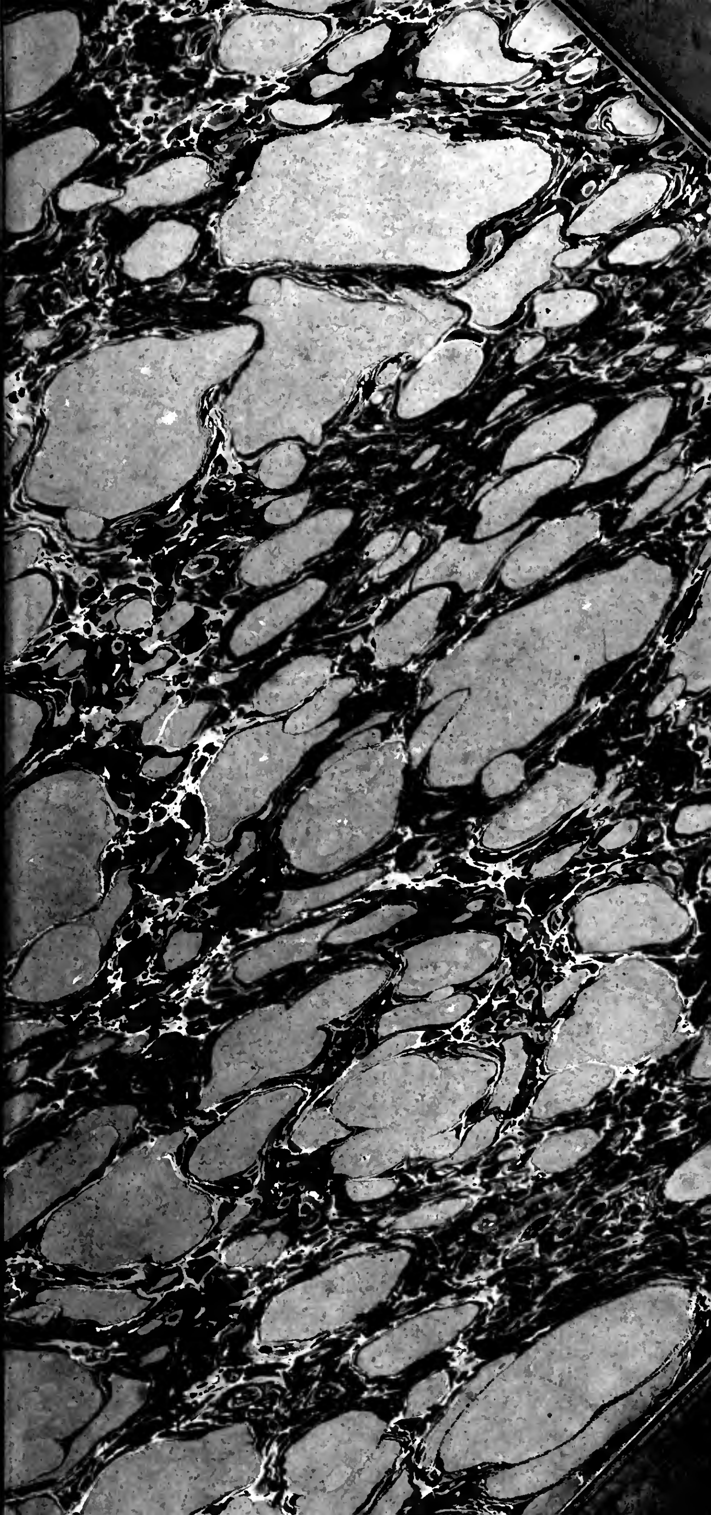

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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NUMBER CLXXVI.—SUPPLEMENT TO VOL. XXVI.

PLATE

Illustrative of Prof. Forbes's and Prof. Gordon's Communications on the Motion of Flowing Pitch, as confirming the Viscous Theory of Glaciers.

Errata and Addenda.

Page 15, line 2, *for* $\omega \sin \alpha$ *read* $\omega (\sin \alpha - \sin \beta) = \omega \sin \alpha$.
... 378, Figure 5. is printed in an inverted position.
... 384, par. 3, line 15, before "*then*" add, "and multiplying by α the term from which α disappears and all the terms which follow that one,"
... 451, line 7, *for* ammonium *read* aluminum.
... 456, at the end of the article of intelligence on the discovery of Mastodon Bones in New Jersey, beginning at p. 453, add, *Proceedings of the American Philosophical Society*, vol. iv. p. 118.

THE
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JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1845.

I. *On the Motion of Glaciers.*

By W. Hopkins, *Esq., M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE interest which the phænomena of glaciers has recently excited induces me to offer for insertion in your Journal, a somewhat detailed discussion of the causes of glacial motion. In doing this I shall confine myself to the *sliding theory*, with which we usually associate the name of De Saussure, and to that which has recently been propounded by Prof. Forbes, and which attributes the motion of glaciers to the *viscosity* of glacial ice. I shall omit all consideration of those theories which refer the motion in question to the freezing and consequent expansion of water contained within the mass of the glacier, because I believe there are at present in this country few persons who have sufficient confidence in those theories to feel much interest in the discussion of them.

Prof. Forbes's theory has been some time before the public in his excellent work on the Alpine regions, and in various letters addressed to Prof. Jameson and others; and in the Transactions of the Philosophical Society of Cambridge there are two memoirs of mine, the object of which is to explain the nature and causes of the motion of glaciers according to the sliding theory. In my present communication I propose to embody the more important parts of these memoirs, and to add to them some further investigations, which render the analysis of the problem more complete. I shall also offer some critical remarks on the theoretical views of Prof. Forbes. In certain stages in the progress of any branch of science the discussions connected with it will necessarily assume more or less of a controversial character. Such is the case in glacial theories at the present time. In the memoirs, however, just referred to, I have endeavoured to avoid, as far as possible, mere controversy and criticism, as inconsistent with the legitimate objects of a scientific memoir. I would still rather have preserved the same line in the further discussion of the subject, but when Prof. Forbes, at the conclusion of his Eighth Letter on Glaciers, intimates that I have adopted views opposed to his, without having carefully studied his writings; and that, if I had so studied them, I should not have advanced objections against the viscous theory "very easily refuted," I feel myself called upon to show that I have not opposed his theory without a careful study of it, and to endeavour to show that my objections to it are not of easy refutation. For these purposes I shall in this communication give an explicit statement of my own views, combined with a free, and, I trust, a candid criticism on those of Prof. Forbes. I proceed in the first place to the consideration of the sliding theory, to which this first part of my communication will be restricted.

The Sliding Theory.—One of the first consequences of the recent researches in glacial phænomena has been to cast great doubt on the adequacy of De Saussure's theory to account for the motion of glaciers. The inclination of the surface over which some of the Alpine glaciers move is found to be so small as to render it apparently inconceivable that such glaciers should not only descend, but overcome powerful obstacles to their descent, if there were no other moving force than that of gravity. The mean inclination of the surface of the Aar glacier is stated not to exceed 3° (and that of its bed must be still less), an inclination much smaller than that at which a very smooth hard body will descend down an equally smooth and hard plane*. Nor is the difficulty diminished by the consideration of the great weight of the moving mass, or of the extent of its surface in contact with that over which it moves; for, according to the observed laws of sliding bodies, the mo-

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* The following results are given by Prof. Whewell in his Mechanics of Engineering, on the authority of Morin. If θ be the angle of the plane down which sliding will just take place, and μ the coefficient of friction, we have for

	Values of μ.	Values of θ.
Hard limestone on hard limestone	·38	20° 50′
Brass on brass	·20	11 20
Brass on iron	·16	9 5
Cast iron on cast iron	·15	8 30
Cast iron on cast iron, greased	·10	5 45
Brass on iron, greased	·08	4 35

tion is independent of both these circumstances. This difficulty has been hitherto regarded, and with reason, as a most serious if not an insuperable one to the sliding theory. Another has also been frequently urged, for which, however, there is no real foundation. It has been contended that if a glacier moved by sliding over its bed from the mere action of gravity, it ought to move with an *accelerated* motion, whereas the motion is observed to be *unaccelerated**. If the force retarding the motion were solely that of ordinary *friction* of the surface over which it moves, the objection would be valid, because the retarding force of friction is *independent of the velocity acquired*; but in the case of a glacier moving down an irregular valley and over an irregular surface, all the retarding forces do not act on the mass in the same manner as friction in the ordinary cases of sliding bodies. Besides the friction, there may be retarding forces acting along the sides or bottom of the glacial valley, and depending on the velocity of the glacier, in which case the whole accelerating force on the mass will be some function of the velocity, and the motion will not necessarily be an accelerated motion†. The difficulty now spoken of, therefore, seems to have arisen from an imperfect conception of the problem; but the one first mentioned requires to be removed, if possible, by direct experiment. This I have attempted to do in the manner which I shall now describe.

1. *Experiments.*—A slab of sandstone was so arranged that the inclination of its surface to the horizon could be slowly and continuously varied by the elevation of one edge. The surface was in the state in which it had been sent from the quarry, and in which such stones are sometimes laid down as paving stones, retaining the marks of the pick with which the quarryman has shaped them, without any subsequent process for rendering the surface smooth. The slab thus presented a grooved surface (the grooves running in very nearly parallel directions), having some resemblance to those over which existing glaciers move, but having little of the smoothness of *roches polies*. The

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* "Of this I was persuaded, that if the motion should appear to be continuous and *nearly* uniform, it could not be due to the mere sliding of the glacier on its bed, as De Saussure supposed; for, admitting the possibility of gravity to overcome such intense friction as the bed of a glacier presents, it seemed to me quite inconsistent with all mechanical experience that such a motion, unless so rapid as to be an accelerated one, and that the glacier should slide before our eyes out of its hollow bed (which would be an avalanche), could take place except discontinuously, and by fits and starts."—*Travels through the Alps of Savoy*, p. 132.

† The descent of water along a river-course, or of ice floating down its current, is not necessarily with an *accelerated* motion, and for a reason exactly similar to that assigned in the text.

best measure, however, of the degree of its roughness is this —when placed at an inclination of about 20°, a piece of polished marble would just rest upon it.

The slab was so placed that the direction of the grooves coincided with that of greatest inclination. A frame of about 9 inches square and 6 inches in depth, without top or bottom, was then placed on the slab and filled with lumps of ice from a neighbouring ice-house, in such a manner that the ice, and not the frame (which merely served to keep the ice together as one mass), was in contact with the slab. In the experiments in which the following results were obtained, weights were placed on the ice such that the pressure on the slab was at the rate of about 150 lbs on the square foot.

Inclination of the slab.	Spaces in decimals of an inch through which the loaded ice descended in successive intervals of 10 minutes.	Mean space in inches for 1 hour.
3°	·08 ·05 ·07 ·03 ·04 ·05 ·07 ·06 ·04	·31
6	·09 ·10 ·09 ·07 ·08	·52
9	·14 ·12 ·17 ·14 ·19 ·20	·96
12	·38 ·34 ·36 ·27	2·0
15	·43 ·41	2·5
20	The mass descended with an *accelerated motion.*	

When the inclination was 9° about two-thirds of the weight was removed; the velocity was diminished by nearly one half.

When the inclination of the slab did not exceed 1°, there was a small but very appreciable motion.

On the surface of a slab of the same kind of stone *smooth* but not *polished*, there was appreciable motion at an angle of forty minutes. Nor am I prepared to say that either in this, or the preceding case, the angle was the least at which sensible motion would take place.

When the surface used was that of *polished marble*, there was sensible motion with the smallest possible inclination. The motion, in fact, afforded almost as sensitive a test of deviation from horizontality as the spirit-level itself.

In all these experiments the ice melted continually, but very slowly, at its lower surface in immediate contact with the slab. During the night the temperature descended below that of freezing, and the motion entirely ceased.

The angle at which the accelerated motion just begins to take place is that at which the ice would just rest upon the inclined plane, if the temperature of the slab and of the air were at or below the freezing temperature, so that no disintegration of the ice should take place. This angle appears to be nearly the same in the case of ice, on the grooved slab I made use of, as for that in which polished marble was the sliding body, and is that whose tangent determines the coefficient of friction between the slab in question and *solid* ice. When the slab was of polished marble this angle was very small.

2. In the experiments above detailed we have these results:—

(1.) For all angles less than that just mentioned the motion was *not an accelerated motion.* This result was verified in every experiment I made.

(2.) For inclinations not exceeding 9° or 10°, the velocity, *cæteris paribus*, was approximately *proportional to the inclination.* This, I doubt not, would hold in all cases in which the inclinations should be sufficiently small compared with the angle of accelerated motion. It is manifestly equivalent to the assertion, that the velocity is proportional to the moving force.

(3.) The velocity of the mass was increased by an increase of weight.

(4.) The motion was due to the circumstance of the lower surface of the ice being in a state of constant, though slow disintegration.

The fact that motion takes place down planes of such small inclination compared with that necessary to make the ice slide independently of its disintegration at the lower surface, may simply be stated as due to this circumstance—that, whereas the particles of ice in contact with the plane are capable, so long as they remain a part of the *solid* mass, of exerting a considerable force to prevent sliding, they are incapable of exerting any sensible force when they become detached from the mass by the liquefaction or disintegration of its lower surface.

When the sliding mass is small (as in the experiments above described) the exact uniformity of the motion will be destroyed by local irregularities in different parts of the inclined plane down which it takes place, or temporary irregularities in the disintegration; but where the whole inclined surface on which the motion takes place is always the same (as in the case of a glacier), and the mass is sufficiently large, all local or temporary irregularities will, in a great measure, counteract each other, and will therefore not materially disturb the uniformity of the motion, which will be preserved so long as the intensity of the causes of disintegration remains unaltered.

I have stated above that there was very appreciable motion on a smooth unpolished slab, when the inclination did not exceed 40′, and that I have no reason to suppose that a similar result would not be obtained at still smaller angles. Perhaps one of the best tests of the correctness of this conclusion is afforded by the second of the laws of the observed motion, viz. that the velocity, at small inclinations, is nearly proportional to the inclination. If the law were accurately true, there would be appreciable motion for the smallest appreciable inclination.

3. By means of this law we easily establish the relation between the intensity of any additional retarding force and the retardation produced by it. Let $f=$ the retarding force, $v=$ the velocity of descent when that force is applied, V being the velocity when the mass descends by its own weight. The retardation will be $\mathrm{V}-v$. Also let $\mathrm{W}=$ the weight of the mass, and α the inclination of the plane. Then, if f act in a direction parallel to the plane, the moving force will $=\mathrm{W}\sin\alpha-f$, and we shall have, by the law referred to,

$$\frac{v}{\mathrm{V}}=\frac{\mathrm{W}\sin\alpha-f}{\mathrm{W}\sin\alpha};$$

and therefore the retardation, which

$$=\mathrm{V}-v=\frac{f}{\mathrm{W}\sin\alpha}\cdot\mathrm{V}.$$

In order that the whole velocity may be destroyed by the retarding force, that force must $=\mathrm{W}\sin\alpha$. This, in the case of a glacier, in which α may vary from 3° or 4° to 10° or 15°, becomes enormous.

4. It should be observed that the velocities ($\mathrm{V}\ v$) here spoken of, are the constant velocities of descent. In the experiments the motion appeared to begin with the uniform velocity with which the mass continued to descend; but this velocity is manifestly a *terminal velocity*, like that to which a body acted on by a constant force will rapidly approximate when moving in a resisting medium; and in the experiments the approximation to the terminal velocity must have been too rapid to admit of my observing, by the means I made use of, the variation of velocity in the first stage of the motion. The whole action of the plane retarding the motion must be some function of the velocity $\phi(u)$, so that the equation of motion will be

$$\frac{du}{dt}=g\sin\alpha-\phi(u),$$

where u denotes the velocity at any time t, before the mass has acquired the terminal velocity v; and since, in a small time, u becomes $=v$, $\phi(u)$ soon becomes $=g\sin\alpha$. Also, assuming the approximate truth of the second of the laws above stated, there will be motion for very small values of α, or when $g \sin \alpha$ is extremely small; consequently $\phi(u)$ will become nearly = zero when $u = 0$. Hence at the instant the motion begins from rest, the tangential action of the plane on the mass, or conversely that of the mass on the plane, is approximately $=0$; but in a very short time that action (estimated as a moving force) becomes = weight of the mass resolved in the direction of the plane. I have entered into this explanation to point out the distinction between the action of the retarding force in the motion we are considering, and that of *friction* in the common and established acceptation of the term.

5. *Temperature of the Lower Surface of a Glacier.*—The essential condition under which gravity becomes effective in putting the loaded ice in motion in the experiments above described, is that the lower surface of the ice shall be in a state of disintegration, or that its temperature shall be that of zero of the centigrade thermometer. In order, therefore, that our results may be applicable to any proposed glacier, we must show that the temperature of its lower surface must be zero. For this purpose let us conceive the earth to be covered with a superficial crust of ice. The temperature of the ice to a certain depth will be subject to sensible annual variations; at that depth the temperature will be constant, and at greater depths will increase. The rate of increase will depend on that which obtains within the earthy nucleus, the conductive power of ice, and the external temperature, supposing the thickness of the icy crust to be so small that the temperature shall not rise to zero before we reach the earthy nucleus. Assuming this condition to hold, we are able to determine the greatest thickness of the icy crust compatible with it. If the actual thickness of the crust be greater than that thus determined, it is manifest that the lower surface of the ice will be kept in a constant state of liquefaction by the flow of heat from the earthy nucleus.

The numerical determination of the greatest depth at which the ice could retain its perfect solidity at its lower surface depends partly, as just remarked, on the conductive power of ice, of which we have no exact knowledge. It is quite sufficient however for our purpose to know that it is small. I shall suppose it (for the greater simplicity of investigation) to be the same as that of the earthy matter supposed to constitute the nucleus of the sphere; and for the same reason I shall also suppose the conductive power from the nucleus to the icy envelope to be the same as in the interior of the nucleus, or in that of the icy crust. I shall also assume the *external* temperature to be represented by $V + C \cos\left(2\pi t + \frac{\pi}{2}\right)$. So long as this is less than zero, the problem will present no peculiarity arising from the circumstance of the exterior crust being composed of ice; but however greatly the external temperature may exceed zero, the *superficial* temperature of the crust cannot, from the nature of ice, rise higher than zero. Hence, while the external temperature is below zero, we shall have the ordinary case of a solid body placed in a medium of which the temperature varies according to a given law; but when the external temperature rises above zero, the condition at the surface will be that the *superficial* temperature of the mass shall be constantly at zero. Instead of this last condition, however, we may suppose that, during the time it would hold, the *external* temperature shall be zero; for it is manifest that the two conditions will in the case we are contemplating be very approximately the same. Hence, then, the case for investigation will be that of a sphere of large dimensions cooling in a medium of which the temperature is $V + C \cos\left(2\pi t + \frac{\pi}{2}\right)$ when this quantity is negative, and zero for those values of t which render the expression positive. If $V = 0$ the first of these conditions will be satisfied from $t = 0$ to $t = \frac{1}{2}$, from $t = 1$ to $t = \frac{3}{2}$, &c.; and the second from $t = \frac{1}{2}$ to $t = 1$, from $t = \frac{3}{2}$ to $t = 2$, &c. If V do not $= 0$, the former of these periods will be shortened and the latter lengthened, or the converse, according as V is positive or negative; if, however, V be small compared with C, the periods will be approximately as above stated, and such, therefore, we shall consider them. They will be semi-annual, if we take one year as the unit of time.

Let v denote the temperature which would exist at a point within the sphere at a depth x beneath its surface, if the external temperature were always equal zero. We shall have (x being small compared with the radius of the sphere)

$$v = v_0 + \gamma x,$$

where v_0 is the superficial temperature of the sphere, and γ the rate at which the temperature depending on the original heat of the sphere, increases with the depth. Assuming the time of cooling to have been very great (as in the case of the earth), v_0 will be extremely small.

Again, let u' denote that part of the internal temperature which depends on the external temperature. Then, if u denote the whole internal temperature at the depth x, we have

$$\begin{aligned} u &= v + u' \\ &= v_0 + \gamma x + u'. \end{aligned}$$

The theorems given by Poisson in his *Théorie de la Chaleur*, articles 194, 195 and 196, will enable us to obtain the expression for u'. For the investigation I must refer to my first memoir on glaciers.

We obtain

$$\left.\begin{aligned} u = {} & \frac{V}{2} - \frac{C}{\pi} + v_0 + \gamma x \\ & + \frac{b}{D}\left(\frac{2V}{\pi} - \frac{C}{2}\right) \varepsilon^{-\frac{x}{a}\sqrt{\pi}} \cos\left(2\ t + \frac{\pi}{2} - \frac{x}{a}\sqrt{\pi} - \delta\right) \\ & + \frac{b}{D_1} \cdot \frac{C}{1\,.\,3\,.\,\pi} \varepsilon^{-\frac{x}{a}\sqrt{2\pi}} \cos\left(4\pi t - \frac{x}{a}\sqrt{2\pi} - \delta_1\right) \\ & + \&c. \end{aligned}\right\} (1.)$$

I am not aware of any experiments for the determination of a and b for ice. Poisson has given their values for the case of the earth, deduced from observations made at Paris on the annual variations of temperature at different depths. They are

$$\left.\begin{aligned} a &= 5{\cdot}11655 \\ b &= 1{\cdot}05719 \end{aligned}\right\} \text{ in metres.}$$

He also gives

$$\left.\begin{aligned} v_0 &= 0^\circ{\cdot}0265 \\ \gamma &= 0^\circ{\cdot}0281 \end{aligned}\right\} \text{ (centigrade).}$$

D, D_1, &c. are constants, such that with the above values of a and b, we have

$$\frac{b}{D} = {\cdot}7 \text{ nearly,}$$

$$\frac{b}{D_1} = {\cdot}63\ldots\ldots$$

$$\&c. = \&c.$$

A year is taken for the unit of time.

In this investigation the sphere has been supposed to have a complete shell of ice. The result will also be sensibly the same, if, instead of the whole surface of the sphere being covered with ice, a small portion only of it be so covered, provided the thickness of the ice be small compared with its superficial dimensions. This is the actual case of a glacier, to which therefore equation (1.) will be approximately applicable. Let us proceed then to the interpretation of that equation.

We observe that when x = a few multiples of a, the value of the periodical terms becomes insensible, on account of the exponential involved in them. Let x_1 be the least value of x for which we may neglect these terms. Then, if u_1 be the temperature at that depth,

$$\left.\begin{aligned} u_1 &= \frac{V}{2} - \frac{C}{\pi} + v_0 + \gamma x_1 \\ &= \frac{V}{2} - \frac{C}{\pi} + \gamma x_1 \end{aligned}\right\}, \quad . \; . \; . \; . \; . \quad (2.)$$

neglecting the small quantity v_0. Consequently the temperature at a certain depth is independent of annual variations, and lower by $\frac{C}{\pi} + \frac{V}{2}$ than it would be if the exterior shell were composed of rock instead of ice; for in that case the value of the constant term would be the mean external temperature V, instead of $\frac{V}{2} - \frac{C}{\pi}$.

If x_2 be the depth for which the temperature = 0, we shall have

$$\left.\begin{aligned} 0 &= \frac{V}{2} - \frac{C}{\pi} + \gamma x_2, \\ \therefore x_2 &= \frac{1}{\gamma}\left(\frac{C}{\pi} - \frac{V}{2}\right), \end{aligned}\right\} \quad . \; . \; . \; . \; . \quad (3.)$$

which, if we give to γ the value above stated, will be the numerical value of x_2 in metres.

If x_2 be less than the thickness of the glacier, the formula (1.), and therefore (3.), will be no longer applicable; for (1.) would give the temperature of the ice at depths greater than x_2, higher than zero, which, from the nature of the ice, is impossible. In such cases, the lower surface of the ice, at whatever depth it might be, would be necessarily at zero, because the heat which, if the superficial crust were not ice, would elevate its temperature, will be employed in melting the ice at its lower surface, which will thus be kept at the zero temperature.

With the value of γ above given, equation (3.) gives the value of x_2, supposing the ratio of the conductive power of ice to its specific heat to be the same as for the rocky crust of the earth. If this be not the case, the equation (3.) will still give the depth at which the temperature = zero, by assigning the proper value to γ as depending on the ratio just mentioned for ice.

As a numerical example, suppose V=0, and C=15° (cent.). We shall have at the depth x_1

$$u_1 = -5^{\circ} \text{ nearly};$$

and $$x_2 = \frac{5}{\cdot 028} \text{ feet,}$$

$$= 178 \text{ feet nearly.}$$

The temperature -5° (cent.) appears, however, to be much lower than that observed at different depths by M. Agassiz, and which did not exceed half a degree. The difference may, I conceive, be easily accounted for. In our investigation the surface of the glacier has been supposed to be exposed to the winter temperature, whereas, as soon probably as the mean temperature of the twenty-four hours descends to zero, the surface is protected from the external cold by a coating of snow, which increases as the temperature diminishes, and thus it is probable that the temperature of the surface of the *ice* may descend but little below zero during the whole winter. If we suppose its lowest temperature to be about $-1^\circ{\cdot}5$ (cent.), we shall have $u_1 = -0^\circ{\cdot}5$, and $x_2 = 54$ feet nearly. If the conductive power of ice be less than that of common rock, the value of x_2 will be proportionally less.

Taking this last value of x_2, it follows that if the thickness of a glacier should exceed fifty or sixty feet, the temperature of its lower surface would necessarily be zero, as already explained. Now the thickness of glaciers is doubtless much greater in general than fifty or sixty feet, and therefore we conclude, *that generally the temperature of the lower surface of a glacier cannot be less than zero, and must, consequently, be in a state of constant disintegration, unless the conductive power of glacial ice be much greater than that of the ordinary matter forming the crust of the globe.*

6. *Agency of Subglacial Currents.*—The internal heat of the earth, however, is not the only cause producing this constant disintegration. Another and probably very effective agency exists in the subglacial currents, which, during the summer, are principally produced by the rapid melting of the ice at the upper surface of the glacier, whence they descend through open fissures, and afterwards force their way between the glacier and the bed on which it rests. I cannot appeal to any direct experiments to determine the effect of water at the temperature of zero in dissolving ice at the same temperature, when running in contact with its surface, but its efficiency in this respect is sufficiently proved by its action on the upper surface of a glacier when the direct rays of the sun and the temperature of the atmosphere are sufficient to dissolve the superficial ice, and thus to create innumerable rivulets running upon the surface till they meet with a fissure into which the water is precipitated, and finds its way to the bed of the glacier. These little superficial streams show their effect in disintegra-

ting the ice by the manner in which they cut out for themselves their own channels, thus assisting greatly in the degradation of the surface. Its effect on the lower surface of the glacier is probably greater than on the upper, on account of the hydrostatic pressure under which it must there act. The descending water must reach the bed of the glacier at almost every point of it, and cannot afterwards collect and proceed in uninterrupted channels, because if such channels were once formed, they must necessarily be immediately destroyed, or at least impeded at numerous points by the motion of the glacier. The existence of such impediments to the motion of the water, and the consequent formation of subglacial reservoirs, is proved by the continued flow of the streams which issue from the lower extremities of glaciers during the night, though the supply from the upper surface is entirely stopped immediately after sunset, when the melting ceases, and does not recommence till a considerable time after sunrise the next morning. During the intervening ten or twelve hours the whole of the water beneath the glacier at sunset would necessarily discharge itself if its course were unimpeded, even from the longest and least inclined of the alpine glaciers, before sunrise the next morning; whereas the volume of water issuing from the glacier of the Aar is very little less in the morning than in the evening. This equable supply can only arise from the discharge during the night from reservoirs formed during the day. Hence it will follow, that these subglacial currents, commencing from almost every point of the glacier, will be forced under every part of it by hydrostatic pressure, by which, as above asserted, its disintegrating action on the lower surface of the ice will doubtless be increased.

M. Agassiz appears to entertain the opinion, that glaciers, throughout the greater part of their length, are firmly frozen to their bed, and, consequently, that all sliding, except at their lower extremities, is out of the question; and others, I believe, have also held the same opinion. The preceding investigation, however, leaves no doubt, I conceive, that such cannot possibly be the state of the inferior surfaces of glaciers of considerable thickness.

7. M. de Charpentier has insisted on the small inclinations of the beds of many glaciers as a conclusive objection against the sliding theory, and Prof. Forbes has spoken of the "intense friction" exerted by the bed of a glacier*, and of the impossibility of its sliding uniformly, if it should slide at all. These objections have arisen from an imperfect conception of the real action of the bed of a glacier on the ice in contact with it in a state of disintegration, assuming the general sur-

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* See note *, p. 3.

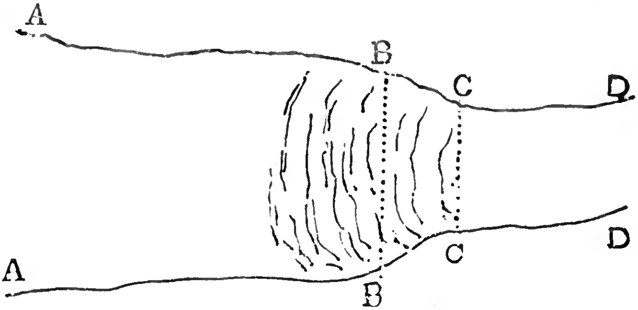
face of the bed to be *smooth*, in the sense in which the term may be applied to a slab rough-hewn for a pavement. I have already explained (art. 4.) that this action is extremely small when the velocity = 0, but increases rapidly with the velocity till it becomes equal to the moving force of gravity along the inclined plane; and thus it is that while the action of the bed cannot prevent motion altogether, it never allows the velocity to exceed a very small value. If, however, the bed of the glacier should present sharp asperities of surface, such as would penetrate into the solid mass, the action of these penetrating points would no longer be like that here contemplated, and might be much greater than the action of a more even surface. Its effectiveness in arresting the motion would depend almost entirely on the weight of the mass and inclination of the plane. If the weight were small, as in an experiment, comparatively few projecting points, penetrating but little into the mass, might be sufficient to destroy the motion, while, if the mass were increased so as to bear any analogy in this respect to the case of a glacier, the retarding effect might be entirely inappreciable. In estimating the effect of local obstacles, acting on the sides or bottom of a glacier, to destroy its motion, the enormous weight of the glacier must always be borne in mind.

With respect to the bottoms of glacial valleys, however, I conceive it to be impossible that they can present those local asperities of surface which, if they existed generally, would be most likely to prevent the sliding motion of a glacier; for how could the hardest rocks resist for thousands of years the unceasing efforts of infinitely the most powerful polisher that nature has put in action? If it be said that this argument assumes the point at issue—the sliding of glaciers—I reply that, of their sliding more or less, we have the most incontrovertible evidence, as every one must allow who has examined the *roches polies* at the end of a glacier, or along the continuations of existing glacial valleys. Nor must we limit the grinding and polishing power of former glaciers by that of existing ones, for the glaciers of the Alps must, at some former epoch, have been, probably, three or four times as thick as they are at present, varying perhaps from 1000 to 3000 feet for great portions of their length. I venture therefore to assert, without fear of contradiction, that the surfaces of the rocks forming the beds of glaciers must necessarily be free from such asperities of surface as are here contemplated as most likely to impede the sliding of a glacier.

Again, we may observe that it is not necessary, according to this view of the subject, that the surface of a glacial valley should either be a plane surface, or should consist of a series of plane surfaces. If a chain of given length be placed along the line of greatest inclination on an inclined plane, the force exerted to move along the plane depends on the height of its upper above its lower extremity; and the same is true if the chain, instead of being laid in a straight line, be placed on an undulating plane curve lying in a vertical plane. Similarly, if the bed of a glacier be an *undulating* surface, the glacier will descend along it with very nearly the same facility as if it were plane.

If, then, a glacier be entirely arrested in its descent, it must be by *local obstacles*, which exert retarding forces on the mass at comparatively few points, and not, as in the case of ordinary friction, at every point of the sliding surface. Consequently the pressure thrown on these local obstacles might become enormous, and much greater than glacial ice could bear without being rent and broken, especially at the bottom of the mass, where the texture of the ice is probably affected by its temperature approximating so near to zero, and from its being saturated with water at that temperature.

It is not to be doubted that the bottom of a glacial valley must be far more free from those abrupt inequalities which would most interfere with the sliding of the glacier than the sides of the valley, where the grinding action of the glacier must be much less than on the bottom, and where there are causes constantly tending, as in the sides of other valleys, to produce irregularities faster in many cases than they can be smoothed down by the glacier. A sudden contraction in the width of a glacial valley, like that represented in the annexed diagram at C and C′, forms probably the most serious obstacle which a glacier has to encounter. In explaining how it may be surmounted, let us suppose that the transverse portion B C C′ B′ is neither pulled onwards by the lower part C D D′ C′ of the glacier, nor pushed forward by the upper part A B B′ A′, so that B C C′ B′ is left to descend by its own weight along the inclined plane which forms its bed. Let the inclination of this plane be α; then if β be the least inclination of the plane



down which the mass would descend if its motion were unimpeded by any lateral obstacle, we shall have (art. 3.) $\omega \sin \alpha$ nearly, as the force which this portion alone of the glacier is capable of exerting to overcome any obstacles to its motion. Now it is well known that the flanks of almost every glacier are much *crevassed* and dislocated, especially where there is any abrupt obstacle to the motion as above represented; and this being the case, there is no reason to suppose that the ice, of which the motion must be arrested in the corners at C and C′, should present a greater impediment to the motion of the central portion of the glacier than the rocky sides of the valley present in other places. B C C′ B′ will no more be able to support itself at rest in the position above represented, than a beam is able to support itself in a horizontal position resting on its extremities, when the parts near the extremities are fissured and broken.

If B C C′ B′ would advance by its weight, it must, *à fortiori*, advance when acted on by the mass A B B′ A′, which, if the former portion were impeded, would exert an additional force to urge it onwards.

If the impediment should offer itself in the form of a hill rising suddenly from the centre of the valley, the motion through the valleys on either side of the hill would admit of the same explanation. Whenever a glacier meets with an obstacle to its motion, its state of dislocation immediately shows itself.

This explanation was given in my second memoir, and its importance insisted on in opposition to the opinion that the power of a glacier to move through a comparatively narrow gorge is derived from the great plasticity of glacial ice. For the purpose of proving this explanation incorrect, Prof. Forbes recently made some observations on the Mer de Glace, by which he showed that a line originally straight on the surface of the glacier gradually assumed the form of a curve of continuous curvature, proving thereby that the parts of the glacier did not slide past each other as supposed in the preceding explanation. He found with some difficulty (as he has stated) a portion of the glacier near its side uninterrupted by fissures. It was doubtless subjected to more than ordinary pressure, by which the different portions into which it must have been previously divided by *crevasses*, were closely packed together, and constrained to move as a continuous mass. Now, under such circumstances, I should contend that glacial ice has probably a very considerably greater degree of *flexibility* than his observations prove it to have, and therefore these observations were altogether unnecessary for the purpose of refuting my opinions. To render them a test of the truth of the explanation above given, they should have been made in those places where *the glacier is most dislocated*, and not where it is most compact, because it is in those places that I conceive the sliding of one part past another to take place. That such is the case in many places is as unquestionable as the continuity of the motion observed by Prof. Forbes. It must not be supposed, however, that I reject the notion of ice having a certain degree of *plasticity* and *flexibility**. It would be absurd for any one to do so who has witnessed how the glacier de l'Echaud and the glacier du Tacul form, by their union, the Mer de Glace, and the glaciers of Finsteraar and Lauteraar form that of the Lower Aar. But I believe that these properties are only developed in glacial ice to any considerable extent, by the enormous forces to which certain portions of a glacier are subjected; and I contend that this plasticity is not such that the mechanism of glacial motion can be correctly represented by that of a semifluid or viscous substance. But I must reserve the subject of the mechanism of glacial motion for my next communication.

Your obedient Servant,

Cambridge, November 19, 1844. W. HOPKINS.

II. *Report from* Messrs. FARADAY *and* LYELL *to the* Rt. Hon. Sir James Graham, *Bart., Secretary of State for the Home Department, on the subject of the Explosion at the Haswell Collieries, and on the means of preventing similar accidents*†.

To the Right Honourable Sir James Graham, Bart., &c. &c.

SIR,

HAVING, in our former letter, expressed our entire concurrence in the verdict of the jury, which exonerated the proprietors of the Haswell Colliery from any blame in connexion with the late fatal accident (September 28, 1844), we

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* The terms *plastic, flexible, viscous, semifluid,* &c., appear to have been used by Prof. Forbes too indiscriminately, as if they were convertible terms, which is very far from being the case. The application of the two latter terms to a hard mass, like glacial ice, capable of supporting itself when bounded by a free vertical section of a height at least a hundred, and perhaps many hundred feet, I cannot but regard as a departure from all propriety of established language.

† [To this Report, which has been published by the Home Office with illustrative plans, and which has been obligingly communicated to us by the Authors, the following notice is prefixed:—

"Home Office, Nov. 13th, 1844.

"On the occasion of the late fatal Explosion in the Colliery at Haswell, Messrs. Lyell and Faraday were appointed by Sir James Graham to attend at the Coroner's Inquest, for the purpose of affording any information in their power, and for reporting also whether any means could be suggested for preventing similar accidents. The following Report has been received from those Gentlemen."]

now proceed to consider how the recurrence of similar catastrophes may be obviated in future.

The Haswell Collieries, where the explosion occurred, are situated about seven miles east of Durham, in that part of the Durham coal field which is overlaid by the magnesian limestone, and about two miles within the outer limit or escarpment of that formation. In sinking the main shaft, which is 155 fathoms deep, they passed through, below the outset or artificial elevation, first, 18 feet of soil, gravel, &c., then 363 feet of limestone, red shale, and sandstone of the magnesian limestone formation, and afterwards through 540 feet of the coal measures; the strata of both formations being so nearly horizontal, that they may be considered as being here in parallel or conformable position. The accompanying section [as annexed to the original report] of the beds passed through in excavating the shaft, will show that no less than ten seams of coal were met with, varying in thickness from 1 inch to 3 feet 7 inches; and that at a depth of 925 feet from the surface, the coal called the Hutton seam was gained, which is 5 feet 5 inches thick, comprising, first, the top coal of superior quality, 4 feet 1 inch thick; secondly, layers of impure coal, 16 inches thick, the upper portion of which, called the brassy coal, is much charged with pyrites; and the lowest part, of better quality, is said to give out much more gas than the top coal. The dip of the beds is about 1 foot in 24 to the S.E.*

In the Great Pit, which lies to the S.W. of the Little Pit, and is ventilated by the same shafts, a large dike of trap or greenstone was encountered, which had turned the coal into coke, with numerous veins of calcareous spar, for a distance of about 40 yards from the point of contact. Notwithstanding the intrusion of this igneous rock, the strata in general are remarkably undisturbed. In the Little Pit, which we examined carefully, only two or three slips of a few inches, and one fault of two or three feet, appeared. The roof near the entrance shaft was of white sandstone, with occasional seams of mica, but throughout the greater part of the workings, which are about 250 acres in extent, the roof or ceiling is composed of shale, very unbroken and secure, and having given rise to extremely few accidents, by falling in, in the course of the thir-

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* As no part of the section obtained in sinking the shaft of the Haswell Pit was exposed to view at the time of our visit, except the lowest ten or twelve feet, the division into coal measures and magnesian limestone, and of the latter into upper magnesian and lower red sandstone, with about twenty feet of superficial gravel and clay, has been inferred from the description of the beds given in the miner's section. We have substituted geological names for the miners' terms, as far as we were able to do so.

teen years during which the pit has been opened. The chief danger to be guarded against, arises from what are termed "caldron bottoms," which are the stools or lowest portions of erect fossil trees, filled with sandstone or shale, and having their bark converted into coal, which gives way when they are undermined, and allows the heavy cast of the interior of the trunk, several feet or yards in height, to descend suddenly. The Hutton seam rests on a sandy clay, which has not been found adapted for a fire clay. The floor composed of it does not often rise in creeps.

It will be seen by the section, that several upper seams of good coal, the united thickness of four of which is no less than 13 feet, have been left untouched, in order that the thicker and more valuable seam called the Hutton, should first be worked out. In adopting this plan, the proprietors have been guided by considerations of present profit, which the competition of other neighbouring coal works renders indispensable. Nevertheless, it may not be improper for us to advert to two evils which result from this system.

First. The upper seams which are undermined on the extraction of an inferior bed of coal, sink down, often unequally, from failure of support, and become fractured, and therefore much more liable to give out gas, which gas (it is well known by experience) has sometimes found its way into the workings far below, as attested by Mr. Buddle in his evidence given to the Committee of the House of Commons in 1835; by which some of the most serious accidents from fire-damp have been occasioned. The greater the number, and the larger the size of the upper seams, and the nearer they lie to the seam which is worked at a lower level, the greater the risk of such communication by fissures.

Secondly. The higher beds of coal, which might have been worked to advantage before they were undermined, and when the expense of a shaft had been already incurred, may never be available after the workings have been for many years abandoned, and the shaft partially filled up, and after the coal has been injured by the continued permeation of water and gas through its fissures, whereby property of great value may be irrecoverably lost to the country. With a view to prevent this prospective evil in the mines belonging to the Duchy of Cornwall in Somersetshire, it has recently been proposed to make provisions in the new leases to secure the more regular extraction of all the workable seams which, exclusive of the great seam, range from 14 inches to 2 feet in thickness, the whole of them being less considerable than five of the seams neglected in sinking the Haswell shaft.

Before going into a particular consideration of the causes of the late accident at Haswell, and the possible means of preventing the recurrence of the like in future, we wish to point out the fact well known to the viewers in this district, that the pits on the north side of the Wear, in which the Hutton seam is worked, are more infested with fire-damp than those on the south side of that river. If, therefore, at Haswell Colliery, situated among the latter, the danger has proved to be so great, still more necessary will it be to endeavour to take additional precautions elsewhere.

There can be no doubt, that as regards the safety of the men in coal mines from injury consequent upon fire-damps, ventilation is of the utmost importance, but there is a practical limit beyond which it cannot be carried, for in works deep and extensive, as the coal mines often are, to dig shaft after shaft would quickly involve an expense more than the value of the produce of the mine, and have the effect of closing it altogether. There is one point in ventilation, however, which, at the same time that it appears to us capable of improvement, touches a part of the mine of the utmost consequence to the safety of the whole; and though our observations and thoughts are not founded upon long experience, or the examination personally of many mines, yet considering that the one we have so recently been called to observe is as simple in the character of its workings, contains as small a proportion of fire-damp, and is as well-ventilated as any in that part of England, surpassing in these respects most of the mines, we do not think that they will be exaggerated in respect of, or less applicable to, other cases. We allude to the ventilation and general character of the *goaf*.

The *goaf* is a mass of ruins in the middle of the works, growing from day to day, as the workings of the mine extend. The miner, as he works in the undisturbed coal (which is called the *whole*), removes it, so as to form passages which are usually parallel to, and at right angles with, each other; the square portions of coal left between them are called *pillars*; the passages or ways are, upon the average, about 5 yards wide, and the pillars are about 16 yards by 22 yards; the pillars, whilst they remain, support the roof, and the superincumbent rocks and strata; this part of the works is called the *broken*.

Afterwards the coal of the pillars has to be removed, and as it is carried off, the roof so exposed is supported by many wooden props; this state of things constitutes a *jud*: at last these props are withdrawn, and this is called *drawing a jud*; during or after which the roof falls in masses, larger or smaller according to circumstances, a fall being sometimes many tons in weight. The pillars are not removed indifferently, but those next the mass of ruins already formed are taken away first, so that the first *jud* which is drawn produces a heap of broken strata, and this increasing with every succeeding jud that is removed, forms the *goaf.*

These goafs grow to a great size*. There are three in the Haswell Little Pit; two are small as yet; the largest has an extent of 13 acres. At the edges they are very loose and open, having accidental cavities and passages for air running into them, as might be expected from the falling of rocks from a height of five feet one upon another. There is every reason to believe, that the falling goes on towards the middle of the goaf, but how high the heap of broken strata and the vault inclosing it extend, is not known in a large goaf, or, as far as we are aware, even in a small one. The goaf may be considered as a heap of rocky fragments rising up into the vault or cavity from which it has fallen, perhaps nearly compact in the parts which are the oldest, lowest, and nearest the middle, but open in structure towards and near its surface, whether at the centre of the goaf or at the edges; and the vault or concavity of the goaf may be considered as an inverted basin, having its edge coincident with the roof of the mine, all round the *goaf.* Within this basin there must be air space (as long as the surface of the country above has not sunk), either in the space between it and the goaf, or in the cavities of the goaf itself, nearly equal to the bulk of coal removed; this in a goaf of 13 acres, and a seam in which 5½ feet of coal, including the top and bottom, are taken away, is equal to a vault 5½ feet high and 13 acres in extent.

Let us now consider this goaf as a receptacle for gas or fire-damp, a compound of hydrogen and carbon, known as light hydro-carbonate, and by other names. The weight of pure fire-damp is little more than half that of air; it gradually and spontaneously mixes with air, and the weight of any mixture is proportionate to the quantities of air and fire-damp. Any gas that may be evolved in the goaf, or that may gradually creep into it along the roof of the workings, against which it will naturally flow, will ascend into the goaf vault, and will

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* Goafs vary in size: that at the Meadows Flat Little Pit is 13 acres; the goaf of the High Brockley Whin is 1¾ acre, and the one at the Low Brockley Whin 1½ acre. In the North Way of the Little Pit at Haswell, there is a goaf of 35 acres, and in the Engine or Great Pit, one of 17 acres. Perhaps the greatest goaf is that at Felling, near Newcastle-upon-Tyne; it is in the same seam as the Haswell, and has an extent of upwards of 100 acres.

find its place higher, in proportion to its freedom from air; and this will go on continually, the goaf vault forming the natural basin into which all gas will drain (upwards), from parts inclining to the goaf, just as a concavity on the side of a gentle hill will receive water draining downwards from its sides, and from the parts above inclining towards it.

The gas which may thus be expected to collect in the goaf of a mine, where there is any fire-damp at all, cannot escape at the top of the goaf vault; instead of passing away there, the whole surface of the vault may rather be viewed as having a tendency, more or less, to evolve gas from the upper broken and bruised coal measures (often containing small seams not worked) into the space beneath; and the only escape for the gas is by the flowing of it under the edge of the goaf vault into the workings of the mine. Two main circumstances tend to this effect; the one, the continued accumulation of gas in the upper part of the goaf vault; the other, the continual tendency to mix with the air beneath, and consequent formation of mixtures larger and heavier than the gas itself. As Sir Humphry Davy has stated, any mixture containing from one-fifth to one-sixteenth of the gas will explode. These mixtures are of course from six to seventeen times greater in volume than the fire-damp in them, and evidently not much lighter than air (0·91 and 0·96). Except, therefore, in the almost impossible case of a goaf quite filled with fire-damp, it will be these or weaker mixtures that underflow the edge of the vault, unless upon extraordinary occasions.

The underflow will not take place all round the edge of the goaf basin, but at that point which is highest; for just as water takes its level in a pond on the side of a hill, and flows over the lower edge, so here, air strata of equal density will be horizontal. Coal seams are rarely quite horizontal; in the Little Haswell Pit, the rise is about 1 in 24, and the coal very regular. At the lower edge of such a goaf, nothing but pure air might be present in the air space, and also for a considerable distance up into the vault; yet at the upper edge, a mixture of gas and air, and even a highly explosive mixture might be escaping.

Thus goafs are evidently, in mines subject more or less to fire-damp, reservoirs of the gas and explosive mixtures; giving out their gas into the workings of the mines by a gradual underflow in smaller or larger quantities under ordinary circumstances, or suddenly, and in great proportion on extraordinary occasions: and they may either supply that explosive mixture which first takes fire, as appears to have been the case at the spot called Williamson's *jud*, close to the goaf of the *Mea-*

dows working of the Haswell Little Pit; or they may add their magazine of fire-damp and explosive mixtures, to increase the conflagration when the fire reaches them from an explosion in some other part of the mine: this appears to have been the case at the goaf of the *High Brockley Whins* working, on the occurrence of the Haswell event.

We are bound from all the evidence, and from our own personal examination, to state on the part of the owners and officers of the Haswell Colliery, that, as far as the principles of ventilation in coal mines have been developed and applied, and in comparison with the general practice of the trade, the Little Pit appears to have been most admirably ventilated. No expense seems to have been spared in the first setting out of the works, or in carrying them through, or in the daily arrangements under ground; and this care was further favoured by the natural circumstances of the mine, the seam of coal being very regular, having a strong shale roof or ceiling, and with scarcely any fault. The mine has the character of being one of the best ventilated in the whole trade, a circumstance which, though it leads us in the fullest degree to exonerate the owners and officers from all blame in reference to the late terrible event, makes us more anxious, if possible, to discover its cause, and suggest some practical guard against its recurrence in future. With this intention, and without going into the ventilation generally, we will state our view of its effect at the goaf. A great body of air, equal to 25,400 cubic feet per minute, is sent into the Little Pit, and a third part of that goes to each of the three workings. This is directed, according to the judgement of the viewer, to various parts; the main portion to where the men are at work, and certain portions to the waste and the *goaf.* In the main passages, as the Rolley way, Mothergate, &c., the wind is so strong, that it is almost impossible to keep a candle lighted; but where the works expand, it becomes slower, and the speed is least in the waste and the goaf. As a matter of observation, we found the speed small at the goaf, though full care had been taken by stoppings, &c. to make the current good and strong in the workings near it, *i. e.* in the upper boards. If it be considered that the goaf is about 13 acres in extent, and that with the surrounding workings it can hardly be less than 26 or 30 acres, the diminution in speed of the current of air there can easily be understood.

The air which flows into a mine will generally tend to move along the floor, for it is colder, and therefore denser than the air against the ceiling, warmed as it is by the men and the lamps, and it is heavier than any mixture of air and fire-damp.

Where the men are at work, this tendency is guarded against by the force of the current sent in, which sweeps the air already there before it; but in the goaf, near which the current is slow, where the roof is a large concavity, where the gas, if present, is likely to be present in greater quantities, and so to make a mixed atmosphere which is lighter than that in the working parts of the mine, there the current probably never ascends to any height, but takes its way sluggishly through the lower parts of the goaf, or moves round the outside of it. We think it probable that the current does not rise much above the level of the highest point in the edge of the goaf basin, and that the top of the goaf is seldom, if ever, reached by it in any sensible degree.

We have thus far considered the goaf as if in something like a constant state, but there are occasions of sudden and limited disturbance which affect the atmosphere of gas within and about it. The evidence at the inquest states, that a rumbling was heard on the morning of the accident within the goaf, and this was probably a fall somewhere from its roof. Such falls tend to mix the lighter and heavier strata of gas and air, and so virtually cause the gas to descend. Again, if the atmosphere four or five feet up in the goaf be an explosive mixture, and a fall of this kind take place there or near it, such an event is very likely to throw out a portion of explosive mixture into the workings of the mine, not merely by the agitation, but also by the mixture of upper with lower strata of air, making the lower explosive.

One cannot but suppose that another source of sudden and partial evolution of gas or explosive mixture from the *goaf*, may be the fall of upper parts of its roofs, and the crushing of the rocks there, by which gas pent up into the seams of coal above and the strata associated with them, has passages open for its escape into the goaf. If a bag of gas (as it is called) were thus opened into the goaf, it would rapidly increase the quantity of gas in it, and might soon cause explosive mixtures, or the gas, almost pure, to underflow the edge of the concavity into the mine.

If the goaf cavity were full of gas or explosive mixture to the highest edge level, the mechanical fall of the roof, in drawing a jud close to that edge, would, by mere agitation, drive some portion of the gas or mixture into the workings of the mine.

When a jud is drawn, and the roof has fallen in, the fall becomes part of the goaf, and the cavity left by it becomes a part of the goaf basin, the edge of the basin extending to, and enclosing the new fall. If this take place at the highest point of the basin, it suddenly opens a passage into the mine for a great quantity of air and gas, which before, by its relative levity to the air, was retained in the goaf basin. Thus, assuming a goaf of 13 acres in a coal seam inclined 1 in 24, and that a fall of 6 feet in extent takes place in the roof at the highest edge of the goaf, it would heighten the edge at that spot 3 inches; and if the goaf were full to the edge, either with fire-damp or explosive mixture, these would flow out more or less rapidly into the workings of the mine, until a horizontal stratum of 3 inches in thickness had thus escaped. Even if the roof of the goaf rose very slowly, making an exceedingly flat dome, this stratum would extend to four-fifths or more of the horizontal area of the goaf: and assuming that the greater part of this space is occupied, not by gas, but by the solid materials of the goaf, and that only a band round the goaf could be considered as air space—still from what we saw of the goafs at the Haswell Little Pit, this would be from 4 to 6 feet in horizontal extent, so that a mass of explosive atmosphere or fire-damp might escape equal to a band about 3000 feet long, by 5 feet wide, and 3 inches deep, making about 3750 cubic feet. It is not likely that this would escape all at once; but the tenth, the twentieth, or the fiftieth part, or even the hundredth part, would be enough to take fire at an injured lamp, and to communicate fire to the whole, though the whole condition between safety and danger up to that moment may have depended upon 3 inches of the roof.

The above is no hypothetical case, but must occasionally, and as to the evolution of gas, frequently occur. If there be gas in the mine, it is expected at the goaf: gas does come from the goaf. All working at the goaf, except with safety lamps, is forbidden: the seams in mines are usually more or less inclined; and this mine at Haswell, where gas *has come* from the goaf, is very free from gas, and well-ventilated, as compared with other pits.

There is one other point connected with what may be called the action of the goaf, and the occasional sudden and temporary discharge of gas from it. One of the witnesses on the inquest, Mr. G. Hunter, pointed out the effect he had observed in the mine on a change in the barometer:—that as the barometer fell, fire-damp would tend to appear, and that it did this the more suddenly and abundantly, if the barometer, having continued high for some time, fell suddenly: and Mr. Buddle has already strongly stated his opinion that accidents from fire-damp always occur with a low barometer. This is very natural; for during a high barometer, the fire-damp, tending to escape from the coal and strata, would be in some degree pent up or restrained by the pressure of the atmosphere; and the diminution of pressure indicated by a sudden fall taking off this restraint, would let the gas expand and escape more freely, and hence its more abundant appearance. Now, without reference to the fire-damp which would ooze out of the strata and from the surface of the goaf basin, let us for a moment consider what would happen as respects the gas already free, but held by its small specific gravity in the upper part of the basin. The barometer will sometimes sink an inch in twelve hours: on such an occurrence, any portion of air or gas pressed on merely by the atmosphere will expand about one-thirtieth part in that time. The portion of air or gas contained in the inverted basin of the goaf is, as has been said, equal to the volume of coal withdrawn beneath, as long as the surface of the country above has not sunk; but because of the inclination of the coal seam, which we may for the present assume as that at the Haswell Little Pit, the air space which is above the level of the highest point of the edge of the concavity may be taken for illustration, as four-fifths of the bulk of the coal, or four-fifths of 13 acres by a thickness of 5 feet (2,265,120 cubic feet); of which the one-thirtieth part, or 75,500 cubic feet, will by expansion be driven below the level of the highest point of the goaf basin. If it contain any portion of gas, it will by its lightness begin to flow out at that particular part; if it contain much, it will flow out the more rapidly, and be the more dangerous; and if, in a mine much infected with fire-damp, it be an explosive mixture, it is easy to imagine that such a cause may occasionally bring about most fatal results.

A fall of an inch in the barometer, of a sudden, is rare, but a fall of one-tenth of an inch is not, and that in such a goaf as the one supposed would place 7550 cubic feet below the edge of the cavity; this all tends to issue forth at one place, and that generally a place where the ventilation is weakest. If, as an influential circumstance tending to diminish the quantity of issuing atmosphere, we assume that the country above has descended, so as not to leave more air space in the goaf than one-fourth of the volume of coal removed, still that would permit 1887 cubic feet to issue forth at one spot, on the occurrence of a fall in the mercury of the barometer equal to one-tenth of an inch. Hence it does appear to us that the goaf, in connexion with barometer changes, may in certain mines be productive of sudden evolutions of fire-damp and explosive mixtures, and that the indication of the barometer, and the consequent condition of the mine, ought to be very carefully attended to.

The recent terrible event appears to have originated at the Meadows Flat workings, at a point near to the upper edge of the goaf concavity, at a place where a *jud* was in the act of being drawn. A man of the name of Williamson, and other men, were engaged in this work at the time of the accident; all were killed, and the jud has since been named after Williamson. All the evidence derivable from the way in which the stoppings were blown, the charring of the posts, and the adhesion of charred coal dust to them on this or that side, as also to the walls of the mine and edges of the irregularities of the walls, confirm this view in the opinion of practical men, the viewers of the mines, and with this conclusion the results of our own close inspection perfectly agree. At this place Davy lamps were found. The state of the gauze indicated that they had been in good condition prior to the accident, but two of them were much crushed and bruised, and one of the others had the oil plug out; this and the fourth were probably found lying on their sides, for the oil was out of the bottom part of the lamps, and had soaked into half the gauze along the cylinder, as they may have lain on the ground. We could get no exact evidence as to how the lamps were in respect of position and other circumstances when found. The gauze of one of these lamps had been heated all round for about 2 inches from the bottom, as if fire-damp had been burning inside at that part of the cylinder; and there was also on the side of the upper part of the gauze of the same lamp, an oblong spot of oxidation exactly such as would have been produced at the first entrance of increasing fire-damp into the lamp, and consequent elongation of the flame, supposing the lamp had been placed a little obliquely against the wall of coal or any other upright object. These appearances accord perfectly with the idea that fire-damp came into the workings whilst this lamp (which had been given out that morning perfect) was there and in use.

At this place the men were drawing a *jud*. It may be that fire-damp issued into the workings there independent of anything the men were doing, or it may be that in the falls of the roof (for it had fallen, as was evident by the stone and timbers) they broke away a portion of the upper edge of the goaf concavity, and by that, both let out explosive mixture into the works, as before explained, and mechanically mixed it up with the air beneath. This issue of gas would not of itself have caused the explosion if the lamps had been right; but of these lamps there are now three that might have fired the gas, for two of them are so bruised, that if these bruises were occasioned by a fall of stones either before or at the time of the issue of gas, then the gas may have taken fire at them; or if they were not bruised by a fall before the explosion, but by one consequent upon it, then it is possible (though not probable) that the third lamp, with the oil plug out, may have occasioned the firing.

When once the combustion began, even though from only a small quantity of gas sent out of the goaf, it would instantly reach up into that greater portion within the goaf vault, and we believe that it was the inflammation of this large portion which gave such force to the blast as to blow down the stoppings between the Meadow and High Brockley Whin workings, and to reach so far as to the goaf of the latter works. Here, from the appearance of the posts and walls, and also from the burnt bodies found, it would appear as if the fire-damp in this goaf had been driven out, mixed with air and inflamed; a very natural result of the circumstances.

In considering the extent of the fire for the moment of explosion, it is not to be supposed that fire-damp is its only fuel; the coal dust swept by the rush of wind and flame from the floor, roof, and walls of the works would instantly take fire and burn, if there were oxygen enough in the air present to support its combustion; and we found the dust adhering to the face of the pillars, props, and walls in the direction of, and on the side towards the explosion, increasing gradually to a certain distance, as we neared the place of ignition. This deposit was in some parts half an inch, and in others almost an inch thick; it adhered together in a friable coked state; when examined with the glass it presented the fused round form of burnt coal dust, and when examined chemically, and compared with the coal itself reduced to powder, was found deprived of the greater portion of the bitumen, and in some instances entirely destitute of it. There is every reason to believe that much coal-gas was made from this dust in the very air itself of the mine by the flame of the fire-damp, which raised and swept it along; and much of the carbon of this dust remained unburnt only for want of air.

At first we were greatly embarrassed by the circumstance of the large number of deaths from choke-damp; and the evidence that that had been present in very considerable quantities compared with the small proportion of fire-damp, which, in the opinion of those in and about the works just before, must have occasioned the explosion. But on consideration of the character of the goafs, as reservoirs of gaseous fuel, and the effect of dust in the mine, we are satisfied that these circumstances fully account for the apparent discrepancy. The blowing down of the stoppings, by destroying the ventilation of the mine, caused all this choke-damp to be left for a time in the workings; and there is reason to believe, from the circumstances, that the men met with a death comparatively sudden.

With such views of the character and effect of the goaf as we have ventured to express, and with a strong belief that it has been the cause of the recent sad event at Haswell, it will not be thought surprising, that, in thinking of the means of preventing such calamity in future, we should turn our attention almost exclusively to it. The first idea is to ventilate the goaf. If a shaft could be sunk over the crown of the goaf, it might perhaps carry away all the fire-damp; but the probability is that where furnaces are used in the upcast shaft, *that* over the goaf would become a downcast shaft, so that all the fire-damp evolved into it would have to pass into the mine and out with the ordinary ventilation. Besides that shafts of such magnitude are very expensive, the bottom part would be liable to fall in; the crown of the goaf vault, also, is in many cases probably changing its place continually, and in inclined strata it might easily happen in the course of working that the bottom edge of the shaft would soon be below the upper edge of the goaf basin *in* the mine, when it would be of comparatively little use. These are difficulties and objections which occur to us even in our theoretical considerations: whether practical men would set them aside, or whether they would add to their number, we cannot say.

Another mode of action has occurred to us, which, the more we think of it, seems the more practical, and offers greater hopes of service to humanity, and which, therefore, we shall venture somewhat minutely to explain. It is founded on the principle of drawing away the atmosphere in the goaf, not of ventilating it by blowing air into it; it is better in principle than blowing into the goaf, because it proposes to take away the fire-damp in a concentrated form, and never to give it to the air of the workings; whereas blowing would first dilute and expand the gas, and then throw it into the works. The difference is especially important for mines where the gas is abundant; for suppose for a moment a goaf cavity full of fire-damp, and an apparatus that could either blow into it or take from it an equal quantity in each case, of air or gas; to take out one cubic foot of fire-damp would be to prevent the formation of from 6 to 15 feet of explosive mixture; to drive in one cubic foot of air would be to send an equal quantity of fire-damp by displacement into the mine, there to form, at one moment or another, from 6 to 15 feet of explosive mixture, which would afterwards have to be carried out of the works by the usual mode of ventilation.

In the first place, we propose that the pillars and juds should be so worked and drawn as to have reference to the form which it is desirable to give to the goaf. This form must be dependent on circumstances, but the point required is that it should draw together at its termination in the upper part of the works, *i.e.* that as the strata rise, the goaf should not have several projections or bays, running independently into the higher works, but one only where the highest point of the goaf basin may occur, and towards which all the fire-damp in the goaf may drain and tend to run under. It is as if in making a pond on the side of a hill,—there should not be two or three low places on the bank where the superfluous water may run over, but one only, *that* being the lowest, as in the fire-damp case it would require to be the highest. This condition would probably be obtained with facility by continually keeping one jud drawn in advance of the rest; and the inspection of a working plan with the inclines drawn on it would easily determine, in every case, what should be done. The next point is to drain or clear this place as well as may be of its fire-damp, which if it could be done effectually, would, in all probability, prevent danger from the goaf, if not absolutely, yet to an extent far beyond what is the case at present; and for this purpose two plans suggest themselves, the same in principle, but differing in extent.

The first plan consists in laying a pipe from the goaf to the upcast shaft, introducing the one extremity into the vault of the goaf at the upper edge of its brim, and furnishing the other extremity with necessary means of drawing the air out of the pipe. The pipe itself may be of cast iron in lengths, joined together with sockets and caulked joints, or in any other of the many well-known manners: its diameter may be about 12 inches, until experience may have directed some other dimensions. Its place, for the chief part of its course, would probably be in the return way; for it ought to be tight, having no other opening than the two extremities; and in the return way it could be best examined from time to time, and would be safest from the effects of creep.

The exit or upcast end of the pipe is to be supplied with means of draught or suction; this might be either a blowing cylinder, or a rough box double bellows, or a revolving fanner, any of which might be worked by the engine, or even by a man or boy, for the work would be easy, there being no resistance to the exit of the air analogous to that offered by the contraction of the stream of air in the ordinary use of blowing machines. But even these are, in our opinion, not required; for from the powerful draught in the return of the Haswell Works, we are at present fully persuaded, that if the goaf ventilation pipe, of the size proposed, simply entered the upcast shaft, there would be draught enough to draw away the atmosphere of the goaf. It is true, that if the atmosphere in the goaf vault, to which the end of the pipe might penetrate, were pure fire-damp, we should have to consider its lightness, and the vertical height between that extremity and the end in the upcast shaft. But this probably is a state of things which could happen very rarely, if ever, in the Haswell Pit; explosive or still lower mixtures, there and in most cases, are rather to be expected, and these, as has been shown, are not so much lighter than air as to offer difficulty in this respect. If a case of pure fire-damp, or a mixture so rich as to offer difficulty on account of its lightness, were to be reached by the pipe, then, indeed, it would be well worth putting on the mechanical means already referred to, to drain it out*.

The goaf end of the pipe offers more difficulties, but we do not see at present that there are any that are not easily surmountable. It has to rise up into the cavity of the goaf, at the point nearest to the highest part of its edge; to enter into this cavity, four, five, or six feet or even more, if possible; and to be temporary, moveable, and tight. The iron tube

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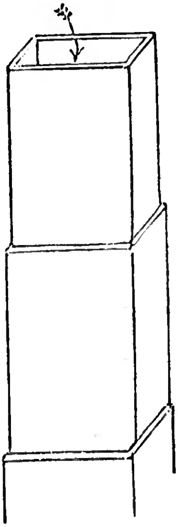
[* In the Phil. Mag., First Series, vol. xxxviii. p. 120, was reprinted from the Transactions of the Society for the Encouragement of Arts, vol. xxviii. (for 1810), a paper by Mr. John Taylor, "*On the Ventilation of Mines, with the Description of a new Machine for that purpose,*" dated April 9, 1810.

The method of ventilation proposed in this paper and effected by the machine described, " instead of using the machines which serve as condensers," is that of exhaustion—pumping out of the mine all the air that was impure as fast as it became so.

In Thomson's Annals of Philosophy (for March 1814), vol. iii. p. 196, Mr. John Taylor published a paper "*On the Ventilation of Coal Mines,*" in which he refers to the paper cited above, extracts from it certain suggestions relative to the application to coal mines of the method of ventilation he had proposed and successfully practised, and enters into some details on this particular subject, insisting on the principle of "the removal of the noxious air by some apparatus proper for the purpose, connected with pipes which are to be fixed so that they may be made to draw from any part or parts of the works, as occasion may require." To this paper Dr. Thomson appended a figure and description of the machine extracted from the paper in the Trans. Soc. Arts. We recommend the perusal of these papers to all who are interested in the subject.

In a lecture on mines, delivered at the London Institution, at the Soirée of February 13, 1833, Mr. Taylor (who was at that time one of the managers of the Institution) exhibited a working model of the machine he had described in 1810, showing its action by experiment, and explaining the method of ventilating mines by its means.—Ed. Phil. Mag.]

may in the first place be continued from the upcast shaft, by any course which on consideration of the works may be thought most expedient, towards, and within a certain distance of, the draining point of the goaf, as near to it as is practicable, probably to within 15 or 20 feet. From thence the pipe may be continued, by wooden trunks fitted together temporarily, having the same sectional area as the pipe itself; and where the pipe rises in the cavity of the goaf, it does not seem difficult to fix wooden adjusters together, either square or round, the joints of which may be made tight by a little mortar. It is even probable that the upper extremity of the pipe might be a tube of air-tight cloth kept open by rings, and raised into its place from a safe distance by a rod of iron or wood. In all cases this end of the tube should be fully open, not allowed to collapse or be contracted in any part; and the joints, if adjusters are used, should be in the direction which opposes least resistance by irregularities to the passage of the air: the roof should of course be propped up as much as may be required about this end of the pipe to protect it; the character of a fixture being in some degree given to the arrangement, until such time as it is again necessary to extend the goaf in that direction.



The *second plan* which we propose is of the same nature, but more local in its arrangement. In this plan, we contemplate carrying the exit end of the drainage pipe only into the return way, but into a part where there is such current of air that the goaf gas thrown in is sure to be thoroughly diluted and carried away. It ought not to be near the furnace, lest at any time there should be gas enough to take fire at the furnace, and communicate explosion back through the pipe into the goaf.

The goaf termination of the pipe will be as before. But in some part of the course of this pipe is to be placed a blowing apparatus, either bellows or fanner, as before directed, drawing from the goaf, and blowing towards the return way. The apparatus may be placed in any convenient part of the course of the pipe, but would probably be more effectual, the nearer it were to the goaf, as there is then the chance of fewer leakages between it and the goaf. It would probably have to be worked by a man, or by any power that was available; whether the draught into that part of the return where the goaf pipe entered, would be sufficient alone or not, is a point which must depend on the nature of the works, and must be determined, if determined at all, by experience.

Such is the general plan, which, with some degree of confidence in its principles, we venture to submit to practical men for their consideration; and we do it with more readiness, believing, from the spirit which we have seen manifested at the Haswell mine, that they are earnestly desirous of carrying into effect everything that can be practically useful. The effect of it would be to remove that condition of the goaf atmosphere, which ordinarily exists at the upper edge of the goaf basin, to a distance of four, five, or more feet higher up within the basin, so as to allow abundant space for all the variations of this atmosphere, without the underflowing of any explosive, or perhaps even contaminated portion of it into the mine. In the case of a regular mine working downwards or into deeper parts, there does not seem much difficulty of application, since the highest edge of the goaf is there stationary. Where the pitmen work towards the rise or upwards, as is the case at the Haswell Little Pit, then the goaf end of the pipe must change its place from time to time. Where faults occur, and the mine is irregular in its workings, each case must be considered by itself, and met, if it can be, upon the same principles. The more faults there are in a mine, the more difficult it may be to regulate the place and form of the upper edge of the goaf cavity; but on the other hand, the more faults there are, the more fire-damp is there generally in the workings, and therefore the more occasion for some means of obtaining the end proposed. If, as is manifested by the Haswell Pit, mines considered the most safe and best ventilated need some such means, how much more must others require it!

It would be a very important addition to the information requisite to indicate and lead to the fittest means of guarding against such events as that at the Haswell Pit, if the state of the atmosphere in the goaf vault were from time to time *examined*, and especially upon fallings of the barometer; so that we might have a general knowledge of its nature. This would not be at all difficult in the hands of an intelligent man; for a piece of small copper pipe about one-third of an inch in diameter, and 25 or 30 feet in length, might easily be introduced by hand into the cavity of the goaf, at the place where the edge is highest, and this being attached to an air-pump syringe below, a few strokes of the hand would suffice to make the latter bring down the gas or air from the place reached by the upper end of the pipe; and if, after the pipe and syringe were filled with such air, a large and sound bladder, or gas-bag, were screwed on to the syringe, it could then easily be filled with other portions of air drawn from the same place. The bag being carried away to a safe part of the mine, could easily have the character of its contents examined into, either by a Davy lamp, or by a candle placed in a glass cylinder, as, for instance, the chimney of an Argand lamp, the air from the bag being then passed in from beneath. Specimens of the air from the goaf might be obtained in a still simpler way, by having a tin, copper, or other close vessel, of the capacity of three or four quarts, with a stop-cock at the top and another at the bottom, filling it with water, attaching it to the lower end of the small copper pipe proceeding up into the goaf, and then opening the cocks until the water had run out. On shutting the cocks, the vessel would be filled with, and would retain a specimen of, the air of the goaf.

Supposing that such a ventilating arrangement as that proposed were established, *another place* for examination would be at the exit end of the goaf ventilation pipe. Ordinarily, we trust, but little gas would be found there, because of the effect of the continued drainage by the arrangement. But this examination, like all others, should be made with every care, lest upon any sudden evolution of gas, or fall of the barometer, an explosive mixture should be issuing forth, and this by a naked light, if such were used for the examination, communicate combustion to the goaf gas through the pipe itself.

Both in the mines and at the inquest, our attention was called to the *stoppings* and *doors* in the workings, upon which the course of the general ventilation depends. When these are blown away by an explosion the ventilation is altered, and at times, as in the Haswell Pit accident, entirely withdrawn. Mr. Buddle proposed to have dam doors so arranged, that when the stoppings were thrown down, these should come into action. We do not think that it would be impossible, or even very difficult, to carry such a plan into effect in some of the permanent ways of the mine; but, considering that if the stoppings were not blown down, the probable effects after an explosion would be the firing of the mine, and also that Mr. Buddle did not himself work out his own proposition in the many mines where he had power, we are not prepared to say that it is a matter that can be brought into practice, and ought to be enforced; or to give any opinion on the subject.

We perhaps ought to apologise for this lengthened statement, especially as we have no right to assume that we have that kind of knowledge that can be gained only by practical men; but we have been encouraged to proceed by the hope of being useful; and have endeavoured to write this report, not in technical phrase, but in plain and simple language, which, if useful in its suggestions, may be comprehended by all.

In conclusion, we cannot but express a hope that some step may be taken without delay, with a view to afford a better education to the persons engaged in working in collieries. When attending the late inquest, we were much struck with the fact that more than half of the pitmen who gave evidence, some of them persons of great intelligence, and one master wasteman, were unable to write, or even to sign their names as witnesses. The best-conducted and well-informed from amongst the pitmen are occasionally promoted to some of the subordinate offices of charge in the mines; and it would be in the highest degree useful, if greater facilities were given to the underviewers, overmen, wastemen and deputies, to learn the elementary knowledge more immediately bearing upon their business. They might be taught, for example, such simple parts of chemistry and pneumatics as relate to the nature of gases and air; the first principles of hydrostatics and of geology, as far as concerns the position and dislocation of strata, the intrusion of trappean or volcanic rocks, and other points.

In countries such as France and Germany, where a far less amount of capital is embarked in mining enterprises, there are large schools of mines and scientific establishments, in which professional men, of different grades, are carefully instructed in those branches of knowledge which are closely connected with the art of mining. We are aware that, notwithstanding the want of such institutions, viewers in this country combine a large amount of scientific information with great practical experience. But such qualifications are enjoyed by a comparatively small proportion of those engaged in the superintendence of coal pits, especially of that class to whom the subordinate offices are intrusted*. If peculiar difficulties attend the organization of schools for the mining population, owing to its migratory habits, and because the workpeople are often congregated suddenly at places far distant from towns and villages, and do not remain permanently resident at fixed

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* In the present state of science it is unworthy of the viewers and other mining agents that the nomenclature employed by them in the description of rocks should neither be intelligible to the geologist nor uniform in neighbouring mining districts. Such terms as *post, metal, whin, splint coal, mild* and *strong, thill, scares, girdles,* and others used in the original of the section copied for this report, are illustrations of the strange phraseology which prevails, and which cannot easily be interpreted, even where the miner attaches a definite meaning to the terms he uses.

points, it is the more necessary to endeavour to overcome these obstacles; and provision might, perhaps, be made for appointing teachers, whose duty it should be to visit, in succession, the different localities where the large pits are opened from time to time. Among the many thousands whose thoughts are now continually engaged in the coal mines, there will be always some individuals of strong natural powers, who, if they had mastered the elements of the sciences above enumerated, might be enabled to invent new methods, or at all events would be far more capable than persons unconnected with the business to appreciate the dangers to which they are exposed, and to judge correctly of the adaptation of philosophical principles to practice. We believe, therefore, that if the education of the miners generally, and especially of those set over them, can be materially raised, it will conduce to the security of the lives of the men, and the perfecting of the art of mining, more effectually than any system of parliamentary inspection which could be devised.

There is no reason to fear but that the owners, and all the authorities, high and low, would combine with the men in enforcing regulations for the application of scientific principles to practice, if their minds were prepared by instruction to estimate the true value of the new methods proposed, and if by that instruction those prejudices were removed which disincline the ignorant to every change of system.

There are here no conflicting interests to contend with, for the proprietors are always anxious to prevent explosions and accidents, not only by their feelings of humanity, but by a regard to the property they have at stake; while the viewers, underviewers, and other officers, are continually risking their own lives, and share in every danger with the men.

We have the honour to be, Sir,
.. Your obedient humble Servants,
M. FARADAY,
CHAS. LYELL.

III. *On the Purification of the Soluble Salts of Manganese from Iron.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

YOUR correspondent Mr. Kemp, in the last Number (169) of your valuable Magazine, has given a process for the purification of the soluble salts of manganese from iron, which, doubtless, he considers new, but which is identical with the formula of Sir J. Herschel, published in the Philosophical

Transactions for 1821, with only one slight point of difference, viz. that Mr. Kemp neutralizes his solution *cold*, while Sir J. Herschel recommends the neutralization to be performed at a boiling temperature, a precaution which, as will appear on a perusal of the paper in question, is by no means to be neglected when a mathematically rigorous separation of the oxides is desired.

Mr. Kemp also notices that where cheapness is an object, carbonate of soda may be substituted for that of ammonia. Sir J. Herschel has shown that any earthy or metallic carbonate will equally answer the purpose, so that (for the preparation of the sulphate at least, for manufacturing purposes) *chalk* would be that substance to which the manufacturer would resort, as no harm can arise from adding it in any excess, and the small quantity of sulphate of lime remaining in solution would be readily separated by evaporation to dryness and redissolution. An experiment is related in the paper referred to, where a very minute per-centage of manganese was insulated from an enormous excess of iron by simply filtering the hot solution through chalk.

I am, Sir, &c.,
ANNOTATOR.

IV. *Mineralogical Notices*[*].

Analysis of the Greenovite, by M. Delesse.

THIS mineral, found at Saint Marcel in Piedmont by M. Bertrand de Lom, has been described by M. Dufrénoy, who has given its crystallographic characters; the analysis made by M. Cacarrié on a too small quantity presented much uncertainty, which has induced me to submit it to fresh investigation. I have in fact found that if the crystallization gave an exact idea of that species, its composition had caused it to be regarded, erroneously, as a titanate of manganese, whereas it is a silico-titanate of lime, analogous to sphene, as results from the following analyses:—

	I.	Oxygen.	II.	Oxygen.	
Silica	29·80	15·48	30·40	15·79	2
Oxide of titanium . .	43·00	17·07	42·00	16·68	2
Lime	23·60	6·33	24·30	6·83	1
Protoxide of manganese	2·90	0·65	3·80	0·85	
Protoxide of iron, traces.					
	99·30		100·50		

These elements are well enough represented by the formula

$$(2\,\dddot{\mathrm{Si}}\,\dot{\mathrm{R}}^3 + \ddot{\mathrm{Ti}}^2\,\dot{\mathrm{R}}) + 3\,\dddot{\mathrm{Si}}\,\ddot{\mathrm{Ti}}^2.$$

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[*] From the *Comptes Rendus*, November 11, 1844.

Analysis of the Bornine of Brazil (Telluret of Bismuth), by M. A. Damour.

The bornine, the analysis of which I present, is in micaceous laminæ having the brilliancy of polished steel, slightly flexible, and very thin; heated on charcoal it fuses, becoming surrounded with a white ring and a green ring, and finally disappears in the pores of the charcoal.

In the open tube it fuses on the first application of heat, disengages a sulphurous smell, then white fumes of oxide of tellurium, and toward the close of the operation a marked smell of selenium. In the upper part of the tube a white crust is observed surmounted by a light brick-red layer, attributable to the condensation of the selenium. The lower part of the tube remains covered with a yellowish residue of oxide of bismuth.

Nitric acid dissolves it very easily with a disengagement of nitrous gas; analysed by this means the bornine of Brazil has given the following results:—

		Relations.		
Sulphur . . .	3·15	156	186	3
Selenium . . .	1·48	30		
Tellurium . .	15·93		198	3
Bismuth . . .	79·15		594	8
	99·71			

In taking the number 1330·376, adopted by MM. Regnault and Rose, as the atomic weight of bismuth, and that of 802·131 for the atomic weight of tellurium, the preceding results might be expressed by the formula

$$Bi^2 S^2 + 3Bi^2 Te.$$

This composition differs considerably from the results obtained by MM. Berzelius and Wehrle for the bornine of Chemnitz, as well as those which the bornine of Deutsch-Pilsen yielded to the latter chemist.

These differences might perhaps lead us to adopt three species of bornine, but we rather think that, if tellurium and bismuth can combine in exact proportions, they may also combine in very variable quantities. To decide this delicate question—one too which occurs with many metallic minerals—it is necessary to wait until crystals of bornine combine the two characters which determine the formation of mineral species, namely the atomic composition and the crystalline form.

V. *Memoir of the late* Francis Baily, *Esq., F.R.S., &c., President of the Astronomical Society. By* Sir J. F. W. Herschel, *Bart.*[*]

At a Special General Meeting of the Royal Astronomical Society, held November 8, 1844, called in pursuance of a Resolution of the Council on the 20th of September last,

G. B. Airy, Esq., President, in the Chair,

Sir J. F. W. Herschel read the following Memoir:—

In the performance of the melancholy duty imposed on me by the wishes of the Council, that I should endeavour, on this occasion, to place before the assembled Members of this Society a sketch of the scientific life and character of our late lamented President, I have been careful both to examine my own competency to the task, and to consider well the proper limits within which to confine myself in its execution. In the first of these respects, indeed, though tolerably familiar with some of the leading subjects which I shall have to touch upon, there are others on which I have seriously felt the want of a longer interval for preparation. On these, of course, I shall take care to express myself with becoming diffidence, and in so vast a field of laborious inquiry and of minute, yet important research as I shall have to range over, it may easily be supposed I have more than once found occasion to wish that the duty had fallen into abler hands. A duty, however, it is, and a very sacred one, which we owe to departed merit, to society and to ourselves, to fix as speedily as possible, while its impress is yet fresh and vivid upon us, its features in our minds with all attainable distinctness and precision, and to store them up beyond the reach of change and the treachery of passing years.

As respects the limits within which I feel it necessary to confine myself on this occasion, it is to astronomers to whom I have to speak of an astronomer—to members of a large and, in the simplicity of truth I may add, a highly efficient public body—of an officer to whom more than to any other individual, living or dead, it owes the respect of Europe. To make what I have to say complete as a biography, however interesting to us all, however desirable in itself, is very far either from my intention or my power. Nor is the time fitting for the attempt. The event is too recent, the particulars which can be collected at the present moment too scanty, the grief of surviving relations too fresh, to admit of that sort of close and pertinacious inquiry into facts, anecdotes, documents and evidence, which personal biography requires to be satisfactory.

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[*] From the Monthly Notices of the Royal Astronomical Society for November 8, 1844, No. 10, vol. vi.

In this respect, therefore, a mere sketch is all that I can pretend to give.

Francis Baily was born on the 28th of April, 1774, at Newbury, in the county of Berks. His father was Mr. Richard Baily, a native of Thatcham, in the same county, who became established as a banker at Newbury. He married Miss Sarah Head, by whom he had five sons and two daughters. Francis, who was the third son, received his education at the school of the Rev. Mr. Best of Newbury, an establishment of considerable local reputation, where, although probably little of an abstract or mathematical nature was imparted, the chief elements of a liberal and classical education were undoubtedly communicated. From his early youth he manifested a propensity to physical inquiry, being fond of chemical, and especially of electrical experiments,—a propensity sufficiently marked (in conjunction with his generally studious habits) to procure for him, among his young contemporaries, the half-jesting, half-serious *soubriquet* of "the Philosopher of Newbury."

It does not appear that he received any further instruction beyond the usual routine of an establishment of the kind above mentioned, so that, in respect of the sciences, and especially of that in which he attained such eminent distinction, he must be regarded as self-educated. This taste for and knowledge of electricity and chemistry were probably acquired from Dr. Priestley, with whom, at the age of seventeen, he became intimately acquainted, and of whom he always continued a warm admirer. But that his acquaintance with the subject was considerable, and his attachment to it permanent, may be concluded from the fact, that Mr. Welsh, the organist of the parish church of Newbury, who had a very pretty electrical apparatus, and at whose house I remember myself to have first witnessed an electrical experiment, is stated to have imbibed his taste for that science, and to have acquired its principles from his example and instructions at a somewhat subsequent period.

He quitted Mr. Best's school at fourteen years of age, and, having chosen a mercantile life, which accorded with the views of his parents, he was sent to London, and placed in a house of business in the City, where he remained till his twenty-second year, when, having duly served his time, and either not feeling an inclination to the particular line of business in which he had commenced his life, or being desirous of the general enlargement of mind which travel gives, or from mere youthful love of adventure and enterprise, he embarked for America on the 21st of October, 1795, which, however, he was not destined to reach without twice incurring the most imminent danger from shipwreck, both on our own coast, under most awful circumstances, on the Goodwin Sands, and off New York, which he was prevented from reaching, being driven to sea in a gale, and, after endeavouring in vain to reach Bermuda, was driven into Antigua, whence he subsequently embarked for Norfolk in Virginia.

In America he remained one or two years, travelling over the whole of the United States and through much of the western country; in which travel he experienced, at various times, much hardship and privation, having, as I remember to have heard him state in conversation (and which must have referred to this period of his life), passed eleven months without the shelter of a civilized roof. During his residence in America he was not unmindful of his intellectual and social improvement, having not only read much and observed much, as a copious journal which he transmitted home proves, but formed the acquaintance of some eminent persons, among whom may be mentioned Mr. Ellicot, the Surveyor-General of the United States, from whom he obtained some curious information bearing on the periodical displays of meteors on the 12th of November, of which that gentleman observed a superb instance in 1799, and from whom it is not impossible he may have acquired a taste for observations of a more distinctly astronomical and geographical nature.

Whatever may have been the more direct object of this journey, if indeed it had any other than to gratify a youthful inclination for travel and adventure, it does not appear to have exercised any material influence on his after-life, since, on his return to England, in place of immediately entering into business, he continued to reside for some time with his parents at Newbury, which, however, at length he quitted for London, to engage in business as a stock-broker, being taken into partnership by Mr. Whitmore of the Stock Exchange. The exact date of this partnership I have not been able to learn: I believe it to have been 1801; but that it must have been prior to 1802, may be concluded from the subject of his first publication, which appeared in that year, viz. 'Tables for the Purchasing and Renewing of Leases for Terms of Years certain and for Lives, with Rules for determining the Value of the Reversions of Estates after any such Leases.' This work (as well as the next) is preceded by a highly practical and useful Introduction, and followed by an Appendix, which shows, that at the age of twenty-eight he had become well versed in the works of the English mathematicians, and had also consulted those of foreign ones. It speedily attained a standard reputation on account of its intrinsic utility, and went through several editions. His next work, a pamphlet in defence of the rights of the Stock-Brokers against the attacks of the City of London, printed in 1806, at all events shows him at that time to have become identified in his feelings and interests with that body of which he lived to be an eminent and successful member. A similar conclusion may be drawn from his next publication, which appeared in 1808, 'The Doctrine of Interest and Annuities Analytically Investigated and Explained,' a work than which no one more complete had been previously published, and which is still regarded as the most extensive and standard work on compound interest. It was speedily followed by other works on the same subject, viz. in 1810 by 'The Doctrine of Life Annuities and Insurances Analytically Investigated and Explained'; to which, in 1813, he added an Appendix. This is a work in many ways remarkable, and its peculiarities are of a highly characteristic nature; method, symmetry, and lucid order being brought in aid of practical utility in a subject which had never before been so treated, and old routine being boldly questioned and confronted with enlarged experience. A friend, of great mathematical attainments and extensive practical acquaintance with subjects of this nature, thus characterizes it:—"It is not easy to say too much of the value of this work in promoting sound practical knowledge of the subject. It was the first work in which the whole of the subject was systematically algebraized; the first in which modern symmetry of notation was introduced; and the first modern work, since Price and Morgan, in which the Northampton Tables were not exclusively employed, and in which the longer duration of human life was contended for; and the first in which some attempt was made to represent by symbols the various cases of annuities and assurances, afterwards more systematically done by Mr. Milne." In the Appendix to this work, a method originally proposed by Mr. Barrett of forming the tables, by which cases of temporary and deferred annuities, formerly requiring tedious calculations, become as easy as the others, and which, in the improved form subsequently given to it by Mr. Griffith Davies, has come into very general use in this country, was, by the penetration of Mr. Baily, given to the public, but for which it would probably have been altogether lost. It may serve to give some idea of the estimation in which this work was held, that when out of print, its copies used to sell for four or five times their original price. A chapter of this work is devoted to the practical working of the several life assurance companies in London, containing some free remarks on several points of their practice. Mr. Babbage has subsequently followed in the same line (as he has also advocated extending the estimation of the duration of life to still more advanced ages). However unpleasing it may be to public bodies, especially commercial ones, to see practices of whose injustice they may perhaps have been unaware, convicted of it, and made matter of public animadversion, there can be no doubt that criticisms of this kind, when really well grounded and expressed with temperance and moderation, are both salutary to the parties concerned, and merit in a high degree the gratitude of the public. A higher praise is due to the candour and boldness of openly entering the lists on such occasions, and despising the anonymous shield of which so many avail themselves.

But while devoting his attention thus assiduously to matters of direct commercial interest, he could yet find time for other objects of a more general nature. Astronomical pursuits had already begun to assume in his eyes that attraction which was destined ultimately to draw him aside entirely from business, and to constitute at once the main occupation and the chief delight of his life. As everything to which he turned his thoughts presented itself to them, if I may use the expression, in the form of a palpable reality, a thing to be turned and examined on all sides—to be reduced to number, weight, and measure—to be contemplated with steadiness and distinctness, till everything shadowy and uncertain had disappeared from it, and it had moulded itself, under his scrutiny, into entire self-consistency, the practical branches of astronomical calculation early became, in his hands, instruments of the readiest and most familiar application, as the touchstones of the truth of its theories and the means of giving to them that substantial reality which his mind seemed to crave as a condition for their distinct conception by it. His first astronomical paper, on the celebrated solar eclipse, said to have been predicted by Thales, which was written in November 1810, and read before the Royal Society on the 14th of March, 1811, affords a remarkable instance of this. That eclipse had long been a disputed point among chronologists. It was easy to perceive, and accordingly all had perceived, that an eclipse of the sun, so nearly central as to produce great darkness, being a rare phænomenon in any part of the globe, and excessively so in any precisely fixed locality, must afford a perfectly certain means of determining the date of a coincident event, if only the geographical locality be well ascertained, and some moderate limits of time within which the event must have happened be assigned, and provided the means were afforded of calculating back the moon's place for any remote epoch. In this case, both the locality and the probable historical limits were sufficiently precise; and the account of Herodotus, which agrees only with the character of a total and not of an annular eclipse (as Mr. Baily was the first to remark), still further limits the problem. But the tables of the moon employed by all prior computists were inadequate to carry back her place with the requisite exactness, nor was it till the publication of Burg's Lunar Tables that the means of doing so were in the hands of astronomers. The course of Mr. Baily's reading at this period (being then, no doubt, employed in collecting the materials for the Chronological Tables in his 'Epitome of Universal History,' which appeared not long after) brought him necessarily into contact with this subject. He perceived at once both the uncertainty of all former calculations of this eclipse, and the possibility of attacking it with a fresh prospect of success. None, however, but a consummate astronomical calculator would have ventured on such an inquiry, which involved the computation of all the solar eclipses during a period of seventy years, six centuries before the Christian æra. These calculations led him to assign, as the eclipse in question, that of September 30, B.C. 610, which was central and total, according to these tables, at the very point where all historical probability places the scene of action.

Most men would have regarded such a result, obtained by so much labour, with triumphant complacency: not so Mr. Baily. His habit of examining things on all sides, instead of permitting him to rest content with his conclusion, led him on to further inquiry, and induced him to calculate the phænomena of another total eclipse recorded in ancient history, that of Agathocles, which happened August 15, B.C. 310, an eclipse of which neither the date nor the locality admits of any considerable uncertainty, and which, therefore, appeared to him well fitted to test the accuracy of the tables themselves. Executing the calculation, he found indeed a total eclipse on the year and day in question, and passing near to the spot, *but not over it.* An irreconcilable gap of about 3°, or 180 geographical miles, remains between the most northerly limit of the total shadow, and the most southerly supposable place of Agathocles's fleet. Although this may justly be looked upon as a wonderful approximation between theory and historical fact (indicating, as it does, a correction of only 3′ in the moon's latitude, for an epoch anterior by more than twenty-one centuries to that of the tables), yet it did not escape Mr. Baily's notice, nor did his love of truth permit him to conceal the fact, that no presumed single correction of the tabular elements will precisely reconcile *both* eclipses with their strict historical statement. There seems, however, no reason to doubt that the eclipse of 610 B.C. is, in fact, the true eclipse of Thales. It seems extraordinary that neither Professor Oltmanns, who investigated the eclipse of Thales about two years subsequently, and who came to the same conclusion, nor M. Saint Martin, who read an elaborate memoir on the same subject to the French Institute in 1821, should have made any mention of this very remarkable paper of Mr. Baily.

'The Epitome of Universal History,' of which mention has already been made, was published in 1813, and intended to accompany an Historical Chart published the year before, an extension and improvement of Dr. Priestley's, in which the political alterations of territory are represented through the whole of history. It is an easy and useful work of reference, in which the number and accuracy of the dates, and the utility of the appended tables, are especially valuable. There can be little doubt that the object of this work was much less to produce a book than to systematize and concinnate the author's own knowledge. When such a task is undertaken by a mind at once vigorous in its grasp, and simple, practical and natural in its points of view, it can hardly fail to result in a picture of the subject where all the parts are truly placed, and easily apprehended by the general reader. The chart with its explanation, forming a distinct work, was in considerable request, and went through three editions in five years.

About the 22nd of January, 1814, occurred the celebrated fraud of De Beranger, that being the assumed name of an impostor employed to bring important but false intelligence from the scene of war abroad, for the purpose of influencing the price of the British funds. The imposture was so adroitly managed that many bargains were made on the strength of this intelligence, and much confusion caused. In the detection and exposure of this fraud Mr. Baily had a considerable share, and was appointed by the committee of the Stock Exchange to get up the evidence against the perpetrators,—a task which he is said to have performed in so masterly a manner, that no more complete and conclusive chain of evidence was ever produced in a court. The result of these inquiries, and the steps taken in consequence, were made the subject of three Reports of the above-mentioned committee, drawn up by him, and printed in that and the subsequent year.

From this time astronomy appears to have been continually engaging more and more of his attention. The subject of eclipses and occultations with their connected calculations, together with that of the improvement of the Nautical Almanac, which, whatever might be said on specific points, had certainly at that time begun to fall considerably behind the requisitions of astronomical, and even of nautical science, were those with which he may be said to have commenced his more active astronomical career. But I wish to call attention at present to two pamphlets which he published in 1818 and 1819 respectively, which will afford occasion for some remarks of moment. The first of these is a notice of the annular eclipse of September 20, 1820, whose path lay along the whole medial line of Europe from north to south. Two points in this tract merit our attention. In it he adopts a practice, which he subsequently on a great many occasions adhered to, of introducing in the way of prefatory statement a brief but very clear sketch of the history of the subject, and the observations of former astronomers. These little historical essays are for the most part extremely well drawn up and highly interesting, and show a perfect knowledge of the subjects treated of, drawn from very extensive reading. The next point, and one of more importance, is the studious consideration shown to observers possessed of slender instrumental means, in pointing out to them modes and forms of observation by which those means might be rendered available and useful. At no period of his life himself possessing any large and elaborate instrument or luxurious appliances, one of his constant aims was to render astronomical observation popular and attractive by showing that much of a highly useful character might be accomplished with even moderate instruments. There is no question more frequently asked by the young astronomer who has possessed himself of one or two tolerably good instruments which he desires to employ his time upon, than this, "How can I make myself useful?" nor any which can be more readily answered by a reference to the innumerable notices on almost every point of practical astronomy which Mr. Baily from this time forward for many years continued to scatter profusely to the public, and which have probably done more to create observers, and to cherish and foster a taste for practical astronomy among Englishmen, than any single cause which can be mentioned.

In 1819 he printed for private distribution a translation of Cagnoli's memoir on a "Method of deducing the Earth's Ellipticity from Observations of very Oblique Occultations," with an appendix recommendatory of the method, which is precisely such as requires for its perfect execution only a sufficient telescope, a moderately good clock, and an observer diligent in watching opportunities. This was, no doubt, Mr. Baily's chief reason for translating and distributing it, and for subsequently following it up by his chart and catalogue of the Pleiades, through which the moon had to pass at each lunation in 1822 and the following years, thereby affording admirable opportunities for applying the principle in question. I should not, however, have thought it necessary, in the midst of so many claims on our notice, to draw especial attention to this work, but for one passage in it deeply interesting to all of us. I mean that in which he alludes to the formation of an Astronomical Society as an event earnestly to be desired.

"It is much to be regretted," he observes, "that in this country there is no association of scientific persons formed for the encouragement and improvement of astronomy. In almost all the arts and sciences, institutions have been formed for the purpose of promoting and diffusing a general knowledge of those particular subjects, ... the beneficial effects of which are too evident to be insisted on in this place. But astronomy, the most interesting and sublime of the sciences, ... cannot claim the fostering aid of any society. . . . . The formation of an Astronomical Society would not only afford this advantage, but would in other respects be attended with the most beneficial consequences," &c. &c.

It is thus that coming events cast their shadows before them. But looking back from this point, as it were, to the then embryo state of our corporate existence, it would be ungrateful not to associate with the name of Francis Baily that of Dr. Pearson, as having at or about the same time made the same suggestion. It was happily and speedily responded to, and on Wednesday the 12th of January 1820, a preliminary meeting of the fourteen founders of our Institution took place, which resulted in its final establishment, and in which during the first three years of its existence Mr. Baily filled the office of secretary, in other words, undertook and executed the more laborious and essential duties. The establishment of this Society may, indeed, be considered as a chief and deciding epoch in his life, and to have furnished, though not the motive, yet at least the occasion, for the greater part of his subsequent astronomical labours. Looking to it, as every one must do, as a most powerful instrument for the advancement of the science itself, and the propagation of a knowledge of and a taste for it among his countrymen, he yet appeared to regard it as something more than simply as a means to an end. He made it an object of personal attachment and solicitude, which led him to watch over its infant progress with parental care, and to spare no exertion in its behalf. As years passed on, and as the Institution flourished, (as every institution must do which is constituted on sound principles, whose members are loyal to those principles, and willing to work heartily in its cause,) this sentiment, so far from diminishing, seemed to grow upon him, till he regarded its welfare and interests as identical with his own. I shall reserve a more distinct statement of our obligations to him for a more advanced period of this notice; but in a narrative of his life it becomes impossible from this epoch to separate the Astronomical *Society* from astronomical *science*, in our estimate of his views and motives, or to avoid noticing the large and increasing devotion to its concerns of his time and thoughts. To the Transactions of the new Society he became, as might be expected, a frequent and copious contributor. In the interval between the first establishment of the Society and the year 1825 (the reason for this limit will presently be seen), he contributed five papers, viz.: "On the Meridian Adjustment of the Transit Instrument;" "On the Determination of Time by Altitudes near the Prime Vertical;" "On the Solar Eclipse of September 20, 1820;" "On the Mercurial Compensation Pendulum;" and "On the Determination of Longitudes by Moon-culminating Stars." The two first-mentioned of these turn on somewhat elementary points of astronomical observation, and contain tables, and suggest facilities which he had found useful in his own practice. The eclipse was observed by him at Kentish Town, where, not being annular, he must have felt severely the sacrifice, imposed probably by the calls of business, of the opportunity of witnessing, by a short continental trip, a phænomenon which had engaged so much of his thoughts. His paper on the mercurial pendulum, though practical in its object, was of a much more elaborate kind than anything which had previously emanated from him, with exception of his memoir on the eclipse of Thales. It contains a minute and excellent view of the whole subject of this most useful compensation, is prefaced (*more suo*) with a clear synoptic view of the then actual state of the subject, and goes into the whole subject of the expansion of the materials, the formulæ for determining with more precision than heretofore the proportional length of the mercurial column, and the mode of adjustment both for rate and compensation. This paper must certainly be regarded as a very valuable one, and an astronomer can hardly be said thoroughly to understand his clock who does not possess it. The object of the paper on moon-culminating stars is to recommend, facilitate and render general, that most useful and widely available method of determining the longitude on land.

