)$ to be inappreciable; then nM is the weight of unity of volume under those circumstances, and it is evident that

$$\frac{D}{P_0}=nM.$$

Consequently

$$\tau=T+C=Cn\mu\left(\frac{v^2}{3gk}+b\right), \quad \ldots \ldots \quad (16)$$

being the complete expression for that function of heat called *temperature.*

It follows that the function θ, which enters into the expressions for the elasticity of gases, is given in terms of temperature by the equation

$$\theta=\frac{v^2}{3gkb}+1=\frac{\tau}{Cn\mu b}. \quad \ldots \ldots \quad (16A)$$

If, according to the expression 4, for the quantity of heat in one atom, we substitute $\frac{2gq}{M}$ for v^2 in equation (16), we obtain the following equations:

$$\left.\begin{array}{l} \tau = \mathrm{C}n\mu\left(\dfrac{2q}{3k\mathrm{M}}+b\right) \\ q = \dfrac{\mathrm{M}v^2}{2g} = \dfrac{3k\mathrm{M}}{2}\left(\dfrac{\tau}{\mathrm{C}n\mu}-b\right); \\ \text{and if Q represent the quantity of heat in \textit{unity of weight},} \\ \mathrm{Q} = \dfrac{q}{\mathrm{M}} = \dfrac{v^2}{2g} = \dfrac{3k}{2}\left(\dfrac{\tau}{\mathrm{C}n\mu}-b\right) \end{array}\right\} \quad . \quad . \quad . \quad (17)$$

(19.) The real specific heat of a given substance is found by taking the differential coefficient of the quantity of heat with respect to the temperature. Hence it is expressed in various forms by the following equations, in which the coefficient $\frac{1}{k}$ is supposed not to vary sensibly with the temperature.

$$\left.\begin{array}{l} \text{Real specific heat of one atom,} \\ \dfrac{dq}{d\tau} = \dfrac{3k\mathrm{M}}{2\mathrm{C}n\mu}; \\ \text{real specific heat of unity of weight,} \\ \dfrac{d\mathrm{Q}}{d\tau} = \dfrac{3k}{2\mathrm{C}n\mu}; \\ \text{real specific heat of so much of a perfect gas} \\ \text{as occupies unity of volume under unity of} \\ \text{pressure at the temperature of melting ice,} \\ n\dfrac{dq}{d\tau} = \dfrac{3k\mathrm{M}}{2\mathrm{C}\mu}. \end{array}\right\} \quad . \quad . \quad . \quad . \quad (18)$$

The coefficient $\frac{k\mathrm{M}}{\mu}$, representing the ratio of the total *vis viva* of the motions of the molecular atmospheres to the portion of *vis viva* which produces elasticity, multiplied by the ratio of the total mass of the atom to that of its atmospheric part, is the specific factor in the capacity of an atom for heat. The view which I have stated as probable in article 13,—that the first factor of this coefficient is, like the second, a function of some permanent peculiarity in the nature of the atom,—is confirmed by the laws discovered by Dulong: that the specific heats of all simple atoms bear to each other very simple ratios, and generally that of equality; that the same property is possessed by the specific heats of certain groups of similarly constituted compound atoms; and that the specific heats of equal volumes of all simple gases, at the same temperature and pressure, are equal.

The coefficient $\frac{k\mathrm{M}}{\mu}$ varies in many instances to a great extent for the same substance in the solid, liquid, and gaseous states. So far as experiment has as yet shown, it appears not to vary, or not sensibly to vary, with the temperature; and this I consider probable *à priori,* except at or near the points of fusion of solid substances.

Apparent specific heat differs from real in consequence of the consumption and production of certain quantities of heat by change of volume and of molecular arrangement, which accompany changes of temperature.

This subject belongs to the theory of the mechanical action of heat.

Section IV. *Of the Coefficients of Elasticity and Dilatation of Gases.*

(20.) If in equation (11) we substitute for $\frac{v^2}{3gk}+b$ its value $\frac{\tau}{\mathrm{C}n\mu}$, we obtain the following value for the elasticity of a gas,

$$\mathrm{P}=\frac{\mathrm{D}}{n\mathrm{M}}\cdot\frac{\tau}{\mathrm{C}}\big(1-\mathrm{F}(\mathrm{D},\theta)\big)+f(\mathrm{D}); \quad . \quad . \quad (19)$$

in which $\frac{\mathrm{D}}{n\mathrm{M}}$ denotes the ratio of the actual weight of unity of volume to the weight of unity of volume under unity of pressure, at the absolute temperature C, in the theoretical state of perfect gas;

τ is the absolute temperature;

$-\mathrm{F}(\mathrm{D},\theta)$ is a function of the temperature and density, representing the effect of the attraction of the atomic nucleus or centre in diminishing the superficial elasticity of its atmosphere;

and $f(\mathrm{D})$ is a function of the density only, representing the effect of the mutual attractions and repulsions of the atoms upon the whole elasticity of the body.

From this equation are now to be determined, so far as the experiments of M. Regnault furnish the requisite data, the laws of the deviation of gases from that theoretical state in which the elasticity is proportional to the density multiplied by the absolute temperature.

(21.) The value of $-\mathrm{F}(\mathrm{D},\theta)$ is given by the infinite series of equations (10), (10A), substituting in which for θ its value $\frac{\tau}{\mathrm{C}n\mu b}$, we obtain the following result:

$$-\frac{\tau}{\mathrm{C}}\,\mathrm{F}(\mathrm{D},\theta)=-\frac{\mathrm{A}_1}{\tau}-\frac{\mathrm{A}_2}{\tau^2}-\frac{\mathrm{A}_3}{\tau^3}-\text{\&c.};$$

A_1, A_2, A_3, &c. being a series of functions, the value of which is given by the following equation:

$$-A_m = \frac{C^m n^{1+m} M \mu^m b^{1+m}}{3^{1+m}} \mathfrak{C}_{1+m} ; \quad . \quad . \quad . \quad (20)$$

$\mathfrak{C}_{1+m}$ being the coefficient of x^{1+m} in the development of the reciprocal of the series

$$1-(\omega')x+(\omega'')x^2-(\omega''')x^3+\&c.,$$

when (ω') &c. have the values given in equation (9).

Equation (19) is thus transformed into

$$P = \frac{D}{nM}\left(\frac{\tau}{C} - \frac{A_1}{\tau} - \frac{A_2}{\tau^2} - \&c.\right) + f(D). \quad . \quad (21)$$

The series in terms of the negative powers of the absolute temperature converges so rapidly, that I have found it sufficient, in all the calculations I have hitherto made respecting the elasticity of gaseous bodies, to use the first term only, $-\frac{A}{\tau}$.

(22.) Instead of making any assumption respecting the laws of the attractions and repulsions which determine the functions A and $f(D)$, I have endeavoured to represent those functions by empirical formulæ, deduced respectively from the experiments of M. Regnault on what he terms the *coefficient of dilatation of gases at constant volume*, which ought rather to be called the *coefficient of increase of elasticity with temperature*, and from his experiments on the *compressibility of elastic fluids at constant temperature.*

From the data thus obtained I have calculated, by means of the theory, *the coefficients of dilatation of gases under constant pressure*, which, as a test of the accuracy of the theory, I have compared with those deduced by M. Regnault from experiment.

(23.) The mean coefficient of increase of elasticity with temperature at constant volume between 0° and 100° of the Centigrade thermometer is found by dividing the difference of the elasticities at those two temperatures by the elasticity at 0°, and by 100°, the difference of temperature. It is therefore represented by

$$E = \frac{P_1 - P_0}{100^\circ P_0}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

where E represents the coefficient in question, and P_0 and P_1 the elasticities at 0° and 100° Centigrade respectively.

Now by equation (21), neglecting powers of $\frac{1}{\tau}$ higher than the first, we have

$$P_0 = \frac{D}{nM}\left(1 - \frac{A}{C}\right) + f(D)$$

$$P_1 = \frac{D}{nM}\left(\frac{100^\circ + C}{C} - \frac{A}{100^\circ + C}\right) + f(D),$$

whence

$$E = \frac{D}{nMP_0}\left(\frac{1}{C} + \frac{A}{C(C+100^\circ)}\right). \quad . \quad . \quad . \quad (23)$$

Supposing the value of $\frac{D}{nMP_0}$ to be known, this equation affords the means of calculating the values of the function A corresponding to various densities, from those of the coefficient E as given by experiment.

As a gas is rarefied, $\frac{D}{nMP_0}$ approximates to unity, A diminishes without limit, and the value of E consequently approximates to $\frac{1}{C}$, the reciprocal of the absolute temperature at 0° Centigrade. This conclusion is verified by experiment; and by means of it I have determined the values already given, viz. $C=274°{\cdot}6$ Centigrade, and $\frac{1}{C}=\cdot 00364166$ for the Centigrade scale.

(24.) In order to calculate the values of $\frac{D}{nMP_0}$, I have made use of empirical formulæ, deduced from those given by M. Regnault in his memoir on the Compressibility of Elastic Fluids. In M. Regnault's formulæ, the unit of pressure is one metre of mercury, and the unit of density the actual density corresponding to that pressure. In the formulæ which I am about to state, the unit of pressure is an atmosphere of 760 millimetres of mercury, or 29·922 inches; and the unity of density, the theoretical density in the perfectly gaseous state at 0° Centigrade, under a pressure of one atmosphere, which has been found from M. Regnault's formulæ by making the pressure $=0$ in the value of $\frac{MnP_0}{D}$. M. Regnault's experiments were made at temperatures slightly above the freezing-point, but not sufficiently so to render the formulæ inaccurate for the purpose of calculating the ratio in question, $\frac{D}{nMP_0}$.

The formulæ are as follows:—

Supposing $\frac{D}{Mn}$ given,

$$\frac{CnMP}{\tau D}=1+\alpha\frac{D}{Mn}+\beta\left(\frac{D}{Mn}\right)^2;$$

which, when T is small, or τ nearly $=C$, gives an approximate value of $\frac{nMP_0}{D}$.

Supposing P_0 given,

$$\frac{\tau D}{CnMP}=1+\gamma P_0+\epsilon P_0^2;$$

which, when T is small, gives an approximate value of $\frac{D}{nMP_0}$. . . . (24)

The values of the constants α, β, γ, ϵ, and of their logarithms, are given, together with the mean temperatures above the freezing-point at which M. Regnault's experiments were made, for atmospheric air, carbonic acid gas and hydrogen.

Atmospheric Air. $T=4^\circ{\cdot}75$.

	Constants.	Logarithms.
$\alpha=-\gamma=$	$-{\cdot}000860978$	$\bar{4}{\cdot}9349920$
$\beta=$	$+{\cdot}000011182$	$\bar{5}{\cdot}0485140$
$\epsilon=$	$-{\cdot}000009700$	$\bar{6}{\cdot}9867717$

Carbonic Acid Gas. $T=3^\circ{\cdot}27$.

$\alpha=-\gamma=$	$-{\cdot}00641836$	$\bar{3}{\cdot}8074242$
$\beta=$	$-{\cdot}0000041727$	$\bar{6}{\cdot}6204126$
$\epsilon=$	$+{\cdot}0000865535$	$\bar{5}{\cdot}9372846$

Hydrogen. $T=4^\circ{\cdot}75$.

$\alpha=-\gamma=$	$+{\cdot}000403324$	$\bar{4}{\cdot}6056546$
$\beta=$	$+{\cdot}0000048634$	$\bar{6}{\cdot}6869401$
$\epsilon=$	$-{\cdot}0000044981$	$\bar{6}{\cdot}6530291$

The three substances above-mentioned are the only gases on which experiments have yet been made, under circumstances sufficiently varied to enable me to put the theory to the test I have described in article 22.

(25.) M. Regnault has determined the values of the coefficient of elasticity E for carbonic acid at four different densities, and for atmospheric air at ten. By applying equations (23) and (24) to those data, I have ascertained that the function A for these two gases may be represented empirically, for densities not exceeding that corresponding to five atmospheres, by the formulæ given below, which lead to formulæ for the coefficient E.

For Carbonic Acid,

$$A=a\frac{D}{nM};$$

where $\log a=0{\cdot}3344538$, and consequently

$$E=\frac{D}{nMP_0}\cdot\frac{1}{C}\left(1+\frac{a}{C+100^\circ}\cdot\frac{D}{nM}\right)$$

$$\log\frac{a}{C+100^\circ}=\bar{3}{\cdot}7608860. \quad \quad (25)$$

For Atmospheric Air,

$$A=a\left(\frac{D}{nM}\right)^{\frac{6}{10}};$$

where log a=0·3176168, and consequently

$$E=\frac{D}{nMP_0}\cdot\frac{1}{C}\left(1+\frac{a}{C+100^\circ}\cdot\left(\frac{D}{nM}\right)^{\frac{6}{10}}\right) \qquad (26)$$

$$\log\frac{a}{C+100^\circ}=\bar{3}\cdot7440490$$

The value of $\log\frac{1}{C}$ is

$$\bar{3}\cdot5612995.$$

The following table shows that those empirical formulæ accurately represent the experiments, the greatest differences being less than one-half of ·0000136, which M. Regnault, in the seventy-first page of his memoir, assigns as the limit of the errors of observation due to barometric measurements alone.

As the coefficient E for hydrogen has been determined for one density only, it is impossible to obtain an empirical formula for that gas. The single ascertained value of E is nevertheless inserted in the table.

Table of Coefficients of Increase of Elasticity with Temperature at Constant Volume.

	Pressure at 0° Cent. in atmospheres $=P_0$.	Density $\frac{D}{nM}$.	Coefficient E according to the formulæ.	Coefficient E according to experiment.	Difference between the formulæ and experiment.
		Carbonic Acid.			
I.	0·9980	1·00448	·0036865	·0036856	+·0000009
II.	1·1857	1·19487	·0036951	·0036943	+·0000008
III.	2·2931	2·32788	·0037465	·0037523	−·0000058
IV.	4·7225	4·87475	·0038647	·0038598	+·0000049
		Atmospheric Air.			
I.	0·1444	0·1444	·0036484	·0036482	+·0000002
II.	0·2294	0·2294	·0036507	·0036513	−·0000006
III.	0·3501	0·3502	·0036535	·0036542	−·0000007
IV.	0·4930	0·4932	·0036564	·0036587	−·0000023
V.	0·4937	0·4939	·0036564	·0036572	−·0000008
VI.	1·0000	1·00085	·0036652	·0036650	+·0000002
VII.	2·2084	2·2125	·0036810	·0036760	+·0000050
VIII.	2·2270	2·2312	·0036812	·0036800	+·0000012
IX.	2·8213	2·8279	·0036880	·0036894	−·0000014
X.	4·8100	4·8289	·0037081	·0037091	−·0000010
		Hydrogen.			
	1·0000	0·9996	No formula.	·0036678	

(26.) The empirical formulæ (24), representing the experiments of M. Regnault on the compressibility of carbonic acid gas, atmospheric air, and hydrogen at certain temperatures, give for these temperatures the values of a function which is theoretically expressed by

$$\frac{\mathrm{C}n\mathrm{MP}}{\tau\mathrm{D}} = 1 - \frac{\mathrm{CA}}{\tau^2} + \frac{\mathrm{C}n\mathrm{M}f(\mathrm{D})}{\tau\mathrm{D}}. \quad . \quad . \quad . \quad . \quad (27)$$

It is evident, that supposing the value of $\frac{\mathrm{C}n\mathrm{MP}}{\tau\mathrm{D}}$ for any given density to be known by experiment, and that of A to be calculated from the value of the coefficient E, or from the empirical formulæ (25) and (26), the corresponding value of the function $\frac{n\mathrm{M}f(\mathrm{D})}{\mathrm{D}}$ may be determined by means of equation (27).

