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

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

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

J. B. Pinelli ad Mazonium.

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Proceedings of the Geological Society:—

ERRATA.

Page 322, line 20, *for* coordinates x, y, z *read* the coordinates y, z.
— 323, line 9, *for* $\phi\ (w, t, y, z)$ *read* $\phi\ (wt, y, z)$.
— 324, line 2 from bottom, *for* behind, the opening in the screen must *read* behind the opening in the screen, r must.

At page 373 the parentheses in lines 12, 13 left vacant should be filled in with the number 32. And at page 375 the following words explanatory of the Table at foot (which were accidentally omitted) should be supplied:—

By means of the substitutions

$$\left|\begin{array}{cccc} 5 & 6 & 8 & 9 \\ 6 & 5 & 8 & 9 \\ 5 & 6 & 9 & 8 \\ 6 & 5 & 9 & 8 \end{array}\right|$$

the preceding eight aggregates become developed into thirty-two, which together constitute the exhaustive Table in question.

Page 376, line 2, *for* eight *read* thirty-two.

PLATES.

I. Illustrative of Mr. T. A. Hirst's Paper on Ripples, and their relation to the Velocities of Currents.

II. Illustrative of M. W. Siemens's Paper on a Standard Measure of Resistance to Galvanic Currents, and Dr. Forbes's Paper on the Repulsion of a Rectilinear Electrical Current on itself.

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[FOURTH SERIES.]

JANUARY 1861.

I. *On Ripples, and their relation to the Velocities of Currents.*

By T. ARCHER HIRST, *Mathematical Master at University College School, London*[*].

[With a Plate.]

1. ALTHOUGH we are all familiar with the ripples which solid bodies produce upon the surface of a stream in which they are partially immersed, their precise nature and their relation to the velocity of the current appear to have received but little investigation. The reason of this is no doubt to be sought in the well-known difficulties presented by the hydrodynamical problem whose solution is here, strictly speaking, involved. Upon this problem Newton, Laplace, Lagrange, Poisson, Cauchy, and others have expended the greatest analytical power and mathematical skill, and in every case the inherent difficulties of the subject have compelled them to introduce hypotheses and restrictions which more or less vitiate the results at which they at length arrived. The brothers Weber, again, by their elaborate researches, have shown that, in the experimental investigation of the problem in question, difficulties of equal magnitude are encountered.

But instead of considering the phenomena of ripples as a particular case of this general and complicated hydrodynamical problem, the question arises, can we not in some more direct and simple manner arrive at the general relation which must exist between these beautifully symmetric ripples, and the velocities of the several parts of the current upon whose surface they are produced. Professor Tyndall, in his recent work 'On the Glaciers of the Alps' (p. 398), has, in fact, prepared the way for us, in his chapter 29, "On the ripple theory of the veined structure of

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[*] Communicated by the Author.

glaciers,' by giving an exceedingly clear and simple explanation of the origin of ripples on the surfaces of streams. In the present paper I propose to pursue the subject somewhat further than he found it necessary to do, and to put his views into a mathematical form.

2. When a spherical body—or a drop of water—falls upon the surface of still water, a system of concentric and circular waves are formed around the point of impact. The foremost of these waves generally exceeds the rest in magnitude, and being on that account most visible, will be referred to as *the* wave: its height and breadth, as well as the velocity λ with which it recedes from the point of impact, all depend upon the magnitude of the body, and the height from which it fell. According to Weber*, this velocity λ of propagation varies also with the time, or, more strictly, decreases as the radius of the circle formed by the wave increases: this variation, however, is admitted by Weber to be small†, and according to Poisson's calculations, has no existence‡. In the present paper this possible variation of λ is not overlooked, although, to obtain definite results capable of being compared with those of experiment, λ is often treated as a constant; the error incurred by so doing being rendered less important by the circumstance that the waves with which we shall then be concerned cease, in reality, to be visible before their radii have reached any great magnitude.

3. If we suppose the spherical body to fall into a current of water whose velocity v is everywhere the same, the particles forming the surface of the current will still be relatively at rest, and the wave will again be circular in form, the centre of the circle being carried down the current whilst its radius increases with a velocity λ, which we may assume to be the same as before. If the velocity and direction of the current vary from point to point, the circular form of the wave will be destroyed as it floats downwards, and the variations of form through which it will pass will, as Weber remarks, indicate in some measure the variations in the direction and velocity of the current at its several points.

4. Let us next suppose a succession of drops to fall into the stream, the points of impact being fixed in space. Each drop will occasion a wave; and if there be no current, the several waves will form a system of concentric circles around the point of impact; if there be a current, however, and its velocity be not too small, the successive waves will intersect one another, and at the points of intersection the water will be raised to a height exceeding that of either of the intersecting waves. Lastly, if the

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* *Wellenlehre*, p. 182.

† Ibid. p. 210.

‡ *Mémoires de l'Acad. Roy. des Sciences de l'Institut*, 1816, vol. i. p. 165. See also *Wellenlehre*, p. 423.

drops succeed each other with sufficient rapidity, in short, if they constitute a jet of water falling into the current, the successive waves will, by their intersection, give rise to a continuous series of points elevated above the general level of the liquid, and forming a *ripple* more visible than the waves which it envelopes. If we replace the jet by a solid cylinder, the effect will be essentially the same: the waves, it is true, may differ in form and even in the velocity with which they are propagated; but, as before, the ripple will be their envelope. It is in this manner that the pebbles and other partially immersed bodies on the banks of a stream give rise to ripples whose forms, as we shall see, indicate in every case the velocities of the adjacent parts of the current. It is scarcely necessary to add that bodies moving on the surface of still water produce precisely similar effects; the ripples caused by boats and water-fowl are examples familiar to all.

5. The relation between the form of the ripple and the velocity of the stream may be easily determined without even knowing the forms of the waves of which the ripple is the envelope. Deferring this determination, however, to art. 10, let us first, for the sake of completeness, consider the following question:—

The initial form of a wave being known, through what variations will it pass as it floats down a stream where the velocity and direction of the current vary from point to point according to a given law?

Let x and y be the coordinates of any point m on the surface of the stream, and let v and α be given functions of x and y, denoting respectively the velocity of the current at the point m, and the angle between its direction and the abscissa axis; the problem is to find the equation

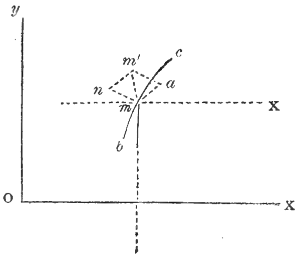
$$y=f(x, t) \qquad \ldots\ldots\ldots \qquad (1)$$

of the wave at the expiration of the time t, the equation

$$y=f(x, 0) \qquad \ldots\ldots\ldots \qquad (2)$$

of the wave at the origin of that time being known.

At the time t in question, the point m of the wave bmc will have two velocities; one, λ, in the direction of the normal mn to the wave, and another, v, in the direction ma determined by the angle $a\hat{m}\mathrm{X}=\alpha$. At the expiration of the element of time dt, therefore, the point m will arrive at the opposite angle m' of a small parallelogram, whose sides $mn=\lambda dt$ and $ma=vdt$ have the directions above defined. If we call x' and y' the coordinates of m', and $\phi=n\hat{m}\mathrm{X}$ the angle between the *external* normal



and the *positive* direction of the abscissa axis,

$$x' = x + (\lambda \cos\phi + v\cos\alpha)dt,$$
$$y' = y + (\lambda \sin\phi + v\sin\alpha)dt.$$

But on changing t into $t+dt$, the coordinates x', y' should satisfy the equation (1); hence

$$y + (\lambda\sin\phi + v\sin\alpha)dt = f[x + (\lambda\cos\phi + v\cos\alpha)dt,\quad t+dt].$$

Developing the function on the right, neglecting powers of dt higher than the first, and recalling the equation (1), we have

$$\lambda\sin\phi + v\sin\alpha = \frac{dy}{dx}(\lambda\cos\phi + v\cos\alpha) + \frac{dy}{dt};$$

or, since

$$\sin\phi = \frac{1}{\sqrt{1+\left(\frac{dy}{dx}\right)^2}}, \text{ and } \cos\phi = -\frac{\frac{dy}{dx}}{\sqrt{1+\left(\frac{dy}{dx}\right)^2}},$$

$$\lambda\sqrt{1+\left(\frac{dy}{dx}\right)^2} + v\sin\alpha = v\frac{dy}{dx}\cos\alpha + \frac{dy}{dt}. \quad . \quad . \quad . \quad (3)$$

This is the partial differential equation whose general integral will include the equations of all possible waves which can be formed under the given conditions. The arbitrary function which this integral involves will in each case be determined by the known equation (2) of the wave which corresponds to $t=0$. With respect to this equation, however, it must be remembered that λ varies with the nature of the displacement to which the waves owe their origin; in our case it depends upon the magnitude and velocity of the jet—a fact which Weber's experiments* establish beyond doubt. Apart from this variation, however, it follows from art. 2 that, even when the jet remains the same throughout, λ *may* vary from point to point of a wave; in other words, it may be a function of x and y. In assuming λ to be constant, therefore, approximate results can alone be expected.

6. If we assume the direction of the current to be everywhere the same, and parallel to the abscissa axis, then α is always zero, and the equation (3) becomes

$$\lambda\sqrt{1+\left(\frac{dy}{dx}\right)^2} = v\frac{dy}{dx} + \frac{dy}{dt}. \quad . \quad . \quad . \quad . \quad (4)$$

The velocity v still remains a function of both x and y; but without departing too much from the actual state of things in rectilinear streams, we may regard v as a function of y alone; that is to say, we may suppose the velocity of the current to

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* *Wellenlehre*, p. 183.

remain the same at the same distance from its banks, and to vary only on *crossing* the stream. Under these conditions the equation (4) is integrable; and if λ be constant, one of its complete integrals will be found to be

$$\int dy\sqrt{(c-v)^2-\lambda^2}=\lambda(x-ct)+\Phi(c)\,; \quad . \quad . \quad (5)$$

where c is an arbitrary constant, and Φ an arbitrary function. The general integral will result from the elimination of c between this equation and its differential according to c, which is

$$\int\frac{(c-v)dy}{\sqrt{(c-v)^2-\lambda^2}}+\lambda t=\Phi'(c). \quad . \quad . \quad . \quad . \quad . \quad (6)$$

7. The arbitrary function Φ may be determined from the known equation

$$\mathrm{F}(x,\, y)=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

of the initial wave in the following manner. Putting $t=0$ in the equations (5) and (6), we know that the result of eliminating c from the equations

$$\int dy\sqrt{(c-v)^2-\lambda^2}=\lambda x+\Phi(c), \quad . \quad . \quad . \quad . \quad (5a)$$

$$\int\frac{(c-v)dy}{\sqrt{(c-v)^2-\lambda^2}}=\Phi'(c) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6a)$$

must coincide with the equation (7). But eliminating c from these equations is equivalent to replacing c in the first by a function of x and y determined from the second. Now on differentiating (5a) on the hypothesis that c is a function of x and y thus determined, we have, in virtue of (6a),

$$dy\,\sqrt{(c-v)^2-\lambda^2}=\lambda\,dx,$$

an equation which ought also to coincide with the result of differentiating (7), that is, with

$$\frac{d\mathrm{F}}{dy}\,dy+\frac{d\mathrm{F}}{dx}\,dx=0.$$

We conclude then that

$$\frac{d\mathrm{F}}{dx}\sqrt{(c-v)^2-\lambda^2}+\lambda\frac{d\mathrm{F}}{dy}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The result of eliminating x and y from the equations (7), (5a), and (8), therefore, will lead to the required relation between $\Phi(c)$ and c.

8. As an example, let the velocity v of the current be constant, or the same at all points. The complete integral (5) of the equation (4) will then become

$$\sqrt{(c-v)^2-\lambda^2}=\lambda(x-ct)+c_1, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where c_1 is a function of c to be determined from the initial form of the wave. If we suppose this initial form to be that of a circle with radius a around the origin, the required relation between c and c_1 will result from eliminating x and y from the following equations, to which (7), (5a), and (8) become respectively reduced:

$$\left.\begin{aligned} x^2+y^2&=a^2,\\ y\sqrt{(c-v)^2-\lambda^2}-\lambda x&=c_1,\\ x\sqrt{(c-v)^2-\lambda^2}+\lambda y&=0. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (10)$$

From these we easily deduce

$$a(c-v)=\pm c_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

by means of which (9) becomes

$$y\sqrt{c_1{}^2-a^2\lambda^2}=a\lambda(x-vt)+c_1(a\mp\lambda t).$$

Differentiating this according to c_1, we have, corresponding to (6), the equation

$$\frac{c_1 y}{\sqrt{c_1{}^2-a^2\lambda^2}}=a\mp\lambda t.$$

The equation of the wave at the end of the time t results from the elimination of c_1 from these two equations, or from the following two, to which they are equivalent:

$$c_1 y^2=a\lambda(x-vt)(a\mp\lambda t)+c_1(a\mp\lambda t)^2,$$

$$c_1=-a\lambda\frac{a\mp\lambda t}{x-vt}.$$

The result is clearly

$$y^2+(x-vt)^2=(a\mp\lambda t)^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

which, as might have been anticipated, is the equation of a circle whose centre is on the abscissa axis at a distance from the origin equal to vt—the space described in the time t by each point of the current—and whose radius, from being a, has become $a+\lambda t$, in consequence of the propagation of the wave with the constant velocity λ. The upper sign in (12) is, of course, foreign to the present inquiry; it refers to the propagation of the wave *inwards*, a case which is included in the differential equation (4)*.

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* It is worthy of notice that when $v=0$, the differential equation (3) or (4) becomes

$$\left(\frac{dy}{dk}\right)=1+\left(\frac{dy}{dx}\right)^2,$$

where $k=\lambda t$, a relation which all *parallel curves* must satisfy, and which may be at once obtained from the definition of these curves. If x, y, and k be regarded as the coordinates of a point in space, the above partial differential equation represents a developable surface generated by a plane

9. Let us next consider the ripple which envelopes a system of waves having at their origin the same position and form. If, as in art. 5,

$$y=f(x,t) \quad . \quad . \quad (1)$$

represent the equation of any wave,

$$y=f(x,\ t+dt)=f(x,\ t)+\frac{df}{dt}\,dt+\&c\ldots$$

will be that of the next preceding wave, and the values of x and y which satisfy both equations, or which satisfy (1) and the equation

$$\frac{dy}{dt}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

will refer to a point on the required ripple. In short, the equation of this ripple will be the result of the elimination of t from the equations (1) and (13). If we differentiate (1), regarding t as a function of x determined by (13), and use brackets to distinguish partial from complete differential coefficients, we have

$$\frac{dy}{dx}=\left(\frac{dy}{dx}\right)+\left(\frac{dy}{dt}\right)\frac{dt}{dx};$$

hence by (13),

$$\left(\frac{dy}{dx}\right)=\frac{dy}{dx}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

an equation which merely expresses the well-known fact, that at their point of contact the wave and ripple have the same tangent. But it was shown in art. 5 that the equation (1) satisfies the partial differential equation (3); and the latter, by means of (13) and (14), becomes transformed into the ordinary differential equation

$$\lambda\sqrt{1+\frac{dy^2}{dx^2}}+v\sin\alpha=v\frac{dy}{dx}\cos\alpha, \quad . \quad . \quad . \quad . \quad (15)$$

which is clearly that of the ripple. If the coordinate axes be turned around the origin until the abscissa axis is parallel to the direction of the current at any point M(xy) of the ripple, then, since $\alpha=0$ at that point, (15) becomes

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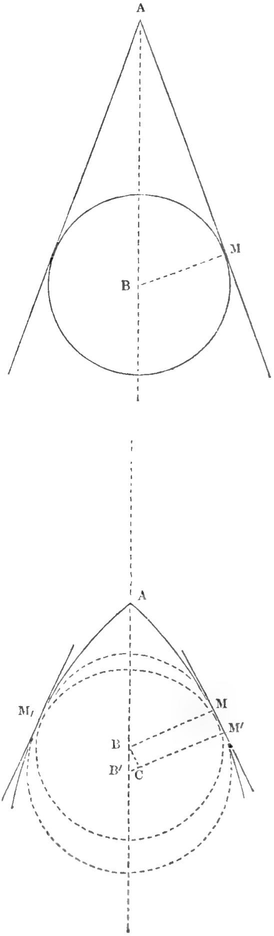
which is constantly inclined at an angle of 45° to the plane (xy) or axis k, and the sections of this surface made by planes parallel to (xy) will constitute a system of parallel curves. If the *base* of this system be a curve traced on the coordinate plane of (xy), the generating planes of the developable will always touch the same. The developable, in fact, has for its edge of regression a curve which cuts at an angle of 45° all the generators of a right cylinder whose base is the evolute of the curve traced on the plane (xy).

$$\frac{\frac{dy}{dx}}{\sqrt{1+\frac{dy^2}{dx^2}}}=\sin\theta=\frac{\lambda}{v}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where θ is the inclination of the ripple to the direction of the current at any point M of the former. We are thus led to the following simple and interesting result :—

At any point of a ripple, the sine of the angle between its direction and that of the current is inversely proportional to the velocity of the latter, and directly proportional to the velocity of propagation of the wave which touches the ripple in that point.



10. This result may be arrived at in a simpler manner. When the velocity and direction of the current remain the same at all points, the waves produced at a point A retain their circular form as they float down the current. If the radii of the several circular waves increase with the same velocity λ, then the ripple, their envelope, will clearly consist of two right lines diverging from A and touching all the circles. If from the centre B of any wave the radius B M be drawn to its point of contact with the ripple, then, since the wave has been propagated over the space B M in the same time that a point of the current has described the space A B, we have clearly

$$\frac{\mathrm{BM}}{\mathrm{BA}}=\sin \mathrm{BAM}=\frac{\lambda}{v}. \quad . \quad (17)$$

If, as Weber asserts (see art. 2), the velocity λ with which the wave is propagated diminishes as its magnitude or radius BM increases, the ripple will no longer be rectilineal, but at the point M of the ripple the law of art. 9 will still hold. To prove this, it is only necessary to consider two immediately succeeding circular waves around B and B′, and from these points to let fall the perpendiculars B M, B′M′ upon their common tangent M M′, which will also be the tangent to the ripple at the point M;

drawing B C parallel to M M′, we have, as before,

$$\frac{B'C}{B'B} = \sin B'BC = \sin\theta = \frac{\lambda}{v},$$

where θ is the angle between the directions of the ripple and the current at the point M. The velocity v being constant, θ and λ will decrease simultaneously; so that, according to Weber, the ripple should consist of two curved lines, A M, A M_1, turning their concavities towards each other. This property of the wave suggests a crucial experiment as to the variation of λ; to apply it, however, a perfectly uniform current would be required, or what is equally difficult to realize, a jet must be made to describe a right line with perfectly uniform velocity over still water. In ordinary experiments, as will be hereafter seen, the ripple, as long as it remains visible, and as far as the eye can judge, is rectilinear.

Again, it may easily be shown that when the velocity and direction of the current vary from point to point, so as to destroy the circular form of the waves, the law of art. 9 is still fulfilled. In fact, in the immediate vicinity of the point M of the ripple we may regard this velocity and direction as constant; and in place of the non-circular wave, to whose intersection with the immediately preceding and succeeding waves the ripple at M is due, we may substitute a circular one osculating the real wave in M, increasing with the same velocity λ, and moving parallel to the current at M with the velocity v which exists at that point. This fictitious circular wave will clearly produce a ripple coincident with the actual one in the neighbourhood of the point M, and thus the relation (16) between λ, v, and θ will still exist at that point.

11. From the relation

$$\sin\theta = \frac{\lambda}{v},$$

it follows that when λ and v are equal, $\theta = \frac{\pi}{2}$; that is to say, when the velocity of the current is equal to the velocity with which the wave is propagated, the several waves all touch a line at right angles to the direction of the current; they will consequently touch each other, and the ripple will become reduced to their point of contact. When λ exceeds v, θ is imaginary; in fact, in this case the waves will clearly be propagated up the stream, and will no longer intersect. Strictly speaking, however, this is the case only when the waves are produced by a discontinuous series of drops; experiment shows that when a solid cylinder or jet is partially immersed in so slow a current, the water flows past it without its surface suffering any visible disturbance. From this it follows that a ripple which has been produced in a stream, where the velocity diminishes from the centre towards the sides, will end abruptly as soon as it has reached a point where that velocity is less than λ; the pebbles on the banks of such a stream will produce no ripples. In a similar manner, too, λ may be regarded as a limit beyond which the velocity of a body moving through still water cannot be increased without visibly rippling its surface.

12. For the sake of further illustration, let us assume, as in art. 6, that the direction of the current is everywhere parallel to the abscissa axis, and that its velocity v varies only with the distance y from this axis. The differential equation (15) of the ripple then becomes

$$dy\sqrt{v^2-\lambda^2}=\lambda\, dx, \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

whose integral, λ being considered as a constant, is

$$\int dy\sqrt{v^2-\lambda^2}=\lambda x+\mathrm{C}, \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where the constant C will be determined as soon as any point in the ripple is known. By (18) we can determine v whenever the form of the ripple is known, and by (19) we may find the equation of the ripple whenever the law in the variation of the current's velocity is given.

For instance, we may determine the nature of the current, the ripples upon whose surface are parabolas. For in this case the equation of any ripple being

$$y^2=2px,$$

we have at once

$$\frac{dy}{dx}=\frac{\lambda}{\sqrt{v^2-\lambda^2}}=\frac{p}{y},$$

whence we deduce

$$\frac{v^2}{\lambda^2}-\frac{y^2}{p^2}=1,$$

and conclude that, to produce parabolic ripples, the velocity of the current at any distance y from the axis of the parabola will be represented by the abscissa of a hyperbola having that axis for transverse axis (2λ), the vertex of the parabola for centre, and a conjugate axis equal to the parameter of the parabola.

Conversely, if the velocity of the current at any distance y from the abscissa axis satisfy the relation

$$\frac{v^2}{a^2}+\frac{y^2}{b^2}=1,$$

that is to say, if this velocity can be represented by the abscissa, corresponding to the ordinate y, of an ellipse having its centre at the origin, then the equation of the ripple, as given by (19), would be

$$\int dy\sqrt{a^2-\lambda^2-c^2y^2}=\lambda x+\mathrm{C},$$

where $c=\frac{a}{b}$; on integrating, we find the equation of the ripple in this case to be

$$cy\sqrt{a^2-\lambda^2-c^2y^2}+(a^2-\lambda^2)\text{ arc sin}\frac{cy}{\sqrt{a^2-\lambda^2}}=2c\lambda x,$$

where the arbitrary constant has vanished on assuming that the ripple passes through the origin. The tangent of the inclination of the ripple to the current at any point x, y has of course the value

$$\frac{dy}{dx}=\frac{\lambda}{\sqrt{a^2-\lambda^2-c^2y^2}},$$

which varies between the minimum limit $\frac{\lambda}{\sqrt{a^2-\lambda}}$ at the origin, and the maximum ∞ at the point A, whose coordinates are

$$x=\frac{\pi}{4}\frac{a^2-\lambda^2}{c\lambda}\text{ and }y=\frac{\sqrt{a^2-\lambda^2}}{c}.$$

Further, since the equation is unchanged when x and y are replaced by $-x$ and $-y$, it is evident that the ripple consists of two similar branches in opposite quadrants, and has a point of inflexion at the origin, where it is least inclined to the current. Proceeding from this point, its inclination to the current increases until, at the points A and A′, current and ripple are at right angles to each other. Here the latter ends abruptly, in consequence of the velocity of the current having become equal to that with which the wave is propagated (art. 11).

In Plate I. fig. 1, let O X be the direction of the current, and let its velocity at any point of a line parallel to O X be represented by half the chord which is intercepted upon that line by an ellipse, X Y X′ Y′. Let the semi-chords A B and A′ B′ represent the velocity λ with which the wave is propagated. The curve A′ O A will then represent the ripple produced by partially immersing a body at any one of its points, as A′. The tangent O C at the point of inflexion O is easily constructed, since it passes at the distance $\lambda=\mathrm{XC}=\mathrm{AB}$ from the vertex X*.

13. In order to test by experiment the law enunciated in art. 9, two methods suggest themselves. *First*, to examine the ripples produced by a stationary jet falling into currents of different but known velocities; and *secondly*, to give to the jet or partially immersed body a definite motion, the water being motionless. This second method has many advantages, arising from the fact that the motion of the jet is more under our control than that of a current, which in general varies from point

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* Compare the fig. in chapter 29 of 'Glaciers of the Alps.'

to point according to imperfectly known laws. Now of all conceivable motions which might be imparted to the jet, a circular one is beyond doubt most feasible; so that we are naturally led to inquire *what will be the form of the ripple produced by a jet which, as it falls into still water, describes a circle with a given constant velocity* u.

We shall throughout assume the velocity λ with which the waves are propagated to be independent of the magnitude of the latter, and, in accordance with art. 4, we shall seek the envelope of the several waves which the moving jet originates. There is one case where the nature of this envelope can be at once determined: it is when the jet moves with the same velocity λ as the waves. For at the moment when the jet arrives at a point A (fig. 2), the wave which it produced when at B will have acquired a radius equal to the arc A B; instead of intersecting, therefore, every two successive waves around B and B′ will touch each other, the difference between their radii being equal to the distance B B′ between their centres, and their point of contact A′ will be in the tangent to the circle at the point B; in fact, the ripple in this case will be the *involute of the circle* A A′ C, or the curve formed by unwrapping, under tension, a string originally wrapped round the circle as far as A. If the velocity of the jet were less than λ, the successive waves would precede the jet, and neither intersect nor touch each other; in other words, there would be no ripple. But if the velocity of the jet exceed λ, the ripple will separate into two branches (fig. 7), one of which will be outside the circle, whilst the other will enter it; but since the waves continue to increase in radius, this latter branch will necessarily leave the circle again, and never re-enter it. In art. 20 we shall find, in fact, that this branch, after approaching to within a certain distance of the centre, suddenly turns and recedes, the turning-point or cusp C being due to the intersection in that point of *three* successive waves. In experiments this cusp is tolerably well defined; and its position is the more important, since it bears a very simple relation to the velocities u and λ; in fact, we shall find that *the distance of the cusp from the centre of the circle described by the jet has to the radius of that circle the same ratio that the velocity* λ *of propagation has to the velocity* u *with which the jet moves.*

14. For the sake of future applications, it will be more convenient to deduce the form of the ripple above considered from the solution of the following more general problem.

A jet which describes, with uniform velocity u, a fixed circle with radius a, falls into a current whose velocity v and direction are the same at every point of its surface; required the form of the ripple.

Let the centre of the circle be taken as the origin of coordinate axes, one of which—the ordinate axis—is parallel to the direction of the current. To fix our ideas, let us suppose, too, that after having rotated for an indefinite period in the direction opposed to that of the hands of a clock, the jet has at length reached the position A (fig. 3) defined by the angle $AOX = \psi$. The centre of the circular wave which the jet originated when in any position B, will have been carried with the current along the line BC parallel to the ordinate axis, and its radius, from being zero, will have increased to a certain magnitude CM. The arc $\overset{\frown}{AB}$, the line BC, and the radius CM being described in the same time, we shall clearly have the proportions

$$\overset{\frown}{AB} : \overline{BC} : \overline{CM} = u : v : \lambda\,;$$

so that, on representing the angle AOB by ϕ, and the ratios $\frac{\lambda}{u}$ and $\frac{v}{u}$ by α and β respectively, we shall have

$$\overset{\frown}{AB} = a\phi, \quad \overline{CM} = a\alpha\phi, \quad \overline{BC} = a\beta\phi\,;$$

and the coordinates ξ, η of the centre C of the circular wave will be

$$\left.\begin{aligned} \xi &= a\cos(\psi-\phi), \\ \eta &= a\sin(\psi-\phi) + a\beta\phi\,; \end{aligned}\right\} \quad . \quad . \quad . \quad (20)$$

whilst the equation of this wave will be

$$[x - a\cos(\psi-\phi)]^2 + [y - a\sin(\psi-\phi) - a\beta\phi]^2 - a^2\alpha^2\phi^2 = 0. \quad (21)$$

The equation of the immediately succeeding wave will be obtained from this by changing ϕ into $\phi + d\phi$; the intersections of the two waves will be two points on the ripple, and their coordinates will clearly satisfy the equation (21), as well as its differential according to ϕ. The equation of the ripple, therefore, at the moment the jet reaches the point A defined by the angle ψ, will result from the elimination of ϕ between (21) and the equation

$$\begin{aligned} &[x - a\cos(\psi-\phi)]\sin(\psi-\phi) - [y - a\sin(\psi-\phi) - a\beta\phi] \\ &\qquad [\cos(\psi-\phi) - \beta] + a\alpha^2\phi = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22) \end{aligned}$$

which is simply that of the chord of intersection of the two successive circular waves. In a similar manner, the equation of the chord of intersection of the second of the above waves, and a third, immediately following the same, will be found by putting $\phi + d\phi$ in place of ϕ in (22); and the coordinates of the point in which these two chords cut each other will satisfy both (22) and its differential according to ϕ, which is

$$\begin{aligned} &[x - a\cos(\psi-\phi)]\cos(\psi-\phi) + [y - a\sin(\psi-\phi) - a\beta\phi] \\ &\qquad \sin(\psi-\phi) + a[1 - \alpha^2 + \beta^2 - 2\beta\cos(\psi-\phi] = 0. \quad . \quad (23) \end{aligned}$$

If the three successive waves intersect in a point, the two chords will also pass through that point, and its coordinates will consequently satisfy, simultaneously, the three equations (21), (22), (23). The position of the *cusps* of the ripple at the moment the jet reaches A will be found, therefore, from the two equations which result from eliminating ϕ from the last three equations. These two equations will of course contain the angle ψ which defines the position of the jet; if the latter be also eliminated, the resulting equation will be that of the curve described by the cusps as the jet rotates.

It will be at once observed that the elimination of ψ and ϕ from the above equations is equivalent to the elimination of the three variables $\sin(\psi-\phi)$, $\cos(\psi-\phi)$, and ϕ from those equations in conjunction with

$$\sin^2(\psi-\phi)+\cos^2(\psi-\phi)=1;$$

so that in the result all circular functions will disappear; that is to say, as the jet rotates, the cusps of the ripple will describe an *algebraical* curve. Particular cases excepted, the order of this curve is high; for instance, when the three velocities λ, u, and v are equal, it reaches the eighth order; when v vanishes, however, it becomes a circle (art. 20).

If, lastly, we eliminate x and y from the equations (21), (22), (23), we shall obtain the relation between ϕ and ψ which corresponds to the cusps at the moment under consideration, *i. e.* when the jet reaches the position A.

15. Before proceeding further, however, it will be useful to examine the locus (20) of the centres of the circular waves at the moment under consideration. On eliminating ϕ from the equations (20), the equation of this locus will be found to be

$$\eta-a\beta\psi=\sqrt{a^2-\xi^2}-a\beta\cos^{-1}\frac{\xi}{a};$$

from which we learn that the curve undulates between the two lines $\xi=\pm a$ parallel to the ordinate axis, and that the successive undulations are precisely similar, the length of each undulation being $2\pi a\beta$. The form of each undulation differs according as β is less than, equal to, or greater than 1, and in the following manner:—*First,* when $\beta<1$, *i. e.* when the velocity of the current is less than that of the jet, the curve consists of a series of loops, A B C D E F (fig. 5), the distances between the points B, D, F, where it touches the line X′F, as well as between the points C, E, where it touches the line X E, being $2\pi a\beta$. The branches B C and C D, however, are not symmetrical. As β increases, the loops diminish until, *secondly,* when $\beta=1$, they become transformed into cusps B′, D′, at which the tangents are parallel to the abscissa axis. Lastly, when $\beta > 1$, the loops and cusps disappear, and are replaced by points of inflexion, H, K, which lie on the line $\xi = -\frac{a}{\beta}$. A somewhat clearer image of the ripple may be obtained by regarding it as the envelope of a circle whose centre M moves along one of the three curves of fig. 5, and whose radius increases proportionally to the distance M N, measured along a parallel to the ordinate axis, between its centre and that portion X Y′ X′ of the circumference of the circle whose concavity is turned in the same direction as that of the portion B′ M C′ of the curve along which the centre is moving.

16. To return to the equations of the ripple: let us, for brevity, put

$$\sin(\psi - \phi) = \mu, \quad \cos(\psi - \phi) = \nu;$$

the equations (21), (22), (23) will then become

$$\left.\begin{array}{l} (x-\nu a)^2 + (y - \mu a - \beta\phi a)^2 - \alpha^2\phi^2 a^2 = 0, \\ \mu(x - \nu a) + (\beta - \nu)(y - \mu a - \beta\phi a) + \alpha^2\phi a = 0, \\ \nu(x - \nu a) + \mu(y - \mu a - \beta\phi a) + (1 - \alpha^2 + \beta^2 - 2\nu\beta)a = 0. \end{array}\right\} \quad (24)$$

The first two equations, when solved for x and y, give

$$\left.\begin{array}{l} \dfrac{x}{a} = \nu - \alpha\phi\dfrac{\alpha\mu \pm (\beta - \nu)\sqrt{\mathrm{R}}}{\mathrm{R} + \alpha^2}, \\ \dfrac{y}{a} = \mu + \beta\phi - \alpha\phi\dfrac{\alpha(\beta - \nu) \mp \mu\sqrt{\mathrm{R}}}{\mathrm{R} + \alpha^2}, \end{array}\right\} \quad . \quad . \quad (25)$$

where

$$\mathrm{R} = 1 - \alpha^2 + \beta^2 - 2\beta\nu. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

In these values of the coordinates of any point of the ripple the upper and lower signs correspond, and refer to the two distinct branches into which the ripple divides itself.

On eliminating x and y from the three equations (24), the result will be found to be

$$[\alpha^2 - (1 - \beta\nu)^2]\alpha^2\phi^2 - 2\alpha\beta\mu\mathrm{R}\alpha\phi + (1 + \beta^2 - 2\beta\nu)\mathrm{R}^2 = 0. \quad (27)$$

This is the equation, mentioned at the end of art. 14, which is satisfied by the values of ϕ corresponding to the cusps.

17. When the value of ϕ is such as to make $\mathrm{R} = 0$, the two branches of the ripple meet, and from (25) the points of junction lie in the curve represented by

$$\left.\begin{array}{l} x = a\nu - a\mu\phi, \\ y = a\mu + a\nu\phi, \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

which may be easily shown to be the involute of the circle formed by unwrapping the circle *backwards* from the point A, where the jet has reached. Further, if $\hat{\lambda}$ be the angle, opposite to the side λ, in a triangle whose sides are respectively proportional to the velocities λ, u, and v, we shall have the relation

$$\lambda^2 = u^2 + v^2 - 2uv\cos\hat{\lambda}; \quad . \quad . \quad . \quad . \quad (29)$$

or, dividing by u^2 and introducing the ratios α and β (art. 14),

$$\alpha^2 = 1 + \beta^2 - 2\beta\cos\hat{\lambda}.$$

This, compared with (26), shows that the condition $R=0$ is satisfied when

$$\nu = \cos\hat{\lambda}, \text{ and } \mu = \pm\sin\hat{\lambda}; \quad . \quad . \quad . \quad (30)$$

so that the result of eliminating μ, ν, and ϕ from (28) and the equation $R=0$ will be

$$x\cos\hat{\lambda} \pm y\sin\hat{\lambda} = a.$$

This is the equation of the curve described by the junction points of the branches of the ripple as the jet describes its circle, which curve, as is at once seen, consists of two right lines touching that circle at points B and B′ (fig. 6), whose angular distances B O X, B′ O X from the abscissa axis are each equal to $\hat{\lambda}$. Now, since an angle $\hat{\lambda}$ fulfilling the condition (29) can always be found when the velocities λ, u, v form a triangle, that is to say, *when any two of these velocities together exceed the third*, we conclude that under these conditions the ripple will always break up into closed curves or loops, in consequence of the two branches of the ripple, which in other cases are always distinct, meeting each other. These meeting points describe the tangents (30) BC, B′C′ as the jet rotates, and their position at any moment is determined by the intersections C, C′, &c. with these tangents, of the involute A C C′ of the circle,—the latter being supposed to move with the jet.

The physical character of these meeting points, as will be immediately shown, is that the ripple is there least prominent, so that the tangents (30) B C, B′ C′ represent two lines along which the surface of the current is apparently *least disturbed*. If the angle between them, which is equal to $2\hat{\lambda}$, were determined by experiment, and the velocity u of rotation were also known, the equation (29) would serve to determine either of the velocities λ or v as soon as the other was given.

18. The above results will be further elucidated by considering the resultant relative velocity of the current and jet at any point A, fig. 4. This resultant will clearly be represented in direction and magnitude by the diagonal A R of a parallelogram whose sides A V parallel to O Y, and A U perpendicular to A O, respectively represent, in direction and magnitude, the velocities

of the current and jet. The angle at U being equal to the angle AOX$=\psi$, the magnitude of this resultant is expressed by

$$(\mathrm{R})=\sqrt{u^2+v^2-2uv\cos\psi}, \quad . \quad . \quad . \quad . \quad (31)$$

and becomes equal to λ, the velocity with which circular waves are propagated, when $\psi=\hat{\lambda}$, that is to say, when the jet reaches either of the points B, B$'$ of the foregoing article (fig. 6). For all positions of the jet in the arc B X B$'$ the resultant (R) will be less than λ, and for all points in the arc B X$'$ B$'$ greater; so that by art. 11, the jet will only commence producing a ripple when it reaches the point B, and will cease doing so as soon as it reaches the point B$'$. At these points, B and B$'$, the immediately adjacent circular waves of the incipient ripple touch each other, and the point of contact is very slightly elevated above the surface of the current at adjacent points (art. 11); further, since the centres of these waves proceed with the same uniform velocity down the current, whilst their radii increase with the same velocity, they will clearly continue to touch each other along the line B C or B$'$ C$'$; so that these lines, as above remarked, will be lines of least apparent disturbance. They do not in general touch the ripple; in fact, they do so only when the resultant velocity $(\mathrm{R})=\lambda$ coincides in direction with that of the radius O B (or O B$'$), in other words, when $v^2=\lambda^2+u^2$, as may be seen by a glance at fig. 4. This case of contact is represented in fig. 6, where A D C E represents the first closed loop of the ripple when the jet has reached the point A, and B$'$ D$_{\prime}$ C$_{\prime}$ E$_{\prime}$ what this loop becomes when the jet reaches the point B$'$. The curve D$'$ C$'$ E$'$ represents a portion of the second loop of the ripple corresponding to the position A; it is, in fact, a portion of what B$'$ D$_{\prime}$ C$_{\prime}$ E$_{\prime}$ becomes as it floats down the current during the time that the jet is describing the arc B$'$ B A.

19. The law enunciated in art. 9 leads to a simple method of determining the velocity of a current; to apply it, however, the velocity λ must be first determined, and the velocity of the current experimented upon must be greater than λ. The formula (31), however, suggests two other methods of determining the velocity of a current, which do not require a previous determination of λ, and may be applied to all currents. They are briefly the following:—

1st. The jet having a known velocity u, let the positions A and A$'$ (fig. 4) be found at which the divergence between the branches of the ripple has a given magnitude, and call 2ψ the angle between the radii O A and O A$'$. The line bisecting this angle will be perpendicular to the direction of the current. Let the experiment be now repeated with a different velocity u_1, and let $2\psi_1$ represent the angle now subtended at the centre of the circle by the two positions of the jet at which the divergence between the branches of the ripple is the same as before. Then, since the divergences are the same in both experiments, it follows from art. 10 that the resultant velocities (R) and (R_1) will be equal; so that by (31) we have

$$u^2+v^2-2uv\cos\psi=u_1{}^2+v^2-2u_1v\cos\psi_1,$$

whence we deduce

$$v=\frac{1}{2}\,\frac{u^2-u_1{}^2}{u\cos\psi-u_1\cos\psi_1}.$$

2nd. Let the positions A and A′ be found at which the angle between the branches of the ripple is bisected by the production of the radius. In this case the resultant velocity (R) will coincide in direction with the radius at each of the points A and A′, whose angular distance asunder is 2ψ, and from fig. 4 we deduce at once the relation

$$v=\frac{u}{\cos\psi}.$$

20. The case alluded to in art. 13, where the jet describes a circle in still water, may now be easily disposed of by making $v=0$, and therefore $\beta=0$, in the general case treated in art. 14. In this manner the equations (24) become

$$\left.\begin{aligned}(x-a\nu)^2+(y-a\mu)^2-\alpha^2a^2\phi^2&=0,\\(x-a\nu)\mu-(y-a\mu)\nu+\alpha^2a\,\phi&=0,\\(x-a\nu)\nu+(y-a\mu)\mu+(1-\alpha^2)a&=0;\end{aligned}\right\}\quad .\quad .\quad (32)$$

or, by simplifying,

$$\left.\begin{aligned}x^2+y^2-2a(x\nu+y\mu)+a^2-\alpha^2a^2\phi^2&=0,\\x\mu-y\nu+\alpha^2a\phi&=0,\\x\nu+y\mu-\alpha^2a&=0;\end{aligned}\right\}\quad .\quad (33)$$

of which the first two represent the ripple, and all three are satisfied by the coordinates of its cusp. If ϕ_1 be the value of ϕ which corresponds to this cusp, we find immediately, by eliminating x and y from (32),

$$\phi_1=\frac{\sqrt{1-\alpha^2}}{\alpha}=\frac{\sqrt{u^2-\lambda^2}}{\lambda},\quad .\quad .\quad .\quad .\quad (34)$$

which will always be real and positive when u exceeds λ. This same value of ϕ might be obtained from (27) by putting $\beta=0$; by means of it and the last of equations (33) the first becomes

$$x^2+y^2=\alpha^2a^2,$$

which is the equation of the circle described by the cusp as the jet rotates. Putting r in place of $\sqrt{x^2+y^2}$, it leads to the relation

$$\frac{r}{a}=\alpha=\frac{\lambda}{u}\quad.\;.\;.\;.\;.\;.\;.\;.\;.\;.\quad(35)$$

expressed in art. 13.

To obtain simple expressions for the coordinates x_1, y_1 of the cusp corresponding to a given position of the jet, it is merely necessary to observe that the form of the present ripple does not alter as the jet rotates; so that we may refer its equation to coordinate axes which rotate with the jet,—the abscissa axis being at the angular distance $\mathrm{AOX}=\phi_1$ behind the jet (fig. 7). By so doing μ and ν become respectively $\sin(\phi_1-\phi)$ and $\cos(\phi_1-\phi)$; and the two last of the equations (33) give, on substituting for ϕ its value (34),

$$\left.\begin{aligned} y_1&=\alpha a\sqrt{1-\alpha^2},\\ x_1&=\alpha^2 a;\end{aligned}\right\}\quad.\;.\;.\;.\;.\;.\quad(36)$$

so that if we call θ_1 the angular distance AOC of the cusp C behind the jet, we have, in virtue of (35), and since $\mathrm{C\hat{O}X}=\phi_1-\theta_1$,

$$\left.\begin{aligned} &\sin(\phi-\theta_1)=\sqrt{1-\alpha^2},\ \cos(\phi_1-\theta_1)=\alpha,\\ &\frac{y_1}{x_1}=\tan(\phi_1-\theta_1)=\phi_1,\\ \textit{i. e.}\qquad &\theta_1=\frac{\sqrt{1-\alpha^2}}{\alpha}-\tan^{-1}\frac{\sqrt{1-\alpha^2}}{\alpha};\end{aligned}\right\}\quad.\;.\quad(37)$$

whence it follows that θ_1 increases as α diminishes, in other words, as u increases.

Again, since the tangent to the ripple at any point coincides with the tangent to the circular wave which there touches it, we have from the first of equations (32),

$$\frac{dy}{dx}=-\frac{x-a\nu}{y-a\mu}.\quad.\;.\;.\;.\;.\;.\;.\;.\;.\quad(38)$$

With reference to our new coordinate axes, the values of ν and μ at the cusp are 1 and 0 respectively; and these, together with the values in (36), give, when substituted in (38),

$$\left(\frac{dy}{dx}\right)_1=\frac{a-x_1}{y_1}=\frac{\sqrt{1-\alpha^2}}{\alpha}=\frac{y_1}{x_1},$$

and show that the radius vector OC is the tangent at the cusp.

The polar equation of the ripple, with reference to an axis passing through the jet and moving with it, is easily seen to be

$$\left.\begin{aligned} r^2-2ar\cos(\phi-\theta)&=\alpha^2a^2\phi^2-a^2,\\ r\sin(\phi-\theta)&=\alpha^2 a\phi,\end{aligned}\right\}\quad.\;.\quad(39)$$

where θ and ϕ are estimated in the same direction. To find the points A and B where the ripple cuts the circle, r must be replaced by a in these equations; on squaring and adding the results, it will be found that

$$\text{either}\quad \phi=0, \text{ or } \phi=2\phi_1;$$

the former has reference to the point A, and the latter to the point B. Substituting the latter value of ϕ in (39), we have in virtue of (37),

$$\tan(2\phi_1-\theta)=\frac{2\phi_1}{\frac{1}{\alpha^2}-2\phi_1{}^2}=\frac{2\tan(\phi_1-\theta_1)}{\frac{1}{\alpha^2}-\tan^2(\phi_1-\theta_1)}=\tan(2\phi_1-2\theta_1),$$

whence we conclude $\theta=2\theta_1$; that is to say, the radius OC to the cusp bisects the angle formed by the radii OA, OB to the two points where the ripple enters and leaves the circle.

Lastly, when $\alpha=1$ or $\lambda=u$, the two first of equations (33) reduce themselves to the system (28), which, as we know, represents the involute of the circle. This verification of the result contained in art. 13 may be most easily made by putting $\alpha=1$, $\beta=0$, and hence $R=0$, in the equations (25), which are equivalent to the system (33).

II. *On certain Affections of the Retina.*
By Sir David Brewster, *K.H., D.C.L., F.R.S.*[*]

In three articles published in the Philosophical Magazine for 1834[†], I have described several affections of the retina, which since that time I have frequently had occasion to study. Mrs. Mary Griffiths of New York had observed "a reticulated or network structure upon opening her eyes for the first time in the morning[‡]. "At one moment," she says, "the meshes of the network were of a dull brickdust colour, and the spaces between were of a pale dingy yellow; and in the next moment the case was reversed—the meshes and intersections were of this pale dingy yellow, and the spaces or interstices were of a dull brick-colour." She describes the meshes "as generally the *fifth* of an inch in diameter," but without telling us the distance of the surface upon which this measurement was made.

Having believed it possible to determine experimentally whether a beam of light was a continuous stream like a stream of water, or a stream in which the parts were at such a distance as to maintain a continued impression on the retina, I received a narrow beam of solar light upon a white ground, and made a strip of white card pass rapidly back and forwards across the beam,

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[*] Communicated by the Author.

[†] Phil. Mag. 1834, pp. 115, 241, and 353.

[‡] Ibid. p. 43.

comparing the luminosity of the disc which it reflects with that of the fixed circular spot upon the paper. In these experiments I observed two interesting facts,—that the reflected disc of light exhibited different colours in different parts of it, and that it was more luminous than the beam received upon the white ground when no part of it was reflected by the moving card.

Before I had examined these phenomena more minutely, I found that M. Benedict Prevost had published an interesting paper in the *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève*[*], and had anticipated me in the first of the above results. He agitated, to use his expression, a piece of white card across a sunbeam in a dark room, and he observed that the circular spot which it reflected, in place of being white, was *white* only in the centre. Around this white spot was a *violet* spot, growing deeper as it receded from the centre. The violet spot was surrounded with a zone of *deep indigo,* very well defined, and resembling the colour of the *Viola tricolor*. Round the indigo zone was one of *greenish yellow,* equally well defined, and round it a *red* shade. He considers the light as thus decomposed into seven principal colours!

"This phenomenon," he adds, "is produced by a single passage of the card across the sunbeam, which proves that it is independent of the fatigue of the eye. Nor does it depend immediately on the agitation or motion of the card, but only, without doubt, on some effect of this motion, and particularly on this, that the illuminated area strikes the eye only during a very short time; for if the card is sufficiently large, so that the illuminated space does not go out of it, and notwithstanding the agitation of it the eye continues always to see it, it appears *white* as if it was seen at rest, and there is no longer any appearance of the decomposition of the light."

Reckoning from the centre of the disc, the colours observed by M. Prevost are as follow:—

White.
Violet.
Deep indigo.
Greenish yellow.
Red shade.

In the experiments which I made, I sometimes agitated a narrow slit, or a series of parallel slits, in a black card, across a circular aperture in another black card, or across an aperture in the window-shutter of a dark room; or I looked at a white surface, or a white disc through the slits of a revolving disc, as described in one of the papers already referred to. By these

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[*] Vol. iii. part 2. p. 121.

methods I found that the white disc, or aperture, was *whitest* in the centre at certain velocities, and when the light was strong, but that it was bluish when the light was moderate. The following was the order and character of the tints, reckoning from the centre :—

White or bluish.
Darker blue.
White.
A dark ring, pretty well defined.
White.
Greenish yellow.
Reddish.

These colours vary, within certain limits, with the sensibility of the eye and the intensity of the light. I have sometimes observed the centre of the disc *darkish blue*, and sometimes *yellow*. When the velocity is great and the disc seen distinctly, there is not the slightest trace of colour.

Phenomena somewhat analogous to these may be seen in the flame of a spirit-lamp, and in other flames, where the effect is caused by the shooting up of the flame, which produces successive impulses on the retina. At the top of the flame are seen several curves, convex upwards, like elongated parabolas. They are generally of a *sap* or *olive-green* colour ; and sometimes the most brilliant *red* tints appear where the uppermost curve opens at its vertex, and leaves two lateral and almost parallel branches.

Colours similar to those discovered by M. Prevost have been recently observed by Mr. John Smith of the Perth Academy*, who draws from them what he justly calls many "startling conclusions." M. Prevost and this writer have greatly misapprehended the nature of these phenomena. While the Swiss philosopher considers them as "independent of the fatigue of the eye," and as exhibiting a new decomposition of light by motion, Mr. Smith pronounces them to be "produced by alternate light and shade in various proportions ;" and he regards them "as *proving* the non-homogeneity of æther—as *proving* the undulatory hypothesis, but *opposing* the undulatory theory—as contrary to the idea of the waves of light having different lengths—as helping to explain many of the phenomena of polarization—and as giving a new explanation of prismatic refraction" different from that of Newton!

Although we cannot adopt these conclusions, yet the phenomena, when carefully studied, as being the effect of successive impulses on the retina, acting in the manner which I

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* "On the Production of Colour and the Theory of Light," Report of the British Association at Aberdeen, 1859, Trans. p. 22.

have described in the Philosophical Magazine for 1834, have an interest of a different kind. When the luminous impressions succeed each other at a certain interval, the luminous disc, even when small, exhibits a reticulated structure like a mosaic pavement composed of distinct hexagons delineated in black lines (sometimes with a black spot in their centre), indicating that those portions of the retina are temporarily insensible to light.

In order to see this phenomenon distinctly, we must employ a large disc uniformly illuminated, such as a glass globe ground on both sides, or a plate of ground glass. When the revolving disc has a particular velocity, the illuminated surface will be seen covered with the hexagonal pattern already mentioned; and as the velocity diminishes, the pattern breaks up, exhibiting portions of circles and imperfect crosses, the exact forms of which it is difficult to describe. In daylight, when the light of the sky is used, the colours which accompany these phenomena are better seen, and also the variations which they undergo in reference to the *foramen centrale* of the retina, as described in a former paper.

When we maintain the revolving disc at that particular velocity which produces the hexagonal pattern, so that the retina may be greatly excited, a beautiful pattern of a very different kind makes its appearance, occasionally mixing itself with the first pattern, but most beautifully seen opposite the dark intervals between the slits of the disc when the velocity has become very slow, and the disc is nearly at rest. This pattern, which is too evanescent to permit it to be drawn, consists of a series of dark quadrangular spaces separated by a triple or multiple line of light. It has no relation whatever to the hexagonal pattern, and is never seen unless when the eye has been strongly impressed by the successive impulses of the luminous disc.

When we observe these phenomena at different distances from the illuminated disc, the hexagons always subtend the same angle, which I have found to correspond with a space on the retina equal to the 420th part of an inch; and there can be no doubt that they are produced by a structure of a hexagonal character. In so far as the human retina has yet been studied, no structures of a hexagonal character have been discovered. In the choroid coat, however, in front of the retina nucleated cells of a slightly hexagonal form have been seen in man and in almost all mammalia*; and it is not improbable that the parts of the retina immediately behind these hexagons may be so affected by them as to produce the hexagonal forms which we have described. With regard to the quadrangular pattern, in which the dark

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* Nunneley, 'On the Organs of Vision,' p. 171, and plate 2. fig. 7.

quadrangles are separated by several parallel bright lines, it is obviously produced by some structure in the retina itself; and it is possible that it may arise from some regular arrangement of the rods in the columnar or bacillar layer which has not yet been detected by the microscope.

The hexagonal pattern is very distinctly seen in the flame of a coal or a wood fire, at that particular part of it where the jets of ignited gas succeed each other at the proper interval.

After making these experiments for some days in succession, I have found that the hexagonal pattern, and sometimes even the quadrangular one, is seen when the eye is accidentally directed to faintly illuminated surfaces. This fact seems to show that the pattern is rendered visible merely by the excitement of the retina with the action of light, and not by its successive impulses, the black lines of the hexagons and the dark spaces of the triangles requiring a longer time to exhibit the action of the faint light than the other parts of the retina—a property which I have found at the *foramen centrale*, and at various points of the retina at or near the *ora serrata*, its anterior margin.

As an inducement to optical observers to investigate any of the abnormal phenomena of vision which may come under their notice, I give the following list of properties or structures in the retina which have been discovered, or are manifested, by optical observations.

1. The polarizing structure which produces Haidinger's brushes.
2. The insensibility of the retina at the entrance of the optic nerve.
3. The exhibition of the *foramen centrale* by its inferior sensibility to feeble light.
4. The different sensibility to light possessed by different parts of the retina.
5. The inability of the retina beyond the foramen to maintain a sustained vision of objects.
6. The increased luminosity of objects seen indirectly, or by the parts of the retina beyond the foramen.
7. The hexagonal and quadrangular structure described in the preceding pages.

To these we may add the existence, in the vitreous humour, of the remains of vessels no longer required for its support, and the existence of cells in the same humour, as proved by the phenomena of Muscæ volitantes, formerly described in this Magazine.

Allerly by Melrose,
November 30, 1860.

III. *Proposal for a new reproducible Standard Measure of Resistance to Galvanic Currents.* *By* M. WERNER SIEMENS*.

[With a Plate.]

THE want of a generally received standard measure of current resistance, and the great inconveniences thence arising, especially in technical physics, suggested to me some years ago the experiment I am about to describe.

My original object was to procure for Jacobi's standards a more general introduction into the arts. I soon found, however, that this could not be effected without inconvenience. On one occasion, several of Jacobi's standards that I had procured were so entirely at variance with each other, and their actual resistances agreed so little with what they ought to have been, that I should have been obliged to have had recourse to Jacobi's original measure, only that it was not then at my disposal. Independently of this, however, I became convinced that a standard measure of resistance is only adapted for general use when it is easily reproducible. Whether the resistance of a metal wire is altered by time, by the shaking of transport, by the passage of currents, or by any other cause, is not yet absolutely decided. It is, however, very probable that some such alteration takes place, and it is therefore altogether inadmissible to take the resistance of such a wire as the unit-measure of resistance. Moreover such standards being copies one of another, as must unavoidably be the case in the event of their general adoption, their deviation from accuracy would be continually increasing; and for the purpose of researches which are to be conducted with improved instruments and with great accuracy, mere copies which are themselves inaccurate are useless. Besides, it would be very desirable and convenient to be able to unite a definite geometrical conception with the standard measure of resistance, which could never be the case with a metallic wire, since the resistance of a solid body depends greatly on its molecular state, and on the almost unavoidable impurity of the metal.

The absolute standard measure appeared to me equally ill adapted to general use. It can only be reproduced by means of very perfect instruments, in places especially arranged for the purpose, and by those possessed of great manual dexterity; and moreover it is liable to the grave practical objection, that it does not exhibit itself in a physical form; and lastly, the numbers it involves are exceedingly inconvenient on account of their magnitude.

The only practicable method of establishing a standard measure of resistance which should satisfy all requirements, and

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* Translated by Mr. F. Guthrie from Poggendorff's *Annalen*, No. 5, for 1860.

especially which any one could reproduce with ease and with sufficient accuracy, seemed to me to be to adopt the resistance of mercury as the unit. Mercury can easily be procured or rendered of sufficient, almost indeed of perfect purity. As long as it is fluid, it has no different molecular states affecting its conducting power; its resistance is more independent of temperature than that of any other simple metal; and finally, its specific resistance is very considerable, so that numerical comparisons founded on it as a standard are small and convenient.

I therefore determined to try if it were possible, by means of the ordinary glass tubes of commerce and purified mercury, to obtain by suitable methods fixed standard measures of resistance of sufficient accuracy. The great difficulty seemed to be the impossibility of obtaining glass tubes of a form exactly cylindrical. The diameter of the internal cavity of ordinary glass tubes generally varies irregularly. By gauging them, however, with a thread of mercury, some pieces of about 1 metre long may be selected out of a great number, the diameters of which vary almost uniformly. Such tubes as these may be regarded as truncated cones, the resistance of which can be calculated. The volume of the cone filled with mercury can be determined with great ease and accuracy by weighing the metal contained.

Let A B C D, Plate II. fig. 1, be such a truncated cone, r and R being the radii of its upper and lower circular extremities, and l its length. Let M N be a section parallel to the flame A B, and at the distance x from it; let dx be the thickness of this section, and z its radius. Then, if W be the resistance of the cone in the direction of its axis, and dW the resistance of the element MN in the same direction,

$$dW = \frac{dx}{\pi z^2}.$$

But

$$z = \frac{(R-r)x}{l} + r.$$

Therefore, differentiating with respect to x,

$$\frac{dz}{dx} = \frac{R-r}{l},$$

whence

$$dx = \frac{l}{R-r}\,dz;$$

and by substituting this value of dx in the first equation, we get

$$dW = \frac{l}{(R-r)\pi}\,\frac{dz}{z^2};$$

whence, integrating with respect to z,

$$\mathrm{W}=\int_r^{\mathrm{R}}\frac{l}{(\mathrm{R}-r)\pi}\frac{dz}{z^2}=\frac{l}{(\mathrm{R}-r)\pi}\left(\frac{1}{r}-\frac{1}{\mathrm{R}}\right),$$

or

$$\mathrm{W}=\frac{l}{\mathrm{R}r\pi}. \quad \dots\dots\dots\dots \quad (1)$$

If, now, V be the volume of the truncated cone, G the weight of the mercury contained in it, and σ its specific gravity, then

$$\mathrm{V}=(\mathrm{R}^2+\mathrm{R}r+r^2)\frac{l\pi}{3};$$

and dividing both sides by $\mathrm{R}r$, we have

$$\frac{\mathrm{V}}{\mathrm{R}r}=\left(\frac{\mathrm{R}}{r}+1+\frac{r}{\mathrm{R}}\right)\frac{l\pi}{3};$$

or calling $\frac{\mathrm{R}^2}{r^2}=a$,

$$\frac{\mathrm{V}}{\mathrm{R}r}=\left(\sqrt{a}+1+\frac{1}{\sqrt{a}}\right)\frac{l\pi}{3},$$

whence

$$\mathrm{R}r=\frac{\mathrm{V}}{l\pi}\frac{3}{\left(\sqrt{a}+1+\frac{1}{\sqrt{a}}\right)};$$

and putting for V its value $\frac{\mathrm{G}}{\sigma}$,

$$\mathrm{R}r=\frac{\mathrm{G}}{l\pi\sigma}\frac{3}{1+\sqrt{a}+\frac{1}{\sqrt{a}}},$$

which, substituted in equation (1), gives

$$\mathrm{W}=\frac{l^2\sigma}{\mathrm{G}}\frac{1+\sqrt{a}+\frac{1}{\sqrt{a}}}{3}. \quad \dots\dots \quad (2)$$

The value of W found from this equation is obviously correct for every conductor of pyramidical form, as long as a represents the ratio of the greatest and least sections. It is moreover equally true when, instead of a single truncated cone of length l, any number of equal cones be substituted whose collective length is l, provided only that in each the ratio of the greatest to the least section, or its reciprocal, is a.

For in this case, if

$$l=n\lambda,$$

where λ is the length of one of the cones,

$$\mathrm{W}=n\frac{\lambda\sigma}{\frac{\mathrm{G}}{n}}\cdot\frac{1+\sqrt{a}+\frac{1}{\sqrt{a}}}{3},$$

or

$$W=\frac{n^2\lambda^2\sigma}{G}\;\frac{1+\sqrt{a}+\frac{1}{\sqrt{a}}}{3},$$

or

$$W=\frac{l^2\sigma}{G}\;\frac{1+\sqrt{a}+\frac{1}{\sqrt{a}}}{3}.$$

When R and r are nearly equal, the correction for the conical form of the conductor, viz. the factor

$$\frac{1+\sqrt{a}+\frac{1}{\sqrt{a}}}{3} \text{ or } \frac{1+\frac{R}{r}+\frac{r}{R}}{3},$$

differs little from unity; whence every tube not accurately cylindrical may be regarded as a truncated pyramid without noticeable error, the ratio a being determined from the greatest and least lengths of the mercury thread employed in gauging its capacity.

By a series of experiments I now ascertained whether the calculated amount of the resistances of a number of tubes of very different mean sections agreed sufficiently well with the results of measurement. The method I pursued was as follows:—

Glass tubes, from about $\frac{3}{4}$ to 2 millims. internal diameter, were fastened to a long graduated scale, drops of mercury were introduced into them, and the length of the thread of mercury in the tube was measured. By inclining the tube under examination, the thread of mercury was made to pass gradually down its entire length; and thus every piece of tube of about 1 metre long which appeared most nearly cylindrical or uniformly conical, was detected. These pieces were then cut out of the tube, and, by means of a small apparatus constructed for that purpose by M. Halske, were reduced to the exact length of 1 metre. The tubes so prepared were then carefully cleaned. This was most conveniently done by twisting together two thin German silver or steel silk-covered wires, inserting them through the tube, twisting a pad of clean cotton to the projecting end, and drawing it slowly and cautiously through the tube. This operation required to be performed with some care to avoid, breaking the tube. The tube was then filled with clean mercury and its contents weighed. This operation was conducted as follows:—One end of the glass tube was fastened, by means of a piece of vulcanized caoutchouc, to the opening of a small retort receiver, such as is used in chemical laboratories, so that the end of the tube projected into the receiver. About the other end of the tube an iron collar was attached (fig. 2), by means of which a smooth iron plate could be screwed up against the mouth of the tube. The receiver was then properly secured and filled with clean mercury, which was suffered to run down the slightly inclined tube into a vessel placed below. When all the air-bubbles, which at first appeared adhering to the sides of the tube, seemed to be carried away by the descending stream of mercury, the lower orifice of the tube was tightly closed by means of the screw and plate; the tube was placed in a vertical position, and its upper end withdrawn from the caoutchouc covering. When this was done with care, the vertical tube was found entirely filled with a column of mercury which terminated in a projecting hemisphere of the metal. The upper orifice was next closed by pressing against it a glass plate ground flat, the superfluous metal being thus removed. The tube being then freed, by means of a brush, from any globules of mercury that might have adhered to its surface, was emptied into a glass vessel, and its contents weighed on an accurate chemical balance. When the precaution was taken of letting the mercury flow slowly from the tube by inclining it very slightly, and suffering the air to enter gradually at the upper orifice, it was found that no globules were left behind in the tube, as is generally the case under other circumstances. Warming the tube when full by contact with the naked hands was of course avoided. The temperature at the time the tube was filled was observed, and the weight of the contents corrected for the difference of this above 0° C. Of the following Tables, Table I. gives the lengths of the threads of mercury observed in gauging the capacity of the tubes employed, and the ratio *a* of the greatest and least section thence determined. Table II. the weight of the mercury at the actual temperature, and the same as corrected for 0° C.

Table I.

	1.	2.	3.	4.	5.	6.
	125·0	101·2	48·2	143·0	115	111
	116·4	98·4	47·5	145·0	116	109
	115·3	96·9	45·0	146·0	119	107
	114·0	94·5	45·0	145 0	121	105
	112·0	94·0	44·8	143·5	121	105
	110·2	93·3	44·2	142·5	122	103
	108·2	94·5	43·9	142·5	121	101
	107·0	95·7	43·7	140·0	120	100
	107·0	97·5	42·5	139·0	119	101
	106·0	99·4	41·0	……	……	102
		101·1	40·1	……	……	100
Therefore	$a = \frac{125}{106}$	$\frac{101{\cdot}2}{93{\cdot}3}$	$\frac{48{\cdot}2}{40{\cdot}1}$	$\frac{146}{139}$	$\frac{122}{115}$	$\frac{111}{100}$

And consequently the respective correction factors were—

1.	2.	3.	4.	5.	6.
1·00225	1·00055	1·00282	1·000201	1·000289	1·000906

Table II.

1.	2.	3.	4.	5.	6.
13·208	27·1915	24·3825	62·368	69·802	11·767
13·210	27·1900	24·3830	62·366	69·796	11·768
13·209	27·1915	24·3840	62·357	69·803	11·767
13·209	27·1920	24·3833			
at	at	at	at	at	at
13°·5 R.	14° R.	13°·5 R.	18° R.	14°·7R.	15°·2 R.
			61·395	69·795	11·776
			62·398	69·795	11·777
			63·393	69·794	11·774
					11·774
			at	at	at
			14°·5 R.	18° R.	14°·7 R.

Weight in grammes at 0°.

1.	2.	3.	4.	5.	6.
13·2491	27·277	24·457	62·774	70·057	11·808

If in the formula (2) for the resistance, found above, viz.

$$W=\frac{l^2\sigma}{G}\,\frac{1+\sqrt{a}+\dfrac{1}{\sqrt{a}}}{3},$$

we substitute the values of G (in milligrammes) and the correction factor as determined by these Tables, and if we take for the specific gravity of mercury at 0°

$$\sigma=13{\cdot}557,$$

and for the mean length of all the tubes

$$l=1000 \text{ millims.},$$

then we shall have the resistance of the tubes expressed in terms of the resistance of a cube of mercury 1 millim. each way. Table III. gives the values of W so calculated.

Table III.

1.	2.	3.	4.	5.	6.
1025·54	497·28	555·87	216·01	193·56	1148·9

The resistance of these tubes filled with mercury at 0° was next compared with one of Jacobi's standards (B), by means of a Wheatstone's bridge. As the bridge in question, in the form used by Halske and myself, is adapted to very accurate measurements, its more particular description will not be without interest.

Fig. 3 is a perspective view of the bridge. A A is a brass frame on which the slide B B moves. The button C on the slide B B is provided with a toothed wheel, which works into a toothed rack attached to the frame. The slide may therefore be moved either by direct pressure or by turning this button. Attached to the frame are the insulated pieces of metal E E, and the graduated scale *m m* divided into millimetres. Between the insulated pieces of metal E E, whose inner surfaces are perpendicular to the scale *m m*, and are exactly 1000 millims. apart, a platinum wire of about 0·16 millim. in diameter is stretched. This wire, the ends of which correspond exactly with the division marks 0° and 1000 of the scale, is clasped by two small platinum rollers, whose axes are connected with the slide B by means of the springs G. The bodies whose resistances are to be compared are inserted between the metallic band H, which can be connected by means of the contact lever I with one pole of the battery, and the two thick copper rods, L and L, which move freely in the eyes K and K. The other pole of the battery (a single-celled Daniell's battery was generally employed) is connected with the slide B and the platinum roller. The eyes K K, and the insulated pieces E E, which serve as points of attachment for the platinum wire, are placed in perfect connexion, by means of thick copper rods, with the four plates of the plug reverser S. By changing the plugs, the two resistances to be compared could be exchanged. The ends of the multiplying wire of the galvanometer employed are connected with the pieces of metal E E. In the following measurement I used a mirror galvanometer, with a round steel mirror 32 millims. in diameter, and 36,000 coils of a copper wire 0·15 millim. thick. The distance of the scale divided into millimetres from the mirror was about $6\frac{1}{2}$ metres.

The measurements obtained by means of this apparatus, which are collected together in the following Table, were for the most part determined by Dr. Esselbach. The method he pursued was as follows:—

Each end of the glass tube to be tested was inserted in a receiver, and retained there by means of a caoutchouc band. This receiver was so placed that the unused neck projected upwards, and in this position it was, together with the tube, plunged into a trough filled with lumps of ice. One of these receivers was then supplied with clean dry mercury, which filled the tube and ran through it into the other. By the time the mercury in the two vessels was at the same level, the tube was generally found free from air-bubbles. Thick amalgamated copper wires were now inserted into the mercury through the top of the receivers, and the resistance of the tube was compared with that of one of Jacobi's standards by means of the bridge above described*.

The resistance of the conducting rods was determined by plunging the amalgamated copper cylinder into a vessel filled with mercury. It was found to be quite evanescent in comparison with the resistance of the tubes.

The experiments whose results are collected in the following Table were conducted as follows:—In the first position of the commutator, the slide B B was moved to such a position that, on lowering the contact lever I, the galvanometer exhibited no permanent deviation. The resistances to be compared were then changed by means of the commutator, and the slide was again adjusted. These two positions of the slide are given in the columns headed *a* and *b*. If the observations were free from error, the sum of these magnitudes would be exactly 1000, which was generally the case, or very nearly so. We must here remark that, after establishing the equality of the currents, a small temporary deviation of several divisions, indicating a greater resistance on the part of the closely packed coils of Jacobi's standard, was observed immediately on completing the circuit. As on breaking the circuit an opposite deviation of the same magnitude resulted, this phenomenon was obviously to be attributed to the extra current of the wire coil of the Jacobi's standard. It appeared, moreover, that on a long continuance of the current, the mercury in the tube began to grow warm, even though only a single cell of a Daniell's battery was employed. On account of the slow oscillation of my mirror and the resistance encountered by its prolongations, the error arising from this cause was easily eliminated by allowing only short currents to traverse the instrument. The slide was always so placed that, immediately on completing the circuit, there was a slight movement towards the left, which, on the mercury becoming warm owing to the continuance of the current, gradually passed over

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* At first, instead of amalgamated copper wires, iron cylinders were used as conductors; under these circumstances, however, we found that there was a very considerable resistance to the passage of the current from the iron to the mercury, even though the surface of the former was perfectly clean. This resistance, which was also considerable when unamalgamated copper wire was employed, was particularly strong when the cylinder had been some time exposed to the atmosphere after having been cleaned, so that it is probable that this phenomenon is to be attributed to the gaseous condensation on the surface of the metal.

into a deviation to the right. By again slightly altering the position of the slide, the first movement to the left could be rendered so small as to be imperceptible, and the effect of the change of temperature could thereby be entirely obviated.

Table IV.

Tubes	1.		2.		3.	
	a.	*b.*	*a.*	*b.*	*a.*	*b.*
Observed resistances	605·7	394·3	429·1 429·0	570·9 571·1	456 456·3 456·2 456·2	543·7 543·6 543·3 543·6
Mean value	605·7	394·3	429·05	571·0	456·2	543·6
For $b=1$............	1·536	......	0·7514	......	0·8392	
W_1	1016·52	......	497·28	......	555·38	
$\frac{W}{W_1}$	1·008	......	1·00	......	1·0008	

Tubes	4.		5.		6.	
	a.	*b.*	*a.*	*b.*	*a.*	*b.*
Observed resistances	247·6	752·6	227·4 227·3	772·8 772·8	633·2 633·15 633·10	366·8 366·85 366·90
Mean value	247·6	752·6	227·35	772·8	633·15	366·85
For $b=1$	0·329	......	0·2942	......	1·726	
W_1	217·73	......	194·7	......	1142·3	
$\frac{W}{W_1}$	0·992	......	0·994	.....	1·005	

The line distinguished by the letter W_1 is found from the preceding one by multiplication by 661·8, which number was furnished by comparing the calculated resistance of tube 2 with that of the Jacobi's standard employed. The numbers in this line ought therefore to agree with the resistances as derived from calculation contained in Table III. The numbers in the line headed $\frac{W}{W_1}$, which are the ratios of the calculated and observed resistances, show that these magnitudes do not differ more than was to be expected. The most considerable errors in our measurements arose from the fact that neither the temperature of the mercury nor that of the copper standard was constant. The temperature of the ice-water varied between 0° and 2° C., and that of the standard between 19° and 22° C.; and as the conductibility of copper is diminished about 0·4 per cent. for every degree Centigrade, the differences in the above measurements, which are all under 1 per cent., are thereby fully accounted for; and there can be no doubt that in this way standard measures of resistances can be reproduced of any degree of accuracy.

The observed resistances in Table IV. ought strictly to have been diminished by the resistance of the mercury in the glass vessel to the spreading of the current, that is, by the resistance encountered by the current in passing from the orifice of the tube to the amalgamated copper conductors. This resistance may, without any considerable error, be taken as the resistance of a hemispherical shell, whose lesser radius is equal to the inner radius of the tube, and whose greater radius, on account of its comparative magnitude, may be taken as infinite. Now the resistance of a hemispherical shell of radius x and thickness dx being called $d\mathrm{W}$, we have

$$d\mathrm{W}=\frac{dx}{2x^2\pi},$$

whence

$$\mathrm{W}=\int_r^\infty\frac{dx}{2x^2\pi}=\frac{1}{2r\pi}=\frac{r}{2r^2\pi}.$$

The resistance to the spreading of the current in the mercury at both ends of the tube is therefore equal to the resistance of a portion of the tube whose length is half its diameter. This resistance ought to be still further increased, because the surface of the mercury in the tube is flat and not hemispherical, as is assumed in the above calculation; but these two sources of error are each of them so trifling, that their joint effect may be neglected.

The straight tubes used in the experiments hitherto described cannot be very conveniently employed as standards; I therefore got M. Giessler of Berlin to make me some spiral glass tubes, having their extremities turned up and provided with small vessels in which to receive the conducting wires. These glass spirals were fastened, as shown in fig. 4, into the wooden cover of a broad glass vessel filled with water. The temperature of the water in the vessel was observed by means of a thermometer introduced through an opening in the cover. The glass spirals were easily filled with mercury, so as to be free from air-bubbles, in the following manner:—The orifice of the tube in one of the glass vessels was first stopped by means of a suitably shaped cork; the other vessel was then filled with mercury, the air being suffered to escape gradually by the cork at the other end, which was only removed when the mercury had slowly passed entirely through the windings of the tube.

As mercury is not to be found in the list of metals the alteration of whose specific resistance by heat has been determined by Arndtsen*, this deficiency had first to be remedied. This was effected by Dr. Esselbach, by means of the apparatus already described. The resistance of one of the spiral tubes was compared with that of the straight tube 2, first at the temperature of ice-water, and then when the temperature of the spiral was raised. If w represent the resistance of tube 2 (which according to Table III. is 498·7), and $w_{\prime}$ the resistance of the spiral tube, then, since the resistance of the conducting wires was rendered equal for both tubes, and was equivalent to that of 11 cubes of mercury 1 millim. each way, we had

$$\frac{w_{\prime}+11}{w+11}=\frac{a}{b},$$

where a and b represent the pieces of platinum wire of the bridge when no current passed through the galvanometer. This was the case when

$$\frac{a}{b}=\frac{311{\cdot}3}{688{\cdot}7},$$

whence

$$w_{\prime}=219{\cdot}4.$$

The temperature of the straight tube was now maintained at 0° by means of melting ice, while the temperature of the water surrounding the spiral was raised. In the following Table, t indicates the temperature of the straight, and $t_{\prime}$ of the spiral tube, a and b the lengths of wire read off when the currents were equal, y the required coefficient calculated according to Arndtsen's formula,

$$\frac{w_{\prime}(1+yt_{\prime})+11}{w(1+yt)+11}=\frac{a}{b}.$$

Table V.

t.	$t_{\prime}$.	a.	b.	y.
0°	47°·0 C.	320·4	679·5	0·000964
0	34·5	318·0	682·0	0·000960
0	16·5	314·6	685·4	0·000981

From this it appears that, of all simple metals, mercury is that whose resistance is least increased by an increase of temperature.

By means of the coefficient y, the resistances of the two other spirals A and B were determined, which were afterwards used

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[*] Poggendorff's *Annalen*, vol. cii. p. 1.

in constructing a standard of German silver wire. The resistance of spiral A at 0° was 514·45, that of B 673·9.

German silver wire is very well adapted for standards of resistance, because its conducting power is very small, and varies little with the temperature; according to Arndtsen, only about ·0004 per cent. for a degree Centigrade.

In the foregoing experiments, the resistance of a cube 1 millim. each way has been assumed as the unit. For small resistances, and especially in calculation, this has many conveniences. It appears, however, more advisable that the measure of resistance should be in entire agreement with the ordinary metrical scale. I therefore determined to take as the unit of resistance,—

The resistance of a prism of mercury 1 *metre long and* 1 *millim. in section at the temperature* 0° C.

If this proposal be generally acted on, all resistances can be at once reduced to the metrical scale. Every experimenter will then be able to provide himself with a standard measure as accurate as his instruments permit or require, and to check the variations of resistance of the more convenient metal standard. Of course also, in that case, the conducting power of mercury must be taken as the unit of conducting power, and not that of copper or silver. Unfortunately but few comparisons have been made between the conducting power of mercury and the solid metals, from which a table could be calculated in which the conducting power of mercury should be the unit; and in most of the comparisons that have been made between the conducting power of the solid metals, it is not stated whether the wire employed was cold drawn or annealed. From the following Table, however, it appears that the conducting power of annealed wire is considerably greater than that of unannealed.

1.	2.	3.	4.	5.	6.
Species of wire.	Length in millims.	Weight in milli-grammes.	Specific gravity.	Resistance at 0° C.	Conducting power, that of mercury = 1.
1. Silver, hard	4014·4	4884·9	10·479	614·55	56·252
,, annealed	4014·4	4889·1	10·492	537·2	64·38
2. ,, hard	4014·4	3233·1	10·502	896·1	58·20
,, annealed	4014·4	3009·6	10·5132	889·08	63·31
3. Copper, hard...........	4014·4	3099·5	8·925	890·5	52·109
4. ,, hard...........	4014·4	4409·1	8·916	622·7	52·382
,, annealed	4014·4	4355·2	8·903	599·05	52·013
5. ,, hard...........	2007·2	1260·4	8·916	545·8	52·217
,, annealed	2007·2	1252·7	8·894	517	55·419
6. ,, hard...........	2007·2	1263·2	8·916	545·6	52·121
,, annealed	2007·2	1241·5	8·894	520·8	55·338
7. Platinum, hard	436·4	544·1	21·452	910·6	8·244
8. ,, hard	436·4	550·1	21·452	897·7	8·27
9. Brass, hard	1003·6	1406·1	8·473	530·6	11·439
,, annealed.........	1003·6	1397·8	8·464	451·7	13·502

It appears from this Table that the specific conducting power of annealed silver wire is 10 per cent. greater than that of the same wire unannealed; that of annealed copper wire on the average 6 per cent. greater. The difference of conducting power is especially great in the case of brass. As the density of wire depends on the amount it has been stretched since the last heating, that, as well as the conducting power, must vary much even when the metal is perfectly uniform. The temperature to which the wire was raised, the duration of the operation, and the rapidity of the cooling, all affect its specific conducting power. Column 5 of the above Table was calculated by means of the formula

$$\mathrm{W}=\frac{l^2\sigma}{\mathrm{Q}}\,\frac{a+\sqrt{a}+\dfrac{1}{\sqrt{a}}}{3}.$$

The factor $\frac{a+\sqrt{a}+\frac{1}{\sqrt{a}}}{3}$, the correction for the conicality of the conductor, may, in the case of metal wires, be almost always neglected, since it never differs sensibly from unity. This method is obviously much more exact than that hitherto employed, in which the mean diameter of the wire had to be determined by direct measurement; and the square of the magnitude so obtained, which was never exact, entered into the calculation. By my method all the data may be determined with great accuracy, especially the length of the wire, which alone enters in the square.

If the above Table be compared with that of Arndtsen, it appears that the mean conducting power of unannealed platinum wire, viz. 8·257, and the smallest value found for unannealed silver wire, viz. 56·252, are exactly in the ratio given by Arndtsen; while the resistance of copper wire according to Arndtsen, agrees pretty well with that of annealed copper in the above Table. As the silver and platinum wires employed both by myself and Arndtsen were chemically pure, I have in the following Table taken the resistance of platinum and hard silver as a standard. The values taken from Arndtsen's Table are indicated by an A, those observed by myself by an S.

Table VI.

Conducting power of the metals at the temperature t compared with that of mercury at 0° C.

Mercury	$\frac{1}{1+0{\cdot}00095\,.\,t}$ (S).
Lead	$\frac{5{\cdot}1554}{1+0{\cdot}00376\,.\,t}$ (A).
Platinum	$\frac{8{\cdot}257}{1+0{\cdot}00376\,.\,t}$ (A. S).
Iron	$\frac{8{\cdot}3401}{1+0{\cdot}00413.t+0{\cdot}00000527.t^2}$ (A).
German silver	$\frac{10{\cdot}532}{1+0{\cdot}000387.t-0{\cdot}000000557.t^2}$ (A).
„ annealed	4·137 (S).
Brass, hard	11·439 (S).
„ annealed. . . .	13·502 (S).
„ „	$\frac{14{\cdot}249}{1+0{\cdot}00166.t-8{\cdot}00000203.t^2}$ (A).
Aluminium	$\frac{31{\cdot}726}{1+0{\cdot}003638.t}$.
Copper	$\frac{55{\cdot}513}{1+0{\cdot}00368.t}$ (A).
„ hard	52·207 (S).
„ annealed. . . .	55·253 (S).
Silver, hard	$\frac{56{\cdot}252}{1+0{\cdot}0003414\,.\,t}$ (A. S).
„ annealed . .	64·38 (S).

For convenience I have given Arndtsen's results, together with the coefficients for correcting the resistances for the temperature. Whether these are the same for annealed and unannealed wires I have not been able to determine. The brass that I experimented on was found on analysis to consist of 29·8 per cent. zinc, and 70·2 copper.

In conclusion I should mention, for the benefit of those who may wish to make standard measures of resistance in the manner here described, that it is necessary to warm the mercury to be employed for several hours under a covering of concentrated sulphuric acid mixed with a few drops of nitric acid, to get rid of all metallic impurities, as well as the free oxygen, which greatly *increase* its conducting power.

IV. *Remarks on* Mr. Harley's *paper on Quintics.*
By G. B. Jerrard.

[Concluded from vol. xix. p. 274.]

4. In my former paper I contented myself with showing, from a comparison of certain results at which Mr. Harley had arrived, that there were decisive marks of the existence of an error in his processes, leaving to him and Mr. Cockle the task of tracing the error to its source. But after the lapse of many months they have as yet failed to perform the part thus fitly assigned to them. This is the more remarkable, as, in answer to an attempt made by the latter mathematician to infer the failure of my researches from the failure of theirs, I said at the time, in a postcript to my paper, what ought, I think, to have suggested to their minds the true origin of their error, and thence have shown them the irrelevancy of the objections urged against my method of solving equations of the fifth degree. Mr. Cockle, however, not seeing the meaning of my words, still adheres to his objections. I must now, therefore, abandoning suggestions the drift of which may escape observation, have recourse to statements of the plainest kind.

5. If I were asked in what respect the method first explained by me in the Philosophical Magazine for June 1845, and afterwards given at greater length in Chapter II. of my 'Essay on the Resolution of Equations*,' differed from all other methods constructed for the purpose of solving equations,—on what, while all these were seen to have failed, I grounded my hope of success, —I should answer by pointing to the theorem†

$$\Theta_{n,f}(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots=$$

$$\left.\begin{array}{ll} & \dfrac{\iota^{\zeta}}{5}(x_{\alpha}+\iota^{4n}x_{\beta}+\iota^{3n}x\;\;+\iota^{2n}x_{\delta}+\iota^{n}x_{\epsilon})(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots \\ \text{if} & \Theta_{n,f}(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots=\iota^{\zeta}(\Theta'_{n,f})(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots \end{array}\right\}\quad .\qquad \text{(v)}$$

or

$$\Theta_{n,f}(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots=$$

$$\left.\begin{array}{ll} & \dfrac{\iota^{\eta}}{5}(x_{\alpha}+\iota^{n}x_{\beta}+\iota^{2n}x_{\gamma}+\iota^{3n}x_{\delta}+\iota^{4n}x_{\epsilon})(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots \\ \text{if} & \Theta_{n,f}(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots=\iota^{\eta}(\Theta''_{n,f})(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\ldots \end{array}\right\}\quad ..\qquad \text{(w)}$$

From this theorem we learn that, in constructing equations

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* Published by Taylor and Francis, Red Lion Court, Fleet Street, London.

† 'Essay,' p. 75.

between functions of the form

$$\Theta_{n,f(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\,\cdot\cdot}$$

and those of the class

$$\frac{\iota^k}{5}(x_\alpha+\iota^l x_\beta+\cdot\cdot)(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\cdot\cdot,$$

a complex process of a peculiar character must take the place of the simple method of substitutions.

Thus from (v_2) we see that

$$\Theta''_{n,f(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta})}=(\Theta'_{n,f})(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta}),$$

and analogously from (w_2), that

$$\Theta'_{n,f(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta})}=(\Theta''_{n,f})(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta});$$

in each of which equations the Θ of one member is accented differently from the Θ of the other in consequence of a transfer of Θ' and Θ'' from branch to branch for the affix $(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta})$.

This shows the necessity of carefully distinguishing between

$$\Theta_{n,f(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\,\cdot\cdot} \quad\text{and}\quad (\Theta_{n,f})(\underset{\cdot\cdot}{\mathrm{a\,b}})(\underset{\cdot\cdot}{\mathrm{c\,d}})\cdot\cdot$$

if we would avoid admitting relations among the roots through equations of the class

$$(\Theta''_{n,f})(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta})=(\Theta'_{n,f})(\underset{\cdot\cdot}{\beta\,\epsilon})(\underset{\cdot\cdot}{\gamma\,\delta}).$$

I might go on to elicit other properties of the theorem (v, w), but what I have already said is, I think, sufficient for my purpose.

6. Turning now to Mr. Harley's paper, I find that the very existence of the theorem (v, w) is ignored. No wonder then was it—where so much circumspection was needed, and such a safeguard against fallacies as the theorem (v, w) was flung aside—that he as well as Mr. Cockle fell into error.

7. But Mr. Cockle, not reflecting that the functions

$$\phi_n(x_1,\, x_2,\, \cdot\cdot\, x_5), \quad \chi_n(\dot{x}_1,\, \dot{x}_2,\, \cdot\cdot\, \dot{x}_5)$$

(wherein $\dot{x}_1, \dot{x}_2, \cdot\cdot\, \dot{x}_5$ are known to be such as not to involve any irreducible radical of the form $\sqrt[5]{z}$, while $x_1, x_2, \cdot\cdot\, x_5$ are not proved to be subject to any condition) do not come within the scope of Lagrange's theory for expressing one of two homogeneous functions of the roots of a *given equation* in rational terms of the other, attempts by means of that theory to apply objections drawn from the failure of the method given in Mr. Harley's paper to the method of solution in my 'Essay.' The nature of Mr. Cockle's second error is now manifest. He implicitly assumes as a direct consequence of Lagrange's theory, that he is permitted to express*

$$\chi_n(\dot{x}_1, \dot{x}_2, \ldots \dot{x}_5)$$

as a rational function of

$$\phi_n(x_1, x_2, \ldots x_5).$$

Such an assumption is clearly inadmissible. It is not a little remarkable that a most striking confirmation of the validity of my method may be derived from that very theory†.

8. Again, the objection which he urges against a statement of mine in reference to Taylor's theorem, is founded on a misapprehension of my meaning. The statement relates to equations of condition‡. But Mr. Cockle, ignoring their existence, speaks of a remainder. It would be easy, from what has been said in art. 7, to show the irrelevancy of the rest of his objections. It seems needless, however, to proceed further.

December 1860.

V. *On the Principles of the Science of Motion (Mechanics, Physics, Chemics)* §. *By* J. S. STUART GLENNIE, *M.A.*

1. IT is proposed in the following paper to give a brief introductory exposition of the conceptions by the analytical application of which it has been found that the laws and methods of mechanics can be more fully applied to the phenomena of physics and chemics.

2. In order that mechanical principles may be rigorously applied to physical and chemical phenomena, it seems clear that physical and chemical forces must be conceived in the same way as mechanical forces. Now a mechanical force, or the cause of a mechanical motion, we know to be, in general, the condition of a difference of pressure. Whether the motion is uniform, or accelerated, depends on whether the force, or difference, is instantaneous, or continuous. And whether that difference is between polar or extreme pressures, depends on whether the moved body transmits pressure with, or without, a tangential resistance. Hence the condition of the translation of a solid is

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* See his paper in the Philosophical Magazine for March 1860. The same error runs through all his subsequent papers on the subject of equations, and thus renders nugatory every objection based on Mr. Harley's paper.

† Compare arts. 107, 112 of my 'Essay.'

‡ See note on art. 44 of my paper for June 1845.

§ Communicated by the Author, being a restatement of principles enounced in papers read at the two last Meetings of the British Association, and of which abstracts have been published in the 'Reports' for 1859 and 1860 (Trans. of the Math. and Phys. Section).

a difference of polar pressures; and of the motion of a fluid, a difference of maximum and minimum pressures; and similarly, it is evident that the causes of the other mechanical motions are, in fact, conditions of pressure. Thus, rotation is the effect of equal, a compound motion of translation and rotation of unequal, differences of polar pressures in opposite directions with respect to a line joining the points of application of each pair of polar pressures. It need hardly be said that it is not proposed that we should use the phrase "differences of polar pressures" instead of the term "forces." But, preparatory to a mechanical consideration of physical and chemical motions, it seems of importance clearly to see that, whatever force may be absolutely, whatever the unknowable ultimate cause of motion may be, or have been, the ordinary mechanical forces* called "a single force at the centre of gravity," "a couple," and "a single force not at the centre of gravity," are, in fact, such conditions of pressure as above stated.

3. Thus, as the cause of a mechanical motion is, in general, the condition of a resultant difference of the pressures on the moved body, and as the general law of the motion of the body is, that it is in the direction of the greater of the two opposed resultant pressures, if the great object of modern physical research is accomplishable, it must be attained by showing that physical forces are, like mechanical forces, conceivable as differences of pressure under certain characteristic but interchangeable conditions, and that, for instance, the movement of iron towards a magnet, or the movement of a paramagnetic body towards a place of stronger action, are motions in the direction of least resistance. And there is evidently this further condition of a general mechanical theory of physical and chemical phenomena —that pressure be, in physics and chemics, conceived, as in mechanics, as "a balanced force†," or "momentum virtually and not actually developed‡." Hence it appears that, if a general mechanical theory is possible, the ultimate property of matter must be conceived to be a mutual repulsion of its parts, and the indubitable Newtonian law of universal attraction be herefrom, under the actual conditions of the world, deduced.

4. The general experimental condition of the fitness of the mechanical conception of pressure as the basis of a general physical and chemical theory, evidently is that there be a plenum.

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* "Toutefois, c'est une chose très-remarquable, qu'un même livre, écrit sur la science des forces, pourrait, sans cesser d'être exact et de traiter régulièrement la même science, être entendu de deux manières différentes, selon qu'on attacherait au mot de *force* l'idée d'une cause de translation, ou l'idée toute différente d'une cause de rotation."—Poinsot, *Théorie Nouvelle de la Rotation des Corps*, p. 13.

† Rankine, 'Applied Mechanics.'

‡ Price, 'Treatise on Infinitesimal Calculus,' vol. iii.

But such discoveries as that the tangential force of a resisting medium is given in the very formulæ of Encke's comet[*], and that of inductive action through contiguous particles, and not at a distance[†], will, it is believed, cause this first postulate of the theory to be generally granted[‡].

5. To give distinctness to this idea of the parts of matter as mutually repulsive, a molecule, or a body (an aggregate of molecules), is conceived as a centre of lines of pressure; the lengths and curves of these lines are determined by the relative pressure of the lines they meet; and lines from greater, are made up of lesser molecules and their lines, and so on *ad infinitum.*

In speaking of a molecule or body as such a centre of pressure, it will be convenient to have a technical name. Rather than coin a new term, it is proposed to use "atom" in this sense. In chemistry I shall use the term "equivalent" exclusively, and not, as at present, as more or less synonymous with "atom," which I have thus ventured to appropriate for a new conception. Atoms, or mutually determining centres of lines of pressure, may also be defined, and their relations analytically investigated, as mutually determining elastic systems with centres of resistance.

6. This is the fundamental conception (not hypothesis) of the theory. What can at present be called hypothetical in the theory, is this only—that the application of analysis to the conception of elastic systems with such conditions, will in all cases give results corresponding with phenomena. An atom, as above defined, is the postulate of a conception, not of an agent, of a relation, not of an entity. And this conception, it is believed, distinguishes the theory from the many others of which the general object has been the same[§]. But in this attempt to found a general theory cleared of æthers and fluids, of properties and virtues, the author owes whatever there may be of truth in its present imperfect form and incomplete application, chiefly to the discoveries of the immortal 'Experimental Researches;' and the constant endeavour has been to work out the theory in the

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[*] *Ueber die Existenz eines widerstehenden Mittels im Weltraume,* von J. F. Encke, p. 52. Berlin, 1858.

[†] Faraday, 'Experimental Researches,' Series I., II., IV., IX., XI., XII., XIII.

[‡] See also on this point Humboldt, 'Cosmos,' iii. 33; and compare Newton's third letter to Bentley; the old aphorism, "Nature abhors a vacuum," and Grove's remark thereon, 'Correlation of Physical Forces.' And see Bacon, *Nov. Org.* ii. 8.

[§] But compare the conception of an atom by Boscovitch, in his *Theoria Philosophiæ Naturalis redacta ad unicam legem virium in Natura existentium.* Venetiis, 1763; and Faraday, in his 'Experimental Researches,' iii. 447. Also Seguin's memoir, *Sur l'origine et la propagation de la force,* and his *Considérations sur les causes de la cohésion.*

spirit of such remarks as these:—"What we really want is not a variety of different methods of representing the forces, but the one true physical signification of that which is rendered apparent to us by the phenomena, and the laws governing them. Of the two assumptions most usually entertained at present, magnetic fluids and electric currents, *one* must be wrong, perhaps *both* are. . . . . It is evident, therefore, that our physical views are very doubtful; and I think good would result from an endeavour to shake ourselves loose from such preconceptions as are contained in them, that we may contemplate for a time the force as much as possible in its purity*."

7. Now, in a system of atoms as above defined, let the centres be of equal mass, and at equal distances; there will be no difference of pressure on any one centre, no moving force will be developed, and the conditions of equilibrium will be satisfied. But it is clear that forces will be developed, or the general conditions of motion be fulfilled, either (α) by a difference in the masses of the centres, or (β) by a difference in the distances of the centres, in consequence of a displacement of any one of them, or (γ), supposing a state of dynamic equilibrium established in the system, by its being brought in contact with another system in a different state of such equilibrium.

8. Consider more particularly the first of these conditions of the development of a force, and with the postulate that the pressures of atomic lines are directly proportional to the mass of the atomic centre, and inversely to the square of the distance therefrom. But observe that this last property is conceived to arise, not from an absolute change in any individual line, whatever its distance from the centre, but because, from the lateral expansion of these lines, fewer will, when such expansion is unresisted, be cut at a greater than at a lesser distance by the same surface.

In a system of atoms in which there is a difference in the masses of any one or more centres, and in which the law of the pressure of the atomic lines is as above, any two masses, whether unequal or equal, will tend to approach.

And first, let the masses be unequal.

Then the pressure of the lines from the one is greater than that of the lines from the other; consequently these lines will be mutually deflected, and hence the centres approach.

Let the two masses be equal.

Then, if all the masses of the system were equal, and all at the same distance from each other, their mutual repulsions would be equal in all directions, and they would remain at rest. But if, though two masses may be equal, either has, on the other side of it, a mass of a greater size, or at a greater distance than

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* Faraday, 'Experimental Researches,' vol. iii. p. 530.

the other, it is evident that the mutual pressures of these two equal masses will, under such conditions, be unequal, and hence, as in the first case, they will approach. It is also evident that a body may thus cause the approach to itself of another body, whatever the number of interposed bodies.

9. Thus, if the conception of atoms is applied to the unequal and unequally placed bodies of such a world as that presented to us, the law of universal attraction follows, and gravity is mechanically *explained,* that is, referred to a mechanical conception.

But it must be understood that the above proposition is given rather to show that, as an actual law, universal attraction may be deduced from the theoretical conception of universal repulsion, than with any pretension to its being the best attainable form of an explanation of the law. It may, however, be remarked that such an explanation is in accordance with the chief characteristics of the force of gravity: it is not polar; and it seems to be so far different in kind from other physical forces, that it is not interchangeable with them, as they are among each other; for the attraction of gravity is thus referred to difference of mass, either between the two attracted bodies, or in the system of which they are parts. And other attractions, as will presently appear, are referred to differences of tension, or of dynamic equilibrium; and such conceptions do, though that of difference of mass does not, involve polarity.

10. Consider now the second (β) of the above-stated (7) general conditions of the development of a force. And, to fix our thoughts, let the system consist of three, and not of an indefinite number of bodies. That it must consist of at least three bodies, will be evident from the general reasoning of this theory; for the fundamental difference between the established and the proposed conception of a force is this:—a cause of motion is ordinarily defined as "an action between *two* bodies;" it is here conceived as a differential action between *three* bodies,—a conception, it is submitted, equally defensible by metaphysical reasoning and by physical experiment.

11. The displacement of a molecule, conceived as an atomic centre, may be evidently under such physical and analytically expressible conditions as to cause such displacement to be permanent or alternating. The first gives the conception of tension, by which electric and magnetic, the second that of vibration and undulation, by which optic and acoustic, phenomena are explained.

Let the conditions of the displacement of the molecular centres of lines of pressure be such that they are (and consequently, the body they constitute) in a state of tension.

12. The theory of electricity here proposed is the development of the idea of the tension of atoms as above defined. Hence it is an immediate, not a mediate, mechanical conception of the phenomena; for the conceptions of atoms or of centres of lines of pressure are not hypotheses, but convenient forms of the general conception of the parts of matter as mutually repelling. And the ground of the theory is this, that if matter is so conceived, experimental facts may themselves at once, and without the aid of hypothetical virtues or æthers, be mechanically conceived. For the theory has arisen from finding that the *more simply* facts were worded the clearer expressions they became of the general laws of motion; and that hypotheses only obscured the meaning and necessity of the relations of phenomena. It should therefore seem to be here sufficient briefly but clearly to express the conception of tension in its general relations, without entering into any detailed explanation of how it applies to different phenomena; for if there is any truth in the theory, such application ought at once to appear to those who can look at phenomena, and not at their hypothetical representations only, at least admissible. The clear proof that phenomena can be thus immediately (from a general conception of matter, and without aid of hypotheses) mechanically conceived, rests, of course with analysis, on the data of recorded and further experiments.

13. Little more, therefore, will here be offered than *primâ facie* evidence of the truth of the theory. The above considerations suggest that this may perhaps best be given by a statement of those general mechanical conceptions of (I.) the nature, (II.) the states, and (III.) the effects of electricity, which appear to be rather inductive generalizations of phenomena than hypotheses by which they are to be explained. A few experimental facts will be recalled under each head, but rather as suggestive of others than as by any means exhaustive of those which might be cited in support of these generalizations.

14. As to (I.) the nature of electricity: it is conceived as a permanent (not alternating) displacement of molecular centres of lines of pressure. Hence duality and polarity. For by such displacement there is evidently a tendency to produce motion of opposite characters in opposite directions; the pressure of the lines being increased in the direction of displacement, and correspondingly diminished in the opposite direction. Hence also the identity of the various forms in which electricity may be developed; the various conditions of such displacement differing only in intensity and quantity; and these being inversely as each other. For if intensity is conceived as amplitude of displacement, it is clear that, as the condition of a great displacement of a centre of

lines of pressure is evidently the resistance or non-displacement of the centres of one or more of the limiting atoms in the direction of displacement, the more equally and quickly displacement of one atomic centre causes displacement of all neighbouring centres, the less will be the amplitude attained by the originally displaced molecule.

15. The fitness of the conceptions here offered of the *nature and states* of electricity will appear chiefly in the proof of the generalizations to which they lead of the *effects* of electricity. But the above view of the nature of electricity is also founded on such facts as these. The "sources de l'électricité*," mechanical, thermal, or chemical, are all motions, or conditions of displacement. These motions are of different momenta. Compare facts as to the heterogeneousness of the bodies in frictional and thermal electricity; and as to their compound character in chemical electricity, interpreted by the theory of the chemical constitution of bodies hereinafter enunciated. In reference to the above conception of the difference of electricities, compare facts proving "the identity of electricities derived from different sources†," and, more particularly as to intensity, the various facts defining the correlative conceptions of insulation and discharge.

16. (II.) The states of electricity are thus conceived. (1) In a statically electrified body, the tension, or displacement of the molecular centres of pressure, is in closed curves forming the surface of the body, and the direction of displacement is either outwards (positive) or inwards (negative); and it seems demonstrable that there will be such a displacement *only* on the surface of the body.

17. Compare such facts as:—it is the least resisting of two rubbed bodies that is found negatively electrified; if an electrified sphere is hollow, there will be little or no electricity on the inner surface, &c.

18 Two conditions have been well distinguished by Ampère‡ in (2) dynamically electrified bodies—that of "courant ouvert," and that of "courant fermé." The former is conceived as a condition of longitudinal, the latter of transverse or spiral tension. Hence a polarity corresponding to the duality of statically electrified bodies. For electric poles are conceived as extremities towards, and from, which molecular centres of lines of pressure have been moved, at which, therefore, there is not only a change in the mechanical relations to outward atoms of the lines of pressure from these extremities, but a change at the one extremity of increase, at the other of diminution, of pressure. And

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* De la Rive, *Traité de l'Electricité*, cinquième partie, ii. 456—828.

† Faraday, 'Experimental Researches,' Series IV.

‡ Ampère, *Théorie des Phénomènes Electrodynamiques*. Paris, 1826.

thus a magnet is conceived as a body the molecular centres of pressure of which are transversely displaced about an axis joining its poles.

19. As has been said, the fitness of conceptions of the nature and states of electricity is to be proved chiefly by their explanations of the effects of electricity; but note also, in support of these conceptions of current electricity, such facts, as to the origin of open currents, as that mere difference in the motions of metals in contact develope in them electric currents; and, as to closed currents, note that the experimental conditions which define a closed current, and the facts as to the molecular structure of magnets, however much they may still require investigation, seem already to justify a conception of magnetism wholly referable to conditions of mechanical action and resistance—a conception the antithesis of that which "associates" with bodies "latent virtues" and "neutral fluids," and a conception which evidently differs also, though with respect, from the later theories in which mechanical principles are applied, not immediately to phenomena, but to, as it should seem, needless hypotheses of "æthers*."

20. (III). The effects of electricity may be generalized under these heads:—(A) induction; (α) insulation; (β) discharge; the latter distinguishable into (1) conduction; (2) electrolytic discharge; (3) disruptive discharge; (4) convection, or carrying discharge†: (B) motion (α) of bodies; conveniently classed as (1) ordinary attractions and repulsions; (2) paramagnetism and diamagnetism; (3) right- or left-handed deviations or rotations; (β) motion of the medium, including heat and light, &c. vibrations.

21. (A) Induction is conceived as the necessary mechanical consequence in a plenum of the displacement of centres of lines of pressure. For it is clear that, if the parts of matter are conceived as mutually repelling, and such repulsion is mechanically conceived in such forms as those above suggested—as the elasticity of atomic centres, or as lines of pressure from molecules,—the displacement of the molecules forming a line of centres of pressure implies a disturbance of the previously existing mechanical relations, not only at the extremities of, but all round such a line of displacement; and such a conception implies, further, that the character of such disturbance depends entirely on the relations between the direction and intensity of the original

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* I would desire more especially to express the diffidence and respect with which I venture to differ from Professor Challis as to the necessity or advantage of the fundamental hypothesis of his theory. See Phil. Mag. February, October, and December 1860.

† Faraday, 'Experimental Researches,' Series VII. 1319.

displacement and the varying resistance offered by the diverse conditions of motion and aggregation of opposed centres of lines of pressure.

22. Now, as to the facts which justify the proposal of this conception as a true generalization by which mechanical principles can be immediately applied to the phenomena of induction, it may be remarked that, as the full proof of such a conception depends on an experimentally based, and analytically expressed, mechanical theory of the constitution of bodies, it is only surprising that, while so little has been done towards the establishment of such a theory, the general conclusions of researches on induction, independent of, or with wrong, theories of chemical constitution, should go so far to make induction mechanically conceivable. Among such conclusions, each of which will call up a vast number of experimental facts, may be mentioned:—Induction is the origin and effect of all electrical phenomena; and is an action, not "at a distance," but "through contiguous particles" in lines of any curve. Insulation and discharge, or conduction, are differences only of degree; and bodies have specific inductive capacities which are but degrees of resisting power. The degree to which particles are affected before discharge constitutes intensity; and, in order to discharge, intensity must be raised much higher for a solid than for a fluid, and higher for a fluid, than for a gaseous, dielectric. An electric current has not only polar, but lateral, inductive effects; and the "lines of force" about a magnet take the form of a "sphondyloid." As to such facts as that gases, having the same inductive capacity, differ in insulating power, and that the effect of a magnet on a body without it is not affected by great rarefication of the medium (or a vacuum), their explanation more particularly depends on a mechanical theory of the constitution of bodies, and the principle that the character of inductive effect depends on the conditions of molecular motion and aggregation of the body acted on.

23. The general principle by which this theory gives a common explanation of the above classified (B) (α) motions of bodies in presence of an electrified or magnetized body, is that the *mechanical motions of bodies* in such circumstances *are effects of differential molecular displacement*; or, as it may be otherwise expressed, if, of two bodies, one resists molecular displacement from a centre of disturbance less than the other, the former moves towards that centre as a direction of least resistance. For it is evident that if a force has its full effect in a molecular displacement, the body will, as far as the direct action of that force is concerned, remain at rest; and that if the molecules of a body resist displacement, the force will have its effect in the

repulsion of the body. Molecular and bodily motion, or resistance thereto, are inversely as each other. Hence, if a force has more effect in producing molecular displacement in one body than in another, the difference will be seen in a tendency to repel this second body, the reaction of which will evidently urge the first towards the centre of force.

24. A corollary of this theorem is, that electrified bodies of which either the molecular tension or the inductive lateral disturbance is in the same direction approach; or, as it may be otherwise more concretely expressed, opposite poles, and similar currents, attract. For it is evident that, when the directions of the molecular displacement of two bodies are in the same line, a point of increased, is opposite a point of diminished, molecular pressure. Hence, transmission of similar molecular displacement from the one is in this position less, in the opposite, more, resisted by the other than by the medium. And hence, as above, attraction in the former, and repulsion in the latter case.

Further, it is evident that, according as two parallel currents or lines of tension are in the same, or in opposite directions, will their lateral disturbance of equilibrium be in the same, or in opposite directions inwards; and hence, that the reverse lateral motions, or at least tendencies to motion, of the bodies, according as their currents are in the same, or opposite directions, are explicable in the same way as, above, the motions of bodies with the same, or opposite directions of molecular tension, that is, with opposite, or the same poles opposed.

25. The special facts which seem to justify the advancing of the above theorem and its corollary as a true generalization and mechanical explanation of electric and magnetic motions, may be summed up under the following experimental conclusions:—Paramagnetism and diamagnetism are not absolute, but relative conditions of bodies. Paramagnets tend to pass from weaker to stronger, and diamagnets from stronger to weaker, places of action. Two of either class repel, and one of each attract. These motions would be explicable as due to differences of conduction; but magnetic, is quite different from electric, conduction. As to these facts, if their mechanical meaning is not from the foregoing sufficiently clear, remark that, the tension of a magnetized body being spiral, while that of a body with an "open current" is longitudinal, the directions of the lateral inductive actions of a magnet and an ordinarily electrified body will be different, and hence the molecular conditions which permit of electric, will be different from those which favour magnetic, conduction. And if differences of conduction, that is, of molecular displacement, are thus admitted in the explanation of paramagnetic and diamagnetic phenomena, it is evident, from what has been already said of the

effect of differential molecular action, that a better conductor will, in moving in the direction of least resistance, pass to a stronger place of action.

The above conclusions are Faraday's[*]; but the theorem and corollary as generalizations rest also on the facts adduced by Ampère and his successors in support of his helix-theory of the magnet.

A third most important class of facts by which the view here given of the mechanical conditions of the electric motions of bodies may be supported, is the disposition of iron filings about one or more electrified wires or magnets in various positions, and the information given by a moving wire as to magnetic forces.

26. The fundamental importance of the conceptions of a force as, in general, a difference of pressures, and of polar attraction and repulsion as the effect of a differential molecular action, has induced me to give such disproportioned length to their illustration, that it will be impossible within the brief limits of this paper to do more than note the other chief points of this general theory.

What, therefore, has to be said on (B) (β) the effects of electricity as manifested in motions of the medium, must be referred to the paragraph on the correlation of forces.

27. There will not, it is hoped, be thought to be presumption in offering new views in theories which have been elaborated with such admirable genius as those of light and heat; for the most strenuous supporters of the present form of the undulatory theory candidly admit that "there undoubtedly are several classes of phenomena which the wave theory has not merely *failed to explain*, but which are apparently at *direct variance* with its principles[†]."

It will be evident that the chief new view necessitated by this general theory (and which alone can be here noticed) resolves itself into a theory of the connexion of the elastic medium with the vibrating molecules in it. Now, though according to the present theory "it is certain that light is produced by undulations propagated with transversal vibrations through a highly elastic æther, yet the constitution of this æther, and the laws of its connexion (if it has any connexion) with the particles of bodies, are utterly unknown[‡]." But the theory here proposed implies such "laws of connexion." For its practical result is, that the "æther" is conceived as the mutually determined lines of

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[*] But compare Tyndall's Memoirs "On the Reverse Polarity of Bismuth" (Phil. Trans. 1855 and 1856).

[†] Baden Powell, 'Undulatory Theory,' Introduction, p. xxiv.

[‡] MacCullagh, "Laws of Crystalline Reflexion," &c., Mem. R. I. A., xviii. 38.

pressure from molecules of the size to give by their vibrations the sensations of light and heat.

It would seem that this conception of the "æther" leads to the explanation of more than one difficulty in the established theory; but nothing can, of course, be advanced on such a point except as the result of analysis.

It will be understood that I thus speak of the conception of atoms, as above defined, as a mode of conceiving the "æther" of the undulatory theory, only in order to make clear the application to that theory of the fundamental conception of the general theory here proposed; and that this in nowise contradicts what has been above said as to the seeming needlessness of hypotheses of *special* fluids, or æthers, acting on, or through, matter.

28. A general chemical theory is made up of two—a theory of the constitution, and a theory of the combination, of bodies. As to the constitution of bodies, the principal views here offered are:—Bodies are conceived as states of dynamic molecular equilibrium, that is, as states of molecular motion in which, while there is no decomposition, the intensity of motion is at all points equal. Hence, their differences are conceived as resulting from different conditions of molecular motion; and thus specific heat becomes one of the chief exponents of the nature of a body.

The distinguishing mechanical characteristic of the gaseous, fluid, and solid, states of matter is degree of tangential resistance. In an absolutely perfect gas the molecules would be of equal mass and at equal distances: hence perfect equality of resistance in all directions. But let there be inequality either in the masses or distances of the molecular centres of pressures, it is evident, from the same reasoning as that above applied to the mechanical explanation of gravity, that there would be a cohesive force developed, the consequence of which would be inequality of resistance. Fluids, therefore, as opposed to solids, are conceived as bodies the molecules of which are of sensibly equal masses and at equal distances; and gases, as opposed to liquids, as bodies in which the distances of the molecules, though equal, are greater than in liquids. Hence the greater amplitude of their motion, or specific heat.

The consideration of this theory of bodies with reference to that of Krönig* and Clausius†, cannot be at present entered upon.

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* "Grundzüge einer Theorie der Gase," Pogg. *Ann.* xcix. 315.

† "Ueber die bewegende Kraft der Wärme," Pogg. *Ann.* lxxix. 394; and "Ueber die Art der Bewegung welche wir Wärme nennen," ibid. c. 353. See also Prof. Maxwell's "Illustrations of the Dynamical Theory of Gases," Phil. Mag., January and July 1860. Dr. Tyndall's discoveries and researches "On the Transmission of Heat of different qualities through Gases of different kinds" (Proceedings of the Royal Institution, June 10, 1859), are of the greatest importance in such a dynamical theory of the constitution of bodies.

29. The combination of bodies, in whatever way it takes place, and not confining the meaning to the formation of salts*, is conceived as the establishment of a new state of dynamic equilibrium†. Hence there must necessarily be definite laws of chemical combination.

But as it is proposed to give the outlines of this mechanical theory of chemistry in a future paper or papers, it is unnecessary to enter more fully upon it at present. A theory so important has been introduced in a paper in which it must be so inadequately expressed, only to complete the general outline of the science of motion, and because a mechanical theory of chemistry is the necessary complement of the above-given mechanical theory of electricity.

30. In such a theory of forces as that here proposed, the great experimental truth of the "correlation of forces"‡ assumes an axiomatic clearness. Especially is it to be noted that it gives an account of the difference of the correlation between electricity and magnetism, and that of either of these with light, heat, &c. The former is a correlation of coexistence, the latter of change of conditions.

If I have been successful in making clear the conceptions offered in this theory of electricity and magnetism and of induction, it will be unnecessary here to say anything further of this correlation of coexistence; for it is implied in the conception of induction as a mechanical effect varying with resistance, and manifested at right angles to a longitudinal or transverse (spiral) line of tension, that is, of permanent (not alternating) molecular displacement. And if electricity is a permanent, light an alternating, molecular displacement, their correlation is evident, the conditions of their interchange assignable, and analytically expressible.

But not only is the correlation thus clear of physical motions among themselves, but also of these, as a class, with mechanical motions on one side, and chemical motions on the other—clear, that is, that the stopping of a mechanical, or the beginning of a chemical, motion implies more or less molecular displacement under such conditions as those above assigned for electricity, light, and heat. But what is the proof of such a correlation of forces but the sublime "persuasion that all the forces

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* Berthelot's discoveries have definitively broken down the distinction between Inorganic and Organic Chemistry. See his *Chimie Organique.*

† Compare Williamson's "Theory of Etherification," Chem. Soc. Quart. Journ. iv. 110.

‡ Grove. See also Helmholtz in Taylor's Scientific Memoirs, 1853, p. 124; and compare Rendu's "*circulation*" of fire, light, electricity, and magnetism, *Théorie des Glaciers de la Savoie,* Memoirs of the Royal Academy of Sciences of Savoy, 1841, cited in Tyndall's 'Glaciers of the Alps,' p. 299.

of nature are mutually dependent, have one common origin, or rather are different manifestations of one fundamental power[*]" —become, at least for all the phenomena of Motion as distinguished from those of Growth, a scientific truth?

31. For the sake of distinctly defining by its relations the Science of Motion, a few words may be added on the Classification of the Sciences proposed by the author. The general divisions of each of the two great classes—the Natural and Humane Sciences—are (A) the Systematic, (B) the Descriptive, and (C) the Historic Sciences.

The Systematic Natural Sciences are the sciences of (I.) Motion, (II.) Growth, (III.) Species ("the Classificatory Sciences" of Whewell)[†].

The twofold subdivision of each of the three sciences of motion will be evident from the foregoing.

Each of these rational sciences, as well as those making up the Science of Growth, have a corresponding applied science.

The Science of Motion, as a distinct science, and not as merely a general name for the sciences of mechanics, physics, and chemics, has a twofold division.

Under the first, the general relations, laws, or principles of motion, without regard to the particular conditions of its cause, would be considered: such general principles, for instance, as Galileo's laws of Inertia, and of the Composition of Motions; Newton's law of the Equality of Action and Reaction; its generalization in D'Alembert's principle; Jean Bernoulli's (?)[‡] principle of Virtual Velocities; Varignon's geometrical theorem on Moments; Poinsot's theory of Couples; Newton's principle of the Conservation of the movement of the Centre of Gravity; Kepler's principle of Areas; Laplace's theorem on the Invariable Plane; Euler's on Moments of Inertia, and Principal Axes; Huygens's principle of the Conservation of Vires vivæ; Daniel Bernoulli's theorem on the Coexistence of small Oscillations, &c.

The object of the second division of the General Science of Motion would be the generalization of the conditions giving rise to the various Forces of Motion, mechanical, physical, and chemical, and their correlations. This paper, therefore, has treated of the principles of but one division of the science.

The divisions of the special science of Mechanics, that on Solids, and that on Fluids, would be conveniently considered in kinematical[§], statical, and dynamical sections.

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[*] Faraday, 'Experimental Researches,' 2702.

[†] History of the Inductive Sciences, iii. p. 211.

[‡] Poisson, *Traité de Mécanique*, i. p. 654.

[§] See Ampère's *Essai sur la Philosophie des Sciences*; Willis's preface to his 'Principles of Mechanism,' and the arrangement of Rankine's 'Manual of Applied Mechanics' (*Encyc. Metrop.*).

32. In conclusion, as every one of these theories is so dependent on every other, that it was necessary to give first a general outline of them all, it is hoped that allowance will be made for the imperfection, or even inaccuracy, unavoidable in so brief an exposition of each. And lest, from imperfect expression, the general principles themselves may be misunderstood, it may be added that the author's confidence in them arises only from their long-tested seeming accordance, not only with *experimental* generalizations, but with *scientific* metaphysics. For every physical theory must be implicitly, or explicitly founded on metaphysical views as to the nature of Knowledge, as to the mode in which Matter and Force are to be conceived, and as to the meaning of Law. And the metaphysical bases of this physical theory have been chiefly found in the Philosophy of Bacon, and of the Scottish School.

The necessary limitation of human knowledge, the central doctrine of Sir William Hamilton's system, and, indeed, the great result of the modern Critical School founded by Kant, is the citadel of those who would clear the Natural Sciences of absolute, and essentially distinct, force or forces acting on, associated with, or emanating from, matter. The fundamental conception of this theory—that of a force as a condition—is but a development of the doctrine of "Forms," "the investigation of which is the principal object of the Baconian method of induction*." The conception of matter is in accordance with that of substances, as "corpora individua edentia actus puros individuos ex lege†;" and that of a Law, as not an imposed rule to be discovered, but a relation to be expressed, seems to agree no less with the principles of Bacon, with whom "the statement of the distinguishing character of the motion or arrangement, or of whatever else may be the form of a given phenomenon, takes the shape of a Law‡," than with the principles of Mill§.

6 Stone Buildings, Lincoln's Inn,
10th Dec. 1860.

VI. *On the Opacity of the Yellow Soda-Flame to Light of its own Colour.* By WILLIAM CROOKES||.

IN their remarkable investigations on the colours which certain substances impart to the flames in which they are heated¶, Professors Kirchhoff and Bunsen describe certain experiments by

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[*] Bacon's Works, by Ellis and Spedding, i. p. 31.

[†] Ibid.

[‡] Ibid. p. 29.

[§] System of Logic, Ratiocinative and Inductive.

[||] Communicated by the Author.

[¶] Chemical Analysis by Spectrum Observations. By Professors Kirchhoff and Bunsen. Phil. Mag. S. 4. vol. xx. p. 89. August, 1860.

which they prove that the luminous lines which are produced in the spectrum of a spirit- or gas-flame, when salts of the alkalies or alkaline earths are caused to volatilize therein, become *reversed* (*i. e.* that the bright lines become changed to dark ones) when a source of light of sufficient intensity, and giving a continuous spectrum, is placed behind the coloured flame.

During some researches on the spectra of artificial flames which I have been carrying on simultaneously with MM. Kirchhoff and Bunsen, one or two forms of experiment suggested themselves which, while they perfectly corroborate most of the facts mentioned by them, seem to merit attention, from the facility with which they may be performed, and the striking manner in which the phenomena can be exhibited to several persons at once without the necessity of employing any optical apparatus. The atmosphere of the room—or rather that part of it in which the illustration is performed—is to be first impregnated with soda-smoke, by igniting a piece of sodium, the size of a pea, on wet blotting-paper. Any flame, whether of gas, spirit, a candle, &c., which may now be burning anywhere in that part of the room, will exhibit in a marked manner the well-known yellow soda-flame; and if the full amount of gas in an ordinary wire-gauze air-burner is turned on and ignited, it will give a uniformly brilliant yellow flame, upwards of a foot high and 3 inches in diameter.

If a smaller flame be now moved in front of this large one, it will exhibit a curious phenomenon. Those parts of it which are ordinarily seen to be luminous will suffer no change, other than that slight diminution in intensity which might be anticipated from their projection in front of a broad but not very brilliant source of light; but beyond these there will appear a sharply-cut and intensely black narrow border, closely surrounding the visible flame, and presenting the curious appearance of the latter being set in an opake frame. A closer scrutiny will show that the position of this black rim is not, as I at first supposed, in that outer cone in which the yellow soda-flame is most distinctly seen, but that it lies in the dark space immediately outside the luminous part of the flame, affording proof of the existence of another invisible cone of vapour. The flame from a tallow candle shows this appearance better than that of wax or sperm, probably on account of its inferior luminosity. A small spirit- or gas-flame will also answer very well; but I think a tallow candle shows the phenomenon in a more striking manner.

The fact of the cone of yellow soda-flame being transparent, while the outer, non-luminous space is perfectly opake to the same kind of light placed behind it, appears worthy of attention. It seems to show that the yellow flame caused by the presence of incandescent solid particles of a sodium compound has no very marked absorptive action on light of its own colour; but that to give rise to this kind of opacity it is necessary that the sodium compound should be in the state of vapour. It appears, moreover, to prove that it is not necessary for this vapour to be in the metallic state; for it could hardly be supposed that so highly combustible a vapour as that of metallic sodium could be present in that part of the flame which is seen to possess this great opacity. That soda salts are easily volatile at the temperature of flame, is a fact abundantly proved by Bunsen*. The reason why the opacity is only exhibited by that part of the outer shell of vapour which is situated at the edge of the flame, and not by its entire extent, is owing to its thickness being insufficient to produce sensible absorption on rays which traverse it perpendicularly; an appreciable action taking place only when they pass as a tangent to the edge of the flame, and thus traverse a considerable extent of absorbing medium.

VII. *Experimental Researches on the Laws of Absorption of Liquids by Porous Substances.* By THOMAS TATE, *Esq.*

[Continued from vol. xx. p. 510.]

II. *On the Filtration of Liquids through different Porous Substances.*

FILTRATION is in general produced by the action of two forces, viz. by the force of absorption and that of pressure. Filters may be divided into two classes. The first class comprises those substances which are highly porous, and which undergo little or no change during the process of filtration. The second class comprises those substances with close pores, which under certain circumstances undergo a decided change during the process of filtration. The following laws (with certain limitations) apply to both kinds of filters:—

1. The rate of filtration, other things being the same, varies directly as the area of the surface of the filter in contact with the liquid, and inversely as the thickness.

2. The rate of filtration, other things being the same, increases in a high ratio with the increase of temperature.

3. The rate of filtration, other things being the same, varies as the depth of the column of liquid upon the filter.

And so on to other laws which will be hereafter illustrated.

These experiments were, for the most part, made with the apparatus represented in the annexed diagram.

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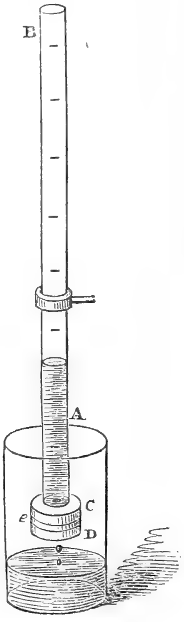
* Phil. Mag. S. 4. vol. xviii. p. 513.



AB a wide glass tube, containing the liquid, about 2 feet in length, graduated into units of cubic inches, or into units of half cubic inches, as the case may be; C and D two equal plates of polished slate having equal orifices bored through their centres; the plate C is cemented (with a resinous cement) to the bottom of the tube, and the filter *e* is cemented to both plates, so that all lateral discharge from the filter is stopped, and at the same time the filter presents a surface, in contact with the liquid, equal to the section of the orifices of the plates. All liquids, before being used in the filtrometer, were twice filtered through ordinary filtering paper, and the top of the filter tube AB was covered during the experiment to prevent any dust from falling into the liquid. The tube AB was filled up, with the liquid, to a certain point of the graduation, and then the time at which the liquid, in its descent, arrived at the different points of the graduation was duly noted.

Experiment XVI.

The filter used in this experiment was common wood-charcoal half an inch in thickness. The liquid was distilled water. The diameter of the orifice of the plate was $\frac{4}{10}$ths of an inch. The filter-tube was graduated into 8 units, each containing half a cubic inch, and the 8 divisions measured 9·2 inches. The temperature was 56° throughout the experiment. The results recorded in the third column of the following Table are obtained by dividing the unit of space by the mean of the times taken in describing the two consecutive units of space; thus the velocity at $4 = 1 \div \frac{46+55}{2} = \frac{1}{50}$.

Height of column of liquid, h,	Corresp. time in seconds, T.	Corresp. velocity of descent, v.	Value of v by formula $v = \frac{h}{203}$.	Value of T by formula (4).
8	0	...	$\frac{1}{25}$	0
7	27	$\frac{1}{29}$	$\frac{1}{29}$	26·5
6	58	$\frac{1}{33}$	$\frac{1}{33}$	57·5
5	94	$\frac{1}{41}$	$\frac{1}{40}$	94·0
4	140	$\frac{1}{50}$	$\frac{1}{50}$	139·0
3	195	$\frac{1}{66}$	$\frac{1}{67}$	196·0
2	272	$\frac{1}{102}$	$\frac{1}{101}$	277·0
1	400	...	...	415·0

The near coincidence of the results in the third and fourth columns shows that the *velocity of discharge varies directly as the height of the column of liquid upon the filter.*

Let S′ = the whole depth of the liquid at the commencement of the experiment, in units of the divisions of the filtrometer; S = the descent of the liquid in the time T; v = the corresponding velocity of descent; α, γ, ρ = constants; then we have generally,

$$\mathrm{T} = \alpha - \gamma \log (\mathrm{S}' + \rho - \mathrm{S}). \quad . \quad . \quad . \quad . \quad (1)$$

By differentiation, we get

$$\frac{d\mathrm{S}}{d\mathrm{T}} = \frac{\log_e 10}{\gamma} (\mathrm{S}' + \rho - \mathrm{S});$$

$$\therefore v = \frac{2\cdot30258}{\gamma} (h + \rho). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In the foregoing experiment, $\rho = 0$, $\frac{2\cdot30258}{\gamma} = \frac{1}{203}$, and the relation of time and space is expressed by the formula

$$\mathrm{T} = 415\cdot74 - 460\cdot4 \log h. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This experiment, upon being repeated, gave a slight diminution in the velocity of descent of the liquid, showing that the filter had undergone only a very slight change during the process of filtration. This observation applies to the filters used in the three following experiments.

Experiment XVII.

The filter in this experiment was coke, $\frac{4}{10}$ths of an inch in thickness. The liquid was distilled water. The diameter of the orifice of the plate was $\frac{7}{10}$ths of an inch. The filter-tube was the same as in the last experiment, and the temperature was 57° throughout the experiment.

Height of column of liquid, h.	Corresp. time in seconds, T.	Corresp. velocity of descent, v.	Value of v by formula $v = \frac{h}{205}$.
8	0	...	$\frac{1}{25}$
7	27	$\frac{1}{29}$	$\frac{1}{29}$
6	59	$\frac{1}{34}$	$\frac{1}{34}$
5	95	$\frac{1}{40}$	$\frac{1}{41}$
4	139	$\frac{1}{50}$	$\frac{1}{51}$
3	195	$\frac{1}{68}$	$\frac{1}{68}$
2	275	$\frac{1}{102}$	$\frac{1}{102}$
1	400		

Here it will be observed that the liquid followed the same law of descent as that of the preceding experiment.

Reducing the coke filter to the same thickness and diameter of orifice as in the case of the charcoal filter, we find, under the same circumstances of pressure, &c., the filtering power of the charcoal to be $3\frac{3}{4}$ times that of the coke.

Experiment XVIII.

The filter in this experiment was stout woollen cloth. The liquid was distilled water. The diameter of the plate was $\frac{3}{10}$ths of an inch. Each half cubic inch graduation of the filter-tube measured on an average 1·55 inch, so that the height of the liquid column at the commencement of the experiment was 15·5 inches.

Height of column of liquid, h.	Corresp. time in seconds, T.	Corresp. velocity of descent, v.	Value of v by formula $v=\frac{h}{208}$.
10	0		
9·5	...	$\frac{1}{22}$	$\frac{1}{21}$
9·0	22		
8·5	...	$\frac{1}{24}$	$\frac{1}{24}$
8·0	46		
7·5	...	$\frac{1}{28}$	$\frac{1}{28}$
7·0	74		
6·5	...	$\frac{1}{32}$	$\frac{1}{32}$
6·0	106		
5·5	...	$\frac{1}{37}$	$\frac{1}{38}$
5·0	143		
4·5	...	$\frac{1}{47}$	$\frac{1}{46}$
4·0	190		
3·5	...	$\frac{1}{60}$	$\frac{1}{59}$
3·0	250		
2·5	...	$\frac{1}{83}$	$\frac{1}{83}$
2·0	330		
1·5	...	$\frac{1}{147}$	$\frac{1}{139}$
1·0	480		

Here the results of the third and fourth columns show that the velocity of discharge varies according to the same law as in the two foregoing experiments.

Experiment XIX.

In this experiment the filter was sponge plugged tight into the bottom of the filter-tube, which was the same as that of the last experiment.

Height of column of liquid, h.	Corresp. time in seconds, T.	Corresp. velocity of descent, v.	Value of v by formula $v=\frac{h}{308}$.
11·5	0		
11·0	...	$\frac{1}{28}$	$\frac{1}{28}$
10·5	28		
10·0	...	$\frac{1}{30}$	$\frac{1}{31}$
9·5	58		
9·0	...	$\frac{1}{34}$	$\frac{1}{34}$
8 5	92		
8·0	...	$\frac{1}{38}$	$\frac{1}{38}$
7·5	130		
7·0	...	$\frac{1}{44}$	$\frac{1}{44}$
6·5	174		
6·0	...	$\frac{1}{51}$	$\frac{1}{51}$
5·5	225		
5·0	...	$\frac{1}{61}$	$\frac{1}{61}$
4·5	286		
4·0	...	$\frac{1}{77}$	$\frac{1}{77}$
3·5	363		
3·0	...	$\frac{1}{97}$	$\frac{1}{102}$
2·5	460		
2·0	...	$\frac{1}{175}$	$\frac{1}{154}$
1·5	635		

The results of this experiment show that, for the first nine units of descent, the velocity of discharge varies almost exactly as the height of the column of liquid. For the last two units, the rate of discharge considerably exceeded that which the formula $v=\frac{h}{308}$ would give, showing that the effect of the absorbent power of the filter greatly exceeded that which would be due to the pressure alone.

This experiment was repeated with little or no variation in the results.

Similar results were obtained from filters formed of plugs of different sorts of soft porous material; also from a filter formed of fine sea-sand laid upon a perforated plate.

The following experiment was made to determine the variation of the rate of filtration due to increase of temperature.

Experiment XX.

The liquid used in this experiment was distilled water, and the filters were those employed in Experiments XVI. and XIX.

With the charcoal filter under a constant pressure, the time required to discharge one cubic inch of liquid at 52° temperature was 114 seconds; whereas at the temperature of 90° it only required 65 seconds. In this case an increase of 38° of temperature increased the rate of discharge $1\frac{3}{4}$ times; that is, the rate of discharge was nearly doubled by the addition of 38° of temperature.

With the sponge-filter the following results were obtained:—

Time in seconds to discharge 11 cubic inches of water.			
At 50°.	At 80°.	At 90°.	At 100°.
420″	286″	246″	204″

Here an increase of temperature from 50° to 90° caused the rate of discharge to be increased 1·707 times, a result nearly coinciding with that determined for the charcoal filter. At 100° temperature the rate of discharge is a little more than double the rate at 50°. These results further show that, *for equal volumes of discharge, the decrements of time are for the most part proportional to the increments of temperature.*

The following experiment was made to determine the rates of filtration of different liquids as compared with that of water.

Experiment XXI.

The filter used in this experiment was charcoal; and the liquids compared were distilled water, and three different solutions of carbonate of soda. Solution No. 1 contained 2 per cent. of carbonate of soda; No. 2 contained 4 per cent.; and No. 3 contained 8 per cent.; that is to say, the per-centage of the salt in these different solutions were in the geometrical progression 2, 4, 8. The discharge in each case was produced under the pressure of a column of 9·2 inches of the liquid.

	Time in minutes to discharge one cubic inch of the liquids.	
Water . . .	$1{\cdot}73 = 1{\cdot}73 \times 1{\cdot}08^0$	
No. 1. . . .	$1{\cdot}87 = 1{\cdot}73 \times 1{\cdot}08^1$	nearly.
No. 2. . . .	$2{\cdot}03 = 1{\cdot}73 \times 1{\cdot}08^2$	,,
No. 3. . . .	$2{\cdot}20 = 1{\cdot}73 \times 1{\cdot}08^3$	,,

Here it will be observed that the rates of discharge are very nearly in geometrical progression. This property of filtration is analogous to that of absorption, as shown in connexion with experiments XI. and XII. Thus it appears that the chemical composition of a liquid affects its relative rate of filtration.

In general the rate of filtration or filtrativeness of a liquid seems to depend mainly upon its viscosity, and not so much upon its specific gravity. Alcohol, oils, &c., which have a less specific gravity than water, have a low rate of filtration. Solutions of sugar and starch, even when much diluted, have very low rates of filtration as compared with that of water; whilst diluted acids and weak solutions of alkaline salts, for the most part, have a rate of filtration nearly equal to that of water. In these respects the law of filtration is analogous to that of absorption; at the same time it must be observed that the relative absorbent power of a substance does not always correspond to its filtering power.

The filters used in the following experiment belong to the second class of filters.

Experiment XXII.

The filter used in this experiment was thick unsized paper, and the liquid was spring water. The diameter of the orifice of the plate was $\frac{4}{10}$ths of an inch. The filter-tube was the same as that of Experiment XVI. At the commencement of the experiment the liquid stood at the eighth division of the tube measured from the filter. The temperature was 64° throughout the experiment.

Descent of the liquid, S.	Corresp. time in seconds, T.	Velocity per second, v.	Value of v by formula $v=\frac{1}{80}\cdot 1{\cdot}355^{-S}$.
0	0	$\frac{1}{80}$	$\frac{1}{80}$
$\frac{1}{4}$	20		
1	97	$\frac{1}{112}$	$\frac{1}{108}$
$1\frac{1}{4}$	125		
2	225	$\frac{1}{140}$	$\frac{1}{146}$
$2\frac{1}{4}$	260		
3	400	$\frac{1}{200}$	$\frac{1}{199}$
$3\frac{1}{4}$	450		
4	620	$\frac{1}{260}$	$\frac{1}{269}$
$4\frac{1}{4}$	685		
5	870	$\frac{1}{360}$	$\frac{1}{365}$
$5\frac{1}{4}$	960		
6	1360	$\frac{1}{480}$	$\frac{1}{494}$
$6\frac{1}{4}$	1480		
7	1990	$\frac{1}{680}$	$\frac{1}{670}$
$7\frac{1}{4}$	2160		

The formula expressing the relation between the time T and space S of descent is

$$T=262{\cdot}3\,(1{\cdot}355^{S}-1). \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

It will be seen how very nearly the velocity of descent of the liquid is represented by the formula $v=\frac{1}{80}\cdot 1{\cdot}355^{-S}$; showing that *if the spaces of descent be taken in arithmetical progression, the corresponding velocities of descent will be in geometrical progression.* The common ratio of the velocities in this experiment is $\frac{1}{1{\cdot}355}$.

The relation of T and S for both kinds of filters may be represented by the general formula

$$T=\alpha(\beta^{S}-1)-\gamma\log(S'+\rho-S),$$

where α, β, γ, and ρ are constants.

When $\beta=1$, this formula becomes the same as equation (1); and when $\gamma=0$, or very small, it becomes

$$T=\alpha(\beta^{S}-1), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which is the general formula applying to the second class of filters.

By differentiation, we find

$$v=\frac{1}{\alpha\log_{e}\beta}\cdot\beta^{-S}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

which is a general formula for the rate of descent of the liquid.

In the foregoing experiment, $\alpha=262{\cdot}3$, $\beta=1{\cdot}355$, and

$$\therefore v=\frac{1}{80}\cdot 1{\cdot}355^{-S}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Towards the close of the experiment the velocity of descent became exceedingly small, showing that the adhesion of the liquid to the bottom of the filter-tube interfered with the law of velocity expressed by equation (8).

This experiment being immediately repeated, it was found that the rate of filtration had sensibly diminished, showing that the filtering power of the paper had undergone a decided change during the process of filtration. This change, as will be hereafter shown, is progressive, being in proportion (within certain limits) to the quantity of liquid filtered. But after the filter had been dried, it somewhat regained its original power.

In this manner various experiments were made, which gave precisely similar results.

Although the liquid in these experiments had been carefully filtered through ordinary filtering-paper, yet it is possible that certain minute particles may have passed through the filter, sufficient to deteriorate the filtering power of a small filtering surface such as that used in the foregoing experiment.

The following experiments were made on *upward* filtration.

Experiment XXIII.

In this experiment the filter was immersed in a jar of liquid to the depth of 7 units of the filter-tube. As the liquid rose in the tube through the filter, the liquid in the jar was maintained at a constant level. The filter was the unsized paper of the last experiment, the diameter of the plate being $\frac{4}{10}$ths of an inch. The liquid was a diluted solution of carbonate of soda.

Depth of the column of liquid, h.	Corresp. time in minutes, T.	Corresp. velocity of ascent, v.	Value of v by formula $v=\frac{h}{8{\cdot}69}$.
6	0	......	$\frac{1}{1{\cdot}44}$
$5\frac{1}{2}$	......	$\frac{1}{1{\cdot}58}$	$\frac{1}{1{\cdot}58}$
5	1·58		
$4\frac{1}{2}$	......	$\frac{1}{1{\cdot}93}$	$\frac{1}{1{\cdot}93}$
4	3·51		
$3\frac{1}{2}$	......	$\frac{1}{2{\cdot}4}$	$\frac{1}{2{\cdot}48}$
3	5·91		
$2\frac{1}{2}$	......	$\frac{1}{3{\cdot}35}$	$\frac{1}{3{\cdot}49}$
2	9·26		
$1\frac{1}{2}$	......	$\frac{1}{5{\cdot}65}$	$\frac{1}{5{\cdot}79}$
1	14·91		
$\frac{1}{2}$	......	$\frac{1}{17{\cdot}84}$	$\frac{1}{17{\cdot}38}$
0	32·75		

The near coincidence of the results in the third and fourth columns shows that in this case the *rate of filtration varies directly as the pressure upon the filter.*

With a double filter the rate of discharge was found to be reduced to one-half very nearly; and with a filter of $\frac{2}{10}$ths of an inch diameter, that is, one-fourth of the surface of the filter in the above experiment, the rate of discharge was found to be reduced to one-fourth of that of the above experiment. Hence we conclude that *the rate of filtration varies directly as the areas of the surface of the filter in contact with the liquid, and inversely as the thickness.*

[To be continued.]

VIII. *A Theory of Magnetic Force.*
By Professor Challis, *F.R.S.*[*]

In my last communication, containing a theory of galvanism, no allusion was made to *thermo-electric* phenomena. This omission I propose to supply before proceeding to the theory of magnetism, the fact that galvanic currents may be produced by heat, being confirmatory of the explanation given by hydrodynamics of the generation of secondary currents generally, whether electric, or galvanic, or magnetic.

1. It is assumed in that explanation that in the neighbourhood of the earth's surface there are steady ætherial currents, which eventually may be found to be secondary with respect to other more general currents, but for considerable spaces may be considered to be uniform as to velocity and density. These currents are supposed to flow freely through the interior of bodies, with only such modifications as may result from the arrangement of

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[*] Communicated by the Author.

the atoms, and the contraction of the channel by the atomic occupation of space. At the parts of the boundary of any substance where the stream *enters*, there will be a sudden *increment* of velocity, and at the parts where it *issues*, a sudden *decrement*; but not to a large amount, because there is reason to conclude that even in dense bodies the space occupied by atoms is very small compared to the vacant space. Under all circumstances the motion remains steady, if there be no extraneous force, and no variation of the primary current. If, throughout the interior of the body, the atoms within a given space (as one thousandth of a cubic inch) occupy a portion of it which has a constant ratio to the vacant space, they produce no acceleration or retardation of the velocity of the stream. But if from any cause there should be a gradation of internal density of the atoms, the atomic composition remaining the same throughout, secondary streams will be produced in the following manner:—Conceive the external primary current to be cut at right angles by a plane in a given position. Then since it is supposed to be steady and uniform, the velocity will be always the same at all points of the plane. Hence, tracing two contiguous and equal filaments of the stream into the interior of the body, it will be seen that if the density of the atoms be greater at certain portions of the course of one filament than at the adjacent portions of the course of the other, the velocity will be *greater* in any element of the *former* portions, than in the adjacent element of the *latter*, assuming, as may be done, that in other respects the channels of the two filaments are at adjacent parts of equal dimensions. Hence by the general hydrodynamical equation for steady motion, the density and pressure of the æther will be *less* in the *former* elements than in the *latter*. Consequently there will be an accelerative force of the fluid always tending from the *rarer* to the *denser* parts of the body, and taking effect, whatever be the direction of the original stream, in the directions of normals to surfaces of equal density, because in these directions the change of density of the atoms in a given space is greatest. These accelerative forces produce secondary currents, the velocities of which will depend on the magnitudes of the forces, and on the extent through which they act. Considering the vast elasticity of the æther, as shown by the rate of the propagation of light, a difference of its density equal to a ten-millionth part of the whole density would correspond to an enormous difference of pressure.

2. According to these views it might be expected that a lamina of metal, heated unequally at the two extremities, would become electro-dynamic, as Volta found to be the case with a lamina of silver. In this instance the heat produces local disturbances of the condition of *superficial* atoms, and from these disturbances

probably the internal gradation of density chiefly results, as in a body subject to the influence of electricity by induction. If a closed circuit of any metal be heated at any point, currents, according to this theory, will be generated in consequence of the different degrees of expansion of the metal at different points, and, flowing in opposite directions, will neutralize each other if they be equal. But if by any mechanical means they be made unequal, as by contortions of the metallic circuit, a galvanic current should result, as is found to be the fact by experiment. When the circuit is formed by two metals soldered together at their extremities, and heat is applied at one of the positions of junction, inequality of the opposite currents might be expected to arise from difference of the capacity of the two metals for generating currents, owing to difference of their atomic constitution; and accordingly it is found that under these circumstances a galvanic current is produced. I proceed now to the theory of magnetism.

3. In the preceding theories the generation of secondary ætherial currents has been ascribed to a disturbance of the atomic condition of bodies by external agency,—in electricity, by friction; in galvanism, by the mutual molecular actions of dissimilar substances in contact; and in thermo-electricity, by heat. In the theory of magnetism it must be assumed that there are substances in which a gradation of interior density exists independently and permanently,—that iron, for instance, is found in this state in nature; that the same state may be induced in steel by mechanical means, with different degrees of permanence; and that it may be momentarily induced in soft iron. Also it must be supposed that the direction of the gradation of density depends on the *form* of the magnetized body. These suppositions rest immediately on facts of experience, the explanations of which, since they relate to qualities of the bodies, and not to the agency by which magnetic phenomena are produced, are not now under consideration.

4. Let us take the case of a bar of magnetized steel of the form of a long rectangular parallelopiped, and let it be assumed that there exists permanently a uniform decrease of its atomic density from the end A to the end B. By the argument in art. 1, on the supposition of a steady and uniform primary current, there will be impressed on the æther within the bar a uniform accelerative force, acting throughout its length from B towards A. To the accelerative force in that half of the bar which lies towards B, may be ascribed the effect of overcoming the inertia of the æther in motion within and without the bar on the side of B, and to the accelerative force in the other half, the effect of overcoming the resistance opposed to the flow of the current by the

inertia of the æther on the side of A. Thus the motion will be maintained so as to be symmetrical with respect to a neutral position N, mid-way between A and B, the partial streams converging towards the parts about B, and diverging in like manner from the parts about A. The velocity of the æther will be greatest, and its density least, at N, and the velocity will decrease, and the density increase, in both directions from this position by the same gradations. Hence the atoms, assumed to be of finite dimensions, will be urged on both sides towards N, by reason of the excess of pressure on the halves of their surfaces turned from N, and the total moving forces in the opposite directions will be equal. Consequently the theory not only accounts for the well-known fact, that *the magnetism of a steel bar is equal and opposite on the opposite sides of a middle neutral line*, but explains also why terrestrial magnetism, being supposed, for reasons that will be hereafter adduced, to act as the primary current of the theory, produces no perceptible motion of translation of the bar. These inferences will not be altered when the dynamic effect of the *motion* in the secondary streams is taken into account, as will be shown in a subsequent part of the theory.

5. It also follows, conformably with experience, that if a magnetized bar be divided into two or more parts by being cut transversely, each part becomes a magnet, because it may be assumed that the gradation of density from end to end is the same, and in the same direction, in each, as when it formed a part of the whole bar.

6. The theory of the action between two magnets requires the investigation of the mutual influence of two steady streams, having separate origins and interfering courses. The following laws, which may be admitted on hydrodynamical principles, will suffice for my present purpose. The resultant of two interfering steady streams is steady. Where two streams meet, the velocity is in general less, and consequently the density greater, than that which would be due to either stream flowing separately. When two streams unite in the same course, the velocity is greater, and the density less, than in either of the component streams. When the courses of two streams cross at right angles, the gradations of density in the directions of the courses are very nearly the same as in the separate streams.

7. Conceive, now, that two bar-magnets, B N A, B′N′A′, are brought near each other, with their axes in the same straight line; and, for distinctness, let the axes be in the plane of the magnetic meridian, and let A, A′ be the ends from which the secondary streams always issue. The two currents under these circumstances will interfere with each other, so that the symmetrical distribution of density with reference to the neutral

positions N and N$'$ will be destroyed, and the disturbance will be in greater degree as the distance between the magnets is less. Also the consequences of the disturbance will be different according to the different directions of the currents. First, let B and A$'$ be adjacent ends. Then the streams, always flowing towards A and A$'$, will be in the *same* direction. Hence, by hydrodynamics, the density and pressure of the fluid are diminished by the junction of the streams, in greatest degree in the space intervening between B and A$'$, and in N B and N$'$A$'$ to a greater degree than in N A and N$'$B$'$. Thus the increment of density from the neutral lines towards the ends is less rapid in the adjacent halves, than in the remote halves. Hence the moving force of the æther urging the individual atoms towards the neutral lines is in excess in the remote parts, causing the magnets to move towards each other as if they were *attracted.* The same effect would be produced if the adjacent ends were B$'$ and A, because the two currents would still be in the same direction.

Next, let A and A$'$ be adjacent. In this case, the streams issue from the magnets in *contrary* directions towards the space between A and A$'$, and by their meeting the æther is condensed in such a manner that the decrements of density towards the neutral positions are more effective in the nearer halves of the magnets than in the more remote. The magnets consequently move from each other, or are apparently *repelled.* Lastly, let B and B$'$ be the adjacent ends. The streams now flowing from B towards A and from B$'$ towards A$'$, a diminution of velocity and consequent increment of density, result from their contrary tendencies between B and B$'$. The increase of density in this case may be conceived to be produced by the accelerations of the æther resolved in directions perpendicular to the common axis of the magnets, the resolved parts, in both streams and on both sides of the axis, conspiring to produce motion *towards* that line. Thus by the same reasoning as in the preceding case, the magnets will be *repelled.* Consequently the known law, that *like poles mutually repel and unlike poles mutually attract,* is accounted for on the principles of the hydrodynamical theory.

I take occasion to add that the above considerations respecting the mutual influence of opposing and conspiring magnetic streams, equally apply to the streams to which in the theory of electricity the attractions and repulsions of electrified bodies were attributed, and may be regarded as supplementary to the explanations given in arts. 18, 19, and 20 of the communication to the Number of the 'Philosophical Magazine' for last October.

8. In a similar manner the mutual action between magnets and galvanic currents may be explained. In the theory of galvanism, reasons were given for concluding that the movement of

galvanic currents relative to an electrode is composed of uniform motions parallel to the electrode, and of uniform circular motions about its axis. (See art. 10 of the communication to the 'Philosophical Magazine' for December.) The theory I am about to give of the mutual action between magnets and galvanic currents, essentially depends on the existence of the circular motion: but it is remarkable that the facts to be explained require that *the direction of this motion should be always the same*; that is, if the electrode be parallel to the earth's axis, and the current flow from south to north, the circular motion must be in the same direction as that of the earth's rotation about its axis. I do not at present profess to account antecedently either for the circular motion, or for its having a determinate direction; but I can conceive that both these characteristics of the theoretical galvanic currents may be referable to the primary currents, which, as I shall hereafter attempt to show, have their origin in the earth's rotation.

9. Assuming galvanic currents to be such as are described above, suppose a straight horizontal electrode to be placed in the plane of the magnetic meridian, and the current to flow from south to north; and let a horizontal magnetic needle be placed directly underneath the electrode at a small distance from it. Since the needle and electrode are parallel, if the galvanic current were wholly longitudinal, there would seem to be no cause of disturbance of the needle, because the circumstances of the æther would be alike on the opposite sides of the plane of the magnetic meridian. But suppose the motion of the æther along the electrode to be accompanied by a circular motion about its axis in the direction from above towards the right hand of a person looking northward; and calling the end of the needle which points to the north A and the other end B, let A designate, as heretofore, that end from which the magnetic streams *issue* in curved diverging courses; also, abstract for the present from the longitudinal motions both galvanic and magnetic. Then the circular motion produces a stream which crosses the magnet from *east* to *west*. This stream *meets* the parts of the issuing magnetic streams resolved in the horizontal direction, on the *east* side of the *north* portion of the needle, and *flows with* them on the *west* side. There is consequently an increase of density and pressure on the east side, and a diminution of density and pressure on the west side, and the needle is consequently urged towards the *west*. About the end B and the *south* portion of the needle, where the entering streams converge in curved courses, the parts resolved horizontally conspire with the circular streams on the *east* side, and oppose them on the *west* side. The greater density of the æther is therefore on the west side, and the south

end of the needle is consequently urged towards the *east.* As the actions on the opposite halves of the needle are equal and in contrary directions, the total effect is simply *a motion of rotation,* and the north end of the needle deviates towards the west, the direction of the galvanic current being from south to north.

If the direction of the current be reversed, the circular stream passes across the magnet from *west* to *east,* and consequently by the same reasoning as before, the *north* portion of the needle is urged *eastward,* and the *south* portion *westward.* It is clear that the directions of the deviations of a needle *above* the current are the opposite to those of a needle below, the direction of the effective parts of the circular streams being opposite.

This directive action of the galvanic current vanishes when the axis of the needle is transverse to the direction of the current. If the current acts simultaneously with the earth's magnetism, the needle must take a position intermediate to the transverse position and the plane of the magnetic meridian.

All these inferences from the theory are in accordance with well known results of experiment.

10. The curvilinear *paths of the ætherial streams* in the above theory of the magnet, correspond to Faraday's *lines of magnetic force,* and points of greater or less *velocity* correspond to points of greater or less *magnetic intensity.*

11. The explanation of the reciprocal action of magnets on galvanic currents on the same principles appears to be as follows. In the case first supposed, the magnet being *under* and *parallel* to the galvanic current, let the electrode be moveable about a vertical axis, and the magnet be fixed. Then the moving force which urges the magnet when moveable, reacts upon the circular current, and disturbs its uniformity. It has been previously argued that the circular movement is necessary for maintaining the galvanic current, by preventing its flowing towards the axis of the electrode. Hence under this disturbance there will be a tendency of the fluid to rush to the parts towards which the flow of the circular stream is impeded by the partial interruption, that is, to the *west* side of the *north* portion of the electrode. This impetus, being unopposed, will cause the electrode to deviate towards the *east.* Like considerations would show that the *south* portion of the electrode is made to deviate towards the *west.* Thus the magnet and electrode will have the same relative positions as when the former was moveable. Similar explanations may be given in the other cases. In general, it may be said that, as the galvanic current always tends to maintain itself by a uniform circular motion about the axis of the electrode, it tends also to impress on the electrode a movement by which the uniformity of the circular motion, when interrupted, may be most readily restored.

12. The directive action of *terrestrial magnetism* on galvanic currents admits of the following explanation. Let the current flow in a circular electrode moveable about a vertical axis, and let the electrode be so placed in the plane of the magnetic meridian, that the direction of the current at the lowest part may be from *north* to *south*. A diameter to the circle being drawn in the direction of the terrestrial current, that is, from *south* towards *north* in the line of the dipping needle, at and near the *south* extremity of the diameter this current will be confluent with the circular movement about the electrode on the *east* side, and opposed to it on the *west* side. Hence on the latter side there will be an excess of pressure which will tend to move the electrode *eastward*. At the other end of the diameter the terrestrial current flows with the circular stream on the *west* side and opposes it on the *east* side, and consequently tends to move the electrode *westward*. Hence on the whole the electrode will rotate in the direction which accords with observation, till it takes a position perpendicular to the plane of the meridian.

13. The directive action of terrestrial magnetism on the *dipping needle* is explained as follows. When the needle is in its normal position, the extremity A directed northward, the secondary streams flow longitudinally in the same direction as the primary terrestrial stream, and their transverse motions are perpendicular to the latter. There is consequently no tendency of the needle to move from this position. Now let the axis of the magnet be inclined to its normal position in any given direction. The secondary streams will not be altered by this change, because, as before explained, the accelerative forces to which they owe their origin are independent of the direction of the primary stream. But in the new position the two streams will influence each other so as to give rise to dynamic action on the needle. Conceive, for the sake of distinctness, the end A of the needle to deviate about 30° from the normal position towards the *west*. Then resolving both the primary and the secondary streams in the directions perpendicular and parallel to the needle, the respective perpendicular streams along the north portion of the needle (this being the portion from which the secondary streams *issue*) will be opposed to each other on the *west* side of the needle, and conspire on the *east* side. Thus the needle will be urged *eastward*. On the *south* portion of the needle, at which the secondary streams are confluent, the perpendicular components are opposed to each other on the *east* side and conspire on the *west* side, and that portion is consequently urged *westward*. As the two actions are equal and opposite, the needle has simply a *motion of rotation*, and the directive action is always *towards the normal position*.

14. It follows that the directive force of the earth's magnetism

is a measure of the *velocity* of the terrestrial current, that is of the *total intensity* of the magnetic force. According to the theory, this force may be supposed to be the resultant of horizontal and vertical components, and *the ratio of the latter to the former is the tangent of the Inclination.*

Cambridge Observatory,
December 22, 1860.

[To be continued.]

IX. *On a new Resistance Thermometer.*

To Professor John Tyndall, F.R.S., &c., Royal Institution.

3 Great George Street, Westminster, S.W.
December, 1860.

My dear Sir,

YOU will probably be interested to hear about a very direct application of physical science to a purpose of considerable practical importance, which I had lately occasion to make. Having charge, for the British Government, of the Rangoon and Singapore telegraph cable, in so far as its electrical conditions are concerned, I was desirous to know the precise temperature of the coil of cable on board ship at different points throughout its mass, having been led by previous observations to apprehend spontaneous generation of heat. As it would have been impossible to introduce mercury thermometers into the interior of the mass, I thought of having recourse to an instrument based upon the well-ascertained fact that the conductivity of a copper wire increases in a simple ratio inversely with its temperature. The instrument consists of a rod or tube of metal about 18 inches long, upon which silk-covered copper wire is wound in several layers so as to produce a total resistance of, say 1000 (Siemens) units at the freezing temperature of water. The wire is covered for protection with sheet india-rubber, inserted into a tube and hermetically sealed. The two ends of the coil of wire are brought, by means of insulated conducting wires, into the observatory, where they are connected to measuring apparatus, consisting of a battery, galvanometer, and variable resistance coil. The galvanometer employed has two sets of coils, traversed in opposite directions by the current of the battery. One circuit is completed by the insulated thermometer coil, and the other by a variable resistance coil of German silver wire. Instead of the differential galvanometer, a regular Wheatstone's bridge arrangement may be employed.

You will readily perceive that if the thermometer coil before described were placed in snow and water, and the variable resistance coil were stoppered so as to present 1000 units of resistance, the currents passing through both coils of the differential galvanometer would equal one another, and produce, therefore, no deflection of the needle. If, however, the temperature of the water should rise, say 1° Fahr., its resistance would undergo an increase of $1000 \times \cdot 0021 = 2 \cdot 1$ units of resistance, necessitating an addition of 2·1 units to the variable resistance coil in order to re-establish the equilibrium of the needle.

The ratio of increase of resistance of copper wire with increase of temperature may be regarded as perfectly constant within the ordinary limits of temperature; and being able to appreciate the tenth part of a unit in the variable resistance coil employed, I have the means of determining with great accuracy the temperature of the locality where the thermometer resistance coil is placed. Such thermometer resistance coils I caused to be placed between the layers of the cable at regular intervals, connecting all of them with the same measuring apparatus in the cabin.

After the cable had been about ten days on board (having left a wet tank on the contractors' works), very marked effects of heat resulted from the indications of the thermometer coils inserted into the interior of the mass of the cable, although the coils nearer the top and bottom surfaces did not show yet any remarkable excess over the temperature of the ship's hold, which was at 60° Fahr. The increase of heat in the interior progressed steadily at the rate of about 3° Fahr. per day, and having reached 86° Fahr., the cable would have been inevitably destroyed in the course of a few days, if the generation of heat had been allowed to continue unchecked.

Considering the comparatively low temperature of the surface of the cable, much incredulity was expressed by lookers-on respecting the trustworthiness of these results; but all doubts speedily vanished when large quantities of cold water of 42° temperature were pumped upon the cable, and found to issue 72° Fahr. at the bottom.

Resistance thermometers of this description might, I think, be used with advantage in a variety of scientific observations,—for instance, to determine the temperature of the ground at various depths throughout the year, or of the sea at various depths, &c. &c. In the construction of this instrument, care has to be taken that no sensible amount of heat is generated by the galvanic currents in any of the resistances employed.