About this period also Mr. Baily began, and thenceforward continued, to be a frequent contributor to the Philosophical Magazine, published by Messrs. Tilloch and Taylor, of articles interesting in a great variety of ways to the practical astronomer. These articles are so numerous, and so miscellaneous in their subject-matter, that it would be vain to attempt any detailed account of them, within such limits as I must confine myself to. Nor, indeed, is it requisite to do so; as many of them, however useful at the time, have now ceased to present any especial interest apart from their general object, which was that of diffusing among the British public a knowledge of the continental improvements in the art of observing, and the practice of astronomical calculation, and placing in the hands of our observers and computers a multitude of useful tables and methods, which, though sure to work their way ultimately into use, were undoubtedly accelerated in their introduction into English practice by coming so recommended. More special objects were those of recommending to general attention and use certain eminently practical methods, such as those of determining latitudes by the pole-star, longitudes by moon-culminations and occultations, copious lists of which were, on several occasions, either procured from abroad and reprinted here, or calculated by himself for the purpose.

The circulation of notices, also, of other remarkable expected phænomena, with a view to procuring them to be observed,—the description of newly-invented foreign instruments, or of such as had been long known but little used in England,—the analysis of foreign astronomical publications,—everything, in short, which could tend to excite curiosity, to cherish emulation, and to render the British astronomical mind more excursive and more awake than heretofore, found a place in these contributions; of which so constant and copious a fire was kept up, as may well excite our surprise at the industry which sustained, no less than our admiration of the zeal which prompted it.

A volume of astronomical tables and formulæ, printed in 1827 for private distribution (as was frequently his custom), and then largely circulated, but since published with corrections, is of the utmost convenience and value, and will be highly prized by every astronomer who may be fortunate enough to possess a copy, as a work of ready and continual reference for all the data and coefficients of our science. A series of zodiacal charts was also commenced by him, but I am not able to say if more than one plate was engraved.

One of the most practically important and useful objects, however, to which Mr. Baily's attention was about this period turned, was the facilitating, by tables properly contrived for the purpose, the reductions of apparent to mean places of the fixed stars. It seems almost astonishing that these computations, which lie at the root of all astronomy, and without which no result can be arrived at, and no practical observer can advance a single step, should have remained up to so late a period as the twentieth year of the nineteenth century, in the loose, irregular, and troublesome state which was actually the case, and *that* not from their theory being ill understood, but from their practice not having been systematized. Each of the uranographical corrections had to be separately computed by its own peculiar tables, and with coefficients on whose magnitude no two astronomers agreed. The latter evil, indeed, might be tolerated at a time when the tenth of a second of space was not considered of so much consequence as at present; but the calculations were formidable and onerous in the extreme to private astronomers, whatever they might be rendered in public establishments by habit and the use of auxiliary tables. So far as the fundamental stars were concerned, the subject had for some time attracted attention, and had begun to receive its proper remedy by the publication, by Professor Schumacher in Denmark, of their apparent places for every tenth day; and by the laudable exertions of Sir James South in our own country, who, for some years, prepared and circulated similar tables for every day, not without urgent representations of the necessity of taking it up as a public concern, which was at length done. But for stars out of this list, except about 500 somewhat facilitated by Zach, there was no provision of any kind, nor any auxiliary tables to have recourse to; so that sidereal astronomy, beyond the bounds of this favoured list, might be almost said to be interdicted to the private astronomer, owing to the excessive irksomeness of these calculations. This was precisely the sort of case for Mr. Baily to take pity on. He perceived a desert where, with a moderate expenditure of capital, a plentiful harvest might be made to grow, and forthwith proceeded to remedy the evil. Accordingly, with the aid of Mr. Gompertz, he investigated the subject generally, and succeeded in devising a method of arranging the terms of the corrections for aberration, solar and lunar precession, adapted to the purpose, and identical in principle with that adopted by M. Bessel, who, on his part, was at the same time, and, actuated by the same motives, engaged on the subject unknown to Mr. Baily. The latter had actually proceeded to the computation of his tables, when the labours of Bessel reached his knowledge, who had, moreover, included the precession under the same general mode of expression. Mr. Baily, with characteristic frankness and candour, immediately acknowledged this as an improvement in advance of his own idea, and at once adopted and recommended it for general use. He did more, he carried out the idea into a wide and most useful field; and in the Catalogue of the Astronomical Society he has put the astronomical world in possession of a power which may be said, without exaggeration, to have changed the face of sidereal astronomy, and must claim for him the gratitude of every observer. It detracts nothing from the merit of Mr. Baily, or from his claim to be considered the author of this precious work, that the numerical computations were chiefly executed by Mr. Stratford, and the expenses borne by the Astronomical Society. The conception was all his own, and the work prefaced, explained, and superintended, in every stage of its progress, by himself alone. The gold medal of this Society was awarded to him for this useful work.

On the 22nd of February, 1821, Mr. Baily was elected a Fellow of the Royal Society. He was also a Member of the Linnæan and Geological Societies, but I am unable to state the precise date of his election in either.

In 1825 he retired from the Stock Exchange, after a career in which his consummate habits of business, his uprightness, intelligence, and prudence, had established his fortune, and might, if continued, have led him on to any eminence of worldly wealth. But there was that in his disposition which the mere acquisition of wealth could not satisfy. All that he had before done for his favourite science seemed only preparatory to what he might do; and with the best years of his intellectual life before him, and with objects worthy of his efforts now opening to his view in that direction, he resolved henceforward to devote himself to their pursuit, though at the sacrifice of prospects whose attractions always prove irresistible to minds of a lower order. In thus calmly measuring the relative worth of intellectual and worldly pursuits, and stopping short in the full career of success, when arrived at a point which his undazzled judgement assured him to be the right one, he afforded an example of self-command as uncommon as it was noble. In the satisfaction which the decision afforded him, and the complete fulfilment of those aspirations which led him to form it, we have one proof (if proofs be wanting) how entirely a well-chosen and elevated scientific pursuit is capable of filling that void in the evening of life, which often proves so intolerably irksome to men who have retired early from business from mere love of ease or indolence. On no occasion did he ever appear to regret the sacrifice he had made, or even to regard it as a sacrifice.

No desire of listless ease or self-indulgence, however, could by possibility have mixed with Mr. Baily's motives in taking this step; for immediately on doing so he entered on a course of devoted and laborious exertion, which continued without interruption during the remainder of his life, and of which the history of science affords few examples. The mass of work which he got through, when looked at as such, is, in fact appaling, and such that there seems difficulty in conceiving how it could be crowded into the time; the key to which is, however, to be found in his admirably conceived methodical arrangement of every piece of work which he undertook, and his invaluable habit of finishing one thing before he undertook another.

At this epoch, or very shortly subsequent to it, he purchased and took up his permanent residence in his house in Tavistock Place, excellently adapted in every respect both to his future comfort and convenience as a place of abode, and for those important and delicate researches of which it was destined to become the scene; standing, as it does, insulated in a considerable garden, well-enclosed on all sides, and, from the nature of the neighbourhood, free from any material tremor from passing carriages. A small observatory was constructed in the upper part, for occasional use and determination of time, though he never engaged in any extensive series of observation. The building in which the Earth was weighed and its bulk and figure calculated, the standard measure of the British nation perpetuated, and the pendulum experiments rescued from their chief source of inaccuracy, can never cease to be an object of interest to astronomers of future generations.

In endeavouring, according to the best of my ability, to give some account of the astronomical labours of Mr. Baily subsequent to this period, it will no longer be advisable to adhere, as I have hitherto done, to the chronological order in which they were undertaken and executed. It will rather be preferable (with exception of a few memoirs and publications of a miscellaneous nature) to consider them under distinct heads, according as they refer to one or other of the following subjects, viz.—

1. The Remodelling of the Nautical Almanac;
2. The Determination of the Length of the Seconds-Pendulum;
3. The Fixation of the Standard of Length;
4. The Determination of the Density of the Earth;
5. The Revision of Catalogues of the Stars;
6. The Reduction of Lacaille's and Lalande's Catalogues; and
7. The Formation of a New Standard Catalogue.

The Nautical Almanac.—The end of the eighteenth and the commencement of the nineteenth century are remarkable for the small amount of scientific movement going on in this country, especially in its more exact departments. It is not that individuals were not here and there busied in extending the bounds of science even in these, but they met with little sympathy. Their excursions were limited by the general restriction of view which had begun to prevail, and by a sense of loneliness and desertion (if I may use such an expression) arising from that want of sympathy. Mathematics were at the last gasp, and astronomy nearly so; I mean in those members of its frame which depend upon precise measurement and systematic calculation. The chilling torpor of routine had begun to spread itself largely over all those branches of science which wanted the excitement of experimental research. I know that I have been blamed on a former occasion for expressing this opinion, but it is not the less true, though we may now happily congratulate ourselves that this inanimate period has been succeeded by one of unexampled activity. To break the dangerous repose of such a state, and to enforce that exertion which is necessary to healthy life, there is always need of some degree of friendly violence, which, if administered without rudeness, and in a kindly spirit, leads at length the revived patient to bless the disturbing hand, however the urgency of its application might for a moment irritate. It is in this light that we are to regard the earnest and somewhat warm remonstrances of Mr. Baily on the deficiencies which had long begun to be perceived and felt in the Nautical Almanac, in its capacity of an astronomical ephemeris.

The subject once moved gave rise to a great deal of discussion, from more than one quarter, which was from time to time renewed for some years; but as I have no intention to make this notice an occasion of dilating on any matter of a controversial nature, I shall merely add that, on the dissolution of the late Board of Longitude, followed almost immediately by the death of Dr. Young, on whom the charge of its superintendence rested (the new Berlin Ephemeris, by Encke, having also recently appeared, in which many of the principal improvements contended for were adopted), it seemed fitting to the Lords Commissioners of the Admiralty to place unreservedly before the Astronomical Society the subject of a complete revision and remodelling of that great national work—a high proof of confidence, which speaks volumes for the good sense, prudence, and activity which had continued to pervade its administration during the ten years which had now elapsed since its first institution.

It is hardly necessary to add that this important business received the most unremitting attention from Mr. Baily, as well as from every other member of the committee, in all its stages. To him also was confided the task of drawing up the final report of the committee appointed to carry out the wishes of the Admiralty, which will be found in the fourth volume of our Transactions, and which is a model of good sense, clearness, and lucid arrangement. The report was immediately acted upon by government, and the result was the present British Nautical Almanac; a work which, if it continue to be carried on, as I trust it ever will, on the principles which prevailed in its reconstruction, will remain a perpetual monument to the honour of every party concerned in it.

The Pendulum.—The seconds-pendulum having been constituted the legal source from which, in the event of the loss of the national standard of length, the yard might at any time be recovered, it may be easily imagined with what intensity of interest the announcement was received among all conversant with these fundamental determinations, that a very material correction had been entirely overlooked in the reduction of the experiments, on which the Act of 5 Geo. IV. c. 74 was founded. This correction is, in fact, no other than the correction due to *the resistance of the air*, and, placed in this light, it would seem somewhat wonderful that such an oversight could have been committed; but it had been customary to consider the effect of resistance on the time of vibration to be wholly confined to its influence in diminishing the arc, and this secondary effect being allowed for in the formulæ employed to compute what is called the correction for the arc of vibration, the primary or direct effect of resistance dropped altogether out of notice, or, rather (owing to an entire misconception of the nature of the mechanical process by which resistance is operated), had been supposed to be altogether inappreciable in its amount. The real effect of resistance, though under a somewhat confused statement as to its nature, had, however, been long before noticed, and its amount even ascertained with tolerable correctness by the Chevalier Buat, in 1786; but his experiments and theory had so entirely fallen into oblivion as to have escaped the notice not only of Captain Kater, but of his own countrymen, Borda and Biot, and were unknown even to Bessel himself, who, in 1828, rediscovered the correction in question, and, for the first time, made it an integrant feature in the modern system of pendulum reductions. The light in which this correction was placed by Buat, and even in some respects by Bessel, tended not a little in my opinion to obscure the clear perception of its nature, by representing it as due to a certain portion of air adhering to and bodily *dragged along* by the pendulum in its motion, thus adding to its inertia without adding to its relative weight when corrected for buoyancy; and in this view, also, Mr. Baily regarded it. That this is not a complete and adequate view of the subject is easily made a matter of ocular inspection, by causing a pendulum to vibrate, or any body to move, near the flame of a candle, when it will be at once evident that the movement of the air consists in the continual transfer of a portion of air from the front to the rear of the body, by performing a circuit half round it. Its hydrodynamical investigation, therefore, is of an infinitely higher order of difficulty than the ordinary problems of resistance, which turn upon a theory of molecular impulse, simple indeed, but very far from satisfactory. It properly refers itself to the theory of sound, and has, in fact, been so investigated in an admirable memoir by Poisson*.

But to return from this digression (which, however, will not have been without its use, if it shall tend to diffuse clear conceptions of the subject, and to disentangle from one another corrections which seem to have got unduly mixed up together in the minds of practical inquirers). No sooner were the ideas of M. Bessel promulgated in England, than Captain Sabine, whose attention was pointedly directed to a subject which had occupied so large and active a portion of his life, resolved to ascertain the true amount of this new, or newly mentioned, correction, in the only way in which it could be effectually done, viz. by vibrating the pendulum *in vacuo*, which he accordingly effected by a series of highly interesting experiments, carried on at the Royal Observatory at Greenwich, and recorded in the Philosophical Transactions, in a paper read March 12, 1829. His result makes the total reduction to a vacuum about one and two-thirds of that usually called "the correction for buoyancy." It should, however, be borne carefully in mind that the particular correction now in question has, in fact, nothing whatever to do with the buoyancy correction, either in its mode of production or its form of expression, and ought, therefore, to be very studiously kept apart from it

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* If this view of the subject be correct, as I am persuaded it is, it seems not impossible that, by making a section of the pendulum coincident in form with the "wave-formed outline" of Mr. Russel's ships, the resistance correction might be annihilated altogether, or so nearly as to render it quite inappreciable.

I trust that, in what is said above, I shall not be supposed to undervalue M. Bessel's analytical treatment of this intricate problem, especially as it conducts to results which, regarded as a first approximation, represent sufficiently well the results of experience.

in all theoretical views, though of course they must be numerically amalgamated in the "reduction to a vacuum."

Meanwhile the attention of Mr. Baily had, about the same time, been called to the pendulum, in consequence of the contemplated expedition about to sail under the command of Captain Foster, on that memorable and most unfortunate expedition which cost him his life. It was on this occasion, and with a view to the use of this expedition, that Mr. Baily (still acting for the Astronomical Society, whose aid had been requested in suggesting useful objects of inquiry) devised that capital improvement in the system of itinerant pendulum observation, which consists in making each transferable pendulum a convertible one, by the simple addition of another knife-edge, and in doing away with extra apparatus of tail-pieces, sliders, &c., by the initial adjustments of the instrument. And I may here incidentally remark, that the general principles of reducing, as far as possible, the number of moveable parts in every instrument intended for standard determinations of whatever kind, is one which cannot be too strongly recommended, and has been successfully acted on by the present Astronomer Royal in more than one recent construction. Two pendulums, a copper and an iron one, on Mr. Baily's principle, were furnished by the Society for this expedition, an account of which may be found in the Notices of the Society for June 13, 1828.

The adjustment and trial of these pendulums previous to the sailing of the expedition, were performed by Mr. Baily at his own house, and, thus engaged in actual experiment, he at once became led on into a minute examination of all the possible sources of practical error in the experiments and consequent uncertainty in the important results of which they had become the basis. It was in this stage of his experience that he became acquainted with Prof. Bessel's results, which determined him (as it had already done Captain Sabine) to go into the whole subject of the new correction by experiments performed *in vacuo*. But not content with assuming any fixed proportionality between it and the buoyancy correction, he resolved so to vary the form, magnitude, and materials of the vibrating masses, as to make its true nature and amount an object of inductive experimental inquiry: thus, though adopting the language of Buat and Bessel, disengaging himself in effect from any theoretical view of the *modus operandi* or mechanical process by which the effect was produced.

The result of these inquiries was a very elaborate and masterly paper read to the Royal Society, on the 31st of May, 1832, containing the results of experiments in air and *in vacuo*, on upwards of eighty pendulums of various forms and materials, by which the new correction is clearly shown to depend not only on the dimensions but on the form and situation of the vibrating body. Independent of the excellence of this paper as a specimen of delicate experimental inquiry and induction, in which, to use the expression of one best capable of appreciating and admiring them, his generalizing powers seem to have been held in abeyance till the right moment for their exercise arrived, it had the further merit of bringing into distinct notice a number of minute circumstances, chiefly relative to the mode of suspension (important, however, from their influence on results), which it is absolutely necessary to attend to in these delicate and difficult inquiries, if the pendulum be ever again resorted to as a means of verifying or fixing anew the standard of length.

The return of the Chanticleer in 1831, without its lamented commander, threw the whole task of arranging and digesting for publication Captain Foster's pendulum observations on Mr. Baily—a labour of love, prompted by the warmest friendship, and which he executed in the spirit of one determined to erect a monument to the fame of that truly amiable and talented officer, of the most durable and precious materials. His Report on the subject to the Admiralty was presented by the Lords Commisioners to the Council of the Astronomical Society, and printed at the expense of Government as the seventh volume of our Transactions. In this Report the observations are given in full, and with the most scrupulous fidelity, and those at each of the numerous stations discussed with the utmost care. The final re-examination of the pendulums in London was also personally executed by Mr. Baily, and the whole series of stations combined into a general result, which gives for the ellipticity of the earth $\frac{1}{289 \cdot 48}$. Not content with this, he has here also collected into one synoptic view the results obtained at various stations all over the globe with the invariable pendulum, by observers of all nations, so as to place them in comparison with each other, and to deduce from them a general result. Of these, by far the most numerous and prominent, in every respect, are those of our own countrymen, Captains Foster and Sabine; and nothing can be more gratifying, in estimating our own national share in this sublime application of science, than to find these principal authorities, whose observations were made and reduced with the most absolute independence of each other, agreeing at all the stations where they admit of comparison, with a precision truly admirable. In fact, the greatest disagreement of each of their final results, from a mean of them both, amounts to a quantity less than half a vibration out of 86,400, or in a mean solar day*.

Standard of Length.—From the pendulum to the standard of length, or the fixation of the *scientific unit*, the transition is easy, and, in Mr. Baily's case, was unavoidable. For, being once satisfied by experience of the innumerable minute circumstances on which perfect precision in these inquiries depends, and finding the parliamentary enunciation of the relation between the conventional and natural standards nullified, as it were, under his eye, he felt himself irresistibly urged to inquire how far the conventional unit itself might be depended upon, and within what limits of error it might certainly be reproduced in copies. His first step in this direction was to obtain the most perfect possible representative of this unit, and (as the Astronomical Society was now identified with almost all his undertakings) justly considering the possession of such a standard by that body as a thing in itself desirable, and the instrument itself likely, if thoroughly well executed, to become in its hands of universal scientific reference, he procured himself, to be named by the Council, a Committee for superintending its execution, and comparing it with the most authentic standards at present existing in this country. Perhaps there is no subject of inquiry more perplexing, or one whose investigation calls for more patience and perseverance, than the detection and exact estimation of those minute sources of error which influence these delicate measurements, which can only be satisfactorily performed by endless repetition and systematic variation of every circumstance by which error can possibly be introduced. Another and peculiar source of annoyance, and even vexation, consisted in the rough and careless usage to which those precious instruments, on which the conservation of our national units depends, had been subjected in too many instances, by which rude and ignorant hands had irrecoverably marred some of those refined productions of human workmanship, which ought not even to be approached but with precaution, or touched but with the utmost delicacy. Few things seem to have excited Mr. Baily's indignation more than the continual occurrence of evidence, only too palpable, of the small respect in which these standards appear to have been held by those under whose protection they had been placed, and of the violence which has been repeatedly suffered to be perpetrated on them.

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* The stations of comparison are London, Maranham, Ascension, and Trinidad. Taking London for a term of departure, each station affords a ratio whose extremes (see Report, p. 86) differ only by 0·0000103, the half of which multiplied by 86,400 gives $0^{s}{\cdot}44446$.

I shall by no means go into any minute analysis of the admirable "Report" to the Council of this Society, which contains his account of the construction of our standard scale, its comparison with the parliamentary standard, and its most authentic existing representatives—and with the French metre, as we have it represented in this country by two platina metres, in the possession of the Royal Society; or the means taken to secure it from loss, by the formation of carefully compared copies, two of which have been sent abroad, and two retained in England. Suffice it to say, that the delicacy of the means employed, the minuteness of the precautions used, and the multiplicity of the comparisons, surpassed everything of the kind which had ever before been done in this country. This Report, too, is valuable in another way. Under the modest title of 'A Short History of the Standard Measures of this Country,' it presents a summary of the subject so complete as almost to obviate the necessity of referring elsewhere for *historical* information*.

The immediate result of this useful and most laborious undertaking has been to put this Society in possession of, perhaps, the most perfect standard measure and divided scale in existence, in which every division, even to the individual inches, has been micrometrically verified, and their errors ascertained and placed on record. It would almost seem, too, as if a prophetic spirit had actuated the undertaking, and urged it to its completion without any of those delays which so often and proverbially attend the construction and optical examination of delicate instruments. For the comparison of the new scale with the imperial standard yard had hardly been completed six months, when the latter, together with the other original standard by Bird (that of 1758), as well as the imperial standard of weight, were destroyed in the conflagration of the Houses of Parliament in October 1834. Thus the operation in question has been the fortunate means of preserving, to the latest posterity, that unit which has pervaded all our science, almost from the first dawn of exact knowledge.

The scientific unit is indeed preserved; but the nation remained, and remains up to this moment, without a legal standard either of weight or measure. In the early part of 1838, however, in consequence (as I have been led to understand)

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* Mr. Baily was assisted in the actual comparisons by several Fellows of the Society, among whom the late Lieut. Murphy was conspicuous, an observer whose temper and scientific habits peculiarly fitted him for co-operating with Mr. Baily, and whose name would probably have occurred more than once in this memoir but for his untimely death, which took place in the service of Astronomy in a distant region, and was probably the unfortunate consequence of over-exertion in its cause.

of some communications on the subject between Mr. Baily, Mr. Bethune, and the Astronomer Royal, the latter was induced to draw the attention of Government to the subject, an occasion having arisen which rendered the mention in an official form unavoidable. And on the 11th of May of the same year a commission was appointed, consisting of seven* members (Mr. Baily being one), to report on the course most advisable to be pursued under these circumstances. To this duty, which involved the hearing of a vast deal of evidence and much personal attendance, Mr. Baily gave his unceasing attention; suggesting many valuable points, both practical and theoretical; and, on the Report of the Commission being agreed on, and the practical formation of new standards, in conformity with the view therein taken of the subject, being referred by Government to the same commissioners, Mr. Baily undertook, to the general satisfaction of the whole body, and at their particular request, the delicate and important task of reconstructing the standard of length—a task which, unhappily, he did not live to complete. On whomsoever may devolve the completion of this standard, it will be satisfactory to the members of this Society to know that, among the evidence adduced for its restoration, the scale prepared for it by Mr. Baily necessarily forms a most important and prominent feature.

Density of the Earth.—The accurate determination of one fundamental quantity naturally leads to inquiry into others. To make our globe the basis of measurement for the dimensions of the planetary system and of the visible universe, its form and magnitude must first be accurately known. To make it afford a scale by which the masses and attractive forces of the sun and planets can be expressed in terms conveying a positive meaning, its density must be ascertained, as compared with that of substances which occur on its surface, with which our experience is familiar, and from which our notions of material existence are drawn. The fine experiment of Cavendish, confirmed as it was, in its general result, by the operations on Schehallien, had satisfactorily demonstrated the continuity of the Newtonian law of gravity, from such vast distances as astronomy is conversant with, through the intermediate steps of the diameters of the earth, and of a mountain, down to those minute intervals which intervene between the parts of a philosophical apparatus, and their agreement within as moderate limits as could have reasonably been expected, had even led to something like a probable estimate of the earth's density, which, however, could never be regarded as

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* An eighth was subsequently added.

satisfactory, otherwise than as a first step towards more precise determinations. Mr. Baily's labours, therefore, on the pendulum were hardly brought to a conclusion when he was led to enter upon this subject, the immediate occasion of his doing so being an incidental suggestion at the council-table by Mr. De Morgan, of the desirableness of repeating the experiment of Cavendish*—a suggestion immediately seconded both by the Astronomer Royal and by Mr. Baily. The experience of the latter had shown him how indispensably necessary, in such inquiries, are extensive repetition and variation of circumstance. The Schehallien experiment, from its very nature, admitted of neither; and, on carefully examining Cavendish's record of his own experiment, he found abundant reason to perceive how much was left to be desired, in both these respects, even in that form of the inquiry.

In resolving on a repetition of this experiment, the difficulty of the undertaking itself, and his own preparation for it, must have been, and no doubt were, very seriously considered. However confident in his own resources and perseverance, it was no holiday task in which he was now about to engage. The pendulum experiments, with all their delicacy, could hardly be regarded as more than an elementary initiation into the extreme minuteness necessary for this inquiry. There are two branches of research in physical astronomy which task to the utmost the resources of art, the delicacy of manipulation, and the perseverance of the inquirer—the parallax of the fixed stars, and the density of the earth. In both, an immense object has to be grasped by the smallest conceivable handle. But of the two problems, the latter is probably that which throws the greatest burden on the inquirer, inasmuch as it is not merely a series of observations to be carried on under well-ascertained circumstances and known laws, but a course of experiments to be entered on for eliminating or controlling influences which war against success in every part of the process, and where every element, nay, even the elementary powers of heat, electricity, magnetism, the molecular movements of the air, the varying elasticity of fibres, and a host of

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* *Fiat justitia, ruat cœlum.* The original design of this beautiful experiment was Mitchell's, who actually constructed the identical apparatus which Cavendish used, but died before he could execute the experiment. The apparatus came, after his death, into the possession of the Rev. W. H. Wollaston, D.D., *who gave it* to Cavendish, who used it, indeed, to excellent purpose, but who assuredly neither devised the experiment, nor invented, nor constructed, nor even, so far as I can perceive, materially improved the apparatus. All this is distinctly stated by Cavendish himself, who is, therefore, no way to blame for any misconception which may prevail on the subject.

ill-understood disturbing causes, set themselves in opposing array in their most recondite and unexpected forms of interference. Nor could it have been overlooked by him that it was necessary, not merely to do over again what Cavendish had done before him, a thing in itself not easy, but to do it much more thoroughly and effectually.

Mr. Baily, however, was not to be discouraged by such considerations. He saw that there existed a blank in our list of exact data which it was necessary to fill, and he felt himself in possession of those gifts of nature and position which enabled him to fill it. Accordingly in 1835, on the occasion above alluded to, the Astronomical Society appointed a committee to consider the subject; and Mr. Baily having offered to perform the experiment, in 1837, the Government (at the instance of Mr. Airy) granted the liberal sum of 500*l.* to defray the cost of the experiment.

This great work was brought to a satisfactory conclusion in 1842, and a complete account, with a full detail of the experiments, printed in one volume, published in 1843, forming the fourteenth of the series of Transactions of this Society. The experiments were varied with balls of different materials, and with suspensions no less various, combined so as to form no less than 62 distinct series, embodying the results of 2153 experiments; and which, formed into groups according to the nature of the combination, afford 36 distinct results, taking those only in which the balls were used, the extremes of which are 5·847 and 5·507, and the most probable mean 5·660, none of them being so low as Cavendish's mean result, 5·448. The probable error of the whole (0·0032) shows that the mean specific gravity of this our planet is, in all human probability, quite as well determined as that of an ordinary hand-specimen in a mineralogical cabinet,—a marvellous result, which should teach us to despair of nothing which lies within the compass of number, weight and measure. I ought not to omit mentioning that, of all the five determinations of this element we possess, Mr. Baily's is the highest*.

Though it would be equally remote from my present purpose, and superfluous in presence of such an assembly, to enter

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* The five determinations alluded to are, in order of magnitude, as follows:—

Schehallien experiment from Playfair's data and calculations	Max...4·867 Min...4·559	Mean 4·713
Carlini, from pendulum on Mont Cenis, corrected by Giulio		4·950
Reich, repetition of Cavendish's expt. (most probable combination)		5·438
Cavendish, computation corrected by Baily		5·448
Baily (most probable combination)		5·660

minutely into a discussion of these experiments, there is one point in their conduct which I cannot pass over in silence. The experiments had been carried on for eighteen months, a vast number of preliminary trials had been made, and upwards of 1000 registered results obtained, when it became apparent that the coincidence of Cavendish's results, one with another, was rather to be attributed to the paucity of his trials than to any especial accuracy in his observations or felicity in his mode of operating. Even in the few experiments made by Cavendish, discordances had shown themselves, of which no account could be given other than by reference to the movements of included air; but, on Mr. Baily's extensive scale of operation, the limits of disagreement obviously arising from this cause became so enormous as to render it hardly possible to draw any line for the reception and rejection of results. In fact, at one period he had almost begun to despair of bringing the matter to any positive conclusion. The happy suggestion of Mr. Forbes, *to gild* the torsion-box and leaden balls, at once dispelled all this vagueness and uncertainty, and reduced the results to a high degree of uniformity*. Most experimenters would have been content to reject the discordant results. Mr. Baily unhesitatingly sacrificed the whole and began anew, without appearing to regard with an instant's regret the time and labour lost. The gold medal of this Society was awarded to him for this important memoir.

Revision of Catalogues of the Stars.—The contributions of Mr. Baily to this branch of sidereal astronomy are so numerous and so important, as alone would suffice to rank him among the greatest benefactors to the science, since, without being himself an observer, he has conferred, by his indefatigable industry and perseverance in collating authorities, rescuing original observations from oblivion, and rectifying printed errors, a vast and unhoped-for accession of value to the works of all those on whom he has commented. In fact, this, which may be termed the archæology of practical astronomy, formed his staple and standing work, which, though from time to time interrupted by other subjects, was always resumed; always with increasing interest, and always on a larger and more effective scale, up to the very year of his death. His object appears to have been, so far as is now practicable, to destroy the gap which separates us from the elder astronomers, and to multiply, or at least to preserve from

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* This was not, however, the *only* precaution used. Mr. Baily carried out the suggestion, by swathing the torsion-box in flannel, and applying over this defence an exterior *gilded* case. Should the experiment ever again be repeated, it should be attempted *in vacuo.*

further destruction, the links which connect us with them; to ascertain *all* that *has really been* recorded of the stars, and to make that totality of knowledge the common property of astronomers—a precious and a pious labour, of which we have no examples, except in that spirit of loyal reverence which prompted Ptolemy to secure from oblivion the observations of Hipparchus, and make them the foundation of all future astronomy; and in that which animated Bessel, when on the basis of Bradley's observations he may be said to have afforded the means of reconstructing the whole fabric of the science.

The catalogues which Mr. Baily has re-edited are those of Ptolemy, Ulugh Beigh, Tycho Brahe, Halley, Hevelius, Flamsteed, Lacaille and Mayer; a mass of commentation, expurgation and minute inquiry, before which the most stout-hearted might quail, since there is not one of them in which each individual star has not been made the subject of a most scrupulous and searching examination, and in which errors that had escaped all prior detection,—errors of reading, errors of entry, of copying, of calculation, of printing, out of number,—have not been detected and corrected. But for these labours, the catalogues of Ptolemy and Ulugh, indeed, must have remained sealed books to any but professed antiquaries; and although we can now hardly ever have occasion to appeal to these earliest authorities for any practical purpose, we cannot but look on the labour thus cheerfully bestowed in embalming and consecrating their venerable relics as the sure pledge that our own works, if really worthy, will not be suffered to perish by time and neglect.

But while we admire both the diligence and the scrupulous exactness, of which the notes appended to these catalogues bear ample evidence, we must not omit to mention that there are two of them, those of Mayer and Flamsteed, in respect of which Mr. Baily's researches have been pushed far beyond the mere duties of comparison and comment, having been extended to the conservation and minute examination of the original records from which the catalogues were formed. In the case of Mayer, his influence with the late Board of Longitude secured the publication of the original observations of that eminent astronomer at Göttingen, which had never before seen the light*. In the case of Flamsteed, his labours were much more extensive and require a more particular statement, inasmuch as not only Flamsteed's greatest work, the 'British Catalogue,' found in him its restorer to that high rank, as an astronomical document, which it is justly entitled to hold, but

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* In 1826.

the fame and character of its author their defender and rescuer from grievous misapprehension and misstatement.

In 1832 it happened, by a most singular coincidence, that Mr. Baily became aware of the existence, in the possession of his opposite neighbour in the same street, E. Giles, Esq., of the whole of Flamsteed's autograph letters to Abraham Sharp, and was permitted to peruse and copy them. Their perusal convinced him that Flamsteed's life, astronomical labours, and personal character, had never been fairly placed before the world, and induced him to examine with care the mass of his papers preserved (or rather neglected and mouldering) at Greenwich. His first care was to arrest the progress of their further decay. His next, to avail himself of the original entries of the observations, and of the manuscript records of the computations founded on them, to trace out the sources, and to rectify the numerous errors and inconsistencies of the 'British Catalogue' as it then stood before the world, and to present it to the public under quite a new aspect—as a noble monument of its author's skill and devotion, and a work worthy of the age and country which produced it. Among the papers thus examined, however, were also found an almost complete autobiography of Flamsteed, and a voluminous correspondence illustrative of those points so painfully at issue between Flamsteed, Newton, and Halley, relative to the publication of the Catalogue and observations, and to other matters of a more personal nature, which had hitherto all along been stated in an infinitely more unfavourable light towards Flamsteed than that which appears from Mr. Baily's thorough and voluminous exposition of the whole affair, and the evidence of the almost innumerable letters which he has printed at length, truly and properly to belong to them. Indeed it seems impossible not to admit, on the evidence here produced, that great and grievous injustice was done, and hardship imposed, in these transactions on Flamsteed, whose character stands forward, on the whole showing, as that of a most devoted and painstaking astronomer, working at extreme disadvantage, under most penurious arrangements on the part of government, making every sacrifice, both personal and pecuniary, and embroiled (as I cannot help considering, by the misrepresentations and misconduct of Halley) with the greatest man of his own or any other age, holding a position with respect to the Observatory, as Visitor, which, under mistaken impressions of the true bearings of the case, might cause severity to assume the guise of public duty.

The volume which contains this important work of Mr. Baily was commenced (as we have seen) in 1832, and published in 1835, a rapidity of execution truly astonishing, when we consider that the volume extends to nearly 800 pages quarto; that the notes to the Catalogue alone occupy no less than 144 of them closely printed, not a line of which but involves some question of identity, of nomenclature, of arithmetical inquiry, or of reference to other authorities; that the examination and selection of the letters and other biographical matter for publication was a matter of the utmost delicacy and responsibility; and that the preface, which contains Mr. Baily's own summary of Flamsteed's life, the introduction to the Catalogue and the Supplement, in further vindication of Flamsteed's character and justification of his own views of it,—are all of them works of a very elaborate nature and of the highest interest.

Catalogues of Lacaille and Lalande.—But Mr. Baily's views were not confined to the mere correction of existing catalogues. The labour of the commentator and collator, which has filled and satisfied so many minds, was to him only a means to an end of real practical importance. His aim was to render readily available to every astronomer all recorded observations of the sidereal heavens which could be depended on. Two great masses of observation might be said to exist buried under their own weight, and affording matter of grief and reproach to astronomy, now to be exchanged for congratulation and triumph. These were Lacaille's observations at the Cape of nearly 10,000 stars, and those of D'Agelet and Michel Lefrançais Lalande at Paris, of nearly 50,000. Neither of these collections of observations had been more than partially reduced. Lacaille himself had performed this task for 1942 of his stars. A considerable number of the stars of the *Histoire Céleste* (Lalande's observations) had also been reduced and catalogued by Bode. But the great mass of both remained unreduced and unarranged, though it is true that Lacaille had accompanied each page of his observations with a table of reductions; and that in 1825, Professor Schumacher had published and dedicated to this Society a volume of assistant tables, enabling any one, with little trouble, to reduce any single observation of the *Histoire Céleste*. Still they remained unreduced, and therefore useless, except on those rare occasions when, for special reasons, it might be necessary to search out and reduce any particular object.

Thus was a treasure of great value held in abeyance. This Mr. Baily perceived, and after some correspondence with the French Bureau des Longitudes, which, however, led to no result, he resolved to bring the subject before the British Association. That liberal and energetic body at once acceded to his views, and in 1838 appointed two committees, each with funds at their disposal, to execute the reductions and prepare the catalogues. The reduction and arrangement of Lacaille's stars was executed under the superintendence of Mr. Henderson, that of Lalande's under Mr. Baily, the arrangement of the work in both (if I mistake not) having been effected on a plan concerted and matured by the latter. Both works were reported as complete (the prefaces alone excepted) in 1843, and it only remained to provide for their printing. This also was done by the liberality of the British Government, who assigned 1000*l.* for the purpose; and this work was especially placed under Mr. Baily's direction. These catalogues, unhappily, he did not live to see published. The printing, however, of each was found advanced at his decease as far as 8320 stars[*], and is now continuing under the more immediate inspection and superintendence of Mr. Stratford.

Catalogue of the British Association.—I have yet to speak of another and a magnificent work undertaken and brought to a successful conclusion by Mr. Baily; a work which, perhaps, deserves to be considered as the greatest boon which could have been conferred on practical astronomy in its present state, and whose influence will be felt in all its ramifications, giving to them a coherence and a unity which it could hardly gain from any other source. I allude to the general standard catalogue of nearly 10,000 stars, which the British Association are about to publish, at the instance of Mr. Baily. The plan of this great and useful work is an extension of that of the Astronomical Society, of which I have already spoken. The stars (selected by Mr. Baily) form a universal system of zero-points, comprehending probably every star of the sixth and higher magnitudes in the whole heavens. All the coefficients for their reduction are tabulated, and the greatest pains bestowed upon their exact identification and synonymes in other catalogues; so that this, in all human probability, will become the catalogue of universal reference. It is preceded by a valuable preface from the pen of Mr. Baily, his last contribution to astronomical science.

A very important feature of this and the two catalogues last noticed is their nomenclature. The system adopted is the same in all; and *that*, a system not capriciously adopted or servilely copied, but founded on a most searching and careful revision of all existing catalogues, and of the charts of Bayer, Flamsteed and Lacaille, rectifying the boundaries of constellations which had become strangely confused, correcting in-

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[*] The total number of stars in the two catalogues respectively, will amount to 9766 and 47,400.

numerable errors of naming, numbering, and lettering, and reducing, in short, to order and regularity a subject which had become almost hopelessly entangled. The way is thus at length opened to a more rational distribution of the heavens into constellations, and that final step which must sooner or later be taken, of introducing a systematic nomenclature into sidereal astronomy, rendered easy, whensoever astronomers shall be prepared on other grounds to take it. The trouble and difficulty attending this part of the work exceed what any one unused to such tasks can easily imagine.

There are two papers by Mr. Baily relating to sidereal astronomy, of which mention ought to be made here; viz. one "On the Proper Motions of the Stars," which was read before the Astronomical Society on the 9th of December, 1831, in which a list of about 200 stars, whose proper motion appears sufficiently sensible to merit further inquiry, is discussed. In drawing up this list, he was much aided by a series of transit observations by Dr. Robinson, observed expressly with a view to this inquiry. But as no positive conclusion of a general nature is arrived at in this memoir, and as the subject is yet hardly ripe for a complete discussion, I shall dilate no further on it. The other paper to which I allude (which was read also to this Society on the 14th of November, 1834) states the result of an examination of Dr. Halley's MSS. at the Royal Observatory. The appointment of Astronomer Royal was held by Halley twenty-two years, and though for the first two of them the observatory was entirely deprived of instruments, and for the next four a five-feet transit only was available, it might, at least, have been expected that he should have used diligently the means he did possess, or, at all events, have recorded the observations he did make in a regular, methodical, and intelligible manner. From Mr. Baily's examination of these papers, however, this appears to have been very far indeed from the case; and that, with the exception of differences of right ascension between the moon and planets and neighbouring fixed stars, which alone he seems to have considered worthy of attention, little of interest could be expected to repay the trouble and expense of their reduction. Of these papers Mr. Baily, ever anxious for the preservation of records, and mindful of the dormant value which they so often possess, obtained from the Admiralty a transcript, which, being carefully collated with, and corrected by, the original MSS., is now deposited in our library.

The mention of the Royal Observatory induces me to notice here a change which has been lately made in the constitution of that noble institution, by a revision of the royal warrant, defining the number and mode of appointment of the visitors, and placing this Society on a similar and equal footing with the Royal Society in the discharge of that important duty. This change was made at Mr. Baily's suggestion, with the entire concurrence, however, of the then President of the Royal Society, as to its expediency, on the occasion of the demise of the crown by the death of George IV., which rendered a new warrant necessary. The new system has been found to work admirably well, and to have secured a perfect harmony of feeling between the visitors and the eminent individual who now fills the post of Astronomer Royal, as well as entire confidence in the recommendations and suggestions of that body on the part of Government. Aware, as all are now, of the fatal and soporific influence of routine in public institutions, they have only henceforward to guard against the opposite extreme; to which end, they cannot do better than take for their guide and example that admirable combination of energy, gentleness and judgement, which distinguished Mr. Baily, no less on every public occasion than in his conduct as a visitor, in which capacity, under both the old and the new system of visitation, he was an invariable attendant, being never absent during a period of twenty-eight years from any meeting but the last.

About the end of June 1841, an accident happened to him which had very nearly proved fatal. Crossing Wellington Street for the purpose of taking some MSS. to a printer, a deafness, which had for some years been increasing on him, rendered him unaware of a rider recklessly urging his horse to furious speed, who either did not see him or was unable to pull up. In consequence a collision took place, and Mr. Baily received a stunning fall, accompanied with a severe scalp-wound. So violent, indeed, was the shock, that he lay for a whole week senseless, and for an equal period after his life was considered in imminent danger. His sound and excellent constitution, however, carried him through it, and no ill consequences remained. By the end of September he was enabled to resume the observations of the Cavendish experiment, which this unfortunate occurrence had interrupted, and a few weeks' residence in the country completed the cure.

On the 8th of July, 1842, he was gratified by the observation of a phænomenon which it had from his youth upwards been one of his most ardent wishes to witness, viz. a total eclipse of the sun. To this he looked forward, indeed, with a curiosity peculiarly intense; having, on the occasion of the annular eclipse of May 15, 1836, which he travelled to Scotland to observe, and which he succeeded in observing under very favourable circumstances at Jedburgh, noticed a very singular phænomenon attending the formation of the annulus. I mean the appearance of beads of light, alternating finally with long, straight, dark threads, cutting across the narrow line of the sun's limb, which he described in a highly interesting paper read to this Society on the 9th of December, 1836. On the occasion of the total eclipse he selected Pavia for his station, that town lying in the path of the centre of the shadow. There, by especial good fortune, he obtained an excellent view of it, and there he witnessed, not only a repetition of the phænomenon of the beads, but that much more astonishing and previously unheard-of one, of the flame-like, or conical rose-coloured protuberances, seen to project, as it were, from the hidden disc of the sun beyond the border of the moon. This truly wonderful appearance (which was corroborated by several other observers at different places, among others by Mr. Airy at Turin) was described by him, on his return from Italy, in a paper read to this Society on the 11th Nov. 1842; and it is not a little singular that the two most remarkable solar eclipses on record should thus have furnished the subjects of his first and last astronomical memoirs,—

> "Servatur ad imum
> Qualis ab incepto processerit."

On his return from this journey he resumed his astronomical labours on the catalogues, as we have seen, which he continued, as well as his usual unremitted attendance to the business and at the meetings of this Society, till the spring of the present year, when his health began to decline, and several weeks of serious illness, a thing utterly unknown to him at any former period of his life (except as a result of accident), gave intimation of a failing constitution. For the first time since the reorganization of the visitation of the Royal Observatory he was unable to attend the annual meeting of the Visitors in June. He, however, rallied somewhat, so as to be able to be present at the commemoration at Oxford on July 2, on which occasion the honorary degree of Doctor of Civil Law was conferred on him by that university, as well as on Mr. Airy and Professor Struve. On his return from Oxford his health again rapidly declined, and all efforts of medical skill proving unavailing to relieve an internal complaint, which had at length declared itself, he expired, after a protracted, but happily not painful illness, during which he was fully sensible of his approaching end, in a state of the utmost calmness and composure, at half-past nine o'clock in the evening of the 30th of August, at the age of seventy years and four months.

In passing in review, as I have attempted to do, the scientific works of Mr. Baily, and noticing, as we cannot help doing, the gradual expansion of his views, and the progressively increasing importance of the objects they embraced, we are naturally led to ask by what means he was enabled thus to live as it were two distinct lives, each so active and successful, yet so apparently incompatible with each other? how, in what is generally regarded as the decline of life, he could not only accomplish so much with such apparent ease to himself, but go on continually opening out wider and wider plans of useful exertion in a manner which seems only to belong to the freshness of youth? The answer to such an inquiry is, no doubt, partly to be found in his uninterrupted enjoyment of health, which was so perfect that he has been heard to declare himself a stranger to every form of bodily ailment, and even to those inequalities of state which render most men at some hours of the day or night less fit for business or thought than at others. But though this is in itself a blessing of the most precious kind, and, if properly used, a vantage ground of power and success to any one favoured enough to possess it, it must be regarded in his case as subordinate to, though no doubt intimately connected with, a gift of a much higher order,—that of an equable and perfectly balanced intellectual and moral nature,—that greatest of gifts, which has been regarded, and justly, as the only one really worthy to be asked of Heaven in this life,—*mens sana in corpore sano.* Few men, indeed, have ever enjoyed a state of being so habitually serene and composed, accompanied with so much power and disposition to exert it. A calm, the reverse of apathy, a moderation having nothing in common with indifference, a *method* diametrically opposed to routine, pervaded every part of his sentiments and conduct. And hence it arose that every step which he took was measured and consequent—one fairly secured before another was put in progress. Such is ever the march of real power to durable conquest. Hence, too, it arose that a clear natural judgement, and that very uncommon gift, a sound common sense viewing all things through a medium unclouded by passion or prejudice, gave to his decisions a certainty from which few were ever found to dissent, and to his recommendations a weight which few thought it *right* to resist.

It is very difficult in speaking of Mr. Baily's character to convey a true impression through the medium of a language so exaggerative as that which men now habitually use. Its impressiveness was more felt on reflection than on the instant, for it consisted in the absence of all that was obtrusive or imposing, without the possibility of that absence being misconstrued into a deficiency,—like a sphere whose form is perfect simply because nothing is protuberant. Equal to every occasion which arose, either in public or private life, yet, when not called forth, or when others occupied the field, content to be unremarked; to speak of his conduct as unassuming would convey but a faint idea of the perfect simplicity with which he stood aside from unnecessary prominence or interference.

Hardly less inadequate would it be to say of his temper that, always equable and cheerful, it was a source of peace and happiness to himself and others. It was much more,—it was a bond of kindness and union to all around him, and infused an alacrity of spirit into every affair in which the cooperation of others was needed, which was more than a simple reflex of his own good humour. It rendered every relation between himself and others easy and natural, and brought out all the latent warmth of every disposition. One would have been ashamed to evade a duty or refuse a burden when it was seen how lightly his share was borne; how readily he stepped out of his way to offer aid wherever he saw it needed, and how frankly every suggestion was received, and every aid from others accepted and acknowledged. This is the secret of all successful cooperation.

Order, method and regularity are the essence of business, and these qualities pervaded all proceedings in which he took a part, and, indeed, all his habits of life. In consequence, all details found their right place and due provision for their execution in every matter in which he engaged. This was not so much the result of acquired habits as a man of business, as the natural consequence of his practical views, and an emanation of that clear, collected spirit, of which even his ordinary handwriting was no uncertain index. Among hundreds of his letters which I possess, there is hardly an erasure or correction to be found, but everywhere, on whatever subject, or whatever the haste, the same clear, finished, copperplate characters.

Of his choice of life I have already spoken something. Fortune he regarded as a mean to an end, but that end he placed very high; and fortune, he well knew, though a mean to its attainment, was not the only or the chief mean. As a member of civilized society, to add something to civilization, to ennoble his country and improve himself, by enlarging the boundaries of knowledge, and to provide for his own dignity and happiness by a pursuit capable of conferring both,—these were the ends which he proposed and accomplished. In choosing the particular line which he did, it is impossible too highly to appreciate the self-knowledge and judgement which enabled him to see and adopt those objects best adapted to his powers, and on which they could be, on the whole, most availably and usefully employed. Both in his public and private capacity he was liberal and generous in the extreme, and both his purse and his influence were ever ready, whether to befriend merit, or to promote objects of public, and especially of scientific utility.

To term Mr. Baily a man of brilliant genius or great invention, would in effect be doing him wrong. His talents *were* great, but rather solid and sober than brilliant, and such as seized their subject rather with a tenacious grasp than with a sudden pounce. His mind, though perhaps not excursive, was yet always in progress, and by industry, activity, and using to advantage every ray of light as it broke in upon his path, he often accomplished what is denied to the desultory efforts of more imaginative men. Whatever he knew he knew thoroughly, and enlarged his frontier by continually stepping across the boundary and making good a new and well-marked line between the cultivation within and the wilderness without. But the frame of his mind, if not colossal, was manly in the largest sense. Far-sighted, clear-judging, and active; true, sterling, and equally unbiassed by partiality and by fear; upright, undeviating, and candid, ardently attached to truth, and deeming no sacrifice too great for its attainment;—these are qualities which throw what is called genius, when unaccompanied, or but partially accompanied, with them, quite into the shade.

In speaking of his conduct with respect to this Society, and the infinite obligations we owe to him, we must regard him in the first place as the individual to whom, more than to any other, we owe the titles of a parent and a protector, and our early consolidation into a compact, united, and efficient body. As Secretary *pro tempore*, the draft of our Rules and the first Address explanatory of our objects, circulated at the commencement of our existence, were entirely, or in great measure, prepared by him; and, governed by these rules, with hardly any change, we have continued to flourish for twenty-four years, which is the best test of their adaptation to our purposes. As I have already stated, he acted as Secretary during the first three years of our existence, during which period the business of our meetings and of our council was brought into that systematic and orderly train of which the benefit has never since ceased to be felt. On retiring from this office he was elected Vice-President, and on the next biennial demise of the chair he became our President, an office which he afterwards filled for three subsequent periods of two years, including that of his lamented death. Altogether, during eight years as President and eleven as Vice-President, he filled the highest offices of our institution, and was never off the Council, nor was there any Committee on which he did not sit as one of its most active and efficient members.

With the exception of the Meeting of May 12, 1836, when he was in Scotland observing the annular eclipse, he was never absent from any Council, or from any Ordinary, General, or Committee Meeting, until finally prevented by illness. Nor during the whole period of the Society's existence was there any matter in which its interests were concerned in which he was not a mover, and, indeed, the principal mover and operator. Nor was this care of our interests and respectability confined to formal business or to matters of internal management. On every external occasion which offered he bore those interests in mind. He watched and seized the precise opportunity to procure for us from Government the commodious apartments we occupy. He obtained for us the respected and dignified position of Joint-Visitors of the Royal Observatory; he let no opportunity pass of enriching our library with attested copies of the most valuable astronomical documents, such as 'Flamsteed's Letters' and 'Halley's Recorded Observations.' He husbanded and nursed our finances with the utmost judgement and œconomy, thereby rendering us rich and independent. He printed at his own cost the thirteenth volume of our 'Transactions,' and procured to be defrayed by Government the expense of the seventh, and by subscription among the members, without entrenching on the funds of the Society, that of the computation and printing of our Catalogue. He prepared all our Annual Reports, and his addresses from the chair will always be read with pleasure and instruction. He also prepared all Committees' Reports, and translated for reading at our meetings numerous notices and communications in the German language: among others the memoir relating to the Berlin charts. In fine, he superintended everything in every department. But it was the manner and delicate tact of this superintendence which gave it its value and rendered it efficient. In respect of this point I may, perhaps, be permitted to use the expressions of a distinguished member of our body, to whom we owe many and great obligations, and who has witnessed the working of its machinery from the beginning, an advantage of which for some years I have myself been deprived by non-residence in London and absence from England. "Of his management of our Society," says Mr. Sheepshanks, "it is difficult to speak so as to convey a correct idea. No assumption, no interference with other people, no martinet spirit (which seems almost natural to all good business men), but everything carried on smoothly and correctly, and without bustle. He hit, better than any chairman I have ever seen, the mean between strictness and laxity, and, while he kept everything going in its proper channel, he also kept everybody in good humour. This natural tact was a great gift, but there was another quality which I never saw in any one but him, and that was his readiness to give precedence and room to every one who wished to do anything useful, and his equal readiness to supply every deficiency and do the work of everybody else. He was also the person who never was asleep and never forgot anything, and who contrived by his good humour, hospitality and good sense, to keep everything in train." To much of this view, as a matter of general character, I have given my own independent expression, but I could not deny myself the satisfaction of corroborating my own judgement by that of one so well qualified, from intimate knowledge, to form opinions.

Mr. Baily, as I have already stated, was a member of the Royal, Geological, and Linnæan Societies, to which I may also add, the Royal Irish Academy. In the Royal Society his eminence as an astronomer and a man of general science made his presence valuable, and the universal respect in which he was held gave him much influence. He filled in that body the office of Vice-President for six years, of Treasurer for three, and was fifteen times elected on the Council. I have already mentioned two of the three papers he contributed to its 'Transactions.' The third contains a minute account of the standard barometer of that Society, fixed up in their apartments in the year 1837, in which he enters into every particular of its construction, mode of registry, and corrections: it was read on the 16th of November, 1837. He was also one of the earliest members of the Royal Geographical Society, and took a very active part in its establishment. He was also a member and one of the trustees of the British Association, at whose meetings he was an occasional attendant, and acted, as we have seen, on some important committees. In 1835 the University of Dublin conferred on him the honorary title of Doctor of Civil Law, as I have already stated was also done by Oxford in 1844. Among the foreign Academies, which in honouring him honoured themselves, I find him to have been a correspondent of the Royal Institute of Sciences of Paris, and of the Royal Academies of Berlin, Naples, and Palermo, as well as the American Academy of Arts and Sciences at Boston.

His portrait by Phillips, presented by some Fellows of the Society, has long adorned, and, though for the present removed from its frame, will speedily again adorn our meeting-room. May his mantle descend on our future presidents, and his spirit long continue to preside over our councils, and animate our exertions in the cause he had so much at heart!

On the conclusion of the reading of the preceding Memoir, the thanks of the Society were proposed by the Dean of Ely, and unanimously voted to Sir John Herschel.

It was then moved by Mr. De Morgan, seconded by Mr. Donkin, and unanimously resolved,

"That the Society feels it impossible to express in adequate terms its obligations to its late President; and it desires to impress on the minds of all the Fellows, that such imitation of his example as their occupations will allow is the mode of testifying their gratitude and respect for his memory with which he would have been most pleased."

VI. *On* Jacobi's *Elliptic Functions.*

By the Rev. Brice Bronwin.

I DID not expect to hear again from Mr. Cayley on this subject, as my paper printed in this Journal in August 1843 made the matter in dispute between us exceedingly plain. Every step of it is clear. The most difficult is—But for the third and fourth forms of ω the denominator

$$\mathrm{s}\,a\,(\mathrm{K}-2\,\omega)\,\mathrm{s}\,a\,(\mathrm{K}-4\,\omega)\,\ldots\ldots$$

cannot be reduced to the form $\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,.\,3\,\omega\ldots\ldots$ And this is perfectly easy (see my paper in this Journal, April 1843, p. 260). The next step—u cannot take any of the forms $2\,r\,\omega-\mathrm{K}$—is evident; for the imaginary part of u must be equal to the imaginary part of $2\,r\,\omega$, and then the real part also will be equal to the real part of $2\,r\,\omega$, and not equal to the real part minus K. This is all exceedingly easy, and very unlike the slippery path Mr. Cayley has trod in his last paper, a path in which a false step is easily made.

Besides, I called Mr. Cayley's attention to a transformation depending on the third form of ω, at p. 54 of Jacobi's work, and requested him to make trial of it in its simplest case. I pointed out to him the means of very easily testing this form. The result would have been a stubborn fact on one side or the other. I suppose the readers of this Journal, whether they understand Jacobi's functions or not, will know how to estimate this circumstance.

I will now enlarge on the other mode of deciding this matter, which I just hinted at in the conclusion of my paper of August 1843. In order to determine the constant M, Jacobi makes $x=\mathrm{s}\,a\,.\,u=1$ (see p. 41). No form of ω therefore which does not satisfy this condition can consist with his theory. As ω has n for a denominator, we must have $u = n\omega$, or some odd integer multiple of this quantity. But $\mathrm{s}\,a\,(n\,\omega) = 1$ for the first form of ω only. I expect Mr. Cayley will object to this, and think that some other value of u may be found to satisfy the required condition. Let then $p\,n\,\omega$ be such a value, and let us determine it so as to satisfy the required condition for the third and fourth forms of ω if possible, in order to oblige that gentleman.

By pages 32 and 34 of Jacobi,

$$\mathrm{s}\,a\,(p\,n\,\omega) = \mathrm{s}\,a\,(p\,m\,\mathrm{K} + p\,m'\,i\,\mathrm{K}'),$$

$$= \frac{\left\{\begin{array}{l} \mathrm{s}\,a\,(p\,m\,\mathrm{K})\,\mathrm{c}\,a\,(p\,m'\,i\,\mathrm{K}')\,\Delta\,.\,a\,(p\,m'\,i\,\mathrm{K}') \\ +\,\mathrm{s}\,a\,(p\,m'\,i\,\mathrm{K}')\,\mathrm{c}\,a\,(p\,m\,\mathrm{K})\,\Delta\,.\,a\,(p\,m\,\mathrm{K}) \end{array}\right\}}{1 - k^2\,\mathrm{s}^2\,a\,(p\,m\,\mathrm{K})\,\mathrm{s}^2\,a\,(p\,m'\,i\,\mathrm{K}')}$$

$$= \frac{\left\{\begin{array}{l} \mathrm{s}\,a\,(p\,m\,\mathrm{K})\,\Delta\,.\,a\,(p\,m'\,\mathrm{K}') + \\ i\,\mathrm{s}\,a\,(p\,m'\,\mathrm{K}')\,\mathrm{c}\,a\,(p\,m'\,\mathrm{K}')\,\mathrm{c}\,a\,(p\,m\,\mathrm{K})\,\Delta\,.\,a\,(p\,m\,\mathrm{K}) \end{array}\right\}}{\mathrm{c}^2\,a\,(p\,m'\,\mathrm{K}') + k^2\,\mathrm{s}^2\,a\,(p\,m\,\mathrm{K})\,\mathrm{s}^2\,a\,(p\,m'\,\mathrm{K}')} \,. \quad (a.)$$

The modulus of ω, and therefore of $i\,\mathrm{K}'$, is k; but that of K' in $(a.)$ is k', its own modulus.

The imaginary quantity i must vanish from $(a.)$. Let then

$$\mathrm{s}\,a\,(p\,m'\,\mathrm{K}') = 0, \quad \text{or } p\,m' = 2\,r, \quad p = \frac{2\,r}{m'},$$

r as well as m and m' being any integers, positive or negative. The result is that

$$(a.) = \mathrm{s}\,a\,(p\,m\,\mathrm{K}) = \mathrm{s}\,a\left(\frac{2\,r\,m}{m'}\,\mathrm{K}\right).$$

If $\frac{2\,r\,m}{m'}$ be a fraction, this cannot answer; if it be an integer, it is an even one, and gives $\mathrm{s}\,a\,(p\,m\,\mathrm{K}) = 0$, which does not answer; for m is even, m' odd, or both are odd.

Next make $\mathrm{c}\,a\,(p\,m'\,\mathrm{K}') = 0$, or $p\,m' = 2\,r + 1$, $p = \frac{2\,r+1}{m'}$. The result in this case is

$$(a.) = \frac{1}{k\,\mathrm{s}\,a\,(p\,m\,\mathrm{K})},$$

which must be greater than unity, and therefore cannot answer.

Make $\mathrm{c}\,a\,(p\,m\,\mathrm{K}) = 0$, or $p\,m = 2\,r + 1$, $p = \frac{2\,r+1}{m}$, the result is

$$(a.) = \frac{1}{\Delta\,.\,a\,(p\,m'\,\mathrm{K}')},$$

which cannot be unity for m even, m' odd, or for both odd. The third and fourth forms of ω therefore never can give

$s a . u = 1$, as required by Jacobi's theory. It is not necessary, I presume, to say anything about the second form of ω.

Mr. Cayley has not condescended to state his objections to the reasoning in my last paper; had he done so, I should have had a chance of obviating them on his own ground, but as it is I am left entirely in the dark respecting them; and unhappily the darkness is in no measure dispelled by the cloud of mystery in which his last paper has enveloped the subject.

I must now notice Mr. Cayley's logic in the 16th No. of the Cambridge Mathematical Journal. He was at liberty to make $u = \omega$, or any other quantity; and in so doing he must determine the true value of C, if he proceeded by a right method. How then does it happen that for two forms of ω he obtains faulty or indeterminate results, no results as he calls them? Plainly because the forms of ω were faulty, not from any fault in the denominator. If Jacobi's denominator would have set all right, how did it happen that he did not fall upon it in these two cases? In the last case also he has in reality arrived at no result; it was quite ridiculous to argue against my denominator when he had obliterated it. If Jacobi's had been the true form, he would in every case have fallen upon it; and had there been no fault in the forms of ω, he would in no case have been led to faulty results. Had he carried his method out fully, and drawn from it the proper inferences, he would have proved all that I have asserted with regard to Jacobi's functions.

Gunthwaite Hall, Nov. 19, 1844. B. BRONWIN.

VII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

Nov. 27, 1843. "ON the Foundation of Algebra," No. III. By Augustus De Morgan, of Trinity College, Professor of Mathematics in University College, London, &c.

In the second paper of this series a general definition of the operation A^B was laid down, A and B being each of them any form of $p+q\sqrt{-1}$. The logarithm (or as Mr. De Morgan calls it, the *logometer*) of a line is thus described:—a line whose projection on the unit-axis is the logarithm of the length, and whose projection on the perpendicular is the angle made with the unit-axis (or its arc to a radius unity). Thus a line r inclined at an angle θ has for its logometer a line $\sqrt{(\log^2 r+\theta^2)}$ inclined at an angle whose tangent is $\theta : \log r$ This being premised, the universal definition of A^B is the line whose logometer is $B \times \text{logom}. A$.

The object of this third paper is to show that the preceding definition of the logometer is not the most general. Take any two lines whatsoever passing through the origin, and style them the bases of length and direction. Set off on the first a line representing the logarithm of the length in question, and on the second a line representing the angle it makes with the unit-axis, both on any scale of representation. Then the diagonal of the parallelogram described on the lines just set off is a logometer to the length and direction from which it was derived; and if under this meaning of the word logometer the preceding definition of A^B be employed, the equations

$$A^B A^C = A^{B+C}, \quad A^B C^B = (AC)^B, \quad (A^B)^C = A^{BC}$$

are universally true.

There is no necessity for the introduction of this more general system, since all its results can be expressed in terms of those of the more simple definition in the second paper. This new definition of the logometer is really nothing more than the process answering to the extension of the theory of logarithms from the system constructed on the Naperean base, to that which is on any base whatsoever.

"On the Measure of the Force of Testimony in cases of Legal Evidence." By John Tozer, Esq., M.A., Barrister-at-Law, Fellow of Gonville and Caius College.

The object of this paper is to show that the assumptions made by some English legal authorities on this subject, in opposition to the principles established by scientific processes, are not justified.

The views more particularly dissented from, as extracted from a work of high legal authority, are thus enunciated:—

"The notions of those who have supposed that mere moral probabilities or relations could ever be represented by numbers or space, and thus be subjected to arithmetical analysis, cannot but be regarded as visionary and chimerical.

"Whenever the probability is of a definite and limited nature (whether in the proportion of one hundred to one or of one thousand to one, is immaterial), it cannot be safely made the ground of conviction; for to act upon it in any case would be to decide, that for the sake of convicting many criminals the life of one innocent man might be sacrificed.

"The distinction between evidence of a conclusive tendency which is sufficient for the purpose, and that which is inconclusive, appears to be this: the latter is limited and concluded by some degree or other of finite probability beyond which it cannot go; the former, though not demonstrative, is attended with a degree of probability of an indefinite and unlimited nature."

The method pursued is that of investigating algebraic expressions for the probabilities that the allegations made in a case which actually occurred, the trial of a female for murder, are true; and thence deducing an expression for the probability of the truth of the charge, in passing from the symbolical to the numerical expression, the numbers employed are not the actual values of the symbols but their limiting values; the resulting number is therefore a fraction which is not less than the value of the probability of the truth of the principal allegation, this being what in practice is required.

The conclusion arrived at is, that the mode of estimating the force of evidence employed in a court is a process which algebraic investigation analyses, and of which it explains the theory, and an approximation to a result which is obtained with accuracy by assigning numerical values to the algebraic symbols : a clear conception of the nature of the practical process, it is conceived, must render its application more accurate, and to the extent of affording this the investigation is deemed to be of practical utility.

VIII. *Intelligence and Miscellaneous Articles.*

ON SULPHIPIANIC ACID. BY M. WÖHLER.

THIS acid is formed by the action of sulphurous acid upon opianic acid, which dissolves in considerable quantity in a hot solution of sulphurous acid, without being deposited on cooling ; this solution has a peculiar bitter taste and a sweetish after-taste ; the carbonates of barytes and lead dissolve in it and form well-crystallized salts.

When the solution of opianic acid in sulphurous acid is evaporated at a moderate temperature, the sulphipianic acid remains in the state of a transparent crystalline mass : this mass is usually rather moist, because it accidentally contains a little sulphuric acid, but none is ever found in the recently-prepared solution. It is colourless, and on the addition of water it becomes milky, and acquires a strong odour of sulphurous acid ; the white substance which separates is opianic acid : the new acid therefore, when in a crystalline state, is decomposed by water into opianic and sulphurous acid, but it takes place only partially.

The analyses of the salts of barytes and lead showed that the sulphipianic acid may be represented by $C^{20}\,\underline{H}^6\,O^7\,S^2$. It was requisite to analyse these salts after drying at the usual temperature, and consequently retaining their water of crystallization, a portion of which they lose at a slight increase of temperature, whereas a further quantity is not expelled till the salt begins to decompose. Nitric acid does not separate the acid from these salts, chlorine decomposes them slowly and incompletely : the quantity of sulphur was determined by calcination in a tube with a mixture of carbonate of soda and nitre.

The sulphipianate of lead contained—

	Experiment.	Equivalents.	Calculation.
Carbon	29·23	20	29·17
Hydrogen	3·00	12	2·92
Oxygen	33·00	17	33·02
Sulphur	8·10	2	7·81
Oxide of lead	26·67	1	27·08
	100·		100·

Calculation corresponds to the formula $Pb + C^{20}\,\underline{H}^6\,O^7\,\ddot{S}^2 + 6\underline{H}O$.

The six equivalents of water which are expelled at 338° F. correspond to 13·10 per cent. ; experiment gave 13·68.

The salt of lead crystallizes in four-sided prisms, but the crystals are usually in the form of six-sided tables. This salt undergoes no change by exposure to the air; at 212° it becomes opake but does not effloresce; it then becomes yellow in the light. When moderately heated it loses 3 equivalents, or half its water of crystallization, and the remaining 3 are expelled at a higher temperature, but it becomes yellow and continues to smell; at a still higher temperature it fuses and loses much sulphurous acid.

The barytic salt crystallizes in rhombic tables which are colourless and brilliant. It dissolves slowly in water; it loses all its water of crystallization at a high temperature, becomes opake and begins to decompose. It contains 3 equivalents of water.

M. Wöhler states that he shall hereafter consider the formation and composition of this acid, as to whether it should be considered to contain 1 equivalent of hyposulphuric acid or 2 equivalents of sulphurous acid. The mode in which it is acted upon by water favours the latter view, and its action on selenious acid also supports it. When it is employed alone it is not altered by it; but by admixture with hydrochloric acid, the selenious acid is reduced. When the acid is mixed with chloride of gold, there separate, after some time, opianic acid and reduced gold in brilliant tables.—*Ann. de Ch. et de Phys.*, Octobre 1844.

ON SULPHOPIANIC ACID.

M. Wöhler gives this name to the acid produced by the action of sulphuretted hydrogen on opianic acid.

When sulphuretted hydrogen gas is passed into a boiling solution of opianic acid, no visible action occurs; but if the temperature be considerably reduced, the liquor loses its transparency, and a precipitate resembling sulphur is formed, the quantity of which goes on increasing. By these means all the opianic acid is converted into this new compound, but the change occupies several days. If the mixture be heated to ebullition, the yellow precipitate disappears, and is converted into a liquid of the same colour, which soon falls to the bottom of the vessel, where it becomes solid on cooling.

Sulphopianic acid thus obtained is an amorphous transparent mass of a sulphur-yellow colour, which liquefies at 212°. If it be more strongly heated, it decomposes, emitting a yellowish vapour which condenses in fine crystalline needles; these burn with flame and evolve sulphurous acid; they are insoluble in water, but dissolve readily in alcohol, to which they impart a yellow colour. If the acid has been fused previously to being dissolved in the alcohol, an amorphous transparent mass merely is obtained by spontaneous evaporation; but if in the preparation of the acid the heat has been so managed as to prevent its fusion, it is then obtained in the state of small transparent crystals, which when collected have a yellow colour. This acid therefore undergoes by fusion a change analogous to that which opianic acid suffers under similar circumstances. Concentrated sulphuric dissolves it and becomes yellow, and when heated of a deep purple. Chlorine and the alkaline hypochlorites oxidize the sulphur which it contains slowly and imperfectly; the alkalies dissolve it and become yellow, and from them acids precipitate it unchanged, and without the evolution of sulphuretted hydrogen; after some time, however, the solutions contain some alkaline sulphuret.

The mean of repeated analyses gave as the composition of this acid,—

	I. and IV.	II.	III.	Equivalents.	Calculation.
Carbon......	53·15	52·52	52·51	20	53·30
Hydrogen....	4·19	4·10	4·24	9	3·90
Oxygen	28·30			8	28·44
Sulphur	14·32			2	14·28
	99·96				99·92

Sulphopianic acid may be considered as opianic acid, in which 2 equivalents of oxygen are replaced by 2 equivalents of sulphur = $HO+C^{20}H^{8}O^{7}S^{2}$; its formation is therefore very simple.

These results were obtained by the direct decomposition of the acid, and not of a salt containing it; for M. Wöhler found it impossible to determine its atomic weight in the latter mode, for he could not obtain salts of silver and lead of constant composition.

When nitrate of silver is added to a solution of sulphopianate of ammonia, a brownish-yellow precipitate is formed, which is soon converted into blackish-brown sulphuret of silver. When it is quickly filtered it may be dried *in vacuo*; its colour nevertheless deepens; it is then fusible on paper without losing weight. When burnt, it disengages, as the acid does a yellow vapour, and leaves sulphuret of silver.

Acetate of lead yields a bulky precipitate of an orange yellow colour; in twenty-four hours it becomes blackish-brown. Ebullition converts it immediately into sulphuret of lead.—*Ann. de Ch. et de Phys.*, October 1844.

PREPARATION OF FERRATE OF POTASH.

M. Fremy has described several processes for obtaining the above-mentioned compound; that which he prefers is the following:—surround a Hessian crucible with burning fuel, and put into it 80 grains of pure iron filings, and when these are red-hot, throw upon them 160 grains of nitre which has been previously fused and afterwards powdered: the action is instantaneous and very strong; as a portion of the matter is thrown out of the crucible the experiment should be cautiously performed: the crucible is afterwards to be covered and allowed to cool; there remains a reddish-violet coloured mass, which is easily detached from the crucible, containing a large quantity of ferrate of potash. The product always contains some peroxide of potassium, which is decomposed when it is mixed with water.

It will be observed that in this experiment the nitre is decomposed by the iron under such circumstances, that the potash does not act upon the silica of the crucible, and the ferrate is not formed unless the action is instantaneous. If the iron be too long heated it becomes oxidized at the surface, and then the heat requisite for the production of the salt is not excited on the addition of the nitre.

It is indeed true, that when using the above-mentioned proportions of iron and nitre, but a very small portion of the iron is acted upon:

it was attempted to increase the proportion of nitre, but by this the crucible was so much cooled that the ferrate was not formed.—*Ann. de Ch. et de Phys.*, November 1844.

PREPARATION OF FERRATE OF POTASH IN THE MOIST WAY.

When a current of chlorine gas is passed into a concentrated solution of potash containing suspended hydrated sesquioxide of iron, M. Fremy states that the liquid soon assumes a fine purplish-red colour, and the sesquioxide of iron is dissolved; when the potash is greatly in excess, a blackish powder is precipitated; this is merely ferrate of potash, which is insoluble in the alkali. The ferrate thus precipitated carries with it considerable quantities of chloride of potassium; in order to purify it, it must be dissolved in water and again precipitated by concentrated potash. The ferrate of potash cannot be thrown on a filter; for not only will the excess of potash react upon the paper, but the ferrate would be decomposed by the organic matter.

Ferrate of potash is to be dried by placing it upon absorbent pieces of porcelain; when the salt is dry it should be quickly inclosed in small tubes sealed by the lamp, for the humidity of the air decomposes it.

When pure ferrate of potash is required, the sesquioxide of iron should be first precipitated either by ammonia or excess of potash, and then washed and afterwards suspended in a very concentrated solution of potash; through this the chlorine is to be passed, but not too rapidly; as the chlorine combines with the potash, and as the ferrate cannot be formed except the liquor be very alkaline, fragments of potash must be occasionally added to supply the place of that which combines with the chlorine.

The liquid becomes very hot and disengages considerable quantities of oxygen [chlorine?]; but it was ascertained by M. Fremy that this high temperature does not decompose the ferrate of potash, that being fixed by the excess of potash; on the other hand, it is inconvenient to employ too great an excess of potash, for a saturated solution of alkali absorbs chlorine with difficulty.

Properties of the Ferrate of Potash.—When precipitated from its solution by excess of potash, it is black, very soluble in water, and imparts to it a fine reddish-violet tint; the solution decomposes spontaneously, evolving oxygen, precipitating sesquioxide of iron, and leaving potash in solution; at a boiling heat this reaction is instantaneous, and this decomposition appears to take place more rapidly under the receiver of the air-pump than at usual pressures; the presence of finely-divided substances also hastens the decomposition; thus a solution remains for some time without showing any signs of decomposition; but when it has deposited small portions of sesquioxide of iron, the decomposition goes on rapidly, and it can only be stopped by pouring off the solution. Other metallic oxides, such as those of silver and manganese, produce the same effect: these reactions resemble some of those of oxygenated water, but they do not take place with the same intensity, for the metallic oxides do not lose their oxygen: metals produce the same effect as the oxides, in accelerating the decomposition of the ferrate of potash in solution; the hypochlorites impart stability to the ferrate of potash, for a mixture of ferrate of potash and an alkaline hypochlorite is very slowly altered by ebullition, whereas pure ferrate of potash is instantly decomposed by a temperature of 212° F. Excess of chlorine decomposes ferrate of potash, and the same happens with ammonia, which disengages azote and precipitates sesquioxide of iron.

All acids decompose ferrate of potash; the ferric acid set free is immediately decomposed into oxygen and sesquioxide of iron, which dissolves in the acid when in excess; the solution becomes completely colourless. Acids which are susceptible of superoxidizement absorb the nascent oxygen.

The instantaneous decoloration of ferrate of potash by the action of acids, is a property which immediately distinguishes ferrate from permanganate of potash.

Hydrochloric acid decomposes ferrate of potash, forming chloride of iron and disengaging chlorine; organic substances also react upon and decompose the ferrate of potash, in which respect it resembles the manganates and permanganates.—*Ann. de Ch. et de Phys.*, November 1844.

HYDRATED IODIC ACID.

M. Millon remarks that the action of nitric acid upon iodine depends on the degree of hydratation of the acid.

Nitric acid combined with 4 equivalents of water, or that which contains more, does not oxidize iodine; it dissolves in them when heated, and produces a violet colour, but is deposited on cooling without the formation of any iodic acid. This refusal to oxidize is explained by an interesting phænomenon presented by nitric acid, of this degree of hydratation, in contact with iodic acid itself. The iodic acid dissolves in it without any apparent reaction; but if a few bubbles of nitric oxide be passed into the solution, the nitrous acid formed soon reduces the iodic acid, and iodine separates. The iodine cannot undergo oxidizement by weak nitric acid without producing a nitrous compound; but this reducing the iodic acid, the iodine exists in weak nitric acid, between two equal contrary forces, which produce inertia.

When nitric acid contains two or three equivalents of water it oxidizes iodine with the assistance of heat. The nitrous products then formed do not react upon the iodic acid, and it is formed as long as the acid is sufficiently concentrated. The iodic acid separates in white mammillated crystals, which, whatever may be the degree of concentration of the acid, always contain one third of an equivalent of water: its formula is $3IO^5+HO$. It is insoluble in alcohol, and is perfectly free from any combination with nitric acid.

The concentration of the nitric acid requisite to the oxidizement of iodine, explains satisfactorily why Sérullas succeeded in effecting it by means of a mixture of nitric and nitrous acids. The latter mixing with hydrated nitric acid, in fact introduced anhydrous nitric acid, and diminished the degree of hydratation.

M. Millon states that when nitric acid which is not too nitrous, and contains less than 2 equivalents of water, is saturated with iodine, it is oxidized without the assistance of heat.

The nitric acid should be pure; and although the iodine of commerce when finely pulverized will answer the purpose, it is better to employ precipitated iodine, obtained by mixing solutions of chloride of iodine and potash. The iodine ought to be carefully washed and dried at the usual temperature of the air; then put into a glass mortar about 200 grains of iodine and ten times their weight of nitric acid containing one or two equivalents of water; triturate the mixture, and in a short time the iodine disappears and a bulky yellow powder is formed: the trituration is to be continued for some time, and if all the iodine has not evidently been acted upon, the acid is to settle for a short time and then to be poured off. A fresh quantity of acid equal to the first is then to be added, to be allowed to settle as before, and poured off.

The yellow powder thus obtained is a compound of iodine, oxygen and nitric acid, the two former not being in the proportions which form iodic acid. This powder is decomposed with great facility: thus when water is added to it, it suddenly gives rise to iodic and nitric acids, and iodine is deposited. Heat produces the same results; and if the contact of the concentrated nitric acid be continued r some hours, the new compound is replaced by iodic acid. This yellow powder was not separable by any method whatever; but there may be procured from it another yellow compound, the formula of which is IO^4 [iodous acid].

This new compound is obtainable only by attending to the following very minute directions. After the original compound has been separated from the excess of nitric acid, it is to be transferred to a funnel containing amianthus, through which the nitric acid, which yet moistens it, is to be allowed to drop. In about a quarter of an hour the product is removed and placed on a dry brick, which is to be exposed to the air as long as nitric acid vapour arises from it; when this ceases, the brick and product are to be placed over hydrate of lime and covered with a receiver, till all change of appearance ceases to take place. In two or three days the yellow powder which remains on the brick is to be washed, either with water or weak spirit, or, which is still better, alternately with water and spirit, which remove the iodine and iodic acid intermixed with the iodous acid IO^4; this is then to be thrown on a filter and dried, either by exposure to the air or over sulphuric acid.

A very small portion of iodous acid is obtained by employing a comparatively large quantity of iodine; thus 150 to 220 grains never yield more than 15 to 22 grains in the most successful operations.

The mean of two experiments gave 20·09 per cent. as the quantity of oxygen contained in this compound; it therefore must consist of

One equivalent of iodine	126	or	20·25
Four equivalents of oxygen	32	or	79·75
Equivalent	158		100·

Ann. de Ch. et de Phys., November 1844.

ACTION OF IODINE ON CERTAIN SALTS. BY M. FILHOL.

Several years since M. Berthemot produced, for the first time, a compound of iodine and lead possessing a fine blue colour; it was mentioned rather later by M. Denot, and it has been more recently examined by M. Durand.

In order to determine its chemical composition, M. Filhol first examined that of the solutions which are used in forming it; this included the action exerted by iodine on carbonates; the results showed that iodine acting in the cold upon the alkaline carbonates, forms bicarbonate of the base, an iodide and iodate.

By the action of a solution of bi-iodide of potassium upon a solution of acetate of lead, a violet-red coloured precipitate was formed, which was very unstable; when heated to 230° F. it loses 26·66 per cent., or 1 equivalent of iodine, and leaves a residue of $I^2Pb. + PbO$. If care be taken not to heat the compound to above 230°, the residue is of a fine green colour; but if the temperature be raised to 266° or 284°, the green colour is lost, and it becomes of a pale yellow without losing a trace of iodine. The composition of the red powder is such, that it may be considered as formed of bi-iodide of lead combined with oxide of lead $(2I^2+Pb)+PbO$, or as formed of I^2Pb+I^2PbO.

When treated with excess of acetic acid this powder is decomposed; iodine separates, some oxide of lead combines with the acetic acid, and the residue is a new oxi-iodide of lead composed of $2I^2 + PbO$.

M. Filhol states that this red powder is one of the constituents of the blue compound, and that it may be produced by mixing the red powder while moist with nascent carbonate of lead; or, which is better, by employing a solution which produces both at the same time; the blue powder constitutes a compound of a new class, and the author proposes for it the name of iodo-carbonate of lead: it is obtained of a very fine colour by precipitating a solution of acetate of lead, with one formed of 1 proportion of bi-iodide of potassium and 4 of carbonate of potash.

The analysis of the blue compound gave the following as its formula: $I^2Pb+I^2PbO+4(CO^2PbO)$.

[The atomic weight of iodine in the above notice is, we believe, only half of that assigned to it in England.—Edit.]

Journ. de Pharm. et de Ch., Decembre 1844.

ON A NEW COMPOUND OF SULPHUR, CHLORINE AND OXYGEN.

M. Millon, in attempting several years ago to combine a larger proportion of chlorine with sulphur than had been previously done, remarked the formation of a crystalline compound which he supposed to consist entirely of sulphur and chlorine. But on reproducing this substance repeatedly, the author found that it was obtained in proportion to the excess of chlorine and the smallness of the moisture which it contained.

From the period of this discovery, he suspected the presence of oxygen: by adopting a peculiar mode of preparation the product was obtained in considerable quantity; the analysis was then undertaken, and besides a large proportion of oxygen, certain interesting properties were discovered with respect to the isomeric transformation of mineral compounds.

The new compound is immediately obtained by dropping chloride of sulphur into a moist bottle containing moist chlorine. Too large a quantity of moisture would immediately destroy this compound, or would retard its production; but under the circumstances described, the bottle soon becomes covered with colourless transparent crystals, which attach themselves to the sides. It is not always possible to detach the compound thus spread in thin laminæ, and which the moist air destroys rapidly. To prepare the compound in considerable quantity, a bottle, capable of containing 8 to 10 pints, is to be filled with moist chlorine, and then there are to be added to it from 300 to 450 grains of chloride of sulphur, quite saturated with chlorine, and afterwards 30 to 45 grains of water. The bottle is to be shaken and surrounded with a mixture of salt and ice for four or five hours. A great disengagement of hydrochloric acid takes place; the bottle is to be again filled with moist chlorine and again immersed into the freezing mixture: these operations are to be repeated until the chloride of sulphur becomes an abundant crystalline mass, containing an excess of the chloride. The formation of these crystals, which are either fine needles or large rhombic laminæ, is usually preceded by the production of a yellowish liquid, heavier than the chloride of sulphur, and from which it separates like an oil.

It is very difficult to separate the crystals from the chloride of sulphur which they contain; it is effected by passing into the bottle, during six to twelve hours, a current of chlorine gas dried over sulphuric acid: at the same time that the chlorine is passing through the bottle, the crystals are to be volatilized and made to pass from one side of the bottle to the other by means of red-hot charcoal. Notwithstanding this troublesome operation, the crystals always retain one or two per cent. of chloride of sulphur, which analysis discovers to be in proportion to the shortness of the time which the chlorine is passed.

It is scarcely possible to analyse these crystals immediately after their production; they are in fact destroyed and projected with great violence as soon as they are touched with water, alcohol or dilute acids. To determine their composition, the author availed himself of an interesting property which they possess, and which is the following:—when the crystals have been freed as much as possible from the chloride of sulphur, they are made to drop into a very dry glass tube, closed at one end, and the other is drawn out by the lamp; in two or three months the crystals begin to soften, become pasty, moisten, and in six or eight months they are converted into an extremely thin liquid and of a scarcely perceptible yellow tint, when the current of chlorine has been long continued. No absorption takes place, and the liquid cannot be restored to the crystalline state even by exposure to a cold of 2° below zero. It is therefore an isomeric transformation, which is shown, not only by the change of its physical but also of its chemical properties. Thus the liquid when poured into water does not produce the hissing noise of a red-hot iron plunged into water, as the crystals do; it mixes quietly with weak acids, alcohol or water, to the bottom of which it falls quietly like an oil, which is eventually converted into sulphurous, sulphuric and hydrochloric acids.

This transformation agrees perfectly with the analysis, which shows it to be a compound of sulphur, chlorine and oxygen, represented by $S^2O^3Cl^2$. It will be observed that it differs from the chlorosulphuric acid discovered by M. Regnault, SO^2Cl, and from the liquid analysed by H. Rose, represented by S^2O^5Cl.

As to the analysis of the above described compound, and which M. Millon observes may be named hypochlorosulphuric acid, it will be seen that it offers no difficulty as soon as it has assumed the liquid form. It is sufficient to fill a weighed glass bubble with it, which is to be broken in a bottle containing nitrous nitric acid: the action is to be moderated by cooling the bottle; the sulphuric acid is afterwards determined by the quantity of sulphate of barytes obtained, and the chlorine by the chloride of silver.—*Journ. de Ph. et de Ch.*, December 1844.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1844.

Chiswick.—Nov. 1. Overcast: boisterous. 2. Constant heavy rain: boisterous at night. 3. Cloudy and fine. 4, 5. Cloudy. 6. Overcast. 7. Rain: cloudy. 8. Overcast: heavy rain at night. 9. Hazy clouds: fine: clear. 10. Clear and fine: rain. 11. Densely clouded: fine. 12. Rain: drizzly: boisterous, with rain at night. 13. Heavy rain. 14. Rain. 15. Cloudy. 16. Hazy: clear. 17. Foggy. 18, 19. Overcast. 20. Hazy: clear and fine. 21. Foggy throughout. 22. Hazy. 23. Foggy. 24. Hazy: cloudy. 25. Cloudy and fine. 26. Clear and frosty. 27. Sharp frost: foggy: clear and frosty at night. 28. Foggy: cloudy. 29. Easterly haze: foggy. 30. Overcast.—Mean temperature of the month 0°·85 above the average.

Boston.—Nov. 1. Cloudy. 2. Stormy: rain P.M. 3. Rain: rain early A.M.: rain P.M. 4. Rain: rain early A.M.: showery afterwards. 5. Cloudy: rain early A.M.: showery afterwards. 6. Rain: rain early A.M.: rain A.M. and P.M. 7. Fine. 8. Cloudy: rain P.M. 9. Cloudy. 10. Fine: rain P.M. 11. Fine: rain early A.M. 12. Cloudy: rain P.M. 13. Cloudy: rain A.M. and P.M. 14. Fine. 15. Cloudy: rain early A.M. 16. Fine. 17—20. Cloudy. 21. Fine. 22. Cloudy: first ice this morning. 23. Cloudy. 24. Cloudy: total eclipse of the moon visible 11 P.M. 25. Cloudy. 26, 27. Fine. 28—30. Cloudy.

Sandwick Manse, Orkney.—Nov. 1. Cloudy. 2. Damp. 3. Bright: clear. 4—7. Fine. 8. Showers. 9—11. Rain. 12. Cloudy. 13. Frost: fine: cloudy. 14. Frost: fine: cloudy: frost. 15. Rain. 16. Rain: drizzle. 17, 18. Cloudy. 19. Bright: hazy. 20. Showers. 21. Bright: cloudy. 22. Bright: clear: aurora. 23. Bright: clear. 24. Clear. 25. Cloudy. 26. Bright: rain. 27. Cloudy: damp. 28. Cloudy: clear. 29. Rain: clear. 30. Bright: clear.

Applegarth Manse, Dumfries-shire.—Nov. 1. Fair: wind high. 2. Very slight shower. 3. Very slight shower: clear. 4, 5. Fair, but threatening. 6. Cloudy and damp. 7. Fair and fine. 8. Dull A.M.: rain. 9. Fair, but dull. 10, 11. Cloudy A.M.: rain P.M. 12. Rain. 13. Fair and mild. 14. Heavy rain: flood. 15. Fair and fine. 16. Fair and fine, though cloudy. 17. Damp: slight shower. 18. Rain P.M. 19. Fair and fine. 20. Fair and fine: frost A.M.: fog. 21. Fair and fine: frost. 22. Slight rain. 23. Fair, but dull. 24. Fair: frost. 25. Fair: slight frost A.M. 26. Fair, but cloudy. 27. Cloudy A.M.: rain P.M. 28. Fair, but dull. 29. Shower. 30. Fine: frost A.M.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1844. Nov.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8¼ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8¼ p.m.								
1.	29·513	29·248	29·28	29·55	29·42	29·89	29·90	50	40	47·5	49½	44	48	45	e.	se.	e.	ese.	·38	......	......	......
2.	29·245	29·119	28·91	29·40	29·50	29·93	29·99	47	37	44	47½	41	43½	43½	e.	e.	e.	ese.	·49	......	......	·20
☾ 3.	29·384	29·318	29·08	29·55	29·62	30·01	29·97	45	37	43	48½	42	44½	38	ne.	se.	ne.	ese.	......	·51	......	......
4.	29·371	29·215	28·96	29·50	29·30	29·79	29·66	48	27	44	45½	38	40½	40	nw.	ne.	n.	ne.	......	·69	......	......
5.	29·215	29·175	28·82	29·33	29·40	29·69	29·74	49	34	45	45½	40	38½	36	n.	calm	nne.	......	·03	·10	......	......
6.	29·276	29·252	28·85	29·37	29·23	29·75	29·73	48	30	43	46	41	39½	41	sw.	calm	ne.	......	......	·41	......	......
7.	29·377	29·364	29·05	29·34	29·34	29·69	29·65	51	44	38	48	38	39	40½	se.	calm	ne.	......	·06	·75	0·10	......
8.	29·096	28·937	28·83	29·21	28·94	29·56	29·45	57	42	48	50½	35	44	45	se.	calm	ne.	e.	·58	......	......	·10
9.	29·077	28·947	28·60	28·87	28·90	29·32	29·23	56	35	48·5	51	42½	43	42½	w.	calm	n.	e.	......	·11	......	·50
● 10.	29·194	28·919	28·75	28·96	28·96	29·23	29·23	54	35	43	47	38	42	41½	s.	calm	nw.	ene.	·34	......	......	·29
11.	29·495	29·177	28·72	29·00	29·16	29·23	29·23	50	37	42·5	46	36	41	40	w.	calm	nw.	ene.	·16	·49	......	·45
12.	29·503	29·417	29·03	29·23	29·18	29·35	29·33	58	48	43	44½	34	40	39½	sw.	calm	se.	ene.	·50	......	......	·14
13.	29·828	29·506	29·09	29·30	29·53	29·48	29·63	57	37	50	51	39	37	37½	sw.	calm	sw.	e.	·38	·06	......	......
14.	30·109	30·012	29·55	29·90	29·97	29·97	30·08	55	49	43	48½	39½	36	35	sw.	calm	ne.	nw.	·12	·24	0·85	......
15.	30·078	30·025	29·50	29·55	29·75	29·33	29·47	59	45	55	53½	40½	47	50	sw.	w.	sw.	wsw.	......	·04	......	·67
16.	30·237	30·191	29·68	29·97	30·04	29·90	29·89	60	43	47	53	47½	43	50	sw.	calm	ssw.	se.	......	......	......	·31
☽ 17.	30·296	30·280	29·75	30·10	30·10	29·90	29·85	56	44	53	54½	48½	50	50	sw.	calm	ssw.	wsw.	......	......	......	·12
18.	30·258	30·178	29·75	30·00	29·90	29·85	29·76	55	51	48	51½	42	50	50	sw.	calm	ssw.	......	......	......	......	......
19.	30·177	30·143	29·68	29·94	29·88	29·86	29·95	55	47	49	54	48	49	44½	s.	calm	ssw.	sw.	......	......	......	......
20.	30·204	30·111	29·60	29·86	30·10	29·73	29·97	59	30	49	52	48	47	47	sw.	w.	ssw.	w.	......	......	......	·08
21.	30·304	30·291	29·88	30·20	30·20	30·18	30·20	49	30	39·5	48	35	47½	47	sw.	calm	sse.	w.	·02	......	......	......
22.	30·246	30·185	29·87	30·14	30·02	30·18	30·02	45	39	34	42	30	43½	43	w.	calm	sse.	sse.	......	......	0·48	......
23.	30·146	30·124	29·75	29·94	30·00	29·80	29·96	45	28	43	45½	34	43	41	e.	calm	s.	se.	......	......	......	......
○ 24.	30·031	29·947	29·68	29·94	29·89	29·93	29·88	44	31	42	44½	37	43	40	ne.	calm	sse.	sse.	......	......	......	......
25.	30·102	29·983	29·61	29·90	29·98	29·93	29·98	45	29	37	41	32½	43	45	n.	calm	n.	se.	......	......	......	......
26.	30·296	30·221	29·85	30·02	30·00	29·96	29·82	45	22	32	46½	34	43	46	sw.	calm	ne. se.	se.	......	......	......	......
27.	30·326	30·237	29·95	29·99	29·90	29·72	29·72	46	23	32	50	44	49	51	sw.	calm	sse.	ssw.	......	......	......	·28
28.	30·110	29·986	29·73	29·80	29·80	29·57	29·89	44	38	36	49½	45	49	46½	se.	calm	sse.	sse.	......	......	......	......
29.	30·038	29·958	29·69	29·90	30·00	30·02	30·07	44	37	42	48	43	45	45	e.	calm	se.	se.	......	......	0·15	·11
30.	30·099	30·073	29·79	30·09	30·10	30·09	30·18	40	30	40	44	32½	43	41	ne.	calm	se.	sse.	......	......	......	......
Mean.	29·821	29·717	29·37	29·961	29·661	29·761	29·781	50·53	36·63	43·3	48·2	39·6	43·71	43·40					3·06	3·40	1·58	3·25

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[THIRD SERIES.]

FEBRUARY 1845.

IX. *On* Fresnel's *Theory of Diffraction. By* R. MOON, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society*[*].

THE Fresnelian theory of diffraction has so long received the sanction of the scientific world, that it may appear almost presumptuous to attempt to controvert it in any material point. I shall so far show deference to an opinion very generally and very strongly pronounced, as not to enter upon the consideration of the antecedent probability or improbability of the fundamental hypothesis connected with it, that of secondary waves emanating from the general front, since it might be difficult, however much I may be impressed with its unphilosophicalness, to advance anything very positive against it. But the mathematics of the subject are less matters of opinion and more matters of fact, and upon this ground I enter with more confidence; for I believe that, unless I am very blind or very ignorant, it can be shown that in this part of the subject very grave errors have been committed. With this much of explanation, I have to crave the indulgence of my readers while I enter upon the discussion of the first experiment treated of by Fresnel in his Theory of Diffraction; and for the clearer elucidation of my views on the subject, I shall transcribe up to a certain point Sir John Herschel's investigation of that experiment contained in his treatise on Light (art. 718), presuming it to be substantially the same as that of Fresnel.

"Let us consider a wave A M F propagated from O, and of which all that part to the right of A is intercepted by an opake body A G; and let us consider a point P in a screen at the distance A B behind A as illuminated by the undulations emanating simultaneously from every point of the portion

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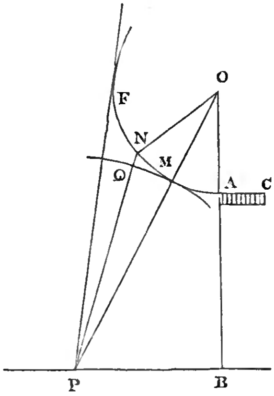
[*] Communicated by the Author.