By this method I have obtained the following empirical formulæ for calculating the values of that function:—

$$\left.\begin{array}{l} \text{For Carbonic Acid,} \\ \qquad \dfrac{n\mathrm{M}f(\mathrm{D})}{\mathrm{D}} = h\,\dfrac{\mathrm{D}}{n\mathrm{M}}, \\ \text{where } \log h = \bar{3}\cdot1083932. \\ \text{For Atmospheric Air,} \\ \qquad \dfrac{n\mathrm{M}f(\mathrm{D})}{\mathrm{D}} = h\left(\dfrac{\mathrm{D}}{n\mathrm{M}}\right)^{\frac{1}{3}}, \\ \text{where } \log h = \bar{3}\cdot8181545. \end{array}\right\} \quad . \quad . \quad . \quad . \quad (28)$$

As only one value of $\frac{n\mathrm{M}f(\mathrm{D})}{\mathrm{D}}$ for hydrogen can at present be ascertained, it is impossible to determine a formula for that gas. The single value in question is—

$$\text{For } \mathrm{P}_0 = 1 \text{ atmosphere, } \frac{n\mathrm{M}f(\mathrm{D})}{\mathrm{D}} = \cdot01059. \quad . \quad . \quad (29)$$

(27.) I now proceed to determine theoretically, from the data which have already been obtained, the *mean coefficients of dilatation at constant pressure*, between 0° and 100° of the Centigrade scale, for the three gases under consideration, at various pressures.

Let E' represent the coefficient required; S_0 and S_1 the respective values of $\frac{n\mathrm{M}}{\mathrm{D}}$ for 0° and 100° under the pressure P, that is to say, the volumes occupied by the weight $n\mathrm{M}$ at those temperatures; A_0 and A_1, f_0 and f_1, the corresponding values of A and $f(D)$. Then from equation (21) we deduce the following results:—

$$S_0 = \frac{1}{P}\left(1 - \frac{A_0}{C} + S_0 f_0\right)$$

$$S_1 = \frac{1}{P}\left(1 + \frac{100°}{C} - \frac{A_1}{C+100°} + S_1 f_1\right);$$

and consequently

$$E' = \frac{S_1 - S_0}{100° S_0}$$

$$= \frac{1}{S_0 P}\left(\frac{1}{C} + \frac{A_0}{100\,C} - \frac{A_1}{100(C+100)} - \frac{S_0 f_0 - S_1 f_1}{100}\right). \quad (30)$$

In applying the empirical formulæ (25), (26), and (28) to determine the values of A_1 and $S_1 f_1$ in the above equation, it will produce no appreciable error to use $\frac{C}{C+100} D_0$ as an approximate value of D_1, for that purpose only. By making the necessary substitutions, the following formulæ are obtained:—

For Carbonic Acid,

$$E' = \frac{D_0}{nMP}\left(\frac{1}{C} + \alpha . \frac{D_0}{nM}\right),$$

where $\log \alpha = \bar{5}{\cdot}5189349$.

For Atmospheric Air,

$$E' = \frac{D_0}{nMP}\left(\frac{1}{C} + \alpha\left(\frac{D_0}{nM}\right)^{\frac{6}{10}} - \beta . \left(\frac{D_0}{nM}\right)^{\frac{1}{2}}\right),$$

where $\log \alpha = \bar{5}{\cdot}4717265$

$\log \beta = \bar{6}{\cdot}9759738$ . . . . . (31)

(28.) The following table exhibits a comparison between the results of the formulæ and those of M. Regnault's experiments. It is not, like the preceding table (article 25), the verification of empirical formulæ, but is a test of the soundness of the theoretical reasoning from which equations (30) and (31) have been deduced.

It is impossible, from the want of a sufficient number of experimental data, to give a formula similar to (31) for hydrogen. I have calculated, however, the value of the coefficient E′ for that gas, corresponding to the pressure of one atmosphere, on the assumption that at that pressure a formula similar to that for carbonic acid gas is applicable without sensible error.

The table shows only one instance in which the difference between the result of the theory and that of experiment exceeds ·0000136; the limit, according to M. Regnault, of the errors of observation capable of arising from one cause alone,—the uncertainty of barometric measurements. That discrepancy takes place in one of the determinations of the coefficient E′ for carbonic acid gas under the pressure of one atmosphere. In the other determination, the discrepancy is less than the limit.

The agreement between theory and experiment is most close for the highest pressures; and M. Regnault has shown (p. 100) that the higher the pressure the less is the effect of a given error of observation in producing an error in the value of the coefficient.

The theory is therefore successful in calculating the coefficients of dilatation of gases, so far as the means at present exist of putting it to the test.

Table of Coefficients of Dilatation under Constant Pressure, showing a Comparison between Theory and Experiment.

Pressure in atmospheres.	Coefficient E′ according to the theory.	Coefficient E′ according to M. Regnault's experiments.	Difference between theory and experiment.
	Carbonic Acid Gas.		
1·000	·0036988	First Memoir. ·0037099	−·0000111
		Second Memoir. ·003719	−·0000202
3·316	·0038430	First Memoir. ·0038450	−·0000020
	Atmospheric Air.		
1·0000	·0036650	First Memoir. ·0036706	−·0000056
		Second Memoir. ·003663	+·0000020
		·003667	−·0000020
3·3224	·0036955	First Memoir. ·0036944	+·0000011
3·4474	·0036969	·0036965	+·0000004
	Hydrogen.		
1·0000	·0036598	·0036613	−·0000015

Section V. *Of the Elasticity of Vapour in contact with the same Substance in the Liquid or Solid State.*

(29.) As the most important phænomena of evaporation take place from the liquid state, I shall generally use the word *liquid* alone throughout this section in speaking of the condition opposed to the gaseous state; but all the reasonings are equally applicable to those cases in which a substance evaporates from the solid state.

(30.) In considering the state of a limited space, entirely occupied by a portion of a substance in the liquid form, and by another portion of the same substance in the form of vapour, both being at rest, the most obvious condition of equilibrium is, that the *total elasticity* of the substance in each of the two states must be the same, that is to say,

$$P = p_0 + f(D_0) = p_1 + f(D_1), \quad . \quad . \quad . \quad . \quad (32)$$

where p_0 represents the superficial atomic elasticity in the liquid state, p_1 that in the gaseous state, and $f(D_0)$, $f(D_1)$ the corresponding values of the pressures, positive or negative, due to mutual actions of distinct atoms.

(31.) A second condition of equilibrium is, that the superficial elasticities of two contiguous atoms must be equal at their surface of contact. Hence, although there may be an abrupt change of *density* at the bounding surface between the liquid and the vapour, there must be no change of superficial atomic elasticity except by inappreciable degrees; and at that bounding surface, if there is an abrupt change of density (as the reflexion of light renders probable), there must be two densities corresponding to the same superficial atomic elasticity.

(32.) A third condition of equilibrium is to be deduced from the mutual attractions and repulsions of the atoms of liquid and of vapour. In a gas of uniform density, those forces, acting on each individual particle at an appreciable distance from the bounding surface, balance each other, and have accordingly been treated as merely affecting the total elasticity of the body by an amount denoted by $f(D)$; but near the bounding surface of a liquid and its vapour, it is obvious that the action of the liquid upon any atom must be greater than that of the vapour. A force is thus produced which acts on each particle in a line perpendicular to that bounding surface, and which is probably attractive towards the liquid, very intense close to the bounding surface, but inappreciable at all distances from it perceptible to our senses. Such a force can be balanced only by a gradual increase of superficial atomic elasticity in a direction towards the liquid. Hence, although at perceptible distances from the surface of the liquid, the density of vapour is sensibly uniform, the layers close to that surface are probably in a state of condensation by attraction, analogous to that of the earth's atmosphere under the influence of gravity.

Professor Faraday has expressed an opinion, founded on his own experiments and those of MM. Dulong and Thenard, that a state of condensation exactly resembling that which I have described is produced in gases by the superficial attraction of various substances, especially platinum, and gives rise to chemical actions which have been called *catalytic.*

To express this third condition algebraically, let the boundary between the liquid and the vapour be conceived to be a plane of indefinite extent, perpendicular to the axis of x; and let positive distances be measured in a direction from the liquid towards the vapour.

Let x, $x+dx$ represent the positions of two planes, perpendicular to the axis of x, bounding a layer whose thickness dx is very great as compared with the distance between two atomic centres, but very small as compared with any perceptible distance, and let a portion of the layer be considered whose transverse area is unity.

Let ρ represent the mean density of the layer. Then it is acted upon by a force

$$-\rho \mathrm{X} dx,$$

the resultant of the actions of all the neighbouring atoms, which has the negative sign, because it is attractive towards the liquid, X being a function of the position of the layer in question, and of the densities and positions of all the neighbouring layers.

The superficial atomic elasticity behind the layer being p, and in front of it $p+\frac{dp}{dx}dx$, it is also acted on by the force

$$-\frac{dp}{dx}dx;$$

hence its condition of equilibrium is

$$\frac{dp}{dx}+\rho \mathrm{X}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

In order to integrate this equation, so as to give a relation applicable at perceptible distances from the surface, let x_0, x_1 represent the positions of two planes perpendicular to the axis of x, the former situated in the liquid, the latter in the vapour, and so far asunder that the densities beyond them are sensibly uniform, and equal respectively to D_0 for the liquid and D_1 for the vapour, the corresponding superficial atomic elasticities being p_0 and p_1. Then dividing equation (33) by ρ, and integrating between the limits x_0 and x_1, the result obtained is

$$\int_{p_0}^{p_1} \frac{dp}{\rho} = -\int_{x_0}^{x_1} dx \,.\, \mathrm{X}. \quad . \quad . \quad . \quad . \qquad (34)$$

Had we a complete knowledge of the laws of molecular forces in the solid, liquid and gaseous states, this equation, taken in conjunction with the two conditions previously stated, would be sufficient to determine formulæ for calculating the total elasticity, and the respective densities of a liquid and its vapour when in contact in a limited space, at all temperatures.

(33.) In the absence of that knowledge, I have used equation (34), so as to indicate the *form* of an approximate equation suitable for calculating the elasticity of vapour in contact with its liquid, at all ordinary temperatures, the coefficients of which I have determined empirically, for water and mercury, from the experiments of M. Regnault, and for alcohol, æther, turpentine, and petroleum from those of Dr. Ure.

It has been shown (equation 19) that the superficial atomic elasticity is expressible approximately in terms of the density and temperature for gases by

$$p=\rho \cdot \frac{\tau}{\mathrm{C}n\mathrm{M}} \left(1-\mathrm{F}\left(\rho, \frac{\tau}{\mathrm{C}n\mu b}\right)\right),$$

where the function F is a very rapidly converging series, in terms of the negative powers of the absolute temperature, the coefficients being functions of the density. It is probable that a similar formula is applicable to liquids, the series being less convergent.

It follows that the density is expressible approximately in terms of the superficial atomic elasticity by

$$\rho=p\frac{\mathrm{C}n\mathrm{M}}{\tau}\left(1+\Phi\left(p, \frac{\tau}{\mathrm{C}n\mu b}\right)\right),$$

the function Φ being also a converging series in terms of the negative powers of the absolute temperature, and the coefficients being functions of p.

Making this substitution in the first side of equation (34), and abbreviating $\Phi\left(p, \frac{\tau}{\mathrm{C}n\mu b}\right)$ into Φ, we obtain the following result:—

$$\int_{p_0}^{p_1} dp \cdot \frac{1}{\rho} = \frac{\tau}{\mathrm{C}n\mathrm{M}}\int_{p_0}^{p_1} dp \cdot \frac{1}{p(1+\Phi)}$$

$$=\frac{\tau}{\mathrm{C}n\mathrm{M}}\left(\log_e p_1-\log_e p_0-\int_{p_0}^{p_1} dp \cdot \frac{\Phi}{p(1+\Phi)}\right)$$

$$=-\int_{x_0}^{x_1} dx \,.\, \mathrm{X}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (35)$$

from which, making $\log_e p_0 + \int_{p_0}^{p_1} dp \cdot \frac{\Phi}{p(1+\Phi)} = \Psi$ and $CnM \int_{x_0}^{x_1} dx \, . \, X = \Omega$, the following value results for the hyperbolic logarithm of the superficial atomic elasticity of the vapour at sensible distances from the surface of the liquid :—

$$\log_e p_1 = \Psi - \frac{\Omega}{\tau}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

In the cases which occur in practice, the density of the vapour is very small as compared with that of the liquid. Hence it follows, that in such cases the value of Ψ depends chiefly on the superficial atomic elasticity of the liquid, and that of Ω on its density. The density is known to diminish with the temperature, but slowly. The superficial atomic elasticity, according to equation (32), is expressed by

$$p_0 = p_1 + f(D_1) - f(D_0),$$

where p_1 and $f(D_1)$ are obviously small as compared with $f(D_0)$, a function of the density of the liquid, so that the variations of p_0 and of Ψ with the temperature are comparatively slow also.

Therefore when the density of the vapour is small as compared with that of the liquid, the principal variable part of the logarithm of its superficial atomic elasticity, and consequently of its whole pressure, is negative, and inversely proportional to the absolute temperature; and

$$\alpha - \frac{\beta}{\tau}$$

(α and β being constants) may be regarded as the first two terms of an approximate formula for the logarithm of the pressure.

A formula of two terms, similar to this, was proposed about 1828 by Professor Roche. I have not been able to find his memoir, and do not know the nature of the reasoning from which he deduced his formula. It has since been shown, by M. Regnault and others, to be accurate for a limited range of temperature only. The quantity corresponding in it to τ is reckoned from a point determined empirically, and very different from the absolute zero.

Thus far the investigation has been theoretical. The next step is to determine empirically what other terms are requisite in order to approximate to the effect of the function $f(D)$, and of the variation of the functions Ψ and Ω.