By substituting an open coil of platinum wire for the insulated copper coil, this instrument would be found useful also as a pyrometer.

But, finding this letter already exceeds its intended limits, I shall not enlarge upon these applications, which, no doubt, are quite obvious to you.

I am, dear Sir,
Yours very truly,
C. Wm. Siemens.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xx. p. 550.]

March 22, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read:—

"On the Insulating Properties of Gutta Percha." By Fleeming Jenkin, Esq.

The experiments described in this paper were undertaken with the view of determining the resistance opposed by the gutta-percha coating of submarine cables at various temperatures to the passage of an electric current.

The experiments were made at the works of R. S. Newall and Co., Birkenhead. The relative resistance of the gutta percha at various temperatures was determined by measuring the loss on short lengths immersed in water. These experiments are described in the first part of the paper. The absolute resistance of gutta percha has been calculated from the loss on long submarine cables. These experiments and calculations are described in the second part of the paper.

PART I.

The loss of electricity was measured upon three different coils, each one knot in length. One was covered with pure gutta percha; the two remaining coils were covered with gutta percha and Chatterton's compound. The coils were kept at various temperatures by being covered with water in a felted tub; and the water was maintained at a constant temperature for twelve or fourteen hours before each experiment.

The loss or current flowing from the metal conductor to earth through the gutta-percha coating was measured on a very delicate sine-galvanometer. The loss from the connexions when the cable was disconnected, was measured in a similar manner. The electromotive force of the battery employed was on each occasion measured in the manner described by Pouillet. Corrections due to varying electromotive force and loss on connexions were made on the result of each experiment.

A remarkable and regular decrease in the loss was observed for some minutes after the first application of the battery to the cable; a phenomenon, which the author thinks may be due to the polarization of the molecules of gutta percha, or of the moisture contained in the pores of the gutta percha. The loss was therefore measured from minute to minute for five minutes, with each pole of the battery.

Nineteen tables containing the results, with the reductions and curves representing the results, accompany the paper. The following results were obtained from the first coil; this was prepared with Chatterton's patent compound. With a negative current between the limits of 50° and 80° Fahrenheit, the decrease of resistance is sensibly constant for equal increments of temperature; and the increase of resistance due to continued electrification is also nearly constant. At 60° the resistance increases about 20 per cent. in five minutes from this cause. With a positive current, similar results appear between the temperatures of 50° and 60°; but the resistance is somewhat greater than with the negative current. The extra resistance due to continued electrification is unchanged by a change in the sign of the current. Above the temperature of 63° great irregularities occur in the observations, which could not even be included in regular curves. The difference in the resistance of the gutta-percha coating when the copper is positively and negatively electrified, may be caused by the contact between the resinous compound and the copper: no such difference was observed when pure gutta percha was in contact with the copper.

The curves resulting from the experiments on the second coil, which was covered with pure gutta percha, present an entirely different character from those resulting from the first coil. The copper and gutta percha were of the same size in these two coils. The resistance of pure gutta percha at low temperatures is greater than that of the compound covering. At 65° the resistance of the two coverings is equal; at higher temperatures the resistance of pure gutta percha diminishes extremely rapidly. The curves obtained with positive and negative currents are identical up to about 75°; a slight difference occurs above this temperature, which may have been accidental. The extra resistance is less with pure gutta percha than with the compound; it increases slightly at high temperatures, and is not affected by a change in the sign of the current.

The curves derived from the experiments on the third coil, which contained a smaller proportion of Chatterton's compound than the first coil, appear in some respects intermediate between those derived from the first and second coils. The extra resistance due to continued electrification was still greater in this coil than in the others. 40 per cent. of the entire resistance is at 70° due to this cause. This increase is believed to be due to the greater mass of gutta percha used in covering this coil, which was of larger dimensions than the two others.

Part II.

Professor Thomson has supplied an equation expressing the law which connects the resistance of a cylindrical covering, such as that of a cable, with the resistance of the unit of the material forming the covering.

Let S be the specific resistance of the material, or the resistance of a bar one foot long, and one square foot in section; let G be the resistance of the cylindrical cover of a length of cable L; let $\frac{a}{b}$ be the ratio of the external to the internal diameter of the covering; then

$$S=\frac{2\pi LG}{\log\frac{a}{b}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The resistance G was calculated from cables of various lengths, lying in iron wells at the works of R. S. Newall and Co., Birkenhead.

The cables were not wet; but direct experiment proved that covering a sound iron-covered cable with water has no effect on the loss. The details of this experiment are given in the paper.

The resistance G was obtained in the following manner. The copper conductor of the cable to be tested was arranged so as to form a complete metallic arc with a battery of 72 cells and a tangent galvanometer: the deflection on this galvanometer was read and entered as the continuity test. Deflections were then read on the same galvanometer with the battery and several known resistances in circuit, for the purpose of measuring the resistance and electromotive force of the battery, in the manner described by Pouillet. The deflection caused by the loss was next read on a second tangent galvanometer: the same battery was used. This deflection was entered as the insulation test. The temperature of the tank containing the cable was observed by means of a thermometer inserted in a metal tube, extending from the circumference into the mass of the coil.

The relative delicacy of the galvanometers was ascertained by experiment, or, in other words, the coefficient was found by which the tangents of the deflections of the first were multiplied to render them directly comparable with the tangents of the deflections of the second galvanometer.

The resistances of the galvanometer coils, of the artificial resistance coils, and of the copper conductor of the cable were measured by Wheatstone's differential arrangement. Special experiments were made by means of this differential arrangement to determine the change of resistance of the copper conductor in the cable, produced by a change of temperature.

The equation (No. 2) $R=r(1+0{\cdot}00192t)$ gives the value of the resistance R of the copper wire at any temperature $t+a$ in function of the resistance r at any temperature a (Fahrenheit). The length and temperature of any coil being known, the resistance of the copper wire was thus at once obtained from the resistance of one knot at 60°, which was very carefully determined.

Now let G = resistance of cylindrical coating.
D = deflection called the continuity test.
d = deflection called the insulation test.
C = coefficient expressing the relative delicacy of the two galvanometers.
BR = resistance of the battery.
T_1 = resistance of the coil of first galvanometer.
T_2 = resistance of the coil of second galvanometer.

Then
$$G=\frac{C\tan D\times(BR+T_1+M)}{\tan d}-BR+T_2+\frac{M}{2}\cdot\ldots\quad(3)$$

G having been thus obtained in any desired units, S, the specific resistance of the material, can be at once obtained by equation No. 1, which appears from several experiments to give constant values for S when calculated from cables of different dimensions. In extreme cases, however, the influence of extra resistance would render the formula defective, especially after continued application of the current: thus the resistance of a foot-cube would be very different to that of an inch-cube.

The values of G for the covering of the Red Sea cable, after continued electrification for periods of one, two, three, four, and five minutes, were calculated in Thomson's Absolute British Units, from four sets of tests made specially for this purpose on four different cables, each about 500 knots long. Tables containing the results of these calculations accompany the paper.

A Table is also given of the resistance of the Red Sea covering after one minute's electrification, and after five minutes' electrification, at each degree of temperature, from 50° to 75° Fahrenheit. This Table was formed by means of the temperature curves described in the first part of the paper: this Table is here annexed (No. 1).

Similar Tables were given for the covering of the two experimental coils mentioned in the first part of the paper. The coil composed of pure gutta percha, gave very regular and complete results. An abbreviation of the Table is annexed.

It was remarked that in the tests of the cable in the iron tanks, the resistance after five minutes' electrification was invariably greater with zinc than with copper to cable, whilst the reverse was the case with the single knot covered by water. The length of the cable, and the condition of immersion or non-immersion, have probably some influence on the phenomenon of extra-resistance. This phenomenon appears to the author to be of much importance, and to demand further investigation.

The values of G were also calculated from the daily tests of the cables during manufacture at many temperatures. These values agreed with those given in the Tables above described. The general results of the experiments may be summed up as follows.

The relative loss at various temperatures through pure gutta percha has been pretty accurately determined for all ordinary temperatures. To a less extent the same knowledge has been gained concerning two other coatings containing Chatterton's compound. The latter appears superior at high, and inferior at low temperatures.

Attention has been drawn to the considerably increased resistance which follows the continued electrification of gutta percha and its compounds. Some of the laws of this extra resistance have been determined, and some suggestions made as to the cause of the phenomenon.

The bounds have been pointed out within which formulæ may be used, which consider gutta percha as a conductor of the same nature as metals.

The resistance of gutta percha has been obtained in units, such as are employed to measure the resistance of metals; and by the use of Professor Thomson's formula, the specific resistance of a unit of the material has been fixed with some accuracy.

The resistance of other non-conductors, such as glass and the resins, may probably, by comparison with gutta percha, be obtained in the same units.

Incidentally, the increase of resistance in copper with increased temperature has been given from new experiments; and it has been shown that the insulation of a sound wire-covered cable is little, if at all, affected by submersion.

Finally, tables and formulæ are given by which the resistance of, or the loss through any new cable coated with gutta percha, may be at least approximately estimated:—

TABLE I.

Specific Resistance in Thomson's Units of the Red Sea Covering at various Temperatures.

Temperature.	Zinc to cable.		Copper to cable.	
	After electrification for one minute.	After electrification for five minutes.	After electrification for one minute.	After electrification for five minutes.
60°	2162×10^{17}	3330×10^{17}	2239×10^{17}	3405×10^{17}
65	$1810 \times$ „	$2947 \times$ „	$1720 \times$ „	$2770 \times$ „
70	$1460 \times$ „	$2378 \times$ „	$1318 \times$ „	$2239 \times$ „
75	$1160 \times$ „	$1753 \times$ „	$1000 \times$ „	$1739 \times$ „

TABLE II.

Specific Resistance in Thomson's Units of pure Gutta Percha at various Temperatures.

Temperature.	Zinc to cable.		Copper to cable.	
	After electrification for one minute.	After electrification for five minutes.	After electrification for one minute.	After electrification for five minutes.
50	4113×10^{17}	5663×10^{17}	4113×10^{17}	5663×10^{17}
55	$2917 \times$ „	$3636 \times$ „	$2917 \times$ „	$3636 \times$ „
60	$2163 \times$ „	$2549 \times$ „	$2163 \times$ „	$2549 \times$ „
65	$1634 \times$ „	$1858 \times$ „	$1634 \times$ „	$1858 \times$ „
70	$1162 \times$ „	$1291 \times$ „	$1193 \times$ „	$1291 \times$ „
75	$805 \times$ „	$877 \times$ „	$796 \times$ „	$866 \times$ „
80	$566 \times$ „	$613 \times$ „	$548 \times$ „	$591 \times$ „

"On Scalar and Clinant Algebraical Coordinate Geometry, introducing a new and more general Theory of Analytical Geometry, including the received as a particular case, and explaining 'imaginary points,' 'intersections,' and 'Lines.'" By Alexander J. Ellis, Esq., B.A., F.C.P.S.

XI. *Intelligence and Miscellaneous Articles.*

THE LITHIUM SPECTRUM.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

PERMIT me through you to ask MM. Kirchhoff and Bunsen, or other experimentalists in this country who may be in possession of the requisite apparatus, whether the "very weak yellow line Liβ," described by them[*] as accompanying the brilliant red line Li *α*, requires any extraordinary precautions to render it visible. Two specimens of lithium salts which I have examined in a very perfect apparatus, somewhat similar to the one described by them[†], have failed to give the slightest evidence of its presence, although I have repeatedly examined them with that object; and as I know that other experimentalists in this country have been equally unsuccessful, it is possible that the presence of this line in the spectrum given by MM. Kirchhoff and Bunsen's specimens of lithia may really be due to the presence of another element hitherto unknown. The spectrum of the new alkali metal Cæsium (which it may be of interest to know I have detected in some highly concentrated mother-liquors from sea-water) is in no respect similar to it. It will be a sufficient proof of the delicacy of my apparatus, to say that it is constructed principally of quartz, and that it easily separates the double line D.

I remain, Gentlemen,
Your obedient Servant,
William Crookes.

A CONSTANT COPPER-CARBON BATTERY. BY JULIUS THOMSEN.

In the ordinary galvanic apparatus, zinc usually officiates as positive element. This metal is, however, readily attacked by acid if it is not either chemically pure or well amalgamated. If the sulphuric acid is not greatly diluted, the zinc cylinders are strongly attacked by continuous use, in spite of the amalgamation, by which a great loss of metal is caused; if, on the other hand, the acid is much diluted, it is soon saturated, and the action of the apparatus is enfeebled.

In my investigations I use a galvanic apparatus consisting of *copper* in dilute sulphuric acid (1 part acid and 4 parts of water) as positive element, and, as a negative element, *carbon* in the mixture of bichromate of potass, sulphuric acid, and water, recommended by Wöhler and Buff. (Buff uses 100 parts water, 12 of bichromate, and 25 of sulphuric acid.) The electromotive force of this combination is $\frac{9}{10}$ths of that of a Daniell's battery.

Its advantages are as follows:—The copper is not at all attacked by the acid when the circuit is open; the resistance of the sulphuric acid, from its being so little diluted, is a minimum: the sulphuric acid is so strong that it can be used for months without becoming saturated. As, further, the mixture of chromate of potass and sulphuric acid is inodorous, this combination is very convenient for working with in closed spaces.

This combination is very interesting theoretically; for as copper cannot decompose dilute sulphuric acid, the copper-carbon element is an example of a powerful apparatus in which chemical action and the disengagement of electricity are quite inseparable.—Poggendorff's *Annalen*, October 5, 1860.

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[*] Phil. Mag. S. 4. vol. xx. p. 96. August, 1860.

[†] Ibid. p. 90.

Fig: 3.

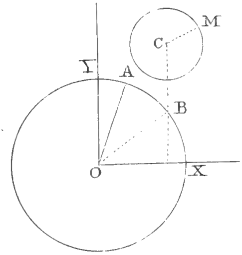


Fig: 4.

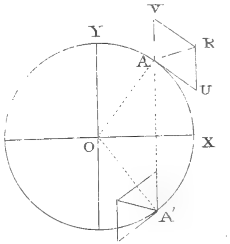
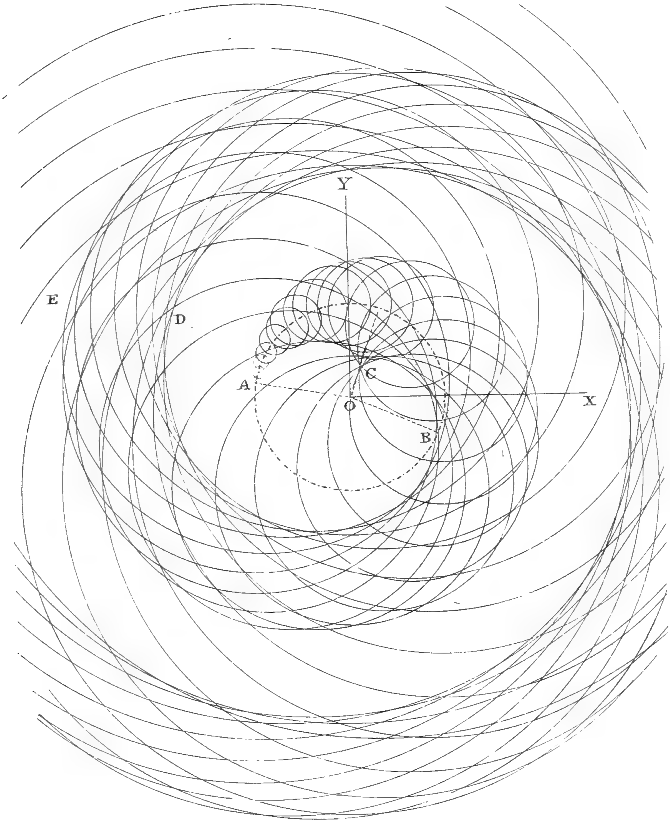


Fig: 7.



J. Basire sc.

Fig. 1.

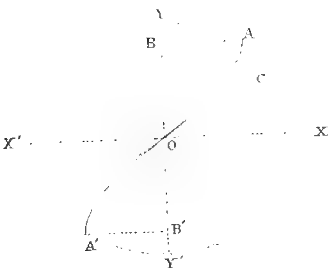


Fig: 2.

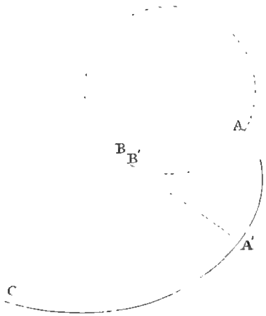


Fig: 3.

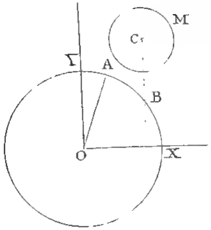


Fig: 4.

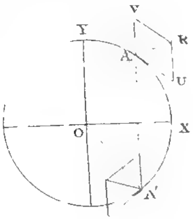


Fig: 5.

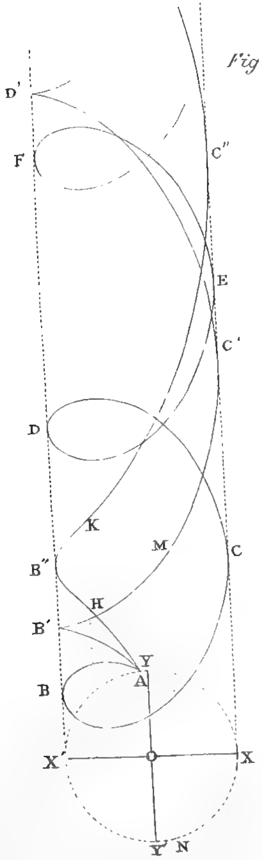


Fig: 6.

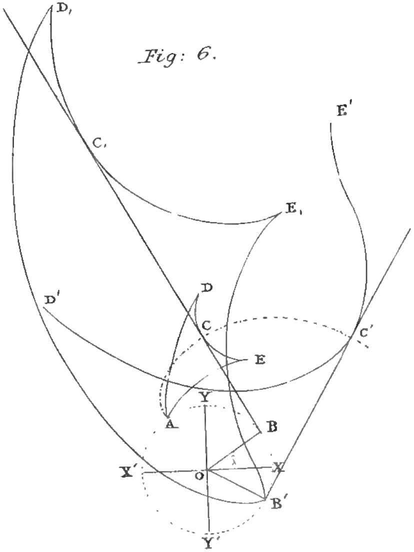
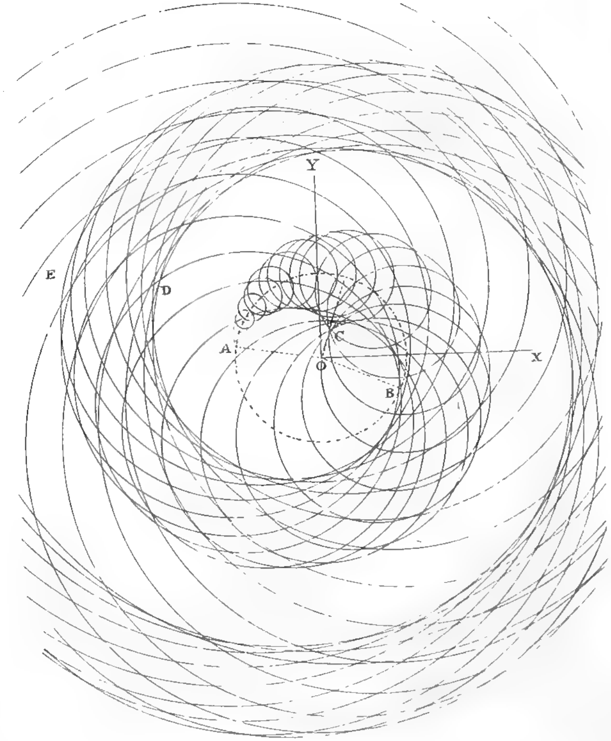


Fig: 7.



J. Basire sc.

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

[FOURTH SERIES.]

FEBRUARY 1861.

XII. *Note respecting* Ampère's *Experiment on the Repulsion of a Rectilinear Electrical Current on itself.* *By* JAMES D. FORBES, *D.C.L., V.P.R.S.E., Principal of the United College, St. Andrews, late Professor of Natural Philosophy in the University of Edinburgh*[*].

[With a Plate.]

IN a communication to the Royal Society of Edinburgh, on the 3rd of January, 1859, I related some experiments on the vibrations of metallic bodies and of carbon, produced by the passage of electricity through them[†]. I learned soon after, from a paper by Dr. Tyndall[‡], that these vibrations had been already described by Dr. Page of the United States[§]. The analogy of the experiment to that of Mr. Trevelyan, on the vibrations of metallic bodies by heat, led me to suspect that the resistance to the propagation of heat in the first case, and of electricity in the second, in its passage from the one to the other of the bodies in contact, was the cause of a sensible mechanical repulsion. So strongly was I persuaded of this, that twenty-seven years ago, when making my experiments on the "Trevelyan" bars, I attempted to set them in motion by electricity, though then ineffectually.

I am aware that the idea of the idio-repulsive quality of the heat-current has not been favourably received, and that the high authority of Mr. Faraday is still given for an explanation of the

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[*] Communicated by the Author, having been read to the Royal Society of Edinburgh, January 7, 1861.

[†] Proceedings of the Royal Society of Edinburgh, vol. iv. p. 151; and Phil. Mag. vol. xvii. p. 358.

[‡] Phil. Mag. vol. xvii. p. 358.

[§] Silliman's Journal (1850), vol. ix. p. 105.

phenomena founded on the familiar laws of conduction and expansion alone. Dr. Tyndall thinks that the effects of electricity are also to be ascribed to the indirect action of the heat which it produces, in the way of expansion. I have never been satisfied with either of these explanations. The conviction to which I still hold is that the alleged expansion and contraction cannot sensibly operate in the almost infinitely short periods of successive contact and separation, and that the effect must be due to a molecular impulse of a far more sudden and instantaneous character*. We seem to be sufficiently conversant with such impulses in galvanic, and especially in secondary induced currents.

This inquiry, however, into the effects of an electrical current *upon itself*, and upon the different portions of one and the same conductor conveying it, is one of extraordinary difficulty. We cannot fail to be struck by the hesitation with which that great Master of this branch of science, Mr. Faraday, expresses himself in different parts of his writings respecting them †.

There is one experiment, quoted in modern treatises on electricity, which might seem to throw light on the subject; and that is Ampère's experiment on the repulsion of one portion of an electrical current upon another portion which is a continuation of it in the same right line. This repulsion appeared to be the necessary complement of Ampère's theory of the mutual action of currents placed in different positions relatively to one another. He considered it to be sufficiently demonstrated by the following experiment:—A and B (Plate II. fig. 5) are two little troughs filled with mercury, or rather a single trough divided lengthwise into two by the glass partition C D. Two straight pieces of copper wire *a*, *b*, united by the bridge *c*, float on the mercury, which, however, they are everywhere prevented from touching metallically by means of a coating of sealing-wax, except at the extreme ends nearest to A and B, where they are amalgamated, and of course touch the mercury. The two poles of a *powerful* battery being inserted in the mercury troughs near the letters A, B, the circuit is evidently completed through the wire *a c b*. It is stated that when this is done, the floating wire is repelled from the vicinity of the terminal wires of the battery. This repulsion is ascribed to the reaction of a rectilinear current from A to *a* through the mercury, upon the continuation of the current in the copper conductor *a*, and to a similar action on the side *b* B.

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* That the electrical vibrations go on during the copious affusion of the apparatus with cold water, and that they take place with remarkable energy in an almost inexpansible substance like carbon (see my paper cited above), are direct arguments in favour of this conclusion.

† See particularly the Ninth and Thirteenth Series of his 'Researches.'

This remarkable experiment has been circumstantially described in more places than one by Ampère himself*; but the description is in all essentially alike, and the accounts, being nearly contemporaneous, may be assumed to refer to one and the same trial, which was made, it appears, at Geneva, and in the presence of M. Auguste De la Rive. This last is a circumstance of some importance; for it would perhaps be difficult to establish that it has ever been successfully repeated since. The authors whom I have consulted usually content themselves with the barest citation of Ampère's authority. The only exception which I know of is in Müller's *Lehrbuch der Physik* (vol. ii. p. 318), where the writer notes the difficulty he found in repeating the experiment: "Ferner muss noch angeführt werden dass dieser Versuch keineswegs zu den leicht gelingenden gerechnet werden kann." From which I infer that the writer had *not* succeeded in repeating it. He also raises doubts (which seem to be reasonable) as to whether, supposing it successful, the conclusion of Ampère could with certainty be legitimately drawn from it.

It appeared to me to be a matter of some interest in connexion with the experiment of electrical vibration, to repeat the observation of Ampère. I had an apparatus made for the purpose, but circumstances have hitherto prevented me from using it; and I have requested my successor in the Chair of Natural Philosophy, Professor Tait, to make trial of it when he happens to have a powerful voltaic battery in action. I did, however, attempt the experiment in a different form, and, as it appears to me, in one both more sensitive in its indications, and less ambiguous in its interpretation†.

I fitted up a Coulomb's torsion apparatus (one that had been used for experiments on diamagnetism) in the following way. It is represented in outline in fig. 6. A moderately fine platinum wire A, about 18 inches long, was used to suspend a wooden torsion-rod B C, near one end of which (laid on a piece of pasteboard fastened to the rod) was poised a copper wire bent in the form of a horseshoe, as shown in the figure at *a b*‡. Two strong copper wires P, N, the terminals of a Grove's battery of four moderate-sized pairs in good action, were brought into the position indicated in the figure, so as to be exactly opposite to, and in continuation of, the limbs of the horseshoe. The terminals P, N were kept firmly in their place, and the extremities *a*, *b*,

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* *Recueil d'Observations Electro-dynamiques*, 8vo, pp. 285, 318. Also *Bibliothèque Universelle*, vol. xxi. p. 47; and Ampère's *Théorie*, p. 39 (this last citedby Dr. Roget in his 'Electro-Magnetism,' art. 187).

† The following experiments were made in December 1858 and January 1859.

‡ The distance *a b* might be half an inch, and the distance from the suspension wire was relatively greater than the figure indicates.

of the horseshoe were adjusted by moving it on the pasteboard shelf until simultaneous contact of the two limbs with the fixed wires was obtained on moving the graduated head of the torsion wire A from right to left. The completion of the current was effected without much difficulty by gently pressing the rough extremities of the wires into contact by the help of the torsion head.

I found it convenient to increase the tension of the current by introducing into the circuit an ordinary electrodynamic coil of stout wire, with or without a core of iron wires.

So far from finding anything like *repulsion* of the moveable horseshoe on the continuation of the circuit, I soon observed that *attraction* took place, resisting the force of twist of the platinum wire exerted through a considerable arc, when it tended to produce separation. This attraction continued to subsist even after the current was withdrawn.

With a horseshoe composed of two straight limbs, *a* and *b*, of bismuth, the connexion *c* being of copper, a still more decided attraction was the result; and it continued to subsist after withdrawing the current of electricity, even if only one of the poles touched the horseshoe.

When the terminals P, M were somewhat flatly rounded (as in fig. 6 *a*), and the extremities of the horseshoe also carefully rounded and polished, I was unable to obtain electrical contact by the pressure due to mere torsion. The current did not pass until the extremities were touched with nitrate of mercury and wiped dry, when it passed readily; and a marked adhesion took place while the current lasted, and for some time after it stopped. While the electricity passed, a fizzing noise was audible from the extremities of the wires. This *might* possibly be due to an insensible vibration. It thus appears that the irregularities of surface of the wires at the points of contact in the first form of the experiment was favourable to the electrical propagation, as it is in the case of ordinary frictional electricity.

A current from a very powerful Ritchie's induction coil, excited by four pairs of Grove's, was next passed through the apparatus last described. Distinct adhesion took place of the ends in contact, opposing a force amounting to 90° or more of torsion. It continued while the current passed, and for some time after. By and by they separated through the effect of torsion.

This experiment was very instructive. If it commenced when the terminations were a little apart, the high-tension electricity passed readily in the form of sparks from P and N to the horseshoe—the horseshoe steadily approaching—the sparks getting shorter and finally vanishing at both contacts, the attraction continuing and facilitating electrical contact. Apparently the very same process, of polarity preceding conduction, obtains in the ordinary galvanic current.

The form of the terminals P, N was again altered to a hammer-shape, as in fig. 6 *b*; and now electrical contact with the copper horseshoe was even more difficult to obtain than before. Though for intensity eight pairs of Grove's battery and a coil were used, the current did not pass without amalgamation, and also decided pressure being used; then the adhesion became strong, amounting to 140° of torsion of the suspending wire.

A similar result was obtained when the current was passed from one of the hammer-shaped poles A (fig. 7) through a straight wire B, laid on the torsion-rod and terminating in a small trough of mercury C, connected with the opposite pole of the battery. The adhesion and other phenomena were in all respects the same as before.

I think it will not be doubted that these experiments are far more delicate tests than Ampère's floating wire, of the mutual action of two portions of one and the same current, and also that the first form is free from the ambiguity which the introduction of a fluid conductor occasions. At the same time the result, with reference to Ampère's theory, would be more satisfactory if the two rectilinear currents whose mutual action is examined were independent of one another, and not parts of one and the same current; for the break in the conductor, necessary to the mobility of the parts, introduces a peculiarity due rather to the force required to propagate the current at all, than to the reciprocal action of two currents propagated independently.

It is needless to add that the result of these experiments is not favourable to the cause which I have assigned to the electrical vibrations; but I have not the less thought it desirable to record the result, though it leaves that experiment, in my opinion, more in need of explanation than ever.

I have not overlooked the probability of the maintaining power of electricity in the rocking-bar being due to instantaneous induction currents (Faraday's) excited at the two points of contact of the bar and block round which the rocking takes place; but some experiments made on this supposition led to no result.

I would, before concluding this paper, direct the attention of electricians to another of those isolated experiments connected with this subject which require confirmation: I mean the production of "Davy's cones," said to occur when two terminals of a very powerful battery, coated with sealing-wax and naked only at the ends, are introduced, parallel and vertically, through the bottom of a cup filled with mercury, and reach to within a little space of the surface of that fluid. On two occasions—once with 500 or 600 pairs of the Royal Institution battery, and again with

Mr. Pepys's gigantic single galvanic pair at the London Institution—cones of mercury rose over the general level of the fluid and above the polar wires[*]. Even Mr. Faraday has failed in repeating the experiment, which is remarkable since, though he only used 100 pairs, the electromotive power of modern batteries vastly exceeds that of those used in Davy's time, and the success with Pepys's apparatus shows that intensity is not indispensable[†]. Mr. Faraday thinks that Davy's cones and Ampère's repulsion experiment are due to one and the same cause. But I incline to believe that Davy's cones intimate an attraction, and not repulsion, and so far concur with the results of my experiments with the torsion-balance.

I have had Davy's apparatus constructed and placed in the Natural Philosophy Class Collection in Edinburgh, where I hope that it may one day be tested with an adequate power.

Pitlochry, N. B.,
July 11, 1860.

Postscript.—Soon after writing the preceding notice, I requested Professor A. De la Rive to give me any confirmation which his memory could supply, of the success of Ampère's experiment; and this he has kindly done with all the precision which might be expected from him. He adds that the motion could not be due to the action of the earth's magnetism, as it was independent of the direction of the current.

United College, St. Andrews,
November 26, 1860.

22 *January* 1861.—I have just learned from Professor Tait that he has succeeded in repeating Ampère's experiment.

XIII. *On the existence of a Fourth Member of the Calcium group of Metals.* *By* F. W. *and* A. Dupré[‡].

DURING our recent examination of London waters by the beautiful method of Kirchhoff and Bunsen, we several times noticed a faint blue line not due to strontium or potassium, or to the lately discovered cæsium. Having since worked with larger quantities of the deep well-water which had given this line most distinctly, we believe we are now justified in stating that the group of calcium, strontium, and barium, like that of the alkali-metals, contains a fourth member. This new metal gives but

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[*] Sir H. Davy in Philosophical Transactions for 1823. Compare Mr. Faraday's 'Researches in Electricity,' Ninth and Thirteenth series, arts. 1113 and 1609.

[†] Mr. Faraday's 'Researches,' as above cited.

[‡] Communicated by the Authors.

one blue line*, situated between the lines Sr δ† and K β, about twice as far from the former as from the latter. In brightness and sharpness of definition it is quite equal to the line Sr δ. The method which, after repeated trials, we found most advantageous for obtaining it in a state of comparative purity is the following. The deposit formed by boiling the water was dissolved in hydrochloric acid, and a small quantity of sulphuric acid added to the clear solution. The precipitate formed, consisting principally of sulphate of calcium, but containing some sulphate of barium, sulphate of strontium, and sulphate of the metal under consideration, was collected, washed, dried and fused with carbonate of sodium. The fused mass was then boiled with water, and the insoluble carbonates of the above-named metals collected and treated several times successively in the same manner as the original deposit. By these means the lime was gradually removed, the carbonates becoming proportionally richer in barium, &c. Owing to the small quantity of substance at our command, we did not attempt to remove the lime entirely. The carbonates finally obtained were once more dissolved in hydrochloric acid, and the solution, after considerable dilution, mixed with a few drops of sulphuric acid. After standing at rest for twenty-four hours, the slight quantity of deposit formed, consisting of almost pure sulphate of barium‡, was filtered off, and some alcohol added to the filtrate, by which a further deposit was obtained, composed chiefly of the sulphates of strontium and the new metal, though not quite free from sulphate of calcium. These sulphates were again converted into carbonates, which were then dissolved in hydrochloric acid, and the solution evaporated to dryness. When a portion of this dry residue was brought into the flame of the apparatus, the spectra of calcium and strontium appeared; and, in addition, beyond the line Sr δ, in the position indicated above, a blue line, rivalling the strontium-line in brilliancy and distinctness of outline.

As far as we have been at present able to ascertain, the carbonate, oxalate, and sulphate of the new metal are insoluble in water, the last-named possessing about the same insolubility as sulphate of strontium. The chloride of the metal does not seem to be hygroscopic, resembling in this respect that of barium rather than those of strontium and calcium.

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* Having been unable to separate it completely from calcium and strontium, we are not quite positive whether or not it gives any lines at the red end of the spectrum.

† In our last communication there is a typographical error, Sr γ being put for Sr δ.

‡ We have detected the presence of barium in several London waters since the publication of our notice in the November Number of the Philosophical Magazine.

We hope soon to be in possession of a sufficient quantity of some salt of this metal to give a fuller account of its properties.

In reference to the letter of Mr. Crookes in the last Number of this Magazine, stating his inability to recognize the yellow line Li β, we may say that all the different specimens of lithium-salts we have experimented with have shown this line distinctly. Since the publication of his letter, we have examined some very pure specimens of lithium-salts, all of which gave the yellow line Li β with equal distinctness, though showing no other lines except Li α and β. The red line may certainly be recognized with quantities of lithium-salt much smaller than are required to exhibit the yellow line.

The apparatus we use is one similar to that described in Kirchhoff and Bunsen's paper, and we find no particular precautions necessary to exhibit the line in question, beyond using a comparatively large quantity of lithium-salt, say $\frac{1}{20}$ to $\frac{1}{50}$ of a grain, and a thin platinum wire of about $\frac{1}{5}$ millim. diameter.

We may here mention incidentally, that we have found it advisable to discontinue the use of a brass Bunsen's burner, to prevent the occasional appearance of a series of green and blue lines, due to copper. A Bunsen's burner made of steatite is quite free from any such defect. The flame of the burner should also be regulated in such a manner as to have the part placed opposite the slit free from any marked blue tint, as otherwise a number of lines, one green and three blue being the most conspicuous, make their appearance. The blue or lower part of the flame of a candle or of an oil-lamp shows precisely the same lines.

XIV. *On the Insulation of Antozone. In a Letter to* Professor FARADAY. *By* Professor SCHŒNBEIN*.

I HAVE been working very hard these many months to obtain the "antozone," or ⊕, in its insulated state, and I flatter myself that I have succeeded, at least to a certain extent. You are aware that, from a number of facts, notably from the reciprocal deoxidizing influence exerted by many oxy-compounds upon each other, I drew the inference that there existed two series of oxides, one of which contains Θ, the other ⊕,—the ozonides and antozonides. The mutual deoxidation of those compounds I made dependent upon the depolarization or neutralization of ⊕ and Θ into O. Now ⊕ and Θ being capable of being transformed into O, I thought it possible, even probable, that the contrary might be effected, *i. e.* the chemical polarization of O into ⊕ and Θ; and you know that in the course of the last and present year I

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* Communicated by Professor Faraday.

have ascertained a great number of facts that speak, as far as I can see, highly in favour of that idea. As the typical or fundamental fact of this chemical polarization of O, I consider the simultaneous production of $\ominus$ and $HO+\oplus$ which takes place during the slow combustion of phosphorus. This simultaneity is such, that ozone never makes its appearance without its equivalent $HO+\oplus$. All the metals which oxidize slowly, HO being present, such as zinc, lead, iron, &c., give rise to the formation of $HO+\oplus$; and so do a great number of organic substances, such as ether, the tannic, gallic, and pyrogallic acids, hæmatoxidine, &c.; and even reduced indigo, associated with potash, &c., makes no exception to the rule. The same simultaneity takes place during the electrolysis of water; never ozone without peroxide of hydrogen. I admit therefore that O, on being brought into contact with an oxidable substance and water, undergoes that change of condition which I call "chemical polarization;" *i. e.* is converted into $\ominus$ and $\oplus$, the latter of which combines with HO to form $HO+\oplus$, whilst $\ominus$ is associated to the oxidable matter, such as phosphorus, zinc, &c. In the preceding statements you have only a very rough outline of my late researches on the chemical polarization of neutral oxygen: the details on that subject are contained in a number of papers lately published, and of which your English periodicals have as yet not taken any notice. Having gone so far, I could not but be very curious to try whether it was not possible to obtain $\oplus$ in its insulated or free state. I directed of course my attention to that set of peroxides which I call "antozonides," and tried in different ways to eliminate from them that part of their oxygen which I consider to be $\oplus$. Years ago I remarked, in accordance with an observation made by M. Houzeau, that the oxygen disengaged from the peroxide of barium by means of the monohydrate of SO^3, exhibits an ozone-like smell, and the power of turning my ozone test-paper blue. Not having at that time a notion of two opposite active conditions of oxygen, I was inclined to ascribe these properties to the presence of minute quantities of ozone in the said gas; but on examining it more closely, I found it to be neutral oxygen mixed up with a very small portion of antozone, or $\oplus$. A most important and distinctive property of antozone is the readiness with which it unites with water to form peroxide of hydrogen, whilst ozone, like neutral oxygen, is entirely incapable of doing so. Hence it comes to pass that the oxygen disengaged from $BaO+\oplus$, under certain precautions, becomes inodorous on being shaken with water, and that this fluid contains HO^2. The simple cause of the minute quantities of $\ominus$ obtained from $BaO+\oplus$, is the heat disengaged during the action of SO^3 upon the peroxide, by which most of the $\oplus$ eliminated is transformed into O.

Now, what do you say to the extraordinary fact, that the antipode of ozone has these many thousand years been ready formed and incarcerated, only waiting for somebody to recognize and let it loose out of its prison? A dark blue species of fluor-spar has for years been known by the German mineralogists, being distinguished by its property of producing a peculiarly disagreeable smell on being triturated. Many conjectures were made as to the chemical nature of the odorous matter emitted from the spar: chlorine, iodine, and even ozone were spoken of, but it turned out to be a different thing.

M. Schafhaeutl of Munich sent me, a month ago, some hundred grammes of the said fluor-spar (occurring within the veins of a granitic rock at Wulsendorf, a Bavarian village near Amberg), asking me to try my luck in ascertaining the nature of the smelling matter; and I think I have fully succeeded in making out what it is. Surprising as it may sound to you, and unique as the fact certainly is, that matter happens to be nothing but my insulated antozone. "But how do you prove that?" you will ask me. In the first place, it smells exactly like $\ominus$ disengaged from BaO^2. "But smells are fallacious tests." They are. You shall have another proof that will irresistibly carry conviction with it: on triturating the fluor-spar with water, peroxide of hydrogen is formed, not in homœopathic, but very perceptible quantities. When I first found out this extraordinary fact (I think it was on the 17th of November last), I could not help laughing aloud, though I happened to be quite alone in my laboratory. I laughed because I strongly suspected my foe to be hidden in the spar, and I broke his mask under water with the view of catching him by that fluid. Indeed, it was to me as if I had caught a very cunning fox, long sought after, in a trap put up for him. To show you that in saying this I have neither been joking nor dreaming, I shall send you, as soon as I can, a sample of the wonderful spar, with which you may easily satisfy your curiosity and convince yourself of the correctness of my statements. I must not omit to tell you that, according to some previous experiments of mine, the fluor-spar of Wulsendorf contains $\frac{1}{5000}$th part of antozone—a quantity, as you see, not at all homœopathic. How that subtle matter got into the spar I cannot tell.

XV. *Note on the Remarks of* Mr. Jerrard.

By James Cockle, *Esq.*

[Concluded from vol. xix. p. 332.]

THE sequel to Mr. Jerrard's important 'Essay' may remove the new doubts and difficulties which arise upon his 'Remarks,' and which, numbered in conformity with his paragraphs, are here briefly noticed. Mr. Jerrard has to meet the additional objections that—

1, 2, 4. There is no error in Mr. Harley's processes. The error of inferring the sextic to be an Abelian I have already traced to its source (vol. xix. p. 197 *et seq.*).

3. If the functions ${}_1\Xi$, ${}_2\Xi$, ${}_3\Xi$, and ${}_4\Xi$ are not extraneous, let two quintic surds be expelled, as reducible, from them and from ${}_0\Xi$. Then, according to Abel's theorem, all the roots of (ab) are roots of (ac). Denote (ac) and (ab) by

$$\xi=0, \qquad \xi'=0$$

respectively. Then

$$\xi=\xi'(\Xi_\zeta-\mathrm{r}\{\mathrm{P}_{f(\underset{\cdot\cdot}{\beta\epsilon})}\}).$$

But what is the form of ζ? Certainly not f, for the root Ξ_f is common to (ab) and (ac). Unless Mr. Jerrard shows that ζ cannot be replaced by $f(\underset{\cdot\cdot}{\beta\epsilon})$, his conclusion is vitiated by an error in comparing (ab) and (ac).

5. The complex process need not supersede the simple method of substitutions when we are dealing with symmetric functions of Θ' and Θ''. Such functions are Mr. Jerrard's Ξ, or

$$\Theta_1^5+\Theta_2^5+\Theta_3^5+\Theta_4^5,$$

and my θ, or

$$5^4\Theta_1\,\Theta_2\,\Theta_3\,\Theta_4.$$

To take a simple example,

$$\Theta'_{n,f(\underset{\cdot\cdot}{\beta\epsilon})(\underset{\cdot\cdot}{\gamma\delta})}+\Theta''_{n,f(\underset{\cdot\cdot}{\beta\epsilon})(\underset{\cdot\cdot}{\gamma\delta})}=(\Theta''_{n,f}+\Theta'_{n,f})(\underset{\cdot\cdot}{\beta\epsilon})(\underset{\cdot\cdot}{\gamma\delta}).$$

6. It can scarcely be said that Mr. Jerrard's theorem is ignored by Mr. Harley. That theorem is inapplicable to the processes dealt with in Mr. Harley's paper, into which no symbol corresponding to P enters.

7. Actual calculation shows that Ξ and θ are similar functions (*fonctions semblables*). And if in my function γ we substitute Ξ for θ, the result is an equation which, combined with (ac), shows that, if the latter be an Abelian, the root of a general quintic can be expressed without quintic surds.

8. If, in expanding a function of two independent quantities, we do not obtain an expression symmetric with respect to those quantities, and such as to admit of an interchange between them, I cannot consider the expansion as algebraically (rigorously) true.

In generalizing the theory of transcendental roots which I have sketched, and partially developed, in these pages, we may start from

$$x^n-nx+(n-1)a=0.$$

We next deduce an n-ic in y, in which

$$y=(1-a^{n-1})\frac{dx}{da};$$

and, hence, we pass to an n-ic in x', in which

$$x'=\mathrm{P}+\mathrm{Q}y+\mathrm{R}y^2+\ ..\ +\mathrm{T}y^m,$$

where m is not greater than $n-1$, and P, Q, R, . . T are so determined that the n-ic in x' is of the form

$$x'^n-nx'+(n-1)a'=0.$$

From the equations in x and x' we obtain

$$x=\phi a, \qquad x'=\phi a', \qquad x''=\phi a'', \ldots\ x^{(r)}=\phi a^{(r)},$$

whence the form of ϕ is to be sought.

Mr. Jerrard's trinomial transformation indicates that this process is applicable (*primâ facie*) to equations as high as the fifth degree inclusive. Beyond that degree a trinomial cannot be a general equation. But possibly the properties of trinomial equations, or of equations involving only one parameter, may enable us to extend the process further by obtaining the n-ic in x'.

4 Pump Court, Temple, London,
January 12, 1861.

XVI. *A Theory of Magnetic Force.*
By Professor Challis, *F.R.S.*

[Continued from p. 73.]

In art. 6 of the foregoing part of the Theory of Magnetic Force, certain laws of the motion of an elastic fluid, essential both to that theory and to the previous theories of electric and galvanic forces, were enunciated without being supported by mathematical evidence. Before proceeding further with the explanation of magnetic phenomena, I propose to adduce the mathematical investigation of those laws.

15. It was stated that the resultant of the composition of steady motions is steady motion. The proof of this theorem is as follows:—For cases of steady motion, the general hydrodynamical equation which expresses constancy of mass, viz.

$$\frac{d\rho}{dt}+\frac{d\,.\,\rho u}{dx}+\frac{d\,.\,\rho v}{dy}+\frac{d\,.\,\rho w}{dz}=0,$$

becomes, to the *second* order of approximation,

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0,$$

because for that kind of motion $\frac{d\rho}{dt}=0$, and $\frac{d\rho}{\rho dx}$, $\frac{d\rho}{\rho dy}$, $\frac{d\rho}{\rho dz}$ are each of the order of the *square* of the velocity, as is shown by the general differential equations involving the pressure, no extraneous force being supposed to act. Now if u_1, v_1, w_1; u_2, v_2, w_2, &c. be velocities due to different disturbances acting separately, and each set of values satisfy separately the second of the above equations, it is evident that if $u=u_1+u_2+$ &c., $v=v_1+v_2+$ &c., $w=w_1+w_2+$ &c., u, v, w will satisfy the same equation. From this reasoning it follows that when several causes of steady motion coexist, the velocity they produce conjointly at any point, is the resultant of the velocities which they would produce at the same point by acting separately. Hence since the component motions, being steady, are constant in magnitude and direction at each point, the resultant motions will be constant in magnitude and direction at each point; that is, the whole motion will be steady.

16. Hence in the general hydrodynamical equation applicable strictly to steady motion, when no extraneous force acts, viz.

$$a^2 \text{ Nap. log } \rho = \text{C} - \frac{\text{V}^2}{2},$$

we may assume, since terms involving the square of the velocity have been taken into account, that V is the resultant of several velocities, v', v'', &c., given in magnitude and direction. Take, for instance, the two velocities v' and v'', and suppose their directions to make an angle α with each other. Then

$$\text{V}^2 = v'^2 + v''^2 + 2v'v'' \cos \alpha.$$

From this equation it is seen that if the value of C be given, the velocity is greatest and the density and pressure least, where $\alpha=0$, that is, where the two streams coincide in direction; and that the velocity is least, and the density and pressure greatest, where $\alpha=\pi$ and the two streams are in opposite directions.

In general the constant C will be different for different lines of motion. But in the cases of convergent streams flowing from an unlimited distance, and divergent streams flowing to an unlimited distance, at remote points $\rho=1$ where $\text{V}=0$, and consequently $\text{C}=0$ for each of the lines of motion.

17. The hydrodynamical theorems above demonstrated are those which have been used in accounting for electric attractions and repulsions, the mutual action between two electrodes, that between an electrode and a magnet, and the mutual attractions and repulsions of magnets. It is to be remarked that in these explanations the moving forces of the æther acting on the ultimate atoms of bodies have been supposed to be due to variations of its density, producing statically differences of pressure at different points of each atom. But if motion resulted solely from variation of the density of the æther from point to point

of space, there would seem to be no reason why a magnet should not move various substances as an electrified body does. In the one case, as in the other, variation of pressure is produced, according to the theory, by ætherial streams. What, then, is the reason that an electrified body acts indifferently on many kinds of substances, whilst a magnet acts in a special manner on certain substances, which from this peculiarity have been named magnetic?

18. In answering this question, it is first to be considered that, according to the hydrodynamical theory of electricity, an electrified body has had its *superficial* atoms forcibly disturbed, and that this state of disturbance gives rise to *vibrations* of the æther, the dynamic effect of which on a neighbouring body is to put its superficial atoms also into a state of disturbance, and thus to *induce* electricity. It was explained in the theory referred to, that this state of induced electricity coexists with a gradation of internal density, which generates the secondary currents to which electric attractions and repulsions are attributable. But in a magnet there is nothing corresponding to that superficial disturbance, the internal gradation of density being an independent property of the body. Hence there are no *vibrations* to disturb the superficial atoms of adjacent bodies, the magnet acting dynamically wholly by *currents*, and consequently, as is known by experience, it is incapable of inducing electricity. At the same time, between a magnet and a body partially or inductively electrified, there must be interference of ætherial currents, and consequently mutual action, as is found experimentally to be the case.

Still it may be argued that magnetic streams, varying from point to point of space as to density and velocity, must act dynamically on the atoms of bodies of all kinds, independently of their electric or magnetic states. That such action does really take place to a sensible amount under the influence of *powerful* magnets, will be made apparent by the following considerations applied to the class of facts discussed experimentally by Faraday in the Twentieth and Twenty-first Series of his 'Researches in Electricity' (Phil. Trans. part 1. 1846).

19. It has been already stated that, according to hydrodynamics, a single spherical atom, subject to the action of a *uniform* steady current, suffers no change thereby of a condition either of rest or of uniform motion. But this is no longer true if, the stream being steady, the lines of motion are not parallel to each other, because in that case there will be inequality of the pressures on opposite hemispherical surfaces of the atom. Also when such a stream enters into any substance, the partial convergences or divergences of its course, which must result from its encountering an aggregation of atoms, may materially influence its

dynamic action on any given atom. We have not the data for inquiring what that action may be in any particular substance, because for that purpose we require to know its composition and the magnitudes and arrangement of its atoms. I shall therefore assume, as a result of the experiments above referred to, that divergent and convergent magnetic streams act dynamically in different manners and in different degrees on a great variety of substances which they permeate.

20. If the substance be *iron,* the moving force of the magnetic streams on its individual atoms, apart from any action resulting from interference with secondary streams, tends along the lines of motion *towards* the poles of the magnet. The following remarkable experiment by Faraday (2368) establishes this law. Cylindrical glass tubes containing ferruginous solutions of different degrees of strength were suspended with the axes vertical in a ferruginous solution of given strength between the poles of the magnet. If the solution in the tube was *stronger* than that of the surrounding fluid, the tube was *attracted* towards either pole; and if *weaker,* it was *repelled. Both* these effects may be explained on the assumption that the magnet *attracts* the solutions in proportion to the quantity of iron they contain. The tube was drawn towards the poles in the first case, because the contained fluid being more ferruginous, was more attracted than the surrounding fluid. In the other case, the hydrostatic pressure of the surrounding fluid, due to the assumed magnetic attraction towards the poles, is greater on the side of the tube nearest either pole, than on the opposite side, and this difference of pressure, not being wholly counteracted by the attraction on the contained weaker solution, causes a repulsion of the tube. The cases are exactly analogous to those of a body specifically heavier than water sinking in it, and a body specifically lighter rising in it, by the action of gravity. Now it is true that the magnetic forces here assumed have the same *directions* as those which the variation of the pressure of the æther would of itself produce. But if the action were either wholly or chiefly due to this cause, no reason appears why the observed motions should depend, not upon the difference of the specific gravities of the fluids within and without the tube, but upon one being more or less *ferruginous* than the other. The experiment, therefore, allows us to infer theoretically, that ætherial streams, flowing through *iron,* and affected by the number, size, and state of aggregation of its atoms, exert on each atom an accelerative force, which is in the direction of the current where it is convergent, and in the direction contrary to that of the current where it is divergent. The atoms being by hypothesis spherical, it may be inferred from

hydrodynamical considerations, that converging and diverging streams impress upon them the same moving forces, if the velocities be the same, and the degree of convergency be equal to that of divergency; but the forces will be in opposite directions with respect to the course of the streams. Hence, under those circumstances, if one magnetic pole be attractive, the other will be attractive also, and in the same degree. This result experience confirms.

21. If a bar of *bismuth* be suspended either horizontally or vertically between the poles of a magnet, it is found to be impelled in the directions of the lines of motion *from* the poles. The action is therefore opposed to that which results from variation of density of the æther. The same effect took place when the bismuth was broken into very small pieces. Also it appeared that two pieces of bismuth subject to the action of magnetic streams, exerted no influence on each other; that is, no secondary streams were generated. From these facts it must be concluded theoretically, that convergent and divergent magnetic streams, when they enter bismuth and substances of the same class, and are modified by the number, size, and arrangement of the constituent atoms, impress on the individual atoms accelerative forces, which impel them along the lines of motion from the magnetic poles. For the same reason as that above given in the case of iron, the action is alike from both poles, and in the same degree, although the streams diverge from one and converge towards the other.

22. It is found by experiment that *air* and *gases* are not sensibly acted upon by magnetic streams. This fact may be explained by the theory on the supposition that such substances, on account of their small density, do not perceptibly, by the state of aggregation of their atoms, modify the ætherial streams, which consequently act on each atom as if it were alone. Now in that case a divergent stream *impels* the atom in the direction of the course, and a convergent stream *draws* it in the direction contrary to the course, and it is possible that these actions may be just counteracted by the opposite effects of the variation of density of the æther.

23. Since the magnetic streams attract one class of substances (the magnetic) towards the poles, and repel another class (the diamagnetic) from the poles, we might expect to meet with other substances which are neither attracted nor repelled, or are only acted upon very feebly. This appears to be the case with *copper*. The peculiar phenomena (Faraday, 2309, &c.) which this metal exhibits when placed between the poles of a magnet, seem to admit of explanation on the principles of this theory, by supposing its atoms to be easily moveable from their normal

positions,—a property to which it may perhaps owe the facility with which it conducts galvanic currents. Iron and bismuth, which are not good conductors of galvanism, present very different phenomena, in consequence, probably, of a great degree of fixity of their atoms, by reason of which, and of atomic arrangement and constitution, the magnetic streams permeating them may be so modified as to become respectively attractive and repulsive. A bar of copper, when first acted upon by the magnetic streams, begins to move as if it were a magnetic body, this motion being probably due only to the variation of pressure from point to point of the streams. But quickly its atoms, on account of their mobility, will be moved by that pressure into new positions, which may be such as to render it for a brief interval diamagnetic, so that the incipient motion is stopped, the atoms finally settling into positions for which the bar is neither magnetic nor diamagnetic. These are positions of constraint, in which they are held by the dynamic action of the pressure of the magnetic streams. When the streams are interrupted, the atoms, by the proper molecular action of the copper, return to their normal positions; and in the mean time the reaction of the æther on the bar produces the observed *revulsion.* The magnetic streams continuing in action, if an impulse be given to the bar tending to move it into a position of *greater* magnetic action, for the same reason as above an increment of diamagnetic force is generated, and a momentary *excess* of diamagnetic action, arising from the atoms being carried by their momentum beyond the positions in which they finally settle, stops it; and if it be moved into a position of *less* magnetic action, a momentary *defect* of diamagnetic action, also due to the momentum of the atoms in taking up new positions, equally stops it. The sluggish motion of the bar may be thus accounted for. The movement which occurs when the streams commence, would not take place if the bar were placed in the axial or equatorial direction, because the initial magnetic action would be symmetrical with respect to each of these directions. If the copper were of the form of a globe, with its centre on the axial direction, the sluggishness would still exist, because any movement of it about a vertical axis would bring each point into a position of either greater or less magnetic action.

24. Since it appears from the foregoing discussion that, independently of the dynamic effect of magnetic streams resulting from gradations of the density of the æther, they exert, when they enter a magnetic body, a particular moving force due to their divergence or convergence as affected by the atomic constitution of the body, this force ought to be taken into account in the theory of the magnet (art. 4), and in the explanation

of the mutual action between two magnets (art. 7). As the two kinds of force act along the same lines in the same directions, the consideration of this second force will not alter the conclusion arrived at in art. 4, as, in fact, is stated at the conclusion of that article. With respect to the manner in which the mutual action of two magnets is affected by the motion of the ætherial streams within them, it is to be remarked that divergence and convergence are increased by the opposition of two streams, and diminished by their confluence; and that the particular force under consideration is always *towards* positions from which the streams diverge, or towards which they converge. Hence this additional force, being some function of the degree of convergency or divergency, produces effects exactly like those of the pressures considered in art. 7, and the conclusion there drawn remains the same when both kinds of force are taken into account.

25. The following theory of *magnetic induction* rests on the foregoing views. It is found that when a bar of soft iron is placed in the plane of the magnetic meridian, it is magnetized inductively, the magnetism disappearing when its axis is placed perpendicular to that plane. The induced magnetism is greatest when the axis coincides with the direction of the dipping-needle; and the magnetism of its north end is the same as that of the north end of the needle. These facts admit of being explained on the supposition that the primary terrestrial stream in its passage through the bar exerts a force on the atoms in the direction from south to north, so as to produce a gradation of density of the bar towards the north end. This force may originate in the circumstance that when the stream enters the bar its velocity is increased by the contraction of the channel by the atoms, and, the density and pressure being consequently diminished, the surrounding fluid is drawn towards the axis of the stream. Thus there will be lines of motion without and within the bar converging northward, and consequently, from what is argued in art. 20, a force will be generated proper for producing the increment of density towards the north end. The same effect would be produced even if the streams were *parallel*, because as *divergent* streams flowing through iron, draw the atoms towards the quarter from which they flow, *à fortiori*, parallel streams would do the same. Thus the bar is converted into a magnet. But it is to be observed that the atoms retain their positions in consequence of the counteraction of this disturbing force by the *molecular* repulsion of the iron due to the variation of its density. Hence when the bar, by being put into a position transverse to the magnetic meridian, ceases to be under the influence of the terrestrial current, the atoms return to their normal positions, and the bar ceases to be a magnet. It is clear that this

induction of magnetism will be greater, the more directly the primary stream flows through the bar, as is known to be the case by experiment.

26. When a *coil* of wire in the form of a *helix* surrounds a cylinder of soft iron, and a galvanic current is sent through the wire, magnetism is found to be induced in the iron. This fact is explained by the theory as follows. The turns of the helix being supposed to be very little apart, each turn will be nearly in a plane perpendicular to the axis of the cylinder. Hence the circular motions, which, according to the theory of galvanism, take place about the wire, will be very nearly in planes passing through that axis. Suppose the course of the current to be from the end B towards the end A of the cylinder, and the turns of the helix to proceed from the left hand, over the cylinder, to the right hand of a person looking from B towards A. Then, since the circular motion about any portion of an electrode in which the current is conceived to flow parallel to the earth's axis from south to north is always in the direction of the earth's rotation, it follows, in the case supposed, that if the circular motion within the cylinder be resolved into parts parallel and perpendicular to its axis, the former will all be in the direction from B towards A, and the latter will very nearly destroy each other. Thus the result will be a longitudinal stream from B towards A, the velocity of which, if the helix be pretty close to the cylinder, will be greatest along its axis. The explanation of the manner in which this stream induces magnetism in the iron core, is precisely the same as that applied in the preceding article to induction by terrestrial magnetism. The pole A of the magnetized cylinder corresponds to that pole of a magnetic needle which points northward. The contrary would plainly be the case if the helix, instead of being *dextrorsum,* as supposed above, had been *sinistrorsum.* All these results agree with well known experimental facts.

27. The theory I now proceed to give of *terrestrial magnetism* rests on no other hypotheses than those on which the general physical theory is based. It will be assumed that the earth and all the bodies of the solar system are composed of spherical inert atoms, of different magnitudes, and in different states of aggregation, and that the æther pervading their interiors is in the same state of density as in the external spaces. Any investigation of the motions impressed on the æther by the known motions of the bodies of the solar system, must, in order to be consistent with the whole preceding argument, set out from these hypotheses. The following considerations will, I think, show that they lead to conclusions which are in accordance with observed facts of terrestrial and cosmical magnetism.

We are to assume that the mass of the earth consists of discrete spherical atoms, and that the whole of its interior, excepting the space occupied by atoms, is filled with æther in the same state of density as in the external regions of space, with only such variations of density as are produced by its motion. The same assumption is to be extended to the sun, the moon, and all the planets. In the first place, let us endeavour to ascertain what kind of movement is impressed on the æther by the earth's *rotation*; and for the sake of distinctness, I shall, at first, suppose that the earth has no motion of translation, and that it is perfectly symmetrical with respect to its axis and its equatorial plane. Then as the terrestrial atoms will impress a part of their velocity on the æther, a rotatory motion of the latter will be produced, which by its centrifugal force will tend to draw the fluid from the earth's axis. This tendency cannot give rise to a stream from one pole to the other, because there is no reason why it should flow in one direction rather than the other. Neither can streams be generated setting from the equatorial parts towards the poles, because as the velocity will be greatest in and near the plane of the equator, the fluid would rather be urged *towards* this plane, and *circulate* by return currents along the axis towards the poles. The tendency of the centrifugal force to draw off the fluid from the axis of rotation, will be counteracted by accelerative forces due to increments of density of the æther which result from the resistance of its inertia to the centrifugal movement. These forces, acting always towards the axis, cause the circular motion immediately impressed by the atoms to extend into the æther beyond the earth's surface. But it is evident that so long as the terrestrial atoms have greater velocity than the æther contiguous to them, the centrifugal force is on the increase, and the permanent state is reached only when there is no motion of the atoms relative to the æther. The opposite forces then maintain a steady motion at all points, there being no reason why the motion should not be steady at one point rather than at another. Consequently in the earth's interior, and at the surface, and sensibly to considerable heights above, the æther, like the atmosphere, partakes of the earth's rotation. This gyratory motion must spread to remote distances, the velocity decreasing with the distance according to a law which eventually may be found to admit of mathematical investigation on hydrodynamical principles. As the motion of rotation must be combined with the circulating currents above mentioned, the total motion is *spiral*.

28. Now let the earth be supposed to have a motion of translation, either uniform or variable. Then on the principle that there can be neither gain nor loss of the momentum of the æther

due to the earth's rotation, it may be asserted that the gyratory motion is transferred without alteration through space with the earth. It may not be that the motion of a given particle of the fluid remains the same; but the motion is constantly the same in positions which in successive instants are the same relative to the earth's centre and equatorial plane. Coexistent with the gyrations of the æther, motions of translation must be impressed upon it, having constant relations to the earth's motion. With respect to that portion of æther which has sensibly the same rotation as the earth, the most probable supposition is that it has also very nearly the same motion of translation, and that consequently the relative motion of the earth's atoms and the æther in its interior is very small.

These conclusions apply to the other rotatory bodies of the solar system. All are consequently centres of gyratory motions, which, according to the reasoning in art. 15, may coexist in the cosmical spaces without interfering with each other.

29. But it is not true, as supposed, that the earth is symmetrical with respect to its axis and equatorial plane, the superficial distribution of land and water showing that this is not the case. On the supposition of symmetry, the centrifugal force of the earth's rotation would induce a variation of its internal density, proper, according to the theory, for generating secondary streams under the influence of a primary current. But as by reason of the symmetry equal and opposite effects would be produced on every diameter of the earth, no streams would become sensible. For the same reason no streams result from the increase of the earth's density towards the centre by the effect of gravity. In the actual case of deviation from symmetry, the motion of the æther cannot be wholly in symmetrical gyrations as above inferred, although it will be approximately such. There must be motion relative both to the equator and the axis, steady in its character, as depending on constant causes, and constituting a primary stream, which flowing through the earth, put into an unsymmetrical state of restraint by the centrifugal force, will give rise to secondary streams. The primary current may be of feeble intensity, and yet by originating accelerative forces which act through large spaces, may generate streams of great intensity. The earth's magnetic streams may be regarded as resulting from a combination of the primary and secondary streams. According to these views the deviations of the earth's form and matter from symmetry determine the directions of the magnetic streams, which appear from experiment to *enter* the earth on the *north* side of the magnetic equator, and to *issue* from it on the *south* side. *The earth is thus a vast magnet, the streams of which are of constant intensity, excepting so far as they may be disturbed by cosmical influences.*

The terrestrial streams probably *circulate*, those which issue nearly perpendicularly to the earth's surface in the antarctic regions, after rising to great heights; turning back in curved courses distant from the earth, so as to enter again nearly perpendicularly in the arctic regions. To this circulation must be added that spoken of in art. 27, which conspires with the antarctic streams and opposes the arctic, and thus probably accounts for the observed excess of antarctic magnetic intensity.

30. If the earth had been symmetrical with respect to its axis, but not with respect to its equator, the magnetic streams would also have been symmetrical with respect to the axis. But the actual irregular distribution of land and sea is opposed to this law, and it is found in fact that there is an approximation to *two* magnetic poles as well in the arctic as the antarctic regions. Also it is established by observation that these poles have not fixed positions on the earth's surface. Taking account of this circumstance, and of the character of the irregularities to which the theory ascribes the generation of the currents, we might infer that the magnetic poles would be unsymmetrically situated with respect to the earth's poles, that the streams would be disposed unsymmetrically about them, and that in their movement they would not retain the same relative positions. These inferences observation appears to countenance.

31. The *arches* and *streamers* of the *Aurora Borealis* and the *Aurora Australis* are portions of terrestrial magnetic streams made visible by particular disturbances. According to my view of the undulatory theory of light, I should have no difficulty in admitting that ætherial streams that are steady, and on that account emit no light, become luminous upon being disturbed.

32. We have now to consider the disturbances which the steady terrestrial currents may undergo from cosmical influences. And in the first place it may be stated, as a deduction from hydrodynamical principles, that the steady streams to which magnetic force has been attributed, and the vibrations which were assumed to exist in the theory of the force of gravity, *may coexist without mutual interference*, so that the two kinds of force act independently of each other.

33. The effect which any motion of translation which the earth's atoms may have relatively to the æther, may be presumed to be the same, in respect to generation of secondary currents, as that of a current within the earth at rest. In the investigation of such effect, the motion of translation of the solar system would have to be taken into account, together with the motion of the earth in its orbit. As observation has not detected a variation of terrestrial magnetism corresponding to the law of the variation of velocity which would result from the combina-

tion of these motions, it must be concluded, in conformity with what is said in art. 28, that there is no perceptible effect from motion of translation of the atoms relative to the æther.

34. The terrestrial streams will be disturbed by the streams due to the gyratory motion about the sun, and the direction and amount of the resultant, at a given place, of the combination of the solar and terrestrial streams, will vary with the time of day. Hence there will be a *diurnal variation* of magnetic intensity, as is also known from observation. The epoch of maximum intensity will depend in part on the position of the place relative to the magnetic poles.

35. The amount of the variation of intensity on a given day will depend on the sun's declination, and therefore will be subject to an *annual variation,* being in north latitudes greatest at the summer solstice, and least at the winter solstice. These results are confirmed by observation.

It may deserve to be considered whether the magnetic intensity is sensibly affected by currents which must be produced in the atmosphere by *solar heat,* setting in all directions *from* the parts of the atmosphere most heated by the sun. The effect of these currents would be subject to diurnal and annual variations, and if of considerable amount, would have an influence on the local hour of maximum intensity.

36. There will also be an *annual variation* of intensity independent of the sun's declination, being due solely to the change of the velocity of the solar gyrations, caused by change of the earth's distance from the sun. Such a variation has been detected; but its amount is very small.

37. Through the telescope the *sun* presents itself to our view as a globular body, surrounded by a thick envelope consisting of matter which accumulates and divides, floating and whirling most probably by the action of an atmosphere, and which is separated from the interior solid globe by an intervening space. We cannot be far wrong in calling such matter *cloud.* It is then reasonable to suppose that the observed changes are attributable to the action of galvanic or magnetic streams, generating and precipitating from time to time the cloudy matter. But *changes* can hardly be attributed to the proper magnetic streams of the sun, because these, as in the case of the earth, will most probably be of a constant character. The changes are rather due to *disturbances* of the solar streams by external influence, as by the gyratory streams of the *planets.* It is therefore quite in accordance with this theory to find, as has been done lately, that observations indicate periods of maxima and minima of *solar spots.* These periods will be determined by changes of the combined action of the gyratory motions about the planets, as

resulting from changes of their positions relative to the sun; and it is therefore not surprising to find that one of them (that of $11\frac{1}{9}$ years) differs little from the periodic time of Jupiter, the influence of this planet being likely to be predominant on account of his large size and rapid rotatory motion. The periodicity of the magnetic action of any planet must clearly depend in part on the position of the plane of its equator, the gyratory motion being at a maximum in this plane.

38. If the preceding account of the periodicity of the maxima of solar spots be true, it will be seen to be a necessary consequence that the planetary gyrations produce *disturbances of terrestrial magnetism*, having in the long run a regular and periodic character. The researches of General Sabine, applied to magnetic observations taken at various positions on the earth's surface, have in fact conducted him to the result that *magnetic storms* are periodic, and led him to assign to them a period of ten years. Considering that the same planetary magnetic streams must operate on the earth as on the sun, and that the sun's position is central with respect to the earth's orbit, it might have been anticipated, on theoretical grounds, that a like periodicity of effects would be detected, and that the periods in the two cases would not greatly differ. Possibly the period of the magnetic storms may eventually be found to be in some degree variable, and the mean period inferred from observations extended over a longer interval, to be somewhat different from ten years.

39. The theory, by giving to extraordinary magnetical disturbances a planetary origin, accounts for their occurring simultaneously at places on the earth's surface widely distant. But the *amounts* of disturbance at the same instant may differ greatly at different places, owing to difference of latitude, and difference of position relative to the magnetic poles. Considering, however, the changes of the configurations of the planets, the amounts of disturbance, regarded as functions of the time of day, may be expected in the long run to follow the same law at different places. This is found to be the case, although, as might have been anticipated, the local hours of the maximum values are different.

40. The fact of there being local hours of maximum disturbance, in the case of the planetary as in that of the solar disturbances, seems to be referable to the circumstance that the disturbances of the superior planets will be greatest when they are in opposition, or about midnight, they being then least distant from the earth, and the disturbances of the inferior planets will for the same reason be greatest when they are in inferior conjunction, or about mid-day. If this explanation be true, we should expect to find

a set of maxima prevailing in hours of the night, and another set in hours of the day, and that the directions of the deviations of the needle in the two cases would be opposite, the directions of the gyratory streams of the superior and inferior planets being opposite for their positions of maximum disturbance. These laws General Sabine has in fact detected by a discussion of numerous observations taken at Kew and Hobarton. (See 'Proceedings of the Royal Society,' vol. x. No. 41, p. 632.)

41. The facts relating to the movements of the earth's magnetic poles do not appear to be sufficiently well ascertained to allow of applying the theory to explain them. Conceive, for the sake of illustration, that there are two north magnetic poles at the same distance from the earth's pole, and 180° from each other, that the currents converging to them are of the same intensity, and that they have the same uniform movement from *east* to *west*. On these suppositions it is evident that when the poles are on the great circle passing through the pole of the earth and a given place, the Inclination is at a *maximum*, and the Declination is changing from *east* to *west*. When they have passed over 90° the Declination will have gone through a western maximum, and be on the point of changing from *west* to *east*, because the needle, being equally acted upon by the two poles in the new positions, must point exactly between them. At the same time the Inclination will be at a *minimum*. After the poles have advanced 90° further, the needle will have gone through a maximum eastern Declination, and returned to its original position, and the Inclination will again be at a maximum: and so on. These hypothetical circumstances are, perhaps, not so far different from the actual as to prevent our concluding from them that neither the direction nor the amount of the motion of the magnetic poles can be inferred with any certainty from mere observations of "points of convergence" of the horizontal needle—at any rate not with so much certainty as the direction of the movement may be inferred from the direction in which the Declination is changing when it is passing through zero at any place, as London or Paris, *if at the same time the Inclination is near its maximum at the same place.* Now early observations at Paris show that the Declination was zero there about the year 1663, and was changing from east to west, and observations both at Paris and London indicate a constant diminution of Inclination from that date, while, according to Hansteen's Isoclinal lines, the Inclination was *less* in 1600 than in 1700. It may therefore be inferred that a maximum of Inclination occurred between those two epochs, and consequently, from the above argument, that the motion of one of the north magnetic poles is from *east* to *west*. If this

law should eventually be found to be true of all the magnetic poles, the theory will account for it in this manner. The north magnetic streams may be supposed to descend into the earth from heights at which the gyratory motion of the æther is sensibly less than at the earth's surface, and the south magnetic poles to issue from the earth to heights at which the same circumstance prevails. The streams being supposed to *circulate* from south to north in courses distant from the earth's surface, that diminution of the velocity of gyration will cause them to lag behind the earth in rotatory motion, and thus to draw the magnetic poles *westward*.

I have now completed an outline of a general physical theory, of which the leading idea is that all quantitative physical laws may be mathematically deduced from a few fundamental facts, distinct conceptions of which may be formed from sensation and experience. The hypothetical facts on which the theory rests are, that all substances consist of minute spherical atoms, of different, but constant, magnitudes, and of the same intrinsic inertia, and that the dynamical relations and movements of different substances are determined by the motions and pressures of a uniform elastic medium pervading all space not occupied by atoms, and varying in pressure in proportion to variations of its density. I am well aware that many of the explanations I have given of physical phenomena on these hypotheses are expressed in general terms, and are too little supported by exact analytical or numerical calculation. Some of the explanations, requiring a knowledge of the interior constitution of bodies, could not be conducted in an exact manner in the present state of science. Still so comprehensive a theory, resting on so few hypotheses, could hardly fail of meeting with contradictions in the attempt to explain facts, unless the hypotheses were true. The number and the variety of the explanations of physical phenomena which have been drawn from them without the support of symbolical calculation, seem almost of themselves to justify the conclusion that the ultimate atoms of bodies are really such as they have been assumed to be, and that the physical forces are modes of action of a single elastic medium.

But no doubt the general theory and the explanations derived from it are not fully established till they have borne the test of numerical verification. And here I take occasion to add that every complete physical theory is necessarily *mathematical*. Observation and experiment are essential for furnishing facts both for the foundation and for the verification of a theory, and may also discover laws and relations of facts; but the theory is not complete till all facts not fundamental, and all laws, have been referred by mathematical calculation to the fewest possible fundamental facts. These principles have been admitted in physical astronomy, and belong equally to other departments of physical science. Hence if the hypothesis of the existence of the æther as the sole source of physical power be true, the mathematical investigation of the motions of an elastic fluid become essential; and the *new principles* of the application of partial differential equations to the determination of fluid motion, which I have proposed, will have the same relation to the future progress of theoretical physics, as the discovery by Newton of the principles of the application of differentials in dynamics had to the progress of physical astronomy. On account of the important applications those principles may eventually receive, I purpose, as soon as I shall be able, to bring them again under the notice of mathematicians.

Cambridge Observatory,
January 17, 1861.

XVII. *On an Alloy which may be used as a Standard of Electrical Resistance.* *By* A. Matthiessen, *Ph.D.*[*]

THE expression of electrical resistances in the absolute measure proposed by Weber[†] is, and probably will always remain, the best; but its determination requires so much apparatus and room, as well as so much skill in manipulation, that it is placed beyond the means of most experimenters. I have therefore deemed it worth while to test some alloys, to see whether I should not be able to find one—

I. Whose resistance will remain the same, whether it be made of absolutely pure or commercially pure metals; in other words, that such an alloy may be made by any chemist or assayer, and its conducting power will always be the same.

II. That its conducting power will not be altered by the process of annealing.

III. That its conducting power will not vary much with an increase or decrease of temperature.

IV. That the alloy will not alter by exposure to the atmosphere.

The great difficulty in obtaining absolutely pure metals, together with the fact that the smallest traces of impurity materially increase the electrical resistance of most metals, pre-

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[*] Communicated by the Author.

[†] Pogg. *Ann.* vol. lxxxii. p. 337.

clude the use of them as standards, and make it desirable that, instead of comparing, as heretofore, the conducting powers of metals, alloys, &c. with that of silver, copper, or, as has lately been proposed, with mercury*, they should in future be compared with an alloy having the properties mentioned above.

The alloy best adapted for a standard of resistance for galvanic currents, is that composed of

2 parts by weight of gold †,
1 „ „ „ silver;

for on looking at the curve which expresses the conducting powers of the gold-silver alloys ‡, we find part of it almost at a straight line; that is to say, to the alloy containing 50 volumes per cent. of gold (which is the middle point of the straight line) one or two per cent. more or less gold may be added without altering its conducting power to any great extent.

In order to test the first condition, I have had the alloys made in different parts of the world; and my best thanks are due to those gentlemen who kindly undertook the making and procuring of them. The following order was given:—

Take 6 *grammes proof, or purest gold, and* 3 *grammes proof, or purest silver; fuse and cast three times* §, *and then draw into wire of about* 0·5 *millim. diameter. The wire to be hard-drawn.*

The alloys experimented with were:—

No. 1. Made of pure gold and silver by Mr. R. Smith, in Prof. Percy's laboratory. Drawn by Messrs. Watts and Son, of Kirby Street. These wires were annealed in a red-hot crucible.

No. 2. Made and drawn in Brussels. Procured for me by my friend Mr. G. C. Foster, through the kindness of Prof. Stas. Annealed on wire-gauze over a four Bunsen burner.

No. 3. Made and drawn in New York. Procured through my friend Mr. C. M. Warren. Annealed in the same way as No. 2.

No. 4. Made and drawn in Paris. Procured through Mr. S. Reuter. This wire came to hand already annealed; therefore no determinations of the hard-drawn wire could be made.

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* Siemens, Phil. Mag. (Jan. 1861); Schröder von der Kölk, Pogg. *Ann.* vol. cx. p. 452. It should be borne in mind that the use of this metal as a standard is open to the following grave objection: viz. that the copper wires or plates dipping in the mercury will after a time make it impure; and *as traces of foreign metals* (0·1 *or* 0·2 *per cent.*) *cause a decrement in the conducting power of pure mercury* (*not as stated by Siemens, an increment*), it would become necessary often to change it, thereby requiring a large supply of chemically pure metal.

† Corresponding nearly to equal volumes of gold and silver.

‡ Phil. Trans. 1860.

§ It would have been better to have added, *fuse with a little borax and saltpetre.*

No. 5. Made and drawn in Frankfort. Procured through my friend Dr. Dupré. These wires were annealed on a wire-gauze over a Bunsen's glass-blower's lamp.

No. 6. Made of proof-gold and silver, by Messrs. Johnson and Matthey, of Hatton Garden, and drawn by them. These wires were annealed in the same manner as No. 2.

No. 7. Made and drawn by myself, of proof-gold and silver, lent me by Professor Hofmann. Annealed in the same manner as No. 2.

No. 8. Made and drawn by myself, of pure gold and silver. Annealed as No. 2 was.

In Table I. are given the values found for conducting power of the above alloys. They have been compared with a hard-drawn silver wire, whose conducting power has been taken = 100 at 0° C.

Table I.

No. of alloy.					Mean reduced to 0° C.	
	Hard-drawn.	Temp.	Annealed.	Temp.	Hard-drawn.	Annealed.
No. 1.						
1 wire	14·99	14°·5 C.	15·02	15°·0 C.		
2 wire	14·87	15·0	14·92	15·2	15·08	15·13
Mean	14·93	14·8	14·97	15·1		
No. 2.						
1 wire	14·98	9·8	15·02	10·0		
2 wire	14·95	9·6	15·04	10·4	15·06	15·14
Mean	14·96	9·7	15·03	10·2		
No. 3.						
1 wire	14·60	16·8	14·66	17·0		
2 wire	14·75	16·2	14·82	17·4	14·85	14·92
Mean	14·68	16·5	14·74	17·2		
No. 4.						
1 wire	......	...	14·94	6·0		
2 wire	......	...	15·02	9·8	......	15·06
Mean	......	...	14·98	7·9		
No. 5.						
1 wire	15·02	8·5	15·06	9·0		
2 wire	14·99	9·2	15·03	9·8	15·09	15·14
Mean	15·00	8·8	15·04	9·4		
No. 6.						
1 wire	14·87	14·8	14·82	16·0		
2 wire	14·79	17·2	14·86	15·0	14·99	15·00
Mean	14·83	16·0	14·84	15·5		
No. 7.						
1 wire	14·94	14·0	14·99	16·2		
2 wire	14·91	16·2	14·97	17·2	15·07	15·16
Mean	14·92	15·1	14·98	16·7		
No. 8.						
1 wire	14·86	18·4	14·92	18·2		
2 wire	14·88	16·6	14·90	17·0	15·05	15·10
Mean.........	14·87	17·5	14·91	17·6		
				Mean.........	15·03=100	15·08=100·3

Calling the mean of the values at 0° of the hard-drawn wires 100, we find that, as Table II. shows, the differences between the conducting powers of the alloys are very small, in fact, that the greatest is only 1·6 per cent.

Table II.

No. of alloy.	Hard-drawn.	Difference.	Annealed.	Difference.
1.	100·3	+ 0·3	100·6	+ 0·3
2.	100·2	+ 0·2	100·7	+ 0·3
3.	98·8	− 1·2	99·2	− 1·1
4.	......	...	100·2	− 0·1
5.	100·4	+ 0·4	100·7	+ 0·4
6.	99·7	− 0·3	99·8	− 0·8
7.	100·3	+ 0·3	100·8	+ 0·5
8.	100·1	+ 0·1	100·4	+ 0·1

The great concordance in the above results is partially due to the wire drawing so very well. It draws as well as, if not better than, any wire I have as yet tried.

For the sake of comparison, I will give in Table III. the values found by different experimenters for the metals with which the others are usually compared.

Table III.

	Becquerel *.		Lenz†.	Siemens‡.		Matthiessen.	
	Hard-drawn.	Annealed.		Hard-drawn.	Annealed.	Hard-drawn.	Annealed.
Silver	100·0	107·0	100·0	100·0	108·8	100·0	110·0
Copper	95·3	97·8	73·3	89·7	95·2	99·5	102·0
Gold	68·9	70·0	58·5	......	......	78·0	80·0
Mercury	1·86		3·42 at 18°·7	1·72		1·63 at 22°·8	

When no temperature is given, the observations have been made at 0° C.

Part of these differences may be due to the silver, which in some cases may not have been chemically pure; but if any of the others be taken as unit, we do not arrive at any better result.

Table IV. shows the differences between annealed and hard-drawn wires,—the annealed being the better conductor, according to—

Table IV.

	Becquerel.	Siemens.	Matthiessen, Holzmann §.
Silver	7·0 per cent.	8·8 per cent.	10·0 per cent.
Copper	2·6 per cent.	6·1 per cent.	2·5 per cent.
Gold	1·6 per cent.	....	2·6 per cent.

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* *Ann. de Chim. et de Phys.* vol. xvii. (1846), p. 242.

† Pogg. *Ann.* vol. xxxiv. p. 418, and vol. xlv. 105.

‡ *Loc. cit.*

§ Phil. Trans. 1860

The gold-silver alloy, however, only varies 0·3 per cent.,—another reason why the alloy may be drawn by anybody, and still have the same conducting power.

The following experiments show the effect of an increase of temperature on the conducting power of this alloy. The details of the experiments, together with the apparatus employed, will be published shortly in a paper by Dr. von Bose and myself, "On the Conducting Power of the Metalloids, Metals, and Alloys at different temperatures." Our results at present do not agree with those of Arndtsen and Siemens, who state that the resistances of most metals vary in direct ratio with the increase of temperature, but with those of Lenz, who calculates the conducting powers for different temperatures by the formula $\lambda = x + y\,t + z\,t^2$, where x is the conducting power at 0° C., y and z constants. I may also mention that the metalloids conduct better on being heated, being the reverse of that which the metals do.

An annealed wire of No. 1 alloy was heated in a glass tube placed in a water-bath (*a*), and afterwards heated in an oil-bath (*b*). Its conducting power was determined at different temperatures; and the values found are given in Table V. The values found for an annealed wire of No. 5 alloy (*c*), heated in an oil-bath, are also added.

Table V.

(*a.*)	(*b.*)	(*c.*)
T. Conducting power.	T. Conducting power.	T. Conducting power.
0·4=15·053	9·1=14·954	6·1=15·088
19·5=14·854	30·9=14·728	33·3=14·798
41·3=14·626	53·5=14·501	51·6=14·607
60·6=14·431	71·4=14·325	73·3=14·391
82·7=14·219	95·1=14·094	96·7=14·162
100·0=14·052	69·4=14·342	73·3=14·391
79·3=14·251	47·9=14·550	51·4=14·609
59·1=14·453	30·8=14·730	31·7=14·811
39·3=14·647	10·5=14·939	10·3=15·039
17·9=14·865		
0·7=15·049		

The diameter of (*a*) was 0·539 millim. and its length 683 millims., and that of (*c*) 0·915 millim. and its length 987 millims.

Taking the mean of the two temperatures and conducting powers, and calculating (by the method of least squares) the probable values for x, y, z for the formula $\lambda = x + yt + zt^2$, where $\lambda =$ conducting power x at t degrees, x conducting power at 0° C., y and z constants, we find,

for
$$(a)\ \lambda=15{\cdot}059-0{\cdot}01077\,t+0{\cdot}00000722\,t^2,$$
$$(b)\ \lambda=15{\cdot}052-0{\cdot}01074\,t+0{\cdot}00000714\,t^2.$$
$$(c)\ \lambda=15{\cdot}152-0{\cdot}01098\,t+0{\cdot}00000774\,t^2.$$

Table VI. gives the mean of the observed conducting powers, those calculated from the above formulæ, and their differences.

Table VI.

(*a.*)

T.	Observed conducting power.	Calculated conducting power.	Diff.
0·6	=15·051	15·053	−0·002
18·7	=14·860	14·860	0·000
40·3	=14·637	14·638	−0·001
59·8	=14·442	14·441	+0·001
81·0	=14·235	14·234	+0·001
100·0	=14·052	14·054	−0·002

(*b.*)

T.	Observed conducting power.	Calculated conducting power.	Diff.
9·8	=14·946	14·947	−0·001
30·8	=14·729	14·728	+0·001
50·7	=14·526	14·526	0·000
70·4	=14·333	14·331	+0·002
95·1	=14·094	14·096	−0·002

(*c.*)

T.	Observed conducting power.	Calculated conducting power.	Diff.
8·2	=15·063	15·063	0·000
32·5	=14·800	14·803	−0·003
51·5	=14·608	14·608	0·000
73·3	=14·391	14·389	+0·002
96·7	=14·162	14·162	0·000

If we now take the conducting power at 0° = 100·3, being the mean of the observed conducting powers (see Table II.), we find

for
$$(a)\ \lambda=100{\cdot}3-0{\cdot}07216\,t+0{\cdot}0000484\,t^2,$$
$$(b)\ \lambda=100{\cdot}3-0{\cdot}07196\,t+0{\cdot}0000478\,t^2,$$
$$(c)\ \lambda=100{\cdot}3-0{\cdot}07247\,t+0{\cdot}0000511\,t^2;$$

and taking the mean of the mean of *a* and *b* (this being the same wire) and *c*, we find the conducting power of the annealed wire at different temperatures

$$=100{\cdot}3-0{\cdot}07226\,t+0{\cdot}0000496\,t^2.$$

The next step was to determine the conducting powers of hard-drawn wires for different temperatures: but here we had to contend with great difficulties; for when a hard-drawn wire is heated to 100°, a different conducting power is generally found on cooling; and to obtain concordant results, it is necessary to heat the wire several times; but when once obtained, the values found will remain the same, no matter how often the wire may be heated, showing that the apparatus and method are not faulty, whether by letting the wire remain for a length of time it will gradually assume its original conducting power or not is a question now under consideration. With annealed wires this is also the case, but in a much less degree. All the above wires, as well as the following, were heated several times before concordant results were obtained. I cannot at present state the cause of this behaviour; but experiments are now being made on the subject by Dr. von Bose and myself.

Hard-drawn wires of No. 5 (*a*) and No. 3 (*b*) alloys were heated, (*a*) in a glass tube in a water-bath, (*b*) in an oil-bath. The values found are given in Table VII.

Table VII.

(*a*.)		(*b*.)	
T.	Conducting power.	T.	Conducting power.
0·0	=15·075	9·3	=14·870
20·9	=14·858	35·6	=14·608
46·5	=14·605	50·8	=14·460
76·4	=14·318	67·6	=14·300
100·0	=14·094	98·1	=14·011
71·6	=14·370	70·4	=14·278
51·4	=14·566	54·4	=14·426
23·8	=14·838	35·2	=14·613
0·0	=15·075	14·1	=14·824

The diameter of (*a*) was 0·616 millim., and its length 876 millims., and that of (*b*) 0·551 millim., and its length 348 millims. Taking, as before, the mean of the two temperatures and conducting powers, and calculating the values of x, y, z from them, we find

for (*a*) $\lambda=15{\cdot}074-0{\cdot}01012\,t+0{\cdot}00000329\,t^2$,
(*b*) $\lambda=14{\cdot}964-0{\cdot}01011\,t+0{\cdot}00000410\,t^2$.

Table VIII. gives the mean of the observed conducting powers, and those calculated from the above formulæ, with their differences.

Table VIII.

(*a*.)				(*b*.)			
T.	Observed conducting power.	Calculated conducting power.	Diff.	T.	Observed conducting power.	Calculated conducting power.	Diff.
0·0	=15·075	15·074	+0·001	11·7	=14·847	14·847	0·000
22·3	=14·848	14·850	−0·002	35·4	=14·610	14·611	−0·001
49·0	=14·586	14·586	0·000	52·6	=14·443	14·443	0·000
74·0	=14·344	14·343	+0·001	69·0	=14·287	14·286	+0·001
100·0	=14·094	14·095	−0·001	98·1	=14·011	14·011	0·000

Taking the conducting power at 0°=100, we have

$$\begin{aligned} \text{for } (a),\ \lambda &= 100-0{\cdot}06714\,t + 0{\cdot}0000218\,t^2, \\ (b),\ \lambda &= 100-0{\cdot}06753\,t + 0{\cdot}0000274\,t^2, \\ \text{mean}\ \ \lambda &= 100-0{\cdot}06734\,t + 0{\cdot}0000246\,t^2, \end{aligned}$$

which shows there is a difference in the conducting powers at different temperatures between annealed and hard-drawn wires of this alloy.

A similar difference we have already found between hard-drawn and annealed silver wires.

Although the values found for x and y do not agree very well with each other, yet for all purposes it will not make much difference which is used, as they will lead to the same results; thus, if we calculate the conducting power for the highest ordinary summer temperature (in a room), say 30° C, we find it in the one case to be

98·005, and in the other 97·999.

In Table IX., I have given the differences in the conducting powers of some metals between 0° and 100°, taking the conducting power at 0°=100.

Table IX.

Silver........................	28·5 per cent. (annealed).
Copper	29·0 per cent. (annealed).
Gold	28·0 per cent. (annealed).
Mercury......................	8·7 per cent. (Siemens).
The gold-silver alloy......	6·5 per cent. (hard-drawn). 6·7 per cent. (annealed).

From the foregoing Table it will be seen how well the alloy is adapted for use as a standard to compare the resistances of other metals.

Whilst making these experiments, I have found that as soon as most of the pure metals are alloyed with traces of any other, these differences rapidly decrease, in fact, almost in the same proportion as the conducting power of the metals themselves. This may explain why the copies of Weber's standard vary so much one from another. For instance, I have tested a commercial copper wire whose conducting power only varied between 0° and 100° 7 per cent (about), whilst pure copper varies 29 per cent. Now, suppose a wire of that copper whose conducting power only varies 7 per cent. between 0° and 100° be compared with one of Weber's standards at a certain temperature, and then with a pure copper wire at another temperature, say 20° difference, it is obvious that the pure copper wire will not have the same resistance as the original standard. It has as yet been generally assumed that the conducting power of all copper wire, whether pure or commercial, varies with an increase of temperature to the same degree, which, however, is far from true, and should be borne in mind when constructing a resistance thermometer as described by Siemens*. The fourth condition needs no comment. It is too well known how gold-silver alloys behave when exposed to the atmosphere.

With regard to the expense, the 9 grammes alloy cost, drawn into wire, about £1 4*s*., but the gold in it is always worth about 15*s*.; so that the real expense is very small. Care must, of course, be taken to prevent the alloy coming in contact with mercury, which amalgamates readily with all gold-silver alloys. The best way to prevent any such accident is to varnish the wires.

In having this alloy made, it would be advisable always to have two made by different parties, so as to be sure no mistake has occurred.

I therefore propose that all those who study the electrical resistance of metals, should compare one of their metals with this alloy, calling its conducting power 100 at 0° (hard-drawn wire of 1 millim. length and 1 millim. diam.); for then we should be able to compare the results obtained by different experimenters with one another.

I am sorry that I am not in a position to give the value of the absolute resistance of this alloy in terms of Weber's standard; for if this be once determined, we shall, of course, be able to reproduce an alloy of known resistance in absolute measure.

1 Torrington Street, January 1861.

XVIII. *Experimental Researches on the Laws of Absorption of Liquids by Porous Substances.* By THOMAS TATE, *Esq.*

[Concluded from p. 65.]

THE following experiments were made with filters of larger surfaces.

Experiment XXIV.

This experiment was made with upward filtration, as in the foregoing experiment. The filter was unsized paper, presenting a surface of $\frac{2}{10}$ths of an inch in contact with the water. The temperature was 43° throughout the experiment, other things being the same as in the last experiment.

The velocity of ascent of the liquid in the filter-tube for the first five units was found to be correctly represented by the formula

$$v=\frac{h}{11}.$$

Towards the close of the experiment, however, it was observed

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* Phil. Mag. January 1861.

that the filtering power of the paper had sensibly decreased; but, owing to the rapidity of the process, the effect arising from this cause had not materially interfered with the normal law of filtration.

Upon repeating the process, it was found that the velocity of ascent followed, for the most part, the law expressed by the general equation (7).

Hence it would appear that the normal law is, that the rate of filtration varies directly as the pressure upon the filter, the deviations from this law being due to the change which takes place in the molecules of the filter during the process.

The following experiment shows that by alternating the *direction* of the current of filtration, the original power of the filter may be maintained nearly unimpaired.

Experiment XXV.

The filter in this experiment was coke, presenting an interior surface of half an inch in contact with the water, and an exterior surface of about two inches, other things being the same as in the last experiment.

By *upward* filtration, the law of ascent of the liquid was accurately expressed by the formula

$$v=\frac{h}{144},$$

where v is the velocity per second, h being the column of liquid pressure on the filter expressed in units of the graduation of the tube.

The water thus filtered into the tube was allowed to be discharged by *downward* filtration. In this case the liquid followed nearly the same law of descent; but upon repeating this process of downward filtration, the velocity of descent was found to follow the law expressed by equation (7). Upon reversing the *direction* of the current of filtration, the law of ascent was found to be accurately expressed by the formula $v=\frac{h}{180}$. And so on to other alternations.

Similar results were obtained with wood-charcoal filters.

The following experiment was made to determine the rate of change which takes place in the filter during the process of filtration under a constant pressure.

Experiment XXVI.

This experiment was made with a close coke filter about an inch and a quarter in depth, and presenting three-fourths of an inch of surface in contact with the water, which had been carefully filtered through ordinary filtering-paper. The discharge was produced by downward filtration, the exterior portion of the filter being exposed to the air. The pressure on the filter, throughout the experiment, was produced by a column of 15 inches of the liquid; and the temperature was carefully maintained at 51° throughout the experiment. The successive intervals of time requisite for the discharge of one cubic inch of water were noted, and recorded in the following Table of results.

Succession of cubic inches discharged, n.	Corresp. time in minutes for each cubic inch, T.	Value of T by formula $T_{n-1}=3{\cdot}16\times 1{\cdot}42^{n-1}$.
1st cub. in.	3·16	3·16
2nd "	4·56	4·49
3rd "	6·36	6·37
4th "	9·00	9·05
5th "	13·50	12·85
6th "	18·00	18·24
7th "	26·00	25·91
8th "	33·00	36·80

Here the near coincidence of the results in the second and third columns shows that, *under a constant pressure on the filter* (within certain limits), *the times requisite to produce equal successive quantities of discharge are in geometrical progression.*

At or near to the eighth cubic inch of water discharged the progression seemed to have reached its limit; for the time required for the discharge of the succeeding cubic inch was found to be nearly the same as that of the eighth cubic inch.

The experiment, as above recorded, extended over a period of two hours nearly, and during that time the rate of filtration had changed from $\frac{1}{3{\cdot}16}$th of a cubic inch per minute to $\frac{1}{33}$rd of a cubic inch; that is, the rate of filtration had decreased eleven times nearly.

I offer no hypothesis on these remarkable results, beyond the mere statement of the fact that *during the process of filtration these filters undergo a progressive molecular change, causing the rate of filtration to decrease according to a general law expressed by the formula* $T_{n-1}=\alpha\beta^{n-1}$, *where* α *and* β *are constants for each particular filter, and* T_{n-1} *is the time required to filter the* nth *unit of water.*

At the close of the foregoing experiment, the water being discharged, the tube was filled with water by *upward* filtration, and then, this water being allowed to discharge itself by downward filtration, it was found that one cubic inch of water was discharged in 15·6 minutes, showing that by thus reversing the direction of the current of filtration, the filter had to a considerable extent regained its original power.

Precisely similar results were obtained with filters of wood-charcoal and unsized paper.

As it is scarcely possible to obtain filters, especially of charcoal and coke, of precisely the same internal structure, different filters, even of the same substance, vary very much with respect to the value of the ratio expressing the change in their filtering power. The rate of change of the filter in the foregoing experiment was unusually great.

The results recorded in Experiment XXI. were no doubt to some extent affected by the progressive deterioration of the filtering power of the coke; but it must be observed that this filter, having been previously tested, underwent an exceedingly small change during the process.

The following experiments were made to determine the analogy subsisting between the filtration of liquids through porous substances, and the discharge of liquids through small perforations made in thin plates.

The results of these experiments showed that the velocity of discharge through the orifices of thin plates varies according to a certain power of the column of liquid pressure, that is, $v \propto h^n$, where the exponent n is constant for orifices of the same diameter, but for orifices of different diameters it varies between the limits $n=\frac{1}{2}$ and $n=1$.

For orifices less than $\frac{1}{50}$th of an inch diameter, $n=\frac{1}{2}$; that is, the velocity of discharge, in this case, varies as the square root of the column of liquid pressure.

For orifices about $\frac{1}{100}$th of an inch diameter, $n=1$, that is, the velocity, in this case, varies as the depth of the column of liquid pressure.

Moreover it was found that the velocity of discharge increases in a high ratio with the increase of temperature, especially for the smallest orifices, and also that it varies with the adhesiveness, and even, in some cases, with the chemical composition of the liquids.

Hence it would appear that the laws of filtration are, in some respects, analogous to the discharge of liquids through minute perforations not exceeding $\frac{1}{100}$th of an inch diameter.

Experiment XXVII.

In this experiment the liquid was discharged from a small orifice $\frac{1}{45}$th of an inch in diameter, made in a thin plate cemented to the bottom of the filter-tube of the foregoing experiments.

Depth of the column of liquid, *h*.	Corresp. time in seconds, T.	Value of T by formula (9).
6	0	0
5	33	32·6
4	68	68·0
3	108	108·2
2	173	179·0
1	218	218·0
0	378	368·0

The formula expressing the relation between T and *h* is

$$T=368-150h^{\frac{1}{2}}. \quad \ldots \ldots \ldots \quad (9)$$

Hence we find

$$v=\frac{h^{\frac{1}{2}}}{75}; \quad \ldots \ldots \ldots \quad (10)$$

that is to say, for orifices of this diameter, *the velocity of discharge varies directly as the square root of the depth of the column of liquid.*

In general it has been found that

$$v=\epsilon h^{n}. \quad \ldots \ldots \ldots \quad (11)$$

By integrating the equation $v=\frac{dh}{dT}$, we find

$$T=\alpha-\gamma h^{1-n}, \quad \ldots \ldots \ldots \quad (12)$$

where $\gamma=\frac{1}{1-n}\cdot\frac{1}{\epsilon}$.

In formulæ (9) and (10), $n=\frac{1}{2}$, $\epsilon=\frac{1}{75}$, and $\gamma=150$. When $n=1$, we find from equation (11),

$$T=\alpha-\gamma\log h, \quad \ldots \ldots \ldots \quad (13)$$

where $\gamma=\frac{2\cdot30218}{\epsilon}$.

This formula represents the law of descent of the liquid in the following experiment.

Experiment XXVIII.

In this experiment the diameter of the orifice was $\frac{1}{100}$th of an inch nearly.

Depth of the column of liquid, *h*.	Corresp. time in minutes, T.	Value of T by formula (14).
6	0	0
5	4·0	4·0
4	9·0	8·8
3	15·2	15·1
2	23·5	23·9
1	36·6	38·9

In this case the formula expressing the relation between T and h is

$$T = 38{\cdot}95 - 50 \log h, \quad . \; . \; . \; . \; . \; . \quad (14)$$

and

$$\therefore\ v = {\cdot}046h; \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (15)$$

that is, *the velocity of discharge varies directly as the depth of the column of liquid.*

XIX. *Chemical Notices from Foreign Journals.* *By* E. ATKINSON, *Ph.D., F.C.S., Teacher of Physical Science in Cheltenham College.*

[Continued from vol. xx. p. 523.]

M. PASTEUR has for a considerable length of time been engaged in a research on the nature of alcoholic fermentation, a complete account of which he has published in the *Annales de Chimie et de Physique.* Some of the results have already appeared in this Journal; but the importance of the subject induces us to lay before our readers the following brief summary of the whole investigation, taken from the *Répertoire de Chimie.*

According to Pasteur, *alcoholic fermentation* is the peculiar transformation which sugar experiences under the influence of beer yeast; the author shows that glycerine and succinic acid are products of the alcoholic fermentation, and treats of their estimation and separation.

When the fermentation is terminated, the fermented liquid is passed through a weighed filter; the increase in weight of the filter after being dried at 100°, gives the weight of the yeast which has collected at the bottom of the vessel. The filtered liquid is then gradually evaporated until it is reduced to a small bulk, and the evaporation is terminated *in vacuo.* This residue is exhausted with a mixture of alcohol and rectified ether, which dissolves out succinic acid and glycerine. This liquid is evaporated first on the water-bath, and then, water having been added, the evaporation is continued over a gentle flame in order to get rid of the ether. The liquid is next exactly neutralized by milk of lime, carefully evaporated, and exhausted by a mixture of alcohol and ether, which only dissolves the glycerine. The residue, which consists of impure succinate of lime, is digested with alcohol of 80°; this dissolves the foreign matters and leaves the succinate of lime, which is dried and weighed.

The alcoholic liquid, which contains glycerine, is also evaporated in the water-bath, and finally *in vacuo,* where it must not remain more than two or three days; for it loses weight *in vacuo*, even at the ordinary temperature, when free from water. The glycerine is then weighed.

Using a very small quantity of yeast to produce the fermentation, Pasteur finds that the weight of succinic acid obtained exceeds the total weight of the soluble matters contained in the yeast. The same is the case with the glycerine, as compared with that of the yeast.

Glycerine, succinic acid, alcohol, and carbonic acid are not the only products of fermentation. The yeast assimilates something from the sugar: in one experiment 100 grms. of sugar gave up $1\frac{1}{2}$ grm. to the yeast; doubtless the cellulose of the new globules produced in the fermentation forms part of this increase.

The equation

$$C^{12}H^{12}O^{12}=2\,C^4H^6O^2+4\,CO^2,$$

by which the alcoholic fermentation was formerly expressed, does not exactly represent the change. The quantity of carbonic acid formed is less than that required by the equation. Hence a certain quantity of sugar disappears without being accounted for.

Pasteur assumes that this portion of sugar is resolved into succinic acid, glycerine, and carbonic acid, and he represents the change by the equation

$$49(C^{12}H^{11}O^{11})+109\,HO=12\underbrace{(C^8H^6O^8)}_{\text{Succinic acid.}}+72\underbrace{(C^6H^8O^6)}_{\text{Glycerine.}}+60\,CO^2.$$

Succinic acid and glycerine are constant and necessary products of alcoholic fermentation.

Lactic acid is an accidental production of the fermentation. Whenever it occurs (and it is very rare), the yeast must have contained some lactic ferment. Each of the ferments effects its usual transformation, and then the fermented liquid contains, besides succinic acid and glycerine, lactic acid and mannite, as well as a new acid.

In the second part of his research the author examines what becomes of the yeast in the fermentation. He shows that the nitrogen of the yeast is never changed into ammonia during alcoholic fermentation. Far from forming ammonia, that substance disappears; for yeast is formed in a mixture of sugar, an ammoniacal salt, and phosphates.

The globules of yeast are formed of small vesicles with elastic sides, full of a liquid containing a soft substance more or less granular and vascular. This is usually near the side; but in proportion as the globule becomes older, it tends towards the middle of the cell.

The globules are reproduced by means of gemmation, as M.

Cagniard Latour showed. The translucent globules without granular contents gemmate most rapidly; there are more granulations in proportion as the globule is older, less active, and less capable of gemmation.

Mitscherlich supposed these globules to burst and discharge their granules, dispersing seminules in the liquid, which increase and become globules of ordinary yeast. Pasteur does not agree with this.

It has been usually thought that, in the fermentation of solution of sugar, the ferment, far from being destroyed, is developed by gemmation; a close examination has shown the author that, in the fermentation of sugar in the presence of albuminous substances, *neither more nor less yeast is produced* than when the fermentation takes place with pure solution of sugar.

In all cases of the fermentation of pure solution of sugar, the weight of nitrogenous matter dissolved in the fermented liquid, added to the weight of the yeast, perceptibly exceeds the total weight of the original yeast. The increase amounts from 1·2 to 1·5 per cent. of the weight of the sugar.

The disappearance of the yeast in certain cases is merely apparent. Less yeast is obtained than was taken for fermentation because the quantity dissolved is greater than the weight of the new globules which are formed. In the fermentation of solutions of sugar containing albuminous matters, about 1 per cent. (of the sugar) of yeast and soluble products is formed, and therefore a little less than in working with yeast already formed, and with pure solution of sugar.

Hence the result is the same whether albuminous substances are present or not; the small difference observed arises doubtless from the fact that the globules, formed in a medium where the nitrogenized aliment is in excess, are more active, and for the same weight decompose more sugar than those formed in a medium poor in mineral or nitrogenized aliments.

Hence yeast, placed in solution of sugar, lives at the expense of the sugar and of its nitrogenous matter, which is dissolved or which becomes soluble from the changes taking place during fermentation between the principles which it contains. Fermentations which take place in the presence of excess of sugar are virtually of indefinite duration. This is readily conceived, for there is no destruction of nitrogenized matter; only displacements or modifications of this substance occur; and it remains in the complex state in which we are accustomed to meet with it in these products. The soluble part of the nitrogenous matter becomes partially fixed in the globules in an insoluble state. But the power of organization which these globules possess is such that the old globules can yield their nitrogenized matter in the soluble state to serve as food for the recent globules; and thus this fermentation continues for a long time.

The nitrogen of the yeast diminishes during fermentation from two reasons:—first, because the yeast increases in weight during fermentation by assimilating the elements of sugar, which contains no nitrogen; secondly, in consequence of the solubility of certain nitrogenized principles of the yeast.

In all alcoholic fermentation, part of the sugar becomes fixed on the yeast in the form of cellulose. When the yeast is formed in a medium consisting of pure sugar, of phosphates, and of an ammoniacal salt, it is clear that the cellulose is formed from the elements of the sugar, and that the ammonia combines with another part of the sugar to form the soluble and insoluble albuminous matters in the globules.

Are the phenomena analogous in the case in which sugar ferments in the presence of albuminous substances? Experiment proves that there is more cellulose in the yeast after than before the fermentation; so that it is very probable, if not certain, that all the cellulose of the yeast-globules is formed from the elements of sugar. But, besides the formation of cellulose, a perceptible quantity of sugar doubtless becomes assimilated by the yeast; for the weight of the yeast taken, added to the weight of the cellulose fixed during fermentation, does not equal the total weight of the yeast and of its soluble part, such as is found when the fermentation is terminated.

The weight of the cellulose increases considerably during fermentation, which furnishes a further proof of the vitality of the yeast during this act.

In every alcoholic fermentation, part of the sugar becomes assimilated to the yeast in the form of fatty matter. If solution of pure sugar be mixed with an aqueous extract of yeast which has been repeatedly extracted by alcohol and ether, and also with an imponderable weight of fresh globules, a few grammes of yeast are obtained containing one to two per cent. of its weight of fatty bodies, which are readily saponifiable, forming crystallizable fatty acids. This fat is formed from the elements of sugar; for yeast prepared in a mixture of water, sugar, ammonia, and phosphates also forms fatty matter.

Permanent Vitality of Yeast.—When yeast is mixed with a proportionally small quantity of sugar, after the latter has been decomposed, the activity of the yeast continues, but is turned upon its own tissues with an extraordinary energy and rapidity; a weight of alcohol and carbonic acid is thus obtained, exceeding that which the sugar could yield. Under these conditions the following facts are observed:—

1st. The action of the yeast is at first exerted on the sugar.

2nd. The yeast reacts on itself when the sugar has been completely destroyed.

3rd. The effect produced by the yeast on itself is not proportional to the weight of the yeast.

The author assumes that the globules formed by the fermentation of the sugar cannot attain their complete development for want of sufficient sugar, and that the young globules needing this nourishment live at the expense of the parent globules,—which produces a secondary fermentation, or the yeast destroys itself.

Lastly, M. Pasteur speaks of the application of some of the results to the composition of fermented liquors.

Since glycerine and succinic acid are constant products of the alcoholic fermentation, they ought to be found in wine, beer, cider, &c. This the author has already shown to be the case.

He terminates his memoir by the following passage, containing the fundamental conclusion which he draws from his important researches :—"As to the interpretation of the whole of the new facts which I have met with in my researches, I think that whoever considers them impartially will see that fermentation is a correlative act of life, and of the organization of globules, and not of death or the putrefaction of these globules; still less does it appear to be a phenomenon of contact, where the transformation of sugar proceeds in the presence of the ferment without yielding anything to it, or taking anything from it."

Kekulé has described* a mode of preparing the brominated derivatives of succinic acid, which is simpler than the methods hitherto employed.

To obtain bibromosuccinic acid, $\mathrm{C}^4\,\mathrm{H}^4\,\mathrm{Br}^2\,\Theta^4$, he heats in sealed tubes (at 150°—180°) twelve parts of succinic acid with thirty-three parts of bromine and twelve parts of water. After the reaction is complete, the whole mass is changed into small greyish crystals, and on opening the tube a large quantity of hydrobromic acid escapes. The crystals are purified by washing with cold water, solution in boiling water, and treatment with animal charcoal. Bibromosuccinic acid is formed in all cases in which a small quantity of water is taken, even when the proportion of bromine is such as to form monobromosuccinic acid.

Monobromosuccinic acid, $\mathrm{C}^4\,\mathrm{H}^5\,\mathrm{Br}\,\Theta^4$, is obtained by heating succinic acid with bromine and with a *larger* quantity of water. It is purified in a similar manner, but, being more soluble, it crystallizes less easily.

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* *Bulletin de la Société Chimique*, p. 208.

Butlerow[*] has investigated the action of ammoniacal gas on dioxymethylene[†]: these substances act on each other with great energy, and the mixture becomes ultimately converted into a magma of granular crystals, which, when purified, present the appearance of colourless, transparent, lustrous rhombohedra. This body can be sublimed when heated slowly. It has distinctly basic properties; it forms with hydrochloric acid a compound which crystallizes in long prismatic needles. The composition of the base is $\text{Ꞓ}^6 H^{12} N^4$, and its hydrochlorate is $\text{Ꞓ}^6 H^{12} N^4 HCl$. He considers that it has the rational formula $\left.\begin{array}{l} 2(\text{Ꞓ} H^2) N \\ 2(\text{Ꞓ} H^2) N \\ 2(\text{Ꞓ} H^2) N \end{array}\right\} N$, derived from the type NH^3, in which three atoms of hydrogen are replaced by three atoms of dimethylenammonium. He names it *hexamethylenamine*. Its nearest congener is Debus's glycosine. Like that body, Butlerow's new base is formed with elimination of water, according to the equation

$$3\text{Ꞓ}^2 H^4 \Theta^2 + 4NH^3 = \text{Ꞓ}^6 H^{12} N^4 + 6H^2 \Theta.$$

Butlerow has also found that diacetate of methylglycol, when heated with an excess of water in a sealed tube, is converted into dioxymethylene and free acetic acid.

The amalgam of sodium and mercury, obtained by adding sodium in small quantities to mercury, is very convenient for applying sodium in many reactions; in most cases it simply acts by dividing the sodium and increasing its surface.

Löwig and Scholz[‡] have investigated the action of this amalgam on a mixture of sulphide of carbon and iodide of ethyle. A very brisk reaction took place, and the vessel in which it was effected required cooling; the product was then treated with ether, which dissolved out a new body, as well as the excess of iodide of ethyle and bisulphide of carbon. The etherial solution, mixed with water, was distilled in the water-bath to drive off the ether, the excess of iodide of ethyle, and of the bisulphide of carbon; on cooling, the new body collected under water in the form of a yellow oil with a penetrating alliaceous odour. When this was fractionally distilled, it was found to consist principally of mercaptan, and of a new body which boiled at 188°, the analysis of which led to the composition $C^6 H^5 S^3$. It is formed thus:—

$$C^4 H^5 I + C^2 S^4 + 2 Na = C^6 H^5 S^3 + Na I + Na S.$$

It is a sulphur-yellow liquid, very fluid, and highly refracting.

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[*] *Bulletin de la Société Chimique*, p. 221.

[†] Phil. Mag. vol. xviii. p. 287.

[‡] *Journal für Prakt. Chemie*, vol. lxxix. p. 441.

Its density is 1·012 at 15°. It has a very disagreeable odour; it is insoluble in water, but soluble in alcohol, ether, and sulphide of carbon. Nitric acid attacks it violently, as also do chlorine, bromine, and hypochlorite of lime. Its alcoholic solution, mixed with an alcoholic solution of bichloride of mercury, gives a white precipitate which dissolves in hot alcohol. It consists of the new body combined with 6 equivalents of bichloride of mercury.

Löwig* has examined the action of sodium on oxalic ether. In the presence of water, sodium acts on oxalic ether, liberating carbonic oxide and forming carbonic ether. The mass becomes coloured, and a new acid is formed which the author calls myrinic acid.

With a pulverized sodium amalgam the action is quite different; oxalate of soda is deposited, only a few bubbles of carbonic oxide are disengaged, and not a trace of carbonic ether is formed. Treated with water, a colourless solution is formed, which, agitated with ether, gives up a neutral body of a bitter taste, which, on evaporation of the ether, is left as a syrupy colourless liquid. In two or three days this deposits beautiful white crystals, which have the crude formula $C^4 H^3 O^3$.

Neither baryta water nor subacetate of lead act in the cold on the aqueous solution of these crystals; but on the application of heat, a white precipitate is formed which contains no oxalic acid.

Graham showed long ago that phosphuretted hydrogen, which is not spontaneously inflammable, is made so by admixture with a very small quantity of nitrous acid. Landolt describes† a convenient mode of performing this experiment. Phosphuretted hydrogen, which is not spontaneously inflammable, is generated by heating phosphorus with concentrated soda lye, to which about double its volume of alcohol has been added, and is passed through some nitric acid placed in a porcelain dish. If the nitric acid is about 1·34, and has been previously freed from hyponitric acid by boiling, the bubbles of gas burst without inflaming. But if now a few drops of fuming nitric acid be added, each bubble takes fire, forming the usual rings. The spontaneous inflammability is again destroyed by an excess of hyponitric acid, for then the phosphuretted hydrogen is destroyed in the liquid. The hyponitric acid doubtless causes the formation of a small quantity of the spontaneously inflammable liquid compound $P H^2$. At the same time the nitric acid plays some part; for water to which hyponitric acid has been added produces no such effect.

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* *Journal für Prakt. Chemie*, vol. lxxix. p. 453.

† Liebig's *Annalen*, November 1860.

XX. *Note on the Numbers of* Bernoulli *and* Euler, *and a new Theorem concerning Prime Numbers.*

By J. J. SYLVESTER, *M.A., F.R.S., Professor of Mathematics at the Royal Academy, Woolwich*[*].

FOLLOWING the accepted *continental* notation, I denote by B_n[†] the positive value of the coefficient of t^{2n} in $\frac{t}{1-e^t}$ multiplied by the continual product $1.2.3\ldots n$.

The law which governs the fractional part of B_n was first given in Schumacher's *Nachrichten*, by Thomas Clausen in 1840; and almost immediately afterwards a demonstration was furnished by Professor Staudt in Crelle's Journal, with a reclamation of priority, supported by a statement of his having many years previously communicated the theorem to Gauss.

The law is this, that the positive or negative fractional residue of B_n (according as n is odd or even) is made up of the simple sum of the reciprocals of all the prime numbers which, respectively diminished by unity, are contained in $2n$. The proof, which is of an inductive kind, is virtually as follows: Suppose the law holds good up to $(n-1)$ inclusive; if we expand $\Sigma\,(x)^{2n}$ under the form $\frac{1}{e^{\frac{d}{dx}}-1}x^{2n}$, we shall evidently obtain $\frac{\Sigma(x)^{2n}}{x}\pm B_n$ under the form of a finite series, of which the terms are numerical multiples of the products of powers of x by the Bernoullian numbers of an order inferior to the nth. If, now, we make x equal to the product of all the primes which, diminished by unity, are contained in $2n$, it will at once be seen (on inspection of the series) that all its terms become integer numbers, and consequently $\frac{\Sigma x^{2n}}{x}\pm B_n$ becomes an integer; and therefore the law will hold good up to n, since it may easily be shown, by an application of Fermat's theorem and elementary arithmetical considerations, that if N be the product of any prime numbers whatever, and if p is the general name of such of them as diminished by unity are factors of μ, then $\frac{\Sigma N^{\mu}}{N}-\Sigma\frac{1}{p}$ is an integer.

Hence, since the law holds good for $n=1$, it is universally true.

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[*] Communicated by the Author.

[†] Were it not for the general usage being as stated in the text, I certainly think it would be far more convenient to use a notation agreeing with the continental method as to sign, and nearly, but not quite, with Mr. DeMorgan's as to quantity, viz. to understand by B_n the coefficient of t^n in $\frac{1}{2}t\frac{e^t+1}{e^t-1}$ taken positively, so that B_n should be equal to zero for all the odd values of n, not excepting $n=1$.

This theorem, then, of Staudt and Clausen, *inter alia*, gives a rule for determining what primes alone enter into the denominators of the Bernoullian numbers when expressed as fractions in their lowest terms; it enables us to affirm that only simple powers of primes enter into those denominators, and to know *à priori* what those prime factors are. This note is intended to supply a law concerning the *numerators* of the Bernoullian numbers, which I have not seen stated anywhere, and which admits of an instantaneous demonstration, *to wit*, that the whole of n will appear in the numerator of B_n, save and except such primes, or the powers of such primes, as we know by the Staudt-Clausen law must appear in the denominator.

I am inclined to believe that this law of mine was not known, at all events, in 1840, from the circumstance that in Rothe's Table, published by Ohm in Crelle's Journal in that year, which gives the values of B_n up to $n=31$, the numerators are, with one exception (about to be named), all exhibited in such a form as to show such low factors as readily offer themselves, but for B_{23} the fact of the divisibility of the numerator by 23 is not indicated. This numerator is 596451111593912163277961, which in fact $=23 \times 25932657025822267968607$. It is obvious, indeed, under my law, that whenever p is a prime number other than 2 and 3, the numerator of B_p must contain p, because in such case $p-1$ cannot be a factor of $2p$. When $p=3$ or $p=2$, $2p$ always contains $(p-1)$, so that 2 and 3 are necessarily constant factors of the Bernoullian denominators, and can therefore never appear in the numerators. In Schumacher the law of the denominator is given as "a passing" (or *chance* ?) "specimen" of a promised memoir by Clausen on the Bernoullian numbers, which I shall feel obliged if any of the readers of this Magazine will inform me whether anywhere, and if so, where it has appeared. Now for my demonstration of the law of the numerators.

By definition, $B_n = \Pi(2n) \times$ coefficient of t^{2n-1} in $\dfrac{1}{e^t-1}$. Let μ be any integer number; then $\pm(\mu^{2n}-1)B_n = \Pi(2n) \times$ coefficient of t^{2n-1} in $\dfrac{\mu}{e^{\mu t}-1} - \dfrac{1}{e^t-1}$, or in

$$\frac{(\mu-1)-(e^{(\mu-1)t}+e^{(\mu-2)t}+\ldots+e^t)}{e^{\mu t}-1},$$

or in

$$-\frac{e^{(\mu-2)t}+2e^{(\mu-3)t}+\ldots+(\mu-2)e^t+(\mu-1)}{e^{(\mu-1)t}+e^{(\mu-2)t}+\ldots+e^t+1}.$$

But obviously, by Maclaurin's theorem, the coefficient of t^{2n-1} in the expansion of this last generating function will be of the form $\pm \frac{1}{\Pi(2n-1)} \cdot \frac{\mathrm{I}}{\mu^{2n-1}}$, where I is an integer, and therefore B_n will be of the form $\frac{2n\mathrm{I}}{\mu^{2n-1}(\mu^{2n}-1)}$.

Suppose now, when $\frac{2n\mathrm{I}}{\mu^{2n-1}.(\mu^{2n}-1)}$ is reduced to its lowest terms, that p (a prime contained in $2n$) does not appear in the numerator, this can only happen by virtue of p being contained in $\mu^{2n-1}(\mu^{2n}-1)$; let now μ be taken successively $2,3,4,\ldots(\mu-1)$, then $\mu^{2n}-1$ in all these cases is divisible by p; and therefore, by an obvious inverse of Fermat's theorem, $(p-1)$ must be contained in $2n$, i. e. p must be a factor of the denominator of B_n under the Staudt-Clausen law, which proves my theorem.

As a corollary to the foregoing, using Herschel's transformation, we see that if μ be taken any integer whatever,

$$\pm \mathrm{B}_n = \frac{2n}{\mu^{2n}-1} \cdot \frac{(1+\Delta)^{\mu-2}+2(1+\Delta)^{\mu-3}+\ldots+(\mu-1)}{\Delta^{\mu-1}+\mu\Delta^{\mu-2}+\mu\frac{\mu-1}{2}\Delta^{\mu-3}+\ldots+\mu} 0^{2n}$$

$$= \frac{2n}{\mu^{2n}-1} \frac{\Delta^{\mu-2}+\mu\Delta^{\mu-1}+\mu\frac{\mu-1}{2}\Delta^{\mu-3}+\ldots+\mu\frac{\mu-1}{2}}{\Delta^{\mu-1}+\mu\Delta^{\mu-2}+\mu\frac{\mu-1}{2}\Delta^{\mu-3}+\ldots+\mu\frac{\mu-1}{2}\Delta+\mu} 0^{2n};$$

and if we write 0^{2n+1} instead of 0^{3n}, the result vanishes. For the case of $\mu=2$, this theorem accords with one well known. As this subject is so intimately related to that of the Herschelian differences of zero, I may take this occasion of stating a proposition concerning the latter, which (simple as it is) appears to have escaped observation, viz. that $\frac{\Delta^r 0^{n+r}}{\Pi(r)}$ is in fact the expression for the sum of the homogeneous products of the natural numbers from 1 to r, taken n and n together. For

$$\frac{1}{(x-n)(x-n+1)\ldots(x-1)x}$$

$$= \frac{1}{\Pi r}\left\{\frac{1}{x-n} - \frac{r}{(x-n+1)} + \frac{r.\frac{r-1}{2}}{(x-n+2)} \ldots \pm \frac{1}{r}\right\}$$

Hence obviously

$$\frac{1}{\Pi r}\left\{r^n - r(r-1)^n + r.\frac{r-1}{2}(r-2)^n \mp \text{\&c.}\right\},$$

i. e.

$$\frac{\Delta^r 0^n}{\Pi(r)} = \text{coefficient of } \frac{1}{x^{n-r}} \text{ in } \frac{1}{(x-n)(x-n+1)\ldots(x-1)}$$

$$= \text{the sum of the } (n-r)\text{ary homogeneous products of } 1, 2, 3, \ldots r.$$

Thus, then, we are able to affirm, from what is known concerning $\frac{\Delta^r 0^{r+n}}{\Pi r}$ (see Prof. De Morgan's Calculus), that the r-ary homogeneous product-sum of $1, 2, 3 \ldots n$ (which is of the degree $2r$ in n) always contains the algebraic factor $n(n+1)\ldots(n+r)$.

Addendum.—Since sending the above to press, I have given some further and successful thought to the Staudt-Clausen theorem. Staudt's demonstration labours under the twofold defect of indirectness and of presupposing a knowledge of the law to be established. In it the Bernoullian numbers are not made the subject of a direct contemplation, but are regarded through the medium of an alien function, one out of an infinite number, in which they are as it were latently embodied; and the proof, like all other inductive ones, whilst it convinces the judgment, leaves the philosophic faculty unsatisfied, inasmuch as it fails to disclose the reason (the title, so to say, to existence) of the truth which it establishes. I present below an immediate and a direct proof of this beautiful and important proposition, founded upon the same principle as gives the law of the necessary factor in the numerators (viz. the arbitrary decomposition of the generating function of Bernoulli's numbers into partial fractions), and resting upon a simple but important conception, that of *relative* as distinguished from absolute integers.

I generalize this notion, and define a quantity to be an integer relative to r (or, for brevity's sake, to be an r^{th} integer) when it may be represented by a fraction of which the denominator does not contain r.

The lemma* upon which my demonstration rests is the fol-

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* This lemma is the converse of a self-evident fact, and it virtually embodies a principle respecting an arithmetical fraction strikingly analogous to a familiar one respecting an algebraical one; viz. in the same way as a rational algebraical function of x can be expressed *in one, and only one, way* as an integral function augmented by a sum of negative powers of linear functions of x, so a rational arithmetical quantity can be expressed in one, and only one, way as an integer augmented by the sum of negative powers of simple prime numbers multiplied respectively by numbers less than such primes. In drawing this parallel, the arithmetical quantity $\frac{c}{p^i}$, where $c<p$, is regarded as the analogue of the algebraical one $\frac{1}{(ax+b)^i}$,

lowing, which is itself an immediate corollary from the arithmetical theorem that if $a, b, c, \ldots l$, with or without repetitions, are the distinct prime factors of the denominator of a fraction, the fraction itself may be resolved into the sum of simple fractions,

$$\frac{A}{a^{\alpha}}+\frac{B}{b^{\beta}}+\frac{C}{c^{\gamma}}+\text{\&c.}+\frac{L}{l^{\lambda}}$$

(itself a direct inference from the familiar theorem that if p, q be any two relative primes, the equation $px-qy=c$ is soluble in integers for all values of c). The lemma in question is as follows: If the quantity above described is representable under the several forms,

$$\frac{a'}{a^{f}}+\text{ an } (a^{\text{th}}) \text{ integer } \frac{b'}{b^{g}}+\text{ a } (b^{\text{th}}) \text{ integer } \ldots \frac{l'}{l^{k}}+\text{a } (k^{\text{th}}) \text{ integer,}$$

then it is equal to

$$\frac{a'}{a^{f}}+\frac{b'}{b^{g}}+\ldots+\frac{l'}{l^{k}}+\text{ an \textit{absolute} integer.}$$

From what has been already shown, it is obvious that μ being any prime number, the highest power of μ which *can* enter into the denominator of $(\mu^{2n}-1)B_n$ is μ^{2n}, and consequently $\mu^{2n}B$ is an integer relative to μ. Also it is clear that only those values of μ can appear in the denominator of B_n which, diminished by unity, are factors of $2n$. We have, moreover,

$$(-)^{n-1}(\mu^{2n}-1)B_n=\Pi(2n)\times\text{coefficient of } t^{2n-1} \text{ in } \frac{\mu}{e^{\mu t}-1}-\frac{1}{e^{t}-1},$$

i. e. coefficient of t^{2n-1} in $\dfrac{-N}{e^{\mu t}-1}$, where

$$\begin{aligned} N&=\Pi(2n)\left(e^{(\mu-1)t}+e^{(\mu-2)t}+\ldots+e^{t}-(\mu-1)\right)\\ &=\nu_1 t+\nu_2 t^2+\quad\ldots\quad+\nu_{2n}t^{2n}+\text{\&c.,}\end{aligned}$$

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as is quite proper, for both of them are fractions in their simplest forms, which would not be the case for the former were c equal to or greater than p, since in such case $\dfrac{c}{p^{i}}$ could be more simply expressed under the form $\dfrac{\gamma}{p^{i-1}}+\dfrac{\gamma'}{p^{i}}$.

This principle amounts to an affirmation that the equation in positive integers,

$$(b\ldots kl)x+(ab\ldots l)y+\ldots+(ab\ldots k)t-(ab\ldots kl)u=N,$$

where $a, b, \ldots k, l$ are relative primes, and $N<(ab\ldots kl)$, always admits of a solution, which may be termed the primitive one, and which will be unique, that namely in which $x, y, \ldots z, t$ are respectively less than $a, b, \ldots k, l$.

where obviously $\nu_1, \nu_2, \ldots \nu_{2n}$ are all integers, and the last of them

$$=(\mu-1)^{2n}+(\mu-2)^{2n}+\ \ldots\ +2^{2n}+1^{2n}.$$

Suppose now that $2n$ contains $(\mu-1)$, then by Fermat's theorem

$$\nu_{2n}\equiv(\mu-1)\ [\text{mod }\mu].$$

Again, a very slight consideration* will serve to show that when μ is any prime other than 2, $e^{\mu t}-1$ is of the form

$$\mu(t+\mu\delta_1 t^2+\mu\delta_2 t^3+\ \ldots\ +\mu\delta_{2n-1}t^{2n-1}+\text{\&c.}),$$

where $\delta_1, \delta_2, \ldots \delta_{2n-1}$ are all integers *relative* to μ. Now suppose

$$\frac{\mu\text{N}}{e^{\mu t}-1}=q_0+q_1t+q_2t^2+\ \ldots\ +q_{2n-1}t^{2n-1};$$

then by multiplication and comparison of coefficients we obtain the identities following:

$$q_0=\nu_1,\quad q_1+\mu q_0\delta_1=\nu_2,\quad q_2+\mu q_1\delta_1+\mu q_0\delta_2=\nu_3,\ \ldots$$

$$q_{2n-1}+\mu q_{2n-1}\delta_1+\ \ldots\ \mu q_0\delta_{2n-1}=\nu_{2n};$$

obviously therefore $q_{2n-1}=\mu\times$ (an integer relative to μ) $+\nu_{2n}$. Hence

$$(-1)^n\text{B}_n=(\text{an integer relative to }\mu)-\frac{\nu_{2n}}{\mu}$$

$$=(\text{an integer relative to }\mu)+\frac{1}{\mu}.$$

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[*] For μ being a prime number greater than 2, if we put $\frac{\mu^r}{\Pi(r)}$ (the coefficient of t^r in $e^{\mu t}-1$) under the form of (an integer *quâ* μ)$\times\mu^i$, we have

$$i=r-\text{E}\frac{r}{\mu}-\text{E}\frac{r}{\mu^2}-\text{E}\frac{r}{\mu^3}-\text{\&c.},$$

$$=\text{ or }>r-\frac{r}{\mu-1}=\text{or}>\frac{r}{2}>1\text{ when }r>2;\text{ also when }r=2, i=2-\text{E}\frac{2}{\mu}=2.$$

When $\mu=2$, this would be no longer true; and in fact it is easily seen that in this case, whenever r is a power of 2, i will be only equal to 1.

For the benefit of my younger readers, I may notice that the *direct* proof of the theorem that the product of any r consecutive numbers must contain the product of the natural numbers up to r, or, in other words, that the trinomial coefficient $\frac{\Pi n}{\Pi\nu\Pi\nu'}$, where $\nu+\nu'=n$ is an integer, is drawn from the fact that this fraction may be represented as an integer *quâ* μ (any prime) multiplied by μ^i, where

$$i=\left(\text{E}\frac{\lambda}{\mu}-\text{E}\frac{\nu}{\mu}-\text{E}\frac{\nu'}{\mu}\right)+\left(\text{E}\frac{n}{\mu^2}-\text{E}\frac{\nu}{\mu^2}-\text{E}\frac{\nu'}{\mu^2}\right)+\text{\&c.}$$

(Ex meaning the integer part of x), so that i is necessarily either zero or positive, because the value of each triad of terms within the same parenthesis is essentially zero or positive. This is the natural and only direct procedure for establishing the proposition in question.

And this relation obtains for any value of μ other than 2, which (or a power of which) *could* be contained in $2n$. When $\mu=2$, the δ series will *not* all of them be the doubles of relative integers to 2; but the ν series, on account of the factor $\Pi(2n)$, will obviously, up to ν_{2n-1} inclusive, all contain 2 and $\nu_{2n}=1$; consequently q_{2n} will be twice (an integer *quâ* 2)+1, and B_n will still be (an integer relative to μ) $+\frac{1}{\mu}$ as before. Hence it follows from the lemma that $(-1)^n B_n =$ an absolute integer $+\Sigma\frac{1}{\mu}$, or

$$B_n = \text{an integer } + (-)^n\Sigma\frac{1}{\mu},$$

which is the equation expressed by the Staudt-Clausen theorem*.

My researches in the theory of partitions have naturally invested with a new and special interest (at least for myself) everything relating to the Bernoullian numbers. I am not aware whether the following expression for a Bernoullian of any order as a quadratic function of those of an inferior order happens to have been noticed or not. It may be obtained by a simple process of multiplication, and gives a means (not very expeditious, it is true) for calculating these numbers from one another without having recourse to the calculus of differences or Maclaurin's theorem, viz.

$$-\frac{B_n}{\Pi(2n)} = (2^2-1)\frac{B_1}{\Pi(2)}\cdot\frac{B_{n-1}}{\Pi(2n-2)} + (2^4-1)\frac{B_2}{\Pi(4)}\cdot\frac{B_{n-2}}{\Pi(2n-4)}$$

$$+ \&c. \dots + (2^{2n-4}-1)\frac{B_{n-2}}{\Pi(2n-4)}\cdot\frac{B_2}{\Pi(4)}$$

$$+ (2^{2n-2}-1)\frac{B_{n-1}}{\Pi(2n-2)}\cdot\frac{B_1}{\Pi 2},$$

in which formula the terms admit of being coupled together from end to end, excepting (when n is even) one term in the middle.

To illustrate my law respecting the numerators of the numbers of Bernoulli, and its connexion with the known law for the denominators, suppose twice the index of any one of these

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* I ought to observe that in all that has preceded I have used the word *integer* in the sense of positive or negative integer, and the demonstration I have given holds good without assuming B_n to be positive. That this is the case, or, in other words, that the signs of the successive powers of t^2 in $\frac{e^t-1}{e^t+1}$ are alternately positive and negative, may be seen at a glance by putting $t=2\sqrt{-1}\theta$, and remembering that all the coefficients in the series for $\tan\theta$ in terms of θ are necessarily positive, because $\left(\frac{d}{d\theta}\right)^i\tan\theta$ obviously only involves positive multiples of powers of $(\tan\theta)$ and $(\sec\theta)$.

numbers to contain the factor $(p-1)p^i$, where p is any prime; then this number will contain the first power of p in its denominator; but if the factor p^i is contained in double the index in question, but $(p-1)$ *not*, then p^i will appear bodily as a factor of the numerator.

It has occurred to me that it might be desirable to adhere to the common definition of "*Bernoulli's numbers*," but at the same time to use the term Bernoulli's *coefficients* to denote the actual coefficients in $\frac{e^t+1}{2(e^t-1)}$; so that if the former be denoted in general by B_n and the latter by β_n, we shall have

$$\beta_{2n}=(-)^{n-1}B_n,$$
$$\beta_{2n+1}=0.$$

In the absence of some such term as I propose, many theorems which are really single when affirmed of the *coefficients*, become duplex or even multifarious when we are restrained to the use of the *numbers* only.

Postscript.—The results obtained concerning Bernoulli's numbers in what precedes, admit of being deduced still more succinctly; and this simplification is by no means of small importance, as it leads the way to the discovery of analogous and unsuspected properties of Euler's numbers (namely the coefficients of $\frac{\theta^{2n}}{\Pi 2n}$ in the expansion of $\sec\theta$), and to some very remarkable theorems concerning prime numbers in general.

In fact, to obtain the laws which govern the denominators and numerators of Bernoulli's numbers, we need only to use the following principles:—(1) That μ being a prime, $\Sigma\mu^n=0$, or $=-1$ to the modulus μ, according as $\mu-1$ is, or is not, a factor of m,—the second part of this statement being a direct consequence of Fermat's theorem, the first part a simple inference from its inverse. (2) That $e^{\mu t}-1$ is of the form $\mu t+\mu^2t^2T$, where T is a series of powers of t, all of whose coefficients are integers relative to μ, except for the case of $\mu=2$, when $e^{\mu t}-1$ is of the form $2t+2t^2T$. We have then $(\mu^{2n}-1)(-)^{n-1}B_n=-\Pi(2n)\times$ coefficient of t^{2n-1} in $\frac{e^{(\mu-1)t}+e^{(\mu-2)t}+\ldots+e_t}{e^{\mu t}-1}$ by actual division (in virtue of principle (2)) $=I-\frac{R}{\mu}$, where I is an integer relative to μ, containing n, and $-R=-\frac{1}{\mu}(1^{2n-1}+2^{2n-1}+\ldots+(\mu-1)^{2n-1}$. Hence $(-)^nB_n=$ an integer relative to μ or to such integer $+\frac{1}{\mu}$, according as $2n$ does not or does contain $(\mu-1)$, which proves the law for the numerators; and so if μ^i is a factor of n, but $(\mu-1)$ not a factor of $2n$, $\frac{R}{\mu}$ will vanish, and $\mu^{2n}-1$ will not contain μ; hence $(\mu^{2n}-1)B_n$, and consequently B_n will be the product of μ^i by an integer relative to μ, which proves my numerator law.

So by extending the same method to the generating function $\frac{1}{e^t+\sqrt{-1}}$, it may very easily be proved that if we write

$$\sec\theta = E_0 + E_1\frac{\theta^2}{1.2}t + E_2\frac{\theta^4}{1.2.3.4} + \ldots + E_n\frac{\theta^{2n}}{1.2.3\ldots 2n} + \&c.,$$

every prime number μ of the form $4n+1$, such that $(\mu-1)$ is a factor of $2n$, will be contained in E_n; and every such factor, when p is of the form $4m-1$, will be contained in $E_n+(-)^n 2$.

I call the numbers $E_1, E_2, \ldots E_n$ Euler's 1st, 2nd, ... nth numbers, as Euler was apparently the first to bring them into notice. In the *Institutiones Calculi Diff.* he has calculated their values up to E_9 inclusive: in this last there is an error, which is specified by Rothe in Ohm's paper above referred to; had Euler been possessed of my law this mistake could not have occurred, as we know that E_9+2 ought to contain the factors 19 and 7, neither of which will be found to be such factors if we adopt Euler's value of E_9, but both will be such if we accept Rothe's corrected value. But in still following out the same method, I have been led, through the study of Bernoulli's and the allied numbers, and with the express aid of the former, to a perfectly general theorem concerning prime numbers, in which Bernoulli's numbers no longer take any part. Fermat's theorem teaches us the residue of $q^{\mu-1}$ in respect to μ, viz. that it is unity; but I am not aware of any theorem being in existence which teaches anything concerning the relation of $\frac{q^{\mu-1}-1}{\mu}$ to μ (or, which is the same thing, of the relation of $q^{\mu-1}$ to the modulus μ^2). I have obtained remarkable results relative to the above quotient, which I will state for the simplest case only, viz. that where q as well as μ is a prime number. I find that when q is any odd prime,

$$\frac{1-q^{\mu-1}}{\mu} \equiv \frac{c_1}{\mu-1} + \frac{c_2}{\mu-2} + \frac{c_3}{\mu-3} + \ldots + \frac{c_{\mu-1}}{l},$$

where $c_1, c_2, c_3, \ldots c_{\mu-1}$ are continually recurring cycles of the numbers 1, 2, 3, ... r, the cycle beginning with that number r'

which satisfies the congruence $pr' \equiv 1 \pmod{r}$. Since we know that $\frac{1}{\mu-1}+\frac{1}{\mu-2}+\frac{1}{\mu-3}+\ldots+\frac{1}{l}\equiv 0$ (to mod. μ) in place of the cycle 1, 2, 3, … r, we may obviously substitute the reduced cycle

$$-\frac{r-1}{2},\quad -\frac{r-3}{2},\ldots -1,\ 0,\ 1,\ldots \frac{r-3}{2},\quad \frac{r-1}{2}.$$

Thus, *ex. gr.*, $\frac{3^{\mu-1}-1}{\mu}$, when μ is of the form $6n+1$,

$$\equiv\frac{1}{\mu-1}-\frac{1}{\mu-3}+\frac{1}{\mu-5}-\frac{1}{\mu-7}\ldots+1,\text{to mod.}\,\mu,$$

and when μ is of the form $6n-1$,

$$\equiv\frac{-1}{\mu-2}+\frac{1}{\mu-3}-\frac{1}{\mu-4}+\frac{1}{\mu-5}\ldots-1,\text{to mod.}\,\mu.$$

When q is 2, the theorem which replaces the preceding is as follows: $\frac{2^{\mu-1}-1}{\mu}$, when μ is of the form $4n+1$,

$$\equiv\frac{1}{\mu-1}+\frac{1}{\mu-2}-\frac{1}{\mu-3}-\frac{1}{\mu-4}+\frac{1}{\mu-5}+\frac{1}{\mu-6}$$
$$-\frac{1}{\mu-7}-\frac{1}{\mu-8}+\frac{1}{\mu-9}\pm\text{\&c.},$$

and when μ is of the form $4n-1$,

$$\equiv-\frac{1}{\mu-1}+\frac{1}{\mu-2}+\frac{1}{\mu-3}-\frac{1}{\mu-4}-\frac{1}{\mu-5}$$
$$+\frac{1}{\mu-6}+\frac{1}{\mu-7}\mp\text{\&c., to mod. }\mu.$$

When q is not a prime, a similar theorem may be obtained by the very same method, but its expression will be less simple. The above theorems would, I think, be very noticeable were it only for the circumstance of their involving (as a condition) the primeness as well of the base as of the augmented index of the familiar Fermatian expression $q^{\mu-1}$,—a condition which here makes its appearance (as I believe) for the first time in the theory of numbers.

XXI. *On a new Method of arranging Numerical Tables.* By W. DITTMAR, *Assistant in the Laboratory of Owens College, Manchester*[*].

[With a Plate.]

THE use of numerical tables constructed in the ordinary manner is, for obvious reasons, inseparably connected with interpolation calculations. Such calculations, although by no means difficult, involve, as every practical mathematician knows, much loss of time, and often give rise to mistakes. This is especially the case when, from a given value of a dependent variable, the corresponding value of the independent, or of another dependent variable has to be found. It is obvious that all interpolations could be avoided by giving all the values which can possibly be required; this, however, is in most cases practically impossible, as the tables would thus become inconveniently voluminous, and the chance of typographical error would be greatly increased. I believe that the completeness thus attainable can be arrived at by the use of the following graphical method, while at the same time the size of the table will not extend beyond the ordinary limits. Let it be required to construct a table giving the values of several functions $y, z, w \dots$ of a variable x. Draw a system of vertical parallels, and call them respectively the $x, y, z, w \dots$ line. On each of the verticals construct a scale, and let every point on each of the scales be the symbol for a number equal either to the number of divisions (and in general one fraction of a division) contained between the origin and that point, or to a simple multiple of this number; so that the marks on each scale represent the terms of an arithmetical series. These several scales must be so constructed that the corresponding values of all the variables are found in one and the same horizontal line. It is true that, strictly speaking, this can only be the case when all the functions are linear ones; it is, however, easy to show that, practically speaking, the problem can always be solved with any degree of exactitude required, provided that the functions are continuous. The mode of construction and use of such tables will perhaps be best explained by an example.

Fig. 1, Plate III. represents the commencement of a Table of logarithms and reciprocals, which is intended to afford about the same degree of exactitude as a common 4-place logarithmic table. Column II. contains the logarithm-scale; each of the divisions is 4 millims. in length, and represents a logarithmic increment of 0·001; each point in this scale has a twofold meaning; it stands, namely, both for the mantissa $x=\lambda$, and for the (posi-

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[*] Communicated by the Author.

tive) mantissa of $\frac{1}{x}$ which equals $1-\lambda$. When this scale had been drawn, the integral numbers from 100 to 1000 were marked on the other two columns by drawing horizontal lines opposite to the corresponding logarithms; the numbers up to num. (mant. $=\cdot5000$) were placed in column I. beyond this number in column III. Lastly, the interval between every two such marks was divided into ten equal parts. Since, now, within the intervals $x=100$ to $x=101$, $x=101$ to $x=102$, &c. the quotients $\frac{\Delta(\log x)}{\Delta x}$ may in our case safely be taken as a constant, any $\Delta \log x$ within such an interval may, together with its Δx, be graphically represented by one and the same straight line, as is in fact done in the Table. It is now clear that any horizontal line will cut the verticals in points which (whether they coincide with marks or not) are symbols for respectively a certain number x, $\log x$, and a number having the same succession of figures as $\frac{1}{x}$. The above will afford complete information for the practical use of the Table*. The division of the spaces between the marks on the scales is to be made by the eye. With some practice an error greater than one-tenth of an interval in the logarithm-scale will rarely be made; the position of the marks themselves may be by far more accurately determined†; the error accompanying any logarithm taken out of the Table will therefore scarcely ever exceed 0·0001, and any number found by its help will be correct within about $\frac{1}{4000}$ of its value. The reliability of the results is not so dependent upon the exactitude of the drawing as one might at first sight be disposed to think, as only that portion of every number is really graphically *determined* which in an ordinary 4-place table is found by computative interpolation. The Table is particularly handy for finding the values of reciprocals, as these may be obtained directly with-

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[*] Let us suppose the logarithm and the reciprocal of 1·0653 were to be found. Divide the interval between the marks 106·5 and 106·6 on the x-scale (in your mind) into ten equal parts, and through the third point from 106·5 draw a horizontal line, which is best done by bringing a straight line etched on one side of a piece of plate-glass into the right position. This line will cut the log-scale in the point $\cdot0274=$ mant. 10653, and the $\frac{1}{x}$-scale in the point $938\cdot7=1000\times\frac{1}{1\cdot0653}$. At the same time $\log\frac{1}{1\cdot0653}$ may be read off directly, the point of intersection in the log-scale standing also for mant. $\frac{1}{1\cdot0653}=\cdot9726$.

[†] When great exactitude is required, the figure may be drawn on a copper plate by help of a dividing engine, and copies printed from the copper.

out the intervention of logarithms. It will therefore be especially useful in chemical calculations, as, for instance, in converting specific gravities into specific volumes, for reducing per-centage compositions to the unit of weight of one constituent, &c. The general applicability of this method is evident. For the sake of illustration I append figs. 2 and 3, giving respectively the beginning of a general interpolation- and of a densimetric Table. The mode of construction and use of these Tables will be understood from the description given of the Table of logarithms.

XXII. *On Graphical Interpolation. By* W. Dittmar, *Assistant in the Laboratory of Owens College, Manchester*[*].

THE principles laid down in the preceding article for the construction of numerical tables may also be employed for the purpose of carrying out graphical interpolations. Let us suppose that the corresponding values x_0y_0, x_1y_1, x_2y_2 . . . belonging to an unknown function $y=f(x)$ are given by observation, and that it is required to complete the series of variables. It is clear that the direct results of observation may be registered in a graphical table in the manner described above. For this purpose it is only necessary to draw a straight line, and to construct on one side of it a scale with a constant unit of length, the points of which are considered as representatives of the values of y, while on the other side of the line marks made opposite to the points y_0, y_1, y_2, . . . are taken as symbols of the respective values of x, i. e. x_0, x_1, x_2, . . . &c. The question now is, how can the gaps on the x-scale be filled up by graphic interpolation? This may be accomplished in the following way:—When there are reasons for supposing that $f(x)$ does not differ much from a linear function, all the divisions on the x-scale may be made equal to one another, and each so long that the points corresponding to x_0, x_1, &c. coincide as nearly as possible with those signifying respectively y_0, y_1, &c. on the y-scale. This is best done by dividing the distance between the two furthest points on the x-scale into the requisite number of equal parts, drawing lines from the points thus obtained to one point situated at some distance, and by moving the y-scale along in this system of lines parallel to the line divided[†], till a position is found in which the points of intersection of the radii with the y-scale yield an x-scale which agrees as closely as possible with the observed values.

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[*] Communicated by the Author.

[†] A sharp-edged drawing measure is most conveniently employed for this purpose.

If a satisfactory result cannot be obtained in this way, it is best to try whether $f(x)$ can be practically represented by an expression of the form $A+Bx+Cx^2$, where A, B, and C signify constants. If this be the case, we have*

$$y=A+Bx+Cx^2. \quad \ldots \ldots \quad \text{(I.)}$$

$$y-\Delta y=A+B(x-\Delta x)+C(x-\Delta x)^2.$$

$$\Delta y=B\Delta x-C(\Delta x)^2+(2C\Delta x)x. \quad \ldots \ldots \quad \text{(II.)}$$

$$\Delta x\frac{y_p-y_n}{x_p-x_n}=B\Delta x-C(\Delta x)^2+(2C\Delta x)\left(\frac{x_p+x_n+\Delta x}{2}\right). \quad \text{(III.)}$$

Comparing equation (III.) with (II.), we see that $\frac{y_p-y_n}{x_p-x_n}\Delta x$ is equal to *this* Δy, the graphical representative of which is contained between the two points in the x-scale corresponding to the numbers $\frac{x_p+x_n+\Delta x}{2}$ and $\frac{x_p+x_n+\Delta x}{2}-\Delta x$.

From the equations (II.) and (III.) the following method for constructing the x-scale may be found:—Combine the observed pairs of variables by twos, and find from every combination, with the help of equation (III.), a certain Δy, the graphical representative of which is contained between the two points which in the x-scale mean $x-\Delta x$ and x. Then construct a rectangular system of coordinates, and represent the values of x thus obtained (with an arbitrary unit of length) as abscissæ, the corresponding values of Δy as ordinates, using for the latter that length as unit which represents $\Delta y=1$ in the y-scale. Next draw a straight line which passes as nearly as possible through the extreme points of the ordinates. The ordinates of this line corresponding respectively to $\Delta x, 2\Delta x, 3\Delta x, \ldots$, when put together in the right order, give the required x-scale. In order to obtain exact results, it is advisable to choose first such a large value for Δx that only a few points of the x-scale are obtained, and to determine the intermediate points by new constructions. As soon as so many points are determined that two successive intervals do not differ perceptibly in length, the subdivisions of each interval may be made equal to one another. Should the indications of a scale thus obtained not agree quite satisfactorily with the observed data, it may often be improved by slightly changing the unit of length used in the construction, and by altering its position with respect to the y-scale. A convenient method for doing this has been already described.

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* x and y mean any values of the variables belonging together; x_p, y_p, and x_n, y_n mean particular pairs of variables; Δx stands for the constant numerical difference corresponding to one division in the x-scale; Δy for the *corresponding variable* increment of y.

If an interpolation of the second degree proves to be insufficient for representing the observations, the series of values given is divided into several intervals, and each of these is then treated in the manner described.

In some cases it will be advisable to represent, not y, but some function of y like y^n, $\log y$, &c., on a scale with equal divisions.

The advantages which the method of graphical interpolation described appears to me to possess, as compared with the usual one of drawing a curve in a rectangular system of coordinates, are the following:—

1. All the lines drawn are straight lines; the personal error in the drawing is therefore reduced to a minimum.
2. The drawing can be executed with less trouble and greater exactitude, and it takes up less space than in the ordinary way.
3. When the drawing is finished, the value of y belonging to any given x may be read off at once, and *vice versâ*.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 79.]

March 22, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read:—

"On the Theory of Compound Colours, and the Relations of the Colours of the Spectrum." By J. Clerk Maxwell, Esq., Professor of Natural Philosophy, Marischal College and University, Aberdeen.

Newton (in his 'Optics,' Book I. part ii. prop. 6) has indicated a method of exhibiting the relations of colour, and of calculating the effects of any mixture of colours. He conceives the colours of the spectrum arranged in the circumference of a circle, and the circle so painted that every radius exhibits a gradation of colour, from some pure colour of the spectrum at the circumference, to neutral tint at the centre. The resultant of any mixture of colours is then found by placing at the points corresponding to these colours, weights proportional to their intensities; then the resultant colour will be found at the centre of gravity, and its intensity will be the sum of the intensities of the components.

From the mathematical development of the theory of Newton's diagram, it appears that if the positions of any three colours be assumed on the diagram, and certain intensities of these adopted as units, then the position of every other colour may be laid down from its observed relation to these three. Hence Newton's assumption that the colours of the spectrum are disposed in a certain manner in the circumference of a circle, unless confirmed by experiment, must be regarded as merely a rough conjecture, intended as an illustration of his method, but not asserted as mathematically exact. From the results of the present investigation, it appears that the colours of the spectrum, as laid down according to Newton's method from actual observation, lie, not in the circumference of a circle, but in the periphery of a triangle, showing that all the colours of the spectrum may be *chromatically* represented by three, which form the angles of this triangle.

	Wave-length in millionths of Paris inch.
Scarlet........	2328, about one-third from line C to D,
Green	1914, about one-quarter from E to F,
Blue	1717, about half-way from F to G.

The theory of three primary colours has been often proposed as an interpretation of the phenomena of compound colours, but the relation of these colours to the colours of the spectrum does not seem to have been distinctly understood till Dr. Young (Lectures on Natural Philosophy, Kelland's edition, p. 345) enunciated his theory of three primary sensations of colour which are excited in different proportions when different kinds of light enter the organ of vision. According to this theory, the threefold character of colour, as perceived by us, is due, not to a threefold composition of light, but to the constitution of the visual apparatus which renders it capable of being affected in three different ways, the relative amount of each sensation being determined by the nature of the incident light. If we could exhibit three colours corresponding to the three primary sensations, each colour exciting one and one only of these sensations, then since all other colours whatever must excite more than one primary sensation, they must find their places in Newton's diagram within the triangle of which the three primary colours are the angles.

Hence if Young's theory is true, the complete diagram of all colour, as perceived by the human eye, will have the form of a triangle.

The colours corresponding to the pure rays of the spectrum must all lie within this triangle, and all colours in nature, being mixtures of these, must lie within the line formed by the spectrum. If therefore any colours of the spectrum correspond to the three pure primary sensations, they will be found at the angles of the triangle, and all the other colours will lie within the triangle.

The other colours of the spectrum, though excited by uncompounded light, are compound colours; because the light, though simple, has the power of exciting two or more colour-sensations in different proportions, as, for instance, a blue-green ray, though not compounded of blue rays and green rays, produces a sensation compounded of those of blue and green.

The three colours found by experiment to form the three angles of the triangle formed by the spectrum on Newton's diagram, *may* correspond to the three primary sensations.

A different geometrical representation of the relations of colour may be thus described. Take any point not in the plane of Newton's diagram, draw a line from this point as origin through the point representing a given colour on the plane, and produce them so that the length of the line may be to the part cut off by the plane as the intensity of the given colour is to that of the corresponding point on

Newton's diagram. In this way any colour may be represented by a line drawn from the origin whose direction indicates the quality of the colour, and whose length depends upon its intensity. The resultant of two colours is represented by the diagonal of the parallelogram formed on the lines representing the colours (see Prof. Grassmann in Phil. Mag. April 1854).

Taking three lines drawn from the origin through the points of the diagram corresponding to the three primaries as the axes of coordinates, we may express any colour as the resultant of definite quantities of each of the three primaries, and the three elements of colour will then be represented by the three dimensions of space.

The experiments, the results of which are now before the Society, were undertaken in order to ascertain the exact relations of the colours of the spectrum as seen by a normal eye, and to lay down these relations on Newton's diagram. The method consisted in selecting three colours from the spectrum, and mixing these in such proportions as to be identical in colour and brightness with a constant white light. Having assumed three standard colours, and found the quantity of each required to produce the given white, we then find the quantities of two of these combined with a fourth colour which will produce the same white. We thus obtain a relation between the three standards and the fourth colour, which enables us to lay down its position in Newton's diagram with reference to the three standards.

Any three sufficiently different colours may be chosen as standards, and any three points may be assumed as their positions on the diagram. The resulting diagram of relations of colour will differ according to the way in which we begin; but as every colour-diagram is a perspective projection of any other, it is easy to compare diagrams obtained by two different methods.

The instrument employed in these experiments consisted of a dark chamber about 5 feet long, 9 inches broad, and 4 deep, joined to another 2 feet long at an angle of about 100°. If light is admitted at a narrow slit at the end of the shorter chamber, it falls on a lens and is refracted through two prisms in succession, so as to form a pure spectrum at the end of the long chamber. Here there is placed an apparatus consisting of three moveable slits, which can be altered in breadth and position, the position being read off on a graduated scale, and the breadth ascertained by inserting a fine graduated wedge into the slit till it touches both sides.

When white light is admitted at the shorter end, light of three different kinds is refracted to these three slits. When white light is admitted at the three slits, light of these three kinds in combination is seen by an eye placed at the slit in the shorter arm of the instrument. By altering the three slits, the colour of this compound light may be changed at pleasure.

The white light employed was that of a sheet of white paper, placed on a board, and illuminated by the sun's light in the open air; the instrument being in a room, and the light moderated where the observer sits.

Another portion of the same white light goes down a separate compartment of the instrument, and is reflected at a surface of blackened glass, so as to be seen by the observer in *immediate contact* with the compound light which enters the slits and is refracted by the prisms.

Each experiment consists in altering the breadth of the slits till the two lights seen by the observer agree both in colour and brightness, the eye being allowed time to rest before making any final decision. In this way the relative places of sixteen kinds of light were found by two observers. Both agree in finding the positions of the colours to lie very close to two sides of a triangle, the extreme colours of the spectrum forming doubtful fragments of the third side. They differ, however, in the intensity with which certain colours affect them, especially the greenish blue near the line F, which to one observer is remarkably feeble, both when seen singly, and when part of a mixture; while to the other, though less intense than the colours in the neighbourhood, it is still sufficiently powerful to act its part in combinations. One result of this is, that a combination of this colour with red may be made, which appears red to the first observer and green to the second, though both have normal eyes as far as ordinary colours are concerned; and this blindness of the first has reference only to rays of a definite refrangibility, other rays near them, though similar in colour, not being deficient in intensity. For an account of this peculiarity of the author's eye, see the Report of the British Association for 1856, p. 12.