A M F, according to the theory laid down in art. 628 *et seq.* For simplicity, let us consider only the propagation of undulations in one plane. Put A O $= a$, A B $= b$, and suppose $\lambda =$ the length of an undulation; and drawing P N any line from P to a point near M, put P N$=f$, N M$=s$, P B $= x$; then, supposing P very near to B, and with centre P radius describing the circle Q M, we shall have

$$f = \mathrm{PQ} + \mathrm{QN} = \sqrt{(a+b)^2 + x^2} - a + \mathrm{QN} = b + \frac{x^2}{2(a+b)} + \mathrm{QN}.$$

Now Q N is the sum of the versed sines of the arc s to radii O M and P M, and is therefore equal to

$$\frac{s^2}{2\,\mathrm{OM}} + \frac{s^2}{2\,\mathrm{PM}} = \frac{s^2}{2}\left(\frac{1}{a} + \frac{1}{b}\right) = \frac{a+b}{2ab} \cdot s^2,$$

so that, finally,

$$f = b + \frac{x^2}{2\,(a+b)} + \frac{a+b}{2\,a\,b}\,s^2.$$

Now if we recur to the general expression in art. 632 for the motion propagated to P from any limited portion of a wave, we shall have in this case $\alpha\,.\,\phi\,(\theta) = 1$, because we may regard the obliquity of all the undulations from the whole of the efficacious part of the surface A M N as very trifling, when P is very distant from A in comparison with the length of an undulation; and as we are now considering undulations propagated in one plane, that expression becomes merely

$$\gamma = \int d\,s \sin 2\,\pi \left\{\frac{t}{\mathrm{T}} - \frac{f}{\lambda}\right\},$$

and the corresponding expression for the excursions of a vibrating molecule at P will be

$$x = \int d\,s \cos \frac{2\,\pi}{\lambda}\left(\frac{t}{\mathrm{T}} - \frac{f}{\lambda}\right).$$

If then we put for f its value, and take

$$2\pi\left(\frac{t}{\mathrm{T}} - \frac{b}{\lambda} - \frac{x^2}{2\lambda(a+b)}\right) = \theta; \quad s.\sqrt{\frac{2\,(a+b)}{a\,b\,\lambda}} = v,$$

and consider that in those expressions t and x remain constant, while s only varies, the latter will take the form

$$x=\sqrt{\frac{a\,b\,\lambda}{2(a+b)}}\left\{\cos\theta\int dr\cos\left(\frac{\pi}{2}r^2\right)+\sin\theta\int dr\sin\left(\frac{\pi}{2}r^2\right)\right\},$$

which shows that the total wave on arriving at P may be regarded as the resultant of two waves, $x'\cos\theta$ and $x''\sin\theta$, differing in their origin by a quarter undulation, and whose amplitudes x' and x'' are given by the expression

$$x'=\sqrt{\frac{a\,b\,\lambda}{2\,(a+b)}}.\int d\,r\cos\left(\frac{\pi}{2}r^2\right),$$

$$x''=\sqrt{\frac{a\,b\,\lambda}{2\,(a+b)}}\int d\,r\sin\left(\frac{\pi}{2}r^2\right),$$

the integrals being taken between the limits of n corresponding to $s=-\mathrm{A\,M}$ and $s=+\infty$."

It would be difficult to conceive of anything purporting to be a mathematical investigation more full of contradictions and absurdities than the foregoing. Recurring to art. 631 of the treatise on Light, we find that when a wave is transmitted through a limited aperture, the disturbance of any vibrating molecule at distance $a+f$ from the centre of vibration may be represented by

$$\iint d^2s\,\alpha\,\phi\,\theta\sin 2\pi\left(\frac{t}{\mathrm{T}}-\frac{a+f}{\lambda}\right),\quad . \;\; . \;\; . \;\; . \qquad (\mathrm{A.})$$

where d^2s is a small element of the front from which the secondary waves are supposed to diverge, a the radius of that front, "$\phi\,(\theta)$ being the function of the angle made by a laterally divergent vibration with the direct one, which expresses its relative intensity, and which is unity when $\theta=0$, and diminishes with great rapidity as θ increases," the integration being to be effected to the limits of the aperture.

The accomplished author observes that in the above investigation "we shall have $\alpha\,\phi\,(\theta)=1$, because we may regard the obliquity of all the undulations from the whole of the efficacious part of the surface A M N as very trifling when P is very distant from A in comparison with the length of an undulation." What is the efficacious part of the surface A M N? Plainly, if the expression (A.) be correct, it must be that part of the surface for which the value of $d^2s\,\alpha\,\phi(\theta)\sin 2\pi\left(\frac{t}{\mathrm{T}}-\frac{a+f}{\lambda}\right)$ is significant. Now that expression can only become insignificant by $\phi\,(\theta)$ becoming very small; but $\alpha\,\phi\,(\theta)$ passes through all possible values between 1 and 0, and it is surely impossible to leave out of consideration the whole series of intermediate values. If we take into account the case where $\alpha\,\phi\,(\theta)=1$, why should we neglect the cases where $\alpha\,\phi\,(\theta) = \frac{3}{4}$ or $\frac{1}{2}$ and other intermediate quantities?

Such a method of approximation is wholly untenable, and the only way in which I can account for its having been for a moment admitted, is that Fresnel, having proved (vide Herschel, arts. 628, 629) that the motion of a vibrating molecule is governed entirely by that portion of the front of the wave from which the secondary waves emanate, immediately contiguous to the perpendicular drawn from the molecule upon the front, conceived himself to be warranted in coming to this conclusion. But it is one thing to prove that the disturbance is due to the part of the wave about the perpendicular, and another to show that that part of the wave only is effective, the intensity of the secondary waves from which, when they arrive at the molecule, differ insensibly from 1, which cannot be done (as I have shown) without falsifying the formulæ. It may be said that this objection, whatever be its weight as bearing on the actually calculated results, is not fatal to the principle of the explanation; whether it is so or not I shall not at present inquire. I make the remark principally in order that the reader may estimate the degree of care with which this investigation has been conducted.

I next would observe that the formula

$$x = \int d\,s \cos 2\,\pi \left(\frac{t}{\mathrm{T}} - \frac{f}{\lambda}\right),$$

where $$f = b + \frac{x^2}{2\,(a+b)} + \frac{(a+b)\,s^2}{2\,a\,b},$$

is true only when N is very near to M; and yet by taking ∞ as the superior limit of s in the integration, it is assumed to hold for all points of the circumference A M N, as well of that part which is cut off by the body A G as of any other. What is meant by making ∞ the superior limit? A more preposterous absurdity was never propounded.

Draw P F touching A M N in F; then if the expression for x were true for all the elements of the arc A M F, we should find the disturbance at P by taking for the limits of s, $s = -\mathrm{A\,M}$ and $s = \mathrm{M\,F}$ respectively. But that expression only holds when s is small, it being agreed that the effect of all other parts of the front may be neglected; hence we can only assume for the superior limit of s a quantity s', respecting which all we can say is that it is small compared with a. To talk of giving s' a definite value is an absurdity. If it be asked, How are we to find the values of the integrals, since the indefinite integrals $\int dr \cos \frac{\pi}{2} r^2$ and $\int dr \sin \frac{\pi}{2} r^2$ cannot be accurately expressed?—I reply that it is unnecessary that the integrals should be accurately expressed, but they may be approximately. In the above investigation, as in every other where the theory of secondary waves is employed, in making the approximation, Fresnel stops at the point where it suits him; and after neglecting all powers of s higher than the second up to a certain point, immediately begins to give them significance. This will readily appear, for we have without approximation,

$$f = \mathrm{PN} = \sqrt{\mathrm{NO}^2 + \mathrm{PO}^2 - 2\,\mathrm{PO}.\mathrm{NO} \cos \mathrm{NOP}}$$

$$= \sqrt{a^2 + (a+r)^2 - 2a(a+r) \cos \frac{s}{a}},$$

(where $\mathrm{PO} = a + r$) an expression which is unmanageable unless s be small, in which case we have

$$f = \sqrt{r^2 + a(a+r)\frac{s^2}{a^2}} = r + \frac{a+r}{2ar} s^2,$$

and

$$x = \int ds \cos 2\pi \left(\frac{t}{\mathrm{T}} - \frac{f}{\lambda}\right) = \int ds \cos \frac{2\pi}{\lambda}(vt - f)$$

$$= \int ds \cos \frac{2\pi}{\lambda}\left(vt - r - \frac{a+r}{2ar}{}^2\right)$$

$$= \int ds \left\{\cos \frac{2\pi}{\lambda}(vt-r) \cos \frac{2\pi}{\lambda} . \frac{a+r}{2ar} s^2 - \sin \frac{2\pi}{\lambda}(vt-r) \sin \frac{2\pi}{\lambda} . \frac{a+r}{2ar} s^2\right\}.$$

Now since we have agreed that all powers of s above the square may be neglected, we are authorized to put 1 for the cosine of the arc $\frac{2\pi}{\lambda} \frac{a+r}{2ar} . s^2$, and the arc itself for its sine in the above expression. Making these substitutions and integrating, we have

$$x = c + s \cos \frac{2\pi}{\lambda}(vt-r) - \frac{2\pi}{\lambda} . \frac{a+r}{2ar} . \frac{s^3}{3} . \sin \frac{2\pi}{\lambda}(vt-r);$$

and if s_1 and s_2 be the limits of s,

$$x = (s_2 - s_1)\left\{\cos \frac{2\pi}{\lambda}(vt-r) - \frac{2\pi}{\lambda} . \frac{a+r}{2ar} . \frac{s_1^2 + s_2^2 + s_1 s_2}{3} \sin \frac{2\pi}{\lambda}(vt-r)\right\}$$

$$= (s_2 - s_1) \sec \theta \cos \frac{2\pi}{\lambda}(vt - r + \theta),$$

where

$$\tan \frac{2\pi}{\lambda} . \theta = \frac{2\pi}{\lambda} . \frac{a+r}{2ar} \frac{s_1^2 + s_2^2 + s_1 s_2}{3},$$

a result totally irreconcileable with the phænomena observed, and sufficient to disprove (so far as in the face of the analytical difficulties it presents can be disproved) the whole theory of secondary waves.

If it be contended that Fresnel's integrals are correctly deduced, I reply that they are so only by taking into account other terms in the values of the sine and cosine of the arc $\frac{2\pi}{\lambda} \cdot \frac{a+r}{2ar} \cdot s^2$, besides those in which s rises to the second power, and which we are not entitled to do.

For the effect of taking into account such additional terms must be either sensible or insensible. If it is sensible, we are not justified in leaving out of consideration similar terms in calculating the value of f; if the effect is insensible, there is an end of Fresnel's explanation.

I purpose to advert, in a future paper, to other points in this important subject.

Liverpool, December 18, 1844.

X. *Report on the Meteorology of Toronto in Canada.*

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

The subject which I am about to bring before the Section consists of a portion of the results of the meteorological observations which have been made at the magnetical and meteorological observatory at Toronto in Canada, in the first two years of its establishment. It is well known to the members of the Section, that in conformity with the recommendation made by this Association, the British Government has formed establishments in various parts of the globe, for the purpose of making magnetical and meteorological observations on a systematic plan, and has created a department for the reduction and publication of the observations. As the officer entrusted with the conduct of these operations, I regard it as not less a duty than a pleasure, to communicate, from time to time, at the meetings of the British Association, such of the arrangements, or of the observations themselves, or of the conclusions to which they may have led, as I may suppose may be interesting to its members. I have accordingly selected for the present occasion some portion of the results which the meteorological observations at Toronto, in 1841 and 1842, have yielded, when subjected to a full process of reduction, and carefully examined. I have preferred the meteorological to the magnetical observations, partly on account of the more popular character of the subject generally, and partly because the conclusions to which the meteorological observations have already conducted appear to possess a completeness and fullness not yet attained in magnetism. The observations, which will be treated of in this communication, were made at every second hour throughout the year, except on Sundays, Christmas day, and Good Friday. Subsequently to the period which will be now passed in review, they have been made hourly,

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[*] Communicated to the Mathematical and Physical Section of the British Association for the Advancement of Science, at the York meeting, 1844.

and the results of these may possibly be brought before the Section on a future occasion.

For the purpose of rendering this communication more interesting and more useful, I have compared the meteorological results obtained at Toronto with those obtained by M. Kreil at the magnetical and meteorological observatory at Prague in Bohemia*. It is frequently found that we gain more by such comparisons,—by the points of resemblance and points of difference, and by the analogies and contrasts which they bring to our notice,—than we do by a simple direct investigation.

Prague like Toronto is situated at a considerable distance from the ocean (between 300 and 400 miles) in the interior of a great continent, the latitude and elevation moreover not being very dissimilar. The agreement which will be shown in the leading features of their meteorology manifests that these features belong to a locality so circumstanced, whether the continent be Europe or America; whilst the minor differences point to climatological distinctions of a secondary order, important indeed to discuss from their bearing on the health and occupations of mankind, as well as in more purely scientific respects, but into which time will scarcely permit me to enter on the present occasion beyond a mere notice of some of the facts.

In all comparisons between places situated in Europe and in North America, there is one leading difference in respect to temperature which we must expect to find, which is doubtless familiar to all the members of the Section, viz. that in Europe we enjoy a climate of higher mean temperature in proportion to the latitude than is the case in America; in other words, that the isothermal lines descend into a lower latitude in America than they do in Europe. It would occupy far too much time to discuss, on the present occasion, the causes of this great climatological difference; they have been largely discussed by many eminent philosophers; but it may be well, before we proceed to further details, to notice briefly the *amount* of difference in this respect which is shown by the observations at Prague and Toronto.

The following statement exhibits the particulars of the latitude, elevation above the sea, and mean temperature of the two stations; as well as the correction of the difference of their mean temperatures on account of difference of elevation:—

Toronto, latitude	43° 39′	Elevation	330 feet.
Prague, „	50 05	„	582 „
Difference	6 26	Difference	252
Prague should be colder on account of its elevation .			0°·8 Fahr.
Mean temperature, Toronto	44°·4	Difference ...	4 ·3
„ Prague .	48 ·7 †		
Difference of temperature corrected for difference of elevation		Prague warmer	5 ·1

Whence it appears that Prague is 5°·1 warmer than Toronto, although its latitude is 6° 26′ more distant from the equator.

TEMPERATURE.

We will now proceed to the distribution of the mean temperature into the several hours of the day, and into the several months of the year; the first, forming the *diurnal* variation of the temperature, or that variation which has *a day* for its period; the second, the *annual* variation, or that variation which has *a year* for its period.

Diurnal Variation.—The diurnal variation is the well-known consequence

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* Mag. und Met. Beobachtungen: Prag. 1839–1842.

† Kreil, Jahrbuch für 1843.

of the earth's rotation on its axis. It is a single progression; having but one ascending and one descending branch, the turning points being a maximum early in the afternoon, and a minimum about sunrise. Each hourly mean in each year in the subjoined table is an average of about 311 observations, being one on each day, except Sundays, Good Friday and Christmas day. Each hourly mean of the two years is therefore an average of about 622 observations. The mean temperature of each year, or of all the hours on all the days of the year, rests on about 3732 observations; and the mean temperature of the two years on about 7464 observations. The very small amount of the differences which the table exhibits in the results at the several hours in 1841 and 1842, shows a probability that we have already determined the diurnal march of the temperature, (as far as it can be obtained by two-hourly observations,) with a very near approximation to the truth*.

Mean Annual Temperature at every observation hour.

		6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.	Mean.
Toronto.	1841...	39°·0	42°·4	46°·2	48°·8	50°·4	50°·3	48°·1	44°·0	42°·0	40°·7	39°·5	38°·8	44°·2
	1842...	39·8	42·9	46·5	49·1	50·7	50·8	48·2	44·2	42·3	41·0	40·2	39·6	44·6
	Mean.	39·40	42·65	46·35	48·95	50·55	50·55	48·15	44·10	42·15	40·85	39·85	39·20	44·4

Temperature at the several observation hours higher (+) or lower (—) than the Mean Annual Temperature.

	6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.	Mean.
Toronto....	− 5·0	− 1·75	+ 1·95	+ 4·45	+ 6·15	+ 6·15	+ 3·75	− 0·3	− 1·25	− 3·55	− 4·55	− 5·2	
Prague......	− 4·7	− 2·6	+ 0·9	+ 3·8	+ 5·2	+ 5·1	+ 3·7	+ 0·8	− 1·1	− 2·3	− 3·4	− 4·4	

Toronto proportionally colder (—) or warmer (+) than Prague at the several observation hours.

	6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.	Mean.
	− 0·3	+ 0·85	+ 1·05	+ 0·65	+ 0·95	+ 1·05	+ 0·05	− 1·1	− 0·15	− 1·25	− 1·15	− 0·8	

If we take the difference between the mean temperature at Toronto derived from all the observations (44°·4), and the mean of all the temperatures observed at each of the observation hours, we have the mean diurnal march of the thermometer as shown in the table, or how much the temperature amount is above or below its mean at each hour of observation.

In the line immediately beneath the diurnal march of the temperature at Toronto, is placed the diurnal march at Prague, by which means the general resemblance and the minor differences can be at once perceived by the eye.

These latter are further shown in the last line, which points out the hours when the temperature is proportionally warmer at Toronto than at Prague, which hours have a + sign before them, and those when it is proportionally colder, which are characterized by the — sign. It will be at once obvious that the climate at Toronto is proportionally warmer during the hours of the day, and colder during those of the night, than at Prague. Toronto being in a lower latitude and therefore nearer the sun, the sun's influence is proportionally greater during the hours of the day; but in the absence of the sun, the powerful causes which, in spite of the difference of latitude, depress the isothermal lines, show their unchecked influence in the proportionally lower temperature of the hours of the night. So strong indeed are those causes, that at no one hour of the twenty-four does the absolute temperature at Toronto rise to an equality with that of Prague.

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* The building of the observatory at Toronto having been completed in September 1840, the observations now under notice commence with October 1840. The year 1841 in this communication is therefore more strictly the year which commences October 1, 1840, and ends September 30, 1841. In like manner 1842 commences with October 1, 1841, and ends with September 30, 1842.

The nights being proportionally colder and the days warmer than at Prague, the mean daily range of the thermometer is greater, being 9°·9 at Prague and 11°·35 at Toronto. The mean temperature of the 24 hours occurs earlier in the forenoon and earlier in the afternoon at Toronto than at Prague.

Annual Variation.—The next table exhibits the mean monthly temperatures in each month of 1841 and 1842, and their average. In a separate column is shown the amount by which the temperature in each month exceeds or falls short of the mean temperature of the year. This forms the annual variation of the temperature; it is, as is well known, the consequence of the earth's annual motion in its orbit, which regulates the order and succession of the seasons, and occasions a progression of temperature from a minimum in the midwinter to a maximum in the midsummer. This also is a single progression, having but one ascending and one descending branch. The annual variation of the temperature at Prague is placed by the side of that at Toronto, by which means the eye is at once enabled to judge of the general agreement and the minor differences; the latter are also shown more distinctly in the final column.

	Toronto.			Prague. Mean of 20 years.	The several months above (+) or below (−) the annual mean.		Toronto proportionally hotter (+) or colder (−) than Prague.
	1841.	1842.	Mean.		Toronto.	Prague.	
January	25°·6	27°·8	26°·7	26°·9	−17°·7	−21°·8	+4°·1
February	23·2	28·0	25·6	30·8	−18·8	−17·9	−0·9
March	28·1	36·2	32·1	38·6	−12·3	−10·1	−2·2
April	39·5	43·6	41·6	48·8	− 2·8	+ 0·1	−2·7
May	51·2	49·8	50·5	58·0	+ 6·1	+ 9·3	−3·2
June	66·1	56·6	61·3	64·6	+16·9	+15·9	+1·0
July	65·4	64·8	65·1	68·1	+20·7	+19·4	+1·3
August	64·5	65·7	65·1	66·7	+20·7	+18·0	+2·7
September	61·3	55·8	58·5	60·2	+14·1	+11·5	+2·6
October	44·7	41·9	43·3	50·1	− 1·1	+ 1·4	−2·5
November	35·7	35·3	35·5	38·8	− 8·9	− 9·9	+1·0
December	24·8	29·8	27·3	33·0	−17·1	−15·7	−1·4
Mean	44·2	44·6	44·4	48·7			

Difference between the hottest and coldest month.
Prague …… 41°·2
Toronto…… 39°·5

In viewing the minor differences shown in the last column, we must not overlook that our numbers are based on two years only of observation, and that for an *annual* progression, a single year forms in fact but a single experiment. When we view the differences which some of the months present in the columns representing the observations in 1841 and 1842, we shall readily acknowledge that more than two years are required to give that approximation to a mean annual progression which the present state of science requires. There are, however, some features of difference which present such obvious characters of system that we may have reason to expect that the observations of a greater number of years will but make them more assured. Thus the spring months are all proportionally colder, and the summer months hotter, at Toronto than at Prague. There is also one remarkable difference, viz. in January, which is proportionally a colder month by above 4° at Prague than at Toronto; and from the magnitude of the amount, it wears the aspect of a permanent climatological difference. Now it is well known that in the month of January the wind from the east and north-east prevails in Europe, bringing with it our severest winter cold. This feature has not a parallel in North America, where the cold of winter is more equably distributed. It would occupy too much time to discuss the cause of this peculiarity in the European climate; and I must content myself with referring generally to M. Dove's elaborate work on the distribution of temperature; a work which cannot fail to impress the reader strongly with the value of the conclusions to be derived from long-continued series of observations subjected to a laborious and persevering study. It is a curious result from this excess of cold in Europe in January, that notwithstanding the greater proportional warmth in summer and cold in spring at Toronto, the extreme difference, or that between the coldest and the warmest month of the year, is absolutely greater at Prague than at Toronto, being 41°·2 at Prague and 39°·5 at Toronto.

It is a consequence of the minor differences already pointed out, that a temperature equal to that of the mean temperature of the year occurs later in spring and earlier in autumn at Toronto than at Prague; and that the temperature is higher than the mean of the year during seven months at Prague, whilst at Toronto it is only so during five months.

I have inserted in the next table the mean *range* of the thermometer during three years at Toronto and at Prague. It must be understood that the maximum of each month inserted in this table is the *mean* maximum during three years; viz. March 1840 to March 1843 at Toronto; July 1839 to July 1842 at Prague; and the same is to be understood of the minimum: the range is consequently a *mean* range during three years, and is of course exceeded by the range in individual years.

Range of the Temperature in different Months.

	Toronto (3 years).			Prague (3 years).		
	Max.	Min.	Range.	Max.	Min.	Range.
January	+47°·3	+ 1°·8	45°·5	+46°·8	+ 4°·0	42°·8
February	+42·7	− 1·0	43·7	+43·9	+ 1·6	42·3
March	+59·1	+ 7·0	52·0	+54·8	+13·8	41·0
April	+72·3	+23·0	49·3	+72·6	+29·1	43·5
May	+75·7	+29·5	46·2	+83·5	+40·0	43·5
June	+82·3	+37·2	45·1	+88·0	+47·0	41·0
July	+85·5	+45·1	40·4	+92·0	+50·6	41·4
August	+81·8	+47·0	34·8	+84·0	+48·4	35·6
September	+78·2	+32·0	46·2	+82·5	+40·2	42·3
October	+65·6	+25·5	40·1	+69·8	+31·4	38·4
November	+57·6	+12·7	44·9	+59·5	+29·5	30·0
December	+42·6	+ 3·7	39·0	+47·0	+12·4	34·6
		Mean range ...	43·9		Mean range ...	39·7

Toronto { Highest, June 29, 1841 +91·7
Lowest, Feb. 16, 1842 − 8·2*
Range ... 99·9

Prague { Highest, July 18, 1841 +97·8
Lowest, Dec. 15, 1840 − 7·0
Range ... 104·8

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* The thermometer ranged much lower in January 1840, before the commencement of the series under notice, viz.—

January 2nd	− 17·5	January 15th	− 8·5
„ 3rd	− 9·2	„ 16th	− 15·0
„ 4th	− 10·0	„ 17th	− 19·2 lowest observation.

Here also the general character shown by the comparison of the two stations is that of very close resemblance, while the minor differences also stand out prominently. The greater variation to which the temperature is subject at Toronto in March and April is very obvious in the column of range; as is also the small amount of the variation in the month of November at Prague. The mean monthly range deduced from the twelve months is 43°·9 at Toronto, and 39°·7 at Prague; a considerable amount of difference, and which marks the greater general vicissitude of the climate of Toronto: still it is deserving of notice that Prague is occasionally liable to fully as great, and (during these three years at least) even greater extremes of temperature than Toronto, as is shown by the memorandum at the foot of the table; it is indeed curious to remark how very nearly the stations approach each other in the extreme amount of their thermometrical range. July and August are the only months in which during three years the observations at Toronto never show a temperature of the air so low as the freezing point. At Prague there are five months, viz. from May to September inclusive, in which during the three years the temperature was never observed so low as 32°.

If we seek in the old continent a station most nearly isothermal with Toronto, we must refer to a latitude considerably higher than Prague. The station in M. Mahlmann's list (Dove, Repertorium, b. 4, and Humboldt, Asie Centrale, tom. 3.), which most nearly resembles it in the mean temperature of the different seasons, as well as in that of the whole year, is Wexiö in Sweden, in latitude 56° 53′, and height above the sea 450 Parisian feet. Toronto is in 43° 39′, and height above the sea 330 English feet. The mean temperatures are—

	Spring.	Summer.	Autumn.	Winter.	Annual.	Coldest month.	Warmest month.
	°	°	°	°	°	°	°
Toronto	41·4	63·8	45·8	26·5	44·4	25·6	65·1
Wexiö	41·5	63·8	44·8	27·8	44·5	27·0	66·0

Aqueous Vapour.

I proceed to consider the elastic force or tension of the aqueous vapour contained in the atmosphere, and the degree of humidity produced by it, together with the diurnal and annual variations of these phænomena.

The elastic force of the vapour is considered to be one of the constituents of the pressure upon the surface of the mercury in the cistern of the barometer, which, conjointly with the other and much larger constituent, viz. the pressure of the gaseous atmosphere, produces what in common parlance is called the *pressure of the atmosphere*, measured by the height of the mercurial column in the barometer. Although we have no instrument by which we can measure the gaseous pressure independently of that of the aqueous vapour, we possess in Daniell's hygrometer, and in the wet and dry thermometers, the means of ascertaining the aqueous pressure at any instant independently of the gaseous pressure; and therefore, by the combination of the barometer and of the wet and dry thermometers (or of the hygrometer before mentioned), we should be able to obtain separately the pressure due to each constituent, and the annual and diurnal variations of both. It will be understood, therefore, that when the "tension of the vapour" is here mentioned, it expresses also the pressure on the barometer produced by the elastic force of the vapour present in the air.

The scale in which the humidity of the air is expressed is the simple natural scale in which air at its maximum of humidity (*i.e.* when it is saturated with vapour) is reckoned as = 100; and air absolutely deprived of moisture as = 0: the intermediate degrees are given by the fraction

$$\frac{100 \times \text{actual tension of vapour,}}{\text{tension required for the saturation of the air at its existing temperature.}}$$

Thus if the air at any temperature whatsoever contains vapour of half the tension which it would contain if saturated, the degree is 50; if three-fourths, then 75; and so forth.

Air of a higher temperature is capable of containing a greater quantity of vapour than air of less temperature; but it is the proportion of what it *does* contain to what it would contain if saturated, which constitutes the measure of its dryness or humidity.

The capacity of the air to contain moisture being determined by its temperature, it was to be expected that an intimate connexion and dependence would be found to exist between the annual and diurnal variations of the vapour and of the temperature. I shall proceed to show how distinctly and fully this connexion is exhibited by the observations at Toronto. We will commence with the humidity.

Diurnal Variation.—The degree of humidity at the several observation hours exhibits, as in the case of the temperature, a simple progression of one ascending and one descending branch, having its turning points the same as those of the temperature, namely, a maximum at or near the coldest, and a minimum at or near the hottest hours of the day; the progression is inverse, but is in harmony with that of the temperature.

Mean degree of Humidity at Toronto at the several Observation Hours.

	6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.	Mean.
1841...	88	83	77	73	70	69	72	79	82	84	85	86	79
1842...	86	81	73	70	68	67	71	78	81	82	84	84	77
Mean.	87	82	75	71·5	69	68	71·5	78·5	81·5	83	84·5	85	78

The accord of the two years' observations is remarkably satisfactory; they unite in showing that in the average state of the atmosphere at Toronto, the air is charged with between three-fourths and four-fifths (or more exactly with 78 parts in 100) of the vapour required for its saturation.

When we proceed to the mean tension of the vapour at the several observation hours, we perceive an accord with the march of the temperature fully as striking; one ascending, one descending branch;—the turning points in obvious dependence,—and the march harmonious; in this case the progression is direct, in relation to that of the temperature,—as it was inverse in the case of the humidity.

Mean Tension of the Vapour at the several Observation Hours.

	6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.	Mean.
	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
1841...	·249	·268	·282	·293	·296	·287	·276	·264	·254	·251	·243	·240	·267
1842...	·234	·251	·259	·271	·275	·273	·263	·250	·245	·236	·233	·229	·252
Mean.	·242	·260	·270	·282	·285	·280	·269	·257	·250	·243	·238	·234	·259

The direct evidence of connexion and dependence exhibited in the diurnal march of the vapour and temperature at Toronto is the more deserving of our notice, because in many climates, this connexion, though it always exists, is partly obscured by other less direct influences of the temperature. Thus at Trevandrum, in the East Indies, where the zeal of our indefatigable associate Mr. Caldecott, Director of the Magnetical and Meteorological Observatory established by His Highness the Rajah of Travancore, has already accumulated, reduced, and transmitted to England five years of hourly observations with the wet and dry thermometers, the maximum and minimum of the tension are found to occur within three hours of each other; the minimum coinciding with the coldest hour, viz. at 6 A.M.; but the maximum occurring at 9 in the forenoon. This may possibly be a consequence of the sea breeze, which springs up as the sun gains power, and as the earth warmed by the solar rays heats the air in contact with itself and causes it to rise, occasioning an inpouring of the air from over the surface of the ocean. The sea breeze brings an influx of fresh air charged with vapour; the air in its turn is heated and ascends, but the vapour is subject to a different law; and though a portion of it is doubtless rapidly conveyed upwards by the ascending current, it is probably the accumulation below which causes an immediate rapid rise in the tension of the vapour, making its maximum to occur at a very early hour. The few facts which are yet known regarding the diurnal march of the vapour in different parts of the globe, present many phænomena of this nature, which at first sight appear inconsistent with the dependence of the progression of the vapour on that of the temperature; but which, when duly explained, will doubtless be found directly or indirectly in accordance with it. The knowledge of the phænomena of the vapour in different climates and under different circumstances (such as in insular, littoral, or continental situations, &c.), with the explanation of the various peculiarities which they present, will form hereafter a very interesting and beautiful chapter in the physical history of the globe.

Annual Variation.—We will now proceed to the mean *monthly* humidity and mean *monthly* tension exhibited in the following tables:—

Mean Monthly Humidity.

	Toronto.			Greater (+) or less (−) than the annual mean.
	1841.	1842.	Mean.	
Jan......	87	81	84	+ 6·0
Feb......	80	84	82	+ 4·0
March .	79	76	77·5	− 0·5
April ...	70	71	70·5	− 7·5
May.....	67	64	65·5	− 12·5
June ...	72	76	74	− 4·0
July	74	74	74	− 4·0
Aug.....	81	79	80	+ 2·5
Sept. ...	83	78	80·5	+ 3·0
Oct. ...	84	78	80·5	+ 3·0
Nov.....	86	81	83·5	+ 5·5
Dec.....	84	86	85	+ 7·0
Mean....	79	77	78	

Mean Monthly Tension.

	Toronto.			Greater (+) or less (−) than the annual mean.
	1841.	1842.	Mean	
	In.	In.	In.	
Jan. ...	·135	·130	·132	− ·127
Feb. ...	·107	·138	·123	− ·136
March..	·131	·162	·146	− ·113
April ...	·173	·199	·186	− ·073
May ...	·259	·227	·243	− ·016
June ...	·452	·347	·399	+ ·140
July	·449	·438	·443	+ ·184
Aug. ...	·482	·491	·486	+ ·227
Sept. ...	·453	·351	·402	+ ·143
Oct......	·254	·210	·232	− ·027
Nov. ...	·185	·173	·179	− ·080
Dec.....	·122	·153	·138	− ·161
	·267	·251	·259	

We perceive by the table in which the mean monthly humidity is shown, that the months from March to July are drier than the average of the year, and that the remaining months are more humid than the average. The drier months are those in which the temperature of the air is rising; the most humid those in which the temperature is either falling or nearly stationary. When the temperature is rising the warmth increases more rapidly than the air receives the addition to its vapour required to maintain an equal degree of humidity, and the air becomes in consequence drier. This is even the case in the neighbourhood of extensive lakes, as at Toronto. May is the driest and December the most humid month in the year: and this is also stated to be the case in Europe.

When we turn to the table in which the mean monthly tension of the vapour is shown, we see most distinctly marked the connexion between the temperature and the vapour pressure, and the dependence of the one upon the other; we see a simple progression, the turning points being the same as those of the temperature, and a march as harmonious as we are perhaps entitled to expect from observations of only two years' continuance.

I shall reserve what further I may have to say in regard to the *range* of the vapour-pressure in different months, until we have before us the other constituent of the barometric pressure, viz. the gaseous atmosphere, to which I now proceed.

ATMOSPHERIC PRESSURE.

	Toronto.												
	6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.	Mean.
Mean bar. Pressure.. 29 inch. + 1841.	·624	·637	·638	·616	·595	·591	·598	·609	·613	·607	·606	·610	·612
Mean bar. Pressure.. 29 inch. + 1842.	·613	·628	·631	·612	·594	·590	·595	·602	·603	·596	·593	·595	·604
Mean bar. Pressure.. 29 inch. + Mean.	·618	·632	·634	·614	·594	·590	·596	·605	·608	·601	·600	·602	·608
Deduct pressure of the vapour..	·242	·260	·270	·282	·285	·280	·269	·257	·250	·243	·238	·234	·259
Press. of the gaseous atmosph. 29 inches + ..	·376	·372	·364	·332	·309	·310	·327	·348	·358	·358	·362	·368	·349
Pressure at each hour greater (+) or less (−) than the mean annual pressure	+·027	+·023	+·015	−·017	−·040	−·039	−·022	−·001	+·009	+·009	+·013	+·019	

Diurnal Variation.—The first two lines of this table exhibit the mean monthly pressure on the mercurial column at Toronto at the several observation hours of 1840 and 1841,—the mean of the two years is shown in the third line. The close accord of the mean pressure at the same hours in each of the two years is a very satisfactory testimony of the confidence to which these barometrical results are entitled: the mean at each hour of each year represents about 311 observations; consequently in the two years the mean at each observation hour represents about 622 observations, the mean of all the hours in the one year 3732 observations, and in the two years 7464 observations.

The diurnal march of the barometer may consequently be regarded as a very near approximation to the truth. The diurnal march of the vapour pressure is obtained by an equal number of observations, and may therefore also be viewed as a very near approximation to the facts of nature. By deducting the vapour pressure from the whole barometric pressure at each observation hour, we should obtain the daily march of the gaseous atmosphere. This is shown in the fifth line of figures in the table; and by taking the difference between the last column, (*i. e.* between the mean gaseous pressure at all the observation hours in the two years,) and the pressure at each hour, we obtain the amount by which the pressure is greater or less at each observation hour than the mean general pressure at all the hours.

On first casting our eyes (in the last line of the preceding table) on this representation of the diurnal variation of the gaseous atmosphere, freed from the complication which its combination with the vapour pressure produces in the indications of the barometer, we cannot fail to be immediately struck with the very close correspondence of the diurnal march before our eyes with that of the temperature which we have already examined. The maximum of pressure is at 6 A.M.; the minimum at 2 P.M. The progressions take place in the opposite or inverse sense to each other, but they are remarkably harmonious, and leave no doubt of a mutual connexion, and of the dependence either of the one on the other, or of both on a common cause.

An explanation of this connexion, which presents itself to the mind as soon as the facts are clearly perceived, may be thus stated :—As the temperature of the day increases, the earth becomes warmed and imparts heat to the air in contact with it, and causes it to ascend. The column of air over the place of observation thus warmed rises, and a portion of it diffuses itself, in the higher regions of the atmosphere, over adjacent spaces where the temperature at the surface of the earth is less. Hence the statical pressure of the column is diminished. On the other hand, as the temperature falls, the column contracts, and receives in its turn a portion of air which passes over in the higher regions from spaces where a higher temperature prevails; and thus the statical pressure is augmented.

This explanation is merely the extension to the particular case of the diurnal variation, of principles which have long been familiar to meteorologists in accounting for various other atmospherical phænomena, such for example as monsoons, and land and sea breezes. To make the parallel complete, it should be shown that, when the temperature rises, an influx of air takes place towards the lower part of the column, proportioned to the ascending current, and tending to replace the air which is thus removed. The observations which will be cited in the sequel of this communication will show that such is precisely the fact at Toronto. The force of the wind, taken without reference to its direction, has also its diurnal variation, corresponding in all respects with the diurnal march of the temperature and of the gaseous pressure; being a minimum at 6 A.M., and a maximum at 2 P.M.—increasing with the augmentation of the temperature, and decreasing with its diminution. The air which thus flows in, becoming warmed, pursues in its turn the course of the ascending current. We have thus the double evidence of the existence of this current,—1st, in the diminution of pressure, showing the outpouring at one extremity; and 2nd, in the increased force of the wind, showing the inpouring at the other extremity. As the temperature keeps *continually* rising, both the demand for and the supply of fresh inflowing air progressively increase. The diminution which the gaseous pressure continues to undergo as long as the temperature continues to rise, shows, as we might naturally expect, that the supply is continually somewhat in arrear of the demand.

The diminution of the gaseous pressure and increase in the force of the wind being consequent on the rise of the temperature, the turning points of the two former phænomena might be expected to occur somewhat later than the instant of minimum temperature; and this appears by the tables to be the case, but will probably be more clearly shown when the hourly observations shall come under review.

Annual Variation.—Let us now proceed to the mean pressure of the gaseous atmosphere in each *month* of the year, and its consequent *annual* variation. These are shown in the following table :—

MEAN MONTHLY PRESSURE.

	Toronto.						Prague.	
	Barometer.			Vapour.	Gaseous pressure.	Gaseous pressure in each month greater (+) or less (−) than the mean annual pressure.	Gaseous pressure.	Gaseous pressure in each month greater (+) or less (−) than the mean pressure.
	1841.	1842.	Mean.					
January	29·664	29·508	29·586	·132	29·454	+·105	29·213	+·194
February......	·489	·548	·518	·122	·396	+·047	29·227	+·208
March.........	·657	·638	·647	·146	·501	+·152	29·089	+·070
April	·621	·548	·584	·186	·398	+·049	28·973	−·046
May	·545	·586	·565	·243	·322	−·027	28·923	−·096
June	·543	·585	·564	·399	·165	−·184	28·898	−·121
July...........	·620	·655	·637	·443	·194	−·155	28·861	−·158
August	·698	·712	·705	·486	·219	−·130	28·882	−·137
September ...	·606	·662	·634	·402	·232	−·117	28·912	−·107
October	·636	·643	·639	·232	·407	+·058	29·045	+·026
November ...	·615	·568	·592	·179	·413	+·064	29·047	+·028
December ...	·652	·597	·625	·137	·488	+·139	29·163	+·144
Mean	29·612	29·604	29·608	·259	29·349		29·019	

In turning our attention to the column which exhibits the excess or defect of the mean monthly pressure on the mean of all the months, we at once perceive another illustration of the principle which has been just stated. We find the pressure of the gaseous atmosphere diminished in the summer months and augmented in the winter months. The general dependence on the march of the temperature is manifest; and it must remain for the additional evidence which will be produced by the observations of subsequent years, to determine, whether the minor deviations from a perfectly harmonious march are mere accidental differences, which a wider observation basis will cause to disappear, or whether they may not point to some other periodical influence (possibly of the temperature also, but of a less direct nature) which is as yet unrecognized*.

I will now ask the Section to turn its attention for a moment to the column which presents the mean height of the *barometer* in each month of the year. It is curious to observe how completely the annual march of the gaseous atmosphere is masked in the barometer by its combination with the vapour pressure, both being measured in one by the mercurial column; the increase of temperature, which causes the gaseous pressure to diminish, occasions the increase of the vapour, and *vice versâ*; and so nearly are these two opposite effects of the one cause balanced at Toronto, that the height of the barometer remains very nearly the same in every month of the year; or at least, shows no trace whatsoever of an annual period.

The principle which has been thus adduced for the purpose of explaining the annual and diurnal march of the atmospheric pressure should be ge-

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* The very few meteorological registers, which have been maintained with proper care for several years together in Europe, are stated to afford very decided indications of the existence of other fluctuations besides the annual and diurnal variations, which apparently do not proceed from merely local causes, but recur regularly at stated periods of the year, and are recognisable simultaneously over widely extended spaces, such for example as a considerable portion of an entire continent. How far the high pressure of the month of March at Toronto may be a phænomenon of this class it may perhaps take some years to decide. It is of very marked character, and is shown decidedly in both years. As I have already remarked, each year is but a single experiment in investigations of annual phænomena.

neral in its application. I have inserted in the table the gaseous pressure at Prague, as it is given by M. Kreil in his 'Jahrbuch' for 1843, from the observations of three years. The march of the vapour, as far as it has yet been determined at Prague, does not present a curve agreeing quite so satisfactorily with that of the temperature as we have been able to deduce at Toronto: whether this arises from disturbing influences in nature (such possibly as indirect influences of temperature), or whether it will disappear by longer-continued observation, cannot be yet anticipated. What is still uncertain, however, at Prague, is not of magnitude sufficient to obscure the dependence of the annual progression of the gaseous pressure on that of the temperature. The measure of agreement in this respect at the two stations cannot be viewed otherwise than as highly interesting and satisfactory. Mean quantities derived from a greater number of years will in all probability show even a closer accordance.

We will now revert to the maximum, minimum and range of the vapour pressure in the several months of the year, for the purpose of showing that its variations are such, as to seem to claim a greater attention than they have hitherto received, at the hands of those who are engaged in investigating the non-periodic fluctuations of the atmosphere, by the comparison of observed barometrical heights. In the next table we have the maximum, minimum, and range of the vapour pressure at Toronto, taken from the mean of two years. By thus exhibiting the mean quantities only of the two years of observation, extremes are of course somewhat moderated; but, on the other hand, there is the advantage that the numbers are probably a more faithful representation of what may be expected in ordinary course.

Range of the Barometer.			Maximum, Minimum, and Range of the Tension of Vapour.					
	Toronto.	Prague.	Toronto—Mean of 2 years.			Prague—Mean of 2 years.		
			Max.	Min.	Range.	Max.	Min.	Range.
	in.	in.	in.	in.	in.	in.	in.	in.
January	1·335	1·364	·221	·050	·171	·306	·051	·255
February.........	1·221	1·156	·262	·050	·212	·238	·052	·186
March............	1·275	1·158	·350	·045	·285	·278	·067	·211
April	1·190	0·864	·385	·085	·300	·406	·149	·257
May	0·846	0·881	·532	·105	·427	·555	·163	·392
June	0·623	0·873	·709	·143	·566	·659	·222	·437
July..............	0·696	0·593	·775	·202	·573	·662	·245	·417
August	0·656	0·647	·762	·262	·498	·556	·257	·299
September	0·754	0·755	·727	·158	·569	·567	·217	·350
October	0·934	0·829	·487	·096	·391	·447	·124	·323
November	0·945	1·036	·375	·066	·309	·414	·138	·276
December	1·527	1·222	·263	·048	·215	·300	·058	·242
Mean	1·000	0·950	·486	·109	·377	·449	·145	·304

We here perceive that the mean monthly range of the tension of the vapour falls little short of four-tenths of an inch; and that in the summer months of June, July, August, and September, when it is greatest, it is very little less than the whole range of the barometer in the same months. Winter is the season for the great fluctuations of the barometer; summer for those of the vapour pressure. If, as is believed by many modern meteorologists, the fluctuations of the vapour pressure affect the barometer to their whole extent, then the fluctuations of the gaseous atmosphere at Toronto approach much nearer to an equality in the two seasons of summer and winter, than do those of the barometer. A north-west wind at Toronto is usually accompanied by a rise in the barometer and a fall in the temperature with a diminution in the tension of vapour; and a south or south-east wind, by a fall in the barometer and a rise in the thermometer with an increased tension of vapour. In a change from one of these winds to the other, consequently, the alteration of the gaseous pressure would be greater than that of the barometric pressure, which is partially counteracted by the accompanying change in the elastic force of the vapour: and as already noticed, the fluctuations in the vapour pressure are very considerable in summer. I have selected some remarkable instances in a single year, 1841, which are as follows:—

Variations of Vapour Pressure in 1841.

		d.	h.			d.	h.				
Between	May	30	16	and	June	5	4	under	6	days	0·594
,,	June	11	0	,,	June	15	10	,,	5	,,	0·503
,,	June	30	2	,,	July	2	6	,,	3	,,	0·610
,,	July	2	6	,,	July	5	4	,,	3	,,	0·465
,,	July	23	4	,,	July	25	16	,,	3	,,	0·500
,,	Aug.	18	2	,,	Aug.	23	14	,,	6	,,	0·496

If the principles are correct, of which we have here traced a portion of the consequences, barometrical observations generally must lose an essential part of their value when unaccompanied by hygrometrical observations, by means of which the pressures of the air and vapour may be separated. Whenever such complete observations are made, *i.e.* hygrometric as well as barometric, the tension of vapour should be computed on the spot and at the instant. When calculations of this nature are suffered to fall in arrear, unreduced observations accumulate, and danger is incurred that the calculations are never made, and that science will lose the advantage which the observations were capable of affording.

The comparison of the barometric range in the different months at Toronto and Prague exhibits a very satisfactory accordance, and shows how similar are the phænomena which present themselves in this respect over the two continents.

The comparison of the range of the vapour pressure at Prague and Toronto exhibits only such differences as may be reasonably ascribable to the greater range of the temperature at Toronto, and possibly to the greater facility with which the air can acquire vapour at that station from the great lakes in its vicinity.

It may be worthy of notice, that the highest and lowest barometric observations in the two years at Toronto occurred within a very few days' interval of each other, being apparently parts of one great atmospheric wave.

The highest and lowest barometric observations at Prague also took place within a few days of each other, and at the same season, viz. midwinter, but a year earlier. The observations were as follows, viz.—

Extreme Range of Barometer in 1840, 1841.

	Toronto.		Prague.	
Max.	Dec. 22, 1841	30·417	Dec. 27, 1840	30·260
Min.	Dec. 4, 1841	28·672	Jan. 4, 1841	28·654
	Interval 18 days. . . .	1·745	Interval 9 days	1·606

We have undoubtedly made a considerable step in advance in meteorology, if we thus correctly substitute the consideration of the separate daily march of the pressures of the vapour and of the gaseous atmosphere, for the comparatively profitless study of the complex effect produced on the barometer by the operation of these two distinct agencies. The labour has been by no means small which has been bestowed in the endeavour to generalise the diurnal phænomena of the barometer by the formation of empirical formulæ; it has been in many instances the labour of highly accomplished men: but we have the recent acknowledgment of a valued and distinguished member of our own body*, who has himself engaged in this inquiry, that it failed in conducting to a recognition of the causes of the phænomena. On the other hand, the moment we apply ourselves to the contemplation of the separate phænomena of the vapour and of the air, there appears to be revealed to us a simple and beautiful dependence of each upon the diurnal march of the temperature, producing effects which in their combination seem also to afford a full and perfect solution of the problem of the daily rise and fall of the barometric column.

It would be unjust to the meteorologists of Germany if we were not gratefully to acknowledge in how great a degree this advance in the science is to be ascribed to their writings, and especially to those of M. Dove. Their meteorological researches have been pressed with an assiduity and devotion of labour which is beyond all praise. In the consideration which we (the members of the British Association) are likely soon to be called upon to exercise, whether any and what great combined endeavours are further desirable to be made for the advancement of meteorological science, we should be indeed inexcusable if we neglected to avail ourselves of the advice, and look with becoming respect to the opinions, of men, who have spent years of untiring labour, and brought great attainments to bear, on a branch of science which has been comparatively less cultivated by ourselves.

Admitting M. Dove's views, we can easily perceive that an empirical formula, in which the diurnal oscillation of the barometer should be made to vary as a function of the latitude, could never universally represent the phænomena. The difference between an insular or littoral station, where the vapour pressure attains its maximum at 9 in the forenoon, and an interior station in the same latitude where the maximum is at 2 or 3 in the afternoon, cannot both be represented with fidelity by a formula in which this difference is not taken into the account. At stations where the maximum of vapour pressure takes place at 9 A.M., and the tension thenceforward descends until the afternoon,—(as at Trevandrum),—the range of the diurnal oscillation of the barometer will be greater, *ceteris paribus*, than when, as at Toronto, the vapour pressure progressively rises from sunrise to a maximum at 2 or 3 in the afternoon: the hours of maximum and minimum will also be somewhat modified.

The important problem of the equality or inequality of the mean pressure of the gaseous atmosphere at the level of the sea at different points on the surface of the globe, has lately begun to occupy the attention of physical philosophers in a degree which will probably tend, before many years, to its practical solution. In this labour the determinations of our co-operative observatories may perform an important part. Great care has been taken that the barometers of our colonial observatories shall speak precisely the same language as the standard barometer in the Royal Society's Apartments; and steps are now taking to ensure a similar comparison of the barometers, which, in different parts of the United States, are now observed simultaneously with Toronto by our American coadjutors; and which may hereafter, if that observatory should be continued, form a very valuable extensive basis of induction for the movements of the atmosphere over that great continent.

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* Professor J. D. Forbes; Meteorological Report.

Prague and Toronto furnish the materials for an interesting comparison of their respective mean gaseous pressures. I have exhibited this comparison in the subjoined table. After the proper corrections have been applied for the reduction to an invariable scale of pressure, and of the pressure itself to a common elevation above the sea, the residual difference in the pressure is about four hundredths of an inch. This is within the amount of difference that might reasonably be expected in so indirect a comparison. The inference therefore at present must be, that no unaccounted for difference of pressure exists, or at least next to none, at these two stations in Europe and America.

	Inches.
Pressure of the dry atmosphere at Toronto	29·349
Pressure of the dry atmosphere at Prague	29·019
Difference..........	0·330
Reduction to an invariable scale of pressure....	0·017
True difference of pressure..................	0·313
Difference of elevation equivalent to	0·273
Difference of pressure unaccounted for	0·04

Modern researches have shown that the height of the barometer at different points of the earth's surface is not only disturbed by self-adjusting causes which produce temporary displacements, but that there are causes in action which effect persistent differences in the mean height of the barometer in different localities, strictly at the level of the sea; so that, to use the words of Bessel, the mean atmospheric pressure depends on the geographical co-ordinates of a station in latitude and longitude as well as in elevation. This remark of Bessel's is founded chiefly on Erman's observations*; and Erman himself, who has considered the effect of the vapour pressure upon his barometrical heights, concludes that the pressures which the air would have exerted without the presence of aqueous vapour, indicate also persistent differences of mean gaseous pressure depending on geographical position. The instance quoted by Professor Forbes from Captain King†, who found the mean height of the barometer 29·462 in observations repeated five times a day in five consecutive months of summer at Port Famine, is an example of an atmospheric valley, as it has been called, in the former sense, but not in the latter. When allowance is made for the probable vapour pressure, the gaseous pressure at Port Famine will be found greater than its ordinary amount at the Equator; where indeed other observations have indicated a gaseous pressure lower than in the adjacent extra-tropical latitudes.

Assumed equatorial barometer........................	29·95
Deduct vapour pressure (assumed dew point 74°)......	0·83
Mean pressure of the gaseous atmosphere at the Equator	29·12
Barometer at Port Famine in five summer months......	29·462
Deduct vapour pressure (assumed dew point 38°)......	·230
Pressure of the gaseous atmosphere at Port Famine	29·23 ‡

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* Erman, Met. Beob. bei einer Seereise um die Erde.

† Forbes, Reports of the Brit. Assoc., 1832.

‡ This is of course only an approximate comparison; to render it more exact, it would be

In these last remarks I have perhaps ventured further from the strict subject of this communication than I should have been disposed to have done, had I not had in view to call the attention of the Section to what it is in the power of our own country to accomplish, with its widely extended dominions, in the solution of this great problem of the uniformity or otherwise of the mean pressure of the atmosphere, by the establishment of colonial observatories, conducted on a systematic plan, and continuing in operation only until certain specified and definite objects should be attained; such, for example, as the mean values, and the periodical variations, of the several meteorological elements. The present communication is an evidence of the important results which even a very brief duration of such observations may be sufficient to accomplish. When such establishments are proposed, with the sanction and support of the colonial authorities, and with the advantage of men of assured competency to conduct them, we may venture to promise the fullest co-operative aid (that may be compatible with circumstances) on the part of the British Association, which has placed foremost amongst its objects "to give a stronger impulse, and a more *systematic direction* to scientific inquiry."

I have one more point to bring under your notice; a point highly interesting in itself, and completing the evidence of the harmony in the meteorological variations.

It has been noticed that from the diminution of the gaseous pressure as the temperature of the day increases, evidencing an ascending current, we should be prepared to expect a corresponding influx of air at the station, or a diurnal variation in the force of the wind (taken without reference to the direction from which it blows), which should have its minimum at or near the coldest hour of the day, and its maximum at or near the warmest, and its progression in harmony with the curve of temperature, having one ascending and one descending branch. Such is the fact. The subjoined table exhibits the sum of the pressures, expressed in pounds avoirdupois, exerted on a square foot of surface at Toronto, at each of the observation hours in 1841, and the same in 1842. The wind is proverbially uncertain, and our means of measuring its pressure are more imperfect than we could desire; but these numbers afford an ample evidence that there is a diurnal variation in the force of the wind, and furnish a curve which, when projected, is found in remarkable correspondence with the curve of the temperature. This fact, observed at Birmingham by Mr. Osler, has been already brought under the notice of the Association at a former meeting. The diurnal march of the gaseous atmosphere furnishes the additional link in the chain of evidence, by which the connexion between the temperature (producing an ascending current) and the force of the wind (flowing in to replace it) may receive its explanation; placing before us in an intelligible form their mutual relations to each other, as cause and effect.

necessary to regard the influence of the season of the year (summer) at which the barometer was observed at Port Famine; as well as the correction due to the effect of the variation of gravity on the standard of measure. Both corrections would tend to increase the mean pressure of the gaseous atmosphere at Port Famine in comparison with that at the Equator. The barometrical observations made in the Erebus, in the late Antarctic Expedition, furnish a beautiful illustration of the progressive decrease in the height of the barometer from the tropics to the high latitudes, coincident with the diminution of the elastic force of the vapour accompanying the decrease of temperature. I hope that Sir James Ross will shortly publish these interesting observations, with the corresponding pressures of the gaseous atmosphere in the different parallels.

Sum of the pressures exerted by the force of the wind at Toronto on a surface of one foot square at the several observation hours in 1841, 1842.

	6 A.M.	8 A.M.	10 A.M.	Noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mid.	2 A.M.	4 A.M.
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1841...	96	164	168	186	204	169	120	109	121	111	103	101
1842...	126	156	201	238	285	256	181	123	113	112	128	143
Mean	111	160	184	212	244	212	150	116	117	112	116	122

Without ascribing anything like precision to the numbers in this table (which are however likely to be more correct in relative than in absolute value), they lead to the inference that the pressure of the wind, on the average of the whole year, is doubled, or nearly so, between the coldest and warmest hours of the day; *i. e.* between 6 A.M. and 2 P.M. The confirmation, or otherwise, of this remarkable result by the observations of succeeding years cannot fail to be a point of much interest. It appears from the registry of Mr. Osler's anemometer, during four years at Birmingham, that at that station the increase in the pressure of the wind is considerably more than double between the hours of the minimum and maximum temperature. It will influence many reasonings if it shall be found as a fact of pretty general occurrence, that so large a portion of the daily wind is put in circulation to supply an ascending current*.

Synopsis of the Diurnal Variations at Toronto.

Observation hour.	Temperature.	Vapour pressure.	Gaseous pressure.	Force of wind.
	°	in.	in.	lbs.
2 A.M.	39·8	·238	29·362	116
4 A.M.	39·2 Min.	·234 Min.	29·368	122
6 A.M.	39·4	·242	29·376 Max.	111 Min.
8 A.M.	42·6	·260	29·372	160
10 A.M.	46·3	·270	29·364	184
Noon	48·9	·281	29·333	212
2 P.M.	50·5 } Max.	·285 Max.	29·309 Min.	244 Max.
4 P.M.	50·5	·279	29·311	212
6 P.M.	48·1	·268	29·328	150
8 P.M.	44·1	·257	29·348	116
10 P.M.	42·1	·249	29·359	117
Midnight	40·8	·243	29·358	112

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* To the agency of this current we should probably ascribe the upward conveyance of the vapour of increasing constituent temperature as the warmth of the day increases, and which appears to take place more rapidly than the vapour might of itself make its way if the air were tranquil. M. Kreil remarks (Mag. und Met. Beob. Erster Jahrgang, p. 140), that in the summer months, when from the increased amount of the vapour its effects are more noticeable, the clearness of the sky decreases from the commencement of the morning to about noon, and then increases uninterruptedly till towards midnight. And M. Dove notices (Met. Untersuchungen, p. 53), that on fine calm days, when there is little lateral wind to disturb the ascending current, the clear morning becomes clouded towards noon; whilst towards evening, when the ascending current has ceased, these condensed vapours, no longer upborne by its influence, descend into the warmer strata and are redissolved: hence the peculiar transparency and beauty often observed in evening views.

Synopsis of the Annual Variations at Toronto.

Month.	Temperature.	Vapour pressure.	Gaseous pressure.
	°	in.	in.
January	26·7	·132	29·454
February............	25·6 Min.	·123 Min.	29·396
March...............	32·1	·146	29·501 Max.
April	41·6	·186	29·398
May	50·5	·241	29·324
June	61·3	·399	29·165 Min.
July	65·1 } Max.	·443	29·194
August	65·1	·486 Max.	29·219
September	58·5	·402	29·232
October	43·3	·232	29·407
November	35·5	·179	29·413
December	27·3	·138	29·488*

An inference of much practical utility to general observers may be drawn from meteorological observations, made with the frequency which can only be expected at those observatories, where a sufficient establishment is maintained for the express purpose of observation. We may find that comparatively a very few observations in each day, at hours not inconvenient in ordinary life, may furnish a very close approximation to the mean values and to the annual and diurnal march of the atmospherical phænomena. Thus from the complete record at Toronto we find, as shown in the subjoined table, that the mean values of the temperature, of the vapour tension and of the humidity, of the pressure of the gaseous atmosphere, and of the whole atmospheric pressure, may all be obtained, with a very near approximation, by a single observation at 8 P.M. (mean time), provided the observation be made with tolerable precision in regard to the hour. By combining with this an observation about sunrise, and another between 2 and 4 in the afternoon, the maximum and minimum of the temperature, of the aqueous and gaseous pressure, and of the humidity, may also be obtained. These hours are by no means inconvenient for persons whose avocations permit them to keep a register at all; and appear in every way preferable to a selection which makes 3 o'clock in the morning one of the observation hours. That hour is perhaps the most generally inconvenient for the purpose of the whole twenty-four. The hours here suggested must not however be understood to be of universal application: they are not so thoroughly suitable, for example, at stations where, as at Trevandrum, the vapour pressure attains a maximum in the forenoon.

Convenient hours of observation.

For mean values, 8 P.M. mean time (precise); which at Toronto gives the following approximation: viz.—

At 8 P.M. at Toronto.	Temperature	44°·1	Mean annual value	44°·4
	Humidity	78·5	„ „	78·0
	Vapour tension	·257	„ „	·259
	Barometric pressure ..	29·605	„ „	29·608
	Gaseous pressure	29·345	„ „	29·349

For maxima and minima.

From 4 to 6 A.M., for minimum of temperature and tension of vapour, and for maximum of humidity and gaseous pressure.

From 2 to 4 P.M., for maximum of temperature and tension of vapour, and for minimum of humidity and gaseous pressure.

[* Three paragraphs explanatory of the Plates of the meteorological phænomena described, projected in curves, which accompany the original report, are here omitted.]

I have now only to apologize to the Section for the length of time that I have occupied them, and to thank them for their patient attention.

York, September 27th, 1844.

Postscript, Woolwich, Nov. 30.

After the preceding pages were printed, I received from Mr. Airy the volume of the Greenwich magnetical and meteorological observations for 1842, in which the meteorological reductions have been made in almost exactly the same form as those of Toronto. The volume was accompanied by a suggestion from the Astronomer Royal, that it might increase the interest of this communication, if I were to add a few words by way of appendix, showing the points of similarity or dissimilarity in the results at the two stations. I have much pleasure in adopting this suggestion, and in availing myself of Mr. Airy's permission to do so; for I have had great satisfaction in noticing the very remarkable similarity which prevails in the results at Greenwich and Toronto, with reference to several points which have been the objects of especial notice in the preceding discussion. In the diurnal variations of the elastic force of the vapour,—of the gaseous pressure,—and of the force of the wind,—the evidence of a direct dependence on the diurnal march of the temperature is fully as striking at Greenwich as at Toronto, as will be seen by the following synopsis:

Synopsis of the Diurnal Variations at Greenwich.

Observation hours.	Barometer.	Thermometer.	Vapour pressure.	Gaseous pressure.	Force of the wind.
h m	in.	°	in.	in.	Sums of the estimated forces.
1 20 A.M.	29·826	45·4	·307	29·519	97½
3 20 A.M.	·822	44·9 Min.	·302 Min.	·520 Max.	93¼
5 20 A.M.	·824	45·0	·307	·517	89½
7 20 A.M.	·835	47·2	·321	·514	89 Min.
9 20 A.M.	·846	51·0	·335	·511	106½
11 20 A.M.	·845	54·1	·347	·498	117¾
1 20 P.M.	·832	55·7 Max.	·349 Max.	·483	131¼ Max.
3 20 P.M.	·823	55·0	·348	·475 Min.	129
5 20 P.M.	·823	52·8	·338	·485	120¾
7 20 P.M.	·830	49·8	·330	·500	109¾
9 20 P.M.	·838	47·8	·321	·517	102
11 20 P.M.	·836	46·3	·315	·521 Max.	95
Means of the Year.	29·832	49·6	·326	29·506	

At Greenwich the force of the wind is estimated at each observation hour in numbers varying within the limits of 0 to 6. At Toronto the estimation is in lbs. pressure on a square foot of surface kept perpendicular to the current. In single instances the scales are comparable, because the square of the number expressing the force at Greenwich corresponds approximately to the pressure in lbs. avoirdupois. But the comparability of the scales does not hold good when the sums of the forces and the sums of the pressures are taken. The sums of each are however comparable *inter se*, and show the hours of maximum and minimum force, and the regularity of the progression. The registry of the anemometer at Greenwich shows that the pressure of the wind is more than doubled in its mean diurnal range.

The following table exhibits the differences at the several observation hours of Greenwich and Toronto, of the temperature, of the vapour pressure, and of the gaseous pressure, from their respective mean yearly values. The sign + signifies above the mean value of the year, and − below it.

Observation hour.		Temperature.		Vapour pressure.		Gaseous pressure.	
Greenwich.	Toronto.	Greenwich.	Toronto.	Greenwich.	Toronto.	Greenwich.	Toronto.
h. m.	h.	°	°	in.	in.	in.	in.
1 20 A.M.	2 A.M.	−4·2	−4·6	−·019	−·021	+·013	+·013
3 20 A.M.	4 A.M.	−4·7	−5·2	−·024	−·025	+·014	+·019
5 20 A.M.	6 A.M.	−4·6	−5·0	−·019	−·017	+·011	+·027
7 20 A.M.	8 A.M.	−2·4	−1·8	−·005	+·001	+·008	+·023
9 20 A.M.	10 A.M.	+1·4	+1·9	+·009	+·011	+·005	+·015
11 20 A.M.	Noon.	+4·5	+4·5	+·021	+·023	−·008	−·017
1 20 P.M.	2 P.M.	+6·1	+6·1	+·023	+·026	−·023	−·040
3 20 P.M.	4 P.M.	+5·4	+6·1	+·022	+·021	−·031	−·039
5 20 P.M.	6 P.M.	+3·2	+3·7	+·012	+·010	−·021	−·022
7 20 P.M.	8 P.M.	+0·2	−0·3	+·004	−·002	−·006	−·001
9 20 P.M.	10 P.M.	−1·8	−2·3	−·005	−·009	+·001	+·009
11 20 P.M.	Midnight.	−3·3	−3·6	−·011	−·016	+·015	+·009

The mean *monthly* values of the vapour pressure, and of the gaseous pressure at Greenwich exhibit also the same correspondence with the variation of the temperature in the different months of the year as at Toronto.

Synopsis of the Annual Variation of the Temperature, Vapour pressure, and Gaseous pressure at Greenwich.

Month.	Temperature.	Vapour pressure.	Gaseous pressure.	Vapour pressure + or −, *i. e.* greater or less, than the mean annual pressure.	Gaseous pressure + or −, *i. e.* greater or less, than the mean annual pressure.
	°	in.	in.	in.	in.
January ...	32·9 Min.	·186 Min.	29·715 Max.	−·140	+·209
February...	40·8	·250	·626	−·076	+·120
March......	44·9	·272	·475	−·054	−·031
April	45·2	·248	·606	−·078	+·160
May	53·2	·334	·448	+·008	−·058
June	62·9	·433	·468	+·107	−·038
July	60·2	·416	·404	+·179	−·102
August ...	65·4 Max.	·505 Max.	·364	+·095	−·142
September.	56·4	·421	·294 Min.	−·038	−·212
October ...	45·4	·288	·561	−·058	+·055
November.	42·8	·268	·331	−·031	−·175
December.	45·0	·295	·712	....	+·206
Mean of the Year.	49·6	·326	29·506		

It appears therefore that the annual and diurnal variations derived from the observations at Greenwich present a most satisfactory accordance with those at Toronto in those points which were brought most prominently before the Association at York, and to which the attention of the Section was especially called, viz.—

First, in regard to the diurnal variation :

1. The vapour tension and the force of the wind have each a minimum, and the gaseous pressure a maximum, at or near the coldest hour of the day.

2. The vapour tension and the force of the wind have each a maximum, and the gaseous pressure a minimum, at or near the warmest hour of the day.

3. The diurnal march of each from the minimum to the maximum, and from the maximum to the minimum again, is continuous, like that of the temperature, without any interruption deserving of the name.

4. At Greenwich as well as at Toronto the diurnal variations of the vapour tension and of the gaseous pressure, produce by their combination the double

maxima and minima of the diurnal oscillation of the mercury in the barometer.

Secondly, in respect to the annual variation:

The annual march is somewhat less regular at Greenwich than at Toronto, being derived from the observations of a single year only; but we have the same general features: a minimum of temperature and vapour-pressure, and a maximum of gaseous pressure in the mid-winter; and a maximum of temperature and vapour pressure, and a minimum of gaseous pressure in the midsummer. All the summer months are characterised by the + sign in the vapour, and by the − in the gaseous pressure; and all the winter months by the − sign in the vapour, and the + sign in the gaseous pressure.

I am unable at the present moment to pursue the comparison of the Greenwich and Toronto results in many other points in which I can perceive that the interest would prove an ample repayment for the time so employed. But I may hope to enjoy some future occasion of resuming the subject under more favourable circumstances in respect to leisure than I can at present command.

XI. *On the Law of Resistance of a Medium to small vibratory Motion; the Mixture of Prismatic Colours; and the Appearance of the Prismatic Spectrum when viewed through a plate of common blue glass of proper thickness. By the* Rev. Prof. O'Brien, *late Fellow of Caius College, Cambridge.*

[The subject resumed from vol. xxv. p. 528.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

25. BEFORE I proceed to develope more particularly the theory which I have ventured to propose, in a general way, in your Journal for November and December (Supplement) last, I wish to make a few additional remarks on the subject of Resistances, in order to show that I have assumed nothing in the preceding articles but what is warranted by the results of accurate experiments; and this is the more necessary, because the mathematical theory of motion in a resisting medium is too imperfect to be of any use, and therefore we must look to experiment alone, in the present state of science, for whatever information we require in this difficult subject. Fortunately the accurate experiments of Borda and Bessel, on the small oscillations of the pendulum in a resisting medium, are abundantly sufficient for our purpose.

26. By these experiments the following facts have been clearly established:—

1st. That when a pendulum, consisting of a ball suspended by a fine string, oscillates in a resisting medium through a small angle (for instance, an angle not exceeding 40′), the successive amplitudes of vibration are sensibly in geometric progression.

2nd. That the pendulum is deprived (so to speak) of a certain portion of its weight by the resistance of the medium, in addition to what it loses by the buoyancy of the medium*.

From these facts I shall now endeavour to show, that the resistance of the medium must be a linear function of the velocity, involving not only the velocity itself, but also the differential coefficients of the velocity with respect to the time. In other words, that the resistance is expressed by the formula

$$C_1 v + C_2 \frac{dv}{dt} + C_3 \frac{d^2 v}{dt^2} + \&c.,$$

where v is the velocity of the ball, and C_1, C_2, &c. certain constants. Poisson states, as an experimental fact, that the resistance to small motions is proportional to the velocity, but this law is manifestly insufficient to account for the loss of weight of the pendulum caused by the resistance (see Poisson, *Mécanique*, tom. i. p. 363), and therefore it cannot be the true law. I am not aware whether the law I have just stated has been proposed before or not†, but it appears to me to be in complete accordance with the results of experiment, and to be capable of proof, as the following reasoning is intended to show.

27. Let θ be the angle which the string of the ball-pendulum makes with the vertical at any time t, c the length of the pendulum, $c\,R$ the resistance tending to check the motion of the ball, $-\alpha$ the value of θ at the commencement of any oscillation, and α' the value of θ at the end of that oscillation, g the force of gravity deducting the proper allowance for the buoyancy of the medium, and $n^2 = \frac{g}{c}$.

Then we find for a small oscillation,

$$\frac{d^2\theta}{dt^2} = -n^2\theta - R, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

which gives

$$\left(\frac{d\theta}{dt}\right)^2 + n^2\theta^2 = -2\int R\,d\theta,$$

which, taken between the limits of the oscillation, gives

$$n^2(\alpha^2 - \alpha'^2) = 2\int_{-\alpha}^{\alpha'} R\,d\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

[* On this subject some remarks, by Sir John Herschel, will be found in our last Number, p. 53–54.—Edit.]

† I may observe that, as far as I can judge from a hasty perusal, the memoirs written upon the oscillations of the ball pendulum seem to confirm the truth of this law.

Now, R being very small, we have for a first approximation, $\theta = -\alpha \cos nt$; and we may use this value of θ in the second member of (2.), which gives

$$n(\alpha^2 - \alpha'^2) = 2\alpha \int_0^{\frac{\pi}{n}} \mathrm{R} \sin nt \, . \, dt;$$

but, by the first of the experimental facts above stated, we have $\alpha' = \varepsilon \alpha$, where ε is some ratio independent of α; hence we find

$$\int_0^{\frac{\pi}{n}} \mathrm{R} \sin nt \, . \, dt = \frac{1}{2} n (1 - \varepsilon^2) \, . \, \alpha. \quad . \quad . \quad . \quad (3.)$$

Now the resistance at any instant must depend upon the velocity of the ball at that instant, and most probably (or rather certainly) upon the velocities with which the ball was moving during a certain interval preceding that instant. In other words, the resistance may be supposed to be, in general, a function of the velocity and its differential coefficients with respect to the time*. We may therefore assume that R is a function of some sort of $\frac{d\theta}{dt}, \frac{d^2\theta}{dt^2}, \frac{d^3\theta}{dt^3}$, &c., which we may put in the form

$$\mathrm{R} = \mathrm{P}_m + \mathrm{P}_{m'} + \mathrm{P}_{m''} + \text{\&c.},$$

where P_m, $\mathrm{P}_{m'}$, $\mathrm{P}_{m''}$, &c. are homogeneous integral and rational functions of $\frac{d\theta}{dt}, \frac{d^2\theta}{dt^2}, \frac{d^3\theta}{dt^3}$, &c. of the dimensions m, m', m'', &c. respectively, these indices being arranged in ascending order. When the oscillations are sufficiently small, $\mathrm{P}_{m'}$, $\mathrm{P}_{m''}$, &c. will therefore be inconsiderable compared with P_m; in fact, for very small oscillations, we may put $\mathrm{R} = \mathrm{P}_m$ simply.

Supposing then that the oscillations are sufficiently small, we have $\mathrm{R} = \mathrm{P}_m$; and therefore, putting for θ its first approximate value, we find

$$\mathrm{R} = \mathrm{T} \alpha^m,$$

where T is a function of t independent of α. Hence

$$\int_0^{\frac{\pi}{n}} \mathrm{R} \sin nt \, . \, dt = \mathrm{N} \alpha^m,$$

where N is some constant independent of α.

Consequently it follows immediately from (3.) that m must be unity; in other words, P_m must be a linear function of

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* To say that the value of the resistance at any time t is a function of the velocity (v) at that time and the velocities during a certain interval previous to that time, is the same thing as to say, that the resistance is a function of v and its differential coefficients with respect to t.

$\frac{d\theta}{dt}$, $\frac{d^2\theta}{dt^2}$, $\frac{d^3\theta}{dt^3}$, &c., *i. e.* the resistance, in the case of very small oscillations, is a linear function of the velocity and its differential coefficients with respect to the time.

28. If we retain the remaining terms of R, namely $P_{m'}$, $P_{m''}$, &c., we shall find

$$\int_0^{\frac{\pi}{n}} R \sin nt\,.\,dt = N\alpha + N'\alpha^{m'} + N''\alpha^{m''} + \&c.,$$

where N′, N″, &c. are quantities analogous to N. Hence, and by (3.), we may easily show that R does not sensibly differ from P_m when the ball oscillates through any angle not exceeding 40′, and consequently that, in such a case, R may be assumed to be a linear function of the velocity and its differential coefficients.

29. We shall now briefly show, from the second of the experimental facts above stated, that the resistance depends *sensibly* upon the differential coefficients of the velocity, as well as upon the velocity itself.

Taking for granted what we have just proved, we may assume that

$$R = C_1\frac{d\theta}{dt} + C_2\frac{d^2\theta}{dt^2} + C_3\frac{d^3\theta}{dt^3} + \&c.,$$

where C_1, C_2, C_3 are constants depending solely upon the constitution of the medium and the shape of the ball. Now we have for a first approximation,

$$\frac{d^2\theta}{dt^2} = -n^2\theta,\quad \frac{d^3\theta}{dt^3} = -n^2\frac{d\theta}{dt},\quad \frac{d^4\theta}{dt^4} = n^4\theta,\ \&c.,$$

in virtue of which equations the expression for R becomes

$$R = p\frac{d\theta}{dt} - qn^2\theta,$$

where

$$p = C_1 - C_3n^2 + C_5n^4 \ldots\ldots,\qquad q = C_2 - C_4n^2 + C_6n^4 \ldots\ldots$$

Hence the equation (1.) becomes, putting $n^2 = \frac{g}{c}$,

$$\frac{d^2\theta}{dt^2} + p\frac{d\theta}{dt} + \frac{g(1-q)}{c}\theta = 0. \quad .\ .\ .\ .\ . \quad (4.)$$

If we examine the third term of this equation, we see that the coefficient of θ, instead of being $\frac{g}{c}$, is $\frac{g - qg}{c}$; in other words, the ball may be said to be deprived of the qth of its weight by the resistance of the medium. Now if R depended simply upon the velocity, and not upon its differential coefficients, q would manifestly be zero, and there would be no loss of weight. Hence, since experiment clearly indicates a loss of weight, we may certainly conclude that the resistance depends sensibly upon the differential coefficients of the velocity, as well as upon the velocity itself.

30. If we integrate (4.), we shall find

$$\theta = -\alpha e^{-m' t} \cos n' t,$$

where $$m' = \frac{p}{2},$$

and $$n'^2 = \frac{g(1-q)}{c} + \frac{p^2}{4}.$$

Experiment shows that $\frac{p^2}{4}$ is so small that it may be altogether neglected in the expressions for n'^2; but the part of n'^2 depending upon q is by no means inconsiderable, being in some cases as much as half of the whole correction due to the buoyancy of the medium. We see then that the time of oscillation is only affected by that part of the resistance which depends upon the differential coefficients of the velocity; and this proves that the resistance must sensibly depend upon the differential coefficients of the velocity, since the time of oscillation is found to be sensibly altered by the resistance.

31. The foregoing remarks are abundantly sufficient, I think, to warrant the assumption we have been led to in article 19, namely, that the resistance brought into play upon the vibrating æther is a linear function of the velocity and its differential coefficients; and this is the only assumption that has been made in the preceding papers respecting the law of resistance.

On the Mixture of Prismatic Colours.

32. Before returning to the subject of the dispersion and absorption of light, I shall say a few words upon the well-known fact (commonly explained on the hypothesis of "three primary colours"), namely, that two or more simple prismatic colours, when mixed together, may produce a simple prismatic colour. The explanation of this fact, though having no immediate reference to the subject of these papers, will be found useful presently. We shall be very brief, and confine our attention at present to one very simple case, viz. the mixture of blue and yellow in equal intensity.

We may represent the undulations constituting light of any particular colour by the formula $a \cos 2\pi(nt - kx)$, or more simply (putting $x = 0$, as we may do in the present case), by the formula $a \cos 2\pi n t$. The intensity is measured by a^2, and the colour is determined by n, n being here the number of undulations in one second.

Let us suppose that two colours, represented by $a \cos 2\pi n t$

and $a\cos 2\pi n' t$, are mixed together, then the resulting light will be represented by $a\cos 2\pi n t + a\cos 2\pi n' t$, which is equal to

$$\left\{2\,a\cos\frac{n'-n}{2}\,t\right\}\cos\frac{n'+n}{2}\,t. \quad . \quad . \quad . \quad . \quad (1.)$$

Suppose now that the two component colours are the mean yellow and blue of the spectrum, then

$$n = 535{,}000000{,}000000,$$
$$n' = 622{,}000000{,}000000;$$

therefore
$$\frac{n'+n}{2} = 578{,}500000{,}000000,$$
$$\frac{n'-n}{2} = 43{,}500000{,}000000.$$

The numerical value of $\frac{n'+n}{2}$ here obtained is as nearly as possible equal to the number of undulations in a second which constitutes the mean green light of the spectrum; and the value of $\frac{n'-n}{2}$ is about the tenth part of the number of undulations in a second which constitutes the extreme red.

Hence the formula (1.) represents a light whose colour is green, but whose intensity is variable (for we may regard the whole factor in brackets as the square root of the intensity), the variation being periodical, and going through all its values 43,500000,000000 times in one second.

33. Now it is an experimental fact that, when the intensity of light suffers a regular periodical change of any kind, the eye does not perceive that change, if it take place with sufficient rapidity; the eye being perfectly insensible to rapid and regular variations in the intensity of light, on account of the duration of impressions on the retina. We must, of course, except the case where the periodic time of the variation is equal to that of the undulations which constitute any colour of the spectrum; for, in such a case, we have every reason to suppose that the variation would produce the sensation of colour. [In the analogous case of sound, it is well known that if any sound be repeated regularly, at intervals equal to the period of vibration of any particular musical note, the sensation of that note will be produced.] It is likely, also, that if the number of oscillations (so to speak) in a second of the intensity of the light be not much less than 458,000000,000000 (the number of undulations for extreme red), or not much greater than 727,000000,000000 (the number of undulations for extreme violet), then, though the variation will not produce a distinct sensation of colour, it will not be wholly un-

perceived, but will affect the eye in some peculiar manner, which may possibly give the light (supposing it to be a pure colour of the spectrum) an appearance different from that of any of the prismatic colours.

Hence I think it is clear that the light represented by the formula (1.) will appear to be a pure prismatic green, inasmuch as the intensity goes through all its values 43,500000,000000 in one second, which number is considerably less than 458,000000,000000, being about the tenth part of it.

34. If, instead of yellow and blue, we mix together a red and violet, we shall find

$$\frac{n'-n}{2} = 134{,}500000{,}000000,$$

which is between the 3rd and 4th part of 458,000000,000000. We also find

$$\frac{n'+n}{2} = 592{,}500000{,}000000,$$

which is the number of undulations corresponding to the greenish-blue part of the spectrum. In this case the value of $\frac{n'-n}{2}$ being four times what it was in the former case, the compound colour will probably have a peculiar cast.

35. It may be easily shown that if the two colours be mixed in unequal intensities, similar consequences will follow, only the tint of the compound light will be nearer to that of the strongest component; but the consideration of this, and of the mixture of more than two colours, we shall reserve for a future communication. I shall only observe here, with reference to the experimental test of the truth of this explanation, that the colours in certain parts of the purest spectrum are probably mixed colours, and the above explanation applies only to two perfectly pure and simple colours.

On the Effect of Blue Glass upon the Prismatic Spectrum.

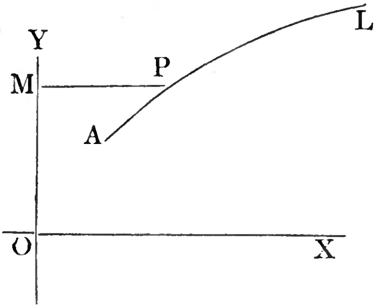
36. When the prismatic spectrum is viewed through a plate of common blue glass of proper thickness, most curious and perplexing phænomena present themselves, as is well known (see the article "Light" in the *Encyclopædia Metropolitana*). From these phænomena it appears, that two decidedly different colours (yellow and red) have almost the same degree of refrangibility, and that two colours apparently identical (two reds) have decidedly different degrees of refrangibility. We shall now attempt to account for these facts.

We have shown, in article 21, the connection between the length of the wave and the colour, and it follows from what is there proved (as we shall show more particularly hereafter) that if μ be the index of refraction, say for the ordinary glass composing a prism, and $\frac{2\pi}{n}$ the time of vibration, we have for μ^2 an expression of the form

$$\mu^2 = A_1 - A_2 n^2 + A_4 n^4. \quad . \quad . \quad . \quad . \quad (1.)$$

Now suppose that we construct the curve whose abscissa is n and ordinate μ, by means of this equation; let OX, OY (fig. 1.) be the axes, and A L the curve. If we wish to find the colour corresponding to any particular refractive index, we have only to measure O M equal to that refractive index, and draw M P parallel to O X; then M P is the corresponding value of n, and determines the colour required.

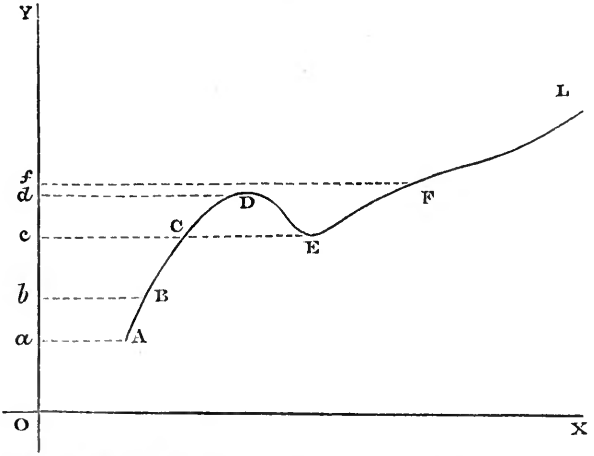
Fig. 1.



37. Now I think the appearances, which it is our object to explain, are most probably caused by the peculiar form of the curve A L, and the peculiar degree of absorption exercised by the blue glass corresponding to different values of n (for we have shown (art. 21.) that the index of absorption, like the index of refraction, is a function of n).

Suppose that the curve A L is of the shape represented in fig. 2; let A be the point corresponding to the extreme red,

Fig. 2.



E the commencement of the orange, F the commencement of the yellow, B, C, D all in the red, the tint from A to D being decidedly red, and from D to E verging towards orange. Draw the lines A *a*, B *b*, E C *c*, D *d*, F *f* parallel to O X. Then the lines O *a*, O *b*, O *c*, O *d*, O *f* will represent the indices of refraction corresponding to the different values of *n* represented by the lines drawn from *a*, *b*, *c*, *d*, *f* parallel to O X to meet the curve. Hence the spaces *a b*, *b c*, *c d*, *d f* will represent the different portions of the prismatic spectrum from the beginning of the red to the beginning of the yellow. From *a* to *c* will be decided red; from *c* to *d* will be a mixture of red, reddish-orange and orange, and will appear to be red gradually changing to orange; beyond *d* we shall have yellow, and so on.

38. Now suppose that the absorbing power of the blue glass, through which the spectrum is supposed to be viewed, is such that all the light from B to C, from D to E, and from E to F is completely destroyed, then the appearance of the spectrum will be evidently as follows: from *a* to *b* will be red; from *b* to *c* will be a black space of some magnitude; from *c* to *d* will be red of the same tint as at *b*, since the reddish-orange and the orange, which before were present from *c* to *d*, are now obliterated; from *d* to *f* will be a small black space; and from *f* to some distance above *f* will be pure yellow; and so on.

Here then we have two reds of very nearly the same tint, *a b* and *c d*, separated from each other by a broad dark space, *b c*; and the red, *c d*, separated from the yellow at *f* by the narrow dark space *d f*. These are exactly the appearances which present themselves when the prismatic spectrum is viewed through a piece of common blue glass of sufficient thickness.

39. I do not, however, venture to say that the curve A L is precisely of the form here supposed, but only that the appearances we have been attempting to explain are possibly caused by some such peculiarity in the form of the curve, which produces a mixture of colours in some parts of the spectrum. I may observe that the portion *c d* of the spectrum contains none of the principal fixed lines, and, if I may judge from plates of the spectrum (for I have not at present apparatus sufficiently good to exhibit the spectrum with sufficient purity), none but very faint lines occur in this space. I may also remark that the intensity of the light in this space is greater than at any other part of the spectrum. Both these circumstances, as far as they go, confirm the above explanation.

If the spectrum produced by means of a grating before the object-glass of a telescope were analysed by absorption (which might be done by putting a piece of blue glass before the object-glass), I fancy that some insight into this subject might be gained. I can show, I think, that exactly the same mixture of colours may possibly take place in the spectrum produced by diffraction, as in the common prismatic spectrum.

Balham, Surrey, December 27, 1844. M. O'BRIEN.

[To be continued.]

XII. *On the Composition of some varieties of South American Guano; with the description of a new mode of estimating Ammonia, and of a process for separating Lime from Magnesia, when these earths exist in combination with Phosphoric Acid.* *By* J. DENHAM SMITH, *Esq.**

THE results of the following examinations of guano, necessary as a matter of business to myself, may prove of some interest to the Society, as they serve to confirm, in most points, those arrived at by previous analysts, especially with respect to the variableness of the composition of this substance. When I commenced these analyses I had but one sample of guano in my possession, which came through the hands of a broker in 1842, of the purity of which I could not be certain before analysis. Subsequently I was successful in obtaining genuine samples of South American guano direct from the warehouses of the importers, the bulks from which these specimens were taken being at the present time in the course of consumption in this country.

South American guano, as imported, presents itself in three distinct states, although these varieties are often mixed together in the same bag. The larger proportion of it consists of a damp pulverulent substance of various tints of brown, intermixed with nodules, usually of a lighter colour than the powder; some of these nodules are soft, easily crushed by the finger, and often present light brown silky crystals when broken; others are much harder, of a dirty white and uniform texture throughout.

The second variety exists as large concretions, often weighing several pounds, presenting various aspects when broken; sometimes appearing merely to be the first variety in a coherent, instead of a pulverulent state; at other times as a regularly stratified deposit of different shades.

The third description of guano occurs in irregularly shaped masses, very heavy in comparison with the other varieties, and

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* Communicated by the Chemical Society; having been read May 6, 1844. On the composition and chemical history of guano, see also Phil. Mag. S. 3. vol. xxi. p. 385, and vol. xxiv. p. 317, 395, 470.

termed "stones" by the labourers; when broken, this variety presents a crystalline fracture, resembling a fused salt of a brown colour; it appears to exist in large quantities in the guano when imported, for at the warehouses whence I obtained my samples, I was told that at least a hundred tons of it had been picked from the guano they had hitherto sent out for consumption. For the sake of distinguishing this variety, I shall term it "Saline Guano," whilst the second kind I shall call "Concrete Guano."

After a preliminary qualitative examination of these varieties of this manure, the utility of another and probably more exact mode of estimating ammonia than by condensing the gas in hydrochloric acid and evaporating the solution to dryness, and a more œconomical and less troublesome plan than by a salt of platina, became obvious. I therefore had recourse to an indirect method of analysis, that of estimating the ammonia from the quantity of carbonate of barytes obtained. I preferred a salt of barytes to any other substance forming a true neutral carbonate, on account of the high atomic number of barium, and its consequent capability of expressing with exactness the quantity of ammonia, as well as the great delicacy of a salt of barytes in evidencing the existence of carbonate of ammonia, for which purpose this substance is superior to a salt of lime, or even to lime water.

The point which appeared most likely to occasion error was, that the carbonate of ammonia might not be obtained as a true neutral carbonate; as a basic carbonate would of course vitiate the results, and the frequency of the occurrence of such combinations we all know since the publication of Rose's excellent paper on this subject. Experiment showed that the neutral carbonates of potash and soda, distilled with a solution of an ammoniacal salt, afforded a basic carbonate of ammonia, the solution being alkaline after a salt of barytes had been added until no further precipitate fell, and that a current of carbonic acid gas produced a precipitate in such a solution when filtered. The acid carbonates of soda and potash answered well enough where the ammonia existed in small quantities, but when a large proportion of the volatile alkali was present in the liquor subjected to distillation, the distilled solution evidently contained a basic carbonate, as when the ordinary carbonates of soda and potash were used. It now remained to be proved whether the acid carbonates of ammonia would afford correct results. To determine this point, 59 grs. of sesquicarbonate of ammonia, taken from the centre of a lump, were dissolved in cold water and precipitated by excess of nitrate of barytes, the solution being exposed to the air in an open dish for twenty-four hours: this precipitate, collected, washed and dried, weighed 98·07 grs., indicating an excess of ·012 in estimating ammonia, merely an error of experiment to this trifling amount. The solution when gently heated evolved a little carbonic acid, but did not deposit any carbonate of barytes. Satisfied by this experiment that the acid, as well as the neutral carbonates of ammonia, precipitate a barytic salt equiatomically as respects the bases, and therefore that the proposed plan of indirect estimation of ammonia was an accurate one, I subjected 54 grs. of muriate of ammonia to distillation with an excess of a solution of the ordinary carbonate of soda, carrying on the distillation until about one-half of the liquid had passed into the receiver, in which distilled water was of course placed; long before this quantity of liquid was distilled a slip of turmeric paper suspended from the tubulure of the retort ceased to be reddened; the contents of the receiver, a solution of a basic carbonate of ammonia, were added to a solution of nitrate of barytes, and then subjected to a stream of carbonic acid gas until the precipitate became arenaceous and the solution slightly affected litmus paper; this exposed to the air for a day, and the precipitate collected, and dried, gave 97·63 grs. of carbonate of barytes. As in the former case, the filtered solution when gently heated gave off a little carbonic acid gas, but deposited nothing. These results being so favourable, and the process simple, the trifling deficiency evidently being traceable to defective manipulation alone, I have adopted this process for estimating ammonia in the analyses subsequently detailed, considering it susceptible of greater accuracy than the hydrochloric acid method, and as accurate as, and far less troublesome and expensive than, the employment of salts of platinum, and of alcohol for this purpose.

The process for separating the phosphate of lime from the phosphate of magnesia depends upon the insolubility of the oxalate of lime in very dilute acetic acid, and the solubility of the magnesian salt in that menstruum. Thus, when a solution of these mixed phosphates in hydrochloric acid is treated with ammonia slightly in excess to precipitate the characteristic gelatinous precipitate, and acetic acid is gradually added till such precipitate is redissolved, the solution being acid to litmus paper, but only faintly so to the taste, oxalate of ammonia produces the characteristic precipitate of oxalate of lime, which base is completely thrown down; the filtered solution, when evaporated and treated with a little phosphate of soda and ammonia, affording the ammoniacal magnesian phosphate, which contains no lime. I do not give this process as one which is necessarily rigidly accurate, for if the acetic acid be in too great excess, more than completely sufficient to redissolve the precipitate produced by ammonia, the clear solution, although containing no magnesia, will at times, as I have found, give indications of the existence of lime on supersaturation with ammonia; but the quantity under such circumstances held in solution is too small to be of importance except in cases where a scientific point is involved. Where oxalate of lime is in solution together with the magnesian and calcareous phosphates, this process requires some modification, which it may be better to explain in the details of the mode I have adopted in the analysis of guano, than at present—that recapitulation may be as much as is possible avoided.

It may be advantageous here to describe the mode of analysis pursued in the examination of these samples of guano, as each analysis, in fact, consists of three separate ones, viz. of those portions soluble in cold water; the substances which dissolved on boiling the residue insoluble in cold water, in abundance of water; and the portion which remained insoluble in either menstruum. This division I found to be a very convenient one, as the solutions obtained were far less complex than if the portions of the guano soluble in water cold and hot, and in acids, had been mixed together; and it of course gives a pretty correct idea of the salts as they actually are presented to the roots of plants manured with guano. My reasons for combining the bases and acids as I have done in the results of these analyses are, that in some cases the proportional quantities of acid and base have tallied, and also that I have obtained by crystallizing the hot and cold solutions, oxalate of ammonia, oxalate of soda, muriate of ammonia, chlorides of potassium and of sodium, sulphates of soda and potash, urate of ammonia and free uric acid, from the various samples of guano; and also that by microscopic examination crystals of the chlorides and of sulphate of soda have been readily observed in the mass of the guano. As I have also given the amount of bases and acids obtained without reference to the state of combination in which they exist, those who differ from me in this respect, can combine them as they think proper.

The mode adopted for determining the respective amounts of the fixed alkaline bases is open to many objections. It is complex; a very slight error in weighing is liable to considerably vitiate the result: if the whole numbers I have adopted as representing the combining equivalents of the various acids and bases are incorrect, so also must the results be. On the other hand, I have tried this method upon an actual mixture of soda and potash salts, and the results approximated very closely to the truth; and upon the whole, weighing the advantages of this indirect system of Griffin's with the positive modes of estimating the potash, either by perchloric acid, tartaric acid or platina salt, I believe this indirect mode to afford, in careful hands, at least as accurate returns, if not more so, than the direct modes of estimating the potash, and ascertaining the soda from the loss. I may be wrong in attributing this process to Mr. Griffin, as Barreswil and Sobrero assign it to M. Mayer, with a date anterior to Griffin's description in the Philosophical Magazine.

With respect to the neutral phosphate of lime stated to exist in the sample of "Concrete Guano, No. 6," and also in No. 2, I may state that it precipitated as neutral phosphate; and if I had estimated it as bone-phosphate, the sum of the analysis would have fallen very far short of the quantity employed; whilst by estimating it as I have done, the loss was most trivial, amounting only to a common error of analysis. Of the solubility of the phosphate of lime, and of the ammoniacal magnesian phosphate in boiling water, it is simply necessary to refer to the already established solubility of these salts in neutral solutions containing organic animal matter, and also to state that these solutions were invariably faintly acid to litmus paper.

I was led to suspect the existence of a volatile acid in guano, but I in vain tried to isolate it, if indeed it exists, and the acid odour and reaction are not truly to be attributed to the reaction of the sulphuric or phosphoric acids, the agents made use of in the attempt, upon the organic portions of the guano.

The quantity submitted to analysis was in each case either 300 or 500 grs., and the results were calculated to 1000 parts for the convenience of ascertaining the per-centage, and for greater readiness of comparison.

The water was determined approximately, by drying till the portion lost no more weight, without any sublimation taking place. Water and ammonia were volatilized. This estimation of the water was corrected by subtracting the sum of the weight of the soluble salts, and portions insoluble in cold water, ascertained during the subsequent process, from the original weight of guano employed. A given weight of the sample of guano to be examined was ground up with cold distilled water; when the insoluble portions had subsided, the solution was passed through a filter and the residue washed with two or three successive portions of distilled water; the insoluble residue was then placed on the filter, and treated with distilled water until salts of lime and barytes, being added to a few drops of the solution, gave no precipitate, or but a faint trace of one. The insoluble residue was dried at 212°, its weight was noted, and the aqueous solution, which usually amounted to about a pint and a half, was accurately measured, and its quantity made up to exactly 32 or 40 fluid ozs. This solution, when freshly prepared, was generally neutral or slightly acid; in no one instance have I found it alkaline, but it became so when kept in a stopped bottle for some days.

One-sixth or one-eighth of this solution acidified with nitric acid and treated with nitrate of silver gave the amount of chlorine—hydrochloric acid. The same quantity similarly treated, but with barytes instead of silver, indicated the quantity of sulphuric acid.

Another portion, slightly acidified with acetic acid (a few drops are sufficient), and treated with hydrochlorate of lime, gave, if any oxalic acid were present, the amount of that acid, the precipitate of course being treated in the usual way; to this filtered solution, having previously boiled it and taken care that an excess of lime salt was present, was added excess of ammonia, which precipitated the phosphoric acid as bone-phosphate; this result was corrected by the weight of phosphate of silver obtained during a subsequent operation.

The quantity of ammonia was determined by distilling one-fourth or one-sixth of the solution in the mode described in a foregoing part of this paper.

If potash and soda were both found to exist in the solution by previous experiment, the following operation was carried completely out; if one only of these alkalies were present, the process was discontinued when the weight of the alkaline sulphate had been determined. One-fourth at least of the solution was gently evaporated, by which it became markedly acid; this evaporation was carried to dryness till no more water was given off, taking care to avoid volatilization of any salt of ammonia, although uncombined ammonia had been liberated during the evaporation; when quite dry it was confined for a few hours over a mixture of powdered lime and sal-ammoniac, then the vessel containing the residue of the evaporated solution was gently warmed and weighed; this afforded the quantity of solid matter contained in the cold aqueous solution, together with traces of water combined with salts and organic matter. This residue was then ignited, the organic matter and ammoniacal salts were expelled, and the loss, deducting from it the weight of the salts of ammonia, was estimated as organic matter with a little water; the fused residue was dissolved in water and precipitated by nitrate of silver; this precipitate, when collected, fused and weighed, and the fused salt of silver treated with nitric acid and its loss of weight noted, expressed the quantities of phosphoric acid and chlorine contained in the fused saline residue. To the original solution hydrochloric acid in slight excess was added; the chloride of silver thus formed was separated, and the clear solution was evaporated with so much diluted sulphuric acid, as would have been sufficient to convert the whole of the fused saline residue into sulphate of soda, had it consisted entirely of chloride of sodium; this evaporation being very carefully conducted and the residue transferred to a platinum crucible, in which it was heated to full redness until no more acid fumes were given off, the crucible with its contents was accurately weighed, and the weight noted; if one alkali only was present, its weight was calculated from that of the sulphate; but if both potash and soda were contained in the specimen of guano under examination, the mixed sulphates thus obtained were dissolved in water, the solution was treated with a salt of barytes in the usual way, and the resulting sulphate of barytes was weighed with the most scrupulous care. The respective weight of each alkaline sulphate contained in the solution was then calculated by the following formula, which I subjoin, as the steps in the calculation are much more readily perceived in figures than expressed in words:—

$KOSO_3$ $BaOSO_3$ $KOSO_3$ $BaOSO_3$ $NaOSO_3$ $BaOSO_3$ $NaOSO_3$ $BaOSO_3$

88 : 116 : : 1 : 1·31818 . 72 : 116 : : 1 : 1·61111 −1·31818 = 0·29293 the difference. Let the weight of the mixed sulphate be 48·54 grs., then 1·31818 × 48·54 = 63·984 −70·525, weight of sulphate of barytes obtained by experiment = 6·541 ÷ 0·29293 = 22·33 sulphate of soda − 48·54 = 26·21 sulphate of potash, and from these weights the respective proportions of the bases may be calculated*.