The analogy of the formulæ for the dilatation of gases, the obvious convenience in calculation, and the fact that the deviations of the results of the first two terms from those of experiment are greatest at low temperatures, naturally induced me to try in the first place the effect of a third term inversely proportional to the *square* of the absolute temperature, making the entire formula for the logarithm of the pressure of vapour in contact with its liquid

$$\left.\begin{array}{l} \log \mathrm{P} = \alpha - \dfrac{\beta}{\tau} - \dfrac{\gamma}{\tau^2}, \\ \text{and the inverse formula, for calculating the absolute temperature from the pressure,} \\ \dfrac{1}{\tau} = \sqrt{\dfrac{\alpha - \log \mathrm{P}}{\gamma} + \dfrac{\beta^2}{4\gamma^2}} - \dfrac{\beta}{2\gamma}, \end{array}\right\} \quad \ldots\ldots \quad (37)$$

the values of the constants α, β, γ being determined by the ordinary methods from three experimental data for each substance.

(34.) The agreement of those formulæ with the results of experiment proved so remarkable, that, as they are calculated to be practically useful, I thought it my duty not to delay their publication until I should have an opportunity of submitting my theoretical researches to the Royal Society of Edinburgh. I therefore communicated the formulæ to the Edinburgh New Philosophical Journal for July 1849, together with the full details of their comparison, graphic and tabular, with the experiments of M. Regnault upon water and mercury, and with those of Dr. Ure upon alcohol, æther, turpentine and petroleum, but without giving any account of the reasoning by which I had been led to them.

Without repeating those details here, I may state, that the agreement between the results of the formulæ and those of observation is in every case as close as the precision of the experiments renders possible. This is remarkable, especially with respect to the experiments of M. Regnault on the elasticity of steam, which extend throughout a range of temperatures from 30° below zero of the Centigrade scale to 230° above it, and of pressures from $\frac{1}{2200}$th of an atmosphere to 28 atmospheres, and which, from the methods of observation adopted, especially those of measuring temperature, necessarily surpass by far in precision all other experiments of the same kind. From 20° to 230° Cent. the greatest discrepancy between calculation and experiment corresponds to a difference of $\frac{8}{100}$ of a Centigrade degree, and very few of the other differences amount to so much as $\frac{1}{20}$th of a degree. Below 20°, where the pressure varies so slowly with the temperature that its actual value is the proper test of the formula, the greatest discrepancy is $\frac{13}{100}$ths of a millimetre of mercury, or $\frac{1}{200}$th of an inch. If the curves representing the formulæ were laid down on M. Regnault's diagram, they would be scarcely distinguishable from those which he has himself drawn to exhibit the mean results of his experiments.

Annexed is a table of the values of the constants α, log β, log γ, $\frac{\beta}{2\gamma}$, $\frac{\beta^2}{4\gamma^2}$, for the fluids for which they have been calculated. As the existing experiments on mercury, turpentine and petroleum are not sufficiently extensive to indicate any precise value for the coefficient γ (which requires a great range of temperatures to evince its effect), I have used for these fluids, as an approximation, the first two terms of the formula only, $\alpha - \frac{\beta}{\tau}$.

For different measures of pressure, the contact α evidently varies equally with the complement of the logarithm of the unit of pressure.

For different thermometric scales, β varies inversely as the length of a degree, γ inversely as the square of that length, $\frac{\beta}{2\gamma}$ directly as the length of a degree, and $\frac{\beta^2}{4\gamma^2}$ directly as the square of that length.

For all the fluids except water, it will probably be found necessary to correct more or less the values of the constants, when more precise and extensive experiments have been made, especially those for the more volatile æther, and for turpentine, petroleum and mercury, which have all been determined from data embracing but a small range of pressures.

In reducing the constants for the Centigrade scale to those for Fahrenheit's scale, 180° of the latter have been assumed to be equal to 100° of the former. In order that this may be the case, the boiling-point of Fahrenheit's scale must be adjusted under a barometric pressure of 760 millimetres, or 29·922 inches, of mercury, whose temperature is 0° Centigrade.

In the ninth and tenth columns of the table are given the limits on the scales of temperature and pressure between which the formulæ have been compared with experiment. It is almost certain that the formula for the pressure of steam may be employed without material error for a considerable range beyond, and probably also that for the pressure of vapour of alcohol; but none of the formulæ are to be regarded as more than approximations to the exact physical law of the elasticity of vapours, for the determination of which many data are still wanting, that can only be supplied by extensive series of experiments.

Table of the Constants in the Formulæ for the Elasticities of Vapours in Contact with their Liquids.

(1.) Names of the fluids.	(2.) Scale of pressures.	(3.) Scale of temperatures.	(4.) a.	(5.) Log β.	(6.) Log γ.	(7.) $\frac{\beta}{2\gamma}$	(8.) $\frac{\beta^2}{4\gamma^2}$	(9.) Range of temperatures.	(10.) Range of pressures.
Water..................	Millims. of mercury.	Centigrade.	7·831247	3·1851091	5·0827176	·0063294	·00004006	Centigrade. −30° to +230°	Millimetres. 0·35 to 20945
Water..................	Inches of mercury.	Fahrenheit.	6·426421	3·4403816	5·5932626	·0035163	·000012364	Fahrenheit. −22° to +446°	Inches. 0·014 to 824·63
Alcohol, spec. gr. 0·813	Inches of mercury.	Fahrenheit.	6·16620	3·3165220	5·7602709	·0017998	·000003239	+32° to 264°	0·41 to 165·58
Æther, boiling at 105° F.	Inches of mercury.	Fahrenheit.	5·33590	3·2084573	5·5119893	·0024856	·000006178	105° to 210°	30·00 to 163·27
Æther, boiling at 104° F.	Inches of mercury.	Fahrenheit.	5·44580	3·2571312	5·3962460	·0036296	·000013174	34° to 104°	6·20 to 30·00
Turpentine...........	Inches of mercury.	Fahrenheit.	5·98187	3·5380701	....	......	....	304° to 362°	30·00 to 62·24
Petroleum	Inches of mercury.	Fahrenheit.	6·19451	3·5648490	....	......	....	316° to 375°	30·00 to 64·50
Mercury	Millims. of mercury.	Centigrade.	7·5305	3·4685511	....	......	....	Centigrade. 72°·74 to 358°	Millimetres. 0·115 to 760
Mercury...............	Inches of mercury.	Fahrenheit.	6·1259	3·7238236	....	......	....	Fahrenheit. 162°·93 to 676°·4	Inches. ·0046 to 29·92

The following are some additional values of the constant α for steam, corresponding to various units of pressure used in practice :—

Units of pressure.	Values of α.
Atmospheres of 760 millimetres of mercury— =29·922 inches of mercury =14·7 lbs. on the square inch =1·0333 kilog. on the square centim.	4·950433
Atmospheres of 30 inches of mercury— =761·99 millimetres =14·74 lbs. on the square inch =1·036 kilog. on the square centim.	4·949300
Kilogrammes on the square centimetre	4·964658
Kilogrammes on the circular centimetre	4·859748
Pounds avoirdupois on the square inch	6·117662
Pounds avoirdupois on the circular inch	6·012752
Pounds avoirdupois on the square foot	8·276025

All the numerical values of the constants are for common logarithms.

(35.) According to the principles which form the basis of calculation in this section, every substance, in the solid or liquid state, is surrounded by an atmosphere of vapour, adhering to its surface by molecular attraction; and even when the presence of vapour is imperceptible at all visible distances from the body's surface, the elasticity of the strata close to that surface may be considerable, and sufficient to oppose that resistance to being brought into absolute contact, which is well known to be very great in solid bodies, and perceptible even in drops of liquid. It is possible that this may be the only cause which prevents all solid bodies from cohering when brought together.

The action of an atmosphere of vapour, so highly dense and elastic as to operate at visible distances, may assist in producing the *spheroidal state* of liquids.

If the particles of clouds are small vesicles or bubbles (which is doubtful), the vapour within them may, according to these principles, be considerably more dense than that which pervades the external air, and may thus enable them to preserve their shape.

Section VI. *Of Mixtures of Gases and Vapours of different kinds.*

(36.) The principle stated in Section II. article 11, that *the elasticity of the atomic atmosphere is proportional to its density,* might be otherwise expressed by saying, that *the elasticity of any number of portions of atomic atmosphere, compressed into a given space, is equal to the sum of the elasticities which such portions would respectively have, if they occupied the same space separately.*

If the same principle here laid down for portions of atomic atmosphere of any one kind of substance be considered as true also of portions of atomic atmosphere of substances of different kinds mixed, and if it be supposed that when two or more gases are mixed, there is no mutual force exerted between atoms of different kinds, except the elastic pressure of the atomic atmospheres, it will then evidently follow,—

First, that the mixed gases will only be *in equilibrio* when the particles of each of them are diffused throughout the whole space which contains them.

Secondly, that the particles of each gas taken separately will be in the same condition as to density, elasticity, arrangement and mutual action, and also as to gravitation, or any other action of an external body, as if that gas occupied the space alone.

Thirdly, that the joint elasticity of the mixed atomic atmospheres at any given point will be the sum of the elasticities which they would respectively have had at that point, if each gas had occupied the space alone.

Fourthly, that the value of the elasticity, positive or negative, resulting from the attractions and repulsions of separate atoms, will be the sum of the values it would have had if each gas had occupied the space alone. And,

Fifthly, that the total elasticity of the mixed gases will be the sum of the elasticities which each would have had separately in the same space.

If there are any mutual actions between the particles of different gases except the elasticity of the molecular atmospheres, these conclusions will no longer be rigidly true; but they will still be approximately true, if the forces so operating are very small. This is probably the actual condition of mixed gases.

(37.) On applying the same principle to the case of a gas mixed with a vapour in contact with its liquid, it is obvious, that if the attractions and repulsions of the particles of the gas upon those of the vapour are null, or inappreciable, the direct effect of the presence of the gas upon the elasticity assumed by the vapour at a given temperature will also be null, or inappreciable.

The gas, however, may have a slight indirect influence, by compressing the liquid, and consequently increasing its superficial atomic elasticity and its attractive power, on which the functions Ψ and Ω in equation (36) depend. The probable effect of this will be, to make the elasticity of the vapour somewhat less than if no gas were present. There appear to be some indications of such an effect; but they are not sufficient to form a basis for calculation.

Supposing the gas, on the contrary, to exercise an appreciable attraction on the particles of vapour, the elasticity of the latter will be increased. Traces of an effect of this kind are perceptible in M. Regnault's experiments on the vapour of mercury, in which air was present.

(38.) I have already referred to the property ascribed by Professor Faraday to various substances, of attracting, and retaining at their surfaces, layers of gas and vapour in a high state of condensation. Supposing a solid body to acquire, in this manner, a mixed atmosphere, consisting partly of its own vapour and partly of foreign substances, the total elasticity of that atmosphere at any point will be equal, or nearly equal, to the sum of the elasticities which each ingredient would have had separately; and thus solid metals, glass, charcoal, earthy matters, and other substances, may acquire a great power of resisting cohesion, although producing no perceptible vapours of their own at ordinary temperatures.

LXXVII. *An Account of some Experiments upon the Electricity of Flame, and the Electric Currents thereby originated.* By W. Hankel*.

It is known that by the combustion of bodies free electricity is generated; all investigations on the electric deportment of flame have hitherto been limited exclusively to the examination of the free electricity which the flame contains. A deeper consideration of the entire electric deportment in such cases conducted me to the idea, that the electric opposition which different portions of the flame exhibit, not unlike the relation which subsists between a copper and a zinc plate, ought, like the latter, to be capable of developing an electric current. This proves to be the case. I have succeeded in establishing, in a manner which excludes all doubt, that a flame when properly closed in a conducting circuit is the origin of an electric current.

During the experiments three lamps were made use of. The parts of the lamps No. 1 and No. 2, which immediately surrounded the flame, were of the same dimensions; in other respects, however, the lamps were very different. The diameter of the outer brass cylinder which encompassed the wick was 32·5 millims., and the diameter of the inner cylinder was 22·7 millims. Between these two cylinders No. 1 carried a new double wick, while No. 2 held a single wick which had been for some time in use. The chimney of No. 1 was of sheet-brass, that of No. 2 was of sheet-iron; both chimneys were 52 millims. high, and reached about 10 or 11 millims. below the rims of the cylinders from between which the wick protruded. The lamp No. 2

* Abridged from Poggendorff's *Annalen*, vol. lxxxi. p. 213, and communicated by Dr. J. Tyndall.

was provided with a bellows, by means of which air could be forced through five small openings into the space between the outer cylinder which inclosed the wick and the surrounding chimney. The lamp No. 3 was an old one, not capable of giving out any great amount of heat.

For the detection of the current a galvanometer was used, which consisted of a pair of astatic needles, around which were wound 16,454 feet of wire.

The first electric current which I obtained from the flame, was by uniting one end of the wire of the galvanometer with the lamp No. 1; while the other end, to which was attached a piece of platinum foil, was held in an inclined position in the flame above the chimney. The time of oscillation of the needles was 16″, and in the present case the action of the stream excited caused a divergence of 1°. The direction of the current could be altered by a commutator which was introduced into the circuit; and by properly managing this, the swing of the needle could be so increased, that after a few reversions it reached an amplitude of 15° or 16°. In this way the action of weaker currents, such as those of small flames, or of the flames of alcohol and æther, was rendered evident. It was, however, desirable to obtain a greater angle than the above; and for this purpose a pair of needles was chosen, whose magnetism was so nearly balanced that the time of oscillation amounted to 45″. Placing a new wick in the lamp, and raising it so as to obtain the greatest flame possible, a deflection of 20° was obtained. The current passed moreover from the top to the bottom of the flame. Most of the experiments have been made with the last-mentioned pair of needles.

The strength of the electric current depends upon the magnitude of the flame. When a piece of platinum foil 0·23 millim. thick was brought over the lamp so that a space of 8 millims. separated it from the upper rim of the chimney, the flame being made so great that the entire platinum was encompassed by it, an angle of 20° was obtained. The platinum being allowed to remain in the same position, and the lamp being lowered, the needle fell to 16°; and as the flame was made still smaller, the needle descended correspondingly. Although an increase of flame in the present case caused an increase of the deflection from 16° to 20°, it by no means follows that this was due to an increased electric tension between the portion of the flame in contact with the platinum and that in contact with the lamp. The same might be the result of a diminution of the resistance within the flame. If even a decrease of electric tension accompanied the enlargement of the flame, still a more powerful stream would be exhibited if at the same time the diminution of the resistance to conduction were sufficiently great. It will be afterwards shown by experiment that the result last mentioned actually occurs.

But it is not the size of the flame alone which determines the character of the current, it also depends upon the intensity of the combustion. The portions of the lamp No. 2 which surround the wick are exactly of the same dimensions as the corresponding parts of No. 1. In the experiment, however, the wick of No. 2 was a simple one, and had been used for some time. On raising it until a flame of equal magnitude with the former was attained, the needle still showed a weaker current. Even the lamp No. 1 itself, after having burnt for some time, was no longer able to cause a divergence of 20°.