By the operator attending to the proper illumination of the paper by the sun, and the observer taking care of his eyes, and completing an observation only when they are fresh, very good results can be obtained. The compound colour is then seen in contact with the white reflected light, and is not distinguishable from it, either in hue or brilliancy; and the average difference of the observed breadth of a slit from the mean of the observations does not exceed $\frac{1}{30}$ of the breadth of the slit if the observer is careful. It is found, however, that the errors in the value of the sum of the three slits are greater than they would have been by theory, if the errors of each were independent; and if the sums and differences of the breadth of two slits be taken, the errors of the sums are always found greater than those of the differences. This indicates that the human eye has a more accurate perception of differences of hue than of differences of illumination.

Having ascertained the chromatic relations between sixteen colours selected from the spectrum, the next step is to ascertain the positions of these colours with reference to Fraunhofer's lines. This is done by admitting light into the shorter arm of the instrument through the slit which forms the eyehole in the former experiments. A pure spectrum is then seen at the other end, and the position of the fixed lines read off on the graduated scale. In order to determine the wave-lengths of each kind of light, the incident light was first reflected from a stratum of air too thick to exhibit the colours of Newton's rings. The spectrum then exhibited a series of dark bands, at intervals increasing from the red to the violet. The wave-lengths corresponding to these form a series of submultiples of the retardation; and by counting the bands between two of the fixed lines, whose wave-lengths have been determined by Fraunhofer, the wave-lengths corresponding to all the bands may be calculated; and as there are a great number of bands, the wave-lengths become known at a great many different points.

In this way the wave-lengths of the colours compared may be ascertained, and the results obtained by one observer rendered comparable with those obtained by another, with different apparatus. A portable apparatus, similar to one exhibited to the British Association in 1856, is now being constructed in order to obtain observations made by eyes of different qualities, especially those whose vision is dichromic.

POSTSCRIPT.

Account of Experiments on the Spectrum as seen by the Colour-blind.

The instrument used in these observations was similar to that already described. By reflecting the light back through the prisms by means of a concave mirror, the instrument is rendered much shorter and more portable, while the definition of the spectrum is rather improved. The experiments were made by two colour-blind observers, one of whom, however, did not obtain sunlight at the time of observation. The other obtained results, both with cloud-light and sun-light, in the way already described. It appears from these observations—

I. That any two colours of the spectrum, on opposite sides of the line "F," may be combined in such proportions as to form white.

II. That all the colours on the more refrangible side of F appear to the colour-blind "blue," and all those on the less refrangible side appear to them of another colour, which they generally speak of as "yellow," though the green at E appears to them as good a representative of that colour as any other part of the spectrum.

III. That the parts of the spectrum from A to E differ only in intensity, and not in colour; the light being too faint for good experiments between A and D, but not distinguishable in colour from E reduced to the same intensity. The *maximum* is about $\frac{2}{3}$ from D towards E.

IV. Between E and F the colour appears to vary from the pure "yellow" of E to a "neutral tint" near F, which cannot be distinguished from white when looked at steadily.

V. At F the blue and the "yellow" element of colour are in equilibrium, and at this part of the spectrum the same blindness of the central spot of the eye is found in the colour-blind that has been already observed in the normal eye, so that the brightness of the spectrum appears decidedly less at F than on either side of that line; and when a large portion of the retina is illuminated with the light of this part of the spectrum, the *limbus luteus* appears as a dark spot, moving with the movements of the eye. The observer has not yet been able to distinguish Haidinger's "brushes" while observing polarized light of this colour, in which they are very conspicuous to the author.

VI. Between F and a point $\frac{1}{3}$ from F towards G, the colour appears to vary from the neutral tint to pure blue, while the brightness increases, and reaches a maximum at $\frac{3}{5}$ from F towards G, and then diminishes towards the more refrangible end of the spectrum, the purity of the colour being apparently the same throughout.

VII. The theory of colour-blind vision being "*dichromic*," is confirmed by these experiments, the results of which agree with those obtained already by normal or "*trichromic*" eyes, if we suppose the "red" element of colour eliminated, and the "green" and "blue" elements left as they were, so that the "*red-making rays*," though dimly visible to the dichromic eye, excite the sensation not of red but of green, or as they call it, "yellow."

VIII. The extreme red ray of the spectrum appears to be a sufficiently good representative of the defective element in the colour-blind. When the ordinary eye receives this ray, it experiences the sensation of which the dichromic eye is incapable; and when the dichromic eye receives it, the luminous effect is probably of the same kind as that observed by Helmholtz in the ultra-violet part of the spectrum—a sensibility to light, without much appreciation of colour.

A set of observations of coloured papers by the same dichromic observer was then compared with a set of observations of the same papers by the author, and it was found—

1. That the colour-blind observations were consistent among themselves, on the hypothesis of *two* elements of colour.

2. That the colour-blind observations were consistent with the author's observations, on the hypothesis that the two elements of colour in dichromic vision are identical with two of the three elements of colour in normal vision.

3. That the element of colour, by which the two types of vision differ, is a red, whose relations to vermilion, ultramarine, and emerald-green are expressed by the equation

$$D = 1{\cdot}198V + 0{\cdot}078U - 0{\cdot}276G,$$

where D is the defective element, and V, U and G the three colours named above.

April 26.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communication was read:—

"Note on Regelation." By Michael Faraday, D.C.L., F.R.S. &c.

The philosophy of the phenomenon now understood by the word Regelation is exceedingly interesting, not only because of its relation to glacial action under natural circumstances, as shown by Tyndall and others, but also, and as I think especially, in its bearings upon molecular action; and this is shown, not merely by the desire of different philosophers to assign the true physical principle of action, but also by the great differences between the views which they have taken.

Two pieces of thawing ice, if put together, adhere and become one; at a place where liquefaction was proceeding, congelation suddenly occurs. The effect will take place in air, or in water, or in vacuo. It will occur at everypoint where the two pieces of ice touch; but not with ice below the freezing-point, *i. e.* with dry ice, or ice so cold as to be everywhere in the solid state.

Three different views are taken of the nature of this phenomenon. When first observed in 1850, I explained it by supposing that a particle of water, which could retain the liquid state whilst touching ice only on one side, could not retain the liquid state if it were touched by ice on both sides; but became solid, the general temperature remaining the same*. Professor J. Thomson, who discovered that pressure lowered the freezing-point of water†, attributed the regelation to the fact that two pieces of ice could not be made to bear on each other without pressure; and that the pressure, however slight, would cause fusion at the place where the particles touched, accompanied by relief of the pressure and resolidification of the water at the place of contact, in the manner that he has fully explained in a recent communication to the Royal Society‡. Professor Forbes assents to neither of these views; but admitting Person's idea of the gradual liquefaction of ice, and assuming that ice is essentially colder than ice-cold water, *i. e.* the water in contact with it, he concludes that two wet pieces of ice will have the water between them frozen at the place where they come into contact§.

Though some might think that Professor Thomson, in his last communication, was trusting to changes of pressure and temperature so inappreciably small as to be not merely imperceptible, but also ineffectual, still he carried his conditions with him into all the cases he referred to, even though some of his assumed pressures were due to capillary attraction, or to the consequent pressure of the atmosphere, only. It seemed to me that experiment might be so applied as to advance the investigation of this beautiful point in molecular philosophy to a further degree than has yet been done; even to the extent of exhausting the power of some of the principles assumed in one or more of the three views adopted, and so render our knowledge a little more defined and exact than it is at present.

In order to exclude all pressure of the particles of ice on each other due to capillary attraction or the atmosphere, I prepared to experiment altogether under water; and for this purpose arranged a bath of that fluid at 32° F. A pail, surrounded by dry flannel, was placed in a box; a glass jar, 10 inches deep and 7 inches wide, was placed on a low tripod in the pail; broken ice was packed between the jar and the pail; the jar was filled with ice-cold water to within an inch of the top; a glass dish filled with ice was employed as a cover to it, and the whole enveloped with dry flannel. In this way the central jar, with its contents, could be retained at the unchanging temperature of 32° F. for a week or more; for a small piece of ice floating in it for that time was not entirely melted away. All that was required to keep the arrangement at the fixed temperature, was to renew the packing ice in the pail from time to time, and also

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[*] Researches in Chemistry and Physics, 8vo. pp. 373, 378.

[†] Mousson says that a pressure of 13,000 atmospheres lowers the temperature of freezing from 0° to −18° Cent.

[‡] Phil. Mag. vol. xix. p. 391.

[§] Proceedings of the Royal Society of Edinburgh, April 19, 1858.

that in the basin cover. A very slow thawing process was going on in the jar the whole time, as was evident by the state of the indicating piece of ice there present.

Pieces of good Wenham-lake ice were prepared, some being blocks three inches square, and nearly an inch thick, others square prisms four or five inches long: the blocks had each a hole made through them with a hot wire near one corner; woollen thread passed through these holes formed loops, which being attached to pieces of lead, enabled me to sink the ice entirely under the surface of the ice-cold water. Each piece was thus moored to a particular place, and, because of its buoyancy, assumed a position of stability. The threads were about 1½ inch long, so that a piece of ice, when depressed sideways and then left to itself, rose in the water as far as it could, and into its stable position, with considerable force. When, also, a piece was turned round on its loop as a vertical axis, the torsion force tended to make it return in the reverse direction.

Two similar blocks of ice were placed in the water with their opposed faces about two inches apart; they could be moved into any desired position by the use of slender rods of wood, without any change of temperature in the water. If brought near to each other and then left unrestrained, they separated, returning to their first position with considerable force. If brought into the slightest contact, regelation ensued, the blocks adhered, and remained adherent notwithstanding the force tending to pull them apart. They would continue thus, even for twenty-four hours or more, until they were purposely separated, and would appear (by many trials) to have the adhesion increased at the points where they first touched, though at other parts of the contiguous surfaces a feeble thawing and dissecting action went on. In this case, except for the first moment and in a very minute degree, there was no pressure either from capillary action or any other cause. On the contrary, a tensile force of considerable amount was tending all the time to separate the pieces of ice at their points of adhesion; where still, I believe, the adhesion went on increasing—a belief that will be fully confirmed hereafter.

Being desirous of knowing whether anything like soft adhesion occurred, such as would allow slow change of position without separation during the action of the tensile force, I made the following arrangements. The blocks of ice being moored by the threads fastened to the lowest corners, stood in the water with one of the diagonals of the large surfaces vertical; before the faces were brought into contact, each block was rotated 45° about a horizontal axis, in opposite directions, so that when put together, they made a compound block, with horizontal upper edges, each half of which tended to be twisted upon, and torn from the other. Yet by placing indicators in holes previously made in the edges of the ice, I could not find that there was the slightest motion of the blocks in relation to each other in the thirty-six hours during which the experiment was continued. This result, as far as it goes, is against the necessity of pressure to regelation, or the existence of any condition like that of softness or a shifting contact; and yet I shall be able to show that there is either soft adhesion or an equivalent for it, and from that state draw still further cause against the necessity of pressure to regelation.

Torsion force was then employed as an antagonist to regelation. The ice-blocks, being separate, were adjusted in the water so as to be parallel to each other, and about 1½ inch apart. If made to approach each other on one side, by revolution in opposite directions on vertical axes, a piece of paper being between to prevent ice contact, the torsion force set up caused them to separate when left to themselves; but if the paper were away and the ice pieces were brought into contact, by however slight a force, they became one, forming a rigid piece of ice, though the strength was, of course, very small, the point of adhesion and solidification being simply the contact of two convex surfaces of small radius. By giving a little motion to the pail, or by moving either piece of ice gently in the water with a slip of wood, it was easy to see that the two pieces were rigidly attached to each other; and it was also found that, allowing time, there was no more tendency to a changing shape here than in the case quoted above. If now the slip of wood were introduced between the adhering pieces of ice, and applied so as to aid the torsion force of one of the loops, *i. e.* to increase the separating force, but unequally as respects the two pieces, then the congelation at the point of contact would give way, and the pieces of ice would move in relation to each other. Yet they would not separate; the piece unrestrained by the stick would not move off by the torsion of its own thread, though, if the stick were withdrawn, it would move back into its first attached position, pulling the second piece with it; and the two would resume their first associated form, though all the while the torsion of both loops was tending to make the pieces separate.

If when the wood was applied to change the mutual position of the two pieces of ice, without separating them, it were retained for a second undisturbed, then the two pieces of ice became fixed rigidly to each other in their new position, and maintained it when the wood was removed, but under a state of restraint; and when sufficient force was applied, by a slight tap of the wood on the ice to break up the rigidity, the two pieces of ice would rearrange themselves under the torsion force of their respective threads, yet remain united; and, assuming a new position, would, in a second or less, again become rigid, and remain inflexibly conjoined as before.

By managing the continuous motion of one piece of ice, it could be kept associated with the other by a flexible point of attachment for any length of time, could be placed in various angular positions to it, could be made (by retaining it quiescent for a moment) to assume and hold permanently any of these positions when the external force was removed, could be changed from that position into a new one, and, within certain limits, could be made to possess at pleasure, and for any length of time, either a flexible or a rigid attachment to its associated block of ice.

So regelation includes a flexible adhesion of the particles of ice, and also a rigid adhesion. The transition between these two states takes place when there is no external force like pressure tending to bring the particles of ice together, but, on the contrary, a force of torsion is tending to separate them; and, if respect be had to the mere point of contact on the two rounded surfaces where the flexible adhesion is exercised, the force which tends to separate them may be esteemed very great. The act of regelation cannot be considered as complete until the junction has become rigid; and therefore I think that the necessity of pressure for it is altogether excluded. No external pressure can remain (under the circumstances) after the first rigid contact is broken. All the forces which remain tend to separate the pieces of ice; yet the first flexible adhesions and all the successive rigid adhesions which are made to occur, are as much effects of regelation as those which occur under the greatest pressure.

The phenomenon of flexible adhesion under tension looks very much like sticking and tenacity; and I think it probable that Professor Forbes will see in it evidence of the truth of his view. I cannot, however, consider the fact as bearing such an interpretation; because I think it impossible to keep a mixture of snow and water for hours and days together without the temperature of the mixed mass becoming uniform; which uniformity would be fatal to the explanation. My idea of the flexible and rigid adhesion is this:—Two convex surfaces of ice come together; the particles of water nearest to the place of contact, and therefore within the efficient sphere of action of those particles of ice which are on both sides of them, solidify; if the condition of things be left for a moment, that the heat evolved by the solidification may be conducted away and dispersed, more particles will solidify, and ultimately enough to form a fixed and rigid junction, which will remain until a force sufficiently great to break through it is applied. But if the direction of the force resorted to can be relieved by any hinge-like motion at the point of contact, then I think that the union is broken up among the particles on the opening side of the angle, whilst the particles on the closing side come within the effectual regelation distance; regelation ensues there and the adhesion is maintained, though in an apparently flexible state. The flexibility appears to me to be due to a series of ruptures on one side of the centre of contact, and of adhesion on the other,—the regelation, which is dependent on the vicinity of the ice surfaces, being transferred as the place of efficient vicinity is changed. That the substance we are considering is as brittle as ice, does not make any difficulty to me in respect of the flexible adhesion; for if we suppose that the point of contact exists only at one particle, still the angular motion at that point must bring a second particle into contact (to suffer regelation) before separation could occur at the first; or if, as seems proved by the supervention of the rigid adhesion upon the flexible state, many particles are concerned at once, it is not possible that all these should be broken through by a force applied on one side of the place of adhesion, before particles on the opposite side should have the opportunity of regelation, and so of continuing the adhesion.

It is not necessary for the observation of these phenomena that a carefully-arranged water-vessel should be employed. The difference between the flexible and rigid adhesion may be examined very well in air. For this purpose, two of the bars of ice before spoken of, may be hung up horizontally by threads, which may be adjusted to give by torsion any separating force desired; and when the ends of these bars are brought together, the adhesion of the ice, and the ability of placing these bars at any angle, and causing them to preserve that angle by the rigid adhesion due to regelation, will be rendered evident; and though the flexible adhesion of the ice cannot in this way be examined alone, because of the capillary attraction due to the film of water on the ice, yet that is easily obviated by plunging the pieces into a dish of water at common temperatures, so that they are entirely under the surface, and repeating the observations there. All the important points regarding the flexible and rigid junction of ice due to regelation, can in this way be readily investigated.

It will be understood that, in observing the flexible and rigid state of union, convex surfaces of contact are necessary, so that the contact may be only at one point. If there be several places of contact, apparent rigidity is given to the united mass, though each of the places of contact might be in a flexible and, so to say, adhesive condition. It is not at all difficult to arrange a convex surface so that, bearing at two places only on the sides of a depression, it should form a flexible joint in one direction, and a rigid attachment in a direction transverse to the former.

It might seem at first sight as if the flexible adhesion of the ice gave us a point to start from in the further investigation of the principle of pressure. If the application of pressure causes ice to freeze together, the application of tension might be expected to produce the contrary effect, and so cause liquidity and separation at the flexible joint. This, however, does not necessarily follow; nor do I intend to consider what might be supposed to take place whilst theoretically contemplating that case. I think the changes of temperature and pressure are too infinitesimal to go for anything; and in illustration of this, will describe the following experiment. Wool is known to adhere to ice in the manner, as I believe, of regelation. Some woollen thread was boiled in distilled water, so as thoroughly to wet it. Some clean ice was broken up small and mixed with water, so as to produce a soft mass, and, being put into a glass jar clothed in flannel that it might keep for some hours, had a linear depression made in the surface, so as to form a little ice-ditch filled with water; in this depression some filaments of the wetted wool were placed, which, sinking to the bottom, rested on the ice only with the weight which they would have being immersed in water; yet in the course of two hours these filaments were frozen to the ice. In another case, a small loose ball of the same boiled wool, about half an inch in diameter, was put on to a clean piece of ice; that into a glass basin; and the whole wrapped up in flannel and left for twelve hours. At the end of that time it was found that thawing had been going on, and that the wool had melted a hole in the ice, by the heat conducted through it to the ice from the air. The hole was filled with the water and wool, but at the bottom some fibres of the wool were frozen to the ice.

Is this remarkable property peculiar to water, or is it general to all bodies? In respect of water it certainly seems to offer us a glimpse into the joint physical action of many particles, and into the nature of cohesion in that body when it is changing between the solid and liquid state. I made some experiments on this point. Bismuth was melted and kept at a temperature at which both solid and liquid metal could be present; then rods of bismuth were introduced, but when they had acquired the temperature of the mixed mass, no adhesion could be observed between them. By stirring the metal with wood, it was easy to break up the solid part into small crystalline granules; but when these were pressed together by wood under the surface, there was not the slightest tendency to cohere, as hail or snow would cohere in water. The same negative result was obtained with the metals tin and lead. Melted nitre appeared at times to show traces of the power; but, on the whole, I incline to think the effects observed resulted from the circumstance that the solid rods experimented with had not acquired throughout the fusing temperature. Nitre is a body which, like water, expands in solidifying; and it may possess a certain degree of this peculiar power.

Glacial acetic acid is not merely without regelating force, but actually presents a contrast to it. A bottle containing five or six ounces, which had remained liquid for many months, was at such a temperature that being stirred briskly with a glass rod crystals began to form in it; these went on increasing in size and quantity for eight or ten hours. Yet all that time there was not the slightest trace of adhesion amongst them, even when they were pressed together; and as they came to the surface, the liquid portion tended to withdraw from the faces of the crystals; as if there were a disinclination of the liquid and solid parts to adhere together.

Many salts were tried (without much or any expectation),—crystals of them being brought to bear against each other by torsion force, in their saturated solutions at common temperatures. In this way the following bodies were experimented with:—Nitrates of lead, potassa, soda; sulphates of soda, magnesia, copper, zinc; alum; borax; chloride of ammonium; ferro-prussiate of potassa; carbonate of soda; acetate of lead; and tartrate of potassa and soda; but the results with all were negative.

My present conclusion therefore is that the property is special for water; and that the view I have taken of its physical cause does not appear to be less likely now than at the beginning of this short investigation, and therefore has not sunk in value among the three explanations given.

Dr. Tyndall added to one of his papers*, a note of mine "On ice of irregular fusibility" indicating a cause for the difference observed in this respect in different parts of the same piece of ice. The view there taken was strongly confirmed by the effects which occurred in the jar of water at constant temperature described in the beginning of the preceding pages, where, though a thawing process was set up, it was so slow as not to dissolve a cubic inch of ice in six or seven days. The blocks retained entirely under water for several days,

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* Philosophical Transactions, 1858, p. 228.

became so dissected at the surfaces as to develope the mechanical composition of the masses, and to show that they were composed of parallel layers about the tenth of an inch thick, of greater and lesser fusibility, which layers appear, from other modes of examination, to have been horizontal in the ice whilst in the act of formation. They had no relation to the position of the blocks in the water of my experiments, or to the direction of gravity, but had a fixed position in relation to each piece of ice.

ADDENDUM.

The following method of examining the regelation phenomena above described may be acceptable. Take a rather large dish of water at common temperatures. Prepare some flat cakes or bars of ice, from half an inch to an inch thick; render the edges round, and the upper surface of each piece convex, by holding it against the inside of a warm saucepan cover, or in any other way. When two of these pieces are put into the water they will float, having perfect freedom of motion, and yet only the central part of the upper surface will be above the fluid; when, therefore, the pieces touch at their edges, the width of the water-surface above the place of contact may be two, three, or four inches, and thus the effect of capillary action be entirely removed. By placing a plate of clean dry wax or spermaceti upon the top of a plate of ice, the latter may be entirely submerged, and the tendency to approximation from capillary action converted into a force of separation. When two or more of such floating pieces of ice are brought together by contact at some point under the water, they adhere; first with an apparently flexible, and then with a rigid adhesion. When five or six pieces are grouped in a contorted shape, as an S, and one end piece be moved carefully, all will move with i rigidly; or, if the force be enough to break through the joint, the rupture will be with a crackling noise, but the pieces will still adhere, and in an instant become rigid again. As the adhesion is only by points, the force applied should not be either too powerful or in the manner of a blow. I find a piece of paper, a small feather, or a camel-hair brush applied under the water very convenient for the purpose. When the point of a floating wedge-shaped piece of ice is brought under water against the corner or side of another floating piece, it sticks to it like a leech; if, after a moment, a paper edge be brought down upon the place, a very sensible resistance to the rupture at that place is felt. If the ice be replaced by like rounded pieces of wood or glass, touching under water, nothing of this kind occurs, nor any signs of an effect that could by possibility be referred to capillary action; and finally, if two floating pieces of ice have separating forces attached to them, as by threads connecting them and two light pendulums, pulled more or less in opposite directions, then it will be seen with what power the ice is held together at the place of regelation, when the contact there is either in the flexible or rigid condition, by the velocity and force with which the two pieces will separate when the adhesion is properly and entirely overcome.

GEOLOGICAL SOCIETY.

[Continued from vol. xx. p. 486.]

November 21, 1860.—L. Horner, Esq., President, in the Chair.

The following communication was read:—

"On the Geology of Bolivia and Southern Peru." By D. Forbes, Esq., F.R.S., F.G.S. With Notes on the Fossils by Prof. Huxley, F.R.S., Sec. G.S., and J. W. Salter, Esq., F.G.S.

After some observations on the previous researches by others, and on the general features of the region, the author proceeded to describe the Post-tertiary formations of the maritime district. These beds, containing existing species of shells, occur at various heights up to 40 feet above the sea-level. Guano deposits are frequent along the coast, and deposits of salt also in raised beaches a little above the sea. The author could not verify Lieut. Freyer's statement of *Balani* and *Milleporæ* being attached high up the side of the Morro de Arica, a perpendicular cliff at the water's edge; indeed, from the state of old Indian tumuli along the beach, and other circumstances, the author believes that no perceptible elevation has here taken place since the Spanish Conquest, although such an alteration of level has occurred in Chile. The sand-dunes of the coast, and their great mobility during the hot season, were noticed. From Mexillones to Arica the coast is steep and rugged, formed of a chain of mountains, 3000 feet high, consisting of rocks of the Upper Oolitic age. At Arica the high land recedes, leaving a wide plain formed of the débris of the neighbouring mountains; and in the middle of this area was observed stratified volcanic tuff contemporaneous with the formation of the gravel.

The saline formations were next treated of as three groups, according to their height above the sea-level, and were shown to be much more extensive than generally supposed, extending over the rainless regions of this coast for more than 550 miles. They are mostly developed, however, between latitudes 19° and 25° South. These salines are supposed to have originated in the evaporation of sea-water confined in them as lagoons by the longitudinal ranges of hills separating them from the ocean. The nitrate of soda had, in the author's opinion, resulted from the chemical reactions of sea-salt, carbonate of lime, and decomposing vegetable matter (both terrestrial and marine). The borate of lime, occurring with the nitrate, is connected with the volcanic conditions of the district, and was produced by fumaroles containing boracic acid. Where the highest range of salines extend beyond the rainless region, they are much modified in the rainy season, and generally take the form of salt plains encircling salt lakes or swamps.

The great Bolivian plateau, having an average elevation of 13,000 or 14,000 feet above the sea, consists of great gravel plains formed by the spaces between the longitudinal ranges of mountains being filled up by the débris of these mountains. The most western of these consists of Oolitic débris with volcanic tuff and scoriæ; it bears the salines above-mentioned, and is nearly destitute of water. The central range of plains, formed from the disintegration of red sandstones and marls, with some volcanic scoriæ, is well watered. The third range consists of plains made up of the débris of Silurian and granitic rocks, and is auriferous. The thickness of this accumulation of clays, gravel, shingle, and boulders is, at places, immense. At La Paz it is more than 1600 feet. Contemporaneous trachytic tuff was found also in these deposits. In freshwater ponds on this plateau, at a height of 14,000 feet (lat. 15° S.), Mr. Forbes found abundance of *Cyclas Chilensis*, formerly considered to be peculiar to the most southern and coldest part of Chile at the level of the sea (lat. 45° to 50° S.).

The volcanic formations were next noticed. Volcanic action has continued certainly from the pleistocene age to the present. The line of volcanic phenomena is nearly continuous N. and S. Cones are frequent, some of them 22,000 feet high and upwards; but craters are rare. Volcanic matter, both in ancient times and at present, has in a great part been erupted from lateral vents, often of great longitudinal extent; recent trachytic lavas from such orifices have covered in some cases more than 100 miles of country. Besides trachyte, there are great tracts of trachydoleritic and felspathic lavas. On the whole, in these South American lavas silex abounds, and it has been the first element in the rock to crystallize; whereas apparently in granite quartz is the last to crystallize and form the state of so-called "surfusion." Diorites (including the so-called "Andesite") occur in force along two parallel N. and S. lines of eruption in this region, reaching through Chile, Bolivia, and Peru, for more than 40 degrees of latitude. These diorites, and more especially the rocks which they traverse, are metalliferous; and the author looks upon the greater part of the copper, silver, iron, and other metallic veins of these countries as directly occasioned by the appearance of this rock.

Shales and argillaceous limestones, with clay-stones, porphyry-tuffs, and porphyries, form the mass of the Upper Oolite formation of Bolivia, equivalent to Darwin's Cretaceo-Oolitic Series of Chile. At Cobija these are traversed in all directions by metallic veins, chiefly copper, and which, as before mentioned, appear to emanate from the diorite.

Red and variegated marls and sandstones, with gypsum and cupriferous and yellow sandstones and conglomerates, come next in order; they have a thickness of 6000 feet, and are much folded and dislocated. These are considered by the author to resemble closely the Permian rocks of Russia. Fossil wood is not uncommon in some of these strata, which extend for at least 500 miles N. and S.

Carboniferous strata occur chiefly as a small, contorted, basin-shaped series of limestones, sandstones, and shales, with abundant characteristic fossils.

The quartzites which are generally supposed to represent the Devonian formation in Bolivia, but which the author is rather disposed to group as Upper Silurian, are really not of very great thickness, but are very much folded, and perhaps are about 5000 feet thick.

The Silurian rocks (perhaps 15,000 feet thick) are well developed over an area of from 80,000 to 100,000 miles of mountain country, including the highest mountains of South America, and giving rise to the great rivers Amazon, &c. These slates, shales, grauwackes, and quartzites yield abundant fossils even up to the highest point reached, 20,000 feet. The problematical fossils known as *Cruziana* or *Bilobites* occur not only in the lower beds, but (with many other fossils) in the higher part of the series.

Lastly, the differences between the sections made by M. D'Orbigny, M. Pissis, and the author were pointed out, though for the most part difficult of explanation. D'Orbigny makes the mountain Illimani to be granite; it is slate according to the author. M. Pissis describes as carboniferous the beds in which Mr. Forbes found Silurian fossils,—and so on.

"On a New Species of *Macrauchenia* (*M. Boliviensis*)." By Prof. T. H. Huxley, F.R.S., Sec. G.S. &c.

Some bones, fully impregnated with metallic copper, which had been brought up from the mines of Corocoro in Bolivia were submitted to Prof. Huxley for examination. The mines referred to are situated on a great fault; and the bones were probably part of a carcass that had fallen in from the surface,—the copper-bearing water of the mines having mineralized them. A cervical and a lumbar vertebra, an astragalus, a scapula, and a tibia show complete correspondence in essential characters with those bones of the great *Macrauchenia Patachonica* described by Prof. Owen in the Appendix to the 'Voyage of the Beagle;' but the relative size and proportions of the vertebra, the tibia, and the astragalus indicate a distinct species, much smaller and more slender; and in some points of structure this new form (*M. Boliviensis*) approaches more nearly to the recent *Auchenidæ* than to the larger and fossil species. The fragments of the cranium show some peculiarities of form, but, on the whole, it has many resemblances to that of the Vicugna.

Prof. Huxley pointed out that this slender and small-headed *Macrauchenia* may have been the highland-contemporary of the larger *M. Patachonica*; just as now-a-days the Vicugna prefers the mountains, whilst its larger congener the Guanaco roams over the Patagonian plains.

Lastly it was remarked that as *Macrauchenia* was an animal combining, to a much more marked degree than any other known recent or fossil mammal, the peculiarities of certain artiodactyles and perissodactyles, and yet was certainly but of postpleistocene age, it presents a striking exception to the commonly asserted doctrine that "more generalized" organisms were confined to the ancient periods of the earth's history. For similar reasons, the structure of the *Macrauchenia* is also inimical to the idea that an extinct animal can always be reconstructed from a single tooth or a single bone.

"On the Palæozoic Fossils brought by Mr. D. Forbes from Bolivia." By J. W. Salter, Esq., F.G.S.

The Fossils of Carboniferous age brought home by Mr. Forbes are the well-known species described by D'Orbigny. Several are identical with European forms (as *Productus Martini*, &c.), and are cosmopolitan; others are peculiar to the district (as *Spirifer Condor*, *Orthis Andii*, &c.).

Mr. Forbes has brought a "Devonian" trilobite (*Phacops latifrons* or *Ph. Bufo*), in a rolled pebble, from Oruro: it is a widely-spread species. Another allied form was found by Mr. Pentland, many years back, at Aygatchi. In other respects the "Devonian" evidence is scanty.

In Mr. Forbes's fine collection of Silurian fossils none of D'Orbigny's ten Silurian species occur; nearly all are such as are met with in Lower Devonian and in Upper Silurian rocks—*Homalonotus, Tentaculites, Orthis, Ctenodonta, Pileopsis* (?), *Strophomena, Bellerophon.* South Africa and the Falkland Isles yield a similar fossil fauna.

The *Bilobites* in this collection differ, some of them probably generically, from D'Orbigny's figured species. A little *Beyrichia* from the upper part of the Silurian series in Bolivia appears to be like a North American form figured by Emmons as Silurian.

XXIV. *Intelligence and Miscellaneous Articles.*

ON THE POLARIZATION OF LIGHT BY DIFFUSION. BY G. GOVI.

THE polarization of atmospheric light has long since proved that gases, as well as solid and liquid bodies, have the property of polarizing light; but I am not aware that any direct experiments have been made to prove the presence of polarizing power in the case of gases.

I was led to the consideration of this question by the polariscopic study of the light of comets; and the idea occurred to me to investigate how a pencil of light would be affected by being transmitted through a certain thickness of a gaseous medium in which it was reflected or diffused.

The experiment was made in the following manner:—A thick pencil of the sun's rays, reflected from a heliostat, was allowed to pass into a dark room, through a hole in the window-shutter. This light was principally reflected from metal, and showed very feeble traces of polarization. A large quantity of smoke was then produced by burning incense; and the pencil immediately expanded and formed a large cylinder, which diffused white light in all directions. This light, when investigated by a polariscope, was found to be polarized even when the cylinder was viewed at right angles to its axis; but the intensity of the polarization was truly extraordinary when the direction of the visual ray, on the side of the source of light, formed a somewhat small angle with the axis of the cylinder: one would have said that the phenomenon was caused by the action of a solid or liquid body on the molecules of the ether. Viewing the cylinder in this direction, the polarization perceptibly decreased on approaching the source of light or removing from it. The light proceeding from the column of smoke seen by reflexion upon the aperture was only feebly polarized.

Excepting in its intensity, the above phenomenon presents nothing extraordinary; but for a physicist the circumstance appears to me important, that the light polarized by diffusion does not seem to arise from a simple reflexion from gas molecules, for its plane of polarization is at right angles to the plane in which the reflexion ought to occur. For on examining the cylinder of light round its axis in the direction of the maxima of polarization, it was found that the light proceeding from it was polarized tangential to that point of the surface of the cylinder towards which the polariscope was directed. Whether the plane of polarization becomes removed by its repeated reflexions from the gas molecules, or whether the action of gases under certain circumstances is analogous to that of refracting bodies, are questions which hitherto I have not been able to decide by experiment.

I endeavoured to depolarize the light completely on its entry into the dark room, by allowing it to pass through a thin sheet of white paper; but the phenomena, with the exception of the intensity of the light, were quite the same.

Light polarized by reflexion from a black glass experienced no perceptible change by the action of the smoke, and its plane of polarization always retained its original direction.

It is possible, by suitably regulating the incident quantity of polarized light, to succeed in finding a limit to the action of the gas molecules, beyond which the original polarization of the pencil preponderates over the molecular forces of the medium which the light has to penetrate.

The relations which these facts may possibly bear to the phenomena of atmospheric polarization, and perhaps also to fluorescence and the peculiar colour of bodies, have induced me to publish these observations, spite of their incompleteness.

Some days after the preceding experiments had been laid before the Academy, I repeated them with more sensitive polariscopes, and I found exactly the same facts; I can further state that the plane of polarization of diffused light suddenly rotated 90°, on passing the direction in which I had seen all trace of polarization disappear in my previous experiments.

Thus on receiving in the polariscope the rays emanating from the luminous track produced by the passage of the sun's light, or of the electric light, through the smoke of incense, it is found that under a small inclination (the angles being measured from the luminous source) the polarization of diffused light is already very perceptible; that it increases up to a certain angle, which is the maximum; it then decreases, and at the normal it is almost nil. Up to this point the plane of polarization is perpendicular to the plane which passes through the source of light, the place observed, and the eye or the polariscope. Above 90°, the polarization, although very feeble, reappears, but its plane is then perpendicular to the first plane. Still further it diminishes very rapidly, and the diffused light soon shows no sensible traces of polarized rays.

I have investigated the smoke of tobacco in the same way; and the results were the same, though the angle at which I found the neutral point and the reversal of the plane of polarization was perhaps a little less than with the smoke of incense.

It is possible that the nature of the diffused particles has an appreciable influence on these phenomena, and that different gases (if gases do diffuse light), vapours, and powders may in this manner be distinguished. I propose to undertake a series of experiments from this point of view, the results of which I shall lay before the Academy.—*Comptes Rendus*, Sept. 3, and Oct. 29, 1860.

ON ELECTRIC ENDOSMOSE. BY M. C. MATTEUCCI.

Having recently had occasion to examine into the construction and operation of the galvanic batteries used in our telegraph offices, I have been led to make certain original experiments on the subject of electric endosmose, a short description of which, as they seem to throw some light on the true nature of the phenomenon in question, I beg to lay before the Academy. MM. Porret and Becquerel were the first who called attention to the fact that a liquid mass, separated into two compartments by a porous diaphragm, and traversed by an electric current, appears to be transported in the direction of that current; that is to say, the level of the liquid is lowered in the compartment that contains the positive pole, and raised in that which contains the negative pole. The determination of the law of this phenomenon is due to M. Wiedemann, who proved that the quantity of water transported is directly proportional to the intensity of the current and the electric resistance of the liquid. M. Wiedemann seems to have regarded this mechanical effect of the current as a different phenomenon from its electrolytic action; while other physicists have considered that the transportation of the liquid was only a secondary effect of electrolysis. I should mention also that MM. Van Breda and Logemann have in vain endeavoured to ascertain whether, in the absence of a diaphragm, there is any displacement of the electrolysed liquid, or whether a very light moveable diaphragm is itself displaced in the direction of the current. Theoretical considerations, which easily suggest themselves to the mind, and which I need not here specify, founded on the equality of the electrolytic effects, whether endosmose be produced or not, give probability to the conclusion that these phenomena are caused by some secondary action of electrolysis. The following experiments seem to show that this supposition is correct.

I divided a rectangular vessel of varnished wood into six compartments, by means of diaphragms of porous porcelain. All these compartments were filled to the same height with well-water, the level of which was indicated by a line of white varnish. A platinum plate of the same size as the diaphragms was placed in each of the end compartments, Through this apparatus I caused a current to pass, produced sometimes by 10, sometimes by 15, and sometimes by 20 cells of a Grove's battery. The endosmose became apparent after the current had lasted for some hours, and in every case the first effect produced was as follows:—The level of the liquid was raised in the compartment that contained the negative electrode, and was lowered in the compartment next to it, while it was lowered in the compartment that contained the positive electrode (though to a less degree than it was elevated in the compartment at the opposite extremity), and raised in the adjoining compartment. These effects were invariably produced, notwithstanding the change of the diaphragm and the reversal of the position of the vessel with respect to the electrodes. Floats may be put in all the compartments, except those containing the platinum plates, in which the liquid is too much agitated by gaseous bubbles due to electrolysation; and on viewing these floats with a glass, the displacements I have described become sensible much earlier. In the intermediate compartments the liquid remains stationary for several hours; but after a certain time the liquid begins to rise in the compartments towards the positive pole, and to fall in those towards the negative pole. I shall mention but one precaution which must not be neglected in these experiments, namely, that the diaphragms must be as equal as possible.

In a second series of experiments I closed one end of each of two glass tubes with a porcelain diaphragm fixed with mastic. Each of these tubes was then placed in a glass vessel, and both vessels and tubes were filled to the same height with well-water. The same current passed through both tubes, in each case passing from the water in the vessel to that in the tube, the only difference being in the position of the platinum electrodes, which in the one case were very near the diaphragm, while in the other they were placed at the greatest possible distance from it. Under these circumstances I invariably found that the electric endosmose made its appearance much sooner, and with much greater intensity in the first case than in the second.

I shall not stop to discuss the consequences of these experiments, since they appear to me to be obvious, and to prove that the phenomenon in question is no other than that mentioned above, that is to say, a case of endosmose produced by changes in the composition of the liquid in contact with the two electrodes. I should mention here that the liquid round the positive electrode always acquires an acid reaction, while that round the negative electrode becomes alkaline, and that these effects are produced even when distilled water is employed. I did not content myself with the ancient experiments of Dutrochet, which prove that there is a current of endosmose from an acid liquid to water, from water to an alkaline liquid, and from an acid to an alkaline liquid. I repeated the experiment with the two liquids which had been in contact with the electrodes as described above, sometimes making use of both of the liquids, sometimes testing each of them separately with pure water. I invariably found that there was endosmose from the liquid that had been in contact with the positive electrode to pure water, and from pure water to the liquid that had been in contact with the negative electrode. It appears therefore that the conditions for the production of ordinary endosmose are undoubtedly present in the phenomenon called electric endosmose. I should, however, observe that the amount of displacement by endosmose is much less when the liquids which have been in contact with the electrodes are experimented on simply without any electric current, and that it is hardly perceptible in the case of electrolysed distilled water. Without attempting to explain all the phenomena of electric endosmose, it seems natural to suppose that the presence of electricity, and the peculiar state in which the elements of electrolysation are produced, give to these products properties which influence the effect of endosmose, and which cease with the cessation of the current.—*Comptes Rendus*, Dec. 1860.

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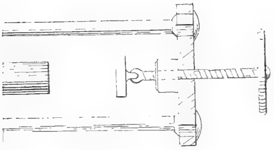

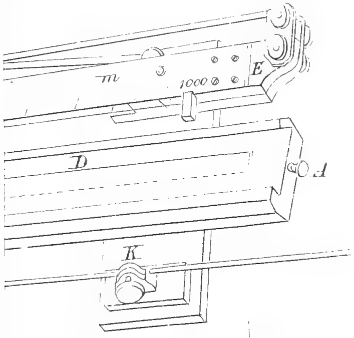

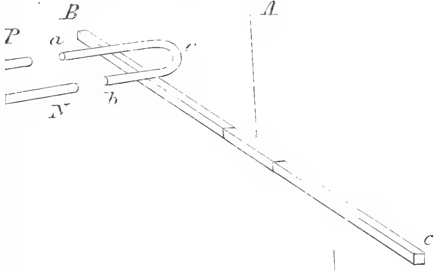

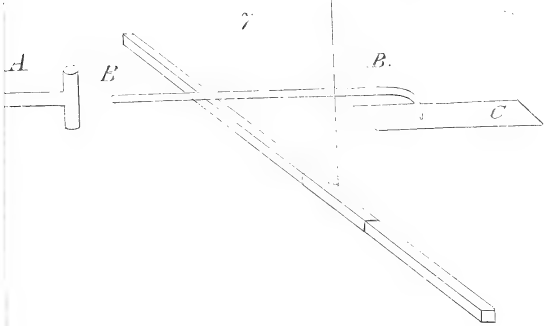

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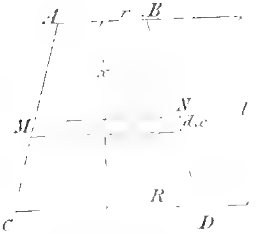

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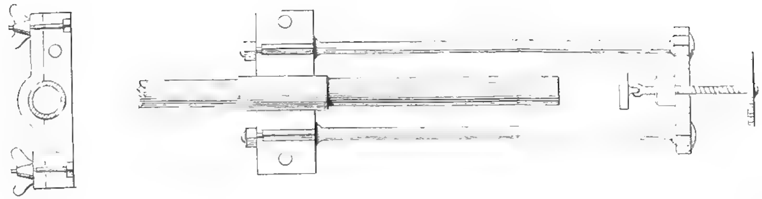

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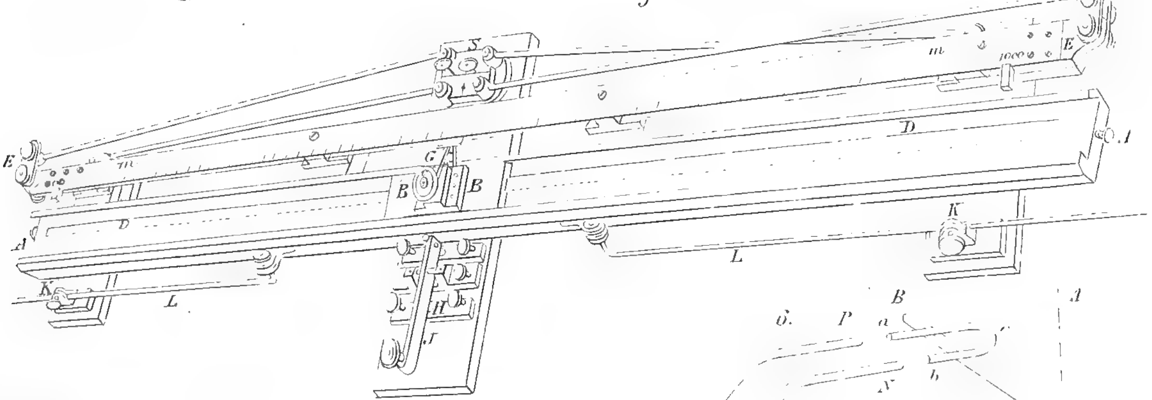

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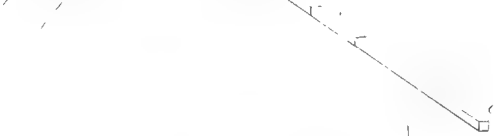

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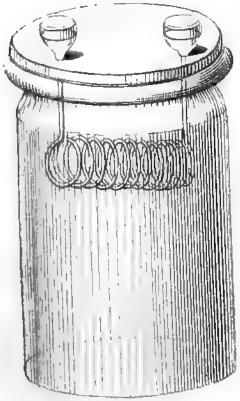

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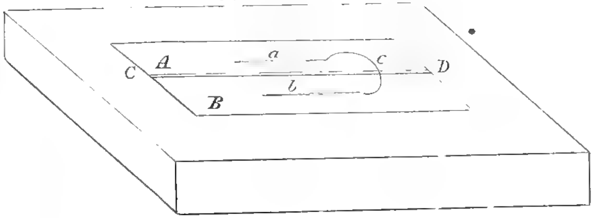

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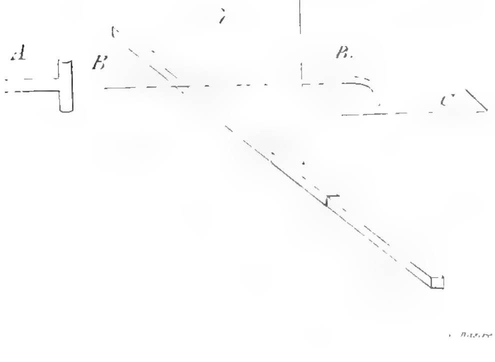

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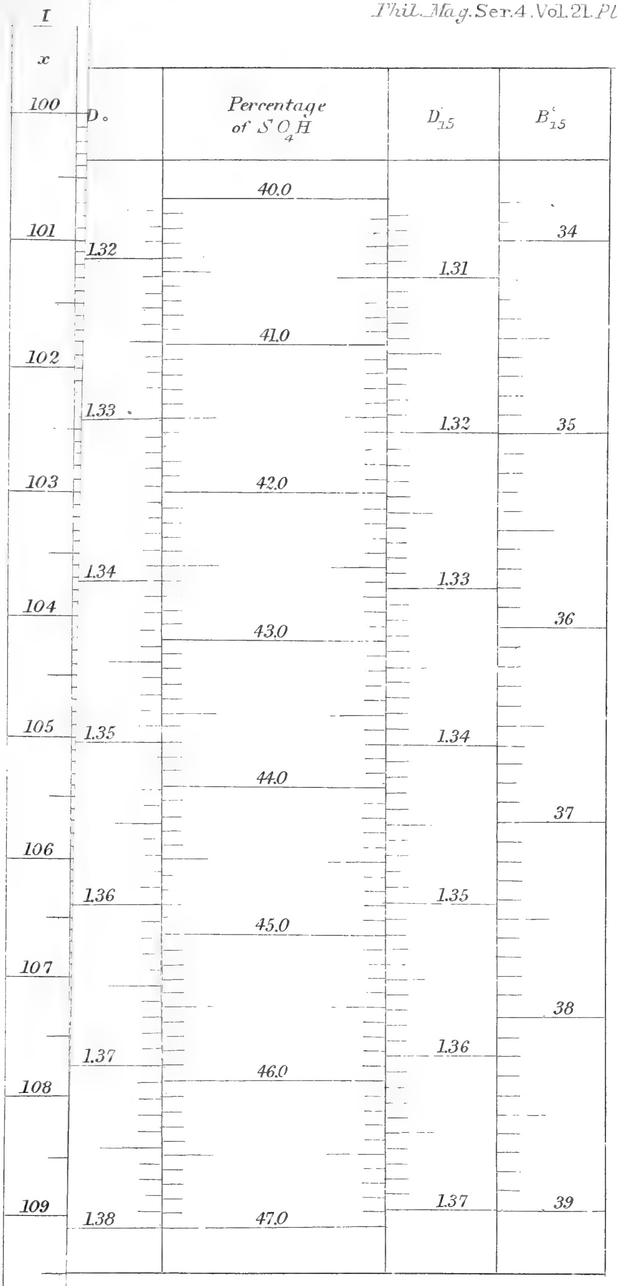

Table giving the density at 0° C and at 15° C, the degrees Beaumé and the corresponding Strength of Aqueous Sulphuric Acid.

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Fig. 1

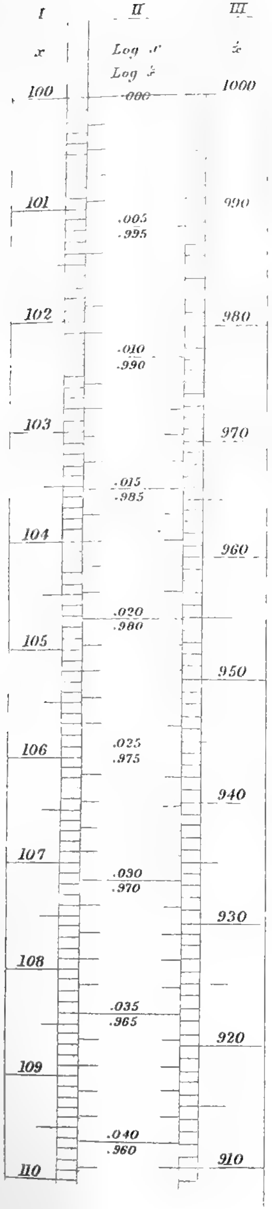

Fig. 2

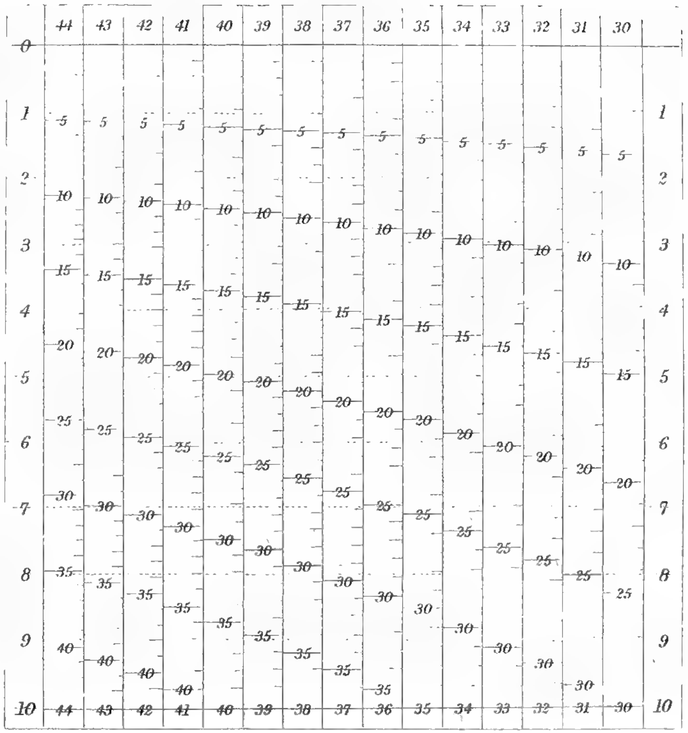

Fig. 3

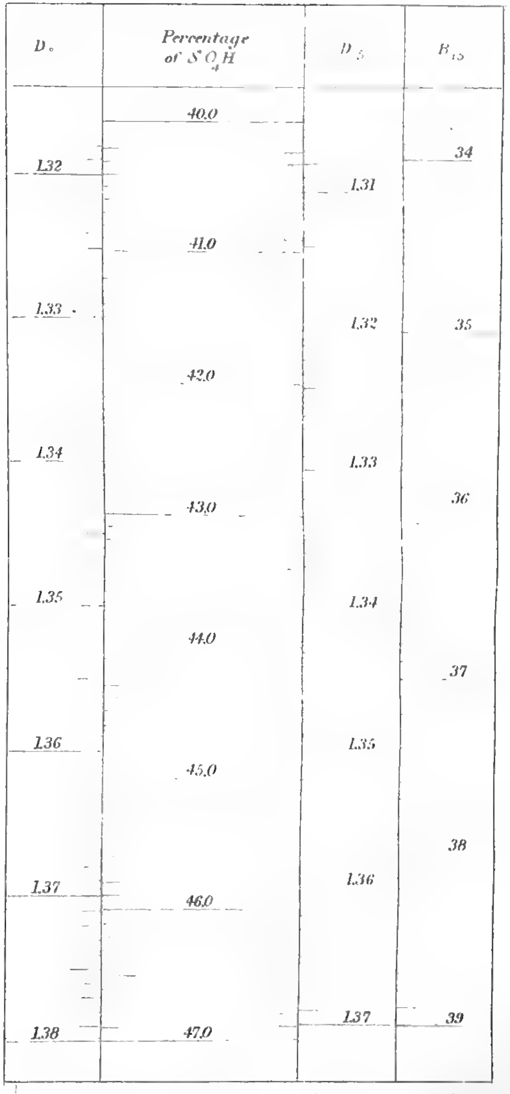

Table giving the density at 0° C and at 15° C, the degrees Beaumé and the corresponding Strength of Aqueous Sulphuric Acid.

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[FOURTH SERIES.]

MARCH 1861.

XXV. *On Physical Lines of Force.* *By* J. C. Maxwell, *Professor of Natural Philosophy in King's College, London*[*].

Part I.—*The Theory of Molecular Vortices applied to Magnetic Phenomena.*

In all phenomena involving attractions or repulsions, or any forces depending on the relative position of bodies, we have to determine the *magnitude* and *direction* of the force which would act on a given body, if placed in a given position.

In the case of a body acted on by the gravitation of a sphere, this force is inversely as the square of the distance, and in a straight line to the centre of the sphere. In the case of two attracting spheres, or of a body not spherical, the magnitude and direction of the force vary according to more complicated laws. In electric and magnetic phenomena, the magnitude and direction of the resultant force at any point is the main subject of investigation. Suppose that the direction of the force at any point is known, then, if we draw a line so that in every part of its course it coincides in direction with the force at that point, this line may be called a *line of force*, since it indicates the direction of the force in every part of its course.

By drawing a sufficient number of lines of force, we may indicate the direction of the force in every part of the space in which it acts.

Thus if we strew iron filings on paper near a magnet, each filing will be magnetized by induction, and the consecutive filings will unite by their opposite poles, so as to form fibres, and these fibres will *indicate* the direction of the lines of force. The beautiful illustration of the presence of magnetic force afforded by this experiment, naturally tends to make us think of

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[*] Communicated by the Author.

the lines of force as something real, and as indicating something more than the mere resultant of two forces, whose seat of action is at a distance, and which do not exist there at all until a magnet is placed in that part of the field. We are dissatisfied with the explanation founded on the hypothesis of attractive and repellent forces directed towards the magnetic poles, even though we may have satisfied ourselves that the phenomenon is in strict accordance with that hypothesis, and we cannot help thinking that in every place where we find these lines of force, some physical state or action must exist in sufficient energy to produce the actual phenomena.

My object in this paper is to clear the way for speculation in this direction, by investigating the mechanical results of certain states of tension and motion in a medium, and comparing these with the observed phenomena of magnetism and electricity. By pointing out the mechanical consequences of such hypotheses, I hope to be of some use to those who consider the phenomena as due to the action of a medium, but are in doubt as to the relation of this hypothesis to the experimental laws already established, which have generally been expressed in the language of other hypotheses.

I have in a former paper* endeavoured to lay before the mind of the geometer a clear conception of the relation of the lines of force to the space in which they are traced. By making use of the conception of currents in a fluid, I showed how to draw lines of force, which should indicate by their number the amount of force, so that each line may be called a unit-line of force (see Faraday's 'Researches,' 3122); and I have investigated the path of the lines where they pass from one medium to another.

In the same paper I have found the geometrical significance of the "Electrotonic State," and have shown how to deduce the mathematical relations between the electrotonic state, magnetism, electric currents, and the electromotive force, using mechanical illustrations to assist the imagination, but not to account for the phenomena.

I propose now to examine magnetic phenomena from a mechanical point of view, and to determine what tensions in, or motions of, a medium are capable of producing the mechanical phenomena observed. If, by the same hypothesis, we can connect the phenomena of magnetic attraction with electromagnetic phenomena and with those of induced currents, we shall have found a theory which, if not true, can only be proved to be erroneous by experiments which will greatly enlarge our knowledge of this part of physics.

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* See a paper "On Faraday's Lines of Force," Cambridge Philosophical Transactions, vol. x. part 1.

The mechanical conditions of a medium under magnetic influence have been variously conceived of, as currents, undulations, or states of displacement or strain, or of pressure or stress.

Currents, issuing from the north pole and entering the south pole of a magnet, or circulating round an electric current, have the advantage of representing correctly the geometrical arrangement of the lines of force, if we could account on mechanical principles for the phenomena of attraction, or for the currents themselves, or explain their continued existence.

Undulations issuing from a centre would, according to the calculations of Professor Challis, produce an effect similar to attraction in the direction of the centre; but admitting this to be true, we know that two series of undulations traversing the same space do not combine into one resultant as two attractions do, but produce an effect depending on relations of *phase* as well as intensity, and if allowed to proceed, they diverge from each other without any mutual action. In fact the mathematical laws of attractions are not analogous in any respect to those of undulations, while they have remarkable analogies with those of currents, of the conduction of heat and electricity, and of elastic bodies.

In the Cambridge and Dublin Mathematical Journal for January 1847, Professor William Thomson has given a "Mechanical Representation of Electric, Magnetic, and Galvanic Forces," by means of the displacements of the particles of an elastic solid in a state of strain. In this representation we must make the angular displacement at every point of the solid proportional to the magnetic force at the corresponding point of the magnetic field, the direction of the axis of rotation of the displacement corresponding to the direction of the magnetic force. The absolute displacement of any particle will then correspond in magnitude and direction to that which I have identified with the electrotonic state; and the relative displacement of any particle, considered with reference to the particle in its immediate neighbourhood, will correspond in magnitude and direction to the quantity of electric current passing through the corresponding point of the magneto-electric field. The author of this method of representation does not attempt to explain the origin of the observed forces by the effects due to these strains in the elastic solid, but makes use of the mathematical analogies of the two problems to assist the imagination in the study of both.

We come now to consider the magnetic influence as existing in the form of some kind of pressure or tension, or, more generally, of *stress* in the medium.

Stress is action and reaction between the consecutive parts of a body, and consists in general of pressures or tensions different in different directions at the same point of the medium.

The necessary relations among these forces have been investigated by mathematicians; and it has been shown that the most general type of a stress consists of a combination of three principal pressures or tensions, in directions at right angles to each other.

When two of the principal pressures are equal, the third becomes an axis of symmetry, either of greatest or least pressure, the pressures at right angles to this axis being all equal.

When the three principal pressures are equal, the pressure is equal in every direction, and there results a stress having no determinate axis of direction, of which we have an example in simple hydrostatic pressure.

The general type of a stress is not suitable as a representation of a magnetic force, because a line of magnetic force has direction and intensity, but has no third quality indicating any difference between the *sides* of the line, which would be analogous to that observed in the case of polarized light*.

We must therefore represent the magnetic force at a point by a stress having a single axis of greatest or least pressure, and all the pressures at right angles to this axis equal. It may be objected that it is inconsistent to represent a line of force, which is essentially dipolar, by an axis of stress, which is necessarily isotropic; but we know that *every* phenomenon of action and reaction is isotropic in its *results*, because the effects of the force on the bodies between which it acts are equal and opposite, while the nature and origin of the force may be dipolar, as in the attraction between a north and a south pole.

Let us next consider the mechanical effect of a state of stress symmetrical about an axis. We may resolve it, in all cases, into a simple hydrostatic pressure, combined with a simple pressure or tension along the axis. When the axis is that of greatest pressure, the force along the axis will be a pressure. When the axis is that of least pressure, the force along the axis will be a tension.

If we observe the lines of force between two magnets, as indicated by iron filings, we shall see that whenever the lines of force pass from one pole to another, there is *attraction* between those poles; and where the lines of force from the poles avoid each other and are dispersed into space, the poles *repel* each other, so that in both cases they are drawn in the direction of the resultant of the lines of force.

It appears therefore that the stress in the axis of a line of magnetic force is a *tension*, like that of a rope.

If we calculate the lines of force in the neighbourhood of two gravitating bodies, we shall find them the same in direction as

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* See Faraday's 'Researches,' 3252.

those near two magnetic poles of the same name; but we know that the mechanical effect is that of attraction instead of repulsion. The lines of force in this case do not run between the bodies, but avoid each other, and are dispersed over space. In order to produce the effect of attraction, the stress along the lines of gravitating force must be a *pressure*.

Let us now suppose that the phenomena of magnetism depend on the existence of a tension in the direction of the lines of force, combined with a hydrostatic pressure; or in other words, a pressure greater in the equatorial than in the axial direction: the next question is, what mechanical explanation can we give of this inequality of pressures in a fluid or mobile medium? The explanation which most readily occurs to the mind is that the excess of pressure in the equatorial direction arises from the centrifugal force of vortices or eddies in the medium having their axes in directions parallel to the lines of force.

This explanation of the cause of the inequality of pressures at once suggests the means of representing the dipolar character of the line of force. Every vortex is essentially dipolar, the two extremities of its axis being distinguished by the direction of its revolution as observed from those points.

We also know that when electricity circulates in a conductor, it produces lines of magnetic force passing through the circuit, the direction of the lines depending on the direction of the circulation. Let us suppose that the direction of revolution of our vortices is that in which vitreous electricity must revolve in order to produce lines of force whose direction within the circuit is the same as that of the given lines of force.

We shall suppose at present that all the vortices in any one part of the field are revolving in the same direction about axes nearly parallel, but that in passing from one part of the field to another, the direction of the axes, the velocity of rotation, and the density of the substance of the vortices are subject to change. We shall investigate the resultant mechanical effect upon an element of the medium, and from the mathematical expression of this resultant we shall deduce the physical character of its different component parts.

Prop. I.—If in two fluid systems geometrically similar the velocities and densities at corresponding points are proportional, then the differences of pressure at corresponding points due to the motion will vary in the duplicate ratio of the velocities and the simple ratio of the densities.

Let l be the ratio of the linear dimensions, m that of the velocities, n that of the densities, and p that of the pressures due to the motion. Then the ratio of the *masses* of corresponding portions will be l^3n, and the ratio of the velocities acquired in traversing similar parts of the systems will be m; so that l^3mn is the ratio of the momenta acquired by similar portions in traversing similar parts of their paths.

The ratio of the surfaces is l^2, that of the forces acting on them is l^2p, and that of the times during which they act is $\frac{l}{m}$; so that the ratio of the impulse of the forces is $\frac{l^3p}{m}$, and we have now

$$l^3mn=\frac{l^3p}{m},$$

or

$$m^2n=p;$$

that is, the ratio of the pressures due to the motion (p) is compounded of the ratio of the densities (n) and the duplicate ratio of the velocities (m^2), and does not depend on the linear dimensions of the moving systems.

In a circular vortex, revolving with uniform angular velocity, if the pressure at the axis is p_0, that at the circumference will be $p_1=p_0+\frac{1}{2}\rho v^2$, where ρ is the density and v the velocity at the circumference. The *mean pressure* parallel to the axis will be

$$p_0+\tfrac{1}{4}\rho v^2=p_2.$$

If a number of such vortices were placed together side by side with their axes parallel, they would form a medium in which there would be a pressure p_2 parallel to the axes, and a pressure p_1 in any perpendicular direction. If the vortices are circular, and have uniform angular velocity and density throughout, then

$$p_1-p_2=\tfrac{1}{4}\rho v^2.$$

If the vortices are not circular, and if the angular velocity and the density are not uniform, but vary according to the same law for all the vortices,

$$p_1-p_2=\mathrm{C}\rho v^2,$$

where ρ is the mean density, and C is a numerical quantity depending on the distribution of angular velocity and density in the vortex. In future we shall write $\frac{\mu}{4\pi}$ instead of $\mathrm{C}\rho$, so that

$$p_1-p_2=\frac{1}{4\pi}\mu v^2, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where μ is a quantity bearing a constant ratio to the density, and v is the linear velocity at the circumference of each vortex.

A medium of this kind, filled with molecular vortices having their axes parallel, differs from an ordinary fluid in having different pressures in different directions. If not prevented by properly arranged pressures, it would tend to expand laterally. In so doing, it would allow the diameter of each vortex to expand

and its velocity to diminish in the same proportion. In order that a medium having these inequalities of pressure in different directions should be in equilibrium, certain conditions must be fulfilled, which we must investigate.

Prop. II.—If the direction-cosines of the axes of the vortices with respect to the axes of x, y, and z be l, m, and n, to find the normal and tangential stresses on the coordinate planes.

The actual stress may be resolved into a simple hydrostatic pressure p_1 acting in all directions, and a simple tension $p_1 - p_2$, or $\frac{1}{4\pi}\mu v^2$, acting along the axis of stress.

Hence if p_{xx}, p_{yy}, and p_{zz} be the normal stresses parallel to the three axes, considered positive when they tend to increase those axes; and if p_{yz}, p_{zx}, and p_{xy} be the tangential stresses in the three coordinate planes, considered positive when they tend to increase simultaneously the symbols subscribed, then by the resolution of stresses*,

$$p_{xx} = \frac{1}{4\pi}\mu v^2 l^2 - p_1$$

$$p_{yy} = \frac{1}{4\pi}\mu v^2 m^2 - p$$

$$p_{zz} = \frac{1}{4\pi}\mu v^2 n^2 - p_1$$

$$p_{yz} = \frac{1}{4\pi}\mu v^2 mn$$

$$p_{zx} = \frac{1}{4\pi}\mu v^2 nl$$

$$p_{xy} = \frac{1}{4\pi}\mu v^2 lm.$$

If we write

$$\alpha = vl, \quad \beta = vm, \text{ and } \gamma = vn,$$

then

$$\left.\begin{aligned} p_{xx} &= \frac{1}{4\pi}\mu\alpha^2 - p_1 & p_{yz} &= \frac{1}{4\pi}\mu\beta\gamma \\ p_{yy} &= \frac{1}{4\mu}\mu\beta^2 - p_1 & p_{zx} &= \frac{1}{4\pi}\mu\gamma\alpha \\ p_{zz} &= \frac{1}{4\pi}\mu\gamma^2 - p_1 & p_{xy} &= \frac{1}{4\pi}\mu\alpha\beta. \end{aligned}\right\} \quad (2)$$

Prop. III.—To find the resultant force on an element of the medium, arising from the variation of internal stress.

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* Rankine's 'Applied Mechanics,' art. 106.

We have in general, for the force in the direction of x per unit of volume by the law of equilibrium of stresses*,

$$X=\frac{d}{dx}p_{xx}+\frac{d}{dy}p_{xy}+\frac{d}{dz}p_{zx}. \qquad (3)$$

In this case the expression may be written

$$X=\frac{1}{4\pi}\left\{\frac{d(\mu\alpha)}{dx}\alpha+\mu\alpha\frac{d\alpha}{dx}-4\pi\frac{dp_1}{dx}+\frac{d(\mu\beta)}{dy}\alpha+\mu\beta\frac{d\alpha}{dy}\right.$$
$$\left.+\frac{d(\mu\gamma)}{dz}\alpha+\mu\gamma\frac{d\alpha}{dz}\right\}. \qquad (4)$$

Remembering that $\alpha\frac{d\alpha}{dx}+\beta\frac{d\beta}{dx}+\gamma\frac{d\gamma}{dx}=\frac{1}{2}\frac{d}{dx}(\alpha^2+\beta^2+\gamma^2)$, this becomes

$$X=\alpha\frac{1}{4\pi}\left(\frac{d}{dx}(\mu\alpha)+\frac{d}{dy}(\mu\beta)+\frac{d}{dz}(\mu\gamma)\right)+\frac{1}{8\pi}\mu\frac{d}{dx}(\alpha^2+\beta^2+\gamma^2)$$
$$-\mu\beta\frac{1}{4\pi}\left(\frac{d\beta}{dx}-\frac{d\alpha}{dy}\right)+\mu\gamma\frac{1}{4\pi}\left(\frac{d\alpha}{dz}-\frac{d\gamma}{dx}\right)-\frac{dp_1}{dx}. \qquad (5)$$

The expressions for the forces parallel to the axes of y and z may be written down from analogy.

We have now to interpret the meaning of each term of this expression.

We suppose α, β, γ to be the components of the force which would act upon that end of a unit magnetic bar which points to the north.

μ represents the magnetic inductive capacity of the medium at any point referred to air as a standard. $\mu\alpha$, $\mu\beta$, $\mu\gamma$ represent the quantity of magnetic induction through unit of area perpendicular to the three axes of x, y, z respectively.

The total amount of magnetic induction through a closed surface surrounding the pole of a magnet, depends entirely on the strength of that pole; so that if $dx\,dy\,dz$ be an element, then

$$\left(\frac{d}{dx}\mu\alpha+\frac{d}{dy}\mu\beta+\frac{d}{dz}\mu\gamma\right)dx\,dy\,dz=4\pi m\,dx\,dy\,dz, \qquad (6)$$

which represents the total amount of magnetic induction outwards through the surface of the element $dx\,dy\,dz$, represents the amount of "imaginary magnetic matter" within the element, of the kind which points north.

The *first term* of the value of X, therefore,

$$\alpha\frac{1}{4\pi}\left(\frac{d}{dx}\mu\alpha+\frac{d}{dy}\mu\beta+\frac{d}{dz}\mu\gamma\right), \qquad (7)$$

may be written

$$\alpha m, \qquad (8)$$

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* Rankine's 'Applied Mechanics,' art. 116.

where α is the intensity of the magnetic force, and m is the amount of magnetic matter pointing north in unit of volume.

The physical interpretation of this term is, that the force urging a north pole in the positive direction of x is the product of the intensity of the magnetic force resolved in that direction, and the strength of the north pole of the magnet.

Let the parallel lines from left to right in fig. 1 represent a field of magnetic force such as that of the earth, *s n* being the direction from south to north. The vortices, according to our hypothesis, will be in the direction shown by the arrows in fig. 3, that is, in a plane perpendicular to the lines of force, and revolving in the direction of the hands of a watch when observed from *s* looking towards *n*. The parts of the vortices above the plane of the paper will be moving towards *e*, and the parts below that plane towards *w*.

We shall always mark by an arrow-head the direction in which we must look in order to see the vortices rotating in the direction of the hands of a watch. The arrow-head will then indicate the *northward* direction in the magnetic field, that is, the direction in which that end of a magnet which points to the north would set itself in the field.

Fig. 1.

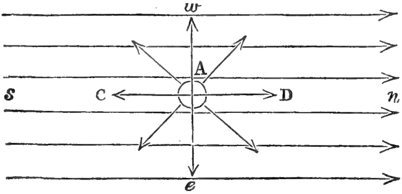


Fig. 2.

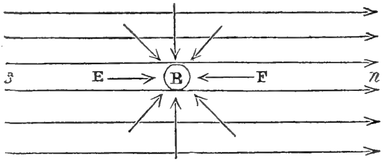
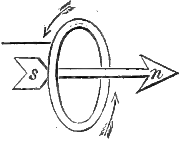


Fig. 3.



Now let A be the end of a magnet which points north. Since it repels the north ends of other magnets, the lines of force will be directed *from* A outwards in all directions. On the north side the line A D will be in the *same* direction with the lines of the magnetic field, and the velocity of the vortices will be *increased*. On the south side the line A C will be in the opposite direction, and the velocity of the vortices will be diminished, so that the lines of force are more powerful on the north side of A than on the south side.

We have seen that the mechanical effect of the vortices is to produce a tension along their axes, so that the resultant effect on A will be to pull it more powerfully towards D than towards C; that is, A will tend to move to the north.

Let B in fig. 2 represent a south pole. The lines of force belonging to B will tend *towards* B, and we shall find that the lines of force are rendered stronger towards E than towards F, so that the effect in this case is to urge B towards the south.

It appears therefore that, on the hypothesis of molecular vortices, our first term gives a mechanical explanation of the force acting on a north or south pole in the magnetic field.

We now proceed to examine the second term,

$$\frac{1}{8\pi}\mu\frac{d}{dx}(\alpha^2+\beta^2+\gamma^2).$$

Here $\alpha^2+\beta^2+\gamma^2$ is the square of the intensity at any part of the field, and μ is the magnetic inductive capacity at the same place. Any body therefore placed in the field will be urged *towards places of stronger magnetic intensity* with a force depending partly on its own capacity for magnetic induction, and partly on the rate at which the square of the intensity increases.

If the body be placed in a fluid medium, then the medium, as well as the body, will be urged towards places of greater intensity, so that its hydrostatic pressure will be increased in that direction. The resultant effect on a body placed in the medium will be the *difference* of the actions on the body and on the portion of the medium which it displaces, so that the body will tend to or from places of greatest magnetic intensity, according as it has a greater or less capacity for magnetic induction than the surrounding medium.

In fig. 4 the lines of force are represented as converging and becoming more powerful towards the right, so that the magnetic tension at B is stronger than at A, and the body A B will be urged to the right. If the capacity for magnetic induction is greater in the body than in the surrounding medium, it will move to the right, but if less it will move to the left.

Fig. 4.

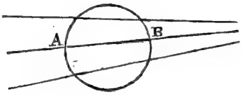
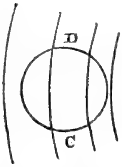


Fig. 5.



We may suppose in this case that the lines of force are converging to a magnetic pole, either north or south, on the right hand.

In fig. 5 the lines of force are represented as vertical, and becoming more numerous towards the right. It may be shown that if the force increases towards the right, the lines of force will be curved towards the right. The effect of the magnetic tensions will then be to draw any body towards the right with a force depending on the excess of its inductive capacity over that of the surrounding medium.

We may suppose that in this figure the lines of force are those surrounding an electric current perpendicular to the plane of the paper and on the right hand of the figure.

These two illustrations will show the mechanical effect on a paramagnetic or diamagnetic body placed in a field of varying magnetic force, whether the increase of force takes place along the lines or transverse to them. The form of the second term of our equation indicates the general law, which is quite independent of the direction of the lines of force, and depends solely on the manner in which the force *varies* from one part of the field to another.

We come now to the third term of the value of X,

$$-\mu\beta\frac{1}{4\pi}\left(\frac{d\beta}{dx}-\frac{d\alpha}{dy}\right).$$

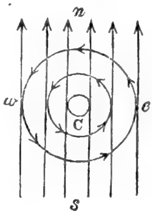
Here $\mu\beta$ is, as before, the quantity of magnetic induction through unit of area perpendicular to the axis of y, and $\frac{d\beta}{dx}-\frac{d\alpha}{dy}$ is a quantity which would disappear if $\alpha dx+\beta dy+\gamma dz$ were a complete differential, that is, if the force acting on a unit north pole were subject to the condition that no work can be done upon the pole in passing round any closed curve. The quantity represents the work done on a north pole in travelling round unit of area in the direction from $+x$ to $+y$ parallel to the plane of xy. Now if an electric current whose strength is r is traversing the axis of z, which, we may suppose, points vertically upwards, then, if the axis of x is east and that of y north, a unit north pole will be urged round the axis of z in the direction from x to y, so that in one revolution the work done will be $=4\pi r$. Hence $\frac{1}{4\pi}\left(\frac{d\beta}{dx}-\frac{d\alpha}{dy}\right)$ represents the *strength of an electric current parallel to z* through unit of area; and if we write

$$\frac{1}{4\pi}\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right)=p,\ \frac{1}{4\pi}\left(\frac{d\alpha}{dz}-\frac{d\gamma}{dx}\right)=q,\ \frac{1}{4\pi}\left(\frac{d\beta}{dx}-\frac{d\alpha}{dy}\right)=r, \quad (9)$$

then p, q, r will be the quantity of electric current per unit of area perpendicular to the axes of x, y, and z respectively.

The physical interpretation of the third term of X, $-\mu\beta r$, is that if $\mu\beta$ is the quantity of magnetic induction parallel to y, and r the quantity of electricity flowing in the direction of z, the element will be urged in the direction of $-x$, transversely to the direction of the current and of the lines of force; that is, an *ascending* current in a field of force magnetized towards the *north* would tend to move *west*.

Fig. 6.



To illustrate the action of the molecular vortices, let *s n* be the direction of magnetic force in the field, and let C be the section of an ascending magnetic current perpendicular to the paper. The lines of force due to this current will be circles drawn in the opposite direction from that of the hands of a watch; that is, in the direction *n w s e*. At *e* the lines of force will be the sum of those of the field and of the current, and at *w* they will be the difference of the two sets of lines; so that the vortices on the east side of the current will be more powerful than those on the west side. Both sets of vortices have their equatorial parts turned towards C, so that they tend to expand towards C, but those on the east side have the greatest effect, so that the resultant effect on the current is to urge it towards the *west*.

The fourth term,

$$+\mu\gamma\frac{1}{4\pi}\left(\frac{d\alpha}{dz}-\frac{d\gamma}{dx}\right), \text{ or } +\mu\gamma q, \quad . \quad . \quad . \quad (10)$$

may be interpreted in the same way, and indicates that a current q in the direction of y, that is, to the north, placed in a magnetic field in which the lines are vertically upwards in the direction of z, will be urged towards the *east*.

The fifth term,

$$-\frac{dp_1}{dx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

merely implies that the element will be urged in the direction in which the hydrostatic pressure p_1 diminishes.

We may now write down the expressions for the components of the resultant force on an element of the medium per unit of volume, thus:

$$X=\alpha m+\frac{1}{8\pi}\mu\frac{d}{dx}(v^2)-\mu\beta r+\mu\gamma q-\frac{dp_1}{dx}, \quad . \quad . \quad (12)$$

$$Y=\beta m+\frac{1}{8\pi}\mu\frac{d}{dy}(v^2)-\mu\gamma p+\mu\alpha r-\frac{dp_1}{dy}, \quad . \quad . \quad (13)$$

$$Z=\gamma m+\frac{1}{8\pi}\mu\frac{d}{dz}(v^2)-\mu\alpha q+\mu\beta p-\frac{dp_1}{dz}. \quad . \quad . \quad (14)$$

The first term of each expression refers to the force acting on magnetic poles.

The second term to the action on bodies capable of magnetism by induction.

The third and fourth terms to the force acting on electric currents.

And the fifth to the effect of simple pressure.

Before going further in the general investigation, we shall consider equations (12, 13, 14,) in particular cases, corresponding to those simplified cases of the actual phenomena which we seek to obtain in order to determine their laws by experiment.

We have found that the quantities p, q, and r represent the resolved parts of an electric current in the three coordinate directions. Let us suppose in the first instance that there is *no* electric current, or that p, q, and r vanish. We have then by (9),

$$\frac{d\gamma}{dy}-\frac{d\beta}{dz}=0,\quad \frac{d\alpha}{dz}-\frac{d\gamma}{dx}=0,\quad \frac{d\beta}{dx}-\frac{d\alpha}{dy}=0, \quad . \quad (15)$$

whence we learn that

$$\alpha dx+\beta dy+\gamma dz=d\phi \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (16)$$

is an exact differential of ϕ, so that

$$\alpha=\frac{d\phi}{dx},\quad \beta=\frac{d\phi}{dy},\quad \gamma=\frac{d\phi}{dz}: \quad . \; . \; . \; . \; . \; . \; . \quad (17)$$

μ is proportional to the density of the vortices, and represents the "capacity for magnetic induction" in the medium. It is equal to 1 in air, or in whatever medium the experiments were made which determined the powers of the magnets, the strengths of the electric currents, &c.

Let us suppose μ constant, then

$$m=\frac{1}{4\pi}\left(\frac{d}{dx}(\mu\alpha)+\frac{d}{dy}(\mu\beta)+\frac{d}{dz}(\mu\gamma)\right)$$

$$=\frac{1}{4\pi}\mu\left(\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}+\frac{d^2\phi}{dz^2}\right) \quad . \; . \; . \; . \; . \quad (18)$$

represents the amount of imaginary magnetic matter in unit of volume. That there may be no resultant force on that unit of volume arising from the action represented by the first term of equations (12, 13, 14), we must have $m=0$, or

$$\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}+\frac{d^2\phi}{dz^2}=0. \quad . \; . \; . \; . \; . \quad (19)$$

Now it may be shown that equation (19), if true within a given space, implies that the forces acting within that space are such as would result from a distribution of centres of force beyond that space, attracting or repelling inversely as the square of the distance.

Hence the lines of force in a part of space where μ is uniform, and where there are no electric currents, must be such as would result from the theory of "imaginary matter" acting at a distance. The assumptions of that theory are unlike those of ours, but the results are identical.

Let us first take the case of a single magnetic pole, that is, one end of a long magnet, so long that its other end is too far off to have a perceptible influence on the part of the field we are considering. The conditions then are, that equation (18) must be fulfilled at the magnetic pole, and (19) everywhere else. The only solution under these conditions is

$$\phi = -\frac{m}{\mu}\frac{1}{r}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (20)$$

where r is the distance from the pole, and m the strength of the pole.

The repulsion at any point on a unit pole of the same kind is

$$\frac{d\phi}{dr} = \frac{m}{\mu}\frac{1}{r^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (21)$$

In the standard medium $\mu = 1$; so that the repulsion is simply $\frac{m}{r^2}$ in that medium, as has been shown by Coulomb.

In a medium having a greater value of μ (such as oxygen, solutions of salts of iron, &c.) the attraction, on our theory, ought to be *less* than in air, and in diamagnetic media (such as water, melted bismuth, &c.) the attraction between the same magnetic poles ought to be *greater* than in air.

The experiments necessary to demonstrate the difference of attraction of two magnets according to the magnetic or diamagnetic character of the medium in which they are placed, would require great precision, on account of the limited range of magnetic capacity in the fluid media known to us, and the small amount of the difference sought for as compared with the whole attraction.

Let us next take the case of an electric current whose quantity is C, flowing through a cylindrical conductor whose radius is R, and whose length is infinite as compared with the size of the field of force considered.

Let the axis of the cylinder be that of z, and the direction of the current positive, then within the conductor the quantity of current per unit of area is

$$r = \frac{C}{\pi R^2} = \frac{1}{4\pi}\left(\frac{d\beta}{dx} - \frac{d\alpha}{dy}\right); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (22)$$

so that within the conductor

$$\alpha=-2\frac{\mathrm{C}}{\mathrm{R}^2}y,\quad \beta=2\frac{\mathrm{C}}{\mathrm{R}^2}x,\quad \gamma=0. \quad . \quad . \quad . \quad . \qquad (23)$$

Beyond the conductor, in the space round it,

$$\phi=2\mathrm{C}\tan^{-1}\frac{y}{x}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (24)$$

$$\alpha=\frac{d\phi}{dx}=-2\mathrm{C}\frac{y}{x^2+y^2},\quad \beta=\frac{d\phi}{dy}=2\mathrm{C}\frac{x}{x^2+y^2},\quad \gamma=\frac{d\phi}{dz}=0. \qquad (25)$$

If $\rho=\sqrt{x^2+y^2}$ is the perpendicular distance of any point from the axis of the conductor, a unit north pole will experience a force $=\frac{2\mathrm{C}}{\rho}$, tending to move it round the conductor in the direction of the hands of a watch, if the observer view it in the direction of the current.

Let us now consider a current running parallel to the axis of z in the plane of xz at a distance ρ. Let the quantity of the current be c', and let the length of the part considered be l, and its section s, so that $\frac{c'}{s}$ is its strength per unit of section. Putting this quantity for p in equations (12, 13, 14), we find

$$\mathrm{X}=-\mu\beta\frac{c'}{s}$$

per unit of volume; and multiplying by ls, the volume of the conductor considered, we find

$$\begin{aligned}\mathrm{X}&=-\mu\beta c'l\\ &=-2\mu\frac{\mathrm{C}c'l}{\rho}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (26)\end{aligned}$$

showing that the second conductor will be attracted towards the first with a force inversely as the distance.

We find in this case also that the amount of attraction depends on the value of μ, but that it varies directly instead of inversely as μ; so that the attraction between two conducting wires will be greater in oxygen than in air, and greater in air than in water.

We shall next consider the nature of electric currents and electromotive forces in connexion with the theory of molecular vortices.

XXVI. *On the Benzole Series.*

By Arthur H. Church, *B.A. Oxon., F.C.S.*[*]

Part III. *Note on the Oxidation of Nitrobenzole and its Homologues.*

RECENT experiments by Hofmann, Berthelot, and others have shown, with reference to many important organic substances, how incorrect is any theory which does not permit them to be viewed in more than one aspect. Is nitrobenzole simply a hydrocarbon in which one equivalent of hydrogen is replaced by the group NO^4? or is it the hydride of nitrophenyle, $C^{12}\left(\begin{matrix}H^4\\NO^4\end{matrix}\right), H$? or, again, is it the nitrite of phenyle, $C^{12}H^5, NO^4$? Some of the metamorphoses to which this interesting body is subject suggest one of these views, and some another. The ready production from nitrobenzole of phenylamine, in which the group phenyle may be reasonably supposed to exist, might induce us to regard nitrobenzole as a compound of phenyle, possibly the nitrite; while a reaction which I pointed out some time ago[†], in which, by the action of sulphuric acid upon nitrobenzole, a compound acid was formed to which the empirical name of nitrosulphobenzolic acid was assigned, and which can hardly be regarded in any other way except as the sulphite of *nitrophenyle* and hydrogen, $C^{12}\left(\begin{matrix}H^4\\NO^4\end{matrix}\right), H, 2SO^3$, almost obliges us to view nitrobenzole as containing *nitrophenyle.* I propose in the present note to cite a few experiments which present some of the nitro-derivatives of the benzole series in a new light. I feel, however, that though an apology is due for the imperfections of the present account, yet a preliminary notice of my results (results which will require much time and labour to bring to a satisfactory conclusion) might not be unacceptable. But I should have deferred publishing any account of my inquiries for some time longer, had not an acid been lately discovered homologous with benzoic acid, and isomeric, but not, I think, identical with an acid mentioned in this paper: I refer to collinic acid, $C^{12}H^4O^4$, obtained by the oxidation of gelatine. Then, too, several of the speculations and anticipations of Berthelot in his work on Organic Synthesis, trench somewhat closely upon one of the inquiries in which I have been lately engaged. I intend, when my inquiries are more advanced, to make the two new acids (phenoic and nitrophenoic) mentioned in the present notice the subject of another communication.

The oxidation of toluole, xylole, and cumole by means of bi-

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[*] Communicated by the Author.

[†] Phil. Mag. April and June 1855.

chromate of potassium and sulphuric acid, has been found in each case to yield benzoic acid. Cymole, on the other hand, the last member of the series, when treated in the same way, yields the insolinic acid discovered by Dr. Hofmann, and not the toluic acid which Mr. Noad obtained by acting upon this hydrocarbon with dilute nitric acid. Benzole remains wholly unacted upon when boiled for a very long period with bichromate of potassium and sulphuric acid. Not so, however, with nitrobenzole, which is slowly converted by this powerful oxidizing mixture into a soft, white, crystalline mass of intensely acid reaction. In the first experiment, I made use of a very excellent sample of commercial nitrobenzole, a portion of which seemed to be acted on with greater facility than the rest. An analysis of the acid thus formed gave numbers which indicated a mixture of nitrobenzoic and *nitrophenoic* acids: I propose the latter name for the new acid, which I believe to be the next lower homologue of the benzoic. The view that the acid burnt was a mixture, is confirmed by another experiment, to be related further on. In order to secure the absence of nitrotoluole from the nitrobenzole operated upon, I converted some benzole from benzoic acid into nitrobenzole, and repeated my experiments with this. The action of a most concentrated oxidizing solution was now found to be very much slower than in the former case; but after long digestion the nitrobenzole solidified in great measure on cooling the mixture, while from the solution itself numerous white crystalline spangles separated. The liquid and solid parts were together poured into a funnel plugged with asbestos. To separate the unchanged nitrobenzole, the solid stopped by the filter was exhausted with boiling water, and the solution filtered twice through paper. When cold, the filtrate was full of large nacreous plates of a very pale straw colour, which by recrystallization became perfectly white. Having had but a few grains of this substance at my disposal, I have not yet made an accurate examination of its physical properties. I found, however, that its reaction is strongly acid, that it is fusible without decomposition, tolerably soluble in boiling water, and that it yields crystallizable salts. Not only does its origin and the method of its formation preclude the existence of more than twelve atoms of carbon in the new acid, but the following determination of silver in a carefully prepared specimen of its silver-salt points to the formula $C^{12} H^2 Ag (NO^4) O^4$ for this compound, and to $C^{12} H^3 (NO^4) O^4$ for the acid itself:—

·971 grm. of silver-salt gave ·537 grm. of chloride of silver = ·4041 grm. of silver, corresponding to a per-centage of—

	Experiment.	Theory, $C^{12}H^{2}Ag(NO^{4})O^{4}$.
Silver	41·61	41·54

So far as they have been yet examined, the properties of nitrophenoic acid confirm the idea that it is a true homologue of nitrobenzoic acid.

I have made an attempt to prepare the original acid, the *phenoic,* of which I have supposed the above-noticed acid to be the nitro-derivative. Although a mixture of bichromate of potassium and sulphuric acid is without action on benzole, it acts most energetically on sulphobenzolic acid formed by dissolving benzole in Nordhäusen sulphuric acid. If to a slightly diluted solution of sulphobenzolic acid at about 70° C. minute fragments of bichromate of potassium be added, one at a time, and the action which ensues at each addition be moderated by cooling the apparatus, an acid distillate will be obtained, on the surface of which small brilliant crystals, generally accompanied by a few oily globules, will be found floating. It would seem that the oily and solid portions of the distillate are alike in composition, since the analysis of the silver-salts of the two bodies, separated mechanically as far as possible, gave almost the same numbers. Of the annexed determinations, I. was made with a silver-salt prepared from the oily part, and II. with one prepared from the crystalline part of the distillate.

I. ·739 grm. gave ·37 grm. of silver,
II. ·322 grm. gave ·163 grm. of silver;

corresponding to the following per-centages of silver:—

Experiment.		Theory,
I.	II.	$C^{12}H^{3}AgO^{4}$.
50·06	50·06	50·23

Sulphotoluolic and sulphocumolic acids, when oxidized as described above, yield benzoic acid in abundance. I have identified the product by all the usual tests. I have not yet experimented with the xylole series. Sulphocymolic acid yields a white powder, apparently identical with insolinic acid.

Nitrotoluole, when oxidized, yielded nitrobenzoic acid, which agreed in every respect with a pure specimen in my possession prepared from benzoic acid. I have before mentioned the difficulty with which nitrobenzole is acted on by the oxidizing mixture; and if this latter be somewhat diluted, it affords a means of separating the nitro-derivatives of toluole, &c. from nitrobenzole, which, when the action is complete, is siphoned off and washed with an alkaline solution to remove the nitrobenzoic acid formed.

Since sulphobenzolate of ammonium, when submitted to dry distillation, yields some quantity of benzole*, I imagined that the *nitro*sulphobenzolate would yield *nitro*benzole: experiment, however, has not corroborated this view.

In 1859 I showed† that nascent chlorine acts powerfully on toluole, xylole, and other homologues of benzole, yielding the chlorides of toluenyle, xylenyle, &c., from which the cyanides, and subsequently the acids (toluic and xyloic), are producible. I am pursuing some inquiries in this direction with benzole, upon which unfortunately nascent chlorine acts with more difficulty, and at the same time does not appear to yield such definite results.

There is a point of view from which some of the experiments which I have made acquire a fresh interest. If 1 vol. of light coal-naphtha containing, say 50 per cent. of benzole, be submitted to the action of 6 vols. of oil of vitriol previously diluted with 1 vol. of water, and the mixture heated for some time in a suitable condensing apparatus, the benzole will remain nearly, if not quite, unacted upon, while the other hydrocarbons will be dissolved by the sulphuric acid. If the acid be absorbed by small fragments of pumice and thus used, it exerts a much more rapid and effectual action on the naphtha. The benzole, after having been washed with water, is nearly pure. The other hydrocarbons which have been dissolved are now contained as sulphotoluolic and similar acids in the liquid, which is to be collected, diluted with half its bulk of water, and poured into a retort provided with a Liebig's condenser. Bichromate of potassium, about one-sixth part in weight of the acid present, is added gradually to the solution, and the mixture cautiously distilled. In this way a considerable proportion of benzoic acid may be obtained.

Postscript, February 8, 1861.

Since writing the above remarks, my attention has been directed to a short notice of some experiments by MM. Cloetz and Guignet, who also seem to have obtained a new acid by the oxidation of nitrobenzole. I think it right to say that I succeeded in producing the acid which I have termed nitrophenoic in June 1860.

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* Phil. Mag. December 1859.

† Chemical News, December 10, 1859.

XXVII. *Note on the Theory of Determinants.*

By A. Cayley, *Esq.*[*]

THE following mode of arrangement of the developed expression of a determinant had presented itself to me as a convenient one for the calculation of a rather complicated determinant of the fifth order; but I have since found that it is in effect given, although in a much less compendious form, in a paper by J. N. Stockwell, "On the Resolution of a System of Symmetrical Equations with Indeterminate Coefficients," Gould's 'Ast. Journal,' No. 139 (Cambridge, U. S., Sept. 10, 1860).

Suppose that the determinant

$$\begin{vmatrix} 11, & 12, & 13 \\ 21, & 22, & 23 \\ 31, & 32, & 33 \end{vmatrix}$$

is represented by $\{123\}$, and so for a determinant of any order $\{123\ldots n\}$.

Let $|1|$, $|2|$, $|12|$, $|123|$, &c. denote as follows: viz.

$$\begin{aligned} |1| &= 11, \quad |2| = 22, \text{ \&c.} \\ |12| &= 12.21, \\ |123| &= 12.23.31, \\ &\text{\&c.,} \end{aligned}$$

where it is to be noticed that, with the same two symbols, *e. g.* 1 and 2, there is but one distinct expression $|12|$ (in fact $|21| = 21.12 = |12|$); with the same three symbols 1, 2, 3, there are two distinct expressions, $|123|$ $(=12.23.31)$ and $|132|$ $(=13.32.21)$; and generally with the same m symbols 1, 2, 3 ... m, there are $1.2.3\ldots\overline{m-1}$ distinct expressions $|123\ldots m|$, which are obtained by permuting in every possible manner all but one of the m symbols.

This being so, and writing for greater simplicity $|1|2|$ to denote the product $|1| \times |2|$, and so in general, the values of the determinants $\{12\}$, $\{123\}$, $\{1234\}$, $\{12345\}$, &c. are as follows: viz.

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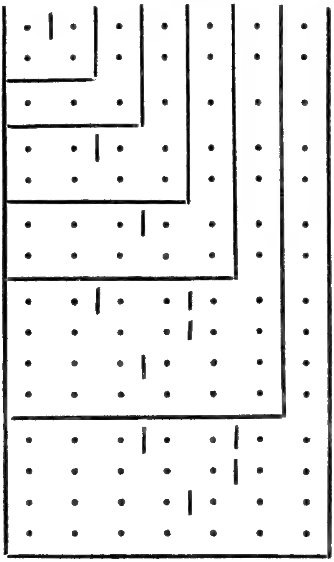
[*] Communicated by the Author.

	No. of terms.	
	+	−
$\{12\} = + \vert 1 \vert 2 \vert$	1	
$- \vert 1\ \ 2 \vert$		1
	$1 + 1 = 2$	
$\{123\} = + \vert 1 \vert 2 \vert 3 \vert$	1	
$- \vert 1\ \ 2 \vert 3 \vert$		3
$+ \vert 1\ \ 2\ \ 3 \vert$	2	
	$3 + 3 = 6$	
$\{1234\} = + \vert 1 \vert 2 \vert 3 \vert 4 \vert$	1	
$- \vert 1\ \ 2 \vert 3 \vert 4 \vert$		6
$+ \vert 1\ \ 2\ \ 3 \vert 4 \vert$	8	
$+ \vert 1\ \ 2 \vert 3\ \ 4 \vert$	3	
$- \vert 1\ \ 2\ \ 3\ \ 4 \vert$		6
	$12 + 12 = 24$	
$\{12345\} = + \vert 1 \vert 2 \vert 3 \vert 4 \vert 5 \vert$	1	
$- \vert 1\ \ 2 \vert 3 \vert 4 \vert 5 \vert$		10
$+ \vert 1\ \ 2\ \ 3 \vert 4 \vert 5 \vert$	20	
$+ \vert 1\ \ 2 \vert 3\ \ 4 \vert 5 \vert$	15	
$- \vert 1\ \ 2\ \ 3\ \ 4 \vert 5 \vert$		30
$- \vert 1\ \ 2\ \ 3 \vert 4\ \ 5 \vert$		20
$+ \vert 1\ \ 2\ \ 3\ \ 4\ \ 5 \vert$	24	
	$60 + 60 = 120$	

where, as regards the signs, it is to be observed that there is a sign − for each compartment $\vert \quad \vert$ containing an even number of symbols; thus in the expression for $\{1234\}$, the terms $\vert 1\ 2 \vert 3\ 4 \vert$ have the sign $- - = +$, and the terms $\vert 1\ 2\ 3\ 4 \vert$ the sign −. Or, what comes to the same thing; when n is even, the sign is + or − according as the number of compartments is even or odd; and contrariwise when n is odd. As regards the remaining part of the expression, this merely exhibits the partitions of a set of n things; and the formulæ for the several determinants up to the determinant of a given order are all of them obtained by means of the form



which is carried up to the order 7, but which can be further extended without any difficulty whatever.

It is perhaps hardly necessary; but I give at full length the expressions of the determinant of the third order: this is

$$\{123\}=\begin{array}{r} |1|2|3| \\ \left.\begin{array}{r} -|1\;\;2|3| \\ -|2\;\;3|1| \\ -|3\;\;1|2| \end{array}\right\} \\ \left.\begin{array}{r} +|1\;\;2\;\;3| \\ +|1\;\;3\;\;2| \end{array}\right\}. \end{array}$$

And by writing down in like manner the expression for the twenty-four terms of the determinant of the fourth order, the notation will become perfectly clear.

The formula hardly requires a demonstration. The terms of a determinant $\{123\ldots n\}$, for example the determinant $\{1234\}$, are obtained by permuting in every possible manner the symbols in either column, say the second column, of the arrangement

$$\begin{array}{cc} 1 & 1 \\ 2 & 2 \\ 3 & 3 \\ 4 & 4 \end{array}$$

and prefixing the sign (+ or −) of the arrangement; and the resulting arrangements, for instance

$$\begin{array}{lll} +\;1\;1, & \quad -\;1\;2, & \quad -\;1\;2, \\ \;\;\;\;2\;2 & \quad\;\;\;\;2\;1 & \quad\;\;\;\;2\;3 \\ \;\;\;\;3\;3 & \quad\;\;\;\;3\;3 & \quad\;\;\;\;3\;4 \\ \;\;\;\;4\;4 & \quad\;\;\;\;4\;4 & \quad\;\;\;\;4\;1 \end{array}$$

are interpreted either into $+11.22.33.44$, $-12.21.33.44$, $-12.23.34.41$, or in the notation of the formula, into

$$+\,|1|2|3|4|,\quad -\,|12|3|4|,\quad -\,|1234|.$$

And so in general.

Suppose that any partition of n contains α compartments each of a symbols, β compartments each of b symbols ... ($a, b, \ldots$ being all of them different and greater than unity), and ρ compartments each of a single symbol, we have

$$n=\alpha a+\beta b+\ldots+\rho.$$

And writing, as usual, $\Pi a=1.2.3\ldots a$, &c., the number of ways in which the symbols $1, 2, 3, \ldots n$, can be so arranged in compartments is

$$\frac{\Pi n}{(\Pi a)^{\alpha}(\Pi b)^{\beta}\ldots\Pi\alpha\,\Pi\beta\ldots\Pi\rho};$$

but each such arrangement gives $\big(\Pi(a-1)\big)^{\alpha}\cdot\big(\Pi(b-1)\big)^{\beta}$ terms of the determinant, and the corresponding number of terms therefore is

$$\frac{\Pi n}{a^{\alpha}\,b^{\beta}\ldots\Pi\alpha\,\Pi\beta\ldots\Pi\rho}.$$

The whole number of terms of the determinant is Πn, and we have thus the theorem

$$1=\Sigma\,\frac{1}{a^{\alpha}\,b^{\beta}\ldots\Pi\alpha\,\Pi\beta\ldots\Pi\rho},$$

in which the summation corresponds to all the different partitions $n=\alpha a+\beta b\ldots+\rho$, where $a, b, \ldots$ are all of them different and greater than unity; a theorem given in Cauchy's *Mémoire sur les Arrangements,*' &c., 1844. But it is to be noticed also that, the number of the positive and negative terms being equal, we have besides

$$0=\Sigma\,\frac{(-)^{\alpha(a-1)+\beta(b-1)+\ldots}}{a^{\alpha}\,b^{\beta}\ldots\Pi\alpha\,\Pi\beta\ldots\Pi\rho};$$

or, what is the same thing,

$$0=\Sigma\frac{(-)^{n-\alpha-\beta\ldots-\rho}}{a^{\alpha}b^{\beta}\ldots\Pi\alpha\,\Pi\beta\ldots\Pi\rho};$$

and thence also

$$\tfrac{1}{2}=\Sigma\frac{1}{a^{\alpha}b^{\beta}\ldots\Pi\alpha\,\Pi\beta\ldots\Pi\rho};$$

where, as before, $n=\alpha a+\beta b\ldots+\rho$ (a, b, … being all different and greater than unity); but the summation is restricted either to the partitions for which $n-\alpha-\beta\ldots-\rho$ is even, or else to those for which $n-\alpha-\beta\ldots-\rho$ is odd.

The formula affords a proof of the fundamental property of skew symmetrical determinants. In such a determinant we have not only $12=-21$, &c., but also $11=0$, &c. Suppose that n, the order of the determinant, is odd; then in each line of the expression

$$\begin{aligned}\{123\ldots n\} = & \mid 1 \mid 2 \mid \ldots \mid n \mid \\ & \pm \text{ \&c.}\end{aligned}$$

of the determinant, there is at least one compartment $\mid 1 \mid$ or $\mid 123 \mid$ &c. containing an odd number of symbols: let $\mid 123 \mid$ be such a compartment, then the determinant contains the terms $\mid 123 \mid \mathrm{P}$ and $\mid 132 \mid \mathrm{P}$ (where P represents the remaining compartments), that is, $12.23.31.\mathrm{P}$ and $13.32.21.\mathrm{P}$. But in virtue of the relations $12=-21$, &c., we have

$$12.23.31=-13.32.21;$$

and so in all similar cases, that is, the terms destroy each other, or the skew symmetrical determinant of an odd order is equal to zero.

The like considerations show that a skew symmetrical determinant of an even order is a perfect square. In fact, considering for greater simplicity the case $n=4$, any line in the foregoing expression of $\{1234\}$ for which a compartment contains an odd number of symbols, gives rise to terms which destroy each other, and may be omitted. The expression thus reduces itself to

$$\begin{aligned}\{1234\} = & +\mid 12 \mid 34 \mid \quad 3 \text{ terms} \\ & -\mid 12 \;\; 34 \mid \quad 6 \text{ terms},\end{aligned}$$

which is in fact the square of

$$12.34+13.42+14.23.$$

For the square of a term, say 12.34, is $\overline{12}^{2}.\overline{34}^{2}$ or $12.21.34.43$, that is, $\mid 12 \mid 34 \mid$, and the double of the product of two terms, say 12.34 and 13.42, is $2.12.34.13.42$, or $-12.24.43.31$

$-13.34.42.21$, that is $-|1243|-|1342|$, and so for the other similar terms, and we have

$$\{1234\}=(12.34+13.42+14.23)^2.$$

And so in general, n being any even number, the skew symmetrical determinant $\{123\ldots n\}$ is equal to the square of the Pfaffian $12\ldots n$, where the law of these Pfaffian functions is

$$1234 \quad =12.34 \quad +13.42 \quad +14.23$$

$$123456=12.3456+13.4562+14.5623+15.6234+16.2345,$$

where, in the second equation, 3456, &c. are Pfaffians, viz. $3456=34.56+35.64+36.45$; and so on.

2 Stone Buildings, W.C.,
December 28, 1860.

XXVIII. *Letter from* Prof. Kirchhoff *on the Chemical Analysis of the Solar Atmosphere.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Owens College, Manchester,
February 1, 1860.

I RECEIVED a short time ago from Heidelberg the enclosed portion of a letter from Prof. Kirchhoff to Prof. Erdmann. As it gives a later account of Kirchhoff and Bunsen's most important researches than has yet appeared in the English journals, I think it may be of interest to your readers.

I am, yours sincerely,
Henry E. Roscoe.

"Since I sent in my last report to the Berlin Academy, I have been almost uninterruptedly engaged in following out the investigation in the direction I there indicated. I will not now speak either of the theoretical proof I have given* of the facts I there announced, or of the experiments by help of which Bunsen and I† have shown that the bright bands in the spectrum of a flame serve as the surest indications of the metals present therein; I will take the liberty, in this communication, of informing you of the progress I have made in the chemical analysis of the solar atmosphere.

"The sun possesses an incandescent, gaseous atmosphere, which surrounds a solid nucleus having a still higher temperature. If we could see the spectrum of the solar atmosphere, we should see in it the bright bands characteristic of the metals

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[*] Phil. Mag. July 1860.

[†] Ibid. August 1860.

contained in the atmosphere, and from the presence of these lines should infer that of these various metals. The more intense luminosity of the sun's solid body, however, does not permit the spectrum of its atmosphere to appear; it *reverses* it, according to the proposition I have announced; so that instead of the bright lines which the spectrum of the atmosphere by itself would show, dark lines are produced. Thus we do not see the spectrum of the solar atmosphere, but we see a negative image of it. This, however, serves equally well to determine with certainty the presence of those metals which occur in the sun's atmosphere. For this purpose we only require to possess an accurate knowledge of the solar spectrum, and of the spectra of the various metals.

"I have been fortunate enough to obtain possession of an apparatus from the optical and astronomical manufactory of Steinheil in Munich, which enables me to examine these spectra with a degree of accuracy and purity which has certainly never before been reached. The main part of the instrument consists of four large flint-glass prisms, and two telescopes of the most consummate workmanship. By their aid the solar spectrum is seen to contain thousands of lines; but they differ so remarkably in breadth and tone, and the variety of their grouping is so great, that no difficulty is experienced in recognizing and remembering the various details. I intend to make a map of the sun's spectrum as I see it in my instrument, and I have already accomplished this for the brightest portion of the spectrum—that portion, namely, included between Fraunhofer's lines F and D. By painting the lines of various degrees of shade and of breadth, I have succeeded in producing a drawing which represents the solar spectrum so closely, that, on comparison, one glance suffices to show the corresponding lines.

"The apparatus shows the spectrum of an artificial source of light, provided it possess sufficient intensity, with as great a degree of accuracy as the solar spectrum. A common colourless gas-flame in which a metallic salt volatilizes, is in general not sufficiently luminous; but the electric spark gives with great splendour the spectrum of the metal of which the electrodes are composed. A large Ruhmkorff's induction apparatus produces such a rapid succession of sparks, that the spectra of the metals may be thus examined with as great facility as the solar spectrum.

"By means of a very simple arrangement, the spectra of two sources of light may be compared. The rays from one source of light can be led through *one* half of the vertical slit, whilst those from another source are led through the *other* half. If this is done, the two spectra are seen directly under one another, separated only by an almost invisible dark line. By this arrangement it is extremely easy to see whether any coincident lines occur in the two spectra.

"I have in this way assured myself that all the bright lines characteristic of iron correspond to dark lines in the solar spectrum. In that portion of the solar spectrum which I have examined (between the lines D and F), I have had occasion to remark about seventy particularly brilliant lines as caused by the presence of iron in the solar atmosphere. Angström only observed three bright lines in this part of the spectrum of the electric spark; Masson noticed only a few more; Van der Willigen says that iron produces only a very few feeble lines in the spectrum of the electric spark. From the number of these lines which I have been able to observe with ease, and map with absolute certainty, some idea may be formed of the capabilities of the instrument which I am fortunate enough to possess.

"Iron is remarkable on account of the number of the lines which it causes in the solar spectrum; magnesium is interesting because it produces the group of Fraunhofer's lines which are most readily seen in the sun's spectrum, namely, the group in the green, consisting of three very intense lines to which Fraunhofer gave the name *b*. Less striking, but still quite distinctly visible, are the dark solar lines coincident with the bright lines of chromium and nickel. The occurrence of *these* substances in the sun may therefore be regarded as certain. Many metals, however, appear to be absent; for although silver, copper, zinc, aluminium, cobalt, and antimony possess very characteristic spectra, still these do not coincide with any (or at least with any distinct) dark lines of the solar spectrum. I hope before long to be in a position to publish more extended information on this point.

"The combination of Ruhmkorff's induction coil with the spectrum apparatus will doubtless also be of importance for the chemistry of terrestrial matter. Very many metallic compounds do not give the spectrum peculiar to the metal when placed in a flame, because they are not sufficiently volatile, but they give it at once when placed on the electrodes of an electric spark. These lines are then indeed seen, together with those of the metal of the electrode, and those of the air through which the spark passes; and owing to the great number of bright lines which compose the spectrum of every electric spark, it would be almost impossible, without a special arrangement, to distinguish the lines caused by the metal of the electrodes from those produced by the metallic salt added. The special arrangement which in this case removes all difficulty, consists in allowing the spark to pass at the same instant between two pairs of electrodes, in such a manner that the light of one spark passes through the upper half of the slit, whilst the light of the other spark passes through the lower half of the slit, so that the two spectra are seen one directly above the other. If both pair of electrodes are pure, both the spectra are alike; if a metallic salt is brought on to one of the electrodes, the lines peculiar to that metal appear in the one spectrum in addition to those present before. These are recognized at the first moment, because they are absent in the other spectrum. The lines which are common to the two spectra may serve, when they are once for all drawn, as the simplest mode by which to represent the position of the lines of the other metals employed.

"I have proved that in this way the metals of the rare earths, yttrium, erbium, terbium, &c., may be detected in the most certain and expeditious manner. Hence we may expect that, by help of Ruhmkorff's coil, the spectrum-analytical method may be extended to the detection of the presence of all the metals. I trust that this expectation may be borne out in the continuation of the research which Bunsen and I are jointly carrying on with the object of rendering this method practically applicable."

XXIX. *On Ripples, and their relation to the Velocities of Currents.* *By* T. ARCHER HIRST, *Mathematical Master at University College School, London.*

[Concluded from p. 20.]

[With a Plate.]

21. IN art. 20 it was shown that when a jet or partially immersed solid cylinder is made to describe a circle of radius a in still water, the ripple it produces has a cusp C (fig. 7, Plate I.), which describes another circle whose radius r has to a the same ratio that the velocity λ, with which the waves causing the ripple are propagated, has to the velocity u of the jet; and further, that the angle θ between the radii to the jet and to the cusp varies with r, in accordance with the relation

$$\tan(\phi_1-\theta)=\phi_1=\frac{\sqrt{a^2-r^2}}{r}. \quad . \quad . \quad . \quad . \quad (40)$$

Hence if the axis of polar coordinates pass through, and rotate with the jet, this is the equation of a curve upon which the cusp of the ripple must lie, no matter with what velocity the jet may be rotating. The curve itself, as may be easily shown (Plate IV. fig. 2), lies entirely within the circle (a); it commences at the jet A, $r=a$, $\theta=0$, where it touches the axis O A, and proceeds inwards, turning its convexity towards the centre O, until on arriving at a point of inflexion i, whose coordinates are $r=\frac{a}{\sqrt{2}}$, $\theta=1-\frac{\pi}{4}$ (equivalent to 12° 17′ 44″ nearly), it commences and continues ever after to turn its concavity towards the centre O, which point it approaches asymptotically. In comparing the foregoing results with those of experiment, the above curve was found to be of service.

Experiments.

22. I proceed to describe a few experiments which will serve to verify the general principles upon which the foregoing results are based, though they are by no means sufficiently complete and accurate to decide, fully, the interesting question as to the velocities of differently shaped waves.

23. The experiments on the ripples produced by a jet or solid cylinder rotating in still water were made with the modification of Barker's mill represented in section by fig. 1, Pl. IV. D C C′ D′ is a tin tube 17 inches long and 2 inches in diameter, whose lower extremity, contracted to a diameter of 1 inch, is soldered into a hollow brass cap D D′ E′ E. Into this cap three brass tubes are fitted; one vertical, projecting 6 inches into the interior of the tin tube, and closed at its upper extremity by a plate F of hard steel; and two, D B and D′ B′, horizontal, in the same straight line, about $4\frac{1}{2}$ inches long, $\frac{3}{10}$ths of an inch in diameter, and terminated by two brass caps A B and A′ B′, which slide over the tubes and can be removed at pleasure. In each cap a lateral aperture is made, into which can be fitted either a smaller brass tube suitable for the issue of a jet $\frac{1}{10}$th of an inch in diameter, or a solid brass cylinder of the same diameter, which completely prevents the efflux of water. The whole weight of the portion of the instrument thus far described, and of the water it may contain, is supported by a steel pin O F about 8 inches long, whose hardened and pointed upper extremity enters a small cavity in the centre of the steel plate F, whilst its lower extremity is screwed firmly into a lead weight M N, placed at the bottom of a bath P Q R S. By this arrangement, the water in the tin tube issuing at one or both of the orifices A, A′ causes a rotation unaccompanied by sensible oscillations, and the issuing jets of water, by falling into the water in the bath, produce the ripples whose forms are to be examined. When a constant velocity of rotation is required, it is merely necessary to retain the same depth of water in the tin tube; and this can be done either by regulating, with a cock, the supply of water entering at H, or by carrying a siphon through H to a reservoir kept at a constant level. When necessary, too, the water in the bath can be kept at a constant level by similar means.

24. The moment the instrument rotates with sufficient velocity the ripples make their appearance, and the general resemblance between their forms and that of the one drawn according to theory in fig. 7, Plate I., is very striking. The external branch A X, and the portion of the internal one between the jet A and the cusp C are quite distinct and well defined; the return branch C B D is far less distinct, the height of this part of the ripple being small, and its breadth great; its existence, however, may be established by a closer inspection, or by a glance at the distorted image at the bottom of the bath caused by the irregular refraction of the light incident upon the rippled surface. Besides the principal ripple there are a number of secondary ones, similar in form but less in extent, which precede the former, and of course tend to diminish its prominence.

25. In order to compare the actual with the theoretical ripple more precisely, the following simple expedient was adopted:—A circular plate of glass K L a foot in diameter, and having a circular aperture at its centre to admit the screw O, was attached by means of this screw to the top of the lead weight. Its lower surface was silvered, and the silvering afterwards coated with shell-lac to protect it from the action of the water. The image of the ripple in this plane mirror was not only more distinct than the ripple itself, but the arrangement had the additional advantage of obviating errors due to parallax, since the image of the observer's eye could always be made to coincide with that of any part of the ripple under examination. Lastly, a second and transparent glass plate A A′, 10 inches in diameter, was attached by means of a screw E E′ to the cap at the bottom of the mill. This plate turned of course with the mill, and could be graduated in any required manner; it had a small indentation at a point in its circumference, in order that the axis of one, A, of the vertical jets (or cylinders) might be made to describe, precisely, the circumference of a circle 5 inches in radius.

26. By means of the equations (32) and (40), one or two ripples A a C and A $a_{\prime}$ $C_{\prime}$ (fig. 2), and the locus A C $C_{\prime}$ of their cusps, were carefully plotted on a sheet of paper; they corresponded to a value of $a=5$ inches, and in the case of the ripples, to arbitrarily assumed values of the ratio α between the velocities λ and u of the wave and jet. These curves were next transferred, in white paint, to the glass plate A A′, and one of the jets A made to coincide with their origin. The instrument being now filled with water, the jet at A′ was made to issue horizontally in order to turn the mill, whilst the jet at A descended vertically into the bath and produced the ripple to be examined. As the velocity of rotation diminished, the cusp of the actual ripple described very accurately the curve $C_{\prime}$ C A traced on the glass plate A A′; and

with equal accuracy the portion of this ripple between the jet and cusp coincided successively with the corresponding portions $A\,a_{\prime}\,C_{\prime}$, $A\,a\,C$ of the theoretical ripples. To test the coincidence of the portion of the ripple outside the circle described by the jet (A X E in fig. 7, Plate I.), a hole was drilled through the glass plate A A′ at a distance of $2\frac{1}{2}$ inches from its centre, and a solid cylinder thrust through the same so as to be partially immersed in the water of the bath. The mill was turned as before by the jet A′ alone, the aperture at A being closed; and when the velocity of rotation was properly regulated, it was found that the complete actual ripple produced by the immersed cylinder coincided very well with as much of the theoretical one as could be traced upon the glass plate.

27. The slight divergence from perfect coincidence between the theoretical and actual ripples being sufficiently accounted for by the conditions under which the experiments were made—the limited extent of the bath, the disturbing effect of the portion of the axis O F immersed in the water, and the magnitude of the jet (which in theory was disregarded),—the accuracy of the principles upon which the foregoing theory is based may be considered as sufficiently established, and we may proceed at once to give an account of the measurements from which an approximate value of the velocity λ with which the waves, producing the ripples in question, are propagated may be deduced.

For this purpose the curves on the glass plate A A′ were obliterated, and replaced by concentric circles whose radii increased by half inches from 1 to 5 inches. A constant rotation was secured in the manner described in art. 23, and its velocity determined in each case by counting the number n of rotations made in a given time t, the latter being measured by an accurate timepiece, the motion of whose index could be instantaneously arrested or renewed by simply pressing or releasing a spring.

Since each jet issues at right angles to the horizontal arm B B′, it is clear that its effect, as far as the rotation of the mill is concerned, depends not only upon the height of the column of water in the tin tube, but also upon the inclination of the jet to the horizon; for upon the former depends the velocity of efflux, and the uncompensated pressure against the part of the horizontal tube opposite to the orifice through which the jet issues, whilst upon the latter depends the magnitude of the horizontal component of this pressure, which component is alone effective in producing rotation. But every change in the velocity of efflux and in the inclination of the jet to the horizon produces a corresponding change in the distance, from the axis, of the point at which the jet falls into the water in the bath, and consequently in the radius a of the circle described by the jet. This radius, too, also depends upon the height of the water in the bath; so that at first sight it would appear, not only that the latter ought to be kept constant throughout one and the same experiment, but that a separate determination of a should be made in each case.

On setting both jets in action, however, at different inclinations, and comparing the two ripples which they produced, it was soon found that, however much the radii a and a' of the circles described by the jets might differ in magnitude, the radii r and r' of the circles described by the cusps of their ripples were in all cases *nearly* equal. This result is confirmed by theory, which also shows that the slight difference between the radii r and r' is due, solely, to the slight difference between the values of the velocities λ and λ' with which the waves, produced under these different circumstances, are propagated. In fact it follows at once from equation (35) of art. 20, that

$$\frac{\lambda}{u}:\frac{\lambda'}{u'}=\frac{r}{a}:\frac{r'}{a'};$$

and the angular velocity of rotation being the same for each jet, their actual velocities u and u' will clearly be proportional to the radii a and a' of the circles they describe; the above proportion, therefore, reduces itself to the simpler one,

$$\lambda:\lambda'=r:r', \qquad \dots\dots \qquad (41)$$

and thus verifies the above remark. When one jet descended vertically and the other issued horizontally, the difference between r and r', although still very small, was at a maximum, and the cusp of the ripple produced by the vertical jet was always furthest distant from the axis; from which we may conclude that the velocity λ is greater when the jet descends vertically into the water of the bath, than when it strikes the surface of the latter obliquely. When the vertical jet was replaced by a solid cylinder, the difference between the positions of the cusps remained about the same as before, thus indicating that the velocity λ was appreciably the same for a jet and for a solid cylinder.

It is also worth noting here, that when the distance, from the axis, of the cusp of the ripple produced by the oblique jet exceeded the radius (5 inches) of the circle described by the vertical jet, the latter *produced no ripple whatever.* The reason of this has already been given in art. 13; and in accordance with the statement there made, it was found that in all such cases the velocity of the vertical jet was less than 7½ inches per second, which, as we shall see, is the mean value of the velocity λ with which the waves produced by each jet are propagated. Attempts were made to regulate the velocity of rotation so that the cusp of the ripple produced by the oblique jet should be exactly 5 inches distant from the axis. Theoretically, the ripple corresponding to the vertical jet should then have been the *involute of a circle* (art. 13); in reality, however, the ripple was scarcely distinguishable in such cases, the disturbance which the vertical jet produced upon the surface of the water in the bath being still so small.

28. Although the apparatus was sufficiently delicate to establish the fact of slight variations in the velocity λ due to differences in the obliquity of the jet and the velocity of efflux, it did not appear suitable for the full investigation of the magnitude and conditions of these variations; I contented myself therefore with a few determinations of λ from a series of observations made under all possible conditions. From them the limits of the variation of λ may be to some extent ascertained and its mean value estimated. In order to calculate λ from the data supplied by experiment, the formula

$$\frac{\lambda}{u}=\frac{r}{a},$$

given in art. 20, requires a slight transformation. The circumference of the circle described by the jet being $2\pi a$, and n rotations being made in t seconds, the velocity u has the value

$$u=\frac{2\pi an}{t},$$

so that the above equation giving the velocity λ per second becomes

$$\lambda=2\pi\frac{nr}{t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

According to this, and as already hinted in the last article, the velocity λ depends, solely, upon the number of rotations in a given time, and upon the distance r of the cusp of the ripple from the axis of the instrument; in other words, it is not dependent upon the radius of the circle described by the jet; so that it was not necessary to determine the distance, from the axis, of the point of impact between the jet and the water in the bath, and therefore not necessary to ascertain the height of the water in the bath, or to render the latter constant.

29. Some of the results of a great many accordant observations are shown in the following Table, wherein n represents the number of rotations per minute, r the distance (in inches) from the axis of the cusp of the ripple, and λ the velocity in inches per second with which the corresponding waves are propagated, calculated according to (42):—

n.	r.	λ.
78	0·5	4·1
63	0·8	5·5
59	0·9	5·6
56	1·0	5·9
53	1·2	6·5
50	1·3	6·6
42	1·5	6·6
41	1·6	6·9
32	2·1	7·0
31	2·3	7·3
28	2·5	7·4
26	2·7	7·4
25	2·8	7·4
24	2·9	7·3
22	3·2	7·3
21	3·5	7·7
20	3·7	7·7
18	4·0	7·5
15	4·4	7·8

Of these results the first five or six are less trustworthy than the rest,—and this for several reasons, amongst which the following may be cited:—The rapidity of the rotation renders the determination of the position of the cusp more difficult; the proximity of the immersed axis interferes with the clear definition of the cusp; and lastly, the consequences of a small error in estimating the distance of this cusp from the axis increase as this distance diminishes, since the cusp approaches the axis asymptotically (art. 21). On the other hand, the rapid diminution of λ shown in the Table, can in some measure be accounted for by the fact that, in order to avoid disturbing too much the surface of the water in the bath, it was found necessary, with rapid rotations, to use only one jet, and it of course issued at a small angle towards the horizon, so that the impact between the jet and the water in the bath was necessarily a very oblique one. But, as already mentioned in art. 20, the tendency of this obliquity is to diminish the velocity λ. There can be no doubt that the rapid diminution of λ in the Table is to be ascribed chiefly to these causes; though it is also worth mentioning that the property of the wave insisted upon by Weber, *i. e.* that its velocity diminishes as its radius increases (see art. 2), would also tend to produce the effect observed.

In the last twelve observations recorded in the Table, two jets (or a jet and a cylinder) were used, and the ripple of the vertical one was chiefly examined. As a consequence, the values of λ vary far less, and 7·5 inches per second may be taken as a mean value of the velocity with which waves thus produced are propagated.

30. As an interesting coincidence, it may be mentioned that Poisson, in his memoir before referred to*, gives the results of four experiments on the velocity of waves made by Biot. In this case the waves were produced by suddenly withdrawing from the water a partially immersed solid of revolution. The velocity of the wave was found to vary with the form of the body, and with the radius of its section at the water's level. In one case, the body being an ellipsoid, and the radius in question equal to $\frac{3}{4}$ of an inch, the velocity was about $5\frac{1}{2}$ inches per second; in another case, where the body was a sphere and the radius of the section $1\frac{1}{5}$ inch, the velocity was 7·87 inches per second. The waves experimented upon by Weber had a far greater velocity: they were produced by allowing a column of liquid suspended in a tube to descend suddenly into the general mass; and their velocities varied from 17 to 34 inches per second.

31. In the foregoing experiments, the velocity λ was determined by causing an immersed cylinder to move with a given velocity in still water. A few experiments were next made with a view of ascertaining the value of λ when a cylinder is simply immersed in a current of known velocity. According to art. 10, the sine of half the angle 2θ between the branches of the ripple caused by immersing a cylinder in a current is inversely proportional to the velocity v of the current, and directly proportional to the velocity λ in question. In fact it was there shown that

$$\lambda = v \sin\theta = v\frac{\tan\theta}{\sqrt{1+\tan^2\theta}}. \quad . \quad . \quad . \quad (43)$$

To determine the velocity v at any point of the surface of a current, a Wollaston's current-meter was used. As is known, this instrument consists of a screw which is made to rotate by the force of the current. The instrument must be immersed to the depth of two inches at least, in order that the screw may be completely covered by the water; and when so immersed, the rotation of the screw can be communicated at any instant to a divided wheel, and the communication as suddenly broken. The space described by the current during the interval between making and breaking this communication—an interval which can be measured by means of an ordinary watch with a seconds' hand—is at once read off on the wheel, the instrument having been previously carefully graduated.

The angle θ was determined by means of a simple instrument, to which we may give the name of *ripple-meter*. It consisted of a glass plate (BCDE, Pl. IV. fig. 3) 5 inches square, through which, at a point A, a hole $\frac{1}{10}$th of an inch in diameter was drilled in

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* *Mémoires de l'Acad. Roy. des Sciences de l'Institut.* Année 1816, vol. i. p. 173.

order to insert a solid cylinder of ivory about 1 inch long. Passing through the centre of A and parallel to the sides of the glass, a fine line A *a*, an inch long, was scratched on its surface with a diamond point; and through the extremity *a* of this line another, *m n*, was drawn perpendicular to the former, and graduated on each side from *a* into tenths of an inch. In using this ripple-meter, the plate of glass was held close and parallel to the surface of the current, so that the ivory pin, by becoming partially immersed, might cause a well-defined ripple, *b* A *c*, visible through the glass; the plate was then turned until A *a* bisected the angle between the branches of this ripple, when of course the ratio $\frac{ac}{a\mathrm{A}}$ gave at once the tangent of the required angle θ. In this case, as in that of the rotating jet, a number of secondary ripples are also visible: after a little practice, however, and when the current was not too slow (the angle *b* A *c* too obtuse), it was not difficult to estimate, approximately, the value of tan θ corresponding to the principal ripple.

The following Table contains a few of the best results of many experiments made on streams. In it the first column gives the values of tan θ as read off on the ripple-meter; the second column shows the corresponding values of the velocity v as indicated in feet per minute by the current-meter; and the third column contains the respective values of λ, calculated according to the formula (43), in inches per second.

tan θ.	v.	λ.
·993	44	6·2
·775	55½	6·8
·705	59	6·8
·577	67	6·7
·545	71	6·8
·392	96	7·0
·388	105	7·6

The observations, of which the above are some of the most trustworthy results, were made at different periods on streams in Hampshire and Gloucestershire. When we take into consideration the fact that a stream could rarely be found where the velocity at any one point remained constant, that in all cases this velocity was determined with the current-meter at a point 2 or 3 inches below the surface, and lastly, that the method of estimating the angle formed by the branches of the ripple can only lead to approximate results, the general agreement of the values of λ with those obtained from the rotatory experiments is as close as could be expected. The results appear to indicate that, with one and the same immersed body, the value of λ varies somewhat with the velocity of the current; but there can be little doubt that, under more favourable circumstances, this variation would be found to be far less than that indicated by the Table.

In many cases no ripple whatever was produced by the immersion of the cylinder of ivory attached to the ripple-meter; and in all such cases the current-meter indicated a velocity less than 7 inches per second; the quickest of such currents in fact had only a velocity of 25 feet per minute, or 5 inches per second. This corroborates the explanation given in art. 11, where it was foreseen that a body immersed in a current whose velocity was less than that of the wave, would allow the water to flow past it without visibly rippling its surface.

The phenomenon represented by the second figure of art. 10, where the branches of the ripple turn their concavities towards each other, and which was shown to be a necessary consequence of Weber's statement, that the velocity of the wave diminishes as its radius increases, was never observed. A slight concavity, however, might easily have escaped detection.

32. The velocity λ being once determined for any ripple-meter, we can of course by its means determine, conversely, the velocity of a current, provided the latter exceeds the limit λ. For this purpose I made use of a more convenient, though perhaps less accurate ripple-meter, with a description of which I will conclude the present paper. A B and A C in fig. 4 represent two strips of brass, each 3 inches long, and made to turn round A. The ends B and C of these strips also turn on axes at the extremities of two brass stirrups B F and C G, through which passes a wooden scale D E divided into twentieths of an inch. The cylinder, of the same dimensions as before, by whose immersion in the current ripples are produced, is pushed through an aperture in the joint A. The stirrup B F being fixed at the zero of the scale, is held there by a clamping screw F, and the stirrup C G is made to slide along the scale until the strips A B, A C are parallel to the branches of the ripple. This adjustment once made, the distance B C, as read off from the scale, being directly proportional to $2 \sin \theta$, is clearly inversely proportional to the velocity of the current (arts. 10 and 31).

The decrease in velocity from the centre to the banks of a stream is clearly indicated by this little instrument. As an illustration, I give the results of a few observations made on a mill stream at Brimscombe near Stroud. A wooden plank was thrown across the stream, which was about 7 feet 6 inches wide, and upon it, commencing at one bank, marks were made 9 inches apart. By kneeling on the plank, the ripple-meter was immersed in the current exactly under each mark. In explanation of the following Table, it is only necessary to add that the first column N shows the number of the mark on the plank; the second column D the distance, in inches, of that mark from the bank; the third column d the depth, in inches, of the stream at each mark; the fourth column the values proportional to $2 \sin \theta$ as read off on the ripple-meter; and the last column v the velocities in feet per minute, of the current as calculated, on the hypothesis of $\lambda = 7{\cdot}5$ inches per second, by formula (43):

N.	D.	*d.*	$2 \sin \theta$.	*v.*
0	0	11	...	...
1	9	16	98	63
2	18	18	74	83
3	27	22	77	80
4	36	24	67	92
5	45	$23\frac{1}{2}$	60	103
6	54	18	57	108
7	63	14	59	104
8	72	13	68	91
9	81	13	92	65
10	90	12	...	...

In conclusion it may be added that, if desired, more accurate ripple-meters might easily be devised, and by means of such the velocities of currents might be determined from the forms of their ripples with a degree of precision little, if at all, inferior to that possessed by the methods now in use. It is from a theoretical point of view, however, that the relation between waves and ripples, which we have endeavoured to establish, promises the greatest interest. For there can be little doubt that a skilful experimenter, pursuing the subject in this direction, would greatly extend our present knowledge with respect to the changes in the velocity of a current at different points of its surface, and especially with respect to the velocities with which different kinds of waves are propagated on the surface of still water, and to the variation of this velocity during the propagation of one and the same wave.

January 15, 1861.

XXX. *On the Equilibrium of a Fluid Mass revolving freely within a Hollow Spheroid about an Axis which is not its Axis of symmetry.* *By* G. R. DAHLANDER[*].

IF we suppose a fluid ellipsoid to revolve alone about an axis which is not an axis of symmetry, we easily perceive that it cannot assume a position of equilibrium. It is, however, dif-

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[*] Communicated by the Author.

ferent if we suppose the fluid mass surrounded by a hollow spheroid, the two bounding surfaces of which are not concentric. The fluid mass can then, under certain circumstances, actually assume a position of equilibrium, even when its axis of rotation is entirely external to the mass itself, and when it thus plays the part of an inner satellite to the hollow spheroid, with which it has a common motion of rotation. The existence of such singular states of equilibrium we will now demonstrate.

Suppose the hollow spheroid, together with the internal fluid mass, to revolve about the axis of the outer bounding surface of the spheroid, the axis of the inner bounding surface being parallel to the axis of rotation. The fluid mass can then assume a position of equilibrium in which its figure is that of an ellipsoid of rotation, whose axis is parallel to the above-mentioned axes.

Let the centre of the outer bounding surface be the origin of a system of rectangular coordinates, the axis of z coinciding with the axis of rotation. Let m, n, and p be the coordinates of the centre of the inner bounding surface, and α, β, γ those of the centre of the fluid mass. The equation of the surface of the fluid will then be

$$\frac{(x-\alpha)^2+(y-\beta)^2}{a^2}+\frac{(z-\gamma)^2}{b^2}=1. \quad . \quad . \quad . \quad (1)$$

If the component parts of the attraction parallel to the axes be denoted by X, Y, Z, we have

$$\begin{aligned} \mathrm{X}&=-\mathrm{M}x+\mathrm{M}'(x-m)-\mathrm{M}''(x-\alpha), \\ \mathrm{Y}&=-\mathrm{M}y+\mathrm{M}'(y-n)-\mathrm{M}''(y-\beta), \\ \mathrm{Z}&=-\mathrm{N}z+\mathrm{N}'(z-p)-\mathrm{N}''(z-\gamma). \end{aligned}$$

If therefore the angular velocity be denoted by w, we get for the differential equation of the *surfaces de niveau*,

$$\begin{aligned} &\big(-\mathrm{M}x+\mathrm{M}'(x-m)-\mathrm{M}''(x-\alpha)\big)dx+\big(-\mathrm{M}y+\mathrm{M}'(y-n) \\ &\quad -\mathrm{M}''(y-\beta)\big)dy+\big(-\mathrm{N}z+\mathrm{N}'(z-p)-\mathrm{N}''(z-\gamma)\big)dz \\ &\quad +w^2(xdx+ydy)=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2) \end{aligned}$$

where M, M′, M″, N, N′, and N″ are independent of x, y, z. Consequently by integration we get

$$\begin{aligned} &(-\mathrm{M}+\mathrm{M}'-\mathrm{M}''+w^2)(x^2+y^2)+(-\mathrm{N}+\mathrm{N}'-\mathrm{N}'')z^2 \\ &\quad +2(-\mathrm{M}'m+\mathrm{M}''\alpha)x+2(-\mathrm{M}'n+\mathrm{M}''\beta)y \\ &\quad +2(-\mathrm{N}'p+\mathrm{N}''\gamma)z=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3) \end{aligned}$$

As equations (1) and (3) are to be identical, we have the following equations of condition:—

$$\left.\begin{aligned}
-\alpha &= \frac{-\mathrm{M}'m+\mathrm{M}''\alpha}{-\mathrm{M}+\mathrm{M}'-\mathrm{M}''+w^2},\\
-\beta &= \frac{-\mathrm{M}'n+\mathrm{M}''\beta}{-\mathrm{M}+\mathrm{M}'-\mathrm{M}''+w^2},\\
-\gamma &= \frac{-\mathrm{N}'p+\mathrm{N}''\gamma}{-\mathrm{M}+\mathrm{M}'-\mathrm{M}''+w^2}\,\frac{b^2}{a^2},\\
\frac{a^2}{b^2} &= \frac{-\mathrm{N}+\mathrm{N}'-\mathrm{N}''}{-\mathrm{M}+\mathrm{M}'-\mathrm{M}''+w^2}.
\end{aligned}\right\} \quad \ldots\ldots \quad (4)$$

From which we obtain

$$\left.\begin{aligned}
\alpha &= \frac{-\mathrm{M}'m}{\mathrm{M}-\mathrm{M}'-w^2},\\
\beta &= \frac{-\mathrm{M}'n}{\mathrm{M}-\mathrm{M}'-w^2},\\
\gamma &= \frac{-\mathrm{N}'p}{\mathrm{N}-\mathrm{N}'}.
\end{aligned}\right\} \quad \ldots\ldots\ldots \quad (5)$$

From the first two of equations (5) it follows that

$$\alpha:\beta=m:n.$$

The geometrical signification of this proportion is, that the centre of the fluid mass be in the plane which passes through the axis of rotation and the centre of the inner bounding surface of the spheroid. We find, moreover, that the position of this centre in a state of equilibrium with a fixed angular velocity is independent of the density of the fluid, supposing the form and density of the outer spheroid to be the same. Further, we find that if a fluid ellipsoid satisfy the conditions of equilibrium, all other similar ellipsoids of the same density will also satisfy these conditions. Generally, from equations (5), real finite positive or negative values of α, β, γ can be obtained if M, M$'$, N, N$'$, m, n, p, and w are given. But one or more of these values may become infinite under certain circumstances. This is the case when the two bounding surfaces of the solid spheroid are similar. Then $\mathrm{M}=\mathrm{M}'$ and $\mathrm{N}=\mathrm{N}'$, whence the value for γ would be infinite, unless at the same time $p=0$, in which case γ can have any value whatever.

The last of equations (4) constitutes the real equation of condition, which determines the relation which must subsist between the density, form, and angular velocity of the fluid mass, in order that equilibrium may be possible for the given values of M, M$'$, N, and N$'$. We shall separately consider the particular case when both the bounding surfaces of the surrounding spheroid are similar.

From what has before been stated, it is evident that p becomes $=0$.

We can take for the axis of y a line which lies in the plane passing through the centre of the fluid mass and the centres of the bounding surfaces. In this case $n=0$ and $\beta=0$, and also $M=M'$ and $N=N'$. We shall now examine if an oblate ellipsoid of rotation can satisfy the conditions of equilibrium. Supposing $\frac{a^2}{b^2}=1+\lambda^2$, and the density of the fluid $=\rho$, then the last of equations (4) will become

$$1+\lambda^2=\frac{-\frac{4}{3}\pi\rho f\frac{(1+\lambda^2)}{\lambda^3}(\lambda-\operatorname{arc}\tan\lambda)}{-\frac{2}{3}\pi\rho f\frac{1+\lambda^2}{\lambda^3}\left(\operatorname{arc}\tan\lambda-\frac{\lambda}{1+\lambda^2}\right)+w^2}.$$

If $\frac{w^2}{2\rho\pi f}$ be taken $=E$, the equation of condition becomes

$$E=\frac{\operatorname{arc}\tan\lambda}{\lambda^3}(\lambda^2+3)-\frac{3}{\lambda^2}. \quad . \quad . \quad . \quad . \quad (6)$$

But this equation is just the same as that we obtained in determining the conditions of equilibrium of a freely revolving fluid mass whose particles attract each other. Thus we find that precisely the same conditions of equilibrium are involved when the fluid is revolving in a hollow ellipsoid with similar but eccentric bounding surfaces, and when it is perfectly free. To a given value for λ there is therefore always a corresponding angular velocity; and to a given angular velocity there corresponds either no ellipsoid, or one ellipsoid, or two different ellipsoids, according as E is $\overset{>}{\underset{<}{=}}$ 0·2246.

Between the rotation of a fluid mass confined in a hollow spheroid and a mass which revolves freely, there is, however, this important difference, that in the former case the rotation does not take place about the axis of symmetry of the fluid unless both the bounding surfaces of the spheroid are concentric, but about a parallel axis which is the axis of symmetry of the outer bounding surface of the spheroid,—the distance between the two axes being

$$\alpha=\frac{M'm}{w^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

whence

$$\alpha w^2=M'm.$$

But $M'm$ is the attraction which the hollow spheroid exerts on any point within it, and αw^2 is the centrifugal force at the axis of symmetry of the fluid mass. Whence we find that at the centre of the fluid mass the acting forces counterbalance each other, which might have been anticipated from a known theorem in mechanics.

Gothenburgh, January 7, 1861.

XXXI. *Remarks on* Sainte-Claire Deville's *Theory of Dissociation.* *By* Thomas Woods, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Parsonstown, February 1861.

In an interesting paper by Sainte-Claire Deville published in this Magazine last December, that author gives his views on the decomposition of bodies by heat. His idea of the relation and behaviour of the constituents forming a compound, towards each other, is, physically considered, the same as that which I published in this Journal so long ago as January 1852; that is, that they act merely as the molecules of a simple body, differing in nothing from the latter, except that, being diverse, they are capable of attaining a greater proximity among themselves, and so of causing a greater opposite movement in the particles of other bodies. A reference to my paper will show a diagram I gave in order to explain this similarity of constitution. The paper was therefore the more interesting to me as it brings forward fresh ideas on a thought-of subject. As I do not, however, yet agree with what is new in it, I beg to offer a few remarks on some of its contents; and first with respect to his theory of *dissociation*.

Sainte-Claire Deville thinks that compound gases and vapours, when heated to a certain temperature, as steam at 1000°, undergo some such change as a solid body does when it liquefies; that the *constituent* particles being removed from each other, as well as the compound particles, the gas loses *stability*, and that heat is rendered latent thereby. This condition he calls the *dissociated* state. I do not find he offers any demonstration of this state, but that he only ascribes to its influence the production of some phenomena previously otherwise explained. For instance, to account for the heat of chemical combination, he takes for granted, as an example, that the molecules of chlorine and hydrogen are double, and, even at low temperatures, in the dissociated state; and then ascribes the heat produced by their union to the latent heat of this particular condition, which he imagines is given out when the gases by their combination get into a state of stability.

Now, if the heat produced in this instance is due to the change of state of the chlorine, how does it happen that the same amount is produced when the chlorine combines, not being in the gaseous state at all? If hydrochloric acid and zinc are placed together, the chlorine unites with the zinc, and the same quantity of heat is evolved as when the zinc burns in the gas; and the same amount is absorbed by the decomposition as if both the constituents again attained the gaseous state; or if (to take an instance where no gas is present, either in combination or decomposition) zinc causes a deposition of copper from chloride of copper, exactly the same heat is produced by the combination of the chlorine and copper, and exactly the same quantity is absorbed by the decomposition, as if the chlorine and copper acted as gases, changing their state as they combined or decomposed. Unless, therefore, it is imagined that when the chlorine leaves the copper or hydrogen it becomes for a time a gas and enters into the *dissociated* state, absorbing heat, and again becomes solid, giving it up, I cannot see how the temperature is raised. But even granting that it does so the phenomenon could not be accounted for, because when zinc decomposes chloride of hydrogen or chloride of copper, more heat is produced by the combination than is lost by the decomposition: and such could not occur if it were due to the latent heat of dissociation; for the heat would be taken in the first instance from the materials afterwards heated, and so an exchange only, and not an increase, would be effected. It might be said that the zinc influences the result; that is, that metals have a certain amount of heat connected with them which is given out in combining, and that this being greater in some instances than in others, might account for the increase of temperature when the zinc displaces the hydrogen. But if all bodies have definite quantities of heat, as Sainte-Claire Deville seems to think, the same order ought to be observed in the amounts evolved by their combination with the gases. For instance, if an equivalent of chlorine, by uniting with zinc, copper, silver, &c., produces heat, the quantity of which varies in the order in which the metals are named, oxygen ought to do the same, if the heat evolved in combination was previously connected with the combining bodies: but it is known that such is not the case. Chlorine produces more heat with silver than it does with copper, and oxygen the reverse. Instances of this kind might, of course, be multiplied; and they prove, I think, that no fixed amount of heat, resulting either from change of condition, or from latent heat becoming evolved as it does in the condensation of vapour, can be connected with matter as part of its constitution, independent of alteration of the relation of particles.

Deville, in fine, thinks that every body possesses a certain amount of heat, or condition in itself whereby heat can be produced; and therefore *one* of the combining bodies *might* evolve from itself the heat of combination; whereas the theory I published in 1852 divests particles of any influence except that of lessening the distance between themselves, or of destroying volume, as they come together, and so that at least *two* particles of matter are essential to its production. In his theory each body engaged in chemical action is said to give out heat by change of condition; in mine the volume only, or distance between the constituent particles, is supposed to be altered. I still think the latter theory is the more reasonable, and more in accordance with our present scientific knowledge.

M. Deville seems to reject this latter theory, because the contraction arising in chemical combination is not equivalent to the expansion or heat produced; and he calculates the contraction when oxygen and hydrogen unite, to show that it is not of the same value or extent as the increase of volume given to other bodies as the accompanying or opposite movement. He also shows how chlorine and hydrogen unite without contraction at all; yet that expansion in other bodies or rise of temperature is the result. This apparent argument against the theory, however, disappears when it is considered that the particles whose combination evolves the heat are not the same as those which determine the volume. When oxygen and hydrogen unite, these elemental gases themselves, by coming together, cause other bodies to expand, and so are said to give rise to heat; but the *volume* attained by the compound they produce is determined by the distance between, not the oxygen and hydrogen, but between the particles of the water that results. In order, therefore, to calculate the contraction which causes the heat, we should know what takes place between the constituents of the compound: the bulk or volume of the compound itself tells nothing.

An argument, therefore, for some necessary change of state in combining bodies as the cause of heat, drawn from the apparent want of coincidence between the contraction on the one hand and the heat or expansion on the other, is valueless. Besides, I have shown (Phil. Mag., January 1852) that the coefficient of expansion increasing with the dilatation, the nearer particles are to each other, the greater is the effect they produce by a given contraction in causing expansion in other bodies; so that it is not only necessary to know the amount of contraction amongst the constituents of a compound at the time they combine, but also the distance they ultimately arrive at with respect to each other, before we can calculate the amount of heat they ought to produce*.

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* In the last edition of Grove's 'Correlation of the Physical Forces,' when speaking of the theory I brought forward in 1852, to account for the

But this state of *dissociation* is altogether founded on gratuitous assumptions. The ground from which it springs is this: that as compound bodies when heated expand, the constituents must recede from each other as well as the compound particles. But this proposition has yet to be proved. I believe many facts favour an opposite conclusion: for instance, not to speak of the manner in which solids and fluids, when *simple* bodies, expand, being somewhat similar to the same process in *compounds,* Gay-Lussac's law with respect to the equal expansion of all gases and vapours for equal increments of heat, would surely show that the constituents of a compound do not recede from each other in expanding. Hydrogen, or any other simple gas, and vapour of ether, or other compound gas, expand exactly according to the same law. Could this occur with the simple particles of hydrogen to the same extent precisely as with compound molecules of ether, where, instead of two, we have ten elementary atoms to divide the distance and moving force between them? The resistance to expansion of a gas by heat seems to be the weight of the atmosphere; and consequently in all gases, the same resistance being present, the same expansion is attained by a certain increase of temperature. Now in a simple gas the weight is the only resistance; whereas, if Deville's theory is correct, there is in compound gases not only the weight, but the affinity of the constituents to be partly overcome; and yet the same expansion is noticed for the same increase of temperature in both. But this would be impossible, except we imagine that the separation of these constituents does not absorb heat, which we know it does.

It seems to me that this fact alone, of the similar and equal expansion of simple and compound gases by heat, shows that no motion takes place in one which does not occur in the other; therefore that no expansion of the particles themselves, that is, that no separation of the simple constituents of the compound molecule is produced by raising its temperature, and consequently that this state of *dissociation* does not exist.

A consideration of other portions of the paper would lead me too far for the present, but I may recur to it if you think the subject sufficiently interesting for your Magazine.

Your obedient Servant,
THOMAS WOODS, M.D.

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heat of chemical combination, he seems to think it an objection (page 177) that the whole expansion which would be an equivalent to the contraction of the combining particles is not seen in the compound produced; but surely, as this volume would be the *temperature* evolved by the combination, it cannot remain longer than a moment in the compound; it must be dispersed to surrounding bodies.

XXXII. *On the Temperature Correction of Siphon Barometers. By* William Swan, *Professor of Natural Philosophy in the United College of St. Salvator and St. Leonard, St. Andrews**.

In the 'Athenæum' of the 5th of January, Admiral FitzRoy, writing on the subject of the temperature correction of siphon barometers, invites attention to an experiment recently made by Mr. Negretti. A siphon barometer was heated to about 110° from some lower temperature, when it was found that, although the mercury rose in the long or vacuum leg of the siphon, it did not rise, but seemed to be depressed, in the short, or open leg. The late Mr. Robert Bryson of Edinburgh invented a self-registering barometer, which is described in the Transactions of the Royal Society of Edinburgh for 1844†. In that instrument, variations in the pressure of the atmosphere were indicated by means of a float resting on the surface of the mercury in the open leg of a siphon tube, precisely as in the ordinary wheel barometer. Mr. Bryson was anxious to ascertain whether his instrument required any notable correction for temperature; and to settle that point experimentally, a Bunten's barometer was heated to a high temperature. In Bunten's barometer the effective height of the mercurial column is ascertained by reading two verniers; one indicating the level of the upper, and the other that of the lower surface of the mercury in a siphon tube. Mr. Alexander Bryson, who made the experiment, found that the reading of the upper vernier rapidly changed with increase of temperature, while the reading of the lower vernier remained sensibly constant,—proving that the level of the mercury in the open leg of the siphon was very little affected by change of temperature. Mr. Bryson having communicated to me the result of his experiment, I immediately gave him an investigation of the temperature corrections of the two surfaces of the mercury in the siphon barometer, of which the following is substantially a reproduction. As Admiral FitzRoy has expressed some doubts regarding the results of observations of siphon barometers "as hitherto obtained," I have deemed it desirable to make the following investigation perfectly general, so as to include every form of tube; and in the first instance I have avoided employing any formula which is only approximately true.

Let h_1, h_2 be the vertical distances of the upper and lower surfaces of the mercury in a siphon barometer, reckoned from any

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* Communicated by the Author; the results of the investigation having been communicated, on the 2nd of February, to the Literary and Philosophical Society of St. Andrews.

† Vol. xv. p. 503.

horizontal plane below the instrument, and h the barometric pressure, all at a temperature of t degrees Centigrade. When the temperature rises to $t+\Delta t$ degrees, let the above quantities become

$$h_1+\Delta h_1, \quad h_2+\Delta h_2, \quad h+\Delta h;$$

then, if m = cubic expansion of mercury for one degree Centigrade,

$$\Delta h = mh\Delta t.$$

And since

$$h=h_1-h_2$$

and

$$h+\Delta h=(h_1+\Delta h_1)-(h_2+\Delta h_2),$$

we have

$$\Delta h_1-\Delta h_2=\Delta h=mh\Delta t.$$

Now if

c = volume of mercury in the barometer;
a = area of the bore of the tube at upper surface of mercury;
b = area of the bore of the tube at lower surface; all at t degrees;
g_1 = the superficial, and g_2 = the cubic dilatation of glass;

it will be easily seen that, at the temperature $t+\Delta t$ degrees, the capacity of that part of the tube which was occupied by the mercury at t degrees will become $c(1+g_2\Delta t)$; while the capacities of the portions of the tube at the ends of the former mercurial column, which are now filled by the expanded mercury, and whose lengths are Δh_1, Δh_2, will be

$$a(1+g_1\Delta t)\Delta h_1, \quad b(1+g_1\Delta t)\Delta h_2.$$

The whole volume of the mercury will therefore be

$$(a\Delta h_1+b\Delta h_2)(1+g_1\Delta t)+c(1+g_2\Delta t).$$

But the volume of the expanded mercury must also be

$$c(1+m\Delta t);$$

whence

$$(a\Delta h_1+b\Delta h_2)(1+g_1\Delta t)=c(m-g_2)\Delta t.$$

This equation, along with

$$\Delta h_1-\Delta h_2=mh\Delta t,$$

gives

$$\Delta h_1=\frac{\{c(m-g_2)+bmh(1+g_1\Delta t)\}\,\Delta t}{(a+b)(1+g_1\Delta t)},$$

$$\Delta h_2=\frac{\{c(m-g_2)-amh(1+g_1\Delta t)\}\,\Delta t}{(a+b)(1+g_1\Delta t)}.$$

Now since m is greater than g_2, the coefficient of c in the above values of Δh_1, Δh_2 is positive; and the coefficient of h is also positive for all possible values of Δt—g_1 being a very small quantity. It is therefore obvious that Δh_1 can never vanish, but that Δh_2 may be positive, negative, or zero, according to the values which may be assigned to a, c, and h. The depression, by heat, of the mercury in the open leg of the siphon, or in other words, the negative value of Δh_2, observed by Mr. Negretti, and the value zero of the same quantity, observed by Mr. Alexander Bryson, are therefore both perfectly accounted for.

It also appears, and this seems to be of practical importance, that we can altogether get rid of the temperature correction for the lower surface of the mercury, for any *one* given atmospheric pressure, by properly adjusting the value of c, and that thus we shall be able to make the temperature corrections for *all* other pressures exceedingly small.

For this purpose it will be convenient to simplify the expressions for Δh_1, Δh_2 by rejecting small terms. We then obtain

$$\Delta h_1 = \frac{c(m-g_2)+bmh}{a+b} \cdot \Delta t,$$

$$\Delta h_2 = \frac{c(m-g_2)-amh}{a+b} \cdot \Delta t;$$

and Δh_2 will vanish when

$$c = \frac{amh}{m-g_2}.$$

A particular case will best illustrate this. Suppose that the siphon consists of two tubes of a uniform bore a, connected at the bottom by a narrow channel whose capacity may be neglected. We have then

$$c = a(h+2l);$$

$$\Delta h_1 = \left\{(m-g_2)l + (m-\tfrac{1}{2}g_2)h\right\} \Delta t;$$

$$\Delta h_2 = \left\{(m-g_2)l - \tfrac{1}{2}g_2 h\right\} \Delta t;$$

and when $\Delta h_2 = 0$,

$$l = \frac{g_2}{2(m-g_2)} \cdot h.$$

We must now select some particular value of h for which the temperature correction is to vanish; and we shall have, upon the whole, the smallest temperature corrections for *extreme* values of h if we make the temperature correction disappear for its *mean* value. Assuming then $h=29{\cdot}5$ inches as sufficiently near the mean atmospheric pressure, and adopting for the coefficients of cubic expansion of mercury and glass for one degree Centigrade the values

$$m = \cdot 0001803, \quad g_2 = \cdot 0000258,$$

we obtain

$$l = 2\cdot 463 \text{ inches.}$$

This indicates a perfectly practicable arrangement. To render the temperature correction insensible at mean atmospheric pressures when the siphon tube has a uniform bore, we must put so much mercury into the tube, that, when the pressure is 29·5 inches, there shall be a column of about 2·5 inches of mercury in the open leg. The temperature corrections throughout all ordinary fluctuations of atmospheric pressure for the *lower* surface of the mercury will then be extremely small, as will be seen by the following Table:—

Atmospheric pressure (h).	Column of mercury in open leg of siphon (l)	Displacement of upper surface for a difference of temperature Δt in Centigrade degrees.	Displacement of lower surface of mercury for difference of temp. Δt in Cent. degrees.
31 inches.	1·713 inch.	$+\cdot 00545\Delta t$	$-\cdot 00014\Delta t$
29·5 "	2·463 "	$+\cdot 00531\Delta t$	$\cdot 00000\Delta t$
28 "	3·213 "	$+\cdot 00518\Delta t$	$+\cdot 00014\Delta t$

It may be well to observe that the numbers in the last column of the Table show that if the barometer to which they refer were heated, as in the experiments already described, and if the atmospheric pressure were much greater than 29·5 inches, we should have the result obtained by M. Negretti—a depression of the mercury in the short leg of the siphon; while if the pressure were nearly 29·5, there would be no sensible change of level, as observed by Mr. Bryson.

I need scarcely remind the reader, in conclusion, that the formulæ I have investigated are intended to be employed when *only* the upper or *only* the lower surface of the mercury is observed. When *both* surfaces are observed, as in the Bunten barometer, we have simply to apply the ordinary and well-understood correction, due to the expansion of mercury by heat.

United College, St. Andrews,
February 16, 1861.

XXXIII. *Note on* Mr. Jerrard's *Researches on the Equation of the Fifth Order.* *By* A. Cayley, *Esq.*[*]

FUNCTIONS of the same set of quantities which are, by any substitution whatever, simultaneously altered or simultaneously unaltered, may be called *homotypical.* Thus all symmetric functions of the same set of quantities are homotypical: $(x+y-z-w)^2$ and $xy+zw$ are homotypical, &c.

It is one of the most beautiful of Lagrange's discoveries in the theory of equations, that, given the value of any function of the roots, the value of any homotypical function may be rationally determined[†]; in other words, that any homotypical function whatever is a rational function of the coefficients of the equation and of the given function of the roots.

The researches of Mr. Jerrard are contained in his work, "An Essay on the Resolution of Equations," London, Taylor and Francis, 1859. The solution of an equation of the fifth order is made to depend on an equation of the sixth order in W; and he conceives that he has shown that one of the roots of this equation is a rational function of another root: "The equation for W will therefore belong to a class of equations of the sixth degree, the resolution of which can, as Abel has shown, be effected by means of equations of the second and third degrees; whence I infer the possibility of solving any proposed equation of the fifth degree by a finite combination of radicals and rational functions."

The above property of rational expressibility, if true for W, will be true for any function homotypical with W; and conversely. I proceed to inquire into the form of the function W.

The function W is derived from the function P, which denotes any one of the quantities p_1, p_2, p_3. And if x_1, x_2, x_3, x_4, x_5 are the roots of the given equation of the fifth order, and if $\alpha, \beta, \gamma, \delta, \epsilon$ represent in an undetermined or arbitrary order of succession the five indices 1, 2, 3, 4, 5, and if ι denote an imaginary fifth root of unity (I conform myself to Mr. Jerrard's notation), then p_1, p_2, p_3, and the other auxiliary quantities t, u, are obtained from the system of equations—

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[*] Communicated by the Author.