The actual amount of the chlorine or hydrochloric acid, the sulphuric, phosphoric and oxalic acid, of the ammonia, potash, soda, organic matter, &c., contained in the cold aqueous solution, were ascertained by calculation, as well as that of the water.

The residue insoluble in cold water was then ground to powder, and a known weight of it was boiled with three or four successive half-pints of distilled water, for about an hour after each addition of water; the mixture was then allowed to deposit the undissolved portions, the solution being kept hot during this subsidence and filtered whilst still at that temperature. When water boiled upon this residue came off colourless, and

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* See Phil. Mag. vol. xiii. pp. 132 *et seq.* and Barreswil and Sobrero's *Appendice aux Traités d'Analyse Chimique*, p. 41.

contained only minute traces of soluble matter, the insoluble portion was collected on a filter, dried at 212° and its weight noted. The mixed solutions being gently evaporated to dryness, the product was weighed and treated with cold water, the weight of the matter insoluble in the cold water was noted, and the substance reserved; the solution obtained by thus treating it, being again evaporated to dryness, was weighed, ignited, and the loss of weight noted, indicating the organic matter, with traces of water and ammonia; the fused residue was treated with water, the filtered solution precipitated with nitrate of silver, and the weight of the phosphate of silver was ascertained; this was assumed to be due to the presence of phosphate of soda, but the quantity was always too small to ascertain whether this alkali or potash was present; the portion insoluble in water was then dissolved in a little hydrochloric acid and treated in the manner before described, to estimate the proportion of phosphate of lime and phosphate of magnesia.

The reserved portion, which was insoluble in the cold water, was then treated to ascertain the existence and proportion of free uric acid. It was boiled in successive small portions of water, four or five fluid ounces each time, until the solution ceased to become turbid on cooling; these solutions were mixed together, boiled, and allowed to cool very gradually: if free uric acid were present a few crystalline grains of this substance would be deposited, which, when weighed, were ascertained to be uric acid by means of potash and nitric acid. The portion left undissolved by these small portions of boiling water was then treated with a few drops of very dilute potash, the absence of ammonia was proved by test-paper, and the solution precipitated by sulphuric acid, which threw down the uric acid as a voluminous precipitate, which by repose was converted into crystalline grains; this was weighed, added to the weight of uric acid previously obtained, and the loss was estimated as organic matter: the solution of urate of ammonia was evaporated, its weight ascertained, then ignited, and the ashes, if necessary, analysed for lime and magnesia, in the manner before described.

The third division of the analysis was commenced by ascertaining, by means of potash, whether any uric acid or urate of ammonia was present in the residue left undissolved by the repeated boiling with water. If these substances were absent, care being taken at the same time to note whether the residue gave off any ammonia when thus treated, and the presence or absence of ammonio-phosphate of magnesia being thus ascertained, (supposing no urate of ammonia nor uric acid existed in the residue) a given weight of it was boiled with diluted muriatic acid and filtered; in no one instance did any evolution of carbonic acid take place; the solution was then treated with ammonia in excess, which precipitated the oxalate and subphosphate of lime, perhaps the magnesia also; acetic acid was then rapidly added until it was distinctly acid to litmus paper, and a slight acidity could be detected by the taste; the insoluble precipitate being collected, thoroughly washed, dried, and ignited with the usual precaution, indicated the amount of the oxalate of lime; this precipitate was invariably examined for phosphate of lime; if it contained any, this was separated in the state of bone-phosphate and estimated as neutral phosphate, in which state of combination it existed mixed with the oxalate of lime.

To the acetic acid solution excess of oxalate of ammonia was added, this precipitated the lime present in the solution originally as bone-phosphate, sometimes mixed with neutral phosphate of lime, and from the weight of the resulting carbonate of lime, and the loss of weight sustained by ignition of another portion, correcting each other, the amount of the phosphate of lime was calculated. The filtered solution evaporated until a slight pellicle began to form, was treated with ammonia in excess, and a little phosphate of soda, observing the usual precautions recommended by Berzelius, and the resulting ammoniacal phosphate of magnesia was collected and weighed. The portion insoluble in hydrochloric acid being treated with potash gave a solution possessing the properties described to belong to an alkaline solution of the substance termed humus or humic acid; this was precipitated, collected and weighed; the residue insoluble in potash was weighed and ignited, the loss of weight noted, and the product treated with hydrochloric acid; if effervescence ensued on this addition from the presence of carbonate of lime, this was estimated as oxalate, and was added to the amount already obtained of that salt; by these means the proportion of the organic matter insoluble in potash, the humus, oxalate of lime (if any), sand, &c. &c. undissolved by hydrochloric acid, were obtained.

The proportions in which these substances existed in the guano were then ascertained by the necessary series of calculation.

Where the proportions of acid and base, as ascertained by analysis, did not exactly agree, the estimate derived from the quantity of the acid was preferred, as being far less subject to error than the process for estimating the respective amounts of the potash and soda; the requisite loss or gain was always placed to the soda, and this will account for the differences of the losses sustained between the constituents of these samples of guano when combined, and the table, which shows the constituents according to the results of analysis.

I cannot deny that this mode of analysis is extremely tedious and complex, and I fear that this complexity, combined with a wish to avoid all unnecessary prolixity of description, has rendered its details in many places obscure. I could wish to have described a simpler and shorter mode; but in a product consisting of some fifteen to twenty substances in various states of combination, a simple mode and brevity of description of analysis are I fear impossible. For ordinary purposes of estimating the comparative value of a sample of guano, this mode of analysis is both unfitted and unnecessary, the estimation of the amount of uric acid, of ammonia, of phosphate of lime and magnesia, with the sand, &c., being quite sufficient for such purposes, and a more detailed analysis would, in the majority of cases, be both time and trouble thrown away.

Composition of a sample of South American Guano in my possession since 1842.—This guano was of the colour of Lundyfoot snuff, with dirty white lumps intermixed, varying in size from that of small shot to that of marbles. The quantity submitted to analysis was 500 grs.

No. 1.

	Calculated Composition.	
Soluble in cold water.	Moisture with combined water	222·00
	Muriate of ammonia	25·5
	Sulphate of potash, trace of sulph. soda	80·0
	Oxalate of ammonia	74·0
	Phosphate of ammonia	63·3
	Organic matter	15·0
Soluble in boiling water.	Urate of ammonia	154·18
	Uric acid	25·16
	Animal matter, with water and trace of ammonia	11·80
	Phosphate of magnesia and ammonia	5·64
	Phosphate of soda?	1·20
	Phosphate of lime	1·86
Insoluble in water.	Oxalate of lime	25·60
	Phosphate of lime	197·50
	Phosphate of magnesia	20·30
	Sand, &c.	15·60
	Humus	26·36
	Other organic matter with combined water	34·56
	Loss	0·44
		1000·00

	Constituents.	
Soluble in cold water.	Moisture and combined water	222·00
	Hydrochloric acid	17·50
	Sulphuric acid	36·40
	Oxalic acid	50·30
	Phosphoric acid	43·00
	Potash	43·71
	Ammonia	52·05
	Organic matter	15·00
Soluble in boiling water.	Uric acid	165·23
	Ammonia	14·77
	Phosphoric acid	4·19
	Lime	1·00
	Magnesia	0·77
	Soda?	0·57
	Animal matter, water	13·31
Insoluble in water.	Oxalic acid	14·40
	Phosphoric acid	102·03
	Lime	118·72
	Magnesia	7·25
	Sand, &c.	15·60
	Humus	26·36
	Water, with organic matter	34·56
	Loss	1·28
		1000·00

The next sample very much resembled the guano just described, in its physical character; some of the small lumps were of a dirty white colour, and when broken showed no appearance of crystalline structure; these were chiefly composed of phosphate of lime; others showed brown silky crystals in the interior, which, examined under the microscope, appeared to be sulphate of soda and oxalate of ammonia. This, as well as all the succeeding varieties of guano mentioned, was obtained in January last; this specimen was considered by the importers to be of a very superior quality. The quantity analysed was 500 grs.

No. 2.

	Calculated Composition.	
Soluble in cold water.	Water	215·10
	Organic matter with combined water	61·74
	Sulphate of soda	37·90
	Phosphate of soda	35·82
	Phosphate of lime	12·56
	Phosphate of ammonia with some biphosphate	30·06
	Phosphate of potash	20·02
	Muriate of ammonia	35·22
	Oxalate of ammonia	100·38
Soluble in boiling water.	Urate of ammonia	25·12
	Phosphate of ammonia and magnesia	4·04
	Phosphate of soda?	1·28
	Phosphate of lime	2·88
	Animal matter	6·38
Insoluble in water.	Oxalate of lime	107·26
	Phosphate of lime	192·00
	Phosphate of magnesia, trace of ammonio-phosphate	19·84
	Humus	20·60
	Organic matter	11·40
	Water	42·42
	Sand, &c	16·48
	Loss	1·50
		1000·00

	Constituents.	
Soluble in cold water.	Water	215·10
	Organic matter with combined water	61·74
	Sulphuric acid	21·06
	Phosphoric acid	55·02
	Oxalic acid	68·18
	Hydrochloric acid	24·14
	Lime	6·56
	Ammonia	51·68
	Potash	11·44
	Soda	33·70
Soluble in boiling water.	Phosphoric acid	4·30
	Magnesia	0·56
	Ammonia	2·66
	Soda?	0·60
	Uric acid	23·08
	Lime	1·26
	Animal matter and water	7·40
Insoluble in water.	Oxalic acid	60·32
	Phosphoric acid	103·96
	Lime	145·74
	Magnesia	7·08
	Humus	20·60
	Organic matter	11·40
	Water	42·42
	Sand	16·48
	Loss, &c	3·52
		1000·00

Two other samples of a similar character, but much darker in colour, were qualitatively examined: one, No. 3, contained substances similar to No. 2, both soda and potash, some urate of ammonia and very little sand and dirt; the other, No. 4, was almost destitute of urate of ammonia or uric acid, the faintest trace being discoverable by nitric acid and ammonia, but in too small a proportion to enable me to isolate a particle of the acid even when acting on 3 to 500 grs. This guano also contained 17 per cent. of sand, &c., consisting apparently of sea-sand mixed with little lumps of granite, and a very dense black sand, resembling Iserine or Menachanite.

The first lump of the "Concrete Guano," No. 5, was about the size of a human skull, soft enough to be readily shred with a knife, and uniform throughout, presenting the appearance of a dark brown mass containing a vast number of minute dirty white granulations. Its scent was very slight, and neither this nor any of the other samples had an ammoniacal odour, but merely the peculiar smell possessed by South American guano. When shred and thoroughly mixed the powder was damp, and almost as dark-coloured as Nos. 3 and 4; very different in this respect from the samples 1 and 2, the analyses of which have already been detailed; 500 grs. was the quantity analysed.

Concrete Guano. No. 5.

	Calculated Composition.	
Soluble in cold water.	Water	204·2
	Oxalate of ammonia	93·9
	Phosphate of ammonia	61·24
	Phosphate of potash	77·32
	Sulphate of soda	259·44
	Chloride of sodium	29·22
	Organic matter	6·68
Sol. in boil. wat.	Phosphate of magnes. and ammonia, and trace of phosp. soda	7·84
	Organic matter	8·60
Insoluble in water.	Oxalate of lime	109·58
	Phosphate of lime	62·70
	Phosphate of magnesia	8·74
	Humus	8·62
	Sand, &c.	7·20
	Water	49·74
	Loss, &c.	4·98
		1000·00

	Constituents.	
Soluble in cold water.	Water	204·2
	Oxalic acid	63·78
	Phosphoric acid	79·50
	Sulphuric acid	144·14
	Ammonia	50·00
	Potash	38·42
	Sodium	11·09
	Soda	115·30
	Chlorine	18·13
	Organic matter	6·68
Soluble in boil. water.	Magnesia	1·08
	Ammonia	0·81
	Phosphoric acid	3·89
	Water and animal matter	10·66
Insoluble in water.	Lime	81·78
	Oxalic acid	61·64
	Magnesia	3·12
	Phosphoric acid	34·48
	Sand, &c.	7·20
	Water	49·74
	Humus	8·62
	Loss	5·74
		1000·00

The second sample of "Concrete Guano," No. 6, was much harder than the former, and appeared to have been deposited in strata; the colour was lighter than any of the preceding samples and the powder drier; 300 grs. were analysed.

No. 6.

Calculated Composition.

Soluble in cold water.	Water	106·66
	Muriate of ammonia	4·43
	Chloride of sodium	9·50
	Phosphate of potash	14·94
	Sulphate of soda	12·23
	Organic matter	2·40
	Oxalate of ammonia	a trace
Sol. in boil. wat.	Organic matter	10·00
	Phosphate of lime, trace of magnesia	11·37
	Phosphate of soda	a trace
Insoluble in water.	Sand, &c.	20·43
	Humus, organic matter, &c.	29·73
	Phosphate of lime	664·47
	Phosphate of magnesia	30·56
	Water	80·60
	Loss	2·68
		1000·00

Constituents.

Soluble in cold water.	Water	106·66
	Chlorine	8·66
	Phosphoric acid	6·40
	Sulphuric acid	6·80
	Organic matter	2·40
	Ammonia	1·40
	Potash and soda not separated	19·07
Sol. in bo. wa.	Organic matter	10·00
	Phosphoric acid	5·25
	Lime	6·12
Insoluble in water.	Organic matter, humus, &c.	29·73
	Sand, &c.	20·43
	Phosphoric acid	385·60
	Lime	298·53
	Magnesia	10·90
	Water	80·60
	Loss	1·45
		1000·00

Having previously described the characteristics of "Saline Guano," No. 7, it is needless to recapitulate them here: it is deliquescent, and the solution yielded crystals of oxalate and sulphate of soda, and the chloride of that base. 300 grs. afforded on analysis—

Saline Guano. No. 7.

Calculated Composition.

Soluble in cold water.	Water	77·00
	Sulphate of soda	191·77
	Oxalate of soda	105·63
	Phosphate of soda	3·60
	Phosphate of potash	49·47
	Chloride of potassium	41·63
	Chloride of sodium	286·31
	Muriate of ammonia	30·30
	Organic matter with combined water	25·53
Soluble in boil. water.	Phosphate of lime	1·10
	Phosphate of ammonia and magnesia	1·33
	Animal matter and water	7·56
Insoluble in water.	Sand, &c.	4·20
	Oxide of iron and alumina	1·50
	Phosphate of magnesia	25·80
	Phosphate of lime	131·13
	Humus, organic matter, water, &c.	18·36
		1002·22

Constituents.

Soluble in cold water.	Water	77·00
	Sulphuric acid	106·54
	Oxalic acid	55·63
	Phosphoric acid	23·10
	Hydrochloric acid	20·76
	Chlorine	192·22
	Soda	136·93
	Sodium	114·52
	Potash	28·27
	Potassium	21·91
	Ammonia	9·54
	Organic matter and combined water	25·53
Soluble in boiling water.	Lime	0·60
	Magnesia	0·18
	Phosphoric acid	0·66
	Organic matter and water	7·90
	Ammonia	0·15
Insoluble in water.	Sand, &c.	4·20
	Oxide of iron and alumina	1·50
	Lime	71·63
	Magnesia	9·21
	Phosphoric acid	76·09
	Organic matter, humus, &c.	18·36
		1002·43

On comparing the results of these five analyses, the extreme variableness of the composition of guano is the first thing that demands attention; no two samples resemble each other: in one, potash salts, abundance of urate of ammonia, with free uric acid exist; in another, the salts of the mixed alkalies are present, but the soda salts in far larger proportion than those of potash, and the amount of urate of ammonia diminished from about 17 to 2 per cent.; here also free uric acid is absent; in another instance of ordinary guano, No. 4, no uric acid whatever is present, and this specimen contains a large proportion of sand. The amount of ammonia also varies very considerably in this description of guano, and even the proportion of those salts which are common to both seldom bear any relation to each other. The state of combination in which the phosphate of lime occurs is also dissimilar; that of sample No. 1 apparently existing wholly as bone-phosphate, whilst that of No. 2 is a mixture of the neutral with the subsesquiphosphate of lime: instances of dissimilarity may readily be multiplied by a glance at the results of these two analyses, the resemblance to each other being for the most part confined to the existence of some similar salts in both cases.

Then, again, the samples of concrete guano differ not merely from the common guano, Nos. 1, 2, 3 and 4, but also widely from each other; sample No. 5, concrete guano, containing large proportions of sulphate of soda and oxalate of lime, no uric acid, and but a very small quantity, when compared to any of the other samples, of phosphate of lime—this existing in this specimen as bone-phosphate; whilst the other kind of concrete guano, No. 6, affords very small quantities of soluble substances, the merest trace of ammonia salts, and consists almost entirely of water and phosphate of lime. The constitution of this salt again varies from that which exists in the specimen No. 5, being chiefly neutral phosphate instead of the bone-phosphate. The saline guano is the very opposite of this last description; the larger proportion consisting of soda salts and others soluble in water: here also the ammoniacal salts are almost absent, whilst common salt, a substance existing in comparatively small quantities in the other samples, and totally absent in two of them, is by far its most prevalent constituent.

Can we then wonder that such widely differing results should have been obtained by various farmers and experimentalists who have used the substance termed guano; even when its application has taken place under almost identical circumstances, as respects quality of land, description of crop, time of application, &c. &c., and differing only in the guano used by either party being obtained through different sources? Putting aside all considerations of the mode of application to the soil, it is obvious that the effects of such manures must vary almost as widely as their composition; and that whilst the employment of such guano as No. 1, or even No. 2, would be in all probability attended with highly successful results, the use of the others, or even of these mixed with several lumps of Nos. 5, 6 and 7, would, on the contrary, completely disappoint the expectations of the agriculturist, and probably in some cases, especially where such a kind as No. 7 exists in quantity, or has deliquesced and been absorbed by the other portions of the guano, its application would be attended with positive injury.

The analyses of the ordinary guano detailed in this paper do not differ so considerably from Voelckel's analysis in composition, and the salts which they respectively contain, as to render many observations on this the most complete analysis I have met with necessary. In no one instance of the seven specimens of guano examined by me could I detect carbonate of ammonia in sufficient quantity to affect lime-water, although in two cases out of the seven, nitrate of barytes indicated a faint trace of this substance. The solutions in cold water were either neutral or slightly acid: these solutions distilled afforded a solution which was distinctly alkaline to test-paper, and contained a considerable quantity of ammonia, but it was in an uncombined state, not as carbonate; the residual solution in the retort subsequent to distillation was markedly acid, so that this ammonia arose from the decomposition of the phosphate of ammonia by boiling the solution containing it. Some portions of guano, gently dried and then sublimed, gave off ammoniacal fumes, and a white sublimate from the oxalate, muriate and other ammonia salts, but the sublimate contained no carbonate of ammonia. In this respect my results differ from those of Girardin, Bidard and some others, who describe carbonate of ammonia to exist in considerable quantities in the South American guano analysed by them. May they not have assumed that the ammonia liberated by heat existed as carbonate, as I did myself until these analyses obliged me experimentally to determine the question? Carbonate of ammonia is a substance, which, considering the composition of guano, does not appear unlikely to be produced; so that whilst I admit the possibility and even the probability of its presence on these grounds, yet experiment on seven distinct samples shows that it is absent.

The existence of the free uric acid which separated from the solution in boiling water of No. 1, may be questioned as to whether it ought not to be considered as a product arising from the decomposition of a portion of urate of ammonia by the presence of a stronger acid, rather than as existing in the specimen of guano in an uncombined state. This I cannot consider to have been the case; for although, like the other solutions made by boiling water, it was faintly acid, yet in No. 2 no free uric acid was discovered, although the urate of ammonia in that specimen existed under precisely the same circumstances as in the analysis of the first sample; and if uric acid had been evolved in the first instance from its combination with ammonia, this salt surely ought not to have escaped decomposition in the second case. I am therefore induced to consider it as highly probable that, when the dung of which guano is composed is freshly deposited, the uric acid which it contains is not wholly in a state of combination with ammonia, but that a portion exists as free uric acid.

I must now allude to the production of the oxalate of ammonia which is so constantly found in guano, being compelled on this point to differ with Dr. Fownes.

I am no friend to the hypothetical deductions which are now so constantly employed in organic chemistry. They may have their uses in leading the experimenter to the selection of the right path and the proper mode of investigating organized substances; but the abuses they are apt to engender, too often, I fear, more than counterbalance their advantages. Instances of the elevation of hypothesis to the rank and consideration due to ascertained and undoubted facts, and the erection of other theories upon the first hypothesis, are to be found thickly strewn throughout nearly every chemical publication of the present day; nor are the works of some of the most distinguished chemists free from this objection, but rather might be referred to as furnishing the most flagrant instances of this indulgence of a luxuriant imagination. In accordance with this usage, the sole requisite conditions to produce any given organic compound which may be known or imagined to exist, is to commence with a something which need contain but two elements, carbon and azote, and these are almost superfluous; add to this substance the oxygen of the atmosphere and water, as much as may be sufficient, liberate hydrogen, ammonia, oxygen, carbonic acid or azote, as may be necessary, and there is the substance sought for—*on paper.*

To return, however, to the formation of the oxalate of ammonia: it must, I presume, be formed from the urate of ammonia, because guano does not contain any other substance, except by such a process as is detailed above, which can produce it; in this, I believe, all chemists are agreed.

Now Dr. Fownes supposes that the uric acid of the urate of ammonia combines with eight equivalents of water and two of oxygen from the air, and produces four equivalents of oxalate of ammonia and two of carbonic acid, which of course unites, although it is not so stated in the note I refer to*, with the equivalent of ammonia before combined with the uric acid, producing the bicarbonate of ammonia, not the most volatile nor soluble of its salts, and which, if formed, surely ought to be found in every sample of guano. Now various specimens of guano have been examined, seven in number, in what we may suppose to be all the progressive stages of this conversion of the urate into the oxalate of ammonia; one containing uric acid, urate of ammonia and oxalate of ammonia; a second containing the two salts only; and others in which the uric acid and urate of ammonia have wholly disappeared, and oxalate of ammonia alone remains: yet in no one of these specimens of the guano has the substance, bicarbonate of ammonia, which is required by Dr. Fownes's theory, been detected; its presence would not necessarily have proved the theory to be correct, as it might fairly and possibly be attributed to the decay of azotized animal matter: but I respectfully submit that the absence of this ammoniacal salt proves this hypothesis of the conversion of urate into oxalate of ammonia to be erroneous. After the opinions I have uttered respecting the employment of hypothetical deductions, it is certainly vastly inconsistent on my part to have recourse to them, but it will tend to prove the truth of my former remarks on the readiness with which a theory in organic chemistry may be constructed, when no inconvenient nor insurmountable facts are opposed to it. In detailing this theory of the conversion of the urate into the oxalate of ammonia, I am more than indifferent to its fate; for gladly will I lend a helping hand to put an extinguisher upon it, if one based upon the actual and experimental conversion of the one salt into the other, a result I have endeavoured in vain to effect, is propounded.

Since Coindet's observation, that the uric acid in the excrement of birds existed as binurate of ammonia, the equivalent number of uric acid seems by general consent to have been doubled, so that this acid salt, according to Coindet's view, must now be considered as neutral urate of ammonia. Like Dr. Fownes's, my hypothesis of this conversion is an absorption of water and very slight oxidation, differing from his only in the proportions of these convenient substances, and in the conversion of the urate into the oxalate of ammonia without the formation of carbonate of ammonia or any other secondary

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* Memoirs of Chemical Society, part 3, p. 38.

products,—one equivalent of urate of ammonia combining with 8 equivalents of water and 1 of oxygen, and producing 5 equivalents of oxalate of ammonia.

$$\begin{array}{lr@{\qquad}lr}
\text{Uric acid . .} & C^{10}\,N^{4}\,H^{4}\,O^{6} & \text{Oxalic acid .} & C^{2}\,O^{3} \qquad\quad \\
\text{Ammonia . .} & N\;H^{3} & \text{Ammonia . .} & N\;H^{3} \\
\cline{2-2}\cline{4-4}
 & C^{10}\,N^{5}\,H^{7}\,O^{6} & & C^{2}\,O^{3}\;N\;H^{3} \\
\text{Water, 8 eqs.} & H^{8}\,O^{8} & & 5 \\
\cline{4-4}
\text{Oxygen, 1 eq.} & O & & C^{10}O^{15}N^{5}\,H^{15} \\
\cline{2-2}
 & C^{10}\,N^{5}\,H^{15}O^{15} & &
\end{array}$$

This theory has simplicity to recommend it, and is in accordance with observed facts, so far as they go; but after all it is merely hypothesis, and rests upon no experimental proof whatever; and I neither have faith in it myself, nor recommend it as worthy of credence by others. A more legitimate subject of speculation than the mode in which the oxalate of ammonia found in guano is produced, is the origin of guano itself and of its varieties. The large lumps found in the bags of guano, which I have termed "concrete guano," appear to me to have been taken from the oldest portions of the beds of this substance, and in which decay of the organic salts and animal substances has proceeded further than in common guano, and that concrete guano is merely ordinary guano in an advanced stage of decay agglomerated by pressure and long contact. This remark applies chiefly to sample No. 5. I consider it probable that samples 6 and 7 are formed from the action of sea water on a bed of guano somewhat deficient in ammonia salts and containing no urate of ammonia; in short, one of the oldest beds on the lower ledges of the coast and rocks. The spray of the sea falling upon a bed of guano in this position mixes with it, the insoluble portions subside in regular horizontal strata, which again dries and hardens, giving rise to the formation of masses consisting chiefly of insoluble substances, as in the specimen No. 6, whilst the solution collecting in the crevices and hollows is subsequently dried by evaporation in a dry atmosphere, and forms the saline guano, No. 7, in which common salt so greatly predominates, the oxalate of ammonia being converted by double decomposition into oxalate of soda and muriate of ammonia; the disappearance of the larger portion of the latter salt I can only account for by its well-known tendency, when in a damp state, as existing as a saturated solution, to creep over the sides of the vessels which contain it, by which means it may have separated itself from the sea-water solution previously alluded to before that became quite solid and dry, and thus extending itself over the ledges of rock, assisted in its progress by nocturnal dews, has been finally washed away by the sea-water. This is rather an unsatisfactory mode of accounting for the absence of the muriate of ammonia, which from what we know of guano, and the existence of oxalate of soda in the saline guano, we may fairly conclude must have been formed, but which in the sample I have examined contains but little more than one equivalent of muriate of ammonia to three of oxalate of soda, so that nearly two equivalents have disappeared.

As to the respective hypotheses of the coprolitic or recent nature of these deposits, I have never held but one opinion, the one I believe generally entertained, that this manure is deposited by sea-fowl inhabiting coasts where no rain falls, and which consequently is never washed away. This view is supported by all ancient and most modern authorities, and has recently received additional confirmation from Mr. Teschemacher of Boston, who has presented specimens of Peruvian guano to the Philosophical Society of that town, containing *feathers*; and after quoting the accounts given by the old Portuguese historian respecting the formation and preservation of this manure, very justly remarks, that the beds of the greatest thickness hitherto observed might, without any extravagant calculation, and at the rate only of two to three inches a year, or less, be deposited in about three thousand years; whilst the theory of its coprolitic origin not merely requires a considerable exercise of the imagination, but is opposed by the direct testimony of eye-witnesses.

P.S. The May Number of the Philosophical Magazine, which has just come into my hands, contains an analysis of African guano by our Foreign Secretary, Mr. E. F. Teschemacher, in which he finds humic acid to exist in a soluble state in the African specimen, but no urate of ammonia. The humus described to exist in the Peruvian specimens analysed by me was extracted by dilute potash from the residue insoluble in water; and if it be true humus, and it possesses all the characters assigned to that substance, the South American varies from the African guano, amongst other differences, in containing uncombined humus.

XIII. *On certain Results relating to Quaternions.* *By* ARTHUR CAYLEY, *Esq., B.A., Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN his last paper on Quaternions, Sir William R. Hamilton has alluded to a paper of mine on the Analytical Geometry of (n) dimensions, in the Cambridge Mathematical

Journal, as one that might refer to the same subject. It may perhaps be as well to notice that the investigations there contained have no reference whatever to Sir William Hamilton's very beautiful theory; a more correct title for them would have been, a Generalization of the Analysis which occurs in ordinary Analytical Geometry.

I will, with your permission, take this opportunity of communicating one or two results relating to quaternions; the first of them does appear to me rather a curious one.

Observing that

$$\left.\begin{aligned}&(\mathrm{A}+\mathrm{B}i+\mathrm{C}j+\mathrm{D}k)^{-1}\\ &\quad=(\mathrm{A}-\mathrm{B}i-\mathrm{C}j-\mathrm{D}k)\div(\mathrm{A}^2+\mathrm{B}^2+\mathrm{C}^2+\mathrm{D}^2)\end{aligned}\right\}, \quad . \quad (1.)$$

it is easy to form the equation

$$\left.\begin{aligned}&(\mathrm{A}+\mathrm{B}i+\mathrm{C}j+\mathrm{D}k)^{-1}(\alpha+\beta i+\gamma j+\delta k)(\mathrm{A}+\mathrm{B}i+\mathrm{C}j+\mathrm{D}k)\\ &=\frac{1}{\mathrm{A}^2+\mathrm{B}^2+\mathrm{C}^2+\mathrm{D}^2}\\ &\left\{\begin{aligned}&\alpha.(\mathrm{A}^2+\mathrm{B}^2+\mathrm{C}^2+\mathrm{D}^2)\\ &+i[\beta(\mathrm{A}^2+\mathrm{B}^2-\mathrm{C}^2-\mathrm{D}^2)+2\gamma.\overline{\mathrm{BC}+\mathrm{AD}}+2\delta.\overline{\mathrm{BD}-\mathrm{AC}}]\\ &+j[2\beta(\mathrm{BC}-\mathrm{AD})+\gamma(\mathrm{A}^2-\mathrm{B}^2+\mathrm{C}^2-\mathrm{D}^2)+2\delta.\overline{\mathrm{CD}+\mathrm{AB}}]\\ &+k[2\beta(\mathrm{BD}+\mathrm{AC})+2\gamma(\mathrm{CD}-\mathrm{AB})+\delta.\overline{\mathrm{A}^2-\mathrm{B}^2-\mathrm{C}^2+\mathrm{D}^2}]\end{aligned}\right.\end{aligned}\right\}, \quad (2.)$$

which I have given with these letters for the sake of reference; it will be convenient to change the notation and write

$$\left.\begin{aligned}&(1+\lambda i+\mu j+\nu k)^{-1}.(ix+jy+kz)(1+\lambda i+\mu j+\nu k)\\ &=\frac{1}{1+\lambda^2+\mu^2+\nu^2}\\ &\left\{\begin{aligned}&i\,[x(1+\lambda^2-\mu^2-\nu^2)+2y(\lambda\mu+\nu)+2z(\lambda\nu-\mu)]+\\ &j[2x(\lambda\mu-\nu)+y(1-\lambda^2+\mu^2-\nu^2)+2z(\mu\nu+\lambda)]+\\ &k[2x(\lambda\nu+\mu)+2y.(\mu\nu-\lambda)+z(1-\lambda^2-\mu^2+\nu^2)]\end{aligned}\right.\end{aligned}\right\} \quad (3.)$$

$$\left.\begin{aligned}&i(\alpha x+\alpha' y+\alpha'' z)\\ &+j.(\beta x+\beta' y+\beta'' z)\\ &+k(\gamma x+\gamma' y+\gamma'' z)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

suppose. The peculiarity of this formula is, that the coefficients α, β ... are precisely such that a system of formulæ

$$\left.\begin{aligned}x_{\prime}&=\alpha x+\alpha' y+\alpha'' z\\ y_{\prime}&=\beta' x+\beta' y+\beta'' z\\ z_{\prime}&=\gamma x+\gamma' y+\gamma'' z\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

denote the transformation from one set of rectangular axes to another set, also rectangular. Nor is this all, the quantities λ, μ, ν may be geometrically interpreted. Suppose the axes Ax, Ay, Az could be made to coincide with the axes A$x_{\prime}$, A$y_{\prime}$, A$z_{\prime}$ by means of a revolution through an angle θ round an axis A P inclined to A x, A y, A z, at angles f, g, h, then

$$\lambda = \tan \tfrac{1}{2}\theta \cos f, \quad \mu = \tan \tfrac{1}{2}\theta \cos g, \quad \nu = \tan \tfrac{1}{2}\theta \cos h.$$

In fact the formulæ are precisely those given for such a transformation by M. Olinde Rodrigues Liouville, t. v., "Des lois géometriques qui régissent les déplacemens d'un système solide" (or Camb. Math. Journal, t. iii. p. 224). It would be an interesting question to account, *à priori*, for the appearance of these coefficients here.

The ordinary definition of a determinant naturally leads to that of a quaternion determinant. We have, for instance,

$$\begin{vmatrix} \varpi, & \phi \\ \varpi', & \phi' \end{vmatrix} = \varpi\,\phi' - \varpi'\,\phi, \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

$$\begin{vmatrix} \varpi, & \phi, & \chi \\ \varpi', & \phi', & \chi' \\ \varpi'', & \phi'', & \chi'' \end{vmatrix} = \varpi(\phi'\chi'' - \phi''\chi') + \varpi'(\phi''\chi - \phi\chi'') + \varpi''(\phi\chi' - \phi'\chi),$$

&c., the same as for common determinants, only here the order of the factors on each term of the second side of the equation is essential, and not, as in the other case, arbitrary. Thus, for instance,

$$\begin{vmatrix} \varpi, & \varpi' \\ \varpi, & \varpi' \end{vmatrix} = \varpi\,\varpi' - \varpi\,\varpi' = 0, \quad . \quad . \quad . \quad . \quad (7.)$$

but

$$\begin{vmatrix} \varpi, & \varpi \\ \varpi', & \varpi' \end{vmatrix} = \varpi\,\varpi' - \varpi'\,\varpi = 0. \quad . \quad . \quad . \quad . \quad (8.)$$

Or a quaternion determinant does not vanish when two vertical rows become identical. One is immediately led to inquire what the value of such determinants is. Suppose

$$\varpi = x + iy + jz + kw, \quad \varpi' = x' + iy' + jz' + kw', \text{ \&c.,}$$

is it easy to prove

$$\left.\begin{vmatrix} \varpi & \varpi \\ \varpi' & \varpi' \end{vmatrix} = -2 \begin{vmatrix} i, & j, & k \\ x, & y, & z \\ x', & y', & z' \end{vmatrix}\right\}, \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

$$\left.\begin{vmatrix} \varpi, & \varpi, & \varpi \\ \varpi', & \varpi', & \varpi' \\ \varpi'', & \varpi'', & \varpi'' \end{vmatrix} = -2 \begin{vmatrix} 3, & i, & j, & k \\ x, & y, & z, & w \\ x', & y', & z', & w' \\ x'', & y'', & z'', & w'' \end{vmatrix}\right\}, \quad . \quad (10.)$$

$$\left.\begin{vmatrix} \varpi & \varpi & \varpi & \varpi \\ \varpi' & \varpi' & \varpi' & \varpi' \\ \varpi'' & \varpi'' & \varpi'' & \varpi'' \\ \varpi''' & \varpi''' & \varpi''' & \varpi''' \end{vmatrix} = 0 \right\}. \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

Or a quaternion determinant vanishes when four or more of its vertical rows become identical.

Again, it is immediately seen that

$$\left.\begin{vmatrix} \varpi, & \phi \\ \varpi', & \phi' \end{vmatrix} + \begin{vmatrix} \phi, & \varpi \\ \phi', & \varpi' \end{vmatrix} = \begin{vmatrix} \varpi, & \varpi \\ \phi', & \phi' \end{vmatrix} - \begin{vmatrix} \varpi', & \varpi' \\ \phi, & \phi \end{vmatrix}\right\}, \quad (12.)$$

&c. for determinants of any order, whence the theorem, if any four (or more) adjacent vertical columns of a quaternion determinant be transposed in every possible manner, the sum of all these determinants vanishes, which is a much less simple property than the one which exists for the horizontal rows, viz. the same that in ordinary determinants exists for the horizontal or vertical rows indifferently. It is important to remark that the equations

$$\begin{vmatrix} \varpi, & \phi \\ \varpi', & \phi' \end{vmatrix} = 0 \text{ or } \begin{vmatrix} \varpi, & \varpi' \\ \phi, & \phi' \end{vmatrix} = 0, \text{ \&c.} \quad . \quad . \quad . \quad (13.)$$

i. e. $\varpi\,\phi' - \varpi'\,\phi = 0$, or $\varpi\,\phi' - \phi\,\varpi' = 0$, &c.

are none of them the result of the elimination of Π, Φ, from the two equations

$$\left.\begin{array}{l} \varpi\,\Pi + \phi\,\Phi = 0, \\ \varpi'\,\Pi + \phi'\,\Phi = 0. \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

On the contrary, the result of this elimination is the very different equation

$$\varpi^{-1} . \phi - \varpi'^{-1} . \phi' = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

equivalent of course to four independent equations, one of which may evidently be replaced by

$$\mathrm{M}\,\varpi . \mathrm{M}\,\phi' - \mathrm{M}\,\varpi' . \mathrm{M}\,\phi = 0, \quad . \quad . \quad . \quad (16.)$$

if $\mathrm{M}\,\varpi$, &c. denotes the modulus of ϖ, &c. An equation analogous to this last will undoubtedly hold for any number of equations, but it is difficult to say what is the equation analogous to the one immediately preceding this, in the case of a greater number of equations, or rather, it is difficult to give the result in a symmetrical form independent of extraneous factors.

I may just, in conclusion, mention what appears to me a possible application of Sir William Hamilton's interesting discovery. In the same way that the circular functions depend on infinite products, such as

$$x\,\Pi\left(1 + \frac{x}{m\,\pi}\right), \text{ \&c.,} \quad . \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

[m any integer from ∞ to $-\infty$, omitting $m=0$]

and the inverse elliptic functions on the doubly infinite products

$$x\,\Pi\left(1 + \frac{x}{m\,\omega + n\,\varpi\,i}\right), \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

[m and n integers from ∞ to $-\infty$, omitting $m=0$, $n=0$], may not the inverse ultra-elliptic functions of the next order of complexity depend on the quadruply infinite products

$$x\,\Pi\left(1+\frac{x}{m\omega+n\varpi i+o\phi j+p\psi k}\right)? \quad . \quad . \quad (19.)$$

[m, n, o, p integers from ∞ to $-\infty$, omitting $m=0$, $n=0$, $o=0$, $p=0$].

It seems as if some supposition of this kind would remove a difficulty started by Jacobi (Crelle, t. ix.) with respect to the multiple periodicity of these functions. Of course this must remain a mere suggestion until the theory of quaternions is very much more developed than it is at present; in particular the theory of quaternion exponentials would have to be developed, for even in a product, such as (18.), there is a certain singular exponential factor running through the theory, as appears from some formulæ in Jacobi's *Fund. Nova* (relative to his functions Θ, H), the occurrence of which may be accounted for, *à priori*, as I have succeeded in doing in a paper to be published shortly in the Cambridge Mathematical Journal.

I remain, Gentlemen,
Your obedient Servant,
Cambridge, December 4, 1844. A. Cayley.

XIV. *Correction of an Error in the author's* "Strictures on Professor Dove's Essay on the Law of Storms." *By* Robert Hare, *M.D., Professor of Chemistry in the University of Pennsylvania.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG leave to correct an error, committed in my Strictures on Dove's Law of Storms, Phil. Mag. S. 3. vol. xxv. p. 100, in assuming that the contents of the zones in a circular area between equidistant concentric parallel lines are to each other as the squares of their mean distances from the common centre; instead of assuming them to be simply as those distances. Of course the velocity of the air in the zone nearest the upward columnar current, in a tornado or hurricane, will not be to the velocity of any greater zone inversely as the *squares* of the mean distances from the axis of the column, but simply in the inverse ratio of those distances.

Hence, supposing the centripetal velocity at a mile from the centre, or say five thousand feet to be one hundred miles per hour, at twenty miles, it would be five miles per hour, or merely a breeze. By this amendment of the calculation my argument is strengthened, so far as it was an object of it to prove that in an extensive hurricane the central area protected by the upward current, from the horizontal impetus of the wind and consequently calm, need not be so spacious as to require more than a very few minutes for it to be passed over by a storm travelling at the rate of thirty miles per hour. Supposing the confluent blasts to proceed only from two opposite quadrants, the extent of the storm, in front and in rear of its path, would of course be doubled, so as to require forty miles distance to effect a reduction of the velocity from one hundred miles per hour to five miles.

I am, Gentlemen,
Respectfully yours,
Philadelphia, November 23, 1844. ROBERT HARE.

XV. *On the Mechanism of Glacial Motion.*

By W. HOPKINS, *Esq., M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN my preceding communication I gave an explanation of the manner in which the general motion of glaciers may be accounted for according to the *sliding theory.* I now proceed to consider more particularly the mechanism of this motion.

1. The most important facts in the motion of glaciers, hitherto determined, appear to be the following:—

(1.) The motion of each point on the surface of a glacier is approximately parallel to the longitudinal axis of the glacier, and the sides of the glacial valley, whenever the axis and sides are approximately parallel, as is very generally the case in those portions of glaciers which have been subjected to detailed observations, and which are descending along valleys of well-defined and determinate forms. If the valleys converge or diverge in descending, the motion of different points of the glacier will partake of the same character.

(2.) If we take any number of points in a transverse line perpendicular to the axis of the glacier, the velocity of those points will be greatest which are nearest to the centre of the glacier.

(3.) This increase of the velocity, in passing from the sides to the centre, takes place much more rapidly near the sides than in other parts of the glacier.

(4.) The motion (at least during summer) is greater near the lower than it is near the upper extremity of a glacier.

(5.) The motion is greater in warm than in cold weather.

(6.) Nothing has been positively ascertained by observation respecting the relative velocities of the upper and lower surfaces of a glacier. The continued verticality of transverse fissures is, however, a conclusive proof that, to the depth to which these fissures extend, the velocity must be sensibly the same as at the surface.

2. The facts above stated are in entire accordance with the conclusions deducible from the sliding theory. According to that theory, the velocity of any portion of a glacier will depend (1) on the inclination of its bed, (2) the disintegration of its lower surface by the internal heat of the earth, (3) on subglacial currents, (4) the depth of the mass*, and (5) local and lateral obstacles. The first and second causes will generally have nearly the same effect both in the central and lateral portions; but the third cause will manifestly produce in general the greatest acceleration in the central parts, and the fourth cause will produce a similar effect, if the glacier be deeper in the centre than at its sides, while the greatest retardation will be produced on the lateral portions by the last of the above-mentioned causes. These causes sufficiently account for the greater velocity of the centre of the glacier.

Again, the second of the above causes will probably act with approximate uniformity throughout the whole length of the glacier, but the third cause will act with the greatest energy at the lower extremity, because the subglacial currents will be increased by innumerable tributaries as they descend. This cause, therefore, will tend to make the velocity greater, as we approach the lower end of the glacier, while the greater depth of the mass at the upper extremity will tend to give the greater velocity to that part of the glacier. In winter the effect of the currents must be very inconsiderable, and we should consequently expect that there will be a tendency in the portions of the glacier in the higher regions to move faster than those in the lower, in which case there must be a longitudinal compression, and consequent closing up of transverse fissures in a greater or less degree. During the summer, on the contrary, the subglacial currents will be most efficient, and we should expect that they will give the greater velocity to the lower extremity of the glacier, in which case the mass will be brought into a state of longitudinal tension.

3. It is manifest that the more rapid motion of the central part of the glacier must bring the whole mass into a state of internal constraint. My first object will be to deduce certain conclusions respecting the internal tensions and pressures thus superinduced, while the continuity of the mass, or of any con-

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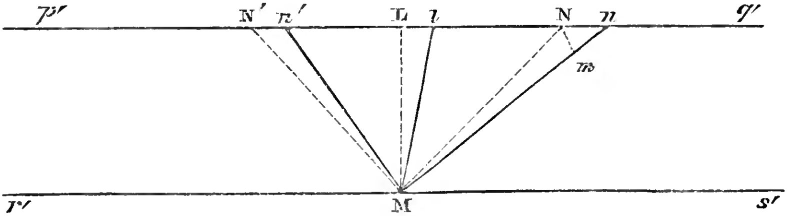
* In the experiments described in my previous communication, the velocity of descent increased with the weight, whence it is inferred that the deeper parts of a glacier must, *cæteris paribus*, tend to move the fastest.

siderable portion of it, remains unbroken. In the second place, I shall show how the continuity will be broken when the internal tensions become greater than the cohesive power of the ice, and shall investigate the forms and positions of the lines or curves of fracture; and thence I shall explain how the progressive motion of the glacier may take place by a constant repetition of such dislocations.

It has been already stated that no conclusive observations have been made respecting the difference between the velocities of the upper and lower surfaces of a glacier. In the following investigations I shall assume these velocities to be the same as they will be approximately, if the general motion be due to *sliding*. There would be no difficulty introduced into our investigations by the adoption of the contrary hypothesis—that the lower surface moves much slower than the upper one—but the resulting formulæ would be of much greater complexity. For the sake of simplicity I have therefore adopted the first hypothesis. Any one wishing to pursue the subject under the latter hypothesis will find no difficulty in doing so. This, however, will not be necessary for our immediate purpose.

4. *Directions of Maximum Tension and Pressure.*—Of the above laws the first and second are those with which we shall be first more immediately concerned. Let $p' q'$ be the line of motion

Fig. 1.



of one set of particles of the glacier, and $r' s'$ that for another set, the distance between them being small; these lines will be parallel to each other and approximately so to the axis of the glacier. Let M and N be particles in $r' s'$ and $p' q'$ respectively. Assuming $r' s'$ to be nearer the side of the glacier than $p' q'$, N will move faster than M, by the second law. If they moved with the same velocity, the physical line M N would neither be lengthened nor shortened by the motion we are considering; and therefore it will be to the difference of motion of M and N that extension or compression will be due. Consequently we may suppose M to be at rest, and N to move with the difference of velocity of M and N, when our object is to calculate the extensions and compressions due to the motion of the mass. For the greater distinctness, let us conceive the mass to be at first in a state in which it is neither extended nor compressed. In a given time, considering M at rest, let $N n$ be the space due to the difference of motion of M and N, or to the relative velocity of N. The physical line whose unextended length was M N will now be *extended* into $M n$; and in like manner, if N′ be to the left of L (M L being perpendicular to $p' q'$ and $r' s'$), M N′ will be *compressed* into $M n'$, where $N' n' = N n$. This shows distinctly the manner in which the motion in question must necessarily produce extension and compression in the mass in different directions.

If the space $N n$ due to the relative motion of N become sufficiently great, the physical line M N will be so extended as to break, and an open fissure will be the consequence. Our first object, however, will be to find those directions in which the extension and compression have their maximum values, before the extension has become so great as to cause the fracture of the mass. And here it is important to remark, that so long as we keep within this limit, we have no concern whatever with the cohesive force of the mass, or the law according to which it may vary in passing from one point to another. That consideration will only enter when we proceed to the ulterior part of our investigation, where the object will be to determine the directions in which the mass will be fractured, when the internal tensions shall become greater than those which its cohesive force is able to resist.

5. The *extension* of the physical line M N will be correctly represented by the ratio $\frac{m n}{\mathrm{M\,N}}$, the point m being so taken that $M m$ shall $= \mathrm{M\,N}$. This ratio will manifestly be different for different lines, such as M N; our first object is to find the position of M N for which the ratio will be a maximum, subject to the condition that $N n$ shall be the same for all positions of N. The problem, as thus presented to us, is not mechanical but geometrical, and a very simple one. To render it still more simple in the calculation, I shall here suppose $N n$ a small quantity of the first order, and shall omit small quantities of the second and higher orders*. Let $\mathrm{M\,L} = a$, $N n = \beta$, and $\mathrm{L\,M\,N} = \theta$; then shall we have, to the requisite degree of approximation,

$$m n = \beta \sin\theta, \quad \mathrm{M\,N} = \frac{a}{\cos\theta};$$

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* A more complete investigation, and independent of this restriction, will be given afterwards.

and therefore

$$\frac{m\,n}{\mathrm{M\,N}} = \frac{\beta}{2\,a}\sin 2\,\theta.$$

Consequently the extension is a maximum when $\theta = 45^\circ$; also it is a minimum when $\theta = 135^\circ$, or the *compression* is a *maximum* for this latter value of θ.

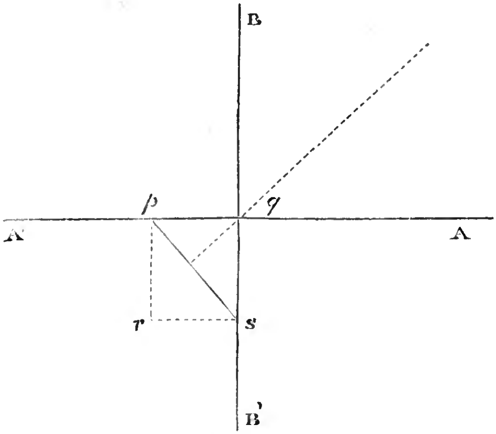
6. To pass generally from the extension and compression to the corresponding values of the *tension* and *pressure*, it is necessary to know the laws which connect them; but here we are not concerned with this more general question, our object being merely to deduce the directions of maximum tension and pressure from those of maximum extension and compression. Now, whatever may be the general law connecting tension and extension, there can be no doubt that when the one is a maximum or minimum the other must be so likewise. Hence the directions above determined will be those of maximum *tension* and *pressure*, so far as the tension and pressure are superinduced by the motion of the glacier. Consequently if M N and M N′ be those directions for the point M, each will be inclined at an angle of 45° to the direction of motion, and they will be at right angles to each other.

The preceding method has the advantage of pointing out very simply and distinctly the manner in which the difference of velocity in different parts of the glacier produces tension and pressure, and the results above given are arrived at with great facility. But I shall now proceed to a more complete investigation of the problem, still preserving the same limitation as before in the hypothesis of the equality of the velocities of the upper and lower surfaces of the glacier. Our results will thus be rendered independent of the depth of the glacier, which may, therefore, be treated as a simple *lamina*. We shall thus only be concerned with space of two dimensions.

7. *Internal Tensions and Pressures of a Glacier.*—When a plain solid lamina, having a certain degree of compressibility and extensibility, is brought into a position of constraint by forces acting in the plane of the lamina, the particles on one side of a geometrical line will exert certain forces on the contiguous particles on the opposite side of the line. If the lamina were formed of fluid particles the resultant action at each point of this *line of separation* would be normal to it; but when the lamina is solid this will not be generally the case, and therefore the force at any point of the line may be resolved into two forces, one being normal and the other tangential to the line of separation; all forces being supposed to act in the plane of the lamina. Suppose the line of separation to be a straight

line A′ A parallel to the axis of x, and let pq be a portion of it so small that the actions on every point of pq may be considered equal. Let $Y_1 . pq$ denote the normal force exerted by the particles immediately above pq in the annexed figure, on those immediately below it, estimated in the direction q B; and let $f_{\prime} . pq$ represent the tangential action on pq. Again, let the line of separation coincide with B′ B, parallel to the axis of y, and perpendicular to A′ A; and let $X_1 . qs$ denote the normal force exerted by the particles immediately on the right of qs on the contiguous particles immediately on the left of it, and $f'_{\prime} . qs$ the tangential action. Join p and s, and let a perpendicular to ps make an angle θ with A′ A or the axis of x. Then if $X . ps$ and $Y . ps$ be the resolved parts of the forces which the particles on one side of ps exert on those on the opposite side, estimated in the direction q A and q B respectively, we shall have

Fig. 2.



$$X = X_1 \cos\theta + f_{\prime} \sin\theta,$$
$$Y = Y_1 \sin\theta + f'_{\prime} \cos\theta.$$

To prove these formulæ, we have only to observe that the forces acting on the sides pq and qs of the triangular element pqs must be in equilibrium with the forces $-X$ and $-Y$ acting externally on the side ps, neglecting small quantities of the third order. Hence we have

$$-X . ps + X_1 . qs + f_{\prime} . pq = 0,$$
$$-Y . ps + Y_1 . pq + f'_{\prime} . qs = 0,$$

which, since $\frac{pq}{ps} = \sin\theta$, and $\frac{qs}{ps} = \cos\theta$, prove the above formulæ*.

We have also the relation

$$f_I' = f_I.$$

To prove this equation, complete the rectangular element $pqsr$. A tangential force will act on the element along the side rs in a direction opposite to that of the tangential force (f) acting along pq, the intensity of which will not differ from f by any finite quantity; and similarly, a force (f') will act on the side pr in the direction opposite to that on qs. The moments of these forces with respect to the middle point of the rectangular element, will be

$$\frac{1}{2}f_I \cdot pq \cdot qs, \text{ and } \frac{1}{2}f_I' \cdot pq \cdot qs.$$

The direction of the resultant of the normal forces on qs will pass at a distance from the middle point of the element small compared with qs; that distance will therefore not exceed a quantity of the second order; and consequently the moment of the force $X_1 . qs$ about the middle point of the element will not exceed a quantity of the third order, and may be neglected in comparison with the moments of the tangential forces f and f', which are of the second order. Hence the equilibrium of the element requires that we should have

$$\frac{1}{2}f_I' \cdot pq \cdot qs = \frac{1}{2}f_I \cdot pq \cdot qs,$$

or

$$f_I' = f_I.$$

With this condition we have

$$X = X_1 \cos\theta + f_I \sin\theta,$$
$$Y = Y_1 \sin\theta + f_I \cos\theta.$$

If a line be drawn through q parallel to ps, the distance between the two lines will be a small quantity of the first order, and therefore the action on the line through q may be considered to have for its resolved parts the forces X and Y, from which they cannot differ by quantities exceeding infinitesimals of the first order.

Let the length of ps, or of an equal and parallel line through q, $= \lambda$; the resolved parts of the forces upon it will be λX and λY. Let λR be the force on λ estimated in a direction making an angle ϕ with the axis of x, then shall we have

$$\lambda R = \lambda X . \cos\phi + \lambda Y . \sin\phi,$$

or

$$R = X\cos\phi + Y\sin\phi;$$

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* See Poisson's memoir, "Sur le Mouvement des Corps Elastiques," in the *Mémoires de l'Institut*, vol. iii. p. 383.

R is therefore a function of the two independent variables θ and ϕ; and I shall now proceed to find the values of θ and ϕ, which render R a maximum or a minimum. Differentiating with respect to ϕ, we have

$$0 = \mathrm{X} \sin \phi - \mathrm{Y} \cos \phi,$$

which shows that for any assigned value of θ, or position of the line of separation, the maximum value of R will be that of the resultant of X and Y, and the corresponding value of ϕ, that of the angle which the direction of that resultant makes with the axis of x. Differentiating with respect to θ, we have

$$0 = \frac{d\mathrm{X}}{d\theta} \cos \phi + \frac{d\mathrm{Y}}{d\theta} \sin \phi.$$

Substituting for X and Y in these two equations, we obtain

$$(\mathrm{X}_1 \cos \theta + f_{\prime} \sin \theta) \sin \phi - (\mathrm{Y}_1 \sin \theta + f_{\prime} \cos \theta) \cos \phi = 0,$$
$$(\mathrm{X}_1 \sin \theta - f_{\prime} \cos \theta) \cos \phi - (\mathrm{Y}_1 \cos \theta - f_{\prime} \sin \theta) \sin \phi = 0.$$

Eliminating ϕ, we have

$$(\mathrm{X}_1 \cos \theta + f_{\prime} \sin \theta)(\mathrm{X}_1 \sin \theta - f_{\prime} \cos \theta)$$
$$- (\mathrm{Y}_1 \sin \theta + f_{\prime} \cos \theta)(\mathrm{Y}_1 \cos \theta - f_{\prime} \sin \theta) = 0,$$
$$\therefore (\mathrm{X}_1 f_{\prime} + \mathrm{Y}_1 f_{\prime})(\sin^2 \theta - \cos^2 \theta) + (\mathrm{X}^2_1 - \mathrm{Y}^2_1) \sin \theta \cos \theta = 0;$$
$$\therefore \tan 2\theta = \frac{2 f_{\prime}}{\mathrm{X}_1 - \mathrm{Y}_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Again, from the two preceding equations containing θ and ϕ, we have

$$(\mathrm{X}_1 + f_{\prime} \tan \theta) \tan \phi - (\mathrm{Y}_1 \tan \theta + f_{\prime}) = 0,$$
$$(\mathrm{X}_1 \tan \theta - f_{\prime}) - (\mathrm{Y}_1 - f_{\prime} \tan \theta) \tan \phi = 0,$$

or

$$\mathrm{X}_1 \tan \phi - \mathrm{Y}_1 \tan \theta + f_{\prime} \tan \theta \tan \phi - f_{\prime} = 0,$$
$$\mathrm{X}_1 \tan \theta - \mathrm{Y}_1 \tan \phi + f_{\prime} \tan \theta \tan \phi - f_{\prime} = 0.$$

θ and ϕ enter exactly in the same manner in these two equations, and must therefore be equal. Hence

$$\tan 2\phi = \frac{2 f_{\prime}}{\mathrm{X}_1 - \mathrm{Y}_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Equation (1.) shows that there are two positions of the line of separation through any proposed point, at right angles to each other, for one of which the resultant action between the particles on opposite sides of the line at the proposed point is a maximum, and for the other a minimum; and since ϕ determines the direction of the resultant action, equation (2.) proves that direction to coincide with the normal to the line of separation, whenever that line is in a position for which the resultant action is a maximum or minimum. These conclusions may also be arrived at by somewhat different though equivalent reasoning, as follows.

8. First, to find the value of θ which gives R a maximum or minimum, we have

$$R^2 = X^2 + Y^2,$$

and therefore

$$0 = X\frac{d\,X}{d\,\theta} + Y\frac{d\,y}{d\,\theta},$$

which by substitution and reduction gives

$$(X_1 f + Y_1 f)\,(\sin^2\theta - \cos^2\theta) + (X^2{}_1 - Y^2{}_1)\sin\theta\cos\theta = 0,$$

or

$$\tan 2\,\theta = \frac{2 f_{\prime}}{X_1 - Y_1}.$$

And, secondly, taking ϕ as the angle which the resultant of X and Y makes with the axis of x, we have

$$\tan\phi = \frac{Y}{X} = \frac{Y_1 \sin\theta + f_{\prime}\cos\theta}{X_1\cos\theta + f_{\prime}\sin\theta};$$

and if we put $\phi = \theta$, we shall determine that position of the line of separation for which the direction of the resultant action at any proposed point of it coincides with the normal. We thus obtain

$$\sin\theta\,\{X_1\cos\theta + f_{\prime}\sin\theta\} = \cos\theta\,\{Y_1\sin\theta + f_{\prime}\cos\theta\},$$

or

$$(X_1 - Y_1)\sin\theta\cos\theta = f_{\prime}\,(\cos^2\theta - \sin^2\theta);$$

$$\therefore \tan 2\,\theta = \frac{2 f_{\prime}}{X_1 - Y_1}.$$

This equation shows that that position of the line of separation for which $\phi = \theta$, is that which corresponds to the maximum or minimum action between the contiguous particles on opposite sides of the line, as before proved.

9. If the forces acting parallel to the axes of x and y be *pressures*, X_1 and Y_1 will be negative.

If $X_1 - Y_1 = 0$, $\theta = 45°$. This accords with the result previously obtained in article 5, where X_1 and Y_1 were both $= 0$*.

10. If (R) and (r) be the maximum and minimum values of R at q, they will act respectively in directions perpendicular and parallel to $p\,s$, the position of that line being determined by equation (1.). (R) will therefore be the maximum tension,

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* It should be observed that in the investigation of the article above referred to, the condition of the transverse force Y_1 being $= 0$ after the point N (fig. 1.) has moved to n, is only secured by supposing β so small a quantity of the first order that all small quantities of higher orders may be neglected. In that case the compression along M L becomes a small quantity of the second order, and therefore such as may be neglected, if β be very small, and not otherwise. If β were more considerable, we should obtain by the same mode of investigation a different result. In this case, however, the second mode of investigation is much the best.

and (r) the minimum tension or maximum pressure at that point. To find their values we have generally,

$$R = X \cos \phi + Y \sin \phi,$$

and, substituting for X and Y,

$$R = (X_1 \cos \theta + f_1 \sin \theta) \cos \phi + (Y_1 \sin \theta + f_1 \cos \theta) \sin \phi.$$

Also, when R is a maximum or minimum,

$$\phi = \theta, \quad \text{and } \tan 2\theta = \frac{2 f_1}{X_1 - Y_1},$$

whence we obtain, after reduction,

$$\left.\begin{aligned} (R) &= \frac{1}{2} \{X_1 + Y_1 + \sqrt{(X_1 - Y_1)^2 + 4 f_1^2}\} \\ \text{and} \quad (r) &= \frac{1}{2} \{X_1 + Y_1 - \sqrt{(X_1 - Y_1)^2 + 4 f_1^2}\} \end{aligned}\right\} \quad . \quad . \quad (3.)$$

If X_1 and Y_1 be negative (*i. e. pressures*), the values of (R) as well as of (r) may become negative, in which case (R) will be the *minimum pressure* and (r) the *maximum pressure* to which the mass is subjected at q.

11. If f denote the tangential force acting along $p\,s$, we have, resolving X and Y along $p\,s$,

$$\begin{aligned} f &= X \sin \theta - Y \cos \theta \\ &= (X_1 \cos \theta + f_1 \sin \theta) \sin \theta - (Y_1 \sin \theta + f_1 \cos \theta) \cos \theta \\ &= \frac{1}{2}(X_1 - Y_1) \sin 2\theta - f_1 \cos 2\theta. \end{aligned}$$

Therefore, when f is a maximum or minimum, we have

$$0 = \frac{1}{2}(X_1 - Y_1) \cos 2\theta + f_1 \sin 2\theta,$$

or

$$\cot 2\theta = -\frac{2 f_1}{X_1 - Y_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

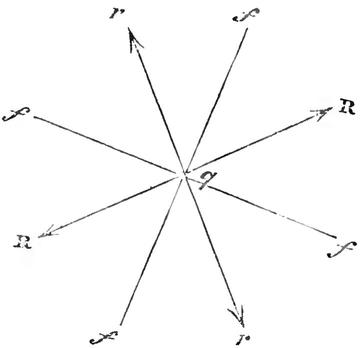
Comparing this equation with (1.), it appears that the directions of the line of separation, for which f is a maximum and minimum respectively, differ from those for which R is a maximum or minimum by 45°. To find (f), the maximum value of f, we have only to substitute from (4.) the values of $\sin 2\theta$ and $\cos 2\theta$ in the above expression for f. We thus obtain

$$(f) = \pm \frac{1}{2} \sqrt{(X_1 - Y_1)^2 + 4 f_1^2} \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

This expression shows that the maximum and minimum values of f are the same in magnitude, and only differing in sign. This ought to be the case, for I have shown that f is the same for any two directions at right angles to each other, and from (4.) we conclude that the maximum and minimum directions are also at right angles to each other. Consequently the maximum and minimum values of f cannot differ in intensity, and only therefore in sign.

12. From these results, with respect to R and f, we have the following conclusions:—Let q be any point in the mass in a state of constraint; then if $\mathrm{R}\,q\,\mathrm{R}$ be the direction of maximum tension at q, $r\,q\,r$ perpendicular to $\mathrm{R}\,q\,\mathrm{R}$, will be that of minimum tension; and if the two lines $f\,q\,f$ be perpendicular to each other, and make angles of 45° with $\mathrm{R}\,q\,\mathrm{R}$ and $r\,q\,r$, they will be the directions in which the intensity of f will be the greatest. It should also be recollected that $f = 0$ in the two directions $\mathrm{R}\,q\,\mathrm{R}$ and $r\,q\,r$.

Fig. 3.



There is also a simple relation between the maximum value (f) of f, and the maximum and minimum values (R) and (r) of R. It is obtained immediately from equations (3.) and (5.), which give

$$(\mathrm{R}) - (r) = 2\,(f).$$

13. If we take a rectangular element at q, of which the sides are respectively parallel to RR and rr, it will be acted on by no tangential forces, and therefore will be held in equilibrium by the maximum and minimum normal tensions alone. Consequently, whatever may be the forces acting on the mass, the state of tension or pressure at any point (q) will be the same as if the forces impressed on that point were those of two systems of forces, of which the intensities should be (R) and (r), and acting in directions perpendicular to each other. This proposition is important, as leaving no doubt as to the direction in which the greatest tendency is exerted by the normal forces to fracture the mass, a point to be considered in the sequel.

That there may be no doubt as to the equivalence of forces asserted in this proposition, let us assume the forces impressed on the mass at q to be (R) and (r), instead of X_1, Y_1 and f_1, and thence deduce the expressions for the forces acting normally and tangentially on the line $q\,s$ (fig. 2). Calling these forces X and f, and considering (R) and (r) as *tensions*, we have

$$\begin{aligned} X &= (R)\cos^2\theta + (r)\sin^2\theta \\ &= \frac{1}{2}\{(R)+(r)\} + \frac{1}{2}\{(R)-(r)\}\cos 2\theta \\ &= \frac{1}{2}(X_1+Y_1) + \frac{1}{2}\sqrt{(X_1-Y_1)^2+4f_{\prime}^2}\cos 2\theta. \end{aligned}$$

But by equation (1.)

$$\cos 2\theta = \frac{X_1-Y_1}{\sqrt{(X_1-Y_1)^2+4f_{\prime}^2}},$$

therefore

$$X = X_1,$$

as it ought to be. Similarly,

$$\begin{aligned} f &= \{(R)-(r)\}\sin\theta\cos\theta \\ &= \frac{1}{2}\{(R)-(r)\}\sin 2\theta \\ &= \frac{1}{2}\sqrt{(X_1-Y_1)^2+4f_{\prime}^2}.\sin 2\theta \\ &= f_{\prime}, \end{aligned}$$

by substitution from equation (1.).

14. It should here be borne in mind that, in the preceding investigations, the internal tensions are supposed to be insufficient to break the continuity of the mass, and that, consequently, our results are independent of the law according to which the cohesion may vary from one point to another, the consideration of which only becomes necessary when we have to determine the directions of fracture. It is also important to remark that the preceding results are independent of the degree of relative displacements of different elements of the mass, the continuity remaining unbroken; it is not necessary that the displacements should be small. Again, it should be observed, that, to render these results absolutely accurate, we must suppose the line *p s* (fig. 2) of infinitesimal magnitude, but still containing a great number of molecules forming so many points of action on each side of this *line of separation.* This hypothesis would not be strictly applicable in the extreme limit, to a mass of which the perfect continuity should be interrupted by innumerable *pores* of sensible magnitude, in every part. If, however, *p s* be of finite length, so as to contain a great many points of contact, there will be no sensible error in our results as applied to a mass constituted as just mentioned, provided the resultant of the actions at the points of contact pass through a point of *p s* not deviating sensibly from its middle point. Thus if *p s* should be conceived to be several inches, or even several feet in length, our results

would be sensibly correct as applied to a mass of the enormous extent of a glacier, or of any considerable continuous portion of it.

15. Some of the preceding results may be still further elucidated. If $X_1 = 0$ and $Y_1 = 0$, the only force acting on an element of the mass will be $f_{,}$, and it has been shown (arts. 6. and 9.) that θ will then $= 45^\circ$. Let $p\,q\,r\,s$ be an element originally square. As already explained, it will be acted on by two couples of forces $f_{,}$, as represented in the figure. It is seen at once, that the greatest tension produced by these forces must be in a direction parallel to the diagonal $r\,q$, or supposing the deviation from rectangularity very small (for the reason assigned in the note, art. 9), in a direction making an angle of 45° with $p\,q$, as before proved. Also, it is easily seen that there will be compression in the direction perpendicular to $r\,q$, and likewise that the compression must be greater in that direction than any other. The maximum tension (R) and the maximum pressure $-(r)$ may be easily expressed; for the whole tangential force on $p\,q$ $= f_{,} . p\,q$; its resolved part parallel to $r\,q = f_{,} . p\,q . \cos 45^\circ$. Similarly, the resolved part of the force on $q\,s = f_{,} . q\,s . \cos 45^\circ$. Therefore

$$(R) p\,q . \sin 45^\circ = f_{,} . p\,q . \cos 45^\circ,$$

or

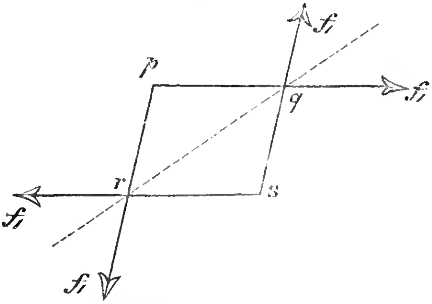
$$(R) = f_{,}.$$

Similarly, we obtain

$$-(r) = f_{,}.$$

These same values would be obtained from equations (3.) (art. 10.), putting $X_1 = 0$ and $Y_1 = 0$.

Fig. 4.

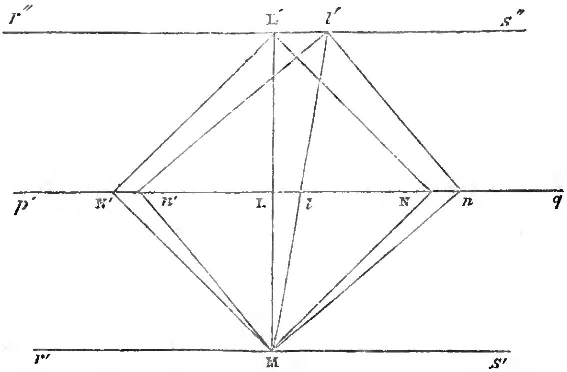


16. We thus see also how the two forces (R) and (r) produce the same effect on the element $p\,q\,r\,s$, in distorting it from its original rectangular form, as the tangential forces $f_{,}$. If, however, we should take a rectangular element whose sides were perpendicular respectively to the forces (R) and (r), it is manifest that no distortion would be produced in it by (R) and (r). In fact the tangential forces on such an element would $= 0$. And in all cases, if we take a rectangular element of the mass in its original state of no constraint, such that the sides of that element shall be perpendicular to the di-

rections of maximum and minimum tension, when the mass is brought into its state of internal constraint, then will the element still retain its rectangular form, its linear dimensions only being changed. Other elements about the same point will be more or less distorted from rectangularity, according to their angular positions.

17. These results may be well exhibited geometrically by an extension of the diagram, fig. 1. Take $M N'$ at right angles to $M N$, and complete the parallelogram $N N$; take $N' n' = L l = N n$ and complete the parallelogram $n n'$. Then, while the physical points at L, N and N' move respectively to l, n and n', that at L' will move to l', since $L' l' = 2 L l$, and the relative velocity at L' = twice that at L. Now if lines be drawn bisecting the angles $N M n$ and $N' M n'$, and these lines make angles of 45° with $p' q'$, it is easily seen that $N' M n'$ must $= N M n$. Consequently, the parallelogram $M L'$ being rectangular, $M n' l' n$ will be so likewise, and will therefore be an element of no distortion. If $N n$ be indefinitely small, $M n$ and $M n'$ will make angles of 45° with the direction of motion, and will therefore coincide with the directions of maximum and minimum tension (art. 5.).

Fig. 5.

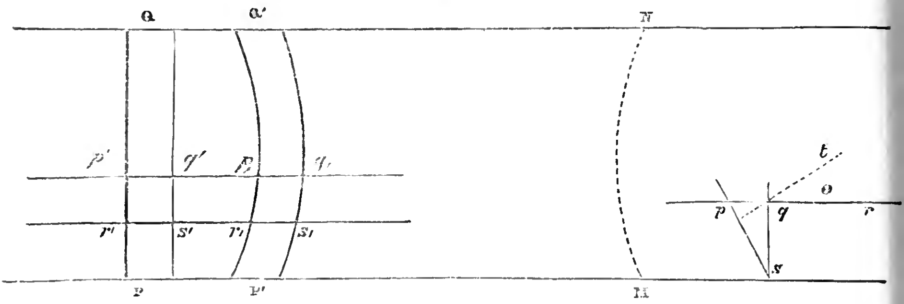


We also thus arrive at the conclusion that $M n$ and $M n'$ are directions in which there is no tangential force; for if there were any such forces acting on the element $M n' l' n$, it could not retain its rectangularity. All these deductions, by different methods, are in perfect harmony with each other.

If we assume, as the result of our previous investigations, that the directions of maximum and minimum tension are coincident with the lines of no tangential action, the geometrical construction given above will serve to determine those directions when the relative motion $N n$ is not small.

18. Let us now take the annexed diagram (fig. 6) to represent a glacier, or rather a horizontal section of it. Any transverse element P Q will, by the more rapid motion of the centre, be brought into the position P′ Q′, and the rectangular element $p' q' s' r'$ will assume the form $p_{\prime} q_{\prime} s_{\prime} r_{\prime}$, the continuity of the mass still remaining unbroken. This distortion is produced by the tangential force $(f_{\prime})$ impressed on the element in the directions $p_{\prime} q_{\prime}$ along that side, and $s_{\prime} r_{\prime}$ along the opposite side, together with equal tangential forces called into action along the other sides in the directions $p_{\prime} r_{\prime}$ and $s_{\prime} q_{\prime}$ respectively. The longitudinal tension will be represented by X_1 in the previous formulæ, and the transversal tension by Y_1, these quantities being negative whenever we have pressures instead of tensions. Hence, if at any point q we take $p\,q\,r$ in the direction of motion, and take $t\,q\,r =$ to θ as determined by equation (1.), $q\,t$ will be the direction of maximum tension, and a line perpendicular to $q\,t$, that of maximum pressure at the point q. They will also be the lines of no tangential force.

Fig. 6.



19. *Formation of Surfaces of Discontinuity in the Mass of the Glacier.*—In the previous investigations the continuity of the mass has been supposed to remain unbroken; let us now suppose the internal actions to become greater than the cohesion, and the continuity, consequently, to be broken. This breach of continuity may be produced in two ways; by the *normal force* (R), which measures what is properly called the *tension*, or by the *tangential force* (f). Open fissures will result in the first case, and merely surfaces of discontinuity in the latter. The directions of fracture may depend on the cohesion of the mass as well as on the internal forces to which it is subjected, but it is important to observe that they will depend on the internal forces alone, however the cohesion may

vary in passing from one point to another, provided it be the same at each point for every direction through that point. This is equivalent to there being no *surface of less cohesion* through any proposed point; for, in such case, it is manifest that the direction in which a fracture would begin or be continued through any point, could not be influenced by a cohesion which should be the same for all directions. If, however, surfaces of less cohesion exist in the mass (*i. e.* if its cohesion vary according to any discontinuous law), they may evidently affect the directions of fracture. This case I shall reserve for subsequent consideration, restricting myself in the first instance to cases in which the variation of cohesion is continuous.

20. Let us first consider the formation of open fissures.

It has been proved (art. 13.) that whatever forces may act at any point of the mass in its state of constraint, they must be equivalent to two systems of tensions whose intensities are (R) and (r), and whose directions are perpendicular to each other. If, therefore, an open fissure commence at any proposed point, it must clearly be in a direction perpendicular to that of the greater of these tensions, or the maximum tension at that point. The directions in which the fissure will be subsequently propagated through other points may be, in some degree, affected by other circumstances, into the explanation of which it would be useless to enter here*; but those directions will be determined approximately by the same rule as at the first point. Hence, then, the normal to a fissure at any proposed point may be considered to coincide approximately with the direction of maximum tension at that point just previous to the fracture.

Let us first suppose the glacial valley to become narrower in descending, as is very commonly the case; there will doubtless be a *transversal compression*, or Y_1 in equation (1.) must be negative. Also, it is probable that X_1 will more frequently be a *tension*, or, if it be a pressure, it will probably be much less than Y_1. We shall thus have

$$\tan 2\theta = \frac{2f_1}{Y_1 \pm X_1},$$

the denominator being positive. Y_1 and X_1 may be taken as nearly constant for the same transversal section of the glacier, while f_1 will vary from its greatest value near the sides, where the tendency to *twist* each element of the mass is greatest, to zero at the centre, where there is no such tendency at all. Consequently θ will be the greatest at the sides of the glacier, and will diminish for points nearer to the centre, or the line

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* See my memoir on Physical Geology, in the Transactions of the Cambridge Philosophical Society, vol. vi. part 1.

of fracture will be a *curve line*. Also its convexity will be turned towards the upper extremity of the glacier; for, by referring to fig. 5, it will be seen that the point in which the normal to the curve of fracture meets the axis of the glacier, lies towards the *lower* extremity of the glacier. The greatest value of θ, in the case before us, must be less than 45°, whatever may be the value of f_l, and may be much less than that quantity, if $Y_1 \pm X_1$ be considerable. The general form of the curve of fracture is represented by M N (fig. 6), supposing the fissure continued across the glacier.

It is here deserving of remark, that the force (R) may be sufficient to produce transverse fissures, though there may be no longitudinal tension such as that denoted by X_1, provided f_l be great enough; for, if we put $X_1 = 0$ in the expression for (R) (art. 10.), we have

$$(R) = \frac{1}{2}\{\sqrt{Y_1^2 + 4f_l^2} - Y_1\}.$$

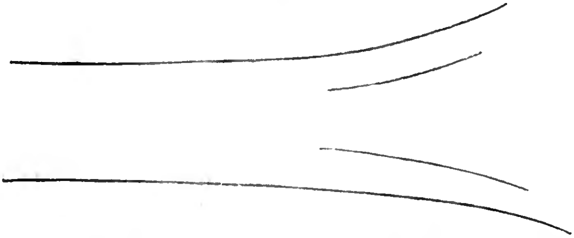
Consequently there may still be fissures due to (R) near the flanks of the glacier, but there can be no transverse fissures in the central portion, where f_l must necessarily be very small. The existence of transverse fissures in this latter portion is as distinctly indicative of longitudinal tension as the absence of longitudinal fissures is indicative of transverse compression.

21. If the width of the valley should increase instead of decreasing, at any part, we shall obtain a different result, assuming what I conceive to be almost universally true,—that when the width of valley increases, its fall diminishes. In such case the portion of the glacier in the more expanded part of the valley will be pressed upon by the mass immediately behind it, tending to descend with greater rapidity. The longitudinal action will then become a pressure instead of a tension, or X_1 will become negative, and may be great. We shall then have

$$\tan 2\theta = -\frac{2f_l}{X_1 \pm Y_1}.$$

Hence, assuming Y_1 to be a tension, or, if a pressure, smaller than X_1, tan 2θ will be small and negative at points not remote from the centre of the glacier; and, therefore, θ will be either a very little less than 90°, or will be very small and negative. The former value will in this case manifestly correspond to the direction of greatest tension, and consequently, if a fissure be formed, it will tend to *parallelism* with the axis as it approaches the centre of the glacier, making a larger angle with the axis at points more remote from it, since f_l will then be greater. Thus the fissure will become *longitudinal* instead of transverse, as represented in the annexed diagram. This is the law recognised by Prof. Forbes in cases such as that now considered. The form of the curves of fissure deduced in the first case agrees with that recognised by all observers.

Fig. 7.



22. If the mass be so constituted as to present internal planes or surfaces of *less cohesive power*, the only case we need consider is that in which these planes or surfaces are very near and parallel to each other. Such a system may possibly be formed by the alternate bands of blue and white ice described by Prof. Forbes, and appears to be the only one of which there is any indication. If the direction of any surface of such a system, through any proposed point, approximate sufficiently to perpendicularity to the line of maximum tension for that point, it is manifest that the fissure will be formed along the surface of less cohesion; but if, on the contrary, this surface approximate sufficiently to the direction of maximum tension, it is easily seen that it will have no influence whatever on the direction of fracture. Now, according to the observations of Prof. Forbes, the blue bands are very approximately perpendicular to the transverse fissures, and therefore nearly coincident with the lines of maximum tension; consequently those bands, allowing that they form surfaces of less cohesion, can exert no material influence on the formation of the transverse fissures.

23. From this investigation we may also see distinctly by what action the lateral parts of a glacier are made to expand with an expanding valley; for the directions of greatest pressure being perpendicular to those of greatest tension, they must be parallel to the fissures represented in fig. 7, and therefore this greatest pressure must necessarily force out the flanks of the glacier and cause the lateral expansion here spoken of.

24. *Internal state of tension or pressure of the Mass after the formation of transverse fissures.*—Since the fissures will be perpendicular to the direction of the maximum tension (R), and therefore parallel to that of (r), it is manifest that (R) will

be destroyed at all points not remote from each fissure, while the minimum tension or maximum pressure (r) will remain the same. Hence, if at any such points we would find the values of the internal forces, we must refer to the equations at the end of article 13, which become, if we write F for X, and $-(r)$ for (r) (since the transverse force is here a *pressure*),

$$F = (R)\cos^2\theta - (r)\sin^2\theta,$$
$$f = \frac{1}{2}\{(R) + (r)\}\sin 2\theta,$$

where F denotes the normal and f the tangential force before the continuity of the mass is broken, on a line the normal to which makes an angle θ with the direction of maximum tension. In applying these equations after the formation of the fissure we must put $(R) = 0$, which gives

$$F = -(r)\sin^2\theta,$$
$$f = \frac{1}{2}(r)\sin 2\theta.$$

Consequently, if we take a portion of the mass between two fissures not remote from each other, it will be in a state of *pressure* (since F is negative) in every direction except that perpendicular to the fissures, in which there will be neither pressure nor tension. Such portion of the mass may therefore be said to be entirely relieved from its state of *tension*. The tangential force f will remain, but diminished in a ratio depending on that which (r) bears to (R).

When the different portions of the mass are thus far relieved from internal constraint, the motion originally producing that constraint may be continued without causing immediately any further fracture; but by the continuance of the motion, the constraint must necessarily be again increased as at first, till the tension shall again become greater than the cohesion, when new fissures will be formed in the same manner as before, while the old ones, as they become transverse to the lines of greatest pressure by the more rapid motion of the centre of the glacier, will be gradually closed; and thus this opening of new fissures and closing of old ones may be conceived to go on for any length of time, independently of local causes constantly acting at particular points, and which, acting in conjunction with the general cause above considered, continually produce systems of fissures originating at those points, and deviating more or less from the general type which would be impressed on them if they were due solely to the more rapid motion of the centre of the glacier.

Hence we may understand how the centre of a glacier may move more rapidly than its sides, without actually producing longitudinal planes of discontinuity, such that the portion of the mass immediately on one side of such plane shall *slide* past the contiguous portion on the opposite side without forming open fissures like those above considered. But though this is not a necessary consequence of the state of constraint of the mass, it is one of the ways, as already stated, in which the constraint *may* be destroyed. I proceed to consider the conditions under which this will take place.

25. If we take a geometrical line along the surface of a continuous portion of a glacier, the tendency to form an *open fissure* along that line will be measured by the intensity of the force which is normal to it; and in like manner the tendency to form a vertical plane or surface of discontinuity along the line will be measured by the tangential force f. If $f = 0$ along the proposed line, there can be no tendency in contiguous particles on opposite sides of this geometrical line to separate from each other by different velocities estimated along the line, since such a tendency must necessarily generate the tangential force (f) in question, and the greater the tendency the greater will be the force. Consequently there must be the greatest tendency to form these planes of discontinuity in those directions in which the force f is a maximum or minimum*. These directions are given by equation (4.) (art. 11), and are represented in diagram 3 of the same article. They are perpendicular to each other, and, in all cases, make angles of 45° with the directions of (R) and (r), those of maximum and minimum tensions. In these latter directions the values of f are always zero.

Hence, if there be no planes of less cohesion in the mass, and its cohesion should give way to the tangential force, before it yields to the normal tension, it must be along one of the lines for which f is a maximum or minimum, *i. e.* along a line inclined at an angle of 45° to the direction in which an open fissure would be formed, if formed at all, by the normal tension (R). If $X_1 = 0$ and $Y_1 = 0$, one of these directions will coincide with that of the axis of the glacier, to which the other will be perpendicular. Consequently there will be an equal tendency to fracture the mass in the way we are now considering, longitudinally and transversely. If the mass, instead of being constituted as just supposed, have a system of parallel planes of less cohesion, each fracture will take place along some one of such planes, provided they approximate sufficiently to those directions in which the fractures would take

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* It must be recollected that the minimum only differs from the maximum value of f in *sign* and not in *magnitude*. Consequently there will be the same effort in both cases to overcome the cohesion of the mass.

place in a homogeneous mass; but if, on the contrary, the planes of less cohesion approximate to those directions in which $f=0$ (*i. e.* to those perpendicular to the open fissures), they will have no influence on the directions of the lines of fracture, for such fractures must either be formed exactly along the lines of less cohesion, or in the directions in which f is the greatest.

26. It may at first sight appear paradoxical that there should be the same tendency to fracture the mass by one particle sliding past another in a transversal as in a longitudinal direction; it is, however, unquestionably true. The difference between the two cases does not consist in any difference of conditions previous to fracture (or, therefore, in the tendency to produce fracture), but in the motion which may subsequently take place in the respective directions. The relative motion of particles on opposite sides of the transversal plane of fracture, will be limited to that which is just necessary to relieve the previous constraint of the mass; while the relative motion subsequent to a longitudinal fracture may be continued to any extent by the continued tendency of the mass on one side of such fracture to move faster than that on the opposite side.

Whether a continuous portion of ice of any considerable extent be ever fractured in the manner last considered, is uncertain, for no accurate observations have yet been made on the subject; but there can be no doubt that, in those portions of a glacier where it is much dislocated, the sliding of one portion past another must frequently take place. Such is doubtless the case along the dislocated flanks of glaciers, and thus the motion of the central part is much facilitated. If a glacier, continuous throughout its whole extent, could be formed, and then subjected to the existing conditions of glaciers, I have no doubt that the first fractures would be *transversal*, produced by the tension (R); and that after the mass had thus become considerably dislocated along its sides, the sliding of one finite portion past another would also take place; after which any internal constraint which might be superinduced by the peculiar motion of the glacier, would be relieved both by transverse fracture and by the sliding of one part past another.

27. If a column of glacial ice, of which the unextended length should $=l$, would just bear to be stretched to the length l' before it would break, the ratio $\frac{l'-l}{l}$ might be taken as a measure of the *extensibility* of glacial ice. If the extension should be produced within the comparatively short time of making an ordinary experiment of the kind, this ratio would, I think, unquestionably be very small; but if the extended

piece of ice should form a portion of an actual glacier, where the extension may be produced so slowly as to be continued for a number of years without producing fracture, it is very conceivable that the above ratio might become very much greater than in the former case. I have explained how the tension of the mass will be relieved in the direction of maximum tension by the formation of fissures. If the *elastieity* of the ice were *perfect*, a physical line in that direction would return to its original unstretched length, but will otherwise remain permanently elongated in a degree depending on its want of perfect elasticity. In a similar manner another line may become permanently compressed, and thus, by a repetition of the same processes, the linear dimensions of an element of the mass may become much altered in the course of a long period of time. In like manner a rectangular element may, in a long period, very slowly change its form so as to deviate very considerably from rectangularity without fracture. Thus it may happen that the flanks of a glacier may become dislocated with a much less degree of extension than the central portion, because the extension takes place much more rapidly near the sides than near the centre. As an example we may take the results of observations made by Prof. Forbes on the Mer de Glace. He ascertained the velocities at four stations in a transverse line, the first station being near the side of the glacier, and the second about 100 yards, and the third about 230 yards from the first; the fourth was near the centre of the glacier. The velocities were in proportion to the following numbers:—

A	B	C	D
1·000	1·332	1·356	1·367.

Consequently the *mean relative velocity* between A and B may be taken as $=\cdot 332$, and that between B and C

$$= (1\cdot 356 - 1\cdot 332)\,\frac{100}{130},$$

(since the distance B C = 130 and A B = 100)

$$= 0\cdot 2 \text{ nearly}.$$

The distance between C and D is not given, but assuming it to be nearly the same as between B and C, the mean relative velocity between C and D would be less than ·01, *i. e.* only about one-fortieth of that between A and B. Now, in the investigation of articles 4 and 5, $\frac{\beta}{a}$ is proportional to the relative velocity, and therefore the maximum extension, which $=\frac{\beta}{2a}$, is proportional to the relative velocity. Consequently, in the case before us, the extension would increase forty times faster between the first and second stations than for a considerable space near the centre of the glacier; and if the extreme flanks were taken, the ratio would probably be still considerably greater. Thus, if the glacier at the present time were perfectly continuous, the flanks would probably be dislocated by transverse fissures in a few months, while the continuity of the central portion, independently of the action of local causes, might be preserved for many years, even if it should break with an extension no greater than that required to fracture the flanks. If also it be allowed that the extensibility becomes greater as the extension is produced more slowly, the glacier might move on for a still greater number of years without central dislocations due to the more rapid progression of its centre.

If this greater extensibility of glacial ice during a long period, or what may be called its *secular extensibility*, be allowed, we must also allow it to possess the property of *secular plasticity*, by which I mean that plasticity which shall require for its development the continuous application of force, possibly of great force, for a long period of time, in contradistinction to that plasticity by which a mass submits to an immediate and considerable change of form by the application of forces of comparatively small intensity. This latter kind of plasticity, when sufficiently great, becomes *semifluidity*; and if the observed phænomena of motion in a case like that of a glacier should justify the conclusion that the mass possesses this high degree of plasticity, we might be justified in asserting that the effectiveness of gravity to put the mass in motion was due to that property. But if, on the contrary, the observed circumstances of the motion only establish the existence of secular plasticity, and when, moreover, the motion is always accompanied by numerous and repeated fractures, we have no right thence to conclude that the motion is effectively due to the plasticity of the mass. I shall recur again to this point in the sequel.

28. With respect to the conclusions I have drawn from the preceding analysis of the problem of glacial motion, it is important to remark how entirely they are independent of the experimental determination of the values of the *constants* which afford us measures of such properties, for instance, as the elasticity, the extensibility, &c. of glacial ice; and also of all arbitrary constants and functions introduced into analytical results by the integration of the fundamental differential equations of the problem, and requiring for their determination a knowledge of the conditions of each particular problem. My

conclusions have, in fact, been drawn from the conditions of equilibrium of an *element* of the mass, conditions, the analytical expression of which is only equivalent to the differential equations of the problem. The quantities involved in the preceding results are X_1, Y_1 and $f_{\prime}$. The directions in which the couples depending on $f_{\prime}$ act is determined by the observed fact of the central being greater than the lateral motion; the facts of the forces X_1 and Y_1 being pressures or tensions are inferred, and without any risk, as I conceive, of essential error, from the form and inclination of the glacial valley. And these are the points on which our conclusions depend; they are in a great degree independent of the actual values of the above forces. It would seem impossible to draw any accurate conclusions depending on less ambiguous circumstances. It may, however, be remarked that the whole investigation is tacitly based on the assumption of the continuity of the mass being preserved in the same sense as in the ordinary investigations of the motion of fluids. In the case before us the assumption has been, that, while a continuous mass having elasticity changes its form by the application of external forces, the particles which constitute an element of the mass, such as $p'q'r's'$ (fig. 6), in its original state of no constraint, continue to constitute a separate element, $p_{\prime}q_{\prime}r_{\prime}s_{\prime}$, during the whole change of form up to the state bordering on fracture. It is the common assumption on which all mathematical investigations of this nature are founded, and it is one which appears to me to possess the strongest *à priori* claims to our confidence, except under particular conditions with respect to the constitution of the mass. But I must reserve any further observations on this point for my next communication, in which, after this detailed exposition of my own views of the mechanics of the problem, I shall have some comments to make on those of Prof. Forbes.

I am, Gentlemen,
Your obedient Servant,
W. HOPKINS.

Cambridge, January 6, 1845.

Erratum in Mr. Hopkins's *First Paper.*
Page 15, line 2, *for* $\omega \sin \alpha$ *read* $\omega (\sin \alpha - \sin \beta) = \omega \sin \alpha$.

XVI. *On the Ventilation of the Coal-Mine Goaf.*
By MICHAEL FARADAY, *Esq., D.C.L., F.R.S.*

To Richard Taylor, Esq.

DEAR SIR,

YOU have honoured the Report by Mr. Lyell and myself with a place in the Philosophical Magazine, p. 16, and this induces me to send for insertion also, certain considerations which have occurred to me since the Report was written, and also some practical results which were brought generally before our members here at the last Friday Evening Meeting. I need hardly say that the Report proposes to draw away the lower aërial contents of the goaf by an iron pipe laid down in one or other of the ways of the mine, and either entering into the return way, or having a fanner or bellows or other blowing apparatus upon it. The points I wish to speak to now are, first, the draught, and next the nature and place of the pipe.

By experiments which I have made with a small furnace, flue and pipes of 6 inches diameter and less, I am quite satisfied that such a draught as that of the return at the Haswell mine would be sufficient to effect that which we propose in the Report without the use of any extra blowing or withdrawing apparatus, so that the plan is so far relieved from the necessity of keeping a man or boy working at such a machine.

With regard to the pipe, I think that instead of laying it down in the flue of the mine, it had better be hung up or sustained upon props in the open space of that way or passage which may be chosen for its direction. If then any derangement of position occur it can easily be remedied. I have had pipes 6 inches in diameter, made both of air-proof cloth and common sheet-iron; the former were kept open by whalebone rings run round them at equal distances of 2 feet, and answered in my trials exceedingly well. Square trunks, also made by nailing four boards together with copper or iron nails, are easily available as tubes. Such tubes it may be said when placed as proposed in the air would easily be deranged by falls. No doubt a fall might destroy a part of the tube, but if it did there seems to be no great difficulty in restoring it; and further, if a judicious selection were made for the direction of the tube, there appears no reason why the roof over it cannot be as well and securely propped up as the roof of the mothergate, the rolley way, or any other important part.

Finally, it is not necessary, on the principle proposed, that the goaf end of the tube should always be at the very extremity of the goaf towards the rise, but only that it should be 3, 4, or more feet above its upper edge; so that a jud or two may sometimes be drawn in advance before the goaf end of the pipe need be readjusted. Apparently there can be no difficulty in selecting the place of the goaf end of the pipe so that there shall be no interference with the general plan upon which the coal itself is worked.

I am, my dear Sir,
Your faithful Servant,
Royal Institution, January 20, 1845. M. FARADAY.

XVII. *On Glacier Theories.* *By* Dr. Whewell.

To Richard Taylor, Esq.

Dear Sir,

I SHALL be obliged by your allowing me to make a remark or two in your pages on the subject of glaciers; which subject has been recently discussed both there and elsewhere. In the course of these discussions, expressions have been let fall, as if there were some particular care requisite to avoid confusion with regard to notions generally considered very elementary, as *rigidity*, *solidity*, *flexibility*, *fluidity*. I will therefore, at the risk of provoking a smile from your mathematical readers, begin with distinguishing these. *Rigid* bodies are absolutely incapable of any motion of their parts among each other. To these are opposed *flexible* bodies, in which the parts move without changing their attachments; and *fluid* bodies, in which the parts have no attachment to each other, and move amongst each other freely. Flexible bodies are *elastic*, when they tend to resume their shape. Lines and developable surfaces may be flexible without being elastic; but a flexible solid must be elastic, for when it is bent, some of its dimensions must be lengthened, or shortened, or both. Fluids are *imperfectly* fluid when their parts have a slight attachment which requires some force to overcome it. In this case, when a particle is detached from its original companions, it attaches itself, with a like force, to a new set among whom it is drawn. Such bodies are also called *viscous* or *plastic*, these terms implying various degrees of force in the attachment of the particles. And when this force is so great as not to be easily overcome, the body *may* be termed *solid*, as opposed to fluid; but in this sense *solidity* is merely a comparative term, and cannot be properly opposed to *rigidity*, which is an absolute one. Flexible and viscous are very clearly distinguished, for in viscous bodies the particles slide *past* each other, though not freely; in flexible bodies they move *round*, but not past each other. A block of caoutchouc is flexible; if melted, it is viscous; and this, however little it be melted.

This being premised, we shall have no difficulty in comparing the results of supposing the ice of glaciers flexible and viscous. Let an alpine sloping valley be filled with a vast mass of solid caoutchouc, the slope and friction being such that the mass slides slowly downwards. What will be the condition of the mass? Plainly the sides and bottom will be held back by friction; the middle and upper parts will drag forwards. The straight lines originally transverse will become curves, with the convexity downwards. The whole mass will be in a state of tension, produced by this distortion; but the distortion will be limited, and the downward projection of the curved lines also limited, by the flexure which is possible for the texture. If in any part the texture gives way, we shall have fissures nearly perpendicular to the transverse curves. On this supposition of a flexible glacier, we have (1) original transverse straight lines drawn into curves of *limited flexure*; (2) the substance in a state of distortion, from which *hand specimens* would instantly recover; (3) a *homogeneous* texture of such specimens.

Now let this mass of caoutchouc become viscous or plastic. What will now be the condition of the mass? The sides and bottom will still be held back by the friction; the middle and upper part will slide forwards, but not freely. This want of freedom in the motion (arising from the viscosity) will produce a drag towards the middle of the valley, where the motion is freest; hence the direction in which filaments slide past each other will be obliquely directed towards the middle. The sliding will separate the mass according to such lines; and though new attachments will take place, the mass may be expected to retain the results of this separation in the traces of parallel fissures in this direction. If the texture of the substance yield to the tension which tends to separate it directly, we shall have fissures in directions perpendicular to the directions just described, and therefore convex upwards. Original transverse straight lines will become curves, projecting downwards; and the projection will increase without limit as the sliding goes on. Hence, on this supposition of a viscous or plastic glacier, we should have (1) original transverse straight lines drawn into curves of *unlimited* flexure; (2) the texture of the substance in a state of equilibrium, so that *hand specimens* have no distortion to recover from; (3) a *banded* texture, the bands being traces of parallel fissures.

It is well known that the latter set of phænomena agree with the actual phænomena of glaciers, and the former set differ from them. To some persons it may seem harsh to call the mass of a glacier plastic or viscous, when it seems *so* solid. It may be said that we can hardly call a mass viscous, when we have in it cliffs perhaps a hundred feet high. But let it be considered, as I have said, that the difference of plastic and *very* solid is only a difference of degree. Soft pitch will stand in cliffs some inches high; soft clay will stand in cliffs many feet high; clay may stand in cliffs hundreds of feet high, and yet be plastic, if the mass be very large and the pressures distributed through it powerful enough to make one part move past another part. We cannot doubt that clay might be hard enough to stand in such cliffs, and yet soft enough to slide down a sloping valley as a plastic substance, if the valley were filled with it for many miles long and hundreds of feet thick; and still more, if there were streams of water running through all parts of the mass.

I will only make one other remark. Since the banded structure is, as we have seen, *an experimentum crucis* between the flexible and the viscous theory, one of the ways in which the application of mathematics to this subject may be really useful, is in determining the form which such bands would assume in the motion of a viscous mass. The problem may be resolved into two: (1) an imperfect fluid slides down a sloping canal; find the form of the lines of separation on the upper surface; (2) on the same supposition, find the form of the lines of separation in the medial vertical plane.

I, and I think many other persons, will be very grateful to any mathematician who will help us to the solution of these problems.