The inclined position, which, as before stated, was given to the platinum foil, is more favourable than the horizontal, for the latter causes a weaker combustion. When the platinum was held at some height above the chimney, and was changed from a horizontal to an inclined position, the angle was increased by the change from 9° to 10°; this is a natural result of the fact, that a greater draft is permitted by the latter position, and thus the power of the flame is increased. The platinum being held high above the chimney in a horizontal position, an angle of 5° was observed; and on inclining it, the angle increased to 6°·8.

As might be expected, the artificial introduction of a current of air also increases the action. This was effected by means of the bellows attached to lamp No. 2. The platinum foil was first held over the chimney, and before the bellows were applied, an angle of 1° was obtained; when the bellows were set in action the needle mounted to 10°·6, and on continuing to blow, rose to 116° (11°·6 ? T.).

The different specimens of alcohol and æther which I applied gave currents of different power. In order to obtain results which might be compared with each other, I poured the fluid into a small platinum crucible and ignited it. With the crucible one end of the wire of the galvanometer was connected, and to the other end a piece of platinum foil was attached which was dipped into the flame in an inclined position, so that the distance of its upper edge from the rim of the crucible was 12·1 millims., while its lower edge was 21·5 millims. distant. The crucible as well as the platinum remained unmoved during the entire series of experiments. The fluid in this case burnt without a wick. On pouring alcohol of the specific gravity 0·850 into the vessel and igniting it, an angle of 0° 8 was observed; this was increased to 1°·1 when alcohol of the specific gravity 0·835 was used, and amounted to 2°·6 with absolute alcohol. The latter angle, however, remained constant only so long as the alcohol remained without boiling; when it boiled, an angle of 5°·5 was obtained;

and this was also the case when æther was added, which boiled during its combustion.

When instead of the platinum crucible an iron vessel of somewhat similar shape was used, on igniting the æther therein contained an angle only half the size of that with the platinum crucible was observed.

When sheet-iron or a plate of zinc was substituted for the platinum foil which was held in the flame, the current was weaker. With iron it was about four-fifths, with zinc about two-thirds of its amount with the platinum. This was equally true, whether the æther burnt in the platinum crucible or in the iron vessel; in the latter case the diminution was proportionate.

To exhibit the current, it is not necessary to introduce metal into the flame; the same can be effected by bringing the moist hand into it, or what is more convenient, a strip of paper saturated with moisture. When, for instance, one end of the wire was connected with the lamp, the other end being held in one of my hands or in my mouth, the current could be developed in the manner described. As was natural to expect, a less angle was observed in the present case.

The magnitude of the resistance offered by the flame, in comparison with the not inconsiderable resistance of the 16,454 feet of exterior wire, is exhibited in the following experiments. The lamp No. 1 alone with the piece of platinum held in the flame above it, gave an angle of 8°·3. The lamp No. 2 with its plate of platinum gave an angle of 3°·8; and the lamp No. 3 with its platinum gave a considerably less angle. When the lamps No. 1 and No. 2 were placed one after the other like a galvanic battery, the angle was 8°·4; and when all three were used in this manner, the angle was only 2°·7. When, however, the lamps 1 and 2 were both connected with one end of the wire of the galvanometer, while the two pieces of platinum foil placed over their flames were connected with the other end, an angle of 10°·3 was obtained; and when all three lamps were used, the angle amounted to 11°·7*.

* It is perhaps worth remarking, that the two arrangements of the lamps here spoken of correspond to two distinct arrangements of the galvanic battery; the first being used when an exterior resistance is to be overcome, the second when the resistance within the cells is to be diminished. Let p be the strength of the current exhibited, e the electro-motive force, n the number of cells, R the resistance within them, r the resistance without them; in the former arrangement p would be expressed by the formula $p=\frac{ne}{nR+r}$, while in the latter arrangement we should have the formula $p=\frac{ne}{R+nr}$. The above experiments therefore show that the resistance of the flame itself is great in comparison to the resistance of the exterior wire.—T.

The investigation of these currents by means of the galvanometer carries along with it the doubt, as to whether the increase of the angle is due to an increase of the electric tension between the different parts of the flame from which the electric fluid is conducted, or to a diminution of the resistance within the flame. It is therefore necessary to separate these quantities. This separation is generally effected by the introduction of other known resistances. To obtain a general notion of the electric tension of the different portions of the flame, I have resorted to a somewhat shorter method, which consists in introducing the current caused by the flame into a circuit in which another current of known value circulates. Heretofore the circuit was composed of the flame, the connecting wires and the galvanometer; into this I now introduced a bit of zinc and copper which dipped into a vessel of water. Let this arrangement of zinc and copper be called an element. By means of the commutator, the current developed by this element might be transmitted in the same direction as the current due to the flame, or in an opposite direction. I will name the side to which the flame-current causes the needle to move the positive side, and the opposite side the negative.

When the lamp No. 1 was so regulated that its flame did not quite reach the top of the chimney, and the platinum foil held at a small distance above the latter in the inclined position already mentioned, before the zinc and copper element was introduced the angle due to the flame-current was not quite 10°. By introducing the element so that its current moved in the same direction as that proceeding from the flame, the angle increased some degrees; when the direction was reversed, the needle went back to 5°. A similar result was observed when the chimney was taken away and the flame increased at pleasure. In the case of lamp No. 2, when the chimney was removed and the bellows set in action, the position of the needle indicated that the direction of the current was the same as before; thus compelling the inference that the electro-motive force of the flame (the electric difference between its top and bottom) is greater than the electro-motive force of the zinc and copper element. When, however, two such elements were introduced, and the current which they originated was directed against that proceeding from the flame, the latter was overcome and the needle moved to the negative side.

The deviations of the needle were different when the chimneys were set upon the lamps. When the lamp No. 1 had its chimney set on, the current of a single element was unable to compete with the flame-current as long as the luminous portion of the flame did not reach the full height of the chimney. When, however, the wick was raised by degrees, and the flame by this means increased, the needle receded more and more towards 0; and by continually increasing the flame, was caused to cross to the negative side. In this case, therefore, the current of the single copper and zinc element overcame that of the flame, although both had to contend with the same amount of resistance. This change in the direction of the needle must therefore be referred to a change in the electro-motive force which originates the current. As the zinc and copper have remained unchanged, the cause of the phænomenon must be sought in a diminution of the electro-motive force of the flame. This diminution is without doubt due to the fact, that the flame when thus increased acts against the inner surface of the chimney so as to originate a number of conducting threads of flame between the chimney and the platinum, which, owing to their shortness and position in the flame, possess a greater power of conduction, but a less tension at their extremities, than that existing between the top and bottom of the flame. That the resistance to conduction in this state of the flame is very much decreased, may be inferred from the magnitude of the negative angle. Even this reversion of the angle could be brought about with a single element when the chimney was removed, and a piece of platinum was pushed sideways into the flame at a greater or less distance above the wick, and then connected with the lamp; for in this case also the electric difference between the respective portions of the flame which encompassed both pieces of platinum was less than the difference between the top and bottom of the flame. When a platinum wire, which was smelted into a thin glass tube, was introduced into the flame, and when the plate of platinum was held a little above it, both being connected with the wires of the galvanometer (the lamp was not in the circuit), an angle of 15° was observed. By directing the current of the zinc and copper element against this, the needle was brought down and set on the negative side. When the flame was diminished until its luminous point reached merely to the summit of the chimney, the flame-current gave an angle of 20°; which increase was certainly due, not to an increase of conductibility, but to a higher tension. The opposition of the zinc and copper element reduced this angle only to 14°.

These experiments furnish a convincing proof that the flame itself is the birth-place of an electric current; for if it merely played the part of a conductor, then an increase in the conductibility of a certain portion of the circuit could not cause the needle to pass from the positive to the negative position.

The experiments prove further, that between the different portions of the flame the powers of conduction, as also the electric tensions, are very different. By plunging two platinum wires into certain portions of the flame, we might even obtain a current which passes, not from top to bottom, as in the cases heretofore described, but from bottom to top. On this point I will remain for the present silent, as my intention at the commencement was to limit myself to the simplest cases of the phænomena.

LXXVIII. *On the Influence of Pressure upon the Freezing of Fluids.* *By* R. Clausius*.

MR. WILLIAM THOMSON has described an experimental investigation, conducted by himself†, and originating in a theoretic view entertained by his brother, James Thomson. The latter had concluded, from the known principle of Carnot, that by an increase of pressure the freezing-point of water must be lowered, which view was completely verified by experiment.

Some time ago I published a theoretic memoir‡, in which the principal part of Carnot's law is retained, but altered in one minor particular. This alteration rendered certain of the conclusions heretofore deduced from the principle impossible, while others remained valid; the latter being those whose correctness or high probability had been demonstrated by experiment. Now as the above conclusion regarding the freezing-point of fluids has also been substantiated experimentally, and thus in a scientific point of view has obtained a greater significance than one would be inclined at first sight to attribute to so small a difference, I feel myself called upon, in behalf of my theory, to show that my alteration of Carnot's principle is in no way opposed to this result§. And applying at the same time the original maxim which I have assumed, a new conclusion is arrived at, which, although practically unimportant, on account of the smallness of the numbers which it embraces, deserves nevertheless theoretic expression.

A lengthened analysis of the subject is not here necessary. The considerations dwelt upon in my former paper regarding the evaporation of a fluid, may be applied almost *verbatim* to the freezing of the same. We have only to conceive the vessel impervious to heat to be filled partly with a solid body and partly with a fluid one, instead of, as in the former case, partly with a

* From Poggendorff's *Annalen*, vol. lxxxi. p. 168.

† Proceedings of the Royal Society of Edinburgh, February 1850; and Phil. Mag., S. 3. vol. xxxvii. p. 123.

‡ Phil. Mag. S. 4. vol. ii. pp. 1, 102.

§ I need hardly mention that I have here no thought of disputing with Mr. Thomson the *priority* of his ingenious application of the principle of Carnot.

fluid and partly with a vaporiform body; and then, instead of permitting a fresh portion of the fluid to evaporate, to allow a portion of it to freeze, &c.

One of the two principal equations deduced therefrom was—

$$r=\mathrm{A}(a+t)\,(s-\sigma)\,\frac{dp}{dt}; \quad . \quad . \quad . \quad . \quad . \qquad \text{(Va)}$$

and this holds good for the freezing also, p and t again denoting the pressure and temperature, and σ the volume of a unit of weight of the fluid, whereas s denotes the volume of a unit of weight of a solid body (instead of vapour, as in the former case), and r the latent heat of the freezing (instead of the evaporation). The latter, however, must be here taken as negative, because by freezing, heat will be *liberated,* and not rendered *latent.* We have therefore—

$$\frac{dt}{dp}=-\frac{\mathrm{A}(a+t)\,(s-\sigma)}{r}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Let the value of $\frac{1}{\mathrm{A}}$, given by Joule in his last investigation* as the most probable result of all his experiments, that is 423·55 (772 English), be here substituted, as also for a the number 273; further, with regard to the water, $t=0$, $r=79$, $\sigma=0{\cdot}001$, and $s=0{\cdot}001087$; and, finally, let p be expressed in atmospheres pressing upon a square metre, instead of in kilogrammes, we then obtain—

$$\frac{dt}{dp}=-0{\cdot}00733,$$

which may be regarded as equal to the value calculated by James Thomson, and corroborated by William Thomson, namely $-0{\cdot}0075$.

The other principal equation deduced from the maxim on the equivalence of heat and work was—

$$\frac{dr}{dt}+c-h=\mathrm{A}\,(s-\sigma)\,\frac{dp}{dt}. \quad . \quad . \quad . \quad . \quad . \qquad \text{(III.)}$$

To apply this to the case of freezing, besides their former meaning, we must regard c and h as two quantities which differ from the specific heat of the fluid and solid body only so far as they express, not the heat which must be imparted to a body when it is simply warmed, but that which is necessary when the pressure varies with the temperature in the manner indicated by equation (I.). This difference cannot be considerable, as Regnault† has found that water, by an additional pressure

* Phil. Trans. of the Royal Society of London for the year 1850, Part I. p. 61.

† *Mém. de l'Acad. de l'Inst. de France,* vol. xxi. Mém. VII.

of 10 atmospheres, does not increase $\frac{1}{30}$th of a degree Cent. in temperature; besides this, as the differences of c and h take place both in the same sense, and hence in the difference $c-h$ are subtracted, we can set with a near approach to accuracy for $c-h$ the difference of both specific heats simply. If the value of $\frac{dp}{dt}$ estimated from (I.) be substituted in (III.), and if the sign of $\frac{dr}{dt}$ be changed like that of r in the former case, we have—

$$\frac{dr}{dt}=c-h+\frac{r}{a+t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From this we must conclude, that when the freezing-point changes, the latent heat must also change; for water is $c=1$, and, according to Person*, $h=0·48$. Hence we have—

$$\frac{dr}{dt}=0·52+0·29=0·81\,;$$

that is to say, when the freezing-point of water is lowered by pressure, the latent heat decreases 0·81 for every degree.

We must not confound this result with that already expressed by Person†. From the circumstance that the specific heat of ice is less than that of water, the latter concluded with great probability, that when the freezing-point, without increasing the pressure, is simply lowered by preserving the fluid perfectly motionless, the latent heat must then be less than at 0°. This decrease may be expressed by the equation—

$$\frac{dr}{dt}=c-h\,;$$

the above equation (2) therefore shows, that when the freezing-point is lowered by pressure, the latent heat, besides the change due to the last-mentioned cause, suffers a still further diminution expressed by the quantity $\frac{r}{a+t}$; this in the case of water is $=0·29$, and it is this which corresponds as equivalent to the exterior work accomplished.

The late observation of Person‡, that ice does not melt completely at a definite temperature, but becomes softer immediately before it reaches the melting-point, I have left unnoticed, as its introduction would merely render the development more difficult, without serving any important end; for the decrease of latent heat which corresponds, as equivalent, to the produced work, must be independent of the little irregularities which may take place during the melting.

* *Comptes Rendus*, vol. xxx. p. 526. † Ibid. vol. xxiii. p. 336.
‡ Ibid. vol. xxx. p. 526.

LXXIX. *Applications of the Principle of Mechanical Effect to the Measurement of Electro-motive Forces, and of Galvanic Resistances, in absolute Units.* *By* Prof. W. Thomson*.