[†] The *à priori* demonstration shows the cases of failure. Suppose that the roots of a biquadratic equation are 1, 3, 5, 9; then, given $a+b=8$, we know that either $a=3$, $b=5$, or else $a=5$, $b=3$, and in either case $ab=15$; hence in the present case (which represents the general case), $a+b$ being known, the homotypical function ab is rationally determined. But if the roots are 1, 3, 5, 7 (where $1+7=3+5$), then, given $a+b=8$, this is satisfied by $\begin{pmatrix} a=3 \\ b=5 \end{pmatrix}$ or by $\begin{pmatrix} a=1 \\ b=7 \end{pmatrix}$, and the conclusion is $ab=15$ or 7; so that here ab is determined, not as before, rationally, but by a quadratic equation.

$$\begin{aligned}
x_\alpha^3+p_1x_\alpha^2+p_2x_\alpha+p_3&=\ \ t+\ \ u,\\
x_\beta^3+p_1x_\beta^2+p_2x_\beta+p_3&=\ \iota t+\iota^4u,\\
x_\gamma^3+p_1x_\gamma^2+p_2x_\gamma+p_3&=\iota^2t+\iota^3u,\\
x_\delta^2+p_1x_\delta^2+p_2x_\delta+p_3&=\iota^3t+\iota^2u,\\
x_\epsilon^2+p_1x_\epsilon+p_2x_\epsilon+p_3&=\iota^4t+\iota u.
\end{aligned}$$

If from these equations we seek for the values of p_1, p_2, p_3, t, u, we have

$$1:p_1:p_2:p_3:-t:-u=\Pi_1:\Pi_2:\Pi_3:\Pi_4:\Pi_5:\Pi_6,$$

where Π_1, Π_2, .. denote the determinants formed out of the matrix

$$\left|\begin{array}{cccccc}
x_\alpha^3, & x_\alpha^2, & x_\alpha, & 1, & 1, & 1\\
x_\beta^3, & x_\beta^2, & x_\beta, & 1, & \iota, & \iota^4\\
x_\gamma^3, & x_\gamma^2, & x_\gamma, & 1, & \iota^2, & \iota^3\\
x_\delta^3, & x_\delta^2, & x_\delta, & 1, & \iota^3, & \iota^2\\
x_\epsilon^3, & x_\epsilon^2, & x_\epsilon, & 1, & \iota^4, & \iota
\end{array}\right|$$

i.e., denoting the columns of this matrix by 1, 2, 3, 4, 5, 6, we have $\Pi_1=23456$, $\Pi_2=-34561$, $\Pi_3=45612$, &c. In particular, the value of Π_1 is

$$=\left|\begin{array}{ccccc}
x_\alpha^2, & x_\alpha, & 1, & 1, & 1\\
x_\beta^2, & x_\beta, & 1, & \iota, & \iota^4\\
x_\gamma^2, & x_\gamma, & 1, & \iota^2, & \iota^3\\
x_\delta^2, & x_\delta, & 1, & \iota^3, & \iota^2\\
x_\epsilon^2, & x_\epsilon, & 1, & \iota^4, & \iota
\end{array}\right|$$

And developing, and putting for shortness $\{\alpha\beta\}=x_\alpha x_\beta(x_\alpha-x_\beta)$, &c., we have

$$\begin{aligned}
\Pi_1=&(\{\alpha\beta\}+\{\beta\gamma\}+\{\gamma\delta\}+\{\delta\epsilon\}+\{\epsilon\alpha\})(-2\iota+\iota^2-\iota^3+2\iota^4)\\
&+(\{\alpha\gamma\}+\{\gamma\epsilon\}+\{\epsilon\beta\}+\{\beta\delta\}+\{\delta\alpha\})(+\iota+2\iota^2-2\iota^3-2\iota^4).
\end{aligned}$$

And this is also the form of the other determinants, the only difference being as to the meaning of the symbol $\{\alpha\beta\}$, which, however, in each case denotes a function such that $\{\alpha\beta\}=-\{\beta\alpha\}$. Writing for greater shortness,

$$\{\alpha\beta\gamma\delta\epsilon\}=\{\alpha\beta\}+\{\beta\gamma\}+\{\gamma\delta\}+\{\delta\epsilon\}+\{\epsilon\alpha\},$$

and in like manner

$$\{\alpha\gamma\epsilon\beta\delta\}=\{\alpha\gamma\}+\{\gamma\epsilon\}+\{\epsilon\beta\}+\{\beta\delta\}+\{\delta\alpha\},$$

Π_1 is an unsymmetric linear function (without constant term) of

$\{\alpha\beta\gamma\delta\epsilon\}$, $\{\alpha\gamma\epsilon\beta\delta\}$; or, what is all that is material, it is an unsymmetric function, containing only odd powers, of $\{\alpha\beta\gamma\delta\epsilon\}$, $\{\alpha\gamma\epsilon\beta\delta\}$.

If for $\alpha\ \beta\ \gamma\ \delta\ \epsilon$ we substitute any one of the five arrangements

$$\begin{array}{l} \alpha\ \beta\ \gamma\ \delta\ \epsilon, \\ \beta\ \gamma\ \delta\ \epsilon\ \alpha, \\ \gamma\ \delta\ \epsilon\ \alpha\ \beta, \\ \delta\ \epsilon\ \alpha\ \beta\ \gamma, \\ \epsilon\ \alpha\ \beta\ \gamma\ \delta, \end{array}$$

then $\{\alpha\beta\gamma\delta\epsilon\}$ and $\{\alpha\gamma\epsilon\beta\delta\}$ will in each case remain unaltered.

But if we substitute any one of the five arrangements

$$\begin{array}{l} \alpha\ \epsilon\ \delta\ \gamma\ \beta, \\ \epsilon\ \delta\ \gamma\ \beta\ \alpha, \\ \delta\ \gamma\ \beta\ \alpha\ \epsilon, \\ \gamma\ \beta\ \alpha\ \epsilon\ \delta, \\ \beta\ \alpha\ \epsilon\ \delta\ \gamma, \end{array}$$

then in each case $\{\alpha\beta\gamma\delta\epsilon\}$ and $\{\alpha\gamma\epsilon\beta\delta\}$ will be changed into $-\{\alpha\beta\gamma\delta\epsilon\}$ and $-\{\alpha\gamma\epsilon\beta\delta\}$ respectively. Hence Π_1 remains unaltered by any one of the first five substitutions; and it is changed into $-\Pi_1$ by any one of the second five substitutions. And the like being the case as regards Π_2, &c., it follows that the quotient $\frac{\Pi_1}{\Pi_2}$, or say P, remains unaltered by any one of the ten substitutions. Now the 120 permutations of α, β, γ, δ, ϵ can be obtained as follows, viz. by forming the 12 different pentagons which can be formed with α, β, γ, δ, ϵ (treated as five points), and reading each of them off in either direction from any angle. To each of the 12 pentagons there corresponds a distinct value of P, but such value is not altered by the different modes of reading off the pentagon; P is consequently a 12-valued function.

But there is a more simple form of the analytical expression of such a 12-valued function; in fact, if $[\alpha\beta\gamma\delta\epsilon]$ be any function which is not altered by any one of the above ten substitutions—if, for instance, $[\alpha\beta]$ is a symmetrical function of x_α, x_β, and

$$[\alpha\beta\gamma\delta\epsilon]=[\alpha\beta]+[\beta\gamma]+[\gamma\delta]+[\delta\epsilon]+[\epsilon\alpha],$$

and $\therefore$

$$[\alpha\gamma\epsilon\beta\delta]=[\alpha\gamma]+[\gamma\epsilon]+[\epsilon\beta]+[\beta\delta]+[\delta\alpha],$$

then any unsymmetrical function of $[\alpha\beta\gamma\delta\epsilon]$ and $[\alpha\gamma\epsilon\beta\delta]$ will be a 12-valued function homotypical with P.

Mr. Jerrard's function W is the sum of two values of his function P; the substitution by which the second is derived from the first can only be that which interchanges the two functions $[\alpha\beta\gamma\delta\epsilon]$ and $[\alpha\gamma\epsilon\beta\delta]$; and hence any symmetrical function of $[\alpha\beta\gamma\delta\epsilon]$ and $[\alpha\gamma\epsilon\beta\delta]$ is a function homotypical with Mr. Jerrard's W; such symmetric function is in fact a 6-valued function only. Indeed it is easy to see that the twelve pentagons correspond together in pairs, either pentagon of a pair being derived from the other one by *stellation*, and the six values of the function in question corresponding to the six pairs of pentagons respectively.

Writing with Mr. Cockle and Mr. Harley,

$$\tau = x_\alpha x_\beta + x_\beta x_\gamma + x_\gamma x_\delta + x_\delta x_\epsilon + x_\epsilon x_\alpha,$$

$$\tau' = x_\alpha x_\gamma + x_\gamma x_\epsilon + x_\epsilon x_\beta + x_\beta x_\delta + x_\delta x_\alpha,$$

then ($\tau+\tau'$ is a symmetrical function of all the roots, and it must be excluded; but) $(\tau-\tau')^2$ or $\tau\tau'$ are each of them 6-valued functions of the form in question, and either of these functions is linearly connected with the Resolvent Product. In Lagrange's general theory of the solution of equations, if

$$f\iota = x_1 + \iota x_2 + \iota^2 x_3 + \iota^3 x_4 + \iota^4 x_5,$$

then the coefficients of the equation the roots whereof are $(f\iota)^5$, $(f\iota^2)^5$, $(f\iota^3)^5$, $(f\iota^4)^5$, and in particular the last coefficient $(f\iota f\iota^2 f\iota^3 f\iota^4)^5$, are determined by an equation of the sixth degree; and this last coefficient is a perfect fifth power, and its fifth root, or $f\iota f\iota^2 f\iota^3 f\iota^4$, is the function just referred to as the Resolvent Product.

The conclusion from the foregoing remarks is that *if the equation for* W *has the above property of the rational expressibility of its roots,* the equation of the sixth order resulting from Lagrange's general theory has the same property.

I take the opportunity of adding a simple remark on cubic equations. The principle which furnishes what in a foregoing footnote is called the *à priori* demonstration of Lagrange's theorem is that an equation need never contain extraneous roots; a quantity which has only one value will, if the investigation is properly conducted, be determined in the first instance by a linear equation; one which has two values by a quadratic equation, and so on; there is always enough, and not more than enough, to determine what is required.

Take Cardan's solution of the cubic equation $x^3+qx-r=0$, we have $x=a+b$, and thence $3ab=-q$, $a^3+b^3=r$; and to obtain the solution we write

$$a^3b^3 = -\frac{q}{27}, \quad a^3+b^3=r.$$

But *these* two equations are not enough to precisely determine x, they lead to the 9-valued function

$$\sqrt[3]{\frac{r}{2}+\sqrt{\frac{r^2}{4}+\frac{q^3}{27}}}+\sqrt[3]{\frac{r}{2}-\sqrt{\frac{r^2}{4}+\frac{q^3}{27}}}:$$

in order to precisely determine x, it is (as everybody knows) necessary to use the original equation $ab=-\frac{q}{3}$. But seek for the solution as follows; viz. write $x=ab(a+b)$, which gives

$$3a^3b^3=-q,\quad a^3b^3(a^3+b^3)=r,$$

or what is the same thing,

$$a^3b^3=-\frac{q}{3},\quad a^3+b^3=-\frac{3r}{q};$$

these equations give $x=ab(a+b)$, where

$$a=\sqrt[3]{-\frac{3r}{2q}+\sqrt{\frac{9r^2}{4q^2}+\frac{q}{3}}},\quad b=\sqrt[3]{-\frac{3r}{2q}-\sqrt{\frac{9r^2}{4q^2}+\frac{q}{3}}},$$

which is a 3-valued function only, ab in this case being not given.

2 Stone Buildings, W.C.,
January 28, 1861.

XXXIV. *On some further applications of the Ferrocyanide of Potassium in Chemical Analysis.* By Edmund W. Davy, *A.B., M.B., M.R.I.A., Professor of Agriculture and Agricultural Chemistry to the Royal Dublin Society**.

I HAVE recently been engaged in making some experiments on the ferrocyanide of potassium or yellow prussiate of potash, with a view to extend its applications in chemical analysis; for though this important salt has already been applied to a number of useful purposes in analytical research, still my experiments have shown me that its use might be advantageously extended, particularly as a reagent in volumetric analysis, a form of analysis which has of late come into very general adoption, especially for technical purposes, on account of the great quickness and at the same time accuracy with which different substances may by its means be determined. The principles upon which volumetric analysis depend are so well known, that I need not refer to them; and though it possesses so many advantages over the older gravimetrical method, in which the different substances are determined by weight instead of by volume, it yet has this drawback, that the preparation of the necessary standard solutions often takes considerable time, first, in order

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* Part of a paper read before the Royal Dublin Society, December 17, 1860; and communicated by the Author.

to obtain the substance to be used for this purpose in a sufficiently pure and dry state, and secondly, to form a solution of it the exact strength of which may be known: for though it may appear a very simple operation to dissolve a known weight of a certain substance in a given bulk of water or other solvent, yet, when this has to be done with such great precision as is necessary in these cases, it is a tedious and troublesome operation, and any inaccuracy in the graduation of the standard solution will render all determinations made with it more or less inaccurate. It is obvious, therefore, that it would be most desirable that the substances which are intended to be used as reagents in volumetric analysis should be easily obtained in a pure state, and that where considerable time and trouble have been expended in graduating solutions of those substances, they should not be liable to undergo changes whereby their strength would be more or less altered, but that when standard solutions have once been made, they might be kept and used for a great number of determinations.

The ferrocyanide of potassium fulfils both those conditions; for it is in general met with in commerce almost chemically pure, and in a state in which it can at once be employed as a volumetric reagent; and if at any time it should happen to occur not quite so pure, it can readily be purified by recrystallization; and in addition to these important considerations, its solution is not prone to change, especially if it be not left exposed to the action of the light. In this latter respect it has a decided advantage over several of our most useful volumetric reagents, viz. the permanganate of potash, the protosalts of iron, sulphurous acid, &c., which, from their being so prone to undergo spontaneous decomposition, must be either freshly prepared, or the strength of their solutions accurately ascertained every time they are used, if a day or so has elapsed between each determination.

The employment of the ferrocyanide of potassium as a volumetric reagent depends on the following circumstances: viz., that it is readily converted into the ferridcyanide of potassium (red prussiate of potash) under different circumstances, and that the point where the whole of the former salt has been changed into the latter may easily be known, either by the use of a diluted solution of a persalt of iron (which gives with a drop of the mixture a blue or green coloration as long as any of the ferrocyanide remains unchanged) or by some other simple indication. Thus, for example, when chlorine is brought in contact with the ferrocyanide of potassium, this change, as is well known, takes place, which is expressed by the following symbols:—

$$2\,(K^2, Fe\,Cy^3) + Cl = (K^3\,Fe^2\,Cy^6) + K\,Cl.$$

The same occurs, as far as the conversion of the ferrocyanide into ferridcyanide, when an acidified solution of the former salt is brought in contact with a solution of the permanganate of potash, which is instantly decolorized by the reducing action of the ferrocyanide of potassium, which is thereby converted into the ferridcyanide, and this decoloration of the permanganate continues as long as any of the ferrocyanide remains in the mixture.

Again, if a solution of the ferrocyanide of potassium, acidified strongly with either hydrochloric or sulphuric acid, be brought in contact with a solution of the bichromate of potash, the same change of the ferrocyanide into the ferridcyanide immediately takes place.

The first reaction has been long known, and is the means employed at present for obtaining the ferridcyanide or red prussiate of potash for manufacturing and other purposes; the second reaction has been more recently discovered; but I am not aware that the third, in the case of the bichromate, is generally known, or that the changes which occur in the reaction have been previously studied.

From experiments which I made, it would appear that when a solution of ferrocyanide of potassium, acidified with hydrochloric acid, was mixed with one of the bichromate of potash, the following reaction was produced, viz. $6(K^2\,Fe\,Cy^3)+KO,\,2\,Cr\,O^3+7HCl=3(K^3\,Fe^2\,Cy^6)+4\,KCl+Cr^2\,Cl^3+7HO$; for, amongst other facts, I may observe that when I mixed together solutions of the two salts in the proportions corresponding to 6 equivalents of the ferrocyanide of potassium to 1 of the bichromate of potash (as indicated in the above formula), acidifying the mixture with hydrochloric acid, I found that the whole of the ferrocyanide was converted into the ferridcyanide, and that any quantity less than that proportion of the bichromate of potash left more or less of the ferrocyanide unchanged. The same results followed the use of sulphuric acid; and it appears that a similar reaction occurs with this acid as with hydrochloric acid, with the exception that in this case the 4 equivalents of chloride of potassium and the 1 equivalent of sesquichloride of chromium are replaced by 4 equivalents of sulphate of potash and 1 of the sesquisulphate of chromium.

The proportion of either acid used, provided there is enough to strongly acidify the mixture, does not appear to affect the reaction; for I obtained precisely the same results where a very large amount of acid was employed as where the quantity necessary only to strongly acidify the mixture had been added.

On these three reactions which I have noticed, may be based the means of employing the ferrocyanide of potassium in several useful determinations, the first, and one of the most important, of which is *the ascertaining the amount of available chlorine in the chloride of lime or bleaching powder*, which is a matter of much importance in many of the chemical arts, but particularly in bleaching; for not only does the commercial value of this substance depend on the quantity of available chlorine that it contains, which is subject to great variation from exposure to the air and other causes, but likewise it is of the greatest importance that the bleacher should readily be able to determine from time to time the strength of the bleaching liquor which he employs: for if it be too strong, he knows that the fabric which he bleaches will be injured; and if too weak, it will not be sufficiently bleached, and the process must be repeated, which incurs much additional expenditure of time.

Various methods have from time to time been proposed for the determination of the value of chloride of lime; but the greater number of them, from the trouble required to make the test-solutions, and their not keeping when made, as well as the skill required in their use, render them inapplicable for general purposes.

I shall therefore merely refer to the two methods which are chiefly used at present to determine the value of this important substance. The first is Gay-Lussac's, in which the amount of chlorine is ascertained by seeing how much chloride of lime is necessary to convert a given quantity of arsenious into arsenic acid; the second is Otto's, in which protosulphate of iron is substituted for arsenious acid, and the determination of chlorine is made by seeing how much of the bleaching powder is required to change a given weight of the protosulphate of iron into a persalt of that metal: these processes are so well known that I need not describe them.

In both these methods I find that more or less chlorine is always lost, which, however, may be reduced to a minute quantity by very carefully adding the solution of chloride of lime either to that of arsenious acid, or of protosalt of iron; but in ordinary hands they (especially the latter process) will yield results in which too small a proportion of chlorine will be indicated, from the loss of that substance which will invariably take place.

The ferrocyanide of potassium answers admirably for the estimation of available chlorine in the chloride of lime, when used in the manner I shall presently explain, and according to my experiments will give in ordinary hands far more accurate results than either Gay-Lussac's or Otto's method. I am aware, indeed, that this salt was proposed by Mr. Mercer some years ago for this purpose; but the way which he recommended it to be used (which consisted in dissolving a certain weight of the ferrocyanide in water, acidifying it, and then adding the solution of bleaching powder from a burette till all the ferrocyanide was converted into ferridcyanide) is, I find, not a good manner of employing the ferrocyanide in this estimation, and, like the other methods, will lead to a loss of chlorine; for when the solution of chloride of lime is added to the acidified ferrocyanide, a portion of the chlorine is separated, especially if the bleaching liquor be added too quickly, or is not greatly diluted. But the way I propose of using the ferrocyanide of potassium in this important valuation, is to mix together a certain quantity of a standard solution of ferrocyanide with a given amount of a graduated solution of the chloride of lime, using more of the former salt than the latter can convert into ferridcyanide; then adding hydrochloric acid to dissolve the precipitate formed and render the mixture strongly acid, and finally ascertain, by means of a standard solution of bichromate of potash, how much of the ferrocyanide remained unconverted into the ferridcyanide by the action of the chlorine of the chloride of lime,—which is effected by adding slowly from a graduated burette the standard solution of bichromate till a minute drop taken from the well-stirred mixture by means of a glass rod, ceases to give, with a small drop of a very dilute solution of perchloride of iron placed on a white plate, a blue or greenish colour, but produces instead a yellowish brown*. When this latter effect is observed, it indicates that all the ferrocyanide has been converted into ferridcyanide; and as 147·59 (one equivalent) of bichromate of potash is capable of converting 1267·32 (six equivalents) of crystallized ferrocyanide of potassium into ferridcyanide, and as 422·44 (two equivalents) of the ferrocyanide are converted into the same substance by 35·5 (one equivalent) of chlorine, as is seen by the formulæ already given, knowing the amount of chloride of lime employed, we have all the data necessary to calculate the per-centage of chlorine.

Having made two standard solutions, the first containing 21·122 grammes of ferrocyanide of potassium in a litre of the solution, and the second 14·759 grammes of bichromate of potash in the same quantity of solution (weights which are to each other as their atomic equivalents), I made several estimations of chloride of lime with them, adopting the method I have just described, and found that it gave the most consistent results, and which agreed very closely with those obtained by Gay-Lussac's and Otto's methods when the latter were performed with the greatest care,—the only difference being that the results obtained by

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* The yellowish-brown coloration which is at first produced when enough of the bichromate has been added, quickly changes to a greenish colour by some secondary reactions which take place when the persalt of iron is left in contact with the mixture. But this does not interfere with the test; for it is the first effect which is produced which indicates the completion of the reaction, and not the after changes which may result.

my method indicated a few hundredths of a part more of chlorine than either of those methods did, which may be accounted for by the unavoidable loss of a minute quantity of chlorine which takes place in those processes.

In order to simplify the process, and render the calculation as short as possible, I would recommend for commercial valuations the following way of carrying out this principle:—Having obtained a flat-bottom flask or bottle which will contain 10,000 grains of distilled water when filled up to a certain mark in the neck, make two standard solutions, the first by placing in the flask or bottle 1190 (or exactly 1189·97*) grains of the purest crystallized ferrocyanide of potassium (yellow prussiate of potash) reduced to powder, adding distilled water to dissolve the salt, and when this is effected, filling up with water to the mark; and having mixed the solution thoroughly, place it in a well-stoppered bottle. The second standard solution is made in the same manner, substituting for the ferrocyanide 138·6 (or exactly 138·58) grains of bichromate of potash which has been purified by recrystallization and fused in a crucible at as low a heat as possible. Both these solutions will keep unchanged, and will answer for a number of determinations if they are preserved in well-stoppered bottles, and the ferrocyanide solution be kept, when not in use, excluded from the light. Get a burette or alkalimeter capable of holding or delivering 1000 grains of distilled water, and divided into 100 equal divisions; also two small bottles, one capable of delivering 1000 grains, and the other 500 grains of distilled water when filled up to a certain mark on the neck of each†, which may both be readily made by filling them with water, emptying them, and after they have drained for a minute or two, weighing into each the above weights of distilled water; or, what will be sufficiently accurate for most purposes, pour from the burette into one 100 divisions of distilled water, and into the other 50, and mark with a file where the fluid stands in the neck of each bottle. Having these all ready, take an average specimen of chloride of lime, and weigh out 100 grains of it, and make in the usual way a solution of it by trituration in a mortar with some

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* The above numbers are obtained as follows:—35·5 parts of chlorine are capable, as before stated, of converting 422·44 parts of the crystallized ferrocyanide of potassium into ferridcyanide; therefore 100 parts of the former will convert 1189·97 parts of the latter into the same compound. Again, as before observed, 1267·32 parts of the crystallized ferrocyanide require 147·59 parts of the bichromate of potash to convert them into the ferridcyanide; 1189·97 parts, therefore, will take 138·58 parts of that salt to produce the same effect.

† Two small pipettes capable of delivering the above quantities would be found still more convenient.

water; pour it into the flask which was used in preparing the two standard solutions, and having filled up with water to the mark in the neck, mix the solution thoroughly; and before each time that any of the chloride of lime is taken out, shake well the contents of the flask.

Measure out into a beaker-glass, by means of the two little bottles, 100 divisions of the chloride of lime solution, and 50 of the standard solution of ferrocyanide; and having mixed them well together, add some hydrochloric acid to dissolve the precipitate formed and acidify the mixture strongly; and having mixed the whole well, pour from the burette slowly the standard solution of bichromate (stirring well all the while) till a drop taken from the mixture and brought in contact with a drop of a very weak solution of perchloride of iron produces a yellowish-brown colour, as already noticed. Then read off the number of divisions of the standard solution of bichromate which was necessary to produce this effect; and this being deducted from 50, gives the per-centage by weight of chlorine.

For the standard solution of ferrocyanide having been made so that the 10000-grain measures should be equivalent to 100 grains of chlorine, and as every division of the burette equals 10 grains, each of these divisions of the ferrocyanide solution converted into ferridcyanide will indicate 0·1 grain of chlorine. Again, the 100 divisions of the solution of chloride of lime represent 10 grains of that substance, and we want to know how many divisions of the ferrocyanide solution its chlorine has converted into ferridcyanide. This is readily ascertained by the bichromate solution, which has been so graduated that each division represents a division of the ferrocyanide solution. So that to determine the per-centage of chlorine we have only to deduct, as before stated, the number of divisions of the bichromate solution employed from the 50 of the ferrocyanide solution, and the difference gives us the per-centage of chlorine by weight in the sample; thus in four experiments 50 divisions of the ferrocyanide solution, mixed with 100 divisions of the solution of chloride of lime, required 18·5 divisions of the bichromate solution to convert the whole of the ferrocyanide employed into ferridcyanide; this number, taken from 50, leaves 31·5 divisions of ferrocyanide, which were converted into ferridcyanide by the chlorine of the chloride of lime; and as each division represents 0·1 grain of chlorine, 31·5 will be equivalent to 3·15 grains of chlorine, which is the amount contained in 10 grains of the sample; consequently 100 grains will contain 31·5 grains of chlorine, which is the same amount as is obtained by simply deducting the number of divisions of bichromate solution employed from 50 of ferrocyanide used in the estimation.

Though this process appears a long one, from the details which are necessary to explain its principle, yet in practice it is very expeditious, and requires only a very few minutes for its performance, and is much quicker than either Gay-Lussac's or Otto's method.

Though I have as yet chiefly confined my attention to the use of the ferrocyanide of potassium in the estimation of chlorine in bleaching powder, I have no doubt that it may be advantageously employed in many other useful determinations by carrying out the principles already explained: thus, for example, it may be used as a means of determining the amount of bichromate of potash present in a sample of that salt, or the quantity of chromic acid that exists under different circumstances. Again, the same salt may be used in different determinations where a certain amount of chlorine is liberated, which represents a proportional quantity of some other substance: thus, for example, in the estimation of manganese ores for commercial purposes, where they are heated with hydrochloric acid, the quantity of chlorine disengaged will indicate a certain amount of peroxide of manganese in the ore, on the presence of which its commercial value almost entirely depends; and the chlorine evolved may be estimated by absorbing the gas in a dilute solution of caustic potash, and then determining the amount of chlorine in it by precisely the same process as that I have recommended in the valuation of chloride of lime. To test the accuracy of this method, I heated in a small flask a given quantity of pure bichromate of potash with an excess of strong hydrochloric acid, and collected the evolved chlorine by means of a dilute solution of caustic potash, employing the bulbed retort and curved dropping tube as recommended by Bunsen in the "Analysis of the Chromates" (see the last edition of Fresenius's 'Quantitative Analysis,' page 234), and ascertained afterwards, by the use of the ferrocyanide of potassium, the amount of chlorine evolved, which corresponded almost exactly with the calculated amount of that substance which should have been obtained by the action of the quantity of bichromate used on the hydrochloric acid. Again, a standard solution of ferrocyanide of potassium may be used, as E. de Haen has shown, to determine the strength of the permanganate of potash in the analyses of the ferrocyanide and ferridcyanide of potassium, as an acidified solution of the ferrocyanide, as before stated, rapidly decolorizes a solution of permanganate of potash, whereas the ferridcyanide has no action on that salt; and this reaction might be taken advantage of, in the valuation of chloride of lime, to determine the excess of ferrocyanide used in my process: but from my experiments I found that more precise and accurate results were obtained by the use of the bichromate of potash.

The reaction of the bichromate of potash on the ferrocyanide might be employed in the valuation of the ferrocyanide of potassium and other ferrocyanides—having previously, in the case of those which were insoluble, converted them into the ferrocyanide of potassium by boiling them with caustic potash, and separating the insoluble oxides by filtration.

It might also be employed for the valuation of the commercial red prussiate of potash, which is now to some extent employed as a bleaching agent in calico-printing, and which consists of varying quantities of ferro- and ferrid-cyanide of potassium together with chloride of potassium. By ascertaining first how much a given quantity of the sample requires of a standard solution of bichromate of potash to convert the ferrocyanide present into ferridcyanide, the per-centage of that substance would be known; and then by taking another portion of the sample and converting the ferridcyanide it contained, by reducing agents, such as the sulphites of soda and potash, &c., into the ferrocyanide, and finally determining the amount of bichromate necessary to bring the whole of the ferrocyanide then present into the state of ferridcyanide, the difference in the two results would indicate the proportion of ferridcyanide originally present in the sample.

The last application of ferrocyanide of potassium which I shall notice in the present communication, is its employment as a reducing agent. It has long been known that the cyanide of potassium possesses most powerful reducing properties, and has been very usefully employed for that purpose in the reduction of different metallic salts under various circumstances; but I am not aware that the ferrocyanide of potassium has been proposed or used for similar purposes: at least, I have referred to a great number of analytical and general chemical works, and in none of them is this salt recommended as a reducing agent, though the cyanide is so much extolled for that purpose. According to my experiments, the ferrocyanide is a far more convenient reducing agent than the cyanide, and may be substituted for it in many cases of reduction with the best results, as it possesses many unquestionable advantages over that salt for this purpose. Thus the ferrocyanide does not deliquesce and decompose when exposed to the air, whereas the cyanide rapidly absorbs moisture, and, unless kept in very well-stoppered bottles, becomes quite wet, and in this state quickly decomposes; and this deliquescence on the part of the cyanide is often a source of much inconvenience in its use as a reducing agent, owing to the almost unavoidable absorption of more or less moisture which takes place in mixing it with the substance to be reduced, and during the introduction of the mixture into the reducing tube. The ferrocyanide, on the other hand, in a thoroughly dried and finely powdered state, can be intimately mixed with the substance without any appreciable absorption of moisture. I made the following comparative experiment to ascertain the relative absorptive properties for moisture of the two salts under the same circumstances. Having thoroughly dried in a water oven, till it ceased to vary in weight, some finely powdered ferrocyanide, I placed 50 grains of it in a counterpoised watch-glass, and powdering in a warm mortar some fresh cyanide of potassium, I placed the same quantity of it in a similar counterpoised watch-glass, and left them both exposed to the air. On examining them after four hours' exposure, I found that the former had only gained $\frac{6}{100}$th parts of a grain of moisture, whereas the latter had taken up 3·6 grains, or sixty times as much moisture under the same circumstances. After two days' exposure I found that nearly all the cyanide had passed into the liquid condition, having taken up 46 grains of water; whereas the ferrocyanide appeared perfectly dry, and had only absorbed 1·4 grain.

The great fusibility of the cyanide is sometimes rather a disadvantage, which has to be lessened by mixing it with a certain proportion of dried carbonate of soda; but the ferrocyanide not fusing at so low a temperature, does not require in most cases this admixture to lessen its fusibility. Again, the ferrocyanide is not a poisonous salt, whereas the cyanide is highly so, and must be used with great caution; and lastly, the former salt is little more than half the price of the latter. Combined with the above advantages, I find that the ferrocyanide is equally effective in reducing metallic oxides and sulphurets, and is especially convenient for the reduction of different combinations of arsenic and mercury, which are reduced by it with the greatest ease.

I made several comparative experiments with the dried ferrocyanide and with the cyanide as reducing agents for the sulphuret of arsenic and arsenious acid, employing the same quantity of arsenical compound with each salt under similar circumstances; and in almost every case, particularly where the quantities operated on were minute, I obtained more satisfactory results with the dried ferrocyanide than with the cyanide.

The following were amongst my experiments:—I mixed the $\frac{1}{10}$th of a grain of sulphuret of arsenic with 3 grains of the dried ferrocyanide, and made a similar experiment, substituting the same quantity of cyanide; and on heating the mixtures in similar glass tubes, obtained almost identically fine and characteristic rings of metallic arsenic.

I then intimately mixed the same quantity of sulphuret of arsenic with 49·9 grains of very finely powdered glass, and taking 5 grains of this mixture, containing the $\frac{1}{100}$th part of a grain of the sulphuret, mixed it with 5 grains of the dried ferrocyanide, and made a comparative experiment with another 5 grains of the mixture, substituting the same quantity of cyanide; on heating both these mixtures in small reduction tubes, I got the characteristic metallic rings in both, but better defined in the case of the ferrocyanide.

I finally took 2·5 grains of the mixture of sulphuret and glass, containing about $\frac{5}{1000}$th parts of a grain of sulphuret of arsenic, and treated them in the same manner, using in one case 2·5 grains of ferrocyanide, and in the other 2·5 grains of cyanide, and obtained in each case a minute metallic ring, which, however, was much more distinct and satisfactory where the ferrocyanide had been used as the reducing agent.

The same comparative experiments were made with arsenious acid, when results similar to those in the case of the sulphuret of arsenic were obtained.

The ferrocyanide, therefore, is a most delicate reducing agent in the case of arsenical compounds, and where very minute quantities have to be detected, appears from my experiments to give more satisfactory results than the cyanide.

Whether the addition of dried carbonate of soda would improve the ferrocyanide for some cases of reduction, I am not at present able to say; but in one experiment which I made with the sulphuret of arsenic, I obtained as good results, using the ferrocyanide alone, as where it was mixed previously with its own weight of dried carbonate of soda. In many cases the ferrocyanide may be used as a reducing agent in a state of powder without separating its water of crystallization; but, in most cases, it will be rendered a far better reducing agent by being previously dried at 212° in a water-bath or oven; and in this dried condition it may be kept for any length of time in a good-stoppered or well-corked bottle.

Though as yet my experiments have been chiefly confined to the reduction of different compounds of arsenic and mercury, I entertain no doubt that the ferrocyanide of potassium will be found an equally effective reducing agent in the case of the combinations of other metals, and that it may with great advantage be substituted for the cyanide of potassium in many cases where the latter salt is used as a reducing agent.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 153.]

April 26, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communication was read :—

"On the Effect of the Presence of Metals and Metalloids upon the Electric Conductivity of Pure Copper." By A. Matthiessen, Esq., and M. Holzmann, Esq.

After studying the effect of suboxide of copper, phosphorus, arsenic, sulphur, carbon, tin, zinc, iron, lead, silver, gold, &c., on the conducting power of pure copper, we have come to the conclusion *that there is no alloy of copper which conducts electricity better than the pure metal.*

May 3.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

"On the relations between the Elastic Force of Aqueous Vapour, at ordinary temperatures, and its Motive Force in producing Currents of Air in Vertical Tubes." By W. D. Chowne, M.D., F.R.C.P.

In 1853 the author of this communication made a considerable number of experiments which demonstrated that when a tube, open at both ends, was placed vertically in the undisturbed atmosphere of a closed room, there was an upward movement of the air within the tube of sufficient force to keep an anemometer of light weight in a state of constant revolution, though with a variable velocity. An abstract of the results of these experiments was printed in the Philosophical Magazine, vol. xi. p. 227.

In order to further investigate the immediate cause or nature of the force which set the machine in motion, the author instituted a series of fresh experiments.

These experiments were made in the room described in the former communication, guarded in the same manner against disturbing causes, and with such extra precautions as will be hereafter explained. The apparatus used was a tube 96 inches long and 6·75 inches uniform diameter, the material zinc. The upper extremity was open to its full extent; at the lower, the aperture was a lateral one only, into which a piece of zinc tube 3 inches in diameter, and bent once at right angles, was accurately fitted with the outer orifice upward. Within this orifice, which was about 5 inches above the level of the floor, an anemometer, described in the former paper, and weighing 7 grains, was placed in the horizontal position. About midway between the upper and the lower extremity of the tube, a very delicate differential thermometer was firmly and permanently fixed, with one bulb outside and the other inside, and the aperture through which the latter was inserted completely closed. The scale was on the stem of the outer bulb.

The results of a long series of observations were recorded. The state of the dry and the wet bulb of the hygrometer, as well as the indications of the differential thermometer, was noted, in connexion with the number of revolutions performed per minute by the anemometer. While the differential thermometer indicated the same relative differences between the heat of the atmosphere within and without the tube, the velocity of the revolutions was found to vary considerably. This variation was discovered to be chiefly, if not wholly, dependent on the *elasticity* of vapour, due to the hygrometrical state of the atmosphere, as estimated from the dry- and the wet-bulb thermometers, and calculated from the tables of Regnault.

240 observations were recorded and afterwards separated into groups, each group comprising those in which the differential thermometer gave the same indication.

If in either of these groups we separate into two classes the cases in which the elasticity was highest, from the cases in which it was lowest, and multiply the mean of each with the corresponding mean of the number of the revolutions of the anemometer, their product is nearly a constant, thus showing that the velocity of ascent of the atmospheric vapour is inversely as its elasticity; and hence it follows that the velocity of the ascending current in the tube varies inversely as the density or elastic force of the vapour suspended in the atmosphere. This was rendered evident by the aid of Tables appended to the paper.

When the mean elastic force of vapour calculated from the dry and the wet bulbs is multiplied by the constant, 13·83, the result gives the whole amount of water in a vertical column of the atmosphere in inches; it follows therefore that when the difference of temperature between the external air and that in the tube, as shown by the differential thermometer, is constant, the velocity of the current in the tube varies inversely as the weight of the vapour suspended in the atmosphere.

In an Appendix the author describes some additional experiments, made with the view of ascertaining whether the readings of the differential thermometer were mainly due to actual changes of temperature within the tube, or to extraneous causes acting on the external bulb. He found that when the external bulb was covered with woollen cloth or protected by a zinc tube of about 4 inches diameter and 6 inches long, the temperature of the bulb was increased about 2° on the scale of the instrument, and that when they were removed the prior reading was restored, while the number of revolutions of the anemometer per minute was not appreciably affected by the change. This explains why the readings of the differential thermometer varied from 33°·0 to 33°·5 as described in the paper, without producing a corresponding change in the velocity of the anemometer.

For the purpose of obtaining a more correct estimate of the influence of a given increase of heat within the tube, the author introduced into the tube at its lowest extremity, a phial containing eight ounces of water at the temperature of 100° Fahr., corked so that no vapour could escape. The result showed that in thirteen observations a quantity of heat equal to an increase of one-tenth of a degree on the scale of the differential thermometer, was equivalent to a mean velocity of the anemometer of 3·6 revolutions per minute, the greatest number being 3·8, the least 3·3 per minute.

These observations render it still more evident, that if a higher temperature within the tube had been the main cause of the revolutions of the anemometer, the variations in their velocity would not have been in such exact relation to the elastic force of the atmospheric vapour, as has been shown to be the case. They also lead to the inference, that the apparent excess of heat within the tube alluded to by the author in his Paper read before the Society in 1855 did not really exist, and to the conclusion that, if such excess had been present, the anemometer would not have been brought to a state of rest by depriving the air of the room of a portion of the moisture ordinarily suspended in it.

"On the Relation between Boiling-point and Composition in Organic Compounds." By Hermann Kopp.

The author was the first to observe (in 1841) that, on comparing pairs of analogous organic compounds, the same difference in boiling-point corresponds frequently to the same difference in composition. This relation between boiling-point and composition, when first pointed out, was repeatedly denied, but is now generally admitted. The continued experiments of the author, as well as of numerous other inquirers, have since fixed many boiling-points which had hitherto remained undetermined, and corrected such as had been inaccurately observed. In the present paper the author has collected his experimental determinations, and has given a survey of all the facts satisfactorily established up to the present moment regarding the relations between boiling-point and composition.

The several propositions previously announced by the author were:—

1. An alcohol, $C^n H_{n+2} O_2$, differing in composition from ethylic alcohol ($C_4H_6O_2$, boiling at 78° C.) by $x\ C_2H_2$, more or less, boils $x \times 19°$ higher or lower than ethylic alcohol.

2. The boiling-point of an acid, $C_nH_nO_4$, is 40° higher than that of the corresponding alcohol, $C_nH_{n+2}O_2$.

3. The boiling-point of a compound ether is 82° higher than the boiling-point of the isomeric acid, $C_nH_nO_4$.

These propositions supply the means of calculating the boiling-points of all alcohols, $C_nH_{n+2}O_2$; of all acids, $C_nH_nO_4$; of all compound ethers, $C_nH_nO_4$. The author contrasts the values thus calculated for these substances with the available results of direct observation. The Table embraces eight alcohols, $C_nH_{n+2}O_2$, nine acids, $C_nH_nO_4$, and twenty-three compound ethers, $C_nH_nO_4$; the calculated boiling-points agree, as a general rule, with those obtained by experiment, as well as two boiling-points of one and the same substance determined by different observers. We are thus justified in assuming that the calculated boiling-point of other alcohols, acids, and ethers belonging to this series will also be found to coincide with the results of observation.

The boiling-points of other monatomic alcohols, $C_nH_mO_2$, other monatomic acids, $C_nH_mO_4$, and other compound ethers, $C_nH_mO_4$, are closely allied with the series previously discussed. A substance containing xC more or less than the analogous term of the previous class, in which the same number of oxygen and of hydrogen equivalents is present, boils $x \times 14°{\cdot}5$ higher or lower; or, what amounts to the same thing, a difference of xH more or less of hydrogen lowers or raises the boiling-point by $x \times 5°$. Thus benzoic acid, $C_{14}H_6O_4$, boils $8 \times 14°{\cdot}5$ higher than propionic acid, $C_6H_6O_4$, or $8 \times 5°$ higher than œnanthylic acid, $C_{14}H_{14}O_4$; cinnamate of ethyl, $C_{22}H_{12}O_4$, boils $10 \times 14°{\cdot}5$ higher than butyrate of ethyl, $C_{12}H_{12}O_4$, or $10 \times 5°$ higher than pelargonate of ethyl, $C_{22}H_{22}O_4$.

The author compares the boiling-points thus calculated for five alcohols, $C_nH_mO_4$; for six acids, $C_nH_mO_4$; and for sixteen compound ethers, $C_nH_mO_4$, with the results of observation. In almost all cases the concordance is sufficient.

The author demonstrates in the next place that in many series of compounds other than those hitherto considered, the elementary difference, xC_2H_2, likewise involves a difference of $x \times 19°$ in the boiling-point. He further shows that on comparing the boiling-points of the corresponding terms in the several series of homologous substances hitherto considered, many other constant differences in boiling-point are found to correspond to certain differences in composition. Thus a monobasic acid is found to boil 44° higher than its ethyl compound, and 63° higher than its methyl compound; and this constant relation holds good even for acids other than those previously examined, *e. g.* for the substitution-products of acetic acid. Also in substances which are not acids, the substitution of C_4H_5 or C_2H_3 for H, occasionally involves a depression of the boiling-points respectively of 44° and 63°; the relation, however, is by no means generally observed.

The author, in addition to the examples previously quoted, shows that compounds containing benzoyl ($C_{14}H_5O_2$) and benzyl ($C_{14}H_7$) boil 78° ($=4 \times 14°{\cdot}5 + 4 \times 5°$) higher than the corresponding terms containing valeryl ($C_{10}H_9O_2$) and amyl ($C_{10}H_{11}$), a relation, however, which is likewise not generally met with. He discusses, moreover, other coincidences and differences of boiling-points of compounds differing in a like manner in composition. Not in all homologous series does the elementary difference xC_2H_2 involve a difference of $x \times 19°$ in boiling-point. The author shows that this difference is greater for the hydrocarbons, C_nH_{n-6} and C_nH_{n+2}; for the acetones and aldehydes, $C_nH_nO_2$; for the so-called simple and mixed ethers, $C_nH_{n+2}O_2$; for the chlorides, bromides, and iodides of the alcohol radicals, C_nH_{n+1}, and for several other groups; that it is, on the contrary, smaller for the anhydrides of monobasic acids, $C_nH_{n-2}O_6$; for the ethers, $C_nH_{n-2}O_8$ (which may be formed either by the action of one molecule of a dibasic acid, $C_nH_{n-2}O_8$, upon two molecules of a monatomic alcohol, $C_nH_{n+2}O_2$, or by the action of two molecules of a monobasic acid, $C_nH_nO_4$, upon one molecule of a diatomic alcohol, $C_nH_{n+2}O_4$), and several other series.

The author thinks that the unequal differences in boiling-points corresponding in different homologous series to the elementary difference xC_2H_2, are probably regulated by a more general law, which will be found when the boiling-points of many substances shall have been determined under pressures differing from those of the atmosphere.

"From the observations at present at our disposal it may be affirmed as a general rule, that in homologous compounds belonging to the same series, the differences in boiling-points are proportional to the differences in the formulæ. Exceptions obtain only in cases when terms of a particular group are rather difficult to prepare, or when the substances boil at a very high temperature, at which the observations now at our command are for the most part uncertain. Again, it may be affirmed that the difference in boiling-points, corresponding to the elementary difference C_2H_2, is in a great many series $=19°$; in some series greater, in some series less."

The author proceeds to discuss the boiling-points of isomeric compounds. He shows that in a great many cases isomeric compounds belonging to the same type, and exhibiting the same chemical character, boil at the same temperature, and that there is no reason why, for the class of bodies mentioned, this coincidence should not obtain generally. On the other hand, different boiling-points are observed in isomeric compounds possessing a different chemical character, although belonging to the same type (*e. g.* acids and compound ethers, $C_nH_nO_4$; alcohols and ethers, $C_nH_{n+2}O_2$), and in isomeric compounds belonging to different types (*e. g.* allylic alcohol and acetone).

The author shows that the determination of the boiling-point of a substance, together with an inquiry into the compounds serially allied with it by their boiling-points, constitutes a valuable means of fixing the character of the substance, the type to which it belongs, and the series of homologous bodies of which it is a term. He quotes as an illustration eugenic acid. The boiling-point of this acid, $C_{20}H_{12}O_4$, is 150°; and on comparing this boiling-point with the boiling-points of benzoic acid, $C_{14}H_6O_4$ (boiling-point 253°), and of hydride of salicyl, $C_{14}H_6O_4$ (boiling-point 196°), it is obvious that eugenic acid cannot be homologous to benzoic acid, whilst, on the other hand, it becomes extremely probable that it is homologous to hydride of salicyl, and consequently that it belongs rather to the aldehydes than to the acids proper.

The author, in conclusion, calls attention to the importance of considering the chemical character in comparing the boiling-points of the volatile organic bases, and shows the necessity of distinguishing between the primary, secondary, and tertiary monamines in order to exhibit constant differences of boiling-point for this class of substances. He discusses the boiling-points of the several bases, $C_nH_{n-5}N$ and $C_nH_{n+3}N$, and points out how in many cases the particular class to which a base belongs may be ascertained by the determination of the boiling-point.

The comprehensive recognition of definite relations between composition and boiling-point is for the present chiefly limited to organic compounds. But for the majority of these compounds, and indeed for the most important ones, this relation assumes the form of a simple law, which, more especially for the monatomic alcohols, $C_nH_mO_2$, for the monobasic acids, $C_nH_mO_4$, and for the compound ethers generated by the union of the two previous classes, is proved in the most general manner; so much so, indeed, that in many cases the determination of the boiling-point furnishes most material assistance in fixing the true position and character of a compound.

The author points out more especially that the simplest and most comprehensive relations have been recognized for those classes of organic compounds which have been longest known and most accurately investigated, and that even for those classes the generality and simplicity of the relation, on account of numerous boiling-points incorrectly observed at an earlier date, appeared in the commencement doubtful, and could be more fully acknowledged only after a considerable number of new determinations. Thus he considers himself justified in hoping that also in other classes of compounds, in which simple and comprehensive relations have not hitherto been traced, these relations will become perceptible as soon as the verification of the boiling-points of terms already known, and the examination of new terms, shall have laid a broader foundation for our conclusions.

May 10.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The Bakerian Lecture was delivered by Mr. Fairbairn, F.R.S. The Lecturer gave a condensed exposition of the experiments and results detailed in the following Paper. He also exhibited the apparatus employed, and explained the methods followed.

"Experimental Researches to determine the Density of Steam at all Temperatures, and to determine the Law of Expansion of Superheated Steam." By William Fairbairn, Esq., F.R.S., and Thomas Tate, Esq.

The object of these researches is to determine by direct experiment the law of the density and expansion of steam at all temperatures. Dumas determined the density of steam at 212° Fahr., but at this temperature only. Gay-Lussac and other physicists have deduced the density at other temperatures by a theoretical formula true for a perfect gas:

$$\frac{VP}{V_1P_1}=\frac{459+T}{459+T_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

On the expansion of superheated steam, the only experiments are those of Mr. Siemens, which give a rate of expansion extremely high, and physicists have in this case also generally assumed the rate of expansion of a perfect gas. Experimentalists have for some time questioned the truth of these gaseous formulæ in the case of condensable vapours, and have proposed new formulæ derived from the dynamic theory of heat; but up to the present time no *reliable direct experiments* have been made to determine either of the points at issue. The authors have sought to supply the want of data on these questions by researches on the density of steam upon a new and original method.

The general features of this method consist in vaporizing a known weight of water in a globe of about 70 cubic inches capacity, and devoid of air, and observing by means of a "*saturation gauge*" the exact temperature at which the whole of the water is converted into steam. The saturation gauge, in which the novelty of the experiment consists, is essentially a double mercury column balanced upon one side by the pressure of the steam produced from the weighed portion of water, and on the other by constantly saturated steam of the same temperature. Hence when heat is applied the mercury columns remain at the same level up to the point at which the weighed portion of water is wholly vaporized; from this point the columns indicate, by a difference of level, that the steam in the globe is superheating; for superheated steam increases in pressure at a far lower rate than saturated steam for equal increments of temperature. By continuing the process, and carefully measuring the difference of level of the columns, data are obtained for estimating the rate of expansion of superheated steam.

The apparatus for experiments at pressures of from 15 to 70 lbs. per square inch, consisted chiefly of a glass globe for the reception of the weighed portion of water, drawn out into a tube about 32 inches long. The globe was enclosed in a copper boiler, forming a steam-bath by which it could be uniformly heated. The copper steam-bath was prolonged downwards by a glass tube enclosing the globe stem. To heat this tube uniformly with the steam-bath, an outer oil-bath of blown glass was employed, heated like the copper bath by gas jets. The temperatures were observed by thermometers exposed naked in the steam, but corrected for pressure. The two mercury columns forming the saturation gauge were formed in the globe stem, and between this and the outer glass tube; so long as the steam in the glass globe continued in a state of saturation, the inner column in the globe stem remained stationary, at nearly the same level as that in the outer tube. But when, in raising the temperature, the whole of the water in the globe had been evaporated and the steam had become superheated, the pressure no longer balanced that in the outer steam-bath, and, in consequence, the column in the globe stem rose, and that in the outer tube fell, the difference of level forming a measure of the expansion of the steam. Observations of the levels of the columns were made by means of a cathetometer at different temperatures, up to 10° or 20° above the saturation point; and the maximum temperature of saturation was, for reasons developed by the experiments, deduced from a point at which the steam was decidedly superheated.

The results of the experiments, which in the paper are given in detail, show that the density of saturated steam at all temperatures, above as well as below 212°, is invariably greater than that derived from the gaseous laws.

The apparatus for the experiments at pressures below that of the atmosphere was considerably modified; and the condition of the steam was determined by comparing the column which it supported with that of a barometer. The results of these experiments, reduced in the same way, are extremely consistent.

As the authors propose to extend their experiments to steam of a very high pressure, and to institute a distinct series on the law of expansion of superheated steam, they have not at present given any elaborate generalizations of their results. The following formulæ, however, represent the relations of specific volume and pressure of saturated steam, as determined in their experiments, with much exactness.

Let V be the specific volume of saturated steam, at the pressure P, measured by a column of mercury in inches; then

$$V=25{\cdot}62+\frac{49513}{P+{\cdot}72}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$P=\frac{49513}{V-25{\cdot}62}-0{\cdot}72. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

In regard to the rate of expansion of superheated steam, the experiments distinctly show that, for temperatures within about ten degrees of the saturation point, the rate of expansion greatly exceeds that of air, whereas at higher temperatures the rate of expansion approaches very near that of air. Thus in experiment 6, in which the maximum temperature of saturation is 174°·92, the coefficient of expansion between 174°·92 and 180° is $\frac{1}{190}$, or three times that of air; whereas between 180° and 200° the coefficient is very nearly the same as that of air (steam $=\frac{1}{637}$, air $=\frac{1}{639}$), and so on in other cases. The mean coefficient of expansion at zero of temperature from seven experiments below the pressure of the atmosphere, and calculated from a point several degrees above that of saturation, is $\frac{1}{438}$, whereas for air it is $\frac{1}{459}$. Hence it would appear that for some degrees above the saturation point the steam is not decidedly in an aëriform state, or, in other words, that it is watery, containing floating vesicles of unvaporized water.

Table of Results, showing the relation of density, pressure, and temperature of saturated steam.

Number of Exper.	Pressure		Max. temp. of saturation, Fahr.	Specific Volume.		Proportional error of formula (2).
	in lbs. per sq. in.	in inches of mercury.		From experiment.	By formula (2).	
1	2·6	5·35	136°·77	8266	8183	$+\frac{1}{100}$
2	4·3	8·62	155·33	5326	5326	0
3	4·7	9·45	159·36	4914	4900	$-\frac{1}{350}$
4	6·2	12·47	170·92	3717	3766	$+\frac{1}{74}$
5	6·3	12·61	171·48	3710	3740	$+\frac{1}{123}$
6	6·8	13·62	174·92	3433	3478	$+\frac{1}{76}$
7	8·0	16·01	182·30	3046	2985	$-\frac{1}{50}$
8	9·1	18·36	188·30	2620	2620	0
9	11·3	22·88	198·78	2146	2124	$-\frac{1}{97}$
1′	26·5	53·61	242·90	941	937	$-\frac{1}{235}$
2′	27·4	55·52	244·82	906	906	0
3′	27·6	55·89	245·22	891	900	$+\frac{1}{100}$
4′	33·1	66·84	255·50	758	758	0
5′	37·8	76·20	263·14	648	669	$+\frac{1}{32}$
6′	40·3	81·53	267·21	634	628	$-\frac{1}{100}$
7′	41·7	84·20	269·20	604	608	$+\frac{1}{150}$
8′	45·7	92·23	274·76	583	562	$-\frac{1}{29}$
9′	49·4	99·60	279·42	514	519	$+\frac{1}{100}$
11′	51·7	104·54	282·58	496	496	0
12′	55·9	112·78	287·25	457	461	$+\frac{1}{114}$
13′	60·6	122·25	292·53	432	428	$-\frac{1}{108}$
14′	56·7	114·25	288·25	448	456	$+\frac{1}{56}$

Adopting the notation previously employed, and putting r for the rate or coefficient of expansion of an elastic fluid at t_1 temperature, we find

$$r=\frac{1}{\epsilon_1+t_1}=\frac{\frac{V_2 p_2}{V_1 p_1}-1}{t_2-t_1}, \quad \ldots \ldots \quad (4.)$$

where $\frac{1}{\epsilon_1}=$ the rate of expansion at zero of temperature. In the case of air $\epsilon_1= 459$.

The following Table gives the value of the coefficient of expansion of superheated steam taken at different intervals of temperature from the maximum temperature of saturation.

Number of the Exper.	Max. temp. of saturation.	Temperatures between which the expansion is taken.		Coefficient of expansion of superheated steam.	Coefficient of expansion of air.
1	136°·77	140°	170°	$\frac{1}{593}$	$\frac{1}{599}$
2	155·33	160	190	$\frac{1}{556}$	$\frac{1}{619}$
3	159·36	159·36	170·2	$\frac{1}{150}$	$\frac{1}{618}$
		170·2	209·9	$\frac{1}{624}$	$\frac{1}{629}$
5	171·48	171·48	180	$\frac{1}{200}$	$\frac{1}{630}$
		180	200	$\frac{1}{604}$	$\frac{1}{639}$
6	174·92	174·92	180	$\frac{1}{190}$	$\frac{1}{634}$
		180	200	$\frac{1}{637}$	$\frac{1}{639}$
7	182·30	182·3	186	$\frac{1}{230}$	$\frac{1}{641}$
		186	209·5	$\frac{1}{630}$	$\frac{1}{645}$
8	188·30	191	211	$\frac{1}{604}$	$\frac{1}{650}$
1′	242·9	243	249	$\frac{1}{517}$	$\frac{1}{702}$
4′	255·5	257	259	$\frac{1}{392}$	$\frac{1}{716}$
		257	264	$\frac{1}{600}$	$\frac{1}{716}$
6′	267·21	268	271	$\frac{1}{210}$	$\frac{1}{727}$
		271	279	$\frac{1}{640}$	$\frac{1}{730}$
7′	269·2	271	273	$\frac{1}{232}$	$\frac{1}{730}$
		273	279	$\frac{1}{551}$	$\frac{1}{733}$
9′	279·42	283	285	$\frac{1}{298}$	$\frac{1}{742}$
		285	289	$\frac{1}{533}$	$\frac{1}{744}$
13′	292·53	297	299	$\frac{1}{281}$	$\frac{1}{756}$
		299	302	$\frac{1}{638}$	$\frac{1}{758}$

Hence it appears, that as the steam becomes more and more superheated, the coefficient of expansion approaches that of a perfect gas. The authors hope that these experiments may be continued, and that the results obtained at greatly increased pressures will prove as important as those already arrived at.

GEOLOGICAL SOCIETY.

[Continued from p. 157.]

December 5, 1860.—L. Horner, Esq., President, in the Chair.

The following communication was read :—

" On the Structure of the North-west Highlands, and the Relations of the Gneiss, Red Sandstone, and Quartzite of Sutherland and Ross-shire." By Professor James Nicol, F.R.S.E., F.G.S.

The author first referred to his paper in the Quart. Journ. Geol. Soc., vol. xiii. pp. 17, &c., in which the order of the red sandstone on gneiss, and of quartzite and limestone on the sandstone, was established, and in which the relation of the eastern gneiss or mica-schist to the quartzite was stated to be somewhat obscure on account of the presence of intrusive rocks and other marks of disturbance. Having examined the country four times, with the view of settling some of the doubtful points in the sections, the author now offered the matured result of his observations. He agrees with Sir R. Murchison as far as the succession of the western gneiss, red sandstone, quartzites (quartzite and fucoid-bed), and limestone is concerned; but differs from him in maintaining that there is no upper series of quartzite and limestone, and that there is no evidence of an "upward conformable succession" from the quartzite and limestone into the eastern mica-slate or gneiss—the o-called "upper gneiss." The "upper quartzite" and "upper limestone" the author believes to be portions of the quartzite of the country, in some cases separated by anticlines and faults and cropping out in the higher ground, and in other instances inverted beds with the gneiss brought up by a contiguous fault and overhanging them. This latter condition of the strata, as well as other cases where the eastern gneiss is brought up against the quartzite series, have, according to the author, given rise to the supposed "upward conformable succession" above referred to. In some cases where "gneiss" is said to have been observed overlying the quartzite, Professor Nicol has determined that the overlying rock is granulite or other eruptive rock, not gneiss.

The sections described by the author in support of his views of the eastern gneiss not overlying the quartzite and limestone, but being the same as the gneiss of the west coast, and brought up by a powerful fault along a nearly north and south line passing from Whiten Head (Loch Erriboll) to Loch Carron and the Sound of Sleat, are chiefly those which had been brought forward as affording the proofs on which the opposite hypothesis is founded; and in all, the author finds irruptions of igneous rocks, and other indications of faults and disturbance, depriving them, in his opinion, of all weight as evidence of a regular order of "upward conformable succession."

Prof. Nicol further argues that the mode of the distribution of the rocks shows that there is through Sutherland and Ross-shire a real fault, and no overlap of eastern gneiss of more than a few feet or yards at most, and that the fact of different strata of the quartzite series being brought against the gneiss at different places supports this view, and points to a great denudation having taken place along the line of fault. Though the quartzite is here and there altered by the igneous rocks, yet it is truly a sedimentary rock, and so is the limestone; but the eastern gneiss or mica-schist is a crystalline rock throughout: this fact, according to the author, is inimical to the hypothesis of the eastern gneiss overlying the limestone and quartzite. It has been insisted upon, that the strike of the western gneiss is different from that of the east; but the author remarks that the strike is not persistent in either area, and that great movements subsequent to the deposition of the quartzite series have irregularly affected the whole region. With regard to mineralogical characters, Prof. Nicol insists that both the eastern and the western gneiss are essentially the same. Both are locally modified with granitic and hornblendic matter near igneous foci; but no proof of a difference of age in the two can be obtained therefrom. The alteration in bulk of the gneiss in the western area, by the intrusion of the vast quantities of granite now observable in it, may perhaps have caused the great amount of crumpling and faulting along the N. and S. line of fault, dividing the western from the eastern gneiss,—a fault comparable with and parallel to that running from the Moray Firth to the Linnhe Loch, and to the one passing along the south side of the Grampians.

December 19, 1860.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Geological Structure of the South-west Highlands of Scotland." By T. F. Jamieson, Esq. Communicated by Sir R. I. Murchison, V.P.G.S.

In this paper the author attempts to throw light on the relations of those rocks which figure in geological maps as the mica-schist, clay-slate, the chlorite-slates, and the quartz-rock of the South-western Highlands, which range N.E. through the middle of Scotland, forming an important feature in the geology of that country. An examination of these rocks, as displayed in Bute and Argyleshire, has led Mr. Jamieson to believe that, from the quartz-rock of Jura to the border of the Old Red Sandstone, there is a conformable series of strata, which, although closely linked together, may be classed into three distinct groups, namely, 1st, a set of lower grits (or quartz-rock), many thousand feet thick; 2ndly, a great mass of thin-bedded slates, 2000 feet or more thick; and 3rdly, a set of upper grits, with intercalated seams of slate of equal thickness. Beds of limestone occur here and there sparingly in all the three divisions; the thickest being deep down in the lower grits. All the limestones are thickest towards the west. The siliceous grits also appear to be freer from an admixture of green materials towards the west. All the members of the series (namely, the upper grits, slates, and lower grits) have a persistent S.W.–N.E. strike, sometimes in Bute approaching to due N. and S. They are conformable, and graduate one into another in such a way as to show that they belong to one continuous succession of deposits. The materials of which they have been formed seem to have been derived from very similar sources. The upper and the lower grits are very similar in composition, being made up of water-worn grains of quartz, many of which are of a peculiar semitransparent bluish tint.

The rocks of the district have been thrown into a great undulation, with an anticlinal axis extending from the north of Cantyre through Cowal by the head of Loch Ridun on to Loch Eck (and probably by the head of Loch Lomond on to the valley of the Tay, at Aberfeldy), and with a synclinal trough lying near the parallel of Loch Swen. The anticlinal fold is well seen in the hill called Ben-y-happel, near the Tighnabruich quay in the Kyles of Bute. Southward of this ridge, which is composed of the lower grits or quartzite, the thin-bedded greenish slates and the upper grits succeed conformably; and the latter are separated by a trap-dyke from the Old Red Sandstone of Rothsay. This section the author described in detail; also the corresponding section to the north of the anticlinal axis, towards Loch Fyne, and along the west shore of Loch Fyne. The lower grits extend as far as Loch Gilp, and are then succeeded by the green slates and the upper grits, which falling in the synclinal trough are repeated through Knapdale towards Jura Sound, where the green slates again form the surface along the eastern coast of Jura, lying on the quartzite or grits of that island. Throughout the synclinal trough and the neighbouring district (that is, from

Loch Fyne to Jura Sound) the grits and slates are intimately mixed, with numerous intercalated beds of greenstone, some being of great thickness. Mr. Jamieson pointed out that this feature of the district has hitherto in great part been misunderstood, and that Macculloch was in error when he denominated these rocks "chlorite-schist."

The probable relationship of the rocks of the Islands of Shuna, Luing, and Scarba to those of Jura and Bute were then dwelt upon; the greenstones of Knapdale, &c., and their relation to the sedimentary rocks, were described in detail; and the limestones of the district briefly noticed. As no fossils have hitherto been found, palæontological evidence of the age of these rocks is wanting; but the author, regarding their general resemblance to the quartz-rocks, limestones, and mica-schists of Sutherlandshire, thinks them to be of the same date as those rocks of the North-west Highlands.

2. "On the position of the beds of the Old Red Sandstone in the Counties of Forfar and Kincardine, Scotland." By the Rev. Hugh Mitchell. Communicated by the Secretary.

In Forfar- and Kincardine-shire, south of the Grampians, the Old Red Sandstone is developed in the following series, with local modifications:—1st (at top), Conglomerate; 2nd, grey flagstone with intercalated sandstone (about 40 feet thick at Cauterland Den, 120 feet at Carmylie); 3rd, gritty ferruginous sandstone, with occasional thin layers of purplish flagstone. Of the last, 120 feet are seen at Cauterland Den; it occurs also at Ferry Den, &c. The flagstone of this third or lowest member of the group yields Ripple-marks, Rain-prints, Worm-markings, and Crustacean tracks (of several kinds, large and small). *Parka decipiens* has been found in the lowest grits; and *Cephalaspis* in the sandstone at Brechin, immediately under the grey flagstones.

In the second member, namely the grey flags, Fish-remains have of late been found more or less abundantly throughout the district, together with Crustacean fossils. *Cephalaspis Lyellii*, Ichthyodorulites, Acanthodian fishes, *Pterygotus*, *Eurypterus*, *Kampecaris Forfariensis*, *Stylonurus Powriensis*, *Parka decipiens*, and vegetable remains are the most characteristic fossils.

The author pointed out that some few genera of Fish and Crustaceans were present both in this zone and in the Upper Silurian formation, and that still fewer links existed to connect the fauna of the Forfarshire flags with the Old Red Sandstone north of the Grampians, with which it appears to have, in this respect, almost as little relation as with the Carboniferous system. With the Old Red of Herefordshire these flags appear to have some few fossils in common; but of about twenty *species* found in Forfarshire, only about four could be quoted from Herefordshire.

In conclusion, the author noticed the vast vertical development of the whole series, and its great geographical extent; and particularly dwelt upon the distinctness of the fauna of the flagstones of Forfarshire, as giving good grounds for the treatment of the Old Red Fauna as peculiar to a separate geological period, both as distinct from the Silurian System, and in some degree as divisible into two or more members of one group:—three members, if the upper or Holoptychius-beds of Moray, Perth, and Fife, the middle or Fish-beds of Cromarty and Caithness, and the lowest or Forfarshire beds be counted separately; but two, if we regard some of the Old Red beds north of the Grampians as equivalent in time to those on the south.

January 9, 1861.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Distribution of the Corals in the Lias." By the Rev. P. B. Brodie, M.A., F.G.S.

From observations made by himself and others, the author was enabled to give the following notes. In the Upper Lias some Corals of the genera *Thecocyathus* and *Trochocyathus* occur. The Middle Lias of Northamptonshire and Somersetshire has yielded a few Corals. The uppermost band of the Lower Lias, namely the zone with *Ammonites raricostatus* and *Hippopodium ponderosum*, contains an undescribed Coral at Cheltenham and Honeybourn in Gloucestershire; and a *Montlivaltia* in considerable abundance at Down Hatherly in Gloucestershire, at Fenny Compton and Aston Magna in Worcestershire, and at Kilsby Tunnel in Northamptonshire. The middle members of the Lower Lias appear to be destitute of Corals. In the zone with *Ammonites Bucklandi*, called also the Lima-beds, *Isastræa* occurs in Warwickshire and Somersetshire. Dr. Wright states that *Isastræa Murchisoni* occurs in the next lowest bed of the Lower Lias, namely the White Lias with *Ammonites Planorbis*, at Street in Somerset; and another Coral has been found in the same zone in Warwickshire. Lastly, in the "Guinea-bed" at Binton in Warwickshire another Coral has been met with.

The *Montlivaltiæ* of the Hippopodium-bed and the *Isastræa* of the Lima-beds appear to have grown over much larger areas in the Liassic Sea than the other Corals here referred to.

2. "On the Sections of the Malvern and Ledbury Tunnels, on the Worcester and Hereford Railway, and the intervening Line of Railroad." By the Rev. W. S. Symonds, A.M., F.G.S., and A. Lambert, Esq.

In this paper the authors gave an account of the different strata exposed by the cuttings of the Worcester and Hereford Railway (illustrated by a carefully constructed section), including the different members of the New Red Sandstone (on the east of the Malvern Hills), the syenite and greenstone (forming the nucleus of the Malverns), and the Upper Llandovery beds, the Woolhope shales, the Woolhope limestone, Wenlock shales, Wenlock limestone, and Lower Ludlow rock on the west side of the syenite, followed by some beds of the Old Red series, violently faulted against the Ludlow rock at the west end of the Malvern Tunnel. Then the open railway passes over Upper Ludlow rocks and some lower beds of the Old Red series, here and there covered by drift, until the Lower Ludlow rock is again traversed at the east end of the Ledbury Tunnel, and is shown to be much faulted and brought up against Upper Ludlow shales and Aymestry rocks. The Wenlock shales and the Wenlock limestone are then traversed; these are much faulted, the Lower Ludlow beds again coming in, followed by Aymestry rock, Upper Ludlow shales, Downton sandstone, and, at the east end of the tunnel, by red and mottled marls, grey shales and grits, purple shales and sandstones, with the Auchenaspis-beds, forming the passage-beds into the Old Red Sandstone, as described in a former paper (Quart. Journ. Geol. Soc. vol. xvi. p. 193).

In a note, Mr. J. W. Salter, F.G.S., described the great abundance of Upper Silurian fossils found in these cuttings, and now chiefly in the collection of Dr. Grindrod and other geologists at Malvern and the neighbourhood.

XXXVI. *Intelligence and Miscellaneous Articles.*

ON THE FIBROUS ARRANGEMENT OF IRON AND GLASS TUBES.

[Extract from a Letter from Dr. Debus to Professor Tyndall.]

Queenwood College, Feb. 17, 1861.

A FEW weeks ago Mr. E. brought me a piece of iron tube which had been exposed for several years to the action of moist air. Nearly the whole of the tube was converted into oxide of iron. This suggested certain thoughts, the results of which were new and interesting to me.

You know, when a piece of glass tube, sealed and filled with water, is heated, the tube is cracked, and cracked in a longitudinal direction. Why is this? Glass tubes are made by taking a piece of hollow glass in a viscous state and pulling it at both ends. Now, the particles of the glass do not adhere to each other on all sides with a force of the same strength, but in some directions this force is stronger, in others weaker. Under the strain produced by the pulling, they arrange themselves so as to offer to the pulling force the greatest resistance. Therefore the greatest cohesion of the particles is found, in the formed tube, parallel to the length of the tube, and the weakest cohesion in a direction perpendicular to this. This explains the cracking of the tube as mentioned above.

Mr. E. could not give me exact information as to how iron tubes are made, but he said they were passed through rollers. Now, if this is correct, the particles of iron ought to arrange themselves in a similar way to the particles in a glass tube. If such a tube oxidizes, the oxygen naturally would overcome the cohesion of the iron first in those places where this cohesion is weakest, that is, in lines parallel with the tube. Such is actually the case. The tube mentioned had deep furrows, so to say, hollowed out by the oxygen along its length, just in the same way as a glass tube would crack under pressure.

I need not mention to you other examples; but one case more, and then I have done. You gave some years ago an explanation of cleavage*. The paper wherein this explanation was given never came to my hands; and I do not remember that you explained the thing to me when we had personal intercourse. The principle alluded to appears to me to be the true cause of the phenomenon.

If a plastic mass is exposed to pressure, the particles turn until they are in such a position as to offer the greatest resistance to the pressure brought to bear upon them. But that direction wherein they offer the greatest resistance to pressure is also that where the cohesion is least. Consequently cleavage ought to take place in a direction perpendicular to the pressure exerted.

Am I right or wrong? Of course I could say more; but why should I carry water to the well?

H. Debus.

ON THE CALCIUM SPECTRUM.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a communication published in the last Number of the Philosophical Magazine, we pointed out the appearance of a well-defined blue line in the spectrum produced by igniting the evaporated residue of a deep-well water. From the circumstance of this line, which is situated somewhat more towards the violet end of the spectrum than the line Sr δ, not being referred to in the paper of Profs. Kirchhoff and Bunsen, nor indicated in their beautiful representations of the spectra of the alkaligenous metals, we were induced to attribute its production to the probable existence of a previously unrecognized member of the calcium group of metals. Further experiments, however, have satisfied us that the blue line in question really belongs to the calcium spectrum. On finding this to be the case, we communicated with Professor Bunsen, who in return informed us that Professor Kirchhoff and himself had observed this line, but, not thinking it sufficiently bright to be suitable for the recognition of calcium, had not made reference to it in their paper. It can, however, be seen with a degree of brilliancy at least equal to that of many of the lines represented, when perfectly pure chloride of calcium is ignited in a somewhat darkened room.

We remain, Gentlemen,
Your obedient Servants,
F. W. and A. Dupré.

ON THE LUNAR TABLES AND THE INEQUALITIES OF LONG PERIOD DUE TO THE ACTION OF VENUS. BY M. DE PONTÉCOULANT.

In the Number of the *Comptes Rendus* of the labours of the Academy of the 12th of November last, M. Delaunay has inserted a memoir in which he gives an account of the researches in which he has been engaged, concerning the two lunar disturbances of long

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* Fibrous iron was one of my illustrations.—J. T.

period depending on the action of the planet Venus, which Professor Hansen has proposed to introduce into the expressions for the moon's motions. According to M. Delaunay's calculations (the accuracy of which I have at present no intention of disputing) the value of the coefficient of the first of these disturbances—that, namely, whose period is about 273 years—is nearly the same as that attributed to it by the astronomer of Gotha; but the coefficient of the second, whose period is about 240 years (and which is the most important of the two, since its coefficient, calculated at first by Professor Hansen at $23''\cdot2$, is according to his account at least $21''\cdot47$), ought to be considered, according to the researches of M. Delaunay, as altogether insensible, if not absolutely nothing.

This conclusion, which is moreover perfectly in accordance with the announcement of the illustrious geometrician Poisson, published more than twenty-seven years ago in his memoir of 1833, gives rise to several questions of extreme importance, not only, as it seems to me, with respect to the perfection of the lunar tables, but also on the subject of scientific priority, and even of national honour. In order that the members of the Academy may be able to judge of this for themselves, it will be sufficient to mention that the principal corrections which the astronomers of the Greenwich Observatory have thought it necessary to make in the precious tables of our fellow-countryman Damoiseau—tables which are so remarkable from the fact that they are the first that were constructed from theory alone, without recourse to observation,—and the preference which they have accorded to the new lunar tables of Professor Hansen, are principally founded on the existence (considered by them to be conclusively demonstrated) of the two inequalities arising from the action of Venus, which M. Delaunay has just calculated. To this it will be sufficient to add that it was on these grounds that the extraordinary prize of £1000 was allotted to the same Professor by the Lords of the Admiralty, at the suggestion of the learned director of the Greenwich Observatory, for the really marvellous addition, as Mr. Airy calls it, which he has made to the lunar theory. This assertion, if suffered to pass without refutation, might lead us to undervalue the labours of those astronomers, French and foreign, who have brought about the rapid progress of this difficult theory, and have advanced it to its present state of perfection.

I shall not now dwell further on these observations, which from their length would extend beyond the limits prescribed by the Academy to its own members, and still more to strangers whose claims it admits to the honour of an insertion in the *Comptes Rendus*; but I thought it advisable to lose no time in announcing that I am busily occupied in drawing up a memoir, in which all the observations called for by a question of gravity so great that the history of science has rarely furnished one similar to it will be detailed at length, so that it may not be supposed, either in France or abroad, that a memoir so important as that of M. Delaunay has escaped unnoticed or remained unanswered.—*Comptes Rendus,* December 1860.

Fig: 1.

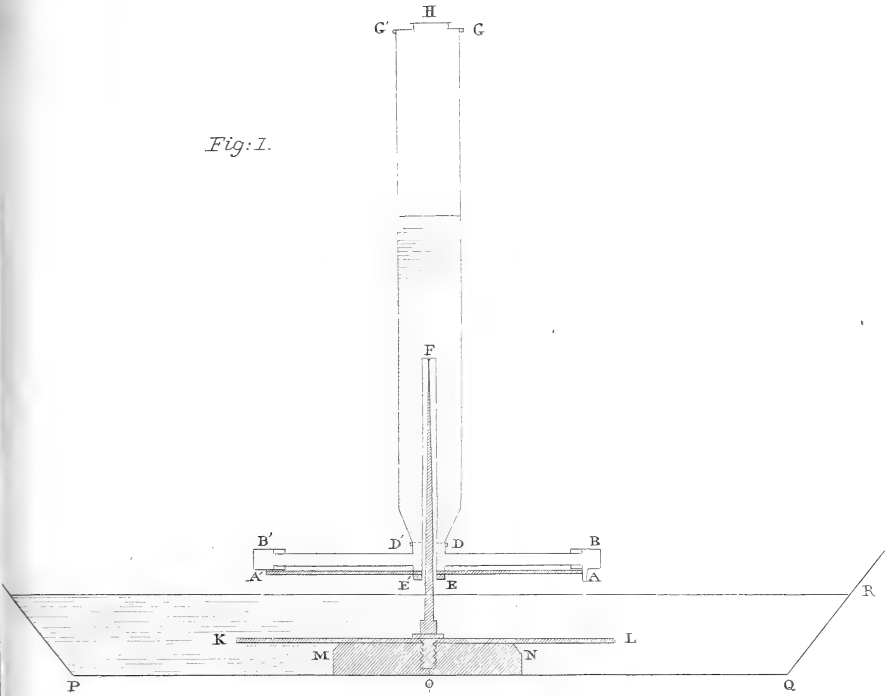


Fig: 3.

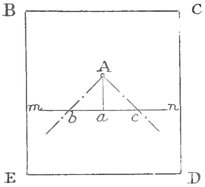


Fig: 4.

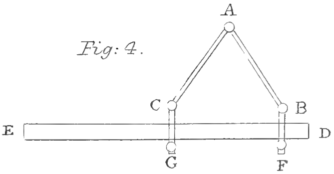
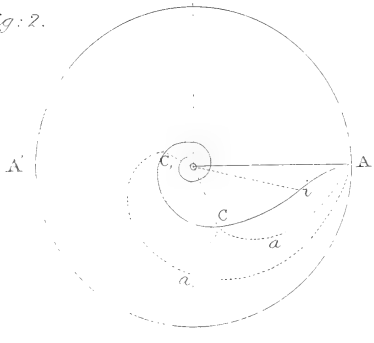


Fig: 2.



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[FOURTH SERIES.]

APRIL 1861.

XXXVII. *On a New Proposition in the Theory of Heat.*
By Professor KIRCHHOFF*.

SOME few months ago I communicated to the Society certain observations, which appeared of interest because they give some information respecting the chemical composition of the solar atmosphere, and point the way to further knowledge on this subject. These observations led to the conclusion that a flame whose spectrum consists of bright lines is partially opake for rays of light of the colour of these lines, whilst it is perfectly transparent for all other light. In this statement we find the explanation of Fraunhofer's dark lines in the solar spectrum, and the justification of the conclusions regarding the composition of the sun's atmosphere; for we find that a substance which, when brought into a flame, produces bright lines coincident with the dark lines of the solar spectrum, must be present in the sun's atmosphere. The fact that a flame is partially opake solely for those rays which it emits, was, as I stated at the time, a matter of some surprise to me. Since that time I have arrived, by very simple theoretical considerations, at a proposition from which the above conclusion is immediately derived. As this proposition appears to me to be of considerable importance on other accounts, I beg to lay it before the Society. A hot body emits rays of heat. We feel this very perceptibly near a heated stove. The intensity of the rays of heat which a hot body emits, depends on the nature and on the temperature of the body, but is quite independent of the nature of the bodies on which the rays fall. We *feel* the rays of heat only in the case of very hot bodies; but they are

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* Abstract of a Lecture delivered before the Natural History Society of Heidelberg. Communicated by Professor Roscoe.

emitted from a body, whatever be its temperature, although the amount diminishes with the temperature. In proportion as a body radiates, it loses heat, and its temperature must sink unless this loss is made up. A body surrounded by substances of the same temperature undergoes no change of temperature. In this case the loss of heat caused by its own radiation is exactly compensated by the rays which surrounding substances give out, a part of which the body absorbs. The quantity of heat which this body absorbs in a given time must be equal to that which in the same time it emits. This holds good whatever the nature of the body may be; the more rays a body emits, the more of the incident rays must it absorb. The intensity of the rays which a body emits has been called its *power of radiation or emission*; and the number denoting the fraction of the incident rays which is absorbed has been called the *power of absorption*. The larger the power of emission a body possesses, the larger must its power of absorption be.

A somewhat closer consideration shows that the relation between the powers of emission and absorption for *one* temperature is the same for all bodies. This conclusion has been verified in many special cases, both in the last ten years and in former times. The foregoing proposition requires, however, that all the rays of heat under consideration are of one and the same kind; so that these rays are not qualitatively so far different that one part of them are absorbed by the bodies more than another part; for, were this the case, we could not speak of the power of absorption of a body, simply because it would be different for different rays. Now we have long known that there really are different kinds of heating rays, and that in general they are unequally absorbed by bodies. There are both dark- and luminous-heating rays; the former are almost all absorbed by white bodies, whilst the latter rays are thus scarcely absorbed at all. Indeed the variety of the rays of heat is even greater than the variety of the coloured rays of light. The rays of heat, the dark as well as the luminous, are influenced in the same manner as the rays of light, by transmission, reflexion, refraction, double refraction, polarization, interference, and diffraction. In the case of the luminous rays of heat, it is not possible to separate the light from the heat; when one is diminished in a given relation, the other is diminished in the same ratio. This has led to the conclusion that rays of light and heat are essentially of the same nature; that rays of light are simply a particular class of the heat-giving rays. The dark rays of heat are distinguished from the rays of light, just as the differently coloured rays are distinguished from each other, by their period of vibration, wave-length, and refractive index. They are not visible because the media of our eyes are not transparent to them.

A difference of quality is noticed amongst the rays of light, not only in respect to the colour, but also in respect to the state of polarization. Hence not only have we to distinguish the heating rays according to the wave-lengths, but we have also to divide rays of one wave-length into those variously polarized. If we take into consideration these various kinds of rays of heat, the conclusions which we had drawn concerning the relation between the powers of absorption and emission cease to be binding.

Whether this relation is still found to exist when these variations are taken into consideration, is a question which has, as yet, not been decided either by theoretical considerations or by an appeal to experiment. I have succeeded in filling up this gap; and I have found that the proposition concerning the ratio between the power of emission and the power of absorption remains true, however different the rays which the bodies emit may be, as long as the notions of emissive and absorptive powers be confined to *one kind* of ray.

The proposition which I have discovered may be thus more precisely defined:—Let a body C be placed behind two screens S_1 and S_2, in which two small openings are made. Through these openings a pencil of rays proceeds from the body C. Of these rays we consider that portion which corresponds to a given wave-length λ, and we divide this into two polarized components, whose planes of polarization are two planes a and b at right angles to each other, passing through the axis of the pencil of rays. Let the intensity of the polarized component a be E (emissive power). Now suppose that a pencil of rays, having a wave-length $=\lambda$, and polarized in the plane a, falls through the openings 2 and 1 upon the body C. The fraction of this pencil which is absorbed by the body C is called A (absorptive power). Then the relation $\frac{A}{E}$ is independent of the position, size, and nature of the body C, and is alone determined by the size of the openings 1 and 2, by the wave-length λ, and by the temperature. I will point out the way in which I have proved this proposition. I began by considering that bodies are conceivable which, although very thin, absorb all the rays which fall upon them, or which have the capacity of absorption $=1$. I call such bodies *perfectly black*, or simply *black*. I first investigated the radiation of such black bodies. Let C be a black body. The body C is supposed to be enclosed in a black envelope, of which the screens S_1 and S_2 are a part, and the two screens are supposed to be connected by a black surface surrounding all. Lastly, let the opening 2 be closed by a black surface, which I will call "surface 2." The whole system is to be considered to possess the same temperature, and to be protected against loss of heat from without by an absolutely non-conducting medium. Under these circumstances the temperature of the body C cannot alter; the sum of the intensities of the rays which it emits must therefore be equal to the sum of the intensities of the rays which it absorbs; and because it absorbs all those that fall upon it, the sum of the intensities of the rays it emits must be equal to the sum of the intensities of the rays which fall upon it. If, now, we suppose the following change: The "surface 2" is removed and replaced by a circular mirror which reflects all the rays falling upon it, and whose centre is in the middle of opening 1. The equilibrium of the heat must still be kept up; the sum of the rays which fall on the body C must still remain equal to the sum of the rays which it emits. But, as it emits just as much as before, the quantity of rays which the mirror reflects upon the body C must be equal to that which the surface 2 emitted. The mirror produces an image of opening 1, which is coincident with opening 1. For this reason, just those rays come back to the body C, after one reflexion from the mirror, as the body C would have emitted through the openings 1 and 2 if this last one had been open; and the intensity of these rays is equal to the intensity of the rays which the surface 2 sent back through the opening 1. This last intensity however, is, evidently independent of the nature of the body C; and hence it follows that the intensity of the pencil of rays which the body C radiates through the openings 1 and 2, is independent of the form, position, and constitution of the body C; supposing of course that this body is black, and that its temperature is a given one. According to this, however, the qualitative composition of the pencil of rays might become different if the body C were replaced by another black body of the same temperature. This is, however, not the case. If I call *e* the power of emission of this black body compared with a certain wave-length and a given plane of polarization—that, therefore, which I have called E under the supposition that C is a body of any kind—then *e* is independent of the nature of the body C, if it only be black. In order to render this evident, a further arrangement is necessary. Into the pencil of rays which passes from the opening 1 towards the surface 2, let us suppose a small plate placed, which is of so slight a thickness that it shows in the visible rays the colours of thin plates; let it be so placed that the pencil of rays is incident at the polarizing angle; let the material of the plate be so chosen that it neither absorbs nor emits a sensible amount of rays; let the envelope joining the screens S_1 and S_2 be so shaped that the image which the plate reflects of the surface 2 lies in the envelope. At the position, and of the size of this image, let an opening in the envelope be made; this I will call "opening 3." Let a screen be so placed that no straight line can be drawn from any point of opening 1 to any point of opening 3 without passing through the screen. Let the opening 3 be now closed with a black surface, which I will call "surface 3." The whole system is then supposed to possess the same temperature; there is therefore in this case equilibrium as regards the heat. This equilibrium is supported by rays which, proceeding from surface 3, suffer reflexion on the plate, pass through opening 1, and fall on the body C. These rays are polarized in the plane of incidence of the plate, and contain, according to the thickness of the plate, sometimes more of one, sometimes more of another kind of ray. Let the surface 3 be removed and replaced by a circular mirror whose centre is situated at the spot where the plate reflects an image of the centre of the opening 1; then the rays emitted by surface 3 will no longer fall on body C, but instead of them those reflected from the mirror will fall upon it, and the equilibrium of the temperature remains unchanged. If we reflect that it does not matter what thickness the plate possesses, or in what position we turn it round the axis of the pencil determined by passing through openings 1 and 2, we arrive, by means of similar considerations, at the conclusion that the power of emission of the black body C, considered with respect to a given wave-length and a given plane of polarization, is quite independent of the constitution of this body. A conclusion which naturally arises from this proposition is, that *all* rays which a black body emits are completely unpolarized.

If we imagine that in the foregoing arrangement the body C is not black, but of any other colour, the following equation is found by similar reasoning:—

$$\frac{E}{A}=e. \qquad (1)$$

This equation indicates that the relation between emission and absorption remains constant for all bodies. The equation may obviously be written

$$E=Ae, \qquad (2)$$

or

$$A=\frac{E}{e}. \qquad (3)$$

I will now notice some remarkable conclusions derived from my proposition. If we heat any body, a platinum wire for example, gradually more and more, it first emits only dark rays; at the temperature at which it begins to glow, red rays begin to appear; at a certain higher temperature yellow rays are seen; then green rays, until at last it becomes white-hot, *i. e.* emits all the rays present in solar light. The power of emission (E) of the platinum wire is therefore equal to 0 for the red rays at all temperatures lower than that at which the wire begins to glow; for yellow rays it ceases to be equal to 0 at a rather higher temperature; for green at a still higher temperature, and so on. According to equation (1), the emission-power (e) of a completely black body must cease to be equal to 0 for red, yellow, green, &c. rays at the same temperatures at which the platinum wire began to emit red, yellow, green, &c. rays. Let us now consider the case of any other body which is gradually heated. According to equation (2), this body must begin to give off red, yellow, and green rays at the *same temperatures* as the platinum wire. All bodies must therefore begin to glow at the same temperature, or at the same temperature begin to give off red, and at the same temperature yellow rays, &c. This is the theoretical explanation of an experimental conclusion obtained by Draper thirteen years ago. The intensity of the rays of given colour which a body radiates at a given temperature may, however, be very different,—according to equation (2) it is proportional to the power of absorption (A). The more transparent a body is, the less luminous it appears. This is the reason why gases, in order to glow visibly, need a temperature so much higher than solid or liquid bodies.

A second deduction which I will mention brings me back to my special subject. The spectra of all opake glowing bodies are continuous; they contain neither bright nor dark lines. Hence we can conclude that the spectrum of a glowing *black* body (the term being used in the sense already defined) must also be a continuous one. The spectrum of an incandescent gas consists, at any rate most frequently, of a series of bright lines separated from each other by perfectly dark spaces. If the power of emission of such a gas be represented by E, the relation $\frac{E}{e}$ has an appreciable value for those rays which correspond to the bright lines of the spectrum of the gas, but it has an inappreciable value for all other rays. According to equation (3), however, this relation is equal to the absorptive power of the incandescent gas. Hence it follows that the spectrum of an incandescent gas *will be* the *converse* of this, as I express it, when it is placed before a source of light of sufficient intensity, which gives a continuous spectrum; *i. e.* the lines of the gas-spectrum, which before were bright, will be seen as dark lines on a bright ground. A remarkable deduction from my proposition which I will mention is, that, if the more remote source of light is an incandescent solid body, the temperature of this body must be higher than that of the incandescent gas in order that such a conversion of the spectrum may occur.

The sun consists of a luminous nucleus, which would by itself produce a continuous spectrum, and of an incandescent gaseous atmosphere, which by itself would produce a spectrum consisting of an immense number of bright lines characteristic of the numerous substances which it contains. The actual solar spectrum is the converse of this. Were it possible to observe the spectrum belonging to the solar atmosphere with all its attendant bright lines, no one would be surprised to hear that, from the existence of the characteristic bright lines of sodium, potassium, and iron in the solar spectrum, the presence of these bodies in the sun's atmosphere has been ascertained. According to the proposition which I have just laid down, there can, however, be just as little doubt concerning the truth of this assertion, as if we saw the real spectrum of the solar atmosphere.

I will, lastly, mention a phenomenon which, although apparently trivial, was of peculiar interest to me, because I foresaw it theoretically, and afterwards verified it by experiment. According to theory, a body which absorbs more rays polarized in *one* direction than in another, must also emit those rays in the same proportion. A plate of tourmaline cut parallel to the optical axis absorbs, at common temperatures, more of those rays falling perpendicularly, whose plane of polarization is parallel to the axis of the crystal, than of those whose plane is at right angles to the axis. At temperatures above a red heat, tourmaline also possesses this same property, although in a less marked degree. Hence the rays of light which the plate of tourmaline emits perpendicular to its surface must be partially polarized; and, moreover, they must be polarized in a plane perpendicular to the plane of polarization of the rays which have been transmitted by the tourmaline. This theoretical conclusion is borne out by experiment.

XXXVIII. *Remarks on* Ampère's *Experiment on the Repulsion of a Rectilinear Electrical Current on itself. By* Mr. JAMES CROLL, *Glasgow*[*].

IN reference to Dr. Forbes's "Notes on Ampère's Experiment on the Repulsion of a Rectilinear Electrical Current on itself," which appeared in the Philosophical Magazine for February, the following remarks may perhaps be acceptable.

I have long been under the impression that Ampère's experiment, although successful, does not prove the thing intended, namely, that the different parts of a rectilinear electrical current are mutually repulsive; for the motion of the wire is evidently

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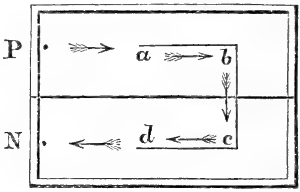
[*] Communicated by the Author.

due to the action of angular currents, and not to a repulsion existing between the current in the mercury and the current in the branch of the wire in the same straight line.



Let *a b c d* be the wire floating on the mercury, P the point where the current enters the mercury, and N the point where it leaves it, after passing through the wire in the direction indicated by the arrows. The common explanation is, that the movement of the wire is due to there being in each of the branches *a b* and *c d*, separately, a repulsion between the current which traverses them, and the current that is transmitted into the mercury before penetrating into the wire or after going out from it; and as the current of the mercury and that of the wire are only the prolongation of each other in a right line, this is considered sufficient proof that the one part of the rectilinear current repels the other part.

The following is, however, I think, the true explanation. The current P *a* in the mercury is at right angles to the current *b c* in the cross part of the wire. The former current is directed towards, and the latter current from the summit of the angle *a b c* formed by them. Now, according to Ampère's well-known law of angular currents, the two currents will repel each other. In this case the current *b c* being the moveable one, it will of course recede, maintaining a position parallel to itself. This cross current *b c* is also at right angles to the current N *d* on the other side of the glass partition; the former moving towards, and the latter from the summit of the angle *b c d* formed by them. These two currents for the same reason will repel each other. The combined influence of the currents P *a* and N *d* in the mercury will be to cause the cross section of the wire which is at *right angles* to them to recede, maintaining a position parallel to itself. It follows, therefore, according to the law of angular currents, that Ampère's experiment must be successful; but then its success does not prove that the parts of a rectilinear electrical current are repulsive of each other, but simply that a moveable current at right angles to a fixed one is repelled when the one current is directed towards, and the other from the summit of the angle formed by these two currents.

It would appear from Dr. Forbes's experiments, that the different sections of a rectilinear current are mutually attractive; not repulsive, as is generally supposed. To remove as much as possible all resistance to the motion of the wire, and also to simplify the conditions of the experiment, instead of floating the moveable wire upon mercury, he placed it upon one of the arms of a delicate torsion balance. The ends of the moveable wire were made to bear slightly against the extremes of the two wires in connexion with the poles of the pile. When the current was established, the moveable wire placed upon the balance was attracted by the wires proceeding from the pile, and not repelled as in the case of Ampère's experiment; and the stronger the current, and the more complete the contact of the ends of the wires, the greater the attraction was found to be.

There is one objection which may be urged against the conclusiveness of the experiment. It is well known that the ends of two wires connecting the poles of a voltaic pile before they are brought into contact, are statically charged, the one with positive, and the other with negative electricity, the intensity of the charge depending upon the force of the pile. Now it is evident that these two wires being charged with different electricities, must attract each other. It is evident also that, however close the two ends of the wires may be brought together, unless they are in absolute contact in every part, a thing impossible without soldering the ends together, the current will not pass from the extremity of the one wire to that of the other through the intermediate space, which is almost non-conducting, unless there is an excess of positive electricity on the one wire and of negative on the other; and the more so, considering the low tension of voltaic electricity. This being the case, the ends of the conducting wire and those of the moveable branch will always be charged with different quantities of electricity, and hence attraction will be the consequence; yet one would suppose that, unless the current is in reality self-attractive, the attraction arising from this cause would not be able to overcome the repulsion which must ensue from the action of angular currents just considered.

There is one objection to the common notion that the parts of a rectilinear current are mutually repulsive, that I have never seen adduced, which is as follows: It results from a law of Ohm, which has been confirmed experimentally by Kohlrausch, that in the conductor connecting the poles of a voltaic pile, while the current is circulating, different sections of the conductor are differently charged. In any part of the conductor whatever, a section taken towards the positive pole is always positive in relation to a section taken towards the negative pole; and *vice versâ,* a section towards the negative pole is negative in relation to any section taken towards the positive pole. It follows from this that the different sections of the current must attract each other.

The difference of the tension of any two sections depends upon the resistance to conduction; that is to say, the force by which any two sections of the current attract each other when the current is passing, is always as the amount of the resistance which opposes this attraction. The attraction must always exceed the resistance, or else there can be no current. What does all this mean if we do not admit that the sections of the current mutually attract each other?

The same difference of electric state exists in the various sections of the pile itself; for we know that there is always an excess of positive electricity at the one pole, and of negative at the other, and these electricities must tend to unite through the pile itself. But there is opposed to the attractive force of the electricities for uniting, not only the resistance in the pile itself, but also the electromotive force which decomposes the electricity. This electromotive force must always exceed the attraction of the electricities: this must be admitted; for unless the force which separates the electricities is greater than the attractive force which tends to unite them, there could be no decomposition of the electricity, and of course no current.

In the pile itself there are two forces—one tending to unite the various sections of the current, and the other tending to separate them; the latter force being the strongest, the sections of the current in the pile will mutually repel each other. In the external conductor which unites the poles of the pile, the latter force has no existence; hence the various sections of the current in the conductor are mutually attractive. By virtue of the repulsion in the pile and the attraction in the conductor, the electricity decomposes in the former and unites in the latter; and this constitutes what we call an electric current.

XXXIX. *On Theories of Magnetism and other Forces, in reply to Remarks by* Professor Maxwell. *By* J. Challis, *F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

IN an article on "Molecular Vortices applied to Magnetic Phenomena," contained in the March Number of the Philosophical Magazine, Professor Maxwell has made, respecting certain points of the general physical theory which I have lately proposed, some remarks which call for a reply. I refer chiefly to two paragraphs in p. 163, the first of which is as follows:—"Currents, issuing from the north pole and entering the south pole of a magnet, or circulating round an electric current, have the advantage of representing correctly the geometrical arrangement of the lines of force, if we could account for the pheno-

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[*] Communicated by the Author.

mena of attraction, or for the currents themselves, or explain their continued existence." The generation of such currents I have explained on hydrodynamical principles in my theories of galvanism and magnetism, as also in that of electricity. They are shown to be *secondary* currents, which are always produced when a uniform primary current traverses a medium, in which there is a gradation of density, such as that which must exist from the top to the bottom of a heavy mass resting on a horizontal plane, in order that the force of gravity on the individual particles may be counteracted. The *primary* currents are ascribed exclusively to motions of the æther caused by the rotations of the earth and of the other bodies of the solar system about their axes. As this is a constant cause, the streams are constant. The retention of an induced state of gradation of density from end to end, is considered to be the distinctive property of a magnetized bar. The observed attractions and repulsions are satisfactorily accounted for by the variation of the fluid pressure from point to point of space in the secondary currents considered as instances of steady motion, such variation, together with the dynamical effect of the currents, producing differences of pressure at different points of the surfaces of the atoms of which the substances attracted or repelled are supposed to consist. Thus the three explanations which Professor Maxwell considers to be requisite respecting currents to which the phenomena of galvanism and magnetism are attributed, are in fact given by my general theory quite consistently with its original hypotheses.

The other paragraph commences thus:—"Undulations issuing from a centre would, according to the calculations of Professor Challis, produce an effect similar to attraction in the direction of a centre." I consider that both central attraction and central repulsion are accounted for by my calculations. Professor Maxwell then adduces the following objection:—"Admitting this to be true, we know that two series of undulations traversing the same space do not combine into one resultant as two attractions do, but produce an effect depending on relations of *phase* as well as intensity." This point I have considered in articles 2 and 5 of the Theory of Gravity contained in the Philosophical Magazine for December 1859. There is no limitation as to the function W, which expresses the velocity or condensation of the ætherial waves, excepting that it must either be a single circular function, or consist of the sum of several such functions. Let it in general be represented by $\Sigma \, . \, m \sin (bt+c)$. Then, according to the theoretical calculation, the motion of translation given to an atom in the direction of the propagation of the waves, that is, *the repulsive action,* depends on the non-periodic part of the *square* of this function, the quantity which I have called q being in this case insignificant. Now, whatever be the phases of the several component functions, the non-periodic part of the square of their sum is equal to the sum of the non-periodic parts of the squares of the separate functions. In the case of *attractive action*, according to the investigation given in the Mathematical Theory of Attractive Forces contained in the Philosophical Magazine for November 1859, the motion of translation depends on the value of q, and on the product of W and $\frac{d^2W}{dt^2}$. But it is easily seen that if W have the form above assigned, the non-periodic part of the product for the sum of the terms is the sum of the non-periodic parts of the products for the component terms taken separately, independently of their phases. In short, as we are only concerned with *squares* of circular functions, the mutual interferences by difference of phase do not come under consideration. On this account the dynamical effects of two series of undulations from separate sources, take place independently of each other, and combine according to the laws of the composition of accelerative forces. To the additional objection, that, "if the series of undulations be allowed to proceed, they diverge from each other without any mutual action," I can make no reply, because I do not understand it. I can only conclude that it was written under some misapprehension.

Professor Maxwell goes on to assert that "the mathematical laws of attractions are not analogous in any respect to those of undulations." In making this assertion he must surely have overlooked the very remarkable analogies exhibited by the facts, that undulations, like *central* forces, diverge from a centre, and diminish in intensity according to some law of the distance. Each body in the universe on which a series of undulations is incident, becomes a centre of minor undulations, just as when waves on the surface of water encounter an insulated obstacle, the obstacle becomes a centre of subordinate waves. The continuous generation of subordinate undulations corresponds to the maintenance of the gravitating power of the body.

Perhaps, however, the assertion, although it is not limited, was intended to apply only to such attractions as are observed in a magnetic field. If so, it is in accordance with my general theory, which makes a distinction between attractions and repulsions by means of *undulations*, and attractions and repulsions due to *currents*. Electric, galvanic, and magnetic forces are of the latter kind. But, while it is admitted that the laws of these forces "have remarkable analogies with those of currents," I should not say that they are analogous "to the laws of the conduction of heat and electricity, and of elastic bodies," because, according to the views which I maintain, these are phenomena which ultimately may receive explanations by means of ætherial undulations and currents, and therefore ought not to be put in the same category.

As the article I have been referring to contains a theory of magnetic phenomena wholly different from that which I have advanced, it may be worth while to point out a difference in principle between the fundamental hypotheses of the two theories. Professor Maxwell assumes the existence of a magnetic medium, which is not fluid, but "mobile," and which acts along lines of magnetic force by *stress* combined with hydrostatic pressure; in other words, there is greater pressure in the equatorial, than in the axial, direction of the magnetic field. To account for this difference of pressure, it is assumed that "molecular vortices" circulate about axes parallel to the lines of magnetic force. Why they are called "molecular" is not expressly stated in this article; but it may be gathered from other of the author's writings, that he conceives the matter of the vortices to consist of molecules which by their motions may come into collision, the resulting dynamic effect depending on the number and frequency of such collisions. It is not my intention to criticise these hypotheses, which have been adduced merely for the sake of remarking that, as they are of a particular character, and have been framed apparently with special reference to observed laws of magnetic phenomena, the results of a mathematical investigation founded on them can hardly amount to more than an empirical expression of those laws. After all that can be done by this kind of research, an independent and *à priori* theory of the same kind as that which I have proposed, if not the very one, is still needed.

The hypotheses on which my investigations have been founded are these only. The physical forces are modes of action of the pressure of the æther, which is a continuous fluid medium, having the property of pressing in proportion to its density, and filling all space not occupied by the discrete atoms of sensible bodies, which atoms are inert, spherical, and of different, but constant, magnitudes. It may be remarked that these hypotheses include no ideas that are not intelligible by sensation and experience, and therefore conform to the rule of philosophy according to which our knowledge of natural operations must ultimately rest on such ideas. Also they are strictly related to antecedent and existing physical science. The prominent terms, *æther* and *atom*, which had their origin in ancient speculations, have obtained remarkable significance in modern science,—the first, by explanations of the phenomena of light, and the other, by aiding us to conceive of chemical analyses. The hypotheses under the above form are mainly due to Newton, who gave a definition of atoms, and suggested the dynamic action of the æther; but the state of mathematics in his day did not allow of investigating the consequences of the latter idea. In attempting to do this in the present advanced state of mathematics, I have only added, for that purpose, to the Newtonian hypotheses, an exact definition of the æther, and the supposition that the atoms are spherical. In the first instance I applied the hypotheses as a foundation for a theory of light, having long since seen that the theory which proposes to account for the phenomena of light by the oscillations of the discrete atoms of a medium having axes of elasticity, is contradicted by facts, and must therefore be abandoned. This charge I have brought against it in an article on elliptically polarized light in the Philosophical Magazine for April 1859 (p. 288); and it has found no defender. When this first application was made, I had no conception of any modes of applying the same hypotheses to explain the phenomena of gravity, electricity, galvanism, and magnetism. If they are found to admit of applications so varied and extensive, the explanations are no longer *personal*, the hypotheses themselves explain, because they are true. The only advantage I pretend to possess in these researches is, the discovery of the true principles of the application of partial differential equations to determine the motion and pressure of an elastic fluid. But this kind of reasoning, though it is indispensable for the establishment of the truth of the general physical theory, may be tried on its own merits, quite independently of its application in that theory. For this reason I expressed the intention of carefully revising the proofs of the propositions in hydrodynamics which have been already enunciated in this Journal, and the results of which have been applied in the physical theory. But my occupations do not allow of entering on this task at present.

Cambridge Observatory,
March 16, 1861.

XL. *On certain peculiar Forms of Capillary Action.*
By Thomas Tate, *Esq.*[*]

LIQUIDS rise in small tubes by what is called capillary action, that is, by the cohesion of the particles of the liquid for one another, as well as by their adhesion to the sides of the tube. It has been ascertained that the height to which a liquid is raised by capillary action varies inversely as the diameter of the tube. The chief object of this paper is to determine, by direct experiment, the law of capillary resistance exerted

[*] Communicated by the Author.

by a liquid filling small orifices or perforations made in rigid plates of different thicknesses.

Fig. 1.

Let A C represent a wide glass tube, closed at the top, and having a perforated plate E *e*, capable of being wetted, cemented to its lower extremity; E B a glass tube, about half an inch in diameter, cemented to this plate, open at its lower extremity, and communicating with the tube A C; *e* a small perforation made in the plate; D B a glass vessel containing water or any other liquid to be examined. The tube E B is graduated from the exterior surface of the plate into inches and decimal parts of an inch. The tubes being filled with water, and the extremity B inserted in the water contained in the vessel D B, it will be found that the orifice *e* may be raised for some inches above the level F D of the water in the vessel before the atmospheric air will enter the orifice. The height C D, at which the external air enters the orifice, obviously gives us the measure of the capillary resistance of the liquid in the orifice. The following results of experiments show that, the temperature being constant, the height of the column C D, measuring the capillary resistance of the liquid, varies inversely as the diameter of the orifice.

The experiments recorded in the following Table of results, were made with the apparatus represented in fig. 1. The orifices were made in plates of gutta percha by means of fine steel wires, whose diameters had been previously determined. Slight oscillations and other extraneous causes having been found to affect the results of the experiments, each result here given is the mean of five experiments.

Table of results of experiments giving the columns of capillary resistance corresponding to different diameters of the orifices.

The liquid was water at the temperature of 56° F., and the thickness of the plate was ·05 of an inch.

Diameter of the orifice in parts of an inch, D.	Corresp. column C D of capillary resist. in inches, h.	Value of h by formula $h=\frac{1}{22}\cdot\frac{1}{\mathrm{D}}$.
$\frac{1}{75}$	3·35	3·32
$\frac{1}{40}$	1·80	1·82
$\frac{1}{30}$	1·40	1·36
$\frac{1}{25}$	1·12	1·14

The near coincidence of the results of the second and third columns shows that, *other things being the same, the height of the column* C D, *measuring the capillary resistance, varies inversely as the diameter of the orifice.*

The column of resistance, C D, is slightly affected by the thickness of the plate: thus with a half thickness of plate, and with an orifice of $\frac{1}{75}$th of an inch, the height of this column was found to be 3 inches nearly.

The number of perforations made in the plate does not affect the results.

An increase of temperature sensibly reduces the height of the column C D: thus at the temperature of 84°, with an orifice of $\frac{1}{75}$th of an inch, this column was found to be about $\frac{1}{10}$th of an inch less than it was at the temperature of 56°.

The column of capillary resistance, C D, for viscous liquids, such as diluted solutions of gum and sugar, was found to be considerably greater than the corresponding column for water.

Let the tube E B (fig. 2), closed at the top by the perforated plate E *e*, be depressed in the liquid, the orifice *e* being wet; it will be found that the liquid will not rise in the tube to the same level as the liquid in the vessel: the column of depression C D, in this case measuring the capillary resistance under the same circumstances, is *nearly* equal to the elevation of the column C D of fig. 1. Here the resistance is simply due to the film of liquid filling the orifice *e*. It is scarcely necessary to state that, when the orifice *e* is dry, or nearly dry, the liquid will rise in the tube to the same level as the liquid in the vessel E B.

Fig. 2.

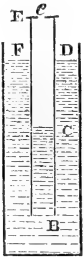

Fig. 3.

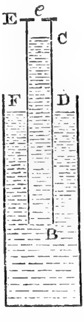

Fig. 4.

In like manner, if the tube, partially filled with liquid, be raised as represented in fig. 3, it will be found that the liquid will stand in the tube some distance higher than the level of the liquid in the vessel. The column of elevation, C D, in this case measures the capillary resistance to the pressure of the external air.

If the tube, completely filled with liquid, be raised as represented in fig. 4, it will be found that the liquid will stand in the tube some distance higher than the level of the liquid in the vessel. The column of elevation C D, measuring the capillary action, was found, under the same circumstances, to be nearly the same as in the preceding cases. Here the capillary action is restricted to the orifice of the thin plate, the diameter of the tube E B with which the experiment was performed being about one inch. This experiment strikingly shows that *the height of the column measuring capillary action for any given liquid at a constant temperature depends* (chiefly, if not entirely) *upon the diameter of that portion of the tube immediately in contact with the upper surface of the liquid.*

March 18, 1861.

XLI. *On a Theorem of* Abel's *relating to Equations of the Fifth Order.* *By* A. CAYLEY, *Esq.*[*]

THE following is given (Abel, *Œuvres*, vol. xi. p. 253) as an extract of a letter to M. Crelle:—

"Si une équation du cinquième degré, dont les coefficients sont des *nombres rationnels*, est résoluble algébriquement, on peut donner aux racines la forme suivante,

$$x=c+\mathrm{A}a^{\frac{1}{5}}a_1^{\frac{2}{5}}a_2^{\frac{4}{5}}a_3^{\frac{3}{5}}+\mathrm{A}_1a_1^{\frac{1}{5}}a_2^{\frac{2}{5}}a_3^{\frac{4}{5}}a^{\frac{2}{5}}+\mathrm{A}_2a_2^{\frac{1}{5}}a_3^{\frac{2}{5}}a^{\frac{4}{5}}a_1^{\frac{3}{5}}+\mathrm{A}_3a_3^{\frac{1}{5}}a^{\frac{2}{5}}a_1^{\frac{4}{5}}a_2^{\frac{3}{5}},$$

où

$$\begin{aligned}
a\ &=m+n\sqrt{1+e^2}+\sqrt{h(1+e^2+\sqrt{1+e^2})},\\
a_1&=m-n\sqrt{1+e^2}+\sqrt{h(1+e^2-\sqrt{1+e^2})},\\
a_2&=m+n\sqrt{1+e^2}-\sqrt{h(1+e^2+\sqrt{1+e^2})},\\
a_3&=m-n\sqrt{1+e^2}-\sqrt{h(1+e^2-\sqrt{1+e^2})},
\end{aligned}$$

$$\begin{aligned}
\mathrm{A}\ &=\mathrm{K}+\mathrm{K}'a\ +\mathrm{K}''a_2+\mathrm{K}''aa_2, &\quad \mathrm{A}_1&=\mathrm{K}+\mathrm{K}'a_1+\mathrm{K}''a_3+\mathrm{K}'''a_1a_3,\\
\mathrm{A}_2&=\mathrm{K}+\mathrm{K}'a_2+\mathrm{K}''a\ +\mathrm{K}''aa_2, &\quad \mathrm{A}_3&=\mathrm{K}+\mathrm{K}'a_3+\mathrm{K}''a_1+\mathrm{K}'''a_1a_3.
\end{aligned}$$

Les quantités c, h, e, m, n, K, K$'$, K$''$, K$'''$ sont des *nombres rationnels*.

"Mais de cette manière l'équation $x^5+ax+b=0$ n'est pas résoluble tant que a et b sont des quantités quelconques. J'ai trouvé de pareils théorèmes pour les équations du 7ème, 11ème, 13ème, &c. degré. Fribourg, le 14 Mars, 1826."

The theorem is referred to by M. Kronecker (Berl. *Monatsb.* June 20, 1853), but nowhere else that I am aware of.

It is to be noticed that in the expressions for a, a_1, a_2, a_3, the

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[*] Communicated by the Author.

radicals are such that

$$\sqrt{1+e^2}\sqrt{h(1+e^2+\sqrt{1+e^2})}\sqrt{h(1+e^2-\sqrt{1+e^2})}=he(1+e^2),$$

a rational number.

The theorem is given as belonging to numerical equations; but considering it as belonging to literal equations, it will be convenient to change the notation; and in this point of view, and to avoid suffixes and accents, I write

$$x=\theta+\mathrm{A}\alpha^{\frac{1}{5}}\beta^{\frac{2}{5}}\gamma^{\frac{4}{5}}\delta^{\frac{3}{5}}+\mathrm{B}\beta^{\frac{1}{5}}\gamma^{\frac{2}{5}}\delta^{\frac{4}{5}}\alpha^{\frac{3}{5}}+\mathrm{C}\gamma^{\frac{1}{5}}\delta^{\frac{2}{5}}\alpha^{\frac{4}{5}}\beta^{\frac{3}{5}}+\mathrm{D}\delta^{\frac{1}{5}}\alpha^{\frac{2}{5}}\beta^{\frac{4}{5}}\gamma^{\frac{3}{5}},$$

where

$$\begin{aligned}\alpha&=m+n\sqrt{\Theta}+\sqrt{p+q\sqrt{\Theta}},\\ \beta&=m-n\sqrt{\Theta}+\sqrt{p-q\sqrt{\Theta}},\\ \gamma&=m+n\sqrt{\Theta}-\sqrt{p+q\sqrt{\Theta}},\\ \delta&=m-n\sqrt{\Theta}-\sqrt{p-q\sqrt{\Theta}};\end{aligned}$$

the radicals being connected by

$$\sqrt{\Theta}\sqrt{p+q\sqrt{\Theta}}\sqrt{p-q\sqrt{\Theta}}=s,$$

and where

$$\begin{aligned}\mathrm{A}&=\mathrm{K}+\mathrm{L}\alpha+\mathrm{M}\gamma+\mathrm{N}\alpha\gamma, & \mathrm{B}&=\mathrm{K}+\mathrm{L}\beta+\mathrm{M}\delta+\mathrm{N}\beta\delta,\\ \mathrm{C}&=\mathrm{K}+\mathrm{L}\gamma+\mathrm{M}\alpha+\mathrm{N}\alpha\gamma, & \mathrm{D}&=\mathrm{K}+\mathrm{L}\delta+\mathrm{M}\beta+\mathrm{N}\beta\delta,\end{aligned}$$

in which equations θ, m, n, p, q, Θ, s, K, L, M, N are rational functions of the elements of the given quintic equation.

The basis of the theorem is, that the expression for x has only the five values which it acquires by giving to the quintic radicals contained in it their five several values, and does not acquire any new value by substituting for the quadratic radicals their several values. For, this being so, x will be the root of a rational quintic; and conversely.

Now attending to the equation

$$\sqrt{\Theta}\sqrt{p+q\sqrt{\Theta}}\sqrt{p-q\sqrt{\Theta}}=s,$$

the different admissible values of the radicals are

$$\begin{array}{rrr}\sqrt{\Theta}, & \sqrt{p+q\sqrt{\Theta}}, & \sqrt{p-q\sqrt{\Theta}},\\ -\sqrt{\Theta}, & \sqrt{p-q\sqrt{\Theta}}, & -\sqrt{p+q\sqrt{\Theta}},\\ \sqrt{\Theta}, & -\sqrt{p+q\sqrt{\Theta}}, & -\sqrt{p-q\sqrt{\Theta}},\\ -\sqrt{\Theta}, & -\sqrt{p-q\sqrt{\Theta}}, & \sqrt{p+q\sqrt{\Theta}},\end{array}$$

corresponding to the systems

$$\begin{array}{cccc} \alpha, & \beta, & \gamma, & \delta \\ \beta, & \gamma, & \delta, & \alpha \\ \gamma, & \delta, & \alpha, & \beta \\ \alpha, & \beta, & \gamma, & \delta \end{array}$$

of the roots $\alpha, \beta, \gamma, \delta$; *i. e.* the effect of the alteration of the values of the quadratic radicals is merely to cyclically permute the roots $\alpha, \beta, \gamma, \delta$; and observing that any such cyclical permutation gives rise to a like cyclical permutation of A, B, C, D, the alteration of the quadratic radicals produces no alteration in the expression for x.

The quantities $\alpha, \beta, \gamma, \delta$ are the roots of a rational quartic. If, solving the quartic by Euler's method, we write

$$\begin{aligned} \alpha &= m+\sqrt{F}+\sqrt{G}+\sqrt{H}, \qquad \sqrt{FGH}=\nu, \text{ a rational function,} \\ \beta &= m-\sqrt{F}+\sqrt{G}-\sqrt{H}, \\ \gamma &= m+\sqrt{F}-\sqrt{G}-\sqrt{H}, \\ \delta &= m-\sqrt{F}-\sqrt{G}+\sqrt{H}, \end{aligned}$$

then the expressions for F, G, H in terms of the roots are

$$(\alpha+\gamma-\beta-\delta)^2, \quad (\alpha+\beta-\gamma-\delta)^2, \quad (\alpha+\delta-\beta-\gamma)^2,$$

which are the roots of a cubic equation

$$u^3-\lambda u^2+\mu u-\nu^2=0,$$

where λ, μ, ν are given rational functions of the coefficients of the quartic. We have

$$\begin{aligned} \sqrt{G}+\sqrt{H} &= \sqrt{(\sqrt{G}+\sqrt{H})^2}=\sqrt{G+H+2\sqrt{GH}} \\ &= \sqrt{\lambda-F+\frac{2\nu}{F}\sqrt{F}}. \end{aligned}$$

So that, taking $\Theta=F$, the last-mentioned expressions for $\alpha, \beta, \gamma, \delta$ will be of the assumed form

$$\alpha=m+\sqrt{\Theta}+\sqrt{p+q\sqrt{\Theta}}, \text{ \&c.}$$

The equation

$$\sqrt{\Theta}\sqrt{p+q\sqrt{\Theta}}\sqrt{p-q\sqrt{\Theta}}=s$$

thus becomes

$$\sqrt{F}\sqrt{(G-H)^2}=s, \text{ or } F(G-H)^2=s^2;$$

that is,

$$-F^3+F(F^2+G^2+H^2)-2FGH=s^2;$$

or, what is the same thing, and putting Θ for F,

$$-\lambda\Theta^2+(\lambda^2-\mu)\Theta-3\nu^2=s^2.$$

Hence in order that the roots of the quartic may be of the assumed form,

$$\alpha=m+\sqrt{\Theta}+\sqrt{p+q\sqrt{\Theta}},\ \&c.,$$

where m, p, q, Θ are rational, and where also

$$\sqrt{\Theta}\sqrt{p+q\sqrt{\Theta}}\sqrt{p-q\sqrt{\Theta}}=s, \text{ a rational function,}$$

the necessary and sufficient conditions are that the quartic should be such that the reducing cubic

$$u^3-\lambda u^2+\mu u-\nu^2=0$$

(whose roots are $(\alpha+\beta-\gamma-\delta)^2$, $(\alpha+\gamma-\beta-\delta)^2$, $(\alpha+\delta-\beta-\gamma)^2$) may have *one rational root* Θ, and moreover that the function

$$-\lambda\Theta^2+(\lambda^2-\mu)\Theta-3\nu^2$$

shall be the *square of a rational function s*. This being so, the roots of the quartic will be of the assumed form,

$$\alpha=m+\sqrt{\Theta}+\sqrt{p+q\sqrt{\Theta}},\ \&c.$$

And from what precedes, it is clear that any function of the roots of the quartic which remains unaltered by the cyclical substitution $\alpha\beta\gamma\delta$, or what is the same thing, any function of the form

$$\phi(\alpha,\beta,\gamma,\delta)+\phi(\beta,\gamma,\delta,\alpha)+\phi(\gamma,\delta,\alpha,\beta)+\phi(\delta,\alpha,\beta,\gamma)$$

will be a rational function of m, Θ, p, q, s, and consequently of the coefficients of the quartic. The above are the conditions in order that a quartic equation may be of the Abelian form.

It may be as well to remark that, assuming only the system of equations

$$\begin{aligned}\alpha&=m+\sqrt{\Theta}+\sqrt{\Upsilon},\\ \beta&=m-\sqrt{\Theta}+\sqrt{\Upsilon'},\\ \gamma&=m+\sqrt{\Theta}-\sqrt{\Upsilon},\\ \delta&=m-\sqrt{\Theta}-\sqrt{\Upsilon'},\end{aligned}$$

then any rational function of $\alpha, \beta, \gamma, \delta$ which remains unaltered by the cyclical substitution $\alpha\beta\gamma\delta$ will be a rational function of Θ, $\Upsilon+\Upsilon'$, $\Upsilon\Upsilon'$, $\sqrt{\Upsilon\Upsilon'}(\Upsilon-\Upsilon')$, $\sqrt{\Theta}(\Upsilon-\Upsilon')$, $\sqrt{\Theta}\sqrt{\Upsilon\Upsilon'}$. In fact, suppose such a function contains the term

$$(\sqrt{\Theta})^\alpha(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma;$$

then it will contain the four terms

$$(\ \sqrt{\Theta})^\alpha(\ \sqrt{\Upsilon}\,)^\beta(\ \sqrt{\Upsilon'})^\gamma,$$
$$(-\sqrt{\Theta})^\alpha(\ \sqrt{\Upsilon'})^\beta(-\sqrt{\Upsilon}\,)^\gamma,$$
$$(\ \sqrt{\Theta})^\alpha(-\sqrt{\Upsilon}\,)^\beta(-\sqrt{\Upsilon'})^\gamma,$$
$$(-\sqrt{\Theta})^\alpha(-\sqrt{\Upsilon'})^\beta(\ \sqrt{\Upsilon})^\gamma,$$

which together are

$$(\sqrt{\Theta})^\alpha\{(1+(-)^{\beta+\gamma}1)(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma+(-)^\alpha[(-)^\beta 1+(-)^\gamma 1](\sqrt{\Upsilon})^\gamma(\sqrt{\Upsilon'})^\beta\},$$

an expression which vanishes unless $(-)^\beta$, $(-)^\gamma$ are both positive or both negative. The forms to be considered are therefore

$(-)^\alpha$,	$(-)^\beta$,	$(-)^\gamma$
+	+	+
−	+	+
+	−	−
−	−	−

The first form is

$$(\sqrt{\Theta})^\alpha\{(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma+(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma,$$

which, α, β, γ being each of them even, is a rational function of Θ, $\Upsilon+\Upsilon'$, $\Upsilon\Upsilon'$.

The second form is

$$(\sqrt{\Theta})^\alpha\{(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma-(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma\},$$

which, α being odd and β and γ each of them even, is the product of such a function into $\sqrt{\Theta}(\Upsilon-\Upsilon')$.

The third form is

$$(\sqrt{\Theta})^\alpha\{(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma-(\sqrt{\Upsilon})^\gamma(\sqrt{\Upsilon'})^\beta\},$$

which, α being even and β and γ each of them odd, is the product of such a function into $\sqrt{\Upsilon\Upsilon'}(\Upsilon-\Upsilon')$.

And the fourth form is

$$(\sqrt{\Theta})^\alpha\{(\sqrt{\Upsilon})^\beta(\sqrt{\Upsilon'})^\gamma+(\sqrt{\Upsilon})^\gamma(\sqrt{\Upsilon'})^\beta\},$$

which, α, β, γ being each of them odd, is the product of such a function into $\sqrt{\Theta}(\Upsilon-\Upsilon')$.

Hence if $\Upsilon=p+q\sqrt{\Theta}$, $\Upsilon'=p-q\sqrt{\Theta}$, and

$$\sqrt{\Theta}\sqrt{p+q\sqrt{\Theta}}\,\sqrt{p-q\sqrt{\Theta}}=s,$$

$$\Theta,\quad \Upsilon+\Upsilon'(=2p),\quad \Upsilon\Upsilon'(=p^2-q^2\Theta),\quad \sqrt{\Upsilon\Upsilon'}(\Upsilon-\Upsilon')\left(=\frac{2q}{s}\right),$$

$$\sqrt{\Theta}(\Upsilon-\Upsilon')(=2q\Theta),\text{ and }\sqrt{\Theta}\sqrt{\Upsilon\Upsilon'}(=s)$$

are respectively rational functions. This is the *à posteriori* verification, that with the system of equations

$$\alpha=m+\sqrt{\Theta}+\sqrt{p+q\sqrt{\Theta}},\ \&\text{c.},\quad \sqrt{\Theta}\sqrt{p+q\sqrt{\Theta}}\sqrt{p-q\sqrt{\Theta}}=s,$$

any function

$$\phi(\alpha,\ \beta,\ \gamma,\ \delta)+\phi(\beta,\ \gamma,\ \delta,\ \alpha)+\phi(\gamma,\ \delta,\ \alpha,\ \beta)+\phi(\delta,\ \alpha,\ \beta,\ \gamma)$$

is a rational function.

The coefficients of the quintic equation for x must of course be of the form just mentioned; that is, they must be functions of α, β, γ, δ, which remain unaltered by the cyclic substitution $\alpha\beta\gamma\delta$. To form the quintic equation, I write

$$\theta-x=a,$$

$$\mathrm{A}\alpha^{\frac{1}{5}}\beta^{\frac{2}{5}}\gamma^{\frac{4}{5}}\delta^{\frac{3}{5}}=b,\quad \mathrm{D}\delta^{\frac{1}{5}}\alpha^{\frac{2}{5}}\beta^{\frac{4}{5}}\gamma^{\frac{3}{5}}=c,\quad \mathrm{B}\beta^{\frac{1}{5}}\gamma^{\frac{2}{5}}\delta^{\frac{4}{5}}\alpha^{\frac{3}{5}}=d,\quad \mathrm{C}\gamma^{\frac{1}{5}}\delta^{\frac{2}{5}}\alpha^{\frac{4}{5}}\beta^{\frac{3}{5}}=e;$$

then we have

$$0=a+b+c+d+e,$$

and the quintic equation is

$$f1\, f\omega\, f\omega^2\, f\omega^3\, f\omega^4=0,$$

where ω is an imaginary fifth root of unity, and

$$f\omega=a+b\omega+c\omega^2+d\omega^3+e\omega^4.$$

We have

$$f\omega\, f\omega^4=\Sigma a^2+(\omega+\omega^4)\Sigma' ab+(\omega^2+\omega^3)\Sigma' ac,$$

$$f\omega^2 f\omega^3=\Sigma a^2+(\omega^2+\omega^3)\Sigma' ab+(\omega^2+\omega^3)\Sigma' ac,$$

where Σ' is Mr. Harley's cyclical symbol, viz.

$$\Sigma' ab=ab+bc+cd+de+ea\,;$$

and so in other cases, the order of the cycle being always *abcde*. This gives

$$\begin{aligned}f\omega\, f\omega^2 f\omega^3 f\omega^4=\Sigma a^4+\Sigma a^2b^2-\Sigma a^3b+2\Sigma a^2bc-\Sigma abcd\\ -5\Sigma' a^2(be+cd)\,;\end{aligned}$$

and multiplying by $f1$, $=\Sigma a$, and equating to zero, the result is found to be

$$\Sigma a^5-5abcde-5\Sigma' a^3(be+cd)+5\Sigma' a(b^2e^2+c^2d^2)=0.$$

Or arranging in powers of a, this is

$$\left.\begin{array}{ll} a^5 & \\ +a^3\,. & -5(be+cd) \\ +a^2\,. & 5(bc^2+ce^2+ed^2+db^2) \\ +a\ . & \left\{\begin{array}{l}5(b^3c+c^3e+e^3d+d^3b)\\ +5(b^2e^2+c^2d^2-becd)\end{array}\right. \\ + & \left\{\begin{array}{l}b^5+c^5+e^5+d^5\\ -5(b^3de+c^3bd+e^3cb+d^3ec)\\ +5\ bd^2e^2+cb^2d^2+ec^2b^2+de^2c^2\end{array}\right. \end{array}\right\}=0,$$

the several coefficients being, it will be observed, cyclical functions to the cycle b, c, e, d.

Putting for a its value $-(x-\theta)$, and for b, c, d, e their values, the quintic equation in x is

$$\left.\begin{array}{l}
(x-\theta)^5 \\
+(x-\theta)^3.\quad -5(AC+BD)\alpha\beta\gamma\delta \\
+(x-\theta)^2.\quad -5(A^2B\gamma\delta+B^2C\delta\alpha+C^2D\alpha\beta+D^2A\beta\gamma)\alpha\beta\gamma\delta \\
+(x-\theta)\left\{\begin{array}{l} -5(A^3D\beta\gamma^2\delta+B^3A\gamma\delta^2\alpha+C^3B\delta\alpha^2\beta+D^3C\alpha\beta^2\gamma)\alpha\beta\gamma\delta \\ +5(A^2C^2+B^2D^2-ABCD)\alpha^2\beta^2\gamma^2\delta^2 \end{array}\right. \\
+\left\{\begin{array}{l} \quad (A^5\beta\gamma^3\delta^2+B^5\gamma\delta^3\alpha^2+C^5\delta\alpha^3\beta^2+D^5\alpha\beta^3\gamma^2)\alpha\beta\gamma\delta \\ -5(A^3BC\gamma\delta+B^3CD\delta\alpha+C^3DA\alpha\beta+D^3AB\beta\gamma)\alpha^2\beta^2\gamma^2\delta^2 \\ +5(AB^2C^2\alpha\delta+BC^2D^2\beta\alpha+CD^2A^2\gamma\beta+DE^2A^2\delta\alpha)\alpha^2\beta^2\gamma^2\delta^2 \end{array}\right.
\end{array}\right\}=0,$$

where, as before,

$$\begin{aligned}
A&=K+L\alpha+M\gamma+N\alpha\gamma,\\
B&=K+L\beta+M\delta+N\beta\delta,\\
C&=K+L\gamma+M\alpha+N\gamma\alpha,\\
D&=K+L\delta+M\beta+N\delta\beta\,;
\end{aligned}$$

and the coefficients of the quintic equation are, as they should be, cyclical functions to the cycle $\alpha\beta\gamma\delta$.

2 Stone Buildings, W.C.,
February 10, 1861.

XLII. *On the Stability of Satellites in small Orbits, and the Theory of Saturn's Rings.* By DANIEL VAUGHAN*.

THE mysterious revolutions of planets and comets were not rendered intelligible to astronomers until mathematical investigations revealed the peculiar curves which moving bodies must describe when left to the exclusive control of solar gravity. The process of deductive inquiry, which proved so beneficial in this and other departments of celestial mechanics, may be successfully applied to another problem which the results of telescopic observation have forced on the attention of mathematicians. The physical constitution of Saturn's rings, the circumstances on which their stability depends, and the causes which prevent their conversion into satellites, have already been made the subject of many able essays; but, though regarding these productions as valuable contributions to science, I think it advisable to select a

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* Communicated by the Author.

less difficult road to the solution of the curious problem, and to seek a clue to the stability of the annular appendage of Saturn by investigating the form which matter must necessarily assume in very great proximity to a central body.

In my communication published in the Philosophical Magazine for last December (1860), I treated on the equilibrium of satellites revolving extremely near to their primaries; and I endeavoured to give an estimate of the smallest orbits which they could describe in safety. In the cases I considered, the satellite was supposed to have its movements adjusted for keeping the same point of its surface always directed towards the primary, not merely because the hypothesis facilitated the investigation, but because observation lends it every support, and the principles of natural philosophy furnish most cogent reasons for its adoption. In describing a very small orbit without such an adjustment, a satellite must experience, not only excessive tides in its seas, but even incessant commotions in its solid matter; and the destruction of power by friction necessarily involves a continual change in the rotatory movement of the subordinate world, after a manner analogous to that which I described in a paper presented to the British Association for the Advancement of Science in 1857. This must have the ultimate effect of establishing a synchronism of the orbital and diurnal movements, together with a coincidence of the planes in which they are performed; so that the disturbing force may give the secondary planet a permanent elongation, without rendering it a prey to the effects of violent dynamic action. From late researches, however, I am convinced that a want of these peculiar conditions would not seriously affect the fate of a large satellite when brought into dangerous proximity to its primary; and would not change, to any great extent, the magnitude of the orbit in which its dismemberment must be inevitable.

A homogeneous fluid satellite, having its motions adapted for keeping one part of its surface in perpetual conjunction with the primary, must find repose in a form differing little from an ellipsoid. This proposition, which in my last article was assumed as true, may be proved by showing that the relation between the forces exerted on every part of the fluid mass is almost precisely such as is necessary for equilibrium when the figure is an ellipsoid, the dimensions being small compared with the diameter of its orbit. For this purpose put A, B, and C for the major, mean, and minor semiaxes of the ellipsoid, while P, Q, and R express the forces of attraction at their extremities in the absence of all disturbances. Now at any point in the surface, the coordinates of which referred to the centre are represented by a, b, and c, the components of the attraction in the direction of each axis will be expressed by

$$\frac{aP}{A}, \quad \frac{bQ}{B}, \quad \frac{cR}{C}\text{*}. \quad \ldots \ldots \quad (1)$$

If N denote the centrifugal force at the extremities of the major axis, the intensity at the proposed point will be $\frac{N\sqrt{a^2+b^2}}{A}$, and the components in the direction of the three semiaxes will be

$$-\frac{aN}{A}, \quad -\frac{bN}{B}, \quad 0. \quad \ldots \ldots \quad (2)$$

To find the components of the disturbing force of the primary when the major axis ranges with its centre, we may use methods analogous to those pursued in the lunar theory for estimating the amount of solar disturbance. Thus, putting x for the radius of the circular orbit which the satellite describes, and M for the attractive force of the primary at the distance x, the attraction which it exerts on the satellite at the point under consideration will be

$$\frac{Mx^2}{x^2-2ax+a^2+b^2+c^2}. \quad \ldots \ldots \quad (3)$$

This is equivalent to two forces—one acting in the direction of the major axis and expressed by

$$\frac{Mx^3}{(x^2-2ax+a^2+b^2+c^2)^{\frac{3}{2}}}, \quad \ldots \ldots \quad (4)$$

the other directed to the centre of the satellite and expressed by

$$\frac{Mx^2\sqrt{a^2+b^2+c^2}}{(x^2-2ax+a^2+b^2+c^2)^{\frac{3}{2}}}. \quad \ldots \ldots \quad (5)$$

From the first, (4), arises a disturbance operating exclusively in the direction of the major axis, and represented by

$$M-\frac{Mx^3}{(x^2-2ax+a^2+b^2+c^2)^{\frac{3}{2}}}, \text{ or } -3\frac{Ma}{x}, \quad . \quad (6)$$

the squares and higher powers of $\frac{a}{x}$, $\frac{b}{x}$, and $\frac{c}{x}$ being rejected as too small to affect the result to any appreciable degree. Under the same conditions, the radial force (5) resolved with reference to the three axes gives the components

$$\frac{Ma}{x}, \quad \frac{Mb}{x}, \quad \frac{Mc}{x}. \quad \ldots \ldots \quad (7)$$

Accordingly, if X represent the sum of the components acting in

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* A demonstration of this theorem may be found in the article on "Attraction" in the eighth edition of the *Encyclopedia Britannica.*

the direction of A, Y the sum of those in the direction of B, and Z the sum of those in the direction of C, it appears from (1), (2), (6), and (7) that

$$X=\frac{aP}{A}-\frac{aN}{A}-\frac{2aM}{x}, \quad \ldots \ldots \quad (8)$$

$$Y=\frac{bQ}{B}-\frac{bN}{B}+\frac{bM}{x}, \quad \ldots \ldots \quad (9)$$

$$Z=\frac{cR}{C}+\frac{cM}{x}. \quad \ldots \ldots \quad (10)$$

Now it is well known that, to satisfy the conditions of equilibrium, or to make gravity perpendicular to the surface in all parts of the satellite, it is necessary that

$$Xda+Ydb+Zdc=0. \quad \ldots \ldots \quad (11)$$

Substituting their values for X, Y, and Z, there results

$$\left(\frac{P-N}{A}-\frac{2M}{x}\right)ada+\left(\frac{Q-N}{B}+\frac{M}{x}\right)bdb+\left(\frac{R}{C}+\frac{M}{x}\right)cdc=0. \quad (12)$$

But the equation of an ellipsoid is $\frac{a^2}{A^2}+\frac{b^2}{B^2}+\frac{c^2}{C^2}=1$, and its differential, multiplied by the constant quantity S, becomes

$$\frac{Sada}{A^2}+\frac{Sbdb}{B^2}+\frac{Scdc}{C^2}=0. \quad \ldots \quad (13)$$

A comparison of the corresponding terms of equations (12) and (13) will enable us to fix the necessary relations of the constants for satisfying the former. It thus appears that

$$\frac{P-N}{A}-\frac{2M}{x}=\frac{S}{A^2}, \quad \ldots \ldots \quad (14)$$

$$\frac{Q-N}{B}+\frac{M}{x}=\frac{S}{B^2}, \quad \ldots \ldots \quad (15)$$

$$\frac{R}{C}+\frac{M}{x}=\frac{S}{C^2}. \quad \ldots \ldots \quad (16)$$

These relations being independent of the values of the coordinates *a*, *b*, and *c*, they will be the same for every part of the surface of the body; and it follows that an equilibrium established at any one locality must extend to every part of the entire mass. Accordingly the relative magnitudes which the axes A, B, and C must possess, to make gravity perpendicular to the surface at any intermediate point, must give gravity a like vertical direction in all places, and secure the same stability to every portion of the satellite which has an ellipsoidal form. This, however, would not appear to be rigorously correct if, in the expressions for the disturbances by the primary, the squares and higher powers of $\frac{a}{x}$, $\frac{b}{x}$, and $\frac{c}{x}$ were retained; and accordingly the very close approximation to a true ellipsoid can be exhibited only when the size of the satellite is very small compared with that of its orbit. If the disproportion between both were not very great, the form of the satellite would resemble that of an egg slightly flattened by lateral pressure; yet even in such extreme cases the hypothesis in regard to the ellipsoidal form can lead to no material error in estimating the intensity of gravity on its surface, and the dimensions of the smallest orbit in which its parts can be held together by their mutual attraction.

From equations (8) and (14), (9) and (15), and (10) and (16), the following are readily deduced:—

$$X=a\left(\frac{P-N}{A}-\frac{2M}{x}\right), \text{ or } =\frac{aS}{A^2}, \quad . \quad . \quad . \quad (17)$$

$$Y=b\left(\frac{2-N}{B}+\frac{M}{x}\right), \text{ or } =\frac{bS}{B^2}, \quad . \quad . \quad . \quad (18)$$

$$Z=c\left(\frac{R}{C}+\frac{M}{x}\right), \text{ or } =\frac{cS}{C^2}. \quad . \quad . \quad . \quad (19)$$

But calling the force of gravity at the given locality F, it is evident that F is equal to $\sqrt{X^2+Y^2+Z^2}$. On substituting for X, Y, and Z their values given by the last equation, there results

$$F=\sqrt{\frac{a^2S^2}{A^4}+\frac{b^2S^2}{B^4}+\frac{c^2S^2}{C^4}}, \text{ or } =\frac{S}{C^2}\sqrt{\frac{C^4}{A^4}a^2+\frac{C^4}{B^4}b^2+c^2}. \quad (20)$$

The quantity under the radical in the last expression is the value of the normal of the ellipsoid; and hence the force of gravity everywhere on the surface is proportional to the length of the normal corresponding to the locality. At the extremity of each axis this gravitative power, like the normal, is inversely proportional to the lengths of the axes themselves—a result which might be more readily deduced from equations (17), (18), and (19). In the first, for instance, if the point be situated at the end of the major axis, a becomes equal to A and X, which then expresses that the entire gravity at the point is equal to $\frac{S}{A}$; while the two other equations, (18) and (19), treated in a similar manner, would give $\frac{S}{B}$ and $\frac{S}{C}$ for the values of the intensity of gravity at the terminations of the mean and minor axes.

The cases in which equilibrium is impossible will be indicated by the occurrence of imaginary radicals, when we determine the relation between the constant quantities in formulæ (14), (15), and (16); and as this may be found by simple equations for all except the semiaxes A, B, and C, it is to their values alone that we must look for imaginary expressions. The formulæ referred to give

$$\left.\begin{aligned} A &= \frac{x}{4M}(P-N) \pm \frac{1}{4M}\sqrt{x^2(P-N)^2-8SMx}, \\ B &= -\frac{x}{2M}(Q-N) \pm \frac{1}{2M}\sqrt{4MSx+x^2(2-N)^2}, \\ C &= -\frac{Rx}{2M} \pm \frac{1}{2M}\sqrt{4MSx+R^2x^2}. \end{aligned}\right\} \quad . \quad (21)$$

Now it is evident that none of the above radicals can become imaginary except the first; and the stability of the body ceases to be possible when $x^2(P-N)^2-8MSx$, in passing from a positive to a negative value, becomes equal to nothing. In this case

$$A=\frac{x}{4M}(P-N). \quad . \quad . \quad . \quad . \quad . \quad (22)$$

But by comparing the expressions given in my last article for centrifugal force and the disturbance of the primary at the extremity of the major axis of the satellite, it appears that the latter is double the former, or that N equals $\frac{MA}{x}$. We may deduce the same result by considering that the orbital velocity of the satellite's centre is equal to $\sqrt{Mx}$; and from this the rotatory velocity of the extremity of the greater axis is equal to $\frac{A}{x}\sqrt{Mx}$, or $A\sqrt{\frac{M}{x}}$. Calling this v,

$$N=\frac{v^2}{A}, \text{ or } N=\frac{MA}{x}. \quad . \quad . \quad . \quad . \quad . \quad (23)$$

This value being substituted for N in the last equation, gives

$$A=\frac{x}{4M}\left(P-\frac{MA}{x}\right); \text{ whence } P=\frac{5AM}{x}. \quad . \quad . \quad (24)$$

The diminished force of gravity which, at the extremity of A, is represented by $P-N-\frac{2MA}{x}$, thus becomes $\frac{2AM}{x}$; so that if the satellite were a homogeneous fluid, the stability must become impossible when more than three-fifths of the attraction along the major axis is neutralized by centrifugal force and the disturbing influence of the central sphere.

The cause of the unstable equilibrium in such cases will be rendered more intelligible by a further examination of equation (14), which, on multiplying its members by A, becomes

$$P-N-\frac{2MA}{x}=\frac{S}{A}. \quad . \quad . \quad . \quad . \quad . \quad (25)$$

The terms of the first member constitute the expression for the force of gravity at the extremity of A; and the impossible root merely shows that gravity at this point, after having lost over three-fifths of its intensity by the disturbances, cannot amount to $\frac{S}{A}$, and consequently can no longer maintain the inverse ratio to the length of the axis. This peculiar relation between the length of each axis and the gravity at its extremity has already been deduced from formula (20), and is indispensable to the equilibrium of similar columns of fluid extending from these points, either to the centre of the body, or through shells of matter equally dense, and bounded by the surfaces of concentric ellipsoids similar in position and dimensions. This leads to the conclusion maintained on different grounds in my last communication, in which I regarded the rupture of the satellite as inevitable, when an increase of elongation would fail to give any preponderance to the pressure along the greater axis, or when the ellipticity required to be increased to an infinite extent to counteract a very slight augmentation of the disturbing forces. My former estimates, indeed, do not agree very closely with the present investigation in determining the amount of disturbance necessary to bring stability to an end; but in these estimates the eccentricity of the elliptical section containing the mean and minor axes of the satellite was neglected; and from more exact calculations, which are not yet in a condition to be published, it appears that some reduction must be made in the value I first assigned to the smallest orbit in which a homogeneous satellite could be preserved.

To furnish another proof that the central and the superficial conditions of equilibrium necessarily lead to the same results in every respect, let us suppose a portion of the fluid to be enclosed in three tubes; two of which are connected at the centre and extend to the nearest and most distant part of the surface, while the third stretches along the surface to meet their extremities, while it coincides with the plane in which they are situated. Now the force of gravity being $\left(P-N-\frac{2MA}{x}\right)$ at the extremity of the major axis, it must be reduced to $\left(\frac{P-N}{A}-\frac{2M}{x}\right)a$ along this line at a distance from the centre denoted by a, and the element of the pressure in the tube (taking the transverse section and density of the fluid as unity) will be

$$\left(\frac{\mathrm{P}-\mathrm{N}}{\mathrm{A}}-\frac{2\mathrm{M}}{x}\right)ada. \quad \ldots \ldots \quad (26)$$

The integral of this expression, taken within the limits of $a=0$ and $a=\mathrm{A}$, gives for the central pressure of the fluid in the longer tube

$$\frac{\mathrm{A}}{2}\left(\mathrm{P}-\mathrm{N}-\frac{\mathrm{MA}}{x}\right). \quad \ldots \ldots \quad (27)$$

A similar process applied to the fluid in the tube coinciding with the minor axis, will give for the differential of pressure,

$$\left(\frac{\mathrm{R}}{\mathrm{C}}+\frac{\mathrm{M}}{x}\right)cdc; \quad \ldots \ldots \quad (28)$$

and a similar integration will give for its pressure at the centre,

$$\frac{\mathrm{C}}{2}\left(\mathrm{R}+\frac{\mathrm{MC}}{x}\right). \quad \ldots \ldots \quad (29)$$

For stability it is necessary that the contents of both tubes should press to the centre with the same amount of force, or that

$$\frac{\mathrm{A}}{2}\left(\mathrm{P}-\mathrm{N}-\frac{\mathrm{MA}}{x}\right)-\frac{\mathrm{C}}{2}\left(\mathrm{R}+\frac{\mathrm{MC}}{x}\right)=0. \quad . \quad (30)$$

Now from the peculiar position which the third tube is supposed to occupy on the surface, the general equation for the equilibrium of its contents will become $\mathrm{X}da+\mathrm{Z}dc=0$, or by substitution,

$$\left(\frac{\mathrm{P}-\mathrm{N}}{\mathrm{A}}-\frac{2\mathrm{M}}{x}\right)ada+\left(\frac{\mathrm{R}}{\mathrm{C}}+\frac{\mathrm{M}}{x}\right)cdc=0. \quad . \quad (31)$$

Integrating within the limits of $a=\mathrm{A}$, $c=0$, and $a=0$, $c=\mathrm{C}$, this becomes

$$\frac{\mathrm{A}}{2}\left(\mathrm{P}-\mathrm{N}-\frac{2\mathrm{MA}}{x}\right)-\frac{\mathrm{C}}{2}\left(\mathrm{R}+\frac{\mathrm{MC}}{x}\right)=0. \quad . \quad (32)$$

The identity of equations (30) and (32), and the relation between (26), (28), and (31), show that the equilibrium of the internal and external parts of the mass depend on precisely the same conditions, and that the fluid should rush to the most prominent parts of the satellite from the surface, as well as from its internal regions, whenever gravity along the major axis was diminished more than 60 per cent. by the disturbing forces. Brevity compels me to omit the more lengthy investigation which would be required to show that such consequences are not peculiar to special localities, but are the same on all parts of the surface of the body.

These results might lead us to infer that a satellite which had been introduced into the region of instability by the action of a resisting medium, must undergo a sudden and not a gradual dismemberment. Before embracing this opinion, however, a few modifying circumstances should be considered. The change in the figure of the body must increase the time of rotation, while the diminished size of the orbit calls for a shorter period of revolution; and the synchronism of the diurnal and progressive movements will be destroyed. But we may safely assert that the effects of the resisting medium in producing this change are exceedingly small compared with the influence of tidal action in keeping the same side of the satellite always turned to its primary, especially when the distance from the latter became very small. The result in such cases must be a little different for a solid satellite, which accommodates its form to the new conditions of equilibrium by a limited number of paroxysmal changes separated by intervals of many millions of years. On such occurrences, the reduction of the velocity of rotation, together with the tendency of the major axis to range with the primary*, would lead to a series of librations, which, in a dangerous proximity to the latter body, would tend more to promote than to prevent the final dismemberment.

It must be also recollected that our formulæ have been deduced on the supposition that all parts of the satellite are equally dense; and some modifications are therefore required in applying them to the cases likely to occur in the realms of Nature. If the density increased very rapidly from the surface to the centre, gravity might be entirely suppressed at the ends of the greater axis before it became incapable of maintaining the stability of the internal matter; and it would seem that in such a case the satellite might part successively with many layers of the fluid of which it is composed, before the increased disturbance called for a general disunion of the internal mass. If, however, the increasing density towards the centre merely results from the great pressure in these localities, the separation of matter from the surface must weaken the tie which holds the remainder of the satellite together; and the dismembering action, when once begun, will proceed without interruption until a dissolution of the entire mass is completed. But it is in cases where the satellite is solid that the mighty change in its condition assumes the most awful character, as the cohesion of its parts must prevent the gradual loss of matter from its surface, and keep the disturbing forces under restraint until they become capable of

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* There is some inaccuracy in my last article in the incidental statement respecting the intensity of this directive force at different distances.

effecting a simultaneous dilapidation of the entire planetary structure.

I have regarded it as important to trace the precise manner in which these sublime catastrophes must take place; not so much on account of their connexion with the existence of planetary rings, as for the light which they throw on the nature of temporary stars. In an article published in the Supplemental Number of the Philosophical Magazine for December 1858, I maintained that these singular displays of stellar brilliancy were great meteoric displays in the atmospheres, or rather the dormant photospheres, of dark central bodies of space, as they were traversed by the wrecks of dilapidated worlds. The same theory has been set forth in my paper presented to the British Association in 1857; and I have endeavoured in other publications to support it with satisfactory proof. But the most conclusive evidence on which it depends, is to be derived from the instantaneous manner in which the attendant of a dark central body must undergo a total dismemberment, as it explains the sudden manner in which these celestial curiosities are ushered into existence with all the splendour of distant suns. Humboldt, in the third volume of his 'Cosmos,' calls special attention to the fact of the extreme brilliancy of the temporary stars in their incipient stages, regarding it as a remarkable peculiarity, and one well deserving of consideration.

Without adducing any further evidence on this subject, I shall now proceed to trace the condition which matter must assume in the region where such disturbing forces render it incapable of forming a single mass, held together by the power of gravity. On the dismemberment of a satellite on this dangerous ground, the resulting host of fragments would scatter into numberless orbits; and the wide range over which they must extend may be estimated from the greatest and least size of the elliptical paths which their velocities and positions should assign to them. For these, however, we can only give at present approximate values, taking no cognizance of the mutual disturbances of the fragmentary host. And in this case the matter from the most distant part of the satellite would describe an ellipse, the diameter of which is equal to

$$\frac{2x^3(x+\mathrm{A})}{2x^3-(x+\mathrm{A})^3}.$$

The fragments from the nearest point of the dismembering mass would describe an elliptical orbit the diameter of which is

$$\frac{2x^3(x-\mathrm{A})}{2x^3-(x-\mathrm{A})^3}.$$

But the size of the smallest orbits might fall considerably below this limit, in consequence of the rupture of many of the fragments at their least distances from the primary, either by the attraction of that body, or by the heat evolved when they are transformed into blazing meteoric masses.

The condition which matter must ultimately assume in the central zone, where it can no longer exist as one great satellite or in a limited number of smaller ones, must depend in some degree on the form of the primary planet. If this body be an oblate spheroid, considerably flattened by rapid rotation, as is the case with Saturn, the orbits of the several fragments must be subject to apsidal motion, to an extent depending on their transverse axes and excentricities. Accordingly those fragments describing the same track will be equally affected by it, and will form a line which remains unbroken during many revolutions. As one ring of fragments is thus made to roll within another, it is evident that both must ultimately become circular; and the fragmentary host will at length exhibit the nearest approximation to a state of repose, by moving in exact circles around the central planet.

There are even more cogent provisions for equalizing the distribution of the great ocean of disconnected matter over the vast zone in which it circulates. The attraction of the central body which led to the great dismemberment, must be adequate not only to forbid the reconstruction of a satellite, but even to prevent the parts of the mighty wreck from congregating to any point in an undue proportion. Whenever a preponderance of matter occurred at any locality, the impediments of friction would tend to equalize the angular velocity of the nearest and most distant fragments in the group; and the new relations between gravity and centrifugal force would immediately lead to their dispersion by the disturbing action of the primary. If the latter body were a very flattened spheroid, it would serve to confine the great annular ocean of fragments to the same plane, in opposition to small effects arising from the disturbances of distant spheres; and Laplace has shown that, supposing Saturn's ring to consist of numerous independent satellites, they will be prevented from departing from a common plane, in consequence of the action of his equatorial matter.

In addition to the foregoing agencies for securing the peculiar characters of the annular appendage, I must notice another which is inseparable from the movements of such collections of fluid or solid matter circulating in independent orbits. A vast amount of heat must be developed by their friction and their mutual collisions; while the calorific influence of such a mechanical action will be augmented by the slight excentricity impressed on their orbits by the disturbances of the external satellites. The increased temperature originating from this cause, must not only permit the existence of fluids in the extensive fields of floating matter, but also maintain an atmospheric covering of vapour, to give more continuity and symmetry to the annular appendage. If the physical characters of Saturn's rings be such as matter, not having an improbably great density, must necessarily assume in the region which they occupy, the independent movements of its parts may be regarded as a continual source of heat, which may perhaps in some degree mitigate the sway of intolerable cold in the frigid zone of the solar system.

Cincinnati, Feb. 19, 1861.

XLIII. *On the Principles of Energetics.*—Part I. *Ordinary Mechanics. By* J. S. Stuart Glennie, *M.A., F.R.A.S.*[*]

1. IN the introductory paper, "On the Principles of the Science of Motion," I suggested that this name might be given to a new general science, ranking with the similar science of Growth, and not be used merely as a general name for the sciences of Ordinary[†] Mechanics (Stereatics and Hydratics) and Molecular Mechanics (Physics and Chemics[‡]). The science of Motion would, as a distinct science, or as a philosophy of the Mechanical Sciences, consider, first, the relations of motions, as motion, and without reference to their originating or determining causes or forces; and secondly, the conditions, and correlations of the conditions, of Pressure, bodily, or molecular, to which modern experiment and analysis give us the hope of being able to refer all the forces of motion. To the former section of General Mechanics I would give the name, coined or made current by Ampère, Kinematics[§]; for the latter section I would adopt the term Energetics[||], already introduced by Rankine with a similar meaning to that given by the above statement of its object.

2. It is proposed in the following papers partially to develope the conceptions of the introductory paper, by stating the fundamental principles of the proposed Science of Energetics, with their applications to the mechanical interpretation of phenomena. The justification and development of these principles is the work of the sciences of Mechanics, Physics, and Chemics respectively.

3. The science of Energetics may be defined as the theory of

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[*] Communicated by the Author.

[†] Would there not be a more definite distinction between the two branches of Mechanics by means of the adjectives Corporal and Molecular?

[‡] Phil. Mag. January 1861, p. 54.

[§] *Essai sur la Philosophie des Sciences.*

[||] "A science whose subjects are material bodies, and physical phenomena in general," Edinb. Phil. Journ. N. S. ii. 1855, p. 125.

Mechanical, as distinguished from Biological Forces. And without such a theory it is evident that no general and concurrent laws or relations can be established between phenomena of Motion, as distinguished from phenomena of Growth. Attractions (gravic, electric, and magnetic) and Waves (acoustic, optic, and thermotic) are the motions offered by Physics for explanation by mechanical forces, or conditions of pressure. The constitution and combination of bodies are the phenomena of which Chemics require a similar mechanical interpretation. These sciences may be distinguished from Mechanics, with its ordinary limitation of meaning, as forming together the science of Molecular Mechanics. But in Ordinary Mechanics also there are phenomena, the causal relations of which have been hitherto as little established as those of the phenomena of Attraction or of Affinity. Such unexplained mechanical phenomena are the uniform motion of the planets, and their velocities of rotation, as yet unconnected even by an empirical law.

The principles of Energetics more particularly belonging to Ordinary Mechanics will therefore be in this paper applied to the explanation of these mechanical facts.

4. (I.) A Force is the condition of difference between two pressures in relation to a third.

5. To establish the principles of Energetics applicable to the first part of Mechanics, it seems unnecessary to use the term pressure with other than its usual limited meaning as Statical pressure. Such a meaning would at least be wide enough for this first principle. For it might be otherwise expressed :—the general cause of the movement of a body is a difference between two (previously) equilibrating pressures upon it. But in order that it may be seen, at least generally, how I propose to bring the idea of Pressure into Physics and Chemics, and hence to make this principle the foundation of Molecular as well as of Ordinary Mechanics, it may be well to state at once that "under the term Pressure I shall include every kind of force which acts between elastic bodies, or the parts of an elastic body, as the cause (condition) or effect of a state of strain, whether that force is tensile, compressive, or distorting*;" and that I consider elasticity to be "une des propriétés générales de la matière. Elle est en effet l'origine réelle, ou l'intermédiaire indispensable des phénomènes physiques les plus importans de l'univers. . . . . La gravitation et l'elasticité doivent être considérées comme les effets d'une même cause qui rend dépendantes ou solidaires toutes les parties matérielles de l'univers†."

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* Rankine, Camb. and Dub. Math. Journ. 1851, vol. ii. p. 49.

† Lamé, *Théorie Mathématique de l'Elasticité des Corps Solides*, pp. 1 and 2.

6. The special application of this principle is less to phenomena than to physical hypotheses. For as Force is thus conceived, not as an absolute entity acting upon matter, but as a condition of the parts of matter itself, and as a condition determined by the relative masses and distances of these parts, any valid hypothesis of a force or of a motion to account for any set of phenomena is thus seen to imply an assertion as to relative masses and distances which can be more or less readily submitted to experiment or observation and analysis. And hypotheses of Forces which, like electric and magnetic fluids, or "uniform elastic ethers, the sole source of physical power*," exist absolutely, and are not merely expressions of facts of mass- and distance-difference, are by this principle rejected as unscientific. "Lorsq'une branche de la Physique mathématique est ainsi parvenue à écarter tout principe douteux, toute hypothèse restrictive, elle entre réellement dans une phase nouvelle. Et cette phase paraît définitive, car la série historique, et en même temps rationnelle, des progrès accomplis, signale une tendance constante vers l'indépendance de toute loi préconçue†."

How the mutually pressing or repelling parts of matter are to be conceived in order that from facts of difference in relative masses and distances alone, the forces of Molecular may be referred to similar conditions with those of Ordinary Mechanics, has been in the introductory paper indicated, and will in the second part of this paper be more fully developed.

7. (II.) Motion, the effect of Force, whether mechanical, physical, or chemical, may be distinguished as beginning or continuous; and continuous motion as uniform or accelerated. The condition of the beginning of motion is a difference of pressure on the body that begins to move; the condition of a uniform continuous motion is a neutralization of the resisting pressure; the condition of an accelerated continuous motion is a uniform or varying resisting pressure.

8. This principle evidently embodies those of the Inertia of Matter, of the Composition of Motions, and of Accelerating Force.