I am, Mr. Editor,
Your faithful Servant,
W. WHEWELL.

Trinity College, Cambridge,
January 21, 1845.

XVIII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxv. p. 315.]

November 8, 1844.—G. B. Airy, Esq., President, in the Chair.

THE Secretary read the following extract from the Minutes of Council, dated September 20, 1844:—

"The office of President having become vacant since the last Meeting of the Council, in consequence of the death of Mr. Baily, it was moved by Mr. Sheepshanks, seconded by Mr. De Morgan, and Resolved unanimously,

That Mr. Airy be appointed to fill the office of President of the Society, until the next Annual General Meeting.

Resolved also,

That Mr. Christie be appointed Vice-President in the room of Mr. Airy, until the next Annual General Meeting."

Among the presents received since the last Meeting of the Society the following were announced:—

I. An equatorial telescope, by Smeaton, presented by Mrs. Somerville, and accompanied by the following note:—

"September 12, 1844.

"Sir,—May I request you will have the goodness to present to the Royal Astronomical Society, in my name, an equatorial, which was made by the late Mr. Smeaton the engineer, and left to me by his daughter, Mrs. Dixon, at her death, who had frequently told me that it was her father's invention, and the first instrument of the kind that had been constructed?

"I remain, Sir,
"Your obedient Servant,
"MARY SOMERVILLE."

"*Thomas Galloway, Esq.,*
Secretary."

II. A cast taken from Sir Francis Chantrey's marble bust of Mrs. Somerville, presented by Frederick S. Archer, Esq.

III. Several copies of the "Plane Tables for conveniently laying down Portions of a Celestial or Terrestrial Map," constructed by the late Rev. Francis Wollaston, presented by his daughter, Mrs. A. H. Wollaston.

The President announced that the office of Judge (for Great Britain) of the fulfilment of the conditions necessary for the award of the Comet Medal, offered by His Majesty Christian VIII., king of Denmark, having become vacant, His Majesty had been pleased to appoint him (the Astronomer Royal) to succeed to that office, and he read to the Meeting the conditions under which the medal would be granted, from the Monthly Notice, vol. iii. p. 132*.

For the convenience of Fellows and others who may not have the volume of the Monthly Notices at hand, the regulations are here reprinted, with the alterations which have been made in consequence of the death of Mr. Baily and Dr. Olbers of Bremen:—

"His Majesty the King of Denmark has been pleased to found a gold medal, of the value of twenty ducats, to be given to the first discoverer of a telescopic comet, subject to the following conditions, which are, in some respects, different from those published in the year 1832†.

"1. The medal is to be given to the person who may first discover a telescopic comet (that is, a comet not visible to the naked eye at the time of its discovery), and not of known revolution.

"2. The discoverer, if in any part of Europe except Great Britain, must send *immediate* notice to Professor Schumacher, of Altona; and if in Great Britain, or any other quarter of the globe except Europe, must send *immediate* notice to G. B. Airy, Esq., Astronomer Royal, Royal Observatory, Greenwich.

"3. Such notice must be sent by the *first* post after the discovery, and in case no post should be established in the place, then by the *first* conveyance that presents itself, without waiting for more observations. A strict attention to this condition is absolutely necessary, for when it is not complied with, the medal will not be awarded at all, if there be only one who has seen the comet; and, where it has been seen by more than one, it will be given to the discoverer next in order of time who does comply with this condition.

"4. The first notice should contain, not only the time of the discovery, as nearly as the same can be ascertained, in order to avoid any disputed claims, but also the best possible determination of the position of the comet, and the direction of its course, if these points

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* Reprinted in Phil. Mag. S. 3. vol. ix. p. 294.

† Given in Phil. Mag. S. 2. vol. xi. p. 155.

can (even approximately) be ascertained from the observations of one night.

"5. If the first night's observations are not sufficient to determine all these points with sufficient accuracy, the discoverer must, as soon as he gets a second observation, send *another* communication as above directed, together with a statement of the longitude of the place, if it should not be a known observatory: but the hope of getting a second observation will not be admitted as an excuse for delaying the communication of the first.

"6. The medal is to be adjudged twelve months after the discovery of the comet, and no claim can be admitted after that period has elapsed.

"7. Professor Schumacher and Mr. Airy are to determine whether a discovery is to be considered as established or not: but should they differ in opinion, Professor Gauss, of Göttingen, is to decide between them."

The President then announced that, in pursuance of a resolution of the Council, which had been duly intimated to the Fellows, as required by the Bye-Laws, the business of the Ordinary Meeting would now be concluded, and that a Special General Meeting would be held immediately, for the purpose of hearing read a Memoir of the late President, which Sir John Herschel, upon the request of the Council, had undertaken to prepare, and which will be found in the last Number, p. 38.

XIX. *Intelligence and Miscellaneous Articles.*

M. MATTEUCCI ON ELECTRO-PHYSIOLOGY.

THE following are some of the most important of the experiments detailed in M. Matteucci's recently published work on Electro-Physiology, which were shown by him to several members of the British Association at York in September last:—

1st. *The Muscular Current.*—If the sciatic nerve of the limb of a prepared frog be made to touch at the same time the external and internal muscle of a living or recently-killed animal, the limb is convulsed. By forming a series of external and internal muscles, for instance, severing the lower halves of the thighs of a certain number of frogs, and inserting the knee of the one into the central muscle of the second, and so on, a voltaic pile will be formed, six or eight elements of which M. Matteucci showed were capable of deflecting a galvanometer, or producing convulsions in an electroscopic frog.

The direction of the voltaic current is from the interior to the exterior of the muscle, and the current is more feeble in proportion as the animal is higher in the scale of creation.

2nd. M. Matteucci explained *the specific voltaic current* (*courant propre*) *of the frog* as being a current which is detected only in the frog, and which is directed from the feet to the head of the animal.

3rd. M. Matteucci showed an experiment by which it appeared that a muscle whilst undergoing contraction is capable of exciting the nerve of another recently-killed animal, so as to produce muscular contraction in the latter. He laid the sciatic nerve of one leg of a prepared frog on the thigh of another, and by touching the nerve of the latter with an arc of zinc and copper this was convulsed, and at the same time the first leg, the nerve of which formed no part of the voltaic circuit, was simultaneously convulsed, the legs all moving as though they formed part of the same animal.

4th. M. Matteucci explained some joint researches of himself and M. Longet, by which it was proved that a different galvanic result is produced upon the nerves of an animal at a certain period after death, if the current acts upon the nerve of motion, or centrifugal nerve only, from that which ensues if the mixed nerve, centrifugal and centripetal, be subjected to the current; in the former case the muscular contraction takes place at the interruption of the direct current, or that which passes from the nervous centre to the extremities, and the commencement of the inverse current, or that which passes in the opposite direction; while in the latter case the reverse effect obtains, the contraction taking place at the commencement of the direct and at the interruption of the inverse current.

M. Matteucci also communicated to the Chemical Section the results of some experiments made by him with the view of establishing the relation which the amount of mechanical work realized by the consumption of a given quantity of zinc acting as a voltaic combination upon the limbs of a frog, bears to the amount of work realized by the same quantity of zinc employed as a generator of mechanical force in other inorganic applications. A given weight is attached to the feet of a recently-prepared frog, this and the weight are suspended from a platina wire by a portion of the spine, and another platina wire passes through the lower part of the sciatic nerves, these wires are connected with the terminals of a voltaic battery, a voltameter being interposed in the circuit.

By making and breaking voltaic contact, the muscles contract, and the weight is raised.

By connecting a contact breaker with the moving limbs, these are enabled to interrupt and complete the voltaic circuit by their own contractions, and a register attached shows the number of interruptions in a given time.

An index is also attached to the weight, which bearing upon a revolving sooted disc, registers the distance and velocity of the motion of the weight. Thus we get the elements of time, space, and weight. From experiments performed in this manner M. Matteucci finds that 3 milligrammes of zinc consumed in twenty-four hours gives 5·5419 kilogrammes of weight raised through a given space, while the same quantity of zinc, or its equivalent of carbon, employed to generate motion by combination in a steam-engine gives $0^{k}\cdot834$; or employed to work an electro-magnetic machine, gives $0^{k}\cdot96$.

Several reductions must be made to eliminate extraneous actions which do not contribute to the resulting effect; thus a voltaic battery of sufficient intensity to decompose water must be much more powerful than is requisite to convulse the limbs of the frog. The conducting power of the pelvis muscles, which if cut off weaken too much the general effect, must also be deducted, as well as the antagonist force of the extensor muscles. The necessity for all these reductions makes the problem a very complex one. M. Matteucci believes, however, that he has done sufficient to establish the general result, that a far greater amount of work can be realized from the consumption of a given quantity of zinc acting on the limbs of a recently-killed animal, than when the same quantity is employed to work an inorganic machine.

NEW APPLICATION OF ELECTRICITY TO SURGERY.

A novel application of electricity has been described by Mr. Smee at his lectures on Surgery, delivered at the Aldersgate Street School of Medicine. He states that needles and other steel instruments are frequently impacted in the human body, and do irretrievable mischief, but may be detected by rendering them magnetic. The following extracts will sufficiently explain the *modus operandi*:—

"You are all acquainted with the curious condition which steel assumes under certain circumstances, whereby it evinces properties which are called magnetic; you know, moreover, that like magnetic poles repel, and opposite attract each other. You have, therefore, but to render a piece of enclosed steel a magnet, and you will be able not only to ascertain its presence, but to determine by its polarity its general direction; and by the amount of magnetism it evinces, you may even infer its probable bulk.

"When you suspect the presence of a piece of needle, or other steel instrument, you must subject the suspected part to a treatment calculated to render the needle magnetic; and there are two principal methods by which this object may be effected;—the first, by transmitting a galvanic current, at right angles, to the suspected part; the second, by placing a large magnet near the part affected, so that the object may be magnetized by induction. You may accomplish the first end, by taking a copper wire, covered with cotton, or still better with silk (in fact, you may employ the covered wire as generally used for the formation of electro-magnets), and wind it round the parts suspected to contain steel, several times, so that the same current may act at right angles, many times, upon the piece of steel; you may then take a galvanic battery (one of my little tumbler batteries will amply suffice), and connect one end of the wire to the zinc, the other to the platinized silver. The current might be continued for half an hour, or more, when the steel would become magnetized, and thereby give strong indications of its presence.

"For my own part, I should use the second plan, or the plan of magnetizing by induction, to render the needle magnetic. For this purpose I have employed a temporary electro-magnet, which I magnetized by the voltaic battery, and you will find, that by keeping the part affected as close as possible to the instrument for about half an hour, you will sufficiently obtain the desired object.

"The electro-magnet might be made of the horse-shoe form, if we knew the direction of the object; but in that case we should not require its use at all, as the proof of the existence of the needle is our only aim. I have used the horse-shoe magnet, but should prefer, in most cases, an electro-magnet like this, made for me by Messrs. Horne of Newgate Street, which is made of a simple straight bar of soft iron, wound round with wire. Your chemical lecturer has, doubtlessly, made you aware that the magnetic effect is proportionate to the power of the battery, so that if you were desirous of producing but slight effect, you would employ this tumbler battery; but if you required the action to be manifested at a greater distance, you would use a compound battery, such as this trough battery upon the table. The compound battery will magnetize a needle, in conjunction with the electro-magnet, in the space of two or three minutes. A powerful permanent magnet would answer as well as the temporary magnet; but it would be very expensive, and not so constantly at hand. When soft iron is impacted in any part of the body, we do not require either the electro- or permanent magnet, for on this substance we are unable to confer magnetic properties.

"To test the existence of a magnet within the body, we may take a magnetized sewing needle, and suspend it by a piece of silkworm's silk, when it will exhibit certain phænomena upon the approach of the suspected part, provided it contain a piece of magnetized steel. Although this simple contrivance will amply suffice, I myself possess a needle which was made for me by Messrs. Willats of Cheapside, and which is well-adapted for the purpose.

"It consists, as you perceive, of a delicate needle, about six inches long, centred upon a small agate cup, resting upon a steel point; so that the smallest possible amount of resistance is offered to its free play.

"When a part containing magnetic steel is brought near the needle, it may be either attracted or repelled, it may move upwards or downwards, or it may exhibit disquietude according to the position in which the new magnet is held. We may detect the position of the foreign body, when it is of any size, by ascertaining where its north and south poles lie, and these are determined by their repelling and attracting the opposite poles of the magnetic needle. The disquietude, or motion upwards and downwards, merely indicates magnetism, but not the direction of the magnet.

"You will, doubtless, be surprised when I tell you, that in this manner I have detected a piece of needle impacted in the finger of a young woman, although it weighed but the seventh of a grain. This gave such marked indications, that I found out tolerably well the position of its north and south poles, though I could not ascertain the presence of a foreign body in any other way. I tried experiments on smaller pieces, at short distances, such as half an inch to an inch, and I found that a piece of needle, weighing 1-60th of a grain, gave decided indications after having been magnetized, and perhaps even a still smaller amount of steel might in some cases be detected.

"I have now satisfactorily demonstrated to you, that magnetism may be used for the detection of steel particles impacted within the body with absolute success; and though but a very trifling application of natural philosophy to the practice of surgery, I have no doubt that, had it been adopted before, many joints would have been saved; and I confidently anticipate that it will be the means, in future, of frequently saving these parts from destruction."—*Lecture on the Detection of Needles and other Steel Instruments.*

ON TWO NEW METALS, PELOPIUM AND NIOBIUM, DISCOVERED IN THE BAVARIAN TANTALITES. BY PROF. H. ROSE.

I have discovered in the tantalite of Bavaria two new metals, in the present memoir however I shall only treat of one: the oxide of the second metal, to which I have for the present assigned the name of *oxide of pelopium,* greatly resembles tantalic acid, although I am fully persuaded that it is distinct. I shall however institute a series of experiments to determine their precise difference.

For more than four years I have been engaged in investigating the tantalites from various localities, and the tantalic acids derived from them. An observation of my brother engaged me to undertake these researches. He found that the tantalites of Bodenmais in Bavaria, and those of North America, have the same crystalline form as Wolfram. I had also noticed that the tantalites from the same or from different localities, having the same form of crystal and the same chemical composition, differed from each other by their specific gravity.

The analysis of these different tantalites was executed according to Berzelius's method, by fusing the mineral reduced to a very fine powder with bisulphate of potash in a platinum crucible.

To obtain an accurate idea of the composition of the tantalites of Bavaria and of North America, it was requisite to submit the tantalic acid obtained from them to a very strict examination. On comparing the amount of oxygen of the tantalic acid, derived from these localities, with the amount of oxygen of the bases, protoxide of iron and protoxide of manganese, the relation existing between them is found to be not simple. This relation is, on the other hand, perfectly simple in the tantalites from Finland, viz. as 3 : 1. The tantalic acid derived from the tantalites of Finland only is pure. Its characters have been described by Berzelius, and it alone will retain the name of tantalic acid. The acid obtained from the tantalites of Bavaria is composed of two acids, one of which greatly resembles the tantalic acid procured from the Finland tantalites, and will form the subject of a future communication. The other likewise resembles tantalic acid, but differs from it in many essential points: it is the oxide of a metal which differs from all known metals. I have called it *Niobium,* and its acid *niobic acid,* from Niobe, daughter of Tantalus, a name which calls to mind the resemblance between the two metals and their oxides.

Tantalic and niobic acids bear most resemblance in their properties to titanic acid and the binoxide of tin: all four have probably the same composition. Both of them, when calcined in the hydrated state, present the same luminous phænomenon. Both are white before and after calcination. When tantalic acid is heated, it acquires a faint yellow colour; niobic acid, on the contrary, a deep yellow; both became colourless on cooling. Tantalic acid forms on cooling a white dull powder; niobic acid, fragments possessed of great lustre, similar to that presented by titanic acid precipitated by ammonia and calcined; with this difference,—that the titanic acid exhibits a brownish tint, while the niobic acid remains colourless.

Tantalic and niobic acids readily combine with the alkalies. When fused with carbonated alkalies, they expel the carbonic acid. These combinations are soluble in water, and also in an excess of a solution of caustic or carbonated potash, but with difficulty in an excess of caustic or carbonate of soda. The niobate of soda indeed is almost insoluble in an excess of a solution of soda. Acids precipitate the tantalic and niobic acids from these solutions. Sulphuric acid precipitates both completely on the application of heat; but in the cold the tantalic acid is only partially thrown down by it, while the niobic acid is entirely precipitated. Hydrochloric acid in the cold only produces a turbidness in a solution of tantalate of soda; an excess of acid even causes this to disappear; with the assistance of heat it precipitates the tantalic acid, but not completely. In a solution of niobate of soda it produces a considerable turbidness in the cold, without precipitating the whole of the niobic acid, which it effects however with the assistance of heat. Oxalic acid has no effect on either of the alkaline solutions, while acetic acid produces precipitates. Hydrochlorate of ammonia forms precipitates.

When an infusion of galls is added to a solution of tantalate of soda, rendered slightly acid with hydrochloric or sulphuric acid, it produces a bright yellow precipitate. In a solution of niobate of soda it gives rise to a deep orange-yellow precipitate, which has some resemblance to the precipitate formed under similar circumstances in a solution of titanic acid. Both the precipitates dissolve in caustic alkalies.

The tincture of galls is the best test for small quantities of tantalic or niobic acids in acid solutions; but it should be observed, that the presence of oxalic acid, or of any other non-volatile organic acid, prevents the formation of the precipitate.

The yellow ferrocyanide of potassium produces in a solution of tantalate of soda, rendered acid with a few drops of sulphuric acid, a yellow flocculent precipitate, which is slightly soluble in a large excess of hydrochloric acid. It gives rise to a decided red precipitate in a solution of niobate of soda, resembling that produced in the same solution by tincture of galls. The red ferrocyanide of potassium forms a white flocculent precipitate in a solution of tantalate of soda, a deep yellow one in a solution of niobate of soda.

If a blade of zinc be immersed in a solution of tantalate of soda rendered slightly acid, it produces no effect at first; but after some time a white deposit is formed consisting of tantalic acid, which is thrown down when the excess of acid begins to dissolve the oxide of zinc formed. In a similar solution of niobate of soda the zinc produces a blue precipitate, which in the course of time becomes brown.

Perchloride of tantalum, prepared from tantalic acid by means of charcoal and chlorine, is yellow, very fusible and volatile. Perchloride of niobium prepared in the same manner is colourless, non-fusible, and but very slightly volatile.

When dry ammonia is passed over perchloride of tantalum, the gas is absorbed, but very slowly, and the perchloride becomes less heated than is the case with the other liquid and volatile metallic chlorides placed under the same circumstances. This is not owing to the perchloride not possessing great affinity for the ammonia; but the new combination formed envelopes the perchloride of tantalum, and prevents the reaction from extending further. If this new compound be heated, metallic tantalum is obtained, and hydrochlorate of ammonia disengaged. The whole of the hydrochlorate of ammonia is removed by washing with water. On heating the metal with access of air, it is converted into tantalic acid with evolution of light. The reduction of tantalum requires a much higher temperature than that of titanium under similar circumstances. Metallic tantalum presents the appearance of black crusts. Water has no action on it.

Perchloride of niobium submitted to the action of dry ammonia becomes yellow, with considerable evolution of heat, because from its being infusible it offers a greater surface to the action of the ammonia. On applying heat to the new compound it immediately becomes black, with disengagement of hydrochlorate of ammonia. The reduction is effected at a much lower temperature than that of the tantalum. On washing it with water to free it from hydrochlorate of ammonia, the water passes clear as long as it contains any of that salt; but when the metal is nearly pure the water becomes turbid. This inconvenience is avoided by adding a few drops of alcohol to the water. When heated in the air the metal burns with evolution of light into white niobic acid. Nitric acid and *aqua regia* are without action on it even on boiling; but it is attacked, with disengagement of red vapours, by a mixture of nitric and hydrofluoric acids. Tantalum behaves in the same manner towards these acids.

If the same atomic composition be admitted for tantalic and niobic acids, the weight of niobium will be higher than that of the tantalum*.—From the *Chemical Gazette* for Jan. 15th, 1845.

ON THE CALCINATION OF HYDROUS OXIDE OF CHROMIUM.

BY M. KRÜGER.

It is well known that when hydrated oxide of chromium ($\underline{\dddot{\mathrm{Cr}}}$) is heated to low redness in a platina crucible, it loses its water, becomes strongly incandescent, and loses its solubility in dilute acids. Several interesting circumstances have however remained entirely unnoticed in this experiment.

If hydrated oxide of chromium be heated only to a little above 392° F. in contact with the air, it loses all its water and becomes almost black, it is however brown, when reduced to fine powder; if

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* We are also informed by Mr. W. Francis that another new metal called *Ruthenium* has recently been discovered in the platinum residues, by Prof. Claus of Kasan.—R. T.

it be more strongly heated, incandescence then begins, and the oxide becomes green. If however the oxide which has become black be heated in a small retort, it will be observed that a great disengagement of gas occurs both before and during ignition; this gas is oxygen, and the oxide becomes at the same time green.

Hydrated oxide of chromium is completely converted by heat in contact with air into peroxide, $\ddot{Cr}$, which at a low red heat is changed, with incandescence, into oxide of chromium. M. Krüger finds the peroxide to consist of 63·7 metal and 36·3 oxygen.

This peroxide is a peculiar compound, and does not contain chromic acid; if it be mixed with chloride of sodium and the mixture be treated with sulphuric acid, chlorine is given out without the formation of any trace of the red chromate of chloride of chromium. When it is boiled with hydrochloric acid, the peroxide yields chlorine, and if heated with sulphuric acid it gives oxygen; the best method of making these experiments is to precipitate hydrate of oxide of chromium from dilute solutions by ammonia. If the precipitation be effected in a boiling solution, it is obtained in a more compact state, which renders it but slightly soluble in diluted acids.

If to a solution of bichromate of potash there be added a neutral solution of sulphate of chromium and then ammonia, there is obtained a brown precipitate, from which, as shown by M. Mons, all the sulphuric acid may be removed by very long washing; there remains only hydrate of oxide of chromium; according to this chemist this precipitate is not peroxide of chromium, but chromate of basic oxide of chromium $\underline{\dddot{Cr}}+\dddot{Cr}$.

If however the brown precipitate be washed only until sulphuric acid cannot be discovered in the filtered liquor, and then after drying in the air it is treated with chloride of sodium and fuming sulphuric acid, no chromate of oxide of chromium is disengaged, but merely chlorine, proving that the precipitate is merely peroxide of chromium, but unquestionably in the state of hydrate.

It is not decomposed even by long treatment with water into chromic acid and oxide of chromium. If peroxide of chromium obtained in this manner be heated, it loses water, and is converted, by a stronger heat, into oxide, with the disengagement of oxygen and with ignition.—*Journ. de Pharm. et de Ch.* December, 1844.

PREPARATION OF FERRATE OF SODA AND OF BARYTES, &C.

M. Fremy states that, after having formed ferrate of potash in the mode described (Phil. Mag., January, p. 81), he endeavoured to procure ferrate of soda by the same means; but he found that it was impossible to obtain ferrate of soda by acting upon iron by nitrate of soda in a Hessian crucible, this nitrate appearing to be much more fixed in this case than the nitrate of potash; the former salt is decomposed slowly, and the free soda attacks the crucible.

Ferrate of soda was readily obtained in the moist way by passing chlorine into concentrated soda containing suspended hydrate of peroxide of iron.

Ferrate of soda is soluble in water and gives a red-coloured solution, which strongly resembles that of ferrate of potash; it is, however, impossible to precipitate it by excess of soda; in other respects it resembles the potash salt.

Ferrate of Barytes.—This salt is prepared by precipitating ferrate of potash with nitrate of barytes or chloride of barium; a fine purplish-red precipitate is formed, which is ferrate of barytes. This salt is insoluble in water, and appears to be much more fixed than the soluble ferrates; it may, indeed, be boiled for some time in water without being decomposed. Organic substances exert but little action upon it; when washed upon a filter the organic matter of the filter does not decompose it. Strong acids evolve oxygen from it, and form salts of the sesquioxide of iron and barytes; but when ferrate of barytes is treated with dilute acetic acid, it dissolves and forms a solution of a fine red colour; it may be supposed in this case, either that the ferrate of barytes is dissolved without decomposition, or that it is decomposed and that the red colour is owing to the free ferric acid; but when the mixture is heated, peracetate of iron is immediately formed and oxygen is evolved. Thus ferric acid, combined with a less powerful base than potash, may be separated by an organic acid, and remain some time without being decomposed.

Other ferrates are insoluble and possess but little interest; they may be prepared by double decomposition; M. Fremy appears to have minutely examined the above-described ferrate of barytes only of this class.—*Ann. de Ch. et de Phys.*, November 1844.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1844.

Chiswick.—December 1—3. Overcast. 4. Cloudy and cold: sharp frost. 5. Frosty: severe frost at night. 6. Frosty and foggy: severe frost. 7. Hazy: slight frost. 8. Dry and frosty: sleet. 9. Overcast. 10. Cold and dry. 11. Dusky haze: clear and frosty. 12. Foggy: overcast. 13. Thick haze: overcast. 14. Hazy: densely overcast. 15. Slight thaw: foggy. 16. Foggy. 17. Dense fog. 18. Foggy: fine at night. 19. Drizzly: boisterous. 20. Overcast: clear and cold: boisterous. 21, 22. Cold and dry: overcast. 23. Overcast. 24. Hazy and cold. 25. Overcast. 26, 27. Dense fog. 28. Fine: overcast. 29. Drizzly: rain. 30. Foggy. 31. Hazy.—Mean temperature of the month 6°·8 below the average.

Boston.—Dec. 1. Cloudy. 2. Cloudy: very dark day. 3. Cloudy: rain early A.M. 4. Cloudy. 5, 6. Fine. 7. Cloudy. 8. Fine: snow P.M. 9—16. Cloudy. 17. Foggy: rain early A.M. 18. Rain: rain P.M. 19. Cloudy. 20—22. Fine. 23—25. Cloudy. 26—30. Foggy. 31. Foggy: rain early A.M.

Sandwick Manse, Orkney.—Dec. 1. Bright: cloudy. 2. Cloudy: drops. 3. Cloudy. 4. Bright: clear. 5. Bright: cloudy. 6. Bright: frost: cloudy: frost. 7, 8. Bright: frost: hazy. 9. Showers. 10, 11. Showers: sleet. 12. Cloudy: showers. 13—15. Snow-showers. 16, 17. Showers. 18. Showers: clear: frost. 19. Clear: frost: clear: showers. 20. Bright: clear: frost. 21. Clear: frost. 22. Frost: clear: thaw. 23. Cloudy: thaw. 24. Cloudy. 25. Cloudy: showers. 26. Cloudy: frost: cloudy. 27. Cloudy: frost: clear. 28. Cloudy: damp. 29. Fog: damp. 30. Bright: cloudy. 31. Showers: cloudy.

Applegarth Manse, Dumfries-shire.—Dec. 1. Fair, but cloudy. 2, 3. Fair and mild. 4—11. Frost. 12. Frost: slight fall of snow. 13. Frost. 14, 15. Frost: cloudy. 16. Frost: very slight. 17. No frost. 18. No frost: rain. 19—21. Frost. 22. Frost: very mild. 23—26. Frost. 27. Frost: mild and fine. 28. Frost A.M.: rain P.M. 29. Thick fog. 30. Frost: shower P.M. 31. Frost: fog.

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

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
1844. Dec.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·123	30·083	29·83	30·14	30·10	30·28	30·21	42	33	39	43	38	41	42½	ne.	calm	ne.	s.	......	......	......	......
2.	30·053	29·998	29·68	29·98	29·91	30·09	30·04	40	27	39	42	37	43	43	ne.	calm	ese.	se.	......	......	......	......
☾ 3.	30·073	30·043	29·75	30·12	30·20	30·16	30·21	39	33	38	41	36½	43	40	ne.	calm	se.	s.	......	·02	......	......
4.	30·223	30·169	29·97	30·22	30·15	30·21	30·10	40	22	37·5	39½	28	40	38	ne.	calm	e.	sse.	......	......	......	......
5.	30·111	30·069	29·85	30·08	30·08	30·05	30·11	35	14	31	33	24½	37	36½	ne.	calm	ne.	sw.	......	......	......	......
6.	30·216	30·203	29·92	30·09	30·10	30·10	30·06	30	14	25·5	32	20	34	36	e.	calm	ne.	sse.	......	......	......	......
7.	30·294	30·298	30·03	30·18	30·18	30·10	30·14	34	20	33	31½	23	38	37½	e.	calm	s.	sse.	......	......	0·00	......
8.	30·172	30·058	29·98	30·19	30·12	30·18	30·21	32	26	33·5	34½	24½	37	38	ne.	e.	e.	sse.	·01	......	......	......
● 9.	30·119	30·104	29·87	30·05	30·17	30·24	30·31	32	28	29	36	28½	37	38	ne.	e.	ese.	sse.	......	......	......	·15
10.	30·104	30·044	29·88	30·16	30·05	30·32	30·21	32	28	35·5	37	35	37	37	ne.	e.	ene.	se.	......	......	......	·22
11.	29·962	29·952	29·70	29·89	29·82	29·98	29·88	30	22	31	36	32	38	38	ne.	calm	e.	se.	......	......	......	·09
12.	29·924	29·750	29·71	29·85	29·74	29·96	29·96	31	24	32	33	29	38	38	e.	calm	e.	sse.	......	......	......	·05
13.	29·545	29·443	29·40	29·51	29·46	29·91	29·80	32	25	27	33	26½	35	36	e.	e.	e.	e.	......	......	......	·10
14.	29·523	29·430	29·40	29·44	29·50	29·86	29·92	32	28	30	38½	30	37	36	e.	e.	e.	e.	......	......	0·00	·07
15.	29·556	29·436	29·33	29·51	29·47	29·88	29·78	40	34	33	39	31	34½	38	e.	calm	ese.	e.	·03	......	......	·08
☽ 16.	29·336	29·316	29·17	29·39	29·33	29·73	29·70	43	35	37	40½	32	37	38	e.	e.	e.	e.	......	......	......	·09
17.	29·434	29·365	29·05	29·30	29·32	29·69	29·67	44	35	38·5	41½	36	39½	39	e.	calm	ene.	e.	......	·10	......	·12
18.	29·806	29·581	29·30	29·52	29·82	29·85	30·11	44	40	38	41	36	37½	32½	ne.	calm	ne.	ne.	·05	......	......	·27
19.	30·214	30·025	29·79	30·12	30·21	29·24	30·33	42	29	40	42	36½	36	35	e.	calm	ne.	ssw.	·01	·08	......	......
20.	30·288	30·260	30·00	30·34	30·35	30·39	30·45	38	28	33·5	36	25	37	34½	ne.	calm	e.	s.	......	......	......	·15
21.	30·462	30·295	30·05	30·35	30·34	30·49	30·49	37	30	33	38	25½	33	37	ne.	calm	e.	......	......	......	......	......
22.	30·239	30·176	30·05	30·32	30·20	30·45	30·36	35	27	33·5	37	29	36	39	ne.	calm	e.	sse.	......	......	0·05	......
23.	30·238	30·095	29·93	30·14	30·18	30·31	30·29	34	28	32	36	31	38	38½	ne.	calm	e.	se.	......	......	......	......
○ 24.	30·270	30·245	30·00	30·19	30·17	30·30	30·30	34	31	32	34½	32	40	39½	e.	calm	ese.	se.	......	......	......	......
25.	30·260	30·194	29·95	30·12	30·09	30·30	30·18	35	31	33·5	36½	30	40	37½	e.	calm	e.	se.	......	......	......	......
26.	30·116	30·072	29·88	29·99	29·90	30·10	30·02	39	30	36	41	30	34	33½	se.	calm	se.	se.	......	......	......	......
27.	30·038	29·998	29·80	29·87	29·87	30·04	30·01	38	30	33	43½	32½	33½	35½	ne.	calm	se.	se.	......	......	......	......
28.	29·944	29·906	29·80	29·75	29·68	29·91	29·86	48	42	33	42	29½	41	43½	se.	calm	sse.	se.	·15	......	......	......
29.	29·932	29·910	29·57	29·75	29·80	29·82	29·88	49	38	40	39½	35½	43	45	se.	calm	sse.	......	·15	......	0·10	......
30.	30·008	29·951	29·66	29·88	29·93	30·01	30·03	42	32	33	38	27½	41	42½	ne.	calm	ne.	n.	......	......	......	·34
31.	30·105	30·067	29·78	29·98	30·05	30·00	30·11	44	32	38	38	32	45½	43	ne.	calm	ne.	w.	......	·20	......	·04
Mean.	30·019	29·984	29·74	29·949	29·944	30·030	30·088	37·32	28·90	34·1	37·9	30·4	38·11	38·25					·40	·40	0·15	·77

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XX. *Is Capillary Attraction an Electric Phænomenon? A Memoir by* John William Draper, *M.D., Professor of Chemistry in the University of New York**.

WRITERS on the mechanical theory of capillary attraction have contented themselves with the statement, that the phænomena exhibited by tubes of small diameter are due to two causes,—the action of the tube upon the liquid, and of the liquid upon itself.

In our philosophical treatises capillary attraction is spoken of as a force *sui generis*. It occupies an isolated position.

But it appears to me, that without disturbing in any way the characters which have been assumed for it as a specific force, or the mathematical conditions under which it acts, there is a point of view from which it may be regarded, which at once brings it into an exceedingly interesting position.

In this memoir I propose to show,—

I. That the principal phænomena ascribed to capillary attraction may be explained by regarding them as manifestations of electrical action.

II. That the voltaic battery has the power of controlling the capillary relations of certain bodies.

III. That, admitting the foregoing theory to be true, we are able to explain several curious and obscure facts.

I. (1.) If we take two pieces of flat glass, and press them tightly together, they will adhere; and as the adhesion continues *in vacuo*, the phænomenon is explained by supposing they are maintained in contact with one another by capillary attraction.

But, if we draw them apart, and present them in succession to a gold-leaf electrometer, we find they are both electrically excited; the one is positive, the other negative. By the well-

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* Communicated by the Author.

known laws of electricity they *must* therefore attract one another.

Is it philosophical to assume the existence of a new force when there is one so palpably efficient at hand?

(2.) If a plate of glass, hanging from one of the arms of a balance, is lowered on the surface of some mercury in an earthen cup, adhesion takes place; and it requires a considerable weight to be added on the other arm before the glass can be separated from the mercury. This is a phænomenon of capillary attraction.

But on trying, as before, with an electroscope, the glass is found positive; and though the mercury is unelectrified, we see obviously that its electricity has escaped into the earth. And that this is true may be shown by putting the mercury into a glass cup, or otherwise insulating it; then the glass is positive and the mercury negative; they therefore ought to attract each other.

(3.) Two lead bullets pressed together adhere,—I presume for the same reason that two pieces of glass adhere. On putting them apart no electric excitement is discoverable, owing to their conducting power *.

(4.) When melted sulphur is allowed to cool in a conical glass vessel, it adheres thereto. On forcibly pulling it out, the glass is found to be positive and the sulphur negative; they ought therefore to attract.

I might add other instances. These probably are sufficient to communicate the idea in question.

In all these cases the conducting power of the surfaces employed determines the apparent electric development; and in those instances in which electric excitement cannot be detected the reason is obvious.

If a plate of polished iron is laid on a mercurial surface, adhesion takes place, as in the case of glass; but on examining the iron and mercury, no electric development appears, or, at all events, the traces are very feeble. It could not, however, be otherwise, for, owing to the conducting power of the surfaces employed, a discharge from the one to the other must take place in the act of separation, and all electric phænomena cease.

If a plate of glass be placed upon some water, it adheres thereto by capillary attraction; but on examining the glass and water at the electroscope, neither of them indicates excitement. And the reason is plain; for the separation of the glass from the water is only apparent. Its under surface is wetted by a film of the liquid, and we have in reality only

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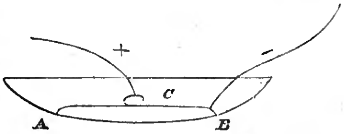
* What takes place if each bullet is insulated, taking care that there be no difference of mechanical condition between them?—EDIT.

overcome the cohesion of the particles of water, but not effected the separation of the glass. The surface of water which has been torn away, and that which remains in the cup, are both equally good conductors; and hence a discharge from one to the other takes place, in the same manner as in the act of separating iron and mercury.

That the capillary attraction of glass for water is nothing more than an electrical phænomenon, appears when we so arrange things as to be able to separate the touching surfaces completely. In a conical vessel let some water be frozen, and when perfect solidification has taken place, and no water is present, pull the cone of ice out of the glass: examined at the electroscope the glass and ice are found to be in opposite conditions. Now is it not reasonable to suppose that the physical cause of the adhesion of ice to glass is the same as that of water to glass? Does not therefore this experiment prove that the capillary attraction of glass and water is only an electric phænomenon?

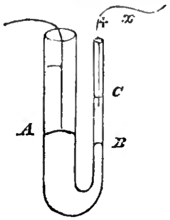
II. If capillary attraction is nothing more than the result of an electrical disturbance which ensues when bodies are brought in contact with one another, we might expect that any process which can change the electro-chemical relations of such substances, could also probably alter their capillary phænomena.

The following experiments may be cited as substantiating this view:—1st. In a watch-glass place a quantity of pure mercury, A B, and upon it a drop of water, C. Bring the water in contact with the positive platina electrode of a voltaic battery of twenty or thirty pairs, and touch the mercury with the negative. The moment the contact is made the drop of water loses its spherical form, and spreads out into a thin circular disc, wetting the surface of the mercury. The diameter of the disc seems to be greater in proportion as the battery is more powerful.



Under ordinary circumstances water does not wet quicksilver; a drop of water remains on the surface of quicksilver in the same manner that a drop of oil remains on water. As soon, however, as their electro-chemical relations are disturbed by the aid of a voltaic battery, the phænomenon of wetting at once takes place.

2nd. Take a tube in the form of an inverted siphon, one branch of which, A, is about half an inch wide, and the other, B, not more than the tenth of an inch. Tubes of this kind are used to exhibit the ordinary phænomena of capillary attraction.



Fill the siphon to a given altitude, A B, with quicksilver; the metal of course does not rise in the narrow branch, B, to its hydrostatic level, for mercury is depressed in a capillary tube, inasmuch as it cannot wet glass. Introduce a small column of water, B*c*. The mercury may now be regarded as being in contact with a tube of water, because that liquid wets the sides of the glass, intervening between it and the mercury.

Pass a slender platina wire, x, down the tube so as to touch the water; let it be in communication with the positive electrode of the voltaic battery: with the negative electrode touch the mercury in the wide branch of the siphon A, and in an instant the metal will rise in the narrow tube, and fall again to its former position as soon as the current is stopped.

III. Admitting the correctness of this theory, we can give a very clear explanation of several curious facts.

1st. Huygens observed, that mercury might be made to stand in a narrow barometer tube at an altitude of seventy inches.

This deviation from the true level appears to depend on the cleanliness of the interior of the tube, and most barometers are liable to it to a small extent. By tapping upon the case the adhesion is overcome, and the mercury falls to its proper level.

If this movement is accomplished in the dark, a pale flash of electric light attends it. I infer, therefore, that the mercury adheres to the tube, simply because the one is positive and the other negative; a disturbance which took place upon contact.

2nd. P. Abat noticed, that if in an inverted siphon of uniform diameter some mercury is poured, and the siphon inclined so as to make the mercury flow up one of the branches, on restoring it to the vertical the metal does not return to its hydrostatic level, but remains elevated to a certain extent. The reason is probably the same as in the foregoing case.

3rd. When a large globule of quicksilver is placed in a watch-glass beneath some water containing a little ammonia, the metal being placed in relation with the negative, and the water with the positive electrode, the figure of the mercurial globule changes, and it becomes ellipsoidal. This is due to an increased pressure between the metal and the water, a pressure which is unequal at the different points of contact, being at a maximum in those regions nearest the positive electrode.

At the same time currents are seen traversing the water in a spiral course; they originate in a movement which takes place in the mercurial globule, a movement which is well known to be connected with the presence of small quantities of certain metals. The path of these currents is determined by the figure of the mercurial globule; their existence by the unequal pressures exerted upon its various parts.

If we suppose thus that capillary attraction is only a manifestation of electric disturbance which takes place on the contact of bodies, we obtain the advantage of assigning a distinct physical cause for an extensive class of facts. We replace an unknown and hypothetical agent by one with the qualities and laws of which we are familiar; and we bring the phænomena in question into a very interesting relation with those of chemical affinity; for, recollecting Clairaut's law, which determines the position of a liquid in a tube, "that if the double of the action of the matter of the tube is greater than the action of the liquid upon itself, there is an elevation; if the two quantities are equal, the hydrostatic level is maintained; if the former is less than the latter, there is a depression;" and connecting this with the well-known law of chemistry, "that if the attraction of the particles of a liquid for those of a solid is greater than the attraction of the particles of the latter for one another, solution of that solid ensues," we are led at once to see that the phænomena of capillary attraction stand in a singular relation to those of chemical affinity. They are examples of facts belonging to the same physical class. The adhesion of a dye to cloth-fibre, or of the amalgam to the back of a mirror, are merely instances of the general electro-chemical relations of matter*.

XXI. *Examination of a Specimen of diseased Wheat.*
By Mr. J. Carty†.

My attention was lately directed by Mr. Briggs, of Overton, near Wakefield in Yorkshire, to a kind of disease which he considers to be burnt-ear, and analogous to smut, which had attacked some wheat growing on his land. The stalk of the wheat was of the usual height and strength, and the ears seemed externally healthy; but on breaking into the grain it was found to be filled with a black powder, instead of the white, thick juicy substance found in healthy wheat at the

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* On the adhesion of a dye to cloth-fibre, see Phil. Mag., S. 3. vol. xxiv. p. 241; and we may also refer Dr. Draper to a discussion by Prof. Challis and Mr. Brayley, on the relations between the attraction of aggregation and chemical affinity, bearing immediately on the subject of his paper, in Phil. Mag. S. 3. vol. viii. p. 94, 172, 288.—Edit.

† Communicated by the Chemical Society; having been read November 4, 1844.

same period of growth. It was a thick-set red wheat, sown in December on a light and good turnip soil, to which an unusually large quantity of guano (4 cwt. per acre) had been applied as a manure. About one-tenth of the whole quantity was diseased, while several other kinds of wheat, sown in the same field, but to which no guano had been applied, were perfectly sound.

The ears had a much greener appearance than the same kind of wheat in a healthy condition, and emitted a very disagreeable smell, which is not easily described.

On close examination the grains were found filled with a black unctuous powder in place of the milky pulp of the healthy seed. This powder was separated in quantity by bruising the ears in a mortar and sifting through fine muslin, and submitted to chemical examination. It had the same disagreeable smell as the ears, but stronger; it was oily to the touch, and heavier than alcohol and water. When heated in the air it burned with a bright flame, leaving a residue of charcoal, and eventually a trace of white ash. When heated in a tube, it gave off water, empyreumatic and oily matters, and a little ammonia. It was insoluble in a solution of potash and in hydrochloric acid; nitric acid made it yellow, and hot sulphuric acid dissolved it with purplish-red colour. Boiled with water, it yielded merelya little gum and bitter brown extractive matter, the greater part remaining undissolved. Alcohol extracted a fat oil, and a waxy or resinous matter; the undissolved portion appeared to consist of lignin mixed with charcoal. The black powder greedily absorbed oxygen from the air when in a moistened condition, giving rise to carbonic acid.

A carefully conducted proximate analysis of the substance led to the following results:—

Wax or resin with fixed oil . . .	7·0
Gum and extractive matter, &c. .	7·8
Lignin and charcoal	82·7
Ashes	2·5
	100·

The ash consisted chiefly of earthy phosphates and silicate of potash.

XXII. *Description of a Chemical Lamp-Furnace.*
By Edward Solly, *Esq.*[*]

At the meeting of the British Association last September in York, the Rev. W. Vernon Harcourt exhibited a hydrogen lamp-furnace, the object of which was to afford a

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[*] Communicated by the Chemical Society; having been read November 18, 1844.

steady, uniform heat to a platinum vessel, sufficiently high to effect the ready fusion of vitreous substances by means of a fuel, free from those objections attending the use of one containing solid carbonaceous matter. Mr. Harcourt's furnace consisted of a number of jets arranged round the crucible to be heated, which, to ensure uniformity of temperature, was suspended by three platinum wires from a watch movement, which caused it to rotate slowly on its vertical axis; the fuel employed was hydrogen, generated in a strong iron reservoir, and burnt under a pressure of from 10 to 30 atmospheres. The mechanical arrangements of this lamp-furnace were very beautiful, and the heat produced by the combustion of the jets of compressed hydrogen directed against the bottom and sides of the revolving crucible was steady, intense, and apparently manageable; but the instrument was of course very expensive, and required considerable care in its use.

The exhibition of this apparatus suggested to me the possibility of constructing a lamp on a similar principle, but far less expensive, and sufficiently simple for ordinary use; which, though it might not afford so high or pure a source of heat as the arrangement of Mr. Harcourt, might yet constitute a useful instrument in the laboratory: as, on trial, I found my plan perfectly successful, I am led to think that a brief description of the lamp will be interesting to others.

It is well known that when coal-gas is mixed with a certain proportion of air it burns with a pale blue flame containing no solid carbon, and therefore giving only a very feeble light, but possessing a very high temperature, so much so, that the large pale flame of gas mixed with air and burnt above a diaphragm of wire gauze is very commonly employed when a higher temperature is required than can be obtained by the mere use of an Argand lamp. It appeared probable, after seeing Mr. Harcourt's lamp, that a very high temperature might be obtained by burning a mixture of air and coal-gas, in place of hydrogen, in a suitable arrangement of jets. The form of lamp which, after a few trials, I found best adapted for this purpose, consists of two circles of jets, one vertical and the other horizontal, the latter being raised nearly two inches higher than the former. The vertical circle consists of a metal ring, about an inch in diameter, pierced on its upper side by six small holes or jets; the horizontal circle consists of a hollow metal ring, having an internal diameter of rather more than three inches, with twelve holes drilled on its inner side, so as to form altogether a series of eighteen little jets of flame, six vertical and twelve horizontal, all converging to a common centre, which is of course the point of the greatest heat. The mixture of coal-gas and air burnt in this lamp, is made by injecting a sufficient quantity of air into the pipe which supplies the coal-gas. To the end of a common gas-cock connected with the street main, a piece of copper pipe about four inches long is attached, having a diameter of about a quarter of an inch; outside this is another piece of copper tube, of considerably larger diameter, and six inches longer than the internal tube. The air which is to mix with the coal-gas is admitted into this longer external tube, the quantity being regulated by cocks. The object of this is to effect the mixture of the air and coal-gas under the most favourable circumstances; if mixed otherwise than as two currents flowing in the same direction, a diminution of pressure is caused; whilst by causing them to mix in the manner just described, the pressure of the whole current is rather augmented.

The pressure of the ordinary street gas does not, I believe, generally exceed two inches; if the air was introduced by a pipe entering the gas-pipe at right angles, and if the pressure of the air at all exceeded that of the gas, it was very apt to stop the current of gas altogether, the air forcing the gas back into the pipe; by using the above double pipe this was obviated, and it was found possible slightly to increase the pressure of the air over that of the gas without at all diminishing its force. At first a gasometer was employed as a source of air, but after a few trials it was found that a common double bellows was more convenient; when the lamp is used, the crucible to be heated is supported on the top of the horizontal circle of jets by a triangle of platinum wire, so as to place it in the centre of greatest heat. If only the gas is lighted, it of course burns with a large and very smoky flame, depositing abundance of solid carbon; but the moment the air is admitted, the flame begins to diminish in size and also in brilliancy, burning, when a sufficient proportion of air is allowed to enter, with a pale blue colour, and perfectly free from solid carbon. The crucible is, in fact, heated by eighteen little blowpipes, and of course becomes brightly ignited in a few seconds, the heat continuing to increase as the ignited platinum facilitates the more complete combustion of the gas and air, causing the blue flame of each jet to be edged with pale yellow, and considerably increasing them in size; lastly, the furnace is rendered complete by a thin cylinder of sheet iron, three inches in diameter and two high, which is placed above the horizontal circle to prevent the flames being blown about by draught of air, and a circular disc of the same metal, having a hole in its centre of an inch across, to place at the top of the cylinder:

this causes the heated air to pass round the upper edges and over the lid of the crucible, so as to bring the whole to the same temperature.

With this arrangement it is easy to keep a platinum crucible two inches high and an inch and a half across at a bright red or yellow heat for any required time; the heat is perfectly manageable, the gas may be turned off at any moment, and as quickly lighted; hence any operation is far more under control than in a furnace. The heat obtained with a crucible of the size just mentioned is rather above the melting-point of silver, for a piece of that metal is easily fused in a porcelain crucible placed in the interior of the platinum one. It is of course equal to the fusion of mixtures of silicates with carbonate of soda, three or four hundred grains of a mixture of carbonate of soda and a siliceous compound being perfectly fused in about eight minutes, whilst from the entire command at all times had over the source of heat, it is easy to moderate it when the evolution of carbonic acid is too rapid, and to stop it as soon as the operation is complete; in fact, the progress of the experiment may be watched from the commencement to the end with the greatest facility.

The object which I had originally in view in the construction of this lamp, was to obtain a cheap, simple and efficacious method of heating a platinum crucible to bright redness without exposing it to the contact of solid fuel, and in this the attempt was perfectly successful. The decomposition of earthy silicates may, it is true, be very easily effected in an ordinary furnace; the crucible being protected from contact with the fuel by placing it in an earthen one, or by wrapping it in a thin piece of platinum foil, which preserves the crucible very perfectly from the action of the fuel in a cheap and very convenient method; but still, the above lamp-furnace is decidedly superior to that or indeed any other mode I have tried. Independent even of the question of œconomy, I prefer it to the Argand spirit-lamp.

As the use of such a lamp must in part depend on its simplicity, I was anxious not to complicate it more than was absolutely necessary; but I have no doubt that its power might be very greatly augmented; in its present state, however, it is useful for several purposes besides the mere ignition of a crucible, such as, for example, glass-blowing and bending large or thick tubes; in fact, in all cases where a bright red or yellow heat is required, and where an ordinary furnace is inapplicable.

XXIII. *Notice of a new Hydrated Phosphate of Lime.*
By JOHN PERCY, *M.D.**

TO a solution of common phosphate of soda, in a *large* quantity of distilled water, add a solution of chloride of calcium till precipitation ceases. Immediately pass through the mixture, containing the phosphate of lime suspended, a current of carbonic acid gas, and continue this for about an hour; then filter immediately, and set aside the filtered liquor in a jar, the mouth of which is covered loosely with paper to keep out dust. In the course of a day or two, and sometimes in a much shorter time, crystals will appear on the surface, and will, when stirred with a rod, sink. After the lapse of several days a very sensible quantity of crystals may be collected. Transfer them to a filter, and wash with cold or tepid distilled water, and dry in a *warm* place.

It occurs in the form of brilliant white scales, which, on examination with the microscope, are seen to consist of modified rhomboidal plates more or less broken and confused.

The salt melts with difficulty at an intense heat by the blowpipe on platinum foil. Heated to bright redness in a platinum crucible, 3·21 grs. of salt lost 0·85, and in a second experiment 3·16 grs. lost 0·84 gr. I found that every trace of water was given off after exposure for 10 minutes or a quarter of an hour to a bright red heat.

Rose's plan was followed in determining the lime and phosphoric acid, which consists in dissolving in hydrochloric acid, then adding sulphuric acid, then alcohol, which precipitates the lime completely as sulphate.

I. 3·21 grs. of salt gave 2·51 of sulphate of lime.
II. 3·16 grs. ... 2·48 ... ...

Taking 28 as the equivalent of lime, and phosphoric acid as 72, we have—

	First analysis.	Second analysis.
Water	0·85	0·84
Lime	1·04	1·02
Phosphoric acid . .	1·32	1·30
	3·21	3·16

The oxygen of the water is three times as much as that of the base, and the salt agrees in composition with the formula

$$2CaO + PO_5 + 6HO.$$

The rational formula will, doubtless, be $HO, 2CaO, PO_5 + 5HO$, 1 equiv. of water being basic and 5 constitutional.

2·40 grs. of this phosphate heated in the oil-bath at a temperature between 134° and 148° centig., lost 0·30 gr. in $1\frac{1}{2}$

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* Communicated by the Chemical Society; having been read November 18, 1844

hour, and no more than 0·31 gr. in $4\frac{3}{4}$ hours, while at a red heat it would lose 0·60 gr. 0·60 − 0·31 = 0·29. We may therefore conclude that one-half of the water is expelled at the temperature above mentioned, giving another hydrate, $HO, 2CaO + PO_5 + 2HO$.

XXIV. *On a curious Change in the Composition of Bones taken from the Guano.* *By* ROBERT WARINGTON, *Esq.*[*]

MY friend Mr. Edwin Quekett, put into my hands a few days since, for the purpose of chemical examination, a curious crystalline substance taken from the guano, as imported from Ichaboe, and which he considered to have entirely replaced, and thus have assumed the form of, the original bony structure. The exterior surface, in many specimens, has portions of muscular fibre in a dry and pulverulent state closely in contact with the substance; small pieces of bony matter are also now and then found remaining, and in some cases the cylindrical part of the bone is perfectly modelled by this saline deposition or infiltration. The structure is highly crystalline and laminated, and in the cylindrical parts slightly radiating from the sides towards the centre; its colour is nearly white, with a slight shade of yellow, and generally very free from foreign matter, except at the parts where the enlarged or natural terminations of the bones may be supposed to have been, and in these places the substance is interspersed with small brown particles. It decrepitates in the flame of the spirit-lamp, assumes a gray colour, gives off ammoniacal vapour, and ultimately becomes perfectly white and opake; and by increasing the heat, by means of the blowpipe, it fuses and communicates a pinkish purple tinge to the flame, indicative of potash, and without any trace of yellow, showing its perfect freedom from soda salts. It was readily dissolved in hot distilled water, with the exception of the interspersed brown particles just alluded to; solution of nitrate of barytes threw down an abundant white granular precipitate insoluble in dilute nitric acid, consequently proving the presence, to a considerable extent, of sulphuric acid; solution of chloride of calcium occasioned no change, nor did the subsequent addition of ammonia cause any precipitation, so that the oxalic and phosphoric acids were both absent. Solution of oxalate of ammonia gave no indication of the presence of lime. By boiling, after adding solution of caustic potash, ammonia was evolved; nitrate of silver in a dilute solution, free nitric acid being present, caused no precipitation. The addition of tar-

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[*] Communicated by the Chemical Society; having been read November 18, 1844. On Guano, see p. 123 of the present volume.

taric acid in excess caused the rapid and plentiful separation of bitartrate of potash; and on evaporating a little of the solution of this substance, to which a few drops of nitric acid had been added, to dryness, a small quantity of uric acid was evidenced by the production of the crimson stain so peculiar to that compound; magnesia was also tested for but was not detected.

It is evident from this qualitative examination that this substance was composed, for the most part, of sulphuric acid, potash and ammonia, with a little uric acid. The following quantitative analyses were therefore undertaken:—5·84 grs. of the whitest and cleanest part were selected and heated to redness in a closed platinum crucible for some time, it lost 1·02 gr., leaving a perfectly white and opake residue, without much alteration of the original form of the fragments. During this operation a slight quantity of acid vapour was given off. This residue was boiled in distilled water, which dissolved nearly the whole, with the exception of a few particles weighing 0·06 gr., having an opake white colour, similar to the bulk of the residue, and which proved on examination to be the bone-earth phosphate of lime; they were slowly dissolved in dilute nitric acid without any appearance of effervescence, and gave indications of the presence of phosphoric acid and lime, the first by solution of nitrate of silver, and by a solution of the ammonio-chloride of magnesium, the latter by oxalate of ammonia. We have, therefore,

4·76 grs.	fixed residue assumed to be sulphate of potash.
0·06 ...	bone-earth phosphate of lime.
1·02 ...	loss by heat, assumed to be sulphate of ammonia and water.
5·84	

14·54 grs. of the substance were digested with boiling distilled water, and the undissolved portion (*a*) collected on a filter; it weighed 1·44 gr., and consisted entirely of the muscular fibres and the brown particles previously mentioned. The solution was then divided accurately into two equal parts, to one of which a little nitric acid was added, heated to the boiling-point and precipitated by a solution of nitrate of barytes as long as any sulphate of barytes was thrown down; it was then collected, washed, ignited, and gave 8·8 grs. of sulphate of barytes, equal to 3·00 grs. sulphuric acid (*b*); this precipitate was accompanied by a very small quantity of a flocculent matter of a pale brown colour. The excess of barytes was next separated by the addition of sulphuric acid, and the filtrate evaporated to dryness, when the crimson stain, denoting the existence of uric acid, was evident; on re-solution in water a pale brown flocculent matter separated (*c*), similar to what had been just noticed; this was separated by a filter and weighed 0·11 gr.; the solution was then evaporated to dryness and heated to expel the excess of sulphuric acid which had been added; the residue (*d*) of bi-sulphate of potash weighed 7·85 grs., equivalent to 2·94 grs. of potash. The second half of the solution was treated with caustic potash and submitted to distillation until reduced to a very small volume, the volatile products being passed into dilute hydrochloric acid; to this a solution of chloride of platinum was added, and the whole evaporated to dryness and then re-dissolved in a mixture of alcohol and æther, which left 3·44 grs. of ammonio-chloride of platinum, equivalent to ·26 gr. of ammonia (*e*).

We have, therefore, from this analysis, the following results:—

Sulphuric acid (*b*)	3·00 grs.	× 2 =	6·00 grs.
Potash (*d*) . . .	2·94 ...	× 2 =	5·88 ...
Ammonia (*e*) . .	0·26 ...	× 2 =	0·52 ...
Flocculent matter (*c*)	0·11 ...	× 2 =	0·22 ...
Insoluble (*a*) . .			1·44 ...

If we now arrange the bases with the acid to form salts, we shall have—

Potash . . .	5·88 grs.	= 10·78 grs. sulph. of potash.
Sulphuric acid .	4·90 ...	
Ammonia . . .	0·52 ...	= 2·29 ... sulph. of ammonia.
Sulphuric acid .	1·22 ...	
Water of crystallization . . .	0·55 ...	

There is, however, a deficiency in the experimental quantity of sulphuric acid amounting to 0·12 gr., as will be seen on comparing this arrangement with the analytical results. These two salts appear to be associated in this substance in atomic proportions, thus:—

		Theory.	Experiment.
Sulphate of potash	4 eqs. =	10·8	10·78
Sulphate of ammonia	1 eq. =	2·3	2·29

It will be evident from the above examination, that this substance is not perfectly constant in its composition; for in the first quantitative analysis a small quantity of phosphate of lime was present, which did not exist in the portion which formed the subject of the second analysis, nor in that of the qualitative examination.

The curious question which arises from these investigations, is as to the probable source of the sulphate of potash in a substance taken from the midst of a material, the guano, abounding in soda and ammoniacal salts, and for the moment this appears to baffle our present information on the localities of these deposits. I have been lately thrown into a train of communication from which I have gained some curious information, which may, to a certain extent, assist in elucidating the point. It appears that the seal-fishers have for a long series of years been in the constant habit of frequenting this island, Ichaboe, as a convenient spot for the purpose of extracting the oil from the fat of the seals which they may have caught; and this is effected by melting the fat over a wood fire, the fuel for the purpose being collected on the island, the carcases of the seals being thrown aside as useless, and becoming the food of the innumerable sea birds frequenting the district, and thus giving rise to the deposit of the guano on, or in close proximity to, the spot where the ashes of the wood fires and the bodies of the seals had been left; and hence we may, to a certain extent, argue on the source of the potash and its decomposing the ammoniacal salts existent in the putrefying carcases or the deposit of the birds. It will also account for the frequent occurrence of the skin and bones of the seal as observed in much of the imported guano.

XXV. *On a means of detecting Kinic Acid.*

By John Stenhouse, *Ph.D.*[*]

THOUGH kinic acid does not possess any very striking characters by which it can be easily recognised, its presence may be readily collected by converting it into that very remarkable product of its decomposition, kinone. As there is good reason for believing that kinic acid exists in the state of kinate of lime in all the barks which contain the alkaloids quinine and cinchonine, it may perhaps be of some importance to describe an easy mode of detecting kinic acid, as this may facilitate the discrimination of the true cinchona barks from the spurious ones.

To examine a bark for kinic acid, it is merely necessary to boil a little of it, say a quarter of an ounce, with a slight excess of lime. The liquor may be poured off and concentrated, as it is not necessary to filter it. It is then to be introduced into a retort and distilled with a mixture of half its weight of sulphuric acid and peroxide of manganese. If the bark contains the smallest quantity of kinic acid, the first portion of the liquid which distils over has a yellow colour and the very peculiar smell of kinone. If the liquid is treated with a little ammonia, it immediately becomes of a deep brown colour, which in a few minutes changes to brownish black; or if a little chlorine water is added to a second portion of the liquid,

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[*] Communicated by the Chemical Society; having been read December 2, 1844.

it changes from a yellow to a bright green colour. The distillation need not be long continued, as the kinone is very volatile, and comes nearly all over at the first. I have tried this experiment with the genuine barks *China rubra*, *China loxa*, and *China regia*, and detected the kinic acid very readily, when less than a quarter of an ounce of each was employed. I have also operated on two ounces of the false bark, *China nova Surinamensis*, but was unable to detect the least trace of kinic acid.

It has long been stated in most systems of chemistry, on the authority of Berzelius, that the alburnum of *Pinus sylvestris* contains a half per cent. of kinate of lime, and that kinic acid exists along with gallic acid in the barks of various other trees. A pound and a half of the inner bark and alburnum of the *Pinus sylvestris* was treated in the way already described, but I was unable to detect any trace of kinic acid; though on adding less than two grains of kinate of lime the presence of kinone in the liquid which distilled over was immediately perceptible.

I may state, in conclusion, that the presence of the vegetable alkaloids quinine or cinchonine in a bark may be readily detected by the following simple experiment. Macerate the bark with dilute sulphuric acid and precipitate with a slight excess of carbonate of soda or potash. Collect the dark-coloured, very impure precipitate, and distil it with a great excess of caustic soda or potash, cincholin will distil over in oily drops if the bark has contained either of these vegetable alkaloids. Cincholin is easily recognisable by its peculiar taste and smell and its strongly-marked alkaline properties. It is nearly insoluble in water, unless first neutralized by an acid, when it readily dissolves; but it is immediately reprecipitated in oily drops on the addition of an alkali. It is quite true that the production of cincholin is not a decisive proof that a bark must necessarily contain quinine or cinchonine, as other alkaloids, such for instance as strychnine, also yield it when distilled with potash. The production of cincholin, however, clearly indicates the existence of at least one alkaloid in the bark, the exact nature of which may be subsequently ascertained by the usual methods.

XXVI. *Remarks upon Chloranil.*

By Augustus William Hofmann, *Ph.D.*[*]

ERDMANN[†], in his researches upon the action of chlorine on indigo, obtained, as the last product of this reaction, a

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[*] Communicated by the Chemical Society; having been read December 2, 1844.

[†] *Journ. für Prakt. Chem.*, Bd. xxii. S. 279. [Phil. Mag. S. 3, vol. xix. p. 192.]

crystalline body free from hydrogen, to which he gave the name chloranil. This substance, remarkable on account of the series of interesting transformations which it undergoes when in contact with the alkalies, has been diligently studied by Erdmann. From his experiments, the composition of chloranil is represented by the formula—

$$C_6\,Cl_2\,O_2.$$

This body has lately become the starting-point of some interesting discussions, which Laurent* brought before the Academy at Paris. The view of this chemist assumes the equivalent of chloranil to be double, agreeing with the formula,—

$$C_{12}\,Cl_4\,O_4.$$

The method which Erdmann employed for obtaining chloranil is circuitous, and yields very little of that substance. He passed a stream of chlorine gas through an alcoholic solution of chlorisatin or bichlorisatin. The following equation represents the transformation of the chlorisatin into chloranil:—

$$\underbrace{C_{16}\left\{\begin{matrix}H_4\\Cl\end{matrix}\right\}NO_4}_{\text{Chlorisatin.}} + 8O + 4Cl = \underbrace{C_{12}\,Cl_4\,O_4}_{\text{Chloranil.}} + 4CO_2 + NH_3 + HCl.$$

The process is not, however, so simple, as there is formed at the same time chlorinated chlorindoptic acid (Laurent's chlorophenussic acid†), as well as many other secondary products, and there is obtained a comparatively small quantity only of chloranil.

Somewhat later chloranil was re-observed under other circumstances. Fritzche‡ found, in some experiments upon the blue colour assumed by aniline from the action of the oxygen compounds of chlorine, that by a continued digestion of a mixture of chlorate of potash and hydrochloric acid upon aniline, chloranil is also formed.

I have had the opportunity, in a research upon the organic bases of coal-gas naphtha§, to confirm this statement; the whole of the carbon of the aniline enters into the chloranil, as the following equation shows:—

$$\underbrace{C_{12}\,H_7\,N}_{\text{Aniline.}},\,4O + 8Cl = \underbrace{C_{12}\,H_4\,O_4}_{\text{Chloranil.}} + NH_3 + 4HCl.$$

The close relation subsisting between the aniline and phenyle series induced me to try whether chloranil could not be obtained from different members of this group of bodies. I soon learnt that chloranil is often formed, as the last product of decomposition of organic bodies, under the

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* *Comptes Rendus*, t. xix. No. 6. S. 316.

† *Ann. de Chem. et de Phys.*, 3me ser. t. iii. p. 498. [Phil. Mag. S. 3. vol. xix. p. 191.]

‡ *Bullet. Scient. de St. Pétersb.*, 1843, t. i. p. 103.

§ Philosophical Magazine, Third Series, vol. xxiv. p. 199.

united influence of oxygen and chlorine. I commenced my investigation with the hydrate of phenyle, and selected as a means of oxidation a mixture of hydrochloric acid and chlorate of potash, in which oxygen and chlorine are disposable. When hydrate of phenyle in a porcelain basin is treated with strong hydrochloric acid, and there is added at intervals small crystals of chlorate of potash, a violent reaction ensues, and the colourless oil acquires a reddish-brown colour and inspissates. If the action has lasted sufficiently long, the whole mass changes to a light yellow crystalline matter, which is no other than chloranil. As it takes a long time to ensure perfect decomposition, it is advisable towards the end of the process to apply heat, in order to promote the action. I have endeavoured to shorten the time by employing an alcoholic solution of hydrate of phenyle; the decomposition then takes place indeed quicker, but a great quantity of chlorate of potash is wasted by the conversion of the alcohol into acetic æther. When alcohol is used, the addition of strong hydrochloric acid, or too quick an introduction of large quantities of chlorate of potash must be avoided, as the mass inflames readily with explosion. These explosions even sometimes take place when no alcohol has been added; it is therefore always advantageous to conduct the operation in basins.

If a boiling saturated aqueous solution of hydrate of phenyle is employed it saves time, and also chlorate of potash; or a watery extract of coal-gas naphtha may be at once taken, as it contains aniline and hydrate of phenyle, both being convertible into chloranil. The produce is in this way, however, very limited, owing to the slight solubility of these two compounds in water*.

To purify the chloranil it is only necessary to affuse with water, which dissolves any adhering chloride of potassium, and to crystallize from alcohol. This menstruum dissolves the chloranil with difficulty in the heat; and on cooling the whole deposits.

I have compared minutely the properties of the substance obtained in this way with those given by Erdmann in his treatise on chloranil, and find that they agree perfectly. The body I procured from hydrate of phenyle, as was before remarked, is insoluble in water, dissolved with difficulty by alcohol, but more easily by æther, and crystallizes from the two latter solvents in shining golden-yellow plates; the mother-liquor assumes a violet tint when exposed to the air. It begins

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* In an experiment where I treated the coal-gas naphtha with the oxidizing mixture there was no chloranil found, apparently owing to its quantity being so minute when compared with that of the other products.

to volatilize at 302° F., and between 410–428° F. the sublimation is rapid; if it be heated above this temperature it melts, and is finally carbonized. The sublimate has the appearance of an extremely soft, light, iridescent powder, which remains intact with acids, but when treated with alkalies is changed with the greatest facility. When the crystals are drenched with a dilute potash-ley, they acquire immediately a dark colour with a shade of green, but on the application of heat dissolve to a blood-red fluid, from which purple needles of an organic salt are deposited upon cooling. These needles, when immersed in dilute hydrochloric acid, assume, without altering their form, a bright red colour. They are quite insoluble in very dilute acids, but dissolve in pure water with a beautiful violet hue. By the addition of hydrochloric acid the liquid is discoloured immediately, and after some time the red crystals again deposit.

There can be no doubt from the foregoing that the substance obtained from the hydrate of phenyle and Erdmann's chloranil are identical. I have notwithstanding made some combustions of this compound. The analyses were however made with a product which was only recrystallized from alcohol, and not sublimed; the substance was therefore not quite pure, a circumstance which Erdmann had found previously. In five analyses this chemist always obtained a constant excess of carbon: my analyses were also encumbered with the same fault, owing to which there is a discrepancy between the chlorine found and that of theory.

I. 0·5845 grm. of substance ignited with chromate of lead, gave 0·6474 grm. of carbonic acid and 0·0212 grm. water.

II. 0·4615 grm. of substance ignited with chromate of lead, gave 0·5090 grm. of carbonic acid and 0·0120 grm. water.

III. 0·2555 grm. of substance ignited with lime and dissolved in nitric acid, gave 0·5820 grm. of chloride of silver.

These numbers, represented in the 100 parts, give,—

	I.	II.	III.
Carbon .	30·20	30·07	
Hydrogen	0·40	0·28	
Chlorine .	……	……	56·20

Erdmann found as a mean of five combustions of the chloranil crystallized from alcohol, 30·20 per cent. carbon *. The following is the theoretical composition of the chloranil:—

12 equivs. Carbon . .	900·00	29·33
4 equivs. Chlorine . .	1770·60	57·66
4 equivs. Oxygen . .	400·00	13·01
	3070·60	100·00

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* Carbon taken at 75.

If the formulæ of hydrate of phenyle and chloranil be compared, the transit of the one into the other is at once perceived.

$$\underbrace{C_{12}H_6O_2}_{\text{Hydrate of phenyle.}}+2O+10Cl=\underbrace{C_{12}Cl_4O_4}_{\text{Chloranil.}}+6H\,Cl.$$

The foregoing equation gives the final result of the decomposition; but I must remark that another compound precedes the formation of the chloranil. If the process be interrupted when the hydrate of phenyle becomes somewhat glutinous, a solid crystalline mass will result upon cooling, in which the chloranil is enveloped by a red, tenacious, disagreeably-smelling resin, dissolving in every proportion in alcohol, and this property allows of its easy separation from the former. When the alcoholic solution of the resinous mass is submitted to distillation, what remains in the retort becomes fluid as soon as the spirit has passed off, and along with the abundant disengagement of hydrochloric acid there distils over a heavy red oil, which sometimes solidifies in the receiver, charcoal remaining in the retort.

The distillate obtained in this way dissolves in potash or ammonia. Such a solution not containing an excess of the alkali, gives with copper and silver salts the purple, violet and yellow precipitates, characteristic of chlorophenissic and chlorophenussic acids.

The circumstances under which the acid had formed made it probable that it was chlorinated chlorindoptic acid (chlorophenussic acid), particularly as it appeared collaterally with chloranil in the method originally followed by Erdmann. It might however have been chlorophenissic, or else a mixture of the two acids.

To solve this question, I decomposed the filtered potash compound of the distillate with sulphuric acid, and obtained a feebly-coloured precipitate, which was collected on a filter, edulcorated, desiccated, and in order to remove the last traces of water, fused at a low temperature. By the distillation of this dry mass, a thick colourless oil condensed in the receiver, and also in the lower portion of the neck of the retort, which became a solid mass of a crystalline structure, possessing, with the exception of the odour, the greatest resemblance to stearic acid.

Determination of the chlorine in this compound:—

0·5229 grm. of substance ignited with lime, &c., gave 1·1334 grm. of chloride of silver = 53·47 per cent. chlorine.

The preceding analysis shows the compound to be chlorophenissic acid $C_{12}\begin{smallmatrix}H_2\\Cl_3\end{smallmatrix}O+HO=53{\cdot}51$ per ct. chlorine, while the chlorophenussic acid $C_{12}Cl_5O+HO=66{\cdot}50$ per ct. chlorine.

In this way a large quantity of chlorophenissic acid may be readily obtained as an accompanying product in the preparation of chloranil.

From the foregoing it is seen that the chlorophenissic acid can be transformed into chloranil, of which fact I have also convinced myself by direct experiment. It occurred to me to endeavour to produce chloranil from other members of the phenyle series. By a longer or shorter action of nitric acid upon hydrate of phenyle, Laurent converted it into nitrophenessic and nitrophenissic acids (carbazotic acid). These two acids, when treated with hydrochloric acid and chlorate of potash, yield chloranil with the greatest facility. When nitrophenessic acid is taken, alcohol should be employed, on account of its insolubility in water.

Schunck[*] obtained some years ago, in treating aloes with nitric acid, an acid (chrysolepinic) having the same composition and the greatest similarity with carbazotic acid, but which was said to be different on account of some deviations in its compounds. It was therefore the more interesting to produce chloranil from this body, as Marchand[†] has asserted lately that the carbazotic and the chrysolepinic acids are identical. The chrysolepinic acid is indeed transformed into chloranil. I have repeated this experiment several times with complete success, as well with a product which I prepared as with a specimen which Schunck had left in the collection of Professor Liebig.

The new researches of Gerhardt[‡] and Cahours[§] have shown so close a connection between the compounds of phenyle and those of salicyle, that the conversion of the latter into chloranil was to be expected. To produce this metamorphosis, it is indeed only necessary to boil salicylous acid (salicide of hydrogen), salicylic acid, or nitrosalicylic acid (indigotic or anilic acid), with hydrochloric acid and chlorate of potash. The decomposition in all these cases is very simple, the superfluous carbon escaping in the form of carbonic acid, *e.g.*

$$\underbrace{C_{14}H_5O_3 + HO}_{\text{Salicylous acid.}} + 10Cl + 4O = \underbrace{C_{12}Cl_4O_4}_{\text{Chloranil.}} + 2CO_2 + 6HCl.$$

Although it is known that benzoic acid and salicylous acid are identical, still the former, when treated as above, does not

[*] Liebig's *Ann.*, Bd. xxxix. S. 7.

[†] *Journ. für Prakt. Chem.*, Bd. xxxii. S. 35.

[‡] Liebig's *Ann.*, Bd. xlv. S. 19.

[§] *Ann. de Chim. et de Phys.*, 3me S. t. x. p. 327.

yield chloranil. This is the case also with nitrobenzoic acid, benzine, nitrobenzide, binitrobenzide, and hydruret of benzyle (oil of bitter almonds).

It yet remained to try the preparation of chloranil from salicine, the basis of all the salicyle compounds, when indeed the anticipated transformation occurred. This decomposition is readily comprehended, if it be remembered that the salicine contains the elements of 3 eqs. of salicylous acid and 10 eqs. of water, *e. g.*

$$\underbrace{C_{42}H_{28}O_{22}}_{\text{Salicine.}} + 24Cl + 18O = 3\underbrace{(C_{12}Cl\ \ O_4)}_{\text{Chloranil.}} + 6CO_2 + 16HO + 12HCl.$$

The salicine is, on account of its solubility in water, perhaps the fittest substance for yielding large quantities of chloranil: it is only requisite to dissolve salicine and chlorate of potash in boiling water and to add at intervals small quantities of hydrochloric acid. By degrees the mixture assumes a deep orange yellow colour, which is followed by a violent reaction; and besides the products of decomposition resulting from the chloric acid, there escapes a large portion of carbonic acid, and the surface of the liquid becomes covered with a thick layer of sulphur-yellow crystals of chloranil.

The salicine must not be treated with hydrochloric acid and then the crystals of chlorate of potash added, for the acid will transform the salicine into salicetine, which cannot be resolved into chloranil. Even by a long-continued boiling of the salicetine with the oxidizing mixture, nothing is obtained but a yellow uncrystalline mass insoluble in water and alcohol. I have made a burning of the chloranil procured from salicine and obtained the following numbers:—

0·373 grm. of chloranil gave 0·414 grm. of carbonic acid =30·27 per cent.

Neither phloridzine, phloretine, cumarin, nor cinnamic acid, are transformable into chloranil. The first three furnish, by the action of chlorate of potash, yellow resinous bodies insoluble in water but soluble in alcohol, while the last is converted into a colourless oil, to which I shall recur in a future paper. It is almost superfluous to remark, that if chloranil is prepared from isatine, chlorisatine or bichlorisatine, the transformation takes place quicker and more completely by the employment of a mixture of hydrochloric acid and chlorate of potash than by conducting a stream of chlorine through the alcoholic solution, for in a few minutes the conversion of considerable quantities is effected. Anthanilic acid also naturally suffers the same decomposition.

$$C_{14}H_7NO_4 + 4O + 8Cl = C_{12}Cl_4O_4 + 2CO_2 + NH_3 + 4HCl.$$

I obtained, by the direct action of the oxidizing mixture on indigo, only traces of chloranil.

In concluding, I wish to call the attention of chemists to the application of chlorate of potash as an oxidizing agent, for up to the present time it has been but seldom employed in organic chemistry, and yet the study of its action might in many cases lead to very remarkable results. I have treated *kinone*, which has lately become so interesting, in a similar manner to what has been stated, and found that it was transformed with the greatest facility into chloranil. In a future paper this subject will be examined more closely, and at the same time I shall throw out some hints concerning the relations of kinone to aniline, a base from which it can likewise be produced.

XXVII. *Account of an Experiment on Stockholm Pitch, confirming the Viscous Theory of Glaciers. In a Letter from* Prof. Gordon *of Glasgow, to* Prof. J. D. Forbes *of Edinburgh. Communicated by* Prof. J. D. Forbes *in a Letter to* Richard Taylor, *Esq.*

To Richard Taylor, Esq.

My dear Sir,

The inclosed communication from Mr. Gordon, Professor of Civil Engineering in the University of Glasgow, which he has allowed me to transmit to you for publication, will, I believe, be found interesting to your readers. The fact that pitch is susceptible of slow *fluid* motion, whilst it retains the character (in hand specimens) of a brittle solid, with a conchoidal fracture and glassy lustre, may assist in resolving the doubts of some impartial persons who have thought these characters in ice to be incompatible with such a motion as my theory of glaciers requires, whilst the structural bands having the frontal dip complete the analogy.

I remain, yours, very truly,

Edinburgh, February 6, 1845. James D. Forbes.

To Professor Forbes.

"When you requested me to give you a memorandum of what appeared to me to be the *very glacier-like motion* and appearance of Stockholm pitch flowing from a barrel, I considered my observation to have been too casual to be worth writing, and having foreseen that I could arrange an experiment at Gateshead in the beginning of the year, I delayed giving you the memorandum you wished. I had hoped to have been able to inspect and report on my experiment about this time, but I cannot go to Gateshead for some time to come, nor have I had any report of the progress of my pitch glacier since the 6th of January, when I was informed it had not moved since the day after I left it on the 28th of December. Your note of yesterday induces me to offer you the following still perfectly vivid impressions of the analogy between *ice* and *Stockholm pitch*.

"Allow me in the first place to mention that I read your Travels in the Alps in May last; that on the 24th of June I spent almost twenty hours on the glaciers of the Grindelwald. I went up by the lower glacier prepared with poles to prove its motion, and actually observed a progress of above twelve inches in the course of thirteen hours, from 6 a.m. to 7 p.m. I traced the 'dirt-bands' on the surface. I was let down into several crevasses, one of them to a depth of thirty feet, and could trace the *slaty structure* of the ice; the alternate clear blue thin veins, and the transition to opake gray, or even white. I descended from the glacier with a much better appreciation of the theory of glaciers than I had had, and a strong conviction that the facts I had observed could not be otherwise accounted for than by the mechanical theory you have given. In passing through Gateshead, in August, a broken-headed barrel of Stockholm pitch at the wire-rope factory attracted my attention. Its general appearance is represented in Plate I. fig. 1.

"A mass of Stockholm pitch *broken* from a barrel in August (at the time of the observations I am about to mention) presented a dark brown colour, a glassy lustre, translucent edges. The substance is fragile, fracture conchoidal and very uniform. A mass (fig. 4) which was brought to me by the workman having charge of this department, and which he had broken from the end of such a *stream* as I have represented coming from the barrel, presented generally the same appearance as a mass broken from an entire barrel*, but had this remarkable peculiarity, that there were lines,—structural lines, *a*, *a*, *a*, *a*, whose texture and colour were different from the general colour of the mass recognizable on such points as *b*, *b*, *b*, between any two such structural lines.

"Fig. 2 is an elevation of the stream of pitch, showing pretty nearly the dimensions and outward appearance of the stream. The striated *slaty* structure appears here on the outside, as is more distinctly (intended to be) shown in fig. 3. There were certain well-defined lines, and on either side of these, for some little distance, other small lines or cracks (but not *open* cracks or fissures), and then a space of smooth glassy-looking pitch.

"I am strongly impressed with the idea that the structural

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* The pitch is *fragile* at the same time that it *flows*.—L. G.

lines are a *result of the motion*, and that they correspond with the *veins* of glaciers; the lines incline most when the surface is steepest, as at *h*, fig. 3, and are very faint and nearly horizontal at *i*, where the surface of the stream is nearly so too. I left Gateshead without having an opportunity of getting a sectional view of this stream. I can get no *real* Stockholm pitch in Glasgow, else I should have made the experiment you have incited me to attempt here.

"I am, &c.,

"Glasgow, January 31, 1845." "LEWIS GORDON."

XXVIII. *On* Jacobi's *Elliptic Functions, in reply to* the Rev. Brice Bronwin; *and on Quaternions. By* ARTHUR CAYLEY, *Esq., B.A., F.C.P.S., Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS my last paper on Elliptic Integrals does not appear to have met with Mr. Bronwin's approbation, I will, with your permission, say a few more words on the question, and these will be the last I shall trouble you with on the subject. As Mr. Bronwin's last paper hardly professes to bring forward any new arguments, and complains of my not having condescended to reply to his previous ones, I shall endeavour at present to repair that omission. Mr. Bronwin says (Phil. Mag. S. 3. vol. xxiii. p. 90), "Moreover, he [Jacobi] has set out from an assumed equation $1-y=f(x)$, p. 39, from which all the rest of the formulæ are derived [and correctly derived, for Mr. Bronwin says afterwards, that "he supposes each particular step to be quite correct"]. In this assumed equation he has not actually determined the constants, but only assumed them. If he had actually determined them, it might appear that they are not susceptible of that generality which the author and Mr. Cayley suppose."

Jacobi is, of course, entitled to assume y any function of x that he pleases, and he might, if he had thought proper, have made ω perfectly determinate in his assumption, *e. g.* $\omega = \frac{K' i}{n}$. He then proves that this assumed value of y gives

$$\frac{dy}{\sqrt{\overline{1-y^2}\,\overline{1-\lambda^2 y^2}}} = \frac{dx}{M\sqrt{1-x^2}\sqrt{1-k^2x^2}}.$$

If then

$$x = \mathrm{s}\,a\,(u, k), \quad y = \mathrm{s}\,a\,(v, \lambda),$$

it is proved that

$$dv = \frac{1}{M}\,du, \quad \text{or} \quad v = \frac{u}{M}.$$

Admitting all this, how can a person deny it immediately, and say that the proof only applies to particular forms of ω? [Of course the proof assumes that $\omega = \frac{m\,K + m'\,K'\,\iota}{n}$, the reason of this being in order that the equation $s\,a\,.\,(u + 4\,n\,\omega) = s\,a\,.\,u$ may be satisfied; and this is the case for any value of ω included under the above form.]

But the real ground of Mr. Bronwin's objection is evidently the different results to which his own reasonings lead him. In the Mathematical Journal, p. 124, he says, as the foundation of his formulæ, "Make $\underline{\omega} = \frac{K}{n}$. Moreover, when $u = 0, \omega, 2\,\omega, \ldots$ suppose $v = 0$, H, 2 H, ..." (clearly meaning H to be analogous to K). And he then assumes a certain expression for $s\,a\,.\,v$, not to mention another one for $c\,.\,a\,.\,v$, the identity of which with the former is not satisfactorily proved (at least to me, but I am not pressing upon this at present). I believe in this case the two suppositions of the correspondence of values of u, v, and the assumed form of $s\,a\,.\,v$ are correct; still one ought to be deduced from the other, as Mr. Bronwin appears to admit (Phil. Mag.), "there is certainly room for discussion whether the quantities p, p' in the equation $\frac{u}{M} = p\,H + p'\,H'\,\iota$ are to be assumed or determined." This was my original objection, that they ought to be determined, and moreover, that in the cases Mr. Bronwin objects to, his assumption was incorrect. I had overlooked an equation in Jacobi (p. 59) which tends to confirm this; it is for the case of the impossible transformation $\omega = \frac{K'\,\iota}{n}$, viz. the formula $\Lambda_1' = \frac{K'}{n\,M_1}$, *i. e.* $\iota\Lambda_1' = \frac{\omega}{M_1}$, so that when $u = \omega$, v or $\frac{u}{M_1} = \Lambda'\,i$ (instead of Λ as Mr. Bronwin supposes [Λ is Jacobi's letter corresponding to H]). Of course I am not quoting this as proving the point; it is only that it enables me to retort Mr. Bronwin's challenge about the above transformation. Let him begin with the assumptions $\omega = \frac{K'\,\iota}{n}$, $u = 0, \omega, 2\,\omega, \ldots$ $v = 0$, $H'\,\iota$, $2\,H'\,\iota, \ldots$ and see what his theory will lead him to. I cannot undertake to do it myself, for I do not understand it; *I have worked out the particular case* $\omega = \frac{K'\,\iota}{3}$ *by* Jacobi's method, beginning, as I suppose

Mr. Bronwin wishes me to do, by expressing y by means of the complementary functions, and find that the process agrees step by step with Jacobi's, the only difference being that the transformation to the complementary functions is there made at the end.

An extraordinary assertion is the following one:—"It is sufficient to observe that the first form of ω only will satisfy the conditions $\mathrm{s}\,a\,.\,u = 0$, $\mathrm{s}\,a\,.\,u = 1$, required by Jacobi's theory, pages 40 and 41." If u is a misprint for v, and these equations are to be satisfied for $u = 0$, Jacobi's form of $\mathrm{s}\,a\,.\,v$ certainly satisfies them in any case; the first, because of the factor $s\,.\,a\,.\,u$; the second, because M is just determined by this very condition. If this is not the meaning, the true one has escaped me. One word on my preceding paper: the principal thing gained in it seems to be, its being likely to lead to the complete determination of the values of Λ, Λ' in the general case, a question which Jacobi has not examined; the principle is very clear, and one that is immediately suggested by Abel's formulæ, but I have no wish to force it upon Mr. Bronwin.

I remain, Gentlemen,
Your obedient Servant,
A. Cayley.

Cambridge, January 16, 1845.

P.S. *On Quaternions.*

It is possible to form an analogous theory with seven imaginary roots of (-1) (? with $\nu = 2^n - 1$ roots when ν is a prime number). Thus if these be $\iota_1, \iota_2, \iota_3, \iota_4, \iota_5, \iota_6, \iota_7$, which group together according to the types

$$123,\ 145,\ 624,\ 653,\ 725,\ 734,\ 176,$$

i. e. the type 123 denotes the system of equations

$$\iota_1\iota_2 = \iota_3,\quad \iota_2\iota_3 = \iota_1,\quad \iota_3\iota_1 = \iota_2,$$
$$\iota_2\iota_1 = -\iota_3,\quad \iota_3\iota_2 = -\iota_1,\quad \iota_1\iota_3 = -\iota_2,$$

&c. We have the following expression for the product of two factors:

$$\begin{array}{ll}
\multicolumn{2}{l}{(X_0 + X_1\iota_1 + \ldots X_7\iota_7)(X'_0 + X'_1\iota_1 + \ldots X'_7\iota_7)} \\
\multicolumn{2}{l}{= X_0X'_0 - X_1X'_1 - X_2X'_2 \ldots - X_7X'_7.} \\
+ [\overline{23} + \overline{45} + \overline{76} + (01)]\iota_1 & \text{where } (01) = X_0X'_1 + X_1X'_0 \\
+ [\overline{31} + \overline{46} + \overline{57} + (02)]\iota_2 & \quad\vdots \\
+ [\overline{12} + \overline{65} + \overline{47} + (03)]\iota_3 & \overline{12} = X_1X'_2 - X_2X'_1 \\
+ [\overline{51} + \overline{62} + \overline{47} + (04)]\iota_4 & \text{\&c.} \\
+ [\overline{14} + \overline{36} + \overline{72} + (05)]\iota_5 & \\
+ [\overline{24} + \overline{53} + \overline{17} + (06)]\iota_6 & \\
+ [\overline{25} + \overline{34} + \overline{61} + (07)]\iota_7 &
\end{array}$$

And the modulus of this expression is the product of the moduli of the factors. The above system of types requires some care in writing down, and not only with respect to the combinations of the letters, but also their order, it would be vitiated, *e.g.* by writing 716 instead of 176. A theorem analogous to that which I gave before, for quaternions, is the following:—If $\Lambda=1+\lambda_1 \iota_1 \,..\, +\lambda_7 \iota_7$, $X=x_1 \iota_1 \,..\, +x_7 \iota_7$. It is immediately shown that the possible part of $\Lambda^{-1} X \Lambda$ vanishes, and that the coefficients of $\iota_1, .. \iota_7$ are linear functions of $x_1 \,..\, x_7$. The modulus of the above expression is evidently the modulus of X; hence "we may determine seven linear functions of $x_1 \,..\, x_7$, the sum of whose squares is equal to $x_1^2 + \,..\, + x_7^2$." The number of arbitrary quantities is however only seven, instead of twenty-one, as it should be.

XXIX. *Observations on the Decomposition of Metallic Salts by an Electric Current. By* Mr. JAMES NAPIER*.

IN the paper I had the honour of reading before the Chemical Society at the close of the last session upon this subject, I stated my intention of bringing under their notice, in a series of short papers, such phænomena as seemed interesting which might present themselves in my daily avocations. In fulfilling this promise, I must beg leave to remind the Society that they are phænomena observed during the practical application of electro-metallurgy on the large scale, most of which are apparently at variance with many of the prescribed fundamental laws of electro-chemical decomposition, such as that referred to in my last communication, the non-transference of the base of an electrolyte, a subject I intend to discuss in a separate paper at an early opportunity. Some of the experiments were made on a small scale with different electrolytes from those used on the large scale, in order both to verify the results of practice and to ascertain if they were confined to the electrolytes there employed, which are the double cyanides of gold, silver and copper, with potassium. Some of these experiments will be given in detail.

It is laid down as a fundamental law in electro-decomposition, that there can be no inequality of force in any part of a voltaic current, and that the decompositions dependent upon the current are always in definite proportions, so that the amount of any element or salt radical liberated at one elec-

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* Communicated by the Chemical Society; having been read January 6, 1845.

trode is an exact equivalent of another element or salt radical liberated at the opposite electrode. But the results of experience in the deposition of metals from the above-named salts have induced the electro-metallurgist to follow another law somewhat at variance with that laid down by electro-chemists, and which enables him to strengthen or weaken his solution of metal at pleasure. Indeed there is no law which appears to him more definite than that, if a large positive and small negative electrode be used, there will be more dissolved from the former than is deposited upon the latter; and on the contrary, if a large negative and small positive electrode be used, more will be deposited upon the negative than is dissolved from the positive.

Being aware of the decided manner in which electro-chemists have laid down the law of the decomposition of electrolytes in definite proportions, I at first looked upon the results of practical experience with doubt, and in order to put it to the test, and to ascertain whether the discrepancy was accidental or not, I adopted the following practice:—the positive electrode was carefully weighed every morning, previous to immersing it in the gilding or plating solution, and again when taken out in the evening, and the loss compared with the amount deposited, an account of which was accurately taken. This practice was continued for upwards of nine months, varying at times, as far as practicable, the relative sizes of the two electrodes and the condition of the solutions, and during this period there were only five trials where the loss from the positive was exactly the amount of the gain on the negative. In many cases the difference did not exceed 3 per cent., but the average difference was 25 per cent., and occasionally as high as 50 per cent., varying in a great measure with the condition of the solution and the relative sizes of the electrodes.

During these operations two questions suggested themselves as of importance; namely, Is this discrepancy of equivalent electrolyzation confined to the double cyanides of the metals? and second, Does the amount of electricity, as indicated by the oxidation of the zinc in the battery, correspond with the amount of metal deposited upon the negative electrode, or the quantity dissolved from the positive?

I had observed, while making some experiments upon the relative endosmose of different substances, that if sulphate of copper be used in one division of a decomposition cell in which the negative electrode is placed, the other division being charged with water acidulated with hydrochloric acid, and a copper positive electrode immersed, there is dissolved in the positive cell an equivalent of copper for that deposited upon the negative pole, but there is formed besides a great quantity of white powder resembling a subchloride of copper, slightly soluble in water, which adheres to the pole with great tenacity, sometimes, if the current be continued several hours, to the depth of $\frac{1}{16}$th of an inch, by which means the positive pole will lose, according to the strength of acid and the power of battery, from $1\frac{1}{2}$ to 3 times the quantity of that deposited upon the negative electrode.

The substance formed and collected upon the pole I have not yet had an opportunity of analysing, but its general properties differ from those of the chloride or subchloride of copper; it is white when newly formed, but becomes green by washing and exposure to the air, and then slightly deliquescent. It gives an immediate deep blue colour with ammonia, dissolves in nitric acid with effervescence, also in hydrochloric acid, and is not precipitated by dilution, but is converted into the chloride: 25 grains gave 6·3 grs. of chlorine and 11·8 grs. of copper; it is probably an oxychloride.

By way of comparing these results with the practical experience alluded to, I instituted a series of small experiments, in which the second question referred to was particularly noticed; a few of these I will state in detail.

400 grains of cyanide of potassium were dissolved in 12 ounces of distilled water, divided into equal parts and placed in the two divisions of a decomposing apparatus, being separated by a porous diaphragm; silver electrodes of equal size were put into each and connected with a battery of 9 pairs of zinc and copper plates; the negative division was so constructed that the gas evolved from the electrode was received into a graduated jar; the current was continued until 25 cubic inches of gas were obtained; the two electrodes were then weighed; the negative was found unchanged, the positive had lost 62 grains, which is 8 grains more than the equivalent of hydrogen evolved.

The same experiment was repeated with 600 grains of cyanide of potassium to the 12 ounces of water; the positive electrode in this instance had lost 70·5 grs., being 16·5 grs. more than the equivalent of gas evolved.

Again repeated, having 800 grs. of cyanide of potassium, when the positive electrode had lost 85 grs., or 31 grs. more than the equivalent of gas evolved. The waste or destruction of zinc in the battery for these three experiments, amounted to 506 grs., which is more than an equivalent of zinc for the gas evolved at the negative pole, but is not an equivalent for the silver dissolved from the positive.

In case of any absorption of gas in the negative cell producing an erroneous result, I charged the negative division with a solution of sulphate of copper, in which was immersed a copper electrode, the amount of metal deposited being taken as the measure of electrolyzation in this cell; the positive division was charged with a solution of cyanide of potassium, in which was placed a silver electrode, the whole being attached to a single pair of zinc and copper elements excited by a solution of sal-ammoniac. In the three following experiments the current was only continued one hour for each:—

	I.	II.	III.
Copper deposited on the negative electrode	5·0 grs.	6·2 grs.	5·5 grs.
Silver dissolved from the positive electrode	37·0 ...	39·3 ...	40·7 ...
Zinc dissolved in the battery	5·8 ...	6·5 ...	6·3 ...

In these experiments we have nearly two equivalents more dissolved from the positive pole than is deposited upon the negative.

Another experiment with the same electrolytes, but both positive and negative electrodes being pure copper, the current was continued for sixteen hours,—

The negative electrode had gained .	22 grs.
The positive electrode had lost . .	75 ...
The zinc dissolved in the battery was	25 ...

Here we have upwards of three equivalents dissolved from the positive pole to one deposited upon the negative; in all these experiments the cyanide of potassium was in the proportion of 40 grs. to the ounce of water.

In the three following experiments hydrochloric acid was used in the positive division instead of cyanide of potassium, the negative containing a solution of sulphate of copper; the battery consisted of a single pair excited with a solution of common salt:—

I. Hydrochloric acid in the proportion of 1 ounce to 12 of water, the current kept up for eighteen hours,—

Deposited upon the negative electrode	15 grs.
Dissolved from the positive electrode .	22 ...
Zinc dissolved in the battery . . .	18 ...

II. Hydrochloric acid, 2 to 12 of water, current passing for twenty-four hours,—

Deposited upon the negative electrode	40 grs.
Dissolved from the positive electrode .	62 ...
Zinc dissolved in the battery . . .	48 ...

III. Hydrochloric acid, 3 to 12 of water, current passing for twenty-four hours,—

Deposited upon the negative electrode 47 grs.
Dissolved from the positive electrode . 87 ...
Zinc dissolved in the battery . . . . 50 ...

Similar experiments were made with the positive division charged with a solution of chloride of sodium, chloride of potassium, chloride of ammonium and several other chlorides, in the whole of which there was more dissolved from the positive electrode than was deposited on the negative. With dilute sulphuric acid, sulphate of copper, or any of the soluble sulphates, in the positive division, the quantity deposited was exactly equivalent to that dissolved from the positive, except where the current was continued for several days, when the positive electrode was slightly covered with a black gelatinous substance, supposed by some to be oxide of copper, formed from an imperfect transfer of the sulphuric acid, which leaves it undissolved; but in such cases I have always found an equivalent of copper held in solution to that deposited, this black matter being also formed. I do not refer here to the black matter which is a constant attendant when impure copper or salts of copper are used, and which is in a great measure caused by the iron in the salt or in the metal forming the electrode. When the nitrates were used in the positive division, the results were the same as with the sulphates, and when long continued, a little brownish matter was formed on the positive electrode, and there was consequently more dissolved from the positive than was deposited on the negative.

When 1 part of nitric acid was used to 12 of water, there was nearly one-fourth more dissolved from the positive than was deposited upon the negative, although a piece of copper put into acid of the same strength and for the same length of time as the battery was in action, was not perceptibly acted upon. Other phænomena were observed during these experiments, which I shall take occasion to notice on a future opportunity, but will now offer a few general remarks upon the results detailed. In the first place it may be stated generally, that when the double cyanides are the electrolytes, the amount of metal liberated at the cathode and dissolved from the anode is not in equivalent proportions, but varies according to their relative size and the condition of the solution; when the solutions are neutral, there is a tendency in favour of more being deposited upon the cathode than is dissolved from the anode, even when they are equal in size; and when there is an excess of cyanide of potassium, the tendency is great for the solution of more from the positive than what is deposited on the negative electrode. If, for example, the positive is made to surround the negative, exposing about three times more surface, there will be an average of one-third more dissolved than is deposited; and if, on the contrary, we deposit on the inside of the vessel, the positive pole being suspended within, there will be about one-fourth more deposited upon the article than what is dissolved from the pole; but of course this always varies with the condition of the solution.