1. IN a short paper "On the Theory of Electro-magnetic Induction," communicated to the British Association in 1848†, I demonstrated that "the amount of mechanical effect continually *lost* or spent in some physical agency (according to Joule, the generation of heat) during the existence of a galvanic current in a given closed wire, is, for a given time, proportional to the square of the strength of the current;" and I showed that Neumann's beautiful analytical expression for the electro-motive force experienced by a linear conductor moving relatively to a magnet of any kind, is, in virtue of this proposition, an immediate consequence of the general principle of mechanical effect. At that time I did not see clearly how the reasoning could be extended to inductive effects produced by a magnet (either of magnetized matter or an electro-magnet) of varying power upon a fixed conductor in its neighbourhood, or to "the induction of a varying current on itself;" but I have recently succeeded in making this extension, and found that the same general principle of mechanical effect is sufficient to enable us to found on a few elementary facts, a complete theory of electro-magnetic or electro-dynamic induction. The present communication, which is necessarily very brief, contains some propositions belonging to that part of the theory which was communicated to the British Association; but it is principally devoted to practical applications with reference to the measurement of electro-motive forces arising from chemical action, and to the system of measurement of "galvanic resistance in absolute units," recently introduced by Weber‡.

2. Prop. I.—*If a current of uniform strength be sustained in a linear conductor, and if an electro-motive force act in this conductor in the same direction as the current, it will produce work at a rate equal to the number measuring the force multiplied by the number measuring the strength of the current.*

3. Let the electro-motive force considered be produced by the motion of a straight conductor of unit-length, held at right angles to the lines of force of a magnetic field of unit-intensity, and carried in a direction perpendicular to its own length and to those lines of force. The velocity of the motion will be numeri-

* Communicated by the Author.

† Report, 1848; Transactions of Sections, p. 9.

‡ "Messungen galvanischer Leitungswiderstände nach einem absoluten Maasse;" von Wilhelm Weber.—Poggendorff's *Annalen*, March 1851, No. 3.

cally equal to the electro-motive force, which will be denoted by F, thus inductively produced, since the unit of electro-motive force adopted by those who have introduced or used absolute units in electro-dynamics is that which would be produced in the same circumstances if the velocity of the motion were unity. If the ends of the moveable conductor be pressed on two fixed conductors, connected with one another either simply by a wire, or through any circuit excited by electro-motive forces, so that a current of strength γ is sustained through it, it will experience an electro-magnetic force in a direction perpendicular to its own length and to the lines of magnetic force in the field across which it is moving, of which the amount will be the product of γ into the intensity of the magnetic force, or, since this is unity, simply to γ*. The motion of the conductor being in that line, the force will be directly opposed to it when the current is in the direction in which it would be if it were produced solely by the electro-motive force we are considering; and therefore, if we regard γ as positive when this is the case, the work done in moving the conductor during any time will be equal to the product of γ into the space through which it is moved, and will therefore in the unit of time be $F\gamma$, since F is numerically equal to the velocity of the motion. But this work produces no other effect than making the electro-motive force act, and therefore the electro-motive force must produce some kind of effect mechanically equivalent to it. Now if an equal electro-motive force were produced in any other way (whether chemically, thermally, or by a common frictional electrical machine) between the same two conductors, connected in the same way, it would produce the same effects. Hence, universally, the mechanical value of the work done in a unit of time by an electro-motive force F, on a circuit through which a current of strength γ is passing, is $F\gamma$.

4. If the algebraic signs of F and γ be different, that is if the electro-motive force act against the direction of the current, the amount of work done by it is negative, or effect is gained by allowing it to act. This is the case with the inductive reaction, by which an electro-magnetic engine at work resists the current by which it is excited, or with the electrolytic resistance experienced in the decomposition of water.

5. The application of the proposition which has just been proved, to chemical and thermal electro-motive forces is of much importance. I hope to make a communication to the Royal Society of Edinburgh before the end of this year, in which, by the application to thermal electro-motive forces, the principles

* This statement virtually expresses the definition of the "strength" of a current, according to the electro-magnetic unit now generally adopted.

explained in a previous communication* "On the Dynamical Theory of Heat," will be extended so as to include a mechanical theory of thermo-electric currents. The application to chemical electro-motive forces leads immediately to the expression for the electro-motive force of a galvanic battery, which was obtained by virtually the same reasoning, in another paper published in this Volume of the Magazine† (p. 429): for if ϵ be the electro-chemical equivalent of one of the substances concerned in the chemical action; if θ be the quantity of heat evolved by as much of the chemical action concerned in producing the current as takes place during the consumption of a unit of mass of this substance; and if J be the mechanical equivalent of the thermal unit, the mechanical value of the chemical action which goes on in a unit of time will be $J\theta\epsilon\gamma$, and this must therefore be equal to $F\gamma$, the work done by the electro-motive force which results. Hence we have

$$F = J\theta\epsilon,$$

which is the expression given in the paper referred to above, for the electro-motive force of a galvanic battery in absolute measure.

6. In applying this formula to the case of Daniell's battery, I used a value for θ derived from experiments made by Mr. Joule, the details of which have not yet been published, but which I believe to have consisted of observations of phænomena depending on the actual working electro-motive forces of the battery. I am now enabled to compare that value of the thermal equivalent, with the results of observations made directly on the heat of combination, by Dr. Andrews‡, who has kindly communicated to me the following *data*:—

(1.) The heat evolved by the combination of one grain of zinc with gaseous oxygen amounts to
1301 units.

(2.) The heat evolved by the combination of the 1·246 grains of oxide thus formed with dilute sulphuric acid amounts to
369 units.

(3.) The heat evolved by the combination of the equivalent quantity, ·9727 of a grain of copper, with oxygen, amounts to
588·6 units.

(4.) The heat evolved by the combination of the 1·221 grains of oxide thus formed, with dilute sulphuric acid, amounts to
293 units.

Hence the thermal equivalent of the whole chemical action which goes on in a Daniell's battery during the consumption of a grain

* March 1851. Published in the Transactions, vol. xx. Part II.

† "On the Mechanical Theory of Electrolysis."

‡ Published in his papers "On the Heat disengaged during the Combination of Bodies with Oxygen and Chlorine" (Phil. Mag. vol. xxxii.), "On the Heat disengaged during Metallic Substitutions" (Phil. Transactions, Part I. for 1848), "On the Heat developed during the Combination of Acids and Bases" (Trans. Royal Irish Academy, vol. xix. Part II.), &c.

of zinc is

$$1301+369-(588{\cdot}6+293), \text{ or } 788{\cdot}4 \quad . \quad . \quad . \quad \text{(I.)}:$$

the thermal equivalent of the part of it which consists of oxidation and deoxidation alone is

$$1301-588{\cdot}6, \text{ or } \qquad 712{\cdot}4 \quad . \quad . \quad . \quad \text{(II.)}$$

The thermal equivalent which I used formerly is

$$769 \quad . \quad . \quad . \quad \text{(III.)}$$

If the opinion expressed by Faraday, in April 1834 (Exper. Researches, 919), with reference to the galvanic batteries then known, that the oxidation alone is concerned in producing the current, and the dissolution of the oxide in acid is electrically inoperative, be true for Daniell's battery, the number (II.) is the thermal equivalent of the electrically effective chemical action. Joule's number (III.) is considerably greater than this, and falls but little short of (I.), the thermal equivalent of the *whole* chemical action that goes on during the consumption of a grain of zinc. If we take successively (I.), (II.), (III.) as the value of θ, and take for ϵ and J the values ·07284 and 44758, which were used in my former paper, we find the following values for the product $J\theta\epsilon$:—

(I.) 2570300, which would be the electro-motive force (in British absolute units) of a single cell of Daniell's battery if the whole chemical action were electrically efficient.

(II.) 2322550, which would be the electro-motive force of a single cell of Daniell's battery if only the oxidation and deoxidation of the metals were electrically efficient.

(III.) 2507100, which is the electro-motive force of a single cell of Daniell's battery, according to Joule's experiments.

7. The thermal equivalent of the whole chemical action in a cell of Smee's battery (zinc and platinized silver in dilute sulphuric acid), or of any battery consisting of zinc and a less oxidizable metal immersed in dilute sulphuric acid, is found by subtracting the quantity of heat that might be obtained by burning in gaseous oxygen the hydrogen that escapes, from the quantity of heat that would be obtained in the formation of the sulphate if the zinc were oxidized in gaseous oxygen instead of by combination with oxygen derived from the decomposition of water. Now the quantity of hydrogen that escapes during the consumption of a grain of zinc is $\frac{1}{32{\cdot}53}$ of a grain (if 32·53, which corresponds to the equivalents used by Dr. Andrews, be taken

as the equivalent of zinc, instead of 32·3 which I used in my former paper). According to Dr. Andrews' experiments, the combination of this with gaseous oxygen would evolve

$$\frac{1}{32 \cdot 53} \times 33808, \text{ or } 1039 \cdot 3 \text{ units of heat.}$$

Hence the thermal equivalent of the whole chemical action corresponding to the consumption of a grain of zinc in Smee's battery is

$$1301 + 369 - 1039 \cdot 3, \text{ or } 630 \cdot 7 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{I.})$$

The equivalent of that part which consists of the oxidation of zinc and the deoxidation of hydrogen is

$$1301 - 1039 \cdot 3, \text{ or } \quad 261 \cdot 7 \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{II.})$$

Hence (I.) if the whole chemical action be efficient in producing the current, the electro-motive force is 2056200.

(II.) If only the oxidation and deoxidation be efficient, the electro-motive force is 853190.

The *external* electro-motive force (or the electro-motive force with which the battery operates on a very long thin wire connecting its plates), according to either hypothesis, would be found by subtracting the "chemical resistance*" due to the evolution of hydrogen at the platinized silver, from the whole electro-motive force: but, on account of the feeble affinity of the platinized surface for oxygen, it is probable that this opposing electro-motive force, if it exist at all, is but very slight.

(III.) The external electro-motive force of a single cell of Smee's battery is, according to Joule's experiments†, ·65 of that of a single cell of Daniell's; and therefore if we take the preceding number (III.), derived from his own experiments, as the true external electro-motive force of a single cell of Daniell's, that of a single cell of Smee's is

1,629,600.

This number is nearly double that which was found for the electro-motive force on the supposition that the oxidation and deoxidation alone are electrically efficient; but it falls considerably short of what was found on the suppositions that the whole chemical action is efficient, and that there is no "chemical resistance."

8. It is to be remarked that the external electro-motive force determined for a single cell of Smee's, according to the preceding principles, by subtracting the "chemical resistance" from the value of $J\theta\epsilon$, is the *permanent working* external electro-motive force. The electro-statical tension, which will determine the

* See foot-note on § 6 of my paper on the Mechanical Theory of Electrolysis.

† Phil. Mag. 1844, xxiv. p. 115, and Dove's Rep. vol. viii. p. 341.

initial working external electro-motive force, depends on the primitive state of the platinized silver plate. It could never be greater than to make the initial working force be

$$J \times 1670 \times \epsilon, \text{ or } 5444500,$$

corresponding to the combination of zinc with gaseous oxygen, and of the oxide with sulphuric acid. It might possibly reach this limit if the platinized surface had been carefully cleaned, and kept in oxygen gas until the instant of immersion, or if it had been used as the positive electrode of an apparatus for decomposing water, immediately before being connected with the zinc plate; and then it could only reach it if the whole chemical action were electrically efficient, and if there were no "chemical resistance" due to the affinity of the platinized surface for oxygen.

9. It is also to be remarked, that the permanent working electro-motive force of a galvanic element, consisting of zinc and a less oxidizable metal immersed in sulphuric acid, can never exceed the number 2056200, derived above from the *full* thermal equivalent for the single cell of Smee's, since the chemical action is identical in all such cases, and the mechanical value of the external effect can never exceed that of the chemical action. In a pair consisting of zinc and tin, the electro-motive force has been found by Poggendorff * to be only about half that of a pair consisting of zinc and copper, and consequently less than half that of a single cell of Smee's. There is therefore an immense loss of mechanical effect in the external working of a galvanic battery composed of such elements; which *must* be compensated by heat produced within the cells. I believe with Joule, that this compensating heat is produced at the surface of the tin in consequence of hydrogen being forced to bubble up from it, instead of the metal itself being allowed to combine with the oxygen of the water in contact with it. A most curious result of this theory of "chemical resistance" is, that in experiments (such as those of Faraday, Exp. Researches, 1027, 1028) in which an electrical current passing through a trough containing dilute sulphuric acid, is made to traverse a diaphragm of an oxidizable metal (zinc or tin), dissolving it on one side and evolving bubbles of hydrogen on the other; part (if not all) of the heat of combination will be evolved, not on the side on which the metal is eaten away, but on the side at which the bubbles of hydrogen appear. It will be very interesting to verify this conclusion, by comparing the quantities of heat evolved in two equal and similar electrolytic cells, in the same circuit, each with zinc for the posi-

* "Berl. Acb. 46, 242," Pogg. *Ann.*, lxx. 60. Dove's Repertorium, vol. viii. p. 341.

tive electrode, and one with zinc, the other with platinum or platinized silver for the positive electrode. The electro-motive force of the latter cell would be sufficient to excite a current through the circuit, but it might be found convenient to add electro-motive force from some other source*.

10. Prop. II. *The resistance of a metallic conductor, in terms of Weber's absolute unit, is equal to the product of the quantity of heat developed in it in a unit of time by a current of unit strength, into the mechanical equivalent of a thermal unit.*

11. If H denote the quantity of heat developed in the conductor in a unit of time, by a current of strength γ, the mechanical value of the whole effect produced in it will, according to the principles established by Joule, be JH. But this effect is produced by the electro-motive force, F, and therefore, by Prop. I., we have

$$JH = F\gamma.$$

Now, according to Ohm's original definition of galvanic resistance, if k denote the resistance of the given conductor, we have

$$\gamma = \frac{F}{k}.$$

If the electro-motive force and the strength of the current be measured in absolute units of the kind explained above, the number k, expressing the resistance in this formula, will express it in terms of the absolute unit introduced by Weber. Using the value $k\gamma$ derived from this, for F, in the preceding equation, we have

$$JH = k\gamma^2.$$

This equation expresses the law of the excitation of heat in the galvanic circuit discovered by Joule; and if we take $\gamma = 1$, it expresses the proposition to be proved.

12. In Mr. Joule's original paper on the heat evolved by metallic conductors of electricity†, experiments are described, in

* An examination of the thermal effects of a current through four equal and similar vessels containing dilute sulphuric acid, and connected by means of electrodes of zinc and platinum, varied according to the four permutations of double zinc, double platinum, zinc-platinum, platinum-zinc, in one circuit, excited by an independent galvanic battery or other electromotor, would throw great light on the theory of chemical electro-motive forces and resistances. Vessels containing electrodes of other metals, such as tin, variously combined, and direct and reverse cells of Daniell's battery, might all be introduced into the same circuit. If the exteriors of all the cells were equal and similar, the excesses of their permanent temperatures above that of an equal and similar cell in the neighbourhood, containing no source of heat within it, would be very nearly proportional to the rates at which heat is developed in them.