The principle of Inertia is the fundamental scientific principle of Non-spontaneity, or the impossibility of a motion undetermined by a change in the previously existing relations of the body. The inertia of a body or molecule is simply the relation between its pressure and that of the bodies acting upon it. All the meaning of this principle is in the relativity of the conception it gives of the phenomena of matter; it appears, therefore, to betray

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* Challis, "On a Theory of Magnetic Force," Phil. Mag. February 1861, p. 107.

† Lamé, *Théorie Analytique de la Chaleur, Discours préliminaire,* p. vi.

some obscurity of thought to speak of "intrinsic or absolute inertia."

The law of the Composition of Motions is but an extension of that of Inertia*. For the compounding of a motion is but the beginning of another motion; and the change in velocity and line of motion of the particle due to each force (difference of pressure) is the same as if the others did not act.

There seems to be a clearer conception afforded of uniform and accelerated motion by referring these phenomena, as by this principle, to their actual physical conditions.

9. The application of this principle leads to the following theorem suggestive of an explanation of the apparent effect and non-effect of the resisting medium on the comets and planets respectively.

According as the resultant of a resisting medium passes or not through the centre of gravity of a revolving body is it an accelerating force of revolution, or a partially neutralized accelerating force of rotation.

If the medium is uniform, or if—though it varies in density, according to some such law as that with so great probability assumed for the solar medium, viz. inversely as the square of the distance from the central body†—the face of the revolving body is so small that the resultant of the resisting pressures thereon passes infinitesimally near the centre of gravity of the whole body, it may be easily proved that such a resultant of resistance will act as an accelerating force, which, did the body move on a solid surface, would retard its revolution, but which, as it moves through a fluid medium, will, by the progressive decrease of its major axis and excentricity, cause its orbit to approach more and more to the circular form; and there will hence result, as in the case of Encke's comet, a secular inequality in the expression of the mean longitude, and consequently in the period.

But if, with the above law of decreasing density, the resultant of resistance to revolution falls at a finite distance below the centre of gravity of the body, it is clear that an unbalanced pressure thus applied will affect, not the revolution, but the rotation of the body; and that the tendency either to cause or accelerate rotation will be partly at least neutralized by the resistance of the medium to this new motion.

For let α be the direction of revolution, α' the resultant of the resistance thereto of a medium varying in density. It is evi-

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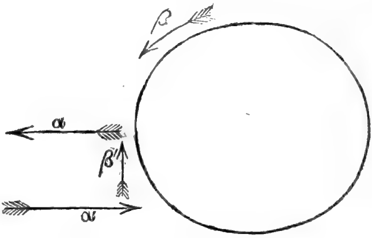
* Price, 'Treatise on Infinitesimal Calculus,' vol. iii. p. 370.

† See Encke, *Ueber die Existenz eines widerstehenden Mittels im Weltraume*; and Pontécoulant, *Théorie Analytique du Système du Monde*, vol. iii. book 4, chap. 5.

dent that α' will act as an accelerating force of rotation in the direction β; and that this rotation will be retarded, and α' partly neutralized by the resistance in the direction β'.



10. In the actual case of the planets, their masses and velocities of rotation are such that the solar medium can be of course conceived, not as causing, but only as tending slightly to accelerate their rotations. And a problem is by this theorem suggested of extreme interest, but also, in the present state of hydrodynamics, of extreme difficulty, as to how far this accelerating force of rotation is neutralized. The earth's rotation has been hitherto considered and proved to be invariable, only in respect of the action of the sun and moon; and it is to be remembered that doubts have been thrown on its actual invariability even during the short period of 4000 years; that one-tenth of a second in 10,000 years would be a large astronomical quantity; and that their *actual* times are all that, at best, we know of the rotations of the other planets. I shall not at present offer any further remarks on this problem, considered either as a purely hydrodynamical one, or with the data afforded by the planetary system, except to note that nothing seems as yet to have been done towards determining the relative effect of a resisting medium on (what may at any moment be called) *the back* of a revolving and rotating body. And it should seem that little further* can be done towards the solution of this problem without experimental data on this point especially. The determination of the secular inequality, the result of the variously directed and most improbably equilibrating forces of the medium, becomes still more complicated when such a triple motion as that of a satellite is considered.

Such, then, is the theorem I would venture to offer as, if not giving as yet the demonstrable explanation of the effect of the resisting medium on the bodies of the solar system†, at least suggesting new and very interesting experimental and analytical problems in hydrodynamics.

11. (III.) The condition of Translation is a difference of polar pressures on a point; the condition of Rotation is equal

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* Stokes, 'On Fluid Friction.'

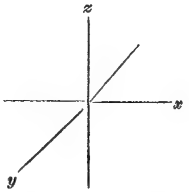
† I may refer to, though I cannot here discuss, the remarks on this subject of Sir John Herschel, 'Outlines of Astronomy,' 5th edit. p. 389 note; and of Prof. Challis in his paper "On the Resistance of the Luminiferous Medium," Phil. Mag. May 1859.

and opposite differences of polar pressures on two rigidly connected points; the condition of compound Translation and Rotation is unequal and opposite differences of polar pressures on two rigidly connected points; and the relation between the former and the latter depends on the distance between the centre of gravity of the body and the point of application of the resultant of such unequal opposite forces.

12. In explanation of this principle, it will be sufficient to remark that it is merely an expression of the ideas of a single force at the centre of gravity, a couple, and a single force not at the centre of gravity, in terms of the above general physical conception of a Force.

13. The latter paragraph suggests the following general problem:—Given a force which, acting instantaneously at the centre of gravity of a body of a given mass in a vacuum, gives it a certain velocity: what are the different relations between the velocity of revolution and that of rotation when the same body is struck at certain different distances from the centre of gravity, and on any axis, by the same force?

The interest of this abstract problem is in the generality of its application to bodies which, while translated along x, rotate about y from a resultant of unequal pressures and resistances, having its point of application in or parallel to z (a wheel); and to bodies which, while translated along x, rotate about z from "a primitive impulse" applied at some point in y (a planet). For "le double mouvement de translation et de rotation des planètes, qui paraît au premier abord si compliqué, a pu résulter d'une seule impulsion primitive qui ne passait pas par leur centre de gravité*." From what previously existing conditions such a primitive impulse originated, whether from the rotation of a genetic ring, as in the Nebular Theory of Laplace, or otherwise, we have not here to inquire. But towards a mechanical explanation of the planetary elements, or a hydrodynamical theory of the formation of the solar system, the experimental† and analytical investigation of the above general problem seems to open the way.



14. In reference to the application of general or particular solutions of such a problem to the planets, it may be remarked that, as we are not of course here given the primitive impulse,

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[*] Pontécoulant, *Théorie Analytique du Système du Monde,* vol. i. p. 144.

[†] Extend Plücker's experiments, for instance. See Taylor's 'Scientific Memoirs,' vol. iv. p. 16, and vol. v. pp. 584 and 621.

the first step towards a rational is the discovery of an empirical law of the rotations, in which such an element as the inclination of the rotation axis to the plane of revolution (easily calculable except for the two innermost and two outermost planets) would evidently be involved.

Such a law seems pointed to by the regularity of the decrease of the rotations, when the angular, instead of the linear velocities or times are considered. The respective angular velocities of rotation of the inner family are ·29811, ·26902, ·26181, and ·25879; and of Jupiter and Saturn, ·63313 and ·59907 respectively.

15. The attempts I have made to discover the law of the planetary rotations have had as yet no complete result*. But the following incidental observation with regard to the angular velocities of revolution and the distances may perhaps be worth noting towards such a theory of the formation of the system as above alluded to.

By Kepler's third law,

$$P=cD^{\frac{3}{2}}; \text{ whence } \frac{V}{D} \text{ or } \omega=c'\frac{1}{D^{\frac{3}{2}}}.$$

But under this law there might, in comparing successive velocities and distances, be found relations of inequality *ad infinitum*. The actual relations may, however, be thus expressed:—The angular velocities of revolution and the distances are in inverse geometrical progressions with inverse differences, except the innermost planet of each family.

To say that the distances are in geometrical progression, each nearer planet being half the distance of the next more remote†, or that the angular velocities of revolution are in geometrical progression, each nearer planet revolving with twice the velocity of the next more remote, would be very far from accurate; but it seems interesting to observe, as by this law, that when the distance of a planet is *more* than twice that of the next inner, its angular velocity of rotation is *less* than half that of the next inner, and *vice versâ*. And that the only exceptions to this rule should be the innermost planet of each family, viz. Mercury and Jupiter, appears significant.

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* The results of an approximative formula were given in a paper "On a general Law of Rotation applied to the Planets," read by me at the Oxford Meeting of the British Association, June 1860.

† See Humboldt's remarks on the Law of Bode, or rather of Titius. Cosmos, vol. iii. pp. 319, 320.

Mercury and Venus.

$$D=36{,}298051=\tfrac{1}{2}\times 68{,}631843+1{,}982129,$$

$$\frac{V}{D}=0{\cdot}0029760=2\times 0{\cdot}0011651+0{\cdot}0006468.$$

Venus and Earth.

$$D=68{,}631843\;=\tfrac{1}{2}\times 94{,}885000\;+21{,}189343,$$

$$\frac{V}{D}=0{\cdot}00116510=2\times 0{\cdot}00071676-0{\cdot}00026842.$$

Earth and Mars.

$$D=94{,}885000\;=\tfrac{1}{2}\times 144{,}575333+22{,}587334,$$

$$\frac{V}{D}=0{\cdot}00071676=2\times 0{\cdot}00038108-0{\cdot}00004540.$$

Jupiter and Saturn.

$$D=493{,}654546\;=\tfrac{1}{2}\times 905{,}087708\;+41{,}110692,$$

$$\frac{V}{D}=0{\cdot}000060411=2\times 0{\cdot}000024332+0{\cdot}0000117470.$$

Saturn and Uranus.

$$D=905{,}087708\;=\tfrac{1}{2}\times 1820{,}020075\;-4{,}922329,$$

$$\frac{V}{D}=0{\cdot}000024332=2\times 0{\cdot}0000085313+0{\cdot}0000072598.$$

Uranus and Neptune.

$$D=1820{,}020075\;=\tfrac{1}{2}\times 2849{,}991384\;+395{,}024383,$$

$$\frac{V}{D}=0{\cdot}0000085313=2\times 0{\cdot}0000043542-0{\cdot}0000001871\,*.$$

6 Stone Buildings, Lincoln's Inn,
March 1861.

XLIV. *On Physical Lines of Force.* *By* J. C. Maxwell, *Professor of Natural Philosophy in King's College, London*[†].

[With a Plate.]

Part II.—*The Theory of Molecular Vortices applied to Electric Currents.*

We have already shown that all the forces acting between magnets, substances capable of magnetic induction, and electric currents, may be mechanically accounted for on the sup-

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[*] This fifteenth paragraph may be taken as an abstract of my paper "On the Revolutional Velocities and Distances of the Planets," read before the Royal Astronomical Society, Jan. 11, 1861.

[†] Communicated by the Author.

position that the surrounding medium is put into such a state that at every point the pressures are different in different directions, the direction of least pressure being that of the observed lines of force, and the difference of greatest and least pressures being proportional to the square of the intensity of the force at that point.

Such a state of stress, if assumed to exist in the medium, and to be arranged according to the known laws regulating lines of force, will act upon the magnets, currents, &c. in the field with precisely the same resultant forces as those calculated on the ordinary hypothesis of direct action at a distance. This is true independently of any particular theory as to the *cause* of this state of stress, or the mode in which it can be sustained in the medium. We have therefore a satisfactory answer to the question, "Is there any mechanical hypothesis as to the condition of the medium indicated by lines of force, by which the observed resultant forces may be accounted for?" The answer is, the lines of force indicate the direction of *minimum pressure* at every point of the medium.

The second question must be, "What is the mechanical cause of this difference of pressure in different directions?" We have supposed, in the first part of this paper, that this difference of pressures is caused by molecular vortices, having their axes parallel to the lines of force.

We also assumed, perfectly arbitrarily, that the direction of these vortices is such that, on looking along a line of force from south to north, we should see the vortices revolving in the direction of the hands of a watch.

We found that the velocity of the circumference of each vortex must be proportional to the intensity of the magnetic force, and that the density of the substance of the vortex must be proportional to the capacity of the medium for magnetic induction.

We have as yet given no answers to the questions, "How are these vortices set in rotation?" and "Why are they arranged according to the known laws of lines of force about magnets and currents?" These questions are certainly of a higher order of difficulty than either of the former; and I wish to separate the suggestions I may offer by way of provisional answer to them, from the mechanical deductions which resolved the first question, and the hypothesis of vortices which gave a probable answer to the second.

We have, in fact, now come to inquire into the physical connexion of these vortices with electric currents, while we are still in doubt as to the nature of electricity, whether it is one substance, two substances, or not a substance at all, or in what way it differs from matter, and how it is connected with it.

We know that the lines of force are affected by electric currents, and we know the distribution of those lines about a current; so that from the force we can determine the amount of the current. Assuming that our explanation of the lines of force by molecular vortices is correct, why does a particular distribution of vortices indicate an electric current? A satisfactory answer to this question would lead us a long way towards that of a very important one, "What is an electric current?"

I have found great difficulty in conceiving of the existence of vortices in a medium, side by side, revolving in the same direction about parallel axes. The contiguous portions of consecutive vortices must be moving in opposite directions; and it is difficult to understand how the motion of one part of the medium can coexist with, and even produce, an opposite motion of a part in contact with it.

The only conception which has at all aided me in conceiving of this kind of motion is that of the vortices being separated by a layer of particles, revolving each on its own axis in the opposite direction to that of the vortices, so that the contiguous surfaces of the particles and of the vortices have the same motion.

In mechanism, when two wheels are intended to revolve in the same direction, a wheel is placed between them so as to be in gear with both, and this wheel is called an "idle wheel." The hypothesis about the vortices which I have to suggest is that a layer of particles, acting as idle wheels, is interposed between each vortex and the next, so that each vortex has a tendency to make the neighbouring vortices revolve in the same direction with itself.

In mechanism, the idle wheel is generally made to rotate about a *fixed* axle; but in epicyclic trains and other contrivances, as, for instance, in Siemens's governor for steam-engines*, we find idle wheels whose centres are capable of motion. In all these cases the motion of the centre is the half sum of the motions of the circumferences of the wheels between which it is placed. Let us examine the relations which must subsist between the motions of our vortices and those of the layer of particles interposed as idle wheels between them.

Prop. IV.—To determine the motion of a layer of particles separating two vortices.

Let the circumferential velocity of a vortex, multiplied by the three direction-cosines of its axis respectively, be α, β, γ, as in Prop. II. Let l, m, n be the direction-cosines of the normal to any part of the surface of this vortex, the outside of the surface being regarded positive. Then the components of the velocity of the particles of the vortex at this part of its surface will be

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* See Goodeve's 'Elements of Mechanism,' p. 118.

$$n\beta - m\gamma \quad \text{parallel to } x,$$
$$l\gamma - n\alpha \quad \text{parallel to } y,$$
$$m\alpha - l\beta \quad \text{parallel to } z.$$

If this portion of the surface be in contact with another vortex whose velocities are α', β', γ', then a layer of very small particles placed between them will have a velocity which will be the mean of the superficial velocities of the vortices which they separate, so that if u is the velocity of the particles in the direction of x,

$$u = \tfrac{1}{2}m(\gamma' - \gamma) - \tfrac{1}{2}n(\beta' - \beta), \quad . \quad . \quad . \quad . \quad . \quad (27)$$

since the normal to the second vortex is in the opposite direction to that of the first.

Prop. V.—To determine the whole amount of particles transferred across unit of area in the direction of x in unit of time.

Let x_1, y_1, z_1 be the coordinates of the centre of the first vortex, x_2, y_2, z_2 those of the second, and so on. Let V_1, V_2, &c. be the volumes of the first, second, &c. vortices, and $\overline{V}$ the sum of their volumes. Let dS be an element of the surface separating the first and second vortices, and x, y, z its coordinates. Let ρ be the quantity of particles on every unit of surface. Then if p be the whole quantity of particles transferred across unit of area in unit of time in the direction of x, the whole momentum parallel to x of the particles within the space whose volume is $\overline{V}$ will be $\overline{V}p$, and we shall have

$$\overline{V}p = \Sigma u\rho dS, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

the summation being extended to every surface separating any two vortices within the volume $\overline{V}$.

Let us consider the surface separating the first and second vortices. Let an element of this surface be dS, and let its direction-cosines be l_1, m_1, n_1 with respect to the first vortex, and l_2, m_2, n_2 with respect to the second; then we know that

$$l_1 + l_2 = 0, \quad m_1 + m_2 = 0, \quad n_1 + n_2 = 0. \quad . \quad . \quad . \quad (29)$$

The values of α, β, γ vary with the position of the centre of the vortex; so that we may write

$$\alpha_2 = \alpha_1 + \frac{d\alpha}{dx}(x_2 - x_1) + \frac{d\alpha}{dy}(y_2 - y_1) + \frac{d\alpha}{dz}(z_2 - z_1), \quad . \quad (30)$$

with similar equations for β and γ.

The value of u may be written:—

$$
\begin{aligned}
u=&\frac{1}{2}\frac{d\gamma}{dx}\Big(m_1(x-x_1)+m_2(x-x_2)\Big)\\
&+\frac{1}{2}\frac{d\gamma}{dy}\Big(m_1(y-y_1)+m_2(y-y_2)\Big)+\frac{1}{2}\frac{d\gamma}{dz}\Big(m_1(z-z_1)+m_2(z-z_2)\Big)\\
&-\frac{1}{2}\frac{d\beta}{dx}\Big(n_1(x-x_1)+n_2(x-x_2)\Big)-\frac{1}{2}\frac{d\beta}{dy}\Big(n_1(y-y_1)+n_2(y-y_2)\Big)\\
&-\frac{1}{2}\frac{d\beta}{dz}\Big(n_1(z-z_1)+n_1(z-z_2)\Big). \qquad (31)
\end{aligned}
$$

In effecting the summation of $\Sigma u\rho dS$, we must remember that round any closed surface $\Sigma l dS$ and all similar terms vanish; also that terms of the form $\Sigma l y dS$, where l and y are measured in different directions, also vanish; but that terms of the form $\Sigma l x dS$, where l and x refer to the same axis of coordinates, do not vanish, but are equal to the volume enclosed by the surface. The result is

$$\overline{V}p=\frac{1}{2}\rho\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right)(V_1+V_2+\&c.); \qquad (32)$$

or dividing by $\overline{V}=V_1+V_2+\&c.$,

$$p=\frac{1}{2}\rho\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right). \qquad (33)$$

If we make

$$\rho=\frac{1}{2\pi}, \qquad (34)$$

then equation (33) will be identical with the first of equations (9), which give the relation between the quantity of an electric current and the intensity of the lines of force surrounding it.

It appears therefore that, according to our hypothesis, an electric current is represented by the transference of the moveable particles interposed between the neighbouring vortices. We may conceive that these particles are very small compared with the size of a vortex, and that the mass of all the particles together is inappreciable compared with that of the vortices, and that a great many vortices, with their surrounding particles, are contained in a single complete molecule of the medium. The particles must be conceived to roll without sliding between the vortices which they separate, and not to touch each other, so that, as long as they remain within the same complete molecule, there is no loss of energy by resistance. When, however, there is a general transference of particles in one direction, they must pass from one molecule to another, and in doing so, may experience resistance, so as to waste electrical energy and generate heat.

Now let us suppose the vortices arranged in a medium in any arbitrary manner. The quantities $\frac{d\gamma}{dy}-\frac{d\beta}{dz}$, &c. will then in general have values, so that there will at first be electrical currents in the medium. These will be opposed by the electrical resistance of the medium; so that, unless they are kept up by a continuous supply of force, they will quickly disappear, and we shall then have $\frac{d\gamma}{dy}-\frac{d\beta}{dz}=0$, &c.; that is, $\alpha dx+\beta dy+\gamma dz$ will be a complete differential (see equations (15) and (16)); so that our hypothesis accounts for the distribution of the lines of force.

In Plate V. fig. 1, let the vertical circle E E represent an electric current flowing from copper C to zinc Z through the conductor E E′, as shown by the arrows.

Let the horizontal circle M M′ represent a line of magnetic force embracing the electric circuit, the north and south directions being indicated by the lines S N and N S.

Let the vertical circles V and V′ represent the molecular vortices of which the line of magnetic force is the axis. V revolves as the hands of a watch, and V′ the opposite way.

It will appear from this diagram, that if V and V′ were contiguous vortices, particles placed between them would move downwards; and that if the particles were forced downwards by any cause, they would make the vortices revolve as in the figure. We have thus obtained a point of view from which we may regard the relation of an electric current to its lines of force as analogous to the relation of a toothed wheel or rack to wheels which it drives.

In the first part of the paper we investigated the relations of the statical forces of the system. We have now considered the connexion of the motions of the parts considered as a system of mechanism. It remains that we should investigate the dynamics of the system, and determine the forces necessary to produce given changes in the motions of the different parts.

Prop. VI.—To determine the actual energy of a portion of a medium due to the motion of the vortices within it.

Let α, β, γ be the components of the circumferential velocity, as in Prop. II., then the actual energy of the vortices in unit of volume will be proportional to the density and to the square of the velocity. As we do not know the distribution of density and velocity in each vortex, we cannot determine the numerical value of the energy directly; but since μ also bears a constant though unknown ratio to the mean density, let us assume that the energy in unit of volume is

$$E=C\mu(\alpha^2+\beta^2+\gamma^2),$$

where C is a constant to be determined.

Let us take the case in which

$$\alpha=\frac{d\phi}{dx},\quad \beta=\frac{d\phi}{dy},\quad \gamma=\frac{d\phi}{dz}. \quad . \quad . \quad . \quad (35)$$

Let

$$\phi=\phi_1+\phi_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

and let

$$\frac{\mu}{4\pi}\left(\frac{d^2\phi_1}{dx^2}+\frac{d^2\phi_1}{dy^2}+\frac{d^2\phi_2}{dz^2}\right)=m_1 \text{ and } \frac{\mu}{4\pi}\left(\frac{d^2\phi_2}{dx^2}+\frac{d^2\phi_2}{dy^2}+\frac{d^2\phi_2}{dz^2}\right)=m_2; \quad (37)$$

then ϕ_1 is the potential at any point due to the magnetic system m_1, and ϕ_2 that due to the distribution of magnetism represented by m_2. The actual energy of all the vortices is

$$E=\Sigma C\mu(\alpha^2+\beta^2+\gamma^2)dV, \quad . \quad . \quad . \quad . \quad (38)$$

the integration being performed over all space.

This may be shown by integration by parts (see Green's 'Essay on Electricity,' p. 10) to be equal to

$$E=-4\pi C\Sigma(\phi_1m_1+\phi_2m_2+\phi_1m_2+\phi_2m_1)dV. \quad . \quad (39)$$

Or since it has been proved (Green's 'Essay,' p. 10) that

$$\Sigma\phi_1m_2dV=\Sigma\phi_2m_1dV,$$

$$E=-4\pi C(\phi_1m_1+\phi_2m_2+2\phi_1m_2)dV. \quad . \quad . \quad . \quad (40)$$

Now let the magnetic system m_1 remain at rest, and let m_2 be moved parallel to itself in the direction of x through a space δx; then, since ϕ_1 depends on m_1 only, it will remain as before, so that $\phi_1 m_1$ will be constant; and since ϕ_2 depends on m_2 only, the distribution of ϕ_2 about m_2 will remain the same, so that $\phi_2 m_2$ will be the same as before the change. The only part of E that will be altered is that depending on $2\phi_1m_2$, because ϕ_1 becomes $\phi_1+\frac{d\phi_1}{dx}\delta x$ on account of the displacement. The variation of actual energy due to the displacement is therefore

$$\delta E=-4\pi C\Sigma\left(2\frac{d\phi_1}{dx}m_2\right)dV\,\delta x. \quad . \quad . \quad (41)$$

But by equation (12), the work done by the mechanical forces on m_2 during the motion is

$$\delta W=\Sigma\left(\frac{d\phi_1}{dx}m_2dV\right)\delta x; \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

and since our hypothesis is a purely mechanical one, we must have by the conservation of force,

$$\delta E+\delta W=0; \quad \ldots \ldots \ldots \quad (43)$$

that is, the loss of energy of the vortices must be made up by work done in moving magnets, so that

$$-4\pi C\Sigma\left(2\frac{d\phi_1}{dx}m_2 dV\right)\delta x+\Sigma\left(\frac{d\phi_1}{dx}m_2 dV\right)\delta x=0,$$

or

$$C=\frac{1}{8\pi}; \quad \ldots \ldots \ldots \quad (44)$$

so that the energy of the vortices in unit of volume is

$$\frac{1}{8\pi}\mu(\alpha^2+\beta^2+\gamma^2); \quad \ldots \ldots \ldots \quad (45)$$

and that of a vortex whose volume is V is

$$\frac{1}{8\pi}\mu(\alpha^2+\beta^2+\gamma^2)V. \quad \ldots \ldots \ldots \quad (46)$$

In order to produce or destroy this energy, work must be expended on, or received from, the vortex, either by the tangential action of the layer of particles in contact with it, or by change of form in the vortex. We shall first investigate the tangential action between the vortices and the layer of particles in contact with them.

Prop. VII.—To find the energy spent upon a vortex in unit of time by the layer of particles which surrounds it.

Let P, Q, R be the forces acting on unity of the particles in the three coordinate directions, these quantities being functions of x, y, and z. Since each particle touches two vortices at the extremities of a diameter, the reaction of the particle on the vortices will be equally divided, and will be

$$-\frac{1}{2}P, \quad -\frac{1}{2}Q, \quad -\frac{1}{2}R$$

on each vortex for unity of the particles; but since the superficial density of the particles is $\frac{1}{2\pi}$ (see equation (34)), the forces on unit of surface of a vortex will be

$$-\frac{1}{4\pi}P, \quad -\frac{1}{4\pi}Q, \quad -\frac{1}{4\pi}R.$$

Now let dS be an element of the surface of a vortex. Let the direction-cosines of the normal be l, m, n. Let the coordinates of the element be x, y, z. Let the component velocities of the surface be u, v, w. Then the work expended on that element of surface will be

$$\frac{dE}{dt}=-\frac{1}{4\pi}(Pu+Qv+Rw)dS. \quad . \quad . \quad . \quad . \quad (47)$$

Let us begin with the first term, $Pu\,dS$. P may be written

$$P_0+\frac{dP}{dx}x+\frac{dP}{dy}y+\frac{dP}{dz}z, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

and

$$u=n\beta-m\gamma.$$

Remembering that the surface of the vortex is a closed one, so that

$$\Sigma nx dS=\Sigma mx dS=\Sigma ny dS=\Sigma mz dS=0,$$

and

$$\Sigma my dS=\Sigma nz dS=V,$$

we find

$$\Sigma Pu dS=\left(\frac{dP}{dz}\beta-\frac{dP}{dy}\gamma\right)V, \quad . \quad . \quad . \quad . \quad (49)$$

and the whole work done on the vortex in unit of time will be

$$\frac{dE}{dt}=-\frac{1}{4\pi}\Sigma(Pu+Qv+Rw)dS$$

$$=\frac{1}{4\pi}\left\{\alpha\left(\frac{dQ}{dz}-\frac{dR}{dy}\right)+\beta\left(\frac{dR}{dx}-\frac{dP}{dz}\right)+\gamma\left(\frac{dP}{dy}-\frac{dQ}{dx}\right)\right\}V. \; (50)$$

Prop. VIII.—To find the relations between the alterations of motion of the vortices, and the forces P, Q, R which they exert on the layer of particles between them.

Let V be the volume of a vortex, then by (46) its energy is

$$E=\frac{1}{8\pi}\mu(\alpha^2+\beta^2+\gamma^2)V, \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

and

$$\frac{dE}{dt}=\frac{1}{4\pi}\mu V\left(\alpha\frac{d\alpha}{dt}+\beta\frac{d\beta}{dt}+\gamma\frac{d\gamma}{dt}\right). \quad . \quad . \quad (52)$$

Comparing this value with that given in equation (50), we find

$$\alpha\left(\frac{dQ}{dz}-\frac{dR}{dy}-\mu\frac{d\alpha}{dt}\right)+\beta\left(\frac{dR}{dx}-\frac{dP}{dz}-\mu\frac{d\beta}{dt}\right)$$
$$+\gamma\left(\frac{dP}{dy}-\frac{dQ}{dx}-\mu\frac{d\gamma}{dt}\right)=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (53)$$

This equation being true for all values of α, β, and γ, first let β and γ vanish, and divide by α. We find

Similarly, and

$$\left.\begin{aligned}\frac{dQ}{dz}-\frac{dR}{dy}&=\mu\frac{d\alpha}{dt}.\\ \frac{dR}{dx}-\frac{dP}{dz}&=\mu\frac{d\beta}{dt},\\ \frac{dP}{dy}-\frac{dQ}{dx}&=\mu\frac{d\gamma}{dt}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

From these equations we may determine the relation between the alterations of motion $\frac{d\alpha}{dt}$, &c. and the forces exerted on the layers of particles between the vortices, or, in the language of our hypothesis, the relation between changes in the state of the magnetic field and the electromotive forces thereby brought into play.

In a memoir "On the Dynamical Theory of Diffraction" (Cambridge Philosophical Transactions, vol. ix. part 1, section 6), Professor Stokes has given a method by which we may solve equations (54), and find P, Q, and R in terms of the quantities on the right-hand of those equations. I have pointed out* the application of this method to questions in electricity and magnetism.

Let us then find three quantities F, G, H from the equations

$$\left.\begin{aligned}\frac{dG}{dz}-\frac{dH}{dy}&=\mu\alpha,\\ \frac{dH}{dx}-\frac{dF}{dz}&=\mu\beta,\\ \frac{dF}{dy}-\frac{dG}{dx}&=\mu\gamma,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (55)$$

with the conditions

$$\frac{1}{4\pi}\left(\frac{d}{dx}\mu\alpha+\frac{d}{dy}\mu\beta+\frac{d}{dz}\mu\gamma\right)=m=0, \quad . \quad (56)$$

and

$$\frac{dF}{dx}+\frac{dG}{dy}+\frac{dH}{dz}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

Differentiating (55) with respect to t, and comparing with (54), we find

$$P=\frac{dF}{dt}, \quad Q=\frac{dG}{dt}, \quad R=\frac{dH}{dt}. \quad . \quad . \quad . \quad (58)$$

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* Cambridge Philosophical Transactions, vol. x. part 1. art. 3, "On Faraday's Lines of Force."

We have thus determined three quantities, F, G, H, from which we can find P, Q, and R by considering these latter quantities as the rates at which the former ones vary. In the paper already referred to, I have given reasons for considering the quantities F, G, H as the resolved parts of that which Faraday has conjectured to exist, and has called the *electrotonic state*. In that paper I have stated the mathematical relations between this electrotonic state and the lines of magnetic force as expressed in equations (55), and also between the electrotonic state and electromotive force as expressed in equations (58). We must now endeavour to interpret them from a mechanical point of view in connexion with our hypothesis.

We shall in the first place examine the process by which the lines of force are produced by an electric current.

Let A B, Pl. V. fig. 2, represent a current of electricity in the direction from A to B. Let the large spaces above and below A B represent the vortices, and let the small circles separating the vortices represent the layers of particles placed between them, which in our hypothesis represent electricity.

Now let an electric current from left to right commence in A B. The row of vortices *g h* above A B will be set in motion in the opposite direction to that of a watch. (We shall call this direction +, and that of a watch −.) We shall suppose the row of vortices *k l* still at rest, then the layer of particles between these rows will be acted on by the row *g h* on their lower sides, and will be at rest above. If they are free to move, they will rotate in the negative direction, and will at the same time move from right to left, or in the opposite direction from the current, and so form an *induced* electric current.

If this current is checked by the electrical resistance of the medium, the rotating particles will act upon the row of vortices *k l*, and make them revolve in the positive direction till they arrive at such a velocity that the motion of the particles is reduced to that of rotation, and the induced current disappears. If, now, the primary current A B be stopped, the vortices in the row *g h* will be checked, while those of the row *k l* still continue in rapid motion. The momentum of the vortices beyond the layer of particles *p q* will tend to move them from left to right, that is, in the direction of the primary current; but if this motion is resisted by the medium, the motion of the vortices beyond *p q* will be gradually destroyed.

It appears therefore that the phenomena of induced currents are part of the process of communicating the rotatory velocity of the vortices from one part of the field to another.

[To be continued.]

XLV. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 126.]

BERNOULLI has published the result of an investigation of tungsten and some of its compounds*. With a view to a scientific investigation of the alloys of this metal, his first endeavour was to obtain the metal in a melted state; the result of his researches proves, however, that all previous statements as to the fusibility of pure tungsten are inaccurate. In his experiments pure tungstic acid was used; the experiments were made with the furnaces of the Royal Iron Foundry in Berlin, where he was able to command temperatures higher than any previously used in such experiments.

In one experiment a Hessian crucible was used lined with charcoal, in which there was a cavity to receive the tungstic acid, and over which there was a layer of charcoal powder. The crucible, provided with a cover, was kept at a white heat for nearly an hour. In this way a metallic mass was obtained free from carbon, but without any traces of fusion. In a subsequent experiment the Hessian crucible was completely fused, and accordingly they were replaced by the best American graphite crucibles. Even these did not resist the continuous heat of the furnace for $2\frac{1}{2}$ hours. A metallic mass was obtained, which was caked together and had some metallic lustre. This was heated again with charcoal in a crucible protected in the most complete manner; and the heat was greater than that ever observed in any puddling furnace, so much so that the slag from the coke dropped in a thin stream through the grates.

Notwithstanding this great heat the tungsten had not melted, although it had sintered to a tolerably compact mass. The metal thus obtained was heated for eighteen hours in a porcelain furnace without any change resulting.

Hence the author concludes that, with our present means, metallic tungsten is infusible.

Bernoulli also investigated the alloys of tungsten with metals, especially iron. Cast-iron turnings were intimately mixed with 1, 2, 3, 4, 5, 10, 15, and 20 per cent. of pure tungstic acid, in the idea that the carbon of the iron would reduce the acid to the state of metal. Some experiments were also made in which a larger per-centage of acid was taken; but in this case some powdered charcoal was added. The mixtures were heated in a graphite crucible to an intense white heat. With an addition of 10 per cent. of acid, the alloy had the properties of steel; it was very sonorous, had a clear grey colour, a pure fracture, and was

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* Poggendorff's *Annalen*, December 1860.

malleable. The addition of 15 per cent. of acid yielded an alloy which might be considered as steel. It was very hard, but was not sufficiently malleable. With 20 per cent. the hardness was still greater, but the malleability much less.

The iron in these experiments was grey iron, and contained a considerable quantity of graphite, and it was found that with white iron a different result was obtained. The experiments were made in the same way as the previous ones; it was found that an alloy was only formed when charcoal dust was added; otherwise the tungstic acid sintered together, and very little tungsten combined with the iron. The alloy obtained with the addition of charcoal had none of the appearance of steel; it was white on fracture, had the structure of the iron used, and was imperfectly malleable.

With an addition of tungstic acid in the proportion of 75 per cent. no regulus was obtained. Analogous experiments were made with the minerals Wolfram and Scheelite, and similar results were obtained. The manganese present in Wolfram exerted a considerable influence on the result; and with Scheelite the lime combines with the silica to form a slag, so that the alloy is purer.

It follows from these experiments, that it is not the carbon present in the iron in a state of chemical combination which reduces the tungstic acid, but that which is mechanically intermingled. From white cast iron no carbon is withdrawn by the tungstic acid, and accordingly no steel is obtained if charcoal be not added.

The waste cast-iron turnings of the workshop may hence be used for preparing directly a cast steel, to which the tungsten imparts great hardness; or if the iron does not contain too much sulphur, phosphorus, or silica, a very useful rough cast steel may be obtained by fusing it directly with a quantity of powdered Wolfram proportionate to the per-centage of carbon which it contains.

The author determined the carbon in these alloys by three methods. In the first, a piece of the alloy was laid upon a fused cake of chloride of silver, and was left for several days, covered with distilled water. In this way the iron gradually dissolved; and when the decomposition was complete, the charcoal and tungsten were collected on an asbestos filter, dried and weighed, and then the carbon determined in the usual way by combustion with oxide of copper. In another case the alloy was decomposed by chloride of copper, and in a third case it was digested with iodine until the iron was dissolved. In these cases the carbon contained in the alloy amounted to about 1 per cent.

Experiments to alloy tungsten with other metals were also made. With copper, reguli were obtained, which, however, were not homogeneous; the individual particles could be distinctly seen. In general it was also found that copper, lead, zinc, antimony, bismuth, cobalt, and nickel only became alloyed with tungsten when the reduction of the two metals took place simultaneously. The alloys are so infusible, that, with more than 10 per cent. of tungsten, no reguli are obtained, and at a higher temperature the more volatile metals escape and metallic tungsten is left behind. Iron differs in this respect from other metals. It alloys in all proportions with tungsten; with above 80 per cent., however, the alloys are infusible.

In order to prepare the tungstic acid used in these experiments, powdered Wolfram was fused with excess of carbonate of soda in an iron crucible, the fused mass dissolved, and boiled to reduce manganic acid, and filtered. The solution, which consisted of tungstate and carbonate of soda, was neutralized with nitric acid, and the tungstate of soda crystallized out. This was dissolved and treated with nitric acid, and the precipitate of hydrated tungstic acid was well washed. It was then dissolved in ammonia, which left a residue of niobic and silicic acids; the evaporated liquor deposited a fine crop of crystals of tungstate of ammonia. This was well washed with water, and then repeatedly treated with fresh quantities of nitric acid for some days to remove nitrate of ammonia. The acid was ultimately washed out and gently heated, by which it was obtained of a fine pure sulphur-yellow colour.

The author found, in all these experiments, that it was not possible to obtain pure yellow acid by directly heating the tungstate of ammonia, even when this was done under access of air. It invariably became of a green colour. This has been observed before, and has been differently interpreted, some ascribing it to the formation of a suboxide, and some to an admixture of yellow acid and the blue oxide $W^2 O^5$.

Bernoulli has found that it is a true compound. When the yellow acid was heated to the highest temperature of a gas blowpipe, it gradually changed to a green colour, and from being amorphous became crystalline. Similar results were obtained by using the high temperature of a stoneware furnace, which has an oxidizing flame. The tungstic acid was placed in suitably protected platinum crucibles, and heated for periods varying from eighteen to seventy-two hours. A green crystalline mass was obtained, while on the upper part of the crucible there were smaller crystals of the same colour. When the caked mass was divided and subjected to a further heat for eighteen hours, the result was confirmed; part had sublimed in fine crystalline laminæ.

Bernoulli analysed these two modifications by reduction with hydrogen, and found that they had the same per-centage composition, WO^3. They must therefore be regarded as two isomeric modifications of the same acid, of which the *yellow* is formed both in the moist and in the dry way, but in the latter case only at a *low* temperature; while the green variety is only formed in the dry way and at a high temperature. The latter he proposes to call *pyrotungstic* acid, in antithesis to Scheibler's acid*, which is *metatungstic* acid. Including the ordinary acid, there are therefore three varieties.

Bernoulli has made a new determination of the equivalent of tungsten, both by oxidizing tungsten, and by reducing tungstic acid. He obtained results varying within very narrow limits, which lead to the number 93·4 as that of the equivalent. Dumas had obtained the number 92†.

Bernoulli finally discusses the formula of the natural tungstates, and attempts to show that the two modifications of tungstic acid also occur in nature. He adduces a great many analyses of Wolfram. Most of them contain a certain quantity of lime and magnesia, which he considers accidental constituents. But in most cases a niobic acid is present—in the tungsten from Zinnwald 1·1 per cent.; this is to be regarded as replacing part of the tungstic acid, with which it is therefore isomorphous.

The author finally describes the mode of analysing the tungsten minerals.

Engaged in investigating the methods of working up the platinum residues for the Russian Government, Deville and Debray had occasion to examine the different methods of preparing oxygen on a large scale. They find‡ that sulphate of zinc, which, as a waste product, is now so plentiful, furnishes an economical source of this gas. When calcined in an earthen vessel, it is converted into light white oxide, which, when the sulphate is pure, may be used for painting. The temperature required for its decomposition does not exceed that necessary for binoxide of manganese. The other products of the decomposition are sulphurous acid and oxygen, which may be separated by means of the solubility of the former in alkalies; or the following method may be used, which is employed by the authors for preparing oxygen by the decomposition of sulphuric acid.

This body at a red heat may be decomposed into sulphurous acid, water, and oxygen, by means of a very simple apparatus, consisting of a retort, of about 5 litres, filled with thin platinum foil, or, better, a serpentine tube filled with platinum sponge and heated to redness. A thin stream of sulphuric acid passes into

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[*] Phil. Mag. vol. xx. p. 374.

[†] Ibid. vol. xvi. p. 211.

[‡] *Comptes Rendus*, November 26, 1860.

this apparatus through an S tube; the products pass first through a cooler which condenses the water, and then through a washer of a special form. In this way pure and inodorous gas is obtained, and a solution of sulphurous acid, which may be changed either into sulphite or hyposulphite of soda, or may be used in the sulphuric acid chambers. The expense of oxygen prepared by this plan is very small; for the method consists essentially in abstracting oxygen from the atmosphere. Even if the sulphurous acid were not utilized, sulphuric acid would still be the cheapest source of oxygen, cheaper even than binoxide of manganese.

M. Carré has applied* the great cold produced by the evaporation of condensed ammoniacal gas to the production of low degrees of temperature.

The apparatus he uses consists of two ordinary cylindrical metal receivers connected by a tube. One of these is four times the size of the other, and is filled to three-quarters its capacity with the strongest solution of ammonia. At the time of closing the vessel, care is taken to expel all air. The largest vessel is placed over the fire, the smaller being immersed in cold water. The solution is heated to 130° or 140°, the temperature being indicated by a thermometer fitted in the larger vessel. At this point nearly all the ammonia is expelled from the solution, and liquefies in the second retort. When the separation is complete, the larger vessel is cooled down: the reabsorption of the liquefied gas commences immediately, and its volatilization produces a degree of cold which readily freezes the water surrounding it. The temperature sinks to $-40°$; and M. Balard, who tried the experiment at the Collége de France, was able to solidify mercury.

Besides this apparatus, M. Carré has devised another form of it, which is continuous in its action, but otherwise depends on the same principle.

M. Leroux, of the École Polytechnique, has made some determinations of the refractive indices of vapours at high temperatures, by means of an apparatus constructed for that purpose, on which M. Babinet† has reported to the Academy.

M. Dulong had found that the refractive index of oxygen was 1·000273; of hydrogen, 1·000138; of nitrogen, 1·000300; and of chlorine, 1·000772, that of air being 1·000294. M. Leroux has found that the refractive indices of the vapours of the following substances, saturated at the ordinary pressure, are respectively—

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[*] *Comptes Rendus,* December 24, 1860.

[†] Ibid. November 26, 1860.

Sulphur	1·001629
Phosphorus . . .	1·001364
Arsenic	1·001114
Mercury	1·000556

The apparatus by which these results were obtained consists of a very large furnace mounted on an axis, and provided at its lower part with a divided circle, by which it can be inclined at any angle. In the centre of the furnace there is a prism analogous to that of Borda, employed by Dulong. This prism is made of solid iron, and the rays enter and emerge through glass plates cemented by a method peculiar to M. Leroux. The exact measurement of the angle of the prism presents some ingenious points. As in Babinet's goniometer, the light is concentrated on the prism, while filled with the vapour, by a telescope, and the light on its emergence is received on another telescope provided at its focus with a micrometric wire. With the first telescope, its distance from the furnace, the size of the furnace, the distance of the second telescope and its focal distance, which is above 2 yards, the extent of this gigantic goniometer is about 23 feet. The means of verifying the results leave nothing to be desired.

Rose*, in a paper on the separation of tin from other metals, and on its quantitative estimation, describes the following method of effecting these objects.

The separation of tin from other metals is usually effected by oxidation with nitric acid, so as to convert the tin into stannic acid: in certain cases, however, this method gives inaccurate results, and can only be said to be quite successful in the case of the strongly basic oxides.

Another method consists in dissolving the binoxide of tin in hydrochloric acid, and precipitating by sulphuric acid. Both modifications of binoxide are precipitated in this manner, in the presence of a large excess of water. The precipitates require a long time in order to settle completely, more especially when there is a large quantity of free hydrochloric acid. The precipitate must be carefully washed free from hydrochloric acid otherwise when it is ignited some tin escapes in the form of chloride. The ignition is best effected with the addition of some carbonate of ammonia.

In the presence of certain substances, phosphoric acid for example, even this method gives inaccurate results. The presence of a large excess of hydrochloric acid does not hinder the precipitation of some phosphoric acid along with the binoxide of tin.

When tin is to be separated from other metals, it must be

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* Poggendorff's *Annalen*, January 1861.

oxidized with nitric acid in the usual manner, and the residue digested with moderately strong hydrochloric acid. On the addition of a large quantity of water, all is dissolved, and the tin is then precipitated with sulphuric acid.

In the separation of copper and tin by the ordinary method, the binoxide always contains a trace of copper; but by the above process the separation of the two metals is complete, the binoxide is quite free from copper.

Tin and lead are best separated by fusing the alloy with sulphur and carbonate of potash. On treating the mass with water, the sulphide of tin dissolves. This is the best method of analysing the fusible alloy of tin, bismuth, and lead. Tin and silver are also best separated in this way.

In an acid solution, tin and bismuth are best separated by sulphide of ammonium.

The separation of iron from tin can only be completely effected by oxidizing the alloy with nitric acid, dissolving in hydrochloric acid, and saturating the hydrochloric solution with sulphuretted hydrogen.

Tin is best separated from titanium by means of sulphide of ammonium.

The separation of the oxides of tin from magnesia and the alkaline earths is best effected by igniting them with sal-ammoniac, by which the tin escapes as chloride. In general all the tin is expelled by one ignition; but two are always amply sufficient to remove the last traces of tin.

In the analysis of minerals, the separation and estimation of tin is best effected by converting it into the oxide. The volumetric analysis, however, is very convenient for the determination of the tin in tin-salts. The ordinary method is to convert the protochloride of tin in hydrochloric acid solution into the bichloride by means of oxidizing agents, such as permanganate or bichromate of potash. But protochloride of tin absorbs atmospheric oxygen, even during the determination, to such an extent as materially to vitiate the results of analyses.

Stromeyer[*] has observed that good results are obtained by the addition of sesquichloride of iron in excess to the freshly prepared solution of tin in hydrochloric acid. The reaction is as follows:—

$$\mathrm{Sn\,Cl + Fe^2\,Cl^3 = Sn\,Cl^2 + 2\,Fe\,Cl.}$$

The protochloride of iron formed is determined by permanganate; and as it is not nearly so susceptible to atmospheric oxygen as protochloride of tin, much more accurate results are obtained.

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[*] Liebig's *Annalen*, February 1861.

Tin may also be directly dissolved in sesquichloride of iron to which hydrochloric acid has been added,

$$Sn+2Fe^2Cl^3=SnCl^2+4FeCl.$$

But this method is only available with tolerably pure tin; for many other metals reduce perchloride of iron, and consume solution of permanganate.

There are several compounds which have the formula $C^4H^4Cl^2$. One of these is obtained by the action of pentachloride of phosphorus on aldehyde, and another by the action of chlorine on chlorinated ethyle. Beilstein observed some time ago that these two bodies were identical, and he has recently proved* that the same is the case with two corresponding isomeric compounds of the benzoic acid series: the one, chlorobenzole, $C^{14}H^6Cl^2$, is obtained by the action of pentachloride of phosphorus on oil of bitter almonds; and the other is the chlorinated chloride of benzyle, $C^{14}(H^6Cl)Cl$.

The latter body is formed, as Cannizaro showed, by the action of chlorine on toluole; and Beilstein used this method of preparing it. He finds that it has all the physical properties of chlorobenzole. A careful comparison also of the chemical actions of the two substances, both by his own direct experiments and by those of other experimenters, leave no doubt as to the complete identity of the two substances.

Reboul has published† the results of a lengthened and important investigation on some derivatives of glycerine. The compounds which the author describes may be derived from an oxygenized body, *glycide*, $C^6H^6O^4$, which has not been isolated, and which might be considered as the anhydride of glycerine, to which it would bear the same relation as lactide does to lactic acid. It would play the part of a diatomic alcohol, and would yield a series of ethers, the general formation of which may be thus expressed:—

$$C^6H^6O^4+A-H^2O^2=C^6H^4AO^2,$$

$$C^6H^6O^4+AA'-2H^2O^2=C^6H^2AA',$$

A and A′ representing the formulæ of monobasic acids.

The starting-point for his research is *hydrochloric glycide*, obtained by the action of potash on bihydrochloric glycerine, $C^6H^6Cl^2O^2$, which simply removes hydrochloric acid,

$$C^6H^6Cl^2O^2-HCl=C^6H^5ClO^2.$$

Bihydrochloric glycerine itself is formed by saturating a mix-

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* Liebig's *Annalen*, December 1860.

† *Annales de Chimie*, September 1860. *Répertoire de Chimie*, November 1860.

ture of glycerine and acetic acid with gaseous hydrochloric acid. The chief product of the action is bihydrochloric glycerine, which may be separated by fractional distillation; or, by heating the crude product with caustic potash, hydrochloric glycide is directly obtained. It is a colourless liquid, heavier than water, and smelling like chloroform. It boils at 118°—119°: it is metameric with Geuther's hydrochlorate of acroleine, with Riche's monochlorinated acetone, and with chloride of propionyle.

Monohydrochloric glycide combines directly with fuming hydrochloric acid to form *dihydrochloric glycide,* $C^6 H^4 Cl^2$: this body may also be formed by acting on hydrochloric glycide with pentachloride of phosphorus, an action which gives rise to the formation of trihydrochloric glycerine,

$$\underset{\substack{\text{Hydrochloric}\\ \text{glycide.}}}{C^6 H^5 ClO^2} + PCl^5 = \underset{\substack{\text{Trihydrochloric}\\ \text{glycerine.}}}{C^6 H^5 Cl^3} + PCl^3 O^2;$$

and this, decomposed by potash, loses hydrochloric acid, and the new body is formed,

$$C^6 H^5 Cl^3 - HCl = C^6 H^4 Cl^2.$$

This body is identical with what Berthelot has called epibromhydrine. It is metameric with bichlorinated propylene, and with Geuther's chloride of acroleine. Similar compounds containing bromine and iodine were also obtained.

By the action of ammonia on bihydrochloric glycide a base is formed, which the author considers identical with that obtained by the action of ammonia on terbromide of allyle.

Hydrochloric glycide unites directly with the oxyacids to form a glyceric ether, containing an equivalent of oxyacid and of hydracid. Thus,—

$$\underset{\substack{\text{Hydrochloric}\\ \text{glycide.}}}{C^6 H^5 ClO^2} + \underset{\substack{\text{Acetic}\\ \text{acid.}}}{C^4 H^4 O^4} = \underset{\substack{\text{Acetohydrochloric}\\ \text{glycerine.}}}{C^6 H^6 (C^4 H^3 O^2) ClO^4}.$$

By the action of water, hydrochloric glycide fixes two equivalents, and monohydrochloric glycerine is formed:—

$$\underset{\substack{\text{Hydrochloric}\\ \text{glycide.}}}{C^6 H^5 ClO^2} + H^2 O^2 = \underset{\substack{\text{Monohydrochloric}\\ \text{glycerine.}}}{C^6 H^7 ClO^4},$$

By the action of alcohol the hydrochloric compounds of glycide are transformed into mixed glyceric ethers, containing an equivalent of acid and an equivalent of alcohol,

$$\underset{\substack{\text{Hydrochloric}\\ \text{glycide.}}}{C^6 H^5 ClO^2} + \underset{\text{Alcohol.}}{C^4 H^6 O^2} = \underset{\substack{\text{Hydrochloric}\\ \text{ethylglycerine.}}}{C^6 H^6 Cl (C^4 H^5) O^4}.$$

When these bodies are treated with potash, hydrochloric acid is removed, and the resultant compound, *ethylglycide,* is a glycide containing the alcohol radical in the place of an equivalent of hydrogen:—

$$\underset{\text{Hydrochloric ethylglycerine.}}{C^6 H^6 (C^4 H^5) ClO^4} - HCl = \underset{\text{Ethylglycide.}}{C^6 H^5 (C^4 H^5) O^4}.$$

Ethylglycide is a mobile liquid boiling at 128°. When treated with hydrochloric acid, it yields the compound $C^6 H^6 (C^4 H^5) ClO^4$. If the compound hydrochloric amylglycerine be treated with ethylate of soda, ethylamylglycerine is formed. Thus,—

$$\underset{\text{Hydrochloric amylglycerine.}}{C^6 H^6 (C^{10} H^{11}) ClO^4} + \underset{\text{Ethylate of soda.}}{C^4 H^5 NaO^2} = \underset{\text{Ethylamylglycerine.}}{C^6 H^6 (C^{10} H^{11})(C^4 H^5) O^6} + NaCl.$$

The glyceric ethers containing two equivalents of the same acid are only a particular case of this reaction.

When hydrochloric glycide is acted upon by hydrosulphate of sulphide of potassium, a compound is obtained analogous to mercaptan,

$$\underset{\text{Hydrochloric glycide.}}{C^6 H^5 ClO^2} + KS\,HS = KCl + \underset{\text{New body.}}{C^6 H^6 S^2 O^2}.$$

A second mercaptan in this series is doubtless formed by the action of dihydrochloric glycide on hydrosulphate of sulphide of potassium, and which would have the formula $C^6 H^6 S^4$.

XLVI. *On the existence of a new Element, probably of the Sulphur Group.* By William Crookes, *F.C.S**.

In the year 1850 Professor Hofmann placed at my disposal upwards of 10 lbs. of the seleniferous deposit from the sulphuric acid manufactory at Tilkerode in the Hartz Mountains, for the purpose of extracting from it the selenium, which was afterwards employed in an investigation upon the selenocyanides†. Some residues which were left in the purification of the crude selenium, and which from their reactions appeared to contain tellurium, were collected together and placed aside for examination at a more convenient opportunity. They remained unnoticed until the beginning of the present year, when, requiring some tellurium for experimental purposes, I attempted its extraction from these residues. Knowing that the spectra of the incandescent vapours of both selenium and tellurium were free from any

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* Communicated by the Author.

† Chem. Soc. Quart. Journ. vol. iv. p. 12, and Gmelin's Handbook (Cavendish Soc. Translation), vol. viii. p. 122.

strongly-marked line which might lead to the identification of either of these elements, it was not until I had in vain tried numerous chemical methods for isolating the tellurium which I supposed to be present, that the method of spectrum-analysis was used. A portion of the residue, introduced into a blue gas-flame, gave abundant evidence of selenium; but as the alternate light and dark bands due to this element became fainter, and I was expecting the appearance of the somewhat similar, but closer, bands of tellurium, suddenly a *bright green line* flashed into view and as quickly disappeared. An isolated green line in this portion of the spectrum was new to me. I had become intimately acquainted with the appearances of most of the artificial spectra during many years' investigation, and had never before met with a similar line to this; and as from the chemical processes through which this residue had passed the elements which could possibly be present were limited to a few, it became of interest to discover which of them occasioned this green line.

After numerous experiments, I have been led to the conclusion that it is caused by the presence of a new element belonging to the sulphur group; but, unfortunately, the quantity of material upon which I have been able to experiment has been so small, that I hesitate to assert this very positively. I am, however, at work upon some of the seleniferous deposit itself, and hope shortly to be able to speak more confidently upon this point, as well as to give some account of its properties.

In the purest state that I have as yet succeeded in obtaining this substance, it communicates as definite a reaction to the flame as soda,—the smallest trace introduced into the burner of the spectrum apparatus giving rise to a brilliant green line, perfectly sharp and well-defined upon a black ground, and almost rivalling the Na line in brilliancy. It is not, however, very lasting: owing to its volatility, which is almost as great as selenium, a portion introduced at once into a flame merely shows the line as a brilliant flash, remaining only a fraction of a second; but if it be introduced into the flame gradually, the line continues present for a much longer time.

The properties of the substance, both in solution and in the dry state, as nearly as I can make out from the small quantity at my disposal, are as follows:—

1. It is completely volatile below a red heat, both in the elementary state and in combination (except when united with a heavy fixed metal). 2. From its hydrochloric solution it is readily precipitated by metallic zinc in the form of a heavy black powder, insoluble in the acid liquid. 3. Ammonia added very gradually until in slight excess to its acid solution, gives no precipitate or coloration whatever, neither does the addition of car-

bonate or oxalate of ammonia to this alkaline solution. 4. Dry chlorine passed over it at a dull red heat unites with it, forming a readily volatile chloride soluble in water. 5. Sulphuretted hydrogen passed through its hydrochloric solution precipitates it incompletely, unless only a trace of free acid is present; but in an alkaline solution an immediate precipitation of a heavy black powder takes place. 6. Fused with carbonate of soda and nitre, it becomes soluble in water,—hydrochloric acid added in excess to this liquid producing a solution which answers to the above tests 2, 3, and 5.

An examination of these reactions shows that there are very few elements which could by the remotest possibility be mistaken for it.

The accompanying list includes every element, with the exception of the gases, bromine, iodine, and carbon. Opposite the name of each I have placed the number of the reaction which eliminates it from the list of possible substances, taking great care, in every case, to give the benefit of any doubt which might arise, on account of an imperfectly known or doubtful reaction, in favour of the opposite opinion to that which I desire to prove, and, in cases where several reactions would prove the same thing, only making use of the most trustworthy.

1, 5.	Aluminium.
	Antimony.
	Arsenic.
2, 3, 5.	Barium.
2, 3, 5.	Beryllium.
1.	Bismuth.
1, 2, 5.	Boron.
6.	Cadmium.
2, 5.	Cæsium.
2, 3, 5.	Calcium.
1, 5.	Cerium.
1.	Chromium.
1.	Cobalt.
1.	Copper.
1, 5.	Didymium.
1, 5.	Erbium.
1.	Gold.
1.	Ilmenium.
1.	Iridium.
1.	Iron.
1, 5.	Lanthanium.
1.	Lead.
2, 5.	Lithium.
2, 5.	Magnesium.
1.	Manganese.
3, 6.	Mercury.
1.	Molybdenum.
1.	Nickel.
1.	Niobium.
1.	Norium.
	Osmium.
1.	Palladium.
5.	Phosphorus.
1.	Platinum.
2, 5.	Potassium.
1.	Rhodium.
1.	Ruthenium.
	Selenium.
1, 5.	Silicium.
1.	Silver.
2, 5.	Sodium.
2, 3, 5.	Strontium.
5.	Sulphur.
1.	Tantalum.
	Tellurium.
1, 5.	Terbium.
1, 5.	Thorium.
1.	Tin.
1.	Titanium.
1.	Tungsten.
1.	Uranium.
1.	Vanadium.
1, 5.	Yttrium.
2.	Zinc.
1, 5.	Zirconium.

There are therefore left the following, amongst which, if already known, it must occur:—antimony, arsenic, osmium, selenium, and tellurium; and although, to my own mind, many of the reactions detailed above are sufficient proof that it cannot be one of the first three elements, yet I have thought it better to let them pass.

Each of the above five bodies, both in the elementary state and in combination, has been rigidly scrutinized in the spectrum apparatus by myself and many friends. Not a trace of such a line is shown by either of them in the green part of the spectrum,—Antimony, arsenic, and osmium, in fact, giving continuous spectra, in which every colour is visible. The remaining elements, selenium and tellurium, might almost be dismissed unchallenged, inasmuch as I was first led to the examination by finding that it was *not* either of these. Nevertheless I have, as stated at the commencement of this paper, repeatedly examined their spectra, and find no trace of such a line, the alternate light and dark bands in the almost continuous spectra of selenium and tellurium forming in fact so strong a contrast to the one single green ray of the new substance, that the latter may readily be detected in the presence of an enormous excess of either of the former.

In order to remove any remaining doubt which there might be as to the green line being due to any of the elements mentioned in the above list, I have, moreover, specially examined the spectra produced by each of these bodies in detail, either in their elementary state, or in their most important compounds. Many of them give rise to spectra of great and characteristic beauty, but none give anything like the green line; nor, in fact, is there any artificial spectrum, except that of sodium, which equals it in simplicity.

There still may be urged the possibility of its being a compound of two or more known elements, or an allotropic condition of one of them; a moment's thought, however, will show that neither of these hypotheses is tenable. They would in reality prove what they are raised to oppose; for nothing less could follow than a veritable transmutation of one body into another, and a consequent annihilation of all the groundwork upon which modern science is based. If an element can be so changed as to have totally different chemical reactions, and to have the spectrum of its incandescent vapour (which is, *par excellence*, an elementary property) altered to an appearance totally unlike that given by its former self, it must have been changed into something which it originally was not. This, in the present position of science, is an absurdity.

The method of exhaustion which I have adopted to prove the elementary character of the body which communicates this green line to the spectrum of the blue gas-flame*, may seem unnecessary as well as unchemical in the present state of the science; I was obliged, however, to rely upon what I may call circumstantial evidence of its not being a known element, owing to the very small quantity of substance at my command

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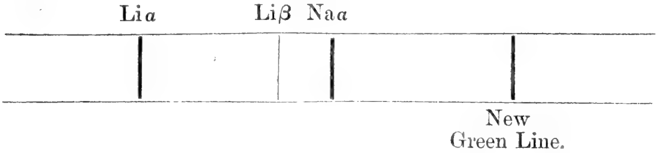
* I need scarcely add that the line is quite distinct from either of the green or blue lines seen in a gas-flame which is undergoing complete combustion. It is moreover far more brilliant than these.

(I believe I overestimate the amount which I have as yet obtained, at two grains), which precluded me from trying many reactions. The method of spectrum-analysis adopted to prove the same fact, although perfectly conclusive to my own mind, might not have been so to others, unsupported by chemical evidence.

The following diagram will serve to show the position in the spectrum which the new green line occupies with respect to the two lithium and the sodium lines.



For confirmatory experiments on many of the observations mentioned in this paper, I am indebted to my friend Mr. C. Greville Williams. The detailed examination of the various spectra are at present being jointly pursued by us, and will be published as soon as completed.

XLVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Contined from p. 238.]

January 23, 1861.—L. Horner, Esq., President, in the Chair.

THE following communications were read :—

1. "On the Gravel and Boulders of the Punjâb." By J. D. Smithe, Esq., F.G.S.

In the Phimgota Valley (a continuation of the great Kangra or Palum Valley) the drift consists of sand and shingle with boulders of gneiss, schist, porphyry, and trap, from 6 inches to 5 feet in diameter. Some of the boulders, having a red vitreous glaze, occur in irregular beds. This moraine-like drift lies on the tertiary beds, which, here dipping gently towards the plains, gradually become vertical, and are succeeded by variegated compact sandstones, gradually inclining away from the plains ; next come various slates, at a high angle ; and gneissic rocks lie immediately over them.

2. "On *Pteraspis Dunensis* (*Archæoteuthis Dunensis*, Roemer)." By Prof. T. H. Huxley, F.R.S., Sec. G.S.

The fossil referred to in this communication is from Daun in the Eifel, and was described by Dr. Ferd. Roemer (in the 'Palæontographica,' vol. iv. p. 72, pl. 13) as belonging to the naked Cephalopods, under the name of *Palæoteuthis Dunensis* (changed to *Archæoteuthis* in the 'Leth. Geogn.'); and in the Jahrb. 1858, p. 55, Dr. F. Roemer described a second specimen from Wassennach on the Laacher See. Prof. Huxley reproduced, with remarks, Dr. Roemer's description of the specimens; and, after observing that Mr. S. P. Woodward had already suggested (Manual of Mollusca, p. 417) that Roemer's fossil was a fish, he stated his conviction that it was really a *Pteraspis*, agreeing in all essential particulars with the British *Pteraspides*, though possibly of a different species.

3. "On the 'Chalk-rock' lying between the Lower and the Upper Chalk in Wilts, Berks, Oxon, Bucks, and Herts." By W. Whitaker, Esq., B.A., F.G.S.

The author has more particularly examined the band which he terms "Chalk-rock" on the northern side of the western part of the London Basin. Here it has its greatest thickness (12 feet) to the west, gradually thinning eastward. It is a hard chalk, dividing into blocks, by joints perpendicular to the bedding; and it contains hard calcareo-phosphatic nodules. It contains no flints; and in the district referred to none occur below it; but there is often a band of them resting on its upper surface. It seems to form an exact boundary between the Upper and the Lower Chalk, being probably the topmost bed of the latter. In this case it will often serve as an index of the relative thickness of these divisions, or as a datum for the measurement of the extent of denudation that the Upper Chalk has suffered. North of Marlborough, where it is thick, the Chalk-rock appears to have given rise to two escarpments (an upper and a lower) to the western portion of the Chalk Range.

Fossils are usually rare in this bed; but Mr. J. Evans, F.G.S., collected several from it near Boxmoor; and amongst them the genera *Belosepia* (hitherto known only as Tertiary), *Baculites, Nautilus, Turrilites, Solarium, Inoceramus, Parasmilia,* and *Ventriculites* are represented; and the following species have been identified—*Litorina monilifera* and a new species, *Pleurotomaria* sp., *Myacites Mandibula, Spondylus latus, Sp. spinosus, Rhynchonella Mantelliana, Terebratula biplicata,* and *T. semiglobosa.*

February 6, 1861.—L. Horner, Esq., President, in the Chair.

The following communication was read:—

"On the Altered Rocks of the Western and Central Highlands." By Sir R. I. Murchison, F.R.S., V.P.G.S., and A. Geikie, Esq., F.G.S.

In the introduction it was shown that the object of this paper was to prove that the classification which had been previously established by one of the authors in the county of Sutherland was applicable, as he had inferred, to the whole of the Scottish Highlands. The structure of the country from the borders of Sutherland down the western part of Ross-shire was detailed, and illustrated by a large map of Scotland coloured according to the new classification, and by numerous sections. Everywhere throughout this tract it could be proved that an older gneiss, which the authors called "Laurentian," was overlain unconformably by red Cambrian sandstones; these again unconformably by quartz-rocks, limestones, and a gneissose and schistose series of strata, as previously shown in the typical district of Assynt. From the base of these quartz-rocks a perfect conformable sequence was shown to exist upward into the gneissose rocks, which is not obliterated by granite or any similar rock.

The tract between the Atlantic and the Great Glen consists, according to the authors, of a series of convoluted folds of the upper gneissose rocks, until, along the line of the Great Glen, the underlying quartzose series is brought up on an anticlinal axis. A prolongation of this axis probably exists along part of the west coast of Islay and Jura, two islands which exhibit a grand development of the lower or quartzose portion of the altered Silurian rocks of the Highlands.

From the line of the Great Glen north-eastward to the Highland border, the country was explained as consisting of a great series of anticlinal and synclinal curves, whereby the same series of altered rocks which occurs on the north-west is repeated upon itself. One synclinal runs in a N.E. and S.W. direction across Loch Leven. The anticlinal of quartzose rocks that rises from under it to the S.E. spreads over the Breadalbane Forest to the Glen Lyon Mountains, where it sinks below the upper gneissose strata with their associated limestones. Ben Lawers occupies the synclinal formed by these upper strata; and the limestones and quartz-rock come up again in another anticlinal axis corresponding with the direction of Loch Tay. The continuity of these lines of axis was traced both to the N.E. and S.W.

It thus appeared that the crystalline rocks of the Highlands are capable of reduction to order; that the same curves and folds could be traced in them as in their less altered equivalents of the South of Scotland; and that in what had hitherto appeared as little else than a hopeless chaos, there yet reigned a regular and beautiful simplicity.

In conclusion, Sir Roderick Murchison vindicated the accuracy of his published sections in the N.W. of Sutherland, which had been approved after personal inspection by Professors Ramsay and Harkness; and he gave detailed reasons for disbelieving the accuracy of the sections recently put forth by Prof. Nicol, which were intended as corrections of his own. He concluded by affirming that, through the aid of Mr. Geikie, the proofs of the truthfulness of his own sections, showing a conformable ascending order from the quartz-rocks and limestones into crystalline and micaceous rocks, had now been extended over such large areas that there could no longer be any misgivings on the subject.

February 20, 1861.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Coincidence between Stratification and Foliation in

the Crystalline Rocks of the Highlands." By Sir R. I. Murchison, V.P.G.S., and A. Geikie, Esq., F.G.S.

Allusion was, in the first place, made to the early opinions of Hutton and Macculloch, who regarded the gneissic and schistose rocks of the Highlands as stratified. Mr. Darwin's views of the nature of the "foliation" of gneiss and schist were then referred to; and it was insisted that this condition was not to be found in the rocks of the Highlands,—the so-called "foliation" which the late Mr. D. Sharpe had described in 1846 as characterizing the crystalline rocks of that country being, according to the authors, really mineralized stratification. It was then pointed out that, as Prof. Sedgwick had previously insisted on the wide difference between "foliated" or "schistose" and "cleaved" or "slaty" rocks, and as Prof. Ramsay had in 1840 recognized interlaminated quartz as being parallel to stratification in the Isle of Arran, "foliation" should be regarded as coincident with stratification, and not with cleavage, in the Scottish Highlands.

After some observations on the occurrence of cleavage in slates at Dunkeld, Easdale, Ballahulish, and near the Spittal of Glenshee, the authors stated their belief that all the "foliation" of the crystalline rocks of the Highlands is nothing more than lamination due to the sedimentary origin of deposits, in which the sand, clay, lime, mica, &c. have subsequently been more or less altered, and that the "arches of foliation" described by Mr. D. Sharpe (Phil. Trans. 1852) correspond in a general way with the parallel anticlinal axes shown by the authors in a former paper to exist in the Highlands. They remarked that the synclinal troughs, however, are not expressed in Mr. Sharpe's figures, and that he has omitted the bands of limestone which they refer to as an important evidence of the stratification of the district. They also pointed to the acknowledged difficulty which the quartzites presented to Mr. Sharpe, but which readily fall into the system of undulated strata that they have described. One of the quartzites having yielded an Orthoceratite, and pebbles being present in one of the schists of Ben Lomond, these facts were adduced as further evidences of the real stratal condition of the schists and quartzites of the Highlands.

2. "On the Rocks of portions of the Highlands of Scotland South of the Caledonian Canal, and on their equivalents in the North of Ireland." By Professor R. Harkness, F.R.S., F.G.S.

The author, having had an opportunity of examining the geology of the North-west of Scotland in the year 1859, and more especially the arrangement of rocks described by Sir R. Murchison as "fundamental gneiss, Cambrian grits, lower quartz-rock, limestones, upper quartz-rock, and overlying gneissose flags," applied the results of his observations during last summer to portions of the Highlands lying south of the Caledonian Canal, and to the North of Ireland. Developed over a large portion of these districts are masses of gneissose rock, of varying mineral nature, and sometimes putting on the aspect of a simple flaggy rock. Where these gneissose masses come in contact with plutonic masses, they exhibit that highly crystalline aspect which induced Macculloch and others of the Scotch geologists to regard them as occupying an extremely low position among the sedimentary series, and to apply to them the Wernerian term "primitive." Many of Macculloch's descriptions, however, show that this assumed low position is not the true place of this gneiss among the sedimentary rocks which make up the Highlands of Scotland.

In a section from the southern flank of the Grampians to Loch Earn (and in other sections, from Loch Earn to Loch Tay, from Dunkeld to Blair Athol, in the Ben y Gloe Mountains, in Glen Shee, &c.), there is seen a sequence which indicates that this gneiss is the highest portion of the series of rocks, with underlying quartz-rock and limestone.

In the county of Donegal, Ireland, a like sequence is seen. A section from Inishowen Head to Malin Head, along the east side of Loch Foyle, presents us with gneissose rocks above limestone and quartz-rocks, exactly as in Scotland. In no portion of Scotland south of the Caledonian Canal, nor in the North of Ireland, did the author recognize any trace of the "fundamental gneiss."

March 6, 1861.—Leonard Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On the Succession of Beds in the Hastings Sand in the Northern portion of the Wealden Area." By F. Drew, Esq., F.G.S., of the Geological Survey of Great Britain.

Having first referred to the division of the Wealden beds by former authors into the "Weald Clay," the "Hastings Sand," and the "Ashburnham Beds," and the subdivision of the "Hastings Sand" by Dr. Mantell into "Horsted Sands," "Tilgate Beds," and "Worth Sands," and having defined the district under notice as lying between and in the neighbourhood of the towns of Tenterden, Cranbrook, Tunbridge, Tunbridge Wells, East Grinstead, and Horsham, Mr. Drew proceeded to describe, first, the several beds in the meridian and vicinity of Tunbridge Wells. The Weald Clay is at least 600 feet thick in this district, and is underlain by sands and sandstones, termed by the author the "Tunbridge Wells Sand," on account of its being well exposed there. This subdivision is about 180 feet thick, and was described in detail,—an important feature being the "rock-sand," or massive sandstone forming the picturesque natural rocks of the neighbourhood. The shales and clays underlying these sands form the "Wadhurst Clay" of the author, and are at places 160 feet thick. This subdivision has yielded much ironstone in former times. It is underlain by other sand and sandstones, more than 250 feet thick, also yielding ironstone. These are termed "Ashdown Sand" by Mr. Drew on account of their forming the heights of Ashdown Forest.

Eastward of the meridian of Tunbridge Wells Mr. Drew has found the same sequence of beds, and he believes a similar succession to occur around Battle and Hastings. Westward of Tunbridge Wells as far as East Grinstead, the same beds occur, but beyond that the Weald Clay and Tunbridge Wells Sand alone are exposed; and the latter is here divided into upper and lower beds by shale and clay (termed "Grinstead Clay" by the author), which thicken westward to 50 feet and more. It is the "Lower Tunbridge Wells Sand" that forms natural rocks near Grinstead. Near Horsham the Weald Clay contains, at about 120 feet from its base, bands of stone known as the "Horsham Stone," used for roofing and paving.

The author then explained at large the grounds on which he proposed to replace Dr. Mantell's term "Horsted Sands" by "Upper Tunbridge Wells Sand," that of "Worth Sands" by "Lower Tunbridge Wells Sand," and that of "Tilgate Beds" by "Wadhurst Clay," and his reason for proposing the name of "Ashdown" for the next lowest bed of the "Hastings Sand."

The paper concluded with a description of some of the chief lithological characters of the clays and sandstones of the Wealden area under notice.

2. "On the Permian Rocks of the South of Yorkshire; and on their Palæontological Relations." By J. W. Kirkby, Esq. Communicated by T. Davidson, Esq., F.G.S.

The author, after defining the area to be treated of, first noticed the results of the labours of former observers in this district; and then succinctly described the several strata, referring to Professor Sedgwick's Memoir on the Magnesian Limestone for descriptions of the physical geography and very much of the lithological characters of the country under notice. The strata treated of Mr. Kirkby recognizes (in descending order) as, 1. the Bunter Schiefer, about 50 feet thick; 2. the Brotherton Beds, 150 feet; 3. the small-grained Dolomite, 250 feet; 4. the Lower Limestone, 150 feet; 5. the Rothliegendes or Lower Red Sandstone, 100 feet. These were then compared and coordinated with the Permian strata of Durham, where the three limestone members are thus represented:—1. The Upper Limestone by the Yellow, Concretionary, and Crystaline Limestone (250 feet); 2. The Middle Limestone by the Shell- and Cellular Limestone (200 feet); and 3. The Lower Limestone by the Compact Limestone (200 feet) and the Marl-slate (10 feet),—the over- and under-lying sandstones being much alike as to thickness in the two areas.

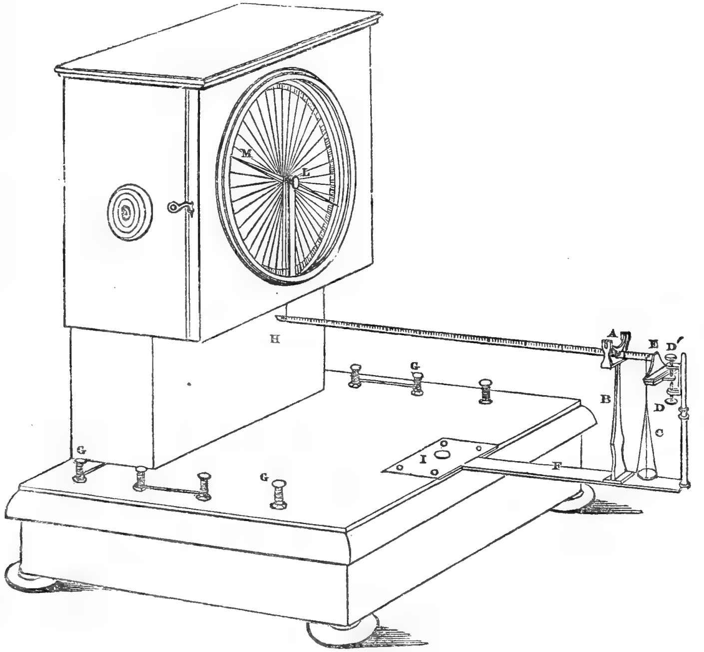
After some remarks on the probable geographical conditions existing in the Permian epoch, the author proceeded to treat of the Permian fossils of South Yorkshire in detail. These belong to about thirty species, and are nearly all from the Lower Limestone,—three species only occurring in the Brotherton beds. With three exceptions they occur also in the several limestones of Durham; five of them are found in the lower part of the red marls of Lancashire; and six of them are found at Cultra and Tullyconnel in Ireland. The distribution of the species in the several beds at different localities having been fully treated of, the Permian fossils of South Yorkshire were compared, first, with those of Durham; next, with those of Lancashire; and thirdly, with those of Ireland. Remarks on the distribution of the Permian Fauna in time concluded the paper.

XLVIII. *Intelligence and Miscellaneous Articles.*

SOME RESULTS IN ELECTRO-MAGNETISM OBTAINED WITH THE BALANCE GALVANOMETER. BY GEORGE BLAIR, M.A.

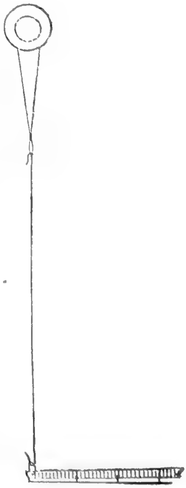
SINCE bringing the balance galvanometer, along with some other apparatus, before the Society in the course of last session, the writer had made some experiments with this new galvanometer, which led to results that he did not anticipate, and which he considered to be of sufficient importance to justify him in presenting them to the Society. The object originally aimed at in its construction was to obtain an exact measure, by weight, of the actual amount of deflective force which the current exerts upon the magnetic needle. The instrument constructed for this purpose is represented in fig. 1. The

Fig. 1.



coil consists of a total length of 1660 feet of No. 22 copper wire, weighing rather more than 6 lbs., and divided into four parts, the ends of which are brought out and connected with their respective terminals G G, so that they can be used separately or as one coil. The needle with which the first experiments were made consisted of a small rectangular steel bar, $1\frac{1}{2}$ inch in length, rather less than $\frac{1}{4}$th of an inch in breadth, and about half the thickness of a shilling. It weighed exactly 18 grains, and, when magnetized, its lifting power was 44 grains, or nearly $2\frac{1}{2}$ times its own weight. The index M, which is 9 inches in length, weighs only 2 grains. A small brass pulley, $\frac{1}{8}$th of an inch in diameter, is fixed upon the axis between the index and the supporting screw L. The balanced lever E H consists of a thin slip of hard spring-brass, placed edgewise for strength, and tapered, for lightness, towards the end of the long arm A H. The short arm A E is loaded to act as a counterpoise; and to this arm a scale-pan C is suspended, at a distance from the fulcrum equal to exactly $\frac{1}{10}$th of the length of the other arm. It carries also at its extremity a thin horizontal projection, which vibrates between two screw-points D, D′, and by which, with the aid of the wooden foot-screws of the instrument, the lever can be always exactly levelled when balanced. The fulcrum B is supported on a stout brass bar F, which is firmly held in its place by means of the screw I, and can be removed at pleasure. When it is desired that the needle shall have liberty to move in both directions, the extremity H of the long arm of the lever is connected with the needle by a slender wire suspended from a very fine thread, fixed to the upper part of the pulley and carried down on both sides of it, as shown in fig. 2. The arm A H is divided into ten equal parts, each of which is subdivided into tenths; and, estimating the poles of the needle to be at a distance of about $\frac{1}{6}$th of its total length from the extremities, the diameter of the pulley is so adjusted that a weight of 100 grains, suspended at the distance of one of the large divisions from the fulcrum, acts with a force of 1 grain at the poles of the needle; suspended at division 2, it acts with a force of 2 grains; at 2·5, with a force of $2\frac{1}{2}$ grains, and so on.

Fig. 2.



The following Table exhibits the results of the first series of experiments made with a small Grove's battery, the platinum plates of which expose only two inches of surface, and having the zinc plates immersed in a saturated solution of chloride of sodium. It is a striking characteristic of Grove's battery that it slightly increases in force after being some time in action; and it would have been preferable, therefore, to use a Daniell's, on account of its remarkable constancy; but the writer had not a sufficient number in series. The fourth column indicates the weight required to bring the index of the balance galvanometer back to zero; the fifth column expresses the same weight reduced to the force which it exerts at the poles of the needle, but increased in each case by half a grain, to compensate for the small preponderance given to the long arm of the lever in order to keep the needle vertical when not deflected by the current :—

Table I.

1. No. of pairs.	2. Angles on tangent galvanometer.	3. Angles on balance galvanometer, without weight.	4. Weights required to bring the needle to zero.	5. Force at poles of needle, in grains.	6. Ratio of tangents reduced.
1	4° 30′	66°	100 grs. at 2·5	3·00	3·12
2	8 30	77 30	„ „ 5·5	6·00	5·96
6	29 30	87	1000 grs. at 2·2	22·50	22·64
12	44 45	89	„ „ 4·9	49·50	39·64

It will be seen that, up to six pairs, the numbers in the fifth column, expressing the force of the current in grains at the poles of the needle, vary very nearly in the same ratio as the tangents of the angles of deflection on the tangent galvanometer reduced to a comparable form in the sixth column. With twelve pairs, however, the weight required to balance the current is 49·5, or very nearly 50 grains; whereas, according to the tangent galvanometer, it should not have exceeded 40 grains. Reflecting on this anomaly, the writer could only arrive at the conclusion that the needle, surrounded by a very powerful current in such a large coil, ceased to act as a permanent magnet, and was temporarily charged with a higher magnetism induced by the current itself. Subsequent experiments completely confirmed this conclusion; and he was led to examine the subject more minutely by observing that the needle, after being several times subjected to the action of the current from twelve pairs, appeared to have lost its permanent magnetism; for he afterwards found repeatedly that when the index was brought back by successive increments of weight to 40°, the smallest possible additional weight sent it back to zero. Before taking out the needle to ascertain this, he submitted it a second time to the action of six and twelve pairs, with the following results :—

Number of pairs.	Current force by tang. galvan.	Weights supported.
6	22·64	7·5 grains.
12	39·64	24·5 „

whereas it will be seen, by referring to the preceding Table, that originally it supported 22·50 and 49·50 grains respectively. The needle was then taken out, and was found to have almost entirely lost its magnetism. It had originally lifted 44 grains; it was now only with the greatest precaution that it lifted 2 grains. But

$$7{\cdot}5 \;:\; 24{\cdot}5 = 1 : 3{\cdot}27$$
$$\text{and } (22{\cdot}64)^2 : (39{\cdot}64)^2 = 1 : 3{\cdot}06.$$

The writer had therefore little doubt that, if not merely demagnetized, but formed of soft iron, the needle, when placed in a favourable position, would turn with a force proportional to the square of the current; whereas it plainly appears from Table I., that so long as its permanent magnetism is sufficient to resist the inducing action of the current, the needle is deflected with a force simply proportional to the current.

To determine this interesting question, two new needles were constructed, similar in shape and size to the former, but somewhat lighter, each weighing only 17·25 grains. The one was of steel, tempered to the hardness of glass; and was magnetized till it lifted with some difficulty 43 grains; the other was of soft hoop-iron, well annealed. With these needles the following results were obtained from experiments conducted very carefully, and using, for greater accuracy, a single-thread suspension:—

Table II.

	1.	2.	3.	4.	5.	6.
	No. of pairs.	Angles on tangent galvanometer.	Tangents to rad. 1.	Deflective force at poles of needle, in grains.	Ratio of tangents.	Ratio of squares of tangents.
With magnetized steel needle.	3	17° 40′	0·318	11·5	11·5	11·4
	6	31 20	0·609	22·0	22·0	42·1
	9	41 0	0·869	32·5	31·5	85·8
	12	47 0	1·072	43·5	38·8	130·6
With needle of soft iron	3	17 40	0·318	6·0	6·0	6·0
	6	31 0	0·601	20·5	11·4	21·5
	9	40 20	0·849	40·0	16·1	42·9
	12	47 0	1·072	57·0	20·4	68·4

A glance at the above Table will show that, with the permanently magnetized needle, the numbers in column 4, expressing the deflective force of the current in grains, are very nearly proportional to the numbers in column 5, expressing the *simple ratio of the tangents or quantities of current*; whereas with the soft iron needle they are nearly proportional to the *squares of the same quantities*, reduced to a comparable form in column 6. In both cases the only marked deviation coincides with the powerful current from twelve elements of the battery, in which case the steel needle, evidently acting under the superadded influence of induced temporary magnetism, is deflected with a force which exceeds the estimated amount by 4·7 grains, whereas the soft iron needle, under the same current force, falls short of the calculated amount by 11·4 grains. The last effect is probably attributable to the fact that, as the needle approaches saturation, the law of the squares gradually merges into the law of simple proportion. The writer regrets that he had not at command sufficient battery power to put this point to the test of decisive experiments, but hopes to do so shortly with a Daniell battery of 50 or 100 elements. In the meantime the results above given, having been arrived at with great care, and amply confirmed by experiments several times repeated, appear to establish very conclusively the following principles :—

1. A permanently magnetized steel needle, suspended in the middle of a galvanometer coil, is deflected with a force simply proportional to the quantity of current transmitted, so long as the current force which acts upon it is not sufficient to impart temporarily a higher magnetism than that which it permanently possesses. Beyond this point, the deflective force exerted on the magnetized steel needle increases in a somewhat higher ratio than the current, and therefore the accuracy of any form of galvanometer can be trusted only within certain limits of current force and of length and proximity of coil.

2. A pure soft iron needle, suspended at an angle of about 40° to the direction of the current (the angle varying according to the shape of the needle), is deflected with a force which, within certain limits of current power, is very exactly proportional to the squares of the quantities of current. Beyond these limits the deflective force exerted on the needle increases in some constantly diminishing ratio lower than that of the squares of the current.

3. The action of the current in deflecting a magnetic needle is precisely the same action, and follows the same law, as that which it exerts in magnetizing a bar of soft iron. The amount of magnetism actually imparted to a bar or needle of soft iron is directly proportional to the quantity of current; for the force with which a soft iron needle is deflected under different currents is not proportional to its temporary magnetism in each case, but to the product of its magnetism multiplied by the force of the current. By increasing the force of the current, two effects are produced; in the *first* place, the magnetism of the needle is increased in the same proportion; and *secondly*, the increased current acting upon this increased magnetism deflects the needle with a force proportional to the product of the two, or in other words, proportional to the square of the actual quantity of current.

It only remains to add the results of two series of experiments, showing the very striking difference between the deflective forces exerted upon the two needles at different angles of inclination. Table III. shows the increasing weights required to balance the needles at angles successively diminished by 10°; Table IV. exhibits the effect produced by successive additions of weights, equivalent to a force of ten grains acting at the poles of the needles. In both cases the battery power employed was twelve small Grove's, but the current declined, in the course of the experiments, from 47° to 45° on the tangent galvanometer, which accounts for the fact that the maximum weights supported are less than in the earlier experiments recorded in Table II. In working out Table III., the weight employed (1000 grains) was simply advanced along the lever, and its reduced amount at the poles of the needle noted when the index, in gradually retreating, pointed to the successive angles specified. The results in Table IV. were obtained by moving the weight from one to another of the successive divisions, marked 1, 2, 3, &c. on the lever; and the differences of the angles vary, as might be expected, in nearly the reverse order of the differences of weights in Table III.:—

Table III.

	Angles on balance galvanometer.	Weights reduced to force at needle, in grains.	Differences of successive weights.
With magnetized steel needle.	90°	0·0	
	80	6·5	6·5
	70	15·5	9·0
	60	23·0	7·5
	50	30·0	7·0
	40	35·0	5·0
	30	39·4	4·4
	20	41·5	2·1
	10	42·0	0·5
With soft iron needle.	90	0·0	
	80	15·5	15·5
	70	29·5	14·0
	60	40·5	11·0
	50	50·0	9·5
	40	53·0	3·0

Table IV.

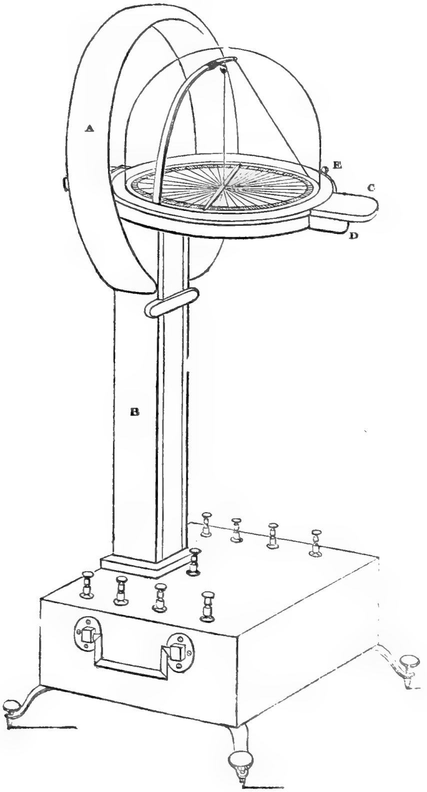
	Weights reduced to force at needle, in grains.	Angles on balance galvanometer.	Differences of successive angles.
With magnetized steel needle.	0·0	90° 0′	
	10·0	75 30	14° 30′
	20·0	63 0	12 30
	30·0	48 30	14 30
	40·0	26 0	22 30
	41·5		
With soft iron needle.	0·0	90 0	
	10·0	84 0	6 0
	20·0	76 30	7 30
	30·0	68 20	8 10
	40·0	59 10	8 10
	50·0	47 10	12 0
	52·0	0 0	

It will be observed from Table III. that a very small weight was sufficient to throw back the steel needle to 80°, and that, on the contrary, it is from 90° to 70° that the soft iron needle sustains itself with comparatively the greatest power, requiring very nearly double the weight which suffices for the steel needle to balance it at the latter angle. When reduced to 40°, however, the smallest possible additional weight throws back the iron needle to zero, and in every case it was necessary to move it aside with the finger to nearly that angle, before it would exhibit the slightest action under the influence of the current, or, in other words, any perceptible trace of *longitudinal* magnetization. In fact, being laterally magnetized when hanging in a vertical position, it necessarily offered a certain resistance to deflection.

On taking out the steel needle after these experiments, it was found to have retained its original magnetism unimpaired.

The tangent galvanometer by which the force of the current was

Fig. 3.



determined in the preceding experiments, and which the writer had also the honour of submitting to the Society last session, is represented in fig. 3. It is a very convenient modification of Gaugain's instrument, described in the *Annales de Chimie*, vol. xli. 1854. The circular frame A, containing a variety of coils of different lengths and sizes of covered wire, is 9·6 inches in external diameter; so that when the instrument is in use, the divided circle must be drawn out till its centre is 2·4 inches in front of the coil or coils through which the current is to be sent. To facilitate this operation, the horizontal bar D, upon which the disk slides, has the proper distances for each coil marked upon it, and these are successively exposed to view at the back of the instrument, in proportion as the disk is drawn smoothly forward by means of the handle C. The needle is only one inch in length, but carries parallel to itself a fine filament of glass for an index*; it is suspended by a silk fibre, and is raised so as to hang freely within its glass shade by turning the pin E. The ends of the coils are carried down through the hollow pillar B, and by connecting the electrodes of the battery with the proper terminals, the current can be sent through one or more of the coils. It can be sent through one convolution of No. 16, through 203 convolutions of No. 34, or any other of the intermediate lengths and sizes of wire; and in this way the resistance and the force exerted by the current upon the needle can be very exactly adapted to the character of the battery, or other rheomotor employed.—*From the Proceedings of the Glasgow Philosophical Society for* January 16, 1861.

ON THE PRESENCE OF ARSENIC AND ANTIMONY IN THE SOURCES AND BEDS OF STREAMS AND RIVERS. BY DUGALD CAMPBELL, ANALYTICAL CHEMIST TO THE HOSPITAL FOR CONSUMPTION, BROMPTON.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Since my communication upon the above subject, published in the Philosophical Magazine of October last, I have repeated my experiments upon several of the sands I then reported upon, and with the like results which I then gave. I have also made experiments upon other specimens since obtained, and in all I have hitherto examined I have found arsenic, and generally, if not always, accompanied with antimony. The process followed was the same as I formerly described, only I invariably used hydrochloric acid without the slightest trace of arsenic in it, as some doubts had been cast upon my former results, in a notice of my paper in the 'Chemical

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* The thickness of the index is grossly exaggerated in the figure; it ought to be as fine as a hair, and short in one arm.

News' of the 20th of October last, because in my anxiety to admit of any one testing the accuracy of my results, I had described how the process might be conducted with what is generally sold as pure acid, but which, if properly tested, is rarely free from arsenic.

During these last experiments, it occurred to me to distil the sands with a second and a third dose of acid, and in most cases I have found the yield of arsenic and antimony to be much greater, say from two to five times, in the second distillate than in the first; and in some I have found the third distillate to give more than the first, but in others less.

These results induce me to say that, before a sand could be pronounced not to contain any arsenic or antimony, it should be distilled to dryness with at least three distinct doses of acid, each distillate being tested carefully in the manner described in my former communication.

I am, Gentlemen,
Your obedient Servant,
DUGALD CAMPBELL.

7 Quality Court, Chancery Lane,
March 25th, 1861.

NOTE ON A MODIFICATION OF THE APPARATUS EMPLOYED FOR ONE OF AMPÈRE'S FUNDAMENTAL EXPERIMENTS IN ELECTRO-DYNAMICS. BY PROFESSOR TAIT.

My attention was recalled by Principal Forbes's note[*] (read to the Royal Society of Edinburgh on January 7), to his request that I should at leisure try to repeat Ampère's experiment for the mutual repulsion of two parts of the same straight conductor, by means of an apparatus which he had procured for the Natural Philosophy Collection in the University. Some days later I tried the experiment, but found that, on account of the narrowness of the troughs of mercury, it was impossible to prevent the capillary forces from driving the floating wire to the sides of the vessel. I therefore constructed an apparatus in which the troughs were two inches wide, the arms of the float being also at that distance apart. Making the experiment according to Ampère's method with this arrangement, I found one small Grove's cell sufficient to produce a steady motion of the float from the poles of the pile; in fact, the only difficulty in repeating the experiment lies in obtaining a perfectly clean mercurial surface.

Two objections have been raised against Ampère's interpretation of this experiment, one of which is intimately connected with the subject of Principal Forbes's note. This is, the difficulty of ascertaining exactly what takes place where a voltaic current passes from one conducting body to another of different material. It is known

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[*] Phil. Mag. for February 1861.

that thermal and thermo-electric effects generally accompany such a passage. To get rid of this source of uncertainty, I have repeated Ampère's experiment in a form which excludes it entirely. In this form of the experiment the polar conductors and the float form one continuous metallic mass with the mercury in the troughs,—the float being formed of glass tube filled with mercury, with its extremities slightly curved downwards so as nearly to dip under the surface of the fluid, and the wires from the battery being plunged into the upturned outward extremities of two glass tubes, which are pushed through the ends of the troughs so as to project an inch or two inwards under the surface of the mercury. A little practice is requisite to success in filling the float and immersing it in the troughs without admitting a bubble of air. This float, being heavier than the ordinary copper wire, plunges deeper in the fluid, and encounters more resistance to its motion; but with two small Grove's cells only, Ampère's result was easily reproduced, even when the extremities of the float rested in contact with those of the polar tubes before the circuit was completed. It is obvious that here no thermo-electric effects can be produced in the mercury; and I have satisfied myself that the motion commences before the passage of the current can have sensibly heated the fluid in the tubes.

The other class of objections to Ampère's conclusion from this experiment, depending on the spreading of the current in the mercury of the troughs, is of course not met by this modification. I have made several experiments with a view to obviate this also; but my time has been so much occupied that I have not been able as yet to put them in a form suitable for communication to this Society.—*From the Proceedings of the Royal Society of Edinburgh*, vol. iv.

NOTE RESPECTING OZONE.

In the Philosophical Magazine, May 1860, page 403, is a short account of "the production of Ozone by means of a Platinum Wire made incandescent by an Electric Current," by M. Le Roux, which has just recalled to my memory the following fact.

I have frequently observed that a coil of platinum wire heated to *whiteness* in a strong jet of purified hydrogen, and then removed from the jet, imparted a feeble ozone-like odour to the ascending stream of hot air above the wire as long as the wire remained nearly white-hot, and ceased to impart this odour at a somewhat lower temperature.

G. Gore.

Birmingham.

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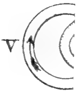

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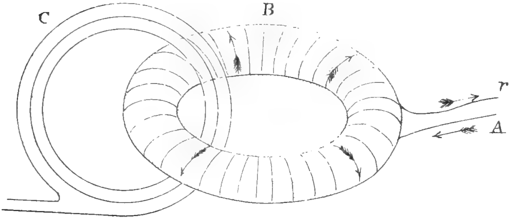

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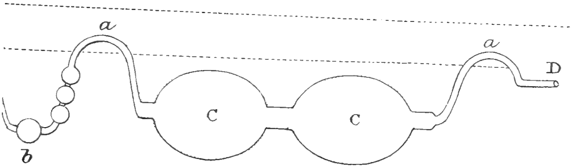

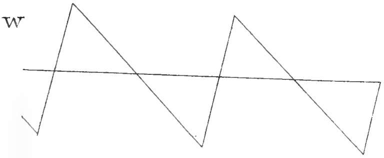

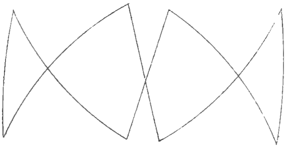

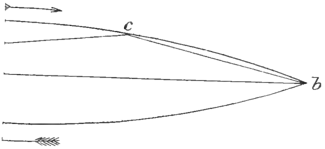

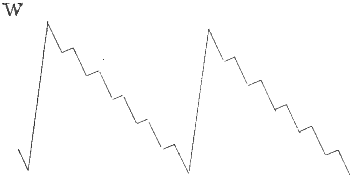

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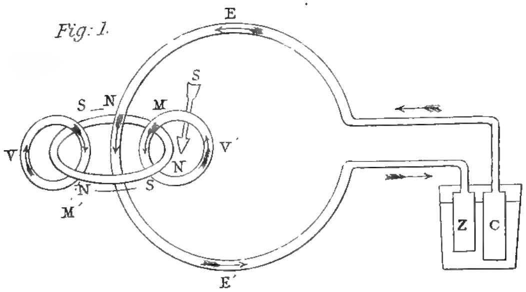

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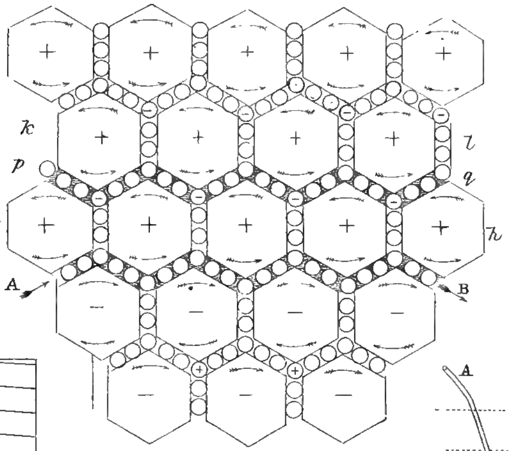

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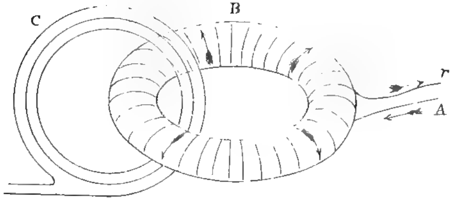

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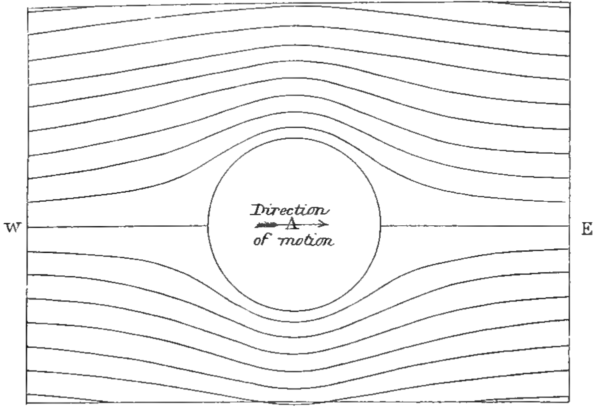

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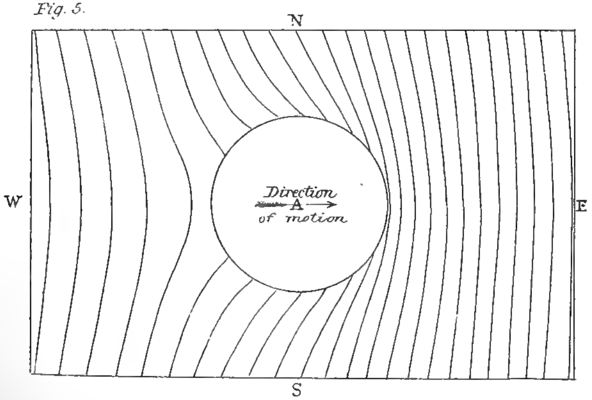

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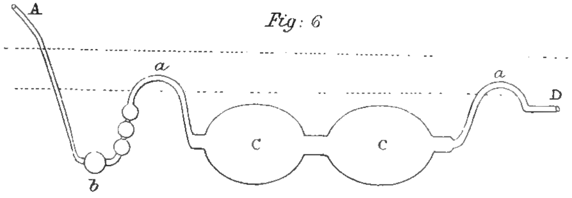

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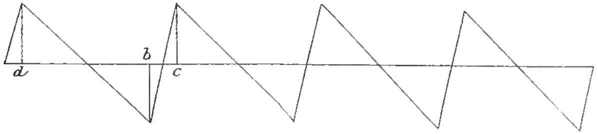

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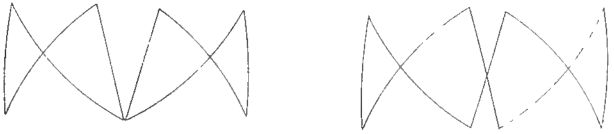

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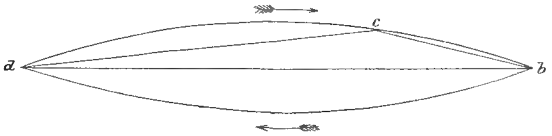

Fig: 10.

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

[FOURTH SERIES.]

MAY 1861.

XLIX. *On the Determination of the Direction of the Vibrations of Polarized Light by means of Diffraction.* By L. LORENZ*.

THE question whether the vibrations of polarized light are perpendicular to the plane of polarization, or in that plane, notwithstanding its great theoretical importance, is still undecided. On comparing the different arguments that may be advanced in favour of either hypothesis, only two will be found that have an essential bearing on the subject,—the experiments, namely, of Jamin on the reflexion of light by transparent media, and the polarization of light caused by diffraction.

The experiments of Jamin have hitherto been explained on the supposition that the vibrations of polarized light are perpendicular to the plane of polarization. This, however, is not decisive of the question, since it has hitherto been assumed that there is an instantaneous change of refractive power at the boundary of two transparent media; whereas I shall prove, in a subsequent essay† "On the Reflexion of Light," that Jamin's experiments can only be brought into complete accordance with Fresnel's formulæ for the reflexion and refraction of light, on the hypothesis that these formulæ hold good for infinitely small changes of the refractive index, that is to say, on the hypothesis that there is a gradual passage from one medium to the other.

It may be asked whether Fresnel's formulæ really hold good for an infinitely small change of refractive power, and whether these formulæ may be deduced on either hypothesis as to the direction of the vibrations of polarized light; two questions which I shall answer in a third essay.

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* Translated by F. Guthrie, from Poggendorff's *Annalen*, vol. cxi. p. 315, 1860.

† See Poggendorff's *Annalen*, vol. cxi. p. 460: a translation of this paper will be given in a future Number of this Magazine.

The change of the plane of polarization by diffraction conducts us by another road to the determination of the same question. Several years ago Mr. Stokes furnished a mathematical proof that the plane of polarization must be changed by diffraction. Doubts have, however, justly been entertained as to the accuracy of his conclusions, because he only succeeded in solving the problem of diffraction imperfectly; and I have therefore sought the complete solution of the problem by other methods, which I have found particularly applicable in the theory of elasticity.

When an undulation passes through an opening in a solid plane, waves proceed from the opening on both sides of the plane. The motion in the plane is not known, except insofar as it is determined by the fact that the sum of the components of the incident and reflected waves are equal to the components of those transmitted, and that the normal and tangential pressures on both sides of the plane of the opening are the same at every point. Let the components of the incident rays be denoted by u, v, and w; those of the transmitted rays by u_1, v_1, and w_1; of the reflected by u_2, v_2, and w_2; and let the plane of coordinates x, y, z coincide with the plane of the opening.

The first condition gives, for $x=0$,

$$u+u_2-u_1=0, \quad v+v_2-v_1=0, \quad w+w_2-w_1=0; \quad . \quad (1)$$

and by the help of these equations it may easily be deduced from the second condition for $x=0$, that

$$\frac{d(u+u_2-u_1)}{dx}=0, \quad \frac{d(v+v_2-v_1)}{dx}=0, \quad \frac{d(w+w_2-w_1)}{dx}=0. \quad (2)$$

If the incident waves are waves of light, then

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0;$$

and equations (1) and (2) may be satisfied by the supposition that

$$\left.\begin{aligned}\frac{du_1}{dx}+\frac{dv_1}{dy}+\frac{dw_1}{dz}=0,\\ \frac{du_2}{dx}+\frac{dv_2}{dy}+\frac{dw_2}{dz}=0,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

from which it is evident that no waves of condensation can be formed.

The law of the motion is expressed by the differential equation

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}=\frac{1}{w^2}\frac{d^2}{dt^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

which must satisfy all the components, where w expresses the rate of propagation, and t the time. This equation will obviously be satisfied by the expression

$$\frac{\phi(wt-r)}{r},$$

where $r=\sqrt{x^2+(y-\beta)^2+(z-\gamma)^2}$, and therefore also by

$$\Phi=-\frac{1}{2\pi}\int d\beta\int d\gamma\,\frac{\phi(wt-r,\beta,\gamma)}{r};$$

which function Φ, when the limits of integration are determined by the boundaries of the opening, also possesses the property that its differential coefficient with respect to x becomes equal to $\phi\,(w, t, y, z)$ when x decreases to nothing, and the point yz is within the opening. If x increases to nothing, the value of the differential coefficient is $-\phi(wt, y, z)$; and if the point yz is without the opening, it becomes nothing when $x=0$; for by differentiating the integral with respect to x, $x=0$ enters as a factor, thus causing every element of the integral to disappear, except those in which $r=0$, that is to say, $y=\beta$, $z=\gamma$. Whence, if x is positive, and the point (yz) is within the limits of the integral,

$$\left[\frac{d\Phi}{dx}\right]^{x=0}=\left[\frac{1}{2\pi}\int d\beta\int d\gamma\,\frac{x}{r^3}\right]^{x=0}\phi(wt, y, z)=\phi(wt, y, z);$$

and if x is negative,

$$\left[\frac{d\Phi}{dx}\right]^{x=0}=-\phi(wt, y, z).$$

Introduce now other functions Ψ, X, Φ_1, Ψ_1, X_1, which are related to the respective functions ψ, χ, ϕ_1, ψ_1, χ_1 in the same way as Φ is to ϕ, and put

$$\left.\begin{aligned}u_1&=\Phi+\frac{d\Phi_1}{dx}-\frac{d(\mathrm{F}+\mathrm{F}_1)}{dx},\\ v_1&=\Psi+\frac{d\Psi_1}{dx}-\frac{d(\mathrm{F}+\mathrm{F}_1)}{dy},\\ w_1&=\mathrm{X}+\frac{d\mathrm{X}_1}{dx}-\frac{d(\mathrm{F}+\mathrm{F}_1)}{dz},\end{aligned}\right\}\quad\ldots\ldots\quad(5)$$

where the functions F, F_1 are so chosen that

$$\frac{du_1}{dx}+\frac{dv_1}{dy}+\frac{dw_1}{dz}$$

becomes equal to nothing, then

$$\left.\begin{aligned}\Delta^2 F &= \frac{d}{dx}\left[\Phi+\frac{d\Psi_1}{dy}+\frac{dX_1}{dz}\right],\\ \Delta^2 F_1 &= \frac{d^2\Phi_1}{dx^2}+\frac{d\Psi}{dy}+\frac{dX}{dz}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (6)$$

To the components u_2, v_2, w_2 of the reflected wave we give the same values as to u_1, v_1, w_1; only it must be observed that in the first x is always negative, in the latter positive.

Assuming now that

$$[F]^{x=0}=0 \text{ and } \left[\frac{dF_1}{dx}\right]^{x=0}=0, \quad . \quad . \quad . \quad . \quad (7)$$

the truth of which will appear from what follows, we get by means of (1) for $x=0$,

$$\left.\begin{aligned}u+u_2-u_1&=u-2\phi_1(wt,y,z)=0,\\ v+v_2-v_1&=v-2\psi_1(wt,y,z)=0,\\ w+w_2-w_1&=w-2\chi_1(wt,y,z)=0;\end{aligned}\right\} \quad . \quad . \quad (8)$$

and by means of (2) for $x=0$,

$$\left.\begin{aligned}\frac{d(u+u_2-u_1)}{dx}&=\frac{du}{dx}-2\phi(wt,y,z)=0,\\ \frac{d(v+v_2-v_1)}{dx}&=\frac{dv}{dx}-2\psi(wt,y,z)=0,\\ \frac{d(w+w_2-w_1)}{dx}&=\frac{dw}{dx}-2\chi(wt,y,z)=0.\end{aligned}\right\} \quad . \quad (9)$$

All the conditions are hereby fulfilled, and the functions ϕ, ϕ_1, ψ, &c. determined. The truth of equations (7) may now also be easily demonstrated.

The problem of diffraction is therefore completely solved by equations (5), (6), (8), and (9). If we now pass to the particular case in which the incident waves lie in a plane, the components are determined by the following equations,

$$u=\xi\mathfrak{C}, \quad v=\eta\mathfrak{C}, \quad w=\zeta\mathfrak{C},$$

where

$$\mathfrak{C}=\cos k(wt-ax-by-cz),$$

$$a\xi+b\eta+c\zeta=0, \quad a^2+b^2+c^2=1.$$

Since, moreover, we only require to determine the motion at a point at a considerable distance behind, the opening in the screen must be very great; and putting ρ for the distance of the observed point from the origin of coordinates, we have

$$r = \sqrt{x^2+(y-\beta)^2+(z-\gamma)^2} = \rho - m\beta - n\gamma,$$

where

$$\rho = \sqrt{x^2+y^2+z^2},\quad m=\frac{y}{\rho},\quad n=\frac{z}{\rho};$$

and l, m, and n being the cosines of the angles made by the diffracted ray with the coordinate axes, $l^2+m^2+n^2=1$. Now from (9) we get

$$\phi(wt, y, z) = \tfrac{1}{2}ak\xi \sin k\,(wt-by-cz),$$

whence

$$\Phi = \tfrac{1}{2}ak\xi\mathrm{S},$$

where

$$\mathrm{S} = -\frac{2}{2\pi\rho}\int d\beta\int d\gamma \sin k[wt-\rho+(m-b)\beta+(n-c)\gamma].$$

And the values of the functions that enter into (5) being found in a similar manner, we get

$$u_1 = \tfrac{1}{2}k(a+l)[\xi - l\,(l\xi+m\eta+n\zeta)]\mathrm{S},$$
$$v_1 = \tfrac{1}{2}k(a+l)[\eta - m(l\xi+m\eta+n\zeta)]\mathrm{S},$$
$$w_1 = \tfrac{1}{2}k(a+l)[\zeta - n\,(l\xi+m\eta+n\zeta)]\mathrm{S}.$$

These expressions hold good also for the waves reflected from the opening, only that in this case l is negative. For a point in the direction opposite to that of the incident ray $a+l=0$, and therefore all the components of the motion are equal to nothing.

Mr. Stokes has arrived at the same result, although he did not regard the reflected wave, and has not completely solved the problem. If a plane be supposed to pass through the refracted and the incident ray, and if α denote the angle which the vibrations of the incident ray make with the normal to this plane, α_1 the angle made by the vibrations of the diffracted ray with the same normal, and β the angle of diffraction, then we easily find, as Stokes has done, that

$$\tan\alpha_1 = \cos\beta\tan\alpha,$$

which is independent of the form and position of the opening. The vibrations therefore become more nearly vertical after passing through a vertical slit or grating. Accordingly, therefore, as experiment shows that the plane of polarization is rendered more vertical or more horizontal by diffraction, so must the vibrations of polarized light be parallel or perpendicular to the plane of polarization. It must, however, be remembered that, mathematically speaking, the screen is supposed to be a plane which does not itself vibrate, and which reflects no light from its edges.

The experiments that have hitherto been tried leave this question still undecided; for while Stokes, from experiments made with glass gratings, found that the plane of polarization is rendered more horizontal, Holtzmann, from experiments made with a smoke-grating, came to the opposite conclusion. In order finally to settle this question, I have instituted a course of experiments with gratings of various kinds.

By means of a heliostat and collecting lens, I introduce a portion of the sun's rays into a chamber. At some distance from the focus of the first lens a smaller lens receives the rays, and transmits them, almost parallel, through a polarizing Nicol's prism, which is fastened in a tube to a vertical circular arc. An index with a vernier gives the angle which the plane of polarization of the transmitted light makes with the vertical line. At the distance of about 7 metres the light falls on a vertical grating, which is fastened to a small plate in the middle of a horizontal circular arc, which is provided with a moveable horizontal telescope. Before the object-glass is placed a doubly-refracting prism of rock-crystal, which divides the polarized pencil into two, polarized perpendicularly to each other. This prism can be turned about the axis of the telescope. In general, therefore, two horizontal bands of light of unequal brightness will be seen in the telescope; but by turning the Nicol's prism or the doubly-refracting crystal about its axis, the intensity of these two images can be rendered equal.

The experiments were generally conducted as follows:—The Nicol's prism was turned in such a direction that the plane of polarization made an angle of 45° with the vertical line, and the telescope was so placed that its vertical thread passed through the two illuminating points, while the horizontal thread lay between the two horizontal bands of diffracted light. When the telescope was turned through the angle β, both bands, owing to the position of the rock-crystal, were of equal intensity. The telescope was then put back into its original position, and the Nicol's prism was turned into such a position that one of the images entirely disappeared.

If the Nicol's prism required to be turned through the angle δ (or $\delta \pm 90°$ or $\delta + 180°$), the plane of polarization must have been turned through the same angle, provided that the light has not been elliptically polarized by diffraction; and if δ be positive, the plane of polarization has been rendered more horizontal.

Sometimes I first determined δ, and then the angle of diffraction β, for which the two bands are equally bright.

There is, however, a source of error in these experiments, of which I only became aware after some time. If, for example, the upper half of the grating produce a more brilliant diffracted image than the lower, then if the grating entirely cover the object-glass, as was always the case in the above experiments, the upper diffracted image will be too bright. And if this image be polarized horizontally, δ will be found too great; if vertically, too small. To render the experiment perfect, it is therefere necessary to turn the rock-crystal through an angle of 180°, and take the mean of the two values of δ so obtained. If this precaution be neglected, very considerable errors may be introduced, especially when smoke-gratings are employed, and I imagine it is this that has misled Holtzmann. He observed, in the case of a smoke-grating, that for a diffraction of 20° there was a very considerable difference in the brightness of the horizontal and vertically-polarized images. This is nearly always so with gratings of this description: the upper or the lower image appears the brightest, without reference to the position of the plane of polarization. With a perfectly accurate grating, M. Holtzmann would not have been able to distinguish the slight difference that really does exist.

My first experiments were made with a gold grating (1000 bars to the Paris inch). Light polarized at an angle of 45° with the vertical, when diffracted with this grating, gave two images, of which neither could be made entirely to disappear for any position of the rock-crystal; and this was still more evident when the grating was placed obliquely. The diffracted light must therefore either have been elliptically polarized, or have been partly converted into ordinary light. That the former was the case, I inferred from the fact that elliptically-polarized light could be converted by diffraction into circularly and plane-polarized. If, for example, I passed light polarized at the angle α through a Fresnel's parallelopiped, whose reflecting surfaces made the angle 45° with the vertical, the angle α could be so chosen that one image in the telescope could be made entirely to disappear, or, on the other hand, so that the two images, on turning the rock-crystal, always retained the same intensity.

By measurements made in this manner, I convinced myself that the phenomenon is essentially the same as that which accompanies the ordinary reflexion of light from polished metal surfaces, and that the effect of the diffracted light is imperceptible in comparison with that reflected from the edges.

I now provided myself with various smoke-gratings. Polished glass surfaces were smoked with burning camphor, and then treated with a few drops of oil of turpentine to fix the smoke to the glass. These were then divided by means of a machine into bars only an inch long (2, 5, 10, and 16 to the millim.).

With these gratings I no longer observed any elliptic polarization. I found no observable differences in the results for the different gratings, and content myself therefore with giving the mean results for them all.

When the grating was perpendicular to the incident ray, and on the side of the glass towards the telescope, as was the case in Holtzmann's experiments, I found the angle δ, through which the plane of polarization was turned, extraordinarily small, and therefore only determined it accurately for a single angle of diffraction (65°). The plane of polarization of the incident light was in all the following experiments inclined at an angle of 45° with the vertical.

The mean result for $\beta=65°$ was

$$\delta=1° 52'.$$

The plane of polarization therefore had become very slightly more horizontal. For greater values of β I found δ still smaller, which at first greatly perplexed me.

When the grating was turned round so as to be on the side towards the incident ray and perpendicular to it, the plane of polarization was turned through a greater angle in the same direction, and for $\beta=65°$ I found

$$\delta=12° 30'.$$

These results agree neither with Holtzmann's experiments, nor with the conclusions that seem to follow from theory. I think, however, they can be explained as follows.

When the light first passes through the glass and then through the grating, the circumstances are almost the same as when it encounters the grating in the substance of the glass, as may be concluded from the fact that there is no reflexion at the boundary between the smoke and the glass. The diffraction therefore takes place within the substance of the glass, and the diffracted light is *afterwards* refracted in passing out of the plate. If β_1 be the diffraction in the glass (β being the observed diffraction), and n the index of refraction of glass, then $\sin\beta=n\sin\beta_1$. In consequence of the refraction, the plane of polarization is again altered and becomes more *vertical*. Supposing now that the vibrations are perpendicular to the plane of polarization, the change of the plane of polarization δ_1, caused by the diffraction β_1, is determined by the equation

$$\tan(45°-\delta_1)=\cos\beta\,.$$

If therefore δ be the angular change of the plane of polarization after efle xion at the first surface of the glass, we have by Fresnel's formulæ,

$$\tan(45°-\delta)=\frac{\tan(45-\delta_1)}{\cos(\beta-\beta_1)}=\frac{\cos\beta_1}{\cos(\beta-\beta_1)}.$$

The mean index of refraction n was determined by experiment as to the angle of polarization, and I found

$$\log n = 0{\cdot}18886.$$

For $\beta = 65°$ we should therefore have $\delta = 2° \, 11'$, which agrees pretty well with experiment, which gave $\delta = 1° \, 52'$. That δ should decrease as β increased, as appeared from experiment, follows also from this calculation.

If, on the contrary, the smoked side of the glass is turned towards the incident ray, the light is diffracted before it reaches the surface of the glass, and is afterwards twice refracted. We have therefore

$$\tan (45° - \delta) = \frac{\cos \beta}{\cos^2 (\beta - \beta_1)};$$

from which it follows that if $\beta = 65°$, $\delta = 16° \, 2' \, 30''$.

In this case experiment gave a decidedly less value for δ, which shows that the actual circumstances are only approximately those assumed, and that the diffraction of the light only takes place partially within the substance of the glass. This is still more evident when the grating is placed obliquely to the incident rays, so as to make equal angles with them and the axis of the telescope; since in that case for $\beta = 90°$ I found $\delta = 20°$ instead of 45°, which is given by calculation.

It is obvious, therefore, that the circumstances, though very complicated, are naturally accounted for in all essential particulars, on the supposition that the vibration of polarized light is perpendicular to the plane of polarization, whereas the other hypothesis is altogether irreconcileable with experiment.

In order to render these results less complicated and more susceptible of calculation, I contrived a different arrangement of the smoke-gratings. Canada balsam was melted over the surface of the glass, and a smooth glass plate was pressed down on it, which it was found could easily be done without injuring the grating. As Canada balsam has almost the same index of refraction as glass, all the circumstances could then easily be calculated. The grating was so placed that it made equal angles with the incident rays and the axis of the telescope. In this position of the apparatus the *vertically* polarized portion of the incident light was found to be weakened more than that polarized horizontally; and therefore the change of the plane of polarization was positive, *although* reflexion at the two glass surfaces tended to turn the plane of polarization in the opposite direction. As the mean of many experiments with several gratings (2, 5, 10 bars to the millim.), I found—

$\beta=$	40°	50°	60°	70°	80°	90°	100°
δ observed ……	2·24	3·0	4·54	6·36	7·42	9·6	12·3
Calculated ……	2·30	3·54	5·37	7·37	9·53	12·22	15·0

where δ is given by the equations

$$\tan(45^\circ-\delta)=\frac{\cos\beta_1}{\cos^2\frac{1}{2}(\beta-\beta_1)},\ \text{or}\ \sin\tfrac{1}{2}\beta_1=\sin\tfrac{1}{2}\beta,$$

$\frac{1}{2}\beta$ being the angle which the incident ray makes with the normal to the grating, $\frac{1}{2}\beta_1$ that made by the refracted ray with the same line, while β_1 is the diffraction within the substance of the glass itself.

It will be seen that experiment agrees very well with calculation, only that it gives results in all cases a little too small. Whatever, therefore, may be the cause of this difference, experiment most decidedly favours the hypothesis that the vibrations of polarized light are perpendicular to the plane of polarization, since in the opposite case δ would be negative and of much greater magnitude.

I next investigated the effect of diffraction by smoked metal gratings. When, however, these were of a perfect dull black, the diffraction images produced by them were far too feeble; they were therefore rendered smoother by passing a drop of oil of turpentine over them. Gratings of this description must, moreover, be rather fine and very accurately made. Some experiments made with a grating of 200 bars to the Paris inch (the thickness of the wire bars was $\frac{1}{600}$th of a millim.), the grating being equally inclined to the incident light and the axis of the telescope, gave approximately the following results:—

$\beta=$	25°	35°	40°	45°	50°	55°
$\delta=$	10°	16°	20°	25°	30°	35°

The change of direction is positive, but much greater than it should be according to calculation. The polarization of the diffracted light was moreover slightly elliptical, from which it was evident that the reflexion from the metal surfaces of the wires was not entirely prevented by the smoke. On endeavouring to smoke the grating more perfectly, I partly destroyed its accuracy, and rendered it unfit for further experiments of this description. I have not succeeded in obtaining reliable results with other gratings of this description: there are peculiar difficulties in the way of checking the reflexion from the metal edges, and at the same time preserving the diffraction image sufficiently large and distinct.

The results obtained are, however, not unimportant, since the excessive values of δ can easily be accounted for on the ground of elliptic polarization.

If it be supposed, for example, that the difference of phase of the vertical and horizontal components is Δ, and that δ_1 is the change of direction when there is no elliptic polarization, an easy calculation gives

$$\tan 2\delta = \frac{\tan 2\delta_1}{\cos \Delta},$$

whence δ must always be greater than δ_1, their signs, however, being always the same.

As experiment gave δ positive, it confirms the result already obtained, *that the vibration of polarized light is perpendicular to the plane of polarization.*

Copenhagen, June 28, 1860.

L. *On certain Laws relating to the Boiling-points of different Liquids at the ordinary Pressure of the Atmosphere.* By THOMAS TATE, *Esq.*[*]

IT is well known that the boiling-point of water is raised by the addition of a soluble salt, or by the addition of a strong acid, and that this augmentation of the boiling-temperature depends upon the relative amount of salt or acid added, as the case may be; but, as far as I know, no general formulæ have hitherto been given to express the relation between the augmentation of boiling-temperature and the relative weight of the substance added to the water.

Different weights of anhydrous salt being dissolved in 100 parts of pure water, and the augmentation of boiling-temperature being observed, we obtain data for expressing the relation of the per-centage of the salt to the corresponding augmentation of the boiling-temperature of the solution. The salts which I have examined in this manner are as follows:—the chlorides of sodium, potassium, barium, calcium, and strontium; the nitrates of soda, potassa, lime, and ammonia; and the carbonates of soda and potassa. I have found for all these salts, that *the augmentation of boiling-temperature may be approximately expressed in a certain power of the per-centage of the salt dissolved*: thus, if k be put for the weight of dry salt in 100 parts of water, and T the corresponding temperature of ebullition above that of boiling water under the same atmospheric pressure, then

$$T = ak^{x}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

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[*] Communicated by the Author.

where a is constant for each salt only, and the exponent α is constant for all the salts contained in certain special groups. The salts enumerated may be divided into four distinct groups, the salts in each possessing certain remarkable points of relationship with respect to their boiling-temperatures; viz., *the augmentations of boiling-temperature of the solutions in each group of salts have a constant ratio to one another for all equal weights of salt dissolved.* Thus if T and T′ be put for the augmentations of boiling-temperature corresponding to any equal portions of two salts dissolved, belonging to the same group, then

$$\frac{T}{T'} = \text{a constant quantity.}$$

The constant quantity here expressed is, in some cases, nearly equal to the inverse ratio of the combining equivalents of the bases of the salts.

Moreover, if the weights of two portions of one kind of salt, separately dissolved in 100 parts of water, be proportional to the two portions of another salt belonging to the same group and similarly dissolved, then the ratio of the augmented temperatures of ebullition of the former will be equal (approximately) to the ratio of the augmented temperatures of ebullition of the latter.

$$\text{Thus if } \frac{k_1}{k_2} = \frac{k'_1}{k'_2}, \text{ then } \frac{T_1}{T_2} = \frac{T'_1}{T'_2}, \text{ and conversely;}$$

where k_1 and k_2 are the respective weights of one kind of salt separately dissolved in 100 parts of pure water, T_1 and T_2 the respective augmented temperatures of ebullition; and k'_1, k'_2, T'_1, and T'_2 are the corresponding symbols for the other salt.

For the sake of conciseness of expression, I shall sometimes speak of this augmentation of temperature simply as the temperature of ebullition, which, in fact, it would be if the temperature of boiling water were taken as zero.

The first group of salts comprises the chlorides of sodium, potassium, and barium, together with carbonate of soda.

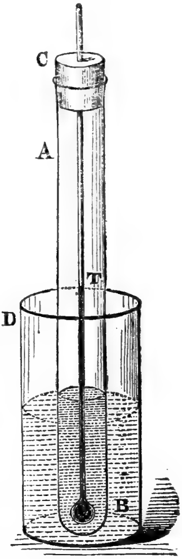
The second group comprises the chlorides of calcium and strontium, and probably other salts.

The third group comprises the nitrates of soda, potassa, and ammonia.

The fourth group comprises the carbonates of potassa, and nitrate of lime.

If $T = af(k)$ represent the relation between T and k of a salt in any group, a being constant, and $f(k)$ a known function of k, then $T = a'f(k)$ will be the formula of relation for any other salt in the same group. For let T and T′ be the temperatures of ebullition in each case respectively for equal values of k, then

$\frac{T}{T'} = \frac{a}{a'}$ = a constant for all values of T and T′ corresponding to equal values of k.



The annexed diagram represents the apparatus with which these experiments were made. D B an oil-bath heated on a sand-bath; A B a wide tube of some length containing the solution of the salt; T a thermometer passing through a perforated cork C, and having its bulb immersed in the liquid to within about one-quarter of an inch of the bottom of the tube. A slit is made in the side of the cork to keep the vapour in the tube at the same pressure as the external air. Pieces of platinum-foil were put in the liquid to facilitate the discharge of vapour; and the oil in the bath was time after time agitated to keep all the parts of the liquid at a uniform temperature. The boiling-temperature of pure water corresponding to the atmospheric pressure was first determined; known weights of anhydrous salt, corresponding to 100 parts by weight of water, were time after time introduced into the tube, and the corresponding temperatures of ebullition were noted: the elevations of these temperatures above that of boiling water were entered in the following Tables of results. The process was continued, in some cases, until the solution of maximum salt was nearly attained. A correction for the observed temperatures was made on account of the column of mercury in the stem of the thermometer not in contact with the liquid.

Augmentations of boiling-temperatures, in degrees Centigrade, of different solutions of the salts contained in Group 1.

Weight of salt in 100 parts of water, k.	Corresponding boiling-temperatures of the different solutions of salt above that of pure water.							
	Chloride of sodium, T.	Chloride of potassium, T′.	Chloride of barium, T″.	Carbonate of soda, T‴.	Value of T by formula $T=\frac{1}{12{\cdot}57}k^{\frac{5}{4}}$.	Value of T′ by formula $T'=\frac{2}{3}T$.	Value of T″ by formula $T''=\frac{1}{2}T'$.	Value of T‴ by formula $T'''=\frac{3}{7}T$.
0	0	0	0	0	0	0	0	0
8	1·1	·8	...	·5	1·07	·71	...	·47
16	2·5	1·86	·8	1·1	2·54	1·70	·93	1·07
24	4·2	2·93	1·4	1·8	4·22	2·80	1·46	1·80
32	6·0	4·10	2·0	2·5	6·05	4·00	2·05	2·57
40	8·0	5·34	2·6	3·3	8·00	5·33	2·67	3·14
48	...	6·60	3·3	4·5	......	......	3·30	
56	...	7·86	4·0	...	......	......	3·93	

Here the near coincidence of the results in the second and sixth columns shows that the experimental values of k and T may be approximately represented by the formula

$$T=\frac{1}{12\cdot 57}k^{\frac{5}{4}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

Again, the near coincidence of the results in the third and seventh, fourth and eighth, fifth and ninth columns shows that in this group of salts the temperatures of ebullition have a constant ratio to each other for all equal weights of salt.

The errors of these formulæ are probably within the limits of the errors of observation. Owing to the oscillations of the mercury column at the boiling-points of the liquids, the errors of the readings of the thermometer might amount to about one-fourth of a degree.

These experimental results for the most part agree with those given by Legrand (see Dr. Miller's 'Chemistry,' second edition, p. 247).

Augmentations of boiling-temperatures, in degrees Centigrade, of different solutions of the salts contained in Group 2.

If T and T′ be put for the augmentations of the boiling-temperatures of the solutions of the chlorides of calcium and strontium respectively for any equal portions of the salts dissolved, then it will be found that $T'=\cdot 34T$ very nearly, as shown in the following examples:—

(1) For $k=16$, $T=2$; then by the formula, $T'=1\cdot 08$.
By experiment, $T'=1$.
(2) For $k=64$, $T=16\cdot 8$; then by the formula, $T'=9\cdot 07$.
By experiment, $T'=9$.
(3) For $k=96$, $T=27\cdot 2$; then by the formula, $T'=14\cdot 68$.
By experiment, $T'=14\cdot 7$.
(4) For $k=112$, $T=32\cdot 3$; then by the formula, $T'=17\cdot 44$.
By experiment, $T'=17\cdot 2$.

Augmentations of boiling-temperatures, in degrees Centigrade, of different solutions of the salts contained in Group 3.

If T, T′, T″ be put for the augmentations of the boiling-temperatures of the solutions of the nitrates of soda, potassa, and ammonia respectively, and k, k', k'' the corresponding percentage of salts respectively dissolved, then the following formulæ will be found to represent the results of experiments very nearly:—

$$T=\frac{1}{8\cdot 1}k^{\cdot 955}; \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

and for all equal weights of salt,

$$T'' = \frac{6}{7}\,T, \quad \dots\dots\dots \quad (4)$$

and

$$T' = {\cdot}7\,T''. \quad \dots\dots\dots \quad (5)$$

Applications of formula (3).

(1.) For $k=24$; then by the formula, $T=2{\cdot}57$.
By experiment, $T=2{\cdot}6$.
(2.) For $k=48$; then by the formula, $T=4{\cdot}97$.
By experiment, $T=5$.
(3.) For $k=96$; then by the formula, $T=9{\cdot}65$.
By experiment, $T=9{\cdot}72$.
(4.) For $k=120$; then by the formula, $T=11{\cdot}93$.
By experiment, $T=12$.
(5.) For $k=168$; then by the formula, $T=16{\cdot}47$.
By experiment, $T=16{\cdot}3$.
(6.) For $k=216$; then by the formula, $T=20{\cdot}93$.
By experiment, $T=20{\cdot}3$.

Applications of formula (4).

(1) For $T=2{\cdot}6$, $T''=2{\cdot}23$. By experiment, $T''=2{\cdot}3$.
(2) For $T=7{\cdot}45$, $T''=6{\cdot}38$. By experiment, $T''=6{\cdot}5$.
(3) For $T=16{\cdot}3$, $T''=13{\cdot}97$. By experiment, $T''=13{\cdot}8$.
(4) For $T=20{\cdot}3$, $T''=17{\cdot}4$. By experiment, $T''=17{\cdot}0$.

Applications of formula (5).

(1) For $T''=2{\cdot}3$, $T'=1{\cdot}61$. By experiment, $T'=1{\cdot}8$.
(2) For $T''=6{\cdot}5$, $T'=4{\cdot}55$. By experiment, $T'=4{\cdot}6$.
(3) For $T''=8{\cdot}5$, $T'=5{\cdot}95$. By experiment, $T'=5{\cdot}9$.
(4) For $T''=17$, $T'=11{\cdot}9$. By experiment, $T'=11{\cdot}3$.

Augmentations of boiling-temperatures, in degrees Centigrade, of different solutions of the salts in Group 4.

Weight of salt in 100 parts of water, k.	Corresponding boiling-temperatures of the different solutions of salt above that of pure water.			
	Nitrate of lime, T.	Carbonate of potassa, T'.	Value of T by formula $T=\frac{1}{33{\cdot}25}k^{1{\cdot}305}$.	Value of T by formula $T=\frac{7}{8}T'$.
0	0	0	0	0
16	1	1·33	1·12	1·16
32	2·7	3·12	2·76	2·73
48	4·7	5·30	4·70	4·63
64	7·0	7·8	6·97	6·82
80	9·2	10·5	9·15	9·18
96	11·63	13·5	11·62	11·81
112	14·2	17·0	14·20	14·87
176	25·0	29·5	25·61	25·81

Here the near coincidence of the results in the second and fourth columns shows that the relation of k and T may be very nearly represented by the formula

$$T=\frac{1}{33{\cdot}25}k^{1{\cdot}305}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

Moreover, the near coincidence of the results in the second and fifth columns shows that $T=\frac{7}{8}T'$ very nearly, or that in this group of salts the temperatures of ebullition, T and T′, have a constant ratio to each other for all equal weights of salt.

On a certain law connecting (approximately) *the boiling-temperatures of particular salts in the same group with the chemical equivalents of their bases, and in one instance with the equivalents of the entire salts.*

For the chlorides of sodium and barium we have found

$$\frac{T''}{T}=\frac{1}{3}, \text{ for all equal weights of the salts;}$$

but
$$\frac{\text{Equiv. of sodium}}{\text{Equiv. of barium}}=\frac{23{\cdot}31}{68{\cdot}66}=\frac{1}{3} \text{ very nearly.}$$

Hence it follows that, *for all equal weights of salt, the boiling-temperatures*, T″ and T, *of these two chlorides are* (approximately) *in the inverse ratio of the equivalents of their bases.*

Again, for the chlorides of sodium and potassium we have

$$\frac{T}{T'}=1{\cdot}5, \text{ for all equal weights of the salts;}$$

but
$$\frac{\text{Equiv. of potassium}}{\text{Equiv. of sodium}}=\frac{39{\cdot}26}{23{\cdot}31}=1{\cdot}68.$$

Hence it appears that the same law holds (approximately) true for these two salts.

In like manner, for the chlorides of calcium and strontium we have

$$\frac{T}{T'}=1{\cdot}85, \text{ for all equal weights of the salts;}$$

but
$$\frac{\text{Equiv. of strontium}}{\text{Equiv. of calcium}}=\frac{43{\cdot}75}{20}=2{\cdot}18.$$

In this case the approximation is not so close.

For the nitrates of soda and potassa we have

$$\frac{T'}{T}={\cdot}60, \text{ for all equal weights of the salts;}$$

but
$$\frac{\text{Equiv. of soda}}{\text{Equiv. of potassa}}=\frac{31{\cdot}31}{47{\cdot}26}={\cdot}66.$$

Hence it appears that the same law holds (approximately) true for these two salts.

For the nitrate of lime and the carbonate of potassa included in the fourth group, we have

$$\frac{\mathrm{T}}{\mathrm{T}'}=\frac{7}{8}=\cdot 87, \text{ for all equal weights of the salts;}$$

but

$$\frac{\text{Equiv. carb. of potassa}}{\text{Equiv. nitrate of lime}}=\frac{69}{82}=\cdot 84.$$

In this case, *for all equal weights of the salts, the boiling-temperatures,* T *and* T′, *are* (approximately) *in the inverse ratio of the equivalents of the entire salts.*

How far these laws may be extended to other substances future researches will determine; at the same time it must be observed that it is quite consistent with analogy to suppose that the chemical composition of a substance should affect the boiling-temperature of its solution. Although the law here indicated is not strictly true, yet it is sufficiently exact to warrant further inquiry, and the cases to which it is found to apply are too numerous to be referred to accidental coincidence.

On the boiling-point of diluted sulphuric acid.

With the exception of the sixth, seventh, and ninth experiments, the following experimental results were given by Dalton.

The per-centages of concentrated acid in the liquids were calculated from the observed specific gravities of the liquids by means of Ure's Table, given at p. 801, fourth edition, of his work on the Arts and Manufactures.

Augmentations of the boiling-temperatures, in degrees Fahrenheit, of diluted sulphuric acid at mean atmospheric pressure, containing different proportions of concentrated acid in 100 *parts, the specific gravity of the concentrated acid being* 1·846.

Weight of concentrated acid in 100 parts of the liquid, k.	Corresponding excess of boiling temperature above 212°, T.	Value of k by formula $k=14\cdot 15\,\mathrm{T}^{\frac{1}{3}}$.
100	363	100·90
98·21	333	98·08
93·66	289	93·55
90·53	261	90·43
86·82	223	85·81
76·88	150	75·18
48·00	40	48·39
41·00	28	42·96
34·00	16	35·66
0	0	0

Here the near coincidence of the results in the first and third columns shows that the relation between k and T may be approximately expressed by the formula

$$k=14{\cdot}15\,\mathrm{T}^{\frac{1}{3}}, \text{ or } \mathrm{T}=\left(\frac{k}{14{\cdot}15}\right)^{3}. \quad . \quad . \quad . \quad (7)$$

Hastings, April 1, 1861.

LI. *On Physical Lines of Force.* *By* J. C. Maxwell, *Professor of Natural Philosophy in King's College, London.*

[With a Plate.]

Part II.—*The Theory of Molecular Vertices applied to Electric Currents.*

[Concluded from p. 291.]

As an example of the action of the vortices in producing induced currents, let us take the following case:—Let B, Pl. V. fig. 3, be a circular ring, of uniform section, lapped uniformly with covered wire. It may be shown that if an electric current is passed through this wire, a magnet placed within the coil of wire will be strongly affected, but no magnetic effect will be produced on any external point. The effect will be that of a magnet bent round till its two poles are in contact.

If the coil is properly made, no effect on a magnet placed outside it can be discovered, whether the current is kept constant or made to vary in strength; but if a conducting wire C be made to *embrace* the ring any number of times, an electromotive force will act on that wire whenever the current in the coil is made to vary; and if the circuit be *closed*, there will be an actual current in the wire C.

This experiment shows that, in order to produce the electromotive force, it is not necessary that the conducting wire should be placed in a field of magnetic force, or that lines of magnetic force should pass through the substance of the wire or near it. All that is required is that lines of force should pass through the circuit of the conductor, and that these lines of force should vary in quantity during the experiment.

In this case the vortices, of which we suppose the lines of magnetic force to consist, are all within the hollow of the ring, and outside the ring all is at rest. If there is no conducting circuit embracing the ring, then, when the primary current is made or broken, there is no action outside the ring, except an instantaneous pressure between the particles and the vortices which they separate. If there is a continuous conducting circuit embracing the ring, then, when the primary current is made, there will be a current in the opposite direction through C; and when it is broken, there will be a current through C in the same direction as the primary current.

We may now perceive that induced currents are produced when the electricity yields to the electromotive force,—this force, however, still existing when the formation of a sensible current is prevented by the resistance of the circuit.

The electromotive force, of which the components are P, Q, R, arises from the action between the vortices and the interposed particles, when the velocity of rotation is altered in any part of the field. It corresponds to the pressure on the axle of a wheel in a machine when the velocity of the driving wheel is increased or diminished.

The electrotonic state, whose components are F, G, H, is what the electromotive force would be if the currents, &c. to which the lines of force are due, instead of arriving at their actual state by degrees, had started instantaneously from rest with their actual values. It corresponds to the *impulse* which would act on the axle of a wheel in a machine if the actual velocity were suddenly given to the driving wheel, the machine being previously at rest.

If the machine were suddenly stopped by stopping the driving wheel, each wheel would receive an impulse equal and opposite to that which it received when the machine was set in motion.

This impulse may be calculated for any part of a system of mechanism, and may be called the *reduced momentum* of the machine for that point. In the varied motion of the machine, the actual force on any part arising from the variation of motion may be found by differentiating the reduced momentum with respect to the time, just as we have found that the electromotive force may be deduced from the electrotonic state by the same process.

Having found the relation between the velocities of the vortices and the electromotive forces when the centres of the vortices are at rest, we must extend our theory to the case of a fluid medium containing vortices, and subject to all the varieties of fluid motion. If we fix our attention on any one elementary portion of a fluid, we shall find that it not only travels from one place to another, but also changes its form and position, so as to be elongated in certain directions and compressed in others, and at the same time (in the most general case) turned round by a displacement of rotation.

These changes of form and position produce changes in the velocity of the molecular vortices, which we must now examine.

The alteration of form and position may always be reduced to three simple extensions or compressions in the direction of three rectangular axes, together with three angular rotations about any set of three axes. We shall first consider the effect of three simple extensions or compressions.

Prop. IX.—To find the variations of α, β, γ in the parallelopiped x, y, z when x becomes $x+\delta x$; y, $y+\delta y$; and z, $z+\delta z$; the volume of the figure remaining the same.

By Prop. II. we find for the work done by the vortices against pressure,

$$\delta W = p_1 \delta(xyz) - \frac{\mu}{4\pi}(\alpha^2 yz\delta x + \beta^2 zx\delta y + \gamma^2 xy\delta z); \quad (59)$$

and by Prop. VI. we find for the variation of energy,

$$\delta E = \frac{\mu}{4\pi}(\alpha\delta\alpha + \beta\delta\beta + \gamma\delta\gamma)xyz. \quad . \quad . \quad . \quad . \quad . \quad (60)$$

The sum $\delta W + \delta E$ must be zero by the conservation of energy, and $\delta(xyz)=0$, since xyz is constant; so that

$$\alpha\left(\delta\alpha - \alpha\frac{\delta x}{x}\right) + \beta\left(\delta\beta - \beta\frac{\delta y}{y}\right) + \gamma\left(\delta\gamma - \gamma\frac{\delta z}{z}\right) = 0. \quad (61)$$

In order that this should be true independently of any relations between α, β, and γ, we must have

$$\delta\alpha = \alpha\frac{\delta x}{x}, \quad \delta\beta = \beta\frac{\delta y}{y}, \quad \delta\gamma = \gamma\frac{\delta z}{z}. \quad . \quad . \quad . \quad . \quad . \quad (62)$$

Prop. X.—To find the variations of α, β, γ due to a rotation θ_1 about the axis of x from y to z, a rotation θ_2 about the axis of y from z to x, and a rotation θ_3 about the axis of z from x to y.

The axis of β will move away from the axis of x by an angle θ_3; so that β resolved in the direction of x changes from 0 to $-\beta\theta_3$.

The axis of γ approaches that of x by an angle θ_2; so that the resolved part of γ in direction x changes from 0 to $\gamma\theta_2$.

The resolved part of α in the direction of x changes by a quantity depending on the second power of the rotations, which may be neglected. The variations of α, β, γ from this cause are therefore

$$\delta\alpha = \gamma\theta_2 - \beta\theta_3, \quad \delta\beta = \alpha\theta_3 - \gamma\theta_1, \quad \delta\gamma = \beta\theta_1 - \alpha\theta_2. \quad (63)$$

The most general expressions for the distortion of an element produced by the displacement of its different parts depend on the nine quantities

$$\frac{d}{dx}\delta x, \ \frac{d}{dy}\delta x, \ \frac{d}{dz}\delta x; \ \frac{d}{dx}\delta y, \ \frac{d}{dy}\delta y, \ \frac{d}{dz}\delta y; \ \frac{d}{dx}\delta z, \ \frac{d}{dy}\delta z, \ \frac{d}{dz}\delta z;$$

and these may always be expressed in terms of nine other quantities, namely, three simple extensions or compressions,

$$\frac{\delta x'}{x'}, \quad \frac{\delta y'}{y'}, \quad \frac{\delta z'}{z'}$$

along three axes properly chosen, x', y', z', the nine direction-cosines of these axes with their six connecting equations, which are equivalent to three independent quantities, and the three rotations θ_1, θ_2, θ_3 about the axes of x, y, z.

Let the direction-cosines of x' with respect to x, y, z be l_1, m_1, n_1, those of y', l_2, m_2, n_2, and those of z', l_3, m_3, n_3; then we find

$$\left.\begin{aligned} \frac{d}{dx}\delta x &= l_1{}^2\frac{\delta x'}{x'} + l_2{}^2\frac{\delta y'}{y'} + l_3{}^2\frac{\delta z'}{z'}, \\ \frac{d}{dy}\delta x &= l_1 m_1\frac{\delta x'}{x'} + l_2 m_2\frac{\delta y'}{y'} + l_3 m_3\frac{\delta z'}{z'} - \theta_3, \\ \frac{d}{dz}\delta x &= l_1 n_1\frac{\delta x'}{x'} + l_2 n_2\frac{\delta y'}{y'} + l_3 n_3\frac{\delta z'}{z'} + \theta_2, \end{aligned}\right\} \quad . \quad . \quad . \quad (64)$$

with similar equations for quantities involving δy and δz.

Let α', β', γ' be the values of α, β, γ referred to the axes of x', y', z'; then

$$\left.\begin{aligned} \alpha' &= l_1\alpha + m_1\beta + n_1\gamma, \\ \beta' &= l_2\alpha + m_2\beta + n_2\gamma, \\ \gamma' &= l_3\alpha + m_3\beta + n_3\gamma. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (65)$$

We shall then have

$$\delta\alpha = l_1\delta\alpha' + l_2\delta\beta' + l_3\delta\gamma' + \gamma\theta_2 - \beta\theta_3, \quad . \quad . \quad . \quad (66)$$

$$= l_1\alpha'\frac{\delta x'}{x'} + l_2\beta'\frac{\delta y'}{y'} + l_3\gamma'\frac{\delta z'}{z'} + \gamma\theta_2 - \beta\theta_3. \quad . \quad (67)$$

By substituting the values of α', β', γ', and comparing with equations (64), we find

$$\delta\alpha = \alpha\frac{d}{dx}\delta x + \beta\frac{d}{dy}\delta x + \gamma\frac{d}{dz}\delta x \quad . \quad . \quad . \quad . \quad (68)$$

as the variation of α due to the change of form and position of the element. The variations of β and γ have similar expressions.

Prop. XI.—To find the electromotive forces in a moving body.

The variation of the velocity of the vortices in a moving element is due to two causes—the action of the electromotive forces, and the change of form and position of the element. The whole variation of α is therefore

$$\delta\alpha = \frac{1}{\mu}\left(\frac{dQ}{dz} - \frac{dR}{dy}\right)\delta t + \alpha\frac{d}{dx}\delta x + \beta\frac{d}{dy}\delta x + \gamma\frac{d}{dz}\delta x. \quad . \quad (69)$$

But since α is a function of x, y, z and t, the variation of α may be also written

$$\delta\alpha = \frac{d\alpha}{dx}\delta x + \frac{d\alpha}{dy}\delta y + \frac{d\alpha}{dz}\delta z + \frac{d\alpha}{dt}\delta t. \quad . \quad . \quad . \quad . \quad (70)$$

Equating the two values of $\delta\alpha$ and dividing by δt, and remembering that in the motion of an incompressible medium

$$\frac{d}{dx}\frac{dx}{dt}+\frac{d}{dy}\frac{dy}{dt}+\frac{d}{dz}\frac{dz}{dt}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (71)$$

and that in the absence of free magnetism

$$\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (72)$$

we find

$$\frac{1}{\mu}\left(\frac{dQ}{dz}-\frac{dR}{dy}\right)+\gamma\frac{d}{dz}\frac{dx}{dt}-\alpha\frac{d}{dz}\frac{dz}{dt}-\alpha\frac{d}{dy}\frac{dy}{dt}+\beta\frac{d}{dy}\frac{dx}{dt}$$
$$+\frac{d\gamma}{dz}\frac{dx}{dt}-\frac{d\alpha}{dz}\frac{dz}{dt}-\frac{d\alpha}{dy}\frac{dy}{dt}+\frac{d\beta}{dy}\frac{dx}{dt}-\frac{d\alpha}{dt}=0. \quad . \quad (73)$$

Putting

$$\alpha=\frac{1}{\mu}\left(\frac{dG}{dz}-\frac{dH}{dy}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (74)$$

and

$$\frac{d\alpha}{dt}=\frac{1}{\mu}\left(\frac{d^2G}{dz\,dt}-\frac{d^2H}{dy\,dt}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (75)$$

where F, G, and H are the values of the electrotonic components for a fixed point of space, our equation becomes

$$\frac{d}{dz}\left(Q+\mu\gamma\frac{dx}{dt}-\mu\alpha\frac{dz}{dt}-\frac{dG}{dt}\right)-\frac{d}{dy}\left(R+\mu\alpha\frac{dy}{dt}-\mu\beta\frac{dx}{dt}-\frac{dH}{dt}\right)=0. \quad (76)$$

The expressions for the variations of β and γ give us two other equations which may be written down from symmetry. The complete solution of the three equations is

$$\left.\begin{array}{l} P=\mu\gamma\dfrac{dy}{dt}-\mu\beta\dfrac{dz}{dt}+\dfrac{dF}{dt}-\dfrac{d\Psi}{dx}, \\[2ex] Q=\mu\alpha\dfrac{dz}{dt}-\mu\gamma\dfrac{dx}{dt}+\dfrac{dG}{dt}-\dfrac{d\Psi}{dy}, \\[2ex] R=\mu\beta\dfrac{dx}{dt}-\mu\alpha\dfrac{dy}{dt}+\dfrac{dH}{dt}-\dfrac{d\Psi}{dz}. \end{array}\right\} \quad . \quad . \quad . \quad (77)$$

The first and second terms of each equation indicate the effect of the motion of any body in the magnetic field, the third term refers to changes in the electrotonic state produced by alterations of position or intensity of magnets or currents in the field, and Ψ is a function of x, y, z, and t, which is indeterminate as far as regards the solution of the original equations, but which may always be determined in any given case from the circumstances of the problem. The physical interpretation of Ψ is, that it is the *electric tension* at each point of space.

The physical meaning of the terms in the expression for the electromotive force depending on the motion of the body, may be made simpler by supposing the field of magnetic force uniformly magnetized with intensity α in the direction of the axis of x. Then if l, m, n be the direction-cosines of any portion of a linear conductor, and S its length, the electromotive force resolved in the direction of the conductor will be

$$e = \mathrm{S}(\mathrm{P}l + \mathrm{Q}m + \mathrm{R}n), \quad . \quad . \quad . \quad . \quad . \quad . \quad (78)$$

or

$$e = \mathrm{S}\mu\alpha\left(m\frac{dz}{dt} - n\frac{dy}{dt}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (79)$$

that is, the product of $\mu\alpha$, the quantity of magnetic induction over unit of area multiplied by $\mathrm{S}\left(m\frac{dz}{dt} - n\frac{dy}{dt}\right)$, the area swept out by the conductor S in unit of time, resolved perpendicular to the direction of the magnetic force.

The electromotive force in any part of a conductor due to its motion is therefore measured by the *number* of lines of magnetic force which it crosses in unit of time; and the total electromotive force in a closed conductor is measured by the change of the number of lines of force which pass through it; and this is true whether the change be produced by the motion of the conductor or by any external cause.

In order to understand the mechanism by which the motion of a conductor across lines of magnetic force generates an electromotive force in that conductor, we must remember that in Prop. X. we have proved that the change of form of a portion of the medium containing vortices produces a change of the velocity of those vortices; and in particular that an extension of the medium in the direction of the axes of the vortices, combined with a contraction in all directions perpendicular to this, produces an increase of velocity of the vortices; while a shortening of the axis and bulging of the sides produces a diminution of the velocity of the vortices.

This change of the velocity of the vortices arises from the internal effects of change of form, and is independent of that produced by external electromotive forces. If, therefore, the change of velocity be prevented or checked, electromotive forces will arise, because each vortex will press on the surrounding particles in the direction in which it tends to alter its motion.

Let A, fig. 4, represent the section of a vertical wire moving in the direction of the arrow from west to east, across a system of lines of magnetic force running north and south. The curved lines in fig. 4 represent the lines of fluid motion about the wire, the wire being regarded as stationary, and the fluid as having a motion relative to it. It is evident that, from this figure, we can trace the variations of form of an element of the fluid, as the form of the element depends, not on the absolute motion of the whole system, but on the relative motion of its parts.

In front of the wire, that is, on its east side, it will be seen that as the wire approaches each portion of the medium, that portion is more and more compressed in the direction from east to west, and extended in the direction from north to south; and since the axes of the vortices lie in the north and south direction, their velocity will continually tend to increase by Prop. X., unless prevented or checked by electromotive forces acting on the circumference of each vortex.

We shall consider an electromotive force as positive when the vortices tend to move the interjacent particles *upwards* perpendicularly to the plane of the paper.

The vortices appear to revolve as the hands of a watch when we look at them from south to north; so that each vortex moves upwards on its west side, and downwards on its east side. In front of the wire, therefore, where each vortex is striving to increase its velocity, the electromotive force upwards must be greater on its west than on its east side. There will therefore be a continual increase of upward electromotive force from the remote east, where it is zero, to the front of the moving wire, where the upward force will be strongest.

Behind the wire a different action takes place. As the wire moves away from each successive portion of the medium, that portion is extended from east to west, and compressed from north to south, so as to tend to diminish the velocity of the vortices, and therefore to make the upward electromotive force greater on the east than on the west side of each vortex. The upward electromotive force will therefore increase continually from the remote west, where it is zero, to the back of the moving wire, where it will be strongest.

It appears, therefore, that a vertical wire moving eastwards will experience an electromotive force tending to produce in it an upward current. If there is no conducting circuit in connexion with the ends of the wire, no current will be formed, and the magnetic forces will not be altered; but if such a circuit exists, there will be a current, and the lines of magnetic force and the velocity of the vortices will be altered from their state previous to the motion of the wire. The change in the lines of force is shown in fig. 5. The vortices in front of the wire, instead of merely producing pressures, actually increase in velocity, while those behind have their velocity diminished, and those at the sides of the wire have the direction of their axes altered; so that the final effect is to produce a force acting on the wire as a resistance to its motion. We may now recapitulate the assumptions we have made, and the results we have obtained.

(1) Magneto-electric phenomena are due to the existence of matter under certain conditions of motion or of pressure in every part of the magnetic field, and not to direct action at a distance between the magnets or currents. The substance producing these effects may be a certain part of ordinary matter, or it may be an æther associated with matter. Its density is greatest in iron, and least in diamagnetic substances; but it must be in all cases, except that of iron, very rare, since no other substance has a large ratio of magnetic capacity to what we call a vacuum.

(2) The condition of any part of the field, through which lines of magnetic force pass, is one of unequal pressure in different directions, the direction of the lines of force being that of least pressure, so that the lines of force may be considered lines of tension.

(3) This inequality of pressure is produced by the existence in the medium of vortices or eddies, having their axes in the direction of the lines of force, and having their direction of rotation determined by that of the lines of force.

We have supposed that the direction was that of a watch to a spectator looking from south to north. We might with equal propriety have chosen the reverse direction, as far as known facts are concerned, by supposing resinous electricity instead of vitreous to be positive. The effect of these vortices depends on their density, and on their velocity at the circumference, and is independent of their diameter. The density must be proportional to the capacity of the substance for magnetic induction, that of the vortices in air being 1. The velocity must be very great, in order to produce so powerful effects in so rare a medium.

The size of the vortices is indeterminate, but is probably very small as compared with that of a complete molecule of ordinary matter*.

(4) The vortices are separated from each other by a single layer of round particles, so that a system of cells is formed, the partitions being these layers of particles, and the substance of each cell being capable of rotating as a vortex.

(5) The particles forming the layer are in *rolling contact* with both the vortices which they separate, but do not rub against each other. They are perfectly free to roll between the vortices

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* The angular momentum of the system of vortices depends on their average diameter; so that if the diameter were sensible, we might expect that a magnet would behave as if it contained a revolving body within it, and that the existence of this rotation might be detected by experiments on the free rotation of a magnet. I have made experiments to investigate this question, but have not yet fully tried the apparatus.

and so to change their place, provided they keep within one *complete molecule* of the substance; but in passing from one molecule to another they experience resistance, and generate irregular motions, which constitute heat. These particles, in our theory, play the part of electricity. Their motion of translation constitutes an electric current, their rotation serves to transmit the motion of the vortices from one part of the field to another, and the tangential pressures thus called into play constitute electromotive force. The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connexion existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is, however, a mode of connexion which is mechanically conceivable, and easily investigated, and it serves to bring out the actual mechanical connexions between the known electro-magnetic phenomena; so that I venture to say that any one who understands the provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena.