From these, and some of the experiments with cyanide of potassium, hydrochloric acid and the chlorides, it is evident with such electrolytes, that when electrodes are used which combine with the elements set free, the amount liberated or combined with one electrode is not a measure of that liberated at or combined with the opposite electrode; but we must observe, as the result of experience, that the element liberated at the negative electrode is a proper measure of the amount of electricity passing through the electrolyte, if that electrolyte be composed of 1 equivalent of base and 1 of acid, or of single equivalents of elements; but if a subsalt be the electrolyte, there will be two proportions liberated at the negative for one proportion of zinc destroyed in the battery; and if a trisubsalt be used, 3 equivalents will be deposited, &c.; all electrolytes being decomposed as if they were constituted of two elements.

It may be remarked in the second place, from these results, that the stronger the solution of cyanide of potassium is, there is comparatively more dissolved from the positive electrode; that this may arise from the metals used as electrodes being very soluble in that menstruum, and that the difference between the electrodes is simply the result of this solubility; that it is the result of solubility rather than direct electrolysis, the experiments detailed fully show; but that the electricity influences this solubility I think evident. Pieces of gold, silver and copper, of the same dimensions with the positive electrode, were immersed in solutions of cyanide of potassium, of the same strength with that used in the experimental cell, and kept in the same length of time. When the gold positive electrode had lost 59 grs. above the equivalent of what was deposited upon the negative, the gold plate in solution of cyanide of potassium had only lost one half-grain, and required eight weeks to lose 59 grs.

When the silver positive electrode had lost 96 grs. over an equivalent of that deposited on the negative, the plate immersed in solution of cyanide of potassium had lost 3 grs., and required twenty-nine days to lose 96 grs.

When the copper positive electrode had lost 32 grs. more than the amount deposited upon the negative, the copper plate in solution of cyanide of potassium had lost 2 grs., and required twelve days to lose 32 grs.; the copper was comparatively quicker in being dissolved.

The same tendency in the positive electrode to dissolve was exhibited with the hydrochloric acid, increasing according to the strength of the acid. It was also manifest in all the chlorides; indeed every electrolyte tried indicated this tendency to a certain extent; the metals used as electrolytes were pure. Whether this increased tendency in the metals to dissolve, when made the positive electrode, be an exalted affinity excited between the metal and the negative element of the electrolyte, we will not as yet venture to assert, although the facts bear evidence to this; and if so, we think it favours the argument, that chemical action is the result of electricity, rather than electricity being the result of chemical action. But there being many phænomena in the practice of electrometallurgy favourable to this view, I will leave the further notice of this subject till opportunity enables me to bring these facts under your notice.

XXX. *Additional Remarks on Glacier Theories.*

By Dr. WHEWELL.

To Richard Taylor, Esq.

DEAR SIR,

I WISH to add a few remarks to those I made on glaciers in the Philosophical Magazine last month. I then pointed out the differences which would exist in the condition of a glacier, according as it was solid and flexible, or plastic. I remarked that plastic or viscous substances are those in which the particles can, when urged by suitable forces, overcome their first attachment, slide past each other, and attach themselves to new particles. Of course this process occupies time; and in a plastic moving mass, the motion of the parts will be slower as the forces of attachment are greater, compared with the forces which produce motion; that is, as the mass is less plastic or more viscous. I now wish to point out the steps by which, in hypothesis, the transition might be made from a solid to a plastic glacier; the latter being, as I before showed, the hypothesis which gives results agreeing with the facts of glaciers.

Supposing a glacier theorist to begin by supposing a glacier to be solid (as opposed to plastic), with or without flexibility. As a glacier evidently is not a simple mass sliding down an inclined plane with an accelerated motion, he would have to consider the force exerted by one part of the mass upon the others.

The parts which were most free to move would exert tensions upon the rest, and would tend to produce fissures. At first, the theorist would attend only to the direct cohesion of the mass. If this were overcome, there would be transverse fissures; and these would be the only results of the first form of the hypothesis. But it is evident in glaciers, that the middle moves faster than the sides. It would be necessary to make some provision for this difference in the hypothesis. The first supposition might be, that the glacier is divided by arbitrary longitudinal separations into strips of finite breadth; and that these move past each other. But on this supposition, original transverse lines on the glacier would be thrown *en échellon* in definite portions. This evidently not being the case in fact, the theorist might remodel his view. He might begin to see, that besides the separation of a mass by overcoming the direct cohesion, there may be separation by sliding of one part past another. He would then be led, by simple mechanical views, to infer that the cohesive force of the mass will yield to such sliding tendency, in directions perpendicular to the directions of the transverse fissures; and as the mass is still coherent after sliding, there will be in it bands, which are the traces of the sliding lines. If this process take place over the whole breadth of the glacier, we are brought very near the plastic view. On this supposition, the differences of velocity of the different parts of a transverse line will be much the greatest near the sides, as they are in a river, and for the same reason. Hence any attempt to calculate the relative motion of the parts of the glacier would lead to the conclusion, that the flanks, at least, must be plastic. If the conception of the change of a plastic body, as requiring time to its performance, were new to the theorist, and had been slowly arrived at, he might, with reference to these circumstances, term it *secular* plasticity: but this epithet, as applied to the property itself, is superfluous, since it merely expresses a necessary condition of all plasticity; and inappropriate, when applied to a process which takes place in a few days in a conspicuous manner.

When we have gone so far towards the plastic view of glaciers, there still remains a point in which that view may be resisted. It may be said, that though plastic when considered horizontally, they are not plastic when considered vertically; that though the middle moves faster than the sides, the top does not move faster than the bottom. And this assertion would be difficult to disprove by any experiments made from the upper surface of a glacier, because, just as the greatest differences of velocity are greatest near the sides, so are they greatest near the bottom; and the upper and central portion of the glacier moves onward without much relative motion of its parts. But still it is to be supposed that a theorist who had accepted the horizontal plasticity, would not long continue to reject the vertical plasticity; and thus, beginning from the hypothesis of solid glaciers, he would be led to the conviction of plastic glaciers.

I have traced these changes of hypothesis, and the final result, of a plastic glacier, as if any theorist might have been led to make these successive steps by the obvious necessity of the case. But we know well, from the history of science, that the progress from wrong to right is not usually made in this direct and simple manner; and that, till some discoverer has firmly seized and clearly presented the ultimate truth, theorists manifest a wonderful ingenuity in taking the wrong turn at every step of their advance. In the case of glaciers, the view of them as plastic, and the application of that view in a very striking and extensive manner to the facts of their motion and structure, is, as all your readers know, due to Prof. Forbes. He is the discoverer on this subject; and to him the scientific world is indebted for a theory of glaciers so convincing, that to it the views of other theorists are steadily converging. This being the obligation for which speculators respecting glaciers are indebted to him, it would be a very unworthy return for such a benefit, if they were to write as if they owed nothing to him; and were to present the above-noticed steps of the transition from the solid to the plastic hypothesis, as if they were the necessary results of their own reasonings, and not merely modified forms of expressing views suggested by the writings of Prof. Forbes.

There is one point on which I have as yet said nothing,—the condition and motion of the lower surface of the glacier. This point, though not unimportant, offers nothing of novelty; the glacier slides by the constant melting of its lower surface in virtue of the heat of the subjacent soil. This was distinctly taught by De Saussure, and has long been the view familiar to the minds of geologists, English, French and German; as it would be easy to show if it were necessary. If we combine this doctrine with that of the plasticity of the mass, we have a complete view of the nature of glacier motion. Prof. Forbes's theory, of course, includes what is true in that of Saussure.

Since these views are thus combined, it may be asked what is the principal *cause* of glacier motion? Is it gravity? or the melting of the lower surface? or the plasticity of the mass? But it is plain that if this theory be adopted in fact, the question among these forms of expression is quite unimportant; just as it would be if, in the case of a river, we were to ask whether the *cause* of the motion is gravity, or the fluidity of the lowest part, or the fluidity in general. Without attaching much value to such distinctions, we may reply, that the cause of the motion is gravity; the cause of the steadiness of the motion is the melting of the lower surface; the cause of the glacier character of the motion is the plasticity.

I am, dear Sir,
Your faithful Servant,
W. WHEWELL.

Trinity College, Cambridge,
February 18, 1845.

XXXI. *On Quaternions; or on a new System of Imaginaries in Algebra. By* Professor Sir WILLIAM ROWAN HAMILTON, *LL.D., P.R.I.A., Corresponding Member of the Institute of France, and Royal Astronomer of Ireland.*

[Continued from vol. xxv. p. 246.]

12. A QUATERNION, Q, divided by its modulus, μ, may in general (by what has been shown) be put under the form,

$$\mu^{-1} \mathrm{Q} = \cos\theta + i_{\mathrm{R}} \sin\theta;$$

in which θ is a real quantity, namely the amplitude of the quaternion; and i_{R} is an imaginary unit, or square root of a negative one, namely that particular root, or unit, which is distinguished from all others by its two directional coordinates, and is constructed by a straight line drawn from the origin of coordinates to the representative point R; this point R being on the spheric surface which is described about the origin as centre, with a radius equal to unity. Comparing this expression for $\mu^{-1}\mathrm{Q}$ with the formula (M.) for the product of any two imaginary units, we see that if with the point R as a positive pole, we describe on the same spheric surface an arc $\mathrm{P}'\,\mathrm{P}''$ of a great circle, and take this arc $=\pi-\theta=$ the supplement of the amplitude of Q; and then consider the points P' and P'' as the representative points of two new imaginary units $i_{\mathrm{P}'}$ and $i_{\mathrm{P}''}$, we shall have the following *general transformation for any given quaternion,*

$$\mathrm{Q} = \mu\, i_{\mathrm{P}'} i_{\mathrm{P}''};$$

the arc $\mathrm{P}'\,\mathrm{P}''$ being given in length and in direction, except that it may turn round in its own plane (or on the great circle to which it belongs), and may be increased or diminished by any whole number of circumferences, without altering the value of Q.

13. Consider now the *product of several successive quaternion factors* Q_1, Q_2, .. under the condition that their ampli-

tudes θ_1, θ_2, .. shall be respectively equal to the angles of the spherical polygon which is formed by their representative points R_1, R_2, .. taken in their order. To fix more precisely what is to be understood in speaking here of these angles, suppose that R_m is the representative point of the *m*th quaternion factor, or the *m*th corner of the polygon, the next preceding corner being R_{m-1}, and the next following being R_{m+1}; and let the angle, or (more fully) the *internal angle*, of the polygon, at the point R_m, be denoted by the same symbol R_m, and be defined to be the least angle of rotation through which the arc $R_m R_{m+1}$ must revolve in the positive direction round the point R_m, in order to come into the direction of the arc $R_m R_{m-1}$. Then, the rotation $2\pi - R_m$ would bring $R_m R_{m-1}$ to coincide in direction with $R_m R_{m+1}$; and therefore the rotation $\pi - R_m$, performed in the same sense or in the opposite, according as it is positive or negative, would bring the *prolongation* of the preceding arc $R_{m-1} R_m$ to coincide in direction with the following arc $R_m R_{m+1}$; on which account we shall call this angle $\pi - R_m$, taken with its proper sign, the *external angle* of the polygon at the point R_m. The same rotation $\pi - R_m$ would bring the positive pole, which we shall call P_{m+1}, of the preceding side $R_{m-1} R_m$ of the polygon, to coincide with the positive pole P_{m+2} of the following side $R_m R_{m+1}$ thereof, by turning round the corner R_m as a pole, in an arc of a great circle, and in a positive or negative direction of rotation according as the external angle $\pi - R_m$ of the polygon is itself positive or negative; consequently, by the last article, we shall have the formula

$$\mu_m^{-1} Q_m = \cos R_m + i_{R_m} \sin R_m = i_{P_{m+1}} i_{P_{m+2}}.$$

Multiplying together in their order the *n* formulæ of this sort for the *n* corners of the polygon, and attending to the *associative* character of quaternion multiplication, which gives, as an extension of the formula (P.), the following,

$$i_{P_1} i_{P_2} . i_{P_2} i_{P_3} . \dots i_{P_n} i_{P_1} = (-1)^n, \quad . \quad . \quad . \quad (\mathrm{P}'.)$$

we see that under the supposed conditions as to the amplitudes we have this expression for the product of the *n* quaternion factors,

$$Q_1 Q_2 Q_3 \dots Q_n = (-1)^n \mu_1 \mu_2 \mu_3 \dots \mu_n;$$

from which it follows, that for *any spherical polygon* $R_1 R_2 \dots R_n$, (even with salient and re-entrant angles), this general relation holds good:

$$\left.\begin{array}{l}(\cos \mathrm{R}_1 + i_{\mathrm{R}_1} \sin \mathrm{R}_1)(\cos \mathrm{R}_2 + i_{\mathrm{R}_2} \sin \mathrm{R}_2) \\ \quad \ldots (\cos \mathrm{R}_n + i_{\mathrm{R}_n} \sin \mathrm{R}_n) = (-1)^n\end{array}\right\} . \quad . \quad . \quad (\mathrm{R}.)$$

14. For the case of a spherical triangle R R′ R″, this relation becomes

$$\left.\begin{array}{l}(\cos \mathrm{R} + i_{\mathrm{R}} \sin \mathrm{R})(\cos \mathrm{R}' + i_{\mathrm{R}'} \sin \mathrm{R}')(\cos \mathrm{R}'' + i_{\mathrm{R}''} \sin \mathrm{R}'') \\ \quad = -1;\end{array}\right\} (\mathrm{I}''.)$$

and reproduces the formula (I′.), when we multiply each member, as multiplier, into $\cos \mathrm{R}'' - i_{\mathrm{R}''} \sin \mathrm{R}''$ as multiplicand. The restriction, mentioned in a former article, on the direction of the positive semiaxis of one coordinate after those of the two other coordinates had been chosen, was designed merely to enable us to consider the three angles of the triangle as being each positive and less than two right angles, according to the usage commonly adopted by writers on spherical trigonometry. It would not have been difficult to deduce reciprocally the theorem (R.) for any spherical polygon, from the less general relation (I′.) or (I″.) for the case of a spherical triangle, by assuming any point P upon the spherical surface as the common vertex of n triangles which have the sides of the polygon for their n bases, and by employing the associative character of multiplication, together with the principle that codirectional quaternions, when their moduli are supposed each equal to unity, are multiplied by adding their amplitudes. This last principle gives also, as a verification of the formula (R.), for the case of an infinitely small, or in other words, a *plane polygon*, the known equations,

$$\cos \Sigma \mathrm{R} = (-1)^n, \quad \sin \Sigma \mathrm{R} = 0.$$

15. The associative character of multiplication, or the formula (Q.), shows that if we assume any three quaternions Q, Q′, Q″, and derive two others $\mathrm{Q}_{\prime}$, $\mathrm{Q}_{\prime\prime}$ from them, by the equations

$$\mathrm{Q}\,\mathrm{Q}' = \mathrm{Q}_{\prime}, \quad \mathrm{Q}'\,\mathrm{Q}'' = \mathrm{Q}_{\prime\prime},$$

we shall have also the equations

$$\mathrm{Q}_{\prime}\,\mathrm{Q}'' = \mathrm{Q}\,\mathrm{Q}_{\prime\prime} = \mathrm{Q}''',$$

Q‴ being a third derived quaternion, namely the ternary product Q Q′ Q″. Let R R′ R″ $\mathrm{R}_{\prime}$ $\mathrm{R}_{\prime\prime}$ R‴ be the six representative points of these six quaternions, on the same spheric surface as before; then, by the general construction of a product assigned in a former article*, we shall have the following expressions for the six amplitudes of the same six quaternions:

$$\begin{array}{ll}\theta = \mathrm{R}'\,\mathrm{R}\,\mathrm{R}_{\prime} = \mathrm{R}_{\prime\prime}\,\mathrm{R}\,\mathrm{R}'''; & \theta_{\prime} = \mathrm{R}''\,\mathrm{R}_{\prime}\,\mathrm{R}''' = \pi - \mathrm{R}\,\mathrm{R}_{\prime}\,\mathrm{R}'; \\ \theta' = \mathrm{R}_{\prime}\,\mathrm{R}'\,\mathrm{R} = \mathrm{R}''\,\mathrm{R}'\,\mathrm{R}_{\prime\prime}; & \theta_{\prime\prime} = \mathrm{R}'''\,\mathrm{R}_{\prime\prime}\,\mathrm{R} = \pi - \mathrm{R}'\,\mathrm{R}_{\prime\prime}\,\mathrm{R}''; \\ \theta'' = \mathrm{R}_{\prime\prime}\,\mathrm{R}''\,\mathrm{R}' = \mathrm{R}'''\,\mathrm{R}''\,\mathrm{R}_{\prime}; & \theta''' = \pi - \mathrm{R}_{\prime}\,\mathrm{R}'''\,\mathrm{R}'' = \pi - \mathrm{R}\,\mathrm{R}'''\,\mathrm{R}_{\prime\prime};\end{array}$$

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* In the Number of this Magazine for July 1844, S. 3. vol. xxv.

$R' R R_\prime$ being the spherical angle at R, measured from $R R'$ to $R R_\prime$, and similarly in other cases. But these equations between the spherical angles of the figure are precisely those which are requisite in order that the two points $R_\prime$ and $R_{\prime\prime}$ should be the two *foci of a spherical conic inscribed in the spherical quadrilateral* $R R' R'' R'''$, or touched by the four great circles of which the arcs $R R'$, $R' R''$, $R'' R'''$, $R''' R$ are parts; this geometrical relation between the six representative points $R R' R'' R_\prime R_{\prime\prime} R'''$ of the six quaternions Q, Q', Q'', QQ', $Q'Q''$, $QQ'Q''$, which may conveniently be thus denoted,

$$R_\prime R_{\prime\prime} (\,.\,.\,) R R' R'' R''', \quad . \quad . \quad . \quad . \quad (Q''.)$$

is therefore a consequence, and may be considered as an interpretation, of the very simple algebraical theorem for three quaternion factors,

$$Q Q' . Q'' = Q . Q' Q''. \quad . \quad . \quad . \quad . \quad (Q.)$$

It follows at the same time, from the theory of spherical conics, that the two straight lines, or *radii vectores*, which are drawn from the origin of coordinates to the points $R_\prime$, $R_{\prime\prime}$, and *which construct the imaginary parts of the two binary quaternion products* $Q Q'$, $Q' Q''$, *are the two focal lines of a cone of the second degree, inscribed in the pyramid which has for its four edges the four radii which construct the imaginary parts of the three quaternion factors* Q, Q', Q'', *and of their continued* (*or ternary*) *product* $Q Q' Q''$.

16. We had also, by the same associative character of multiplication, analogous formulæ for any four independent factors,

$$Q . Q' Q'' Q''' = Q Q' . Q'' Q''' = \&c.; \quad . \quad . \quad (Q'.)$$

if then we denote this continued product by Q^{IV}, and make

$$Q Q' = Q_\prime, \quad Q' Q'' = Q_\prime', \quad Q'' Q''' = Q_\prime'',$$
$$Q Q' Q'' = Q_\prime''', \quad Q' Q'' Q''' = Q_\prime^{\text{IV}},$$

and observe that whenever E and F are foci of a spherical conic inscribed in a spherical quadrilateral A B C D, so that, in the notation recently proposed,

$$E F (\,.\,.\,) A B C D,$$

then also we may write

$$F E (\,.\,.\,) A B C D, \text{ and } E F (\,.\,.\,) B C D A,$$

we shall find, without difficulty, by the help of the formula $(Q''.)$, the five following geometrical relations, in which each R is the representative point of the corresponding quaternion Q:

$$\left.\begin{array}{l}
R_{\prime} R_{\prime}' (\,..\,) R R' R'' R_{\prime}''';\\
R_{\prime}' R_{\prime}'' (\,..\,) R' R'' R''' R_{\prime}^{\text{iv}};\\
R_{\prime}'' R_{\prime}''' (\,..\,) R'' R''' R^{\text{iv}} R_{\prime};\\
R_{\prime}''' R_{\prime}^{\text{iv}} (\,..\,) R''' R^{\text{iv}} R R_{\prime}';\\
R_{\prime}^{\text{iv}} R_{\prime} (\,..\,) R^{\text{iv}} R R' R_{\prime}''.
\end{array}\right\} \quad . \; . \; . \; . \quad (Q'''.)$$

These five formulæ establish a remarkable *connexion between one spherical pentagon and another* (when constructed according to the foregoing rules), through the medium of *five spherical conics*; of which five curves each touches two sides of one pentagon and has its foci at two corners of the other. If we suppose for simplicity that each of the ten moduli is $= 1$, the dependence of six quaternions by multiplication on four (as their three binary, two ternary, and one quaternary product, all taken without altering the order of succession of the factors) will give eighteen distinct equations between the ten amplitudes and the twenty polar coordinates of the ten quaternions here considered; it is therefore in general permitted to assume at pleasure twelve of these coordinates, or to choose six of the ten points upon the sphere. Not only, therefore, may we in general take *one of the two pentagons arbitrarily*, but also, at the same time, may assume *one corner of the other pentagon* (subject of course to exceptional cases); and, after a suitable choice of the ten amplitudes, the five relations (Q'''.), between the two pentagons and the five conics, will still hold good.

17. A very particular (or rather limiting) yet not inelegant case of this theorem is furnished by the consideration of the plane and regular pentagon of elementary geometry, as compared with that other and interior pentagon which is determined by the intersections of its five diagonals. Denoting by $R_{\prime}$ that corner of the interior pentagon which is nearest to the side $R R'$ of the exterior one; by $R_{\prime}'$ that corner which is nearest to $R' R''$, and so on to $R_{\prime}^{\text{iv}}$; the relations (Q'''.) are satisfied, the symbol (. .) now denoting that the two points written before it are foci of an ordinary (or plane) ellipse, inscribed in the plane quadrilateral whose corners are the four points written after it. We may add, that (in this particular case) two points of contact for each of the five quadrilaterals are corners of the interior pentagon; and that the axis major of each of the five inscribed ellipses is equal to a side of the exterior figure.

[To be continued.]

XXXII. *On the Decomposition of Oxides and Salts by Chlorine.* *By* Alexander W. Williamson, *Esq.*[*]

THE compounds formed by the action of chlorine on the hydrates of the alkalies and earths have long since attracted the attention of chemists by their powerful bleaching properties. As they were produced by simply bringing together chlorine with the base in presence of water, they were at first considered as direct and simple combinations of chlorine with the oxide, and were therefore called oxychlorides.

Berzelius first showed this view to be untenable. He considered these compounds as containing a chloride and an oxygen salt of chlorine, the acid of which he assumed identical with that of Stadion, namely, 1 equivalent of chlorine to 3 of oxygen.

Serullas obtained by the evaporation of a solution of soda, which had been treated with chlorine, crystals of chloride of sodium, while the mother-liquid retained its bleaching properties unaltered. This experiment shows that a chloride and an oxygen salt of chlorine, which possesses the bleaching properties, are contained in these compounds.

By acting on the oxide of mercury with chlorine, Balard succeeded in obtaining a combination of oxygen with chlorine, which in its bleaching properties, as well as its salts, showed much resemblance to the oxygen acid supposed to be contained in the oxychlorides. He determined the composition of this acid at 1 equivalent chlorine to 1 of oxygen.

Millon has endeavoured to recall the ancient view with some modification. He considers these compounds as analogous to peroxides, so that the atoms of oxygen which the peroxide of a metal contains beyond its oxide are here replaced by chlorine.

This view was but imperfectly refuted, and indeed the subject remained in considerable doubt until Gay-Lussac made known his interesting researches on the combinations of chlorine with bases.

I will here recall some of the principal facts, but must refer for further particulars to the original treatise of this great master.

If a mixture of a chloride with a corresponding salt of hypochlorous acid in their equivalent parts be treated with sulphuric acid in excess, chlorine is given off. If, on the contrary, the acid be added in a dilute state, and prevented by continual agitation from acting unduly on any particular part, and only so much acid be added as is sufficient to decompose the oxygen salt, then hypochlorous acid is alone set free, and can be di-

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[*] Communicated by the Chemical Society; having been read December 2, 1844.

stilled over unchanged. The so-called oxychlorides, when similarly treated, behave in the same manner.

The process in these instances is extremely simple, if we only recollect that hypochlorous acid with hydrochloric sets chlorine free by forming water; or, which is the same thing, the metal of the chloride deoxidates the hypochlorous acid in order to be able to combine as oxide with sulphuric acid, while from both the chlorine is set free.

In the second instance the salt of hypochlorous acid is alone decomposed, while no excess of sulphuric acid is present to cause a decomposition of the chloride.

Now if these oxychlorides were simple combinations of chlorine with oxide, the first drop of sulphuric acid would set free chlorine.

Founded on the correctness of this view, this great chemist has given us a method of preparing hypochlorous acid, far more practical than that formerly used. He distils the so-called chloride of lime with dilute nitric acid, which must be very carefully added in the manner above described, and distils off the hypochlorous acid. He confirms Balard's statement of its composition. Hypochlorous acid possesses, according to Gay-Lussac, the following properties:—It is a feeble acid, weaker perhaps than carbonic, although they mutually expel one another from their salts. Its salts have a great inclination to decompose into chlorides and chlorates. These salts, as such, possess no bleaching properties. In order to give a bleaching reaction, they must be decomposed by a stronger acid. They are partly decomposed by chlorine. Hypochlorous acid can be distilled over from any of these salts after treating it with chlorine.

I will now communicate some experiments which I have made on the action of chlorine on bases, and the products of this action. My principal object during these experiments was to become better acquainted with the manner of the formation, with the properties and decompositions of this most interesting compound, hypochlorous acid.

The experiments were performed in the laboratory at Giessen, and I had, during the course of them, the advantage of the presence and counsel of Professor Liebig.

The first point which I endeavoured to ascertain was the quantity of chlorine with which a base is capable of combining. Barytes was first subjected to this examination. A concentrated solution of this earth was saturated with chlorine by conducting a stream of the gas, which had been washed by water, slowly into it. This process was not considered complete until the liquid had assumed a strong colouring of chlorine. It was then agitated with a repeatedly renewed quantity of atmospheric air until no free chlorine could be perceived in it. This solution had the peculiar astringent taste of hypochlorous acid, and its powerful bleaching properties. It was oversaturated with ammonia and heated to the boiling-point, and having added an excess of nitric acid, it was precipitated by nitrate of silver. After separating it from this precipitate, the barytes was thrown down by sulphuric acid. Three determinations, conducted in this manner, gave the following results. For each determination a fresh liquid was prepared.

I. 0·550 chloride of silver =0·136 chlorine.
0·243 sulphate of barytes=0·159 barytes.
This gives 818 chlorine to 957 barytes.

II. 1·468 chloride of silver =0·361 chlorine.
0·654 sulphate of barytes=0·429 barytes.
This gives 805 chlorine to 957 barytes.

III. 1·836 chloride of silver =0·453 chlorine.
0·781 sulphate of barytes=0·513 barytes.
This gives 843 chlorine to 957 barytes.

The average of these three gives 822 chlorine to 957 barytes, or nearly 2 equivalents.

The question which now naturally presents itself is, in what manner can the chlorine be contained in this liquid, and in what combinations? The following experiments were instituted in order to ascertain this:—

To part of the original liquid a solution of nitrate of silver was added, by which a *pure white* precipitate of chloride of silver was formed, which increased by standing. Another portion was treated with barytes water until the smell and bleaching reaction of hypochlorous acid had disappeared. Nitrate of silver now formed a black precipitate, which gradually decomposed, giving off oxygen.

The cause of this striking difference in the two reactions is evidently that in the first instance all the base was combined with chlorine in the form of chloride, while hypochlorous acid remaining free in the liquid, gradually increased the precipitate by its own decomposition. In the second case, the hypochlorous acid being combined with barytes gave the black precipitate, consisting of a mixture of chloride with peroxide of silver.

To test the correctness of this explanation the following experiments were made:—

Aqueous hypochlorous acid was neutralized with barytes. This salt possessed no bleaching properties. Carbonic acid precipitated carbonate of barytes. The liquid, after separation from the precipitate, was boiled until all free hypochlorous acid had disappeared. It now contained no traces of a hypochlorous acid salt, as proved by the fact that tincture of litmus was not bleached by it, though an excess of sulphuric acid was added, proving that the barytes salt was completely decomposed by carbonic acid.

A solution of barytes was saturated with chlorine in the manner above described, and being freed from superfluous chlorine, was now treated in a similar manner with carbonic acid. No precipitate whatever was formed, which must have been the case had any hypochlorous acid been combined with barytes. The hypochlorous acid salt, formed by the action of the first equivalent of chlorine with the base, had thus been entirely decomposed by the second equivalent.

When I further state, that on this liquid being subjected to distillation, hypochlorous acid passed over, while the residue consisted of chloride of barium with a small quantity of chlorate of barytes, then I think it will be beyond a doubt, that of the two equivalents of chlorine which the barytes took up, one combined with the barium, the other with its oxygen.

The following is the formula explaining the decomposition:

$$\mathrm{Aq} + 2\mathrm{BaO} + 2\mathrm{Cl} = \mathrm{BaCl} + \mathrm{BaOClO}$$
$$\mathrm{Aq} + 2\mathrm{BaO} + 4\mathrm{Cl} = 2\mathrm{BaCl} + 2\mathrm{ClO}$$

In endeavouring to purify this mixture, of chloride of barium and hypochlorous acid, from all free chlorine contained in it, I noticed a remarkable circumstance, which I will relate. By agitating the solution with repeatedly renewed portions of atmospheric air, I was able to obtain a colourless liquid, which possessed but a very slight smell of chlorine. But when this was allowed to stand for a few minutes, especially in a strong light, the colour and smell of chlorine re-appeared. Now, as no chlorine had access to it from without, this must have been caused by a decomposition of the hypochlorous acid. This decomposition takes place in common daylight, more rapidly in direct sunshine, and most rapidly when the liquid is heated in a water-bath. The solution, after decomposition had ceased, and no more chlorine was given off, contained a considerable quantity of chlorate of barytes. The liquid now contained no hypochlorous acid, either free or combined. The following analysis was made in order to determine the proportion of chlorine remaining in it. The barytes was first precipitated by sulphate of soda. The chloric acid was then deoxidized by means of sulphurous acid, and after separation from the sulphate of barytes, the whole of the chlorine was determined by means of nitrate of silver, thus:—

Sulphate of barytes 1·597 = 0·940 barytes.
Chloride of silver 1·998 = 0·493 chlorine.

This is rather more than 1 eq. of chlorine.

I next endeavoured to ascertain the proportion of chloric acid in this amount. For this purpose a portion of the same liquid was precipitated by nitrate of silver, which gave the quantity of chlorine combined as chloride. After separating it from this precipitate the liquid was treated with phosphorous acid, which naturally precipitated metallic silver and a barytes salt, but also reduced the chloric acid. After heating for some time, the precipitates were dissolved in nitric acid, and the chloride of silver which formed, determined.

First precipitate of chloride of silver 4·992 = 1·182 chlorine.
Second precipitate ... ... 0·787 = 0·154 ...

This gives to 6 parts of chlorine as chloride, nearly 1 as chlorate. The theory requires to 5 of chloride 1 of chlorate; but part of the oxygen had probably escaped either free or as hypochlorous acid.

Having thus found that on treating barytes with chlorine the decomposition takes place in so simple a manner, I next endeavoured to find out whether the same laws would not hold good in the case of the alkalies. Of these I first chose potash, which was prepared by decomposing a solution of pure sulphate of potash by barytes water.

I. 4·051 of this solution gave 0·272 sulphate of potash = 0·107 potash, or 3·628 per cent. 18·819 were saturated with chlorine, in the same manner as the barytes had been, and gave, after reduction by ammonia and sulphurous acid, a precipitate of 3·314 chloride of silver = 0·818 chlorine.

Calculated to the amount of potash, this gives to 590 potash 706·5 chlorine, instead of 443, which is 1 equivalent.

II. 17·397 of the solution which was used for this determination gave 1·337 nitrate of potash = 0·6225 potash.

19·027 gave 3·031 chloride of silver = 0·748 chlorine, or in 590 potash 647 chlorine, instead of 443. The average of these two gives to 590 potash 677 chlorine, or to 2 eqs. of potash 3 of chlorine.

This liquid possessed the smell of hypochlorous acid as well as its bleaching properties. It contained no salt of hypochlorous acid, that is to say, that all this acid contained in it was in a free state, as was proved by the above-described reaction with silver.

A considerable quantity of chloric acid was contained in this liquid, but was not determined, as it depended only on the time allowed to elapse between the preparation of the liquid and its being made use of for analysis.

What now can be the cause of potash combining with a less quantity of chlorine than is the case with barytes? This arises probably from the stronger affinity of potash for chloric acid, which causes a decomposition of hypochlorous acid, by which chloric acid is formed and chlorine set free. Hence it is evident that by the formation of a small quantity of chloric acid the total quantity of chlorine in the liquid must be much diminished.

I now treated carbonate of potash in a similar manner with chlorine, and obtained the well-known liquid, possessing, from the first action of the chlorine, the smell and other properties of free hypochlorous acid, a considerable quantity of which was carried away by the escaping carbonic acid. Now, if this liquid be heated for some time in a water-bath, the hypochlorous acid is completely destroyed, chlorine is disengaged, and the liquid now contains a mixture of chloride of potassium with chlorate of potash. As the hypochlorous acid was from its first formation uncombined, the chloric acid must have, in all probability, been formed by a different process from that which took place in the preceding instance, where, by the action of chlorine on a hypochlorous acid salt, chloric acid formed, partly by taking the oxygen directly from the base, partly, perhaps, by a decomposition of hypochlorous acid. We here have a mixture of hypochlorous acid with chloride of potassium; after heating which we find part of the chloride of potassium is changed into chlorate of potash.

Now if this be the case, I must obtain the same result by treating a solution of chloride of potassium with hypochlorous acid. The experiment was made in the following manner:—In a concentrated aqueous solution of hypochlorous acid were dissolved crystals of chloride of potassium, and the mixture was heated in a water-bath until chlorine ceased to be disengaged. This liquid deposited, on cooling, scales of chlorate of potash, which was also easily detected in the mother-liquid.

This process must not be considered as a direct oxidation of the chloride, for peroxide of hydrogen produces no such effect.

It is well known that aqueous hypochlorous acid has a great tendency, especially when heated, to form chloric acid, setting free chlorine. Now when chloride of potassium is present, this chloric acid, in the moment of its formation, decomposes the chloride, setting free its chlorine, the metal oxidating itself at the expense of hypochlorous acid.

On treating carbonate of soda with chlorine I obtained similar results, with the difference, that chloric acid formed even more easily than was the case with potash, and consequently the total content of chlorine in the liquid was less.

The following is an experiment showing the nature of this decomposition:—Treat a concentrated solution of simple carbonate of soda with chlorine until a considerable quantity of bicarbonate is precipitated, but not until this precipitate commences to be decomposed. The liquid on being now left to itself in the light continues for several hours to give off carbonic acid with slight effervescence. We here have carbonate of soda mixed with chloride of sodium and free hypochlorous acid. These two latter, by gradually acting on each other, in the manner above described, set chlorine free, which decomposes the carbonate.

Now, as by treating carbonates with chlorine hypochlorous acid is obtained free, and can be distilled over quickly, it appeared to me that this might be an easy method of obtaining this acid for use. It is desirable for this purpose to choose a base which has not too powerful an affinity for chloric acid, and consequently from the chloride of which, hypochlorous acid can be quickly distilled off without decomposition. I chose, with this view, carbonate of lime.

This salt, prepared for the purpose by precipitation, was mixed up with about 40 parts of water, and dissolved in chlorine. When quickly heated to the boiling-point, this liquid gave off its hypochlorous acid, while the residue in the retort consisted of a solution of chloride of calcium with a small quantity of chlorate of lime.

This is a cheap and easy method of preparing this most interesting compound. It is well if a slight excess of carbonate of lime be left in the liquid when subjected to distillation; it speedily dissolves as the liquid becomes hot, and the acid is thus obtained free from chlorine, which is not the case without this precaution.

I was induced from this to try the action of chlorine upon other salts with stronger acids, and discovered a series of interesting decompositions, of which I will here state the outline; and hope, after further investigation, to be soon able to communicate the details.

A solution of tribasic phosphate of soda was treated with chlorine, which it absorbed with great avidity. This solution, after saturation, possessed a strong smell of hypochlorous acid, which I distilled over from it. The residue gave a strongly acid reaction, proving that the chlorine had taken 2 atoms of soda from the phosphate, forming with the metal chloride of sodium, and with the oxygen hypochlorous acid.

Common phosphate of soda gave a similar result. The residue after distillation consisted of the same phosphate as the preceding, with 1 atom chloride of sodium.

Two basic pyrophosphates behaved in exactly the same manner.

The next salt I subjected to this treatment was sulphate of soda. A solution of this perfectly neutral salt was saturated at the common temperature with chlorine, which was freely absorbed by it. The liquid remained colourless, which shows that the chlorine absorbed was not contained, as such, in solution. This neutral sulphate of soda was indeed decomposed. The liquid, after saturation with chlorine, consisted of a mixture of bisulphate of soda with chloride of sodium and free hypochlorous acid. Whether any neutral sulphate remained undecomposed I have yet to ascertain. On being subjected to distillation, hypochlorous acid passed over at the beginning of the operation. The liquid which first distils contains most of the acid. Each successive portion contains less of it than the preceding. This compound possesses such powerful oxidating properties, far exceeding, at the common temperature, those of nitric acid, that it might doubtless become a valuable aid to the chemist in many of his operations. It might even be made advantageously on a large scale in the above-described manner; the same sulphate of soda would of course serve for many operations. It is only necessary to boil down the residue of distillation and heat it until all the chlorine is driven out as hydrochloric acid, which also need not be lost. It is now reconverted into neutral sulphate of soda, which can be redissolved and treated with chlorine as before.

The aqueous solution of hypochlorous acid may easily be prepared so as to contain in a given weight more bleaching power than the well-known lime compound. It will keep for a considerable time, even when concentrated, with but very little decomposition if protected from light, and, of course, heat. I offer this as a hint for the consideration of manufacturers of bleaching powders. Should it prove a practical simplification of the method hitherto used for preparing the bleaching compounds, and thereby have any beneficial effect on the manufactures for which these are required, I shall feel myself amply rewarded.

The salts of sulphuric acid with the following bases are decomposed in like manner. Oxide of copper, peroxide of iron, oxide of zinc, protoxide of manganese, also the double salt of alumina and potash; even sulphate of lead, when suspended in water and treated in like manner, was slightly decomposed. Chromate of potash was also decomposed. Borax was entirely decomposed: the solution deposited after concentration crystals of boracic acid. Nitrate of potash slightly. Acetate of lead was decomposed, forming chloride and peroxide.

These decompositions only take place in the presence of water. Crystallized sulphate of soda is not acted upon in its dry state by chlorine, though the crystals contain water.

Concluding from analogy that similar decompositions might be produced by cyanogen, I treated a solution of tribasic and common phosphate of soda with this gas: both were decomposed. The solution assumed a yellow colour, which by the further action of cyanogen became much darker. Upon distilling this liquid hydrocyanic acid passed over, and part of the above-mentioned yellow brown acid was precipitated in the residue in the form of a brownish substance, which probably contains the oxygen of the soda, for no cyanic acid was formed; but, as I have stated already, I hope to be soon able to communicate further particulars regarding these interesting decompositions.

XXXIII. *On some of the Substances which reduce Oxide of Silver and precipitate it on Glass in the form of a Metallic Mirror.* *By* JOHN STENHOUSE, *Ph.D.*[*]

IT has long been known that aldehyde, when heated in a tube with ammonio-nitrate of silver, reduces the oxide to the metallic state, and forms a brilliant coating on the inner surface of the tube. Three other substances, saccharic acid, salicylic acid and pyromeconic acid, were also known to possess the same property, though the coatings which they yield are much darker, and therefore less beautiful than those formed by aldehyde. This was the state of our knowledge previous to the announcement, about six months ago, of Mr. Drayton's process for silvering mirrors in the cold, by means of ammonio-nitrate of silver and an alcoholic solution of the oils of cloves and cassia.[†]

I find that the number of substances which, especially when assisted by heat, give more or less brilliant coatings of reduced silver, is much greater than has hitherto been supposed. Thus grape-sugar forms a pretty brilliant mirror even in the cold. When unassisted by heat the mirror is rather slowly formed, requiring from six to twelve hours; but when a slight heat is applied it forms very readily in the course of a few minutes; the coating is much darker than that produced either by aldehyde or by Drayton's process. Cane-sugar also yields a mirror when assisted by heat, but none in the cold. Gum-arabic and starch also yield dark-coloured mirrors, but more slowly, and require considerable boiling: so do phloridzine

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[*] Communicated by the Chemical Society; having been read December 2, 1844.

[†] See the preceding volume, p. 546.

and salicine. Oils of turpentine and laurel also give mirrors, but with still greater difficulty, the solutions requiring to be very concentrated. Resin of guaiacum acts in a similar manner.

Oil of pimento, as is well known, consists of two oils, one an acid oil, which is heavier than water, and forms crystalline compounds with the bases; this in the course of a few minutes, even in the cold, produces as brilliant a coating of silver as the mixture of the oils of cassia and cloves. The neutral portion of the pimento oil, which is lighter than water, does not reduce nitrate of silver even after long boiling. I could not succeed in forming metallic mirrors with cinnamic, benzoic, meconic, komenic, tannic or pyrogallic acids, with gum benzoin, elemi or olibanum, with oil of rhodium or with glycerine.

Ingenious as Mr. Drayton's patent process certainly is, it labours under a very serious inconvenience, which I greatly fear will not be easily remedied. In the course of a few weeks the surface of the mirrors formed by his process become dotted over with small brownish-red spots, which greatly injure their appearance. The cause of the spots seems to be this—that the metallic silver while being deposited on the surface of the glass carries down with it mechanically small quantities of a resinous matter, resulting, most probably, from the oxidation of the oil. This resinous matter, which is interposed between the glass and the silver, in the course of time begins to act on the metallic surface with which it is in contact, and to produce the small brown spots already mentioned. If an excess of the essential oils is employed to precipitate the silver, the metallic mirror is much darker, and gets sooner discoloured than usual. No doubt the alcohol present in the solution keeps up much of the resinous matter; still a little of it is almost always deposited on the silvered surface, and acts in the injurious way described.

XXXIV. *On the Decomposition of Salts of Ammonia at ordinary Temperatures.* *By* H. BENCE JONES, M.D.*

IT frequently happens that urine is met with which has become alkaline in consequence of the urea passing into carbonate of ammonia. Such urine, though at first showing an alkaline reaction on red or very slightly blue litmus paper, was observed, as it dried in the air, to have a well-marked acid reaction. By adding a slight excess of ammonia to healthy urine,

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* Communicated by the Chemical Society; having been read December 2, 1844.

or by leaving healthy urine to stand until it becomes alkaline, the above-mentioned reactions may always be observed. Slightly blue litmus paper is then at first made more strongly blue, and on drying in the air the paper becomes as red as if the urine were fresh, or no ammonia had been added.

This seemed to show that no free acid existed in the urine, but that the acid reaction was caused by some ammoniacal salt having this property of reddening litmus.

Pure urate of ammonia, when dissolved in water or with excess of ammonia, showed a similar reaction as the solution evaporated from the blue litmus paper, and hence it seemed probable that urate of ammonia was one cause of the acid reaction of the urine. Hippuric acid having been observed by Liebig to be present in healthy urine, I formed hippurate of ammonia with a slight excess of ammonia in the solution, and I found that when this dried on litmus paper it gave a strong acid reaction.

It occurred to me to make the same experiment with other salts of ammonia dissolved in cold water with an excess of ammonia, and I found in all a similar reaction. The salts used were the acetate, oxalate, nitrate, sulphate, hydrochlorate, hydrosulphate, benzoate, phosphate, and carbonate. The last produced much the feeblest acid reaction.

Some sulphate of ammonia was dissolved in water: at first it was neutral to test-paper; a drop of a solution of litmus was added to a small cupful of this solution; after standing some hours in the air the blue colour disappeared, and a pink solution was formed long before all the fluid had evaporated. The temperature was about 67° F.

The same experiment was repeated with urate of ammonia. A warm solution was made, and whilst warm it was poured into a basin with a drop of litmus solution; after standing eight hours a marked change of colour had taken place. A line of pink had formed where the fluid had evaporated, and there was a decided pink tint in the violet-coloured solution. After twelve hours no violet colour was seen, but a well-marked pink tint. The temperature was rather lower than in the previous experiment.

A solution of urate of ammonia when cold was filtered into a watch-glass: on examining the edge of the fluid as it evaporated a granular deposit mixed with very small needles was seen with the microscope, and single crystals and tufts of crystals of uric acid were present in considerable numbers.

Another solution was made with a marked excess of ammonia: on evaporation in a watch-glass, uric acid crystals, as before, were found at the edge.

To avoid the interference of the atmosphere, solutions of urate of ammonia and chloride of ammonium, coloured blue with litmus, were evaporated in a very perfect vacuum over chloride of calcium. The solution of chloride of ammonium became red in less than five hours. The solution of urate of ammonia formed a red edge, where the fluid evaporated, in half an hour; this continued to grow broader, and after fourteen hours the basin was dry, leaving a broad red deposit on the side.

Two solutions of equal quantities of chloride of ammonium were made, the one in alcohol, the other in water. They were both coloured with blue litmus and left to evaporate in the air. The alcoholic solution became red the soonest.

These experiments show that salts of ammonia are decomposed by evaporation at ordinary temperatures. It was long since observed by Sir H. Davy, that if the same quantity of nitrate of ammonia was dissolved in much and little water, the loss on evaporation by boiling was directly in proportion to the quantity of water used. Part was probably carried off by the steam, but the above experiments show that it was partly decomposed, the ammonia being separated from its combination with nitric acid.

H. Rose also observed the decomposition of the sulphate of ammonia when evaporated at a boiling temperature. He says, "If the boiling is effected in a retort a liquid passes over into the receiver, which contains free ammonia. This result evidently arises from the water as base eliminating the oxide of ammonium from its combination with sulphuric acid and combining with the same. The quantity of the sulphate of the oxide of ammonium decomposed in this way is indeed small, but it must also be remembered that the oxide of ammonium is one of the most powerful bases, and this result is chiefly to be ascribed to its greater volatility *."

It seems probable that the act of evaporation of the fluid, in which the salt is dissolved, assists the escape of the oxide of ammonium from its combinations, the alcoholic solution of chloride of ammonium becoming more quickly acid than the watery solution.

The decomposition of ammoniacal salts, although so very perceptible to test-paper (as may be best seen by dipping blue litmus paper into an ammoniacal solution of chloride of ammonium and drying in the air), is scarcely perceptible to balance.

Nitrate of ammonia loses, according to Sir H. Davy, from 3 to 6 grs. per cent. when the temperature of evaporation is

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* Taylor's Scientific Memoirs, vol. ii. p. 371.

212° F. During the evaporation of a solution of spec. grav. 1·146 and 1·15, at a temperature below 120°, he never detected any loss *.

Chloride of ammonium may be evaporated in a water-bath without perceptible loss.

XXXV. *On the Mechanism of Glacial Motion. Third Letter.* *By* W. Hopkins, *Esq., M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I PROCEED to offer some critical remarks on the mechanical reasoning employed by Prof. Forbes to account for phænomena connected with the motion of glaciers.

1. Let us first consider the reasoning by which he determines the direction of *maximum tension.* It appears to me embodied in what he has said on the subject in the note of page 378 of his Travels. He observes, "The experiment on a model, described in this note, is more strictly analogous to the glacier phænomena, than those of a more striking kind described in the text, page 379, where the succession of colours naturally gives to the mind the impression of a primitive structure near the origin of the glacier, which is mechanically *deformed* into these conchoidal surfaces. They strikingly recall, however, this important fact, that the direction of maximum distension of the particles must be, not parallel to the length of the glacier, but in the direction of the branches of these elongated loops, since their elongation is the simple result of the mechanical tension to which they are subjected." The *loops* here spoken of are those formed in models of plaster of Paris, described by Prof. Forbes when the descending stream was composed of "alternate doses of white and blue fluid poured in successively." They are identical, I conceive, with the loop $P' Q'$ represented in fig. 5. of my last letter, as that into which a transverse element, originally rectilinear, would be converted by the more rapid motion of the central part of the glacier.

Again, the Professor observes in the same note, "The least distance which can ever exist between a *side* and a *central* particle of a canal-shaped glacier, is half the breadth of the glacier. But the unequal motion of the centre and sides tends continually to separate them wider apart, and to distend the row of particles which connects them. The structural bands are, therefore, perpendicular to the line of greatest tension, and hence crevasses will naturally occur, *crossing the*

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* On Nitrous Oxide, p. 51. ed. 1839.

structure at right angles, which I have found empirically to be the case."

The error in this reasoning will be at once apparent to any one who has followed the simple reasoning by which, in articles 4 and 5 of my last letter, I have determined the line of greatest extension. The Professor's conclusion is equivalent to the assertion that the line M *l*, in the figure of the articles just referred to, is the line in question, instead of M *n*, as I have distinctly demonstrated it to be. These directions differ by nearly 45° if the extensibility of ice be small, as I conceive it to be, and could not even approximately coincide unless the extensibility were very great*, as is easily seen by referring to the construction given in art. 17 of my previous letter, for determining the lines of no tangential action and maximum and minimum tension.

If we consider the Professor's reasoning with reference to my analytical investigations, it will appear obvious that his error consists in the omission of all consideration of the effects of the tangential forces, those forces, in fact, which give to the problem its distinctive character†. His conclusion would be true provided each loop P′ Q′ (fig. 5) were indefinitely narrow and entirely unconnected with the contiguous portions of the mass, forming an independent physical line of very small section. In such case the whole tension would act in the direction of the line at each point.

2. Nor if the Professor's reasoning were correct, should we be able to account for the fact of the convexity of the transverse curves of fracture being turned towards the upper end of the glacier; or, which is equivalent, for the fact that the inclination of the transverse fissures to the axis of the glacier is less at points near the sides than near the centre, where it approximates to a right angle, except in the cases of *divergent* glaciers. This fact, with the exception of those places where there are local disturbing causes, is perhaps as well established as that of glacial motion itself. The inadequacy of the Professor's reasoning to explain it will appear at once by the annexed diagram, in which two loops only are represented. It is manifest that any curve line, as M N, which meets any number of such loops, must have its convexity turned towards

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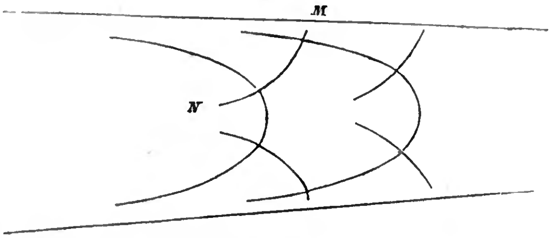
* The extensibility is here supposed to be measured by the extension which glacial ice would bear without fracture, in a comparatively short time (art. 27, Second Letter).

† In my first memoir I was guilty of the same omission, before I attempted any accurate analysis of the problem. The fact is, that the exact influence of the forces in question is extremely difficult to detect by means of mere general and popular reasoning, though very clear and certain when seen as the interpretation of a mathematical analysis of the problem.

the *lower* extremity of the glacier, its inclination to the axis constantly diminishing as it recedes from the sides: and since the fissures must be perpendicular to the direction of maximum tension, they must follow the same law as the curve line M N, and approximate more nearly to parallelism, instead of perpendicularity, to the axis, the more nearly they approach it.

Fig. 1.



3. I now pass to the mechanical reasoning on which Prof. Forbes founds his theory of the laminar structure of glacial ice, as exhibited in the alternate bands of blue and white ice. According to his views, if I understand them correctly, the more rapid motion of the centre of a glacier tends to form longitudinal planes of discontinuity, along which the cohesion of the mass is entirely, or in a great degree, destroyed, so as to facilitate the infiltration of water descending from the surface, which water is afterwards frozen and constitutes the bands of blue ice. The general direction of these planes of discontinuity, however, is not exactly parallel to the sides, but inclined to them, the more accurate law being that they meet the crevasses at right angles, as stated in the second extract given at the commencement of this letter. This statement of Prof. Forbes's theory is independent of the modification introduced into it by the hypothesis that the upper surface of the glacier moves faster than the lower one, a point in his theory to which I shall allude in the sequel.

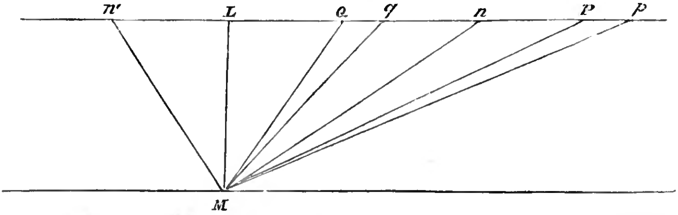
If we suppose the cohesion of a glacier to be uniform, or, more generally, to vary according to any continuous law*, it is manifest that the planes of discontinuity above mentioned will be formed, if formed at all, in that direction in which there is the greatest tendency in one particle to slide past a contiguous one. Prof. Forbes has concluded that this direction is perpendicular to the transverse fissures, because that is the direction of greatest distension and maximum tension†.

* See article 19, Second Letter.

† See note, p. 377 of Travels in the Alps; also Prof. Forbes's Seventh Letter on Glaciers, p. 14.

Now I entirely deny the validity of such reasoning, and contend that the reason here assigned would much rather justify an opposite conclusion. Take two physical lines terminating in the same point, and contiguous to each other, and let a motion be given to the particles of each by distending the lines; the degree in which any one particle of one line will slide by an originally contiguous particle of the other line, will obviously not depend on the whole distension of the two lines, but on their *difference* of distension. If there be no such difference, there will be no tendency in these particles to separate, and therefore there would be no tangential action between them if they cohered. Instead of only two physical lines, conceive any number still terminating in the same point, and lying in one plane, and suppose them to be differently distended according to some continuous law in passing from one string to a contiguous one, such that the distension in a certain direction shall be a maximum. Then by the property of quantities in their maximum state, the distension of two contiguous strings in the maximum direction would be equal, and there would be no relative displacement along the strings in that direction. We should have the case of the two equally distended strings first supposed. And that this supposed case presents no vague analogy with the actual one before us, will be seen by the following simple investigation.

Fig. 2.



Let M n and M n' be the directions of maximum extension and compression, as in fig. 1. of my second letter. Take any line M p, having one extremity in any proposed point M of the mass, and meeting $n'n$ produced in any point p. Consider this line as the line of separation between two linear contiguous elements terminating in M and p. Also, let M P be the original position of these elements. Then, in moving to M p*, both elements will be extended, but the one nearest to M n will be extended more than the other, since the ex-

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* It should here be recollected that P p is not necessarily small compared with M L, but of any magnitude less than that through which the *relative* motion with respect to M of a point in N′N, might carry that point without producing fracture of some kind.

tension along $M n$ is a maximum. Consequently there must be a tangential force called into action between these elements, acting on the former *towards* M and on the latter *from* M. Again, take another pair of contiguous elements terminating originally in M and Q, and, after the motion, in M and q, where $Q q = P p$. Of these two elements along $M q$, the one nearest to $M n$ will be extended the most, and therefore the tangential force called into action will act *from* M on the element most remote from $M n$, and *towards* M′ on the other. Hence the tangential action between two elements for which the line of separation is situated on one side of $M n$ being called *positive*, that for two other elements whose line of separation is on the opposite side of $M n$, will be *negative*, and therefore, when the line of separation coincides with $M n$, the tangential action must $= 0$.

The conclusions from this mode of reasoning are identical with those deduced from the two independent investigations previously given (articles 7—12, and 17, Second Letter). I might have contented myself with an appeal to those investigations, but it appears to me that the reasoning now employed is better calculated to point out the fallacy of that by which, as I conceive, the conclusion of Prof. Forbes has been arrived at.

I maintain, then, that in the directions of maximum tension, which may be considered as approximately perpendicular to the transverse fissures, there is no tendency whatever in contiguous particles of a glacier to slide past each other, or, therefore, to produce in those directions planes of discontinuity; and, consequently, that the alternate bands of blue and white ice cannot have their origin in a system of fractures so produced, assuming always the approximate truth of the law distinctly announced by Prof. Forbes, that the bands do approximate to perpendicularity to the general directions of the transverse fissures, where the two sets of phænomena are best defined.

4. But further than this, I contend that though we should allow the bands of blue and white ice to exist in those directions in which there is the greatest tendency to form these planes of discontinuity, those bands could not be thus accounted for, because I conceive it to be mechanically impossible, under the conditions to which I believe glaciers to be subjected, that a system of planes of discontinuity, in which the planes are parallel and at distances varying from the fraction of an inch to a few inches from each other, could be produced by the *internal forces of constraint* independently of some such molecular action as that to which crystallization

must be referred. I shall perhaps best explain the nature of this difficulty in Prof. Forbes's theory by a reference to the case of fissures formed by the normal force in directions perpendicular to those of greatest tension, when that tension becomes greater than the cohesion. Suppose the tensions to be the same at every point, and the mass perfectly homogeneous. There will be no more reason for the formation of a fissure at one point than at another, and it is conceivable, as a mere abstraction, that an infinite number of fissures should be formed at the same instant; but practically, the perfect homogeneity of the mass cannot exist, and therefore fissures will *begin* at the same instant at comparatively few points, perhaps only at a single point. This will more especially be the case if the tension be slowly and continuously increased till the cohesion give way. But when a fissure is once begun at one point, no other can begin at any point near it and in the line of tension through it, because the tension along that line is immediately relaxed. Hence it will be impossible that a fissure should thus be formed parallel and very near to one previously existing, since in the immediate vicinity of an existing fissure the tension perpendicular to it must necessarily be destroyed, and no system of parallel fissures very near to each other could thus be formed*.

Similar reasoning will apply to the case of longitudinal fissures, or planes of discontinuity, supposed to be formed by the more rapid motion of the centre of a glacier, admitting a condition which, I conceive, cannot be disputed. Let the annexed diagram represent two contiguous longitudinal elements of the mass. If there be no cohesion between them, they will still be capable of exerting a tangential force on each other by *friction*, if they be kept in contact by a compressing force. The greatest tangential force which could thus be produced would be the greatest force of friction under the existing pressure. Let it $=F_1$. If there be cohesion between these elements, there will on this account be an additional power of resisting the tendency of any forces to make one element slide past the other. Let this additional power be denoted by F_2; then F_1+F_2 will be the force which must be

Fig. 3.

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* This may be elucidated by attempting to tear a long strip of paper into more than two parts by equal and opposite forces applied at its extremities. It will be found practically impossible to tear it asunder in more than one place.

overcome before any *discontinuity* can be produced between the two elements, by the one sliding past the other. I know of no experiments which determine the relative magnitudes of F_1 and F_2 under an assigned pressure. If, however, the cohesive power be such as it must be in glacial ice, a hard crystalline substance, and the pressure be small, F_1 will necessarily be small compared with F_2; and in no case will it be contended, I imagine, that F_2 can be small compared with F_1, except perhaps where the pressure is sufficient to crush the mass, and thus to change altogether its molecular constitution. Now, if it be allowed that F_2 is not small compared with F_1 (which is the condition above alluded to), I conceive it to be mechanically impossible, as in the case just considered, that a system of parallel planes of discontinuity, at distances from each other varying from the fraction of an inch to two or three inches, should be produced by such forces as those which are brought into action by the unequal motion of different parts of the mass. For suppose the continuity of the mass to be destroyed along $p\,q$ (fig. 3), the greatest tangential force which could be exerted along that line would be F_1, and, therefore, the greatest tangential force which could be exerted along the opposite side of the element bounded by $p\,q$, could not exceed F_1, as has been before proved. But the mass is capable, by hypothesis, of resisting the force F_1+F_2 before its cohesion can be destroyed by the mechanical action here contemplated. Hence no second plane of discontinuity can be formed parallel and *very near* to the first, by forces such as we are here considering, where F_2 is supposed to be of considerable magnitude.

That longitudinal planes of discontinuity may be formed by internal constraint at irregular and considerable distances, is very possible, but that a system of such planes should be formed of such perfect regularity, and with such minute intervening spaces, is, as I conceive, mechanically impossible.

The above considerations appear to me to involve most serious objections against Prof. Forbes's mechanical theory of the laminated structure*. I am far, however, from supposing that the origin of this structure is unconnected with the motion of the glacier. The fact of the bands, according to Prof. Forbes's statement of the law of their arrangement, being in directions coincident with that of greatest distension and

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* Prof. Forbes has stated his theory in the following passage (Travels, p. 377):—"The crevices formed by the forced separation of a half-rigid mass, whose parts are compelled to move with different velocities, becoming infiltrated with water, and frozen during winter, produce the bands we have described."

perpendicular to that of greatest pressure, would seem to establish a relation between the laminar structure and the internal forces of constraint, which may possibly afford the key to a satisfactory physical explanation of the bands in question. I do not profess however to offer any theory on the subject.

5. There is another point, connected with Professor Forbes's theory of the mode of progression of glaciers, which calls for a few remarks. I mean the *frontal dip* of the structural surfaces, which Prof. Forbes accounts for as follows:—". . . . any particle in a fluid or semifluid mass, urged by a force from above, does not necessarily move in the direction in which the force impels it, it moves *diagonally*; forwards, in consequence of the impulse; upwards, in consequence of the resistance in front. Hence a series of surfaces of separation, shaped (to use a familiar illustration) somewhat like the mouth of a coal-scuttle or sugar-scoop, will rise toward the surface, varied in curvature by the law of velocity of the different layers of the glacier. Near the head or origin of the glacier, where the resistance in front is *enormous*, the tendency of the *separation planes*, which are those of apparent cleavage, will be very highly inclined. As the lower end of the glacier is approached, the resistance continually diminishes, the line of least resistance becomes more and more nearly horizontal; and finally, when the lower end of the glacier is reached, the planes fall away altogether, and the upper layers roll over the lower ones, now wholly unsupported."

In the first place, I would remark that whatever validity this mechanical reasoning may have, depends on the hypothesis of the mass being "urged by a force from above;" in other words, the glacier must be generally in a state of *longitudinal compression.* Now I maintain that the existence of a number of transverse fissures in the central portion of a glacier, whose directions are not remote from perpendicularity to the axis of the glacier, as is the case in the glaciers of the Aar and of the Mer de Glace, is altogether inconsistent with the hypothesis of a *general* longitudinal compression, though such compression may doubtless exist in particular localities. I should think it impossible for any one to inspect more especially the lower portions of these and many other glaciers, and to conclude them to be in that state, at least in the summer months, when the motion is greatest. In fact, Professor Forbes himself appears to be of the same opinion, for he appeals to "M. Elie de Beaumont's acute observation, that a glacier does not so much resemble a body thrust or pushed forward, as one dragged down and pulled." (Travels, p. 178.)

It would seem difficult to reconcile the notion of a glacier's being "dragged down and pulled," with the hypothesis of its being "urged by a force from above."

In the second place, I maintain that the mechanical reasoning by which it is attempted to be proved that there would be an indefinite number of such surfaces, is untenable, as is shown by the reasoning of art. 4*.

Again, allowing for the moment the formation of *separation planes*, I do not allow that they could be "very highly inclined" to the surface of the glacier, assuming the pushing force to act as it would do in the case of a semifluid mass, in which case the *direct tendency* of the force would be to form separation planes approximately parallel to the bed of the glacier. The direction, however, in which there would be the greatest tendency to form these planes, would be that in which the pushing force resolved in that direction should bear the greatest ratio to the resistance in that direction, arising from the cohesion of the mass. I do not believe that the plane of fracture could be "highly inclined." That a nearly horizontal force should produce nearly vertical fractures, is, *primâ facie*, a mechanical contradiction; if it be not really so, it must at least require some proof beyond assertion.

The continued verticality of transverse fissures presents an obvious objection to the general theory of Professor Forbes. He has attempted to answer it by saying that the portion of a glacier near its surface moves with the same velocity to a considerable depth. I am at a loss to reconcile this, as a general statement, with the assertion that, near the lower end of the glacier, "the upper layers roll over the lower ones." If this assertion be correct, how can the verticality of transverse fissures be preserved, as I believe it is, near the lower extremity of a glacier, as well as in every other part of it? It is very possible that the motion may be such as here described at the extremity of a glacier like that of the Rhone, where the inclination of the glacial valley suddenly diminishes at the termination of the glacier. But is this the motion in the glaciers of Chamouni, Grindelwald and the Aar? I believe the assertion rests entirely on theoretical considerations, but it is on the evidence of observation alone that the fact can be admitted.

6. It has been remarked, at the conclusion of my previous

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* In Mr. Hodgkinson's experiments on the breaking of cast iron by a *crushing* force, the fracture always took place along one *surface of separation*, and not along many, and it manifestly must do so whenever the body fractured possesses the cohesion of a solid body. The case is very analogous to that of a glacier, if we suppose, as I am now doing, for the sake of the argument, that the glacier is urged forward by a force *à tergo*.

letter, that my investigations involve the condition of continuity in the same manner as the common investigations of the motions of fluids. In these latter motions, however, there is one obvious case in which that condition is not satisfied—that in which the motion results from the heating of the lower portion of the fluid contained in a vessel, as in the ordinary case of boiling water. It would seem that at every point of the fluid mass, some particles must be ascending and some descending, which is inconsistent with the condition of continuity as here understood. In solid bodies we may conceive a case exactly analogous. Suppose a body composed of two parcels of parallel rods, the rods of one parcel being intermingled with, and placed parallel to, those of the other; and let us suppose the component rods to be parallelopipeds, and so arranged that each of the four longitudinal faces of any rod in one parcel shall be in contact with a rod of the other parcel, with the exception of those faces which form the exterior surface of the whole body. This particular arrangement is not essential for our purpose; it is merely assumed for the greater clearness of conception. Also conceive the whole mass thus formed to be terminated by two planes, perpendicular to the common direction of the rods, of which one parcel is fixed to one terminal plane, and the other parcel to the opposite plane. If forces of sufficient magnitude be applied in opposite directions on the terminal planes, the two parcels of component rods may be drawn out from each other, thus producing extension of the mass, by giving motions in opposite directions to elements laterally in contact. This relative motion of such elements would be exactly similar to that above mentioned in boiling water, and is inconsistent with our condition of continuity.

I would here observe that this case is not introduced as belonging to Prof. Forbes's theory, for it is one which he does not appear ever to have contemplated in his mechanical reasoning on this subject; but I notice it as a case in which numerous parallel surfaces of discontinuity (such as supposed in Prof. Forbes's theory) might be produced by the sliding of contiguous particles past each other in the directions of the lines of maximum tension. It is not unimportant, therefore, to investigate the conditions under which extension may take place in the manner here supposed.

Let δx and δy be the sides of the section of one of the component rods, made by the plane of $x y$ supposed perpendicular to the direction of the rods; then the edges of any element of the rod will be δx, δy and δz. Let Z be the force parallel to the axis of z at the point $(x y z)$. $Z \delta x \delta y$ and

$\left(Z + \frac{dZ}{dz}\delta z\right)\delta x \delta y$ will be the forces acting respectively on the two sides of the element parallel to the plane of $x y$. Also let f denote the tangential force on the element in a direction parallel to the axis of z; then will $2f(\delta x \delta z + \delta y \delta z)$ be the whole tangential force on the element parallel to z. Hence we must have

$$\frac{dZ}{dz}\delta z = 2f(\delta x + \delta y)\delta z,$$

or

$$\frac{dZ}{dz} = 2f(\delta x + \delta y).$$

Consequently, δx and δy being indefinitely small, $\frac{dZ}{dz}$ must be indefinitely less than f, whence it will also follow that the same must hold with respect to Z. Hence, if the surfaces of the contiguous elementary parallelopipeds be capable of exerting a finite tangential force (f) on each other, it would require an indefinitely great force (Z) to make contiguous elements move in opposite directions. Consequently, the relative motion of contiguous particles here considered will be impossible, not only when the mass has any considerable cohesion, but also when there exist planes of no cohesion in the direction of extension, provided those planes be very numerous, and the *friction* along them be not very small*. I maintain, therefore, that *separation planes* cannot be produced in glaciers in this manner.

7. In comparing the *sliding* and *viscous theories*, with reference to the causes assigned by them respectively for the motion of glaciers, we may observe, that they agree in assigning gravity as the primary cause of that motion; but in the one theory it is contended that the efficiency of gravity is principally due to the state of disintegration of the lower surface of the glacier, while in the other it is maintained that this efficiency is due to the plasticity of the general mass. At the same time it will not, I presume, be denied by the advocates of the viscous theory that some sliding does take place over the bed of the glacier, of which, in fact, the polished and striated rocks associated with glaciers afford the most indubitable proof; and on the other hand it will be admitted, that

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* Most persons will be aware from experience of the difficulty of drawing out one bundle of rods from another, when intermingled and arranged as supposed above. I have endeavoured to test the conclusion of the text by stretching elastic substances, as for example, a piece of sealing-wax, but have never detected the slightest indication of the relative motion there spoken of.

some motion may result from the plasticity of glacial ice. A horizontal beam loaded with a heavy weight at its middle point will bend, and thus be put in motion without immediate fracture; but after a certain time the motion will cease or the beam will be broken. With respect to glacial ice, however, I can conceive that the change of form and consequent motion may proceed for a much longer time without fracture than in ordinary cases, for the reasons already stated in my second letter (art. 27). The two theories may agree in admitting both these kinds of motion; but while one of them claims, for instance, nine-tenths of the whole motion as due to sliding, the other claims the same proportion as due to the plasticity of glacial ice. The object of my first letter, more especially, was to place the sliding theory in its true position, and to meet those objections to it which have been urged by Prof. Forbes and others, in the advocacy of their own views. But for the final determination of the claims of the two theories, an appeal must, and doubtless will, be made to further observation. Each theory has its leading difficulty. That of the sliding theory has always been considered to consist in the improbability of a glacier's sliding over a bed of such small inclination as that of some glaciers; that of the viscous theory, in the apparent contradiction offered by the assumed plasticity of glacial ice to the evidence of our senses. The first difficulty has been met by my experiments on the descent of ice along inclined planes, as far as it can be met by experimental evidence. No similar evidence has been brought forward to meet the other difficulty. No experiments have been adduced to show that the plasticity of glacial ice is really greater than common inspection might lead us to suppose; and I maintain that no direct evidence whatever has been offered to show that the compressions and flexures of glaciers are really indicative of a degree of plasticity in which the motion originates, and are not the consequences of a motion due to independent causes. The reasoning by which this deficiency of evidence is attempted to be supplied, calls for a few remarks.

If it be contended that the greater part of the motion of a glacier, as nine-tenths for example, be independent of the sliding over its bed, then, if there were no such sliding, *i. e.* if the lower surface of the ice were firmly frozen to its bed, nine-tenths of the actual motion would still exist. This is perhaps the best form of presenting the leading difficulty of the viscous theory. According to that theory, the upper surface must move much faster than the lower one; but no proof whatever, founded on observation, has been offered to show that such is the case. The only reason which has been assigned in favour of that opinion is derived from the analogy which such motion would bear to the motion of the centre of a glacier relatively to its sides. It is said that as the centre moves faster than the sides, so may we conclude that the upper portion must move faster than the lower. Now it is not denied that the argument is good to a certain extent, but the question is entirely one of degree. We have no right to conclude that the relative motion in the one case will be equal to that in the other, since the conditions in the two cases are different. The depth of a glacier is much less than its semi-width; the continuity of the mass is much broken by vertical planes, and the relative motion of the centre and sides is thus facilitated, whereas the mass is probably altogether unbroken by horizontal planes; also the vertical pressure, where the depth is considerable, is probably much greater near the bottom than the horizontal pressure near the sides of the glacier. All these are reasons why the relative motion of the upper and lower surfaces should be less than that of the centre and sides of the glacier. If, however, we allow these relative motions to be equal, it will not suffice the requirement of the theory; for it would still only account for about half the observed motion, since the excess of the velocity of the centre over that of the sides does not probably exceed half the real motion of the centre of the glacier. It would therefore be necessary generally that the excess of the motion of the upper surface over that of the lower one, should be nearly twice as great as that of the central over the lateral motion. Without asserting that this conclusion involves a mechanical impossibility, it appears to me extremely difficult to admit any theory in which it stands as the immediate consequence of a fundamental assumption unsupported by direct evidence.

To illustrate the inconclusiveness of this reasoning from analogy, let us conceive the ice which constitutes one of the glaciers of Chamouni or Grindelwald, in which the inclination of the bed is probably 10° or upwards, to be removed, and its place supplied by an equal quantity of common earth; and let us suppose the same quantity of water as that which now runs beneath the glacier, to make its way between the mass of earth and its bed. What engineer would recommend the erection of a house on such a foundation? I have no doubt but that we should have the phænomenon of what might be termed a *mud glacier* in slow but constant motion*. But

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* I have, in fact, witnessed a phænomenon precisely of this kind. The upper stratum of earth, to the depth of four or five feet, resting on a re-

would the conviction that such would be the case involve the conclusion, that the mass of earth must have that degree of semifluidity which would cause it to descend when its lower surface should adhere firmly to the rock on which it reposed? Few persons, I imagine, would draw such an inference.

It is not my purpose, however, to dwell on general arguments of this kind, whatever force may, in my own opinion, belong to them, because, as I have already remarked, the claims of the two theories will undoubtedly be determined by other means. The observations required are such as shall determine, as far as possible, the relative motions of the upper and lower surfaces of a glacier. We may never hope to have access to the bottom of a glacier in its deeper portions; but at the extremities of glaciers the amount of sliding may easily be ascertained, as well as at many other points, probably, if sought for, along their flanks; fissures, also, of considerable depth are not unfrequently met with, in which the deviation from verticality, if it exist, might be easily determined; and though the evidence thus obtained might not afford positive demonstration with reference to the deepest portions of a glacier, still, should it all concur in showing an approximate equality in the motions of the upper and lower surfaces, every candid and impartial mind must admit, I conceive, the *sliding* in preference to the *viscous* theory; but if, on the contrary, it should be proved that the velocity of the upper bears a large ratio to that of the lower surface, the claims of the latter theory must be at once admitted.

With respect to the mechanical investigations involved in the foregoing discussion, the case is widely different. They are not dependent on future observations, because they are based on the already well-established fact of the excess of the central motion over that of the sides of the glacier, and will be equally applicable whichever of the two theories may be adopted. In the development of my own views, I have endeavoured to be as explicit as possible, to prevent misapprehension; and for the same reason I have endeavoured to be equally explicit in pointing out what I conceive to be the errors in Prof. Forbes's views respecting the mechanism of the motion in question. This desire to be explicit may possibly have given an appearance of severity to some of my criticisms, in having, perhaps, led me to attach more definite

tentive subsoil, on the brow of a hill, descended very slowly down the side of the hill at the inclination of about 10°. It appeared to be of the extent of several acres, and was strongly fissured transversely, in a manner exactly analogous to a glacier, except that the *crevasses* were not curved, but had their mean directions more directly transverse, owing to there being no excess of the central over the lateral velocity.

meanings to some of the Professor's expressions than they might be intended to convey in a popular work. If so, I stand open to correction. At the same time, however, it should be observed, that any mechanical theory only professing a popular character, must necessarily be superseded by investigations more demonstrative, and leading to more determinate conclusions.

I am, Gentlemen,
Your obedient Servant,
Cambridge, February 17, 1845. W. Hopkins.

XXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Dec. 12, 1844. "On the Laws of the Tides on the Coast of Ireland, as inferred from an extensive series of observations made in connexion with the Ordnance Survey of Ireland." By George Biddell Airy, Esq., F.R.S., Astronomer Royal.

The elaborate investigations of which the results are communicated in the present paper, were suggested by the necessity of adopting some standard mean height of the sea, as a line of reference for the elevations ascertained in the operations of the Ordnance Survey of Ireland. Colonel Colby, R.E, who conducted that survey, had with this view determined to institute a series of observations on the height of the water in different states of the tide; and conceiving that these observations might be made subservient to improvement in the theory of the tides, requested the assistance of the author in laying down the plan of observation best calculated to effect that object. The suggestions which were, in consequence, made by the author were adopted in their utmost extent by Colonel Colby; and the collection of observations was placed in the author's hands in the winter of 1842. The whole number of observations exceeds two hundred thousand; and they derive extraordinary value from the circumstance of the localities, of their simultaneity, their extensive range, and the uniformity of plan on which they were conducted. Their reduction was made by the computers at the Royal Observatory, Greenwich, under the superintendence of the author, and with the authority of the Lords Commissioners of the Treasury. The nature of the different branches of the inquiry may be gathered from the titles of the several sections into which the paper is divided, and which are as follows:—

Section I.—Account of the stations, levellings, times and methods of observation.

Section II.—Methods of extracting from the observations the times of high and low water; of supplying deficient times and heights; and of correcting the times first determined.

Section III.—Theory of diurnal tide as related to observations only; and deduction of the principal results for diurnal tide given immediately by these observations.

Section IV.—Theory of diurnal tide as referred to the actions of the sun and moon.

Section V.—Discussion of the height of apparent mean water, as deduced from the heights of high and low water only, corrected for diurnal tide; with reference to difference of station, and to variations of the phase of the moon, and of the declination of the moon.

Section VI.—Discussion of the range of the tide, and of semimenstrual inequality in height, apparent proportion of solar and lunar effects as shown by heights, and age of tide as shown by heights, from high water and from low water.

Section VII.—Establishment of each port, and progress of semidiurnal tide round the island.

Section VIII.—Semimenstrual inequality in time, proportion of solar and lunar effects as shown by times, and apparent age of tide as shown by times, from high water and from low water.

Section IX.—Formation of the time of diurnal high water; progress of the diurnal tide-wave round the island; comparison of its progress and range with those of the semidiurnal tide.

Section X.—Method of expressing the height of the water throughout every individual tide, by sines and cosines of arcs; and expressions in this form for every tide in the whole series of observations, except those at Courtown.

Section XI.—Discussion of the height of mean water deduced from the analysis of individual tides; with reference to difference of station, and to variations of the phase of the moon, and of the declination of the moon.

Section XII.—Discussion of range of tide, or coefficient of first arc, in the analysis of individual tides; and of semimenstrual inequality in range, apparent proportion of solar and lunar effects, and age of tide as deduced from range.

Section XIII.—Establishment of each port, as deduced from the time of maximum of the first periodical term in the analysis of individual tides.

Section XIV.—Semimenstrual inequality in time, proportion of solar and lunar effects from times, and apparent age of tide as shown by times, deduced from the time of maximum of the first periodical term.

Section XV.—Comparison of the results as to mean height, range, semimenstrual inequality in height, age of tide obtained from height, establishment, semimenstrual inequality in time, and age of tide obtained from time, deduced from high and low waters only, in Sections V., VI., VII., VIII., with those deduced from the analysis of individual tides in Sections XI., XII., XIII., XIV.

Section XVI.—Remarks on the succeeding terms of the expressions for individual tides, as related to the magnitude of the tide, to the position on the sea-coast, to the position on the river, &c.; comparison with the terms given by the theory of waves; discussion of the quarto-diurnal tides.

Section XVII.—Separate discussion of the tidal observations at Courtown.

Section XVIII.—Examination into the question of tertio-diurnal tide.

January 16, 1845.—"On the Liquefaction and Solidification of Bodies generally existing as Gases." By Michael Faraday, Esq., F.R.S., D.C.L., &c.*

The method employed by the author for examining the capability of gases to assume the liquid or solid form, consisted in combining the condensing powers of mechanical compression with that of very considerable depressions of temperature. The first object was obtained by the successive action of two air-pumps; the first having a piston of one inch in diameter, by which the gas to be condensed was forced into the cylinder of the second pump, the diameter of whose piston was only half an inch. The tubes into which the air, thus further condensed, was made to pass, were of green bottle glass, from one-sixth to one-quarter of an inch in external diameter, and had a curvature at one portion of their length adapted to immersion in a cooling mixture: they were provided with suitable stop-cocks, screws, connecting pieces, and terminal caps, all very carefully made, and rendered sufficiently air-tight to retain their gaseous contents under the circumstances of the experiments, and when they were sustaining a pressure of fifty atmospheres, as ascertained by mercurial gauges connected with the apparatus. Cold was applied to the curved portions of the tube by their immersion in a bath of Thilorier's mixture of solid carbonic acid and ether. The degree of cold thus produced, when the mixture was surrounded by the air, estimated by an alcohol thermometer, was a temperature of —106° Fahr. But on placing the mixture under an air-pump, and removing the atmospheric pressure, leaving only that of the vapour of carbonic acid, which amounted only to 1-24th of the former, (that is to the pressure of a column of 1·2 inch of mercury,) the thermometer indicated a temperature of 166° below zero of Fahrenheit's scale. In this state, the ether was very fluid; and the bath could be kept in good order for a quarter of an hour at a time.

The author found that there were many gases which, on being subjected to cold of this extreme intensity, condensed into liquids, even without a greater condensation than that arising from the ordinary atmospheric pressure, and that they could then be preserved, sealed up in glass tubes, in this liquid state. Such was the case with chlorine, cyanogen, ammonia, sulphuretted hydrogen, arseniuretted hydrogen, hydroiodic acid, hydrobromic acid, carbonic acid, and euchlorine. With respect to some other gases, such as nitric oxide, fluosilicon, and olefiant gas, it was difficult to retain them for any length of time in the tubes, in consequence of the chemical action they exerted on the cements used in the joinings of caps and other parts of the apparatus. Hydroiodic and hydrobromic acids could be obtained either in the solid or liquid state. Muriatic acid gas did not freeze at the lowest temperature to which it could be subjected. Sulphurous acid froze into transparent and colourless crystals, of greater specific gravity than the liquid out of which they were formed.

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* Prof. Faraday's former researches on the condensation of gases into liquids will be found in Phil. Mag. S. 1. vol. lxii. p. 413, 416.

Sulphuretted hydrogen solidified in masses of confused crystals of a white colour, at a temperature of −122° Fahr. Euchlorine was easily converted from the gaseous state into a solid crystalline body, which, by a slight increase of temperature, melted into an orange-red fluid. Nitrous oxide was obtained solid at the temperature of the carbonic acid bath *in vacuo*, and then appeared as a beautifully clear and colourless crystalline body. The author conceives that in this state it might, in certain cases, be substituted with advantage for carbonic acid in frigorific processes, for arriving at degrees of cold far below those hitherto attained by the employment of the latter substance. Ammonia was obtained in the state of solid white crystals, and retained this form at a temperature of −103°.

The following liquids could not be made to freeze at −166°; namely, chlorine, ether, alcohol, sulphuret of carbon, caoutchoucine, camphine, and rectified oil of turpentine.

The following gases showed no signs of liquefaction when cooled by the carbonic acid bath, even when subjected to great pressure; namely,

Hydrogen, Oxygen, at a pressure of	27 atmospheres.
Nitrogen and nitric oxide at a pressure of	50 atmospheres.
Carbonic oxide at a pressure of	40 atmospheres.
Coal-gas at a pressure of..............	32 atmospheres.

January 23.—1. "Observations de la Déclinaison et Intensité Horizontales Magnétiques observées à Milan pendant vingt-quatre heures consécutives le 29 et 30 de Décembre 1844." Par M. Carlini.

2. "Remarks having reference to the Earthquake felt in Demerara on the morning of the 30th of August 1844." By Daniel Blair, Esq., Colonial Surgeon of British Guiana. Communicated by the Right Honourable Lord Stanley.

The earthquake here described commenced at twenty-seven minutes past three o'clock, a.m. on the 30th of August, and continued during two or three minutes. It appeared to be composed of two waves or pulsations quickly succeeding each other, and producing gyratory movements of the earth. Though the alarm it occasioned was very great, no serious damage seems to have resulted from it.

3. "An Account of the artificial formation of a Vegeto-alkali." By George Fownes, Esq., Chemical Lecturer in the Medical School of the Middlesex Hospital. Communicated by Thomas Graham, Esq., F.R.S., Professor of Chemistry in University College.

The substance which is the subject of investigation in this paper is a volatile oil, obtained by distillation from a mixture of bran, sulphuric acid and water, and is designated by the author by the name of *furfurol.* Its chemical composition is expressed by the formula $C^{15} H^{6} O^{6}$, and its properties are the following:—When free from water and freshly rectified, it is nearly colourless; but after a few hours, it acquires a brownish tint, which eventually deepens almost to blackness. When in contact with water, or when not properly rendered anhydrous, it is less subject to change, and merely assumes a yellow colour. Its odour resembles that of a mixture of bitter almond oil and oil of cassia, but has less fragrance. Its specific gravity at 60° Fahr. is 1·168; it boils at 323° Fahr., and distils at that temperature without alteration. It dissolves to a large extent in cold water and also in alcohol. Its solution in concentrated sulphuric acid has a magnificent purple colour, and is decomposed by water. Nitric acid, with the aid of heat, attacks the oil with prodigious violence, evolving copious red fumes, and generating oxalic acid, which appears to be the only product. It dissolves in a solution of caustic potash, forming a deep brown liquid, from which acids precipitate a resinous matter. With a slight heat, it explodes when acted upon by metallic potassium.

When placed in contact with 5 or 6 times its bulk of *Liquor ammoniæ*, it is gradually converted into a solid, yellowish-white, and somewhat crystalline mass, which is very bulky, perfectly soluble in cold water, and easily obtained in a state of dryness under a vacuum. The formula expressing the chemical constitution of this substance, or of *furfuroamide*, as the author calls it, is $C^{15} H^{6} N O^{3}$, and it is classed by him with the *amides*. The oil itself appears to be identical with the substance described by Dr. Stenhouse under the name of *artificial oil of ants*. Another substance, isomeric with the amides, and of which the formula is $C^{30} H^{12} N^{2} O^{6}$, was obtained by the author, and termed by him *furfurine*, and found to have the properties of a vegeto-alkali, and to form saline compounds with various acids.

February 6.—"On a new Bleaching Principle produced by the slow combustion of Æther in Atmospheric Air, and by the rapid combustion of Bodies in a jet of hydrogen gas." By C. F. Schœnbein, Professor of Chemistry in the University of Basle, &c. Communicated in a letter to Michael Faraday, Esq., F.R.S., &c.

The author, having observed that a peculiar principle, in many respects similar to chlorine, was developed during the slow combustion of phosphorus in the atmosphere, was led to inquire into the product of the slow combustion of the vapour of æther mixed with atmospheric air. He finds, that besides well-known compounds, such as aldehydic, formic and acetic acids, there is evolved a principle hitherto unnoticed, which possesses oxidizing and bleaching properties in an eminent degree. It decomposes indigo, iodide of potassium, and hydroiodic acid, and also, though more slowly, bromide of potassium. When in contact with water, it converts iodine into iodic acid, and sulphureous into sulphuric acid, changes the yellow ferro-cyanide of potassium into the red, and the white cyanide of iron into the blue; it transforms the salts of protoxide of iron into those of the peroxide; and it discharges the colours produced by sulphuret of lead. The author points out the similarity between the action of this substance, in these instances, and that of chlorine and of ozone.

Analogous results were obtained from the combustion of a jet of hydrogen gas in atmospheric air, and even, under particular circumstances, from the flame of a common candle, and also from various other inflammable bodies when burning under certain conditions The author is hence led to the conclusion that this peculiar oxidizing and bleaching principle is produced in all cases of rapid combustion taking place in atmospheric air, and that its production is therefore independent of the nature of the substance which is burnt.

February 13.—'Αμόρφωτα, No. 1. "On a case of Superficial Colour presented by a Homogeneous Liquid internally colourless." By Sir John Frederick William Herschel, Bart., F.R.S., &c.

The author observed that a solution of sulphate of quinine in tartaric acid, largely diluted, although perfectly transparent and colourless when held between the eye and the light, or a white object, yet exhibits in certain aspects, and under certain incidences of the light, an extremely vivid and beautiful celestial blue colour, apparently resulting from the action of the strata which the light first penetrates on entering the liquid; and which, if not strictly superficial, at least exert their peculiar power of analysing the incident rays, and dispersing those producing the observed tint, only through a very small depth within the medium. The thinnest film of the liquid seems quite as effective in producing this superficial colour as a considerable thickness.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 175.]

December 13, 1844.—Extract from the Translation of a Letter from Professor Bessel, dated Konigsberg, 10th of August, 1844. On the Variations of the Proper Motions of Procyon and Sirius. Communicated by Sir J. F. W. Herschel.

The subject which I wish to communicate to you, seems to me so important for the whole of practical astronomy, that I think it worthy of having your attention directed to it. I find, namely, that existing observations entitle us without hesitation to affirm that the proper motions, of Procyon in declination, and of Sirius in right ascension, are not constant; but, on the contrary, that they have, since the year 1755, been very sensibly altered. If this be so, the observations of the place of a star at two epochs are no longer sufficient to express its place for any indefinite time; but, for this purpose, it is necessary to investigate the law of the change. It follows also from this, that we are yet very far off from the correctness we imagined ourselves to have arrived at in the fundamental determinations of astronomy; and, that a new problem presents itself, whose solution will cost much labour and a long period of time, viz. the problem of determining the special motions of a star. For, even if a change of the motion can, up to the present time, be proved only in *two* cases, yet will all other cases be rendered thereby liable to *suspicion*: and it will be equally difficult, by observations, to free other proper motions from the suspicion of change, and to get such a knowledge of the change as to admit of its amount being calculated.

The earliest suspicion of the want of constancy of the proper motion was derived about the year 1834, from the corrections of the clock-time, which, at this observatory, were registered with every observed culmination of a fixed star and its reduction to the meridian. At that time it began to be remarked in a striking manner, that negative clock-corrections derived from Sirius were greater, and positive corrections less, than those resulting from the other fundamental stars.

As the right ascensions of Sirius, which are given for the beginning of each year in the *Tabulæ Regiomontanæ*, are obtained by comparison of the right ascension in 1755 with that in 1825, their agreement with the observations of the latter years was complete; but, as early as the year 1835, fifty observations showed, when compared with the three fundamental stars following Sirius, viz. β and α Orionis and α Canis Minoris, that $0^{s}\cdot188$ must be added to the *Tabulæ Regiomontanæ*, to make the agreement again perfect. This disagreement has been since still increasing. In the year 1843 I found it, from fifty observations by Dr. Busch with the old instrument, $= +0^{s}\cdot318$, and, from forty, which I made myself with the new meridian circle of Repsold, I found it $= +0^{s}\cdot324$.

A second suspicion of the variability of the proper motions of the stars was awakened in me in the year 1840, by the declination of Procyon, since a new determination of all the elements of reduction of its declination, and of the declinations of the other fundamental stars, gave the observed declination of Procyon more northerly than that of the *Tabulæ Regiomontanæ* (the tabular result being obtained by comparison of the observations of 1755 with those of 1820), by $1''\cdot64$ *. This difference has also increased, since, by observations made with the instrument of Repsold, I find it for $1844 = +3''\cdot18$.

What I have brought forward concerning Sirius and Procyon depends on determinations, whose certainty I esteem as great as can be attained by the present apparatus for observing. At the same time it does not cease to be necessary to subject the important result here given to the strictest scrutiny, by means of all the existing determinations of other observatories, before it can be received as the indisputable result of observation. I would communicate the result which this investigation has produced; but I should go beyond the limits of a letter, should I here give place to the criticism to which some of the numbers must be subjected, before they can be received as valid. Since this is, nevertheless, not the less necessary, I must refer to a paper which will very soon appear on this subject in the *Astronomische Nachrichten*. It is plain that we can obtain for the declination of Procyon comparative results from the different catalogues, only by eliminating the constant errors, which, without doubt, affect all the observations up to the present time, and frequently to the amount of several seconds. This I found to be the case by subtracting from the difference between every determination of Procyon and the *Tabulæ Regiomontanæ*, the mean of the differences of the results for eight stars, α Ceti, α Orionis, β Virginis, α Serpentis, γ, α, β Aquilæ, and α Aquarii, the mean of whose declinations is the same as that of Procyon, within a very few minutes. By this means it will be gathered that the following collection of results does not depend upon the absolute declination of this star and the absolute right ascension of Sirius, but upon the relative declination and right ascension of each respectively, as founded on the comparison with the above-mentioned eight and three stars.

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* *Ast. Nachr.* No. 422.

1. *Relative Declination of Procyon.*

		″	
Fundamenta Astron.	1755	0·00	
Maskelyne	1770	+1·54	For the most part but few observations of the stars; probably not altogether free from errors of observation.
Piazzi	1800	+1·99	New edition of the Catalogue. The results of separate years, which are supplied in the old edition (Supplement), and the determination from the later observations in Libro vi. à Spec. R. di Palermo, give a yet somewhat greater difference.
Bessel..................	1820	0·00	
Pond I.	1822	−0·03	Mr. Pond's own catalogue for this year.
Pond II................	1822	+0·16	M. Olufsen's computations, *Astr. Nachr.* No. 73.
Struve	1824	−0·15	
Argelander...........	1830	+0·03	
Airy	1830	+0·47	This is the mean of seven very beautifully accordant results derived from the observations from 1829 to 1835. It agrees tolerably well with the Catalogue of 1112 Stars; but deserves the preference, since the latter in the event of a variable proper motion cannot be correctly reduced to 1830.
Pond	1832	+0·84	
Henderson	1833	+0·89	
Konigsberg Obs.	1838	+1·59	*Astr. Nachr.* No. 422.
Ditto, with the Repsold circle	1844	+2·62	

2. *Relative Right Ascension of Sirius.*

		s	
Fundamenta Astron.	1755	0·000	
Maskelyne	1767	−0·079	Derived from a new reduction of the observations; this result differs by −0·288 from that contained in the Catalogue for 1770; the reason of which I do not know.
Piazzi	1800	+0·033	
Maskelyne	1806	+0·016	Derived from a new computation of the observations of 1803, and agreeing nearly with the Catalogue for 1805.
Bessel..................	1815	−0·036	
Pond	1819	−0·083	Derived from a new computation of the observations, but agreeing with Mr. Pond's own result.
Bessel..................	1825	0·000	
Struve	1825	−0·006	
Argelander...........	1828	−0·003	
Airy	1830	+0·049	This is the mean of seven results derived from the observations from 1829 to 1835; it is, for reasons mentioned above, preferred to the result derived from the Catalogue of 1112 Stars.
Pond	1832	+0·084	
Konigsberg Obs.	1835	+0·188	
Ditto, with both instruments...	1843	+0·321	

These tables show that the determinations for 1820 and 1825 are fully corroborated by means of nearly contemporaneous observations at other observatories, as indeed the care which has been bestowed on all these observations would lead us to expect. They leave, besides, no doubt of the continual increase of the difference from the *Tabulæ Regiomontanæ*, from 1820 to the present time. This continual increase can be explained on the supposition of an unchanged proper motion only by attributing errors to the determinations for 1755, 1820, and 1825, respectively, of sufficient magnitude to make the *relative* declination of Procyon for 1755 to appear to be 7″ in error, and the *relative* right ascension of Sirius for 1755 more than a second of time. That so great errors cannot exist is proved by the different checks in the *Fundamenta Astronomiæ*: but Lacaille's and Tobias Mayer's contemporary results leave no doubt on this point, although they cannot determine it within 2″ in declination and 1-4th of a second in right ascension. I regard also as a result rendered certain by the observations, that the supposition of an *unchanged* proper motion in the case of the relative declination of Procyon, and in the case of the relative proper motion of Sirius in right ascension, is shown to be false.

The law of the change of each of the two motions is not yet known with sufficient exactness by the observations given. If Piazzi's determination of the relative declination of Procyon is correct (as I believe it to be)*, then has the difference, between 1755 and 1820, reached a positive maximum. In the case of the right ascension of Sirius, I have sought to obtain a more approximate knowledge of its change, through the following up of the results of the observations with the transit instrument which Pond (vol. for 1811–12) published, and by a new reduction of Maskelyne's observations. This has given a positive maximum of the difference from the *Tabulæ Regiomontanæ* of about $0^s{\cdot}3$, between 1790 and 1800; but since the pivots of the axis were unluckily proved to have been injured, and were corrected in 1803, which correction produced a significant effect on the subsequent observations of right ascension of Sirius, it cannot be *certainly* maintained that the maximum was produced in reality by the motion of Sirius, and not, at least in part, through the defect of the instrument. In the meanwhile it follows, from what has been advanced at present, and from the tables, that a period, not very different from that of a half-century, would serve in both cases for a sufficient explanation of the observations. I think it, however, expedient in the present state of the subject to wait for a further development of the nature of the change, from the observations of the next half-century, before pronouncing a judgement thereupon. This has, besides, no real value for the objects of astronomy before the nature of the motion of *all* the stars of the fundamental list are known.

I have investigated the conditions which must be fulfilled, that a sensible change of the proper motion, like that observed, may be capable of explanation by means of a force of gravitation. If the star that exhibits it be represented by S; an attracting mass by m_n, and the corresponding star by S_n; the sun by O; the distance SS_n by r_n; OS_n by r_n'; OS by ρ; the angle at the star S by s_n; the angle made by the plane OSS_n with the plane of motion of the star S by u_n, we easily find the expression of the second differential coefficient of the apparent motion of the star, with respect to the time,

$$= \frac{m_n}{r_n r_n} \cdot \frac{1}{\rho}\left(1 - \frac{r_n^3}{r_n'^3}\right) \sin s_n \cos u_n,$$

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* That of Maskelyne for 1770 has little weight.

and the half of this is the expression for the real motion in a unit of time, answering to a uniform motion with the velocity at the commencement of that unit of time. Take now a century as the unit of time; the sun's mass as the unit of mass; the distance divided by sin $1'' = 206265$ times the mean distance of the earth from the sun as the unit of distance; then this expression becomes

$$= 0{\cdot}00000464 \,.\, \frac{m_n}{r_n r_n} \,.\, \frac{1}{\varrho}\left(1 - \frac{r_n{}^3}{r_n{}'^3}\right)\sin s_n \cos u_n.$$

Since the motion resulting from the foregoing observations is from ten to fifteen millions of times greater than this, it is necessary, for the explanation of it, by means of the attraction of *one* mass,—

1. That either m_n be very great; or,

2. That r_n be small, that is, the attracting mass very near to the disturbed star; or,

3. That r_n' be very small, that is, the attracting mass very near the sun. The smallness of ϱ, the distance of the star in motion from the sun, does not produce this effect, since it can be regarded as a factor of $1 - \frac{r_n}{r_n'}$. But, what *one* mass cannot effect,

4. The joint action of millions of existing stars might produce.

On the supposition that the hypothesis (1) is the true one, but is not connected with (2) or (3), the change of motion which the observations since 1755 have shown, must have existed during a long space of time with a similar amount and direction; for the relative positions of O, S, S_n, change during this time, by the smallness of the existing motions of the sidereal system, so little, that it does not enter at all into the consideration. The change of motion must also increase proportionally to the square of the time, and much greater values are obtained than are consistent with the numbers of Hipparchus: I find, for example, that this increase of the present observed change of the motion of Sirius, would alter its right ascension, in 2000 years, by more than three degrees. Independently of this contradiction of the most ancient observations, there is also very little probability that we should be living precisely at that time when a great proper motion of a fixed star had become changed into a motion in the opposite direction, and again becoming great.