† Proceedings of the Royal Society, Dec, 17, 1840; Philosophical Magazine, vol. xix. (Oct. 1841), p. 260.

which the strengths of the currents used are determined in absolute measure, the unit employed being the strength which a current must have to decompose 9 grains of water in an hour of time. But the electro-chemical equivalent of water, according to the system of absolute measurement introduced by Weber, is, in British units, very nearly ·02, and therefore a current of unit strength would decompose 72 grains of water in an hour. Hence Joule's original unit is very exactly $\frac{1}{8}$th of the British electro-magnetic unit for measuring current electricity. By using the formula

$$k=\frac{JH}{\gamma^2},$$

and taking for γ one-eighth the number of Mr. Joule's "degrees of current;" for H the quantity of heat (measured by grains of water raised 1° Cent.) evolved by the current through the conductor experimented on; and for J the value 44758; I have found

$$k=13240000$$

as the absolute resistance of a certain wire used by Mr. Joule for an absolute standard of resistance in the experiments on the heat evolved in electrolysis, described in the second part of the same paper*.

13. The "specific resistance" of a metal referred to unity of volume, may be defined as the absolute resistance of a unit length of a conductor of unit section; and the specific resistance of a metal referred to unity of mass, or simply "the specific resistance of a metal" (since the term, which was introduced by Weber, is, when unqualified, so used by him), is defined as the absolute resistance of a conductor of uniform section, and of unit length and unit weight. Hence, since the resistance of conductors of similar substance are inversely proportional to their sections, and directly proportional to their lengths, we have

$$\sigma_v=\frac{k\omega}{l}\ \dagger$$

$$\sigma=\frac{km}{l^2};$$

* The three experiments from which the number in the text was deduced as a mean result (described in §§ 25, 26, 27 of the paper, Phil. Mag. S. 3. vol. xix. p. 266), lead separately to the following values for the resistance:—

13260000
13360000
13090000;

none of which differs by as much as $\frac{1}{60}$th from the mean given in the text.

† By means of this I have found 4·1 for the specific resistance of copper, according to the statement made in § 24 of Mr. Joule's paper, that

if l be the length, ω the area of the section, and m the mass (or weight) of a conductor, k its absolute resistance, and σ_v and σ the specific resistances of its substance referred respectively to unity of volume and to unity of mass.

14. The absolute resistance of a certain silver wire, and of a column of mercury contained in a spiral glass tube, may be determined from experimental *data* extracted from a paper of Mr. Joule's laid before the French Institute (*Comptes Rendus*, Feb. 9, 1846), and communicated to me by the author. In four experiments on the silver wire, and in four similar experiments on the mercury tube, a current measured by a tangent galvanometer was passed through the conductor, and, in each experiment, the quantity of heat evolved during ten minutes was determined by the elevation of temperature produced in a measured mass of water, the temperature of the conductors during all the experiments having been nearly 50° Fahr. The mean result of the four experiments on each conductor is expressed in terms of the square root of the sum of the squares of the tangents of the galvanometer-deflections, and the mean quantity of heat evolved in ten minutes. The weight of the silver wire in air and in water, the weight of the mercury contained in the glass tube, and the exact length of each conductor, were determined a short time ago, at my request, by Mr. Joule, and the areas of the sections of the conductors have been deduced. The same galvanometer having been used as was employed in the experiments on electrolysis, referred to in the "Note on Electro-chemical Equivalents," contained in this Volume of the Magazine (p. 429), and the experiments at present referred to having also been made at Manchester in 1845, the strength of the current in absolute measure is found by multiplying the tangent of deflection by ·28186. The various experimental data thus obtained are as follows:—

his standard conductor was "10 feet long and ·024 of an inch thick;" but there must be some mistake here, as it will be seen below that this is about double what we might expect it to be. I have found 2·17 for the specific resistance of copper referred to unity of volume, according to the experiment described in § 9, on a wire stated to be 2 yards long and $\frac{1}{28}$th of an inch thick; and 1·78 and 1·98, according to the experiments described in §§ 9 and 11, on a wire stated to be 2 yards long and $\frac{1}{50}$th of an inch thick; also 7·7 for that of iron, in a wire stated (§ 11) to be two yards long and $\frac{1}{27}$th of an inch thick. It is to be remarked, however, that no attempt was made by Mr. Joule to determine the sections of his wires with accuracy, and that the "thicknesses" are merely mentioned in round numbers, as descriptive of the kinds of wire used in his different experiments.

Conductor.	Length in feet.	Mass in grains.	Sectional area in square feet.	Mean corrected tangent of deflection.	Mean strength of current in absolute units.	Mean quantity of heat produced in 10 minutes.
Silver wire..	$27\frac{3}{8}$	434·51	·0000034462	1·4526	·40943	19375 grs. of water raised 1°·7718 C. in temperature.
Mercury in glass tube	$5\frac{1}{8}$	1511·5	·000048119	The resistance of the mercury conductor was found to be ·74964 of that of the silver wire.		

Taking as the thermal unit the quantity of heat required to raise the temperature of a grain of water by 1° Cent., we find 57·213 as the heat generated in the silver wire in one second, of which the mechanical equivalent is $44758 \times 57{\cdot}213$. Dividing this by the square of the strength of the current, we find

15276000

for the absolute resistance of the silver wire; and by multiplying by ·74964, we deduce

11451000

for the absolute resistance of the mercury conductor. Multiplying each absolute resistance by the sectional area of the conductor to which it corresponds, and dividing by the length; and again, multiplying each resistance by the mass, and dividing by the square of the length, we obtain the following results with reference to the specific resistances of silver and mercury at about 10° Cent. of temperature.

Metal.	Specific resistance referred to units of volume.	"Specific resistance."
Silver	1·9028	8671500
Mercury	119·13	648410000

15. The "conducting powers" of metals, as ordinarily defined, are inversely proportional to their specific resistances referred to unity of volume. Hence, according to the preceding results, the conducting powers of silver and mercury at about 10° Cent. of temperature are in the proportion of 1 to ·01744. According to the experiments of M. E. Becquerel (Dove's *Repertorium*, vol. viii. p. 193), the conducting powers of silver and mercury at 0° Cent. are in the proportion of 1 to ·017387; and at 100° Cent., of 1 to ·022083; at 10° Cent. they must therefore be nearly in the proportion of 1 to ·01786, which agrees very closely with the preceding comparative result. Again, according to M. Becquerel's experiments, the conducting powers of silver and copper are,—

at 0° in the proportion of 1 to ·91517,
at 100° ... 1 to ·91030,
and therefore
at 10° ... 1 to ·915.

Hence the specific resistance of copper at about 10° Cent. referred to unity of volume, may be found by dividing that of silver by ·915; and from the preceding result, it is thus found to be 2·080. Multiplying this by 3810500, the weight in grains of a cubic foot of copper, (found by taking 8·72 as the specific gravity of copper,) we obtain for the "specific resistance" of copper the value 7925800.

16. Weber, in first introducing the measurement of resistances in absolute units, gave two experimental methods, both founded virtually on a comparison of the electromotive forces with the strengths of the currents produced by them, in the conductors examined; and he actually applied them to various conductors, and obtained results which, reduced to British units, are shown in the following table. The first four numbers in the second column are deduced from M. Weber's results, on the hypothesis that the specific gravity of each specimen of copper is 8·72. The only numbers given on the authority of M. Weber are the first four of the column headed "Specific resistance." The specific resistances derived above from Mr. Joule's experiments are shown in the same table for the sake of comparison.

Quality of metal, &c.	Specific resistance referred to unity of volume.	"Specific resistance."
No. 1. Jacobi's copper wire	2·851	10870000
No. 2. Kirchhoff's copper wire	2·365	9225000
No. 3. Weber's copper wire	2·303	8778000
No. 4. Wire of electrolytically precipitated copper	2·079	7924000
No. 5. Copper at about 10° Cent., according to Joule and Becquerel......	2·080	7926000
No. 6. Joule's silver wire at about 10° C...	1·903	8671000
No. 7. Mercury at about 10° Cent.	119·1	648400000

The great discrepancies among the first four numbers of the third column, each of which is probably correct in three of its significant figures, show how very much the specific resistances of the substance of different specimens of copper wire may differ from one another. The specific resistance of copper (No. 5), deduced indirectly from Joule's absolute by means of Becquerel's relative determinations, agrees very closely with that of the electrolytically precipitated copper (No. 4) experimented on by Weber.

17. It is very much to be desired that Weber's direct process, and the indirect method founded on estimating, according to Joule's principles, the mechanical value of the thermal effects of a galvanic current, should be both put in practice to determine the absolute resistance of the same conductor, or that the resistance of two conductors to which the two methods have been separately applied, should be accurately compared. Such an investigation could scarcely be expected to give a more approximate value of the mechanical equivalent of a thermal unit than has been already found by means of experiments on the friction of fluids; but it would afford a most interesting illustration of those principles by which Mr. Joule has shown how to trace an equivalence between work spent and mechanical effect produced, in all physical agencies in which heat is concerned.

Glasgow College,
Nov. 19, 1851.

LXXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 500.]

"On the Mean Temperature of the Observatory at Highfield House, near Nottingham, from the year 1810 to 1850." By Edward Joseph Lowe, Esq., F.R.A.S. Communicated by Marshall Hall, M.D., F.R.S.

The object of the author in this communication is to connect the series of thermometrical observations made by the late Matthew Needham, Esq., at Lenton House, at the distance of only 200 yards from the observatory of Highfield House, with those made by himself from 1842 to the present time at the latter place. He procured Mr. Needham's observations from the Committee of the Bromley House Library, Nottingham, and also the instrument with which they were made, and which, upon comparison with his own standard, was found by Mr. Glaisher to be correct.

Mr. Needham's observations were registered at 8 A.M. and 11 P.M., and to the monthly means of these records corrections have been applied to convert them into mean monthly values. Those made by the author were registered at 9 A.M. and 9 P.M., and these, together with the highest and lowest readings of self-registering thermometers, have been subjected to the same process.

The following tables deduced from the observations are given in the paper:—

1. The mean temperature of each month at Highfield House from 1810 to 1850.

From this table are deduced the mean temperature of each month from all the observations, viz.

January 36°·2; February 38°·9; March 42°·4; April 47°·6; May

53°·6; June 58°·7; July 61°·1; August 60°·2; September 56°·6; October 50°·0; November 42°·9; December 39°·1.

2. The highest and lowest monthly mean temperature in every year, from 1810 to 1850, with the amount of difference of temperature.

From this table it appears that the coldest month in the year has occurred in January 22 times; in February 10 times; in March once; and in December 8 times.

The hottest month in the year has occurred in June 5 times; in July 26 times; in August 12 times; and in September once.

The coldest month in the whole period occurred in January 1814, the mean temperature being 26°·8.

The hottest month during the whole period occurred in July 1847, the mean temperature being 68°·8.

The means of all the differences between the hottest and coldest month in every year is 27°·2: the least difference occurred in 1828, viz. 21°·3; the greatest difference in 1814, viz. 35°·0.

3. The excess of the monthly mean temperature in every year, above the temperature of the month from all the years.

The means of the numbers in each column of this table, taken without regard to sign, gives the variability of the temperature in spring 2°·1; in summer 1°·7; in autumn 2°·0; in winter 3°·0.

The greatest difference in the monthly means in spring is 11°·9; in summer 12°·5; in autumn 13°·9; in winter 18°·3.

The coldest year in this series was 1814, when the mean annual temperature was only 45°·0; the hottest year was 1846, the mean annual temperature being 51°·4.

4. The mean temperature in every month in successive groups of 10 years, and for the whole year.

5. The mean temperature in quarterly periods in successive groups of 10 years.

From this it is stated that the mean temperature of the 1st quarter is 39°·5; of the 2nd, 53°·3; of the 3rd, 59°·3; of the 4th, 44°·0.

6. The mean temperature in spring, summer, autumn and winter, in successive groups of 10 years.

From this it is concluded that the mean temperature of spring is 47°·8; of summer 60°·0; of autumn 49°·9; of winter 38°1.

Cold springs occurred in 1810, 12, 14, 16, 17, 37, 38, 39, 42, 45, 49 and 50; and the mean of the temperatures of these springs is 45°·5. The coldest spring was that of 1837, the mean temperature being only 43°·3.

Hot springs occurred in 1811, 15, 19, 22, 23, 27, 28, 30, 31 and 41; and the mean of the temperatures of these springs is 50°·4: the hottest spring was that of 1841, the mean temperature being as high as 51°·4.

Cold summers occurred in 1816, 17, 21, 23, 41, 43 and 49; and the mean of the temperatures of these summers is 58°·0. The coldest summer was that of 1816, the mean temperature being only 57°·3.

Hot summers occurred in the years 1818, 24, 26, 31, 46 and 47;

and the mean of the temperatures of these summers is 64°·0. The hottest summer was that of 1846, the mean temperature being as high as 65°·0.

Cold autumns occurred in the years 1814, 16, 20, 29, 36, 37, 38, 42, 44, 45, 49 and 50; and the mean of the temperatures of these autumns is 47°·8. The coldest autumn was that of 1849, the mean temperature being only 47°·0.

Hot autumns occurred in the years 1810, 11, 18, 21, 27, 28, 40 and 46; and the mean of the temperatures of these autumns is 52°·3. The hottest autumn was that of 1818, the mean temperature being as high as 54°·5.

Cold winters occurred in 1814, 16, 20, 23, 30, 38, 41, 45 and 47; and the mean of the temperatures of these winters is 34°·4. The coldest winter was that of 1814, the mean temperature being only 32°·7.

Hot winters occurred in 1822, 24, 28, 34, 35, 46, 48 and 49; and the mean of the temperatures of these winters is 41°·5. The hottest winter was that of 1834, the mean temperature being 43°·3.

"On Depressions of the Wet-bulb Thermometer during the Hot Season at Ahmednuggur, in the Deccan." By Colonel Sykes, F.R.S. &c.

The author states that he is indebted to Major William Coghlan for the tables of hourly depressions of the wet-bulb thermometer during the months of March and April of the present year, which form the subject of this communication, and which are a necessary supplement to his paper recently published in the Philosophical Transactions. The observations at Ahmednuggur, lat. 19° 05′ 49″ N., long. 74° 48′ 10″, elevation above the sea 1911 feet, which were undertaken by Dr. Forbes Watson, commenced on the 18th of March, and were continued to the 14th of April inclusive. They were made hourly from 6 A.M. to 9 P.M., giving 16 hourly records daily; but on the 24th and 29th of March, and on the 4th, 8th and 10th of April, they were continued throughout the twenty-four hours. The instruments employed were a dry- and a wet-bulb thermometer, by Adie, perfectly alike and giving precisely the same indications when both were dry, and a self-registering thermometer. They were suspended on a platform attached to a window under the verandah of the house, with a N.W. exposure, and were protected from radiation and reflexion of heat from the ground. As, from some preliminary observations, it appeared that the depression of the wet-bulb varied in every case with the intensity and duration of the draught of air upon it, in each observation a slight current of air was produced by a fan near the mouth of a funnel, the small end of which abutted on the wet-bulb, and the operation was continued until no further depression of the thermometer could thus be produced; a stronger current of air was then forced on the bulb by means of a large double bellows; and the result of each operation was recorded.