The action between the vortices and the layers of particles is in part tangential; so that if there were any slipping or differential motion between the parts in contact, there would be a loss of the energy belonging to the lines of force, and a gradual transformation of that energy into heat. Now we know that the lines of force about a magnet are maintained for an indefinite time without any expenditure of energy; so that we must conclude that wherever there is tangential action between different parts of the medium, there is no motion of slipping between those parts. We must therefore conceive that the vortices and particles roll together without slipping; and that the interior strata of each vortex receive their proper velocities from the exterior stratum without slipping, that is, the angular velocity must be the same throughout each vortex.

The only process in which electro-magnetic energy is lost and transformed into heat, is in the passage of electricity from one molecule to another. In all other cases the energy of the vortices can only be diminished when an equivalent quantity of mechanical work is done by magnetic action.

(6) The effect of an electric current upon the surrounding medium is to make the vortices in contact with the current revolve so that the parts next to the current move in the same direction as the current. The parts furthest from the current will move in the opposite direction; and if the medium is a conductor of electricity, so that the particles are free to move in any direction, the particles touching the outside of these vortices will

be moved in a direction contrary to that of the current, so that there will be an induced current in the opposite direction to the primary one.

If there were no resistance to the motion of the particles, the induced current would be equal and opposite to the primary one, and would continue as long as the primary current lasted, so that it would prevent all action of the primary current at a distance. If there is a resistance to the induced current, its particles act upon the vortices beyond them, and transmit the motion of rotation to them, till at last all the vortices in the medium are set in motion with such velocities of rotation that the particles between them have no motion except that of rotation, and do not produce currents.

In the transmission of the motion from one vortex to another, there arises a force between the particles and the vortices, by which the particles are pressed in one direction and the vortices in the opposite direction. We call the force acting on the particles the electromotive force. The reaction on the vortices is equal and opposite, so that the electromotive force cannot move any part of the medium as a whole, it can only produce currents. When the primary current is stopped, the electromotive forces all act in the opposite direction.

(7) When an electric current or a magnet is moved in presence of a conductor, the velocity of rotation of the vortices in any part of the field is altered by that motion. The force by which the proper amount of rotation is transmitted to each vortex, constitutes in this case also an electromotive force, and, if permitted, will produce currents.

(8) When a conductor is moved in a field of magnetic force, the vortices in it and in its neighbourhood are moved out of their places, and are changed in form. The force arising from these changes constitutes the electromotive force on a moving conductor, and is found by calculation to correspond with that determined by experiment.

We have now shown in what way electro-magnetic phenomena may be imitated by an imaginary system of molecular vortices. Those who have been already inclined to adopt an hypothesis of this kind, will find here the conditions which must be fulfilled in order to give it mathematical coherence, and a comparison, so far satisfactory, between its necessary results and known facts. Those who look in a different direction for the explanation of the facts, may be able to compare this theory with that of the existence of currents flowing freely through bodies, and with that which supposes electricity to act at a distance with a force depending on its velocity, and therefore not subject to the law of conservation of energy.

The facts of electro-magnetism are so complicated and various, that the explanation of any number of them by several different hypotheses must be interesting, not only to physicists, but to all who desire to understand how much evidence the explanation of phenomena lends to the credibility of a theory, or how far we ought to regard a coincidence in the mathematical expression of two sets of phenomena as an indication that these phenomena are of the same kind. We know that partial coincidences of this kind have been discovered; and the fact that they are only partial is proved by the divergence of the laws of the two sets of phenomena in other respects. We may chance to find, in the higher parts of physics, instances of more complete coincidence, which may require much investigation to detect their ultimate divergence.

Note.—Since the first part of this paper was written, I have seen in Crelle's *Journal* for 1859, a paper by Prof. Helmholtz on Fluid Motion, in which he has pointed out that the lines of fluid motion are arranged according to the same laws as the lines of magnetic force, the path of an electric current corresponding to a line of axes of those particles of the fluid which are in a state of rotation. This is an additional instance of a *physical analogy*, the investigation of which may illustrate both electro-magnetism and hydrodynamics.

LII. *Remarks on* Mr. Cayley's *Note.* *By* G. B. JERRARD*.

DESIGNATING by u, v two rational n-valued homogeneous functions of the roots of the equation

$$x^m + \mathrm{A}_1 x^{m-1} + \mathrm{A}_2 x^{m-2} + \,..\, + \mathrm{A}_m = 0,$$

we find by Lagrange's theory that

$$\left.\begin{aligned} v &= \mu_{n-1} + \mu_{n-2}\, u + \mu_{n-3}\, u^2 + \,..\, + \mu_0\, u^{n-1} \\ u &= \nu_{n-1} + \nu_{n-2}\, v + \nu_{n-3}\, v^2 + \,..\, + \nu_0\, v^{n-1} \end{aligned}\right\} ; \quad . \quad . \quad (e)$$

in which $\mu_{n-1}, \mu_{n-2}, \,..\, \mu_0, \nu_{n-1}, \nu_{n-2}, \,..\, \nu_0$ are symmetrical functions of the roots of the original equation in x; and u, v depend separately on two equations of the nth degree

$$u^n + \alpha_1 u^{n-1} + \alpha_2 u^{n-2} + \,..\, + \alpha_n = 0, \quad . \quad . \quad . \quad (\mathrm{U})$$

$$v^n + \beta_1 v^{n-1} + \beta_2 v^{n-2} + \,..\, + \beta_n = 0, \quad . \quad . \quad . \quad (\mathrm{V})$$

$\alpha_1, \alpha_2, \,..\, \alpha_n, \beta_1, \beta_2,, \,..\, \beta_n$ being, as well as $\mu_{n-1}, \,..\, \nu_0$, symmetrical functions of the roots of the equation in x.

I ought to observe that any coefficient, μ_{n-s}, in the equation

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* Communicated by the Author.

(e_1) may take the form

$$\frac{\mathrm{M}_{n-s}}{\mathrm{D}},$$

M_{n-s}, D being expressive of whole functions, and D, which remains constant while M_{n-s} successively becomes $\mathrm{M}_{n-1}, \mathrm{M}_{n-2}, \ldots \mathrm{M}_0$, being such as not to vanish except when (U) has equal roots. We find in fact from the researches of Lagrange that

$$\mathrm{D} = \mathrm{F}(u_1)\ \mathrm{F}(u_2) \ldots \mathrm{F}(u_n),$$

where $\mathrm{F}(u) = n\,u^{n-1} + (n-1)\alpha_1\,u^{n-2} + (n-2)\alpha_2\,u^{n-3} + \ldots + \alpha_{n-1}$; $u_1, u_2, \ldots u_n$ denoting the n roots of the equation (U).

Of the meaning of the analogous expression

$$\frac{\mathrm{N}_{n-s}}{\mathrm{D}'}$$

which obtains in (e_2) for v_{n-s}, it is needless to speak. Indeed, having found one of the two equations (e), say (e_1), we may in general deduce the other, (e_2), from it by the method of the highest common divisor.

Let us now examine the following extract from Mr. Cayley's paper in the last Number (that for March) of the Philosophical Magazine.

"Writing," he says, "with Mr. Cockle and Mr. Harley,

$$\tau = x_\alpha x_\beta + x_\beta x_\gamma + x_\gamma x_\delta + x_\delta x_\epsilon + x_\epsilon x_\alpha,$$

$$\tau' = x_\alpha x_\gamma + x_\gamma x_\epsilon + x_\epsilon x_\beta + x_\beta x_\delta + x_\delta x_\alpha,$$

then $(\tau + \tau'$ is a symmetrical function of all the roots, and it must be excluded; but) $(\tau - \tau')^2$ or $\tau\tau'$ are each of them 6-valued functions of the form in question, and either of these functions is linearly connected with the Resolvent Product. In Lagrange's general theory of the solution of equations, if

$$f\iota = x_1 + \iota x_2 + \iota^2 x_3 + \iota^3 x_4 + \iota^4 x_5,$$

then the coefficients of the equation the roots whereof are $(f\iota)^5$, $(f\iota^2)^5$, $(f\iota^3)^5$, $(f\iota^4)^5$, and in particular the last coefficient $(f\iota\, f\iota^2\, f\iota^3\, f\iota^4)^5$, are determined by an equation of the sixth degree; and this last coefficient is a perfect fifth power, and its fifth root, or $f\iota\, f\iota^2\, f\iota^3\, f\iota^4$, is the function just referred to as the Resolvent Product.

"The conclusion from the foregoing remarks is, that *if the equation for* W *has the above property of the rational expressibility of its roots,* the equation of the sixth order resulting from Lagrange's general theory has the same property."

Here the question arises, Is it certain that $f\iota\, f\iota^2\, f\iota^3\, f\iota^4$ can, by means of Lagrange's theory, be expressed generally in rational terms of $(f\iota f\iota^2 f\iota^3 f\iota^4)^5$?*

Denoting those functions by u, v respectively, we have in this case

$$v=u^{n-1},$$
$$n=6,\ m=5.$$

Now on substituting u^{n-1} or u^5 for v in the equation (e_1), we see that (e_1) will merge into

$$\mu_5+\mu_4 u+\mu_3 u^2+\ldots+(\mu_0-1)u^5=0, \quad . \quad . \quad (e'_1)$$

wherein, since (U) is in general irreducible†, we must have

$$\mu_5=0,\quad \mu_4=0,\ldots\mu_0-1=0.$$

Accordingly, on combining the equations (e_1), (U), that is to say, (e'_1), (U), we find

$$u=\frac{0}{0};$$

the equation (e'_1) being, as we see, illusory.

We are therefore not permitted to assume that the resolvent product can in general—that is, when (U) has no equal roots—be expressed rationally in terms of its fifth power.

Again, it is generally possible to establish a rational communication between that fifth power and the function W, as is evidenced in this latter case from the non-existence of any illusory equation corresponding to (e'_1).

We are thus furnished, as will be seen, with a new confirmation of the validity of my method of solving equations of the fifth degree.

April 1861.

[To be continued.]

LIII. *On the Principles of Energetics.*—Part II. *Molecular Mechanics. By* J. S. Stuart Glennie, *M.A., F.R.A.S.*‡

Section I. *Physics.*

16. I PROCEED to consider the Principles of Energetics, or the science of Mechanical Forces, which seem to afford the bases of an explanation of physical motions. There is at present no attempt at a systematic elaboration of these principles, or mathematical application of them to the expression and expla-

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* It will be understood that our present inquiry relates to the possibility of expressing either of the two quantities u, v as a rational function of the other and of the elements, $A_1, A_2, \ldots A_m$. Thus $R(v, \ldots)$ is supposed to mean the same thing as $R(v, A_1, A_2, \ldots A_m)$; R indicating a rational function.

† As defined by Abel.

‡ Communicated by the Author. In reference to the first part of this paper, note that the word "rotation" in the fourth line from the bottom of p. 280 of the last Number for "revolution."

nation of phenomena. Previous to such an attempt, it is thought advisable to enunciate these principles in their most general form, and give them merely experimental illustration.

The principles to be set forth in this paper will lead me to remark on the physical theories recently published in this Journal by Prof. Challis and Prof. Maxwell. It will be found that as my theory refers attractions to differential conditions of stress and strain, of pressure and tension, among elastic bodies, it agrees rather with the molecular theory of the latter, than with the hydrodynamical theory of the former; that the point of fundamental difference from both is in the conception offered of Matter; and that on this point my theory is a development of the views to which experiment has led Mr. Faraday.

17. (I.) Atoms are mutually determining centres of pressure.

18. If this idea of an atom, as a body of any size, acting and reacting on another similar body by the pressure of the continued, infinitesimal, but similar particles of which each centre is an aggregate, be clearly conceived, it may be expressed in many different forms. I have, for instance, in the introductory paper spoken of a body thus conceived as a Centre of Lines of Pressure, or an Elastic System with a centre of resistance. But here, more clearly to express the idea in contrast with the fundamental hypotheses of Prof. Challis, an atom may be defined as a centre of an *emanating* elastic æther, the pressure of which is directly as the mass of its centre, and the form of which depends on the relative pressures of surrounding atoms. Thus, if you will, matter may be said to be made up of particles in an elastic æther. But that æther is not a uniform circumambient fluid, but made up of the mutually determining æthers (if you wish to give the outer part of the atom a special name) emanating from the central particles. And these central particles are nothing but what (endeavouring to make my theory clear by expressing it in the language of the theories it opposes) I may call ætherial nuclei.

"Hence," according to the conception of Faraday, "matter will be continuous throughout, and in considering a mass of it we have not to suppose a distinction between its atoms and any intervening space . . . . The atoms may be conceived of as highly *elastic*, instead of being supposed excessively hard and unalterable in form . . . . . With regard also to the *shape* of the atoms . . . . . . That which is ordinarily referred to under the term *shape* would now be referred to the disposition and relative intensity of the forces*."

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* Phil. Mag. 1844. vol. xxiv. p. 142; or Experimental Researches, vol. ii. p. 284. See also Phil. Mag. 1846, vol. xxviii. No. 188; or Experimental Researches, vol. iii. p. 447.

19. I venture to offer this conception of atoms, not as a mere hypothesis, but as a fundamental scientific principle. For there is this involved in it—that as a phenomenon is *scientifically* explained only when, and so far as, it is shown to be determined by other phenomena, the conception of Matter itself must be relative, and its parts be conceived as mutually determined.

Now Pressure is not only an ultimate idea, including all those qualities of Matter classed by the metaphysicians as the Secundo-primary, but is, unlike those, for instance, of Trinal extension, Ultimate incompressibility, Mobility, and Situation (the primary qualities), not an absolute, but a relative conception, and, as such, that on which alone can be founded a strictly scientific theory of material phenomena. For in the foundation of a theory based on the conception of the parts of matter as centres of pressure, there is nothing, properly speaking, hypothetical, as no absolute, intrinsic, or independent qualities of form, hardness, motion, &c. are postulated for atoms; and in their definition nothing more is done than an expression given to our ultimate and necessarily relative conception of matter.

In defining Atoms as Centres of Pressure, they are thus no less distinguished on the one hand from Centres of Force, than from the little hard bodies of the ordinary theories; for such Centres of Force are just as absolute and self-existent in the ordinary conception of them as are those little bodies. And in a scientific theory there can, except as temporary conveniences, be no absolute existences,—entities. Hence (Mechanical) Force, or the cause of motion, is conceived, not as an entity, but as a condition—the condition, namely, of a difference of Pressure*; and the figure, size, and hardness of all bodies are conceived as relative, dependent, and therefore changeable. There are thus no *absolutely* ultimate bodies.

20. But the full justification of advancing this conception of Atoms as a fundamental scientific principle, is found in the principles of the modern critical school of philosophy—in that especially of the relativity of knowledge. From such a point of view this principle cannot here be considered. I must limit myself, therefore, to a criticism of the opposed conception of atoms in a uniform æther, as developed by Prof. Challis, and to the attempt to show that, with the conception of atoms here offered, Prof. Maxwell's somewhat arbitrary hypothesis of vortices becomes unnecessary. For I agree with the former in thinking that, "after all that can be done by this kind of research, an independent and *à priori* theory . . . . . is still needed†;" and I observe that

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* See the first part of this paper, Phil. Mag. April, p. 275.

† "On Theories of Magnetism and other Forces, in reply to Remarks by Professor Maxwell," Phil. Mag. April, p. 253.

the object of the latter "is to clear the way for speculation*," rather than to advance a complete general theory.

In examining Professor Challis's "fundamental hypothetical facts," I hope to show that they are opposed (1) by Newton's Rules of Philosophizing; (2) by the principles of Metaphysics, as the modern Science of the Conditions of Thought; and (3) by the conceptions of matter, of the interaction of its different parts, and of motion and force, to which modern experimental researches have led.

21. "The fundamental hypothetical facts on which the [Prof. Challis's] theory rests are, that all substances consist of minute spherical atoms, of different, but constant, magnitudes, and of the same intrinsic inertia, and that the dynamical relations and movements of different substances are determined by the motions and pressures of a uniform elastic medium pervading all space not occupied by atoms, and varying in pressure in proportion to variations of its density†." Prof. Challis further says that he has "been guided by Newton's views on the ultimate properties of matter, especially as embodied in the *Regula Tertia Philosophandi* in the Third Book of the 'Principia';" and that he has merely "added to the Newtonian hypotheses two others, viz. that the ultimate atoms of bodies are spherical, and that they are acted upon by the pressure of a highly elastic medium‡." But on reference to the cited rule, no "Newtonian hypotheses" will be found, only a statement of the actual general qualities of matter. And, setting aside the "additional hypothesis" of sphericity, so far are the hypotheses of ultimate indivisible atoms, and these of an indeterminate number of different sizes, though of the same intrinsic inertia, Newtonian, that Newton says, "At si vel unico constaret experimento quod particula aliqua indivisa, frangendo corpus durum et solidum, divisionem pateretur; concluderemus vi hujus regulæ, quod non solum partes divisæ separabiles essent, sed etiam quod indivisæ in infinitum dividi possent." And Le Seur and Jacquier add in their note: "Hinc patet differentia Newtonianismi et hypotheseos Atomorum; Atomistæ necessario et metaphysicè atomos esse indivisibiles volunt, ut sint corporum unitates; Metaphysicam hanc quæstionem missam facit Newtonus . . . . omnem hac de re Theoriam Metaphysicam experimentis facile postponens." So that not only are Professor Challis's hypotheses as to "ultimate" bodies unwarranted by the rule he vouches, but he appears as of that very metaphysical school of Atomists, New-

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* "The Theory of Molecular Vortices applied to Magnetic Phenomena," Phil. Mag. March, p. 162.

† Phil. Mag. Feb. 1861, p. 106.

‡ Ibid. Dec. 1859, pp. 443 and 444.

ton's opposition to which is implied in his *Regula Tertia Philosophandi.*

No less clear is it that the postulate of two different kinds of matter, one with the qualities of inertia and elasticity, the other without the second of these qualities, is opposed by the very terms, not only of the third rule, "Qualitates corporum quæ intendi et remitti nequeunt, quæque corporibus omnibus competunt in quibus experimenta instituere licet, pro qualitatibus corporum universorum habendæ sunt," but by the terms of the first rule also, "causas rerum naturalium non plures admitti debere, quam quæ et *veræ sint* et earum phænomenis explicandis sufficiant."

22. Consider, secondly, how such hypotheses are judged by the modern principles of Metaphysics. For it is evident that the theories of every science must ultimately be judged by the results of a science, *τέχνη τεχνῶν καὶ ἐπιστήμη ἐπιστημῶν,* which, defining the conditions of knowledge, gives canons for the criticism of hypotheses. As this is no place for a metaphysical discussion, let it suffice to say that the theory of the relativity of cognition seems to justify the enouncement of this canon as a test of theories put forward as scientific. A scientific (physical) theory is founded on postulates of Relations, not on postulates of absolutely existing Entities. According to this rule it is evident that if, for instance, a theory requires an atom of a certain size or hardness, it can only be granted where it will stand as an expression of the relation between the forces distinguished at that point as internal and external; so if a certain elasticity, rotatory, or other motion of a body is required, the theory must take that elasticity or rotatory motion, not as an absolute property, but along with those relative conditions of other bodies which determine such elasticity or motion.

23. Without advancing any other defence, this canon may be justified by the consequence of its neglect. For a theory founded on postulates of absolute qualities—entities—must necessarily reason in a circle, accounting for phenomena by the same phenomena already assumed as ultimate.

Thus, though Professor Challis says that it would be contrary to principle "to ascribe to an atom the property of elasticity, because, from what we know of this property by experience, it is quantitative, and being most probably dependent on an *aggregation* of atoms, may admit of explanation by a complete theory of molecular forces*," he has no hesitation in ascribing elasticity to the particles of the æther, which, if anything, are as much atoms of matter as the "hard" atoms. But further, as to hardness, is it not the case that, "from what we know of this pro-

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* Phil. Mag. February 1860, p. 89.

perty by experience," it also "may admit of explanation by a complete theory of molecular forces?" Is it not therefore self-contradictory to attribute elasticity to one sort of matter, and justify its denial to another, on grounds which would equally apply to the quality by which this other sort of matter is distinguished from the former? And is not a theory fallacious which, if it attempts to explain relative elasticity or relative hardness, must do so by means of hypothetical and inconceivable, absolutely elastic, and absolutely hard entities?

24. But further, examining these fundamental facts by the results of the analysis of the qualities of matter, it will be seen that it is attempted to found a physical theory on the hypothesis of a physical matter acting on a mathematical matter. An elastic matter may be physically conceivable; but the interaction of such a matter and bodies without any physical quality, but mere abstractions of the metaphysical qualities deduceable from the respective conditions of occupying and being contained in space, cannot but be experimentally inconceivable.

25. Consider therefore, thirdly, the experimental conceptions to which these "hypothetical facts" are opposed; and (1) the conception of matter.

The conception of an absolute, or uniform, and universal elastic æther is opposed to the conception now formed of such similar entities as the old electric fluids, &c.; namely, that electricity is not an entity, but the expression of a certain physical relation between bodies, the electric state being kept up by, and entirely dependent upon, the bodies among which the electric body at any time is, or may be brought. Hence it should seem that if a theory requires an elastic æther, its elasticity must be conceived as relative or determined by the masses and distances of bodies, and hence evidently elasticity be conceived as "une des propriétés générales de la matière*"

And the notion of absolutely existing spherical atoms of different magnitudes not only begs as many separate creations of atoms as our fancy may suggest differences in their size, but is opposed to the conception of the transmutation of matter, generalized from the fact that we have in physics at least no creations, but perpetual changes dependent upon the ever-varying relations of bodies.

26. But (2) the idea of motions arising from the action of an elastic fluid on an inelastic absolutely hard and smooth body is opposed to all experimental conceptions of the interaction of the parts of matter. For not only do we seem to be led by experiment to a conception of the continuity of every part of matter by the cohesion of other bodies, so that it should seem to be impos-

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* Lamé, quoted in Part I. of this paper, Phil. Mag. April, p. 275.

sible for a fluid to act on a solid except through a mediate or immediate cohesion, but we are led by the Mechanical Theory of Heat to conceive every impulse communicated to a body to be productive of internal as well as external motion. It is of course necessary to make abstraction, out of the infinite number of effects, of the particular effect we may desire to consider. But an hypothesis of infinitely hard atoms not merely requires, in the consideration of the motion of such an atom, abstraction to be made of the interior relative motions also consequent on that difference of pressures which causes its external relative motion, but explicitly denies any internal motion.

It may be here noted that the Mechanical Theory of Heat would lead us to consider as "ultimate" no special class of bodies or molecules, except simply those, of the internal motions of which we do not in any particular theoretical, or cannot in an experimental, investigation take account. So any hardness may be called "infinite" if we do not consider the internal motions, or change of form, consequent on the application of a force which causes the translation of the body. But Professor Challis requires us to concede as physical facts what are properly but convenient mathematical abstractions.

27. Again (3), the conception of the origination of motion under such conditions as a uniform æther and discrete atoms therein, all of the same mass, is opposed to the experimental conception of motion as originating in difference in the mutual pressures of bodies. For these hypotheses give us the conditions of an eternal equilibrium. In the theory I propose, it is evident that anything short of an absolute equality in the masses and distances of the parts of matter implies infinite mutually determining motions.

And Professor Challis speaks of "the existence of the æther as the sole source of physical power*." But in a mechanical theory, as I have in the introductory, and in the first part of this, paper shown, nothing can be accurately spoken of as, of itself, "a source of power." "A source of power," a cause of motion, or a force, is simply the difference in relation to a third body of two resultant pressures upon it. And there can thus be no conceivable mechanical power in a fluid of which the elasticity is uniform, and on which the reaction of different solids within it should seem, by this theory, to be either nothing or the same.

28. Professor Challis further conceives the physical forces to be correlated as "modes of action of a single elastic medium." But I shall endeavour in the sequel to distinguish these correlations, and to show that they are either coexisting, mutually

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* Phil. Mag. February 1861, p. 106; and December 1859, p. 444.

causative, or sequential *molecular* motions. For the true application of Hydrodynamics would seem to be rather to actual solids and fluids, than to such "hypothetical facts" as form the bases of Professor Challis's Theory of Physics. How much work remains to be done in that true direction is well known; and the greatness of the results in the knowledge that might thereby be given of the formation of the solar and other sidereal systems, makes every contribution to Hydrodynamics, whatever the immediate particular application of the theorems, of peculiar value.

29. If, therefore, the true application of Hydrodynamics has been mistaken, and if a Hydrodynamical Theory of Physics must be founded on entities, hypothetical solids and fluids, to which such objections as the foregoing can be urged, there remains for us only a Molecular Theory of Physics. It is because to such a Theory all the most important modern physical researches seem to point, that I have thought it necessary to examine at such length the "fundamental facts" of Professor Challis. For the great result of modern science may be said to be the relative conception it gives of every phenomenon, and hence the demand that the fundamental facts of any theory be conceived, not as absolute and independent existences, but as expressions of relations. Now a Molecular is distinguished from a Hydrodynamical Theory of Physics in this,—that while in the latter the states and motions of bodies are explained by the action on them of some hypothetical, uniform, absolute, and all-pervading entity, in the former theory, physical states and motions are referred to differential conditions of stress and strain among the actual constituent molecules of bodies. Hence the evident experimental advantage of a Molecular Theory is, that its hypotheses being as to relative conditions of Molecular pressure and tension, transmission of motion, &c., they are more or less capable of experimental proof or disproof; and such a theory will be at least prolific in the suggestion of experiment. But where one deals with the waves or currents of an absolute æther acting on absolute atoms, a plausible theory may indeed be made out, but it is because its conceptions are fundamentally opposed to, so that its minor hypotheses cannot be checked by, experiment.

30. It is as the new fundamental principle of a Molecular Theory of Physics that I venture to suggest the above conception of Atoms. It is because, however convinced of the soundness of this principle, I am very diffident of my own powers of applying it, that I have gone at such length into its illustration, and the criticism of the opposed conception, as developed by Professor Challis. For should the mechanical explanation which, by

means of this principle, I propose to give of physical and chemical phenomena be found liable to serious objection, I hope the above remarks will have made this conception of Atoms sufficiently clear to be applied with greater success by others.

6 Stone Buildings, Lincoln's Inn,
April 11, 1861.

[To be continued.]

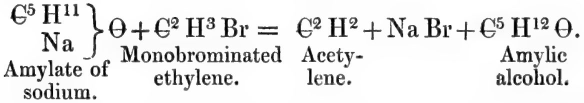
LIV. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 301.]

SAWITSCH* found that, under certain circumstances, monobrominated ethylene, $\bar{C}^2 H^3 Br$ (bromide of vinyle), parted with hydrobromic acid and became converted into acetylene, $\bar{C}^2 H^2$, the gas discovered by Edmund Davy and investigated by Berthelot†. He was led to investigate this deportment more minutely, and having tried the action of monobrominated ethylene on amylate of sodium, has found in it a mode of preparing this gas.

About 45 grammes of brominated ethylene were heated in a closed vessel with amylate of sodium. An abundant precipitate of bromide of sodium was formed, and the mass became liquid from the regenerated amylic alcohol. The vessel was then carefully cooled down in a freezing mixture and opened, when about 4 litres of a gas escaped, which, agitated with an ammoniacal solution of chloride of copper, gave an abundant red precipitate. When this precipitate was treated with dilute hydrochloric acid, it gave off about a litre of a colourless gas with a peculiar odour, and which burned with a very fuliginous flame. The analysis of this gas, and its combination with copper, left no doubt that it was acetylene.

Its formation may be thus expressed:—



$$\underset{\text{Amylate of sodium.}}{\left.\begin{matrix}\bar{C}^5 H^{11}\\ Na\end{matrix}\right\}\Theta} + \underset{\text{Monobrominated ethylene.}}{\bar{C}^2 H^3 Br} = \underset{\text{Acetylene.}}{\bar{C}^2 H^2} + Na\,Br + \underset{\text{Amylic alcohol.}}{\bar{C}^5 H^{12}\Theta}.$$

This reaction is important, as it will probably lead to the formation of a new series of hydrocarbons of the general formula $\bar{C}_n H_{2n-2}$, of which acetylene, $\bar{C}^2 H^2$, is the first member, from the hydrocarbons of the general formula $C_n H_{2n}$. In fact Sawitsch has subsequently‡ examined the action of monobromi-

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* *Bulletin de la Société Chimique*, p. 7.

† Phil. Mag. vol. xx. p. 196.

‡ *Comptes Rendus*, March 4, 1861.

nated propylene on ethylate of sodium, and has obtained a second member of the series, *allylene*, $\mathrm{Ꞓ^{3}\,H^{4}}$. The action is quite analogous to that in the former case: the gas is passed into an ammoniacal solution of copper, with which it forms a voluminous flocculent precipitate. This is decomposed, when heated, with the formation of a reddish flame; with concentrated acids it disengages a gas even in the cold.

Allylene is best obtained from this precipitate by the action of dilute aqueous hydrochloric acid, from which, when heat is applied, there is given off a colourless gas of a strong and disagreeable odour, but less so than that of acetylene. It burns with a fuliginous flame, and precipitates silver and mercury salts, the former grey and the latter white. These compounds are analogous to the copper compound, and, like it, are very unstable. The property of combining with an ammoniacal oxide of copper appears to be characteristic of this group, and will probably lead to the discovery of the higher members.

The formation of allylene may be thus expressed:—

$$\mathrm{Ꞓ^{2}\,H^{5}\,Na\Theta + Ꞓ^{3}\,H^{5}\,Br = Na\,Ƀr + Ꞓ^{3}\,H^{4} + Ꞓ^{2}\,H^{6}\,\Theta}.$$

Ethylate of sodium.	Brominated propylene.		Allylene.	Alcohol.

Miasnikoff[*], in some experiments with monobrominated ethylene, has also noticed a mode of the formation of acetylene, which gives a simple and elegant method of preparing this gas.

When the vapours of crude monobrominated ethylene, prepared in the ordinary way, are passed into an ammoniacal solution of nitrate of silver, a precipitate forms, which is at first yellow, but which quickly becomes converted into grey; at the same time an oil collects at the bottom of the vessel, which is brominated ethylene, and which can easily be separated by heating to 20°. When this bromide further acts upon a fresh portion of ammoniacal solution, it produces no change; but after being passed through a boiling concentrated solution of potash, it again acquires the property of forming this grey pulverulent deposit. By arranging the apparatus so that the vapours of brominated ethylene pass more than once through solution of potash, a considerable quantity of the pulverulent deposit can be formed.

This powder detonates strongly on the application of heat, percussion, or friction, and also by the action of chlorine or of gaseous hydrochloric acid. Treated with dilute hydrochloric acid, it gives a gas which burns with a fuliginous flame, and which reproduces the pulverulent precipitate. The analysis and the properties of this gas show that it is acetylene; and the analysis of the silver compound gives for it the formula

$$\mathrm{Ꞓ^{2}\,H^{2}\,Ag^{2}}.$$

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[*] *Bulletin de la Société Chimique*, p. 12.

From the mode of its preparation, monobrominated ethylene, in passing through strong potash, is simply resolved into acetylene and hydrobromic acid,

$$C^2 H^3 Br - H Br = C^2 H^2.$$

Monobrominated ethylene. Acetylene.

By a similar series of actions, M. Morkownikoff has prepared what appears to be the gas *allylene,* described by Sawitsch.

According to Heinz*, the best method of preparing glycolic acid is from monochloracetic acid, which, under the influence of alkalies, decomposes into an alkaline glycolate and into an alkaline chloride†.

The hydrate of glycolic acid may readily be obtained by the following method :—To the solution of the mixture of glycolate of soda and chloride of sodium obtained in the above reaction, a sufficient quantity of solution of sulphate of copper is added. The glycolate of copper, $C^4 H^3 Cu O^6$, which forms, is a difficultly soluble salt; it precipitates as a crystalline mass, and is readily obtained pure by washing. This salt is then diffused in a large quantity of water, the mixture raised to boiling, and saturated with sulphuretted hydrogen. When all the copper is precipitated it is filtered; and as the filtrate is brownish, from the solution of a small quantity of sulphide of copper, it is evaporated to a small volume, while a slow stream of sulphuretted hydrogen is passed through, and when now filtered, is obtained quite colourless.

By a series of operations analogous to those by which an alcohol of the ethyle series may be transformed into the next higher acid, Cannizaro‡ has obtained from anisic alcohol an acid homologous with anisic acid.

Anisic alcohol, $C^8 H^{10} \Theta^2$§, appears to contain the group $C^8 H^9 \Theta$, which plays the part of a monoatomic radical. When the chloride of this group, $C^8 H^9 \Theta, Cl$, was treated with cyanide of potassium, chloride of potassium was formed, and an oil which was the cyanide, $C^8 H^9 \Theta\, C N$. This oil was obtained in the impure state, and was treated directly with potash at the temperature of ebullition, by which a large quantity of ammonia was disengaged; and on the subsequent addition of hydrochloric acid in excess, an oil deposited which solidified to a crystalline mass. This consisted of the new acid which Cannizaro names *homoanisic*

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* Poggendorff's *Annalen,* January 1861.

† Phil. Mag. vol. xvi. p. 138.

‡ *Comptes Rendus,* vol. li. p. 606.

§ Phil. Mag. vol. xx. p. 294.

acid. It crystallizes in nacreous laminæ. Its formation may be thus expressed:—

$$\mathrm{C^8\,H^9\,O\,C\,N + K\,HO + H^2\,O = C^9\,H^9\,KO^3 + NH^3}.$$

Cyanide of anisyle. Homoanisate of potash.

Rossi* has applied to cuminic alcohol the same series of transformations, and has obtained a new acid homologous with cuminic acid. He prepared the chloride of cumyle, $\mathrm{C^{10}\,H^{13}\,Cl}$, by the action of hydrochloric acid on cuminic alcohol, and treated it with cyanide of potassium, by which means he obtained the corresponding cyanide. This crude cyanide of cumyle was boiled with strong caustic potash until its decomposition was complete; and to the mixture was then added hydrochloric acid in excess, by which the new acid was precipitated. On recrystallization it was obtained in small needles. Its formation is thus expressed:

$$\mathrm{C^{10}\,H^{13}\,CN + KHO + H^2\,O = C^{11}\,H^{13}\,KO^2 + NH^3}.$$

Cyanide of cumyle. Homocuminate of potash.

Homocuminic acid, $\mathrm{C^{11}\,H^{12}\,O^2}$, can be distilled without decomposition. It is difficultly soluble in cold water; but its solution reddens litmus, and decomposes the carbonates. Its salts are obtained by double decomposition: most of them crystallize well.

As the result of a lengthened investigation on filtration of the air in reference to fermentation, putrefaction, and crystallization, Schröder† is led to the following conclusions:—

1. A vegetable or animal can only be formed from living vegetable or animal organisms. *Omne vivum ex vivo.*

2. There is a series of phenomena of fermentation and putrefaction which arise solely from microscopic germs furnished by the atmosphere. These are more especially the formation of mould, of wine-yeast, of the lactic acid ferment, of the ferment which produces the decomposition of urine.

3. Vegetable or animal substances, boiled and closed while hot by means of cotton, remain in that condition quite protected against every kind of fermentation, putrefaction, or formation of mould, if all the germs in them capable of development are destroyed by boiling; for the germs which might reach them from the air are filtered out by the cotton.

4. The germs of most vegetable or animal substances are completely destroyed by simple boiling. A boiling for a short time at 100° C. is also sufficient to kill all germs furnished by the air.

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* *Comptes Rendus,* March 4, 1861.

† Liebig's *Annalen,* March 1861.

5. But milk, the yellow of egg, and meat contain germs which are not completely destroyed by a short boiling at 100°. But boiling at a higher temperature under a pressure of two atmospheres in the digestor, or long-continued boiling at 100°, is sufficient to kill even these germs.

6. The germs of milk, yellow of egg, and of meat, even when they have been submitted to a boiling at 100°, not continued, however, too long, are capable of developing themselves as a specific putrefaction ferment, and not unfrequently, at least in the yelk of egg, in the form of long but inert fibrils.

7. This specific putrefying ferment is of animal nature. It developes and increases at the expense of all albuminous substances. It is, however, incapable of increase under conditions which are all that are necessary to vegetable formations.

8. The crystallization of supersaturated solutions is commenced or induced by the action of the surface of solid bodies.

9. The induction necessary to set up the crystallization of the soluble hydrates from a supersaturated solution, is less than that necessary for the crystallization of the more difficultly soluble hydrates.

10. The surface of a crystal of the same nature exercises the strongest inducing action. Next to that comes the layer of air which forms on the surface of solid bodies. These coatings are destroyed by heating, continued wetting, or by cleaning, and are only formed slowly again in filtered air.

11. The crystallization of the more soluble hydrates from supersaturated solutions, which is set up even by a feeble induction, only experiences a feeble induction on the surface of the crystal of the same kind, and hence only progresses very slowly.

12. Supersaturated solutions closed with cotton keep for a long time unchanged, because the cotton filters all the solid particles from the air which gains access. Agitation has no action on the crystallization; it only induces it if supersaturated solutions are in contact with such places of the surface as are fitted to induce the crystallization.

By oxidizing cymole, $\text{Ꞓ}^{10}\,\text{H}^{14}$, with dilute nitric acid, Noad found that it was converted into toluylic acid, $\text{Ꞓ}^{8}\,\text{H}^{8}\,\Theta^{2}$, and oxalic acid; from cumole, $\text{Ꞓ}^{9}\,\text{H}^{12}$, Abel similarly obtained benzoic acid, $\text{Ꞓ}^{7}\,\text{H}^{6}\,\Theta^{2}$. Hence it might have been expected that toluole, $\text{Ꞓ}^{7}\,\text{H}^{8}$, by analogous treatment would yield a new acid, $\text{Ꞓ}^{5}\,\text{H}^{2}\,\Theta^{2}$, homologous with these.

This is, however, not the case, as experiments by Fittig* have shown. When toluole is oxidized by means of nitric acid, the process is different. There is no formation either of oxalic or of carbonic acid. A colourless acid is formed, almost insoluble in

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* Liebig's *Annalen*, February 1861.

cold water, and but slightly so in hot. It crystallizes from this solution in very small needles.

This body seems to have the composition of salicylic acid, $Ꞓ^7 H^6 \Theta^3$, without, however, being identical with it; for it does not give the well-known reaction with perchloride of iron, characteristic of salicylic acid. The baryta salt has the formula $Ꞓ^7 H^5 Ba \Theta^3$, and the silver salt $Ꞓ^7 H^5 Ag \Theta^3$.

As yet the glycols of the fatty acid series only are known. Wicke obtained a compound, $Ꞓ^{11} H^{12} \Theta^4$, which has the composition of an acetate of benzo-glycol, $\left.\begin{matrix} Ꞓ^7 H^{6\prime\prime} \\ (C^2 H^3 \Theta)^2 \end{matrix}\right\} \Theta^2$; but its properties differ from what might be expected of a glycol derivative, and it is rather analogous to the compounds which Geuther obtained by heating the aldehydes of the fatty acids with anhydrous acids. Beilstein and Seelheim* have made a series of experiments to prepare the glycol, $Ꞓ^7 H^8 \Theta^2$, of the aromatic series of acids, which stands to benzylic alcohol, $Ꞓ^7 H^8 \Theta$, and benzoic acid, $Ꞓ^7 H^6 \Theta^2$, in the same relation as ordinary glycol, $Ꞓ^2 H^6 O^2$, does to alcohol, $Ꞓ^2 H^6 \Theta$, and acetic acid, $Ꞓ^2 H^4 \Theta^2$. By the action of sulphuric acid on benzylic alcohol, they hoped to obtain the bibasic radical $Ꞓ^7 H^6$ in a manner analogous to that by which ethylene is obtained from ordinary alcohol. But the result of the above action was a resinous body, which, when treated with bromine in the expectation of obtaining $Ꞓ^7 H^6 Br^2$, gave off hydrobromic acid and underwent a complete decomposition.

The composition of the desired body would be identical with that of saligenine, which in many points resembles a biatomic alcohol. An attempt was made to prepare from it the biatomic chloride, $Ꞓ^7 H^6 Cl^2$, which did not give the desired result.

By the action of pentachloride of phosphorus, saligenine was resolved into saliretine, $Ꞓ^7 H^6 \Theta$, and water; at the same time a certain quantity of a chlorine compound was formed which seemed to contain the chloride $Ꞓ^7 H^6 Cl^2$, but which could not be separated in the pure state.

Saligenine was heated with anhydrous acetic acid in the expectation of forming acetate of saligenine; but instead of this, saliretine and acetic acid were formed:—

$$\underset{\text{Saligenine.}}{Ꞓ^7 H^8 \Theta^2} + \underset{\substack{\text{Acetic}\\ \text{anhydride.}}}{\left.\begin{matrix} Ꞓ^2 H^3 \Theta \\ Ꞓ^2 H^3 \Theta \end{matrix}\right\} \Theta} = \underset{\text{Saliretine.}}{Ꞓ^7 H^6 \Theta} + \underset{\text{Acetic acid.}}{2Ꞓ^2 H^4 \Theta^2}.$$

When sodium was added to a solution of saligenine in pure ether, hydrogen was evolved, and a white pulverulent precipitate

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* Liebig's *Annalen*, January 1861.

formed, which appeared to have the formula $\mathrm{Ꞓ}^{14}\mathrm{H}^{13}\mathrm{Na}\,\Theta^{2}$. Iodide of ethyle and chloride of acetyle act on this body, and form resinous bodies which could not be purified.

Saligenine dissolves in baryta water, forming a crystalline compound, $\mathrm{Ꞓ}^{7}\mathrm{H}^{9}\mathrm{Ba}\,\Theta^{3}=\mathrm{Ꞓ}^{7}\mathrm{H}^{7}\mathrm{Ba}\,\Theta^{2}+\mathrm{H}^{2}\Theta$.

By the action of pentasulphide of phosphorus, saligenine appears to become converted into an amorphous variety.

The law which regulates the contraction of solutions made it probable that the specific gravity of liquid ammonia must be less than that found by Faraday; and with a view of testing this point, Jolly[*] has made a redetermination of its specific gravity.

The liquid gas was prepared in the usual way, by heating ammoniacal chloride of silver in a bent closed tube, the empty end of which was much narrowed in one part and was provided with an arbitrary graduation. After the ammonia had been expelled from the ammoniacal chloride, and had been condensed in the cooled end of the tube, the part containing the liquefied gas was cooled down to a temperature of −86° in a mixture of solid carbonic acid and ether, and was melted off, which at this temperature could be done without danger.

The tube was now immersed in pounded ice, and the height of the liquid read off. A subsequent weighing gave the weight of the tube, together with the liquid gas and the compressed gas above the liquid.

The tube was now cooled down to −24° C., and the point softened, by which, as the tension of the gas at this temperature does not exceed two atmospheres, the gas escaped without danger or loss of glass. The opened tube was next transferred from the freezing mixture into pounded ice, upon which a violent ebullition commenced, after which, when terminated, and there was no more escape of NH^{3}, the tube was closed and weighed, by which the weight of the vapour at 0° was determined.

At this stage special experiments were made to ascertain if the ammonia was anhydrous and quite free from air, which was found to be the case. The tube was next weighed empty, and then calibrated.

From the data furnished by these various operations, Jolly found the specific gravity in three experiments to be

0·6239, 0·6261, 0·6193,

or in the mean 0·6231, which is one-sixth less than the number

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[*] Liebig's *Annalen*, February 1861.

found by Faraday, 0·73, and agrees with a determination made by Andréeff*, who obtained the number 0·6364.

Jolly also determined the coefficient of absorption of liquid ammonia, and found it to be for 1 degree 0·00155, which is about half as much as that of air.

LV. *On the Duration of the Spark which accompanies the discharge of an Electrical Conductor.* By P. L. Rijke, *Professor of Natural Philosophy at the University of Leyden*†.

1. WHEN a Leyden jar is discharged in the ordinary way, the spark produced may be considered as instantaneous; its duration, at least, is so short that it has hitherto been found impossible to measure it even approximately. This, however, is no longer the case when the charge has to traverse a body which offers any considerable resistance, as, for instance, a copper wire half a mile long. In fact, Mr. Wheatstone found‡ that the spark obtained through a copper wire of $\frac{1}{15}$th of an inch in diameter, and of the length above mentioned, lasted for about $\frac{1}{24,000}$th of a second.

2. This result, if Mr. Wheatstone had published it by itself, would probably have been explained simply on the ground that electricity required precisely this time, viz. $\frac{1}{24,000}$th of a second, to traverse the length of wire in question. This explanation would, however, have been, to say the least, incomplete, since in the same series of experiments Mr. Wheatstone proved that electricity really requires $\frac{1}{1,152,000}$th of a second to travel that distance; and in order to reconcile these results, he considered it necessary to have recourse to a new hypothesis, and to suppose "that the diameter of the wire was not sufficiently great to allow the charge to pass through it except in a *successive* manner."

3. In reflecting on this question, it appeared to me that the results obtained by the above-named illustrious physicist might be easily explained on known grounds; and that it was by no means necessary to have recourse to a hypothesis, in support of which it would be difficult to cite a single direct observation.

We shall see that it is, in fact, easy to prove *à priori* the following proposition:—

The time required by electricity to traverse a given conductor is much less than that required to discharge that conductor.

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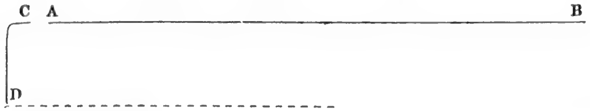
* Liebig's *Annalen*, vol. cx. p. 1.

† Communicated by the Author.

‡ "On some Experiments to Measure the Velocity of Electricity and the duration of Electrical Light," Phil. Trans. 1834.

Let A B be an isolated conductor of such length that a charge of electricity requires a perceptible time, say t'', to traverse it.



Let C D be another conductor much shorter than the first, and so placed that the extremities C and A of the two conductors are at the distance of several millims., the other extremity D of the second conductor being in communication with the ground.

Now suppose that at a given moment a certain quantity of electricity be communicated to the extremity B of the conductor A B. At the end of t'' this electricity will have spread over the whole surface of the conductor. At the same instant, supposing the tension sufficient, a luminous discharge will commence between A and C. We say will *commence,* admitting therefore that the discharge will occupy a certain time. In fact, in order that the conductor might discharge itself instantaneously, it would be necessary that all the electric fluid should be accumulated at A. This, however, is not the case; since at the moment when the discharge commences, the whole surface of the conductor is occupied, though unequally, with electricity. Now it is easy to see that the electricity, which at the moment we are considering is still at B, will only reach the other extremity t'' afterwards*; and even then it must be carefully noted that *all* the electricity at B will not have passed to A; only part will have done so, and the rest will have remained at B, of which a further portion will reach A $2t''$ after the discharge commences, and a third $3t''$ after the same epoch, and so on.

The above reasoning shows that, during the time occupied by the discharge, successive portions of electricity will arrive at A from B at equal intervals of time; and it is clear that the same will be the case with the electricity which at the beginning of the discharge was at any intermediate part of the conductor, only that the intervals of time will be shorter in proportion as the part considered is nearer A. It is therefore evident that there will be a *continuous* current of electricity towards this extremity, and that the discharge will therefore be equally *continuous.* The passage of the electricity from A to C will of course cease immediately the tension at A descends below a certain limit; but, on the other hand, it must not be forgotten

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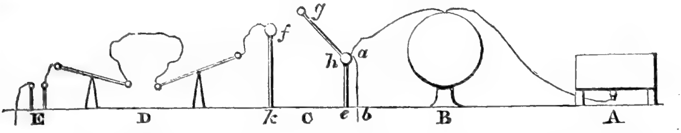
* If, as some physicists believe, the rapidity of the propagation of electricity diminishes with its density, then the electricity at B would require more than t'' to arrive at A.

that, in order to support the discharge already commenced, a much less considerable tension is required than is necessary to establish it in the first instance (Riess, *Die Lehre der Reibungselektricität,* vol. ii. p. 636). When electricity once commences to pass in the disruptive form from A to C, an expansion is produced in the intermediate layers of air, which amply suffices to explain the facility with which the same layers of air allow themselves to be traversed by electricity of much lower tension.

4. It is evident, however, that this expansion will depend on the quantity of electricity which in a given time passes from A to C. Consequently if the experiment be so conducted that, all other things being the same, a less quantity of the electric fluid passes from A to C, the expansion of the air will be less, and the discharge ought to cease sooner. But if the discharge ceases sooner, it will follow that the *residue* will be greater. Now it is easy to verify this theoretical conclusion.



B is a metallic sphere of 0·31 metre diameter. This sphere is connected by means of metal wires, on one side with a sinus electrometer A (Pogg. *Ann.* vol. cvi. p. 438), and on the other with an apparatus C, called by Riess an "Entladungsapparat" (Riess, *Die Lehre der Reibungselektricität,* p. 365), and which consists of a moveable metal rod *g h,* turning at *h* on a horizontal axis provided with a ratchet and clapper, which can be moved by pulling the silk cord *a b,* so that the knob *g* can always be brought to a fixed distance from the metal ball *f.*

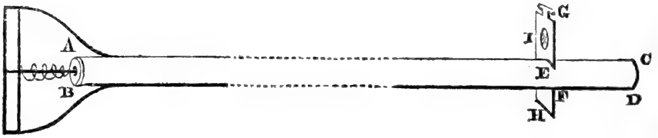
The ball *f* and the moveable rod *g h* are both supported on glass pillars, the former being united by means of a metal wire with one branch of the universal excitator D, of which the other branch communicates by similar means with one of the knobs of the spark-micrometer E. The other knob of the spark-micrometer is in connexion with the ground. Between the branches of the excitator D the substances are placed whose action on the residue we wish to determine.

I confined myself to the comparison of the actions of brass wire and a cord of hemp steeped in water. Both these conductors were 0·3 metre long, the diameter of the brass wire being ·08 millim., that of the hempen cord 2 millims. In each experiment the sphere B received the same charge measured by the electrometer. The clapper which supported the rod *g h* was then pulled by means of the silk cord *a b,* the knob *g* descended towards the ball *f*, establishing a metallic communication between A B C D and E, and at the same instant a spark passed between the two knobs of the spark-micrometer. The amount of residue was then determined by the electrometer. The results I obtained are collected in the following Table, in which the amount of residue is represented by R, that of the charge by L:—

Distance between the knobs of the micrometer.	Name of the substance placed between the branches of the excitator.	Deviation of the magnetic needle		Value of $\frac{R}{L}$.
		Before the discharge.	After the discharge.	
0·88 millim.	Hempen cord.	52° 20′	5° 18′	0·117
" "	Brass wire.	"	3 18	0·073
" "	Hempen cord.	"	4 42	0·103
1 millim.	Hempen cord.	64 26	7 6	0·137
" "	Brass wire.	"	3 56	0·076
" "	Hempen cord.	"	6 56	0·134

From this Table it is obvious that the residue is more considerable when the body through which the discharge takes place offers more resistance.

5. It is possible that the above theory may not at first meet with universal assent. I trust, however, that those physicists who hesitate to admit it will find their doubts dispelled on considering the following experiment:—A B C D is a hollow cylinder closed at the end C D and fitted at the other end A B with a sucker-valve opening outwards. Near the end C D there is a slide G H pierced with a large opening at I. The anterior portion A B E F of the tube must be supposed exhausted of air, while in the remaining part E F C D the air is in a state of great compression. Now suppose the slide G H suddenly depressed until the centre of the opening I coincides with the axis of the cylinder. It is evident that the air enclosed in E F C D will immediately begin to spread throughout the anterior portion of the tube until it reaches A B, where, if its tension be sufficiently great, it will commence escaping from the tube. If the air has taken t'' to arrive at A B, it will of course *begin* to escape t'' after the slide has been depressed; but no one will imagine that the



escape of the air will *cease* in t''. In fact it would suffice to repeat, *mutatis mutandis,* the reasoning in (3) to see that the escape must necessarily last more than t''.

P.S. In my note on the Inductive Spark, which was inserted in the December Number of this Journal, the expression *point* of light should have been *streak* of light.

LVI. *Note on the Historical Origin of the unsymmetrical Six-valued Function of six Letters.* *By* J. J. SYLVESTER, *Professor of Mathematics at the Royal Military Academy, Woolwich*[*].

THE discovery and first announcement of the existence of the celebrated function of six letters having six values, and not symmetrical in respect to all the letters, is usually assigned to my illustrious friend M. Hermite, to whom M. Cauchy expressly ascribes it in a memoir inserted in the *Comptes Rendus* of the Institut for December 8, 1845, p. 1247, and again, January 5, 1846, p. 30.

M. Cauchy adds that the conversation he held with M. Hermite on this subject excited in himself a lively desire to sound to its depths the question of permutations, and to develope the consequences to be deduced from the application of the principles relative thereto, which he had himself long previously laid down.

I was not at that date in the habit of consulting the *Comptes Rendus,* or I should at once have made the reclamation of priority which I now do, not from any unworthy motive of self-love in so small a matter, but out of regard to historic truth. It is a year or two since I first learnt that the origin of this function was usually referred to M. Cauchy or M. Hermite; but although aware that its existence was known to myself long previous to the dates quoted, I did not recollect that I had ever communicated it to the world through the medium of the press, and I therefore kept silence on the subject.

Turning over, a few days ago, for another purpose, the pages of a back volume of this Magazine, my eye chanced to alight on a footnote to a paper of my own inserted therein, under date of April 1844, "On the Principles of Combinatorial Aggregation," which I will take the liberty of quoting at length, as it proves incontestably the priority which I lay claim to.

"When the modulus is four, there is only one synthematic arrangement possible, and there is no indeterminateness of any kind; from this we can infer, *à priori,* the reducibility of a bi-

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[*] Communicated by the Author.

quadratic equation; for using ϕ, f, F to denote rational symmetrical forms of function, it follows that

$$F\left\{\begin{matrix} f(\phi\overline{a,b},\ \phi\overline{c,d}) \\ f(\phi\overline{a,c},\ \phi\overline{b,d}) \\ f(\phi\overline{a,d},\ \phi\overline{b,c}) \end{matrix}\right\}\ \text{is itself a rational symmetric function of } a,\ b,\ c,\ d.$$

Whence it follows that if a, b, c, d be the roots of a biquadratic equation, $f(\phi\overline{a,b}, \phi\overline{cd})$ can be found by the solution of a cubic: for instance, $(a+b)\times(c+d)$ can be thus determined, whence immediately the sum of any two of the roots comes out from a quadratic equation.

"To the modulus 6 there are fifteen different synthemes capable of being constructed. At first sight it might be supposed that these could be classed in natural families of three or of five each, on which supposition the equation of the sixth degree could be depressed; but on inquiry this hope will prove to be futile, not but what natural affinities do exist between the totals; but in order to separate them into families, each will have to be taken twice over; or in other words, the fifteen synthemes to modulus 6 being reduplicated, subdivide into six natural families of five each."

The six families above referred to (in which it is to be understood that $p.q$ and $q.p$ are identical in effect) are the following:—

$a.b$	$c.d$	$e.f$	$a.c$	$d.e$	$f.b$	$a.d$	$e.f$	$b.c$
$a.c$	$b.e$	$d.f$	$a.d$	$c.f$	$e.b$	$a.e$	$d.b$	$f.c$
$a.d$	$b.f$	$c.e$	$a.e$	$c.b$	$d.f$	$a.f$	$d.c$	$e.b$
$a.e$	$b.d$	$c.f$	$a.f$	$c.e$	$d.b$	$a.b$	$d.f$	$e.c$
$a.f$	$b.c$	$d.e$	$a.b$	$c.d$	$e.f$	$a.c$	$d.e$	$f.b$

$a.e$	$f.b$	$c.d$	$a.f$	$b.c$	$d.e$	$a.b$	$c.d$	$e.f$
$a.f$	$e.c$	$b.d$	$a.b$	$f.d$	$c.e$	$a.c$	$b.e$	$d.f$
$a.b$	$e.d$	$f.c$	$a.c$	$f.e$	$b.d$	$a.d$	$b.f$	$c.e$
$a.c$	$e.b$	$f.d$	$a.d$	$f.c$	$b.e$	$a.e$	$b.d$	$c.f$
$a.d$	$e.f$	$b.c$	$a.e$	$f.b$	$c.d$	$a.f$	$b.c$	$d.e$

And it will be observed that every two families have one, and only one, syntheme in common between them; and precisely in the same way as in the note above quoted, it is especially shown that the one single natural family

$$\left|\begin{matrix} a.b & c.d \\ a.c & b.d \\ a.d & b.c \end{matrix}\right|$$

gives rise to a function of four letters with only one value, so the six functions analogously formed with these six families obviously give rise to six functions, which change into one another when any interchange is effected between the letters which enter into them; so that any one of these is a function of six letters having only six values. I conceive that, after this reference, no writer on the subject wishing to specify the function in question would hesitate to call it after my name.

I may also take occasion to observe that, in connexion with my researches in combinatorial aggregation, long before the publication of my unfinished paper in the Magazine, I had fallen upon the question of forming a heptadic aggregate of triadic synthemes comprising all the duads to the base 15, which has since become so well known, and fluttered so many a gentle bosom, under the title of the fifteen school-girls' problem; and it is not improbable that the question, under its existing form, may have originated through channels which can no longer be traced in the oral communications made by myself to my fellow-undergraduates at the University of Cambridge long years before its first appearance, which I believe was in the 'Lady's Diary' for some year which my memory is unable to furnish.

In order to relieve this notice from the mere personal character which it may thus far appear to bear, I will state another question concerning the combinatorial aggregation of fifteen things which may serve as a pendant to the famous school-girl problem.

The number of triads to the base 15 is $\frac{15\times 14\times 13}{3\,.\,2\,.\,1}=5\times 91$. Let it be required to arrange these into 91 synthemes, in other words, to set out the walks of 15 girls for 91 days (say a quarter of the year) in such a manner that the same three shall never *all* come together more than once in the quarter. Of the various ways in which it is probable this problem may be solved, the following deserves notice. Let 15 letters be arbitrarily divided into 5 sets, viz.

$$a_1\, b_1\, c_1;\quad a_2\, b_2\, c_2;\quad a_3\, b_3\, c_3;\quad a_4\, b_4\, c_4;\quad a_5\, b_5\, c_5.$$

The sets as they stand will represent one of the 91 arrangements sought for, which I call the basic syntheme. The remaining 90 may be obtained as follows in 10 batches of 9 each. Write down the 10 index distributions following:—

1 2 3; 4 5
1 2 4; 3 5
1 2 5; 3 4
1 3 4; 2 5
1 3 5; 2 4
1 4 5; 2 3
2 3 4; 1 5
2 3 5; 1 4
2 4 5; 1 3
3 4 5; 1 2

Take any one of these distributions, as for instance 2 3 5; 1 4, and proceed as follows:—In respect of 2, 3, 5, conjugate the three sets $\begin{matrix} a_2 & b_2 & c_2 \\ a_3 & b_3 & c_3 \\ a_5 & b_5 & c_5 \end{matrix}$; and in respect of 1, 4, conjugate the two remaining sets $\begin{matrix} a_1 & b_1 & c_1 \\ a_4 & b_4 & c_4 \end{matrix}$.

From the ternary conjugation form the nine arrangements,

$a_2\ a_3\ a_5$	$b_2\ b_3\ b_5$	$c_2\ c_3\ c_5$
$a_2\ a_3\ b_5$	$b_2\ b_3\ c_5$	$c_2\ c_3\ a_5$
$a_2\ a_3\ c_5$	$b_2\ b_3\ a_5$	$c_2\ c_3\ b_5$
$a_2\ b_3\ a_5$	$b_2\ c_3\ b_5$	$c_2\ a_3\ c_5$
$a_2\ b_3\ b_5$	$b_2\ c_3\ c_5$	$c_2\ a_3\ a_5$
$a_2\ b_3\ c_5$	$b_2\ c_3\ a_5$	$c_2\ a_3\ b_5$
$a_2\ c_3\ a_5$	$b_2\ a_3\ b_5$	$c_2\ b_3\ c_5$
$a_2\ c_3\ b_5$	$b_2\ a_3\ c_5$	$c_2\ b_3\ a_5$
$a_2\ c_3\ c_5$	$b_2\ a_3\ a_5$	$c_2\ b_3\ b_5$

which call

$$L_1\ L_2\ L_3 \qquad L_4\ L_5\ L_6 \qquad L_7\ L_8\ L_9.$$

Again, from the binary conjugation, form the nine arrangements,

$a_1\ b_1\ c_4$	$a_4\ b_4\ c_1$
$a_1\ b_1\ b_4$	$a_4\ c_1\ c_4$
$a_1\ b_1\ a_4$	$c_1\ b_4\ c_4$
$a_1\ c_1\ c_4$	$a_4\ b_4\ b_1$
$a_1\ c_1\ b_4$	$a_4\ b_1\ c_4$
$a_1\ c_1\ a_4$	$b_1\ b_4\ c_4$
$b_1\ c_1\ c_4$	$a_4\ b_4\ c_1$
$b_1\ c_1\ b_4$	$a_4\ a_1\ c_4$
$b_1\ c_1\ a_4$	$a_1\ b_4\ c_4$

which call

$$M_1\ M_2\ M_3 \qquad M_4\ M_5\ M_6 \qquad M_7\ M_8\ M_9.$$

Now combine the L with the M system, each L with some M in any order whatever; the 9 combinations or appositions thus obtained will give a batch of 9 synthemes; and proceeding in like manner with each of the 10 distributions of the indices 1, 2, 3, 4, 5, we shall obtain 90 synthemes, which together with the basic syntheme complete the system required. The M system corresponding to any distribution of the indices is the system which contains the synthematic arrangement of the bipartite* triads which can be constituted out of six things, separated in two sets or parts, and is unique. The L system is *one* of those which represents the synthematic arrangement of the tripartite† triads of nine things separated into three sets or parts. I have set out above one in particular of these for the sake of greater clearness; but any other system having the same property will serve the same purpose, and a careful study will serve to show that the total number of L's corresponding to a given distribution of indices will be ( ) †. Consequently the total number of LM's that we can form for a given distribution will be ( ) ×1.2.3.4.5.6.7.8.9; and the number of *distinct* synthematic arrangements satisfying the given conditions corresponding to any assumed basic syntheme will be this number raised to the tenth power; and as this vastly exceeds the total number of permutations of fifteen things, we see, without even taking into consideration the diversity that may be produced by a change of the base, that this method must give rise to many distinct types of solution (arrangements being defined to belong to the same or different types, according as they admit or not of being deduced from each other by a permutation effected among their monadic elements). The common character of all these allotypical aggregations, and which serves to constitute them into a natural order or family, consists in their being derived from a base formed out of five sets, such that the monopartite triads corresponding to the base form one syntheme, and the other 90 synthemes each contain a conjugation of the tripartite triads belonging to three out of the five sets of the base with the bipartite triads belonging to the other two sets thereof. There is, moreover, no reason to suppose, or at all events no safe ground for affirming, that this family exhausts the whole possible number of types to which the arrangements satisfying the proposed condition admit of being reduced. A further question which I have somewhere raised, and which brings the two problems of the school-girls into *rapport*, is the following:—"To divide the system of 91 synthemes satisfying the conditions above stated into thirteen minor systems, each of which satisfies the conditions of the old problem, *i. e.* of containing all the duads that can be made out of the fifteen elements once and once only;" or to put the question in a more exact form, to exhibit thirteen systems,

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* See note at end of paper.

† Some day or another a new combinatorial calculus must come into being to furnish general solutions to the infinite variety of questions of *multifariousness* to which the theory of combinatorial aggregation, *alias* compound permutations, gives rise.

each satisfying this last condition, which shall together include between them all the triads that can be made out of the fifteen elements.

The reader would have reason to be dissatisfied with the author's reticence, were he to leave altogether unmentioned the synthematic aggregation of the *binomial* triads appertaining to the same three triliteral sets or *nomes*; but space forbids my doing more at present than giving one of these aggregates, and indicating the number and mode of generation of all from this one. It will readily be seen that any such aggregate will be made up of two sub-aggregates, which I shall call A and B respectively, of which one bears the same relation to the disposition of the nomes in the order 123 456 789, as the other to their disposition in the order 123 789 456. Thus we may take for our A and B the following, which will each contain 9 synthemes, the total number of synthemes in the two together being 18*:—

(A.)			(B.)		
124	567	893	127	894	763
125	468	739	128	795	436
126	459	783	129	786	453
134	568	279	137	895	246
135	469	278	138	796	245
136	457	289	139	784	256
234	569	187	237	896	154
235	467	189	238	794	156
236	458	179	239	785	146

The system of triads contained in A may be arranged in twelve different aggregates similar to the one given, and the same will be true for the triads in the B; so that the total number of the combined systems will be 144. All the permutations which leave A or B (separately considered) unaltered will form a natural group,—the theory of groups in this, as in every other case, standing in the closest relation to the doctrine of combinatorial aggregation, or what for shortness may be termed syntax. I have elsewhere given the general name of Tactic to the third pure mathematical science, of which order is the proper sphere,

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* Thus, since there is evidently one mononomial syntheme, the total number of synthemes of all three kinds will be $1+18+9=28=\frac{8\times7}{2}$, as it should be, the total number of triads being $\frac{9.8.7}{3.2}$, and $\frac{9}{3}$ of them going to a syntheme.

as is number and space of the other two. Syntax and Groups are each of them only special branches of Tactic. I shall on another occasion give reasons to show that the doctrine of groups may be treated as the arithmetic of ordinal numbers. With respect to the twelve varieties of the A or B aggregates, they may be obtained from the one given by combining the substitutions corresponding to the six permutations of the three constituents of one nome, as 7, 8, 9, with the permutation of any two constituents of another, as 5, 6. But I have said enough for my present purpose, which is to point out the boundless untrodden regions of thought in the sphere of order, and especially in the department of *syntax*, which remain to be expressed, mapped out, and brought under cultivation. The difficulty indeed is not to find material, of which there is a superabundance, but to discover the proper and principal centres of speculation that may serve to reduce the theory into a manageable compass.

I put on record (as a Christmas offering on the altar of science) for the benefit of those studying the theory of groups, or compound permutations (to which the prize shortly to be adjudicated by the Institute of France for the most important addition to the subject may tend to give a new impulse), and with an eye to the geometrical and algebraical verities with which, as a constant of reason, we may confidently anticipate it is pregnant, an exhaustive table of the monosynthematic aggregates of the trinomial triads that are contained in a system of three triliteral nomes. Let these latter be called respectively 1 2 3; 4 5 6; 7 8 9; then we have the annexed:—

Table of Synthemes of Trinomial Triads to Base 3 . 3.

(1.)	(2.)	(3.)	(4.)
147 258 369	147 258 369	147 258 369	147 258 369
148 259 367	148 259 367	148 259 367	148 259 367
149 257 368	149 257 368	149 257 368	149 267 358
157 268 349	157 268 349	157 269 348	157 268 349
158 269 347	158 269 347	158 267 349	158 269 347
159 267 348	159 267 348	159 268 347	159 247 368
167 248 359	167 249 358	167 248 359	167 248 359
168 249 357	168 247 359	168 249 357	168 249 357
169 247 358	169 248 357	169 247 358	169 257 348
(5.)	**(6.)**	**(7.)**	**(8.)**
147 258 369	147 258 369	147 258 369	147 258 369
148 267 359	148 267 359	148 269 357	148 269 357
149 268 357	149 257 368	149 257 368	149 267 358
157 249 368	157 268 349	157 268 349	157 268 349
158 269 347	158 269 347	158 249 367	158 249 367
159 248 367	159 248 367	159 267 348	159 247 368
167 259 348	167 259 348	167 248 359	167 248 359
168 257 349	168 249 357	168 259 347	168 259 347
169 247 358	169 247 358	169 247 358	169 257 348

The discussion of the properties of this Table, and the classification of the eight aggregates into natural families, must be reserved for a future occasion.

Note.—A triad is called tripartite if its three elements are culled out of three different parts or sets between which the total number of elements is supposed to be divided; bipartite if the elements are taken out of two distinct sets; unipartite if they all lie in the same set. The more ordinary method for the reduction of synthematic arrangements from a given base to a linear one which I employ, consists in the separate synthematization *inter se* of all the combinations of the *same* kind as regards the number of parts from which they are respectively drawn. Thus, *ex. gr.*, if the distribution of the $\frac{30\times 29\times 28}{6}$ triads to the base 30 into $\frac{29\times 28}{2}$ synthemes be required, this may be effected by dividing the 30 elements in an arbitrary manner into 15 parts, each part containing 2 elements. These 15 parts being now themselves treated as elements, are first to be conjugated as in the old 15-school-girl problem, and each of these 7 conjugations can be made to furnish 6 synthemes containing exclusively bipartite triads. The same 15 parts are then to be conjugated as in the new school-girl problem, and the 91 conjugations thus obtained will each furnish 4 synthemes, containing exclusively the tripartite triads. These bipartite and tripartite synthemes will exhaust the entire number of triads of both kinds, and accordingly we shall find

$$7\times 6+91\times 4=406$$
$$=\frac{29\times 28}{2}.$$

A syntheme, I need scarcely add, is an aggregate of combinations containing between them all the monadic elements of a given system, each appearing once only. In the more general theory of aggregation, such an aggregate would be distinguished by the name of a monosyntheme. A disyntheme would then signify an aggregate of combinations containing between them the duadic elements, each appearing once only, and so forth. Thus the old 15-school-girl question in my nomenclature would be enunciated under the form of a problem "to construct a triadic disyntheme, separable into monosynthemes to the base 15;" the new school question, as a problem "to divide the whole of the triads to base 15 into monosynthemes;" the question which connects the two, as a problem "to exhibit the whole of the triads to base 15 under the form of 13 disynthemes, each separated into 7 monosynthemes.

A question of a more general kind, and embracing this last, would be the problem of dividing the whole of the same system of triads into 13 disynthemes, without annexing the further condition of monosynthematic divisibility. So there is the simpler question of constructing a single disyntheme to the base 15 without any condition annexed as to its decomposability into 7 synthemes.

K, Woolwich Common,
December 1860.

LVII. *On the Galvanic Polarization of buried Metal Plates.*

By Dr. Ph. Carl*.

LAST year, in consequence of the disturbances which were observed in the telegraphic wires during the appearance of the northern lights, Professor Lamont was induced to contrive an apparatus at the observatory of Munich in order to examine more closely into the occasional motion of the earth's electricity, and to determine its magnitude and direction. For this purpose large zinc plates were buried on the north, south, east, and west sides of the observatory garden; the north plate being connected with the south, and the east with the west, by means of copper wires, which were brought into the observatory and connected with galvanometers. As Professor Lamont, in testing this apparatus, remarked certain phenomena which he attributed to galvanic polarization, it appeared to me advisable to subject the matter to a more careful examination, and to obtain more accurate measurements.

Through the wire that connects two of the above-mentioned zinc plates, a current, which I shall call the terrestrial current, is perpetually circulating, the intensity of which is indicated by a fixed deviation of the galvanometer. If a galvanic element be inserted in these conducting wires and again removed, then, provided it has caused no modification in the conductor, the needle of the galvanometer will return to its former position. But if, on the other hand, a state of galvanic polarization has been produced in the zinc plates, then the deviation of the needle of the galvanometer, after the removal of the element, will be greater or less than that exhibited by it originally, accordingly as the direction of the galvanic current has been opposite to, or the same as that of the terrestrial current.

On trial, the latter result exhibited itself so unmistakeably that no further doubt could be entertained of the occurrence of galvanic polarization. In order to measure the magnitude of the effect produced, I made use of a weak Daniell's cell, which I inserted

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* Translated by F. Guthrie from Poggendorff's *Annalen*, No. 10, 1860.

for five minutes in the wire conductor, and I thereby obtained the numbers exhibited in the following Table; in which G indicates the effect of the galvanic current, E that of the terrestrial current, and P that of the polarization expressed in divisions of the galvanometer. The effect of the polarization was observed 1 minute 30 seconds after the removal of the cell.

I. When the two currents passed in the same direction.

G+E.	P.	$\frac{P}{G+E}$.
129·8	3·4	0·026
129·3	3·4	0·026
123·0	2·0	0·021
108·2	2·4	0·022

II. When the current passed in opposite directions.

G−E.	P.	$\frac{P}{G-E}$.
124·5	5·4	0·043
122·7	5·1	0·041
107·4	3·8	0·035
109·2	3·5	0·031

From these experiments, it follows that the mean value of $\frac{P}{G+E}$ is 0·0237, that of $\frac{P}{G-E}$ 0·0375; so that when the galvanic and terrestrial currents pass in opposite directions, the polarization of the buried metal plates is greater than when they pass in the same direction.

Immediately on the removal of the cell, the effect of the galvanic polarization was greater by two or three divisions of the galvanometer scale than it was after the lapse of 1 minute 30 seconds; and after that period the effect still continued gradually to diminish. In order to exhibit the law of this diminution I subjoin the following Table:—

Time after the removal of the cell.	Deviation of the galvanometer needle.
1 minute.	23·0 divisions.
2 "	21·8 "
3 "	21·1 "
4 "	20·7 "
5 "	20·3 "
6 "	20·0 "
7 "	19·9 "
8 "	19·7 "
9 "	19·5 "
10 "	19·4 "

At the commencement of the experiment, the deviation of the galvanometer caused by the terrestrial current was read off at 19·2 divisions.

The above experiments disclose nothing at variance with the known laws of galvanism; but it nevertheless appeared to me advisable to make them known, as they afford a simple explanation of certain phenomena which Professor Thomson has described*, and which he seems to attribute to entirely different causes.

LVIII. *On Transcendental and Algebraic Solution.* By JAMES COCKLE, *M.A., F.R.A.S., F.C.P.S., Barrister-at-Law, of the Middle Temple*†.

LET $fx=0$ be an algebraic equation of the nth degree, all the coefficients of which are functions of one parameter a. By differentiation we obtain a result of the form

$$\frac{dfx}{da}=\mathrm{F}x\,.\,\frac{dx}{da}+\mathrm{f}x=0.$$

But, since F and f are rational functions,

$$\begin{aligned}\frac{dx}{da}&=-\frac{\mathrm{f}x}{\mathrm{F}x}\\&=-\frac{\mathrm{f}x\,.\,\mathrm{F}x_2\,.\,\mathrm{F}x_3\,.\,.\,\mathrm{F}x_n}{\mathrm{F}x\,.\mathrm{F}x_2\,.\,\mathrm{F}x_3\,.\,.\,\mathrm{F}x_n}=\mathrm{R}x,\end{aligned}$$

where R is a rational and integral function of x. And

$$\mathrm{R}x=\mathrm{A}_1x^{n-1}+\mathrm{A}_2x^{n-2}+\,.\,.+\mathrm{A}_n,$$

where A is a function of a. Moreover

$$\begin{aligned}\frac{d^2fx}{da^2}=\frac{d\mathrm{R}x}{da}&=\mathrm{F}_2x\,.\,\frac{dx}{da}+\mathrm{f}_2x\\&=\mathrm{F}_2x\,.\,\mathrm{R}x+\mathrm{f}_2x=\mathrm{R}_2x.\end{aligned}$$

Hence, repeating this process, we are conducted to the system

$$\begin{aligned}\frac{dx}{da}&=\mathrm{R}x=\mathrm{A}_1x^{n-1}+\mathrm{A}_2x^{n-2}+\,.\,.+\mathrm{A}_n,\\\frac{d^2x}{da^2}&=\mathrm{R}_2x=\mathrm{B}_1x^{n-1}+\mathrm{B}_2x^{n-2}+\,.\,.+\mathrm{B}_n,\\&\vdots\qquad\vdots\qquad\qquad\vdots\qquad\qquad\vdots\\\frac{d^{n-1}x}{da^{n-1}}&=\mathrm{R}_{n-1}x=\mathrm{G}_1x^{n-1}+\mathrm{G}_2x^{n-2}+\,.\,.+\mathrm{G}_n\,;\end{aligned}$$

and if we assign $n-2$ indeterminates $\lambda, \mu, .\,.\,\nu$ so as to satisfy

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* Report of the Twenty-ninth Meeting of the British Association for the Advancement of Science (Transactions of the Sections), p. 26.

† Communicated by the Author.

the $n-2$ conditions

$$\begin{aligned}
&\lambda A_1+\mu B_1+\ldots+\nu C_1+G_1=0,\\
&\lambda A_2+\mu B_2+\ldots+\nu C_2+G_2=0,\\
&\quad\vdots\qquad\quad\vdots\\
&\lambda A_{n-2}+\mu B_{n-2}+\ldots+\nu C_{n-2}+G_{n-2}=0,
\end{aligned}$$

we shall arrive at a linear differential equation,

$$\frac{d^{n-1}x}{da^{n-1}}+\nu\frac{d^{n-2}x}{da^{n-2}}+\ldots+\mu.\frac{d^2x}{da^2}$$
$$+\lambda.\frac{dx}{da}+L.x=M.$$

For, when the above conditions are satisfied, x^{n-1}, x^{n-2}, .. x^2, all the powers of x in short, save x itself, disappear; and ν, .. μ, λ, L, M are each of them known functions of a, inasmuch as A, B, .. C, G are known functions of a.

Thus the roots of any equation whereof the coefficients are functions of only one parameter may be expressed in terms of algebraic, circular, or logarithmic functions, and of integrals of algebraic functions. These integrals depend upon the quantity M.

To a form involving only one parameter, Mr. Jerrard has shown that the general quintic may be reduced. Its resolvent sextic may also be reduced to the same form.

Mr. Jerrard's memorable discoveries also show that the general sextic may be regarded as involving two parameters only. The general sextic leads us to the consideration of the equation

$$\delta fx=Fx.\delta a+\mathrm{f}x.\delta b=0,$$

where a and b are the independent parameters, of which the coefficients may be considered as functions, and δ is the characteristic of the Calculus of Variations.

If, in art. 62 of my "Observations," &c. (vol. xviii. p. 342), we take the suffixes of θ to the modulus 6, the equations become

$$\begin{aligned}
&\theta_1\theta_4+\theta_2\theta_0+\theta_3\theta_5=\gamma_5=r(x_5),\\
&\theta_1\theta_0+\theta_2\theta_5+\theta_3\theta_4=\gamma_1=r(x_1),\\
&\theta_1\theta_3+\theta_2\theta_4+\theta_5\theta_0=\gamma_2=r(x_2),\\
&\theta_1\theta_5+\theta_2\theta_3+\theta_4\theta_0=\gamma_3=r(x_3),\\
&\theta_1\theta_2+\theta_3\theta_0+\theta_4\theta_5=\gamma_4=r(x_4).
\end{aligned}$$

And this system has a certain relation to the formula

$$\theta_{a^2}\theta_{a^2+1}+\theta_{a^2+2}\theta_{a^2+5}+\theta_{a^2+3}\theta_{a^2+4},$$

which, taking the suffixes to the modulus 6, is, for all integral values of a, equal either to γ_1 or to γ_4. But all the values of γ are not thence evolved; and in order to obtain a convenient representation of the system, I avail myself of certain cyclical forms which may be given to it when one of the roots is supposed to become fixed.

The form here used will admit of an exceedingly simple representation if, throughout my "Observations," we replace θ_3 by θ_4, and *vice versâ*. This requires that in art. 48 (vol. xviii. p. 52) we write

$$\Theta_{f(\underset{\cdot\cdot}{14})}=\Theta'_3, \quad \Theta_{f(\underset{\cdot\cdot}{13})}=\Theta'_4,$$

a change of definition which I shall accordingly suppose to be made.

Further, I shall suppose that we replace θ_6 by θ_i, where i is an imaginary suffix defined by the congruence

$$i+a\equiv i \text{ (mod. 5)},$$

a being an integer. Or, if we agree to regard the infinite suffix ∞ as satisfying the congruence

$$\infty+a\equiv\infty \text{ (mod. 5)},$$

we may replace θ_6 by θ_∞. Lastly, I shall suppose the suffixes, after these changes, to be taken to the modulus 5.

The changes being made, it will be found that all the functions γ are deducible from the expression

$$\theta_i\theta_a+\theta_{a+1}\theta_{a+4}+\theta_{a+2}\theta_{a+3}$$

by writing, successively, 0, 1, 2, 3, 4 for a. In fact we have

$$\begin{aligned}
&\theta_i\theta_0+\theta_1\theta_4+\theta_2\theta_3=\gamma_2=r(x_2),\\
&\theta_i\theta_1+\theta_2\theta_0+\theta_3\theta_4=\gamma_1=r(x_1),\\
&\theta_i\theta_2+\theta_3\theta_1+\theta_4\theta_0=\gamma_0=r(x_0),\\
&\theta_i\theta_3+\theta_4\theta_2+\theta_0\theta_1=\gamma_3=r(x_3),\\
&\theta_i\theta_4+\theta_0\theta_3+\theta_1\theta_2=\gamma_4=r(x_4);
\end{aligned}$$

and if, in these equations, we change

$$\theta_i, \quad \theta_0, \quad \theta_3, \quad \theta_4, \quad \gamma_0, \quad x_0$$

into

$$\theta_6, \quad \theta_5, \quad \theta_4, \quad \theta_3, \quad \gamma_5, \quad x_5$$

respectively, we shall be reconducted to the system of art. 62 of my "Observations." More extensive changes in our fundamental formulæ and definitions would enable us to express the system with a greater concinnity between the suffixes of θ and those of x, and provided that, on the right of the last system, we interchange x_0 and x_2 the system may be deduced from the equation

$$\theta_i\theta_a+\theta_{a+1}\theta_{a+4}+\theta_{a+2}\theta_{a+3}=\gamma_a=r(x_a).$$

If, in the expression

$$\theta_i\theta_a+\theta_{a+1}\theta_{a+3}+\theta_{a+2}\theta_{a+4},$$

we make a equal to 0, 1, 2, 3, 4 successively, we find the following relations between it and the α and β functions of art. 70 of my "Observations" (vol. xviii. p. 508), viz.:—

$$\theta_i\theta_0+\theta_1\theta_3+\theta_2\theta_4=\alpha_0,$$
$$\theta_i\theta_1+\theta_2\theta_4+\theta_3\theta_0=\beta_1,$$
$$\theta_i\theta_2+\theta_3\theta_0+\theta_4\theta_1=\beta_2,$$
$$\theta_i\theta_3+\theta_4\theta_1+\theta_0\theta_2=\alpha_2,$$
$$\theta_i\theta_4+\theta_0\theta_2+\theta_1\theta_3=\beta_0;$$

and in like manner from*

$$\theta_i\theta_a+\theta_{a+1}\theta_{a+2}+\theta_{a+3}\theta_{a+4}$$

we deduce

$$\theta_i\theta_0+\theta_1\theta_2+\theta_3\theta_4=\alpha_4,$$
$$\theta_i\theta_1+\theta_2\theta_3+\theta_4\theta_0=\alpha_1,$$
$$\theta_i\theta_2+\theta_3\theta_4+\theta_0\theta_1=\beta_3,$$
$$\theta_i\theta_3+\theta_4\theta_0+\theta_1\theta_2=\beta_4,$$
$$\theta_i\theta_4+\theta_0\theta_1+\theta_2\theta_3=\alpha_3.$$

A contemplation of these systems of equations seems to lead to the conclusion that a certain equation of the fifteenth degree, which Mr. Jerrard supposes to be capable of decomposition into five cubics, is not irreducible, but composed of a quintic and a 10-ic factor.

I cannot think that Abel's conclusion is at all shaken by the researches of Mr. Jerrard. Of his cardinal proposition, the equation

$$\Xi_f-\mathrm{r}\{\mathrm{P}_{f(\underset{..}{\beta\epsilon})}\}=0$$

of art. 106 of his 'Essay,' Mr. Jerrard gives no proof. Directions to compare (ab) and (ac) are insufficient instructions for attaining a result fraught with difficulties so serious. For reasons already assigned, I believe the proposition to be erroneous, and incapable of proof. And with it the whole argument of Mr. Jerrard falls.

I need not the warning from my own oversight to restrain me from dwelling unduly upon what I believe to be an error of Mr. Jerrard's. But, with every recognition of his great claims to

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* Mr. Harley has completely determined these α and β functions; and I regret that he has not as yet published the whole of his investigations on quintics. The proposition of Mr. Jerrard, which Mr. Jerrard supposes that Mr. Harley has ignored, is, when considered in reference to processes which do not involve transformation to a soluble form, rather an axiom than a theorem. It may be stated thus: If Θ be equal to Θ', it is not in general equal to Θ''.

I would here add the expression of a hope that Mr. Harley, to whom I have communicated the above results on the theory of transcendental roots, may soon publish some developments of them to which he has been led.

the gratitude of mathematicians, it is scarcely possible to ignore the fact that Mr. Jerrard's hope (expressed at the conclusion of his paper of 1845), to discuss the resolution of the trinomial equation $x^5+A_4x+A_5=0$, has not been realized, and that little or no approach has yet been made towards its realization. Mr. Jerrard's subsequent researches on quintics seem to me, for reasons already adduced, to enhance rather than dispel any difficulties which arise upon the paper in question. It is perhaps to be desired that the mathematical world should be made acquainted with the whole of Mr. Jerrard's views on this important subject.

Does an absolutely impossible, or rootless, equation exist? MM. Terquem and Gilain have discussed this question in the *Nouvelles Annales de Mathématiques**, with reference to the equation

$$+\sqrt{1+x}+\sqrt{1-x}=1.$$

But this equation does not in reality raise the question under consideration. For (as I had occasion to write to Mr. Harley during last autumn) every one of the congeneric equations

$$\pm\sqrt{1+x}\pm\sqrt{1-x}=1$$

is soluble. And some one of the four values of x given by

$$x=\pm(\pm1)^2\tfrac{1}{2}\sqrt{3}$$

will satisfy any one of the above four congeners. I shall therefore again (S. 3. vol. xxxvii. p. 281) have recourse, for illustration, to the equations

$$1+\sqrt{x-4}+\sqrt{x-1}=0,$$
$$1-\sqrt{x-4}-\sqrt{x-1}=0,$$

each of which must, I think, be deemed impossible or rootless.

The only gleam of a solution of the last is, so far as I can see, one which springs from the assumptions

$$x=4(+1)^2+(-1)^2,$$
$$4=4(+1)^2,\ 1=(-1)^2,$$

while, for the first, we have the system

$$x=4(-1)^2+(+1)^2,$$
$$4=4(-1)^2,\ 1=(+1)^2.$$

But how can that be called a solution which depends upon a modification of the constants of a problem (compare S. 4. vol. iii. p. 439)? The safer conclusion seems to be that the two equations are rootless.

Midland Circuit, at Lincoln,
March 15, 1861.

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* See Mr. Wilkinson's *Notæ Mathematicæ*, Mechanics' Magazine, vol. lxii. p. 582.

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 233.]

May 24, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read :—

"On a new Method of Approximation applicable to Elliptic and Ultra-elliptic Functions." By C. W. Merrifield, Esq.

"On the Lunar-diurnal Variation of Magnetic Declination at the Magnetic Equator." By John Allan Broun, F.R.S., Director of the Trevandrum Observatory.

This variation, first obtained by M. Kreil, next by myself, and afterwards by General Sabine, presents several anomalies which require careful consideration, and especially a careful examination of the methods employed to obtain the results. The law obtained seems to vary from place to place even in the same hemisphere and in the same latitude, and this to such an extent, that, for example, when the moon is on the inferior meridian at Toronto it produces a minimum of westerly declination, while for the moon on the inferior meridian of Prague and Makerstoun in Scotland it produces a maximum of westerly declination. No two places have as yet given exactly the same result; though the result for each place has been confirmed by the discussion of different periods.

In order to obtain the lunar diurnal action, it has been usual to consider the magnetic declination at any time as depending on the sun's and moon's hour-angles and on irregular causes. Thus, if at conjunction, H_0 be the variation due to the sun on the meridian, and h_0 be that due to the moon on the meridian, H_1 the variation for the sun at 1^h, h_1 for the moon on the meridian of 1^h, and so on; it is supposed that we may represent the variations for a series of days by the following expressions, where the nearest values of h to the whole hour-angles are given :—

$$\begin{array}{llll} \text{1st day.} & H'_0+h'_0+x'_0 & H'_1+h'_1+x'_1 \ \ldots & H'_{23}+h'_{22}+x'_{23} \\ \text{2nd day.} & H''_0+h''_{23}+x''_0 & H''_1+h''_0+x''_1 \ \ldots & H''_{23}+h''_{21}+x''_{23} \\ & \vdots & & \\ n\text{th day.} & H^n_0+h^n_1+x^n_0 & H^n_1+h^n_2+x^n_1 \ \ldots & H^n_{23}+h^n_{23}+x^n_{23}, \end{array}$$

where x is due to irregular causes, and n is the number of days in a lunation nearly.

Summing these quantities we have approximately,

$$\Sigma H_0+\Sigma^0_{23}h+\Sigma x_0,\ \ \Sigma H_1+\Sigma^0_{23}h+\Sigma x_1,\ \ldots\ \Sigma H_{23}+\Sigma^0_{23}h+\Sigma x_{23} \quad \text{(A)}$$

and the means are,

$$H_0+\text{☾}+\frac{\Sigma x_0}{n},\quad H_1+\text{☾}+\frac{\Sigma x_1}{n},\ \ldots\quad H_{23}+\text{☾}+\frac{\Sigma x_{23}}{n}\ \ldots\ldots \quad \text{(B)}$$

Here the hourly means are affected by the constant due to the total action of the moon on all the meridians, and by variables depending on disturbing causes. If, on the other hand, we arrange the series as follows,

$$\begin{array}{lll}
H'_0+h'_0+x'_0, & H'_1+h'_1+x'_1, \; \dots & H''_0+h''_{23}+x''_0 \\
H''_1+h''_0+x''_1, & H''_2+h'_1+x'_2, \; \dots & H'''_1+h'''_{22}+x'''_1 \\
\vdots & \vdots & \vdots \\
H^{n-1}_{23}+h^{n-1}_0+x^{n-1}_{23}, & H^n_0+h^n_1+x^n_0, \; \dots & H^n_{23}+h^n_{23}+x^n_{23}.
\end{array}$$

Summing these quantities we have,

$$\Sigma^0_{23}H+\Sigma h_0+\Sigma^0_{23}x^{(1)},\ \Sigma^0_{23}H+\Sigma h+\Sigma^0_{23}x^{(11)}\dots\Sigma^0_{23}H+\Sigma h_{23}+\Sigma^0_{23}x^{(24)} \quad \text{(C)}$$

and for the means,

$$\Theta+h_0+\frac{\Sigma x^{(1)}}{n-1},\ \Theta+h_1+\frac{\Sigma x^{(11)}}{n-1}\ \dots\dots\ \Theta+h_{23}+\frac{\Sigma x^{(24)}}{n-1}. \quad \dots\dots \text{(D)}$$

In this case Θ is the mean of $n-1$ observations, of which 24 give the true means for the total solar influence, and the remaining $n-25$ being equally distributed through the hour-angles also give the mean approximately.

Instead, however, of combining the observations in this way, the following method has been preferred. Let, in the quantities (B),

$$(H_0)=H_0+\leftmoon+\frac{\Sigma x_0}{n}$$

$$(H_1)=H_1+\leftmoon+\frac{\Sigma x_1}{n}.$$

$$\vdots \qquad \vdots \quad \vdots \qquad \vdots$$

Then

$$\begin{array}{l}
H'_0+h'_0+x'_0-(H_0)=h'_0+(x'_0)=d'_0 \\
H''_1+h''_0+x''_1-(H_1)=h''_0+(x''_1)=d''_0 \\
\vdots \qquad \vdots \qquad \vdots \qquad \vdots \qquad \vdots \qquad \vdots \\
H^{n-1}_{23}+h^{n-1}_0+x^{n-1}_{23}-(H_{23})=h^{n-1}_0+(x^{n-1}_{23})=d^{n-1}_0.
\end{array}$$

Summing the last two columns, we have

$$\frac{\Sigma d_0}{n-1}=h_1+\frac{\Sigma^0_{23}(x')}{n-1}.$$

Similarly we obtain

$$\frac{\Sigma d_1}{n-1}=h_1+\frac{\Sigma^0_{23}(x'')}{n-1}, \text{ and so on.}$$

It will be observed that in these summations there are two assumptions; one, that the lunar diurnal law is constant throughout the lunation, or series of lunations, for which the means are obtained; or that the quantity $\leftmoon$ in the expressions (B) is constant. If this be not exact, then the quantity $\frac{\Sigma x}{n}$ will contain the variation due to this cause, and depend in part on the lunar hour-angle; so that the mean (H) which is employed in taking the differences will eliminate part of the lunar action and partially distort the law. The other assumption is that the mean solar diurnal variation, represented by $(H_0),(H_1)$ ...., is nearly constant throughout the period; for, if not, the differences due to such changes might be sufficient to mask any lunar law, the latter having a small range compared with the former.

Also it should be remarked that the means h_0, h_1, &c. are combined with the irregular effect $\frac{\Sigma^0_{23}(x)}{n-1}$. This effect, as far as it is due to disturbance, we know obeys a solar diurnal law; and if independent of lunar action, a sufficiently large series of observations might suffice to eliminate it, as combining with and forming part of the regular solar diurnal variation. If, however, the series is not very large and the irregular disturbance considerable compared with the variation sought, it may be desirable to omit or modify the marked irregularities.

As regards the first assumption referred to above, the results obtained hitherto seem to show the error to be small, and the only way to determine its amount will be to consider it zero in the first instance, and thereafter a more accurate calculus may be employed. For the second assumption, it is certain that the solar diurnal law varies considerably in some cases within a lunation. At the magnetic equator, for example, the law of magnetic declination is inverted within a few weeks near the equinoxes. The attempt to correct the error due to considerable change in the solar diurnal variation by taking the means, as has been done, from shorter periods than a lunation, is liable to the serious objection that the resulting hourly means are affected unequally by the lunar action, so that the sums (A) take the form,

$$\Sigma H_0+\Sigma^0_p h+\Sigma x_0, \quad \Sigma H_1+\Sigma^1_q h+\Sigma x_1 \ldots\ldots \Sigma H_{23}+\Sigma^{22} h+\Sigma x_{23},$$

where the second term in each expression is a variable. In the discussion to which I am about to allude, the following plan has been followed. The hourly means for the following series of weeks were taken, namely—

m_1 from 1st, 2nd, 3rd, and 4th weeks of the year.
m_2 ,, 2nd, 3rd, 4th, and 5th ,, ,,
m_3 ,, 3rd, 4th, 5th, and 6th ,, ,,
$\vdots$ $\vdots$ $\vdots$ $\vdots$ $\vdots$

The means of m_1 and m_2 were then taken as normals for the 3rd or middle week, of m_2 and m_3 as normals for the 4th week, and so on: these means were then employed for the differences from the corresponding hourly observations of the weeks to which they belonged.

With reference to the irregular effect, it is evidently desirable that we should know in the first instance whether it may not be a function of the lunar, as well as of the solar, hour-angle; for this end it is essential in the first instance to obtain the result including all the supposed irregular actions, and afterwards to eliminate these in the best manner possible.

In the discussion of the Makerstoun Observations I had substituted for certain observations, which gave differences from the mean beyond a fixed limit, values derived by interpolation from preceding and succeeding observations. General Sabine in his discussions has rejected wholly the observations which exceeded the limit chosen by him. The omission of observations accidentally or intentionally, and the taking of means without any attempt to supply the omitted observations by approximate values, require consideration.

Let m be the true hourly mean for an hour h, derived from the complete series of n observations; let m' be the mean derived from $n-1$ observations, one observation o being accidentally lost; then

$$m'=\frac{nm-o}{n-1},$$

$$m=m'-\frac{m'-o}{n}=m'-\frac{m-o}{n-1}.$$

If, however, we supply the omitted observation by an interpolation between the preceding and succeeding observations, and if the interpolated value be $o+x$, we have

$$m''=\frac{nm+x}{n},$$

$$m=m''-\frac{x}{n}.$$

The comparative errors of m' and m'' are therefore

$$\frac{o-m}{n-1} \text{ and } \frac{x}{n}.$$

We may for any given class of observation determine the mean values of these errors.

Example:—At Hobarton, in July 1846, the mean barometer for 3^h (Hobarton mean time) was 29·848 in., and the mean difference of an observation at that hour from the mean for the hour was 0·403 in.; if an observation had been omitted with such a difference, or for which $o-m=0{\cdot}403$ in., we should have an error in the resulting mean of $\frac{0{\cdot}403}{25}=0{\cdot}016$ in., and the error might have been twice as great had the observation with the greatest difference been rejected. If we now seek the error of m'', where the observation is interpolated, we shall find for the same month that the mean value of $x=0{\cdot}005$ in. nearly; whence the error $\frac{x}{n}=\frac{0{\cdot}005}{26}=0{\cdot}0002$ in. only, and the error would never exceed 0·001 in. A similar though less advantageous result will be found in all classes of hourly observations.

In the case where observations are rejected which differ from the mean for the corresponding hour more than a given quantity, let us suppose, to simplify the question, that the sums of $n-1$ out of n observations for each of two successive hours are each equal M, and that the observations for the same hours of the nth day are respectively $m'+l$ and $m'+l+x$, where $m'=\frac{\text{M}}{n-1}$, l is the limit beyond which observations are rejected, and x is the excess of the observation to be omitted. The means retaining all the observations are,

$$m_1 = \frac{M+m'+l}{n} = m' + \frac{l}{n},$$

$$m_2 = \frac{M+m'+l+x}{n} = m' + \frac{l+x}{n};$$

but if we reject the observation $m'+l+x$, we have

$$m_2' = \frac{M}{n-1} = m'.$$

It is assumed that $m_1'-m_2'=0$ (any other hypothesis of variation would give the same final result), and therefore the error of the change from the first hour to the second, when all the observations are retained, is $\frac{x}{n}$; but if the observation be rejected, the change is

$$m' + \frac{l}{n} - m' = \frac{l}{n}.$$

This error, therefore, will be greater than the other if $l > x$; so that the error in the resulting change from one hour to the next will be less by retaining an observation than by rejecting it, if the difference from the preceding observation be not greater than the difference from the hourly mean; that this will most frequently be the case will be obvious from the following fact:—At Makerstoun, in 1844, at 1 A.M. the number of observations which exceeded the monthly means by 3′ and less than double that, or 6′, was 99, while the whole number which exceeded by more than 6′ was only 16.

It will be evident also that the difference l of an observation from the corresponding hourly mean may not be due to irregular causes, or to causes which affect the changes from one hour to the next in a perceptible manner, but to gradual and regular daily change. If we examine the *daily* means most free from irregular or intermittent disturbance, we shall find that they vary plus or minus of the monthly mean; if the difference amounts to l in any case, then the whole observations of the day may be rejected though they follow the normal law. By taking a proper value of l this case may not happen frequently, but cases like the following will. At Hobarton the daily means of magnetic declination differ in some months from the monthly means by 2′·0 nearly; as the limit chosen by General Sabine is 2′·4, any observation in such days differing by 0′·4 from the normal mean would be rejected. The 25th and 26th days of March 1844 had been chosen by me as days free from magnetic disturbance, and following the normal law at Makerstoun (Mak. Obs. 1844, p. 339), yet the means of horizontal force for these days differed 0·00064 and 0·00075 from the monthly means; had the former quantity been the limit, all the observations on these days might have been rejected.

Altogether it appears to me that the method of rejecting observations beyond certain limits should not be employed at all, or if employed, only when interpolated observations are substituted; and that this interpolation should constitute a second part of the discussion, the first including all the observations*.

These considerations may appear somewhat elementary, but it is essential that results which present such anomalies as the lunar diurnal variation of magnetic declination should be obtained in a manner the most free from objection, even though the objections should touch on quantities of a second order compared with those obtained.

The discussion of which I now proceed to note the results, includes all the hourly observations without exception, made in the Trevandrum Observatory (within a degree and a half of the magnetic equator) during the five years 1854 to 1858; the second part of the discussion, in which days of great magnetic irregularity have been wholly rejected, not being completed, I shall reserve the details for a more formal communication to the Royal Society. The results obtained are as follows:—

1st. At the magnetic equator the lunar diurnal law of magnetic declination varies with the moon's declination and with the sun's declination.

2nd. This variation is so considerable that the attempt to combine all the observations to form the mean law for the *year* gives results that are not true for any period. Hence evidently the impossibility of relating the laws at different places. The so-called *mean* law for the year at Trevandrum obtained for the moon furthest north, on the equator going south, furthest south, and on the equator going north, consists of *three* maxima and *three* minima,—a result wholly false, excepting as an arithmetical operation due to combination of very different laws.

3rd. The lunar diurnal law varies chiefly with the position of the *sun*, the variation being comparatively small with the position of the *moon*.

4th. At the magnetic equator the range of the variations is *markedly* greatest in the months of January, February, November and December, or about perihelion.

The following results are derived after grouping the means for different positions of the moon in periods of six months, October to March, and April to September; they are therefore, for the reason given in the 2nd conclusion, not quite accurate; but the change of the law from month to month will be followed when the details are presented to the Society. The following will give a general idea of the changes:—

5th. *When the moon is furthest north.*

a. About perihelion. The lunar diurnal law of magnetic declination consists of two *maxima*† when the moon is near the upper and lower meridians, the maximum for the latter being much the great-

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* I should note here my belief that a peculiarity noticed by General Sabine in his discussions as requiring explanation, namely, that the excursions of the declination needle east and west in the lunar diurnal variation have very different magnitudes, is due to the rejection of observations, while the means by which the differences were obtained included the rejected quantities.

† The declination is easterly at Trevandrum, and the maxima indicate greater *easterly* declination.

est; of the two minima at intermediate epochs, that for the setting moon is the most marked.

b. About aphelion. The law consists of two nearly equal *minima* near the upper and lower transits: of the two intermediate maxima, that near the moonset is the most marked.

c. Thus the law about the winter solstice is inverted about the summer solstice, and the one law passes into the other at the epochs of the equinoxes, *exactly as for the solar diurnal variation*.

6th. *For the moon on the equator going south.*

a. About perihelion. The lunar diurnal law consists of two *nearly equal maxima* near the superior and inferior transits: of the two intermediate minima, the moonset minimum is by far the most marked.

b. About aphelion. The law consists of two *nearly equal minima* near the superior and inferior transits: of the two intermediate maxima, that near moonrise is by far the most marked.

c. In this case also the laws for the solstices are the opposite of each other, and the one law passes into the other near the epochs of the equinoxes.

7th. *For the moon furthest south.*

a. About perihelion. The lunar diurnal law consists of maxima near the upper and lower transits, that at the upper transit being by far the most marked: of the intermediate minima, that near moonset is the greater.

b. About aphelion. The law consists of two minima, the most marked at the inferior transit, the other about three hours before the superior transit; and of two equal maxima, one near moonrise, the other near the superior transit, but varying little till 3 hours before the inferior passage.

c. In this instance the inversion is not so complete as in the other cases; this, it is believed, will be found to be due to the fact that the change from one law to the other takes place after the vernal and before the autumnal equinox; so that in the means for six months, from which the above conclusions are drawn, the lunations following the law *a* are combined with those belonging to *b*.

8th. *The moon on the equator going north.*

a. About perihelion. The lunar diurnal law consists of two nearly equal maxima when the moon is near the superior and inferior meridians; of the two intermediate minima, that near moonrise is by far the most marked.

b. About aphelion. The law consists of two minima at the inferior and superior transits; and of two maxima, the greatest at moonset, the other between the meridians of 16^h and 21^h; between these points there is an inflexion constituting a slight minimum.

c. In this case also the opposition of the laws is sufficiently well marked; the only divergence from opposition being that due to the minor minimum about the meridian of 19^h, due, it is believed, as noted 7th *c*, to the partial combination of opposite laws in the aphelion half-year.

9th. It will be observed that the variations of the law with reference to the moon's declination *for any given period of the year*, consists chiefly in the difference of the relative values of the maxima and minima, the differences of epochs being small. Thus for perihelion, the moon furthest north, the principal maximum occurs at the inferior passage; the moon on the equator going south, the two maxima are nearly equal; the moon furthest south, the maximum at the superior passage is by far the greatest: on the equator going north, the two maxima are again nearly equal; and so on for other epochs.

10th. The moon's action is chiefly, if not wholly, dependent on the position of the sun, or (which is the same thing) on the position of the earth relatively to the sun; and the law of the lunar action at the magnetic equator resembles in some points that for the solar action at the same epochs. Thus about aphelion there is a *minimum* of easterly (maximum of westerly) declination produced by the lunar action, as well as by the solar action, for these two bodies near the superior meridian; whereas about perihelion both actions for the sun and moon near the superior meridian produce *maxima* of easterly declination. A like analogy holds for near the epochs of sunrise and moonrise.

June 14.—General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On the Nature of the Light emitted by heated Tourmaline." By Balfour Stewart, Esq., M.A.

Some months ago I had the honour of submitting to the Royal Society a paper on the light radiated by heated bodies, in which it was endeavoured to explain the facts recorded by an extension of the theory of exchanges.

Having mentioned the difficulty which I had in maintaining the various transparent substances at a nearly steady red heat for a sufficient length of time in experiments demanding a dark background, Professor Stokes suggested an apparatus by means of which this difficulty might be overcome; and it is owing to his kindness in doing so that I have been enabled to lay these results before the Society.

The apparatus consists of a thick, spherical, cast-iron bomb, about 5 inches in external and 3 inches in internal diameter—the thickness of the shell being therefore 1 inch. It has a cover removeable at pleasure. There is a small stand in the inside, upon which the substance under examination is placed, and when so placed it is precisely at the centre of the bomb. Two small round holes, opposite to one another, viz. at the two extremities of a diameter, are bored in the substance of the shell. If, therefore, the substance placed upon the stand be transparent, and have parallel surfaces, by placing these surfaces so as to front the holes, we are enabled to see through the substance, and consequently through the bomb. Let the bomb with the substance on the stand be heated to a good red heat, and then withdrawn from the fire and allowed to cool. It is evident that the cooling of the substance on the stand will proceed very slowly, as it is almost completely surrounded with a red-hot enclosure. It is also evident that, by placing the bomb in a dark room, we may view the transparent substance against a dark background. By this method of experimenting, therefore, the difficulty above alluded to is overcome.

Before describing the experiment performed on tourmaline, it may be well to state what result the theory of exchanges would lead us to expect when this mineral is heated, and we shall perceive at the same time the importance of the experiment with tourmaline as a test of the theory. When a suitable piece of tourmaline, with its faces cut parallel to the axis, is used to transmit ordinary light, the light which it transmits is nearly completely polarized, the plane of polarization depending on the position of the axis. The reason of this is, that if we resolve the incident light into two portions, one of which consists of light polarized in a plane perpendicular to the axis of the crystal, and the other of light polarized in a plane parallel to the same axis, nearly all the latter is absorbed, while a notable proportion of the former is allowed to pass.

Suppose now that such a piece of tourmaline is placed in a red-hot enclosure; the theory of exchanges, when fully carried out, demands that the light transmitted by the tourmaline, say in a direction perpendicular to its surface, *plus* the light radiated by the tourmaline in that direction, *plus* the small quantity of light reflected by the surface of the tourmaline in that direction, shall together equal in quantity and quality that which would have proceeded in the same direction from the wall of the enclosure alone, supposing the tourmaline to have been removed. Let us neglect the small quantity of light which is reflected from the surface of the tourmaline, and, standing in front of it, analyse with our polariscope the light which proceeds from it. This light consists of two portions, the transmitted and the radiated, both of which together ought to be equal in quality and intensity to that which would reach our polariscope from the enclosure alone were the tourmaline taken away. But the light which would fall on our polariscope from the enclosure alone would not be polarized; hence the whole body of light which falls upon it from the tourmaline, and which is similar in quality to the former, ought not to be polarized. Now part of this light, or that which is transmitted by the tourmaline, is polarized; hence it follows, in order that the whole be without polarization, that the light which is radiated should be partially polarized in a direction at right angles to that which is transmitted.

Another way of stating this conclusion is this. The light which the tourmaline radiates is equal to that which it absorbs, and this equality holds separately for light polarized in a plane parallel to the axis of the crystal, and for light polarized in a plane perpendicular to the same.

The experiment was made with a piece of brown tourmaline having a few opake streaks, procured from Mr. Darker of Lambeth. It was placed in a graphite frame between two circular holes made as above described in opposite sides of the bomb, the diameter of the holes being about $\frac{3}{10}$ths of an inch. On looking in at one of these holes you could thus see through the tourmaline and the opposite hole, or, in other words, see quite through the bomb. An arrangement was also made by which part of the tourmaline might be viewed with the graphite behind it.

The apparatus thus prepared was heated to a red or yellow heat in the fire, placed on a brick in a dark room, and the tourmaline viewed by a polariscope which Mr. Gassiot kindly lent me. The following was the appearance of the experiment:—

Without the polariscope the transparent parts of the tourmaline were slightly less radiant than the field around them. When the polariscope was used, the light from the transparent portions of the tourmaline was found to vary in intensity as the instrument was turned round. No change of intensity could be observed in the light radiated by the opake streaks of the tourmaline, or by the graphite.

The light from the transparent portions was therefore partially polarized. The polariscope was then brought to its darkest position, and a light from behind allowed to pass through the tourmaline. The light was distinctly visible in this position, but by turning round the polariscope about 90° it became eclipsed. The mean of four sets of experiments made the difference between the position of darkness for the two cases 88½°. It appears, therefore, that the light radiated by the tourmaline was partially polarized in a plane at right angles to that which was transmitted by it. It was also ascertained that the light from the tourmaline which had the graphite behind it gave no trace of polarization.

LX. *Intelligence and Miscellaneous Articles.*

ON THE MOTION OF THE STRINGS OF A VIOLIN.

BY PROFESSOR H. HELMHOLTZ.

I HAVE been studying for some time the causes of the different qualities of sound; and as I found that those differences depended principally upon the number and intensity of the harmonic sounds accompanying the fundamental one, I was obliged to investigate the forms of elastic vibrations performed by different sounding bodies. Among such vibrations, the form of which is not yet exactly known, the vibrations of strings excited by the bow of a violin are peculiarly interesting. Th. Young describes them as very irregular; but I suppose that his assertion relates only to the motions which remain after the impulse of the bow has ceased. At least, I myself found the motion very regular as long as the bow is applied near one end of the string, in the regular way commonly followed by players of the violin. I used a method of observing very similar to that of Lissajoux. Already, without the assistance of any instruments, one can see easily that a string moved by the bow vibrates in one plane only—the same plane in which the string itself and the hairs of the bow are situated. This plane was horizontal in my experiments. The string was powdered with starch, and strongly illuminated. One of the little grains of starch, looking like a bright point, was observed by a vertical microscope, the object lens of which was fixed to one of the branches of a tuning-fork. The fork, making 120 vibrations in the second, was placed between the branches of a horseshoe electro-magnet, which was magnetized by an interrupted electric current, the number of interruptions being itself 120 in the second. In that way the fork was kept vibrating for as long a time as I desired. The lens of the microscope vibrated in a direction parallel to the string, and therefore perpendicular to its vibrations. The string I used was the second string of a violin, answering to the note A, tuned a little higher than common, to 480 vibrations, and therefore it performed four vibrations for every one of the tuning-fork. Looking through the microscope, I observed the grain of starch describing an illuminated curved line, the horizontal abscissæ of which corresponded to the deviations of the tuning-fork, and the vertical ordinates to the deviations of the string. I found it a matter of importance to use a violin of most perfect construction, and I was fortunate in getting a very fine instrument of Guadanini for these experiments. On the common instruments of inferior quality I could not keep the curve constante nough for numbering the little indentures which I shall describe afterwards, although the general character of the curve was the same on all the instruments I tried: the curve used to move by jerks along the line of abscissæ; and every jerk was accompanied by a scratching noise of the bow. On the contrary, with the Italian instrument, and after some practice, I got a curve completely quiescent as long as the bow moved in one direction, the sound being very pure and free from scratching.

We may consider the motion of the string as being compounded of two different sets of vibrations, the first of which is the principal motion as to magnitude. Its period is equal to the period of the fundamental sound of the string, and it is independent of the situation of the point where the bow is applied. The second motion produces only very small indentures of the curve. Its period of vibration answers to one of the higher harmonics of the string. It is known that a string, when producing only one of its higher harmonics, is divided into several vibrating divisions of equal length, being separated by quiescent points, which are called nodes. In all the nodes of the second motion of the string in the compound result at present considered, the principal motion appears alone; and also in the other points of the string the indentures corresponding to the second motion are easily obliterated, if the line of light is too broad.

The principal motion of the string is such that every point of it goes to one side with a constant velocity, and returns to the other side with another constant velocity.

Plate V. fig. 7 represents four such vibrations, corresponding to one vibration of the fork. The horizontal abscissæ are proportional to the time, the vertical ordinates to the deviation of the vibrating point. Every vibration is formed on the curve by two straight lines. The curve is not seen quite in the same way through the microscope, because there the horizontal abscissæ are not proportional to the time but to the sine of the time. It must be imagined that the curve (fig. 7) is wound up round a cylinder, so that the two ends of it meet together, and that the whole is seen in perspective from a great distance; thus it had the real appearance of the curve, as represented in two different positions in fig. 8. If the number of vibrations of the string is accurately equal to four times the number of the tuning-fork, the curve appears quietly keeping the same position. If there is, on the contrary, a little difference of tuning, it looks as if the cylinder rotated slowly about its axis, and by the motion of the curve the observer gets as lively an impression of a cylindrical surface, on which it seems to be drawn, as if looking at a stereoscopic picture. The same impression may be produced by combining, stereoscopically, the two diagrams of fig. 8.

We learn, therefore, by these experiments,—

1. That the strings of a violin, when struck by the bow, vibrate in one plane.

2. That every point of the string moves to and fro with two constant velocities.

These two data are sufficient for finding the complete equation of the motion of the whole string. It is the following:—

$$y = \mathrm{A}\Sigma\left\{\frac{1}{n^2}\sin\left(\frac{\pi\, n\, x}{l}\right)\sin\left(\frac{2\,\pi\, n\, t}{\mathrm{T}}\right)\right\} \dots\dots\dots (1)$$

y is the deviation of the point whose distance from one end of the string is x; l, the length of the string; t, the time; T, the duration of one vibration; A, an arbitrary constant; and n, any whole number; and all values of the expression under the sign Σ, got in that way, are to be summed.

A comprehensive idea of the motion represented by this equation may be given in the following way:—Let *a b*, fig. 9, be the equilibrium position of the string. During the vibration its forms will be similar to *a c b*, compounded of two straight lines, *a c* and *c b*, intersecting in the point *c*. Let this point of intersection move with a constant velocity along two flat circular arcs, lying symmetrically on the two sides of the string, and passing through its ends, as represented in fig. 9. A motion the same as the actual motion of the whole string is thus given.

As for the motion of every single point, it may be deduced from equation (1), that the two parts *a b* and *b c* (fig. 7) of the time of every vibration are proportional to the two parts of the string which are separated by the observed point. The two velocities of course are inversely proportional to the times *a b* and *b c*. In that half of the string which is touched by the bow, the smaller velocity has the same direction as the bow; in the other half of the string it has the contrary direction. By comparing the velocity of the bow with the velocity of the point touched by it, I found that this point of the string adheres fast to the bow and partakes in its motion during the time $a\,b$, then is torn off and jumps back to its first position during the time $b\,c$, till the bow again gets hold of it.

With these principal vibrations smaller vibrations are compounded, the nature of which I can define accurately only in the case where the bow touches a point whose distance from the nearer end of the string is $\frac{1}{5}, \frac{1}{6}, \frac{1}{7}$, &c. of its whole length, or generally $\frac{1}{m}$, if m is a whole number. Because the point where the bow is applied is not moved by any vibration belonging to the mth, $2m$th, &c. harmonic, it is quite indifferent for the motion of that point, and for the impulses exerted by the bow upon the string, whether vibrations corresponding to the mth harmonic exist or not. Th. Young has proved that if we excite the vibrations of a string by bending it with the finger, as in the harp, or hit it with a single stroke, as in the piano, in the ensuing motion all those harmonics are wanting which have a node in the touched point. I therefore concluded also that the bow cannot excite those harmonics which have a node at the point where it is applied; and I found, indeed, that if this point is distant $\frac{1}{m}$ from the end, the ear does not hear the mth harmonic sound, although it distinguishes very well all the other harmonics. Therefore, in the equation (1), all those members of the sum will be wanting in which n is equal to m, or $2m$, or $3m$, &c. These members, taken together, constitute a vibration of the string with m vibrating divisions. Every such division performs the same form of vibration we have described as the principal vibration of the whole string. These small vibrations must be subtracted from the principal vibration of the whole string for getting its actual vibration. Curves constructed according to this theoretical view represent very well the really observed curves. If $m=6$ and the observed point is distant $\frac{1}{12}$ from the other end of the string, the motion is represented in fig. 10. Near the end of the string, where the bow is commonly applied by players, the nodes of different harmonics are very near to each other, so that the bow is nearly always at, or at least very near to, the place of a node. Striking in the middle between two nodes, I could not get a curve sufficiently constant for my observations. If I strike very near the end, the sound changes often between the fundamental and the second or third harmonic, which is indicated by gradual corresponding alterations of the curve.—*From the Proceedings of the Glasgow Philosophical Society* for Dec. 19, 1860.

ON CLAIRAUT'S THEOREM. BY PROF. HENNESSY, F.R.S.

Laplace has shown that this theorem follows, whatever may be the density of the interior parts of the earth, provided it consists of similar concentric strata, and that the form of the outer stratum is ellipsoidal. In the 'Philosophical Transactions' for 1826, Mr. Airy (the present

Astronomer Royal of England) has presented an equivalent result; more recently, Professor Stokes has shown that we can deduce the law of variation of terrestrial gravity without any hypothesis whatsoever as to the earth's interior structure. He assumes merely that its surface is spheroidal, and that the equation of fluid equilibrium holds good at that surface. In vol. vi. of the 'Cambridge Mathematical Journal,' Professor Haughton presented a demonstration, founded upon the same assumptions as those of Professor Stokes, and in which he uses certain propositions relative to attractions which had been enunciated by Gauss and Maccullagh. While studying the labours of those mathematicians, it appeared to me that the question could be entirely divested of the hydrostatical character, and that Clairaut's theorem may be directly deduced from the equations to the normal of any closed surface, without any considerations as to the physical condition of the matter forming that surface. Thus every surface concentric with the earth, and perpendicular to gravity, will possess the property of exhibiting this relation in the intensity of gravity at its various points.

Let X, Y, Z represent the components parallel to the rectangular axes of the forces by which a point is retained at rest on a given surface whose equation is L=0. Then from the equations of the normal we have

$$\mathrm{Y}\frac{d\mathrm{L}}{dx}-\mathrm{X}\frac{d\mathrm{L}}{dy}=0,\quad \mathrm{Z}\frac{d\mathrm{L}}{dx}-\mathrm{X}\frac{d\mathrm{L}}{dz}=0,$$

when the resultant of these forces is perpendicular to the given surface. If we represent by V the potential of the earth on the particle in question, by w the angular velocity of rotation, we have

$$\mathrm{X}=\frac{d\mathrm{V}}{dx}+w^2x,\quad \mathrm{Y}=\frac{d\mathrm{V}}{dy}+w^2y,\quad \mathrm{Z}=\frac{d\mathrm{V}}{dx},$$

and the above equations become

$$\frac{d\mathrm{V}}{dy}\frac{d\mathrm{L}}{dx}-\frac{d\mathrm{V}}{dx}\frac{d\mathrm{L}}{dy}=w^2\left(x\frac{d\mathrm{L}}{dy}-y\frac{d\mathrm{L}}{dx}\right),$$

$$\frac{d\mathrm{V}}{dz}\frac{d\mathrm{L}}{dx}-\frac{d\mathrm{V}}{dx}\frac{d\mathrm{L}}{dz}=w^2\ x\frac{d\mathrm{L}}{dz}.$$

If, in conformity with General Schubert's* recent determinations, we assume the earth's surface to be that of an ellipsoid, with three unequal axes, we should substitute for L

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}-1=0,$$

or

$$\frac{d\mathrm{L}}{dx}=\frac{2x}{a^2},\quad \frac{d\mathrm{L}}{dy}=\frac{2y}{b^2},\quad \frac{d\mathrm{L}}{dz}=\frac{2z}{c^2};$$

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* *Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg*, sér. 7. tom. i.

whence we have

$$b^2x\frac{dV}{dy}-a^2y\frac{dV}{dx}=w^2xy(a^2-b^2),\ \ c^2x\frac{dV}{dz}-a^2z\ \frac{dV}{dx}=w^2a^2xz.$$

Each of these partial differential equations can be easily integrated; and the value of V, finally obtained, is equivalent to the equation of fluid equilibrium, or

$$V+\frac{w^2}{2}(x^2+y^2)=C.$$

Let θ represent the complement of the latitude, and ϕ the longitude, counted from the meridian of the greatest axis, then

$$z=r\cos\theta,\ \ x=r\sin\theta\cos\phi,\ \ y=r\sin\theta\sin\phi,$$

and

$$V+\frac{r^2w^2}{2}\sin^2\theta=C.$$

In the case of an ellipsoid having the ellipticity e, we have, neglecting small terms,

$$r=a\ (1-e\cos^2\theta).$$

From these equations, and from the properties of Laplace's functions into which V can be expanded, an expression can be obtained of the same kind as that deduced by Professor Stokes from his own and Gauss's theorems relative to attractions.—*Proceedings of the Royal Irish Academy*, Feb. 25, 1861.

ON A METHOD OF TAKING VAPOUR-DENSITIES AT LOW TEMPERATURES. BY DR. LYON PLAYFAIR, C.B., F.R.S., AND J. A. WANKLYN, F.R.S.E.

The authors refer to Regnault's experiments, which have shown that aqueous vapour in the atmosphere has the same vapour-density at ordinary temperatures as aqueous vapour above 100° C.; and they bring forward fresh experiments upon alcohol and ether to show that when mixed with hydrogen these vapours preserve their normal density at 20° or 30° C. below the boiling-points of the liquids, and infer generally that vapours, when partially saturating a permanent gas, retain their normal densities at low temperatures.

From their researches the authors deduce the consequence—remarkable, but quite in harmony with theory—that permanent gases have the property of rendering vapour truly gaseous. Stated in more precise terms, the proposition maintained by the authors is, "The presence of a permanent gas affects a vapour, so that its expansion-coefficient at temperatures near its point of liquefaction tends to approximate to its expansion-coefficient at the highest temperatures."

The authors anticipate that admixture with a permanent gas may serve as a kind of reagent to distinguish between cases of unusually high expansion-coefficient in a vapour, and cases where chemical alteration takes place. It will also be possible, by the employment of a permanent gas, to obtain vapour-densities of compounds which will not bear boiling without undergoing decomposition.

In experimenting upon substances which may be heated above the boiling-point, the authors employ Gay-Lussac's process for taking the specific gravity of vapours. A slight modification, however, is necessary. Previous to the introduction of the bulb containing the weighed substance, dry hydrogen is introduced into the graduated tube and measured with all the precautions belonging to a gas analysis. It will be obvious that in the subsequent calculation the volume of hydrogen corrected at standard temperature and pressure must be subtracted from the volume of mixed gas and vapour, also corrected at standard temperature and pressure.

When the substance will not bear heating to its boiling-point, the authors employ a process resembling that of Dumas in principle, but differing very widely from it in detail. Dumas's flask with drawn-out neck is replaced by two bulbs, together of about 300 cub. cent. capacity, joined by a neck, and terminating on either side in a narrow tube. One of the narrow tubes has some very small dilatations blown upon it (*b*), the other is merely bent (D). (See Plate V. fig. 6.) The apparatus, whose weight should not exceed 70 grms., is weighed in dry air, then placed in a bath, being secured by a retort-holder grasping the neck joining the large bulbs C and C. The end A, projecting over the one side of the bath, is made to communicate with a hydrogen apparatus; the end D passes through a hole in the opposite side of the bath, which is plugged up water-tight by means of putty. Dry hydrogen is transmitted through the whole arrangement, and escapes at D through a long narrow tube joined to it by a caoutchouc connecter.

The bath is next filled with warm water until the bends *a* and *a* are covered. The connexion with the hydrogen apparatus is then for a moment interrupted, to allow of the introduction of a small quantity of the substance at A. The substance, which should not more than half-fill the small bulb *b*, is partially vaporized in the stream of hydrogen, and in that state passes into the part C C. All the while the temperature of the bath is kept uniform throughout by constant stirring, and made to rise very slowly. When within a few degrees of the temperature at which the determination is to be made, the current of hydrogen is almost stopped, so that the bulbs C and C may contain less vapour than will fully saturate the gas at the temperature of sealing. The water of the bath is then made to subside, by opening a large tap placed near the bottom. The bends *a* and *a* are thus exposed, the bulbs C C remaining covered. Immediately the current of hydrogen has been stopped, the flame is applied at *a a*, so as to seal the apparatus hermetically. The temperature of the bath, as well as the height of the barometer, must now be observed.

After being cleaned, the apparatus (which now consists of three portions, viz. the portion C C hermetically sealed and the two ends *b* and D) must be weighed.

The capacity of the apparatus is found by filling it completely with water and weighing; but previously to this operation the volume of hydrogen enclosed at the time of sealing must be found. On breaking one extremity under water, the water will rise in the bulbs, and, after a while, will have absorbed all the vapour, but will leave the hydrogen. The bulbs must then be lifted out of the water, without altering their temperature, and, with the water that has entered, weighed. The difference between the latter weighing and the weight of the bulbs quite full of water gives the weight in grammes, which expresses in cubic centimeters the volume of hydrogen enclosed; the pressure is the height of the barometer minus the column of water which had entered the bulbs; the temperature is that of the water.

An example of a determination of the vapour-density of alcohol at 30° C. below its boiling point is subjoined:—

Height of the barometer (at 0° C.)	763·09 millims.
Temperature of the balance case	7°·5 C.
Weight of apparatus in dry air..........	69·959 grms.
Temperature at time of sealing..........	48° C.
Weight of apparatus + hydrogen + vapour. .	69·5275 grms.
Weight of apparatus + water (at 5°·2 C.). .	191·76 grms.
Weight of apparatus filled with water....	545·36 grms.
Height of water column	122 millims.

From which is deduced—

	Volumes corrected at 0° C. and 760 millims. pressure, cubic centimeters.		Grm.
Hydrogen + vapour	406·43	weighing	0·1695
Hydrogen..............	341·27	,,	0·0306
	65·16		0·1389

Therefore, 65·16 cub. cent. of alcohol-vapour weigh ·1389
but 65·16 cub. cent. of air weigh.......... ·0843

$$\text{Vapour-density of alcohol} = \frac{\cdot 1389}{\cdot 0843} = 1\cdot 648.$$

The authors have extended their experiments to acetic acid and other substances. At low temperatures the vapour-density of acetic acid approximates to 4·00, no matter how much hydrogen be employed. At higher temperatures an approximation to 2·00 is obtained, but without heating so high as Cahours found necessary.

The authors are continuing these researches.—*From the Proceedings of the Royal Society of Edinburgh*, January 21, 1861.

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[FOURTH SERIES.]

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LXI. *On a Law of Liquid Expansion that connects the Volume of a Liquid with its Temperature and with the Density of its saturated Vapour.* *By* JOHN JAMES WATERSTON[*], *Esq.*

[With a Plate.]

§ 1. IN the archives of the Royal Society for 1852, there is an account of observations on the density of liquids and their superjacent vapours at high temperatures, made in sealed graduated tubes filled with the same liquid in different proportions of their volumes. The general law of density in saturated vapours deduced from these observations, and from the various observations of vapour-tension already published by other physicists, is therein set forth, with the assistance of a chart (No. 2 chart) in which the observations are all projected, and lines drawn, that enables the eye to judge of the accordance between theory and observation. (See Note A.)

An account of this general law is also given in the Philosophical Magazine for March 1858, in a paper entitled "On the Evidence of a Graduated Difference between the Thermometers of Air and Mercury between 0° and 100° C."

By the same mode of observing in sealed graduated tubes, I afterwards extended the observations up to the transition-point of three of the liquids, viz. alcohol, ether, and sulphate of carbon, and found that the law of vapour-density was maintained in them without deviation to their extreme limiting temperatures. As these extensive series of observations supplied the curves of expansion of the three liquids, I have occasionally tried by means of them to discover a general law of liquid expansion. I submit the following account of the last attempt of this kind, as it appears to be successful.

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[*] Communicated by the Author.

§ 2. The curves of expansion, drawn to a large and distinct scale, were examined by the following graphical process:—At four or five points nearly equidistant, tangents were drawn (carefully judging of the direction to be given to the straight edge by the sweep of the curve to the right and left of the point of contact). Thus were obtained several values of the quotient of the differential of volume by the volume or proportionate differentials for constant element of temperature $\left[\frac{dv}{dt\,.\,v}\right]$. These were set off as ordinates to the temperatures, and the curve drawn through the points appeared to be the common equilateral hyperbola, having one asymptote coinciding with the axis of temperature and the other perpendicular to it, and intersecting it at a temperature $[\gamma]$ that evidently was above the transition-point. If this were the case, the product of the coordinates to each point of the curve, reckoning from the point γ as origin, ought to be constant $[=\rho]$; and accordingly it was found that when the inverse of the quotients were projected as ordinates to the temperatures, the points ranged in a straight line, which being produced, cut the axis in the point γ. The differential equation is thus

$$\frac{dv}{dt}\cdot\frac{\gamma-t}{v}=\rho,$$

the integration of which is

$$v=\left\{\frac{k}{\gamma-t}\right\}^{\rho},$$

in which $k=(\gamma-t)$ when $v=1$. (See Note B.)

§ 3. There is a relation between this expression and that for saturated vapour-density which seems to prove that it is not empirical, but the true exponent of the physical condition of the molecules of a body in the liquid state. The following is a statement of it.

In the papers above referred to, there will be found an account of the law of saturated vapour-density, and the proofs on which it rests. It is expressed by the equation

$$\left\{\frac{t-g}{h}\right\}^{6}=\mathrm{D}.$$

If we put $\Delta=\frac{1}{v}$, the law of liquid density is

$$\left\{\frac{\gamma-t}{k}\right\}^{\rho}=\Delta.$$

On comparing the constants for different liquids, I find as a general rule that the quotient $\frac{h}{\rho}$ is a constant quantity [E].

Thus for Alcohol my observations give $E=1717$
„ Ether „ „ $E=1739$
„ Sulphate of carbon „ $E=1725$

M. Muncke's observations on sulphuric acid from 50° to 230° C. $E=1600$ to 1800. (See Note C.) MM. Dulong and Petit's observations on the expansion of mercury between 0° and 300° C. give $\rho=\frac{1}{1{\cdot}489}$. The line passing through M. Avogadro's observations on the vapour of mercury from 230° to 300° C. gives $h=1160$. These combined give $E=1727$. I have adopted 1717 as the nearest probable value at present, because the most labour was bestowed on the alcohol series of observations.

These values of E are derived from English measures of pressure and temperature, viz. inches of mercury and degrees of Fahrenheit scale. The value of this constant derived from French measures is $F=504{\cdot}44$, which corresponds with $E=1717$. The ratio of reduction is $a=\frac{5}{9}\left\{\frac{\frac{5}{9}\times 29{\cdot}922}{760}\right\}^{\frac{1}{6}}$, so that $Ea=F$,

$$\left[\begin{array}{l}\log F=2{\cdot}70282\\[4pt] \log\dfrac{1}{a}=0{\cdot}53195\end{array}\right]$$

§ 4. M. Regnault's observations on the tension of the vapour of mercury from low temperatures up to 200° C., respond to the same value of h as those at the higher temperatures by M. Avogadro, but with g augmented 12 degrees, showing a boiling-point 12 degrees higher. It is remarkable that M. Regnault's observations on the expansion of liquid mercury differs so far from those of MM. Dulong and Petit as to be represented with $\rho=\frac{4}{3}$ and $E=870$. At 300° C. this difference amounts to about the equivalent of 3½° C. The acceleration of the rate of expansion is in M. Regnault's observations only one-half what is shown by those of MM. Dulong and Petit. (See Note D.) This is a remarkable discrepancy, both being so eminent in this class of observations.

§ 5. In all cases I have worked with temperatures reduced to the air-thermometer by scales of correction computed from the formula given in Appendix III. to the paper in the Philosophical Magazine for March 1858 above referred to.

The formula is founded on MM. Dulong and Petit's observations. I annex exact tracings of these scales (Plate VI.). In two cases (petroleum and sulphuric acid) the temperatures were taken uncorrected and compared with the results when corrected. In both, the differences between theory and observation were less when the temperatures were corrected.

It is not absolutely necessary to correct the temperatures in order to recognize these laws of density, and for practical purposes may not be required; but it seems best to accustom ourselves to do so, in order to be prepared for the recognition of any other relations of harmony that may exist in the thermo-molecular physics of different bodies.

§ 6. Water is, as might be expected, an exception to the law of liquid density, as it is to the law of capillarity and compressibility (see papers by M. Grassi and M. Simon in the *Annales de Chimie*). I have traced its curve of expansion by observations in sealed tubes up to 210° C. air-thermometer (see Note E), and projected the densities to the value of ρ required by its vapour-gradient; also those of M. Despretz from 0° to 100° C.; but they do not conform to the line required at any point of its range even at the highest temperature. These abnormal features in this first of liquids have had a prejudicial effect on the progress of science in this department. There is no other liquid as yet found with such point of maximum density that remains a liquid under its maximum; yet such a point seems invariably to be sought for. M. Muncke and M. Pierre have bestowed much unavailing labour on this question. (See Note F.)

§ 7. To determine the constants of these two equations for the density of the liquid and of its vapour, not more than four exact observations are strictly required; two of the vapour, and two of the liquid.

If the series of observations on the dilatation of a liquid extend over a considerable range of temperature, and have had their inequalities equalized by graphical processes equivalent to weighing by the method of least squares, the three constants of the equation may be directly determined.

Thus let t_0, t_1, t_2 be the three temperatures, and v_0, v_1, v_2 the corresponding volumes observed, to find ρ and k we have

$$\frac{t_1-t_0}{\left(\frac{1}{v_0}\right)^{\frac{1}{\rho}}-\left(\frac{1}{v_1}\right)^{\frac{1}{\rho}}}=\frac{t_2-t_0}{\left(\frac{1}{v_0}\right)^{\frac{1}{\rho}}-\left(\frac{1}{v_2}\right)^{\frac{1}{\rho}}}=k,$$

which may be solved by trial and error. But few observations as yet published will stand this test, the range of temperature being too small, and the irregularities proportionably too great.

§ 8. A simple and satisfactory way to test both of these laws of density by published observations, is to take two of the vapour-tensions not far from the boiling-point and compute the value of h. *Ex. gr.*, let e_0, e_1 be the two observations of the pressure of vapour in contact with its generating liquid; T_0, T_1 the corresponding temperatures by air-thermometer reckoned from the zero of gaseous tension [−461° F. or −273°·89 C.]; then we have

$$h=\frac{\mathrm{T}_1-\mathrm{T}_0}{\left(\frac{e_1}{\mathrm{T}_1}\right)^{\frac{1}{6}}-\left(\frac{e_0}{\mathrm{T}_0}\right)^{\frac{1}{6}}};$$

and since $\rho=\frac{h}{\mathrm{E}}$, we obtain the index of the power of the density of the liquid, which being set off as ordinates to the temperature, ought to range in a straight line; and this line produced, cuts the axis of temperature at γ. If, when these points are connected by distinct lines, a general convexity in the range can be discovered, viewing it foreshortened with the eye close to the plane of projection, then we may infer that ρ requires to de diminished if the convexity is directed upwards from the axis of temperature, and *vice versâ*.

§ 9. Having found ρ and k and γ, the next step is to compute the values of t from the volumes by the equation, and tabulate the differences between the computed and observed temperatures. This will be found attended with but little additional labour. If we now project these differences as ordinates on an exaggerated scale to the temperatures, we obtain a distinct impression of how far theory and observation accord.

§ 10. Mercury and alcohol being the most important liquids for thermometric purposes, may serve as examples of the mode of computation.

I. *Mercury.*

M. Avogadro's observations on the tension of the vapour of mercury:—At 260° C. the observed tension was 133·62 millims., at 290° it was 252·51 millims. The correction to reduce the temperatures to the air-thermometer from the scale is 5°·60 at 260°, and 6°·91 at 290°. Hence—

$260-5{\cdot}60+273{\cdot}89=528{\cdot}29=\mathrm{T}_0$, 133·62 millims. $=e_0$,
$290-6{\cdot}91+273{\cdot}89=556{\cdot}98=\mathrm{T}_1$, 252·51 millims. $=e_1$;

and we arrive by computation at $\log h=2{\cdot}54796$ and $g=247^{\circ}{\cdot}45$.

[I have computed the temperatures for M. Avogadro's other six observations. The computed, minus the observed, is, at

300° = +0°·15
290 = 0
280 = +0·42
270 = −0·26
260 = 0
250 = −0·26
240 = −1·90
230 = −4·56

= difference in tension amounting to one-third inch mercury.]

This value of h being derived from French measures, is to be applied to F to find $\frac{1}{\rho}$, which thus comes out 1·4284.

The following are MM. Dulong and Petit's volumes of mercury computed with this index:—

Temp. Cent. Air.	Volumes.	Inverse of volumes raised to the power $\frac{1}{\rho}=1{\cdot}4284$.	First differences.	Second differences.
0°	1·000000	1·000000		
100	1·0180180	·974815	·025185	·000028
200	1·0368664	·949602	·025213	·000023
300	1·0566037	·924366	·025236	

The second differences indicate a slight convexity in the line upwards. This shows that $\frac{1}{\rho}$ requires augmentation. The following is the result of computing the same observations with $\frac{1}{\rho}=1{\cdot}489$:—

		First diff.	Second diff.
0°	1·000000		
100	·973760	·026240	·000001
200	·947521	·026239	·000005
300	·921287	·026234	

To find γ, we have $1-0{\cdot}947521 : 200^\circ :: 1 : 3811^\circ{\cdot}05=\gamma$,

and $\left\{\frac{3811{\cdot}05-t}{3811{\cdot}05}\right\}^{\frac{1}{1{\cdot}489}}=\Delta=\frac{1}{v}$, or $(3811{\cdot}05-t)v^{1{\cdot}489}=3811{\cdot}05$.

This expresses MM. Dulong and Petit's observations with a difference at 100° amounting to $\frac{1}{1000}$th of a degree, and at 300° the difference is $\frac{1}{30}$th of a degree.

§ 11. To bring the D of the vapour formula to the same standard as the Δ of the liquid formula, it is requisite to change the value of h in the one and k in the other, so that the weight in grains of a cubic inch of either may be indicated.

Let $\eta=\cdot0216216$ = weight in grains of a cubic inch of hydrogen at the temperature 0° C. and pressure 760 millims.

$\delta=$ vapour-density of the body on the hydrogen scale.

$\tau=$ temperature (reckoned from the zero of gaseous tension) at which the pressure of the saturated vapour is 760 millims.

$\mu=$ required factor

$$\left\{\frac{\tau-g}{\mu h}\right\}^6=\frac{273{\cdot}89}{\tau}\cdot\eta\delta=\beta^6.$$

By the formula,

$$\tau = 247^\circ{\cdot}45,$$
$$g = 611^\circ{\cdot}28,$$

also $\delta = 101$; hence $\mu h = \frac{\tau - g}{A}$, and $\log \mu h = 2{\cdot}56247$. Thus we obtain

$$\left\{\frac{T - 247{\cdot}45}{[2{\cdot}56247]}\right\}^{6} = \omega,$$

the general expression for the weight in grains of a cubic inch of the saturated vapour of mercury at T° (C. A. G.), temperature reckoned by Centigrade Air-thermometer from the zero of Gaseous tension.

§ 12. The weight of a cubic inch of mercury at 0° is 3754·4 grains, hence

$$\frac{3754{\cdot}4}{v} = \left\{\frac{3811{\cdot}05 - (T - 273{\cdot}89)}{M \times 3811{\cdot}05}\right\}^{\frac{1}{1{\cdot}489}};$$

and $M = \left(\frac{1}{3754{\cdot}4}\right)^{1{\cdot}489}$; hence

$$\left\{\frac{3811{\cdot}05 - (T - 273{\cdot}89)}{[8{\cdot}25857]}\right\}^{\frac{1}{1{\cdot}489}} = W,$$

the weight of a cubic inch of mercury in grains at T° (C. A. G. temperature).

We may thus find the temperature at which $\omega = W$, or that at which liquid and vapour would be of equal density if the laws were maintained.

With alcohol, ether, and sulphate of carbon, transition occurs a few degrees below the theoretical temperature of equal density.

II. *Alcohol.*

§ 13. M. Regnault's observed tensions of saturated vapour:—At 40° C. the tension is 134·1 millims.; at 70°, 539·2 millims.

$$\begin{array}{ll} 40 + 0{\cdot}48 + 273{\cdot}89 = 314{\cdot}37 \text{ C. A. G.} = T_0 \\ 70 + 0{\cdot}41 + 273{\cdot}89 = 344{\cdot}30 \qquad\qquad\; = T_1 \end{array}\Bigg\} \begin{array}{rl} \log h = & 2{\cdot}15392 \\ g = & 190{\cdot}70 \\ \frac{1}{\rho} = \frac{F}{h} = & 3{\cdot}5392 \end{array}$$

This gives 78°·83 C. A. as boiling-point at pressure 760 millims. M. Pierre's observations on the expansion of alcohol were made on a specimen that boiled at 78°·63 C. A. under pressure 758 millims., and its specific gravity at 0° was 0·8151.

The following are his second series of observations computed with the above index, 3·5392:—

	C.	C. A.	V.	$\left(\frac{1}{V}\right)^{3\cdot 5392}$	Computed temp. minus observed temp.
(1)	33°·46+0·47=	33·93	1·03714	·87890	+0°·21
(2)	47·52+0·51=	48·03	1·05356	·83140	0
(3)	50·33+0·51=	50·84	1·05676	·82248	−0·19
(4)	56·26+0·50=	56·76	1·06416	·80245	−0·25
(5)	60·41+0·48=	60·89	1·06989	·78736	+0·03
(6)	73·70+0·38=	74·08	1·08780	·74238	0
(7)	76·73+0·35=	77·08	1·09168	·73310	−0·27

The computed densities (·87890, &c.) set off as ordinates to the temperatures, show a trend without any appearance of curvature. The straight line seems to pass exactly through the points of the second and fifth and sixth. Assuming it to pass through the second and sixth, we have

$$\cdot 83140-\cdot 74238=\cdot 08902 : 74^\circ\cdot 08-48^\circ\cdot 03=26^\circ\cdot 05 :: \cdot 74238 : 217\cdot 24,$$

and $74\cdot 08+217\cdot 24=291^\circ\cdot 32=\gamma$. This line gives unity volume at $-1^\circ\cdot 30$, hence $k=292\cdot 62=\gamma-t$ when volume equal unity, and the equation is $(291^\circ\cdot 32-t)v^{3\cdot 539}=292\cdot 62$. The differences between the observed temperatures and those computed from the observed volumes by this equation are given in the last column.

§ 14. This equation answers well to the observations of M. Pierre above 10°; also to those of M. Muncke (St. Petersburgh Memoirs) above the same temperature; but in both the trend of the points below this lies in a line inclined to that of the equation. The divergence is the greatest in M. Pierre's. In neither is it a general convexity, but distinctly the contour shows two lines diverging from about 15° to 20° C. This is most distinct in M. Muncke's observations. I have computed them by the above equation, and tabulated the differences between the observed and computed temperatures, which are set off in Pl. VI. fig. 3 as ordinates to the temperatures on ten times the natural scale. Above these, in fig. 2, M. Pierre's differences are set off to the same scale, and in fig. 4 the differences in my series of observations on alcohol, described in the paper above referred to as being in the archives of the Royal Society. This alcohol was not absolute; it had 19 per cent. water, and the index of its power derived from its line of vapour-density was 3·60; also $\gamma=290\cdot 89$, $k=209\cdot 81$, and its equation (the volume being reckoned as unity at the boiling point)

$$(290^\circ\cdot 89-t)\;v^{3\cdot 60}=209^\circ\cdot 81 \text{ C. A. G.}$$

§ 15. The deflection in M. Pierre and M. Muncke's observasions, it will be remarked, occurs in those below atmospheric temperatures, where the reduction of temperature had to be artificially produced by mixtures of broken ice and muriate of lime, and it represents the temperature of the mercury to be higher than that of the alcohol. This is precisely what took place in some observations I made on the contraction of ether about 20° below the atmospheric temperature. A similar deflection, but in a greater degree, appears in the ether observations of the same authors. They are projected in figs. 5 and 6 to the same scale as the others. (See Note G.)

The application of cold to maintain a constant temperature is by no means under the same command as the application of heat; and, besides, conductibility is very much reduced at low temperatures. There is an evident dislocation, the law of continuity is broken, but it is at the part of the scale where the *mode* of obervation underwent a change. I submit, therefore, that the verdict should be against the observations at the lower temperatures, not against the law of expansion, which, if in fault, would cause the trend of the points to have a general curvature throughout the range.

In judging of the evidence afforded by these graphical projections, it should be kept in view that the vertical scale magnifies the amount of the differences tenfold. The accordance of theory with observation is in some cases remarkable. Thus, for 40° C., Muncke's alcohol and Pierre's ether do not show a difference greater than one-sixth of a degree. We have also to keep in mind that the power $\frac{1}{\rho}$ that reduces the densities to a straight trend, is not arbitrarily assumed to suit a particular series of observations, but that it is determined *à priori* from the vapour. If we take any other value of $\frac{1}{\rho}$ but that which is thus determined, the graphical projection of the computed densities shows a general bend. If $\frac{1}{\rho}$ is too great, the bow is turned downwards; if too small, the bow is turned upwards. The string of the bow only makes its appearance when the value of $\frac{1}{\rho}$ is that deduced from the gradient of vapour-density above described.

§ 16. The time has not perhaps yet arrived for deducing these laws of density from the dynamical theory of heat; but if we are ever to arrive at a conception of the true ultimate nature of molecular force, it seems clear that the inductive path of least difficult approach (if not the only one) is that which sets out from the study of the gaseous state, and proceeds by way of that of the equilibrated condition of saturated vapours in communication with their generating liquids, to the molecular condition of the liquids, where the dynamic condition of the chemical element is constrained by the cohesive force; and the struggle in which this dynamic force is gradually subdued by the increase of temperature is, as now ascertained, represented by one quantitative relation throughout, that seems to indicate a certain simplicity in the ultimate recondite principle on which molecular force is based.

We know that the physics of gases conform to the physics of media that consist of perfectly free elastic projectiles*. Their free concourse and perfectly elastic recoil determines the resolution of their *vis viva* into the six rectangular directions of space; and it is this number that probably fixes the ratio of the proportionate increment of density in a saturated vapour to the corresponding proportionate increment of temperature reckoned from the fixed limit g. But the *absolute* increment of density corresponding to constant increment of temperature differs in different vapours, being ruled by a gradient, the sixth root of which has a constant ratio to the index of density of the generating liquid expressed as a function of the temperature.

The next step that seems within reach, if we had a few more observations to work from, is the discovery of the relation which no doubt exists between the increase of volume and decrease of latent heat or capillarity regarded as the integral of cohesion. The density and the capillarity both diminish as the temperature rises. (See Note H.) If there is a simple law of quantitative relation between them, its discovery would supply all that is now wanting to bring the dynamical theory of heat to bear upon the molecular physics of liquids.

Notes.

Note A. § 1.—The title of the paper is "On a General Law of Density in Saturated Vapours," illustrated by Chart No. 2. In the Philosophical Transactions for 1852 there is a paper with the same title, illustrated by a Chart No. 1. (This paper was originally sent to the British Association.) In Chart No. 1 the sixth root of density is laid off as ordinate to the square root of the temperature reckoned from the zero of gaseous tension. In Chart No. 2 the sixth root of density is laid off as ordinate to the temperatures simply. In Chart No. 1 the lines appear straight at the upper part of their course, but with an increasing flexure at the lower part of the range convex to axis of temperature. Also there is no relation of harmony apparent between them. In No. 2 Chart (of which a tracing is to be found in the archives of the Royal Society for 1852–53) the lines are straight

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* See paper "On the Physics of Media that consist of perfectly elastic Molecules in a state of Motion," in the archives of the Royal Society, 1845–46.

throughout, and relations of paralellism appear; also several radiate from the same point in the axis of temperature, showing that, as a general law, *vapours in contact with their generating liquids have, at the same temperature, or at the same constant difference of temperature, densities that have a constant ratio.*

Note B. § 2.—This mode of graphical analysis seems a natural mode of operating when a law of nature has to be unmasked. If the proportionate differentials of volume had been laid off as ordinates, not to the temperature, but to the volume, the result would be the logarithmic curve, which might not be so easy to recognize with only a few points to lead from. We must be guided in the selection of the coordinate axis by the causal relation of dependent phenomena. Heat being, as it were, the instrument of action in molecular physics, claims the preference as a standard by which to measure the proportionate differentials, and to which other variables may be referred to as coordinate axis. As an example, the following is the analysis of the law of saturated vapours by this process.

The vapour-tensions being divided respectively by the corresponding temperatures reckoned from the zero of gaseous tension, the quotients represent densities of saturated vapour. Setting off these quotients as ordinates to the temperatures, we next draw the curve, and equalize the irregularities as far as possible; then take off the ordinates of the finished curve at equal intervals of temperature, say 10° or 5°; next take the differences of adjacent ordinates and divide each by the intermediate ordinate. These quotients, $\frac{d\mathrm{D}}{dt\mathrm{D}}$, are to be laid off as ordinates to the temperatures. The points appear to range in a conic hyperbola, having the axis of temperature as an asymptote. This conjecture is to be tested by laying off the inverse of these quotients as ordinates to the temperatures. The conjecture is confirmed by the points ranging in a straight line which cuts the axis at a certain temperature g. Hence $(t-g)f=\frac{dt\mathrm{D}}{d\mathrm{D}}$ and $\frac{d\mathrm{D}}{\mathrm{D}}=\frac{dt}{f(t-g)}$. The integration of this gives $\mathrm{D}=(t-g)^{\frac{1}{f}}\times\frac{1}{\mathrm{H}}$, in which $\mathrm{H}=(t-g)^{\frac{1}{f}}$ when $\mathrm{D}=$ unity; or let $h^{f}=\mathrm{H}$, then $\mathrm{D}^{f}=\left(\frac{t-g}{h}\right)$. Comparing the value of f in different vapours, it is found to be constant for all and equal to $\frac{1}{6}$.

As another example, but unconnected with heat, we may inquire as to the possibility of ascertaining the law of gravitation from the changes in the moon's apparent size and motion, its actual distance and the earth's radius being supposed unknown, but assuming that the difficulty caused by an unknown parallax and augmentation of diameter might be evaded by taking lunar distances at equal altitudes on both sides of the meridian.

By observations on consecutive nights, while the diameter is increasing or diminishing at the maximum rate, we might obtain two angular velocities and two measurements of diameter; hence the proportionate differential of the diameter and the correction to be applied to the angular velocities to reduce them to the same radius. We might thus obtain the velocity, the increment of velocity, and increment of distance expressed in terms of r, the radial distance of the moon from the earth at the middle epoch. Now since $\frac{dr}{r^2}=2vdv$, if we had these same quantities for different values of $\frac{1}{D}$ or r, and projected the different values of $\sqrt{\frac{1}{2vdv}}$ as ordinates to the corresponding values of r, the points would converge in a straight line to the zero of r; and if an approximate parallax was obtained, the point corresponding to the value of $2vdv$ at the earth's surface would fit in and confirm the propriety of the projection. If it is a question what should direct us to this particular projection, it might be answered the increment of square velocity is a square quantity, and the inverse form of function is applicable to a power depending on distance.

Note C. § 3.—The longest series of observations on the expansion of a liquid that I have met with is that of M. Muncke, on sulphuric acid from -30° to $+230^\circ$ C. I have been enabled to put them to the test by the following equation for the tension of its vapour, viz. $\left(\frac{t-354{\cdot}7}{1288}\right)^6 t=p$ (English measures). In this the value of $g=354{\cdot}7$ is assumed to be the same as that for steam, and for the vapours of several hydrates of sulphuric acid observed by M. Regnault, and referred to in § 1. of paper in the Philosophical Magazine for March 1858. The value of $h(=1288)$ is derived from the boiling-point. The value of $\frac{1}{\rho}=\frac{E}{h}$ is thus 1·33. The inverse volumes being computed to this power, and laid off as ordinates to the temperatures, were found to range well in a straight line above 30°. The line drawn through 45° and 220° is expressed by the equation

$$(1433^\circ{\cdot}2-t)v^{\frac{4}{3}}=1436^\circ{\cdot}1 \text{ C. A.}$$

The differences between the temperatures computed from the volumes by this equation and those observed are laid off in fig. 1 (Pl. VI.) as ordinates to the temperatures, the scale vertical to horizontal being 10 to 1.

The law of continuity is evidently broken at about 40°, the deflection being similar to the other cases referred to in §§ 14, 15, and probably due to the same cause.

Fig. 7 (Plate VI.) represents the differences of Muncke's observations on petroleum projected in the same way. The equation is

$$(489^\circ{\cdot}5-t)\ v^{2{\cdot}14}=489^\circ{\cdot}5 \text{ C.},$$

in which $\frac{1}{\rho}=2{\cdot}14$ has been deduced from Ure's observations on the vapour of petroleum. It is unlikely that the liquids were exactly the same. A slight convexity directed upwards is apparent in the trend of the points, which a small augmentation in $\frac{1}{\rho}$ would correct.

Note D. § 4.—M. Regnault stands so high as an authority, that an error in his observations that can be clearly demonstrated from internal evidence is of importance to science. Erroneous observations from eminent observers are serious obstacles to progress, as are unsound deductions from eminent men of science. They are weeds difficult to root up, and the attempt to do so is a task so ungracious and so irreverent as to incur every discouragement.

The projection of M. Regnault's observations on the tension of steam above and below 100° C. is given in fig. 8. The dotted line represents the empirical formula which had to be altered at 100°, the point at which the method employed in making the observations was changed. They are projected with temperatures uncorrected, in the manner described in § 1 of paper in the Philosophical Magazine for March 1858, and they are orthographically foreshortened as described in the latter part of § 4. See also § 6, and Appendix I. of the same paper.

The question to ask ourselves when looking at the figure 8 is, Do the points conform to the law of continuity? Is their trend not clearly broken at 100°? To put a series of observations to the test of this law can always be done, but it is attended with considerable labour, and seems to require a speciality different from that which characterizes the eminent observer and experimentalist.

Note E. § 6.—The following are those observations in series along with those of M. Despretz, both equalized graphically by elaborate processes, and the temperatures corrected and reduced to the air-thermometer. My observations from 100° up to 212° F. agree so well with M. Despretz's, that those at the higher temperatures will, I think, be found nearly correct, although there was some uncertainty in consequence of absorption by corrosion of glass.

C. A.	Volume.	C. A.	Volume.	C. A.	Volume.	C. A.	Volume.
−10°	1·00184	55°	1·01413	100°	1·04333	155°	1·09847
− 5	1·00068	60	1·01668	105	1·04743	160	1·10456
0	1·00013	65	1·01940	110	1·05172	165	1·11083
+ 5	1·00001	70	1·02230	115	1·05620	170	1·11731
10	1·00025	75	1·02538	120	1·06086	175	1·12400
15	1·00083	80	1·02863	125	1·06570	180	1·13093
20	1·00168	85	1·03205	130	1·07073	185	1·13811
25	1·00280	90	1·03563	135	1·07593	190	1·14553
30	1·00416	95	1·03939	140	1·08130	195	1·15323
35	1·00575	100	1·04333	145	1·08684	200	1·16124
40	1·00755			150	1·09257	205	1·16958
45	1·00955					210	1·17829
50	1·01175						

The following is an extract from note-book of the experiments:—

"The observations on pure distilled water could only be made up to 305° F., in consequence of the glass being corroded and becoming opake above that temperature. At the higher temperatures, five tubes were employed with water having $\frac{1}{23}$ of carbonate of soda in solution. Two only of these five were sufficiently transparent up to 413°. But on examining them next day, $\frac{1}{200}$th of the volume of liquid was absorbed. This allowed for.

"The expansion of the solution rather less than pure water. The corrosion of the glass began immediately above the surface of the liquid. The vapour was computed from formula, assuming the law of vapour-density maintained."

Note F. § 6.—M. Muncke and M. Pierre have employed the general formula $1+\Delta_x=1+ax+bx^2+cx^3$, &c. to represent their observations, and have computed the constants for each series. They have also sought, by means of the roots of this equation, to find points of maximum density of each liquid beyond the range of their observations. Thus M. Pierre, at p. 358, vol. xv. *Ann. de Chim.*, expresses himself as follows:—" . . . . puisque l'équation $\frac{d(1+\Delta_x)}{dx}=0$, dont les racines *doivent* donner la température de ce maximum, a ses deux racines imaginaires."

I have traced graphically the curve of the equation and of the observations, and find that its course through them is similar to fig. 8, interlacing at the fixed points, and departing altogether from the line of observation beyond the extreme points to which it is bound down. The positive and negative differences at the loops sometimes amount to $\frac{1}{2}$ degree. A conic section may be drawn to represent almost perfectly a series of observations if the range is not great. The hyperbola answers well, and can be simply applied as the increasing rate of expansion adapts itself to the curve, referred to an asymptote parallel to the axis of temperature.

Note G. § 15.—The value $\frac{1}{\rho}=3{\cdot}28$ is taken from Regnault's observations on the tension of its vapour at 0° and 20° C. The observations at 0° and 30° represent $\frac{1}{\rho}=3{\cdot}25$. Dalton's observation on the vapour give it equal to 3·2108, which is probably the most correct, as h is thus represented to be the same for sulphuric ether and water, their lines of vapour-density being parallel.

Note H. § 16.—In a paper on Capillarity in the Philosophical Magazine for January 1858, the proofs are given in detail of a law that connects molecular volume with capillarity and latent heat. It is expressed by the equation $m\doteq\frac{p}{\mathrm{L}}$, in which m is the cube root of the molecular volume of a liquid, p the height of the same in a capillary tube of constant bore, and L the latent heat of the vapour of the same, all taken at the same temperature t. According to Wolf and Brunner (*Ann. de Chim.* vol. xlix.), $p = \mathrm{A} - bt$ is the empirical equation for the capillary height in terms of the temperature. According to the law of expansion, $m = \sqrt[3]{\left(\frac{k}{\gamma - t}\right)^{\rho}}$. Hence

$$\mathrm{L} \doteqdot \frac{p}{m} \doteqdot (\mathrm{A} - bt)\sqrt[3]{\left(\frac{\gamma - t}{k}\right)^{\rho}}$$

is the equation which, at present partly empirical, it is so desirable to convert into one wholly expressive of a general natural law of quantitative relation between L and V.

Edinburgh, May 6, 1861.