But it would be yet far less likely that this circumstance should take place in two cases independent of each other. One is justified, then, both by observations and probability, to fall back upon the explanation (1), with the exclusion of (2) and (3).

Against the explanation (4) the same objections sufficiently hold.

If (3) were the right supposition, a mass existing so near the sun would produce great irregularity in the motions of the planets, which we do not observe to be the case.

There remains then the explanation (2) alone. Stars, whose motions, since 1755, have shown remarkable changes, must (if the change cannot be proved to be independent of gravitation) be parts of smaller systems. If we were to regard Sirius and Procyon as double stars, the change of their motions would not surprise us; we should acknowledge them as necessary, and have only to investigate their amount by observation. But light is no real property of mass. The existence of numberless visible stars can prove nothing against the existence of numberless invisible ones. There have been also stars which seemed to possess the peculiarity of a bright body passing over, and which have again lost it; for example, the star of Tycho. The phænomena then of the varying motions of the stars, which are so important for the results of plane astronomy, seem also to possess interest in relation to our knowledge of the physical constitution of the universe.

(Signed) F. W. Bessel.

GEOLOGICAL SOCIETY.

[Continued from vol. xxv. p. 539.]

November 20, 1844.—A paper was read "On the Geology of Gibraltar." By J. Smith, Esq. of Jordan Hill.

The great rocky masses terminating Europe on the S.W. and Africa on the N.W., and cut through by the Straits of Gibraltar, consist of siliceous sandstones, associated with limestone, chert, shale and coal, all apparently of the oolitic formation. The Gibraltar limestone contains casts of *Terebratula fimbria* and *T. concinna*, species found in Britain in the lower oolite. The covering of the older rocks consists of soil, river alluvium, post-tertiary marine sands, and local patches of diluvium. Wherever the covering is removed, the surface of the rock beneath is seen to be water-worn. The rock of Gibraltar is 1470 feet high. The southern extremity is marked by a triple series of terraces and inland cliffs, formed by the sea at former levels. Its northern terminates in a perpendicular cliff. The elevated part is divided into three distinct eminences, the effects of different local upheavals. The northern of these (the rock gun) does not appear to have undergone any derangement in its stratification since its first upheaval, although it must have been subjected to many elevations and depressions of level. Its older beds (those of the limestone) dip west at an angle of 20°, and those formed since the elevation are horizontal, remaining in their natural position. In this state the whole of the rock must have remained for a lengthened period, until a second upheaval broke it across, leaving the northern portion in its original position, but lifting the whole of the southern 20° more, so that its beds, which formerly dipped 20° west, now dip 40°; and the fresh deposits, formerly horizontal, 20°. On these deposits, others, formed after the upheaval, rest unconformably. A third upheaval in the same direction, but still further to the south, lifted the rock there about 20° more, leaving the northern and middle hills in their former position, but inclining the southern 60°. Thus we have four distinct epochs; of the deposits formed during each we have remains, and at Martin's Cave the whole may be seen in juxtaposition. Immediately under O'Hara's tower, the highest peak, the inclination of the beds to the west is nearly 80°, and a short way to the south of it, they are vertical. Under this point there is, at the height of about 50 feet, sloping inwards 11°, beds of sandstone in a sea-worn cave, proving at least one other disturbance in addition. Subsequent to these great disturbing changes, there occurred a series of elevations and depressions, indicated by mixed beaches and sea-bottoms at different levels and by the surface of the rock perforated by Lithodomi and sea-worn to the very summit, indicating that the amount of change of level in these comparatively modern times—for the fossils in these deposits are in every case identical with species now living in the neighbouring seas—exceeded the height of the mountain, or 1470 feet. There are evidences, also, of a series of movements of depression. All these changes must have preceded the historical period, as previous to the last change, Gibraltar must have been an island, of which there is no record; the most ancient accounts describing it as it is now. The upheaving forces must have been deep-seated, as there are no erupted igneous rocks near.

December 4.—A paper was read, entitled, "Remarks on the Geology of British Guiana." By the Chevalier Robert H. Schomburgk.

The geology of the district of British Guiana is chiefly confined to primitive rocks. At the mouth of the Orinoco is an extensive delta consisting of blue clay, which, when pierced, gives a supply of water, and Artesian wells have been sunk here in many places with success. Below the clay appear the remains of an ancient forest. The alluvial flat is terminated by sand-hills, beyond which occurs granite intersected by numerous greenstone dykes, and then commence the savannahs, which are traversed by large beds of conglomerate often containing iron ore, and pierced with lofty porphyritic hills. The savannahs are supposed to be the bed of an ancient lake. A region in which much jasper occurs next succeeds, and then a remarkable range of granitic mountains; and the author directed especial attention to the insulated rocks of grotesque form abounding in the district. He also remarked on the probability of gold being found in the river-courses, and on the appearance of the well-known diamond-matrix of Brazil.

A letter was next read from Mr. Trevelyan, remarking on the occurrence of polished and scratched surfaces in the neighbourhood of Conway, on the ascent of Moel Siabod, from Capel Carig, on Snowdon, and in other localities in North Wales.

December 18.—A paper was read "On the Pipes or Sandgalls in the Chalk and Chalk-rubble of Norfolk." By Joshua Trimmer, Esq.

The observations recorded in the present paper were made in chalk pits near Norwich, and the surface of the chalk was observed to be furrowed by irregular cavities, or deep cylindrical conical pipes, entering the chalk from the channeled surface.* The contents of the furrows appeared to be fine sand mixed with a light-coloured amber or yellow ochre, the former often filling up the cavities both here and elsewhere. The author considers, that although chemical agency may have assisted in the formation of these cavities and pipes, yet that it is necessary to admit also mechanical action, and he refers to several instances of the known effects of rain-water on cliffs, and ex-

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* Some remarks on this subject by Prof. Ehrenburg will be found in the Annals of Natural History, vol. ii. p. 161.

cavations of basins in river-beds, in proof of the probability of his opinion being correct.*

January 8, 1845.—The following communications were made :—

A paper by Mr. A. G. Bain "On the Geology of the South-Eastern extremity of Africa."

The principal object of this paper was to describe the district in which certain remarkable fossils had been obtained by the author and forwarded to England. The lowest stratified rock in this district is a red sandstone containing fragments of plants, which seem to resemble a common carboniferous species (*Lepidodendron Sternbergii*). Over this rock, and conformable to it, is a conglomerate of claystone porphyry containing pebbles, and to it succeeds clayslate. The next is the fossiliferous rock, and it consists of a disintegrated sandstone containing argillaceous matter in septarian nodules, the fossils being found in the nodules.

A notice, by Prof. Owen, of one of the genera of animals (*Dicynodon*) whose remains were forwarded by Mr. Bain. The most important character in this genus is the possession of two large tusks like those of the walrus, but the general structure of the bones indicates distinctly the reptilian character of the animal. The first of the species described by Prof. Owen was named *D. lacerticeps*, from its analogies with the lizards. In this species there is an exhibition of unusual strength in the bones of the face, but there is no mark of any other teeth than the two which give the peculiar character to the animal. Prof. Owen considers that the whole of the anterior part of the jaws was sheathed with horn in the same manner as the Chelonians, and this is the more interesting from the other analogies presented with the Chelonians. It appears indeed throughout, that this singular animal united the character of the Lacertians, Chelonians and Crocodilians. The second species described was named *D. testudiniformis*, and differed from the former in its greater resemblance to the Chelonians. A third species, *D. strigiceps*, is chiefly remarkable for the singular position of the tusks, placed far back behind the orbit of the eye. The nearest analogue of this singular genus is the *Rhynchosaurus* of the new red sandstone of England. An unexpected point of structure exhibited in these animals is the existence of tusks like those of mammalia, exhibiting no mark whatever of the presence of a succession of teeth, which in all other reptiles known invariably exist. The tusks of the Dicynodon were probably used as weapons of offence and defence, and the habits of the animal seem to have been marine.

XXXVII. *Notices respecting New Books.*

We are desirous of giving publicity in this country, to the subjoined announcement which has recently been issued in the United States :

STATISTICS OF COAL.

The Geographical and Geological Distribution of Mineral Com-

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* A paper on the Sand-pipes in the Chalk near Norwich, by Mr. Lyell will be found in Phil. Mag. S. 3. vol. xv. p. 257.

bustibles or Fossil Fuel, ranging from the True Coal up to the Tertiary Lignites and Peat or Turf, including Notices and Localities of most of the Mineral Bituminous Substances employed in Arts and Manufactures; embracing, from official reports of the coal-producing countries, the respective amounts of their Production, Consumption and Commercial Distribution, together with their Prices, Tariffs, Currency, Duties and International Regulations,

BY RICHARD COWLING TAYLOR,

FELLOW OF THE GEOLOGICAL SOCIETY OF LONDON; MEMBER OF THE AMERICAN PHILOSOPHICAL SOCIETY, AND OF VARIOUS OTHER INSTITUTIONS.

This work being designed for a somewhat limited class of readers, it is necessary to ascertain, in the first instance, if a sale can be effected adequate to ensure the publisher from loss. As soon, therefore, as a sufficient number of names are obtained to justify the risk, the work will be immediately put to press.

The work will be comprised in a royal octavo volume of about 750 pages, illustrated with Maps, printed in the best manner, on fine paper, and handsomely done up in embossed cloth, and will be furnished to subscribers at Five Dollars per copy.

In the meanwhile, the Author would be glad to receive, through the publisher, any useful communications in connection with the design of the work.

J. W. MOORE, Publisher and Bookseller,
Philadelphia, January 1845. 138 Chestnut Street, Philadelphia.

XXXVIII. *Intelligence and Miscellaneous Articles.*

ATMOSPHERIC PHÆNOMENA SEEN AT HIGH FIELD HOUSE, NOTTINGHAMSHIRE, ON THE 20TH AND 21ST OF OCTOBER 1844. BY E. J. LOWE, ESQ.*

ON the above-named days I noticed rare and interesting phænomena, which in my opinion are worthy of being recorded.

For two or three days prior to the 20th the weather had been showery, with slight frosts at night.

20th.—Barometer, max. 29·351; min. 29·248. Thermometer, max. 53°; min. 40°. Wind S.W. Mean amount of cloud $6\frac{8}{10}$. Few cumuli; fine sunshine a.m.; nimbus and cirrostrati; dull, with few drops of rain at 5 p.m.

Mock-Moon and Aurora Borealis.

$8^h\ 30^m$ p.m.—A brilliant halo of 45° in diameter encircled the moon. On turning to the direction of N. a very vivid and beautiful display of aurora borealis was disclosed to my sight. A broad stream of flame extended from the horizon through the constellations Auriga, Capella, Perseus, Cassiopeia, Cygnus and Lyra, and disappeared in the western horizon, near Serpentarius. This band flickered brightly for fifteen minutes, when it vanished.

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* Communicated by the Author, whose former observations will be found in Phil. Mag. S. 3. vol. xxv. p. 390.

8^{h} 40^{m}.—A mock-moon of a pale silvery hue shone out of the lunar halo on the N.E. side, being on the horizontal level of the moon. A flame of aurora crossed (through the broad flame) from the pole star through Perseus.

8^{h} 45^{m}.—Another stream of light extended through Ursa Major and Ursa Minor to Draco. At this time the flame from the polar star to Perseus was very vivid. During the whole of this display, the horizon from N.E. round by N. to N.W. appeared one continual flame, not glaring upwards, but along the horizon: therefore the chief portion of light was reflected from below. Paraselena still visible.

8^{h} 50^{m}.—The mock-moon disappeared. The lunar halo remained all night.

9^{h} 0^{m}.—The aurora borealis, comparatively speaking, vanished, but yet there was a slight glimmering now and then which would hardly have been noticed had not the phænomenon been perceived before.

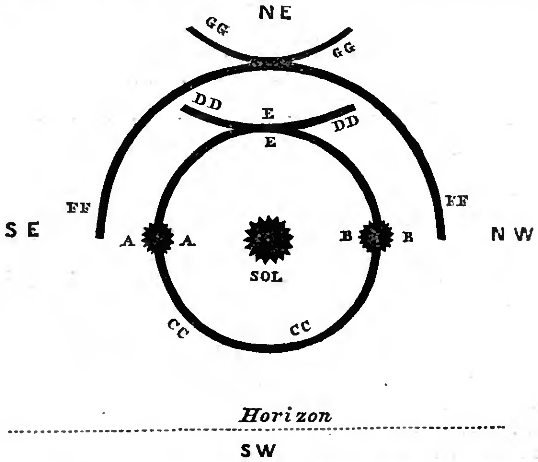
11^{h} 0^{m}.—The stars shone through a haze, which gave them the appearance of being surrounded by burrs.

October 21st.—Few cumuli and cirri; fine a.m., dull p.m.; rainy night. Barometer, max. 29·572; min. 29·241, rising. Thermometer, max. 55°; min. 39°: 9 a.m. 47°; 9 p.m. 46½°. Hygrometer, 9 a.m. 40°; 9 p.m. 40°. Wind S.E. Mean force of wind 2; mean amount of cloud 7.

Parhelia, &c.

11 a.m.—A highly prismatic halo, C C, of 45° in diameter surrounded the sun; the clouds around were thin cirri; those on the horizon cumuli.

11^{h} 30^{m} a.m.—An inverted arc of an iris, D D, resting *on* the summit of the halo C C appeared. Had the iris D D been a perfect



circle, it would have had a diameter of 96½°. At the point E of the iris D D, which cut the halo C C, the light was almost as brilliant as from the sun itself. This lasted till 0h 15m p.m., when it faded away, together with the halo C C.

1h 15m.—The halo C C and inverted arc of iris D D again appeared; the latter was very distinctly prismatic, having the red colour next the sun.

From this until about 2 p.m. no change worthy of mention, except that the inverted arc of the iris D D kept continually increasing and decreasing in brightness, and every now and then was so brilliant as scarcely to be looked at.

2h 1m.—A parhelion, A A, of a silvery colour appeared in the S.E. side of the halo C C and on the horizontal level of the sun.

2h 5m.—Mock-sun A A vanished. The portion E E of the inverted iris D D that cut the halo C C was extremely bright.

2h 9m.—A halo, or more properly speaking an iris F F, became visible, surrounding the upper or N.E. half of the halo C C at a distance of 15° from it; this was of a silvery hue.

2h 10m.—Another inverted arc of an iris, G G, appeared at the vertex of the iris F F; had this described a circle its diameter would have been 64°; the inverted iris D D was brilliant in the highest degree. [Both the inverted irises, D D and G G, were situated on the halo C C and iris F F, and opened in a direction to the N.E.]

2h 15m.—Atmosphere very dense. The iris F F and inverted iris G G at its vertex, together with the S.E. side of the halo C C, disappeared. The inverted iris, D D, was very brilliant. Few cirrostrati floated below the sun, and cumuli round the horizon.

2h 18m.—Halo C C very indistinct, but the inverted iris D D at its summit still very bright.

2h 26m.—Inverted iris D D vanished. The disappearance of this inverted iris was rather singular, for in most instances the brightest portion of the phænomenon continues to the last. A faint parhelion, B B, at this time was visible in the N.W. side of the halo C C, and likewise on the horizontal level of the sun. This lasted till 2h 30m, when all vanished.

2h 30m.—Scud and cirrostrati covered the whole visible surface of the sky, and became more dense every minute. The clouds were evidently formed on the spot, and plainly by the vapours reaching the cold current. Overcast close evening; rainy night.

Excepting the halo C C and inverted iris D D (which were prismatic), the colour was pale silver throughout.

It is somewhat remarkable that I should have been fortunate enough in little more than a year to have seen three such beautiful and rare atmospheric phænomena as those I have described:—the first, parhelia seen at Derby on the 16th of June 1843; second, paraselenæ seen at High Field House, May 1, 1844; and last, parhelia seen at High Field House, October 21, 1844. But it is recorded that John Hevelius was still more fortunate, for he in the short space of eleven months witnessed at Dantzic four of these very rare phænomena:—the first, paraselenæ on March 30, 1660; second, parhelia, April 6, 1660; third, paraselenæ, December 17, 1660; and fourth, parhelia, February 20, 1661. That noticed by Hevelius on the 30th of March, 1660, was very similar to the one seen at High Field House on the 21st of October, 1844; only this was parhelia, and that described by the Polish astronomer paraselenæ.

Since the appearance of the parhelia on the 21st of October, 1844, we have had a very considerable fall of rain.

High Field House, Lenton, Nottinghamshire, E. J. Lowe.
November 8, 1844.

DISTILLATION OF BUTYRATE OF LIME. BY M. G. CHANCEL.

When a small quantity of this salt, anhydrous and pure, is cautiously heated, it is quickly decomposed into carbonic acid, which remains combined with the lime, and a volatile oil which distils; this oil is butyrone, nearly pure and only slightly coloured; the residue consists entirely of carbonate of lime, which is perfectly white and pure. When care is taken not to exceed the temperature requisite for the formation of butyrone, with a quantity not exceeding about 100 grains, not the slightest trace of charcoal is deposited.

Most commonly no gas is evolved, unless the heat be too great; in this case the quantity of gaseous products amounts to 3 or 4 per cent. of the butyrate acted upon. The greater portion is formed of bicarburetted hydrogen absorbable by sulphuric acid.

It is worthy of remark, that, provided anhydrous butyrate be employed, no water is in any case disengaged, and yet it is well known that the distillation of organic matter is almost always accompanied with the formation of some water at the expense of the elements of the organic body.

When, however, considerable quantities of the butyrate of lime are submitted to distillation, the operation is far from being as simple; in this case the author could not avoid the deposition of some charcoal, and the liquid products obtained were always considerably coloured; this may be attributed to the ulterior decomposition of a portion of the butyrone formed, on account of the unequal distribution of the heat throughout the mass. In this case there is thus formed a mixture of several liquid substances, in which butyrone is the prevailing one.

In several distillations 100 parts of anhydrous butyrate of lime yielded from 42 to 43 parts of rough butyrone.

Preparation of pure Butyrone.

When the above-described impure liquid is submitted to distillation, the boiling-point, which is at first under 212°, soon rises nearly to 284°; the fluid which rises between 284° and 293° is to be kept separate, and is entirely butyrone; the products which are collected previously and subsequently are mixtures of butyrone, and two peculiar substances, one of which distils below 212° and the other at above 320°; the butyrone distilled between 284° and 293°, once redistilled, has a constant boiling-point, and may be regarded as pure butyrone.

The properties of butyrone are, that it is a colourless limpid liquid, possessing a strong peculiar odour and a burning taste; its density is 0·83, and it boils at about 290°; when submitted to cold produced by solid carbonic acid and æther, it becomes a crystalline mass. It floats on the surface of water, in which it appears to be nearly insoluble, although it imparts its odour to it; it mixes with alcohol in all proportions; it is readily inflammable and burns with a bright flame. The action of chromic acid on butyrone is extremely vivid, it burns as soon as they come into contact; it does not acquire colour by exposure to the air, although it eventually absorbs a considerable quantity of oxygen.

Several analyses of this substance gave similar results, which indicated as its formula C^7H^7O; the density of the vapour confirms this formula. Experiment gave the number 4·0, very near the theoretic number 3·96. The formula C^7H^7O therefore represents two volumes of the vapour of butyrone.

This composition sufficiently explains the formation of this substance; butyrate of lime, submitted to distillation, is decomposed into carbonic acid, which remains combined with the base; and in butyrone, which distils, this is shown by the following equation:—

$$\underbrace{\text{CaO, } C^8H^7O^3}_{\text{Butyrate of lime.}} = \underbrace{\text{CaO, } CO^2}_{\text{Carb. of lime.}} + \underbrace{C^7H^7O}_{\text{Butyrone.}}$$

Thus the dry distillation of butyrate of lime is not less perfect than that of the acetate of the same base; the decomposition takes place readily, and the quantity of butyrone obtained under proper circumstances agrees with the indications of theory.—*Ann. de Ch. et de Phys.*, October 1844.

POTASH AND OXIDE OF CHROMIUM. BY M. FREMY.

Hydrated oxide of chromium dissolves in the alkalies and forms a green solution, which has the same green colour as the salts of chromium. The author could not succeed in obtaining definite compounds of oxide of chromium and an alkali, owing to the extreme instability of these compounds; the history of the compounds of oxide of chromium with bases may be reduced to the following facts:—

Oxide of chromium, like that of zinc, dissolves only in the alkalies when it is hydrated; when anhydrous it is insoluble in alkaline solutions; when oxide of chromium combined with an alkali is dehydrated, it immediately quits the base and is precipitated; so that when oxide of chromium is dissolved in potash, the solution remains bright while it is not heated, but if boiled the metallic oxide is dehydrated by the action of the potash and is precipitated; if also a solution of oxide of chromium in potash be evaporated *in vacuo*, it is decomposed, and the effect is more rapid when the solution is strongly alkaline; these effects are produced upon several oxides soluble in the alkalies. When potash and oxide of chromium are calcined together in a silver crucible, the oxide absorbs oxygen and a chromate is formed.

The following process yields a pure and perfectly crystallized oxide of chromium:—heat neutral chromate of potash to redness in a porcelain tube, and pass a current of chlorine over it; the chlorine is entirely absorbed, oxygen is disengaged, and there remains in the tube a mixture of oxide of chromium in fine crystals, and chloride of potassium: this crystallization of oxide of chromium appears to have some analogy with that of peroxide of iron in the calcination of a mixture of common salt and sulphate of iron.

The temperature at which the reaction is effected influences the crystallization of the oxide; when the operation is conducted at a low temperature, the oxide is in large transparent laminæ of a green colour; but when the chlorine is passed over the chromate previously made very hot, the oxide of chromium produced is then black and very hard, and resembles that prepared by M. Wöhler's process, which consists in decomposing by heat the bichromate of perchloride of chromium.—*Ann. de Ch. et de Phys.*, December 1844.

PERCHLORIDE OF CHROMIUM. BY M. WÖHLER.

By the following process a chloride of chromium is obtained which contains more chlorine than the protochloride:—put into a porcelain tube some peach-blossom coloured crystallized anhydrous chloride of chromium; heat it and pass over it a current of chlorine; it is readily converted into a brown substance, which sublimes and has all the properties of a perchloride.

This perchloride differs, in the first place, from the protochloride by its colour; secondly, the anhydrous protochloride is well known to be insoluble in water, whereas the perchloride is soluble and even deliquescent; when treated with hot water it is immediately decomposed, and yielding chlorine is converted into protochloride. The author has not yet obtained this compound quite free from protochloride, and has not, therefore, yet submitted it to analysis.—*Ann. de Ch. et de Phys.*, December 1844.

DOUBLE CHLORIDE OF POTASSIUM AND CHROMIUM.

M. Fremy remarks, that it is well known that chloride of chromium does not combine directly with the alkaline chlorides; but he found that when these two chlorides were formed together, they combined and formed double chlorides; thus when chlorine is passed over a mixture of charcoal and chromate of potash, a fine double chloride of potassium and chromium is formed; this is effected by putting the mixture into a tubulated earthen retort, and when red-hot passing a current of chlorine over it; a considerable quantity of anhydrous protochloride is formed, which sublimes, and at the bottom of the retort there is found the double chloride of potassium and chromium. This salt has a fine rose colour; it is very soluble in water and deliquescent; the water, which at first dissolves it, afterwards decomposes it into chloride of chromium and chloride of potassium. This action shows that it is not possible to preserve this salt in the humid way; it was found impossible directly to combine, by the agency of heat, anhydrous chloride of chromium with chloride of potassium.—*Ann. de Ch. et de Phys.*, December 1844.

ON HEMIPINIC ACID. BY M. WÖHLER.

This is the product of the oxidizement of opianic acid. It is prepared with difficulty, because it is destroyed by the same influences as those by which it is produced. It is obtained as follows:—heat together to ebullition opianic acid and peroxide of lead, then drop gradually into the mixture sulphuric acid to disengage carbonic acid. A small quantity of the liquid is then to be allowed to cool, and a sufficient quantity of sulphuric acid is to be added to precipitate all the lead which has been dissolved; filter and evaporate the solution. It often happens that the crystals first obtained are opianic acid, which are however easily separated from the hemipinic acid by crystallization, the latter being much more soluble; peroxide of lead alone does not act upon opianic acid.

Hemipinic acid crystallizes very readily in colourless oblique four-sided prisms; it has a slightly sour and astringent taste, and dissolves readily in cold water. The solution has a strong acid reaction; alcohol dissolves this acid readily; the crystals exposed to 212° lose 13·73 per cent., or two equivalents of water. The effloresced acid fuses at 356° and becomes a crystalline mass on cooling; when heated between pieces of glass, it sublimes like benzoic acid in brilliant laminæ; it burns with flame, and when heated with peroxide of lead and sulphuric acid, it appears to be entirely converted into carbonic acid and water; with ammonia it forms a readily soluble crystallizable salt, which is unalterable in the air. The salts of silver and lead are white and insoluble; the latter is soluble in acetate of lead, and separates later from solution in mammillated crystals.

This acid, after exposure to 212°, yielded by analysis,

	Experiment.	Equivalents.	Calculation.
Carbon....	52·94	10	53·14
Hydrogen..	4·65	5	4·41
Oxygen ..	42·41	6	42·45
	100·		100·

The salt of silver was found to consist of—

		Equivalents.	Calculation.
Carbon........	27·19	10	27·28
Hydrogen......	1·83	4	1·81
Oxygen........	18·10	5	18·18
Oxide of silver..	52·88	1	52·73
	100·		100·

Hemipinic acid is then represented by the formula $C^{10}\underline{H}^{4}O^{5}$; the effloresced acid contains one equivalent of water, which is separable by bases $=\dot{\underline{H}}+C^{10}\underline{H}^{4}O^{5}$.

One equivalent of opianic acid plus one equivalent of oxygen form two equivalents of hemipinic acid. It contains as its radicle half of the radicle of opianic acid, and hence the name of *hemipinic acid.*

The author obtained this acid also by directly acting upon narcotina with sulphuric acid and peroxide of lead, but this process did not always succeed. The narcotina is not acted upon by the peroxide of lead alone; but when dilute sulphuric acid is poured upon a mixture of the peroxide and narcotina, the mass becomes hot, accompanied with a rapid escape of carbonic acid gas; the solution which remains after the reaction possesses a yellow colour and a very bitter taste; dependent upon the nature and duration of the reaction, the liquid contains *cotarnina*, opianic acid, and probably other products. The carbonic acid disengaged sometimes has the smell of cyanogen, but the presence of this substance was not proved.—*Ann. de Ch. et de Phys.*, October 1844.

NEW COMET.

A large comet was discovered at Nevis (West Indies) on the evening of the 5th of January last, at sunset, in the constellation Grus. It is described as a very splendid object, the nucleus being distinctly visible, and the tail extending to a length of 10°. On the 6th of January it was near δ Gruis; on the 9th it made a near approach to the star θ in the same constellation. In the four days, from Jan. 6 to Jan. 10, its motion in right ascension was $+46^m$, and its motion in south polar distance $+52'$.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1845.

Chiswick.—January 1. Foggy. 2. Cloudy: frosty. 3. Frosty: cloudy. 4. Overcast. 5. Rain. 6. Very fine. 7. Thick haze: very fine. 8, 9. Foggy. 10. Thick haze: boisterous at night. 11. Boisterous, with rain. 12. Hazy: rain: fine. 13. Overcast: clear. 14. Rain: drizzly: heavy rain. 15. Rain: fine. 16, 17. Overcast. 18. Densely clouded: heavy rain. 19. Clear: hail-shower at noon: constant heavy rain at night. 20. Boisterous. 21. Clear and frosty. 22. Sharp frost: hazy: fine: overcast. 23. Overcast: rain. 24. Fine: rain: frosty. 25. Clear and frosty: overcast: boisterous. 26. Boisterous: clear. 27. Rain: boisterous: cloudy. 28. Slight snow: fine, with sun: rain. 29. Overcast. 30. Hazy: frosty. 31. Frosty: cloudy: frosty.—Mean temperature of the month 2°·36 above the average.

Boston.—Jan. 1. Foggy. 2. Snow. 3, 4. Fine. 5. Cloudy: rain early A.M. 6—10. Cloudy. 11. Windy: rain early A.M.: rain P.M. 12. Foggy. 13. Foggy: rain A.M. 14. Cloudy. 15. Cloudy: rain early A.M. 16. Cloudy. 17. Fine. 18. Cloudy: rain P.M. 19. Cloudy. 20. Stormy: rain early A.M. 21. Fine. 22. Rain. 23. Cloudy. 24, 25. Fine. 26. Stormy. 27. Snow: rain A.M. 28. Fine: rain A.M. 29. Cloudy. 30, 31. Fine.

Sandwick Manse, Orkney.—Jan. 1. Bright: cloudy. 2. Clear: frost: cloudy. 3. Clear: frost: showers. 4. Showers: rain. 5. Showers: cloudy. 6. Showers: clear. 7. Bright: clear: aurora. 8. Clear: frost: clear. 9. Clear: cloudy. 10. Cloudy. 11, 12. Bright: clear. 13. Cloudy: drops. 14. Cloudy. 15. Bright: clear. 16. Clear: cloudy. 17. Showers. 18. Bright: cloudy. 19. Bright: frost: clear: frost. 20. Bright: frost: clear: aurora. 21. Cloudy: frost: drops. 22. Bright: showers: halo. 23. Showers. 24. Showers: clear. 25. Rain. 26. Showers: hail-showers. 27. Cloudy. 28. Snow-showers: clear: aurora. 29. Snow-showers: drift. 30. Clear. 31. Showers.

Applegarth Manse, Dumfries-shire.—Jan. 1. Frost: dull. 2. Slight frost. 3. Thaw: frost A.M. 4. Frost A.M.: rain P.M. 5. Rain A.M. 6. Dry and mild. 7. Rain early A.M. 8. Fair, but dull. 9. Fair: slight frost. 10. Fair, but dull: rain P.M. 11. Heavy rain A.M.: flood. 12. Fair and mild. 13. Rain: frost. 14. Fair. 15. Dull and cloudy. 16. Frost. 17. Slight frost: thaw P.M. 18. Rain. 19. Snow: rain P.M. 20. Frost: clear. 21. Frost: thaw P.M. 22. Thaw: fog. 23. Rain. 24. Rain early A.M. 25. Frost early A.M.: rain. 26. Rain: flood. 27. Snow: slight frost. 28, 29. Frost, severe: snow lying. 30. Frost, severe: snow. 31. Very hard frost.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Jan.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
☾ 1.	30·146	30·121	29·85	30·05	30·05	30·17	30·18	45	34	36	35½	23½	44	41	ne.	calm	e.	sw.	……	……	……	……
2.	30·061	30·008	29·77	30·02	29·85	30·13	29·92	44	24	36	39½	33	34	37	n.	calm	n.	se.	……	……	……	……
3.	29·974	29·924	29·62	29·69	29·78	29·70	29·66	40	29	32	39	25½	37	43½	sw.	calm	n.	se.	……	……	……	……
4.	30·142	30·141	29·78	29·95	29·74	29·74	29·43	45	36	33·5	43	30	43½	45	sw.	calm	sw.	ssw.	·02	……	……	·14
5.	30·110	30·073	29·60	29·70	29·66	29·38	30·35	53	42	46	48½	41	49	49	sw.	w.	wsw.	sw.	·08	·31	……	·22
6.	30·100	30·084	29·60	29·75	30·00	29·56	30·03	54	43	46	49	44	40	40	sw.	w.	sw.	sw.	……	……	……	·12
7.	30·239	30·206	29·79	30·03	30·03	30·03	30·02	54	32	45	43	33½	42	40	w.	w.	n.	ssw.	……	……	0·55	……
● 8.	30·232	30·137	29·80	29·99	29·92	29·98	29·90	35	31	39	42½	34½	36	40	se.	calm	s.	s.	……	……	……	……
9.	30·089	30·047	29·77	29·88	29·75	29·82	29·66	35	28	35	40½	30	42½	42	se.	calm	nnw.	sse.	……	……	……	……
10.	29·946	29·764	29·63	29·54	29·20	29·43	29·17	49	30	33	47	34½	43	44½	s.	sw.	se.	se.	·04	……	……	……
11.	29·665	29·643	29·24	29·30	29·48	29·14	29·42	51	44	48	47	39½	43	39	sw.	sw.	se.	ssw.	·77	·09	……	……
12.	29·776	29·745	29·48	29·62	29·68	29·58	29·72	47	32	39	42½	35½	39	38	ne.	calm	sse.	s.	·16	·05	……	……
13.	29·662	29·555	29·37	29·46	29·30	29·72	29·64	51	30	39·5	41	31½	38	43	s.	calm	e.	ese.	·04	……	……	……
14.	29·730	29·606	29·33	29·44	29·63	29·78	29·75	50	35	40	44	36½	43	43	se.	calm	e.	se.	·12	·05	0·26	·05
☽ 15.	29·778	29·666	29·35	29·50	29·60	29·66	29·77	50	39	41	41	36	43	39	se.	calm	e.	se.	·02	·08	……	……
16.	30·080	29·983	29·64	29·81	29·81	29·85	29·78	48	32	42	38½	27	38	38	sw.	calm	e.	se.	……	……	……	……
17.	30·095	29·988	29·72	29·76	29·67	29·69	29·65	42	37	37	45½	31½	41½	42	s.	calm	se.	se.	……	……	……	·12
18.	29·801	29·560	29·45	29·43	29·26	29·46	29·32	46	33	40	43½	37	41	41½	s.	s.	se.	se.	·45	……	……	·22
19.	29·568	29·000	29·20	29·20	29·10	29·23	29·23	45	32	35	40	33	37	35½	sw.	calm	se.	s.	·88	·37	……	……
20.	29·878	29·117	28·90	29·40	29·73	29·38	29·82	45	24	43	41	29	40	39	n.	se.	nw.	nw.	·03	·15	……	·12
21.	30·210	29·998	29·77	29·99	29·92	29·96	29·73	45	21	31	41	26	37	44	s.	calm	w.	s.	……	……	……	·05
22.	30·199	30·178	29·82	29·92	29·88	29·81	29·71	48	38	35	47	38½	43½	44	s.	calm	w.	sw.	……	……	0·45	·04
○ 23.	30·135	29·677	29·60	29·59	29·34	29·34	29·40	48	39	47	47	41	43½	43½	sw.	calm	sw.	s.	·03	·04	……	·30
24.	29·937	29·586	29·16	29·36	29·75	29·53	29·65	46	25	40	46	41	43	43	w.	calm	nw.	nw.	……	……	……	·06
25.	30·038	29·680	29·60	29·60	29·14	29·29	29·00	51	42	34·5	48	32	45	49	sw.	calm	sw.	ssw.	·07	……	……	·43
26.	29·707	29·613	28·90	29·09	29·50	28·98	29·39	52	33	44	49	39	37	29	nw.	w.	nw.	nw.	·11	……	……	·91
27.	29·239	29·111	28·90	29·03	28·94	29·17	29·16	45	23	34	36½	30½	28½	32	w.	calm	wnw.	ne.	·04	……	……	……
28.	29·034	28·932	28·69	28·90	29·03	29·18	29·32	45	19	29	36	22	32	26	sw.	calm	nne.	ne.	·11	·20	……	……
29.	29·293	29·257	29·00	29·19	29·15	29·27	29·16	45	20	32	35	23	32	34	w.	calm	ne.	sse.	……	·08	1·25	……
30.	29·212	29·171	28·97	29·10	29·18	29·16	29·30	34	27	18	30	21	32	24½	sw.	calm	ne.	wsw.	……	……	……	……
☾ 31.	29·601	29·131	29·14	29·40	29·74	29·60	29·76	35	22	21·5	31	07	34	35½	n.	calm	ne.	n.	……	……	……	·19
Mean.	29·860	29·700	29·43	29·247	29·574	29·571	29·580	45·90	31·48	37·1	41·9	31·8	39·41	39·50					2·97	1·42	2·51	2·94

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XXXIX. *On the True Geological Position of the Macigno Formation in Italy and in the South of Europe.* *By* Prof. LEOPOLD PILLA*.

AMONG the formations which are the most extended in Italy, the macigno occupies a remarkable place. A great portion of the heights of Tuscany and of Liguria is composed of this formation, which extends also, on one side into the maritime Alps and those of Lombardy; on the other, into the kingdom of Naples and as far as Sicily. On leaving Italy we meet it with the same characters in the Pyrenees, on the northern sides of the Alps, in Greece, in Turkey, and, in general, in all the southern portion of Europe. It appears to mark the principal line of division between the geological zone of the north and the south of Europe,—a line which, as regards the formations under consideration, does not appear to extend on the west beyond Provence and the province of Nice; on the east, the Carpathian Mountains. At the commencement of this century, and during the prevalence of the Wernerian theories, false ideas were entertained respecting the age of this formation, which was generally considered as belonging to the graywacke†. But it was soon seen that it occupied in Italy a place between the Jurassic and the tertiary formations; and from that time the necessity was seen of carrying its age up to the chalk. This idea has been more and more confirmed, and at present it is adopted by all geologists, only that very little attention has been paid to fixing *with precision* the stage which the macigno occupies in the cretaceous group. But a particular study of this deposit, a more thorough examination of its geological relations, of the fossils which characterize it, and of many accidental circumstances

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* From the *Comptes Rendus de l'Acad. des Sciences*, January 1845.

† Brocchi, *Conchiologia Fossile Subapennina*.

which accompany it, have caused doubts in my mind respecting the position which is generally assigned to it; I would even say, have convinced me that this formation is wholly distinct, and independent of the chalk. I have been led to this conclusion by the exposition and the analysis of the facts which will form the object of the memoir which I have the honour to submit to the Academy. I shall divide this memoir into three parts:—1st, the distinction of the chalk formation in Italy; 2nd, a comparison of the chalk formation of Italy with that of France and of England; 3rd, the independence of the macigno and its geological position.

1. *Distinction of the Chalk Formation in Italy.*

It is generally agreed to divide the chalk formation in Italy into two portions, the *upper* and *lower*. The first is composed of the *macigno*; the second of a limestone, which, being characterized principally by species of *Rudista*, may be called *hippuritic*.

The macigno formation is so well known, that I shall not dwell long in describing its characters. I shall only observe that it is composed of two kinds of rocks, of a calcareous marl alternating with schists, which is commonly named *Alberese* in Tuscany, and of the macigno properly so called. The relative geological position of these two rocks is not absolutely constant. M. Pareto assures us that in Liguria the alberese is always superposed on the macigno; I have apparently seen the same thing in Tuscany, but in some localities the two rocks alternate and are mingled together. The fossils which most generally characterize the macigno are *Fucoids*, the most abundant species of which are *F. intricatus, furcatus, Targioni*. With regard to animal species, they are of extreme rarity: Nummulites have been found at Mosciano, near Florence, and I have met with some at Alberona, in the Pouille. I must mention another very important fossil found by the celebrated Micheli in the *pietra forte* of Florence; it is a fragment of a chambered shell, which, by the contour of the spiral, seems to belong to a *Hamites*, or perhaps to an *Ancyloceras*. This valuable fossil has been preserved in the collection of Targioni at Florence, where it was observed by Brocchi[*], by Nesti, and by Savi; the last also took a cast of it in plaster, which is now in the museum of the University of Pisa. It is truly to be regretted that the specimen I speak of has been lost in the changes which the above-mentioned collection has undergone. At the congress held at Milan, my friends MM. Pentland and Pareto assured me that they had found two

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[*] *Conchiologia Fossile Subapennina*, tomo i. p. 17.

Ammonites; one in the *pietra forte*, with which the city of Florence is paved, the other in the macigno of the environs of Genoa. These discoveries are important, on account of the rarity of animal remains in the formation which we are now considering. Carbonaceous deposits are also found in the macigno, such as the stipites of Pupiglio in the Pistojais, of the valley of the Taro, in Lunigiana, &c.*

I will now say a few words of the extent of this formation in Italy. In the map of Sicily by M. Hoffmann, it is represented by the *grès apennin à fucoïdes*, by the conglomerate subordinate to the latter, and by the argillaceous schists. In the country of Naples, on this side of the lighthouse, the formation of the macigno is very rare; I have only observed it in the mountains of Bovino in the Capitanate, with *Fucoids* quite similar to those of the Tuscan macigno. I could not point out the spots in the Papal states where this formation is found, with the exception of the Apennines of Bologna, where it is continuous with that of Florence. The macigno of Tuscany, of the Modenese and of Liguria is so classic, that I shall content myself with merely naming it here. It is also found, and very distinct, at the foot of the Alps of Lombardy, especially in the environs of Gavirate, where it has been examined by the Geological Section of the meeting held at Milan. This locality is very remarkable, not only for the great number of *Fucoids* perfectly similar to those of the macigno of Florence which it contains, but also for other species which remain to be determined by some able algologist.

2. *Comparison of the Chalk Formation of Italy with that of France and of England.*

In this part the author attempts to establish the fact, that three deposits of nummulites are distinguishable in Italy:—1st, the tertiary ones of the Vicentin, if, however, they shall continue to retain the place which they have hitherto occupied; 2nd, the nummulites of the macigno; 3rd, those of the hippuritic formation. We may reason, *à priori*, that their species must be different in these three positions; but it is desirable that, for the interests of science, some able palæontologist should undertake the task of classing them, in order that they may serve as marks for the distinction of the deposits which contain them†.

He ends by concluding, that the facts which he has related tend to establish,—

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* An account of one of these deposits of lignite in Lunigiana, by Prof. Mojon, will be found in Phil. Mag., First Series, vol. xvi. p. 324. Edit.

† This work is promised us by M. Leymerie, who, by its execution, will without doubt render a great service to the geology of the south of Europe. —(Note by M. Pilla.)

1st. That the northern chalk is connected with the nummulitico-hippuritic limestone of the south of Europe, but never with the macigno, which is above this.

2nd. That the nummulitico-hippuritic limestone of Italy represents all the northern cretaceous limestone; in the greatest part the upper and lower greensand, and only in certain localities the white chalk.

3. *Independence of the Macigno Formation.*

If we admit that the nummulitico-hippuritic limestone of the Mediterranean is the representative of the whole of the chalk of the north of Europe, and that the macigno is superposed on this limestone, we must admit also that this formation constitutes a peculiar deposit and one wholly distinct from the chalk. This distinction is based on all the characters which establish the independence of a formation,—on the mineralogical characters, on the superposition, and on the fossils, as we shall presently remark.

The macigno of Tuscany and of Liguria, which is the most classic, has no mineralogical analogy with the chalk of the north-west of Europe; the rocks which compose it have quite peculiar characters. To this difference we must add another very remarkable circumstance; the flint, which appears to be a substance almost inseparable from the upper northern chalk, is *entirely* wanting in the Italian macigno; and, although this circumstance may be considered in general as of little value, it is of great weight in this special case. There have likewise not been found any of those green particles which are frequently met with in the cretaceous greensand of the north, whence it takes its name.

With regard to the superposition, we have seen, first, that the macigno must be considered as above the white chalk. In the second place, we have observed that the different beds of the nummulitico-hippuritic limestone, which are parallel to the beds of the northern chalk, are connected together by insensible gradations, which proves that they have been deposited in the same sea, and under the same circumstances; whilst the macigno is always separated from these deposits by a well-marked line and by different topographical circumstances, and never mixes with them, which is an evident sign that it has been deposited in a different sea, and under different circumstances. Lastly, if one of the principal characters of the independence of a formation is its superposition on rocks of various ages, this is verified in the macigno more than in any other formation, because it is seen superposed at one time on the nummulitico-hippuritic limestone (Liguria), at another on the Jura limestone (Tuscany); lastly, at another locality on crystalline rocks (Isle of Elba). And it is a truly remarkable thing, that in Tuscany, where this formation is very greatly developed, it is never found associated with the nummulitico-hippuritic limestone,—so independent is it of this latter.

In the last place come the fossils to confirm the distinction established. There has never been found, either in the macigno of Tuscany, or in that of any other locality, that I know, any fossil belonging to the chalk formation of the north-west of Europe. It is a well-known fact, that the principal organic remains which characterize this formation are the *Fucoids*. Now these are wholly wanting in the northern chalk formation; and, a fact which deserves to be remarked, they are also wanting in the southern nummulitico-hippuritic limestone. On the contrary, the *Rudistæ*, which are very abundant in this last deposit, may be considered as entirely foreign to the first, not a single individual having hitherto been found in it. The same must be said of the *Actionellæ*, which habitually accompany the *Rudistæ*. M. d'Orbigny has shown that this latter family (comprising in it the *Cranicæ*) occur in *all the strata* of the chalk formation of Europe. If then it has no species which represents it in the macigno, we must conclude that this deposit does not belong to the cretaceous system. Perhaps we might cite the *Nummulites*, which are common to the macigno and to the limestone which is below it, and the *Ammonites*, which have been taken from the macigno of Tuscany and Liguria; but, without regarding the extreme rarity of these fossils in the macigno, can it be affirmed that the species are the same as those which we find in the nummulitic limestone? No one can assert this*.

We may then conclude,—

1st. That the macigno has different mineralogical characters from those of the chalk.

2nd. That it is superposed on the nummulitico-hippuritic limestone, the upper part of which is connected with the white chalk of the north of Europe.

3rd. That it does not contain any fossil of the northern chalk, but that it contains *Fucoids*, absent in the latter as well as in the southern nummulitico-hippuritic limestone.

All the facts which I have here detailed appear to me to

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* The Nummulites of the cretaceous limestone of Italy are ordinarily of a large size, and resemble the Nummulites of Peyrehorade in the Pyrenees, which are figured in Lyell's Elements of Geology (cretaceous group). Of this nature are the species which I have found in the Gargano, and those which M. Pareto cites in the limestone of Mortola, in Liguria. On the contrary, the Nummulites which have hitherto been found in the macigno are much smaller.—(Note by M. Pilla.)

prove evidently that the macigno formation is wholly independent of the chalk formation, and that it may be separated from it by characters of a greater value than those which have served to determine the distinction of the Devonian and Silurian carboniferous formations. We must consider it as the last secondary deposit, occupying a place between the chalk and the tertiary formations. In the period in which it was deposited a change had taken place in the nature of the sediments in relation to those of the anterior (cretaceous) period; the one had been entirely calcareous, the others in great part arenaceous. In the period of the macigno, the family of the *Rudistæ* had ceased to people the seas of the south of Europe, and with it had disappeared also the *Nerineæ*, and almost the whole of the *Actionellæ*; only some few species of *Nummulites* and *Ammonites* had continued their languishing existence, to become extinct at the end of these deposits. These considerations authorize me then to distinguish the macigno as a formation of a particular age, and to assign to it a special name, on account of the great part it acts in the sediments of southern Europe. I propose to name it the *Etrurian System*, for the reason that it has been recognised for the first time in a classical manner in the soil of Tuscany. I conclude these observations on the macigno, and on the chalk formation of Italy, by confronting their divisions in a Table with those of the northern chalk.

	Northern zone.	Southern zone.
Etrurian formation	Wanting	Alberese. Macigno.
Upper chalk formation	White chalk	Indistinct. Limestone with *Ostrea vesicularis*, *Catillus*, *Belemnites mucronatus*, &c.
Lower chalk formation	Upper greensand	*Glauconie* and nummulitic limestone.
	Gault	Wanting, or indistinct.
	Lower greensand	Neocomian formation.

XL. *On the Changes produced in Metallic Wires which have served a long time as Electric Conductors. By* M. Peltier, *in a Letter to* M. Arago[*].

THE important and beautiful experiment which you are about to make on the conductibility of wires, in the establishment of an electric telegraph, induces me to communicate to you two facts which are incidentally connected with this subject. About twelve years ago, being desirous of tracing

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[*] From the *Comptes Rendus*, &c., January 1845.

the arrangement of the molecules of lead in the reduction of the acetate, I employed, as is done to produce the tree of Saturn, a slip of zinc placed in the centre of a broad spiral of thick pure copper wire; the two upper emerging extremities were soldered to form a voltaic couple. At the end of six months or a year, the immersed portion of the copper wire, originally very ductile, became so brittle, that it broke with the least effort; the external portion nearly preserved its ductility, at least it retained it longer. The brittle wire presented a dull granular fracture, which indicated not only a different molecular arrangement, but also that it had formed an alloy by cementation with one of the elements of the solution.

This same fact of the fragility of the wires subsequently occurred again, on the occasion of my establishing a fixed electrical apparatus above my house. These apparatus are formed, as is known, of long copper wires stretched horizontally, intended to draw away the electricity of the atmosphere to conduct it to the common centre, after having traversed a rheometer. These wires, thus exposed to the changes of the seasons, of atmospherical agents, and to the electric currents, became brittle, and I was obliged to renew them at the end of two years. Wires silvered over did not last longer; brass wires broke at the end of six months. Sheltered conductors retain their ductility much longer, but at last become brittle under the influence of a permanent current.

I purpose, the first fine weather, and as soon as my health will permit me to go over the tops of the buildings, to renew all the wires of my apparatus, substituting for them pure copper wire well-annealed and perfectly tinned. I hope that the layer of tin will be less permeable to external agents than copper and silver, and that the cementation which assists the effect of the electricity will be greatly diminished.

I may add, in conclusion, that it is useless to stretch the wires strongly, for too great a tension prevents their retraction when they have been dilated, and the curve of the wire increases at each change of temperature, until there is an equilibrium between the two antagonist forces, the mechanical extension on the one part and the contraction on the other.

XLI. *Measurements of the Rainbow.* *By* J. G. Galle*.

WITH reference to the investigations of Airy† and of Miller‡, I here communicate some distances of the rainbow from the sun deduced in the year 1843.

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* From Poggendorff's *Annalen*, vol. lxiii. p. 342.

† Transactions of the Cambridge Philosophical Society, vol. vi. p. 379.

‡ Phil. Mag. S. 3. vol. xviii. p. 520.

1843.		Distance from the sun.	Reduced to the brightest part of the spectrum.	Distance of the brightest portion from the sun.
		° ′	′	° ′
May 28.	Extreme red	137 48·7	+40·0	138 28·7
	Red	138 1·5	+29·0	138 30·5
	Yellow	138 33·6	0·0	138 33·6
	Blue	139 5·9	−31·2	138 34·7
June 22.	Extreme red	138 16·0	+40 0	138 56·0
July 1.	Yellow	138 16·9	0 0	138 16·9
	Between green and blue	139 8·6	−21·2	138 47·4
	Blue	139 15·3	−31·2	138 44·1
	Violet	139 28·1	−76·0	138 12·1
	Red	138 8·9	+29·0	138 37·9
	Green	138 40·8	−12·0	138 28·8
	Violet	139 6·3	−76·0	137 50·3
	Red	137 38·8	+29·0	138 7·8
	Yellow	138 29·6	0·0	138 29·6
	Green	138 48·5	−12·0	138 36·5
	Blue	139 1·4	−31·2	138 30·2
July 24.	Violet	139 14·0	−76·0	137 58·0

These measurements were executed in the following manner:—I observed the *time* when the inferior portion of the rainbow touched certain terrestrial objects, whose azimuth and height I subsequently determined, and then compared with the places of the sun taken from the astronomical Ephemeris for the time in question. The refraction of the sun, the influence of which is small for the portion of the rainbow situated near the horizon, has been neglected. The azimuths of the terrestrial objects were determined by a terrestrial object, whose azimuth was previously known and was tested by comparison with an azimuth of the sun. The reduction to the brightest part of the spectrum has been computed approximatively according to Fraunhofer's figure of the spectrum of the sun, and cannot for several reasons lay claim to any great accuracy. If, without any regard to the differences which occur, the mean be taken of the seventeen independent determinations, there is obtained 138° 27′·2, or for the radius of the rainbow,

41° 32′·8.

This agrees exactly with what Prof. Miller of Cambridge* observed on an artificial rainbow, viz. 41° 32′. The theoretical radius of the rainbow is 41° 27′, the geometrical radius 41° 53′·9. If in the above measurements we take the colours red to yellow (which are the most definite) and green to violet together, we obtain from the eight measurements by the former colours 41° 29′·9, from the nine measurements by the latter 41° 35′·3.

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* Phil. Mag. S. 3. vol. xviii. p. 520.

XLII. *Observations on a Communication made by* Dr. Fitton *to the Geological Society of France at the Meeting of May* 20, 1844, *on the Lower Greensand of the Isle of Wight. By* Prof. A. LEYMERIE*.

IT will be imagined, that having investigated in a very especial manner the chalk formation of Champagne, I could not remain indifferent to the discovery recently made in England, at the base of the *Greensand*, of strata containing a certain number of Neocomian fossils of the east of France. I took a lively interest in the communications upon this subject, both in the Proceedings of the Geological Society of London, which have been obligingly communicated to me by Dr. Fitton†, and in the *Bulletin de la Société Géologique de France* (vol. i. of the second series, p. 438).

The principal object of this note is to express my opinion on the relations which may connect these new strata with the Neocomian formation of the continent. However, it seems to me indispensable, before coming to this, to cast a critical glance on the list of species which Dr. Fitton considers as being common to the greensand of England and the Neocomian formation of France, and to show that several species which M. Deshayes and myself have established, are there *wrongly* represented as related to types formerly known by Sowerby and Nilson.

The contested species in question are given in the following list, in which I have placed opposite to each other our names and those which Mr. Edward Forbes has substituted for them.

Names given by MM. Deshayes and Leymerie.	Corresponding names given by Sowerby and by Nilson, according to Mr. Edward Forbes.
Pholadomya Prevosti, Desh.	*Mya plicata*, Sow.
Astarte Beaumontii, Leym.	*Astarte obovata*, Sow.
Cardium subhillanum, Leym.	*Cardium striatulum*, Sow.
Cucullæa Gabrielis, Leym.	*Arca exaltata*, Nilson.
Modiola Archiaci, Leym.	*Modiola æqualis*, Sow.
Pinna sulcifera, Desh.	*Pinna tetragona*, Sow.
Pecten interstriatus, Leym.	*Pecten obliquus*, Sow.
Ampullaria lævigata, Desh.	*Natica rotundata*, Sow.

Having submitted these species to a new examination, and having again compared them carefully, under the eyes of M. Deshayes, with the types to which Mr. Forbes has referred them, I believe I can state here with some certainty that the determinations of this conchyliologist are wholly inadmissible.

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* From the *Bullet de la Soc. Géol. de France*, S. 2, vol. ii. p. 41, having been read November 4, 1844.

† The papers here referred to will be found in Phil. Mag. S. 3. vol. xxiv. p. 222, 224, 308; and vol. xxv. p. 220.

To convince the Society of this, it will suffice to recall here the principal differential characters which led me, in 1842, to consider the species in question as new; characters the majority of which had already been indicated in the short descriptions annexed to my memoir.

Pholadomya Prevosti, Desh., differs from the *Mya plicata*, Sow., by an abrupt inflection which it bears on each of its valves, close to the posterior margin, by its more oblong form, diminishing more rapidly in height from one extremity to the other. Moreover the species of Sowerby is a *Panopæa*, according to M. Deshayes, whilst that of l'Aube belongs to the genus *Pholadomya*.

Astarte Beaumontii, Leym.—According to the figures, the idea would not occur to any one to compare this species with the *Astarte obovata*, Sow., which is much less depressed (*bien plus haute*), more equilateral and subtruncated. The description of the English author is moreover not sufficiently explicit to lead to any conclusion.

Cardium subhillanum, Leym.—I have indicated in my text the characters which separate this species from the *Cardium striatulum*, Sow., which toward the middle of its valves has only concentric striæ, whereas in ours the principal striæ are, on the contrary, longitudinal.

Cucullæa Gabrielis, Leym.—We have pointed out with so much care the important differences which separate this species from the *Arca exaltata*, Nilson, that we cannot but think that Mr. Edward Forbes has not consulted our text before deciding on assimilating two such different shells.

Pinna sulcifera, Desh.—The characters by which this *Pinna* is distinguished from the *Pinna tetragona*, Sow., are so obvious and so easy to be perceived at the first glance, that we are very much surprised that Mr. Forbes should have misconceived them so far as to confound these two shells. In fact, besides the general form, which is very different, our species has round and well-marked costæ, separated by well-marked furrows, whereas the *Pinna tetragona*, Sow., only exhibits slight striæ.

Pecten interstriatus, Leym.—This *Pecten* is principally characterized by the striæ which cover the intervals of the costæ, striæ which do not exist in the *Pecten obliquus*, Sow. This last species has moreover costæ much more distant from each other, and a very different general form.

Modiola Archiaci, Leym.—We might perhaps condemn this species, which much resembles in fact the *Modiola æqualis*, Sow. We had considered it as a new *Modiola*, on account of an undulation which the salient angle of each valve presents, a character which is not seen in the species of Sowerby, which is besides more rounded at its inferior extremity.

Ampullaria lævigata, Desh.—This species, which, according to M. Deshayes, is indeed an *Ampullaria*, and not a *Natica*, is assimilated in the list of Mr. Forbes to a *Natica rotundata*, which we have not been able to compare with it, not knowing where to find a figure or a description of it.

In the face of these facts, which every one can verify, we are in the unpleasant alternative of either charging Mr. E. Forbes with having made these determinations too hastily, or of considering the plates of Sowerby so bad that it would henceforth be wholly impossible to use them. In this latter case, which does not seem to us to be the most probable, it would be just, in correcting our errors, to declare that they were indispensable in the state of things, in order not to let a suspicion of levity fall upon us which we had taken every means to avoid.

It will be supposed, from the preceding statements, that we entertained some doubts on the whole of the names which compose the lists on which Dr. Fitton relies. We cannot admit, for example, that our *Exogyra aquilina*, which is a variety of the *Exogyra subsinuata* or *Couloni*, can be considered as belonging to the *Exogyra sinuata*, which differs much from the preceding both in form and position. Nor do we think that the small smooth *Exogyra* which Sowerby has figured under the name of *Gryphæa lævigata*, can represent the *Exogyra Couloni* (our *Exogyra subsinuata*), the surface of which is always more or less covered with varices, knots and other irregularities. It is very difficult to believe also, with Mr. Forbes, that the *Trigonia caudata*, Agass., can be assimilated to the *Trigonia scabra*, Lam.

With these restrictions, the lists in question present but a small number of truly Neocomian species, and thence the conclusions which Dr. Fitton has drawn from them lose their force.

For our part, these conclusions would not appear to us necessary even had there been no error in the lists of Mr. Forbes, and we are even of opinion that, notwithstanding the interesting discoveries which have just been remarked, the *Neocomian formation* represents the *Weald Clay* of England.

Although we have stated in our Memoir on the Chalk Formation of the Aube the principal reasons which have led us to admit this last assimilation, the first idea of which is due, indeed, to Elie de Beaumont, it will not be useless to recall here in a very succinct manner the chief of these reasons, and to oppose them to those which Dr. Fitton on his side adduces.

The chalk formation of Champagne is divided, first, very distinctly, into two principal parts, one of which, our *chalk properly so called*, is formed, in fact, throughout its whole thickness, only of the rock mineralogically called *chalk*. It corresponds exactly to the *chalk* of England, as Mr. Mantell has described it.

Between this stage and the Jurassic system is seen a series of very varied strata and very rich in fossils, in which we can distinguish two more new stages separated palæontologically by a stratum of a remarkable constancy throughout France, and which is characterized principally by the *Exogyra sinuata*.

The lower stage on this horizon represents the Neocomian formation of Switzerland, and offers a multitude of peculiar fossils, only some of which exist also in the lower greensand of Great Britain.

The bed above the *Exogyra sinuata*, comprising also this last layer, corresponds to the greensand of the English, whose subdivisions are not found here, although the thin layer containing the *Exogyra sinuata* has strongly-marked relations with the lower greensand.

The two lower beds which we have just distinguished according to the fossils, and which also differ greatly as to mineralogical characters, have been separated moreover by a geological phænomenon, rather slight indeed, but which has left very marked traces in Champagne. These traces, which we have specially mentioned in a note inserted in the *Bulletin* (vol. i. 2nd series, p. 40), consist principally of an unconformity of stratification: thus near Chaource, for example, we see the clays with *Exogyra sinuata*, and the greensand, quite entire, reposing at the foot of an elevated hill constituted of the Neocomian.

This last formation then constitutes in the Aube, and we might say also in the Jura, in the south of France, and as far as the Crimea, a well-characterized type, which it would be proper in all cases, were it even the contemporary of the lower greensand, to consider apart as the principal type of the lower cretaceous formation. Let us now examine whether this synchronism can be admitted.

If we transport ourselves into the south-east part of England, we see, in a circumscribed space, a large deposit of fresh-water, principally sandy and clayey, which is comprised between two absolutely marine formations, one of which (Portland stone), almost exclusively calcareous, finds its representative in our countries in the compact and oolitic limestones which we designate, with MM. Thirria and Thurmann, by the epithet of *Portlandian*. The other formation, that which occupies the upper part, is the lower greensand, which admits as its peculiar and evidently characteristic fossil the *Exogyra sinuata*, so constant on the continent above the Neocomian formation.

Thus here are two formations, one marine (Neocomian formation), and the freshwater (Wealden), which appear to be comprised between the same limits, and to which it is in the first place very natural to assign the same level. The following considerations support this view.

When the Wealden, the extended prevalence of which bespeaks a lengthened time of deposition, was formed in an estuary into which fresh water flowed abundantly, the ordinary sedimentation was probably not suspended in the seas at that epoch; there must therefore have been sediments deposited on the margin of these seas, and these sediments appear to us to be the Neocomian formation.

Some would now regard the freshwater strata of the Wealds as the representative of the last deposits of the Jurassic sea; truly, we do not see any reason to think so. In fact, the upper strata of the limestone of the Jura, which are intimately connected, in the east of France, with the Kimmeridge group, which forms the base of the deposit, cannot, it seems to us, rise above the level of the Portland stone of England. We should be rather led, by the consideration of the fossils, to place them lower, on the contrary, towards the upper part of the Kimmeridge clay. With respect to the analogy remarked between the forms of the fishes and reptiles of the Weald formation and of the Jura, we may be permitted to say that it is too vague and too uncertain to attract our serious attention.

Moreover, purely geological considerations are also opposed to the adoption of the new theory. In fact, the commencement of the freshwater deposit, which is called the *Wealden*, must have been occasioned by a removal of the waters of the seas, and by a corresponding change in the mineralogical and zoological conditions which it is reasonable to attribute to the general cause which produced the annihilation of the Jurassic fauna, by preparing a new order of things which is designated by the epithet of *cretaceous*. The end of the freshwater period here in question would then find its cause very naturally in this new transport of the waters, which denuded the Neocomian formation before the deposit of the *Exogyra sinuata*, and which gave rise to the unconformity of stratification which we have remarked between the Neocomian formation and the greensand of Champagne.

It would then be at the end of the Neocomian period that the waters of the seas would have invaded the country of the Wealds, carrying with it the germs of the Neocomian species, which are now found among those of the greensand, and the greatest part of which we have already mentioned in our memoir. We shall remark, in conclusion, that these species are found associated, in the lower strata recently discovered in England, with fossils which are only found in France in the bed for which we have reserved the name of greensand. Besides the *Exogyra sinuata*, we could cite the *Ammonites Deshayesi*, *Pecten interstriatus*, *Plicatula pectinoides*, *Terebratula sella*, *T. elegans*, and others which we have never met with in the Neocomian strata.

After the reading of this paper, M. A. d'Orbigny announced that Dr. Fitton had brought him some fossils from the Isle of Wight, and that on seeing them he had not the least doubt left that that formation was the Neocomian formation: there are from fifteen to twenty well-characterized Neocomian species, which admit of no uncertainty. There is not the least separation, in England, between the end of the gault or Aptian formation and the Neocomian formation.

M. de Roys said that he had shown to the geologists of Vaucluse Dr. Fitton's list of fossils; they recognised all these fossils as belonging to the upper portion.

M. A. d'Orbigny cited the *Perna Mulleti* among these fossils as a species of the lower part of the Neocomian formation. It is certain, however, that the greatest number of the species belong to the upper part. In other respects he concurred in the views of M. Leymerie respecting the determinations of the fossils of Dr. Fitton.

M. Leymerie stated that there was one difference between his ideas and those of M. A. d'Orbigny. The strata containing *Exogyra sinuata* are in perfect accordance with the gault and the greensand. If some Neocomian fossils are found in the greensand of England, there is nothing surprising in that. Formations have taken place in estuaries, whilst, on the land, the Neocomian system was forming. The *Perna Mulleti* alone strikes him as being found in the greensand, above the Weald clay. It is a species which survived in England.

M. A. d'Orbigny replied that there is in England the same series of species as in France, of the best characterized Neocomian formation, and at the same level; that there is no difference in the superposition of the strata in the two countries. If Dr. Fitton has changed the names, it matters not; but M. A. d'Orbigny had seen the fossils, and Dr. Fitton himself recognised at Wassy the same species.

XLIII. *On the Laws of Reflexion and Refraction at the Surfaces of Substances of High Refracting and Absorbing Powers, such as Metals. By the* Rev. M. O'Brien, *late Fellow of Caius College, Cambridge, and Professor of Natural Philosophy and Astronomy in King's College, London.*

[The subject resumed from p. 123.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

1. In a former communication I mentioned that I had investigated the laws of reflexion and refraction at the surface of a transparent substance, the particles of which are supposed to *resist* the vibrations of the æthereal fluid; I shall now briefly state and prove these laws, and show how they may be tested by experiment. In a memoir printed in the Cambridge Transactions, vol. viii. p. 7, I have obtained certain results*, which, if true, show the great importance of taking properly into account the *normal* as well as the *transversal* waves, in investigating the laws of reflexion and refraction. These results, as well as the notation and method employed in that memoir, I shall now make use of without further explanation. The waves are supposed to be plane waves, and the refracting surface is also supposed to be plane.

2. The following are the *laws of reflexion and refraction* for vibrations *perpendicular* to the plane of incidence.

Let the general expression for the *incident* disturbance be

$$a\cos\{nt - k(px + sz)\},$$

the refracting surface being taken as the plane of xy, and the plane of incidence as the plane of xz; p and s being the sine and cosine of the angle of incidence, which angle we shall denote by the letter ϕ.

Then the general expression for the *reflected* disturbance will be (as we shall presently prove),

$$-\mathrm{P}a\cos\{nt - k(px - sz) - \theta\};$$

and the expression for the *refracted* disturbance will be

$$ae^{-k\chi z}\Big\{\cos\{nt-k(px+\sigma z)\} - \mathrm{P}\cos\{nt-k(px+\sigma z)-\theta\}\Big\}.$$

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* I shall just state one of these results, to show the importance of taking into account the normal waves. If a^2 be the intensity of a ray incident on a surface of high refractive power, the intensity of the reflected ray, when the tangent of the angle of incidence is μ, will be

$$\frac{(\mu^2-1)^2(\nu^2-1)^2}{4\mu^2(\nu^2+1)^2+(\mu^2-1)^2(\nu^2-1)^2}\cdot a^2,$$

μ being the index of refraction for transversal waves, and ν that for normal. This remarkable result fully explains the peculiarities of reflexion at highly refractive surfaces (see the Memoir, p. 26).

The quantities σ, χ, P and θ in these expressions are to be obtained by equating the possible and impossible parts of the following equations, viz.

$$(\sigma-\chi\sqrt{-1})^2+p^2=\frac{\mathrm{B}}{\mathrm{B}'}\left(1-\frac{\mathrm{C}'}{n}\sqrt{-1}\right),$$

$$s^2\left(\frac{1+\mathrm{P}e^{-\theta\sqrt{-1}}}{1-\mathrm{P}e^{-\theta\sqrt{-1}}}\right)^2+p^2=\frac{\mathrm{B}}{\mathrm{B}'}\left(1-\frac{\mathrm{C}'}{n}\sqrt{-1}\right),$$

where B, B′ and C′ are certain constants which occur in the equations of motion of the æther. B is the same for all colours, but B′ and C′ are different for different colours, being given by the equations

$$\mathrm{B}'=\frac{\mathrm{B}}{1+\mathrm{C}_2-\mathrm{C}_4n^2+\mathrm{C}_6n^4\ldots\ldots},$$

$$\mathrm{C}'=\frac{\mathrm{C}_1-\mathrm{C}_3n^2+\mathrm{C}_5n^4\ldots\ldots}{1+\mathrm{C}_2-\mathrm{C}_4n^2+\mathrm{C}_6n^4\ldots\ldots},$$

where B, C_1, C_2, C_3, &c. are certain constants depending simply upon the constitution of the æther and of the transparent body.

3. The following are the *laws of reflexion and refraction* for vibrations *parallel* to the plane of incidence.

Let the general expression for the *incident* disturbance be

$$b\cos\{nt-k(px+sz)\},$$

then the expression for the *reflected* disturbance will be

$$\mathrm{P\,Q}\,b\cos\{nt-k(px+sz)-\theta-\iota\};$$

and the expression for the *refracted* disturbance will be

$$\mathrm{K}\,b\,e^{-k\chi z}\Big\{\cos\{nt-k(px+\sigma z)+\varkappa\}$$
$$+\,\mathrm{P\,Q}\cos\{nt-k(px+\sigma z)+\varkappa-\theta-\iota\}\Big\},$$

where P, θ, σ and χ are the same quantities as before, and Q, ι, K and $\varkappa$ are to be obtained by equating the possible and impossible parts of the following equations, viz.

$$\frac{1}{\mathrm{K}^2e^{2\varkappa\sqrt{-1}}}=\frac{\mathrm{B}}{\mathrm{B}'}\left(1-\frac{\mathrm{C}'}{n}\sqrt{-1}\right),$$

$$\mathrm{U}^2+2p\,\mathrm{U}\sqrt{-1}=\frac{\mathrm{B}}{\mathrm{B}'}\left(1-\frac{\mathrm{C}'}{n}\sqrt{-1}\right),$$

where for brevity we have put U^2 to represent the expression

$$\frac{\mathrm{A}'}{\mathrm{A}}\,\frac{\mathrm{B}}{\mathrm{B}'}\,\frac{\mathrm{Q}e^{-\iota\sqrt{-1}}-e^{2\varphi\sqrt{-1}}}{1-\mathrm{Q}e^{-\iota\sqrt{-1}}\,.\,e^{2\varphi\sqrt{-1}}},$$

A and A′ being certain constants which occur in the equations of motion for *normal* waves, occupying the same places respectively that B and B′ do in the equations of motion for transversal waves.

4. The equations here given for finding P, Q, θ and ι are apparently complicated, but in point of fact they are not so, as will presently appear. The equations for determining Q and ι may be easily applied to determine the values of the unknown constants involved in them by comparison with experimental results, and appear to be well-adapted, not only for testing the truth of the theory here advanced, but also (supposing that theory to turn out true) for deciding several important questions in the undulatory theory of light. We shall now show how the equations for determining Q and ι may be compared with experiment.

5. These equations apply to the case of reflexion at metallic surfaces and surfaces of high refractive power. The formulæ for the incident light,

$$a \cos \{n t - k(p x + s z)\} \text{ and } b \cos \{n t - k(p x + s z)\},$$

represent together a *plane* polarized ray incident at an angle ϕ ($p = \sin \phi$, $s = \cos \phi$), the angle which the plane of polarization makes with the plane of incidence being

$$\tan^{-1} \frac{b}{a}.$$

The corresponding formulæ for the reflected light are

$$- \mathrm{P} a \cos \{n t - k(p x - s z) - \theta\},$$

and $$\mathrm{P} \mathrm{Q} b \cos \{n t - k(p x - s z) - \theta - \iota\},$$

which, on account of the difference of phase (ι), represent *in general*, not a *plane*, but an *elliptically* polarized ray*. If ι be any multiple of 180°, the polarization becomes plane.

If we suppose the ray to be reflected m times successively at two parallel surfaces of the same metal or substance, the formulæ for the light after the last reflexion will be

$$(-\mathrm{P})^m a \cos \{n t - k(p x - s z) - m \theta\},$$

and $$(\mathrm{P} \mathrm{Q})^m b \cos \{n t - k(p x - s z) - m \theta - m \iota\}.$$

Let us suppose that $m \iota = 180°$, then these two expressions represent together a *plane* polarized ray, the angle which the plane of polarization makes with the plane of incidence being

$$\tan^{-1}\left(\mathrm{Q}^m \frac{b}{a}\right).$$

Now Sir D. Brewster has determined the angles at which a plane polarized ray must be incident upon the surfaces of

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* Sir D. Brewster calls the polarization *elliptical* when $\iota = 90°$, and *partially elliptical* when ι is not equal to 90°. It is easy to see that the polarization is always completely elliptical, only the plane of incidence does not contain either axis of the ellipse except when $\iota = 90°$.

various metals, so that the light may be *plane* polarized after two reflexions, or three reflexions, or four reflexions, &c. In other words, he has determined a set of angles of incidence which respectively make $2\iota=180°$, $3\iota=180°$, $4\iota=180°$, &c. He has also determined the inclination of the plane of polarization of the twice-reflected ray to the plane of incidence, but I do not know whether he has done the same in the case of the rays reflected three times, four times, &c.; there would be of course no difficulty in observing the respective inclinations of the planes of polarization of the rays which have been plane polarized by three or more reflexions. Supposing this done, it is clear that we obtain a series of values of ϕ which respectively make $\iota=90°$, $\iota=60$, $\iota=45°$; and at the same time we determine the corresponding values of Q, for (if we suppose for simplicity that $\tan^{-1}\dfrac{b}{a}=45°$, or $b=a$) the corresponding angles of inclination of the planes of polarization will be $\tan^{-1}(Q^2)$, $\tan^{-1}(Q^3)$, $\tan^{-1}(Q^4)$, &c. Sir D. Brewster has found that there is only one value of ϕ that makes $\iota=90°$, but that there are two values which make $\iota=60°$, two which make $\iota=45°$, &c. Hence we can determine by experiment several sets of corresponding values of ϕ, ι and Q.

6. Suppose then that we know the values of Q, ι and ϕ; substituting these values in the expression for U^2, we may put U^2 in the form

$$U^2=c^2(\alpha+\beta\sqrt{-1})^2,$$

where α and β are known numbers, and $c^2=\dfrac{A'}{A}\dfrac{B}{B'}$, whence the last equation in art. 3 becomes

$$c^2(\alpha+\beta\sqrt{-1})^2+2pc(\alpha\sqrt{-1}-\beta)=\frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right), \quad (1.)$$

p being also a known number.

Let $\alpha_{,}$, $\beta_{,}$, $p_{,}$ be another set of known values of α, β, p; then we have, similarly,

$$c^2(\alpha_{,}+\beta_{,}\sqrt{-1})^2+2p_{,}c(\alpha_{,}\sqrt{-1}-\beta_{,})^2=\frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right). \quad (2.)$$

Equating the possible and impossible parts of the first members of these two equations, we find

$$c^2(\alpha^2-\beta^2)-2p\beta c=c^2(\alpha_{,}^2-\beta_{,}^2)-2p_{,}\beta_{,}c,$$
$$c^2\alpha\beta+p\alpha c=c^2\alpha_{,}\beta_{,}+p_{,}\alpha_{,}c,$$

which give

$$c=2\frac{p\beta-p_{,}\beta_{,}}{\alpha^2-\beta^2-\alpha_{,}^2+\beta_{,}^2}=\frac{p_{,}\alpha_{,}-p\alpha}{\alpha\beta-\alpha_{,}\beta_{,}}.$$

Thus c becomes known, and consequently, by equating the possible and impossible parts of either (1.) or (2.), we determine $\frac{B}{B'}$ and $\frac{C'}{n}$ also. Thus by two sets of observations we may determine all the unknown constants which enter into our equations. We have also an equation of condition, namely,

$$\frac{p\,\beta - p_{\prime}\,\beta_{\prime}}{\alpha^2 - \beta^2 - \alpha_{\prime}^{\,2} + \beta_{\prime}^{\,2}} = \frac{p_{\prime}\,\alpha_{\prime} - p\,\alpha}{\alpha\,\beta - \alpha_{\prime}\,\beta_{\prime}},$$

which must turn out true if our theory be correct.

The simplest way, perhaps, to test the truth of our theory would be this: to find from observation several corresponding values of the two quantities

$$\frac{p\,\beta - p_{\prime}\,\beta_{\prime}}{\alpha^2 - \beta^2 - \alpha_{\prime}^{\,2} + \beta^2} \text{ and } \frac{p_{\prime}\,\alpha_{\prime} - p\,\alpha}{\alpha\,\beta - \alpha_{\prime}\beta_{\prime}},$$

and to try whether they vary with the angle of incidence or not; and whether they are always equal to each other or not. If they turn out to be invariable and always equal to each other, it is clear we have a very decisive proof of the correctness of the above results, and *vice versâ*; especially, because there are only three distinct unknown constants involved in the above equations.

7. The method here explained of determining Q and ι by repeated metallic reflexions is not so well-adapted to test the truth of the above formulæ as another method which gives the following result,

$$Q\,e^{-\iota\sqrt{-1}} = \frac{\tan\beta + \sqrt{-1}\tan\gamma}{\sqrt{-1}\tan\beta\tan\gamma - 1},$$

where β and γ are two angles which may be immediately observed for any angle of incidence we please. The consideration of this method I shall reserve for my next communication, and now briefly prove the laws of reflexion and refraction here stated.

8. If ξ, η, ζ be the displacements of any element of the æthereal fluid from its position of rest, it appears, as in art. 19, that the material particles will bring into play upon that element a resisting force, the components of which (parallel to the three axes) are

$$\frac{P\,d\xi}{dt} + Q\frac{d^2\xi}{dt^2},\quad \frac{P\,d\eta}{dt} + Q\frac{d^2\eta}{dt^2},\quad \frac{P\,d\zeta}{dt} + Q\frac{d^2\zeta}{dt^2};$$

where $P = C_1 - C_3 n^2 + C_5 n^4 \ldots$, $Q = C_2 - C_4 n^2 + C_6 n^4 \ldots$, C_1, C_2, C_3, &c. being constants depending upon the constitution of the æther and of the transparent substance, and $\frac{2\pi}{n}$ the periodic time of the æthereal vibrations.

Hence the general equations of motion, for transversal vibrations, will be (see Cambridge Transactions, vol. vii. p. 409)*,

$$\left.\begin{array}{l}\frac{d^2\xi}{dt^2} = \mathrm{B}\left(\frac{d^2\xi}{dx^2} + \frac{d^2\xi}{dy^2} + \frac{d^2\xi}{dz^2}\right) - \mathrm{P}\frac{d\xi}{dt} - \mathrm{Q}\frac{d^2\xi}{dt^2}, \\ \text{and two similar equations for } \eta \text{ and } \xi.\end{array}\right\} \quad . \quad (3.)$$

For normal vibrations we have three equations of exactly the same form as these, differing only in having the constant A in place of B.

{N.B. By transversal vibrations, we mean those which satisfy the condition

$$\frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz} = 0,$$

and by normal vibrations those which satisfy the three conditions

$$\frac{d\xi}{dy} = \frac{d\eta}{dx}, \quad \frac{d\eta}{dz} = \frac{d\zeta}{dy}, \quad \frac{d\zeta}{dx} = \frac{d\xi}{dz}.$$

These definitions of transversal and normal vibrations apply equally, whether ξ, η, ζ be imaginary or not.}

If, in the equations (3.), we put

$$\frac{\mathrm{P}}{1+\mathrm{Q}} = \mathrm{C}', \quad \frac{\mathrm{B}}{1+\mathrm{Q}} = \mathrm{B}',$$

we have

$$\left.\begin{array}{l}\frac{d^2\xi}{dt^2} + \mathrm{C}'\frac{d\xi}{dt} = \mathrm{B}'\left(\frac{d^2\xi}{dx^2} + \frac{d^2\xi}{dy^2} + \frac{d^2\xi}{dz^2}\right), \\ \text{and two similar equations for } \eta \text{ and } \zeta.\end{array}\right\} \quad . \quad . \quad (4.)$$

These three equations are satisfied by the following imaginary values of ξ, η, ζ, viz.

$$\xi = a' u, \quad \eta = b' u, \quad \zeta = c' u,$$

where
$$u = e^{\{nt - k'(p'x + q's + s'z)\}\sqrt{-1}},$$
$$p'^2 + q'^2 + s'^2 = 1;$$

and consequently by substitution,

$$n^2 - \mathrm{C}' n\sqrt{-1} = \mathrm{B}' k'^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

Also, since the vibrations are transversal, we have, by the definition above-given,

$$p' a' + q' b' + s' c' = 0.$$

From equation (5.) it is manifest that, supposing n real, k' is imaginary. By superposing two proper sets of imaginary solutions thus obtained, we shall obtain real integrals of the equations of motion.

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* To conform to the most usual notation, we have in the present papers represented the displacements by the letters ξ, η, ζ, instead of α, β, γ.

9. We shall now investigate the laws of reflexion and refraction at the surface (supposed to be plane) of the transparent substance to which the above equations apply, taking for granted the results arrived at in the memoir (Cambridge Transactions, vol. viii. p. 7) above-quoted, and assuming the notation there employed (observing that $n=k\ \ v=k'v'$).

10. Referring to section viii. (p. 24), we find that the equations of *motion* and *connection** are satisfied by the following imaginary values of V, $V_{,}$ and V' (the vibrations being *parallel* to the plane of incidence), viz.

$$V = \frac{b}{2} e^{\{nt - k(px+sz)\}\sqrt{-1}},$$

$$V_{,} = \frac{\mu s - s' - \eta}{\mu s + s' + \eta}\,\frac{b}{2}\, e^{\{nt - k(px-sz)\}\sqrt{-1}},$$

$$V' = \frac{1}{\mu}\left\{\frac{\mu s - s' - \eta}{\mu s + s' + \eta} + 1\right\}\frac{b}{2}\, e^{\{nt - k'(p'x+s'z)\}\sqrt{-1}},$$

where $\mu = \dfrac{v}{v'} = \dfrac{k'}{k}$ (since $n = kv = k'v'$),

and $p = \mu p'$, or $k'p' = kp$;

v is the velocity of propagation of transversal waves in vacuum, and k' is given by equation (5.) of the present paper; p' and q' are therefore imaginary.

Let us assume

$$k's' = k(\sigma - \chi\sqrt{-1}),$$

then, since $kp = k'p'$ and $p'^2 + s'^2 = 1$, we find

$$k^2p^2 + k^2(\sigma - \chi\sqrt{-1})^2 = k'^2$$

$$= \frac{n^2 - C'n\sqrt{-1}}{B'} \text{ by (5.)}$$

and $\therefore p^2 + (\sigma - \chi\sqrt{-1})^2 = \dfrac{B}{B'}\left(1 - \dfrac{C'}{n}\sqrt{-1}\right)$,

observing that

$$k^2 = \frac{v^2}{n^2} = \frac{B}{n^2}.$$

11. If the vibrations be *perpendicular* to the plane of incidence, the following are the corresponding values of V, $V_{,}$ and V', viz.

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* The equations of *connection* obtained in section ii. are evidently the same, whether the particles of matter exercise forces of resistance or not; for the forces of resistance are of the *same* order of magnitude as the forces which appear in the equations of *motion*, and therefore they do not appear in the equations of *connection*.

$$V = \frac{a}{2}\, e^{\{nt-k(px+sz)\}\sqrt{-1}},$$

$$V_{,} = \frac{s-\mu s'}{s+\mu s'}\,\frac{a}{2}\, e^{\{nt-k(px-sz)\}\sqrt{-1}},$$

$$V' = \left\{\frac{s-\mu s'}{s+\mu s'}+1\right\}\frac{a}{2}\, e^{\{nt-k'(p'x+s'z)\}\sqrt{-1}}.$$

12. We shall now proceed to deduce real solutions from these imaginary integrals, and first in the case of vibrations *perpendicular* to the plane of incidence.

Assuming

$$\frac{s-\mu s'}{s+\mu s'} = -\,P e^{-\theta\sqrt{-1}},$$

we find

$$\left(\frac{1+Pe^{-\theta\sqrt{-1}}}{1-Pe^{-\theta\sqrt{-1}}}\right)^2 = \frac{\mu^2 s'^2}{s^2}$$

$$= \frac{(k' s')^2}{k^2 s^2}$$

$$= \frac{k'^2-k^2p^2}{k^2 s^2}.$$

Hence

$$s^2\left(\frac{1+Pe^{-\theta\sqrt{-1}}}{1-Pe^{-\theta\sqrt{-1}}}\right)^2 + p^2 = \frac{k'^2}{k^2}$$

$$= \frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right).$$

We have, therefore, for vibrations *perpendicular* to the plane of incidence,

$$V = \frac{a}{2}\, e^{\{nt-k(px+sz)\}\sqrt{-1}},$$

$$V_{,} = -\,Pe^{-\theta\sqrt{-1}}\,\frac{a}{2}\, e^{\{nt-k(px-sz)\}\sqrt{-1}},$$

$$V' = (1-Pe^{-\theta\sqrt{-1}})\,\frac{a}{2}\, e^{\{nt-k(px+\sigma z)\}\sqrt{-1}-k\chi z},$$

where P, θ, σ and χ are given by the two equations

$$(\sigma-\chi\sqrt{-1})^2+p^2 = \frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right),$$

and $$s^2\left(\frac{1+Pe^{-\theta\sqrt{-1}}}{1-Pe^{-\theta\sqrt{-1}}}\right)^2+p^2 = \frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right).$$

We may now immediately find another set of values of V, $V_{,}$ and V', by simply changing the sign of the $\sqrt{-1}$ in these formulæ, which gives us

$$V = \frac{a}{2} e^{-\{nt-k(px+sz)\}\sqrt{-1}},$$

$$V_{,} = -P e^{\theta\sqrt{-1}} \frac{a}{2} e^{-\{nt-k(px-sz)\}\sqrt{-1}},$$

$$V' = (1-P e^{\theta\sqrt{-1}}) \frac{a}{2} e^{-\{nt-k(px+\sigma z)\}\sqrt{-1}-k\chi z}.$$

And superposing these two sets of values we find the following real values of V, $V_{,}$ and V', which satisfy the equations of *motion* and *connection*, viz.

$$V = a\cos\{nt-k(px+sz)\},$$

$$V_{,} = -Pa\cos\{nt-k(px-sz)-\theta\},$$

$$V' = ae^{-k\chi z}\Big\{\cos\{nt-k(px+\sigma z)\} - P\cos\{nt-k(px+\sigma z)-\theta\}\Big\}.$$

Thus we have proved the laws of reflexion and refraction above-stated for vibrations *perpendicular* to the plane of incidence.

13. We may proceed in exactly the same way for vibrations *parallel* to the plane of incidence.

Assuming

$$\frac{\mu s - s' - \eta}{\mu s + s' + \eta} = PQe^{-(\theta+\iota)\sqrt{-1}},$$

we find

$$Qe^{-\iota\sqrt{-1}} = -\frac{s+\mu s'}{s-\mu s'}\,\frac{\mu s - s' - \eta}{\mu s + s' + \eta},$$

which gives, by a simple reduction,

$$\frac{1-Qe^{-1\sqrt{-1}}}{1+Qe^{-\iota\sqrt{-1}}} = \frac{\frac{\mu^2-1}{\mu}p^2 + \mu s'\eta}{(\mu^2-1)ss' - s\eta}.$$

Now, by section viii. (of the Memoir above-quoted), we have

$$\eta = \frac{\nu^2-1}{\nu^2+1}\,\frac{\mu^2-1}{\mu}\,p\sqrt{-1},$$

where ν^2 is what μ^2 becomes when B and B' are changed into A and A', and therefore

$$\nu^2 = \frac{A}{A'}\left(1 - \frac{C'}{n}\sqrt{-1}\right) = \frac{A}{A'}\,\frac{B'}{B}\,\mu^2.$$

Hence

$$\frac{1-Qe^{-1\sqrt{-1}}}{1+Qe^{-1\sqrt{-1}}} = \frac{\frac{p^2}{\mu} + \frac{\nu^2-1}{\nu^2+1}ps'\sqrt{-1}}{ss' - \frac{\nu^2-1}{\nu^2+1}\,\frac{p}{\mu}\,s\sqrt{-1}},$$

$$=\frac{p}{s}\cdot\frac{p'+\dfrac{\nu^2-1}{\nu^2+1}s'\sqrt{-1}}{s'-\dfrac{\nu^2-1}{\nu^2+1}p'\sqrt{-1}}.$$

From this we may easily show that

$$\frac{e^{2\phi\sqrt{-1}}-Qe^{-\iota\sqrt{-1}}}{Qe^{-\iota\sqrt{-1}}e^{2\phi\sqrt{-1}}-1}=\nu^2(s'-p'\sqrt{-1})^2$$

$$=\frac{A}{A'}\frac{B'}{B}\mu^2(s'-p'\sqrt{-1})^2$$

$$=\frac{A}{A'}\frac{B'}{B}(\mu s'-p\sqrt{-1})^2.$$

Hence, assuming

$$\frac{e^{2\phi\sqrt{-1}}-Qe^{-\iota\sqrt{-1}}}{Qe^{-\iota\sqrt{-1}}e^{2\phi\sqrt{-1}}-1}=U^2,\text{ and }c^2=\frac{A'}{A}\frac{B}{B'},$$

we find

$$cU=\mu s'-p\sqrt{-1},$$

and ∴ $$(cU+p\sqrt{-1})^2=\mu^2 s'^2=\mu^2-p^2;$$

and hence, finally,

$$c^2U^2+2cU\sqrt{-1}=\frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right).$$

Furthermore, if we assume

$$\frac{1}{\mu}=Ke^{\varkappa\sqrt{-1}},$$

we have

$$\frac{1}{K^2e^{2\varkappa\sqrt{-1}}}=\frac{B}{B'}\left(1-\frac{C'}{n}\sqrt{-1}\right).$$

We have, therefore, for vibrations *parallel* to the plane of incidence,

$$V=\frac{b}{2}e^{\{nt-k(px+sz)\}\sqrt{-1}},$$

$$V_{\prime}=PQe^{-(\iota+\theta)\sqrt{-1}}\frac{b}{2}e^{\{nt-k(px-sz)\}\sqrt{-1}},$$

$$V'=Ke^{\varkappa\sqrt{-1}}\{1+PQe^{-(\iota+\theta)\sqrt{-1}}\}\frac{b}{2}e^{\{nt-k(px+\sigma z)\}\sqrt{-1}-k\chi z}.$$

Hence, proceeding as before, we find the following real values:

$$V=b\cos\{nt-k(px+sz)\},$$

$$V_{\iota} = P\,Q\,\frac{b}{2} \cos \{n\,t - k\,(p\,x + s\,z) - \theta - \iota\},$$

$$V' = K\,b\,e^{-k\chi z} \Big\{ \cos \{n\,t - k\,(p\,x + \sigma\,z) + \varkappa\}$$
$$+ P\,Q \cos \{n\,t - k\,(p\,x + \sigma\,z) + \varkappa - \theta - \iota\} \Big\}.$$

Thus we have proved the laws above-stated for vibrations *parallel* to the plane of incidence.

[To be continued.]

XLIV. *Analysis of the Bonnington Water, near Leith, Scotland.* *By* Edward G. Schweitzer, *Esq.*[*]

THE soil from which this mineral spring rises, according to the kind information obtained through G. Haig, Esq. of Bonnington House, is, for 20 feet below the surface, a wet, slipping sand of a yellowish colour, which is succeeded, for 20 feet, by a black hard clay, and this is followed, by a hard sandstone rock of considerable thickness, limestone of different qualities, and two small seams of coal; these have been bored, with very little interruption, through the extent of 184 feet. The mineral water when taken from the spring is quite transparent, with an empyreumatic smell, which it likewise imparts to the palate; it has also the taste of alkaline chlorides and iron. When a bottle containing the water for examination was opened, no disengagement of carbonic acid gas was perceptible, and when poured into a glass vessel but few bubbles of gas appeared at the sides; when mixed, however, with an acid, a slight discharge of carbonic acid gas took place. When exposed to the atmospheric air it becomes turbid, and a brownish sediment is formed; a few dark brown flakes were also floating in it, which were separately examined. The natural temperature of this water is about 44° Fahr., its specific gravity at 60° F. is 1·002618, and the quantity emitted from the spring amounts to about from 4 to 5 gallons per minute.

It indicated no action upon blue litmus paper; when boiled it became turbid, oxide of iron with some organic matter precipitated, and a slight alkaline reaction upon reddened litmus paper was developed. The usual well-known application of tests for a qualitative analysis evidenced the presence of potassa, soda, lime, magnesia, oxide of iron, sulphuric acid, chlorine, and carbonic acid gas.

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[*] Communicated by the Chemical Society; having been read November 18, 1844.

A portion of the water was concentrated by evaporation and divided into two parts; one of which, on being treated with fresh starch-paste and nitric acid, gave indication of iodine; the other part, when freed from iodine by nitric acid and æther, was mixed with a solution of chlorine and æther, which proved the existence of bromine. Nitrate of silver added to the mineral water gave a precipitate of chloride of silver, which became gray and afterwards purple, indicating organic matter, which was obviously demonstrated by the residue, after evaporating some of the water to dryness, becoming charred during ignition. Acetate of lead gave a yellowish-white precipitate, proving the absence of hydrosulphuric acid. Caustic soda disengaged ammonia in abundance, which was evinced even by carbonate of soda.

The ingredients, which are present in very small quantities, were determined during the quantitative analysis, where a large volume of the water is evaporated, and consequently their detection rendered more easy.

It may not be irrelevant to mention an excellent test for nitric acid, when accompanied either by iodine or bromine, or both, its value being enhanced by the following results made respecting its action. It is known that the presence of nitric acid is easily indicated, when, to the concentrated fluid to be examined, pure sulphuric acid is added, and to this mixture a concentrated solution of the proto-sulphate of iron. The smallest trace of nitric acid, even the $\frac{1}{4000}$th or $\frac{1}{5000}$th part, is evidenced by the appearance of a pink colour, more or less intense. A large amount of nitric acid is indicated by a black colour, a combination of the oxide of nitrogen with the protoxide of iron; however, here we have only to consider minute proportions. The salts of iodine and bromine, when treated with the same test, tinge the fluid with their own peculiar colour, but a greater addition of the solution of the proto-sulphate of iron destroys the colouring, which is not the case with nitric acid. If the quantity of nitric acid be very small, and that of iodine predominates, it is necessary for comparison to make a standard fluid, from which not only the presence of nitric acid may be inferred with certainty, but even its approximate quantity. For that purpose I employ 50 grs. of distilled water in which half a grain of iodide of potassium is dissolved, to this I add gradually 50 grs. of sulphuric acid, and afterwards 50 grs. of a concentrated solution of proto-sulphate of iron. The sulphuric acid precipitates the iodine, which is redissolved by the addition of the iron solution—forming an iodide of iron—whereby the fluid assumes a yellowish tinge, resembling the colour of a solution of chromate of potassa;

the smallest proportion of nitric acid, however, should it even amount to $\frac{1}{3000}$th part, will in this diluted state be manifested by a brownish tinge, which becomes much darker in proportion as the quantity of nitric acid increases. If a bromide be present instead of an iodide, the nitric acid can be ascertained with equal facility, from its imparting a greenish tinge to the fluid. 50 grs. of distilled water containing half a grain of an alkaline bromide to which 50 grs. of sulphuric acid have been added, will indicate $\frac{1}{2000}$th part of nitric acid, by developing a brownish tinge, whereas a similar mixture without the nitric acid will remain colourless. This is obvious, as in the first instance, by the addition of the sulphuric acid, hydrobromic acid is formed, which is colourless, but becomes again decomposed by the presence of free nitric acid, whereby the fluid contracts a brownish tinge. The presence of both bromides and iodides does not interfere with the delicacy of this test for nitric acid, the reaction is the same as if an iodide only were present. However sure and delicate this test is, the presence of organic matter makes it complicated, and where the organic matter cannot be perfectly separated, the presence of nitric acid is easily overlooked, which indeed happened in this analysis at the first examination. In such cases I find the test with indigo* preferable; yet, where the quantity of nitric acid is very small, it is best to submit the concentrated fluid to distillation with sulphuric acid, as mentioned hereafter in the quantitative analysis, by which process the nitric acid was estimated.

It is obvious that a mineral water, containing a great number of ingredients, has to undergo a series of processes for the purpose of ascertaining the individual quantity of each, by which elaborate proceedings small proportions of them might be lost. It is therefore of paramount necessity to have, not only at the commencement of the analysis, but also during its progress, a means of control, which will confirm the correctness of the results of the analysis, and it is chiefly with this view that I have edited this paper.

Alkaline mineral waters which have no volatile ingredients, such as ammonia, are to be controlled, regarding the total amount of their ingredients, by merely evaporating the water and igniting the residue; but if the water contains chiefly chlorides, and particularly earthy chlorides and chloride of ammonium, the mere evaporation, at a temperature approaching the boiling-point, is sufficient to drive off hydrochloric acid, and even ammonia, and if the water contains organic matter the control becomes still more difficult. As the Bonnington water presents these difficulties, they shall be pointed out du-

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* Rose's *Handbuch der Analytischen Chemie*, Band i. p. 235.

ring the progress of the examination. It is indeed, under all circumstances, a very safe and necessary proceeding, to ascertain the weight of all the solid ingredients, and also the weight of some of the electro-negative substances, such as sulphuric acid, chlorine, &c. This will give a proper control for those results which are afterwards obtained from the evaporated water. For this purpose,

1. Four troy ounces (=1920 grs.) of the water were supersaturated with hydrochloric acid, and, when boiling*, precipitated by chloride of barium.

a. Experiment gave 3·622 grs. of sulphate of barytes.
b. ... ... 3·602 grs. ... ...

The average = 3·612 grs., making for 16 troy ozs. of water 14·448 grs.

2. Four troy ounces of the water acidulated by nitric acid gave a precipitate with nitrate of silver, weighing—

a. Experiment . . . . . . 10·030 grs.
b. ... . 10·077 grs.
The average = 10·053 grs.

This precipitate however had been influenced by the organic matter, but this will be noticed again hereafter.

3. To ascertain the total amount of the solid ingredients, 1000 grs. were evaporated in a platina crucible, the residue dried at various temperatures, and the weight taken, when no further diminution was perceptible. The heated crucible was cooled over sulphuric acid, and then quickly weighed; a proceeding followed throughout the course of the analysis. The results were as follows:—

a. Residue† dried at 212° Fahr. gave 3·712 grs.
b. ... dried at 300° to 310° Fahr. gave 3·311 grs.
c. ... heated over a spirit-lamp till the evaporation of the ammonia had ceased, and as long as the escape of hydrochloric acid was perceptible, gave 2·339 grs.; from the charring of the organic matter the mass appeared black.
d. Residue heated to incandescence until the organic matter had been burned away, leaving the residue almost white, gave 2·123 grs.