To obviate the anomalies which might arise from single observations, and to fix a mean state, for each hour, of the temperature of the air, the temperature of evaporation, and the mean depression of the wet-bulb, the means of these elements have been taken and are presented in a table. In this table are also given the dew-points as determined by means of Mr. Glaisher's factors and by Dr. Apjohn's formula, with the differences by the two methods. The author remarks that the first feature which presents itself, in running the eye over this table, is the enormous amount of the depression of the wet-bulb compared with our European experience. In March, the mean depression at no hour was less than 14°·8 at 7 A.M., increasing to 29°·6 at 3 P.M.; in April, the mean depression was never less than 17°·3 at 7 A.M., increasing to 29°·9 at 3 P.M.; and many observations necessarily much exceeded the maxima means. The next feature is the increase of the mean depression with that of the mean temperature, from 6 A.M. until 3 P.M., and then a decline with the decline of temperature until 9 P.M.; but not in the same ratio as the increase in the morning. With reference to the practical application of these observations with a view to determine the amount of moisture in the atmosphere, or to fix the dew-point, the author remarks that it will be seen from this table that Mr. Glaisher's factors give a higher dew-point than Dr. Apjohn's formula, varying in March from 6°·1 at 8 A.M. to 11°·9 at 6 P.M., and in April from 5°·6 at 7 A.M. to 10°·4 at 9 P.M.; and these varying discrepancies do not appear to have gradations of increment or decrement dependent upon increase or diminution of mean temperature, or increase or decrease of the depression of the wet-bulb. These remarks apply to the means of the observations; but with reference to isolated observations, the discrepancies by the two methods become much greater. On the 9th of April, at 8 P.M., the temperature of the air being 97°, the wet-bulb with a moderate draught 60°·5, and with a strong draught 60°, the depressions were respectively 36°·5 and 37°, and the dew-point for the latter depression determined by Mr. Glaisher's factors would be 41·5, and 12·6 by Dr. Apjohn's formula. In illustration of this part of the subject the author gives an extract of a letter from General Cullen, from which it appears that at Cochin on the Malabar coast, the temperature of the air being 96°, the wet-bulb 61°, the dew-point by Jones's hygrometer 38°, the dew-point by Mr. Glaisher's factors would be 43°·5, and by Dr. Apjohn's formula 22°·1.

"On a General Law of Density in saturated Vapours." By J. J. Waterston, Esq. Communicated by Lieut.-Colonel Sabine, R.A., V.P. and Treas. R.S. &c.

The author of this paper commences by stating that the relation between the pressure and temperature of vapours in contact with their generating liquids has been expressed by a variety of empirical formulæ, which, although convenient for practical purposes, do not claim to represent any general law; and that some years ago, while examining a mathematical theory of gases, he endeavoured to find out, from the experiments of the French Academy, whether the density of steam in contact with water followed any distinct law with reference to the temperature measured from the zero of gaseous tension (situated at −461° Fahr. by Rudberg's experiments, confirmed by Magnus and Regnault). To avoid circumlocution, he calls temperatures measured from this zero G temperatures, and observes that if t represents the G temperature, Δ the density of a gas or vapour, and p its elastic force, the equation

$$t\Delta=p$$

represents the well-known laws of Marriotte and of Dalton and Gay-Lussac. He then states that, as the function which expresses a general relation between p and t, in vapours, must include a more simple function expressing a general relation between Δ and t, the proper course seemed to be to tabulate the quotients $\frac{p}{t}$ from the experiments of the Academy, and to project them in a curve. For reasons connected with the *vis viva* theory of gases, which represents the G temperature as a square quantity, he projected these quotients or densities as ordinates, to the square root of the G temperatures as abscissæ; and found that the curve traced out was of the parabolic kind, but of a high order. Considering the density as a cubic quantity, the cube roots of the densities were set off as ordinates to the same abscissæ, and the author was gratified to find that the resulting curve was the Conic Parabola. To ascertain whether this was accurately the case, the square roots of these ordinates, corresponding to the sixth roots of the densities, were set off to the same abscissæ, that is the square roots of the G temperatures. The result is shown in a chart, in which, as the author observes, the points determined from the observations range with great precision in a straight line, any slight divergence being sometimes to the right and sometimes to the left; precisely as might be expected from small errors of observation. Other series of experiments on steam were projected in a similar manner, and it was found that, although no two exactly agreed with each other, each set ranged in a straight line nearly. The vapours of ether, alcohol and sulphuret of carbon, were found to conform to the same law, as were likewise M. Avogadro's observations on the vapour of mercury, and Faraday's experiments on liquified gases (Phil. Trans. 1845). Of these last olefiant gas is remarkably in accordance with the law, as are nitrous oxide, ammonia, cyanogen, sulphurous acid, and carbonic acid at the upper part of its range; but muriatic acid, sulphuretted and arseniuretted hydrogen, do not show the same regularity.

The co-ordinates of the points being the square root of the G temperatures and the sixth root of the densities, the equation to the straight line which passes through the points expresses the sixth root of the density in terms of the G temperature. The constants to be determined in this equation are the inclination of the straight line to the axis of x or that on which $\sqrt{t}$ is measured, and the distance from the origin at which it cuts this axis, calling the cotangent of this angle h, and this distance g, Δ_1, Δ_2 densities at G temperatures t_1, t_2,

$$h=\frac{\sqrt{t_2}-\sqrt{t_1}}{\sqrt[6]{\Delta_2}-\sqrt[6]{\Delta_1}} \text{ and } g=\sqrt{t_1}-h\sqrt[6]{\Delta_1}.$$

The constants g and h being thus determined from two observations, the equation for the density at any other G temperature is

$$\Delta=\left\{\frac{\sqrt{t}-g}{h}\right\}^6;$$

and for the pressure

$$p=\left\{\frac{\sqrt{t}-g}{h}\right\}^6 t.$$

The several equations, with the numerical values of the constants g and h, for the series of observations previously referred to and represented on the chart, are then given, the G temperatures being in degrees of Fahrenheit's scale, and the values of h being calculated so as to give the pressure in inches of mercury.

The author remarks that the observations on the vapour of water below 80° show a small excess of density above what is required by the line corresponding to those at higher temperatures; and that it is a curious circumstance that the law of expansibility of water also becomes disturbed at about the same temperature. In proof of this, the observations of M. Despretz (Ann. de Chim. vol. lxx.) being projected, by making the volume the ordinate to the square root of the G temperature as abscissa, these observations above 25° C. or 77° F. give in the most exact manner a conic parabola; but below 77° they no longer give that curve.

The equation to the parabola for temperatures above 77° F. is $a(v-\theta)=(\sqrt{t}-\phi)^2$, in which v is the volume at the G temperature t, in terms of its volume unity at 39°·2 F. or 4° C. (its point of maximum density), $a=352{\cdot}38$, $\theta={\cdot}99872$, and $\phi=21{\cdot}977$ or $\phi^2=483°$.

The law of the increase of density and temperature in saturated vapours having a certain analogy with the law of increase of density and temperature in air while suddenly compressed or dilated, the author next discusses the latter subject in a manner similar to that in which he had discussed the former. From this discussion he draws the following conclusions:—

1. When air is compressed or dilated, the G temperature varies as the cube root of the density; and the tension as the 4th power of the G temperature, or the cube root of the 4th power of the density.

2. The mechanical force exerted by a given quantity of air while expanding from one density to another, is proportional to the difference of the cube roots of these densities, or to the difference of their G temperatures: hence the fall of temperature is proportional to the force expended.

3. The mechanical force exerted upon a given quantity of air, while compressing it from one density to another, is proportional to the difference of the cube roots of these densities, or to the difference of their G temperatures: hence the rise of temperature is proportional to the force exerted.

4. The total mechanical force exerted by a volume of air of a given tension, while expanding indefinitely, is equal to that tension acting through three times the volume.

5. The total mechanical force exerted by a volume of air while expanding indefinitely is proportional to its G temperature.

6. A given quantity of air while expanding, under a constant pressure, from one temperature to another, exerts a mechanical force equivalent to one-third the difference of temperature; and the quantity of heat required to change the temperature of air under a constant pressure, is four-thirds of that required to effect the same change of temperature with a constant volume.

The author concludes by observing that it is singular that these simple and, he considers, important deductions from MM. Gay-Lussac and Welter's experiments, have been overlooked by the eminent mathematicians who have elaborately discussed this subject. The artificial position of the zero-point on the ordinary scales of temperature may perhaps account for this by its tendency to confine our ideas. Dalton's and Gay-Lussac's law of expansion seems imperatively to have required that, in all computations having reference to gases and vapours, the temperature should have been reckoned from the zero of gaseous tension; yet it has not been so; and it is impossible to avoid the conclusion, that if it had been otherwise, if no other temperature but what we have had so often to refer to as the G temperature had been indicated in their analyses, we should have profited more by their labours, and been further advanced in the science of heat and elastic fluids.

LXXXI. *Intelligence and Miscellaneous Articles.*

ON THE HYPOTHESES RELATING TO THE LUMINOUS ÆTHER, AND AN EXPERIMENT WHICH APPEARS TO DEMONSTRATE THAT THE MOTION OF BODIES ALTERS THE VELOCITY WITH WHICH LIGHT PROPAGATES ITSELF IN THEIR INTERIOR. BY M. H. FIZEAU.

MANY hypotheses have been proposed to account for the phænomena of aberration in accordance with the doctrine of undulations. Fresnel in the first instance, and more recently Doppler, Stokes, Challis, and many others, have published memoirs on this important subject; but it does not seem that any of the theories proposed have received the entire assent of physicists. In fact, the want of any definite ideas as to the properties of the luminous æther and its relations to ponderable matter, has rendered it necessary to form hypotheses, and among those which have been proposed there are some which are more or less probable, but none which can be considered as proved.

These hypotheses may be reduced to three principal ones. They refer to the state in which the æther existing in the interior of transparent bodies may be considered to be.

This æther is either adherent, and as it were attached to the molecules of bodies, and consequently participates in the motions to which the bodies may be subjected;

Or the æther is free and independent, and is not influenced by the motion of the bodies;

Or, lastly, according to a third hypothesis, which includes both the former ones, only a portion of the æther is free, the other portion being attached to the molecules of bodies and participating in their motion.

This latter hypothesis was proposed by Fresnel, and constructed for the purpose of equally satisfying the phænomena of aberration, and a celebrated experiment of M. Arago, by which it has been proved that the motion of the earth has no influence upon the refraction which the light of the stars suffers in a prism.

We may determine the value which in each of these hypotheses it is necessary to attribute to the velocity of light in bodies when the bodies are supposed to be in motion.

If the æther is supposed to be wholly carried along with the body in motion, the velocity of light ought to be increased by the whole velocity of the body, the ray being supposed to have the same direction as the motion.

If the æther is supposed to be free and independent, the velocity of light ought not to be changed at all.

Lastly, if only one part of the æther is carried along, the velocity of light would be increased, but only by a fraction of the velocity of the body, and not, as in the first hypothesis, by the whole velocity. This consequence is not so obvious as the former, but Fresnel has shown that it may be supported by mechanical arguments of great probability.

Although the velocity of light is enormous comparatively to such as we are able to impart to bodies, we are at the present time in possession of means of observation of such extreme delicacy, that it seems to me to be possible to determine by a direct experiment what is the real influence of the motion of bodies upon the velocity of light.

We are indebted to M. Arago for a method based upon the phænomena of interference, which is capable of indicating the most minute variations in the indexes of refraction of bodies. The experiments of MM. Arago and Fresnel upon the difference between the refractions of dry and moist air, have proved the extraordinary sensibility of that means of observation.

It is by adopting the same principle, and joining the double tube of M. Arago to the conjugate telescopes which I employed for determining the absolute velocity of light, that I have been able to study directly in two mediums the effects of the motion of a body upon the light which traverses it.

I will now attempt to describe, without the aid of a diagram, what was the course of the light in the experiment. From the focus of a cylindrical lens the solar rays penetrated almost immediately into the first telescope by a lateral opening very near to its focus. A transparent mirror, the plane of which made an angle of 45° with the axis of the telescope, reflected the rays in the direction of the object-glass.

On leaving the object-glass, the rays having become parallel among themselves, encountered a double chink, each opening of which corresponded to the mouth of one of the tubes. A very narrow bundle of rays thus penetrated into each tube, and traversed its entire length, $1^{m}\cdot487$.

The two bundles, always parallel to each other, reached the object-glass of the second telescope, were then refracted, and by the effect of the refraction reunited at its focus. There they encountered the reflecting plane of a mirror perpendicular to the axis of the telescope, and underwent a reflexion back again towards the object-glass; but by the effect of this reflexion the rays had changed their route in such a way that that which was to the right before, was to the left after the reflexion, and *vice versâ*. After having again passed the object-glass, and been thus rendered parallel to each other, they penetrated a second time into the tubes; but as they were inverted, those which had passed through one tube in going passed through the other on returning. After their second transit through the tubes, the two bundles again passed the double chinks, re-entered the first telescope, and lastly intersected at its focus in passing across the transparent mirror. There they formed the fringes of interference, which were observed by a glass carrying a graduated scale at its focus.

It was necessary that the fringes should be very large in order to be able to measure the small fractions of the width of a fringe. I have found that that result is obtained, and a great intensity of light maintained, by placing before one of the chinks a thick mirror which is inclined in such a way as to see the two chinks by the effect of refraction, as if they were nearer to each other than they really are. It is in this way possible to give various dimensions to the fringes, and to choose that which is the most convenient for observation. The double transit of the light was for the purpose of augmenting the distance traversed in the medium in motion, and further to compensate entirely any accidental difference of temperature or pressure between the two tubes, from which might result a displacement of the fringes, which would be mingled with the displacement which the motion alone would have produced; and thus have rendered the observation of it uncertain.

It is, in fact, easy to see that in this arrangement all the points situated in the path of one ray are equally in the path of the other; so that any alteration of the density in any point whatever of the transit acts in the same manner upon the two rays, and cannot consequently have any influence upon the position of the fringes. The compensation may be satisfactorily shown to be complete by placing a thick mirror before one of the two chinks, or as well by filling only one of the tubes with water, the other being full of air. Neither of these two experiments gives rise to the least alteration in the position of the fringes.

With regard to the motion, it is seen, on the contrary, that the two rays are subject to opposite influences.

If it is supposed that in the tube situated to the right the water runs towards the observer, that of the two rays which comes from the right will have traversed the tube in the direction of the motion, while the ray coming from the left will have passed in a direction contrary to that of the motion.

By making water move in the two tubes at the same time and in contrary directions in each, it will be seen that the effects should be added. This double current having been produced, the direction may be again reversed simultaneously in the two tubes, and the effect would again be double.