LXII. *On a peculiar Acid (Dianic Acid) met with in the Group of Tantalum and Niobium compounds. By* Professor F. von Kobell*.

BEING engaged in preparing a new edition of my 'Mineralogical Tables,' I was anxious, among other matters, to arrive at as distinct chemical characters as possible for the tantalates and niobates with which we are familiar; and after various experiments, I came to the conviction that in several of these compounds there exists an acid different from the true tantalic acid which occurs in the tantalite from Kimito, and also from the niobic acid met with in the niobite from Bodenmais.

As from the previous labours of MM. Rose, Hermann, Wöhler, and others we are aware that in testing for these acids the one is very liable to be mistaken for the other, inasmuch as the tests give more or less various indications according to the manner of treating the substances and the quality of the reagents themselves, I have endeavoured in the first place to avert any possible error arising from those causes by conducting the whole of the assays in precisely the same manner, which I now proceed to describe in detail.

1·5 grm. of each assay was fused in a silver crucible with 12 grms. of hydrate of potash, and the mass, which melted quietly, was maintained for seven minutes longer in a state of fusion; hot water was then added till the fluid amounted to 20 cubic inches, and when cold it was filtered. The filtrate was acidulated with hydrochloric acid, then neutralized with ammonia, the precipitate allowed to subside, and the liquor poured off; after this, the precipitate, which was frequently coloured by manganese, was shaken with caustic ammonia and filtered. I had taken somewhat more ammonia than would have been required to remove from the precipitate an amount of 10 per cent. of tungstic acid. By

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* From the *Bulletin* of the Academy of Sciences of Munich, Meeting of March 10, 1860. Communicated by W. G. Lettsom, Esq.

this treatment any tungstic or molybdic acids which might have caused the reaction described below was got rid of.

In order to employ in my experiments as equal quantities as possible of the precipitates, which must be used when just thrown down, I made funnels of tinfoil, which I cut into the form of a filter one inch long in the side, and gave them the requisite shape in a small porcelain funnel. One of these funnels was filled with the freshly filtered pasty precipitate by means of a spatula, and then laid in a porcelain dish; the tinfoil having been opened out, one cubic inch of concentrated hydrochloric acid, of the specific gravity of 1·14, was added and heated to boiling, that temperature being maintained for three minutes, and the foil kept continually well stirred about in the fluid. Under this treatment the appearances observed were as follows.

1. The acid of the tantalite from Kimito, and of the niobite from Bodenmais, coloured the liquor bluish (smalt-blue); on adding half a cubic inch of water thereto, when poured into a glass the colour *disappeared rapidly,* the precipitate settled *without being dissolved*; on being filtered the liquor gave a *colourless* filtrate, and the precipitate, which at first was of a bluish tint, became speedily white on a further addition of water.

2. The acids of a so-called tantalite from Tammela, the powder of which was blackish grey, those of euxenite, æschynite, and samarskite, on being boiled with hydrochloric acid and tinfoil as above described, were dissolved to a dark blue cloudy fluid, which, when diluted with half a cubic inch of water or rather more, *became perfectly clear with a deep sapphire-blue colour,* and gave a transparent deep-blue filtrate. On being further diluted by the addition of a twofold or threefold quantity of water, the colour becomes indigo-blue and bluish green; and in open vessels, after some time, olive-green, maintaining that tint for several hours, but becoming paler. The fluid preserves its perfect transparency all the while, and in a closed vessel the colour remains unchanged for weeks.

Both with the assays under (1), and also with those under (2), I kept up the boiling for a longer time, indeed till the liquors were tolerably concentrated; I then added half the volume of water and poured the whole into a glass. The appearances observed were the same as before; the acids of (1) remained undissolved and gave a colourless filtrate, while those of (2) were dissolved and gave a transparent blue solution, the colour of the filtrate being also blue. When treating euxenite on one occasion, and concentrating the liquor by boiling, I obtained an olive-green fluid, which was, however, transparent; on the addition of concentrated hydrochloric acid, and boiling a second time with tinfoil, the blue colour was restored. If, on obtaining a green fluid of this nature, it is diluted with three times its volume of water and then slowly evaporated till it becomes turbid, on the addition of a suitable quantity of concentrated hydrochloric acid and boiling for a few minutes with tinfoil, the blue colour of the solution always appears on adding a little water.

It seems superfluous to say that a transparent blue fluid also gives a filtrate of the same colour; and yet the case occurs of such a fluid being coloured only from some substance being held suspended therein in a state of extreme subdivision, the filtrate being colourless. Such, for instance, is the behaviour of tungstic acid when it is precipitated from tungstate of potash with hydrochloric acid, and the precipitate boiled with concentrated hydrochloric acid and tinfoil. I obtained thus a dark-blue fluid, which, when considerably diluted, was quite transparent and of a bright sapphire-blue; but both the dark-blue and the light-blue diluted fluid gave a colourless filtrate; and when left to themselves, both these fluids also became colourless when the blue oxide of tungsten suspended therein, and which in that condition retains its blue colour, had settled to the bottom.

The tin contained in the blue solution of the acid in question is easily got rid of by a stream of sulphuretted hydrogen, and the acid is obtained again from the filtrate by precipitation with ammonia. The precipitate, on being boiled with hydrochloric acid and tinfoil, again produces the blue fluid. On evaporating slowly the liquor filtered from the sulphide of tin (which from its diluted state is colourless), it becomes turbid when considerably concentrated. On adding a little water the cloudiness disappears, and on the further addition of concentrated hydrochloric acid a white precipitate is produced. If the hydrochloric acid has been added in suitable quantity, and if the fluid is boiled with a slip of tinfoil placed in it, the appearance spoken of above is produced. The fluid becomes of a deep blue, and when poured into a glass appears turbid; but on the addition of half its volume of water it becomes transparent, and presents itself in the glass like a clear sapphire. The original precipitate from the potash solution may be freed from any manganese it may contain by boiling it with a certain quantity of hydrochloric acid; this precipitate can further be boiled with tolerably strong sulphuric acid without being deprived of the property of being soluble in hydrochloric acid in the presence of tinfoil. The acid thus purified is white; on being heated, it assumes a very pale yellow colour, which it loses again on cooling, taking somewhat the appearance of porcelain.

Before the blowpipe it is dissolved in borax and salt of phosphorus to a colourless glass, both in the oxidating and the reducing flame. When the borax glass is saturated, it remains transparent on cooling after exposure to a good heat; on being warmed again it becomes cloudy, and assumes the appearance of enamel.

When the acid in question is boiled with zinc instead of tin, the blue solution is not obtained; the precipitate of the acid is blue, it is true, but the filtrate is colourless, and the acid loses its colour on the addition of water without being perceptibly dissolved. It was only with a very large quantity of hydrochloric acid and zinc that I could obtain a dirty greenish solution, which, however, when diluted with half its amount of water, became speedily reduced in its colour, assuming a pale green hue with opalescence.

If equal quantities of the acid spoken of, of tantalic acid, and of hyponiobic acid, all three being measured in a platinum funnel, are boiled for three minutes with concentrated hydrochloric acid without tin in the manner above described, and are then poured out into a glass, they all three give yellow milky fluids. On the addition of a very moderate quantity of water the acid in question becomes perfectly transparent, whereas the tantalic acid, and also the hyponiobic acid, even on the addition of four or five times their volume of water, remain undissolved.

If the metallic acid in question, when freshly precipitated, is heated to boiling in diluted sulphuric acid (1 volume of concentrated acid to 5 of water), it forms a cloudy fluid; and on this being poured into a glass with a few grains of distilled zinc, in the course of a few minutes the acid, which was previously white, becomes of a decided smalt-blue, even deep blue, and retains this colour for some time on the addition of water; the filtrate, however, is colourless. In this behaviour it resembles hyponiobic acid, whereas tantalic acid treated in the same manner is only coloured pale blue, which colour immediately disappears on the addition of water. The difference in the behaviour of tantalic and hyponiobic acids has been already mentioned by Heinrich Rose as characteristic; as I modified the experiment, by having recourse to a boiling temperature, the effect is not only produced more rapidly, but also in a more marked manner. I look on this reaction for distinguishing tantalic acid from other kindred acids as the most certain, that is to say, if one does not wish to investigate the behaviour of the chlorides. For a qualitative testing of an acid of this class, the first step of the inquiry would be to precipitate it in the manner described from the solution of potash, and then to examine the solubility of the freshly obtained precipitate with hydrochloric acid and tinfoil, with due attention to the conditions above laid down. Should the acid not be dissolved to a blue fluid when, after three minutes' boiling, half a cubic inch or a cubic inch of water is added, it is tantalic or hyponiobic acid, and recourse must be had to an assay with sulphuric acid and zinc to determine between these two acids. I am at least inclined to think that our determinations, as far as correctness is concerned, will have as great a probability in their favour as with the other methods hitherto employed, which, as is shown by the constantly varying indications of the acids of euxenite, yttrotantalite, samarskite, &c., have not given any thoroughly reliable results.

With respect to the acid discovered by me, and which forms a blue solution with hydrochloric acid and tin with such remarkable facility, it is with certainty and ease distinguishable both from tantalic and from hyponiobic acid, and that evidently in a more marked manner than those acids mutually are; the method of its preparation, moreover, as above set forth, as well also as a comparison with kindred acids under closely similar circumstances, appear to me to exclude the idea of its being an allotropic state, or a not hitherto observed stage of oxidation of tantalum or niobium, and to claim for it an existence as a distinct acid. Hermann, as is well known, several years since assumed the occurrence in samarskite, formerly termed uranotantalite, of a peculiar acid which he termed ilmenic acid; he was, however, not enabled to characterize that acid with sufficient precision; and Heinrich Rose could at that time establish point by point for his own niobic acid, now termed hyponiobic, everything that Hermann sought to establish for ilmenic acid, so that at last Hermann ranged his acid under niobium, and has pronounced it to be a niobous-niobic combination*. That the acid discovered by me is an oxide of niobium is, as far as present experience goes, not to be assumed; for if it were a lower grade of oxidation than the hyponiobic acid we are acquainted with, it must, on being fused with potash in an open crucible, be converted to this hyponiobic acid, inasmuch as, according to Heinrich Rose, niobium itself is dissolved into hyponiobate of potash by boiling potash; and if it were a higher oxide than hyponiobic acid, it must, on being reduced with tin, be also converted into that acid, and consequently neither soluble in hydrochloric acid under the conditions spoken of, nor impart a blue colour to the solution, as is, however, the case.

The same argument holds good if it be regarded as an oxide of tantalum: under the treatment referred to, it must be converted into the tantalic acid with which we are familiar, and must agree with it in its reactions, which it does not. Heinrich Rose has duly established that the metallic acid of the tantalite of Bodenmais is distinct from that of certain Finland tantalites; and to mark the difference, he called the former

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* According to Hermann, it colours salt of phosphorus dark brown before the blowpipe.

niobic acid, now termed hyponiobic acid, and has called the mineral formerly designated as tantalite by the name of niobite. According to my experiments, the same case occurs with the acids of the tantalite from Kimito, and of that from Tammela which I have examined. I will therefore name the latter, in which I first remarked the difference, after Diana, and term it *Dianic acid*; the element I shall name *Dianium* (Di), and the mineral from Tammela which contains this acid, *Dianite.*

Besides occurring in the mineral here mentioned, this acid, though in a less pure state, appears to occur in the Greenland tantalite, in the pyrochlore from the Ilmen Mountains, and in the brown Wöhlerite (I have not examined the yellow). I could only employ small quantities, however, of these minerals, and it was not in my power to carry out the requisite investigations in sufficient detail. A small fragment of yttrotantalite, professedly from Ytterby, gave the reaction of dianic acid; in another assay of a specimen, the specific gravity of which I ascertained to be 5·5, from the collection of the late Duke of Leuchtenberg, the acid proved to be tantalic acid. The former assay refers therefore to a different species, the specific gravity of which I could not determine.

When combinations of this nature contain at the same time titanic acid, the latter is found in the residue of the potash-lye, in which it can be easily detected even when this residue contains also a small portion of dianic acid. The residue is boiled with concentrated hydrochloric acid and filtered, the filtrate, with a strip of tin laid in it, being then boiled longer. If no dianic acid, but tantalic acid is present, the liquor on becoming concentrated assumes a violet-blue colour, which on diluting with water is changed very characteristically to pink. The fluid retains this latter colour for several days or longer. When the solution, in addition to titanic acid, contains a portion of dianic acid as well, the blue colour of the latter predominates; on diluting it in an open glass, the pink colour due to titanic acid makes its appearance in the course of a few hours, owing to the colouring of the dianic acid disappearing gradually. In this way I recognized the presence of titanic acid (as it had been established previously by other methods) in æschynite, pyrochlore, and euxenite.

I cannot of course say whether my dianic acid is contained in all varieties, and from all the localities, of the above-named species; with respect to the tantalites from Tammela, it is indeed established that perhaps the majority of them contain tantalic acid. The specific gravity should probably be particularly attended to. The mineral from Tammela examined by me (dianite) has a specific gravity of 5·5, while the tantalites from that locality analysed by H. Rose, Weber, Jacobson, Brooke, Wornum, and Nordenskiöld, had a specific gravity of from 7·38 to 7·5 and more. The tantalite from Kimito, moreover, from which I procured the tantalic acid employed for my investigation, has a specific gravity of 7·06. The colour of the streak of dianite is, as before remarked, black-grey, while that of the Tammela tantalites analysed by Jacobson is stated to be dark brownish red, as is also the case with the Kimito tantalite.

In appearance dianite very closely resembles Finland tantalites. The assay analysed was taken from a large tabular broken crystal about 2 inches in size, on which, however, only two planes occur. Their angle of inclination, as measured by the hand-goniometer, amounts to about 151°; whether those planes are T and R of Naumann's tantalite, or T and G, or other ones, cannot of course be determined. Before the blowpipe, dianite affords no marked difference when compared with the Kimito tantalite.

The samarskite which I examined is from the Ilmen Mountains; I employed quite fresh, pure fragments, with a conchoidal fracture and strong, somewhat metallic, vitreous lustre. The euxenite is from Alva near Arendal (procured from Dr. Krantz); the æschynite, from the Ilmen Mountains, was from the Leuchtenberg collection.

While preparing the above, I forwarded a portion of the dianic acid in question to Professor Heinrich Rose, and communicated to him the leading points of the paper, requesting his opinion on the matter. Professor Rose was so good as to prepare the chloride of this acid, and wrote to me that, in doing so, he had met with a trace of tungstic acid, adding that the reaction described by me might be brought about from that circumstance; and he advised me, as a first step, to purify the acid by the method suggested by him, namely, by fusing it with carbonate of soda and sulphur. The case might be similar to the one which had misled Hermann.

Now I had, it is true, established, by the very ready solubility of dianic acid in hydrochloric acid when compared with true tantalic and hyponiobic acids under similar conditions, a characteristic distinction for the first of these three acids; but it was none the less essential to prove that the property of becoming blue with hydrochloric acid and tin belonged to the acid in question, and is not attributable to tungstic acid. After the treatment with ammonia, to which reference has been made, but little tungstic acid could, it is true, contaminate the dianic acid; nevertheless the turning blue might be ascribed to that. A plan to clear this point up was soon formed. I first sought to impart to the non-colouring tantalic and hyponiobic acids the quality of becoming coloured by an addition of tungstic acid, and then endeavoured to ascertain how far an acid thus mixed was to be purified by ammonia, the process which I followed in my original experiments. I prepared tungstate of potash of a determined strength, and mixed it with a lye of tantalic acid in such proportions that 84 parts of tantalic acid were united to 16 parts of tungstic acid, corresponding, as it were, to a potash solution of a tantalite that consisted of tantalic and tungstic acids only. The mixture was divided into two portions (by means of a graduated glass), and precipitated with hydrochloric acid. The precipitate of one portion was decanted and filtered, and a tinfoil filter, one inch long in the side, being filled therewith, it was boiled for three minutes in 1 cubic inch of hydrochloric acid as described above; $1\frac{1}{2}$ cubic inch of water was added, and it was filtered. The filtrate was greenish yellow; on adding 1 cubic inch more water the fluid was yellowish, the precipitate was *not dissolved*, and after the lapse of twenty-four hours the fluid which was poured off deposited a dark blue precipitate. The same experiment, performed in the like manner with a similar quantity of the hyponiobic acid, gave an olive-green filtrate, which did not materially change in twenty-four hours. When boiled again it became of a blue colour, which was also the hue of the filtrate. The hyponiobic acid was a little dissolved in this experiment, as the tantalic acid was in the corresponding one. When, however, I agitated the precipitates of the mixed acids with ammonia (the approximate quantity required to dissolve the amount of tungstic acid contained therein having been ascertained by experiment), and then allowed them to subside, decanted and filtered them, the precipitates thus treated, on being boiled for three minutes, as above described, with hydrochloric acid and tinfoil, and diluted with half a cubic inch of water, behaved almost entirely like the acids prepared directly from the minerals themselves; the fluid of the tantalic acid passed through the filter colourless, that of the niobic acid had a slight greenish tint. These experiments prove that tantalic and hyponiobic acids, even when containing a great amount of tungstic acid, may be at least so far purified by ammonia as *not* to produce the deep blue colour given by dianic acid; and further, that the presence of tungstic acid in those acids, under the conditions spoken of, does *not* increase their solubility in hydrochloric acid. The latter circumstance, although to be anticipated, was of more importance to me than the absence of the blue colour; for similar experiments to these had already fully convinced me that the removal of tungstic acid by means of ammonia is not a perfect one. To remove, however, the last doubts as to the possible cooperation of the tungstic acid in the production of the blue colour alluded to, the acid of the dianite from Tammela was purified with the utmost care by the process suggested by H. Rose. The filtered acid was dried down till it was capable of being readily reduced to powder. I took 0·5 grm. of it, which was triturated with 1·5 grm of carbonate of potash and then with 0·5 grm. sulphur. I fused the mixture in a covered porcelain crucible over a gas-lamp, dissolved the mass in water, and after decantation transferred the acid remaining into a close glass vessel with sulphuretted hydrogen, which I agitated well, leaving it thus for twenty-four hours. The liquor was then decanted twice, and the residue boiled with diluted hydrochloric acid and well washed; and lastly, the metallic acid was attacked a second time with hydrate of potash in a silver crucible, precipitated with hydrochloric acid and filtered. A tinfoil funnel was filled, as above described, with the acid, and a cubic inch of concentrated hydrochloric acid having been poured into it, the fluid was maintained for three minutes at a boiling temperature. Having been poured into a glass, it proved to be deep blue and turbid; but on the addition of the necessary quantity of water, it afforded a splendid sapphire-blue solution, perfectly transparent, without a trace of undissolved precipitate. There is therefore no doubt, not only that by its very marked difference of solubility under similar conditions dianic acid is distinguishable from tantalic and hyponiobic acids, but also that the property of producing a blue colour, as above described, belongs to it essentially, a property which the other acids do not possess.

I purified in the same way the acid of euxenite and samarskite; and their behaviour was precisely the same as I observed it to be on my endeavouring to remove by agitation with ammonia any tungstic acid they might possibly contain. The blue solution of dianite and samarskite was of a peculiarly deep colour, almost black, so that twice or thrice its volume of water had to be added to it to recognize the blue colour distinctly, and to see that the solution was perfectly transparent. In a stoppered bottle the colour maintained itself quite unaltered for weeks.

The acid of æschynite I have not purified further than by agitation with ammonia; and as in two experiments, independent of the blue colour, it proves to be quite as thoroughly soluble as the acid of dianite, I have no doubt that it is dianic acid. The experiments here described have all been repeated several times, particularly those with true tantalic acid, with hyponiobic acid, and with the dianic acid of dianite.

The very peculiar behaviour of dianic acid above described with respect to zinc as compared with tin, and with hydrochloric acid alone, induced me to make some additional experiments. When the solution obtained by employing a large quantity of hydrochloric acid and zinc is diluted with about thrice its volume of water, it has a dirty yellowish colour, and for the moment is tolerably transparent; in the course of a few minutes it becomes turbid and loses its transparency, owing to a finely-divided greenish-grey precipitate which forms after a while; on the addition of more water it is deposited, so that the liquor may be decanted. An assay with hydrochloric acid and tin shows this deposit to be hydrous dianic acid; for it is dissolved to the characteristic sapphire-blue fluid when treated in the way so often referred to. If this blue solution is boiled for a few minutes with zinc, the dianic acid also is thrown down with the tin; the precipitate is deposited with readiness in light grey flakes on the zinc, which is coated with spongy tin, and the supernatant fluid is transparent and colourless. On filtering the portion containing the flakes and boiling the acid collected with hydrochloric acid and tinfoil, the blue solution is obtained again on the addition of a little water. Thus the behaviour of the acid of the Tammela dianite and that of the acid of samarskite from the Ilmen Mountains is strictly identical. The behaviour of zinc and tin therefore with reference to the solutions of dianic acid in question, is, to an extent not to be anticipated, entirely different, it may be said antagonistic.

Those who wish to repeat the investigations described would do well to adhere to the quantities mentioned by me, or to employ them proportionally; for without this precaution it is possible that the properties may not be as distinctly brought out as they will be by adhering to them.

LXIII. *On the Partitions of a Close.* *By* A. Cayley, *Esq.*[*]

If F, S, E denote the number of faces, summits, and edges of a polyhedron, then, by Euler's well-known theorem,

$$F+S=E+2;$$

and if we imagine the polyhedron projected on the plane of any one face in such manner that the projections of all the summits not belonging to the face fall *within* the face, then we have a partitioned polygon, in which, if P denote the number of component polygons, or, say, the number of parts, $F=P+1$, or we have

$$P+S=E+1,$$

where S is the number of summits and E the number of edges of the plane figure. I retain for convenience the word *edge*, as having a different initial letter from *summit*.

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[*] Communicated by the Author.

The formula, however, excludes cases such as that of a polygon divided into two parts by means of an interior polygon wholly detached from it; and in order to extend it to such cases, the formula must be written under the form

$$P+S=E+1+B,$$

where B is the number of breaks of contour, as will be explained in the sequel.

The edges of a polygon are right lines: it might at first sight appear that the theory would not be materially altered by removing this restriction, and allowing the edges to be curved lines; but the fact is that we thus introduce closed figures bounded by two edges, or even by a single edge, or by what I term a mere contour; and we have a new theory, which I call that of the Partitions of a Close.

Several definitions and explanations are required. The words line and curve are used indifferently to denote any path which can be described *currente calamo* without lifting the pen from the paper. A closed curve, not cutting or meeting itself*, is called a *contour*. An enclosed space, such that no part of it is shut out from any other part of it, or, what is the same thing, such that any part can be joined with any other part by a line not cutting the boundary, is termed a *close*. The boundary of a close may be considered as the limit of a single contour, or of two or more contours lying wholly within the close. The reason for speaking of a limit will appear by an example. Consider a circle, and within it, but wholly detached from it, a figure of eight; the space interior to the circle but exterior to the figure of eight is a close: its boundary may be considered as the limit of two contours,—the first of them interior to the close, and indefinitely near the circle (in this case we might say the circle itself); the second of them an hour-glass-shaped curve, interior to the close (that is, exterior to the figure of eight) and indefinitely near to the figure of eight. The figure of eight, as being a curve which cuts itself, is not a contour; and in the case in question we could not have said that the boundary of the close consisted of two contours. A similar instance is afforded by a circle having within it two circles exterior to each other, but connected by a line not cutting or meeting itself; or even two points, or, as they may be called, summits, connected by a line not cutting or meeting itself; or, again, a single summit: in each of these cases the boundary of the close may be considered as the limit of two contours. But this explanation once given, we may for shortness speak of the close as bounded by a

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* It is hardly necessary to add, except in so far as any point whatever of the curve may be considered as a point where the curve meets itself.

single contour, or by two or more contours; and I shall throughout do so, instead of using the more precise expression of the boundary being the limit of a contour, or of two or more contours. The excess above unity of the number of the contours which form the boundary of a close is the *break of contour* for such close; in the case of a close bounded by a single contour, the break of contour is zero.

Any point whatever on a curve may be considered as the point of meeting of two curves, or, in the case of a closed curve, as the point where the curve meets itself, but it is not of necessity so considered. A point where a curve cuts or meets itself or any other curve, is a *summit*; each point of termination of an unclosed curve is also a *summit*; any isolated point may be taken to be a *summit*. It follows that, in the case of a closed curve not cutting or meeting itself (that is, a contour), any point or points on the curve may be taken to be summits; but the contour need not have upon it any summit: it is in this case termed a *mere contour*. The curve which is the path from a summit to itself, or to any other summit, is an *edge*: the former case is that of a contour having upon it a single summit, the latter that of an edge having, that is, terminated by, two summits, and no more. It is hardly necessary to remark that a contour having upon it two or more summits consists of the same number of edges, and, by what precedes, a contour having upon it a single summit is an edge; but it is to be noted that a contour without any summit upon it, or mere contour, is *not* an edge. It may be added that an edge does not cut or meet itself or any other edge except at the summit or summits of the edge itself.

Consider now a close bounded by $\beta+1$ mere contours: if for any partitioned close we have P the number of parts, S the number of summits, E the number of edges, B the number of breaks of contour; then, for the unpartitioned close, we have $P=1$, $S=0$, $E=0$, $B=\beta$, and therefore

$$P+S+\beta=E+1+B;$$

and it is to be shown that this equation holds good in whatever manner the close is partitioned. The partitionment is effected by the addition, in any manner, of summits and mere contours, and by drawing edges, any edge from a summit to itself or to another summit. The effect of adding a summit is first to increase S by unity: if the summit added be on a contour, E will be thereby increased by unity; for if the contour is a mere contour, it is not an edge, but becomes so by the addition of the summit; if it is not a mere contour, but has upon it a summit or summits, the addition of the summit will increase by unity the number of edges of the contour. If, on the other hand, the summit added be an isolated one, then the addition of such summit causes a break of contour, or B is increased by unity. Hence the addition of a summit increases by unity S; and it also increases by unity E or else B, that is, it leaves the equation undisturbed. The effect of the addition of a mere contour is to increase P by unity, and also to increase B by unity: it is easy to see that this is the case, whether the new mere contour does or does not contain within it any contour or contours. Hence the addition of a mere contour leaves the equation undisturbed. The effect of drawing an edge is first to increase E by unity; if the edge is drawn from a summit to itself, or from a summit on a contour to another summit on the same contour, then the effect is also to increase P by unity; if, however, the edge is drawn from a summit on a contour to a summit on a different contour, then P remains unaltered, but B is diminished by unity. There are a few special cases, which, although apparently different, are really included in the two preceding ones: thus, if the edge be drawn to connect two isolated summits, these are in fact to be considered as summits belonging to two distinct contours, and the like when a summit on a contour is joined to an isolated summit. And so if there be two or more summits connected together in order, and a new edge is drawn connecting the first and last of them, this is the same as when the edge is drawn through two summits of the same contour. The effect of drawing a new edge is thus to increase E by unity, and also to increase P by unity, or else to diminish B by unity; that is, it leaves the equation undisturbed. Hence the equation $\mathrm{P}+\mathrm{S}+\beta=\mathrm{E}+1+\mathrm{B}$, which subsists for the unpartitioned close, continues to subsist in whatever manner the close is partitioned, or it is always true.

In particular, if $\beta=0$, that is, if the original close be bounded by a mere contour, $\mathrm{P}+\mathrm{S}=\mathrm{E}+1+\mathrm{B}$; and if, besides, $\mathrm{B}=0$, then $\mathrm{P}+\mathrm{S}=\mathrm{E}+1$, which is the ordinary equation in the theory of the partitions of a polygon.

If we consider the surface of a plane as bounded by a mere contour at infinity, then for the infinite plane, $\beta=0$, or we have $\mathrm{P}+\mathrm{S}=\mathrm{E}+1+\mathrm{B}$: in the case where the infinite plane is partitioned by a mere contour, $\mathrm{P}=2$, $\mathrm{S}=0$, $\mathrm{E}=0$, $\mathrm{B}=1$ (for the exterior part is bounded by the contour at infinity, and the partitioning contour, that is, for it, $\mathrm{B}=1$), and the equation is thus satisfied. And so for a contour having upon it n summits, $\mathrm{P}=2$, $\mathrm{S}=n$, $\mathrm{E}=n$, $\mathrm{B}=1$, and the equation is still satisfied: this is the case of the plane partitioned into two parts by means of a single polygon.

The case of a spherical surface is very interesting: the entire surface of the sphere must be considered as a close bounded by 0 contour, or we have have $\beta=-1$, and the equation thus becomes $P+S=E+2+B$. Thus, if the sphere be divided into two parts by a mere contour, $P=2$, $S=0$, $E=0$, $B=0$, and the equation is satisfied. And in general, when $B=0$, then $P+S=E+2$; or writing F for P, then $F+S=E+2$, which is Euler's equation for a polyhedron.

2 Stone Buildings, W.C.,
March 8, 1861.

LXIV. *Some Notes on the Drift Deposits of Western Canada, and on the Ancient Extension of the Lake Area of that Region.* By E. J. Chapman, *Professor of Mineralogy and Geology in University College, Toronto*[*].

THE following notes and deductions are the result of a careful examination of the Drift deposits of Western Canada, undertaken during the last three or four summers in an unsuccessful search for marine post-tertiary fossils, such as occur so abundantly in many parts of Eastern Canada and throughout the New England States. The district more especially investigated extends from the Bay of Quinté westward to the mouth of the Saugeen on Lake Huron, and includes the line of country lying along, and immediately within, the outcrop of the Laurentian rocks north of that region. Detached observations have been made, moreover, at various points on the islands and north shore of Lake Huron; and also beyond the limits of the province, as in the district south of Lake Ontario, in Michigan, and along the southern shore of Lake Superior.

The notes recorded here are arranged under two sections, of which the first comprises a collection of data, and the second a corresponding series of deductions.

§ 1. *Data.*

1. The first point observable, with regard to our Drift deposits, is the very evident fact that the rock floor on which these accumulations are spread had been extensively denuded prior to their deposition upon it. They cover thus an undulating and more or less broken surface; and their thickness, consequently, apart from the denudation to which they have been themselves subjected, is exceedingly variable.

2. The lowest of these deposits appear to consist of dark-blue or greyish clays, with thin layers of yellowish or light-coloured clay

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[*] Communicated by the Author.

in places. This deposit is often laminated horizontally, and is generally very calcareous. It appears also to be free from northern or large crystalline boulders. Pebbles of limestone and other fossiliferous rock, mixed with some small pebbles of water-worn gneiss, occur abundantly in it in many localities; but northern boulders, properly so called, are either absent or exceedingly rare. Amongst the localities in which these lower and boulder-free clay deposits are of marked occurrence, the district around Toronto and many parts of the valley of the Saugeen and western shores of Lake Huron may be especially mentioned; but wherever our Drift deposits are found to consist of clay and other materials, the clay-beds are almost invariably seen to occupy the lower place. At the same time, as described more fully in the sequel, beds of yellow and other-coloured clay, it should be observed, are occasionally found with northern boulders in a higher part of the series; but these are quite distinct from the lower clays now referred to. They are, moreover, of no great thickness, but alternate with, and are subordinate to, thick deposits of gravel and sand; whereas the lower clays attain in places to a thickness of over 100 feet, and present a general uniformity throughout. In these latter beds no traces of contemporaneous fossils have as yet been found.

3. It is generally assumed as an established fact, that the harder rocks beneath the Drift exhibit everywhere the marks of glacial action. Although we have numerous examples, throughout this section of the province, of polished and striated rock, I believe it to be still an open question as to whether the rocks which underlie these lower clays have been thus affected. I have not been able to discover any instances of it, nor can I find any recorded cases in our Geological Reports, or in other trustworthy sources. The question hitherto does not seem to have been mooted, the Drift accumulations generally being classed together by most observers under one common term. As the point is of much interest, however, it should be kept in view.

4. Above the lower clay deposits, or resting immediately (where these are absent) on the foundation rock of the country, we meet with a series of sands and gravels of evidently northern origin, containing boulders of gneissoid and other rock, and alternating occasionally with beds of clay, in which northern boulders are also frequently found. This clay, with scarcely an exception, is remarkably free from calcareous matter, the cause of which will be alluded to further on. In some places the clay and gravel are mixed up together, and present no signs of stratification; but more usually they are distinctly stratified, and the boulders are mostly accumulated towards the upper part of the series As a general rule, indeed, the boulders occur in by far the greatest

abundance, scattered, *per se*, over the surface of the gravels, or resting immediately on the underlying rocks where the clays and gravels are absent. This appears to have arisen in some cases from the subsequent removal or washing away of the looser materials in which the boulders were originally imbedded; but the greater number of these were evidently thrown down where they now lie, by melting or stranded icebergs, after the deposition of the other Drift materials. The boulders, whether of gneissoid or other fossiliferous rock, belong always to northern localities in relation to the spots on which they now occur. Here and there the infiltration of water containing bicarbonate of lime has cemented some of these upper Drift deposits into conglomerates of considerable solidity (Burlington Heights; vicinity of Niagara Falls; Georgetown, &c.).

5. Under the gravels and sands, or where the isolated boulders of this series are found, the rocks are always more or less marked by glacial action. The more common effects comprise a smoothed and polished surface, and a fine striation, the striæ running in long straight lines in a general N.E. and S.W. direction, although following to a certain extent, in hilly and broken districts, the natural windings of the rock-slopes on which they occur. These effects are seen in Western Canada, at various heights above the sea-level, up to an elevation of at least 1500 feet. They are well shown on the top of the Collingwood escarpment, at about 1000 feet above the level of Lake Huron; on the same line of escarpment near Niagara Falls; on many of the rock-exposures on the north shore of Lake Huron; and throughout the country at the junction of the Laurentian and Silurian formations, between the river Severn and the County of Frontenac; also in the vicinity of Belleville, Trenton*, &c.

The isolated boulders scattered over the country frequently exhibit in themselves a polished and striated surface; and the small boulders and pebbles imbedded in the gravel deposits often present the same effects (*e.g.* the pebbles found in the terraces north of Toronto; also those in Drift gravel in the environs of Belleville, Marmora, Guelph, Niagara Falls†, &c.).

6. The gravel and sand beds of this series occur in places in oblique stratification, or exhibit what is technically termed "false bedding." This occurs at or near the upper part of the series, and is evidently due to a rearrangement of the materials by the action of currents (*e. g.* Drift-bank seen in Great Western Rail-

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* See a paper, by the writer, "On the Geology of Belleville and its Environs," in the 'Canadian Journal,' vol. v. (new series), pp. 41–48.

† The localities cited in this paper are those which have come more immediately under the author's observation. In most instances the lists given might be greatly added to.

way cutting at Toronto, and extending westward several miles; beds at Orillia, on Lake Couchiching; also near Collingwood, &c. A remarkable example, alluded to more fully in the second part of this paper, Deduction 3, occurs near the village of Lewiston, on the south shore of Lake Ontario). I think it will be rendered clear, by what follows, that the currents in question were not marine, but were produced in the lake waters when these stood at higher levels. In places, moreover, secondary ridges, or ancient spits, have been formed by the same action out of these drift materials (*e. g.* Ridge at Weston, near Toronto, described by Sandford Fleming, C.E., in the Canadian Journal, new series, vol. vi.; also the ridge at Craigleith, in Collingwood Township, mentioned by the writer in the same Journal, vol. v. p. 305). These secondary ridges, it should be observed, are altogether distinct from the terraces of the lake shores and intervening districts. A careful search would no doubt reveal their presence in very many localities.

7. We now come to a fact of great interest—the occurrence of shells of freshwater mollusca in the sands and gravels of these Drift deposits, at various levels above the present surface of our lakes. These shells belong to existing species, inhabitants of the surrounding waters. They must not be confounded with similar shells left in elevated spots by the drying up of streams and ponds, or by the cutting back and lowering of river-beds. As occurring in our modified Drift deposits, they are imbedded in sand or gravel containing northern pebbles and small boulders; and in situations, moreover, in which it is evident that no merely local causes could have been concerned in their deposition. The fragility of most freshwater shells necessarily operates against the preservation of these in the coarser sediments, and explains their absence, probably, as regards the upper Drift beds of many localities.

In some of these re-sorted beds the bones and teeth of both extinct and existing mammals are occasionally found. The extinct forms comprise a species of Mastodon (*M. Ohioticus*? see Canadian Journal, new series, vol. iii. p. 356), the *Elephas primigenius*, and apparently an extinct species of the Horse. The remains of existing species found in these deposits (always confining our remarks to Western Canada) include the Wapiti, the Moose, Beaver, Musk-rat, &c. These two classes of remains have been found together. In a railway-cutting through Burlington Heights near Hamilton, the tusk of a Mammoth (*Elephas primigenius*) and the horns of a Wapiti (*Elaphus Canadensis*) were met with at a depth of about forty feet below the present surface of the ground. I have also seen the lower jaw of a Beaver (*Castor fiber*) obtained from the same locality. The flint arrow-heads and other wrought implements of Amiens and Abbeville, occur apparently in deposits of the same kind and age.

I have discovered freshwater shells, under the conditions described above, in beds of stratified Drift consisting of coarse gravel filled with pebbles of gneiss and other northern rocks, on the Kingston road, about two miles east of Belleville, at an elevation, by rough measurement, of about 40 feet above the present level of Lake Ontario. These belong to *Planorbis trivolvis,* or to some closely related species. Other examples of the same shell were obtained from fine gravel in oblique stratification, near the village of Orillia, at a height of about 18 feet above the level of Lake Couchiching. This lake is about 120 feet higher than Lake Huron, and about 700 feet above the sea. Pieces of nacreous shell (belonging to a species of *Unio*?) were also found in gravel, in the vicinity of Barrie, at an estimated height of about 30 feet above Lake Simcoe. I have found lacustrine and terrestrial shells in many other places; but these I omit from mention, as the shells occurred on the sites of ancient swamps, in gullies, or in flat lands adjacent to running streams, or in other doubtful situations in which they may have been deposited by freshets and other agencies of comparatively recent date.

Mr. R. Bell, of the Geological Survey of Canada, has added greatly to our knowledge of the above localities, in a paper published in the 'Canadian Naturalist' for February of this year (1861). Amongst other spots in which he has discovered freshwater shells, the environs of Collingwood and Owen Sound may be cited. At the former, examples of *Planorbis trivolvis,* associated with several species of *Helix,* were found by him at an elevation of 78 feet above Lake Huron. Specimens of *Melania conica* have been obtained, according to Mr. Bell, from another spot in this locality. Dr. Benjamin Workman of Toronto, has also communicated the discovery of examples of a *Melania* and *Unio ellipsis,* on the high banks of the Don, about 30 feet above the lake. These may have been deposited by the river, however, when flowing at a higher level; but they were covered, according to Dr. Workman, by a considerable deposit of sand.

The upper deposits of the Drift period are separable with difficulty in many places from those of more recent age. As the one period merged gradually into the other, this must necessarily be the case. Among the more recent deposits of Western Canada, however, our river "flats" may be more especially cited, as those of the Grand River, filled with the remains of land mollusca. Also, the closely-similar deposits of the ancient bed of the Niagara, so high above the present level of that river; together with the shell-marls and calcareous tufas of our lakes and streams; and our deposits of bog-iron ore and iron ochres.

§ 2. *Deductions.*

The following deductions appear to flow naturally from the observations recorded above:—

1. A general depression of the land, at the commencement of the Drift period, must have taken place to such an extent as to admit of the deposition of the lower clays. These latter were evidently derived from the limestones and other Silurian and Devonian strata lying beneath and around them. Hence their generally calcareous nature. Their derivation from this source is proved, moreover, by the pebbles of Trenton limestone and other fossiliferous rocks which they frequently contain. Extensive denudation must thus have occurred both immediately prior to and during the deposition of these clays; but it may be questioned whether the bolder contours offered by the denuded rocks, such as the escarpment that sweeps from the Niagara river to Cabot's Head on Lake Huron, were not produced during the first uprise of the palæozoic strata from the earlier seas in which their materials were accumulated, ages before the period now under discussion. It appears, at least, to be a well-admitted point, that these rocks had been elevated into dry land before the deposition of the higher formations in the south and west.

2. After the deposition of the lower Drift clays, a sudden and abrupt change in the character of the sediments took place. A striking example of this may be seen in the natural sections about Hogg's Hollow, a few miles north of Toronto. The change in question must have been effected by a still further depression of the country, bringing the higher lands and gneissoid strata of the north within the influence of the waves, and yielding the sands, gravels, and boulders of the upper Drift accumulations.

This depression permitted an invasion and broad extension southwards of the ice-covered Arctic seas, the true cause, in all probability, of the cold of this epoch. The depression must have exceeded 1500 feet, since northern boulders are found at that height above the sea on the Collingwood escarpment. The gneissoid boulders there met with must at least have traversed the basin of Georgian Bay; but the glacial striæ which also occur there may have been produced by the action of ice originating at the spot itself: the three or four distinct sets of striæ observed at this locality, however, do not radiate from any fixed point, but run in the usual north and south direction, some being a little east and others a little west of north*.

3. At the close of this second series of phenomena, a gradual

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* On a visit to this spot, since the publication of the "Note on the Geology of the Blue Mountain Escarpment" in the 'Canadian Journal,' vol. v. p. 304, some additional sets of striæ were observed.

uprise of the land appears to have taken place, and a vast area, extending over and around our present lake-basins, then became converted into a freshwater sea. This probably found its outlet to the ocean through what is now the broad valley of the Mississippi. Its waters stood at a great elevation above the waters of our present lakes, and were gradually lowered to these levels by physical changes in the surrounding country, and more especially by the depression of a higher region lying to the east. During this gradual fall and retrocession of the great lake-waters, the upper layers of the Drift were re-sorted, mixed with newer sediments, and thrown up here and there into secondary ridges; and the remarkable terraces which form so salient a feature in the general aspect of our lake shores and intervening districts, were then in chief part produced. The escarped faces of these Drift terraces, it should be observed, *always front the present lake-basins,* and thus look in some places towards the north, and in others towards the south, &c., according to the direction of the nearest shores. This would necessarily arise if they were produced, as here imagined, by a gradual lowering of the waters, with intervening periods of repose. The shells of freshwater mollusca buried in the modified Drift, at various levels above the existing lake-waters, and in localities so far apart—for these shells have been found throughout the region south of the lakes, in addition to the localities mentioned in this paper—prove incontestably the former expansion and union of our lakes, or, in other words, the presence in this part of Western America, of a widely-extended freshwater sea covering an enormous area. A curious circumstance, and one of great significance in its bearings on this question, is the fact that all the inclined layers of modified Drift (to the east, at least, of Lake Superior) appear to slope towards the west or south. A remarkable instance of this, hitherto, it is believed, unnoticed, may be seen near the mouth of the Niagara river, at Lewiston. At this spot, oblique layers of modified Drift, in beds made up of coarse gravel and pebbles, point nearly due south, and thus bear witness to the fact that the current which occasioned the inclined stratification must have set directly up the gorge, *or against the direction of the present stream.*

The assumption of an immense freshwater lake of this character gradually falling from a high level, necessarily involves the additional assumption of an eastern barrier, extending at one period between the lake-waters and the Atlantic. This view was maintained by some of the earlier investigators of our geology, and notably by Mr. Roy, in his well-known paper on the terraces of Lake Ontario, communicated to the Geological Society of London in 1837. The difficulty of finding a satisfactory location for a barrier of this kind led Sir Charles Lyell, however, to reject the idea of an original lake extension, and to refer the formation of our terraces entirely to the action of the sea during the slow uprise of the land at the commencement of the present epoch. In this he has been followed by all geologists who have subsequently examined these terraces. The difficulty may perhaps be surmounted by assuming the earlier and greater elevation of that portion of the country lying to the east of the gneissoid belt which connects our northern Laurentian district with the Adirondack Mountains of New York. The subsequent depression of this region would open an eastern outlet to the lake-waters, and gradually lower these to their present levels. But whatever the explanation, the undoubted fact remains, that, at the close of the Drift period, a vast freshwater sea extended over the greater portion of Western Canada, and at a level of at least 500 feet above the present surface of Lake Ontario.

Whilst the mollusca of this ancient lake were identical with existing species, its shores were peopled by the mastodon and mammoth, and probably by other extinct forms of life, together with various species that still survive. A great question remains to be solved. Our gravel beds may perhaps reply to this, and reveal to us that here, as in Europe, man and the departed mammoth once trod the earth together. Could this be established, the discovery would be fraught with even deeper interest than that which attaches itself to exhumed human relics of the ancient plains of Picardy and the gravel-beds of Suffolk. Our Indian arrow-heads are disentombed by hundreds: the connecting link of the extinct tooth or bone may not be long forthcoming*.

University College, Toronto, Canada.
March 16, 1861.

LXV. *On the Neutralization of Colour in the mixtures of Solutions of certain Salts.* By Frederick Field, *F.R.S.E.*†

I BELIEVE Maumené first pointed out the fact that when nitrate of cobalt is added to nitrate of nickel in certain proportions, the green and pink colours of the solution entirely disappear, and the liquid becomes colourless, or assumes merely a pale neutral tint. Ever since the manufacture of the oxides of

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* Since writing the above, Albert Koch's account of the discovery of the Missouri mastodon has come under the author's notice. In this account, published in 1841, it is stated that the mastodon bones were found in more or less immediate association with large arrow-heads. The same writer also attests to the discovery of wrought implements in connexion with Edentate remains in Gasconade county, Missouri.

† Communicated by the Author.

nickel and cobalt from the ordinary *speiss* by the wet method, carried on for many years in Birmingham, this phenomenon must have been observed by those employed in conducting the process; and to one ignorant of the fact, and who has devoted but little time to this particular branch of chemical analysis, the solution of speiss must appear rather surprising, as, although very rich both in cobalt and nickel, in certain instances the solution appears almost destitute of colour, showing no trace either of the red of the cobalt or the green of the nickel. Professor Liebig (Liebig's *Annalen*, vol. xc. p. 112, and Chemical Gazette, vol. xii. p. 300), in remarking upon the decolorizing action of binoxide of manganese upon glass, does not impute the bleaching effect to the oxidation of the protoxide of iron by the reduction of the binoxide, as neither nitrate of potash nor other strongly oxidizing bodies effect the same change, but to the violet colour imparted to the glass by the manganese being complementary to the green produced by the iron, and hence the two affording a colourless mass. And that this is the case is evident, I imagine; for if borax be coloured by protoxide of iron, the resulting glass fused in a platinum crucible, and a little of the same salt (previously coloured by manganese) cautiously added, a point is arrived at where the mixture of the two has lost the individual tint of each and produced a nearly colourless glass. Liebig also mentions that a concentrated solution of sulphate of manganese having a slight rose colour, added to a solution of protosulphate of iron having a pale green tint, affords a perfectly colourless mixture.

A few experiments of my own have been extended to some other solutions and chemical compounds.

When nitrate of cobalt is gradually added to a cold solution of bicarbonate of soda, a beautiful amethyst-coloured liquid, at times almost approaching to violet, is produced. The colour has not the pure rose-red of the nitrate or sulphate of the metal, but has evidently a considerable portion of blue in its composition. If the fluid thus formed be divided into two equal parts, and to one of these a few drops of hypochlorite of soda be added, the liquid changes to an intense green colour, with no trace of blue, but a slightly yellow tinge, very much the colour of chloride of copper when dissolved in strong hydrochloric acid. If the violet and green liquids are united, the mixture becomes colourless, more strikingly so perhaps than by the union of the cobalt and nickel salts: the blue tint in the bicarbonate of cobalt forms green with the slight yellow shade of the peroxidized compound; and this, together with the remaining green of that liquid, is entirely neutralized by the pure rose colour.

Dilute sulphate of nickel (pale green) dissolves crystallized sulphate of manganese (pink), forming a colourless solution.

A solution of permanganate of potash evidently contains two colours, red and blue, forming by their union the magnificent violet tint so characteristic of that salt. When a little chloride of sodium is added to a solution of sulphate of copper, chloride of copper is formed by double decomposition, and the liquid assumes a pure green colour. Permanganate of potash carefully added to this compound changes it to a fine bright blue. The red tint is neutralized by the green, and the visible blue remains. The experiment can also be performed by substituting the chloride of copper for the sulphate. When the chloride, free from acid, is dissolved in water, the solution has a pale blue tint, and the addition of one drop of the permanganate causes a very dark blue shade. If a little acid be introduced into the copper salt, and the permanganate added as before, a similar effect is produced, but in about half an hour the pure blue disappears and the solution becomes green. The acid in this instance first changes the blue colour of the chloride into green, and subsequently decomposes the permanganate, and thus by destroying the red which was neutralized by the green, as well as the blue which remained intact, the original green tint becomes apparent.

When permanganate of potash is cautiously added to a solution of the bichromate of the same base, a bright-red liquid is produced. The solutions, however, must be dilute, and carefully managed. The yellow in the bichromate forms a green with the blue in the permanganate, which, neutralized by the red in both salts, would form a colourless solution, did not an excess of the latter tint prevail.

Most chemists must have observed that, in the estimation of iron by means of permanganate of potash, the last drop, which shows that the reaction is complete, imparts a rose-red to the liquid, differing somewhat from the bluish pink of the permanganate. The pale yellow of the perchloride of iron has combined with the blue of the permanganate, and the resulting green, not sufficiently powerful to destroy the whole of the red, has left a portion of it visible.

M. Terreil estimates copper by the same reagent. The cupreous salt is deoxidized by sulphite of ammonia, the sulphurous acid expelled by ebullition, and permanganate of potash added until the whole is converted into the protoxide. The difference of tint which the last drop of permanganate imparts to this liquid and to that containing the iron salt, is very apparent. In the case of the copper solution it is nearly blue; in the iron, pinkish red. These facts are not without their significance in qualitative analysis. Gibbs* informs us that the beautiful test for manganese, first proposed by Mr. Walter Crum, by the action of nitric

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* Silliman's Journal, September 1852.

acid and peroxide of lead, fails to yield the characteristic tint if nickel be present in large quantity, and the manganese only in minute traces: the nickel salt destroys, or at all events modifies the colour produced by the formation of permanganic acid. If cobalt is present, however, or a solution of a salt of that metal is subsequently added, the colour of the nickel is nullified, and that of the manganese becomes sensible.

When red fire, composed of nitrate of strontia, chlorate of potash, &c., is mixed with green fire containing nitrate of baryta and ignited, the red and green rays become invisible, and a white, or, rather, a bluish-white flame is produced: the crimson of the strontian flame has a dash of blue in its composition, and the red being removed by the green, is clearly shown. If a rose-red fire be prepared by mixing thirty-four parts of carbonate of lime, fifty-two of chlorate of potassa, and fourteen of sulphur, or still better, perhaps, twenty-three of dry chloride of calcium, sixty-one of chlorate of potassa, and sixteen of sulphur, and then ignited with the ordinary green fire, pure white light is produced.

LXVI. *Researches on several Phenomena connected with the Polarization of Light.* *By* M. H. Fizeau[*].

It has long been observed that when a ray of light is received on a mirror whose surface, instead of being perfectly smooth, is scratched with a fine point, it is no longer simply reflected. Part indeed follows the ordinary course, but a considerable portion is dispersed in various directions, some of which deviate greatly from that of the regularly reflected ray. We may mention as an example of this species of phenomenon, the singular reflexions produced by surfaces of metal, such as brass and steel, &c., which have been artificially prepared by being rubbed continually in one direction with emery, the hard angular particles of which produce fine parallel scratches on the surface of the metal. The amount of light dispersed by surfaces so prepared, in directions other than that of the regularly reflected ray, is calculated to excite some surprise; and it is generally agreed to attribute this effect partly to the variously inclined surfaces of the minute furrows with which the mirror is covered, partly to the diffraction caused by them in conjunction, acting as an irregular grating, and producing at the same time, and mingled with each other, the phenomena observed by Fraunhofer with simple gratings of various structure.

In studying the light dispersed in this manner with respect to

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[*] Translated from the *Comptes Rendus* for February 18, 1861, by F. Guthrie.

its polarization, I have observed several unexpected and, as I believe, hitherto unnoticed phenomena, which appear worthy of the attention of some of the learned members of the Academy, to whom I hasten to communicate them.

Many observers, among whom I may mention Fraunhofer, Sir D. Brewster, Foreign Associate of the Academy, and more recently Messrs. Stokes, Holtzmann, and Lorenz, have remarked certain phenomena of polarization in the light emitted by regular gratings. These phenomena, however, seem to be entirely distinct from those I am about to communicate.

On a plate of metal, say of silver, perfectly flat and polished, suppose a straight line to be drawn with a fine steel or diamond point, the surface of the metal being disturbed as little as possible, and the line so traced becoming finer and finer by degrees towards one end until it becomes imperceptible. If the plate on which the line has been so drawn be illuminated very obliquely, and in a direction perpendicular to the line, the latter will be visible in every position comprised in the common plane of incidence and reflexion, except its thin end, which, from its great tenuity, will be imperceptible. If the plate, illuminated in the same manner, be placed in the field of a microscope, a much larger portion of the thin end of the line will be visible, the amount depending on the power of the lenses employed.

Under these circumstances, the plate being viewed perpendicularly to its surface, if a doubly-refracting analysing prism be placed between the eye and the eyepiece of the microscope, the different intensity of the ordinary and extraordinary images at once indicates that the light emitted by the brilliant line thus drawn is more or less polarized,—the amount of polarization being greatest towards the fine end of the line, and the plane of polarization being parallel to the direction of the line. Lines produced in various ways were observed, and, provided they were sufficiently fine, always with the same result.

It was observed, moreover, that the plane of polarization, which was always parallel to the lines at their thinnest extremities, was perpendicular to them where they were a little broader, and became altogether imperceptible towards their thicker ends.

On varying the angle of incidence of the light on the plate and the angle at which the line was viewed, which could be done within certain limits, depending on the shape of the microscope, by inclining the plate and shifting the position of the source of light, certain changes were observed in these phenomena, of which the following are the principal.

The plate being still observed normally, if the line be illuminated less and less obliquely, the source of light being brought nearer to the eyepiece of the microscope, the amount of polarization rapidly decreases and soon becomes insensible.

If the plate be illuminated perpendicularly, but observed very obliquely though perpendicularly to the line, the phenomena are the same as in the first case, the light reflected from the finest extremity of the line being still polarized in the direction of the line itself.

If the plate be kept in the same position as in the last case, but the source of light be brought nearer to the eyepiece, so that the plate is both observed and illuminated obliquely and nearly in the same direction, the polarization of the bright line is much increased, not only the fine end polarizing the light parallel to itself, but the same species of polarization being observed towards the thicker part of the line where the light was formerly polarized in the opposite direction, and even where the light was previously unaffected.

The distinct polarizing power of single lines traced on a silver surface having been thus demonstrated, it was easily foreseen that the same property would be possessed by the innumerable striæ produced on metallic surfaces by rubbing them with a body covered with some hard substance reduced to a fine powder; and it was anticipated that the large number of these lines would compensate for the want of brilliancy in each individually, so that the phenomena would be visible without the need of a microscope.

On a silver surface, a straight striated band about 2 centims. broad was accordingly traced by means of a ruler and a piece of cork charged with emery*. The striated band thus produced exhibits in the most striking manner, on account of the intensity of the light reflected, the phenomena of polarization before observed in the case of single lines.

If the striated band be placed under the microscope and illuminated obliquely, innumerable lines are observed of different degrees of brightness and of various colours, due no doubt to accidental phenomena of diffraction and interference: nearly all these lines polarize the light in their own direction, some of the thicker ones, however, produce the opposite effect.

If the striated band, instead of being straight, is in the form of an arc of a circle of about 50 centims. radius, and if the surface of the plate be horizontal, and the source of light be placed vertically above the centre of the band, at such a distance from the plate that the incident light may make an angle of from 60 to 80 degrees with the normal, then, the eye being placed behind the source of light a little on one side, and being

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* Emery powder, No. 40 of the opticians, answers very well for these experiments, the mean diameter of its particles being about $\frac{1}{200}$ millim. Emery powder No. 20, ordinary Tripoli, or English red may also be employed.

shaded from the direct rays by means of a small screen, the striated band will appear illuminated throughout the entire arc, and the reflected light may be observed directly by means of an analyser, which causes it to appear alternately more or less bright.

The polarization thus produced does not seem to depend on the nature of the metal of which the plate is formed. Gold, platinum, copper, steel, brass, speculum metal, aluminium, tin, &c., have been substituted for silver; and all these metals, when suitably striated, have presented the same phenomena without sensible difference, except with regard to the colour of the light polarized by reflexion.

Metals which themselves possess least colour, give reflexions tinged with yellow, which, when most oblique, become bronze; while in the case of metals that have a marked colour of their own, the reflexions possess the same tinge, and sometimes, indeed, in a striking degree, as is the case with copper and gold.

I should mention here that the striated bands on silver and copper and some other metals are very brilliant when they have recently been made, but that they diminish in intensity on account of the action of the vapours accidentally present in the atmosphere. Gold and platinum are of course exempt from this inconvenience.

Non-metallic substances present similar phenomena, but their reflexions are so dull that the observation is often uncertain. Polarization parallel to the lines has nevertheless been clearly observed with a plate of specular iron and with one of obsidian, both of which I owe to the kindness of M. de Senarmont. With some precautions the phenomenon may even be observed with a plate of glass.

Finally, the groups of lines on silver and copper surfaces have been moulded with black wax, gum-lac, and even with galvanic copper; and the impressions so obtained presented to all appearance the same phenomena as the surface actually furrowed.

Among the various experiments that were tried, I may mention the case of a scratched metal surface which at first gave a reflexion distinctly polarized parallel to the lines, but which was afterwards covered with varnish. Under these circumstances the polarization became hardly sensible—a result which seems most naturally explicable on the ground of the change of the direction of the rays owing to the refraction of the varnish, which prevented the incident light from impinging at that angle which is required to produce the polarization in question. In fact, when glass plates differently cut were fastened to the striated plate by means of a varnish of turpentine or Canada balsam, the polarization was immediately reproduced in the refracting substance whenever the direction of the ray was such that the phenomenon would have been produced in the air. It is, however, well known that instruments constructed of brass, copper, and bronze, which are rendered brilliant by polishing, are generally covered with a coat of varnish for the purpose of preserving the brightness of the metal, and there is therefore no reason for surprise that the reflexions from these metals, however bright they may be, present no sensible polarization. When these surfaces have not been varnished, and when, moreover, they have been obtained by the action of polishing substances not too coarse, the phenomena are invariably observed.

No mention has yet been made of the light regularly reflected by a striated surface. In this direction the observation is not so easy, because the rays reflected by the furrows mingle with those reflected from the smooth surface of the mirror. Nevertheless, on observing with a microscope single lines suitably illuminated, a sensible polarization may be detected which varies little with the angle of reflexion; but what is strange is that the direction of this polarization is opposite to that which is produced in the former case, being *perpendicular* to the lines on the surface. The same fact may even be observed on regarding a striated surface directly by means of an analyser. In this case, to render the phenomenon sensible, it is sufficient to trace two bands of lines at right angles on a silver surface, and to observe the reflected rays almost normally, the source of light being a white surface equally illuminated. The direction of the lines being perpendicular in the two bands, and each band polarizing the light perpendicularly to its direction, it results that the two bands give opposite polarizations, and that the phenomenon becomes more sensible by contrast.

The same effect may be observed, without much difference, whether the incident rays are normal or more or less oblique, the reflected rays invariably possessing a partial polarization sensibly perpendicular to the direction of the lines.

This phenomenon is rendered very obvious on striating a space of some centimetres diameter on a polished plate by means of a lathe; the portion covered with concentric lines, being regarded with an analyser, presents two dark tufts similar to the tufts discovered by M. Haidinger.

This phenomenon may be rendered still more visible by uniformly furrowing the entire surface of two mirrors, placing them parallel and opposite to each other, and causing light to be reflected repeatedly from one to the other: at each of these reflexions an additional portion of light is polarized; so that after several reflexions in a direction which may be as near the normal as possible, the polarized light greatly exceeds the nonpolarized in quantity.

This polarization of the regularly reflected ray may be observed with different metals without sensible modification,—the metals which have the least effect in producing ordinary polarization, such as gold and silver, giving results sensibly the same as those produced by the metals that polarize the most, such as platinum and zinc. This result seems to preclude us from attributing these effects to partial polarization by the sides of the furrows, which is the first idea which naturally occurs to us. Such reflexions appear at least not to be the principal cause of the phenomenon.

In order to advance another step in the study of these singular phenomena, it was necessary to ascertain the exact dimensions of the small furrows which possess properties so singular. For this purpose recourse was had to exceedingly thin layers of silver deposited on glass from certain chemical solutions, and which may not only be used to replace the tin amalgam for silvering looking-glasses, but have even been employed with success by M. Foucault in the construction of a new species of telescope.

The first glass plate, A, was covered with a very thin, though still perfectly opake layer of silver, and a straight band of striæ was produced on the metallic surface by means of a piece of cork covered with very fine emery. This band presented the phenomena above described, viz. polarization of the dispersed light parallel to its own direction, and polarization of the reflected light perpendicular to the same. On examining, by means of the microscope, the state of the striated layer by transmitted light, it was easy to perceive that the furrows had not in general penetrated through the metallic layer, some deeper than the rest forming exceptions.

In order to ascertain the thickness of the metallic layer, a piece of iodine was placed on one point of the surface, and the coloured rings due to the film of iodide of silver produced by the evaporation of the iodine were suffered to spread from this point, which itself became completely transparent, owing to the change of the silver layer throughout its entire thickness into yellow iodide. From the part where the silver had not been in any degree affected by the emanations of iodine, to the transparent spot of iodide, there was a series of coloured rings commencing with white, and which, on being observed through a red glass, appeared to be nine in number. The series terminated in the middle of the ninth brilliant ring. The index of refraction of the iodide of silver being 2·246 (as deduced from the angle of polarization, which was 66°), the ninth bright ring gave for the iodide of silver a thickness of $\frac{1}{838}$ millim. From the thickness, composition, and known density of the iodide, the thickness of the layer of silver was calculated to be $\frac{1}{3400}$ millim.

A second glass plate, B, was covered with silver-leaf, opake like the former, except for rays of more than a certain degree of intensity; for on looking at the sun through this plate, the body of that luminary was visible, stripped of his rays, and of a rich blue colour. I am persuaded that this silver-leaf contained a small quantity of gold.

The metallic surface so produced was found too unequal and too loosely adherent to the glass to be scratched with emery like the former. On being rubbed, however, with pure cotton, the polarization of the dispersed light was very distinct. Under the microscope it appeared to be torn in every direction, so that nothing could be inferred from this experiment; nevertheless, for the sake of comparison, the thickness of the metal was determined by the same means as before. In this case the rings were four in number, the layer becoming transparent in the middle of the fourth, whence it was concluded that the thickness of the metal was not more than $\frac{1}{9068}$ millim. This extreme thinness of beaten silver-leaf agrees with what we know with respect to the gold leaves used in gilding; these leaves, which, as is well known, are transparent and of a green tinge, have a medium thickness of about $\frac{1}{10,000}$ millim., as I ascertained by weighing measured quantities of them. Three different specimens gave the values 0·000108, 0·000095, and 0·000091 millim.

Returning then to the employment of glass plates covered with a layer of silver chemically deposited, one was obtained (which we will call C) not so thick as the former, as was proved by its greater transparency. The light transmitted by this plate was of a greyish blue, the reflected light of a yellowish blue, with this property, that towards the angle of maximum polarization it became of a pure blue for light polarized normally to the plane of reflexion. The rings of iodide formed on this surface terminated in the middle of the second bright line, beyond which the layer was completely transparent, from which it was concluded that the thickness of the silver layer was $\frac{1}{27,200}$ millim. This metallic film, when scratched with the same emery as the former, polarized the dispersed light very distinctly. On being examined microscopically, a very large number of the furrows seemed to have penetrated through the metal; but the majority did not, and consequently must have been less than $\frac{1}{27,200}$ millim. deep. It was ascertained also that the lines which, when illuminated obliquely, produced a distinct polarization in their own direction, were of the latter kind.

Lastly, a fourth layer of silver, D, was prepared thinner than all the others; it was more transparent than the last, and the light reflected by it possessed the same properties to a greater degree. Scratched like the former, the dispersed light was feebler, but still quite sensible and polarized as before. On being viewed with the microscope, it was found that the number of furrows that penetrated entirely through the metal was much greater than before; but that the polarization of the dispersed light was still due solely to those striæ which were merely superficial, and consequently were less than $\frac{1}{54,410}$ millim. deep,—such being the thickness of the silver, as estimated by the coloured rings of iodide, which terminated in the middle of the first dark line.

A general view of the results just described naturally leads one to anticipate that light may suffer changes of the same nature as that above described when it traverses extremely fine slits. The experiments, with a description of which I am about to close this memoir, prove that this is the case, and that under these circumstances also phenomena are produced closely related to the preceding.

It is well known that, to reproduce certain experiments of interference and refraction, small instruments are constructed which are to be found in all physical cabinets: these are slits with narrow walls and straight parallel edges, which can be approximated to each other from a distance of several millimetres to actual contact. If a pencil of light be suffered to pass through such a slit, the opening of which has been reduced to such a degree as to suffer but a trace of the light to pass, the emergent ray is invariably observed to be polarized in a direction perpendicular to the slit, the polarization being stronger in proportion as the slit is narrower.

If a very bright light be employed, and if the observation be conducted with a microscope, openings still narrower may be observed; and by slightly inclining one edge to the other, a slit may be obtained in the field of the microscope which gradually decreases until the edges actually meet. Now in this case the light which passes near the point of contact is almost entirely polarized in a direction perpendicular to the slit.

This phenomenon was at first attributed to the repeated reflexions of the light from one side of the slit to the other, reflexions which necessarily give rise to several phenomena of polarization; but we shall see directly that there are circumstances hard to reconcile with this explanation.

The first trials were made with a slit whose edges were of brass; steel, copper, and, lastly, silver were substituted for this metal: the phenomenon was but little modified, and that in a way which by no means agreed with the polarizing power of these different metals. Silver, for example, which by itself has but little polarizing power, polarizes almost completely the light which traverses a very narrow slit of which it forms the sides.

Moreover, in reducing the thickness of the sides so as to render them mere edges, the phenomenon is equally produced, and it then becomes difficult to imagine the existence of reflexions sufficiently numerous to cause the observed effect.

Bodies of the most dissimilar nature, so disposed as to present a narrow slit, give rise to the same phenomenon, provided the sides of the slit be well polished. Flint, glass, obsidian, ivory, fluor-spar, have in this respect afforded almost the same result.

Having observed that the edges of the slit require to be well polished, which agrees with the theory that the polarization is due to repeated reflexions, the effect of removing this cause of polarization was tried, the edges of the slit being covered with lampblack. Under these circumstances the polarization was entirely destroyed, and the idea of repeated reflexions seemed thus to be confirmed, only, however, to be again overthrown; since on restoring the polish of one side only, the other being still covered with lampblack, the polarization reappeared distinctly, and in this case the existence of repeated reflexions seemed impossible. This experiment, taken in conjunction with the preceding, seems to show that there is some particular cause for the phenomenon.

In order to throw some light on this subject, the attempt was made to examine the phenomenon when the light undergoes total reflexion from the sides of the slit, such reflexion having, as is well known, no polarizing effect.

After several unsuccessful attempts, no better method was discovered than that of observing the edge of a soap-bubble formed in a straight tube. Under these circumstances the soap-bubble forms a very thin film of liquid at the centre, perpendicular to the axis of the tube, and terminated by two opposite concave menisci. Placing this under the microscope and illuminating it properly, it was easy to distinguish through the walls of the tube a bright line, formed by the light that had traversed the film, either directly, or having undergone none but total reflexions from its sides.

When the bubble was thick, the light thus transmitted exhibited no sensible polarization; but when the bubble was sufficiently thin to give rise to coloured rings by reflexion, partial polarization was invariably found present in the bright line, the plane of polarization being, as before, perpendicular to the direction of the slit. This observation was repeated in various ways, and always with the same result.

Finally, the slits in the thin layers of gold and silver mentioned above, were examined with regard to the above phenomena.

Leaves of beaten gold were at first examined with the microscope: they presented numerous rents and slits that offered no particular characteristics; but in some of the leaves, especially in their thicker parts, natural fissures were discovered extremely narrow, certainly less than $\frac{1}{2000}$ millim. across, which were clearly polarized, especially towards their narrowest ends, the plane of polarization being, as before, perpendicular to the direction of the slit. These polarized slits are rather rare in gold-leaf; and I never found them in the thinner and more transparent parts, but only in those whose thickness cannot be much less than $\frac{1}{3000}$ millim. This property seems to indicate that the phenomenon is only produced when the layers have a certain thickness, below which it is insensible.

The same conclusion was come to on observing the thin layers of silver already mentioned. In fact layers C and D, which were excessively thin and transparent, exhibited no sensible polarization even in their finest slits; while the somewhat thicker and more opake layer A, whose thickness amounted to $\frac{1}{3400}$ millim., exhibited the phenomenon in a remarkable manner, and with certain curious peculiarities. Some of the narrowest of the lines which penetrated through the entire thickness of the metal were polarized, some partially, others almost totally, the plane of polarization being still perpendicular to their length. On employing solar light, the phenomenon of coloured light appeared in conjunction with that of polarization; for on observing the polarized lines with a doubly-refracting prism, the two images appeared in certain cases of complementary colours.

I believe I have now communicated the principal facts which I have discovered relatively to the polarization,

1. Of light dispersed from furrowed metal surfaces,
2. Of the rays regularly reflected by such surfaces,
3. Of light which has traversed very narrow slits.

With the permission of the Academy I shall for the present content myself with the foregoing statement of facts, without entering on the premature and, as yet, uncertain question of the causes and connexion of the phenomena above detailed.

LXVII. *On the Cubical Compressibility of certain solid Homogeneous Bodies.* By M. G. Wertheim[*].

In 1848 I published a memoir on the proportion between the elongation and transverse contraction of a homogeneous isotropic elastic bar when subjected to longitudinal traction. After having called attention to the fact that the value of this ratio as determined by Poisson's analysis, viz. $\frac{1}{4}$, had never been confirmed by any conclusive experiments, I shewed in the case of certain substances which I was enabled to submit to direct

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[*] Translated from the *Comptes Rendus*, vol. li. p. 969.

experiment by means of a method susceptible of almost any degree of accuracy, that this ratio was really $\frac{1}{3}$. Subsequent experiments, less direct, but made on a greater variety of bodies, have confirmed this result.

These researches have given rise to numerous discussions. Several distinguished geometricians, without repeating my experiments, and without disputing their accuracy, have endeavoured to bring them into accordance with the ancient theory by various and, unfortunately also, very arbitrary hypotheses. I shall shortly mention and discuss these hypotheses before describing my new experiments on this subject.

In a memoir published shortly after mine, M. Clausius expressly acknowledged that the bodies on which I had experimented may be considered as truly homogeneous and isotropic; but he thinks that the secondary elasticity discovered by M. Weber in silk threads, and which I have observed in several organic bodies, may serve to explain this disagreement between experiment and the ancient theory. This secondary action being added to the true or primary elongation, will give rise to an excessive actual elongation, the numerator of the fraction being thus increased to such a degree that, though really equal to $\frac{1}{4}$ when only the primary elongation is considered, it in fact approximates to $\frac{1}{3}$.

Against this explanation it may be objected, in the first place, that it is founded on a fact absolutely hypothetical, no one having yet observed this secondary action either in metals or in glass, which are the only bodies I experimented on. Some experiments of M. Weber are, it is true, appealed to, according to which the transverse note produced by a metallic wire which has been suddenly stretched, becomes lower for several seconds. Seebeck has in fact endeavoured to explain this phenomenon on the ground of the secondary action above mentioned—contrary, however, to the very plausible opinion of M. Weber himself, who attributes it solely to the diminution of the temperature of the wire produced by its elongation, and its gradual return to the temperature of the surrounding atmosphere. But even admitting the hypothesis of M. Seebeck, this lowering of the note is at all events far too small for its corresponding secondary elongation to account for the numerical result of my experiments; and M. Clausius is therefore obliged to suppose that, in the case of metals, this species of elongation principally takes place during the first quarter of a second, and consequently produces no sensible effect on the note. But the primary action itself not being instantaneous, how is it possible to fix the limit of time beyond which the effect ought to be considered as secondary? It is thus that hypotheses accumulate.

A still graver objection to this hypothesis is that, contrary to all our theoretical notions and all the results of experience, we should be obliged to suppose that this secondary elongation produces no corresponding transverse contraction, since otherwise the ratio between the two observed quantities, namely the total longitudinal elongation and the total transverse contraction, would always remain that indicated by the ancient theory.

I have been obliged to enter into these details in consequence of the perseverance with which certain physicists have for twelve years opposed this theory to mine, and the manner in which they insist on treating as a demonstrated scientific truth that which M. Clausius himself only regarded as a hypothesis, and one indeed to which no great importance was to be attached.

MM. Lamé and Maxwell admit that the ratio above defined, or, what comes to the same thing, the ratio between the cubic and linear compressibilities, may vary in different substances. Experience alone can determine whether this is the case, as I have not failed to remark, both in my original memoir, and in several of those I have since published. M. Verdet is therefore wrong in asserting, as he does in the extract of a memoir which we shall have to discuss hereafter, and of which M. Kirchhoff is the author, that I have "endeavoured to show by numerous experiments that this ratio has in all bodies the same constant value $\frac{1}{3}$." On the contrary, while affirming and maintaining the exactitude of this value for those bodies which were the subject of my researches, I excluded those not yet submitted to experiment.

According to an interesting experiment made by M. Clapeyron on vulcanized caoutchouc, the fraction $\frac{\lambda}{\mu}$, instead of being equal to 1 according to the ancient theory, or 2 as my experiments require, attains in the case of this substance the enormous value of 2201; this fact, to which we shall return hereafter, seems to me to be explained by the results of the present memoir.

Contrary to the opinion of M. Clausius, M. de Saint-Venant attributes the disagreement between the results of my experiments and the ancient theory to a want of isotropism in the bodies on which I experimented: the author thinks "that there are as many species of mechanical homogeneousness as there are of possible curvilinear systems of coordinates, or of systems of conjugate orthogonal surfaces"—in fact, that we may imagine as many species as we please of non-isotropic homogeneousness; but what he has failed to show is that any such heterotropy really exists in the bodies I experimented on, and, which is absolutely incredible, that it exists to the same degree in them all.

But without going so far as this, and without comparing bodies chemically different, if we attribute to a body one of the species of homogeneousness imagined by M. de Saint-Venant, as for instance cylindrical or spherical homogeneousness, or any other, we ought at least to be able thus to explain the results of the various experiments to which these bodies may be submitted.

It would, for example, be easy to invent a molecular arrangement such that a cylindrical piezometer would possess a cubical compressibility conformable to that given by the ancient theory; but it would be necessary to show also that this cylinder, when subjected to longitudinal traction, would exhibit the elongation, and at the same time the transverse contraction, which is shown by experiment, that its resistance to torsion might be determined beforehand, &c.

As long as this demonstration has not even been attempted, all discussion on these hypotheses is necessarily futile.

Lastly, M. Kirchhoff has just published an important memoir on this point, which it will be necessary for me to examine with the degree of attention due to the name of the author and the interest of the subject. Instead of indulging in mere conjectures, M. Kirchhoff has devised the following experiment:—A weight applied to the end of a lever produces simultaneously the flexion and torsion of a homogeneous cylinder; these two displacements are measured exactly by means of an ingenious application of Gauss's method; and their ratio, which is independent of the modulus of elasticity and the radius of the cylinder, gives by known formulæ the required relation between the elongation and transverse contraction.

This method is liable to numerous objections. It would be difficult to imagine one more indirect and consequently more subject to error: the coefficient of the change of volume is determined by two distortions, which are themselves unaccompanied by any change of volume whatever; this at least is what is assumed in order to establish the formulæ, though it is not rigorously true; the experiment may be considered as the flexion of a cylinder which has been rendered non-homogeneous by torsion, or the torsion of a cylinder rendered heterogeneous by flexion; and the ordinary formulæ for torsion and flexion, which are inexact in themselves (as I think I have sufficiently shown in the case of the former, and as I shall hereafter endeavour to prove of the latter), become still less trustworthy in the present case.

M. Kirchhoff's apparatus is one of great delicacy, and does not seem as if it could possess sufficient stability for researches of this nature; the small dimensions of the cylinders subjected to experiment (less than 3 millims. in diameter, and only 145 millims. long), the sensible flexions produced originally by the mirrors and the levers supported by the cylinders, the necessity of soldering the latter in the middle, and lastly the complication of the calculations necessary to reduce the observations, are so many sources of inconvenience and error.

The following are, however, the results of these experiments: M. Kirchhoff found for yellow brass the value 0·387, for tempered steel 0·294: these numbers are, it will be seen, decidedly greater than $\frac{1}{4}$, while $\frac{1}{3}$ is very nearly equal to their mean value. M. Kirchhoff passes somewhat lightly over the former of these results, while he attaches great importance to the second: tempered steel appears to him to be a body eminently isotropic, while yellow brass is neither sufficiently homogeneous nor entirely free from the secondary effect before spoken of.

We have already done justice to the latter observation, which moreover applies as well to steel as it does to brass, since this secondary effect has in fact been observed in neither. As far as isotropism is concerned, it is very gratuitous to attribute that property peculiarly to a tempered body: the action that tempered glass exercises on polarized light abundantly proves this; and indeed if the least homogeneous among non-crystallized bodies were required, it would undoubtedly be on a tempered substance that the choice ought to fall.

I am far, then, I repeat, from affirming that this ratio ought not to be somewhat less than $\frac{1}{3}$ in the case of homogeneous steel; but the present experiment does not seem to me to be conclusive on the subject.

The experiment, on the contrary, made on yellow brass is the first in which my results have been attempted to be verified on one of the substances I myself employed. The number 0·387 is, it is true, greater than $\frac{1}{3}$; but I shall prove, in a memoir on flexion I am about to produce, that the denominator of the fraction which represents in M. Kirchhoff's results the ratio of the torsion to the flexion, is too little, and that this fraction, properly corrected, approaches much more nearly to the value $\frac{1}{3}$.

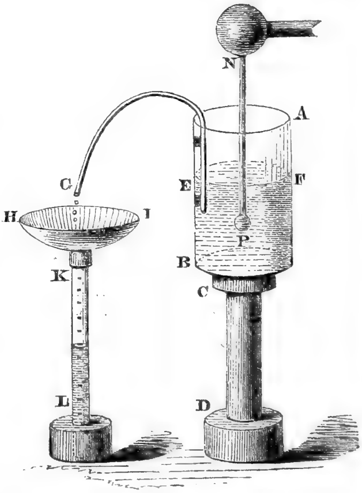
In short, setting aside for the moment the experiment of M. Clapeyron, no fact has hitherto been advanced to show that the required relation is different in different bodies. The experiments that have been made, moreover, refer only to a small number of bodies; they have been made by means of methods all more or less indirect; and the cubical compressibility has never itself been the subject of any direct experiment; so that we know not whether the proportion supposed to exist between the pressures and the diminutions of volume really does exist for changes of pressure, however small. This research will be the subject of the memoir which I shall shortly have the honour of submitting to the Academy.

LXVIII. *On a New Electrometer (the Siphon Electrometer) for measuring the Electrical Charge of the Prime Conductor of a Machine; and on the Dispersion of different Liquids by Electrical Action.* By THOMAS TATE, *Esq.*[*]

THE electrometer most commonly used by electricians, for ascertaining the intensity of the charge of the prime conductor of a machine, is Henley's. According to the experiments of Sir W. Snow Harris, it appears that the degrees of divergence of this instrument for high charges are nearly in proportion to the quantities of electricity generated; at the same time it must be observed that this result does not agree with what theoretical investigation would give. This instrument therefore would serve very well in all ordinary cases for indicating the power of any machine, provided that all the instruments used were constructed exactly alike; but this is practically impossible; and hence it follows that the degree of divergence of one instrument cannot be compared with that of another instrument, as regards its indication of electrical charge. The hydrostatic and balance electrometers are very complete instruments as regards scientific research; but it must be allowed that they are too delicate in their construction and mode of action to be used on ordinary occasions, when only an approximate value of an electric charge is required; moreover, the thermo-electrometer is only applicable to high charges of an electrical battery.

The siphon electrometer, represented in the annexed diagram, is sufficiently delicate and reliable in its indications, and admits of being constructed so that the results derived from one instrument may be fairly compared with those derived from another instrument. It depends on the principle, that different quantities of electricity discharge different quantities of liquid from a siphon-tube in which the liquid is suspended by capillary action.

A B a glass jar, containing water, about 4 inches in diameter, placed upon the insulating stand C D of gutta percha; E G a small siphon about ·15 of an inch diameter, cemented to the side of the jar, as shown in the diagram; H I a funnel-shaped receiver



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[*] Communicated by the Author.

about 3 inches diameter, connected with the ground by a damp cord, and placed directly below the orifice G of the tube, and connected with a glass tube, K L, divided into tenths and hundredths of a cubic inch; N P a conducting wire fixed to the prime conductor of the electrical machine and dipping into the liquid E F. The instrument is used in the following manner:—

A sufficient quantity of water is poured into the jar, so as to cause the siphon to act; the water then flows through the siphon until its pressure in the jar is balanced by the capillary action of the tube G E, when it will cease to flow; it will then be found that the level of the water in the jar stands somewhat above the orifice G of the siphon-tube: scarcely any amount of shaking or oscillation will now cause the water to flow from the orifice G. The graduated tube K L is then placed below the orifice G, the bottom of the funnel being from $2\frac{1}{4}$ to $2\frac{1}{2}$ inches from this orifice. The machine is then turned, and the electric action causes the water to flow in a continuous stream or jet from the orifice G, filling the tube K L; any proposed number of revolutions being given to the machine in a known time, the number of cubic inches of water discharged is taken as the measure of the efficiency of the machine.

It will be hereafter shown, the machine being in a fixed state of action, that in order to produce a given or constant discharge of liquid, the product of the number of revolutions of the machine by the time in which these revolutions are made must be a constant quantity. Thus, if n revolutions, performed in t seconds, produce a discharge of k cubic inches of water, and n_1 revolutions, performed in t_1 seconds, produce the same discharge of liquid, then

$$nt = n_1 t_1.$$

Hence it follows that (within certain limits) the relative efficiency of a machine (in different states of action, or of different machines) will be inversely as the product of the number of revolutions by the time requisite for producing a given or fixed amount of discharge. Thus, if a machine discharges one-half of a cubic inch of water in 20 revolutions per 60 seconds, and another machine discharges the same amount of water in 15 revolutions per 40 seconds, then the powers of the machines will be as $\frac{1}{20 \times 60}$ to $\frac{1}{15 \times 40}$, or as 1 to 2.

The following results of experiments show the uniformity of the action of the instrument.

24 revolutions of the machine, in 45 seconds, produced a discharge of ·61 of a cubic inch of water; and the experiment being repeated for two successive times, the discharges were found to be ·62 and ·61.

When the machine was in a higher state of action, 20 revolutions, in 60 seconds, produced a discharge of 1·06 cubic inch; and upon repeating the experiment the discharge was found to be 1·04 cubic inch.

Within certain limits, the quantity of liquid discharged is not sensibly affected by the diameter of the tube G E, or by the distance of the cup from the nozzle G: other things being the same, the diameter of the tube may vary from $\frac{3}{20}$ths to $\frac{4}{20}$ths of an inch; and the distance of the cup from the nozzle may vary from $2\frac{1}{4}$ to $2\frac{1}{2}$ inches without sensibly affecting the amount of discharge. In like manner, slight variations in the diameter of the jar produced little or no sensible alteration in the amount of the liquid discharged.

At the commencement of the operation, the columns of liquid being in equilibrium, the electrical action has simply to overcome the cohesion of the particles of the liquid for one another; but as the process goes on, the water being more and more discharged, the equilibrium of the columns is destroyed, and the resistance to the discharge increases, so that, with the same electric force, the rate of discharge of the water becomes less and less; but when the section of the jar is considerable, and the volume of the liquid displaced does not exceed a certain limit, the velocity with which the liquid is discharged is nearly uniform.

The following experimental results show that, for *equal quantities of water discharged* (*the machine being in a fixed state of action*), *the product of the number of revolutions by the corresponding time is* (*approximately*) *a constant quantity.*

Experiment I.

Number of revolutions of the machine, n.	Corresponding time in seconds, t.	Corresp. discharge in parts of a cubic inch, k.	Value of $n \times t$.
15	70	·69	1050
25	42	·70	1050
18	60	·69	1080
12	90	·69	1080

The following experiment was made when the machine was in a different state of action.

Experiment II.

24 revolutions of the machine, performed in 80 seconds, produced a discharge of ·72 cubic inch of water; and 33 revolutions, in 60 seconds, produced the same amount of discharge.

In this case $24 \times 80 = 1920$, and $33 \times 60 = 1980$.

Experiment III.

10 revolutions, performed in 50 seconds, produced a discharge of ·41; and 20 revolutions, in 25 seconds, produced nearly the same discharge.

Here $10\times 50=20\times 25=500$.

Now, assuming that the quantity of electricity generated is in proportion to the number of revolutions of the machine, then it follows that a certain quantity of electricity, acting for 50 seconds, produces the same (or nearly the same) discharge as a double quantity of electricity acting for half the time; and so on, similarly to the results of the other experiments.

If ϵ be put for the quantity of electricity generated in t seconds, and ϵ_1 for the quantity generated in t_1 seconds, then for equal amounts of discharge we shall have

$$\epsilon\times t=\epsilon_1\times t_1;$$

that is, *for equal amounts of discharge the quantities of electricity are in the inverse ratio of the times.*

In Experiment III. the angular velocities of the machine are as 1 : 4, and therefore the intensities of the electricity generated would be in the same ratio, provided that no electricity had been carried off by the discharge of the liquid; but the deflections of Henley's electrometer indicated that the ratio of the intensities of the electricity in the two states of the conductor was only about 2 to 3.

Although equal volumes of water are discharged, we cannot infer that the dynamic effects are equal; for the liquids are respectively discharged with different velocities.

Let k = the cubic inches of water discharged in each case, w being its weight in units of lbs.

n = the corresponding number of revolutions in t seconds in the one case, ϵ being the amount of electricity generated, and v the velocity with which the liquid is discharged.

n_1 = the corresponding number of revolutions in t_1 seconds in the other case, ϵ_1 being the quantity of electricity generated, and v_1 the velocity with which the liquid is discharged.

u, u_1 = the accumulated work or dynamic effect in each case respectively.

Then $u=\dfrac{v^2w}{2g}$, and $u_1=\dfrac{{v_1}^2w}{2g}$;

$$\therefore \frac{u}{u_1}=\left(\frac{v}{v_1}\right)^2;$$

but $\frac{v}{v_1}=\frac{t_1}{t}$, on the assumption that the velocities are uniform,

$$\therefore \frac{u}{u_1}=\left(\frac{t_1}{t}\right)^2.$$

Now by experiment we have

$$nt=n_1t_1, \text{ and } \therefore \frac{n}{n_1}=\frac{t_1}{t};$$

$$\therefore \frac{u}{u_1}=\left(\frac{n}{n_1}\right)^2;$$

that is, *the discharge being constant, the dynamic effects are in the ratio of the squares of the number of revolutions of the machine.*

If $n=20$, and $n_1=10$, as in Exp. III., then $\frac{u}{u_1}=4$; that is, in this case the dynamic effects will be as 1 to 4, or a double number of revolutions produces a quadruple effect.

Let x be put for the number of revolutions of the machine (its state being constant) performed in 60 seconds, or 1 minute, to produce the same discharge as n revolutions in t seconds; then

$$x\times 60=nt, \text{ or } x=\frac{nt}{60}.$$

Similarly, we have for the relation of equal discharge corresponding to any other state of the machine,

$$x_1=\frac{n_1t_1}{60}.$$

Now it may be presumed that the efficiency of the machine is inversely proportional to the number of revolutions per minute requisite to produce a given discharge; but we find

$$\frac{x_1}{x}=\frac{n_1t_1}{nt};$$

that is, *the efficiency of a machine varies inversely as the product of the number of revolutions by the corresponding time requisite to produce a given discharge.*

If $t=t_1$, then

$$\frac{x_1}{x}=\frac{n_1}{n};$$

that is, in this case *the efficiency of a machine varies inversely as the number of revolutions* (the time being constant) *requisite to produce a given discharge.*

Thus, if a machine discharges half of a cubic inch of water in twenty revolutions in a certain time, and another machine discharges the same amount of water in ten revolutions in the same time, then the latter machine will have double the power of the former.

On the Dispersion of different Liquids by Electrical Action.

The siphon-electrometer enables us to determine the rate at which electrical charges will disperse different liquids. The liquid to be examined being placed in the jar A B, and the siphon being brought to act in the usual manner, the discharge produced by a given number of revolutions of the machine in a given time is determined; and having previously found the amount of pure water discharged by the same number of revolutions performed in the same time, we are enabled to estimate the dispersiveness of the particular liquid, as compared with that of pure water, under the same electrical action. In this manner, saturated solutions of chloride of sodium, carbonate of soda, and other salts were examined; and it was found that, *under the same electrical action, the volumes of the liquid discharged were in the inverse ratio of their specific gravities.* It will be observed that these liquids are all good conductors of electricity. But the case is very different with respect to liquids which are imperfect conductors of electricity, such as turpentine, fixed oils, and alcohol.

In twenty revolutions per minute the discharge of pure water was about three-fourths of a cubic inch; whereas with the same electrical action only about one-fourth of a cubic inch of turpentine was discharged, and not more than one-tenth of a cubic inch of fixed oil. Under the same electrical action, the volume of alcohol discharged did not exceed one-fifth of a cubic inch. Now although the specific gravities of these liquids are less than that of water, yet their dispersiveness, under the same electrical action, is considerably less than that of water.

Hastings, May 18.

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 393.]

June 14, 1860.—General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communications were read:—

"Notes of Researches on the Poly-Ammonias."—No. VIII. Action of Nitrous Acid upon Nitrophenylenediamine. By A. W. Hofmann, LL.D., F.R.S.

The experiments of Gottlieb have shown that dinitrophenylamine, when boiled with sulphide of ammonium, is converted into a remarkable base, crystallizing in crimson needles, generally known as nitrazophenylamine, and for which, in accordance with the views I entertain regarding its constitution, I now propose the name Nitrophenylenediamine. I owe to the kindness of Dr. Vincent Hall a considerable quantity of this substance, which is not quite easily procured.

I have made a few experiments with this compound in the hope of obtaining some insight into its molecular constitution. If, bearing in mind the numerous analogies between the radicals ethyle and phenyle, we assume that the latter, by the loss of hydrogen, may be converted into a diatomic molecule, phenylene $C_6\,H_4$, corresponding to ethylene, the existence of a group of bases corresponding to the ethylene-bases cannot be doubted.

$$\text{Ethylamine}\quad \left.\begin{matrix}C_2\,H_5\\ H\\ H\end{matrix}\right\}N^{*}.\qquad \text{Ethylenediamine}\quad \left.\begin{matrix}(C_2\,H_4)''\\ H_2\\ H_2\end{matrix}\right\}N_2.$$

$$\text{Phenylamine}\quad \left.\begin{matrix}C_6\,H_5\\ H\\ H\end{matrix}\right\}N.\qquad \text{Phenylenediamine}\quad \left.\begin{matrix}(C_6\,H_4)''\\ H_2\\ H_2\end{matrix}\right\}N_2.$$

With the last-named body agrees in composition the compound known as semibenzidam, or azophenylamine, which Zinin obtained by exhausting the action of sulphide of ammonium on dinitrobenzole.

Those chemists, however, who have had an opportunity of becoming acquainted with the well-defined properties of ethylenediamine, will not be easily persuaded to consider the uncouth dinitrobenzol-product—sometimes appearing in brown flakes, sometimes as a yellow resin, rapidly turning green in contact with the air—as standing to smooth phenylamine in a relation similar to that which obtains between ethylenediamine and ethylamine; we much more readily admit a relation of this description between phenylamine and Gottlieb's crimson-coloured base, in which the clearly pronounced character of the former is still distinctly visible, although of necessity modified by the further substitution which has taken place in the radical.

$$\text{Phenylamine}\qquad \left.\begin{matrix}C_6\,H_5\\ H\\ H\end{matrix}\right\}N.$$

$$\text{Phenylenediamine}\qquad \left.\begin{matrix}(C_6\,H_4)''\\ H_2\\ H_2\end{matrix}\right\}N_2.$$

$$\text{Nitrophenylenediamine}\qquad \left.\begin{matrix}(C_6[H_3\,(NO_2)])''\\ H_2\\ H_2\end{matrix}\right\}N_2.$$

Does the latter formula really represent the molecular constitution of the crimson needles? The degree of substitution of this body might have been determined by the frequently adopted process of ethylation. But even a simpler and a shorter method appeared to present itself in the beautiful mode of substituting nitrogen in the place of hydrogen, lately discovered by P. Griess. The red crystals

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* $H=1$; $O=16$; $C=12$, &c.

undergo, indeed, the transformation, which he has already proved for so many derivatives of ammonia, with the greatest facility.

On passing a current of nitrous acid into a moderately concentrated solution of the nitrate of the base, the liquid becomes slightly warm, and deposits on cooling a considerable quantity of brilliant white needles, the purification of which presents no difficulty: sparingly soluble in cold, readily soluble in boiling water, the new compound requires only to be once or twice recrystallized. Thus purified, this substance forms long prismatic crystals, frequently interlaced, white as long as they are in the solution, but assuming a slightly yellowish tint when dried, and especially when exposed to 100°: they are readily soluble both in alcohol and in ether. The new body exhibits a distinctly acid reaction; it dissolves on application of a gentle heat in potassa and in ammonia, without, however, neutralizing the alkaline character of these liquids; it also dissolves in the alkaline carbonates, but without expelling their carbonic acid. The new acid fuses at 211° C., and sublimes at a somewhat higher temperature, with partial decomposition. The sublimate consists of small prismatic crystals.

Analysis proves this substance to contain

$$C_6 H_4 N_4 O_2,$$

a formula which is confirmed by the analysis of a silver-compound,

$$C_6 (H_3 Ag) N_4 O_2,$$

and of a potassium-salt,

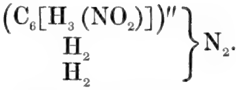
$$C_6 (H_3 K) N_4 O_2.$$

The analysis of the new compound shows that, under the influence of nitrous acid, nitrophenylenediamine exchanges three molecules of hydrogen for one molecule of nitrogen, three molecules of water being eliminated.

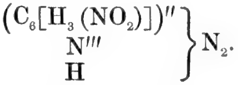
$$\underbrace{C_6 H_7 N_3 O_2}_{\text{Nitrophenylene-diamine.}} + H NO_2 = 2 H_2 O + \underbrace{C_6 H_4 N''' N_3 O_2}_{\text{New acid.}}.$$

I do not propose a name for the new compound, which can claim but a passing interest, as throwing, by its formation, some light on the constitution of nitrophenylenediamine.

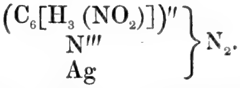
The composition of the new acid, and of its salts, shows that in the crimson-red base four hydrogen molecules are still capable of replacement; in other words, that this body contains four extra-radical molecules of hydrogen. The result of these experiments appears to confirm the view which, in the commencement of this Note, I have taken of the constitution of this body; at all events, the mutual relation of the several bodies is satisfactorily illustrated by the formulæ—

Nitrophenylenediamine 

$$\left.\begin{matrix}(C_6[H_3(NO_2)])'' \\ H_2 \\ H_2\end{matrix}\right\} N_2.$$

New acid 

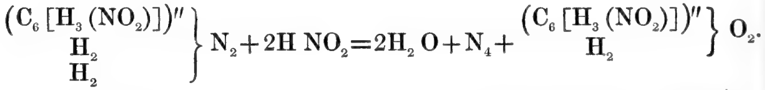
$$\left.\begin{matrix}(C_6[H_3(NO_2)])'' \\ N''' \\ H\end{matrix}\right\} N_2.$$

Silver-salt 

$$\left.\begin{matrix}(C_6[H_3(NO_2)])'' \\ N''' \\ Ag\end{matrix}\right\} N_2.$$

If the admissibility of this interpretation be confirmed by further experiments, the reaction discovered by Griess furnishes a new and valuable method of recognizing the degree of substitution in the derivatives of ammonia.

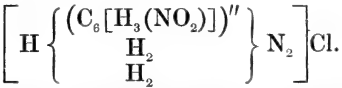
The new acid differs in many respects from the substances produced from other nitrogenous compounds. As a class, these substances are remarkable for the facility with which they are changed under the influence of acids, and more especially of bases. The new acid exhibits remarkable stability; it may be boiled with either potassa or hydrochloric acid without undergoing the slightest change. Even a current of nitrous acid passed into the aqueous or alcoholic solution is without the least effect. The latter experiment appeared of some interest; for if the action of nitrous acid, in a second phase of the process, had assumed the form so frequently observed by Piria and others, it might have led to the formation of the diatomic nitrophenylene-alcohol, according to the equation



$$\left.\begin{matrix}(C_6[H_3(NO_2)])'' \\ H_2 \\ H_2\end{matrix}\right\} N_2 + 2H\,NO_2 = 2H_2\,O + N_4 + \left.\begin{matrix}(C_6[H_3(NO_2)])'' \\ H_2\end{matrix}\right\} O_2.$$

It deserves to be noticed that nitrophenylenediamine, although derived from two molecules of ammonia, is nevertheless a decidedly monacid base. Gottlieb's analyses of the chloride, nitrate, and sulphate left scarcely a doubt on this point. However, as some of the natural bases, quinine for instance, are capable of combining with either one or two molecules of acid, I thought it of sufficient interest to confirm Gottlieb's observations by some additional experiments. The crystals deposited on cooling from a solution of nitrophenylenediamine in concentrated hydrochloric acid, were washed with the same liquid and dried *in vacuo* over lime.

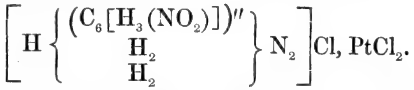
Analysis led to the formula



$$\left[H\left\{\begin{matrix}(C_6[H_3(NO_2)])'' \\ H_2 \\ H_2\end{matrix}\right\} N_2\right] Cl.$$

The dilute solution of this chloride is not precipitated by dichloride of platinum, nor can the double salt of the two chlorides be obtained by evaporating the mixed solutions, which, just as Gottlieb observed it, is readily decomposed with separation of metallic platinum. I had, however, no difficulty in preparing a platinum salt, crystallizing in splendid long brown-red prisms, by adding the dichloride of platinum to the *concentrated* solution of the hydrochlorate.

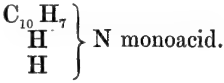
The platinum determination led to the formula



$$\left[H\left\{\begin{matrix}(C_6[H_3(NO_2)])''\\H_2\\H_2\end{matrix}\right\}N_2\right]Cl, PtCl_2.$$

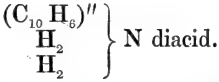
These experiments prove that, even under the most favourable circumstances, nitrophenylenediamine combines only with 1 equiv. of acid, while the ethylene-derivatives are decidedly diacid. The diminution of saturating power in nitrophenylenediamine, at the first glance, seems somewhat anomalous, but the anomaly disappears if the constitution of the body be more accurately examined. It cannot be doubted that the diminution of the saturating power is due to the substitution which has taken place in the radical of the diamine. I have pointed out at an earlier period[*], that the basic character of phenylamine is considerably modified by successive changes introduced into the phenyl-radical by substitution. Chlorphenylamine, though less basic than the normal compound, still forms well-defined salts with the acids; the salts of dichlorphenylamine, on the other hand, are so feeble, that, under the influence of boiling water, they are split into their constituents; in trichlorphenylamine, lastly, all basic characters have entirely disappeared. Again, on examining the nitro-substitutes of phenylamine, we find that even nitrophenylamine is an exceedingly weak base, whilst dinitrophenylamine is perfectly indifferent. What wonder, then, that a molecular system, to which in the normal condition we attribute a diacid character, should, by the insertion of special radicals, be reduced to monoacidity? The normal phenylenediamine, which remains to be discovered, will doubtless be found to be diacid, like the diamines derived from ethylene. Even now the group of diacid diamines is represented in the naphtyl-series:

Naphtylamine



$$\left.\begin{matrix}C_{10}H_7\\H\\H\end{matrix}\right\}N \text{ monoacid.}$$

Naphtylenediamine



$$\left.\begin{matrix}(C_{10}H_6)''\\H_2\\H_2\end{matrix}\right\}N \text{ diacid.}$$

The body which I designate by the term Naphtylenediamine, is the base which Zinin obtained by the final action of sulphide of ammonium upon dinitronaphtaline. This substance, originally designated seminaphtalidam, and subsequently described as naphtalidine, combines, according to Zinin's experiments, with 2 equivalents of hydrochloric acid[†].

"On the Formula investigated by Dr. Brinkley for the general Term in the Development of Lagrange's Expression for the Summation of Series and for Successive Integration." By Sir J. F. W. Herschel, Bart., F.R.S. &c.

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[*] Mem. of Chem. Soc. vol. ii. p. 298.

[†] Liebig's Annalen, vol. lxxxv. p. 328.

June 21.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

"Experimental Researches on various questions concerning Sensibility." By E. Brown-Séquard, M.D.

"On the Construction of a new Calorimeter for determining the Radiating Powers of Surfaces, and its application to the Surfaces of various Mineral Substances." By W. Hopkins, Esq., M.A., F.R.S.

When the author's Memoir on the Conductivity of various substances was presented to the Society, it was intimated to him on the part of the Council of the Society, that it might be advisable to determine absolute instead of relative conductivities, the latter only having been attempted in his previous experiments. It is partly in consequence of this intimation, and partly from the desire to make his former investigations more complete, that the author has given his attention to the construction of a calorimeter which might serve for this purpose. His present memoir contains a description of this instrument, with the results obtained from its application to the surfaces of various substances.

The apparatus used by Messrs. Dulong and Petit was more delicate and complete than the simpler instrument devised by the author of this paper, but it was calculated only to determine the radiating powers of substances of which the bulb of a thermometer could be constructed, or with which it could be delicately coated. The only substances to which, in fact, it was applied, were glass and silver, the radiation taking place, in the first case, from the naked bulb of the thermometer, and, in the second, from the same bulb coated with silver paper. In these cases, too, it was the whole heat radiating in a given time from the instrument, and not that which radiated from a given area, that was determined. For this latter purpose the apparatus was not well calculated, on account of the difficulty of obtaining with accuracy the area of the surface from which radiation took place. The instrument here described can be easily applied to any plane radiating surface, while the area of that surface can be easily determined to any required degree of accuracy. The quantity of heat radiating under given conditions, from a unit of surface in a unit of time, can thus be easily ascertained. The paper contains a detailed description of the instrument, and of the experiments made with it.

The following are experimental results thus obtained,—the unit of heat being that quantity of heat which would raise 1000 grs. of distilled water 1° Centigrade. The formula is that of Dulong and Petit, where

$\theta =$ temperature of the surrounding medium (the air in these experiments), expressed in Centigrade degrees;

$t =$ the excess of the temperature of the radiating surface above that of the surrounding medium, in Centigrade degrees;

$p =$ pressure of the surrounding medium (the atmosphere in these experiments), expressed by the height of the barometer in metres;

$a = 1{\cdot}0077$, a numerical quantity which is always the same for all radiating surfaces and surrounding media.

Then if Q denote the quantity of heat, expressed numerically, which radiates from a unit of surface (a square foot) in a unit of time (one minute), we have the following results for the substances specified:—

Glass.

$$Q = 9{\cdot}566\, a^{\theta}(a^{t}-1) + {\cdot}03720\left(\frac{p}{{\cdot}72}\right)^{{\cdot}45} t^{1{\cdot}233}$$

Dry Chalk.

$$Q = 8{\cdot}613\, a^{\theta}(a^{t}-1) + {\cdot}03720\left(\frac{p}{{\cdot}72}\right)^{{\cdot}45} t^{1{\cdot}233}$$

Dry New Red Sandstone.

$$Q = 8{\cdot}377\, a^{\theta}(a^{t}-1) + {\cdot}03720\left(\frac{p}{{\cdot}72}\right)^{{\cdot}45} t^{1{\cdot}233}$$

Sandstone (building stone).

$$Q = 8{\cdot}882\, a^{\theta}(a^{t}-1) + {\cdot}03720\left(\frac{p}{{\cdot}72}\right)^{{\cdot}45} t^{1{\cdot}233}$$

Polished Limestone.

$$Q = 9{\cdot}106\, a^{\theta}(a^{t}-1) + {\cdot}03720\left(\frac{p}{{\cdot}72}\right)^{{\cdot}45} t^{1{\cdot}233}$$

Unpolished Limestone (same block as the last).

$$Q = 12{\cdot}808\, a^{\theta}(a^{t}-1) + {\cdot}03720\left(\frac{p}{{\cdot}72}\right)^{{\cdot}45} t^{1{\cdot}233}.$$

"On Isoprene and Caoutchine." By C. Greville Williams, Esq.

This paper contains the results of the investigation of the two principal hydrocarbons produced by destructive distillation of caoutchouc and gutta percha.

Isoprene.

This substance is an exceedingly volatile hydrocarbon, boiling between 37° and 38° C.; after repeated cohobations over sodium, it was distilled and analysed. The numbers obtained as the mean of five analyses were as follows:—

Experiment.		Calculation.		
Carbon . . .	88·0	C^{10}	60	88·2
Hydrogen . .	12·1	H^{8}	8	11·8
			68	100·0

Three of the specimens were from caoutchouc and two from gutta percha. The vapour-density was found to be at 58° C. 2·40. Theory requires, for $C^{10}H^{8}$=4 volumes, 2·35. The density of the liquid was 0·6823 at 20° C.

Action of Atmospheric Oxygen upon Isoprene.

Isoprene, exposed to the air for some months, thickens and acquires powerful bleaching properties owing to the absorption of ozone. On distilling the ozonized liquid, a violent reaction takes place between the ozone and the hydrocarbon. All the unaltered hydrocarbon distils away, and the contents of the retort suddenly solidify to a pure, white, amorphous mass, yielding the annexed result on combustion :—

Experiment.		Calculation.		
Carbon . .	78·8	C^{10}	60	78·95
Hydrogen . .	10·7	H^{8}	8	10·52
Oxygen . .	10·5	O	8	10·53
			76	100·00

This directly-formed oxide of a hydrocarbon is unique, as regards both its formula and mode of production.

Caoutchine.

Himly's analysis was correct. The mean results of three analyses are compared in the following Table with those of M. Himly :—

	Mean.	Himly.	Calculation.		
Carbon . .	88·1	88·44	C^{20}	120	88·2
Hydrogen .	11·9	11·56	H^{16}	16	11·8
				136	100·0

Two of the determinations, the results of which are incorporated in the above mean, were made on a substance from gutta percha. The vapour-density was :—

Experiment.	Himly.	Calculation = 4 vols.
4·65	4·46	4·6986

We now for the first time see the relation between the two hydrocarbons. It is the same as between amylene and paramylene. The author discusses the boiling-point of these bodies, and shows that they form most decided exceptions to Kopp's empirical law.

Action of Bromine on Caoutchine and its isomer Turpentine.

Caoutchine and turpentine act on bromine in precisely the same manner. One equivalent of the hydrocarbon decolorizes four equivalents of bromine. To determine this point quantitatively, eight experiments were made, four with turpentine and four with caoutchine. The quantity of bromine-water employed was 20 cub. cents. =0·2527 gramme bromine.

Mean of four turpentine experiments.	Mean of four caoutchine experiments.
0·1074 grm.	0·1091 grm.

Conversion of Turpentine and Caoutchine into Cymole.

By the alternate action of bromine and sodium on caoutchine or turpentine, two equivalents of hydrogen are removed, the final result being cymole, having exactly the odour hitherto considered characteristic of the hydrocarbon obtained from oil of cumin, and quite distinct from that of camphogene. The liquid was identified by the annexed analyses. No. I. was from turpentine, II. and III. from caoutchine.

	Experiment.				Calculation.		
	I.	II.	III.	Mean.			
Carbon . .	89·2	89·5	89·5	89·4	C^{20}	120	89·6
Hydrogen . .	10·5	10·4	10·4	10·4	H^{14}	14	10·4
						134	100·0

Agreeing perfectly with the formula $C^{20}H^{14}$*.

Paracymole.

At the same time that cymole is formed, there is a production of an oil having the same composition, but boiling about 300° C. The author has provisionally named it paracymole.

Action of Sulphuric Acid on Caoutchine.

Sulphuric acid acts on caoutchine, converting it almost entirely into a viscid fluid like hévéène, at the same time a very small quantity of a conjugate acid is formed, having the formula

$$C^{20}H^{16}S^2O^6;$$

the composition was determined from that of the lime salt, which on ignition, &c., gave a quantity of sulphate of lime equal to 8·3 per cent. of calcium; $C^{20}H^{15}CaS^2O^6$ requires 8·5.

The author considers the action of heat on caoutchouc to be merely the disruption of a polymeric body into substances having a simple relation to the parent hydrocarbon. He deduces this view from the similarity in composition between pure caoutchouc, isoprene, and caoutchine.

The following Table contains the principal physical properties of isoprene and caoutchine:—

Table of the Physical Properties of Isoprene and Caoutchine.

Name.	Formula.	Boiling-point.	Specific gravity.	Vapour-density.	
				Expt.	Calculated.
Isoprene	$C^{10}H^8$	37°	0·6823 at 20°	2·44	2·349
Caoutchine	$C^{20}H^{16}$	171°	0·8420	4·65	4·699

In the calculations rendered necessary by the numerous vapour-density determinations contained in this paper, and more especially in those "On some of the products of the Destructive Distillation of Boghead Coal," the author has so repeatedly had to ascertain the value of the expression $\frac{1}{1+0\cdot00367\,T}$, that he was induced to calcu-

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* (Note received July 27.) Both the cymole from turpentine and that from caoutchine were converted into insolinic acid by bichromate of potash and sulphuric acid. The quantitative determinations made on the silver salt of the acid were almost theoretically exact.

late it once for all for each degree of the Centigrade thermometer from 1° to 150°. As it is always easy so to manipulate as to prevent the value of T falling between the whole numbers, the Table proved a most valuable means of saving time; the author has therefore appended it to his paper in the hope of its proving equally useful to other working chemists.

"On the Thermal Effects of Fluids in Motion—Temperature of Bodies moving in Air." By J. P. Joule, LL.D., F.R.S., and Professor W. Thomson, LL.D., F.R.S.

An abstract of a great part of the present paper has appeared in the Phil. Mag. vol. xv. p. 477. To the experiments then adduced a large number have since been added, which have been made by whirling thermometers and thermo-electric junctions in the air. The result shows that at high velocities the thermal effect is proportional to the square of the velocity, the rise of temperature of the whirled body being evidently that due to the communication of the velocity to a constantly renewed film of air. With very small velocities of bodies of large surface, the thermal effect was very greatly increased by that kind of fluid friction the effect of which on the motion of pendulums has been investigated by Professor Stokes.

"On the Distribution of Nerves to the Elementary Fibres of Striped Muscle." By Lional S. Beale, M.B., F.R.S.

"On the Effects produced by Freezing on the Physiological Properties of Muscles." By Michael Foster, B.A., M.D. Lond.

"On the alleged Sugar-forming Function of the Liver." By Frederick W. Pavy, M.D.

"A new Ozone-box and Test-slips." By E. J. Lowe, Esq., F.R.A.S., F.L.S. &c.

The ordinary form of Ozone-box being very cumbersome, the present one has been contrived to supersede it[*]. The box is simple in construction, small in size, and cylindrical in form; the chamber in which the *test-slips* are hung is perfectly dark, and at the same time there is a constant current of air circulating through it, no matter from what quarter of the compass the wind is blowing. The air either passes in at the lower portion of the box and travels round a circular chamber twice, until it reaches the centre (where the test-slips are hung) and then out again at the upper portion of the box in the same circular manner, or in at the top and out again at the bottom of the box.

Fig. 1 represents a section of the upper portion of the box, showing the manner in which the air enters and moves along to the centre chamber (where the test-slip is hung at A), and figure 2 represents a section of the lower half of the box where the air circulates in the opposite direction, leaving the box on the side opposite to that on which it had entered.

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[*] A specimen of the instrument was forwarded with the paper.

Fig. 1.

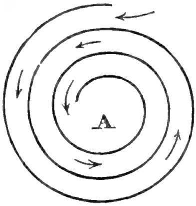
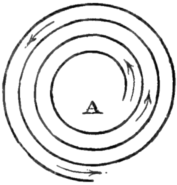


Fig. 2.



The box has been tested and found to work well.

On three different dates, when there was much ozone, test-slips were hung in one box, whilst others were hung in another which had the two entrances sealed up in order that no current of air should pass through; the result was satisfactory, viz.:—

Example.	New ozone-box.	New ozone-box sealed up.
1	10	0
2	9	0
3	9½	0

Then again, in five examples of test-slips being exposed without any box, in comparison with those placed in this new box, the result was:—

Example.	In new ozone-box.	Exposed to north without a box.
1	10	9
2	9	9
3	7	7
4	10	5
5	2	0

The ozone-box is capable of being suspended at an elevation above the ground; and this appears to be a great advantage, because elevation seems necessary in order to get a proper current of air to pass across the test-slips; indeed as an instance it may be mentioned, that at an elevation of 20 feet there is almost always more indication of ozone than at 5 feet.

The plan adopted here is to suspend the box to a ⊤ support, it being drawn up to its proper place by means of a thin rope passing over a pulley; and there is less trouble in examining and changing the test-slips in this manner than there was in the old method.

The box, as described, is made by Messrs. Negretti and Zambra of Hatton Garden.

It has been urged that a box was scarcely necessary for ozone test-slips; but as the papers fade on exposure to light, it must be evident that in order to register the maximum amount of ozone a *dark* box is required.

Test-slips.—Paper-slips being so fragile, I have substituted others made of calico. The calico is to all intents and purposes chemically pure, containing only a few granules of starch, used in the first process of its manufacture, which it is very difficult to remove, being enveloped in the cotton fibre; it is, however, thought to be purer than the paper that is used for these test-slips, every precaution having been taken to make it so.

Results of observations.—The following Tables have been constructed from observations made between the 1st of May, 1859, and the 31st of March, 1860.

Table I.

Mean amount of Ozone observed from Test-slips hung for twelve hours, both at night and in the daytime, in comparison with others hung for twenty-four hours.

During the month of	Papers exposed for twelve hours.			Papers exposed for twenty-four hours.			Difference between twelve hours and twenty-four hours.	
	Day.	Night.	Difference.	Day.	Night.	Difference.		
1859. May	0·4	1·3	0·9	1·1	1·9	0·8	0·7	0·6
June	0·8	0·9	0·1	1·3	1·5	0·2	0·5	0·6
July	0·9	1·0	0·1	1·2	1·3	0·1	0·3	0·3
August......	0·7	1·4	0·7	1·2	1·8	0·6	0·5	0·4
September .	1·9	2·6	0·7	2·5	3·0	0·5	0·6	0·4
October ...	0·5	0·7	0·2	0·7	0·9	0·2	0·2	0·2
November ..	1·5	1·7	0·2	1·8	2·1	0·3	0·3	0·4
December...	1·7	2·0	0·3	2·1	2·5	0·4	0·4	0·5
1860. January ...	2·8	2·8	0·0	3·2	3·5	0·3	0·4	0·7
February ...	2·3	2·8	0·5	2·6	3·0	0·4	0·3	0·2
March	4·9	5·2	0·3	5·2	5·6	0·4	0·3	0·4
Mean	1·7	2·0	0·3	2·1	2·5	0·4	0·4	0·5

The ozone being always in excess in the night, and the tests exposed for twenty-four hours showing always an excess over those only exposed for twelve hours.

Table II.

Number of observations without any visible ozone.

Month.	During the night.		During the day.	
	Twelve hours' exposure.	Twenty-four hours' exposure.	Twelve hours' exposure.	Twenty-four hours' exposure.
1859. May	9	4	19	12
June.........	18	10	15	9
July	18	12	18	13
August......	10	4	15	9
September..	2	0	0	0
October ...	16	12	18	14
November..	10	7	10	10
December...	10	5	7	5
1860. January ...	8	6	7	5
February ...	12	6	9	9
March	0	0	0	0
Number of days...	113	66	118	86

Mean amount of ozone with the box suspended at the height of 25 feet.

1859.	December	24 hours' exposure =3·0	48 hours' exposure =5·0
1860.	January...	24 hours' exposure =3·9	48 hours' exposure =4·5
	February	24 hours' exposure =3·7	48 hours' exposure =5·4
	March ...	24 hours' exposure =5·9	48 hours' exposure =6·4

Mean amount of ozone with the box suspended at the height of 40 feet, March 1860, with twenty-four hours' exposure =7·1.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xviii. p. 316.]

October 31, 1859.—A communication was made by Mr. Hopkins "On the construction of a new Calorimeter for determining the Radiating Power of the Surfaces of Heated Bodies."

November 14.—A communication was made by the Master of Trinity College "On the Mathematical part of Plato's Meno."

November 28.—The Rev. Dr. Donaldson read a paper "On the Origin and proper value of the word 'Argument.'"

The author first investigated the etymology and meaning of the Latin verb *arguo*, and its participle *argutus*. He showed that *arguo* was a corruption of *argruo* = *ad gruo*; that *gruo* (in *argruo*, *ingruo*, *congruo*) ought to be compared with κρούω, which means "to dash one thing against another," especially for the purpose of making a shrill, ringing noise; that *arguo* means "to knock something for the purpose of making it ring, or testing its soundness," hence "to test, examine, and prove anything;" and that *argutus* signifies "made to ring," hence "making a distinct, shrill noise," or "tested and put to the proof." Accordingly *argumentum* means *id quod arguit*, "that which makes a substance ring, which sounds, examines, tests, and proves it."

It was then shown that these meanings were not only borne out by the classical usage of the word, but also by the technical application of "argument" as a logical term. For it is not equivalent to "argumentation," or the process of reasoning; it does not even denote a complete syllogism; though Dr. Whately and some other writers on logic have fallen into this vague use of the word, and though it was so understood in the disputations of the Cambridge schools. The proper use of the word "argument" in logic is to denote "the middle term," *i. e.* "the term used for proof." In a sense similar to this the word is employed by mathematicians; and there can be no doubt that the oldest and best logicians confine the word to this, which is still its most common signification.

The author entered at some length into the Aristotelian definition of the *enthymeme*, which may be rendered approximately by the word "argument." He also explained how the words "topic" and "argument" came to denote the subject of a discourse or even of a picture. He showed, by a collection of examples from the best English poets, that the established meanings of the word "argument" are reducible to three: (1) a proof or means of proving; (2) a process of reasoning or controversy made up of such proofs; (3) the subject matter of any discourse, writing, or picture. And he maintained that the second of these meanings ought to be excluded from scientific language.

December 12.—The following paper from the Astronomer Royal was read, "Supplement to the proof of the Theorem that 'Every Algebraic Equation has a Root.'"

The author expressed his want of confidence in every result obtained by the use of imaginary symbols, and in this supplement demonstrated that the left-hand member of every algebraic equation of the form $\phi(x)=0$ admitted of resolution, either into real linear factors, or into real quadratic factors.

Professor Miller also made a communication "On a new portable form of Heliotrope, and on the employment of Camera Lucida prisms and right-angled prisms in surveying."

February 13, 1860.—The Rev. H. A. J. Munro read a paper "On the Metre of an Inscription copied by Mr. Blakesley, and printed by him in his 'Four Months in Algeria,' p. 285."

February 27.—The Rev. Professor Sedgwick made the following communications:—

1. "An account of Mr. Barrett's progress in the Survey of Jamaica, with some remarks on the Distribution of Gold Veins."
2. "Some account of the Geological Discoveries in the Arctic Regions."

March 12.—The Rev. Professor Challis made a communication "On the Planet within the orbit of Mercury, discovered by M. Lescarbault."

By a recent comparison of the theory of Mercury's orbit with observation, M. Leverrier found that the calculated secular motion of the perihelion of that planet requires to be increased by 38″, and that this difference between observation and theory cannot be accounted for by the attractions of known bodies of the solar system. In a letter addressed to M. Faye, and published in the Paris Meteorological Bulletins of October 4, 5, and 6, 1859, he suggested that the difference might be due to the attraction of a group of small planets circulating between Mercury and the Sun. On December 22 of the same year, M. Lescarbault, a physician and amateur astronomer, residing at Orgères, about sixty miles south-west of Paris, announced in a letter to M. Leverrier that he had seen on March 26, 1859, a small round spot traversing the sun's disc, which he considered to be a planet inferior to Mercury. Naturally much interested by this information, M. Leverrier went to Orgères on December 31, and after closely interrogating M. Lescarbault respecting the particulars of the observation, and the instrumental means by which it was made, he returned with the conviction that the observation was trustworthy, and that a new planet had been discovered (*Comptes Rendus*, January 2, 1860, p. 40).

M. Lescarbault had long conceived the idea of detecting inferior planets by watching the sun's disk for transits, and in 1858 he put his project into execution. He was in possession of a good telescope of $3\frac{3}{4}$ inches aperture and 5 feet focal length, mounted with an altitude and azimuth movement, and provided with a finder magnifying 6 times. The power of the eyepiece employed in the observations of March 26 was 150. Not being furnished with a position-circle, he adopted the following means of obtaining angular measurements. The eyepiece of the telescope and the eyepiece of the finder each had at its focus two wires crossing at right angles, and the wires of the latter were so adjusted that a star seen at their intersection was seen at the same time at the intersection of the wires of the telescope. There were also in the eyepiece of the finder two wires parallel to, and on opposite sides of, each cross-wire, and distant by about 16′. A circular card about 6 inches in diameter, and graduated to half degrees, was placed concentric with the tube of the eyepiece of the finder, and apparently could be moved both about the tube and, with the tube, about the axis of the finder. A cross-wire of the telescope and a cross-wire of the finder were adjusted vertically by looking at a distant plumb-line, and the diameter of the card containing the zero of its graduation was placed vertically by means of a small plumb-line and eye-hole approximately arranged for that purpose. The mode of using this apparatus for angular measurements will be seen by the following account of the observations. The observer had also a small transit-instrument by which he obtained true time, using for timepiece his watch, which, as it only indicated minutes, required the supplement of a temporary seconds' pendulum.

In the account which M. Lescarbault gives of his observations, he says that it had been his practice to examine with the telescope the contour of the sun for a considerable interval on each day in which he had leisure, and that at length, on March 26, 1859, he saw a small round spot near the limb, which he immediately brought to the intersection of the wires of the telescope. Then, according to his statement, he quickly turned the graduated card till *two* of the wires of the finder were tangents to the sun's limbs, or equidistant from them. But it is evident that to effect an angular measurement in this way, *one* of the middle wires of the finder must have been placed tangentially to the sun's limb at the point of their intersection, to which point the spot had just been brought. Assuming that this operation was performed, the angular distance of the point from the vertical diameter of the sun might be read off, as the account states that it was, by applying the plumb-line apparatus to the graduated card. This method could only give a rough measure of the angular position of a point very near the sun's limb; and in fact M. Lescarbault does not appear to have attempted to determine the position of the spot during the interval between the beginning and the end of the transit. He states that the spot had entered a little way on the sun when he first saw it, and that the time and place of entrance were inferred by estimation.

The following are the immediate results of the observations:— The spot entered at $4^h\ 5^m\ 36^s$ mean time of Orgères at the angular distance of 57° 22′ from the north point towards the west, and departed at $5^h\ 22^m\ 44^s$, at 85° 45′ from the south point towards the west, occupying consequently in its transit $1^h\ 17^m\ 8^s$. The length of the chord it described was 9′ 14″, and its least distance from the sun's centre 15′ 22″. M. Lescarbault also states that he judged the apparent diameter of the spot to be at most one-fourth of that of Mercury, when seen by him with the same telescope and magnifying power during its transit across the sun on May 8, 1845. The latitude of Orgères is 48° 8′ 55″, and longitude west of Paris, $2^m\ 35^s$.

From these data M. Leverrier ascertained, by calculating on the hypothesis of a circular orbit, that the longitude of the ascending node is 12° 59′, the inclination 12° 10′, the mean distance 0·1427, that of the earth being unity, and the periodic time 19·7 days. Also he found that the greatest elongation of the body from the sun is 8°, the inclination of its orbit to that of Mercury 7°, the real ratio of its diameter to Mercury's 1 to 2·58, and that its volume is one-seventeenth the volume of Mercury on the supposition of equal densities. This mass is much too small to account for the perturbation of Mercury's perihelion. According to these results, the periods at which transits may be expected are eight days before and after April 2 and October 5, the body being between the earth and sun near its descending node at the former period, and near its ascending node at the latter.

After the announcement of this singular discovery, it was found that other observations of a like kind had been previously made. Several instances are collected by Professor Wolf in the tenth number of his *Mittheilungen über die Sonnenflecken,* eight of which are quoted in vol. xx. (p. 100) of the Monthly Notices of the Royal Astronomical Society. Two of these, the observation of Stark on October 9, 1819, and that of Jenitsch on October 10, 1802, agree sufficiently well with the calculated position of the node of the object seen by Lescarbault. But the spot seen by Stark is stated to have been about the size of Mercury.

Capel Lofft saw at Ipswich, on January 6, 1818, at 11 A.M., a spot of a 'sub-elliptic form,' which advanced rapidly on the sun's disc, and was not visible in the evening of the same day (Monthly Magazine, 1818, part 1, p. 102).

Mr. Benjamin Scott, Chamberlain of London, saw about midsummer of 1847 a large and well-defined round spot, comparable in apparent size with Venus, which had departed at sunrise of the next day (Evening Mail, January 11, 1860).

Pastorff of Buchholz records that he saw on October 28 and November 1, 1836, and on February 17, 1837, *two* round black spots of unequal size, moving across the sun at the respective hourly rates of 14′, 7″, and 28′. Also he announced, January 9, 1835, to the Editor of the *Astronomische Nachrichten,* that "six times in the previous year he had seen two new bodies pass before the sun in different directions and with different velocities. The larger was about $3''$ in diameter, and the smaller from $1''$ to $1''{\cdot}25$. Both appeared perfectly round. Sometimes the smaller preceded, and at other times the larger. The greatest observed interval between them was $1'\ 16''$: at times they were very near each other. Their passage occupied a few hours. Both appeared as black as Mercury on the sun, and had a sharp round form, which, however, especially in the smaller, was difficult to distinguish." Schumacher considered it his duty as editor to insert the communication, but evidently did not give credit to it (*Astron. Nachr.* No. 273).

In vol. ii. of the Correspondence between Olbers and Bessel, mention is made in p. 162 of an observation at Vienna by Steinhübel, of a dark and well-defined spot of circular form which passed over the sun's diameter in five hours. Olbers, from these data, estimates the distance from the sun to be 0·19, and the periodic time thirty days. It is remarkable that Stark saw about noon *of the same day* a singular and well-defined circular spot, which was not visible in the evening. This is one of the instances in vol. xx. of the Monthly Notices of the Astronomical Society.

These accounts appear to prove that transits of dark round objects across the sun are real phænomena; but it would perhaps be premature to conclude that they are planetary bodies. If the object observed by Lescarbault be a planet, it is certainly very surprising that it has not been often seen. Schwabe, after observations of the sun's face continued through thirty-three years, has recorded no instance of such a transit. It is probable that now attention has been especially drawn to the subject, future observations, accompanied by measures (of which Lescarbault's are the first instance), may throw light on the nature of these phænomena.

April 23.—Professor De Morgan read a paper "On the Syllogism, No. IV., and on the Logic of Relations."

In the third paper were presented the elements of a system in which only *onymatic* relations were considered; that is, relations which arise out of the mere notion of nomenclature—relations of name to name, as names. The present paper considers relation in general. It would hardly be possible to abstract the part of it which relates to relation itself, or to the author's controversy with the logicians, who declare all relations *material* except those which are onymatic, to which alone they give the name of *formal*. Mr. De Morgan denies that there is any purely formal proposition except "there is the probability *a* that X is in the relation L to Y;" and he maintains that the notion 'material' *non suscipit magis et minus*; so that the relating copula is as much materialized when for L we read *identical* as when for L we read *grandfather*.

Let X..LY signify that X stands in the relation L to Y; and X.LY that it does not. Let LM signify the relation compounded of L and M, so that X..LMY signifies that X is an L of an M of Y. In the doctrine of syllogism, it is necessary to take account of combinations involving a sign of *inherent quantity*, as follows :—

By X . . LM′Y is signified that X is an L of *every* M of Y.

By $X\,..\,L_{,}MY$ it is signified that X is an L of *none but* Ms of Y.

The *contrary* relation of L, not -L, is signified by *l*. Thus X . LY is identical with X . . *l*Y. The converse of L is signified by L^{-1}: thus X . . LY is identical with $Y\,..\,L^{-1}X$. This is denominated the L-*verse* of X, and may be written LX by those who prefer to avoid the mathematical symbol.

The attachment of the sign of inherent quantity to the symbols of relation is the removal of a difficulty which, so long as it lasted, prevented any satisfactory treatment of the syllogism. There is nothing more in X . . LM′Y than in every M of Y is an L^{-1} of X, or $MY))L^{-1}X$, X and Y being individuals; and nothing more in $X\,..\,L_{,}MY$ than in $L^{-1}X))MY$, except only the attachment of the idea of quantity to the combination of the relation.

When X is related to Y and Y to Z, a relation of X to Z follows: and the relation of X to Z is compounded of the relations of X to Y and Y to Z. And this is syllogism. Accordingly every syllogism has its inference really formed in the first figure, with both premises affirmative. For example, Y . LX and Y . . MZ are premises stated in the third figure: they amount to $X\,..\,L^{-1}Y$ and Y . . MZ, giving $X\,..\,l^{-1}MZ$ for conclusion. This affirmative form of conclusion may be replaced by either of the negative forms $X\,.\,L^{-1}M'Z$ or $X\,.\,l_{,}^{-1}mZ$.

The arrangement of all the forms of syllogism, the discussion of points connected with the forms of conclusion, the extension from individual terms in relation to quantified propositions, the treatment of the particular cases in which relations are convertible, or transitive, or both—form the bulk of the paper, so far as it is not controversially directed against those who contend for the confinement of the syllogism to what Mr. De Morgan calls the *onymatic* form.

An appendix follows the paper, on syllogism of transposed quantity, in which the number of instances included in one premise is equal to the whole number of existing instances of the concluding term in the other premise.

Mr. J. H. Röhrs also read a paper "On the Motion of Bows, and thin Elastic Rods."

May 7.—The Rev. Professor Sedgwick made a communication "On the Succession of Organic Forms during long geological periods; and on certain Theories which profess to account for the origin of new species."

May 21.—The Public Orator read a paper "On the Pronunciation of the Ancient and Modern Greek Languages."

He gave a rapid sketch of the "Reuchlin and Erasmus" controversy in the sixteenth century, especially the part taken in it at Cambridge by Cheke, Smith, Ascham, and Bishop Gardiner; and then proceeded to show how the proper sounds of the Greek letters may be determined from the following sources :—

1. Distinct statements of grammarians.
2. Incidental notices in other ancient authors.
3. Variations in writing of inscriptions and MSS.
4. Phonetic spelling of cries of animals.
5. Puns and riddles.
6. The value of the respective letters in other languages employing the same alphabet, especially Latin.
7. The way in which Latin proper names are spelt in Greek, and *vice versâ*.
8. The traditions of pronunciation preserved in modern Greek.

He concluded that, on the whole, the method of Erasmus approached more nearly to the ancient pronunciation than that of Reuchlin.

"But," he proceeded, "when we consider the untrustworthiness of each of these sources of evidence taken singly, and when moreover we find them often in conflict with one another, it cannot be expected that the result should be very certain or very satisfactory. There are also other considerations which enhance the difficulty of the inquiry. As there were very marked dialectic varieties in Greece, so there may have been local variations even in Attica itself.

"The pronunciation, too, changed from time to time. Plato gives us proof of this in the 'Cratylus.'"

After quoting several instances, and showing that great changes both in pronunciation and spelling had taken place in modern languages, French, Spanish, and English, "it would," he said, "be hopeless to attempt to determine the pronunciation of any language by a reference to its orthography at a time when both were perpetually changing. But in the history of every nation there arrives a time when the creative energy of its literature seems to have spent itself; when, instead of developing new forms, men begin to look back and not forward, to comment and to criticise. Then it is that a language begins to assume, even in minor and merely outward points, such as pronunciation and spelling, a fixity and rigidity which it retains with scarcely any change so long as the nation holds together. Such a period in Greek history was that which began with the grammarian sophists in the fifth century B.C., and culminated in Aristarchus and Aristophanes of Byzantium. In the spelling and pronunciation of Greek there was probably very little change from that time to the end of the third century A.D."

October 19.—Dr. Paget made a communication "On some Points in the Physiology of Laughter."

November 12.—The Public Orator read a paper (a sequel to that on May 21) "On the Accentuation of Ancient Greek."

The question of accents was not discussed in the Reuchlin and Erasmus dispute. At that time all pronounced according to the system of accents introduced by the Greeks of Constantinople, who first taught the ancient language to the Italians.

It was probably in Elizabeth's reign that we began to disuse the old pronunciation of vowels both in Greek and Latin; and concurrently with this change we, as well as the other nations of Europe, began to pronounce Greek, not with the modern Greek, but with the Latin accent. The reasons were :—

1. Teachers speaking the modern Greek were no longer required, so the tradition was not kept up.

2. It saved much trouble to pronounce both languages with the same accentuation.

3. The Greek accent perpetually clashes with quantity; the Latin much more rarely; never, indeed, in that syllable of which the quantity is most marked—the penultima.

Isaac Vossius (1650–60) advocated the disuse of accentual marks altogether, as the invention of a barbarous age to perpetuate a barbarous pronunciation.

After showing the meaning of the word 'accent' as applied to modern languages, and discussing the accentuation of the German, English, French, &c., he proceeded to say :

"There are three methods of emphasizing a syllable:—

1. By raising the note;
2. By prolonging the sound;
3. By increasing its volume.

"Scaliger, *De Causis Linguæ Latinæ*, lib. ii. cap. 52, recognizes this division when he says that a syllable may be considered of three dimensions in sound, having height, length, and breadth.

"Now in our own language, when we accent a syllable, which of these dimensions do we increase? Generally all three, but not necessarily; for when the prayers, for example, are intoned, *i. e.* read upon one note, the accent is marked by increasing the volume of sound (the third method), which involves also a longer time in utterance, *i. e.* a lengthening of quantity. In speaking, all three methods are employed, but one more prominently than the other, according to individual peculiarities of the speakers. What we blend, the Greeks kept distinct.

"We cannot understand the Greek system unless we bear this in mind. They never confounded accent with quantity. Ineradicable habit prevents us from reverting in practice to their method, just as they would have been unable to comprehend ours.

"It is clear from Dionysius, *De Comp. Verb.* lib. xi. cap. 75, that the dialogue in tragedy preserved the ordinary accentuation, which was disregarded only in choral passages set to music."

The practical conclusion was this: that while it would be desirable, if possible, to return to the Erasmian system of pronunciation, it would be extremely absurd to adopt the barbarous accentuation of modern Greek, which has quite lost the old essential distinction between accent and quantity. In this respect, as we cannot recover practically the ancient method, it is better to keep to our own system of the Latin accent, which does not confuse the learner's notion of quantity in verse as the modern Greek does.

An Athenian boy has the greatest difficulty in comprehending the rhythm of Homer or Sophocles. Hence it is not blind prejudice (as Professor Blackie asserts) which makes us keep to our old usage, but a well-grounded conviction that we should lose more by changing than we should gain.

November 26.—Professor Challis made a communication "On the Solar Eclipse of July 18, 1860."

December 10.—Mr. Seeley read a "Notice of Opinions on the Red Limestone at Hunstanton."

Professor Miller also described "An Instrument for measuring the radii of arcs of Rainbows."

February 11, 1861.—Mr. H. D. Macleod read a paper "On the present State of the Science of Political Economy."

The writer took a general survey of the science as it at present exists, testing several generally received doctrines by the principles of inductive logic, and earnestly enforcing the necessity of a thorough reform of the whole science, which must be constructed on principles analogous to those of the other inductive sciences.

February 25.—Dr. Humphry made a communication "On the Growth of Bones."

March 11.—The Master of Trinity made a communication "On the Timæus of Plato."

LXX. *Intelligence and Miscellaneous Articles.*

ON THE OPTICAL PROPERTIES OF THE PICRATE OF MANGANESE.

BY M. CAREY LEA.

BREWSTER and Haidinger have described a remarkable property possessed by certain crystalline surfaces, of reflecting, besides the ray normally polarized in the plane of incidence and reflexion, another ray, polarized perpendicularly to that plane, and differing from the former in being coloured—a property rendered more conspicuous by the fact that the colour of the ray so polarized abnormally is either complementary to, or at least quite distinct from the colour of the crystal itself.

I find that this property is possessed to a remarkable degree by the picrate of manganese. This salt crystallizes in large and beautiful transparent right-rhombic prisms, sometimes amber-yellow, sometimes aurora-red, exhibiting generally the combination of principal prism, and macrodiagonal, brachydiagonal and principal end planes. In describing this substance in a paper on picric acid and the picrates*, I mentioned that in a great number of specimens examined, no planes except those parallel with or perpendicular to the principal axis had been met with. Since then I have obtained in several crystallizations specimens exhibiting a brachydiagonal doma; but this appears to be rather unusual.

The optical properties of this salt are very interesting. It exhibits a beautiful dichroism. If the crystal be viewed by light transmitted in the direction of its principal axis, it appears of a pale straw-colour, in any other direction, rich aurora-red in some specimens, in others salmon-colour. A doubly refracting achromatized prism gives images of these two colours, unless the light be transmitted along the principal axis of the crystal of picrate, in which case both are pale straw-colour.

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* Silliman's American Journal, Nov. 1858.

But it also possesses in a high degree the property of reflecting two oppositely polarized beams; and the great size of the crystals in which it may readily be obtained renders it peculiarly fitted for optical examination. If one of these crystals be viewed by reflected light while it is held with its principal axis lying in the plane of incidence and reflexion, the reflected light is found to be not pure white, but to have a purple shade. Examined with a rhombohedron or an achromatized prism of Iceland spar, having its principal axis in the plane of incidence and reflexion, the ordinary image is white as usual, while the extraordinary is of a fine purple colour, the phenomenon having the greatest distinctness when the light is incident at the angle of maximum polarization.

The experiment may be varied and the purple light beautifully seen without the use of a doubly reflecting prism, by allowing only light polarized perpendicularly to the plane of incidence to fall on the crystal; in this case the surface of the crystal appears rich deep purple, no white light reaching the eye.

This property is not possessed by all the planes of the crystal, but is limited to the principal prism and brachy- and macrodiagonal end planes, in other words, to the planes parallel with the principal axis of the crystal. The brachydiagonal doma and $0P$ planes do not possess it. Nor is it exhibited by the first-mentioned planes when the crystal is turned with its prismatic axis at right angles to the plane of incidence.

All specimens of picrate of manganese do not possess this property to an equal extent. The crystals vary considerably in colour, and those which are full red exhibit it more strongly than the amber-coloured. Picric acid boiled with aqueous solution of cyanhydroferric acid and saturated with carbonate of manganese, gives crystals of a rich deep colour, which exhibit the purple polarized beam particularly well.

These properties are not possessed by the manganese salt alone, but also by the picrates of potash and ammonia (especially when crystallized by very slow spontaneous evaporation in prisms of sufficient size), and the picrates of cadmium and peroxide of iron—with this difference, however, that while the prismatic axis of the crystal in the case of the cadmium and manganese salts must be in the plane of incidence, in the alkaline salts it must be perpendicular to that plane. As they all crystallize in the right-rhombic system, it is probable that either the alkaline salts on the one hand, or the manganese and cadmium on the other, are prismatically elongated in the direction of a secondary axis.

It is convenient that distinct phenomena should have distinct names; and none appears to have been assigned to this. Brewster speaks of it as a "property of light," and Haidinger uses the word "Schiller" for it. The terms dichroism, trichroism, and pleiochroism are limited to properties of transmitted light. I therefore suggest for the phenomenon here in question the name *catachroism*, using the preposition *κατα* in the same sense as in the word *κατοπτριζω*, to reflect (as a polished surface), applying it to express the property of reflecting two beams—one normally polarized in the plane of incidence, and the other polarized in a plane perpendicular to it.

The chromatic properties exhibited by the picrates of ammonia and potash are very remarkable in their variety. Their crystals possess :—

1st. The well-known play of red and green light. If a little very dilute solution of pure picrate of potash be spontaneously evaporated in a hemispherical porcelain basin, so as to form a network of extremely slender needles, and these be viewed by gas-light, the play of colours is singularly brilliant.

2nd. Dichroism. When by spontaneous evaporation of large quantities of solution of potash, or, better, of ammonia salt, transparent prisms of $\frac{1}{12}$ to $\frac{1}{10}$ inch diameter are obtained; these, viewed with a doubly refracting prism by transmitted light, give two images—one pale straw-colour, and the other deep brownish red.

3rd. The above-described property of catachroism, or reflexion in the plane of incidence of oppositely polarized beams.—Silliman's *American Journal,* November 1860.

EXPERIMENTS ON THE POSSIBILITY OF A CAPILLARY INFILTRATION THROUGH POROUS SUBSTANCES, NOTWITHSTANDING A STRONG COUNTERPRESSURE OF VAPOUR. POSSIBLE APPLICATION TO GEOLOGICAL PHENOMENA. BY M. DAUBRÉE.

In the grand phenomena which are to us the principal manifestations of the activity of the interior of the globe, we see every day enormous quantities of water disengaged as steam from great depths. It may be asked if these incessant losses are not partially at least made up by a supply from this surface; and if so, in what way are these infiltrations effected?

It would be difficult to imagine that this supply was produced by a free circulation; for the way open for a descent would at the same time form an outlet also for the escape of vapour; and this objection would apply more especially to the volcanic regions, where the internal vapour has sufficient tension to send columns of lava with a density two or three times that of water, to great heights above the level of the sea. In trying to reconcile these apparent contradictions, I have been led to inquire if water could not reach the deep and heated reservoirs, which yield it in a variety of ways, not by means of extended *fissures,* as has hitherto been supposed, but also by the *porosity* and *capillarity* of rocks.

M. Jamin's ingenious experiments* have shown how considerable is the influence which capillarity exerts in changing the conditions of equilibrium, established through the intervention of a liquid column, between two opposite pressures.

But in previous experiments the temperature was the same in all parts of the capillary tube. It appeared important, more especially in reference to the geological problem which I have indicated, to see what would happen if the temperature was much higher at one part of

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* Phil. Mag. vol. xix. p. 204.

the capillary passage, so as to convert the liquid into vapour, and thus change it into a state in which it would probably not be subject to the laws which at first had caused its infiltration.

I have constructed an apparatus, the principal object of which was to connect, by a porous plate of fine close-grained sandstone, on the one end a closed space in which the tension of vapour measured by a manometer was $1\frac{7}{8}$ atmosphere, and on the other a space in direct communication with the air, half-filled with water, which soon reached the boiling-point, but where the pressure could not exceed that of the atmosphere.

Although the thickness of the interposed plate was only 2 centimetres, the apparatus showed that the water is not driven back by the counterpressure of vapour; the difference of pressure on the two sides of the plate does not prevent the liquid from passing from the relatively cold region towards the relatively hot one, by a sort of capillary process, favoured by the rapid evaporation and drying of the latter.

The effects of this apparatus, which I cannot explain in detail, will manifestly be materially augmented by increasing the thickness of the porous plate, and working with vapour at a higher temperature.

But even these results prove that capillarity, acting in conjunction with gravity, can, in spite of very powerful internal counterpressures, force water from the superficial and cold regions of the globe to the deep and heated parts, where, in consequence of the temperature and pressure which it acquires, the vapour becomes susceptible of producing great mechanical and chemical effects*. Do not the preceding experiments thus touch the fundamental points of the mechanism of volcanos, and of the other phenomena generally attributed to the development of vapours in the interior of the globe, especially earthquakes, the formation of certain thermal springs, the filling metalliferous veins, as well as to various cases of the metamorphism of rocks? Without excluding the primitive water generally supposed to be incorporated in the internal melted masses, do not the same experiments show that infiltrations from the surface may also be operative, so that the deeper parts of the globe would be in a daily state of giving and taking, and that by a most simple process, although very different from the mechanism of the siphon and of ordinary springs? A slow, continuous, and regular phenomenon would thus become the cause of sudden and violent manifestations, like explosions and ruptures of equilibrium.—*Comptes Rendus*, January 28, 1861.

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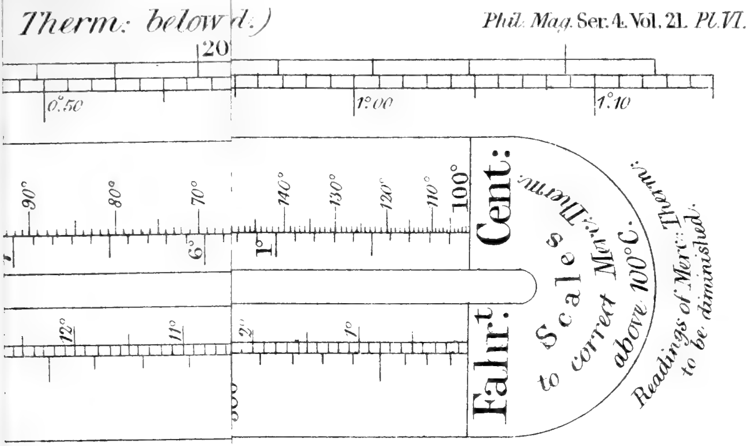
* It is known that water penetrates into the pores of most rocks, especially those belonging to the stratified formations, as is shown by the water which they generally contain in nature. Bischoff has long called the attention of geologists to this fact. Although the granite on which the sedimentary rocks rest is usually very impermeable, it has been traversed in many places by injections of eruptive rocks. Among the latter there are some, like the trachytes, so porous that they might well be particularly suspected of establishing a permanent capillary communication between the water of the surface and the heated masses which form the base of this kind of column.


Therm: below d.)
20
0°.50
1°.00
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90°
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130°
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100°
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Scales
to correct Merc: Therm:
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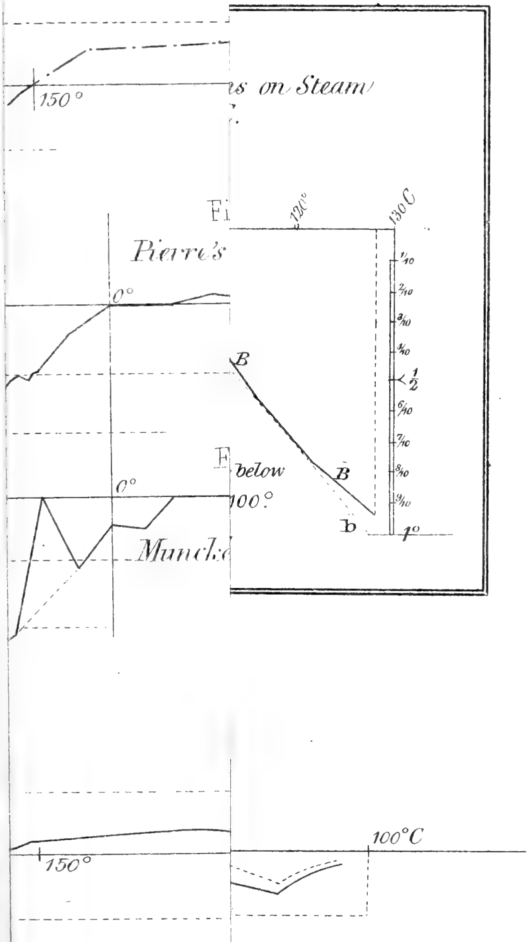
s on Steam
150°
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130 C
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0°
1/10
2/10
3/10
4/10
1/2
6/10
7/10
8/10
9/10
1°
B
B'
b
F
below
100°
0°
Munck
150°
100°C

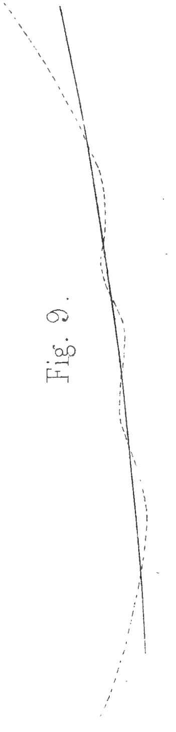
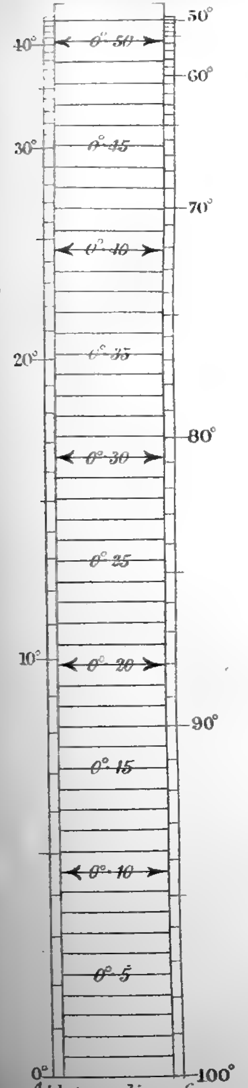


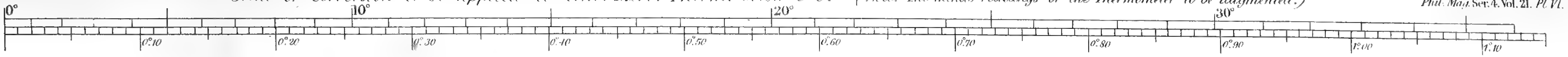
Fig. 9.

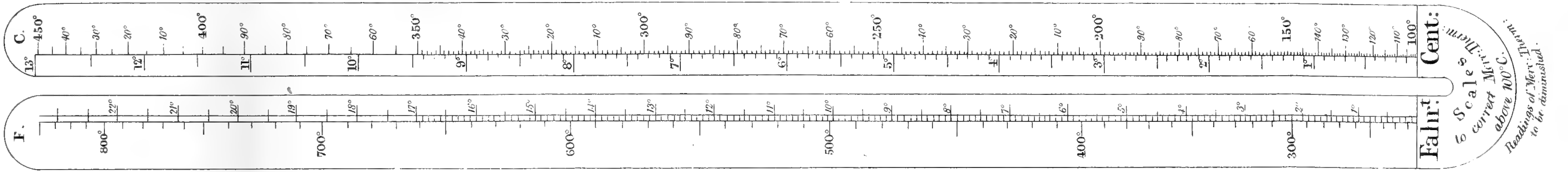
Scale to correct the Cent: Therm: between 0° and 100°



Add to readings of Merc: Therm:

Scale of Correction to be applied to Cent: Merc: Therm: below 0°C. (Note. The minus readings of the Thermometer to be augmented.)





Scales to correct Merc: Therm: above 100°C.

Readings of Merc: Therm: to be diminished.

Fig. 1. Muncke's Sulphuric Acid

Fig. 2. Pierre's Alcohol

Fig. 3. Muncke's Alcohol

Fig. 4. Waterston's Alcohol

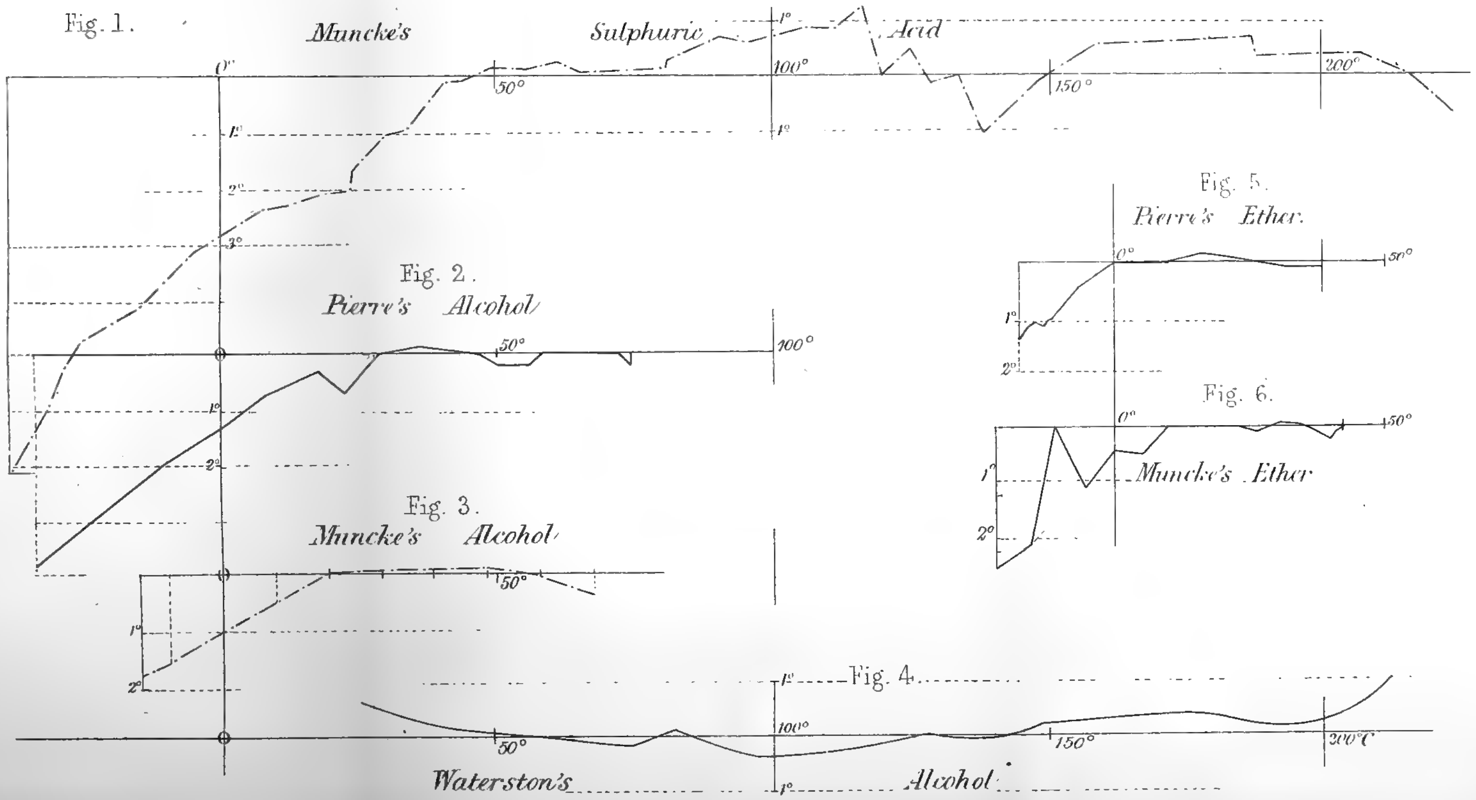


Fig. 5. Pierre's Ether.

Fig. 6. Muncke's Ether

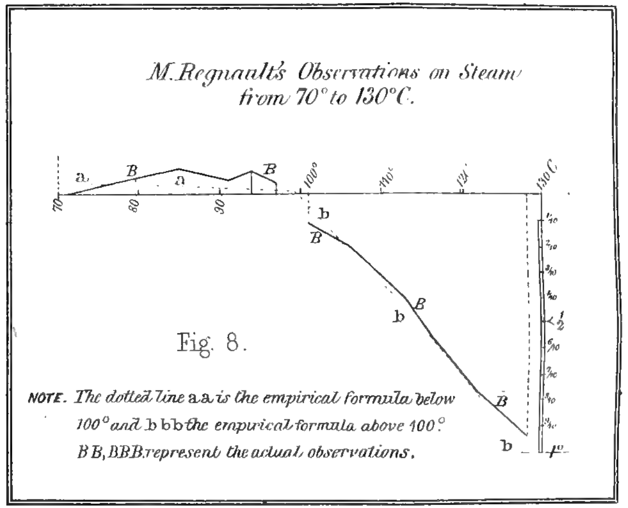
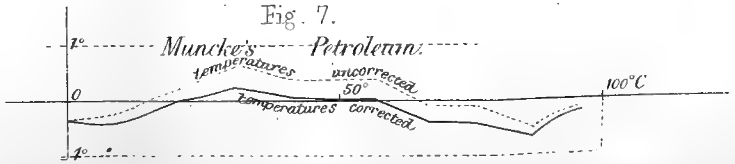

M. Regnault's Observations on Steam from 70° to 130°C.

Fig. 8.

NOTE. The dotted line a.a. is the empirical formula below 100° and b b b the empirical formula above 100°. B B, B B B represent the actual observations.


Fig. 9.

Fig. 7. Muncke's Petroleum.



J. Basire, lithe.

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AND
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SUPPLEMENT TO VOL. XXI. FOURTH SERIES.

LXXI. *On the Reflexion of Light at the Boundary of two Isotropic Transparent Media.* *By* L. LORENZ*.

JAMIN, as is well known, discovered that Fresnel's formulæ for the intensity of the rays reflected and refracted at the boundary of two isotropic transparent media do not perfectly agree with experiment, the difference being very considerable when the angle of incidence approaches to the angle of polarization. Cauchy had already proved that, under these circumstances, waves must be produced with longitudinal vibrations; and having assumed that these waves were absorbed very rapidly (though not instantaneously, since in that case he would have returned to the formulæ of Fresnel), he now introduced a correction into the formulæ which caused them to agree with experiment.

All calculations, however, which have hitherto been made concerning the reflexion and refraction of light, have proceeded on the hypothesis of an instantaneous passage from one medium to the other, and a consequent instantaneous change of the index of refraction. Such a passage is, however, a mere metaphysical abstraction, which cannot possibly exist in nature; and the calculation would be more exact and more satisfactory if a gradual passage were admitted between the two media through a space which might afterwards be assumed to be as small as we please. It is, moreover, a fact that bodies are really surrounded by an atmosphere which must produce such a gradual change of refraction.

The object of this paper is to show that Jamin's experiments can only be reconciled with Fresnel's formulæ when the calculation is made on the hypothesis.

In what follows, the case of total reflexion will not be considered.

If the incident light be polarized in the plane of incidence,

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[*] Translated by F. Guthrie, from Poggendorff's *Annalen,* vol. cxi. p. 460.

the angle of incidence being called x, and that of refraction x_1, the ratio of the amplitudes of the incident, refracted, and reflected light, according to Fresnel, is

$$1:\frac{2\cos x\sin x_1}{\sin(x+x_1)}:\frac{\sin(x-x_1)}{\sin(x+x_1)}. \quad . \quad . \quad . \quad . \quad (1)$$

For the light polarized perpendicularly to the plane of incidence, the ratio of the same three amplitudes is

$$1:\frac{2\cos x\sin x_1}{\sin(x+x_1)\cos(x-x_1)}:-\frac{\tan(x-x_1)}{\tan(x+x_1)}. \quad . \quad (2)$$

Assume now that these formulæ are correct when the difference between x and x_1 is infinitely small, so that $x_1=x+dx$. Substituting this value of x_1, the above expressions become

$$1:1+\frac{dx}{\sin 2x}:-\frac{dx}{\sin 2x}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$1:1+\frac{dx}{\sin 2x}:\frac{dx}{\tan 2x}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

We suppose that the incident ray approaches the bounding surface of the media at an angle α, and that its direction is there gradually changed by having to traverse successive parallel refractive layers, until it emerges completely into the other medium at the constant angle β.

In order to simplify the calculation, we will in the first instance neglect the retardation of the ray.

Let A be the amplitude of the incident ray, and let this become χ and $\chi+d\chi$ for the refracted ray, when the angle of incidence, α, becomes x and $x+dx$. Then, whatever may be the polarization, we have, according to (3) and (4),

$$\frac{d\chi}{\chi}=\frac{dx}{\sin 2x};$$

from which, by integrating and determining the constants, we get

$$\chi=\mathrm{A}\sqrt{\frac{\tan x}{\tan\alpha}}.$$

The ray reflected from this layer, if it be polarized in the plane of incidence, has, according to (3), the amplitude $-\chi\frac{dx}{\sin 2x}$; and if polarized perpendicularly, it has, according to (4), the amplitude $\chi\frac{dx}{\tan 2x}$. These two values we indicate by χdu, where, in the first case,

$$u=-\tfrac{1}{2}\log\tan x; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and in the second,

$$u=\tfrac{1}{2}\log\sin 2x. \quad \ldots\ldots \quad (6)$$

The amplitude of the reflected ray is therefore

$$\chi du = \mathrm{A}\sqrt{\frac{\tan x}{\tan\alpha}}\,du\,;$$

and when this ray encounters a layer whose angle of refraction is x_1, its amplitude becomes

$$\chi\sqrt{\frac{\tan x_1}{\tan x}}\,du=\mathrm{A}\sqrt{\frac{\tan x_1}{\tan\alpha}}\,du.$$

At the boundary between this layer and the following, where the angle of refraction is x_1-dx_1, a portion of the light is again reflected; and u_1 being the same function of x_1 that u is of x, the amplitude of the twice reflected ray is

$$-\mathrm{A}\sqrt{\frac{\tan x_1}{\tan\alpha}}\,du\,du_1\,;$$

and when this ray has traversed all the layers until its angle of refraction has become constant and equal to β, its amplitude is

$$-\mathrm{A}\sqrt{\frac{\tan\beta}{\tan\alpha}}\,du\,du_2.$$

The angle x_1 may now have all values between α and x, and x all values between α and β. The sum, therefore, of the amplitudes of all the twice reflected rays will be represented by the definite double integral

$$-\mathrm{A}\sqrt{\frac{\tan\beta}{\tan\alpha}}\int_{u_\alpha}^{u_\beta}du\int_{u_\alpha}^{u}du_1\,;$$

where u_α and u_β indicate the values of u for x equal to α, and x equal to β.

In this manner the sum of the amplitudes of the rays reflected 4, 6, ... times can easily be calculated; and as the sum of the different rays that have been 0, 2, 4, 6 ... times reflected make up the whole of the refracted ray, the amplitude of the latter is

$$\sqrt{\frac{\tan\beta}{\tan\alpha}}\Big[1-\int_{u_\alpha}^{u_\beta}du\int_{u_\alpha}^{u}du_1+\int_{u_\alpha}^{u_\beta}du\int_{u_\alpha}^{u}du_1\int_{u_1}^{u_\beta}du_2\int_{u_\alpha}^{u_2}du_3-\ldots\Big], \quad (7)$$

which we shall indicate by

$$\mathrm{A}\sqrt{\frac{\tan\beta}{\tan\alpha}}f(u_\alpha),$$

where

$$f(u)=1-\int_{u}^{u_\beta} du \int_{u_\alpha}^{u} du_1 f(u_1).$$

From the last equation we get by differentiation

$$f'(u)=\int_{u_\alpha}^{u} du_1 f(u_1)$$

and

$$f''u=f(u),$$

which gives

$$f(u)=c\epsilon^{u}+c_1\epsilon^{-u},$$

where the constants c and c_1 are to be determined by the equations

$$f(u_\beta)=1, \text{ and } f'(u_\alpha)=0.$$

Whence

$$f(u)=\frac{\epsilon^{u-u_\alpha}+\epsilon^{u_\alpha-u}}{\epsilon^{u_\beta-u_\alpha}+\epsilon^{u_\alpha-u_\beta}};$$

and the value of (7) or the amplitude of the refracted ray is

$$\frac{2A\sqrt{\frac{\tan\beta}{\tan\alpha}}}{\epsilon^{u_\beta-u_\alpha}+\epsilon^{u_\alpha-u_\beta}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If now we wish to find the amplitude of the refracted ray polarized in the plane of incidence, which we will call B, we must in the above expression substitute

$$u=-\tfrac{1}{2}\log\tan x,$$

and we shall find

$$B=2A\frac{\cos\alpha\sin\beta}{\sin(\alpha+\beta)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

If, on the other hand, in (8) we substitute

$$u=\tfrac{1}{2}\log\sin 2x,$$

we find for B′, the amplitude of the portion of the refracted ray polarized perpendicularly to the plane of incidence,

$$B'=2A\frac{\cos\alpha\sin\beta}{\sin(\alpha+\beta)\cos(\alpha-\beta)}. \quad . \quad . \quad . \quad (10)$$

We return, therefore, exactly to Fresnel's formulæ, which is a remarkable property of those expressions. The calculation only assumes the relations indicated by (3) and (4); and these expressions might have been deduced from many other formulæ than Fresnel's.

The amplitude of the reflected ray, which is the sum of the amplitudes of the rays reflected 1, 3, 5 . . . times, may be similarly found, and may be expressed as follows:

$$ {}_{u_\alpha}^{u_\beta} du - \int_{u_\alpha}^{u_\beta} du \int_{u_\alpha}^{u} du_1 \int_{u_1}^{u_\beta} du_2 + \int_{u_\alpha}^{u_\beta} du \int_{u_\alpha}^{u} du_1 \int_{u_1}^{u_\beta} du_2 \int_{v_\alpha}^{v_2} du_3 \int_{u_3}^{u_\beta} du_4 - \ldots \Big], \qquad (11) $$

for which we will put

$$ \mathrm{A}\int_{u_\alpha}^{u_\beta} du(fu) $$

and

$$ f(u) = 1 - \int_{u_\alpha}^{u} du_1 \int_{u_1}^{u_\beta} du_2 f(u_2) $$

From the last equation we get

$$ f(u) = \frac{\epsilon^{u-u_\beta} + \epsilon^{u_\beta - u}}{\epsilon^{u_\alpha - u_\beta} + \epsilon^{u_\beta - u_\alpha}}; $$

whence (11) or the amplitude of the reflected ray is

$$ -\mathrm{A}\frac{\epsilon^{u_\alpha - u_\beta} - \epsilon^{u_\beta - u_\alpha}}{\epsilon^{u_\alpha - u_\beta} + \epsilon^{u_\beta - u_\alpha}}. \quad \ldots\ldots \quad (12) $$

And substituting $u = -\frac{1}{2}\log\tan x$, we get for the amplitude of the portion polarized in the plane of incidence, which we will call R,

$$ \mathrm{R} = \mathrm{A}\frac{\sin(\alpha - \beta)}{\sin(\alpha + \beta)}. \quad \ldots\ldots \quad (13) $$

If R′ is the amplitude of the ray polarized perpendicularly to the plane of incidence, and if in (12) we substitute $u = \frac{1}{2}\log\sin 2x$, we get

$$ \mathrm{R}' = -\mathrm{A}\frac{\tan(\alpha - \beta)}{\tan(\alpha + \beta)}. \quad \ldots\ldots \quad (14) $$

In this case also, therefore, we return to Fresnel's formulæ.

The result is, that even if there be a gradual change of the index of refraction between the two media, and consequently an infinite number of reflexions at the boundary, Fresnel's formulæ nevertheless remain true so long as the thickness of the intermediate layers is infinitely small as compared with the length of a wave. If this be not the case, then the retardations of the different rays must be taken into consideration.

For the refracted light this correction is very small, and could hardly be confirmed by experiment. We shall therefore proceed to calculate it in the case of the reflected light.

A wave which is reflected by the layer whose angle of refraction is x, or x_1, $x_2, \ldots$, and afterwards interferes with the wave reflected by the first layer, will be retarded relatively to the latter; and we may indicate the successive retardations of phase by the letters δ, δ_1, $\delta_2, \ldots$. These quantities are functions of x, x_1, $x_2 \ldots$; but may also be regarded as functions of u, u_1, $u_2, \ldots$

We may therefore represent the amplitude of the ray reflected once at the layer whose angle of refraction is x, by

$$\mathrm{A} \cos (kt-\delta)\, du,$$

where t is the time, and k a constant.

For reasons analogous to those stated above, it is easy to see that the amplitude of all the reflected rays may be expressed by

$$\mathrm{A}\left[\int_{u_\alpha}^{u_\beta} du \cos (kt-\delta) - \int_{u_\alpha}^{u_\beta} du \int_{u_\alpha}^{u} du_1 \int_{u_1}^{u_\beta} du_2 \cos (kt-\delta+\delta_1-\delta_2) + \ldots\right]. \quad (15)$$

This series is the real part of

$$\mathrm{A}\int_{u_\alpha}^{u} du\, \epsilon^{(kt-\delta)\sqrt{-1}} f(u),$$

where

$$f(u) = 1 - \int_{u_\alpha}^{u} du_1 \int_{u_1}^{u_\beta} du_2\, \epsilon^{(\delta_1-\delta_2)\sqrt{-1}} f(u_2).$$

From this last equation we may deduce the differential equation

$$\frac{d}{du}\left[\epsilon^{-\delta\sqrt{-1}} f'(u)\right] = \epsilon^{-\delta\sqrt{-1}} f(u);$$

from which we obtain another expression for series (15), since it is the real part of

$$\mathrm{A}\left[\epsilon^{(kt-\delta)\sqrt{-1}} f'(u)\right]_{u_\alpha}^{u_\beta};$$

or since for $u=u_\alpha$, we have $f'(u_\beta)=0$ and $\delta=0$, the real part of

$$-\mathrm{A}\epsilon^{kt\sqrt{-1}} f'(u_\alpha). \quad (15')$$

If in the above differential equation we substitute

$$f(u) = \epsilon^{\lambda\sqrt{-1}} \frac{\epsilon^{u-u_\beta} + \epsilon^{u_\beta-u}}{\epsilon^{u_\alpha-u_\beta} + \epsilon^{u_\beta-u_\alpha}},$$

λ will be determined as a function of u by the following equation:

$$\frac{d^2\lambda}{du^2} + \frac{\epsilon^{u-u_\beta} - \epsilon^{u_\beta-u}}{\epsilon^{u-u_\beta} + \epsilon^{u_\beta-u}} \cdot \frac{d(2\lambda-\delta)}{du} + \frac{d\lambda}{du} \cdot \frac{d(\lambda-\delta)}{du} \sqrt{-1} = 0,$$

$\lambda=0$ for $u=u_\alpha$, and $\dfrac{d\lambda}{du}=0$ for $u=u_\beta$.

We confine ourselves now to the case in which the values of $\frac{d\lambda}{du}$ and $\frac{d\delta}{du}$ are very small. The differential equation for λ then gives by integration, since the last member vanishes,

$$\frac{d\lambda}{du} = -\frac{1}{(\epsilon^{u-u_\beta}+\epsilon^{u_\beta-u})^2}\int_u^{u_\beta}(\epsilon^{2(u-u_\beta)}-\epsilon^{2(u_\beta-u)})\frac{d\delta}{du}\,du.$$

If in this expression we substitute the value of u, it is obvious that for all angles of incidence $\frac{d\lambda}{du}$ is small so long as $\frac{d\delta}{du}$ is so, which is the only hypothesis.

If now we substitute for $f(u)$ its values as found in (15′), then series (15) becomes the real part of

$$-\mathrm{A}\epsilon^{kt\sqrt{-1}}\left[\frac{\epsilon^{u_\alpha-u_\beta}-\epsilon^{u_\beta-u_\alpha}}{\epsilon^{u_\alpha-u_\beta}+\epsilon^{u_\beta-u_\alpha}}+\left[\frac{d\lambda}{du}\right]^{u=u_\alpha}\sqrt{-1}\right],$$

and its sum is therefore

$$-\mathrm{A}\frac{\epsilon^{u_\alpha-u_\beta}-\epsilon^{u_\beta-u_\alpha}}{\epsilon^{u_\alpha-u_\beta}+\epsilon^{u_\beta-u_\alpha}}\left[\cos kt+\sin kt\int_{u_\alpha}^{u_\beta}\frac{\epsilon^{2(u-u_\beta)}-\epsilon^{2(u_\beta-u)}}{\epsilon^{2(u_\alpha-u_\beta)}-\epsilon^{2(u_\beta-u_\alpha)}}\frac{d\delta}{du}\,du\right].$$

And substituting again in this expression for u its value $-\frac{1}{2}\log\tan x$, we get for R the amplitude of the reflected ray polarized in the plane of incidence,

$$\left.\begin{aligned}\mathrm{R}&=\mathrm{A}\frac{\sin(\alpha-\beta)}{\sin(\alpha+\beta)}[\cos kt+\sin kt\tan\Delta]\\ \tan\Delta&=\frac{\sin\alpha\cos\alpha}{\sin^2\alpha-\sin^2\beta}\int_\alpha^\beta[\cos^2\beta\tan x-\sin^2\beta\cot x]\frac{d\delta}{dx}\,dx.\end{aligned}\right\}(16)$$

If, on the other hand, for u we substitute $\frac{1}{2}\log\sin 2x$, we get for the amplitude of the reflected ray polarized perpendicularly to the plane of incidence,

$$\left.\begin{aligned}\mathrm{R}'&=-\mathrm{A}\frac{\tan(\alpha-\beta)}{\tan(\alpha+\beta)}[\cos kt+\sin kt\tan\Delta']\\ \tan\Delta'&=\frac{\sin 2\alpha\sin 2\beta}{\sin^2 2\alpha-\sin^2 2\beta}\int_\alpha^\beta\left[\frac{\sin 2x}{\sin 2\beta}-\frac{\sin 2\beta}{\sin 2x}\right]\frac{d\delta}{dx}\,dx.\end{aligned}\right\}\quad.\quad(17)$$

From these equations it may be seen that tan Δ, for all angles of incidence, is small provided $\frac{d\delta}{du}$ is so also; while, on the contrary, tan Δ′ may be infinite, as when $\sin 2\alpha=\sin 2\beta$; that is, when the angle of incidence is equal to the angle of polarization, in which case α and β are complementary.

If Δ', for a given angle of incidence, is a small positive quantity, it gradually approaches $\frac{\pi}{2}$ as the angle of incidence approximates to the angle of polarization, and afterwards approaches π. If, on the contrary, Δ' is a small negative quantity, on changing the angle of incidence Δ' approaches $-\frac{\pi}{2}$ and $-\pi$.

The retardation of the phase of the reflected ray R$'$, compared with the other polarized in the plane of incidence R, may be expressed by $\Delta'-\Delta$ if the coefficients of $\cos kt$ have the same sign for both rays, that is to say, when the angle of incidence is *greater* than the angle of polarization. If, however, Δ' and Δ be always taken in the first positive or negative quadrant, we can introduce any multiple we please of 2π, and therefore express the retardation by $\Delta'-\Delta+2p\pi$, where p is a whole number.

If now Δ is *positive* for this angle of incidence, it will increase as the angle of incidence diminishes; and when the angle of incidence becomes less than the angle of polarization, and Δ' is taken in the first quadrant, the retardation of phase will be expressed by

$$\Delta'-\Delta+(2p+1)\pi.$$

If, on the other hand, Δ' is *negative* for an angle of incidence greater than the angle of polarization, the retardation of phase will become $\Delta'-\Delta+(2p-1)\pi$, if the angle of incidence is made less than that of polarization. These results agree with those of Jamin. In the first case Jamin puts $p=-1$, whereby the retardation of phase becomes

$$\Delta'-\Delta-2\pi \text{ for } \alpha+\beta>\frac{\pi}{2},$$

$$\Delta'-\Delta-\pi \text{ for } \alpha+\beta<\frac{\pi}{2};$$

and bodies in which this is the case he calls "bodies of *negative* reflexion."

In the second case (Δ' negative for $\alpha+\beta<\frac{\pi}{2}$) he puts $p=1$; and the retardation of phase of these bodies, which he calls "bodies of *positive* reflexion," then becomes

$$\Delta'-\Delta+2\pi \text{ for } \alpha+\beta>\frac{\pi}{2},$$

$$\Delta'-\Delta+\pi \text{ for } \alpha+\beta<\frac{\pi}{2}.$$

Jamin found, moreover, that most bodies whose index of refraction is less than a given amount (about 1·46) give negative reflexions, while those whose index of refraction is greater than that amount give positive reflexions. Between these are bodies which at the angle of polarization produce a sudden change of phase from 0 to π. These remarkable relations between the difference of phase and index of refraction could not have been anticipated from Cauchy's theory, while on the other hand they can be immediately deduced from the theory above enunciated.

Let ρ be the distance of the first intermediate layer from that in which the angle of refraction is x, and $d\rho$ the distance of the latter from the next whose angle of refraction is $x+dx$. It will easily be seen that if a wave be reflected at these two consecutive layers, the difference of path of the two reflected waves will be equal to $2d\rho\cos x$.

If then l be the wave-length in the first medium, and therefore $l\dfrac{\sin x}{\cos x}$ the wave-length in the layer under consideration, we have for the difference of phase corresponding to this difference of path,

$$\frac{2\pi\sin\alpha}{l\sin x}.2d\rho\cos x=\frac{d\delta}{dx}dx.$$

Instead of x we might introduce, as a new variable, the square of the index of refraction. If this variable be called v, we have

$$v=\frac{\sin^2\alpha}{\sin^2 x}.$$

The limits of the variables ρ and v are, for $x=\alpha$, 0 and 1; and for $x=\beta$ they may be denoted by ρ_1 and v_1, where ρ_1 is the thickness of all the intermediate layers, and v_1 the square of the observed index of refraction.

If now the new values of $\dfrac{d\delta}{dx}$ and x be substituted in (16) and (17), and these expressions be integrated by parts, we have

$$\tan\Delta=\frac{4\pi}{l}\cdot\frac{\cos\alpha}{v_1-1}\int_1^{v_1}\rho\,dv, \quad \ldots\ldots\ldots \quad (18)$$

$$\tan\Delta'=\frac{4\pi}{l}\cdot\frac{v_1^2}{v_1\cos^2\alpha-\cos^2\beta}\int_1^{v_1}\rho\left[\frac{\cos^2\beta}{v_1^2}-\frac{\sin^2\beta}{v^2}\right]dv. \quad (19)$$

From which it is obvious that $\tan\Delta$ is always positive, whereas $\tan\Delta'$ may either be positive or negative.

In one particular case Δ' passes suddenly from 0 to $\pm\pi$ at the angle of polarization; this is when for this angle

$$\int_1^{v_1}\rho\left[\frac{\cos^2\beta}{v_1^2}-\frac{\sin^2\beta}{v^2}\right]dv=0;$$

or, as $\frac{\cos^2\beta}{\sin^2\beta}=v_1$,

$$\int_1^{v_1}\rho\left[1-\frac{v_1}{v^2}\right]dv=0.$$

As this integral includes positive as well as negative elements, it is evident that this equation is possible for some values of v_1. And if it be now supposed that for different bodies ρ is generally only approximately the same function of v, when v_1 increases by the positive increment dv_1, this integral increases by

$$\left[\rho_1\left(1-\frac{1}{v_1}\right)-\int_1^{v_1}\rho\frac{dv}{v^2}\right]dv_1,$$

which is less than

$$\left[\rho_1\left(1-\frac{1}{v_1}\right)-\rho_1\int_1^{v_1}\frac{dv}{v^2}\right]dv_1=0.$$

This increase is therefore *positive*; and it follows that the definite integral in (19), when v_1 exceeds a certain amount, is positive, and consequently Δ' *negative* for $v_1\cos^2\alpha-\cos^2\beta<0$ or for $\alpha+\beta>\frac{\pi}{2}$, if the light, as in Jamin's experiments, passes from a less refracting to a more refracting body ($\alpha>\beta$). But we have, however, seen above that this case answers to that of bodies with a positive reflexion.

Calculation therefore, like experiment, proves that positive reflexion occurs in the case of bodies with a greater index of refraction, negative reflexion in the case of bodies with a smaller index, while the difference of phase in the case of bodies which lie between these passes suddenly from 0 to $\pm\pi$.

The intensity of the reflected light polarized perpendicularly to the plane of incidence is, according to (17),

$$A^2\frac{\tan^2(\alpha-\beta)}{\tan^2(\alpha+\beta)}(1+\tan^2\Delta');$$

the intensity of the reflected light polarized in that plane is, according to (16),

$$A^2\frac{\sin^2(\alpha-\beta)}{\sin^2(\alpha+\beta)}(1+\tan^2\Delta).$$

If the ratio of these two intensities be expressed by k^2, we get

$$k=\frac{\cos(\alpha+\beta)}{\cos(\alpha-\beta)}\frac{\cos\Delta}{\cos\Delta'}. \quad \quad . \quad . \quad . \quad . \quad (20)$$

These results agree with those of Cauchy.

As Jamin has in his experiments determined these magnitudes directly, it is possible from his experiments, that is, from the principal angle of incidence, and the index of refraction, to deduce the value of Δ, and consequently by (18) the value of $\int_1^{v_1} \rho\, dv$. If now it be supposed that ρ can be approximately determined by $\rho_1 \frac{v-1}{v_1-1}$, the thickness of the intermediate layers can be deduced from the experiments.

In this way we have a new means of testing the theory, since this requires that the thickness in question should be small and positive. The calculation from Jamin's experiments shows that this is actually the case, though, as might be expected, no great degree of exactness can be thus attained in the determination of these quantities. I have found that the thickness of the layers in the case of the bodies experimented on lies between $\frac{1}{10}$ and $\frac{1}{100}$ of the length of a wave.

It appears, therefore, that the result of Jamin's experiments can be completely explained on the simple supposition of an exceedingly thin stratum of intermediate layers in which there is a gradual change of refractive power, a supposition which we have obviously more right to make than to omit.

Copenhagen, June 28, 1860.

LXXII. *On a Surface of the Fourth Order.*

By A. Cayley, *Esq.*[*]

Let A, B, C be fixed points; it is required to investigate the nature of the surface, the locus of a point P such that

$$\lambda AP + \mu BP + \nu CP = 0,$$

where λ, μ, ν are given coefficients; the equation depends, it is clear, on the ratios only of these quantities.

The surface is easily seen to be of the fourth order; it is obviously symmetrical in regard to the plane ABC; and the section by this plane, or say the principal section, is a curve of the fourth order, the locus of a point M such that

$$\lambda AM + \mu BM + \nu CM = 0.$$

The curve is considered incidentally by Mr. Salmon, p. 125 of his 'Higher Plane Curves;' and he has remarked that the two circular points at infinity are double points on the curve, which is therefore of the eighth class. Moreover, that there are two double foci, since at each of these circular points there are two tangents, each tangent of the one pair intersecting a tangent of the other pair in a double focus; hence, further, that there are

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[*] Communicated by the Author.

four other foci, the points A, B, C, and a fourth point D lying in a circle with A, B, C, and which are such that, selecting any three at pleasure of the points A, B, C, D, the equation of the curve is in respect to such three points of the same form as it is in regard to the points A, B, C.

Consider a given point M, on the principal section, then the equations

$$\frac{\mathrm{BP}}{\mathrm{BM}}=\frac{\mathrm{CP}}{\mathrm{CM}},\quad \frac{\mathrm{CP}}{\mathrm{CM}}=\frac{\mathrm{AP}}{\mathrm{AM}},\quad \frac{\mathrm{AP}}{\mathrm{AM}}=\frac{\mathrm{BP}}{\mathrm{BM}}$$

belong respectively to three spheres: each of the spheres passes through the point M. The first of the spheres is such that, with respect to it, B and C are the images each of the other; that is, the centre of the sphere lies on the line BC, and the product of its distances from B and C is equal to the square of the radius; in like manner the second sphere is such that, with regard to it, C and A are the images each of the other; and the third sphere is such that, with regard to it, A and B are the images each of the other. The three spheres intersect in a circle through M at right angles to the principal plane (that is, the three spheres have a common circular section), and the equations of this circle may be taken to be

$$\frac{\mathrm{AP}}{\mathrm{AM}}=\frac{\mathrm{BP}}{\mathrm{BM}}=\frac{\mathrm{CP}}{\mathrm{CM}}.$$

It is clear that the circle of intersection lies wholly on the surface.

The spheres meet the principal plane in three circles, which are the diametral circles of the spheres; these circles are related to each other and to the points A, B, C, in like manner as the spheres are to each other and to the same points. The circles have thus a common chord; that is, they meet in the point M and in another point M′. And MM′ is the diameter of the circle, the intersection of the three spheres.

It may be shown that M, M′ are the images each of the other in respect to the circle through A, B, C. In fact, consider in the first place the two points A, B, and a circle such that, with respect to it, A, B are the images each of the other; take M a point on this circle, and let O be any point on the line at right angles to AB through its middle point, and join OM cutting the circle in M′; then it is easy to see that M, M′ are the images each of the other, in regard to the circle, centre O and radius OA (=OB). Hence starting with the points A, B, C and the point M, let O be the centre of the circle through A, B, C, and take M′ the image of M in respect to this circle; then considering the circle which passes through M, and in respect to which B, C are images each of the other, this circle passes through M′; and so the circle through M, in respect to which C, A are images each of the other, and the circle through M, in respect to which A, B are images each of the other, pass each of them through M′; that is, the three circles intersect in M′.

It is to be noticed that M′, being on the surface, must be on the principal section; that is, the principal section is such that, taking upon it any point M, and taking M′ the image of M in regard to the circle through A, B, C, then M′ is also on the principal section. It is very easily shown that the curve of the fourth order possesses this property; for M, M′ being images each of the other in respect to the circle through A, B, C, then A, B, C are points of this circle, or we have

$$\frac{\mathrm{MA}}{\mathrm{M'A}}=\frac{\mathrm{MB}}{\mathrm{M'B}}=\frac{\mathrm{MC}}{\mathrm{M'C}};$$

that is, the equation

$$\lambda\mathrm{AM}+\mu\mathrm{BM}+\nu\mathrm{CM}=0$$

being satisfied, the equation

$$\lambda\mathrm{AM'}+\mu\mathrm{BM'}+\nu\mathrm{CM'}=0$$

is also satisfied.

The points M, M′ of the curve, which are images each of the other in respect to the circle through A, B, C, may be called conjugate points of the curve. The above-mentioned circle, the intersection of the three spheres, is the circle having MM′ for its diameter; hence the required surface is the locus of a circle at right angles to the principal plane, and having for its diameter MM′, where M and M′ are conjugate points of the curve.

In the particular case where the equation of the surface is

$$\mathrm{BC}\,.\,\mathrm{AP}+\mathrm{CA}\,.\,\mathrm{BP}+\mathrm{AB}\,.\,\mathrm{CP}=0,$$

the principal section is the circle through A, B, C, twice repeated. Any point on the circle is its own conjugate, and the radius of the generating circle of the surface is zero; that is, the surface is the annulus, the envelope of a sphere radius 0, having its centre on the circle through A, B, C. Or attending to real points only, the surface reduces itself to the circle through A, B, C. But this last statement of the solution is an incomplete one. The equation of an annulus, the envelope of a sphere radius c, having its centre on a circle radius unity, is

$$\sqrt{x^2+y^2}=1\pm\sqrt{c^2-z^2};$$

and hence putting $c=0$, the equation of the surface is,

$$\sqrt{x^2+y^2}=1\pm zi$$

(if, as usual, $i=\sqrt{-1}$), or, what is the same thing, it is

$$x^2+y^2+(z\pm i)^2=0;$$

that is, the surface is made up of the two spheres, passing through the points A, B, C, and having each of them the radius zero; or say the two *cone-spheres* through the points A, B, C. In other words, the equation

$$\overline{\mathrm{BC}}\,.\,\overline{\mathrm{AP}}+\overline{\mathrm{CA}}\,.\,\overline{\mathrm{BP}}+\overline{\mathrm{AB}}\,.\,\overline{\mathrm{CP}}=0$$

is the condition in order that the four points A, B, C, P may lie on a sphere radius zero, or cone-sphere. Using 1, 2, 3, 4 in the place of A, B, C, P to denote the four points, the last-mentioned equation becomes

$$\overline{12}\,.\,\overline{34}+\overline{13}\,.\,\overline{42}+\overline{14}\,.\,\overline{23}=0;$$

and considering $\overline{12}$, &c. as quadratic radicals, the rational form of this equation is

$$\square=\begin{vmatrix} 0\,, & \overline{12}^2, & \overline{13}^2, & \overline{14}^2 \\ \overline{21}^2, & 0\,, & \overline{23}^2, & \overline{24}^2 \\ \overline{31}^2, & \overline{32}^2, & 0\,, & \overline{34}^2 \\ \overline{41}^2, & \overline{42}^2, & \overline{43}^2, & 0 \end{vmatrix}=0.$$

In my paper "On a Theorem in the Geometry of Position," Camb. Math. Journ. vol. ii. pp. 267–271 (1841), I obtained this equation, the four points being there considered as lying in a plane, as the relation between the distances of four points in a circle, in addition to the relation

$$\begin{vmatrix} & 1\,, & 1\,, & 1\,, & 1 \\ 1, & 0\,, & \overline{12}^2, & \overline{13}^2, & \overline{14}^2 \\ 1, & \overline{21}^2, & 0\,, & \overline{23}^2, & \overline{24}^2 \\ 1, & \overline{31}^2, & \overline{32}^2, & 0\,, & \overline{34}^2 \\ 1, & \overline{41}^2, & \overline{42}^2, & \overline{43}^2, & 0 \end{vmatrix}=0,$$

which exists between the distances of any four points in a plane. The present investigation shows the signification of the equation $\square=0$ between the distances of four points in space; viz. it expresses that the four points lie in a sphere radius zero, or cone-sphere. But the formula in question is in reality included in that given in the paper for the distances of five points in space. For calling the points 0, 1, 2, 3, 4, the relation between the distances of these five points is

$$\begin{vmatrix} 0, & 1\ , & 1\ , & 1\ , & 1\ , & 1 \\ 1, & 0\ , & \overline{01}^2, & \overline{02}^2, & \overline{03}^2, & \overline{04}^2 \\ 1, & \overline{10}^2, & 0\ , & \overline{12}^2, & \overline{13}^2, & \overline{14}^2 \\ 1, & \overline{20}^2, & \overline{21}^2, & 0\ , & \overline{23}^2, & \overline{24}^2 \\ 1, & \overline{30}^2, & \overline{31}^2, & \overline{32}^2, & 0\ , & \overline{34}^2 \\ 1, & \overline{40}^2, & \overline{41}^2, & \overline{42}^2, & \overline{43}^2, & 0 \end{vmatrix} = 0.$$

Hence if 1, 2, 3, 4 are the centres of spheres radii $\alpha, \beta, \gamma, \delta$, and if 0 is the centre of a tangent sphere radius r, we have

$$\overline{01}=r\pm\alpha, \quad \overline{02}=r\pm\beta, \quad \overline{03}=r\pm\gamma, \quad 04=r\pm\delta;$$

so that, for any given combination of signs, it would at first sight appear that r is determined by a quartic equation; but by means of a simple transformation (indicated to me by Prof. Sylvester) it may be shown that the equation for r is really a quadratic one; moreover, the equation remains unaltered if the signs of $\alpha, \beta, \gamma, \delta$ and of r, are all reversed; and r^2 has thus in the whole sixteen values. In particular, if $\alpha, \beta, \gamma, \delta$ are each equal 0, then r^2 is determined by a simple equation (r the radius of the sphere through the four points); and if, moreover, $r=0$, then we have for the relation between the distances of the four points, the foregoing equation $\square=0$.

2 Stone Buildings, W.C.,
March 25, 1861.

LXXIII. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 365.]

MM. LOIR and Drion* describe the following method of obtaining solid carbonic acid, merely requiring for its preparation apparatus within the ordinary reach of the laboratory. It depends on the great cold produced by the evaporation of liquid sulphurous acid. Liquid ammonia is placed in a glass vessel, and connected with the receiver of an air-pump by a vessel containing pumice impregnated with sulphuric acid. On exhausting, the temperature of the liquid ammonia rapidly sinks, and it commences to solidify at −81° C.; when the pressure is reduced to 1 millim., the temperature of the liquid ammonia is −89°·5. This is sufficient for the liquefaction of carbonic acid under the ordinary atmospheric pressure; for when a current of dry carbonic

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* *Comptes Rendus,* April 15, 1861.

acid is passed through a U-tube dipping in the ammonia, a small portion of it liquefies.

By increasing the pressure to some extent, considerable quantities of carbonic acid may be readily solidified. About 150 cubic centims. of liquid ammonia are introduced into an inverted bell-jar provided with a collar, on which a plate perforated with two apertures is hermetically fitted. In the central aperture there is a tube closed at one end and reaching to the bottom of the jar; the other aperture serves to connect the apparatus with the air-pump. The carbonic acid is produced by heating dried bicarbonate of soda to reduess in a copper flask. This flask is connected with the tube dipping in the liquid ammonia, and also with a small air-manometer. All the air having been expelled from the apparatus, and the temperature of the liquid ammonia reduced to near solidification, the flask is heated until the manometer indicates a pressure of 3 to 4 atmospheres. Crystals of carbonic acid soon begin to form on the inside of the tube, and in half an hour about 25 grammes of solid carbonic acid are obtained, forming a thick layer on the inside of the tube which dips in the liquid ammonia.

This solid carbonic acid is a colourless mass, as transparent as glass; it may be detached from the tube by touching it with a glass rod, and is seen to consist of small cubical crystals. Exposed to the air, these crystals slowly evaporate without leaving any residue; they may be placed on the hand without producing any sensation either of heat or of cold: they can be scarcely seized between the fingers. Mixed with ether and exposed to the air, they form a freezing mixture, the temperature of which is −81° C.

The temperatures were observed by MM. Loir and Drion, by means of an alcohol thermometer on which two fixed points had been marked; that is, 0° the temperature of melting ice, and −40° the temperature of melting mercury. The liquid ammonia was prepared by Bussy's method*, of passing gaseous ammonia into a flask surrounded by liquid sulphurous acid, the evaporation of which was promoted by the air-pump. In this way 6 to 7 fluid ounces may be obtained without difficulty in the course of two hours.

The following experiments by Deville† throw considerable light on the formation of some native minerals.

When fluoride of silicon was passed over calcined alumina heated to whiteness in a porcelain tube, fluoride of aluminium was disengaged, and staurotide formed analogous in all its pro-

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* Phil. Mag. vol. xx. p. 202.

† *Comptes Rendus*, April 22, 1861.

perties to the natural mineral. This experiment was repeated in a modified manner.

In a porcelain tube placed vertically, a series of alternate layers of alumina and quartz were arranged, the alumina being at the bottom, and the quartz at the top; fluoride of silicon was then passed through the tube at a white heat. In this way the fluoride of silicon meeting alumina was decomposed, and staurotide formed; but the fluoride of aluminium which was formed at the same time was decomposed on coming in contact with the layer of quartz, with the formation also of staurotide and regeneration of fluoride of silicon. The same process followed with all the successive layers; so that the quartz and alumina were both converted into staurotide, and, as the last layer was quartz, as much fluoride of silicon left the apparatus as entered it. None of the fluorine was fixed, and it served no other purpose than to cause the combination of two of the most stable bodies in nature.

From the formula of topaz, which is a silicate of alumina and fluoride of silicon, it was probable that it might be formed in a similar way. But direct experiments showed that this is not the case; and Deville is inclined to think that it is formed in the moist way.

In the expectation of obtaining phenakite, Deville heated glucina in fluoride of silicon. He obtained a mineral which crystallizes well, and consists of silica and glucina, but could not be identified with any known mineral species.

When fluoride of silicon was passed over zirconia, beautiful octahedral crystals were obtained which had all the characters of the native zircon. An experiment of Deville's seems to show that a very small quantity of fluorine can produce an indefinite quantity of this mineral.

Alternate layers of zirconia and quartz were placed in a porcelain tube, commencing with the former and ending with the latter, and a current of fluoride of silicon was passed through the tube at a white heat. The zirconia in contact with fluoride of silicon was changed into zircon and volatile fluoride of zircon; the latter meeting quartz, gave zircon also and fluoride of silicon; and so on with the whole of the layers. The contents of the tube were entirely mineralized, and the quantity of fluoride of silicon which left the tube was equal to that which entered it. No fluorine had been fixed.

In a subsequent communication Deville will describe a method for obtaining metallic sulphurets by the dry way.

Schützenberger* has described a new class of salts, in which the electro-negative elements chlorine, bromine, iodine, &c.

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* *Comptes Rendus*, January 28, 1861.

are substituted for the basic hydrogen, the metals, &c. He has effected this by acting on salts with such compounds as chloride of iodine and iodide of cyanogen. The formation of acetate of iodine will illustrate this class of actions, which is susceptible of great extension.

$$\underset{\text{Acetate of soda.}}{C^4H^3NaO^4} + \underset{\text{Chloride of iodine.}}{ICl} = NaCl + \underset{\text{Acetate of iodine.}}{C^4H^3IO^4}.$$

These bodies, as may be expected, are endowed with special properties, and especially are very unstable.

Anhydrous hypochlorous and acetic acids mixed in equivalents form a red mixture, which soon becomes decolorized. A slight excess of hypochlorous acid imparts to it a red tint, which is removed by heating the mixture to a temperature not exceeding 30°. This body is the acetate of chlorine, $C^4H^3ClO^4$; its composition is that of chloracetic acid, but it differs greatly in properties. It dissolves immediately in water, producing hypochlorous and acetic acids, and explodes at 100°, with formation of chlorine, oxygen, and anhydrous acetic acid. Singularly enough it is attacked by mercury even in the cold, with liberation of chlorine, and formation of acetate of mercury and a little calomel—

$$\underset{\text{Acetate of chlorine.}}{C^4H^3ClO^4} + Hg = Cl + \underset{\text{Acetate of mercury.}}{C^4H^3HgO^4},\text{—}$$

a curious instance of the replacement of chlorine by a metal.

It dissolves iodine instantaneously without becoming coloured, and disengages chlorine; acetate of iodine is formed, a white crystalline solid isomeric with iodacetic acid.

$$\underset{\text{Acetate of chlorine.}}{C^4H^3ClO^4} + I = \underset{\text{Acetate of iodine.}}{C^4H^3IO^4} + Cl.$$

Another mode of forming this body has been given above. It is decomposed at 100° into iodine, oxygen, and acetate of methyle.

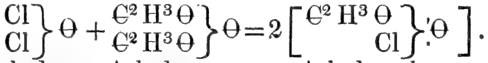
$$2(\underset{\text{Acetate of iodine.}}{C^4H^3IO^4}) = I^2 + C^2O^4 + \underset{\text{Acetate of methyle.}}{C^4H^3(C^2H^3)O^4}.$$

It is decomposed by water into iodic acid, iodine, and acetic acid.

Butyrate of iodine is formed by the action of chloride of iodine on butyrate of soda. Acetate of bromine is obtained by the action of bromine on acetate of chlorine.

Sulphur dissolves in acetate of chlorine with disengagement of chlorine; but the acetate of sulphur which forms is very unstable, for it soon decomposes into anhydrous acetic acid, sulphurous acid, sulphur, and chlorine. The action of iodide of cyanogen on acetate of silver forms iodide of silver, and apparently iodide of cyanogen.

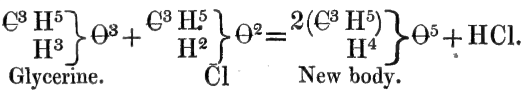
These interesting facts are capable, as Wurtz suggests*, of another interpretation, by assuming that the bodies are mixed anhydrous acids. Thus the acetate of chlorine is hypochloracetic anhydride, and Wurtz expresses its formation in the following manner:—



$$\left.\begin{matrix}Cl\\Cl\end{matrix}\right\}\Theta + \left.\begin{matrix}\text{Ꞓ}^2H^3\Theta\\\text{Ꞓ}^2H^3\Theta\end{matrix}\right\}\Theta = 2\left[\left.\begin{matrix}\text{Ꞓ}^2H^3\Theta\\Cl\end{matrix}\right\}\Theta\right].$$

Anhydrous hypochlorous acid. Anhydrous acetic acid. Anhydrous hypochloracetic acid.

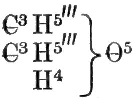
Lourenço has described† a series of new compounds, the polyglyceric alcohols. They bear the same relation to glycerine that the polyethylenic alcohols‡ do to glycol. These latter bodies were obtained by the action of hydrochloric glycol on glycol in excess: the formation of the new bodies is quite analogous; it ensues when hydrochlorate of glycerine acts on glycerine in excess. Lourenço saturated a portion of glycerine with hydrochloric acid gas, and having added to it an equal quantity of glycerine, he heated the whole to 180° C. for several hours in a flask connected with a condenser so that the distillate fell again into the flask. The result of this action was a very thick brown liquid, which was distilled under a pressure of 10 millims. A body was obtained boiling at 220°—230° under this pressure. It had the composition $\text{Ꞓ}^6H^{14}\Theta^5$, and its formation may be thus expressed:—



$$\left.\begin{matrix}\text{Ꞓ}^3H^5\\H^3\end{matrix}\right\}\Theta^3 + \left.\begin{matrix}\text{Ꞓ}^3H^5\\H^2\\Cl\end{matrix}\right\}\Theta^2 = \left.\begin{matrix}2(\text{Ꞓ}^3H^5)\\H^4\end{matrix}\right\}\Theta^5 + HCl.$$

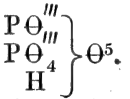
Glycerine. Hydrochloric glycerine. New body.

This body, which Lourenço names pyroglycerine, or diglyceric alcohol, is formed by the condensation of two molecules of glycerine and elimination of one molecule of water. It is analogous in its chemical composition to Graham's pyrophosphoric acid.



$$\left.\begin{matrix}\text{Ꞓ}^3H^{5'''}\\\text{Ꞓ}^3H^{5'''}\\H^4\end{matrix}\right\}\Theta^5$$

Pyroglycerine.



$$\left.\begin{matrix}P\Theta'''\\P\Theta'''\\H^4\end{matrix}\right\}\Theta^5.$$

Pyrophosphoric acid.

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[*] *Répertoire de Chimie*, April 1861.

[†] *Comptes Rendus*, February 25, 1861.

[‡] Phil. Mag. vol. xix. p. 124.

Besides this there was formed at the same time another body of analogous properties, but possessing a greater viscosity. It boils at 275° to 285° under the pressure of 10 millims.; it has the composition $\text{Ȼ}^9 H^{20} \Theta^7$, and is derived from three molecules of glycerine with elimination of two molecules of water. It is analogous to triethylenic alcohol in the series of condensed glycols.

In the crude product from which these bodies were obtained, there were several chlorine compounds which distilled at the ordinary atmospheric pressure, and were separated by fractional distillation. A portion of this, boiling between 230° and 270°, which chiefly consisted of hydrochlorate and dihydrochlorate of pyroglycerine, was treated with potash, by which chloride of potassium was formed, and a body obtained which, on purification and analysis, was found to have the composition

$$\text{Ȼ}^6 H^{12} \Theta^4 = \left.\begin{matrix}\text{Ȼ}^3 H^5 \\ \text{Ȼ}^3 H^5 \\ H^2\end{matrix}\right\} \Theta^4.$$

It is metameric with glycide, the existence of which has been placed out of doubt by Reboul's researches*. Lourenço names it *pyroglycide*; it stands in the same relation to pyroglycerine that glycide does to glycerine, being formed from it by the elimination of water.

$$\left.\begin{matrix}\text{Ȼ}^3 H^5 \\ H^3\end{matrix}\right\} \Theta^3 - H^2 \Theta = \underset{\text{Glycide.}}{\left.\begin{matrix}\text{Ȼ}^3 H^5 \\ H\end{matrix}\right\} \Theta^2}.$$

$$\left.\begin{matrix}2(\text{Ȼ}^3 H^5) \\ H^4\end{matrix}\right\} \Theta^5 - H^2 \Theta = \underset{\text{Pyroglycide.}}{\left.\begin{matrix}2(\text{Ȼ}^3 H^5) \\ H^2\end{matrix}\right\} \Theta^4}.$$

There is another way of obtaining these polyglyceric alcohols, which throws some light on their formation. When glycerine was heated, and the part collected which distilled between 130° and 266°, and this portion treated with ether, an insoluble residue was left. This body gave distillates up to 300°, under a pressure of 10 millims., consisting of pyroglyceric alcohols. It is highly probable that in this decomposition, glycerine losing one molecule of water forms glycide, and this combining with one, two, or three equivalents of glycerine, forms polyglyceric compounds; just as oxide of ethylene, in acting upon one, two, or three equivalents of glycol, forms polyethylenic alcohols.

Lourenço points out that the formation of these polyethylenic alcohols suggests a plausible explanation of the formation of the different modifications of metaphosphoric acid, which are ob-

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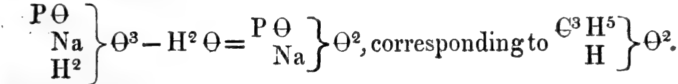
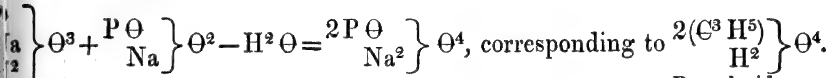
* Phil. Mag. April 1861.

tained by heating microcosmic salt, or acid phosphate of soda. Graham's metaphosphate of soda acts like glycide or oxide of ethylene, and by successive condensations gives rise to the different modifications of the acid.



$$\left.\begin{matrix}P\Theta\\ Na\\ H^2\end{matrix}\right\}\Theta^3 - H^2\Theta = \left.\begin{matrix}P\Theta\\ Na\end{matrix}\right\}\Theta^2, \text{ corresponding to } \left.\begin{matrix}\text{Ȼ}^3H^5\\ H\end{matrix}\right\}\Theta^2.$$

Acid phosphate of soda. Metaphosphate of soda. Glycide.

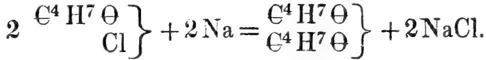


$$\left.\begin{matrix}\\ a\\ 2\end{matrix}\right\}\Theta^3 + \left.\begin{matrix}P\Theta\\ Na\end{matrix}\right\}\Theta^2 - H^2\Theta = \left.\begin{matrix}2P\Theta\\ Na^2\end{matrix}\right\}\Theta^4, \text{ corresponding to } \left.\begin{matrix}2(\text{Ȼ}^3H^5)\\ H^2\end{matrix}\right\}\Theta^4.$$

Maddrell's metaphosphate of soda. Pyroglycide.

Freund* has made a series of experiments on the preparation of the oxygen radicals of the formic acid series by the action of metals on the chloride of these acids, a method analogous to that of the preparation of the ether radicals.

When chloride of acetyle was treated with sodium-amalgam no action ensued in the cold, and the action set up at a higher temperature produced a complete decomposition with formation of empyreumatic substances. The action of chloride of butyryle, $\text{Ȼ}^4H^7\Theta Cl$, on sodium-amalgam gave better results. When slightly warmed together, an action was induced which disengaged heat sufficient to continue it; the flask was connected with a Liebig's condenser, so that the distillate flowed back. After a short time the mixture was distilled; the unaltered chloride passed off; the residue, consisting of mercury, chloride of sodium, and butyryle, was digested with water, and the butyryle which rose to the surface removed. The chloride of butyryle which had distilled off was treated again with sodium-amalgam, and the process repeated until a large quantity of butyryle had been accumulated. This was digested with carbonate of potash, washed, dried over chloride of calcium, and rectified. No product of constant boiling-point was obtained; but a portion distilling between 260° and 280° gave on analysis numbers agreeing with the formula of butyryle, or rather dibutyryle, $\text{Ȼ}^8H^{14}\Theta^2 = \left.\begin{matrix}\text{Ȼ}^4H^7\Theta\\ \text{Ȼ}^4H^7\Theta\end{matrix}\right\}$. Its formation may be thus expressed:—



$$2\left.\begin{matrix}\text{Ȼ}^4H^7\Theta\\ Cl\end{matrix}\right\} + 2Na = \left.\begin{matrix}\text{Ȼ}^4H^7\Theta\\ \text{Ȼ}^4H^7\Theta\end{matrix}\right\} + 2NaCl.$$

Chloride of butyryle. Sodium. Butyryle.

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* Liebig's *Annalen*, April 1861.

The action of strong potash on butyryle is very energetic; butyrate of potash is formed as well as a substance of a pleasant odour, which has the composition of the ketone of butyric acid, but in properties appears to be quite different.

Martius* has published an investigation on the cyanides of the metals associated with platinum. In their preparation he used the residues obtained from the manufacture of Russian platinum. The method of separating the metals which he adopted is a combination of several methods, and presents some interesting points.

The residues were finely powdered, and the larger grains of osmium-iridium separated by decantation. The residue having been dried and heated, was fused with a mixture of lead and oxide of lead, by which all the silicates and other similar impurities passed into the slag, and a lead regulus was obtained containing all the platinum metals. When this was treated with diluted nitric acid, a residue was left consisting principally of iridium and osmium-iridium. The latter was separated by decantation. To bring it into a state of fine powder, which could not be effected in the ordinary way on account of its hardness, it was melted with zinc in a carbon crucible, by which it was dissolved; when this mass was afterwards heated in a wind furnace, the zinc was expelled and the mineral left in a state of fine powder.

The osmium-iridium was then heated in a current of oxygen; some osmic acid was formed, which volatilized, and was collected in a well-cooled receiver. The residue and the iridium were then mixed with an equal weight of common salt, and heated in a current of chlorine; the mass was dissolved in water, and the solution which contained the double chlorides was boiled with aqua regia, by which osmium was removed as osmic acid, and was received in a solution of ammonia. The residual solution was then mixed with sal-ammoniac, which precipitated everything, excepting a little rhodium, as ammonium double salt. The precipitate consisting principally of iridium, but containing also some platinum and ruthenium, was fused with cyanide of potassium to convert it into cyanides; this was boiled with hydrochloric acid to decompose excess of cyanide of potassium, and then sulphate of copper added, which gave a red precipitate of the copper salt.

By digesting this precipitate with baryta water, oxide of copper was formed and the barium double cyanides. They were easily separated by crystallization, the platinocyanide of barium being more insoluble than the iridiocyanide of barium. The small quantity of ruthenium was contained in the mother-liquor of this latter salt.

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* Liebig's *Annalen*, March 1861.

Osmiocyanide of Potassium, $Os\,Cy, 2\,KCy + 3\,HO$.—This salt was prepared by adding cyanide of potassium to a solution of osmic acid, evaporating to dryness, and heating to redness in a covered crucible. On dissolving out the mass in a small quantity of water and crystallizing, the salt was obtained in fine yellow laminæ, which belong to the dimetric system, like the ferrocyanide of potassium: the osmiocyanide of potassium, besides being analogous in composition and crystalline form to this salt, has the still closer resemblance that it exhibits (as a special investigation by Kobell showed) the same abnormities in its optical relations which have lately been found characteristic of ferrocyanide of potassium.

By the action of nitric acid on osmiocyanide of potassium a nitro-compound appeared to be formed. Experiments made to prepare a series of compounds analogous to the ferricyanides were unsuccessful. Chlorine passed into a solution of osmiocyanide of potassium, produced a red colour, but on evaporation only crystals of the double salt of chloride of osmium and chloride of potassium were obtained.

Osmiocyanide of Hydrogen: *Osmiocyanic Acid*, $Os\,Cy, 2\,HCy$.—This substance was obtained in a way analogous to the ferrocyanic acid: by treating a cold saturated solution of osmiocyanide of potassium with fuming hydrochloric acid,

$$Os\,Cy, 2\,KCy + 2\,HCl = Os\,Cy, 2\,HCy + 2\,KCl,$$

—a reaction which distinguishes osmium and ruthenium from other platinum metals—a precipitate was obtained, which was filered, washed with strong hydrochloric acid, dissolved in alcohol, and ether added. In this way the body was obtained in transparent columnar crystals of strongly acid properties.

Perfectly stable in the dry state, osmiocyanic acid decomposes, when exposed to the air in a moist state, into cyanide of osmium and hydrocyanic acid.

When any osmiocyanide is boiled with strong hydrochloric acid, hydrocyanic acid is disengaged, and a dark-violet precipitate is formed, which is cyanide of osmium, $Os\,Cy$.

When osmiocyanide of potassium is mixed with a persalt of iron, a splendid violet precipitate is formed, which is as delicate a test for iron as the ferrocyanide. This precipitate could not be analysed, on account of its retaining a quantity of water, which could not be expelled without decomposition; but it is doubtless formed according to the equation

$$2\,Fe^2\,Cl^3 + 3\,Os\,Cy, 6\,KCy = 3\,Os\,Cy, 2\,Fe^2\,Cy^3 + 6\,KCl.$$

When this precipitate was treated with baryta water, sesquioxide of iron was formed, and osmiocyanide of barium passed into solution and was afterwards obtained in reddish-brown crystals of the trimetric system. Its composition is $\mathrm{Os\,Cy, 2Ba\,Cy + 6HO}$, and it is isomorphous with ferrocyanide of barium.

Iridiocyanide of Barium, $\mathrm{Ir^2\,Cy^3, 3Ba\,Cy + 18HO}$.—This salt was obtained in the process of separating the platinum metals. It forms well-defined crystals of the trimetric system. When treated with a proper quantity of sulphuric acid and the mass exhausted with ether, the iridiocyanide of hydrogen or *iridiocyanic acid,* $\mathrm{Ir^2\,Cy^3, 3KCy}$, is obtained in crystalline crusts on evaporating the etherial solution. It is strongly acid, and its solution decomposes carbonates. Crystallized from ether it is anhydrous. The iridiocyanide of potassium, $\mathrm{Ir^2\,Cy^3, 3KCy}$, has been already described by Wöhler and by Booth. According to Martius, the iridiocyanide of barium, which is easily obtained pure, affords a convenient means of preparing it.

Rhodiocyanide of Potassium, $\mathrm{Rh^2\,Cy^3, 3\,K\,Cy}$.—This salt is analogous in composition and mode of preparation to the preceding salt, but is acted upon by acetic acid, which is not the case with the iridium salt. When treated with acetic acid, hydrocyanic acid is disengaged and a red powder is precipitated, which is the cyanide of rhodium, $\mathrm{Rh^2\,Cy^3}$. This deportment furnishes a means of separating the two metals.

Martius further remarks upon the preparation of the platinocyanides of potassium, and describes some new double cyanides.

LXXIII. *On Theoretical Physics.*
By Professor CHALLIS, *F.R.S., F.R.A.S.*[*]

I HAVE been induced to make the following remarks chiefly because I am unwilling to appear inattentive to the repeated notices which Mr. Glennie has taken of my mathematical theory of the physical forces. But I have occasion to do little more than give my reasons for concluding that nothing which Mr. Glennie has urged calls for a reply. Of course I do not object to my hypotheses being tested and scrutinized in all possible ways that are legitimate: at the same time I must maintain the Newtonian doctrine, that no arguments can be adduced either for or against a hypothesis which are not drawn from experience, or from a comparison of the mathematical results of the hypothesis with experience. Also it is a necessary condition of a hypothesis that it be expressed in terms which experience makes intelligible. Mr. Glennie has not said that the statement of my hypotheses does not fulfil this condition; neither has he adduced any facts contradictory to results mathematically deduced from

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[*] Communicated by the Author.

them. He has not even alluded to my mathematics. I say, therefore, that there is no argument which I have to meet.

In prosecuting physical inquiry, it appears to be necessary to proceed by way of hypotheses. But hypotheses of themselves teach nothing: we *learn* by mathematics, as the very name implies, because by mathematics the truth of a hypothesis may be tested or established. The existence of gravity as a *force*, and the law of gravity, are truths which could not be ascertained by observation alone; but being taken to be true hypothetically, they are proved to be actually true, by the aid of mathematics.

Hence hypotheses respecting the physical forces are deserving of consideration only so far as they afford a basis for mathematical reasoning. In fact this quality of a hypothesis is a criterion of its truth, because all quantitative laws are deducible mathematically from *true* hypotheses. In selecting hypotheses for the foundation of a general theory of the physical forces, I had regard, in the first place, to their conformity with the antecedents of physical science, and then to the possibility of arguing from them mathematically. I have not met with any which in the latter respect are preferable to those I have selected, which, consequently, I have good reason to adhere to.

To assume that an atom is of constant form and magnitude, is, I admit, virtually to call it an indivisible particle, and not, as Newton does, an undivided particle, "particula indivisa." But as the hypothesis is expressed in perfectly intelligible terms, it is open to no objection, provided we admit with Newton, that if *by a single experiment* it can be shown that the supposed indivisible particle is divided when a solid mass is broken, the theory of atoms is untenable. When a physical hypothesis satisfies the condition of being expressed in terms which common experience renders intelligible, special observation and experiment, or comparisons of its mathematical consequences with facts, alone determine whether or not it be true. I do not admit that any *metaphysical* argument can be adduced either in support of, or against, a physical hypothesis. *Meta*-physics come after physics. If a general physical theory should be established on verified hypotheses, we should have a secure basis for metaphysical reasoning; and possibly it might then appear that some of the speculative metaphysics which have prevailed during the last century are without foundation.

By the same mode of reasoning, the hypothesis of a universal fluid æther, the pressure of which varies proportionally to its density, is unobjectionable *as a hypothesis*, simply because it is expressed in terms which experience has made intelligible. Whether it be a true hypothesis, that is, whether such an æther be a *reality*, is another question. It does not admit of *à priori* proof or disproof, but may be disproved by a single contradictory fact, or may receive accumulative evidence by the agreement of its mathematical results with many facts. Now I venture to assert respecting this particular hypothesis, that the mathematical evidence of its truth and of the reality of a fluid æther is so varied and comprehensive, that it may be pronounced to be all but conclusive. My reasons for this assertion are the following:—When a mathematical inquiry is made into the laws of the motion and pressure of a fluid constituted as above supposed, certain results are obtained by the formation and solution of partial differential equations which correspond to various phenomena of light. The difference of the intensities of different rays, the variation of intensity with the distance from a centre, and the law of the variation, the coexistence at the same instant of different portions of light in the same portions of space, the interference and non-interference of different rays, the composite character of light, its colour, results of compounding colours, and lastly the polarization of light, are all phenomena which have their exact analogues in the motions, as mathematically deduced, of a fluid medium whose pressure varies as its density. When the number, variety, and speciality of these analogies are considered, it seems difficult to resist the conclusion that properties of the fluid æther *explain* phenomena of light, and that the phenomena reciprocally give evidence of the reality of the æther. Some of the properties—for instance, that of transverse vibration, which accounts for polarization—have been deduced by mathematical reasoning for which I am responsible. I have, however, given to mathematicians the fullest opportunity of discussing these parts of the general argument; and when, as I hope to be able to do, I go through a revision of the propositions, further opportunity will be given. The proof of the reality of the ætherial medium, drawn from the explanations which the hypothesis of such a medium gives of phenomena of light, is an essential preliminary of my general theory of physical force, and I am well aware that on this ground the truth of the theory must be contested. If this point be carried, the rest, I think, must follow.

There is already evidence from *experiment* that the action of physical force may be explained hydrodynamically. In the Philosophical Magazine for May (p. 348), Professor Maxwell has referred to a paper by M. Helmholtz on Fluid Motion, in which the author points out that lines of fluid motion are arranged according to the same laws as the lines of magnetic force. This, which Prof. Maxwell chooses to call a "physical analogy," I of course take to be confirmatory of the hydrodynamical theory of magnetic force. In the same light I regard the experiments of Professor Wiedemann, mentioned in vol. xi. No. 42 of the 'Proceedings of the Royal Society,' the results of which point to the same conclusion.

Cambridge Observatory,
May 22, 1861.

LXXIV. *On Phenomena which may be traced to the Presence of a Medium pervading all Space.* By DANIEL VAUGHAN.

IF the permanent change which seems to have been detected in the revolution of Encke's comet be not sufficient to establish the doctrine of a space-pervading æther, it may afford reasonable motives for examining other indications of the impediments of such a fluid to celestial motion. The direct information which can be obtained on this subject is at present very limited and uncertain. The approximate investigations hitherto given by mathematicians of the cause of the perturbation of the planets, necessarily overlook many slight effects of their mutual attraction; and we are thus prevented from discovering the unperiodical changes which a small resistance to their movements might occasion. In addition to this, we are incommoded by the want of observations made during very long periods of time; for these are as necessary in tracing the course of remote physical events, as an extensive base-line is in determining the distances of the fixed stars. But by investigating the necessary consequence of a resisting medium, and testing the result by a comparison with observed facts, we may be enabled to base our conclusions respecting this important question on evidence no less satisfactory than that which has already served to establish many of the received doctrines of physical science.

As there has prevailed among some astronomers an impression, not unwholly unfounded, in regard to a modification which the sun's attractive power is supposed to experience from the emission of his light, it seems advisable to give special attention to cases in which the central body is not luminous; and certain phenomena, observed in the secondary systems and in the dark systems of space, afford evidence not vitiated by any effects which light might be expected to produce. In my communication in the Philosophical Magazine for last April, I showed that a satellite impeded by the resistance of a medium would, by an imperceptibly slow diminution of its orbit, be finally introduced into the region of instability, where its dismemberment must be inevitable, and where it must be transformed into a ring, similar

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* Communicated by the Author.

in all respects to those of Saturn. But it might be premature to suppose that the annular appendage of Saturn has originated in this manner, or that it is to be regarded as an index of mutability in the heavens, if the conclusion were not supported by investigations of a different character. Were the rings two integral solid masses, the inner one, even with the most favourable velocity of rotation, would require to be composed of materials having over two hundred times the tenacity of wrought iron to escape being ruptured, in consequence of the enormous strain arising from a preponderance of centrifugal force on one part, and of gravity on the other. Even if this danger were removed, solid rings could not be prevented from striking the planet, unless each were loaded with some inequality; and, according to the investigations of Professor Maxwell, the load must contain about four and a half times as much matter as the remainder of the ring. A slight excess or deficiency in the amount of this load would be fatal to stability; and the tendency of any fluid or loose solid matter to the locality where it occurs must add much to the serious perils and the infirmities of the annular structure.

Regarding the hypothesis of two solid rings as untenable, Professor Maxwell considers the case of their fluidity, and he arrives at the conclusion that the fluid composing them would break up into satellites, unless its density were less than $\frac{1}{42}$ of that of the primary. But, from the result deduced in my articles in the Philosophical Magazine for December 1860 and April 1861, it is evident that, in so great a proximity to the central body, any liquid matter would require a far greater density to exist in the form of independent satellites. In investigating the case of a ring of numerous solid satellites, or fragments, he finds a combination of very extraordinary conditions necessary to prevent the derangements and permanent changes which collisions and friction are expected to occasion. The bodies are to be all equal in mass, and placed in regular array around Saturn; but the intervals between them must be very great compared with the linear dimensions; and the ratio between the planet and the ring must, according to his formulæ, be greater than ·4352 multiplied by the square of the number of satellites composing the latter. When we consider the vast number of such bodies required to maintain the continuity of the ring, and the great improbability that all the immense group should have the peculiar conditions for preventing one from striking another, we may regard the essay of the eminent mathematician as a proof that the disconnected matter composing the annular appendage, whether it be fluid or solid, cannot be maintained in its present condition without the occurrence of friction and collisions between its parts.

Besides the valid objections which Professor Maxwell urges against the common idea which regards the rings as two flat solids, others of a somewhat different character have been suggested by Mr. Bond, who has embraced the opinion that the ring is fluid. But whatever be its composition, or whatever proportions of fluid and solid matter it may consist of, all its parts must have independent movements around Saturn, and velocities depending on their distance from his centre. The attraction of the planet will be an insurmountable obstacle to their conversion into satellites, and will even prevent them from concentrating in excessive numbers in any locality; but their incessant action must be attended with a constant development of heat and a gradual destruction of motion. In consequence of the necessary alteration in the orbit of its parts from this cause, the dimensions of the ring cannot always remain the same; and though it is not likely that the nearest edge is approaching the planet so rapidly as the researches of Struve and Hansen would indicate, yet, as some change of this nature is unavoidable, we cannot resist the conclusion that the rings have been introduced into the zone which they now occupy, from one in which their matter could only exist in the form of two satellites. Accordingly there appears to be no ground for any other inference than that I have adopted, in regard to the imperceptible diminution of the orbits of secondary planets by the action of a resisting medium.

In tracing the ultimate effects of a similar impediment to motion in the dark systems of remote space, we deduce so satisfactory an explanation of the temporary stars, that we may regard these celestial apparitions as indicating the existence of the same æthereal fluid, and manifesting the great revolutions to which it leads in the condition of the heavenly bodies. In my last article, I have shown that the instantaneous manner in which a secondary or a primary planet must undergo a total dismemberment on coming into fatal proximity with the central sphere harmonizes in a very decided manner with the astonishing rapidity with which temporary stars attain their greatest brilliancy. This peculiarity, taken in connexion with the comparatively slow and gradual decline, is sufficient to set aside the theory which ascribes such ephemeral exhibitions of light to the rotation of great orbs, self-luminous on one side and dark on the other. But this theory, though adopted by Arago and other eminent astronomers, is liable to a more fatal objection. This will be apparent when we investigate the circumstances necessary to make a partially luminous sphere or spheroid display its brilliancy to the inhabitants of the earth for only seventeen months, while its period of rotation has been estimated at

309 or 318 years. Under the most favourable circumstances for manifesting such an extraordinary inequality between its periods of light and darkness, the surface of the supposed distant sphere must be nearly 200,000,000 times as great as the part of it sending light to our planet during the period of maximum brightness. The light, moreover, must have proceeded from the verge of the invisible disk; and this circumstance, taken in connexion with the surprising brilliance of the star of 1572, together with the invariability of its position, will compel us to ascribe to the spectral orb in question a diameter far exceeding that of Neptune's orbit. We must also regard these vast bodies as solid; for, if composed of liquid or gaseous matter, they could not have the luminosity confined to particular localities. Even if stellar movements could permit us to suppose the existence of such stupendous spheres, the explanation would be applicable to one or two cases only; and we must therefore reject a hypothesis whose claims rests solely on the greater imperfections of others proposed to account for the same phenomena.

But investigations respecting the necessary course of physical events in the dark systems afford still more important evidence in regard to the æthereal contents of space. Were the central body composed of solid matter, or surrounded with an atmosphere of oxygen, nitrogen, or carbonic acid, a development of heat and light might be expected to attend the dilapidation of one of the satellites, or the ultimate incorporation of its matter with the great orb; but the appearance would not correspond to that exhibited by the temporary stars. Admitting that a solid globe, almost as large as the sun, may be rendered so highly incandescent as to shine like the star of 1572 in its greatest brilliancy, it would be impossible for it to cool so rapidly as to become invisible in the course of seventeen months. Besides this, it may be easily shown that, if our earth had a diameter of 80,000 miles, with its present density and superficial temperature, our atmosphere would have its density reduced a millionfold with an elevation of six or seven miles. Thus, the greater mass we assign to the central body, the more narrow must we regard the atmospheric region where light can be developed by aërial compression; and the less display of lustre could we expect from this cause when a satellite fell from its stage of planetary existence. But this difficulty will disappear when we suppose that the æther of space forms for the several great celestial bodies extensive atmospheres, which are rendered luminous by adequate compression, or rather by the chemical action it induces—a theory which becomes necessary to account for the luminosity of meteors and the perpetual brilliancy of suns.

The theory which ascribes the sun's light to the incessant fall of meteors to his surface, and which I have controverted in my article in the Philosophical Magazine for December 1858, appears to have been suggested by the recently discovered relation between heat and mechanical energy. From this it may be estimated that a pound of solid matter, falling to the sun from a distance of 35,000,000 miles, is capable of generating at his surface an amount of heat about 4000 times as great as could be developed by the combustion of a pound of coal in oxygen gas. But the large amount of heat arising from the combustion of hydrogen, and other facts and principles connected with thermal agencies, give support to the opinion that the development of heat must be proportional to the intensity of the chemical forces by which it is produced; and these must be commensurate with the degree of elasticity between the elements concerned in the calorific or the illuminating action. We have therefore no grounds for supposing that the accession of temperature imparted to the solar orb by the fall of a body, even from an infinite distance, is necessarily greater than that originating from the chemical action of an equal amount of matter, the elements of which were so elastic as to diffuse themselves into space, in opposition to the attractive power of suns and planets. If the undulatory theory of light be admitted, the medium which conveys it with nearly a million times the rapidity of sound, must have a modulus of elasticity almost 1,000,000,000,000 times as great as that of common air. But though not regarding the æther which gives birth to solar light as so inconceivably elastic, we may safely presume that no matter is better adapted for sustaining the great fountain of brilliancy by energetic chemical action, than that whose particles are associated with forces sufficiently powerful to cause its diffusion through universal space.

That the fall of meteors is far more frequent and more conspicuous on the sun than on the earth cannot be questioned. If these small bodies are to be regarded as independent occupants of space, two large spheres, moving with the same velocity through the region in which they are located, would each be likely to receive a number of them proportional to its mass multiplied by its diameter. The circumstances in which the earth and sun are placed will change, to some extent, their relative capabilities of receiving these foreign bodies; but the facts which Mr. Carrington's observations have made known in regard to meteoric phenomena on the solar disk are not inconsistent with what might be reasonably expected, and do not indicate any special provision for feeding our central luminary with regular supplies of meteorites.

The definite information which Arago was enabled to furnish respecting the sun by means of his polariscope, has recently received an important accession from the labours of Bunsen and Kirchhoff. A comparison of the spectrum of the sun with that of various metallic vapours in a state of incandescence, enabled these chemists to show that potassium, sodium, calcium, and other elements widely diffused on our globe, enter into the composition of the solar atmosphere. Their observations proved, however, that these substances, while abundant in the sun's envelope, instead of being concerned in producing his light, only exerted a negative influence—absorbing certain rays, and causing dark lines to replace the bright ones peculiar to their luminous vapours. It is therefore evident that the light from the vapours of the elements alluded to must be overpowered by the rays emanating from some other source. Professor Kirchhoff has embraced the opinion that the solid globe of the sun must have a far higher temperature and a greater illuminating power than his atmosphere; but the observations with Arago's polariscope have afforded positive evidence that solar light does not emanate from an incandescent solid or liquid body. It appears more philosophical to conclude that the light of our great luminary originates, not from the vapours discoverable in its atmosphere, but from a more subtle æthereal medium combined with them, and possessed of far greater illuminating power.

The periodical changes recently discovered in the sun's spots seem to furnish a fatal objection to the idea that the self-luminous condition results from the high temperature of his solid nucleus, or from the heat developed by its compression. It can scarcely be doubted that the periodicity of the spots is dependent on the movements of the planets; and the position of Jupiter seem to exert the greatest influence on their occurrence; as recent observations show, the period of his revolution agrees very closely with the interval between the times at which the spots are most numerous. Although it may be premature to express a decided opinion on so obscure a subject, there seem to be legitimate motives to justify an examination of the more obvious ways in which it would be possible for a planet to affect the luminous condition of the solar disk. If, as Helmholtz contends, the ocean of heat and light be maintained by the compression of the sun, the planets can only exert their influence on his spots by diminishing the weight and pressure of his materials, in the same manner in which the moon acts to raise tides on our oceans. But the alteration in the weight of terrestrial matter from lunar attraction, though extremely small, is about 80,000 times as great as that which the component parts of the sun experience from the attractive force of Jupiter. This planet holds the highest place in its capability of affecting the pressure of the solar matter: it is almost equalled by Venus, but it is far superior to the other members of our system. If, however, the planets moved in circles, the peculiar action alluded to could only increase the tendency of the spots to appear on certain sides of the sun, without materially increasing the numbers visible during the year. When we take into consideration the changes occasioned by the eccentricity of their orbits, the greatest effect must be ascribed to Mercury; Jupiter holds the next place; after which we must rank Saturn and the earth. But even admitting the compressibility of the sun's materials, his mean diameter could not be altered more than the $\frac{1}{80}$th of an inch by the attraction of any of his planetary attendants; and the variation of temperature from this peculiar action cannot exceed $\frac{1}{30,000}$th part of a degree (F.). That so small a variation could be manifested in the appearance of the sun's disk, seems wholly improbable, especially if we adopt the estimate of Mr. Waterston, which assigns to the great orb a mean temperature of one thousand million degrees.

Any effect which the planets may be supposed to occasion by their electric or magnetic forces must be also rendered extremely feeble in consequence of their great distance. If the great ocean of solar light is sensitive to the electricity or magnetism of Jupiter, the nearest satellite of this planet must feel the power of his mysterious influence to an extent several million times as great; yet no indications of such a fact have been observed. But supposing the sun's motion through space to be concerned in maintaining his effulgence, the planets would derive a far more considerable influence from the general movement around the centre of gravity of our system. The position of Jupiter would change the progressive motion of the great luminary about twenty-four miles an hour; and the other planets will be attended with results proportional to their masses multiplied by the square roots of their distances. Now the amount of æther which the sun collects from space, and the density it attains on his surface, will depend on the rapidity of his translatory motion; but I have shown in the Philosophical Magazine for May 1858 another way in which the position of the planets would increase or diminish the supply of æthereal fuel which sustains the great solar conflagration.

The idea that the space-pervading medium is condensed by the attraction of the celestial orbs, is not to be considered a new hypothesis, but rather a necessary inference from that of Professor Encke. Were the density of the subtle fluid uniform, small and large planets would be so unequally affected by its resistance, that their orbits could not retain the relation necessary for their stability, and they must be destroyed by collisions long before the natural term of their existence. This difficulty will, however, disappear when the effects of planetary attraction in condensing the medium are taken into consideration. How far observation of primary or secondary worlds give evidence of unperiodical changes in our system has not been yet determined with positive certainty. The constant acceleration of the moon's orbital velocity, during the past 2000 years, has been traced by Laplace to a periodical change in the eccentricity of the earth's orbits. But an error in his investigations being lately pointed out by Mr. Adams, there appears to be some definite ground for regarding the lunar orbit as subject to a very slow permanent diminution, which, after some allowance for the effects of tidal action, we may consider as depending, to some extent, on the resistance of a medium. The great oblateness which Arago and Sir William Herschel assign to Mars would indicate that the time of its rotation has been considerably lengthened, since the remote period at which it was moulded into its present form; and this may be looked upon as circumstantial evidence of the effects of an æthereal resistance in changing the diurnal motion of planets. It is only to smaller worlds that we could look for such results, for in larger orbs the strength of their solid matter can have little influence in preventing alterations of form to correspond with the relations of gravity and centrifugal force.

As doubts are entertained by some eminent astronomers as to the sensible ellipticity of Mars, it may be well to refer to certain appearances which show a slight deviation, at least, in his form from a figure of equilibrium. The marked indications of atmospheric phenomena around his poles, while they are either wholly absent or only faintly exhibited in the vicinity of his equator, is so much opposed to everything we might expect from the condition of our own globe and the belted appearance of Jupiter, that we cannot avoid concluding that the aërial ocean of Mars is much deeper in his polar than in his equatorial regions. Perhaps this may account for the very discordant results of observations in determining the extent of the atmosphere of this planet by occultations of the fixed stars.

It is to the revolutions of comets that astronomical curiosity has chiefly turned for evidence of the contents of interplanetary space; but the advantages of low density in these bodies have been counterbalanced by the great elongation of their orbits, which exposes them to very great disturbances from the planets. During the past eighteen centuries Halley's comet has occupied in its revolution a period varying from 74·88 to 79·34 years, according to the Table in Mr. Hind's work on Comets (page 57). Of the twenty-four consecutive revolutions here recorded, the first eight average 77·59 years, the next eight 76·84, and the last

76·54 years. This would seem to favour the idea of a permanent diminution of the orbit; and I understand that De Vico regards the movements of his own comet as indicative of a similar result from the widely diffused æther. But the information on this subject hitherto deemed worthy of the most confidence, has been derived from the successive returns of Encke's comet. The advantages which this body affords for such inquiries depend chiefly on the moderate eccentricity of its orbit and the position of the transverse axis, which is nearly perpendicular to the line of the sun's progressive motion. This arrangement must give a more decided preponderance to the perihelion resistance, which has the greatest influence in diminishing the size of the orbit.

As shooting stars are now regarded as small bodies describing very elongated ellipses around the sun, they seem calculated to furnish perhaps the most satisfactory means of testing the perfection of the celestial vacuum. Supposing these bodies to be more sensitive to the resistance of the medium than to planetary disturbances, the transverse axes of their orbits will have a tendency to assume a uniform direction in consequence of the sun's progressive motion. From the same cause the planes in which they move will have their intersections confined for the most part to a very limited range, and will also exhibit, though in a less degree, a tendency to coincidence. This peculiar arrangement of their orbits must cause vast swarms of these minute cosmical bodies to congregate from the most distant parts of the solar domain to a comparatively narrow region at their perihelion passage. For the appearance of the zodiacal light and the periodical fall of meteors, I have endeavoured to account in this manner in a paper sent to the meeting of the British Association and published in the Sections (1854, p. 26). My late researches on the subject exhibit a closer accordance with observed facts than I could then obtain, and they give much support to the ideas very prevalent in scientific circles, with regard to the agency of meteors in reflecting the zodiacal light.

Cincinnati, May 11th, 1861.

LXXVI. *On a Problem in Tactic which serves to disclose the existence of a Four-valued Function of three sets of three letters each. By* J. J. Sylvester, *M.A., F.R.S., Professor of Mathematics at the Royal Military Academy, Woolwich*[*].

At page 375 of the May Number of the Magazine (in that paragraph commencing at the middle of the page) I gave a Table of Synthemes, correct as far as it went, but left in a very

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[*] Communicated by the Author.

imperfect state. It was intended to be supplemented with a material addition which escaped my recollection when, after a long delay, the proofs of the paper passed through my hands. The question to which this Table refers is the following:—

Three *nomes,* each containing three elements, are given; the number of *trinomial* triads (i. e. ternary combinations, composed by taking one element out of each nome) will be 27, and these 27 may be *grouped* together into 9 synthemes (each syntheme consisting of 3 of the triads in question, which together include between them all the 9 elements). It is desirable to know:— 1st. How many distinct *groupings* of this kind can be formed. 2nd. Whether there is more than one, and, if so, how many distinct types of groupings. The criterion of one grouping being cotypal or allotypal to another is its capability or incapability of being transformed into that other by means of an interchange of elements. Be it once for all stated that the question in hand is throughout one of combinations, and not of permutations; the order of the elements in a triad, of a triad in a syntheme, of a syntheme in a grouping is treated as immaterial. As we are only concerned with the elements as distributed into *nomes,* the number of interchanges of elements with which we are concerned is 6×6^3 or 1296; the factor 6^3 arises from the permutability of the elements of each nome *inter se,* the remaining factor 6 from the permutability of any nome with any other. I find, by a method which carries its own demonstration with it on its face, that the number of distinct *groupings* is 40, of which 4 belong to one *type* or *family,* and 36 to a second type or family.

Let the nomes be 1 . 2 . 3, 4 . 5 . 6, 7 . 8 . 9, and let

c_1 denote 1 . 4, 2 . 5, 3 . 6 $\quad$ $\dot{c}_1$ denote 1 . 4, 2 . 6, 3 . 5

c_2 „ 1 . 5, 2 . 6, 3 . 4 $\quad$ $\dot{c}_2$ „ 1 . 5, 2 . 4, 3 . 6

c_3 „ 1 . 6, 2 . 4, 3 . 5 $\quad$ $\dot{c}_3$ „ 1 . 6, 2 . 5, 3 . 4

$$\gamma \text{ denote } \begin{matrix} 7, & 8, & 9 \\ 8, & 9, & 7 \\ 9, & 7, & 8 \end{matrix} \qquad \gamma' \text{ denote } \begin{matrix} 7, & 9, & 8 \\ 9, & 8, & 7 \\ 8, & 7, & 9 \end{matrix}$$

b_1 denote 1 . 7, 2 . 8, 3 . 9 $\quad$ $\dot{b}_1$ denote 1 . 7, 2 . 9, 3 . 8

b_2 „ 1 . 8, 2 . 9, 3 . 7 $\quad$ $\dot{b}_2$ „ 1 . 8, 2 . 7, 3 . 9

b_3 „ 1 . 9, 2 . 7, 3 . 8 $\quad$ $\dot{b}_3$ „ 1 . 9, 2 . 8, 3 . 7

$$\beta \text{ denote } \begin{matrix} 4, & 5, & 6 \\ 5, & 6, & 4 \\ 6, & 4, & 5 \end{matrix} \qquad \beta' \text{ denote } \begin{matrix} 4, & 6, & 5 \\ 6, & 5, & 4 \\ 5, & 4, & 6 \end{matrix}$$

a_1	denote	4.7, 5.8, 6.9	$\dot{a}_1$	denote	4.7, 5.9, 6.8
a_2	"	4.8, 5.9, 6.7	$\dot{a}_2$	"	4.8, 5.7, 6.9
a_3	"	4.9, 5.7, 6.8	$\dot{a}_3$	"	4.9, 5.8, 6.7

$$\alpha \text{ denote } \begin{array}{l} 1,\ 2,\ 3 \\ 2,\ 3,\ 1 \\ 3,\ 1,\ 2 \end{array} \qquad \alpha' \text{ denote } \begin{array}{l} 1,\ 3,\ 2 \\ 2,\ 3,\ 1 \\ 3,\ 1,\ 2 \end{array}$$

I take first the larger family of 36 groupings; these may be represented as follows:

$a_1\alpha$	$a_1\alpha$	$a_1\alpha'$	$a_1\alpha$	$a_1\alpha'$	$a_1\alpha'$	$\dot{a}_1\alpha$	$\dot{a}_1\alpha$	$\dot{a}_1\alpha'$	$\dot{a}_1\alpha$	$\dot{a}_1\alpha'$	$\dot{a}_1\alpha'$
$a_2\alpha$	$a_2\alpha'$	$a_2\alpha$	$a_2\alpha'$	$a_2\alpha$	$a_2\alpha'$	$\dot{a}_2\alpha$	$\dot{a}_2\alpha'$	$\dot{a}_2\alpha$	$\dot{a}_2\alpha'$	$\dot{a}_2\alpha$	$\dot{a}_2\alpha'$
$a_3\alpha'$	$a_3\alpha$	$a_3\alpha$	$a_3\alpha'$	$a_3\alpha'$	$a_3\alpha$	$\dot{a}_3\alpha'$	$\dot{a}_3\alpha$	$\dot{a}_3\alpha$	$\dot{a}_3\alpha'$	$\dot{a}_3\alpha'$	$\dot{a}_3\alpha$
$b_1\beta$	$b_1\beta$	$b_1\beta'$	$b_1\beta$	$b_1\beta'$	$b_1\beta'$	$\dot{b}_1\beta$	$\dot{b}_1\beta$	$\dot{b}_1\beta'$	$\dot{b}_1\beta$	$\dot{b}_1\beta'$	$\dot{b}_1\beta'$
$b_2\beta$	$b_2\beta'$	$b_2\beta$	$b_2\beta'$	$b_2\beta$	$b_2\beta'$	$\dot{b}_2\beta$	$\dot{b}_2\beta'$	$\dot{b}_2\beta$	$\dot{b}_2\beta'$	$\dot{b}_2\beta$	$\dot{b}_2\beta'$
$b_3\beta'$	$b_3\beta$	$b_3\beta$	$b_3\beta'$	$b_3\beta'$	$b_3\beta$	$\dot{b}_3\beta'$	$\dot{b}_3\beta$	$\dot{b}_3\beta$	$\dot{b}_3\beta'$	$\dot{b}_3\beta'$	$\dot{b}_3\beta$
$c_1\gamma$	$c_1\gamma$	$c_1\gamma'$	$c_1\gamma$	$c_1\gamma'$	$c_1\gamma'$	$\dot{c}_1\gamma$	$\dot{c}_1\gamma$	$\dot{c}_1\gamma'$	$\dot{c}_1\gamma$	$\dot{c}_1\gamma'$	$\dot{c}_1\gamma'$
$c_2\gamma$	$c_2\gamma'$	$c_2\gamma$	$c_2\gamma'$	$c_2\gamma$	$c_2\gamma'$	$\dot{c}_2\gamma$	$\dot{c}_2\gamma'$	$\dot{e}_2\gamma$	$\dot{c}_2\gamma'$	$\dot{c}_2\gamma$	$\dot{c}_2\gamma'$
$c_3\gamma'$	$c_3\gamma$	$c_3\gamma$	$c_3\gamma'$	$c_3\gamma'$	$c_3\gamma$	$\dot{c}_3\gamma'$	$\dot{c}_3\gamma$	$\dot{c}_3\gamma$	$\dot{c}_3\gamma'$	$\dot{c}_3\gamma'$	$\dot{c}_3\gamma$

An example of the development of any one of the above symbolisms into its correspondent grouping will serve to render perfectly intelligible the whole Table.

Let it be required to develope

$$\begin{array}{l} \dot{b}_1\,\beta \\ \dot{b}_2\,\beta' \\ b_3\,\beta'. \end{array}$$

Since

$$\begin{array}{llll} \dot{b}_1 = 1.7 & 2.9 & 3.8 & \\ \dot{b}_2 = 1.8 & 2.7 & 3.9 & \\ \dot{b}_3 = 1.9 & 2.8 & 3.7 & \end{array} \qquad \beta = \begin{array}{l} 4,\ 5,\ 6 \\ 5,\ 6,\ 4 \\ 6,\ 4,\ 5 \end{array} \qquad \beta' = \begin{array}{l} 4,\ 6,\ 5 \\ 6,\ 5,\ 4 \\ 5,\ 4,\ 6, \end{array}$$

the development required is the following:—

1.7.4	2.9.5	3.8.6
1.7.5	2.9.6	3.8.4
1.7.6	2.9.4	3.8.5
1.8.4	2.7.6	3.9.5
1.8.6	2.7.5	3.9.4
1.8.5	2.7.4	3.9.6
1.9.4	2.8.6	3.7.5
1.9.6	2.8.5	3.7.4
1.9.5	2.8.4	3.7.6

The whole of this family of 36 may be represented under the following condensed form, according to the notation usual in the theory of substitutions.

$$\left(\begin{vmatrix} a_1\alpha \\ a_2\alpha \\ a_3\alpha \end{vmatrix} \times \begin{pmatrix} 123 & 123 & 123 \\ 123 & 231 & 312 \end{pmatrix} \times \begin{pmatrix} a & \dot{a} \\ a & a \end{pmatrix} \times \begin{pmatrix} \alpha\alpha' & \alpha\alpha' \\ \alpha\alpha' & \alpha'\alpha \end{pmatrix} \times \begin{pmatrix} a\alpha & b\beta & c\gamma \\ a\alpha & a\alpha & a\alpha \end{pmatrix}\right)$$

It remains to describe the principal and most symmetrical family. This contains only 4 groupings, and may be represented indifferently under any of the three following forms:

$$\begin{array}{cccc} a_1\alpha & a_1\alpha' & \dot{a}_1\alpha & \dot{a}_1\alpha' \\ a_2\alpha & a_2\alpha' & \dot{a}_2\alpha & \dot{a}_2\alpha' \\ a_3\alpha & a_3\alpha' & \dot{a}_3\alpha & \dot{a}_3\alpha \end{array} \text{ or } \begin{array}{cccc} b_1\beta & b_1\beta' & \dot{b}_1\beta & \dot{b}_1\beta' \\ b_2\beta & b_2\beta' & \dot{b}_2\beta & \dot{b}_2\beta' \\ b_3\beta & b_3\beta' & \dot{b}_3\beta & \dot{b}_3\beta' \end{array} \text{ or } \begin{array}{cccc} c_1\gamma & c_1\gamma' & \dot{c}_1\gamma & \dot{c}_1\gamma' \\ c_2\gamma & c_2\gamma' & \dot{c}_2\gamma & \dot{c}_2\gamma' \\ c_3\gamma & c_3\gamma' & \dot{c}_3\gamma & \dot{c}_3\gamma' \end{array}$$

In developing, it will be found that each of these three representations gives rise to the *same* family of groupings, which from its importance it is proper to set out in full as follows:—

1.4.7	**2.5.8**	**3.6.9**	**1.4.7**	**2.5.9**	**3.6.8**	**1.4.7**	**2.6.8**	**3.5.9**	**1.4.7**	**2.6.9**	**3.5.**
1.4.8	**2.5.9**	**3.6.7**	**1.4.9**	**2.5.8**	**3.6.7**	**1.4.8**	**2.6.9**	**3.5.7**	**1.4.9**	**2.6.8**	**3.5.7**
1.4.9	**2.5.7**	**3.6.8**	**1.4.8**	**2.5.7**	**3.6.9**	**1.4.9**	**2.6.7**	**3.5.8**	**1.4.8**	**2.6.7**	**3.5.9**
1.5.7	**2.6.8**	**3.4.9**	**1.5.7**	**2.6.9**	**3.4.8**	**1.5.7**	**2.4.8**	**3.6.9**	**1.5.7**	**2.4.9**	**3.6.8**
1.5.8	**2.6.9**	**3.4.7**	**1.5.9**	**2.6.8**	**3.4.7**	**1.5.8**	**2.4.9**	**3.6.7**	**1.5.9**	**2.4.8**	**3.6.7**
1.5.9	**2.6.7**	**3.4.8**	**1.5.8**	**2.6.7**	**3.4.9**	**1.5.9**	**2.4.7**	**3.6.8**	**1.5.8**	**2.4.7**	**3.6.7**
1.6.7	**2.4.8**	**3.5.9**	**1.6.7**	**2.4.9**	**3.5.8**	**1.6.7**	**2.5.8**	**3.4.9**	**1.6.7**	**2.5.9**	**3.4.8**
1.6.8	**2.4.9**	**3.5.7**	**1.6.9**	**2.4.8**	**3.5.7**	**1.6.8**	**2.5.9**	**3.4.7**	**1.6.9**	**2.5.8**	**3.4.7**
1.6.9	**2.4.7**	**3.5.8**	**1.6.8**	**2.4.7**	**3.5.9**	**1.6.9**	**2.5.7**	**3.4.8**	**1.6.8**	**2.5.7**	**3.4.9**

It follows at once from the above Table, that if 3 cubic equations be given, we may form a function of the 9 roots, which, when any of the roots of any of the equations are interchanged *inter se*, or all the roots of one with all those of any other, will receive only *four distinct values*.

It also follows that we may form with 9 letters an intransitive group (of Cauchy) containing $\frac{216}{4}$, *i. e.* 54, or a transitive group containing $\frac{1296}{4}$, or 324 substitutions. So the family of 36 groupings lead to the formation of an intransitive substitution group of $\frac{216}{12}$, *i. e.* 18, and of a transitive group of $\frac{1296}{36}$, or 16 substitutions.

Since 9 letters may be thrown, in $\frac{8\,.\,7}{2} \times \frac{5\,.\,4}{2}$, *i. e.* 280 different ways, into nomes of 3 letters each, it further follows that by repeating each of the above two families 280 times we shall obtain new families remaining unaltered by any substitution of any of the nine elements *inter se*, and consequently indicating the existence of substitution-groups containing

$$\frac{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6\,.\,7\,.\,8\,.\,9}{280 \times 36} \text{ and } \frac{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6\,.\,7\,.\,8\,.\,9}{280 \times 4},$$

i. e. 36 and 324 substitutions respectively.

In the above solution a little consideration will show that the method is essentially based on the solution of a *previous* question, viz. of grouping together the synthemes of *binomial duads* of *two* nomes of three letters each, which can be done in two distinct modes, which (if, *ex. gr.*, we take 1 . 2 . 3, 4 . 5 . 6 as the two nomes in question) are represented in the notation used above by $\begin{matrix} c_1 \\ c_2 \\ c_3 \end{matrix}$ and $\begin{matrix} \dot{c}_1 \\ \dot{c}_2 \\ \dot{c}_3 \end{matrix}$ respectively. So, more generally, the groupings of the q-nomial q-ads of r nomes of s elements may be made to depend on the groupings of the $(q-1)$-nomial $(q-1)$-ads of $(r-1)$ nomes of s elements each. The more general question is to discover the groupings and their families of the synthemes composed of p-nomial q-ads of r nomes of s elements, of which the simplest example next that which has been considered and solved is to discover the groupings of the synthemes composed of 54 *binomial* triads of 3 nomes of 3 elements each*.

The chief difficulty of calculating *à priori* the number of such groupings is of a similar nature to that which lies at the bottom of the ordinary theory of the partition of numbers, namely, the liability of the same groupings to make their appearance under distinct symbolical representations. Of this we have seen an example in the threefold representation of the principal family of 4 groupings just treated of. But for the existence of this

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* I have ascertained, by a direct analytical method, since the above was written, that the number of different groupings of the synthemes composed of these binomial triads is 144. The number of distinct types or families is *three*, one containing 12, another 24, and the third 108 groupings.

multiform representation of the same grouping we could have affirmed *à priori* the number of groupings to be $2 \times 3 \times 2^3$ or 48, whereas the true number is only 40. I believe that the above is the first instance of the doctrine of types making its appearance explicitly, and illustrated by example in the theory of tactic. It were much to be desired that some one would endeavour to collect and collate the various solutions that have been given of the noted 15-school-girl problem by Messrs. Kirkman (in the Ladies' Diary), Moses Ansted (in the Cambridge and Dublin Mathematical Journal), by Messrs. Cayley and Spottiswoode (in the Philosophical Magazine and elsewhere), and Professor Pierce, the latest and probably the best (in the American Astronomical Journal), besides various others originating and still floating about in the fashionable world (one, if not two, of which I remember having been communicated to me many years ago by Mr. Archibald Smith, F.R.S.), with a view to ascertain whether they belong to the same or to distinct types of aggregation.

LXXVII. *Notices respecting New Books.*

A History of the Progress of the Calculus of Variations during the Nineteenth Century. By I. TODHUNTER, *M.A., Fellow and Principal Mathematical Lecturer of St. John's College, Cambridge.* Cambridge: Macmillan and Co. 1861.

MR. TODHUNTER, whose name is already so familiar to the mathematical student, has at length produced a work of much greater originality and research than any of his former and more elementary treatises.

The "Calculus of Variations," one of the most difficult branches of pure mathematics, has been the subject of the labours of several eminent mathematicians, Euler, Lagrange, Gauss, Poisson, &c., whose successive researches and improvements form an exceedingly interesting department of scientific history, which, however, has hitherto been specially treated by only one writer in our own language, viz. Woodhouse, whose 'Treatise on Isoperimetrical Problems and the Calculus of Variations' was published in 1810, and is now an extremely scarce book.

Woodhouse's work has always received very high praise by such competent judges as Messrs. Peacock, Herschel, and Babbage, in their 'Examples;' Professor De Morgan, in his 'Differential and Integral Calculus;' and Professor Jellett, in his 'Calculus of Variations.' But since its publication the calculus has been greatly advanced and improved; and it is to record this progress that Mr. Todhunter has written the volume before us, which commences where Woodhouse left off. It is evidently the work of one who thoroughly understands the science itself, and who has most conscientiously and laboriously consulted and *studied* all the available materials and sources of information. He unites the qualifications of a sound mathematician and a good linguist—a rare combination. The Memoirs and Treatises in the German and Italian languages, as well as those in the French and Latin, have been completely mastered and analysed: and some account is given even of a dissertation in the Russian language. Of those works which are difficult of access to the English student, a more copious account is given; and throughout the whole history, "numerous remarks, criticisms, and corrections are suggested relative to the various treatises and memoirs which are analysed. The writer trusts that it will not be supposed that he undervalues the labours of the eminent mathematicians in whose works he ventures occasionally to indicate inaccuracies or imperfections, but that his aim has been to remove difficulties which might perplex a student" (Preface). We would specially point out the last chapter in the book (pp. 505–530) as deserving attention in this respect.

Fully agreeing with Mr. Todhunter as to the "value of a history of any department of science, when that history is presented with accuracy and completeness," we congratulate him on having produced a History which so well merits this character of "accuracy and completeness;" and we sincerely hope that the success of his present contribution to scientific history may induce him to carry out the intention expressed in the conclusion of his Preface, viz. "to undertake a similar survey of some other department of science."

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 469.]

June 21, 1860.—Sir Benjamin C. Brodie, Bart., Pres., in the Chair.

THE following communications were read:—

"On the Sources of the Nitrogen of Vegetation; with special reference to the Question whether Plants assimilate free or uncombined Nitrogen." By J. B. Lawes, Esq., F.R.S.; J. H. Gilbert, Ph.D., F.R.S.; and Evan Pugh, Ph.D., F.C.S.

After referring to the earlier history of the subject, and especially to the conclusion of De Saussure, that plants derive their nitrogen from the nitrogenous compounds of the soil and the small amount of ammonia which he found to exist in the atmosphere, the Authors preface the discussion of their own experiments on the sources of the nitrogen of plants, by a consideration of the most prominent facts established by their own investigations concerning the amount of nitrogen yielded by different crops over a given area of land, and of the relation of these to certain measured, or known sources of it.

On growing the same crop year after year on the same land, without any supply of nitrogen by manure, it was found that wheat, over a period of 14 years, had given rather more than 30 lbs.—barley, over a period of 6 years, somewhat less—meadow-hay, over a period of 3 years, nearly 40 lbs.—and beans, over 11 years, rather more than 50 lbs. of nitrogen, per acre, per annum. Clover, another Leguminous crop, grown in 3 out of 4 consecutive years, had given an average of 120 lbs. Turnips, over 8 consecutive years, had yielded about 45 lbs.

The Graminaceous crops had not, during the periods referred to, shown signs of diminution of produce. The yield of the Leguminous crops had fallen considerably. Turnips, again, appeared greatly to have exhausted the immediately available nitrogen in the soil. The amount of nitrogen harvested in the Leguminous and Root-crops was considerably increased by the use of "mineral manures," whilst that in the Graminaceous crops was so in a very limited degree.

Direct experiments further showed that pretty nearly the same amount of nitrogen was taken from a given area of land in *wheat* in 8 years, whether 8 crops were grown consecutively, 4 in alternation with fallow, or 4 in alternation with beans.

Taking the results of 6 separate courses of rotation, Boussingault obtained an average of between one-third and one-half more nitrogen in the produce than had been supplied in manure. His largest yields of nitrogen were in the Leguminous crops; and the cereal crops were larger when they next succeeded the removal of the highly nitrogenous Leguminous crops. In their own experiments upon an actual course of rotation, without manure, the Authors had obtained, over 8 years, an average annual yield of 57·7 lbs. of nitrogen per acre; about twice as much as was obtained in either wheat or barley, when these crops were, respectively, grown year after year on the same land. The greatest yield of nitrogen had been in a clover crop, grown once during the 8 years; and the wheat crops grown after this clover in the first course of 4 years, and after beans in the second course, were about double those obtained when wheat succeeded wheat.

Thus, Cereal crops, grown year after year on the same land, had given an average of about 30 lbs. of nitrogen, per acre, per annum; and Leguminous crops much more. Nevertheless the Cereal crop was nearly doubled when preceded by a Leguminous one. It was also about doubled when preceded by fallow. Lastly, an entirely unmanured rotation had yielded nearly twice as much nitrogen as the continuously grown Cereals.

Leguminous crops were, however, little benefited, indeed frequently injured, by the use of the ordinary direct nitrogenous manures. Cereal crops, on the other hand, though their yield of nitrogen was comparatively small, were very much increased by direct nitrogenous manures, as well as when they succeeded a highly nitrogenous Leguminous crop, or fallow. But when nitrogenous manures had been employed for the increased growth of the Cereals, the nitrogen in the immediate increase of produce had amounted to little more than 40 per cent. of that supplied, and that in the increase of the second year after the application, to little more than one-tenth of the remainder. Estimated in the same way, there had been in the case of the meadow grasses scarcely any larger proportion of the supplied nitrogen recovered. In the Leguminous crops the proportion so recovered appeared to be even less; whilst in the root-crops it was probably somewhat greater. Several possible explanations of this real or apparent loss of the nitrogen supplied by manure are enumerated.

The question arises—what are the sources of all the nitrogen of our crops beyond that which is directly supplied to the soil by artificial means? The following actual or possible sources may be enumerated:—the nitrogen in certain constituent minerals of the soil; the combined nitrogen annually coming down in the direct aqueous depositions from the atmosphere; the accumulation of combined nitrogen from the atmosphere by the soil in other ways; the formation of ammonia in the soil from free nitrogen and nascent hydrogen; the formation of nitric acid from free nitrogen; the direct absorption of combined nitrogen from the atmosphere by plants themselves; the assimilation of free nitrogen by plants.

A consideration of these several sources of the nitrogen of the vegetation which covers the earth's surface showed that those of them which have as yet been quantitatively estimated are inadequate to account for the amount of nitrogen obtained in the annual produce of a given area of land beyond that which may be attributed to supplies by previous manuring. Those, on the other hand, which have not yet been even approximately estimated as to quantity —if indeed fully established qualitatively—offer many practical difficulties in the way of such an investigation as would afford results applicable in any such estimates as are here supposed. It appeared important, therefore, to endeavour to settle the question whether or not that vast storehouse of nitrogen, the atmosphere, affords to growing plants any measurable amount of its *free* nitrogen. Moreover, this question had of late years been submitted to very extended and laborious experimental researches by M. Boussingault, and M. Ville, and also to more limited investigation by MM. Mène, Roy, Cloez, De Luca, Harting, Petzholdt and others, from the results of which diametrically opposite conclusions had been arrived at. Before entering on the discussion of their own experimental evidence, the Authors give a review of these results and inferences; more especially those of M. Boussingault who questions, and those of M. Georges Ville who affirms the assimilation of *free* nitrogen in the process of vegetation.

The general method of experiment instituted by Boussingault, which has been followed, with more or less modification, in most subsequent researches, and by the Authors in the present inquiry, was—to set seeds or young plants, the amount of nitrogen in which was estimated by the analysis of carefully chosen similar specimens; to employ soils and water containing either no combined nitrogen, or only known quantities of it; to allow the access either of free air (the plants being protected from rain and dust)— of a current of air freed by washing from all *combined* nitrogen—or of a limited quantity of air, too small to be of any avail so far as any compounds of nitrogen contained in it were concerned; and finally, to determine the amount of combined nitrogen in the plants produced and in the soil, pot, &c., and so to provide the means of estimating the gain or loss of nitrogen during the course of the experiment.

The plan adopted by the Authors in discussing their own experimental results, was—

To consider the conditions to be fulfilled in order to effect the solution of the main question, and to endeavour to eliminate all sources of error in the investigation.

To examine a number of collateral questions bearing upon the points at issue, and to endeavour so far to solve them, as to reduce the general solution to that of a single question to be answered by the results of a final set of experiments.

To give the results of the final experiments, and to discuss their bearings upon the question which it is proposed to solve by them.

Accordingly, the following points are considered:—

1. The preparation of the soil, or matrix, for the reception of the plants and of the nutriment to be supplied to them.

2. The preparation of the nutriment, embracing that of mineral constituents, of certain solutions, and of water.

3. The conditions of atmosphere to be supplied to the plants, and the means of securing them; the apparatus to be employed, &c.

4. The changes undergone by nitrogenous organic matter during decomposition, affecting the quantity of combined nitrogen present, in circumstances more or less analogous to those in which the experimental plants are grown.

5. The action of agents, as ozone; and the influence of other circumstances which may affect the quantity of combined nitrogen present in connexion with the plants, independently of the direct action of the growing process.

In most of the experiments a rather clayey soil, ignited with free access of air, well-washed with distilled water, and re-ignited, was used as the matrix or soil. In a few cases washed and ignited pumice-stone was used.

The mineral constituents were supplied in the form of the ash of plants, of the description to be grown if practicable, and if not, of some closely allied kind.

The distilled water used for the final rinsing of all the important parts of the apparatus, and for the supply of water to the plants, was prepared by boiling off one-third from ordinary water, collecting the second third as distillate, and redistilling this, previously acidulated with phosphoric acid.

Most of the pots used were specially made, of porous ware, with a great many holes at the bottom and round the sides near to the bottom. These were placed in glazed stone-ware pans with inward-turned rims to lessen evaporation.

Before use, the red-hot matrix and the freshly ignited ash were mixed in the red-hot pot, and the whole allowed to cool over sulphuric acid. The soil was then moistened with distilled water, and after the lapse of a day or so the seeds or plants were put in.

Very carefully picked bulks of seed were chosen; specimens of the average weight were taken for the experiment, and in similar specimens the nitrogen was determined.

The atmosphere supplied to the plants was washed free from ammonia by passing through sulphuric acid, and then over pumice-stone saturated with sulphuric acid. It then passed through a solution of carbonate of soda before entering the apparatus enclosing the plant, and it passed out again through sulphuric acid.

Carbonic acid, evolved from marble by measured quantities of hydrochloric acid, was passed daily into the apparatus, after passing, with the air, through the sulphuric acid and the carbonate of soda solution.

The enclosing apparatus consisted of a large glass shade, resting in a groove filled with mercury, in a slate or glazed earthenware stand, upon which the pan, with the pot of soil, &c., was placed. Tubes passed under the shade, for the ingress and the egress of air, for the supply of water to the plants, and, in some cases, for the withdrawal of the water which condensed within the shade. In other cases, the condensed water was removed by means of a special arrangement.

One advantage of the apparatus adopted was, that the washed air was forced, instead of being aspirated, through the enclosing vessel. The pressure upon it was thus not only very small, and the danger from breakage, therefore, also small, but it was exerted upon the inside instead of the outside of the shade; hence, any leakage would be from the inside outwards, so that there was no danger of unwashed air gaining access to the plants.

The conditions of atmosphere were proved to be adapted for healthy growth, by growing plants under exactly the same circumstances, but in a garden soil. The conditions of the artificial soil were shown to be suitable for the purpose, by the fact that plants grown in such soil, and in the artificial conditions of atmosphere, developed luxuriantly, if only manured with substances supplying combined nitrogen.

Passing to the subjects of collateral inquiry, the first question considered was, whether plants growing under the conditions stated would be likely to acquire nitrogen from the air through the medium of ozone, either within or around the plant, or in the soil; that body oxidating free nitrogen, and thus rendering it assimilable by the plants.

Several series of experiments were made upon the gases contained in plants or evolved from them, under different circumstances of light, shade, supply of carbonic acid, &c. When sought for, ozone was in no case detected. The results of the inquiry in other respects, bearing upon the points at issue, may be briefly summed up as follows:—

1. Carbonic acid within growing vegetable cells and intercellular passages suffers decomposition very rapidly on the penetration of the sun's rays, oxygen being evolved.

2. Living vegetable cells, in the dark, or not penetrated by the direct rays of the sun, consume oxygen very rapidly, carbonic acid being formed.

3. Hence, the proportion of oxygen must vary greatly according to the position of the cell, and to the external conditions of light, and it will oscillate under the influence of the reducing force of carbon-matter (forming carbonic acid) on the one hand, and of that of the

sun's rays (liberating oxygen) on the other. Both actions may go on simultaneously according to the depth of the cell; and the once outer cells may gradually pass from the state in which the sunlight is the greater reducing agent to that in which the carbon-matter becomes the greater.

4. The great reducing power operating in those parts of the plant where ozone is most likely, if at all, to be evolved, seems unfavourable to the oxidation of nitrogen; that is under circumstances in which carbon-matter is not oxidized, but on the contrary, carbonic acid reduced. And where beyond the influence of the direct rays of the sun, the cells seem to supply an abundance of more easily oxidized carbon-matter, available for oxidation should free oxygen or ozone be present. On the assumption that nitrates are available as a direct source of nitrogen to plants, if it were admitted that nitrogen is oxidated within the plant, it must be supposed (as in the case of carbon) that there are conditions under which the oxygen compound of nitrogen may be reduced within the organism, and that there are others in which the reverse action, namely, the oxidation of nitrogen, can take place.

5. So great is the reducing power of certain carbon-compounds of vegetable matter, that when the growing process has ceased, and all the free oxygen in the cells has been consumed, water is for a time decomposed, carbonic acid formed, and hydrogen evolved.

The suggestion arises, whether ozone may not be formed under the influence of the powerful reducing action of the carbon-compounds of the cell on the oxygen eliminated from carbonic acid by sunlight, rather than under the direct action of the sunlight itself —in a manner analogous to that in which it is ordinarily obtained under the influence of the active reducing agency of phosphorus? But, even if it were so, it may be questioned whether the ozone would not be at once destroyed when in contact with the carbon-compounds present. It is more probable, however, that the ozone said to be observed in the vicinity of vegetation, is due to the action of the oxygen of the air upon minute quantities of volatile carbo-hydrogens emitted by plants.

Supposing ozone to be present, it might, however, be supposed to act in a more indirect manner as a source of combined and assimilable nitrogen in the Authors' experiments, namely,—by oxidating the nitrogen dissolved in the condensed water of the apparatus—by forming nitrates in contact with the moist, porous, and alkaline soil—or by oxidating the free nitrogen in the cells of the older roots, or that evolved in their decomposition.

Experiments were accordingly made to ascertain the influence of ozone upon organic matter, and on certain porous and alkaline bodies, under various circumstances. A current of ozonous air was passed over the substances for some time daily, for several months, including the whole of the warm weather of the summer; but in only one case out of eleven was any trace of nitric acid detected, namely, that of garden soil; and this was proved to contain nitrates before being submitted to the action of ozone.

It is not, indeed, hence inferred that nitric acid could under no circumstances be formed through the influence of ozone on certain nitrogenous compounds, on nascent nitrogen, on gaseous nitrogen in contact with porous and alkaline substances, or even in the atmosphere. But, considering the negative result with large quantities of ozonous air, acting upon organic matter, soil, &c., in a wide range of circumstances, and for so long a period, it is believed that no error will be introduced into the main investigation by the cause referred to.

Numerous experiments were made to determine whether free nitrogen was evolved during the decomposition of nitrogenous organic compounds.

In the first series of 6 experiments, wheat, barley, and bean-meal were respectively mixed with ignited pumice, and ignited soil, and submitted for some months to decomposition in a current of air, in such manner that any ammonia evolved could be collected and estimated. The result was, that, in 5 out of the 6 cases, there was a greater or less evolution of free nitrogen—amounting, in two of the cases, to more than 12 per cent. of the original nitrogen of the substance.

The second series consisted of 9 experiments; wheat, barley, and beans being again employed, and, as before, either ignited soil or pumice used as the matrix. In some cases the seeds were submitted to experiment whole, and allowed to grow, and the vegetable matter produced permitted to die down and decompose. In other cases, the ground seeds, or "meals," were employed. The conditions of moisture were also varied. The experiments were continued through several months, when from 60 to 70 per cent. of the carbon had disappeared.

In 8 out of the 9 experiments, a loss of nitrogen, evolved in the free state, was indicated. In most cases, the loss amounted to about one-seventh or one-eighth, but in one instance to 40 per cent. of the original nitrogen. In all these experiments the decomposition of the organic substance was very complete, and the amount of carbon lost was comparatively uniform.

It thus appeared that, under rare circumstances, there might be no loss of nitrogen in the decomposition of nitrogenous organic matter; but that, under a wide range of circumstances, the loss was very considerable—a point, it may be observed, of practical importance in the management of the manures of the farm and the stable.

Numerous direct experiments showed, that when nitrogenous organic matter was submitted to decomposition in water, over mercury, in the absence of free oxygen, there was no free nitrogen evolved. In fact, the evolution in question appeared to be the result of an oxidating process.

Direct experiments also showed, that seeds may be submitted to germination and growth, and that nearly the whole of the nitrogen may be found in the vegetable matter produced.

It is observed that, in the cases referred to in which so large an evolution of free nitrogen took place, the organic substances were submitted to decomposition for several months, during which time they lost two-thirds of their carbon. In the experiments on the question of assimilation, however, but a very small proportion of the total organic matter is submitted to decomposing actions apart from those associated with growth, and this for a comparatively short period of time, at the termination of which the organic form is retained, and therefore but very little carbon is lost. It would appear, then, that in experiments on assimilation no fear need be entertained of any serious error arising from the evolution of free nitrogen in the decomposition of the nitrogenous organic matter necessarily involved, so long as it is subjected to the ordinary process of germination, and exhaustion to supply materials for growth. On the other hand, the facts adduced afford a probable explanation of any small loss of nitrogen which may occur when seeds have not grown, or when leaves, or other dead matters, have suffered partial decomposition. They also point out an objection to the application of nitrogenous organic manure in such experiments.

Although there can be no doubt of the evolution of hydrogen during the decomposition of organic matter under certain conditions, and although it has long been admitted that nascent hydrogen may, under certain circumstances, combine with gaseous nitrogen and form ammonia—nevertheless, from considerations stated at length in the paper, the Authors infer that there need be little apprehension of error in the results of their experiments, arising from an unaccounted supply of ammonia, formed under the influence of nascent hydrogen given off in the decomposition of the organic matter involved.

Turning to their direct experiments on the question of the assimilation of free nitrogen, the Authors first consider whether such assimilation would be most likely to take place when the plant had no other supply of combined nitrogen than that contained in the seed sown, or when supplied with a limited amount of combined nitrogen, or with an excess of combined nitrogen? And again—whether at an early stage of growth, at the most active stage, or when the plant was approaching maturity? Combinations of these several circumstances might give a number of special conditions, in perhaps only one of which assimilation of free nitrogen might take place, in case it could in any.

It is hardly to be supposed that free nitrogen would be assimilated if an excess of combined nitrogen were at the disposal of the plant. It is obvious, however, that a wide range of conditions would be experimentally provided, if in some instances plants were supplied with no more combined nitrogen than that contained in the seed, in others brought to a given stage of growth by means of limited extraneous supplies of combined nitrogen, and in others supplied with combined nitrogen in a more liberal measure. It has been sought to provide these conditions in the experiments under consideration.

In the selection of plants, it was sought to take such as would be adapted to the artificial conditions of temperature, moisture, &c. involved in the experiment, and also such as were of importance in an agricultural point of view—to have representatives, moreover, of the two great Natural Families, the Graminaceæ and the Leguminosæ, which seem to differ so widely in their relations to the combined nitrogen supplied within the soil—and finally, to have some of the same descriptions as those experimented upon by M. Boussingault, and M. G. Ville, with such discordant results.

Thirteen experiments were made (4 in 1857 and 9 in 1858), in which the plants were supplied with no other combined nitrogen than that contained in the original seed. In 12 of the cases prepared soil was the matrix, and in the remaining one prepared pumice.

Of 9 experiments with Graminaceous plants, 1 with wheat and 2 with barley were made in 1857. In one of the experiments with barley there was a gain of 0·0016, and in the other of 0·0026 gramme of nitrogen. In only two cases of the experiments with cereals in 1858, was there any gain of nitrogen indicated; and in both it amounted to only a small fraction of a milligramme. Indeed, in no one of the cases, in either 1857 or 1858, was there more nitrogen in the *plants themselves*, than in the seed sown. A gain was indicated only when the nitrogen in the soil and pot—which together weighed about 1500 grammes—was brought into the calculation. Moreover, the gain only exceeded 1 milligramme in the case of the experiments of 1857, when slate, instead of glazed earthenware stands were used as the lute vessels; and there was some reason to believe that the gain indicated was due to this circumstance. In none of the other cases was the gain more than would be expected from error in analysis.

The result was then, that in no one case of these experiments was there any such gain of nitrogen as could lead to the supposition that *free* nitrogen had been assimilated. The plants had, however, vegetated for several months, had in most cases more than trebled the carbon of the seed, and had obviously been limited in their growth for want of a supply of available nitrogen in some form. During this long period they were surrounded by an atmosphere containing free nitrogen; and their cells were penetrated by fluid saturated with that element. It may be further mentioned, that many of the plants formed glumes and paleæ for seed.

It is to be observed that the results of these experiments with cereals go to confirm those of M. Boussingault.

The Leguminous plants experimented upon did not grow so healthily under the artificial conditions as did the cereals. Still, in all three of the cases of these plants in which no combined nitrogen was provided beyond that contained in the original seed, the carbon in the vegetable matter produced was much greater than that in the seed—in one instance more than 3 times greater. In no case, however, was there any indication of assimilation of free nitrogen, any more than there had been by the Graminaceous plants grown under similar circumstances.

One experiment was made with buckwheat, supplied with no other combined nitrogen than that contained in the seed. The result gave no indication of assimilation of free nitrogen.

In regard to the whole of the experiments in which the plants were supplied with no combined nitrogen beyond that contained in the seed, it may be observed that, from the constancy of the amount of combined nitrogen present in relation to that supplied, throughout the experiments, it may be inferred, as well that there was no evolution of free nitrogen by the growing plant, as that there was no assimilation of it; but it cannot hence be concluded that there would be no such evolution if an excess of combined nitrogen were supplied.

The results of a number of experiments, in which the plants were supplied with more or less of combined nitrogen, in the form of ammonia-salts, or of nitrates, are recorded. Ten were with Cereals; 4 in 1857, and 6 in 1858. Three were with Leguminous plants; and there were also some with plants of other descriptions—all in 1858.

In the case of the cereals more particularly, the growth was very greatly increased by the extraneous supply of combined nitrogen; in fact, the amount of vegetable matter produced was 8, 12, and even 30 times greater than in parallel cases without such supply. The amount of nitrogen appropriated was also, in all cases many times greater, and in one case more than 30 times as great, when a supply of combined nitrogen was provided. The evidence is therefore sufficiently clear that all the conditions provided, apart from those which depended upon a supply of combined nitrogen, were adapted for vigorous growth; and that the limitation of growth where no combined nitrogen was supplied was due to the want of such supply.

In 2 out of the 4 experiments with cereals in 1857, there was a slight gain of nitrogen beyond that which should occur from error in analysis; but in no one of the 6 in 1858, when glazed earthenware instead of slate stands were used, was there any such gain. It is concluded, therefore, that there was no assimilation of free nitrogen. In some cases the supply of combined nitrogen was not given until the plants showed signs of decline; when, on each addition, increased vigour was rapidly manifested. In others the supply was given earlier and was more liberal.

As in the case of the Leguminous plants grown without extraneous supply of combined nitrogen, those grown with it progressed much less healthily than the Graminaceous plants. But the results under these conditions, so far as they go, did not indicate any assimilation of free nitrogen.

The results of experiments with plants of other descriptions, in which an extraneous supply of combined nitrogen was provided, also failed to show an assimilation of free nitrogen.

Thus, 19 experiments with Graminaceous plants, 9 without and 10 with an extraneous supply of combined nitrogen—6 with Leguminous plants, 3 without and 3 with an extraneous supply of combined nitrogen, and also some with other plants, have been made. In none of the experiments, with plants so widely different as the Graminaceous and the Leguminous, and with a wide range of conditions of growth, was there evidence of an assimilation of free nitrogen.

The conclusions from the whole inquiry may be briefly summed up as follows:—

The yield of nitrogen in the vegetation over a given area, within a given time, especially in the case of Leguminous crops, is not satisfactorily explained by reference to the hitherto quantitatively determined supplies of *combined* nitrogen.

The results and conclusions hitherto recorded by different experimenters on the question whether plants assimilate *free* or *un*combined nitrogen, are very conflicting.

The conditions provided in the experiments of the Authors on this question were found to be quite consistent with the healthy development of various Graminaceous Plants, but not so much so for that of the Leguminous Plants experimented upon.

It is not probable that, under the circumstances of the experiments on assimilation, there would be any supply to the plants of an unaccounted quantity of combined nitrogen, due to the influence either of ozone, or of nascent hydrogen.

It is not probable that there would be a loss of any of the combined nitrogen involved in an experiment on assimilation, due to the evolution of free nitrogen in the decomposition of organic matter, excepting in certain cases when it might be presupposed.

It is not probable that there would be any loss due to the evolution of free nitrogen from the nitrogenous constituents of the plants during growth.

In numerous experiments with Graminaceous plants, under a wide range of conditions of growth, in no case was there any evidence of an assimilation of free nitrogen.

In experiments with Leguminous plants the growth was less satisfactory, and the range of conditions was, therefore, more limited. But the results with these plants, so far as they go, do not indicate any assimilation of free nitrogen. It is desirable that the evidence of further experiments with such plants, under conditions of more healthy growth, should be obtained.

Results obtained with some other plants, are in the same sense as those with Graminaceous and Leguminous ones, in regard to the question of the assimilation of free nitrogen.

In view of the evidence afforded of the non-assimilation of *free* nitrogen by plants, it is very desirable that the several actual or possible sources whence they may derive *combined* nitrogen should be more fully investigated, both qualitatively and quantitatively.

If it be established that plants do not assimilate free or uncombined nitrogen, the source of the large amount of combined nitrogen known to exist on the surface of the globe, and in the atmosphere, still awaits a satisfactory explanation.

"Reduction and Discussion of the Deviations of the Compass observed on board of all the Iron-built Ships and a selection of the Wood-built Steam-ships in Her Majesty's Navy, and the Iron Steamship 'Great Eastern'." By Frederick J. Evans, Esq.

The analysis of the deviations of the compass in this paper comprises the observations made in forty-two iron ships, varying in size from 3400 to 165 tons, a selection of wood-built screw and paddle-wheel steam-vessels, as also the steam-ship 'Great Eastern' at various times prior to her departure from England.

The observations made in the iron-built ships extend over periods varying between thirteen and five years; and having been made with the same description of compass—the Admiralty standard—and under similar conditions of arrangement and situation, in accordance with the system carried out in Her Majesty's Navy, details of which are given, the general results are strictly comparable.

In the analysis of the Tables, amounting to nearly 250 in number, of deviations observed in various parts of both hemispheres, the formula deduced from Poisson's General Equations by Mr. Archibald Smith, given in the Philosophical Transactions for 1846, p. 348, has been employed.

In this formula, the deviation of the compass on board ship, reckoned positive when the north point of the needle deviates to the east, is given by the following expression:—

$$\text{Deviation } (\delta)=\text{A}+\text{B}\sin\zeta'+\text{C}\cos\zeta'+\text{D}\sin 2\zeta'+\text{E}\cos 2\zeta',$$

ζ' being the azimuth (by compass) of the ship's head, reckoned from the magnetic north towards the east;

A, D, E being constant coefficients depending only on the amount, quality, and arrangement or position of the iron in the ship: B and C, coefficients depending on these, and also on the magnetic dip and horizontal intensity, are each consisting of two parts; one caused by the permanent magnetism of the hard iron, the deviation produced by which varies inversely as the horizontal force at the place; and the other, caused by the vertical part of the earth's force inducing the soft iron in the ship, the deviation produced by which varies as the tangent of the dip: B representing that part of the combined attraction acting in a fore-and-aft direction, C that acting in a transverse, or athwart-ship direction.

From the equation $\tan^{-1}\frac{\text{C}}{\text{B}}$, the direction of the ship's force, and $\sqrt{\text{B}^2+\text{C}^2}$, the total magnetic force of the ship in proportion to the horizontal force at the place of observation is obtained: for convenience, 1000 has been adopted to represent the value of the earth's horizontal force at the English ports of observation, in order, by an easy comparison, to note the changes on foreign stations.

By comparison of the coefficients of the several descriptions of ships, it is observed that in wood-built steam-vessels, the coefficients B and C vary nearly as the tangent of the dip; from whence it may be inferred, as a general rule, that in steam machinery permanent magnetism bears but a small proportion to induced; but in iron-built ships, B and C generally vary more nearly as the inverse horizontal force, showing that they depend more on the permanent magnetism of the iron of the ship, and thus confirming the view of the Astronomer Royal, given in his earliest deductions (Phil. Trans. 1839), that the effect of transient induced magnetism is in these ships small comparatively. Numerous examples are given in detail of this permanency of magnetism, as also of the gradual diminution of the ship's force resulting from time.

An investigation of the coefficient D, which is caused entirely by the horizontal induction of the soft iron in the ship, and which is known as the "quadrantal" deviation, shows, that while in wood-built steam-ships it seldom exceeds 1° or 1½°, it rises in iron-built ships from 1½° to 6° and 7°; the Liverpool Compass-Committee recording even a point of the compass.

The chief characteristics of the quadrantal deviation, as developed in this investigation, are—

1. That it has invariably a positive sign, causing an easterly deviation in the N.E. and S.W. quadrants; and a westerly deviation in the S.E. and N.W. quadrants.

2. Its amount does not appear to depend on the size, or mass of the vessel, or direction when building; or on the existence of iron beams.

3. That a gradual decrease in amount has occurred, after the lapse of a number of years, in nearly every vessel that has been observed.

4. That the value remains unchanged in sign and amount, on changes of geographic position.

5. That a value not exceeding 4°, and ranging between that amount and 2°, may be assumed to represent the average or normal amount in vessels of all sizes.

Numerous examples are given in support of these propositions, as also of the uniformity of the amount of quadrantal deviation when determined in various parts of the ship; and, assuming the normal amount in iron steam-ships as from 2° to 4°, an analysis is given by which it is seen that 75 per cent. of the iron ships of the Royal Navy are included in this condition.

Two questions of importance here arise; are the results of this analysis conclusive, and if so, under what conditions do large quadrantal deviations occur? Reverting to the Astronomer Royal's early experiments in 1838–39, in the iron ships 'Rainbow' and 'Ironsides,' whose values were very small, and presuming that those vessels were built of good material—from their then experimental character—as also that similar conditions of material of good quality exist in the iron ships of the Royal Navy, it is assumed that the value (2° to 4°) represents the average condition of a ship built of the best or superior iron.

On the other hand, can the inference be drawn that large quadrantal deviation in an iron ship implies that inferior material has been used in her construction? Attention is here directed to the ships 'Birkenhead' and 'Royal Charter,' which from their well-known magnetic coefficients may be regarded as the types respectively of "hard" and "soft" iron constructed vessels, and from their consideration, as also from a review of the general results, these conclusions are derived:—

1. That in an iron ship of ordinary dimensions, a standard compass can be placed, the deviations of which will but little exceed those obtaining in wood-built steam-ships; and further, that on changes of geographic position, however distant, these deviations will be within smaller limits, and can be approximately predicted.

2. A divergence from these conditions will arise when the inductive

magnetism of the hull or machinery predominates; and it is inferred, especially from the example of the 'Royal Charter,' that large quadrantal deviation and fluctuating sub-permanent magnetism (due to hull alone) are co-existent, and give rise to conditions of compass disturbance which are beyond prediction, and which have hitherto baffled inquiry and given a complexion to theoretical deductions varying as regarded from different points of view.

In order to examine the change which the original magnetism of an iron ship undergoes after launching, a series of compass observations were made in the steam-ship 'Great Eastern' prior to her quitting the River Thames in 1859, and subsequently at Portland, Holyhead, and Southampton*—at the three first-named places within short periods of time of each other.

The results, from an Admiralty Standard Compass placed in a position the least subject to influence from local masses of iron, were as follows:—In the first five days, from Deptford to Portland, the ship's force had diminished from 0·585 to 0·480 [the earth's force =1·000], or nearly one-fifth; representing a decrease in the "semicircular" deviation from 35° 50′ to 28° 45′; the direction of the force, or neutral points, approaching the fore-and-aft line by 10°, or changing from 47° on the starboard bow to 37°.

At the expiration of the next six weeks, the ship in the interim having made the passage to Holyhead, the ship's force diminished from 0·480 to 0·390, or about one-sixth, corresponding to a decrease of "semicircular" deviation from 28° 45′ to 23° 0′, the direction of the force changing from 37° to 32°.

At Southampton, in June 1860, or nearly eight months after the experiments made at Holyhead, the force had further diminished from 0·390 to 0·235, or by one-half, corresponding to a decrease in the "semicircular" deviation from 23° 0′ to 13° 30′; whilst the direction of the force approached the fore-and-aft line 25°, or from 32° to 7°; the quadrantal deviation remaining nearly constant [$+4\frac{1}{4}$°] the whole time included in the various observations.

The unvarying tendency of the direction of the ship's force in the 'Great Eastern' to assume a fore-and-aft line, supports the view that time, with the vibrations and concussions due to sea service, leads to a distribution of the magnetic lines, of the nature of a stable equilibrium depending on the average of the inducing forces to which the ship is exposed; the respective sections of the hull having north and south polarity, being separated by lines approximating more nearly a horizontal plane and vertical axis through the body of the ship; instead of the inclined axis and equatorial plane of separation due to the magnetic dip of the locality, and divergence from the magnetic meridian, of the hull while building.

The practical information resulting from the example of the 'Great Eastern' is, that prior to a newly built iron ship being sent to sea, her head during equipment should be secured in an opposite direction to that in which she was built; and that the magnetic lines should

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* The observations at Southampton were made after the paper was communicated to the Royal Society, and are introduced by way of supplement.

be assisted to be "shaken down" by the vibrations of the machinery in a short preparatory trip prior to the determination of her compass errors, or their compensation; but especially that in the early voyages vigilant supervision should be exercised in the determination of the compass disturbances.

Another important point, generally neglected when compasses are adjusted by the aid of magnets in a newly built iron ship, is rendered manifest by the results of this investigation; namely, the necessity of the errors of the compass being determined and placed on record prior to the adjustment. Without the knowledge to be derived from these observations of the magnetic force of the ship, all future changes of magnetism and consequent errors of the compass are mere guesswork both to those who adjust, and those in charge of the navigation of the ship.

It is recommended that, in any future legislation for the security of the navigation of our mercantile marine with reference to iron-built ships, the determination and record of these preliminary observations should be secured.

The paper concludes by directing attention to the general principles of practical import which result from the investigation, viz. as to the best direction with reference to the magnetic meridian for the keel and head of an iron ship to be placed in building, to ensure the least compass disturbance; the best position and arrangement for a compass to ensure small deviations, and permanency on changes of geographic position; and the changes to which the compass is liable from various causes on the foregoing conditions being fulfilled.

For the best direction in building, it is shown that, from the nature of the polarity of the hull, and especially of the top sides in the after section of the ship and adjoining the compass, where usually placed, the latter is least affected in those vessels built in the line of the magnetic meridian.

For iron steam-vessels engaged in the home or foreign trades in the northern hemisphere, it is recommended, from the then antagonistic magnetic influence of the hull and machinery, to build them head to the north: for iron sailing vessels, from the top sides, in the usual position of the compass, being magnetically weak if built head to the south, the latter direction is to be preferred.

The selection for the position of the compass depends on the direction of the ship during building; in those built head to north, it must be removed as far from the stern as convenience will permit; in those built head to south, as near to the stern as convenient, but avoiding especially, in all cases, proximity to vertical masses of iron. In ships built head east or west, there is little choice of position: in those built on the intercardinal points, a position approximating to the stern when the action from the top-sides—to be determined experimentally—is at a minimum, is to be preferred.

Ample elevation above the deck and exact position in the middle line of the ship, are primary conditions to be observed; and no compass should be nearer iron deck beams than 4 feet. As every piece of iron not forming a part of, or hammered in the fabrication of the hull, such as the rudder, funnel, fastenings of deck houses, &c., is of a magnetic character differing from the hull of the ship, proximity to any such should be avoided, and, as far as possible, the compass should be so placed that they may act as correctors of the general magnetism of the hull.

As most compasses are affected by the magnetism of the ship to an amount depending on their elevation, and the direction of the ship in building, the disturbances will be large comparatively, except in those vessels built head east or west.

A series of Tables is appended, wherein the magnetic coefficients and ship's force and direction of the various classes of vessels are given, the ships being classed according to the nature of their material and machinery.

GEOLOGICAL SOCIETY.

[Continued from p. 311.]

March 20, 1861.—L. Horner, Esq., President, in the Chair.

The following communications were read:—

1. "On a Collection of Fossil Plants from the Nagpur Territory, Central India." By Sir C. Bunbury, Bart., F.R.S., F.G.S. &c.

The specimens examined by the author were collected by the Rev. Messrs. S. Hislop and R. Hunter, and presented to the Geological Society in 1854 and since. The vegetable remains described in this paper are:—1. *Glossopteris Browniana*, var. *Australasica*, Ad. Brongn. *G. Browniana*, var. *Indica*, Ad. Brongn. By much the most abundant plant in the collection. 2. *G. musæfolia*, sp. nov. 3. *G. leptoneura*. 4. *G. stricta*, sp. n. 5. *Pecopteris*, sp. 6. *Cladophlebis* (?). 7. *Tæniopteris danæoides*, M'Celland (?). 8 and 9. *Filicites*: possibly *Glossopteris*. 10. *Nœggerathia* (?). 11. *Phyllotheca Indica*, sp. n. 12. *Vertebraria* (?). Different from the true *Vertebraria*, and probably *roots*. 13. *Knorria* (?). 14. *Stigmaria* (?). 15. Part of a stem, somewhat Sigillarian in appearance. 16. Yuccites (?). The fruits and seeds are reserved for further examination. On a general survey of all these plant-remains, the author for the present considers the *facies* of the fossil flora under notice to be Mesozoic rather than Palæozoic, but he regards the question as an open one, and requiring much further light for its perfect elucidation.

2. "On the Age of the Fossiliferous thin-bedded Sandstones and Coal-beds of the Province of Nagpur, Central India." By the Rev. Stephen Hislop. Communicated by the President.

The author first pointed out the places near the city of Nagpur where the *plant-bearing sandstone* has been best studied. He next noticed the carbonaceous shales underlying thick sandstones, at the foot of the Mahádewa Hills and the coal-seams of Barkoi, near Umret, 80 miles and more N.W. of Nagpur; and pointed out their relationship to the plant-bearing sandstone near Nagpur, as shown by the *Glossopteris* and other fossils found in each locality.

At Mángali, between 50 and 60 miles S. of Nagpur, dark red sandstones are found, rich in *Estheria*, and containing remains of Plants, Ganoid Fishes, and Reptiles (*Brachyops laticeps*, Owen). These beds Mr. Hislop thinks to be of the same age as those of Nagpur and Chanda. Still further S. (170 miles from Nagpur), at Kotá, there are (under thick sandstones) limestones and shales, containing fishes of the genera *Æchmodus* and *Lepidotus*, Teleosaurian remains, Coprolites, fossil Insects, *Cypridæ*, and *Estheriæ*, with obscure plant-remains. These beds are also regarded by the author as equivalent in age to the plant-bearing sandstones of Nagpur; whilst the sandstone above them may be equal to the sandstone of the Mahádewas; and the red clay beneath them may be the same as that of Maledi 30 miles off (to the N.E.), where *Ceratodus* teeth and Coprolites have been found in abundance.

Mr. Hislop then compared in detail, 1. the fossil flora of the coal-fields of New South Wales with that of Central India; 2. the fossil plants of Western Bengal with those of Central India; and 3. the fossil fauna of these two regions; and came to the conclusion that, on the whole, they probably represent the Jurassic (or possibly the Triassic) period,—at all events some portion of the Lower Mesozoic epoch.

3. "On the Geological Age of the Coal-bearing Rocks of New South Wales." By the Rev. W. B. Clarke, F.G.S.

The author first referred to his report, in 1847, of the occurrence of *Lepidodendron*, *Sigillaria*, and *Stigmaria* in the coal-fields of Australia; and advanced proofs of the occurrence of *Lepidodendron* (*Pachyphlœus* (?), Gœppert) over a region extending from 23° to 37° S. lat., and at least 1000 miles long. After some observations on the association of Carboniferous and Devonian fossils with the coal-beds of Australia and Tasmania, Mr. Clarke stated that in 1859, at Stony Creek, near Maitland, Mr. B. Russell, having sunk two pits in search of coal, found four or five coal-seams lying between beds containing *Pachydomi*, *Spiriferi*, *Orthoceratites*, *Conulariæ*, &c.; and beneath them a shale containing *Nœggerathia*, *Glossopteris*, *Cyclopteris*, &c. From this and other evidence the author is induced to believe that the beds are of palæozoic age, in spite of the "Jurassic" appearance of the plant-remains.

4. "On some Reptilian Remains from North-western Bengal." By Prof. T. H. Huxley, F.R.S., Sec.G.S.

Some bones, found by Mr. Blanford in the uppermost portion of the "Lower Damūda" group of strata in the Ranigunj coal-field, and forwarded to the author by Professor Oldham, have proved to belong to Labyrinthodont Amphibia and Dicynodont Reptiles, hereby affording new and interesting links with the fossil fauna of the Karoo-beds of South Africa, and largely increasing the probability that the rocks in which they were found are of Triassic, or perhaps Permian, age.

April 10.—Sir R. I. Murchison, V.P.G.S., in the Chair.

The following communications were read:—

1. "On the Geology of the Country between Lake Superior and

the Pacific Ocean (between 48° and 55° parallels of latitude), explored by the Government Exploring Expedition under the command of Captain J. Palliser (1857–60)." By James Hector, M.D. Communicated by Sir R. I. Murchison, V.P.G.S.

This paper gave the geological results of three years' exploration of the British Territories in North America along the frontier-line of the United States, and westward from Lake Superior to the Pacific Ocean.

It began by showing that the central portion of North America is a great triangular plateau, bounded by the Rocky Mountains, Alleghanies, and Laurentian axis, stretching from Canada to the Arctic Ocean, and divided into two slopes by a watershed that nearly follows the political boundary-line, and throws the drainage to the Gulf of Mexico and the Arctic Ocean. The northern part of this plateau has a slope, from the Rocky Mountains to the eastern or Laurentian axis, of six feet in the mile, but is broken by steppes, which exhibit lines of ancient denudation at three different levels: the lowest is of freshwater origin; the next belongs to the Drift-deposits; and the highest is the great Prairie-level of undenuded Cretaceous strata. This plateau has once been complete to the eastern axis, but is now incomplete along its eastern edge, the soft strata having been removed in the region of Lake Winipeg.

The eastern axis sends off a spur that encircles the west shore of Lake Superior, and is composed of metamorphic rocks and granite of the Laurentian Series. To the west of this follows a belt where the floor of the plateau is exposed, consisting of Lower Silurian and Devonian rocks. On these rest Cretaceous strata, which prevail all the way to the Rocky Mountains, overlain here and there by detached tertiary basins.

The Rocky Mountains are composed of Carboniferous and Devonian limestones, with massive quartzites and conglomerates, followed to the west by a granitic tract which occupies the bottom of the great valley between the Rocky and the Cascade Mountains. The Cascade chain is volcanic, but the volcanos are now inactive; to the west of it, along the Pacific coast, Cretaceous and Tertiary strata prevail. The description of these rocks was given with considerable detail, on account of their containing a lignite which for the first time has been determined to be of Cretaceous age. This lignite, which is of very superior quality, has been worked for some years past by the Hudson Bay Company, and is in great demand for the steam-navy of the Pacific station, and for the manufacture of gas. Extensive lignite-deposits in the Prairie were also alluded to; and, like those above mentioned, were considered to be of Cretaceous age; but, besides these, there are also lignites of the Tertiary period.

The general conclusion was that the existence of a supply of fuel in the Islands of Formosa and Japan, in Vancouver's Island, in the Cretaceous strata of the western shores of the Pacific, but principally within the British territory, and in the plains along the Saskatchewan, will exercise a most important influence in considering the practicability of a route to our Eastern possessions through the Canadas, the Prairies, and British Columbia.

2. "On Elevations and Depressions of the Earth in North America." By Dr. A. Gesner, F.G.S.

After some observations on the differences between volcanic uplifts of the land and the slow upward and downward shiftings produced by changes in the position of great parallel areas during long periods of time, the author proceeds to enumerate evidences of local elevation and subsidence that he has observed along the coast from the northern part of Labrador to New Jersey.

In the south-eastern part of New Jersey, at Nantucket, Martha's Vineyard, and Portland, submergence of the land is proceeding, locally at the rate of probably four feet in sixty years. In New Brunswick, at St. John's the land has been elevated; at the Great Manan Island and the Great Tantaman Marsh there has been subsidence. At Bathurst and on the opposite coast of Lower Canada the land seems to be rising. In Nova Scotia, near the Bay of Fundy and Mines Basin there is subsidence; on the southern side, however, there are signs of elevation. The sea rapidly encroaches upon Louisberg in Cape Breton; and in Prince Edward's Island, also, at Cascumpec, submergence of the land is taking place.

LXXIX. *Intelligence and Miscellaneous Articles.*

ON THE THEORY OF CYLINDRICAL CONDENSERS.

BY M. J. M. GAUGAIN.

I HAVE in a former note* called attention to the fact that it is very difficult to analyse the phenomena of condensation produced in submarine telegraphic cables, the gutta percha which envelopes them being only imperfectly non-conducting, so that there is at one and the same time propagation by conductibility and condensation. To study the latter phenomenon by itself, I substituted for gutta percha dielectrics, which isolate much more perfectly; I employed for this purpose gum-lac and air; with gum-lac the absorption is very small, with air it is nothing, or altogether imperceptible.

The laws which I have succeeded in establishing are very simple, and may be of some practical utility, since they afford a solution of the different questions which relate to electric condensation in submerged cables; it is, however, from a philosophical point of view that they seem to be of the greatest interest, since they confirm in a remarkable manner the views of Faraday. This illustrious physicist, in a memoir published in 1837 (Experimental Researches, Series XI. No. 1320), expressed himself nearly as follows:—"The power of isolating and that of conducting are only two extreme degrees of the same property, and ought to be considered as being of the same nature in any satisfactory mathematical theory." Now it will be seen that, in the case at least of cylindrical condensers, the laws which regulate the propagation of electricity by *excitation* do not differ from those which Ohm has established for propagation by conductibility. The general results of my researches may be stated shortly as follows:—

1. When the internal cylinder is the collector, that is to say, when it communicates with the source, and the external cylinder commu-

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* *Comptes Rendus,* 28th October, 1860.

nicates with the ground, the *excited* charge of the external cylinder is equal to the *exciting* charge of the internal cylinder.

2. When the external cylinder is the collector, the *excited* charge of the internal cylinder is precisely the same as it would have been if it had been put in direct communication with the same source.

3. When the external cylinder is the collector, its charge may be considered as consisting of two parts, one of which is equal to the *excited* charge of the internal cylinder, the other representing the quantity of electricity which the external cylinder would take up by itself under the influence only of the medium in which it is placed.

The latter law enables us to foresee what would happen in the case of a condenser formed of three concentric cylinders. The charge which the middle cylinder would take up when put in communication with the source, the other two being connected with the ground, must be equal to the charge which would be excited in it by the two other cylinders. I have found by experiment that this is really the case.

It follows that condensers arranged in a spiral form, may serve to collect in a small volume a large quantity of electricity.

4. If we agree to call by the name of *resistance to excitation* a quantity inversely proportional to the charge received by either armature, when the tension of the internal cylinder is maintained at unity, and that of the external cylinder at zero, then this resistance, which I shall call ρ, is expressed by the formula

$$\rho = k \log \frac{R}{r},$$

R and r representing the respective radii of the external and internal cylinders, and k being a constant which depends on the inductive capacity of the dielectric, and on the length of the cylinder employed.

This formula is remarkable, since it might have been deduced *à priori* from the ordinary theory of propagation by conductibility. Suppose, in fact, that the substance which separates the two cylindrical armatures of the condensers possesses a certain conductibility, and let us call by the name of *resistance to conductibility*, a quantity inversely proportional to the amount of electricity which, in a unit of time, traverses the annular space between the two cylinders, the tension of the internal cylinder being maintained at unity, that of the external cylinder at nothing. This *resistance to conductibility* may be calculated according to the principles established by Ohm, and it will be found that it is expressed by the same formula as the *resistance to excitation*. To pass from one formula to the other, it is only necessary to change the meaning of k. We may say therefore that the same theory, that namely of Ohm, regulates propagation by excitation and propagation by conductibility, at least when we confine ourselves to the consideration of spaces bounded by concentric cylinders. I propose to verify this principle under other circumstances, and especially in the case of spherical condensers.—*Comptes Rendus*, Feb. 18th, 1861.

ON THE PRODUCTION OF GRAPHITE BY THE DECOMPOSITION OF CYANOGEN COMPOUNDS. BY DR. P. PAULI.

The mother-liquors obtained from the evaporation of a solution of the so-called black ash are now commercially worked for caustic alkali. These liquors contain the following compounds:—

1. Chiefly hydrate of soda.
2. Some quantity of carbonate of soda.
3. Several sulphur compounds of sodium: viz. sulphide of sodium, hyposulphite of soda, sulphite of soda, and sulphate of soda.
4. Sulphide of iron held in solution by the sulphide of sodium.
5. Chloride of sodium.
6. Several cyanogen compounds of sodium, and especially ferrocyanide of sodium.

These liquors are evaporated in large cast-iron pots, and in order to destroy or oxidize the sulphides of sodium and iron, as also the cyanogen compounds, an equivalent quantity of soda-saltpetre is added. All the oxidable sulphur compounds, together with the small quantity of sulphide of iron, are changed to sulphate of soda and peroxide of iron by the nitrate of soda in the boiling liquor, at a temperature not below 260° to 270° F. The cyanogen compounds, on the other hand, are not decomposed by the nitre until the liquor begins to pass from the watery into the dry fusion, and the uncombined water of the hydrate of soda has been driven off. When the whole mass of alkali (generally about four tons) reaches a low red heat, a regular evolution of gas is observed: this is evidently owing to the oxygen produced by the decomposition of the nitrate, and to the nitrogen from the decomposition of the cyanides; at the same time a plentiful liberation of graphite is observed, covering the whole surface of the liquor with a bright layer of graphite. This liberation of graphite is still more plainly seen if no nitre be added to the liquor at first, or only so much as is sufficient to oxidize the sulphur compounds: but if a few pounds of nitrate of soda be added when the water has been driven off, and the mass is allowed to become red-hot, a violent reaction takes place, and a large quantity of graphite is set free. This sudden liberation of graphite proves that this substance cannot be derived from the cast iron of the pot in which the fusion is made. So violent is the evolution of gas, that a complete cloud of fine particles of caustic soda is carried up into the air, rendering it almost impossible to remain in the neighbourhood of the operation. In this way all the cyanogen compounds are completely decomposed, the iron in the ferrocyanide of sodium becomes peroxide, and in a few hours falls to the bottom of the pot. If the right quantity of saltpetre has been added, a colourless mass of fused caustic soda remains; but if too large an amount of nitre has been added, the liquor becomes coloured deep green, owing to the formation of manganate of soda. It is remarkable that, in the absence of nitrate of soda, the cyanogen compounds act reducingly upon the sulphide of sodium; this is seen from the fact that a portion of the soda-lye, which gives no sulphide reaction with a lead salt, produces a blackening after the caustic alkali has been heated to redness.

The graphite may be skimmed off the surface of the fused alkali;

and when washed with water and hydrochloric acid, it appears in the form of an extremely fine bright powder. If allowed to swim on the top of the almost red-hot fused soda, the graphite is oxidized gradually, and after the lapse of about three or four hours it altogether disappears. Heated in a platinum crucible by itself it is incombustible; but it generally contains small particles of charcoal mixed with it, and these undergo oxidation.

The temperature at which this evolution of graphite takes place is a very low one, compared with that at which graphite is liberated from cast iron; for a thin iron wire can scarcely be brought to a visible red heat by dipping it into the fused alkali.

From this peculiar decomposition it would appear that we have good reason to assume that the carbon contained in cyanogen is present in the graphite modification; for if this be not the case, how is it that the easily combustible charcoal can withstand the oxidizing action of the saltpetre, whilst none of the iron of the ferrocyanide of sodium is reduced to the metallic state.

It has, besides, been lately shown by M. Caron, that the formation of steel, *i. e.* the combination of iron with carbon in the graphite modification, can only take place in presence of cyanogen compounds, and that no carbon whatever is taken up by the iron when this metal is heated with other carboniferous gases. The mode of production of graphite noticed in this communication appears to be an intermediate reaction between that from the carbide of iron and from the nitride of carbon.

As in the process of cementation it is seen that the carbon of the cyanogen is taken up by the iron without being set free, so this reaction proves that cyanogen can be split up into its constituent parts without either of them combining with a third body.

Despretz asserts that the carbonization of iron is always preceded by a combination of this metal with nitrogen, a process which makes it porous and more fit for uniting with carbon. The correctness of this supposition has, however, become rather doubtful, by Caron's recently published experiments (*Comptes Rendus*, Nos. 15 and 24, 1860).

To conclude, I beg to say some words about the formation of native graphite; I do not think that this body has been formed from coal or diamond, but I rather believe it has been separated out of carbon compounds as graphite, by processes perhaps analogous to those above described.—*Proc. Manchester Phil. Soc.*, April 16, 1861.

ON ELECTRICAL PARTIAL DISCHARGES. BY P. RIESS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Professor Rijke has met with a difficulty in the explanation of Wheatstone's experiments on the discharge of a Leyden jar, and has himself instituted experiments*, from which it seems that more electricity remains on a conductor when it is discharged through a wet string than when it is discharged through a metallic wire.

I do not think that this difficulty exists, nor that these experiments have any claim to novelty. Twenty years ago I showed that when

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* Phil. Mag. for May, p, 365.

a battery is discharged through a column of water, three-eighths of the charge remain on the battery, but that two-thirteenths remain when the discharge is effected through a metallic wire; and from this I concluded that the electricity of the battery is discharged in a successive manner (Pogg. *Ann.* vol. liii. p. 14. "Lehre von der Reibungselectricität," § 634).

Since then I have always assumed that the spark accompanying the discharge consists of several sparks; and the discharge of an electrified body, of several individual discharges, which I have called partial discharges. By this assumption Wheatstone's and many other electrical experiments have become capable of explanation.

I am, Gentlemen,
Yours truly,

Berlin, May 10, 1861. P. Riess.

ON THE FREEZING OF WATER AND THE FORMATION OF HAIL.

BY M. L. DUFOUR.

When water is preserved from contact with solid bodies by placing it in a mixture which has the same density, and which does not form aqueous mixtures, its congelation may be materially retarded. Water placed in a mixture of chloroform and oil (the best is oil of sweet almonds) takes the form of perfect globules, and remains at rest in the interior of the mixture. If this mixture be cooled, the water in this condition scarcely ever freezes at 0° C.; its temperature sinks to $-6°$, $-10°$ before this change takes place. Globules have in this way been even reduced to $-20°$ while still liquid.

The globules either change into globules of ice, or they simply freeze on the surface, according to their dimensions and the diminution of temperature. They persist in the liquid state with remarkable stability. In this mixture of chloroform and oil they may be shaken, and foreign bodies introduced, without solidifying; but solidification immediately ensues when they are touched with a piece of ice. The discharge of a Leyden jar or a galvanic current may traverse these globules without their solidifying; but the powerful discharge of a Ruhmkorff's coil causes their immediate solidification.

When an ice-sphericle formed in the mixture of chloroform and oil is surrounded by other spheres which still remain liquid, the congelation of the latter may be effected by bringing them in contact with the first. Different effects are obtained according to the temperature and dimensions of the globules. Sometimes (with small globules and low temperatures) the spheres touched solidify suddenly, and remain separate; sometimes (with larger globules and somewhat higher temperatures) they coalesce more or less completely; they stretch out on each other at the moment of solidification. In this way pieces of ice of the most varied shapes may be obtained—irregular spheres formed of concentric layers (each layer consisting of a globule which enveloped the nucleus at the moment of its formation), spheres with protuberances, &c. These varied forms would have but a subordinate interest, if they did not recall the concentric zones and the irregular shapes observed in hailstones. This resemblance is evident in these experiments; and the question naturally arises whether hailstones are not formed under similar circumstances. In a memoir which I shall publish in the *Bibliothèque Universelle,* I examine this analogy more closely, and endeavour to show that it is not superficial, but extends into numerous details. I attempt to show that this particular case of freezing gives a suitable explanation of the general phenomena, as well as of the accidental peculiarities of hailstones: I attempt to show that these aqueous globules may also be cooled below 0° in the atmosphere; that they can then freeze and unite just as in the mixture of chloroform and oil, and that the grains of ice thus formed, increased by the condensation of atmospheric vapour on their surface, may be hailstones.—*Comptes Rendus,* April 15, 1861.

ON AN APPARATUS FOR EXPERIMENTS ON RESPIRATION AND PERSPIRATION IN THE PHYSIOLOGICAL INSTITUTE AT MUNICH. BY PROFESSOR PETTENKOFER.

In order to determine the quantities of carbonic acid and of water which are eliminated through the skin and lungs, numerous methods have been proposed, and the processes and results of Scharling, Vierordt, Valentin and Brunner, Regnault and Reiset, Smith, and others, are well known to every physiologist and chemist. The objections to all previous methods are of two kinds: first, that the accuracy of the method cannot be determined by control experiments with known quantities of carbonic acid; and second, that the men and animals were compelled to breathe, in these experiments, under more or less unusual and burdensome, and therefore unnatural conditions. The experiments of Bischoff and Voit on the food of carnivora have further shown that the carbonic acid eliminated by the lungs and skin, cannot be obtained by taking the difference between that administered as food, and that secreted in the urine and fæces, allowing for the difference in weight of the body; because two unknown quantities of carbonic acid and water are eliminated from the lungs simultaneously, and in varying quantities. As it was necessary to determine directly at least one of these magnitudes, the author endeavoured to construct an apparatus by which a constant current of air could be passed over a man, and the increase of carbonic acid and water of this air determined.

Pettenkofer's apparatus consists of a small sheet-iron chamber (which will be called the *saloon*) 8 Bavarian feet in every dimension, with an iron door, a light at the top, and windows at the sides. The windows were cemented, and the sides and cover riveted as air-tight as possible. The door had moveable openings in order to ensure access of air at other points besides the joints of the door. On the side opposite to the door there are two apertures, one below and the other above, which, by means of two tubings, are connected with a single wide tube outside, in which the air flows towards that part of the apparatus which serves as an aspirator. This piece of the apparatus, which is placed in a different part of the house, consists of two suction cylinders with valves, which can be uniformly worked to the same height of stroke by means of a powerful clockwork motion. The weight of the clockwork is continually raised in proportion as it sinks, and in this way is obtained a continuous current of air through the doors of the iron room towards the suction cylinders. But the air cannot reach the suction cylinder without first passing through a continuously acting measuring apparatus. For this purpose the author selected a large gas-meter, of such dimensions that 3000 English cubic feet could be measured with it in an hour.

In order to investigate a portion of the air entering by the apertures in the door and any accidental leakages in the apparatus, as well as of the air flowing towards the gas-meter, and to calculate from the observed difference in the proportion of water and carbonic acid the quantities which had entered from the apparatus, there are two aspirators, each of which simultaneously withdraws an equal quantity of air. The water of the air is absorbed by sulphuric acid and weighed, and the carbonic acid is determined by allowing the air to pass in fine bubbles through a determinate quantity of lime-water of known strength, and the strength of the lime-water finally determined by means of dilute oxalic acid.

In order to take a specimen of the air remaining in the saloon, a forcing and suction pump is connected with the outlet pipe, by which flasks holding 6 or 8 litres may be filled with air, and the quantity of carbonic acid determined by means of lime-water. The same pump serves to determine at any time the variations in the carbonic acid during the progress of an experiment. There is an arrangement by which test-quantities of any amount may be taken out without causing any loss in measuring the whole current. For this purpose a flask is connected air-tight with the pump, and by continuous pumping its air is completely replaced by air from the outlet pipe. The air pressed out of the flask is not allowed to escape, but is passed by means of a caoutchouc tube into the current which goes to the gas-meter; of course, in a place where it cannot affect the determinations of carbonic acid.

In order that the air-current may take no water from the large gas-meter by evaporation, the air before entering the gas-meter first passes through an upright cylinder filled with pieces of pumice kept moist.

Where the air issues from this apparatus, there is in the tube a psychrometer, in order to measure the temperature and moisture of the air which passes into the gas-meter. In the tube which leads to the moistening apparatus there is a psychrometer, and several tubes for taking out specimens of air, &c.

The apparatus has been examined since May last in every particular, and the author recommends the methods of investigation as in every way convenient. It was above all important to prove that the carbonic acid disengaged in the saloon could actually be found again and determined—a control which has been omitted in all previous experiments on respiration. After the author, by numerous experiments, had investigated all the influences of the apparatus and of the methods on the accuracy of the results, he took a stearine candle and determined its carbon by elementary analysis. When the suction cylinders of the apparatus, and simultaneously the apparatus for analysis of the air, were at work, a weighed candle was lighted in the saloon from without, and, before the experiment terminated, again extinguished from without and weighed.

The carbonic acid formed by combustion of the candle must be partially contained in the air which had passed through the large gas-meter, and partly in that contained in the saloon. The carbonic acid in the air which had passed through the gas-meter was determined by allowing a continually equal proportion (about 100 cubic centims. in a minute) of the current from the saloon to the gas-meter to bubble through lime-water. The carbonic acid of the air remaining in the saloon was determined, after mixing the different layers of air well together, by filling two or three vessels of 6 to 8 litres capacity, determining by lime-water, and calculating this upon the known capacity of the saloon. It is only after these flasks have been filled that the saloon may be entered to take out the candle and weigh it.

Since the air which passed into the gas-meter, and that which remains in the saloon, contained not only the carbonic acid formed in the experiment, but that already contained in the air as it passed into the saloon, the quantity of the carbonic acid of the air entering must be subtracted. This is obtained from the experiment where the air which enters is withdrawn and investigated in just the same manner and the same quantity as the emergent air. In this way the difference in the quantity of carbonic acid inside and outside is determined; and this ensures the exactitude of the determinations, because all constant errors of the method are thereby eliminated. Of course all the measurements are made with allowances for the tension of aqueous vapour, of temperature, and pressure of the atmosphere.

The author adduces some experiments which he made with stearine candles, and states his reasons for believing that, during an experiment in which more than four-fifths of the disengaged carbonic acid pass into the current between the saloon and the gas-meter, no greater errors than at most 1 or 2 per cent. are to be feared. Inasmuch as the duration of experiments with men and animals can be extended to twelve or twenty-four hours, it is to be hoped that even greater accuracy may be attained.

This is the first apparatus of its kind in which living is possible under normal conditions. Men can live in it just as well as in any well-ventilated room, and can move about, eat, and drink in the ordinary manner. By a moveable window in the door of the saloon, food and other articles can be supplied or taken out—just as, in a small room, provided the draught in the chimney is in order, a stove-door can be opened without admitting smoke. The observer outside the saloon who has charge of the experiment does not by his respiration affect the result in the least; for the carbonic acid in the air passing into the saloon can be continually checked by one of the control apparatus, and can be allowed for.—*Journal für Prakt. Chemie*, vol. lxxxii. p. 40.

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END OF THE TWENTY-FIRST VOLUME.

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