All the movements of the water were produced in a very simple manner, each tube being connected by two conduits situated near their extremities, with two reservoirs of glass, in which a pressure is alternately exercised by means of compressed air. By means of this pressure the water passes from one reservoir to the other by traversing the tube, the two extremities of which are closed by the mirrors. The interior diameter of the tubes was $5^{mm}\cdot3$, their length $1^{m}\cdot487$. They were of glass.

The pressure under which the flowing of the water took place might have exceeded two atmospheres. The velocity was calculated by dividing the volume of water running in *one second* by the area of the section of the tube. I ought to mention, in order to prevent an objection which might be made, that great care was taken to obviate the effects of the accidental motions which the pressure or the shock of the water might produce. Therefore the two tubes, and the reservoirs in which the motion of the water was made, were sustained by supports independent of the other parts of the apparatus, and especially of the two lunettes; it was therefore only the two tubes which could suffer any accidental movement; but both theory and practice have proved that the motion or flexions of the tubes alone were without influence upon the position of the fringes. The following are the results obtained.

When the water is set in motion the fringes are displaced, and according as the water moves in the one direction or the other, the displacement takes place towards the right or the left.

The fringes are displaced towards the right when the water is running from the observer in the tube situated to his right, and towards the observer in the tube situated to his left.

The fringes are displaced towards the left when the direction of the current in each tube takes place in a direction opposed to that which has just been described.

With a velocity of the water equal to $2^{m\cdot}$ a second, the displacement is already very sensible; with a velocity of 4 to 7 metres it is perfectly measurable.

After having demonstrated the existence of the phænomenon, I endeavoured to determine its numerical value with all the exactitude which it was possible to attain.

By calling that the simple displacement which was produced when the water at rest in the commencement was set in motion, and that the double displacement which was produced when the motion was changed to a contrary one, it was found that the average deduced from nineteen observations sufficiently concurring, was 0·23 for the simple displacement, which gives 0·46 for the double displacement, the width of a fringe being taken as unity. The velocity of the water was 7·069 metres a second.

This result was afterwards compared with those which have been deduced by calculation from the different hypotheses relative to the æther.

According to the supposition that the æther is entirely free and independent of the motion of bodies, the displacement ought to be null.

According to the hypothesis which considers the æther united to the molecules of matter in such a way as to participate in its motions, calculation gives for the double displacement the value 0·92. Experiment gave a number only half as great, or 0·46.

According to the hypothesis by which the æther is partially carried along, the hypothesis of Fresnel, calculation gives 0·40, that is to say, a number very near to that which was found by experiment; and the difference between the two values would very probably be still less if it had been possible to introduce into the calculation of the velocity of the water a correction which had to be neglected from the want of sufficiently precise data, and which refers to the unequal velocity of the different threads of fluid; by estimating the value of that correction in the most probable manner, it is seen that it tends to augment a little the theoretical value and to approach the value of the observed result.

An experiment similar to that which I have just described had been made previously with air in motion, and I have demonstrated that the motion of the air does not produce any sensible displacement in the fringes. In the circumstances in which that experiment was made, and with a velocity of 25 metres a second, which was that of the motion of the air, it is found that according to the hypothesis by which the æther is considered to be carried along with the bodies, the double displacement ought to be 0·82.

According to the hypothesis of Fresnel, the same displacement ought to be only 0·000465, that is to say, entirely imperceptible. Thus the apparent immobility of the fringe in the experiment made with air in motion is completely in accordance with the theory of Fresnel. It was after having demonstrated this negative fact, and while seeking for an explanation by the different hypotheses relating to the æther in such a way as to satisfy at the same time the phænomena of aberration and the experiment of M. Arago, that it appeared to me to be necessary to admit with Fresnel that the motion of a body occasions an alteration in the velocity of light, and that this alteration of velocity is greater or less for different mediums, according to the energy with which those mediums refract light, so that it is considerable in bodies which are strongly refractive and very feeble in those which refract but little, as the air. It follows from this, that if the fringes are not displaced when light traverses air in motion, there should, on the contrary, be a sensible displacement when the experiment is made with water, the index of refraction of which is very much greater than that of air.

An experiment of M. Babinet, mentioned in the ninth volume of the *Comptes Rendus*, seems to be opposed to the hypothesis of an alteration of velocity in conformity with the law of Fresnel. But on considering the circumstances of that experiment, I have remarked a cause of compensation which must render the effect of the motion imperceptible. This cause consists in the reflexion which the light undergoes in that experiment; in fact it may be demonstrated, that when two rays have a certain difference of course, that difference is changed by the effect of the reflexion upon a mirror in motion. On calculating separately the two effects in the experiment of M. Babinet, it is found that they have values sensibly equal with contrary signs.

This explanation renders still more probable the hypothesis of an alteration of velocity, and an experiment made with water in motion appears to me completely appropriate to decide the question with certainty.

The success of the experiment seems to me to render the adoption of Fresnel's hypothesis necessary, or at least the law which he found for the expression of the alteration of the velocity of light by the effect of motion of a body; for although that law being found true may be a very strong proof in favour of the hypothesis of which it is only a consequence, perhaps the conception of Fresnel may appear so extraordinary, and in some respects so difficult, to admit, that other proofs and a profound examination on the part of geometricians will still be necessary before adopting it as an expression of the real facts of the case.—*Comptes Rendus*, Sept. 29, 1851.

ON THE FORMATION OF ANHYDROUS CRYSTALLIZED ALUM. BY THE PRINCE OF SALM-HORSTMAR.

Alumina, obtained by precipitating ammonia-alum by ammonia and heating the precipitate to redness, was fused with four times its weight of bisulphate of potash; on treatment of the fused mass with water, six-sided tables which did not doubly refract light were left, and on analysis were found to consist of anhydrous alum.—*Journ. für Prakt. Chem.* vol. lii. p. 319.

ON THE COMPOSITION OF THE GASES EVOLVED ON THE PRODUCTION OF COKE FROM COAL. BY M. EBELMEN.

The question might arise, whether in the formation of coke from coal in a furnace, the air which enters the furnace gives up its oxygen to the matters which are evolved in the gaseous state, or to the solid carbon; and again, whether the oxygen forms carbonic oxide or carbonic acid. Ebelmen examined the composition of the gases of the coke-ovens at Seraing, and found that more than two-thirds of the hydrogen of the coal is burned, the remainder existing in the evolved gaseous mixture. The quantity of carbonic acid is three times that of the carbonic oxide.—*Comptes Rendus*, vol. xxxii. p. 92.

MAGNECRYSTALLIC PROPERTY OF CALCAREOUS SPAR.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Glasgow College, Nov. 7, 1851.

A mistake (I cannot call it a misprint) which occurred in the footnote to § 12 of my paper on Magnetic Induction, published in your Number for last March, has, although corrected in the "Errata" of the volume containing it, caused considerable perplexity regarding my meaning, as I perceive by some remarks of Dr. Tyndall's, contained in a foot-note on his paper on the Polarity of Bismuth, published in your Number for this month. You will oblige me by publishing the following, which is the correct form of the passage referred to.

"Thus, a ball cut out of a crystal of pure calcareous spar, which tends to turn with its optic axis perpendicular to the lines of force, and which tends as a whole from places of stronger towards places of weaker force, would experience this latter tendency less strongly when the optic axis is perpendicular to the lines of force than when it is parallel to them; since, according to § 8 of the text, the crystal must have the greatest inductive capacity, or (the language in the text being strictly algebraic when negative quantities are concerned) least capacity for diamagnetic induction, perpendicular to the optic axis."

In the passage, as originally published, the word "more" occurred in the place of "less." The mistake was pointed out to me last April by Professor Stokes, and I immediately requested you to correct it, which you accordingly did by an intimation in the "Errata." When the perplexity occasioned by the mistake is removed, it is obvious to any one reading the passage carefully, that the mistake itself was only a slip of the pen, as at the conclusion of the sentence it is asserted that a crystal of pure calcareous spar must have the "*least* capacity for diamagnetic induction, perpendicular to the optic axis."

This conclusion is verified by Dr. Tyndall, who describes experiments, in a paper published in your September Number, by which it appears that the diamagnetic inductive capacity of calcareous spar in a direction parallel to the optic axis is to its diamagnetic inductive capacity perpendicular to the optic axis as 57 to 51.

I remain, Gentlemen,
Your obedient Servant,
WILLIAM THOMSON.

[We have also received a communication on this subject from Mr. Tyndall, who in reference to a note received by him from Prof. Thomson writes as follows:—"I have only to say that the facts are precisely what they are here stated to be. Previous to writing the remarks in question, I looked to the Errata, but not it seems with sufficient attention, for Professor Thomson's correction escaped me. Not only do our results agree in principle, but the same substance and form of substance which Professor Thomson had referred to in illustration of his theory was unwittingly examined by me in Berlin, and the exact result which he had theoretically predicted arrived at by way of experiment."—EDIT.]

OBSERVATIONS UPON THE RADIATION OF LUMINOUS BODIES. BY M. BAUDRIMONT.

On looking at a very brilliant light, it sometimes appears to be surrounded by brilliant luminous rays, clear, free from cloudiness, and which must not be confounded with those caused by the eyelashes when the eyes are partially closed. These rays may be observed most distinctly by looking at an image of the sun reflected upon the surface of a convex glass, or still better upon a lens having a considerable curvature. They may be most easily observed by looking at an image of the sun formed in the focus of a lens placed at the extremity of a tube blackened in the interior. If the observer place himself in a room into which the light penetrates only through a narrow opening, the phænomenon appears with great splendour, and it may even be said with extraordinary magnificence. The rays are either white, or present all the colours of the spectrum. In that case a motion appears to take place in their interior, which cannot be compared with anything better than that of a liquid circulating with difficulty in narrow channels in which it meets with obstacles. There are also seen irregular concentric rings, which appear to move from their common centre. Whatever may be the circumstances in which the observer places himself, and whatever may be the precautions which are taken to obviate the complication of the phænomenon, the rays do not appear disposed as those in a circle; they have not all a common centre, but form entangled bundles in a very peculiar manner.

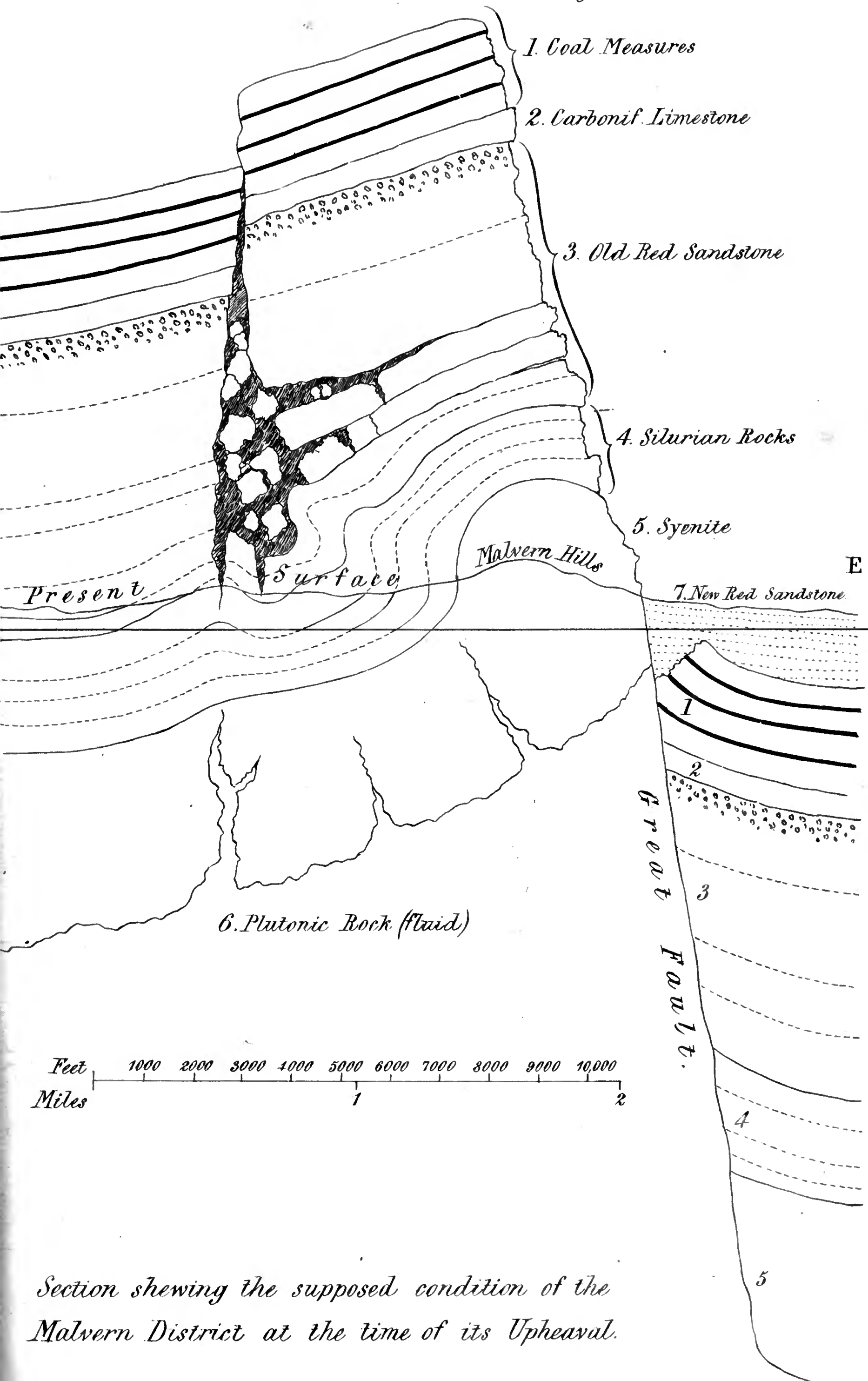
At first sight I was struck with the resemblance which appeared to me to exist between the arrangement of these rays and that of the fibres of the crystalline lens; and I attempted immediately some experiments directed from that point of view. From among those which I have made I will quote the two following, which, if they do not prove that this apparent radiation is to be attributed to the crystalline lens, at least show completely that the phænomenon takes place in the eye, and depends upon the structure of that organ.

1. On looking at an image of the sun produced in the circumstances above described through a black screen with a circular opening of 5 or 6 millims. diameter, the image is seen upon the surface which reflects it; while the rays are separated from it, and appear to be superimposed upon the screen, and this even when it is brought very near the eye.

2. If the head is inclined to the right or the left, the want of symmetry which is observed in the arrangement of the rays follows the movement of the eye, which under those circumstances turns upon its axis in the direction in which the head is inclined.—*Comptes Rendus*, Nov. 3, 1851.

INDEX TO VOL. II.

END OF THE SECOND VOLUME.



Section shewing the supposed condition of the Malvern District at the time of its Upheaval.

J. Basire, Lith.

BRITISH MUSEUM
NATURAL HISTORY

6