From the above statement it is obvious that a very exact result cannot be obtained regarding the amount of the solid ingredients. The earthy chlorides require for their existence

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* It facilitates very much the separation and lixiviation of the sulphate of barytes, when it is precipitated from a boiling menstruum, and the particles are thereby prevented from penetrating the pores of the filter.

† This residue when placed in a humid atmosphere attracted water and became half-fluid.

a small proportion of water. Experiment *a* gives therefore a wrong result, as they still contain this water, but in depriving them of it at a higher temperature some hydrochloric acid would be set free; moreover, as the existence of ammonia has been proved, it is impossible to dry the residue without a small loss of chloride of ammonium, so that experiment *b* must give a result too low. Yet the truth of the result must lie within the limits of the experiments *a* and *b*. The last two experiments, *c* and *d*, from the destruction of organic matter, and the evolution of ammonia and hydrochloric acid, can be no guide whatever. An excellent control, which I frequently apply in various analyses, is to change the chlorides, carbonates, &c. into sulphates; the total amount of the latter must closely approach the amount of the various ingredients obtained by analysis when computed as sulphates. This is a very satisfactory proceeding, and must give a correct result if the water does not contain ammonia, oxide of iron or alumina; the ammonia, however, being dissipated by heat must be deducted from the total amount of ingredients obtained by analysis, and as the oxide of iron and alumina (both losing a large quantity of sulphuric acid during ignition) are generally present in very small quantities, they will still allow of the above proceedings; but should they amount to any considerable quantity, as in some chalybeate and aluminous waters, this mode of control is not applicable. The residue which was derived from experiment 3 *d*, amounting to 2·123 grs., was heated in a platina crucible with a sufficient quantity of sulphuric acid and ignited at a red heat, until all the ingredients were changed into neutral sulphates; it yielded 2·663 grs.

After these preliminary examinations the *quantitative analysis* was perfomed.

76·800 grs. were evaporated in a porcelain basin* with a proportionate quantity of chemically pure carbonate of soda, sufficient to decompose the earthy chlorides and sulphates. The application of carbonate of soda, besides preventing the escape of hydrochloric acid, affords another advantage, namely, it counteracts the formation of sulphate of lime, which is a very troublesome ingredient, inasmuch as it adheres closely to the vessel, and small quantities are not easily visible in a white porcelain basin; moreover, it is not soluble in hydrochloric

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* The dark brown flakes which were floating in the water were collected and analysed by themselves, with the rinsing obtained from the glass bottles by hydrochloric acid, any oxide of iron that might adhere to the sides of the bottles being thus dissolved. The water contained only a small proportion of iron in solution.

acid, and requires 461½ parts of water to re-dissolve it, whereas by the addition of carbonate of soda, the earthy carbonates, hereby produced, do not present any difficulty. For the above quantity of water it was found requisite to add 201·123 grs. of anhydrous carbonate of soda*; the whole was evaporated in a water-bath until a few ounces of fluid remained. It was then placed upon a filter, and the earthy residue A lixiviated with boiling water till the filtered liquor gave no indication of sulphates and chlorides. The fluid obtained from the edulcoration of the earthy residue A was evaporated, when some earthy particles were again separated, which were collected upon a filter B. It is here to be noticed, that by this method it is impossible to separate, with any degree of accuracy, the earthy ingredients from the soluble ones, as, by edulcoration, a small proportion of the former will always be re-dissolved; and however often this process may be repeated, whereby the quantity of the re-dissolved earthy ingredients will certainly be diminished, still the last traces of them cannot be obtained; moreover, the repeated evaporation of the lixiviating fluid must carry with it the unavoidable chances of loss: I therefore ascertain these traces of earthy ingredients by themselves along with the soluble salts. A small portion of the earthy carbonates adhered to the sides of the evaporating vessel; these were removed by hydrochloric acid, and the acid solution C, hereby obtained, was left with the other earthy ingredients, A and B, for further examination.

I. *Quantitative Analysis of the Soluble Ingredients.*

The fluid containing the soluble salts, and the edulcorating fluids † obtained from the earthy ingredients, were evaporated to the weight of 8100 grs.

1. *Earthy ingredients.*—One-tenth of the fluid (810 grs.) was evaporated to dryness and the residue submitted to various temperatures, when the following results were obtained:—

At 212° Fahr.	31·352 grs.
At 260° to 280° Fahr.	31·051 grs.
Ignited and fused over a spirit-lamp till the organic matter was destroyed	30·580 grs.

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* By adding the carbonate of soda in solution to the mineral water, it is requisite to ascertain very exactly its quantity in the anhydrous state; the best method I found is to change the carbonate into a sulphate, and to ignite the latter in an atmosphere of carbonate of ammonia, so as to discharge any possible surplus of sulphuric acid. If the carbonate of soda be ascertained as a carbonate, it must *always* be fused and quickly weighed.

† It is always advisable to evaporate first the edulcorating fluids, and when properly concentrated, to mix them with the fluid which chiefly contains the dissolved ingredients.

This residue was dissolved in water and effervesced by the addition of hydrochloric acid; it was evaporated, the residue moistened with hydrochloric acid, and after a few hours again dissolved in water, when a scarcely visible sediment separated. The acid fluid was supersaturated with ammonia; oxalate of ammonia gave no indication of lime, but phosphate of ammonia gave a small precipitate of the ammonio-phosphate of magnesia; this was edulcorated upon a filter with dilute ammonia, which prevents the re-solution of this salt, dried and ignited it gave 0·0309 gr.* of phosphate of magnesia, equivalent to 0·011 magnesia. Another mode of Berzelius, which gives likewise very exact results, I will briefly notice. The concentrated solution of the alkaline chlorides is to be evaporated to dryness with oxide of mercury. The chloride of magnesium is changed into magnesia, and chloride of mercury is produced, which latter unites with the alkaline chlorides. The further process is obvious†.

2. *Sulphuric acid.*—One-twentieth part of the fluid containing the soluble ingredients was acidulated by hydrochloric acid, and when boiling precipitated by chloride of barium. The sulphate of barytes weighed 7·201 grs., which amounts upon 16 ozs. troy to 14·403 grs., equivalent to 4·950 grs. of sulphuric acid. If we compare this result with the one obtained by the preliminary experiments at 1, it will be seen that it closely agrees.

3. *Nitric acid.*—810 grs. of the fluid containing the soluble ingredients were very carefully neutralized by dilute sulphuric acid and macerated with a due proportion of sulphate of silver, in order to change the chlorides, iodides and bromides into sulphates‡. The fluid separated from the silver precipitate was made alkaline by a little carbonate of soda, concentrated in a water-bath to a few ounces, and mixed in a tubulated retort with an adequate quantity of chemically pure sulphuric acid (previously boiled), more than sufficient to convert all the ingredients into bisulphates. This mixture was heated in a sand-bath and the vapours conducted into a glass receiver containing a solution of hydrate of barytes, and the distillation interrupted as soon as fumes

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* I have to notice that I used the French gramme for ascertaining the weight of the ingredients, which I applied with precision to the first decimal number of a millegramme.

† See Berzelius's *Jahresbericht*. 21st Jahrgang, 2nd Theil.

‡ A fluid containing a large amount of chlorides would require a very considerable quantity of the solution of sulphate of silver, inasmuch as this salt requires a great deal of water for its solution. To avoid this it is only necessary to mix the diluted fluid with the sulphate of silver, which has been previously triturated in a mortar with water, and to leave it for some time in a temperature not exceeding 100° F.

of sulphuric acid appeared. The fluid thus obtained was evaporated in a water-bath nearly to dryness, and the residue left for twelve hours exposed to the atmospheric air, in order to change the excess of hydrate of barytes into a carbonate*. After the separation of the carbonate of barytes the fluid was mixed with sulphuric acid, when 2·416 grs. of sulphate of barytes were obtained, equivalent to 1·1220 gr. of nitric acid.

4. *Chlorine.*—One-twentieth part, equal to 405 grs. of the fluid containing the soluble ingredients, was evaporated, and the residue fused to destroy the organic matter, and again dissolved. The solution supersaturated with nitric acid was precipitated by nitrate of silver. The precipitate was carefully collected upon a filter and lixiviated, first with water slightly acidulated with nitric acid, and afterwards with pure water. The dried precipitate was heated nearly to the point of fusing in a well-covered porcelain crucible, when 19·388 grs. of chloride of silver were obtained, making for 16 ozs. troy of water 38·776 grs.; but, as will be seen hereafter, 0·126 gr. of bromide of silver and 0·010 gr. of iodide of silver are to be deducted from the amount (38·776−0·136), leaving 38·640 grs. of chloride of silver, equivalent to 9·533 grs. chlorine. If we compare the amount of the silver precipitate with that obtained from the preliminary experiment 2, we find a slight difference; the latter giving from two experiments in 4 ounces 10·053 grs., whereas the former, calculated upon the same quantity of water, gives 9·694 grs. This discrepancy is accounted for, from the organic matter in the original water being partly thrown down with the silver precipitate. It is therefore requisite to free the fluid from its organic matter before the amount of its chlorides can be ascertained. The interference of organic matter with correct results is evidenced by the following experiments:—

a. Four troy ounces of the fresh water gave with nitrate of silver 10·053 grs.

b. Four troy ounces of the water evaporated with carbonate of soda and freed from the earthy carbonates†, gave with nitrate of silver 9·883 grs.

c. Four troy ounces of the water evaporated with carbonate of soda and freed from the earthy carbonates, the filtered liquid again evaporated, the residue fused and again dissolved, gave with nitrate of silver 9·694 grs.

5. *Iodine.*—4860 grs. of the fluid containing the soluble in-

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* It is advisable to evaporate the fluid twice nearly to dryness, and filter each time, and to use for lixiviation as little water as possible, so as to prevent the solution of the carbonate of barytes.

† A portion of the organic matter followed the earthy carbonates.

gredients were evaporated to dryness and the residue boiled four times with an adequate proportion of alcohol. [The residue after extraction showed no trace of iodine or bromine.] The alcoholic fluid was evaporated in a retort till an ounce remained, which, with the addition of a few grains of carbonate of soda, was evaporated to dryness in a platina crucible, the residue gently heated to a low red heat for the purpose of destroying the organic matter, again dissolved in water and filtered. The alkaline fluid thus obtained was very carefully neutralized by dilute hydrochloric acid; it held now in solution the whole amount of iodine and bromine. In my account of the analysis of the sea-water in the British Channel*, I described my method for separating iodine from bromine and chlorine. This method, though accurate, is more tedious than the method of Lassaigne, which was published after I had written that analysis. Lassaigne recommends to precipitate the iodine by chloride of palladium, and of this excellent and approved test I availed myself. Protochloride of palladium was added to the above fluid, which became at first turbid, but after the lapse of ten hours transparent, and a black-brown precipitate of protoiodide of palladium was obtained, which, when collected upon a filter, lixiviated and dried at 212°F., amounted to 0·046 gr., equivalent to 0·060 gr. of iodide of silver, making for 16 troy ozs. of water 0·010 gr. of iodide of silver, equivalent to 0·005 gr. of iodine.

6. *Bromine.*—The fluid obtained after the separation of the protoiodide of palladium was saturated with hydrosulphuric acid gas to remove the excess of the palladium salt. Having separated by filtration the sulphuret of palladium the fluid was evaporated, the residue dissolved, the fluid filtered and concentrated by evaporation; to this solution was added a few drops of an ammoniacal solution of chloride of silver prepared thus: one part of a saturated solution of chloride of silver in ammonia, mixed with one part of ammonia and one part of water. A few drops of this mixture produced no turbidity in a saturated solution of chloride of sodium, but would indicate a very minute quantity of bromine. I refer for the further proceeding to my analysis of the sea-water in the British Channel. By this process 0·759 gr. of bromide of silver were obtained, making for 16 ozs. troy 0·126 gr., equivalent to 0·053 gr. of bromine.

7. *Potassa.*—810 grs. of the fluid containing the soluble ingredients were neutralized by hydrochloric acid and the sulphuric acid precipitated by chloride of barium. After the separation of the sulphate of barytes, the fluid was mixed

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* See the London and Edinburgh Philosophical Magazine, No. 93, July 1839.

with carbonate of ammonia and evaporated nearly to dryness, to free it from the surplus of the barytes salt. Having separated the carbonate of barytes, the fluid thus obtained was evaporated, and the residue heated to fusion in a well-covered crucible. The alkaline chlorides were dissolved in water and mixed with a surplus quantity of perchloride of platinum, and evaporated in a temperature of 175° F. nearly to dryness; the residue was macerated with spirits of wine (containing 60 per cent. of alcohol) as long as anything dissolved, when potassio-chloride of platinum was left behind weighing 6·036 grs., equivalent to 1·167 gr. potassa.

8. *Soda.*—The usual way of ascertaining the amount of soda is by an indirect method. This has no difficulties with alkaline mineral waters; the amount of soda is obtained when the whole amount of the ingredients found by analysis in the fluid containing the soluble salts is deducted from the whole weight of the alkaline salts obtained by evaporation; however, this method cannot be applied where mineral waters have no alkaline carbonates, and where carbonate of soda has been added to the water, which by the presence of earthy chlorides, &c. has been rarely changed into chloride of sodium.

The following method I employed here for ascertaining the soda by calculation:—It is requisite to arrange the ingredients established by analysis into binary combinations. Potassa, as the strongest and only base* ascertained, is united to sulphuric acid, and the surplus of the latter with the other electro-negative ingredients must therefore have been in combination with soda. The nitric acid has existed as nitrate of soda; this acid, however, in conjunction with organic matter at the high temperature at which the soluble ingredients were fused, has become decomposed, forming with soda carbonate of soda, an assumption established by facts.

	grs.		grs.	
1.	1·167	potassa give	2·158	sulphate of potassa.
2.	4·950	sulphuric acid,		
less	0·991	united with potassa,		
leaves	3·959	sulphuric acid, give	7·047	sulphate of soda.
3.	1·122	nitric acid give		
	1·7697	nitrate of soda, equivalent to	1·106	carbonate of soda.
4.	9·533	chlorine give	15·798	chloride of sodium.
5.	0·005	iodine give	0·006	iodide of sodium.
6.	0·053	bromine give	0·069	bromide of sodium.
7.			0·011	magnesia.
		Total	26·195	

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* With the exception of a small quantity of magnesia which was united to soda.

It will be seen, however, that the total amount of all the ingredients in an anhydrous state obtained at 1, by the analysis of the earthy bases existing in the soluble salts, amounted to 30·580 grs.; deducting the above amount, 26·195 grs., there remains 4·385 grs. carbonate of soda, which is left unchanged of that quantity of carbonate of soda which was originally added to the mineral water when evaporated. If the soda added to 16 troy ozs. of the water be now deducted from the whole amount of the soda obtained by calculation, the exact quantity of soda existing in the water will be then ascertained.

7·047	grs. of	sulphate of soda	=	3·088	soda.
1·106	...	carbonate of soda	=	0·648	...
15·798	...	chloride of sodium	=	8·418	...
0·006	...	iodide of sodium	=	0·001	...
0·069	...	bromide of sodium	=	0·021	...
4·385	...	carbonate of soda	=	2·569	...
		Total .		14·745	grs.
Carbonate of soda added 20·11230 grs.			=soda	11·782	...
Leaving for soda existing in 16 troy ounces of mineral water				2·963	...

According to the foregoing analysis, the following soluble ingredients are obtained in 16 ounces troy of the water:—

	grs.
Potassa	1·167
Soda	2·963
Magnesia . . .	0·011
Sulphuric acid . .	4·950
Nitric acid . . .	1·122
Iodine	0·005
Bromine . . .	0·053
Chlorine . . .	9·533
Total .	19·804

II. *Analysis of the Earthy Ingredients.*

By separating the soluble ingredients from the earthy carbonates, the precipitates A and B were obtained, and also an acid fluid C, which was boiled with an adequate quantity of carbonate of soda, the precipitate thrown down, well-lixiviated, placed in conjunction with A and B in a platina crucible*, and submitted to a temperature of 280° to 300° F. until

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* Every filter was dried in a platina crucible at 212° F., cooled over sulphuric acid, and its weight quickly taken in the same well-covered crucible. Where the nature of the precipitate or other circumstances did not admit of the combustion of the filter, I found it in most cases advantageous to remove the contents of the filter, still half-moist, into a crucible, and to wash the last traces from the filter with a washing bottle.

its weight did not alter; it gave 84·301 grs. The earthy carbonates were now dissolved very carefully in nitric acid, great care being taken that no loss was sustained during the effervescence, which is best obviated by covering the vessel with a convex glass, having in its centre a very small aperture for admitting the dilute acid to drop gradually into the vessel. The solution thus obtained was evaporated in a water-bath to dryness, (no action upon the glass that covered the platina vessel was perceptible, manifesting the absence of fluates,) moistened with nitric acid, and left in that state for twelve hours, when it was re-dissolved. The substance which did not dissolve was lixiviated and boiled with hydrochloric acid, which left a perfectly white silica (*a*), weighing 1·509 gr*. The acid fluid from *a* was then carefully neutralized with ammonia, and, under the well-known precautions, precipitated by succinate of ammonia; the precipitate was first lixiviated by very dilute ammonia, and afterwards with pure water, then carefully ignited with free exposure to the air, when 0·105 gr. of oxide of iron (*b*) was obtained. The fluid derived from *b* was precipitated by hydrosulphate of ammonia; the precipitate, dissolved in hydrochloric acid and boiled with an adequate quantity of carbonate of soda, gave a precipitate, which, after lixiviation with boiling water and ignition, weighed 0·059 gr., and was red oxide of manganese (*c*). The acid fluid obtained from D was mixed in a closed vessel with ammonia, and after the precipitate had subsided, the supernatant fluid was quickly filtered, protected from the atmospheric air, and the precipitate (E), lixiviated and ignited, weighed 11·819 grs. The menstruum from which this precipitate was obtained was only slightly acid, so that but a small amount of an ammonia-salt could be formed, and hence a larger amount of magnesia was precipitated, as will be hereafter seen. The precipitate (E) was dissolved in hydrochloric acid; it left 0·103 gr. silica (*d*). The filtered fluid was mixed with bicarbonate of potassa, when a precipitate (F) was formed. This precipitate was dissolved in hydrochloric acid, and the solution boiled with caustic soda; the precipitate (G) thus formed, after lixiviation, was redissolved in hydrochloric acid and precipitated by ammonia, when 0·244 gr. oxide of iron (*e*) was obtained, which dissolved in hydrochloric acid without residue and gave no indications of manganese. The menstruum from which this oxide of iron was precipitated gave with hydrosulphate of ammonia a few minute flakes of sulphuret of

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* Very often the silica cannot be easily freed from traces of iron: in such cases it is requisite to fuse it with three or four times of its weight of carbonate of soda, and to dissolve it afterwards in hydrochloric acid.

manganese, scarcely to be weighed. The caustic fluid[*] obtained from G was supersaturated by hydrochloric acid, when ammonia gave a precipitate (*f*) of 0·134 gr. of alumina. The filtered fluid from the alumina was evaporated to dryness; the residue, after being heated till the ammoniacal salt was dissipated, left, when dissolved in water, a small trace (*g*) of alumina, weighing 0·063 gr.[†] The fluid obtained from the precipitate (F) was neutralized by hydrochloric acid, and quickly evaporated with carbonate of potassa, and when nearly dry mixed with boiling water and filtered; the precipitate (F) having been properly edulcorated with boiling water, weighed after ignition 10·857 grs., it was redissolved in hydrochloric acid and treated with hydrosulphate of ammonia; the precipitate hereby obtained was collected upon a filter, and gave after a similar treatment as *c*, 0·043 gr. oxide of manganese (*h*); deducting this amount from 10·857, there remains 10·809 grs. of magnesia (*i*)[‡], equal to 22·372 grs. of carbonate of magnesia. The fluid obtained from the precipitate (F) still contained some traces of magnesia, it was therefore precipitated by ammonia and phosphate of ammonia, when the precipitate, after lixiviation with dilute ammonia and ignition, gave 1·127 gr. of phosphate of magnesia, equivalent to 0·413 magnesia (*k*), equivalent to 0·854 gr. carbonate of magnesia. The ammoniacal fluid obtained from the precipitate (E) was next precipitated by oxalate of ammonia, and as soon as the precipitate had settled it was quickly filtered and lixiviated[§]. This precipitate (I) was ignited under the well-known pre-

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[*] As caustic potassa or soda is seldom if ever quite free from traces of silica or alumina, I ascertain, previous to the application of these tests, the quantity of the latter, for which a due deduction is made.

[†] I have here to observe, that every ingredient obtained was, after its weight had been ascertained, again examined for its purity. The alumina, as well as the oxide of iron, was examined before the blowpipe for phosphoric acid; besides, these precipitates were dissolved in hydrochloric acid, precipitated by ammonia, and re-dissolved by a gentle heat in acetic acid, when after the lapse of one day no sediment had formed, evidencing the absence of phosphoric acid. The caustic fluid from which the last trace of alumina was separated was likewise tested for phosphoric acid, by adding to the fluid, neutralized by hydrochloric acid, chloride of calcium; but no trace of it was discernible.

[‡] It is very difficult to separate the last traces of magnesia by carbonate of potassa; the fluid should be boiled quickly to dryness, and the residue lixiviated by boiling water. The fluid obtained from the residue should invariably be examined if it is free from magnesia. As in the above process the fluid containing the magnesia was not boiled to dryness, it accounts for the larger amount of magnesia obtained afterwards.

[§] When magnesia and lime are to be separated, both in considerable quantity, it is requisite to have the menstruum well-diluted, and to filter the oxalate of lime as soon as possible without interruption, so as to prevent the admixture of oxalate of magnesia.

cautions for carbonate of lime, when it weighed 40·579 grs.; it was again dissolved in nitric acid, when 0·005 gr. silica (*l*) was left. The solution of nitrate of lime gave no indications of manganese nor of strontia when the nitrate was changed into a chloride. [If earthy carbonates be dissolved in nitric acid, and tested for strontia by sulphate of lime, small quantities are not detected, but when the carbonates are changed into chlorides minute proportions are easily evidenced by that test.] Deducting 0·005 from 40·579, there remains 40·574 grs. of carbonate of lime (*m*). The filtered fluid derived from (I) was now examined for magnesia. The simplest method would be to evaporate the fluid to dryness, and to ignite the residue until the ammoniacal salts are dissipated; however, this offers some difficulties, particularly as the mass on approaching the dry state rises and involves the probabilities of loss. It is therefore an advisable method to halve the fluid, and ascertain the quantity of magnesia in the one part by phosphate of ammonia, and in the other by evaporation, by which latter process also a few other ingredients, in very small quantity, which have hitherto escaped, may be found. One portion yielded, when precipitated by phosphate of ammonia, &c., 10·344 grs. phosphate of magnesia, equivalent to 3·793 of magnesia. The other half was evaporated to dryness and the residue ignited, when it left 4·186 grs. of a perfectly white magnesia. This was boiled with water, which left after evaporation 0·370 gr. of carbonate of soda (*n*), equivalent upon the whole mass to 0·740 gr. When re-dissolved it acted strongly upon turmeric paper, effervesced with acids, and gave a very minute crystalline precipitate with perchloride of platinum, indicating that the soda contained a slight admixture of potassa. The soda was derived from a soda-carbonate of magnesia, which became decomposed by ignition, and from which boiling water extracted the soda*. The residue from which the carbonate of soda was separated was dissolved in hydrochloric acid, when again a small proportion of 0·010 gr. silica was left, making the whole silica 0·020 (*o*). If the amount of silica (0·010) and that of soda (0·370) is deducted from 4·186, there remains 3·806 grs. of pure magnesia, which upon the whole gives magnesia 7·612 grs. (*p*), equivalent to 15·759 carbonate of magnesia; this amount corresponds very closely with that obtained by the first experiment. The amount therefore of earthy ingredients obtained by analysis stands thus:—

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* By performing the latter method of ascertaining magnesia, it is indispensably necessary to examine the oxalate of ammonia, which frequently yields after ignition traces of alkaline carbonates.

	grs.
Silica (*a, d, l, o*)	1·637
Oxide of iron (*b, e*)	0·349
Oxide of manganese (*c, h*)	0·107
Alumina (*f, g*)	0·197
Carbonate of magnesia (*i, k, p*)	38·985
Carbonate of lime (*m*)	40·574
Carbonate of soda (*n*)	0·740
Total	82·589*

III. *Analysis for the Carbonic Acid Gas and Ammonia.*

1. *Carbonic acid gas.*—Before the above ingredients are computed to binary combinations, we have to ascertain the amount of carbonic acid gas and of ammonia. It is obvious that these two ingredients could not be ascertained from the evaporated water, and therefore new quantities of the mineral water had to be taken. For the analysis of the first 2880 grs. (=6 ounces troy) of the water were precipitated by chloride of barium, and the precipitate collected upon a small filter; this, while still wet and properly compressed, was introduced into a graduated tube over mercury with an adequate quantity of hydrochloric acid, previously saturated with carbonic acid gas. Having made a due reduction for the elasticity of the aqueous vapour, &c., 0·300 gr. were obtained, making for 16 ozs. 0·802 gr. of carbonic acid gas.

2. *Ammonia.*—3000 grs. of the mineral water were introduced into an appropriate tubulated glass retort, and mixed with half an ounce of caustic soda. The vapours were, during distillation, conducted under dilute hydrochloric acid, and the operation finished, when only 2 ounces were left in the retort. The distilled fluid previously mixed with chloride of platinum was evaporated in a porcelain vessel. [I have to notice, that without this addition of the chloride of platinum no correct results can be obtained, even at a temperature below 212° Fahr., inasmuch as the chloride of ammonium rises with the aqueous vapours. This is easily ascertained by covering the vessel during evaporation with filtering paper, which when lixiviated will clearly evidence the presence of chloride of ammonium, a circumstance which Berzelius has already noticed in his Manual of Chemistry, vol. x.] The dry residue was lixiviated by a mixture of 2 volumes of alcohol and 1 volume of æther, in which mixture the ammonio-chloride of platinum is perfectly insoluble. The edulcoration was carried on until all acid reaction upon litmus paper ceased, when the precipi-

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* An amount which comes near to 84·301 grs., but of course free from organic matter, which the latter amount includes.

tate was dried at 212° Fahr. and weighed 15·406 grs. To control the correctness of the weight, the precipitate and filter were placed in a platina crucible, and ignited in the manner described by H. Rose in his Analytical Manual, when 6·8138 grs. of platina were obtained, which corresponds with the amount of platina derived by calculation from 15·406 grs., viz. 6·8142 grs. The amount of ammonia calculated from these data for 16 ounces is 3·033 grs.

If we now arrange the electro-negative and electro-positive ingredients, thus established, into binary combinations in the ratio of their mutual affinities, it will be found that there is a surplus of bases, one of which must obviously be united to the organic matter; and I am inclined to think that a combination of ammonia and organic matter has existed. I regret that the small quantity of water (12 wine bottles) obtained for analysis from Scotland did not permit me to study the nature of the organic matter, but there is no doubt that it belonged to the group of decaying substances. The few experiments I was enabled to perform evidenced the absence of crenic and apocrenic acid, namely, when the water was evaporated to one-tenth of its volume, neutralized by acetic acid, and mixed with acetate of copper and heated, no indication of these acids was perceptible. Acetate of lead gave a precipitate; it did not however separate the organic matter entirely. That the source of ammonia stands in intimate connection with that of the organic matter admits scarcely of doubt, particularly since Mulder* and R. Herrman of Moscow have established that some of the class of humus acids contain ammonia. If mouldering wood, garden-earth, &c. are extracted by alcohol and then with water, and the residue treated with alkalies, a humus acid is obtained, which, besides carbon and the elements of water, contains a certain proportion of ammonia in chemical combination. By the analysis of such an acid derived from the soil of a meadow, the following results were obtained, expressed in this formula:—

$$2\,N_2\,H_6\,OH_2 + C_{40}\,H_{28}\,O_{14}.$$

Mulder's analyses prove also that in a fertile soil combinations of ammonia with those acids exist, derived from the decay of vegetable matter; but these acids correspond with humus acid, inasmuch as they have the same number of equivalents of carbon (40), but vary in the proportion of oxygen and hydrogen. It is obvious from the foregoing results, that mineral waters containing alkalies, when they come in contact with strata interspersed with humus, must become partly im-

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* See Berzelius's *Jahresbericht,* 21st Jahrgang, 3rd Heft.

pregnated with ammonia and these organic acids, produced by the decay of vegetable matter.

The result of the analysis gives the following binary combinations in 16 oz. troy of the water:—

	Anhydrous ingredients.	In 1000 grs.
Sulphate of potassa	2·15838	0·28104
... soda	1·32389	0·17238
... lime	5·49913	0·71603
Iodide of sodium	0·00637	0·00083
Bromide of sodium	0·06900	0·00899
Chloride of ammonium	8·31609	1·08281
... sodium	3·35254	0·43653
... magnesium	2·73148	0·35566
Nitrate of soda	1·76972	0·23043
Carbonate of magnesia	1·49118	0·19417
Proto-carbonate of iron	0·05080	0·00662
... ... manganese	0·01343	0·00175
Ammonia (united to organic matter)	0·37017	0·04820
Alumina	0·01970	0·00256
Silica	0·16330	0·02126
Total in grs.	27·33518	3·55926

If the total amount calculated upon 1000 grs., namely, 3·559 grs. is compared with that obtained by the evaporation of 1000 grs.—3·712 grs.—we find a deficiency of 0·153 gr., which belongs to the organic matter; yet it is to be observed, that the amount 3·712 cannot be very exact, as during evaporation a small proportion of ammonia must have been dissipated. To prove if the results of the analysis of the fixed bases are correct, they were computed to sulphates and compared with the result obtained by evaporating 1000 grs. of the water, igniting the residue to a red heat, and mixing it with a due quantity of sulphuric acid, and again igniting it until the surplus of sulphuric acid was dissipated, when it yielded 2·663 grs.

Changing in this way the fixed ingredients into sulphates, by computation we have—

	grs.
Sulphate of potassa	2·158
... soda	6·930
... lime	5·499
... magnesia	5·569
... alumina	0·065
Proto-sulphate of iron	0·067
... ... manganese	0·021
Silica	0·163
Total	20·472

This amount gives upon 1000 grs. 2·665, a result very close indeed to 2·663, as obtained by experiment, where the iron, manganese and alumina might have lost during ignition a small quantity of sulphuric acid.

A few small flakes, which were found floating in the water, and which were separated, remain finally to be noticed. These flakes consisted chiefly of oxide of iron with a small proportion of organic matter. The oxide of iron upon 16 ozs. amounted to 0·384 gr., which had become separated (as I was assured) from the water in the bottles, and must have existed previously in the form of a proto-carbonate of iron, which would be equivalent to the amount of 0·56175. On adding this to the iron found in solution, 16 troy oz. of the water would contain at its source 0·61255 gr. of proto-carbonate of iron, or in 1000 grs. 0·07975 gr.

For convenience sake the calculation upon the imperial pint is added.

In 1 pint = 34·659 cubic inches (at 60° F. spec. grav. 1·002618) = 8772·907 grs.

	grs.
Sulphate of potassa	2·46554
... soda	1·51227
... lime	6·28166
Iodide of sodium	0·00728
Bromide of sodium	0·07886
Chloride of ammonium	9·49939
... sodium	3·82963
... magnesium	3·12017
Nitrate of soda	2·02154
Carbonate of magnesia	1·70443
Proto-carbonate of iron	0·05807
... ... manganese	0·01535
Ammonia (united to organic matter)	0·42285
Alumina	0·02245
Silica	0·18651
Total	31·22600

The whole amount of proto-carbonate of iron in 1 pint at the source = 0·69973 gr.

XLV. *On Observation-Hours convenient to Amateur Meteorologists, and adequate for obtaining Mean Values.* By S. M. Drach, *Esq., F.R.A.S.*[*]

THIS important selection for the furtherance of meteorology by those whose avocations do not permit of continual attention to their instruments, having been expatiated

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[*] Communicated by the Author.

upon in Lieut.-Col. Sabine's valuable Toronto report contained in the last Number of the Phil. Mag. p. 20, I beg to refer to my papers in the Phil. Mag. for June and July 1842, in which the Plymouth and Leith observations showed that *if the barometer or thermometer be observed four times a day at six-hour intervals,* COMMENCING AT ANY CONVENIENT HOUR, *the result of an annual series would all but equal the mean of one or two-hourly observations.* These conclusions are confirmed by Col. Sabine's communication; whence, denoting by

$4t$ the sums of the observations at 2^{h}, 8^{h}, 14^{h}, 20^{h}, by
$4t'$ ... ... 4, 10, 16, 22, and
$4t''$ 6, 12, 18, 0,

results the following Table:—

	Place.	t.	t'.	t''.	Mean value.	Mean range.
Temperature	Toronto ...	44°·29	44°·56	44°·35	44°·40	11°·35
	Greenwich .	49·50	49·70	49·50	49·60	10·80
Vapour pressure (inches).	Toronto ...	·260	·259	·254	·259	·057
	Greenwich .	·327	·327	·327	·326	·047
Gaseous pressure 29 inches.	Toronto ...	·349	·350	·348	·349	·067
	Greenwich .	·504	·506	·505	·505	·045
Force of wind, lbs. on 1 inch square.	Toronto ...	161½	159	146	155½	133
	Greenwich .	107	107¾	105¾	106¾	42¼

London, February 17, 1845.

P.S. If one of a party of travellers ascending a high mountain await at one of the stations the return of the others, his six-hourly observations on the thermometer and barometer would give the *mean* daily height of these instruments, and hence the altitude of the station; whence by the method of differences, the exact altitude of the summit would always be ascertainable.

XLVI. *On a Connection between the General Theory of Normal Couples and the Theory of Complete Quadratic Functions of Two Variables.* By JOHN T. GRAVES, *M.A., F.R.S., Examiner in Laws and Jurisprudence in the University of London.*[*]

THE remarkable researches of Sir W. Hamilton on *Quaternions,* followed by Prof. De Morgan's very able and

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[*] Communicated by the Author. The continuation of Sir W. R. Hamilton's paper on Quaternions will be found in our last Number, p. 220.

original paper on *Triplets* (published in the Cambridge Philosophical Transactions), have induced me to recur to the examination of *Couples*. In the present paper I shall confine myself to the algebraical consideration of the subject.

Couples are of the form $ix+jy$; the variables x and y are called the *constituents* of the couple, the constants i and j its characteristic *coefficients*. The couples that I treat of are supposed to follow the analogy of ordinary algebra, subject to the *characteristic rule*, that "if two couples are equal, their constituents are separately equal." We are precluded from supposing the existence of any linear relation between the coefficients. If we have the equation

$$(1.)\qquad ix+jy=ix_1+jy_1,$$

we are to assume $x=x_1$, $y=y_1$, and are not allowed to establish the equation $i=j.\dfrac{y_1-y}{x-x_1}$, if $\dfrac{y_1-y}{x-x_1}$ have a determinate value. Hence, if we have $ix+jy=0$, we are bound to assume $x=0$, $y=0$. This view of couples is derived from the ordinary algebra of imaginary quantities. If

$$(2.)\qquad x+\sqrt{-1}\,y=x_1+\sqrt{-1}\,y_1,$$

we have $x=x_1$ and $y=y_1$.

We are to assume such rules that functions of couples may be couples. The product of two couples is a couple. If we follow the rules of algebra, we shall have

$$(3.)\quad (ix+jy)(ix_1+jy_1)=i^2xx_1+ij(xy_1+yx_1)+j^2yy_1.$$

Sir W. Hamilton, in quaternions, by a happy deviation from the rules of ordinary algebra, makes $ij=-ji$, but we are at present treating of couples which obey ordinary algebraic rules. Possibly science may gain more by the introduction of *anomalous* couples, but I confine myself at present to *normal* couples.

In order that $i^2xx_1+ij(xy_1+yx_1)+j^2yy_1$, which is the product of two couples, may be itself a couple, we must define i^2, ij and j^2 in such a way as to reduce the product to the form ix_2+jy_2. This cannot be effected unless we suppose

$$\left.\begin{array}{ll}(4.) & i^2=i\alpha+j\beta\\ (5.) & ij=i\varkappa+j\varkappa'\\ (6.) & j^2=i\beta'+j\alpha'\end{array}\right\},\quad\ldots\ldots\quad(\mathrm{A.})$$

α, $`\alpha$, $\beta$, $`\beta$, $\varkappa$, $`\varkappa$ being constants capable of being connected by linear relations. They may be termed "the connected constants of multiplication."

If we assume

$$(7.)\qquad (ix+jy)(ix_1+jy_1) = ix_2+jy_2,$$

we shall obtain from (3.) and the equations (A.),

$$(8.)\qquad \left.\begin{array}{l} x_2=\alpha x x_1+\kappa(xy_1+yx_1)+\beta' yy_1 \\ y_2=\alpha' yy_1+\kappa'(xy_1+yx_1)+\beta xx_1 \end{array}\right\}.$$

In normal couples, the equations

$$(9.)\qquad i^2.j=i.ij,\quad j^2.i=j.ij,$$

hold good *by virtue merely of the assumed rules of multiplication,* without the necessity of assuming any linear relation between i and j. We proceed to investigate the relations which must subsist among the constants α, α', β, β', κ, κ', in order to satisfy this condition.

Since $i^2j=i.ij=j.i^2$, we have, by (5.) and (4.),

$$(10.)\qquad i^2\kappa+ij\kappa'=ij\alpha+j^2\beta.$$

Since $j^2i=j.ij=i.j^2$, we have, by (5.) and (6.),

$$(11.)\qquad ij\kappa+j^2\kappa'=i^2\beta'+ij\alpha'.$$

Substituting in (10.) and (11.) the values of i^2, ij and j^2 given by equations (A.), we get from (10.),

$$(12.)\quad (i\alpha+j\beta)\kappa+(i\kappa+j\kappa')\kappa'=(i\kappa+j\kappa')\alpha+(i\beta'+j\alpha')\beta.$$

Similarly, we get from (11.),

$$(13.)\quad (i\kappa+j\kappa')\kappa+(i\beta'+j\alpha')\kappa'=(i\alpha+j\beta)\beta'+(i\kappa+j\kappa')\alpha'.$$

Arranging (12.) and (13.), we obtain

$$(14.)\qquad i(\kappa\kappa'-\beta\beta')+j(\kappa'(\kappa'-\alpha)-\beta(\alpha'-\kappa))=0,$$

$$(15.)\qquad j(\kappa\kappa'-\beta\beta')+i(\beta'(\kappa'-\alpha)-\kappa(\alpha'-\kappa))=0.$$

It is evident that the two equations (14.) and (15.) will be satisfied, independently of any linear relation between i and j, if we suppose, in conformity with the *characteristic rule* of couples,

$$(16.)\quad \kappa\kappa'-\beta\beta'=\kappa'(\kappa'-\alpha)-\beta(\alpha'-\kappa)=\beta'(\kappa'-\alpha)-\kappa(\alpha'-\kappa)=0.$$

The three equations (16.) are (as it is not difficult to perceive) equivalent to the two following:

$$(17.)\qquad \frac{\kappa'}{\beta}=\frac{\alpha'-\kappa}{\kappa'-\alpha}=\frac{\beta'}{\kappa}.$$

The two equations (17.) express the relations which, in every system of *normal couples,* connect the constants of multiplication. It is easy to see that the equations (17.) will be satisfied by *integer* values of the constants, if we suppose that the three equal fractions in (17.) are each equal to $-\frac{p}{q}$; and if we take

$$(18.)\quad \kappa'=p\varrho,\ \beta=-q\varrho,\ \alpha'-\kappa=p\eta,\ \kappa'-\alpha=-q\eta,\ \beta'=-p\theta,\ \kappa=q\theta.$$

By these assumptions we are enabled to make the six connected constants, α, α', β, β', $\varkappa$, $\varkappa'$, depend upon five arbitrary and unconnected constants, p, q, ϱ, η, θ. Certain limited variations of sign might be allowed in our last assumptions consistently with the integer character of the constants of multiplication. Thus it would appear more natural to assume the equal fractions in (17.) each equal to $+\frac{p}{q}$ instead of $-\frac{p}{q}$, and to give *positive* values to every numerator and denominator, taking

$$(19.)\quad \varkappa'=p\,\varrho,\ \beta=q\,\varrho,\ \alpha'-\varkappa=p\,\eta,\ \varkappa'-\alpha=q\,\eta,\ \beta'=p\,\theta,\ \varkappa=q\,\theta.$$

To this there would be no objection on principle, and the results to which it would lead would be convertible with those which follow. The assumptions (19.) were, in fact, nearly those which I at first made, but I found upon trial that the signs of (18.) are those which give the greatest concinnity to the subsequent calculations.

By substituting in equations (A.), in place of the connected constants of multiplication, their values as determined by equations (18.), we get

$$\left.\begin{array}{ll} (20.) & i^2=i\,(p\,\varrho+q\,\eta)-j\,q\,\varrho \\ (21.) & ij=i\,q\,\theta+j\,p\,\varrho \\ (22.) & j^2=j\,(q\,\theta+p\,\eta)-i\,p\,\theta \end{array}\right\}.\quad .\quad .\quad .\quad (B.)$$

From equations (B.) we get the following system of equations, defining a general rule of multiplication to which all normal couples will be found to conform:

$$\left.\begin{array}{ll} (23.) & (i\,p_1+j\,q_1)\,(i\,p_2+j\,q_2)=i\,p_3+j\,q_3 \\ (24.) & p_3=(p\,\varrho+q\,\eta)\cdot p_1p_2+q\,\theta\cdot(p_1q_2+q_1p_2)-p\,\theta\cdot q_1q_2 \\ (25.) & q_3=(q\,\theta+p\,\eta)\cdot q_1\,q_2+p\,\varrho\,(p_1q_2+q_1p_2)-q\varrho\cdot p_1p_2 \end{array}\right\}.\ (C.)$$

If we express the couple $i\,p+j\,q$ by the notation (p, q), and arrange equations (24.) and (25.), the multiplication of normal couples will be expressed by the following system:

$$\left.\begin{array}{ll} (26.) & (p_1, q_1)\,(p_2, q_2)=(p_3, q_3) \\ (27.) & p_3=pp_1p_2\varrho+qp_1p_2\eta+(qp_1q_2+qq_1p_2-pq_1q_2)\,\theta \\ (28.) & q_3=qq_1q_2\theta+pq_1q_2\eta+(pq_1p_2+pp_1q_2-qp_1p_2)\,\varrho \end{array}\right\}.\ (D.)$$

A connection between the theory of multiplication of normal couples and the resolution into factors of quadratic functions of two variables, will appear from the investigation to which I now proceed. Thus the theory of couples becomes connected with that of quadratic equations. We may treat i and j as ordinary algebraic quantities, and endeavour to deduce their values in terms of p, q, ϱ, η, θ, by means of the equations (B.). Eliminating j between (20.) and (21.), we get

$$(29.)\qquad j = -\frac{i^2 - i(p\varrho + q\eta)}{q\varrho} = \frac{iq\theta}{i - p\varrho}.$$

Hence we find upon arranging,

$$(30.)\qquad i^2 - (2p\varrho + q\eta)\,i + (p^2\varrho + pq\eta + q^2\theta)\,\varrho = 0.$$

In like manner, by eliminating i between (21.) and (22.), we find

$$(31.)\qquad j^2 - (2q\theta + p\eta)j + (p^2\varrho + pq\eta + q^2\theta)\,\theta = 0.$$

Upon solving (30.), we get

$$(32.)\qquad i = \frac{2p\varrho + q\eta \pm q\sqrt{\eta^2 - 4\varrho\theta}}{2};$$

and similarly, upon solving (31.), we get

$$(33.)\qquad j = \frac{2q\theta + p\eta \mp p\sqrt{\eta^2 - 4\varrho\theta}}{2}.$$

It will be found upon examination, that, in order to satisfy the equation (21.), we must make the positive sign of the radical in (32.) correspond to the negative sign in (33.), and *vice versâ*.

Let the two values of i given by (32.) be denoted by i_1 and i_2, and let the corresponding values of j be denoted by j_1 and j_2. Let

$$(34.)\qquad p^2\varrho + pq\eta + q^2\theta = u,$$

and let

$$(35.)\qquad p_n^2\varrho + p_n q_n\eta + q_n^2\theta = u_n.$$

Then we shall have (as will be found upon trial) the three following connected equations:

$$\left.\begin{array}{lr} (36.) & i_1 i_2 = u\varrho \\ (37.) & i_1 j_2 + j_1 i_2 = u\eta \\ (38.) & j_1 j_2 = u\theta \end{array}\right\}\ldots\ldots\ldots\ldots \text{(E.)}$$

Hence we obtain the leading theorem,

$$(39.)\qquad (i_1 p_n + j_1 q_n)(i_2 p_n + j_2 q_n) = uu_n.$$

Further, we have, by the equations (C.), since both i_1, j_1 and i_2, j_2 satisfy the equations (B.),

$$\left.\begin{array}{ll} (40.) & (i_1p_1 + j_1q_1)(i_1p_2 + j_1q_2) = i_1p_3 + j_1q_3 \\ (41.) & (i_2p_1 + j_2q_1)(i_2p_2 + j_2q_2) = i_2p_3 + j_2q_3 \end{array}\right\}.$$

p_3 and q_3 having the same values as in (24.) and (25.), or, as in (27.) and (28.)

Multiplying together (40.) and (41.), we get by (39.),

$$(42.)\qquad uu_1 \,.\, uu_2 = uu_3;$$

and, dividing both sides of (42.) by u, we obtain, finally, the interesting result,

$$(43.)\qquad u\,.\,u_1\,.\,u_2 = u_3.$$

This theorem may be expressed as follows:

"*The continued product of three functions of the form*

$$x^2\varrho + xy\eta + y^2\theta$$

is of the same form." x and y are supposed to be constant, and ϱ, η and θ to be variable.

In symbols at length, we have

$$(44.)\qquad (x^2\varrho + xy\eta + y^2\theta)\,(x_1^2\varrho + x_1y_1\eta + y_1^2\theta)\,(x_2^2\varrho + x_2y_2\eta + y_2^2\theta)$$
$$= x_3^2\varrho + x_3y_3\eta + y_3^2\theta,$$

x_3 and y_3 having the values following:

$$(45.)\qquad x_3 = xx_1x_2\varrho + yx_1x_2\eta + (yx_1y_2 + yy_1x_2 - xy_1y_2)\theta.$$

$$(46.)\qquad y_3 = yy_1y_2\theta + xy_1y_2\eta + (xy_1x_2 + xx_1y_2 - yx_1x_2)\varrho.$$

This appears to be the most important quadratic *modulus* theorem which the extended theory of normal couples furnishes. It admits of a great number of interesting particular cases. For example, making x in (44.) equal to 1, $\varrho = 1$, $y = 0$, the first factor is reduced to 1, and we get the theorem,

"*The product of two functions of the form*

$$x^2 + xy\eta + y^2\theta$$

is of the same form."

If, further, we make $\eta = 0$ and $\theta = 1$, we get the well-known theorem,

"*The product of two sums of two squares is a sum of two squares.*"

P.S. "*The product of two sums of eight squares is a sum of eight squares.*" To Euler is due a corresponding theorem relating to sums of *four* squares, which was extended by Lagrange, who added coefficients to the squares. As Euler's theorem is connected with Hamilton's quaternions, so my theorem concerning sums of eight squares may be made the basis of a system of octads or *sets of eight*, and was actually so applied by me about Christmas 1843. I mention this in consequence of the idea suggested by Mr. Cayley in the last Number of this Magazine. But the full statement and proof of the theorem concerning sums of eight squares, and of several other new theorems connected with the doctrine of numbers, must be reserved for another time.

XLVII. *Account of a new Experiment in Electro-Statical Induction. By* Prof. MATTEUCCI, *in a Letter to* Dr. FARADAY.

MY DEAR FARADAY,

I HOPE it will not be disagreeable to you if I describe an experiment which I performed in one of my late lectures on electro-statical induction, in which I dwelt entirely upon the exposition of your theory. This lecture, reported by one of the élèves of this school, will appear in our journal, the *Cimento*.

After having repeated your beautiful experiment of the pieces of silk suspended in essential oil of turpentine, and which unite together between the two electrified balls, I spoke of the figures obtained by sprinkling a mixture of two kinds of powder, sulphur and minium, upon a plate of resin, over which the rod of a charged Leyden phial had previously been passed. The other experiment, which appears to me to be sufficiently decisive, is this. I separate a piece of mica into very thin laminæ, not thicker than the finest paper; I cut these into small squares with a pair of scissors, leaving a small appendage on one side. I pile these little squares one upon the other, subjecting them to a strong pressure, taking care however that the appendages do not correspond. This done, I begin by showing, that on separating these laminæ one from the other, and rapidly approaching them—first with one surface, and then with the other—to the nob of an electroscope sufficiently delicate for the purpose, and furnished with a dry pile, either there was no sign of electricity, or if in any case there were signs, they were undetermined and varied, and in all probability owing to friction. After this I replace the little squares of mica one upon the other, as described above, and lay the heap upon a metallic plate, in communication with the ground; and on the upper lamina of mica I place a little disc of tin. I next put this tin disc in communication with an electrical machine, and after a few turns I disarrange the pile; taking the little squares of mica, one by one by their appendages, and applying them to the electroscope. I present first one surface and then the other; and between each experiment I put the nob of the electroscope in communication with the ground, so as to discharge it. It never fails to happen that the two surfaces of each of the laminæ are in opposite states of electrical excitement, that is, that surface of every lamina is positive which is turned towards the positive coating, the other surface of every lamina which is turned towards the negative coating is negatively excited.

I have only repeated this experiment a very few times, and cannot precisely tell you the laws of this polarization. It has appeared to me that a certain determined thickness of the lamina of mica is necessary to the production of the phænomenon, and therefore that a too thick or a too thin lamina would be unfit. I have also imagined that the charges decreased from the extreme lamina towards the middle layers, and so it appears to me it should be. Engaged in the continuation of my researches on electro-physiology,—on the subject of which I hope soon to be able to address the Royal Society,—I have not been able to continue the study of this phænomenon.

If you think it worth it, give this letter to Mr. Taylor for the Philosophical Magazine.

Yours faithfully,

Pisa, March 7, 1845. CARLO MATTEUCCI.

XLVIII. *On the Digestion of Vegetable Albumen, Fat and Starch. By* ROBERT D. THOMSON, *M.D., Lecturer on Practical Chemistry in the University of Glasgow*[*].

DIGESTION of Vegetable Albumen and Fat.—Nearly three years ago Dr. A. Buchanan communicated to me the fact, that he had frequently observed the serum of the blood to present a white appearance when a person happened to have been bled for some affection of such a circumscribed nature as not to interfere with the appetite or digestion; but an opportunity did not occur to me of examining this peculiar serum till the end of 1843, when Dr. Buchanan was so kind as to place some of it at my disposal. The fluid presented the appearance of thin milk, being covered, after standing, on the upper surface with a whitish scum of a denser character than that which was diffused through the fluid. When the fluid was filtered a portion of the scum remained on the filter while the liquid passed through, possessing still a milky aspect, but obviously being deprived of a considerable portion of its denser particles. Dr. Buchanan however observed that this white matter might be accumulated in larger quantities, or at least collected into smaller bulk, by saturating the serum with common salt, when the white matter speedily rose to the surface in the form of a creamy layer, which did not change its physical character even when kept for months[†]. I found that the white matter, both when separated simply by the filter and also when precipitated, if we may so speak, by common salt, contained a substance which was insoluble in æther and alcohol, and that when dissolved in caustic potash and the solution was boiled with acetate of lead, a black precipitate of sulphuret of lead fell[‡]. These experiments were several times repeated carefully with the same result, and were witnessed by Dr. Buchanan, so that we had no doubt in our own

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[*] Read before the Philosophical Society of Glasgow, February 5, 1845.

[†] See a paper on this subject by Dr. Buchanan, Proceedings of the Philosophical Society of Glasgow, vol. i. p. 226.

[‡] This test for sulphur in albumen was employed in 1812 by Vauquelin.

minds that the presence of traces of an albuminous substance in the white matter of this serum was established; and we had subsequently opportunities of obtaining this matter in larger quantities, so as to confirm the previous experiments.

These results led to a series of researches upon the effect of food on animals, and also on man; from which it was clearly demonstrated that the white colour of the serum in healthy animals is dependent on the introduction of food into the system. The detail of one or two experiments will place this conclusion in a distinct point of view. It was necessary for this purpose to observe the characters of the serum of an individual who had not tasted food for such a space of time that the effects of the previous meal should have disappeared. On the 10th of March 1844, from a stout young man, aged thirty, who had tasted no food from the preceding evening at six o'clock p.m., two ounces of blood were taken at noon, or at an interval of eighteen hours after a meal. The blood coagulated on standing in the usual manner, and the supernatant serum was found to possess a pale yellow colour, and to be perfectly clear and limpid.

After the abstraction of the blood, the individual dined upon twenty-four ounces of a pudding consisting of two parts of wheat flour and one part of suet, seasoned with salt. At three o'clock, or in about three hours after the food had been swallowed, seven ounces of blood were taken by venesection from the arm. The clot formed as usual, no appearance of a buffy coat presenting itself. The serum was whitish and opake; when heated it became more translucent, apparently from the solution of some of the solid particles diffused through it, or, as was afterwards apparent, from the liquefaction of the fatty matter diffused through the serum. The whole fluid possessed a somewhat syrupy cast of appearance, and was very heavy, its specific gravity being as high as 1029·8. Comparing this density with the average density of serum as we find it in physiological works, 1026½ by Dr. Thomson, 1027 to 1029 by Müller, it must be pronounced high; but as the specific gravity of this fluid has never been properly estimated in perhaps the truly healthy condition of animals, and more especially in parallel cases with that described, no conclusion can be deduced in reference to the density of the serum as compared with the normal standard. On throwing the serum upon a filter, a portion of white matter remained attached to its interior surface, while the liquid which passed through retained still a milky aspect; but perhaps the most interesting result obtained by this experiment was, that on drying the filter and holding it between the eye and the light, it was found to have imbibed a considerable amount of oily matter. The white substance remaining on the filter presented characters similar to those of albuminous or fibrinous matter.

The facts detailed having shown that there was a decided distinction between the serum of a person previous to and after taking food, it became an interesting point to pursue the inquiry, and to ascertain how long the milky aspect would continue in the blood. Accordingly, at six o'clock p.m., or six hours after the meal, the individual was a third time bled to the extent of seven ounces. The serum was in this case very milky, and threw up, on standing, a considerable white scum to the surface, which could be drawn up by a sucker and examined. On throwing the serum on a filter, a small portion of white matter adhered to it, possessing albuminous characters; the serum passed through with a milky colour, but no imbibition of oil could be detected on the filter itself. A careful examination of this specimen of serum could detect not a trace of starch by means of iodine and chlorine, and this led to the investigations detailed in the subsequent parts of this paper.

The inferences deducible from this experiment appear to be,—1st, that the serum of a healthy individual at a certain period of time, after partaking of a meal, is clear and limpid, and corresponds with the description of this fluid as we find it detailed in physiological works of authority; 2nd, that in three hours after a meal, when the food consists of vegetable albuminous matter and oil or fat, the albumen begins to make its appearance in the blood, while a still larger quantity of fat in relation to the amount of the albumen taken into the stomach exists in the blood; and 3rd, that in six hours, while the quantity of albuminous matter in the blood can be detected, the fatty matter has comparatively disappeared; a conclusion which is completely in accordance with the chemical properties of these substances, since we know that some kinds of fat liquefy nearly at the temperature of the human body, and will, of consequence, be in a condition immediately after their introduction into the stomach to enter the sanguineous circulation along with the water present in the stomach, since that fluid appears capable of permeating with great facility the coats of the intestinal canal throughout its whole length.

To determine the manner in which the food thus passes into the circulating system, is scarcely the province of the chemist. At the same time, since it appears to exist in the current of the blood without having undergone much modification of state from that which it originally possessed in the stomach, there seem no obvious arguments to present themselves against the conclusion that the food is directly absorbed from the alimentary canal by the blood-vessels themselves.

In confirmation of the experiment already detailed, the physiological part of which was conducted by Dr. Buchanan, whose great merits as a physiologist and original observer require no encomiums from me, various repetitions on the inferior animals were conducted by myself, which it would serve no purpose to enumerate minutely, since they all conducted to the same results.

In the majority of these cases calves were fed on gruel and milk, and after various intervals they were slaughtered. The serum on examination, when the animal was killed from three to six hours after the meal, was found to be milky, and to leave a greasy stain on filtering paper when the amount of milk or fatty matter used was considerable, while the serum taken from an animal which had been subjected to starvation for a space of time varying from twelve to twenty-four hours, presented generally a clear aspect. It has been frequently remarked that the serum of diabetic patients exhibits often a milky appearance, and the circumstance of the coexistence of disease with the white serum was considered as a proof that the disease was the cause of the colour of the liquid part of the blood; but it has been very properly remarked by Dr. Buchanan, that the large amount of food consumed by individuals affected with this disease, affords a satisfactory mode of accounting for the presence of such profuse quantities of albuminous and fatty matter in the blood in such instances. I have recently had opportunities of studying this form of serum through the kindness of Dr. William Thomson, and I have found no reason to conclude that the blood of diabetic patients, so far as its milkiness is concerned, is more diseased than that of healthy individuals after a full meal. It is no doubt highly probable that the blood in these instances remains for a longer time loaded with the white matter than in the healthy state of the organism, and that the appetite may, by continually urging in a new supply of food, thus produce a diminished rate of digestion or assimilation in the circulating system; but the presence of the white matter in the serum it would be erroneous to consider as a symptom of disease, at least in so far as the data entitle us to draw such an inference.

Changes produced on Starch in Digestion.—As it appeared to be a matter of importance in the investigation of the changes occurring in the stomach during digestion, in order to prevent complication of the phænomena, that the food should be as simple as possible, I have chosen for the experiments about to be detailed, cases in which animals were fed on vegetable food alone. In general the species of food selected was porridge, or a mixture of oatmeal and water well-boiled. In such experiments as I have seen detailed, in reference to the free acid of the stomach, there appears to have been too little attention paid to the possible results which might arise from a difference in the nature of the food. Thus the products of the digestion of starch we should naturally expect to be different from those of the digestion of animal fibrin or albumen, since we know that lactic acid can be produced by a modification of starch, although the same change does not attend the decomposition of the animal substances enumerated. For example, in the preparation of starch an acid liquor collects on the surface of the vessels in which the starch is digested, and in the formation of *sowans* from oat-husks and water by steeping, an acid liquor is developed,—in both instances a consequence of the production of lactic acid at the expense of starch; while in the German dish, saurkraut, the same acid is generated by an analogous action. I do not at present intend to discuss the nature of the acid which presents itself in the stomach during the digestion of animal food, but simply to detail the results of a series of experiments upon the changes produced on vegetable food and starch during digestion. I shall merely content myself with stating, that I have never found a volatile acid in the stomachs of animals which were digesting animal food alone, while in these cases I have invariably found an acid to be present which was fixed at the temperature of 212°, and even considerably higher.

The nature of the Acid developed during the digestion of Starch.—It does not appear an invariable rule that an acid reaction should always characterize the liquid present in the stomach during digestion, as appears from the following experiment. On the 11th of July 1844, the different stomachs of a sheep killed twenty-four hours after partaking of grass, contained between two and three pounds of finely-divided green matter exhibiting a pulpy consistence, but being entirely destitute of either an acid or alkaline reaction. The pulpy masses were tested in each of the stomachs with the same negative result, and they were afterwards mixed together, diluted with distilled water and filtered, but still without producing any effect on litmus paper. Experience teaches us, that to determine the presence of a volatile acid in the stomach, great care must be taken in reference to the mode in which the distillation is performed, since if the heat of an open fire is applied to the retort, an indication of the presence of hydrochloric acid will be found in the liquid contained in the receiver by the addition of nitrate of silver. The following experiment affords evidence in favour of this position, and it might be strengthened, if necessary, by the results obtained by other experimenters. The contents of the stomach of a dog which had been fed on porridge made of oatmeal and water, and which afforded an acid reaction with litmus paper, were mixed with distilled water and filtered. On exposing the distilled liquor to the heat of an open fire in a retort, a fluid passed over possessing an acid reaction, and becoming opalescent on the addition of a solution of nitrate of silver, and without disappearance in contact with nitric acid; a result plainly indicative of the presence of chlorine in some form in the liquor of the receiver, but, as appeared by the subsequent inquiries, to be explained by the fact that muriate of ammonia must have been carried over by the vapour of water in consequence of the excess of heat.

To determine in a satisfactory manner therefore the presence of a volatile acid, it is necessary to distil the fluid contents of the stomach in a water-bath. With this consideration in view the following experiment was made. June 1844, a pig was fed on potatoes and greens, and was killed in half an hour after the food was swallowed. The stomach was slit open, and as much of the fluid contents as possible were poured off; the solid contents were then digested in cold distilled water. Both fluids were filtered and mixed, each previous to being united being found to exhibit an acid reaction with litmus infusion. To ascertain whether the statement made by a French physiologist (Blondlot) is correct, viz. that the acid of the stomach cannot be saturated with chalk, a quantity of pure carbonate of lime was prepared by dissolving Irish limestone in hydrochloric acid, precipitating a small portion of phosphate of lime and iron which it contains with caustic ammonia, and then throwing down the lime with carbonate of ammonia, collecting the precipitate on a filter, washing it well with distilled water, and heating it to redness in a platinum crucible. An excess of this chalk was then added to the filtered fluid of the stomach, and the mixture was allowed to remain for twenty-four hours, disturbed only by frequent agitation. It was then filtered, and found to have been completely neutralized. Having often repeated this experiment on various specimens of gastric fluid with the same effect, I can only account for the different result obtained by Blondlot, by supposing that he had attempted to complete his neutralization of the fluid while it was in a heated state, and that possibly, if acetic or lactic acids were present, these acids might resist neutralization at an elevated temperature, since it has been found by manufacturers of pyroligneous acid that they cannot succeed in forming a neutral acetate of lime at a temperature approaching that of the boiling-point by chalk alone, but that they require to add milk of lime in order to overcome the acid reaction. Blondlot has deduced the inference from his experiment, that the stomach owes its acid reaction to the presence of an acid phosphate of lime; but as the experiments now detailed do not coincide with those of the French physiologist, it is sufficiently obvious that they do not support him in his conclusions.

[To be continued.]

XLIX. *On the Mechanism of Glacial Motion. Fourth Letter. By* W. Hopkins, *Esq., M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

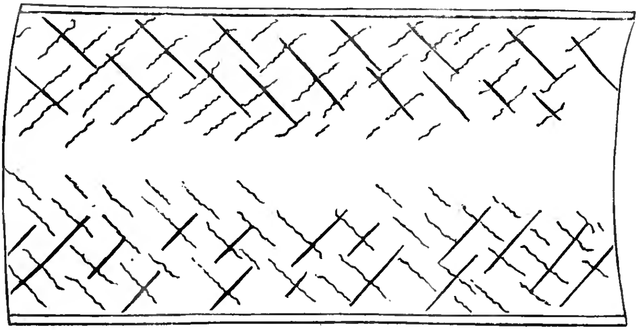
SINCE I addressed my last letter to you on the motion of glaciers, I have devised some experiments, the results of which I wish to communicate, as corroborative of the conclusions at which I have arrived by mathematical investigation.

The weight of a solid mass sufficiently small to experiment upon conveniently, is not sufficient to communicate a motion to it which, as in the case of a glacier, shall superinduce any considerable change of form and consequent fracture, the conditions under which the mass is placed being analogous to those of a glacier. When the width of a mass under such conditions is increased, the tendency of the forces arising from the weight of the mass to fracture it is also increased; and lateral obstacles, which would entirely arrest the motion of a mass of limited width, might produce scarcely a sensible effect on the central motion of a mass of large dimensions. When our object, however, is to determine the effects of an assigned motion and consequent change of form, as in the case before us, it is immaterial whether the motion be produced by the action of gravity, or any other cause. In my experiments the mass was about three feet in length and two in breadth, and two inches in depth. It consisted of fine mortar, which was allowed to assume different degrees of solidity, according to the particular object of each experiment.

The experiments were conducted as follows:—A trough of the length and width above-mentioned and five or six inches in depth, was prepared, open at both ends. Along the bottom of it was placed longitudinally a layer of straight rods parallel to each other, on which the mortar was poured, its consistency being about the same as that of mortar used in building. The quantity was such as to form a uniform layer of about two inches above the rods and to fill up the interspaces between them. The whole was then left to acquire the requisite degree of solidity. The motion was communicated by applying a force to the central rods alone, which, with the superincumbent mortar adhering to them, were thus made to move, while the lateral rods and mortar remained at rest. In this manner a straight transversal line drawn on the surface of the mortar became a *loop*, the form of which could be regulated at pleasure, and the mass was brought into a state of constraint exactly similar to that superinduced in a glacier by the excess of its central over its lateral motion. The results were different according as the mortar was comparatively soft or compact.

I. When the mortar was left for twenty-four hours or upwards, it acquired a considerable degree of solidity, so that, when such a motion had been communicated to its central portion as to give a very slight curvature to the transverse line above-mentioned, a system of parallel fissures began to be formed in each lateral portion, in directions making angles of 45° with the axis of the trough, as represented in fig. 1.

Fig. 1.



This is exactly accordant with my theoretical deduction for the case in which there is neither longitudinal nor transversal extension or compression (Second Letter, art. 20, p. 161), as was the case in the experiment; for, from the manner in which the motion was communicated, there could be no longitudinal extension or compression, and transversal compression could only arise from the relative motion of the centre, and could therefore only be of the second order of small quantities, the motion itself being considered a small quantity of the first order. The incipient fissures were not curved but straight, as they ought to be, according to theory, under the above conditions.

The experiment was also made somewhat differently, by placing the central rods so that they reached only half the length of the trough. When these rods were made to move as before, with the mass immediately superincumbent upon them, that portion along the middle of the trough under which the rods did not pass was extended, and the *curved* fissures across the whole surface were there produced in a manner exactly accordant with theory.

Another system of lines also resulted from the motion communicated to the mass. A great number of extremely small ridges forming discontinuous lines, as represented in the preceding diagram, appeared on the surface, their directions being at right angles to the lines of fissure. They were manifestly due to the compression in directions at right angles to them, these directions being in fact the directions of maximum pressure, as determined by theory. That such was the case was made further evident by continuing the motion of the central portion, and watching the gradual development and increased number of these lines. They were best developed when the surface of the mass had become slightly *crisped* before the motion was given to it.

To obtain more distinct evidence respecting the existence of the internal pressure in question, I cut the mass so as to form an artificial fissure of some width, along one of these lines of elevation, and therefore perpendicular to the directions of the original fissures first described. When the motion of the central part of the mass was continued, the artificial fissure was soon entirely closed up, while the width of the natural fissures continually increased. The evidence was perfectly conclusive.

According to theory, the directions of the natural fissures and that of the artificial one mentioned in the last paragraph, are those in which there is no tendency in the particles on one side of a vertical plane to slide past the contiguous particles on the opposite side. This was perfectly verified by experiment; for after the movement which opened the natural fissures and closed the artificial one intersecting them at right angles, the direction of the artificial fissure, as well as that of each natural one, remained unbroken at the point of intersection. There had not been the smallest sliding motion like that here contemplated, in the direction of either fissure, although there was nothing to resist the tendency to such motion, the cohesion being entirely destroyed along the lines of fissure. This conclusion, it will be recollected, is in direct opposition to the theory of Professor Forbes, according to which the direction of the artificial fissure above described is that in which the greatest sliding motion ought to take place.

The lines exhibited on Prof. Forbes's models of plaster of Paris, and which have been considered by him as representatives of the veined structure in glaciers, are to be referred, I have no doubt, to the same cause as the linear ridges described in my experiment. In fact, if it were possible to continue the central motion in my experiments, to the same extent as in the more fluid mass used in the experiments of Prof. Forbes, the two sets of lines in these cases respectively would assume exactly similar positions. I have shown experimentally that the lines in my experiments were not associated with any discontinuity of the mass, such as that which Prof. Forbes has considered to be indicated by the corresponding lines in his models. How such indication is afforded by the lines in question, I do not in any degree understand.

When the central motion was continued the fissures were necessarily brought more nearly to perpendicularity with the axis; but when their obliquity to their original position, *i. e.* to the line of maximum pressure, became considerable, they began to close. (Second Letter, art. 24, p. 163.). We thus account for the fact, that transverse fissures never remain open long enough to acquire a position in which their convexity would be turned towards the lower extremity of the glacier, a position which they must necessarily assume by the more rapid motion of the centre, unless there were some cause originating in that motion constantly tending to obliterate them.

When the central motion was continued long enough, the fissures along the flanks became more irregular and ran into each other, after which the central portion moved nearly as a continuous mass, sliding past the narrow lateral portions, from which it was severed on either side by the lateral fissures running into each other as just described.

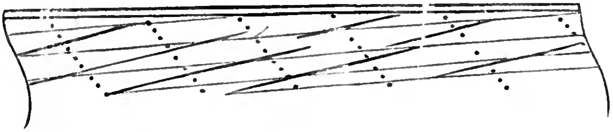
Another effect of the pressure superinduced by the motion of the central portion is not undeserving of notice. When the mass projected beyond the lower end of the trough, it was pushed outwards at the sides by the pressure acting transversely, so that by this action and the dislocation along the flank of the mass, the lateral portions turned round the extremities of the trough almost as completely as if the mass had been fluid. This explains the expansion of glaciers in expanding valleys without the hypothesis of fluidity. (Second Letter, art. 23, p. 163.)

II. When the experiment was made within an hour or two after the layer of mortar had been prepared, and when the mortar was comparatively soft, parallel lines of dislocation appeared on each side of the central portion, but in a position entirely different from those above-described. When first visible their common direction made an angle of 15° or 16° with the axis of the trough, whereas the systems of lines when the mass was more compact, were at an angle of 45° with the axis. To ascertain whether there was any tendency in the particles on opposite sides of one of these new lines of dislocation to slide past each other, I proceeded as in the former case, drawing a short line on the surface perpendicular to one of the dislocations, and continuing the central motion. The short line drawn on the surface immediately became a *broken* line at the point of intersection, proving the existence, in this case, of the sliding motion, of which it will be recollected there was no indication whatever in the experiment previously described, when the mass was more compact.

The lines now spoken of were not visible till the curvature of the transverse loop had become much greater than in the preceding case. On calculating the directions in which the tangential action was a maximum, by the construction previously given (Second Letter, art. 17), it appeared that those directions made an angle with the axis about half as great as that made by the lines of dislocation, when those lines first showed themselves. Now in supposing, as I have in my previous letters, that the lines of greatest tangential action are those along which *separation planes* would be formed, if formed at all, I have tacitly assumed the resistance afforded by the mass to their formation to be the same in all directions. It would seem highly probable, however, that, *cæteris paribus*, this resistance would be greatest in directions perpendicular to the lines of greatest pressure, in which case dislocations would take place in directions deviating *less* than the directions of greatest tangential action from the lines of maximum pressure. Such was the case in the experiments just described. The difference of position in the systems of dislocations, the mass being compact in one case and soft in the other, was owing, I conceive, to the mass yielding most easily in the former case to the direct *normal* tension, and in the latter case to the *tangential* action. It seems probable that such would be the case; and, in fact, in those experiments in which the mass was softest, I doubt whether it was ever brought into a state of *tension* at all, on account of its semifluidity, in which case there would be no tendency at all to form open fissures, as in the first set of experiments, while the formation of separation planes would be facilitated.

In the annexed diagram the stronger lines represent the lines of dislocation in my experiment, when the mass was soft,

Fig. 2.



or the lines along which the particles moved past each other; the finer lines those of greatest tangential action, and the dotted lines those of maximum tension, or those along which the *drag* takes place towards the middle of the glacier. Consequently these latter lines are those along which, according to Prof. Forbes's views, the sliding of one particle past another ought to have taken place. The lines on one longitudinal half only of the mass are represented in the diagram.

In experiments like those of Prof. Forbes, made with plaster of Paris in a semifluid state, the mass at the extreme upper section of the trough, partly from its adhesion to the trough and partly from its fluid condition, remains stationary, and consequently there is a tendency in the particles along each longitudinal line (more especially along the middle of the trough), to separate from each other. In this respect the motion differs from that in my experiments as above described; but with regard to the relative motions of the middle and sides, the cases are precisely similar. In the experiments above described with the soft mortar, the lines of dislocation were straight lines; but when the experiment was varied so as to give an elongation to the central portion (in the manner already described), the lines of dislocation were continued as curved lines across the central portion, forming *elongated loops*, with their convexity turned in a direction opposite to that in which the motion took place. These elongated loops exactly resembled those indicated by the coloured powder spread on the surface of the plaster of Paris in Prof. Forbes's experiments. I may also add, that in repeating the Professor's experiments, previously to making the others, I had observed that when discontinuity in the motion became sensible, as it did near the flanks if the inclination of the trough was made sufficiently great, it took place by a sliding of the particles past each other along the curved loop, as in my experiments. This was shown by a line, originally continuous across the loop, becoming a *broken* line at the point of intersection. The two systems of curves are undoubtedly identical.

It appears, then, that if a mass move in the same manner as a glacier in a canal-shaped valley (independently of local obstacles), two systems of dislocations may be formed, according as the mass is *semifluid*, or possesses at least a certain degree of *solidity*. The curves of dislocation have, in both cases, their convexity turned in the same direction, but in the former case the loop is much more elongated than in the latter. In the more elongated curves the tangent at a point near the side will be inclined to the axis at an angle of about 15°, whereas in the other curves the corresponding angle will be *at least* 45°. Now it is unquestionable that this last system of dislocations affords the accurate type of the transverse fissures of a glacier; but what glacial phænomena correspond to the other system? I challenge any one to point out in a canal-shaped glacier transverse fissures approximating to the elongated loops of that system; nor will the lines of structure suffice. There are, in fact, no phænomena hitherto observed which represent the loops in question, and hence I maintain that *glacial motion cannot be correctly represented by that of a semifluid mass.*

The experimental results I have now described are so clear and determinate, and appear to me to corroborate so distinctly the theoretical results I had previously obtained, that I trust I may be allowed to add this account of them to my previous communications, as the conclusion of the exposition of my views respecting the mechanism of glacial motion.

I am, Gentlemen,
Your obedient Servant,
W. Hopkins.

Cambridge, March 19, 1845.

L. Mr. Hopkins's *Reply to* Dr. Whewell's *Remarks on Glacier Theories.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING completed the exposition of my own views respecting the motion of glaciers, I shall now beg permission to offer a few remarks in reply to the letters addressed to you by Dr. Whewell on the same subject.

With respect to the definitions which your correspondent has given of such terms as *solidity*, *flexibility*, &c., every one, I apprehend, will agree with him, and in so doing must allow the justice of the observation at the close of my first letter, which appears to have called forth these definitions, viz. that Prof. Forbes had used such terms too indiscriminately. He has spoken of having proved the *plasticity* of glacial ice by observations which only proved a small degree of *flexibility*[*],

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[*] In the last paragraph of my First Letter, p. 16.

and he has spoken of the sliding or *gravitation* theory as involving the hypothesis of the *rigidity* of glacial ice*. I have protested against his charging that theory with any such physical absurdity. Whether *solidity* as a relative term may or may not with propriety be opposed to *rigidity* as an absolute one, is a question not involved in anything I have advanced. De Saussure undoubtedly regarded glacial ice as having *solidity*; but to assert that he propounded a theory which tacitly involves his attributing to glacial ice either the property of rigidity, or that of solidity in a degree which would render the motion of a glacier a manifest impossibility under existing conditions, would be a most unjustifiable reflection on the scientific character of that distinguished traveller.

The terms *semifluid*, *plastic* and *solid* are doubtless comparative terms; but we are not justified in using them without discrimination because the properties denoted by them graduate into each other by insensible degrees. Red and green are equally comparative terms; but no one thinks of calling a green field red, or a common brick wall green, because the colours of the spectrum graduate into each other. To call a hard crystalline substance, like glacial ice, semifluid or viscous is scarcely a less departure in my opinion from all propriety of language. It is not, however, on any popular misconception of the nature of glacial ice which may thus arise that I ground the most serious objection to too indiscriminate a use of such terms as those above mentioned. It is that an entirely erroneous conception may thus be formed of the real nature of the mechanical problem which the motion of glaciers presents. All problems which have for their object the determination of the effects of assigned forces on a given mass with reference to its motion, form, internal pressures and tensions, &c. at any proposed time, must necessarily separate themselves, independently of minor subdivision, into two classes; first, that comprising the cases in which the continuity of the mass is preserved during the action of the forces up to the proposed instant; and secondly, that comprehending the cases in which the continuity is destroyed by the action of the forces, as in the instances of the formation of open fissures or internal cavities. These classes are not only mechanically distinct, they are mathematically so; for the complete solution of the first class would not involve that of the second. Now if we are to use the terms *solid* and *semifluid* with any reference to their established popular or mechanical significations, it would appear to me an absurdity to designate as semifluid any mass in which

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* "As I understand the *gravitation* theory, it supposes the mass of the glacier to be a *rigid* one, &c."—*Travels*, p. 362.

the discontinuity characterizing the second class of problems exists, and equally absurd to designate as solid a mass in which on account of the small cohesion between its particles, such discontinuity could not be produced by forces acting continuously upon it, or, if produced by impulsive action, could only be momentarily maintained. I am not asserting that we have thus the means of drawing an accurate line of demarcation between that which is solid and that which is semifluid; nor am I asserting that there are no cases with respect to which there might not be doubt as to which of the above classes they must be referred. Those points are immaterial with reference to any practical application of these considerations to the main subject before us. I only wish to indicate a class of cases in which the term *solid* ought to be used in contradistinction to *semifluid*; and (what is of much more importance) to point out the existence of the two classes of problems above defined, and to remind those interested in the subject, that solutions for one class cannot be applicable to the problems of the other. The case of a glacier is manifestly that of a discontinuous mass, for it is full of dislocations; the case of a semifluid sliding down a sloping canal (the problem proposed by Dr. Whewell as best calculated to elucidate the motion of a glacier) belongs to the other class; and, therefore, I maintain that if the complete solution of this problem were attainable, it could only afford an imperfect and, possibly, a delusive type of glacial motion. The investigations which have for their object the determination of the internal pressures and tensions of a glacier, and the laws according to which the breaches of continuity will take place, setting out with certain well-established data respecting the motion, are likely to prove far more useful in explaining the real mechanism of the motion than any attempt at the mathematical solution of the problem under its most general form. Such investigations I have given in my Second Letter, Phil. Mag., February. The gentleman to whom I am now replying is one of the few interested in the subject who may be expected to grapple with its mathematical difficulties. I invite his criticism on the solutions I have offered.

Dr. Whewell has pointed out the phænomena which would be presented by a *flexible* glacier, in contradistinction to those which would characterize a *viscous* or *plastic* one. He states that, in the first case, "the straight lines originally transverse will become curves, with the convexity downwards. The whole mass will be in a state of tension, produced by this distortion; but the distortion will be limited, and the downward projection of the curved lines also limited, by the flexure which is possible for the texture. If in any part the texture gives way, we shall have fissures nearly perpendicular to the transverse curves. On this supposition of a flexible glacier, we have (1) original transverse straight lines drawn into curves of *limited flexure*; (2) the substance in a state of distortion, from which *hand specimens* would instantly recover; (3) a *homogeneous* texture of such specimens." Again, it is stated that, on the supposition of the glacier being viscous or plastic, filaments will slide past each other in lines directed towards the middle, and that the mass will thus be separated according to such lines. Also, "if the texture of the substance yield to the tension which tends to separate it directly, we shall have fissures in directions perpendicular to the directions just described" (those in which the particles slide past each other). It is then stated, that we should have "(1) original transverse straight lines drawn into curves of *unlimited* flexure; (2) the texture of the substance in a state of equilibrium, so that *hand specimens* have no distortion to recover from; (3) a *banded* texture, the bands being traces of parallel fissures."

It will be seen at once that these statements, made as though they contained only recognised facts or incontrovertible conclusions, do really contain most of the controverted points in the subject. In the first place it is stated, on the supposition of glacial ice being *flexible*, that fissures would be formed nearly perpendicular to the transverse curves above mentioned. In my Second Letter (arts. 4, 7, and 8, p. 148, &c.) I have given three different proofs that such will not be the case. I can only challenge the refutation of any one of them. It is rightly asserted that, previous to fracture, the curves would be of *limited flexure*; but it is not correct that hand specimens would instantly recover from their state of distortion, for that would require the mass to be *perfectly elastic*, a property which it would be absurd to attribute to it. The assertion (3) that the texture would be *homogeneous*, involves the hypothesis that the banded structure can only arise from the formation of planes of discontinuity, applying the assertion to an actual glacier. But this is one of the points at issue, not to be settled assuredly by the mere assertion of any one. Is your correspondent acquainted with all the mysteries of crystallization*?

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* I may here mention a curious effect of crystallization in the structure of hailstones, which proves beyond doubt that the alternations of opake and transparent ice may result from it. In the desolating hailstorm which passed over Cambridge on the 9th of August 1843, many of the hailstones were of the form of rather flat double convex lenses, nearly as large as the palm of the hand, and consisted of white opake ice in the centre, surrounded by a ring of dark transparent ice, with an exterior ring of ice like that in the centre. In some cases there were several dark rings, the cen-

If not, by what process of exhaustion has he proved that the alternation of bands of transparent and opake ice can arise *only* from planes of separation? Again, to explain the formation of such planes, it is asserted, that, because there will be a drag towards the middle, it will be in that direction that filaments of the mass will slide past each other. But I maintain that the fact of the drag being a maximum in that direction is the very reason why the filaments in that same direction will *not* slide past each other. A filament tends to slide past a contiguous one, not because both are dragged, but because they are dragged *unequally*; whereas, in the direction in which the drag is greatest, the filaments are *equally* dragged, simply because the drag in that direction is a maximum. Dr. Whewell appears in the first instance to have objected to Prof. Forbes's mechanical theory of the laminar structure, asserting that the planes of separation ought to be parallel to the sides of the glacier *. This was not accurate, except in the particular case of the absence of transversal pressure (Second Letter, art. 25, p. 165), but was still the kind of approximation to the truth which a first and general view of the problem was almost sure to suggest. What may have been the "simple mechanical views" which led him afterwards to adopt the reasoning of Professor Forbes, I am unable to comprehend. It would have been much more satisfactory if he had stated what those simple views are; and it would have been better, if he would persuade others to adopt them, that he should have given some proof of the fallacy of my investigations on this point, since those investigations (which must have been before him when he wrote his second letter) lead to results so entirely at variance with the conclusions he has adopted. This is what Dr. Whewell or some other advocate of Prof. Forbes's theory must do, or that theory, *as affording a mechanical explanation of the banded structure*, must necessarily fall. It cannot stand unless supported by more conclusive reasoning than that on which alone it has hitherto been made to rest. Such cases as that appealed to by Prof. Forbes in the last Number of your Magazine (p. 206) may afford analogies which may be delusive, or may be ultimately useful in guiding us to the true mechanical solution of the problem before us; but such cases differ too widely from that of a glacier to afford anything more than analogies, even if the problem which each particular case presents were completely solved; and assuredly, an appeal to such cases can

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tral part and the exterior ring being always opake. These successive rings (with the exception of their circular form) exactly resembled the alternate opake and transparent bands in glacial ice. [On this subject, see Mr. Darwin's remarks quoted in p. 354 of the present number.—Edit.]

* See Professor Forbes's Seventh Letter on Glaciers.

never rectify mechanical reasoning which is demonstrably erroneous.

Again, with respect to a plastic or viscous mass, it is asserted that the curves would be of *unlimited flexure*. This would doubtless be true with a sufficient degree of plasticity, but I am perfectly satisfied that there is no great flexure without innumerable fractures in any actual glacier. Along the flanks there are invariably numerous dislocations, and in those parts the relative motions of different points do not take place merely by an extension of the mass, but undoubtedly by the sliding also of one mass past another; and the central portion, though generally less dislocated than the flanks, is still broken into thousands of fragments at particular points of its course, so that the elongation of its loops cannot depend solely on the plasticity of the mass. There is at present a great want of observations for determining the degree of *discontinuity* in the motion of a glacier, without which it is impossible to ascertain how far the elongation of the transverse curves depends on extension or on dislocation. I have in my second letter allowed the probability that considerable, possibly great extension, may take place *in a long period of time*, and have designated this extensibility by the epithet *secular*, in contradistinction to that property by virtue of which glacial ice may admit of extension *in a small time*. Now if the extension which may be produced in glacial ice in many years, before it arrives at the point of breaking, be much greater than that which it would admit of by the application of a force which should bring it to that point in a few hours or a few days, the distinction denoted by the above epithet is not without its due significance*. Thus applied, it does *not* express, as Dr. Whewell asserts, a necessary condition of all extensibility or plasticity, because it only has reference to long periods of time, which is not the case in the common applications of those terms. The observation that "it is *inappropriate* when applied to a process which takes place in a few days," is itself inappropriate, inasmuch as such an application of the term was never contemplated. After all, the recognition or rejection of it is a matter of little consequence, and I cannot but feel surprised that it should have been sufficient to provoke a departure from the rules which strict courtesy, as well as the

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* It might have been more consistent with the analogy which suggested the use of the term *secular*, and perhaps also with the strict propriety of language, if I had applied the epithet to the effect produced, rather than to the property to which the effect is due. I have, however, carefully defined the sense in which the term is used, and it is at least as admissible an epithet to plasticity as *horizontal* and *vertical*, for which we have the high authority of your correspondent.

nature of the subject, would prescribe to us in discussions of this kind. Dr. Whewell has thought proper to reply to my former observations on the secular character of the plasticity of glacial ice by a sarcasm. I think the circumstance deserving of a moment's notice only for the purpose of deprecating any attempt to give a tone to the present controversy inconsistent with the character of a scientific discussion, and with that respect which the parties engaged in it have a right to claim from each other, and which, I have been willing to hope, could not be diminished by a mere difference of opinion on a question of physical science.

One or two other points in Dr. Whewell's letters I would briefly notice. He has remarked that it would be an unworthy return to Prof. Forbes if we should write on glacial theories as if we had derived nothing from him with respect to the plasticity of glacial ice. It might also be added, that it would be equally wrong to write as if we had derived nothing on other points from anybody else. But there was no occasion, in my opinion, for the observation. Everyone recognises the high merits of Prof. Forbes as an observer of glacial phænomena; as a theorist, his claims still remain to be substantiated. As far as the plasticity of glacier ice shall hereafter be proved to be one of the effective causes of the motion of glaciers, directly or indirectly, so far will those claims be at once allowed. The way to establish them is to establish his theory.

Again, Dr. Whewell observes that the question of the state of the lower surface of a glacier, "though not unimportant, offers nothing of novelty; the glacier slides by the constant melting of its lower surface, in virtue of the heat of the subjacent soil. This was distinctly taught by De Saussure." And it may be added, almost as distinctly denied by all those who, in the present day, have attempted to establish some rival theory. Agassiz has contended that the greater part of a glacier is frozen to its bed. Charpentier says, "Nous venons de faire voir que la forte inclinaison du lit d'un grand nombre de glaciers présente une objection bien fondée contre ceux qui prétendent que ces masses avancent en glissant sur leur base par l'effet de leur poids. Mais la pente trop faible du lit de beaucoup d'autres glaciers est également contraire à cette opinion; car pour se convaincre combien il est erroné de croire que leur mouvement soit dû à l'action de la pesanteur, on n'a qu'à se rappeler qu'il existe plusieurs glaciers dont la longueur est de quelque lieues, et qui présentent une pente très faible." With respect to Prof. Forbes's opinion, I have already quoted* a passage from page 132 of his Travels.

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* Note, p. 3 of the January Number of this Magazine.

I may also refer to another passage in page 35, extracted from the Edinburgh Review, in which he considers all sliding, according to De Saussure's theory, as mechanically impossible*.

How far my own investigations on the interior temperature of glaciers, and my experiments on the motion of ice down inclined planes, may have anything of *novelty*, or how far they may have met the objections above-stated, I leave to the judgement of such of your readers as approach the subject in all fairness and candour. I have only brought them forward as subsidiary to De Saussure's theory, and for the purpose of elucidating it, and, I trust, in no spirit calculated to indispose any one to allow to them such subordinate merit as may reasonably attach to them.

There is an observation introduced incidentally in Dr. Whewell's second letter, to the effect that "Prof. Forbes's theory, of course, includes what is true in that of De Saussure." To this somewhat curious appropriation clause I would suggest an amendment, that "De Saussure's theory, of course, includes what is true in Prof. Forbes's." However much I may think this the stronger claim, I am content now merely to place it on record on behalf of De Saussure, and to await the judgement which shall be passed upon it on the evidence of further observation.

Again, your correspondent observes, "Since these views" (I presume those of De Saussure and Professor Forbes) "are thus combined, it may be asked, What is the principal *cause* of glacial motion? Is it gravity? or the melting of the lower surface? or the plasticity of the mass? But it is plain that if this theory" (that, I presume, formed by a combination of the two theories) "be adopted in fact, the question among these forms of expression is quite unimportant." The question is doubtless unimportant to those who only care to know whether glaciers move or not, but to those who would know *how* they move, this statement merges the primary question between the sliding and plastic theories. If it were really of no importance whether it be the state of the lower surface of the mass, or its general plasticity, which renders gravity effective in producing the motion of a glacier, what right would Prof. Forbes have to give to glacial theory a new appellation, with which his own name alone should be hereafter associated, to the exclusion of the name of the distinguished traveller whose steps

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* The objection expressed in the passage referred to would seem to rest partly on the supposition that De Saussure regarded a glacier as a *rigid* mass. I have already protested against an assumption which attributes so glaring an absurdity to that distinguished and philosophic traveller.

he has followed? But the above statement of the question will not be regarded as the correct one by those who wish to understand the real mechanism of glacial motion, and I am far from accusing Prof. Forbes of any thought of doing an injustice to De Saussure. The causes assigned by them for the motion of glaciers are quite distinct. They may, however, be coexistent; the absolute determination of their relative importance must be left to future observation.

I am, Gentlemen,
Your obedient Servant,
Cambridge, March 21, 1845. W. HOPKINS.

LI. *Notices respecting New Books.*

Geological Observations on the Volcanic Islands, visited during the voyage of H.M.S. Beagle, together with some brief notices on the Geology of Australia and the Cape of Good Hope. Being the second part of the Geology of the Voyage of the Beagle, under the command of Captain Fitzroy, *R.N., during the years* 1832 *to* 1836. *By* CHARLES DARWIN, M.A., F.R.S., Vice-President of the Geological Society, and Naturalist to the Expedition. *Published with the approval of the Lords Commissioners of Her Majesty's Treasury.* London, 1844, 8vo. pp. 175. With a Plan of the Island of Ascension, and numerous wood-cuts.

WE are presented in the work now before us with a further portion of the results of the geological researches made by Mr. Darwin, during the expedition in which, with so much zeal and success, he gave his valuable and disinterested services as naturalist.

Almost every branch of natural knowledge has profited from the voyage of the Beagle;—zoology in nearly all its departments, palæontology, physical geography, meteorology, hydrography, mineralogy and geology. And if the present continuation of Mr. Darwin's labours does not present us with those striking and salient points of scientific interest which are within the scope of every lover of nature and of science,—if it does not call up again the extinct gigantic quadrupeds, which in South America, as in so many other parts of the world, almost immediately preceded the existing animals of the same types,—or, if it does not depict the stupendous phænomena attending the simultaneous outburst of volcanic fires along lines thousands of miles in extent;—it supplies the philosophical naturalist with minute information on a multitude of the constituent elements, as it were, of these greater phænomena, essential to their successful investigation, and with the details of physical structure of many volcanic islands and groups hitherto undescribed, and of others respecting which we were before deficient in exact knowledge. Mr. Darwin unites to a bold facility of comprehending the mutual relations of the greater features exhibited by the physical phænomena which have come under his observation, a happy and perspicacious power of recognizing a peculiar order of geological dynamic agents, which, though certainly not the first to notice—for many of them are matters of common and daily observation—he has yet been the first to develope in the before unsuspected greatness of their continual operation, and the first broadly to exhibit as causes—as *veræ causæ*—of geological phænomena. In his works, the agency of the winds, of the chemical elements dissolved in the waves which they impel, of the dust which they raise and diffuse, of the depositions of rivers on the rocks over which they flow, of the matter resulting from the action of the elements on collections of animal excretions and exuviæ, of the earth-worm as a labourer in geological dynamics, of the compound-polyp which, separating lime from sea-water, secretes its carbonate in the form of coral, have been developed and portrayed for the first time in their genuine tenour and importance. With respect to coral reefs and islands, much, indeed, had previously been effected, in part by English naturalists, in part by foreign voyagers; but the true interpretation, we conceive, of many of their observations, has been first given by Mr. Darwin, together with numerous other facts which belong exclusively to himself.

In these geological observations on the volcanic islands visited during the voyage of the Beagle, we have the effects of both the qualities of facile and accurate induction we have just attributed to their author. Unlike some of his former works, they are not adapted to gratify the general reader, or even to please the popular scientist, but they will supply inestimable materials for the elucidation of volcanic geology, in the hands of the profound investigator of nature, to whom they will also be suggestive of some of the deepest questions of geological causation.

We have not before had a suitable opportunity of acknowledging as it deserves the value of Mr. Darwin's contributions to science, and were unwilling to omit such an acknowledgement when directing our readers' attention to a work in which some of them are recorded. We will now proceed to a brief analytical view of these "Observations," and to extract from them certain portions, which we deem peculiarly characteristic of their author's modes of thought and description.

The title of the work states explicitly its contents. The first 130 pages relate almost exclusively to the volcanic islands and their constitution, under the heads of St. Jago, in the Cape de Verde Archipelago, Fernando Noronha, Terceira, Tahiti, Mauritius, Ascension, St. Helena, the Galapagos Archipelago, Trachyte and Basalt, Distribution of Volcanic Isles. The remainder of the volume consists of observations chiefly on the geology of New South Wales, Van Diemen's Land, New Zealand, King George's Sound, and the Cape of Good Hope; and an appendix contains descriptions of fossil shells from a tertiary deposit at St. Jago, of extinct land-shells from St. Helena, and of shells from the palæozoic formation of Van Diemen's Land, all by Mr. G. B. Sowerby; together with descriptions of fossil corals from the same formation by Mr. Lonsdale, who, we are thus happy to see, continues to aid the geologist by his valuable labours in invertebral palæontology.

Of the manner in which the information imparted respecting these localities is distributed, as well as of its nature, the following "contents" of the first and last chapters will give a sufficient notion.

"Chap. I.—*St. Jago, in the Cape de Verde Archipelago.*—Rocks of the lowest series—A calcareous sedimentary deposit, with recent shells, altered by the contact of superincumbent lava, its horizontality and extent—Subsequent volcanic eruptions, associated with calcareous matter in an earthy and fibrous form, and often enclosed within the separate cells of the scoriæ—Ancient and obliterated orifices of eruption of small size—Difficulty of tracing over a bare plain recent streams of lava—Inland hills of more ancient volcanic rock—Decomposed olivine in large masses—Felspathic rocks beneath the upper crystalline basaltic strata—Uniform structure and form of the more ancient volcanic hills—Form of the valleys near the coast—Conglomerate now forming on the sea beach.

"Chap. VII.—New South Wales—Sandstone formation—Imbedded pseudo-fragments of shale—Stratification—Current-cleavage—Great valleys—Van Diemen's Land—Palæozoic formation—Newer formation with volcanic rocks—Travertin with leaves of extinct plants—Elevation of the land—New Zealand—King George's Sound—Superficial ferruginous beds—Superficial calcareous deposit, with casts of branches; its origin from drifted particles of shells and corals; its extent—Cape of Good Hope—Junction of the granite and clay-slate—Sandstone formation."

From the second chapter* we extract the subjoined description of the island called St. Paul's Rocks, including that of a glossy incrustation coating extensive portions of those rocks, which affords an example of the particular class of geological dynamic agents, which Mr. Darwin, as we have observed, has been so happy in recognising in their full importance.

"*St. Paul's Rocks.*—This small island is situated in the Atlantic Ocean, nearly one degree north of the equator, and 540 miles distant from South America, in 29° 15′ west longitude. Its highest point is scarcely fifty feet above the level of the sea; its outline is irregular, and its entire circumference barely three-quarters of a mile. This little point of rock rises abruptly out of the ocean; and except on its western side, soundings were not obtained, even at the short distance of a quarter of a mile from its shore. It is not of volcanic origin; and this circumstance, which is the most remarkable point in its history (as will hereafter be referred to†), properly ought to exclude it from the present volume. It is composed of rocks unlike any which I have met with, and which I cannot characterize by any name, and must therefore describe.

"The simplest, and one of the most abundant kinds, is a very compact, heavy, greenish-black rock, having an angular, irregular fracture, with some points just hard enough to scratch glass, and infusible. This variety passes into others of paler green tints, less hard, but with a more crystalline fracture, and translucent on their edges; and these are fusible into a green

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* A portion of this chapter relates to the effects of steam on the volcanic rocks of Terceira in the Azores, in which the author observed small aggregations of hyalite that had been deposited by the steam. Dr. Daubeny had previously observed the same fact in Hungary, and a similar one in Ischia, as noticed in his work on volcanos, pp. 100, 182.

† This island is afterwards pointed out, p. 125, as one of the very few exceptions to the rule, that all the islands scattered throughout the Atlantic (as well as the Pacific and Indian oceans), are composed either of volcanic or of modern coral-rocks.

enamel. Several other varieties are chiefly characterized, by containing innumerable threads of dark-green serpentine, and by having calcareous matter in their interstices. These rocks have an obscure, concretionary structure, and are full of variously-coloured angular pseudo-fragments. These angular pseudo-fragments consist of the first-described dark green rock, of a brown softer kind, of serpentine, and of a yellowish harsh stone, which, perhaps, is related to serpentine rock. There are other vesicular, calcareo-ferruginous soft stones. There is no distinct stratification, but parts are imperfectly laminated; and the whole abounds with innumerable veins, and vein-like masses, both small and large. Of these vein-like masses, some calcareous ones, which contain minute fragments of shells, are clearly of subsequent origin to the others.

"*A glossy incrustation.*—Extensive portions of these rocks are coated by a layer of a glossy polished substance, with a pearly lustre and of a grayish-white colour; it follows all the inequalities of the surface, to which it is firmly attached. When examined with a lens, it is found to consist of numerous exceedingly thin layers, their aggregate thickness being about the tenth of an inch. It is considerably harder than calcareous spar, but can be scratched with a knife; under the blow-pipe it scales off, decrepitates, slightly blackens, emits a fetid odour, and becomes strongly alkaline: it does not effervesce in acids*. I presume this substance has been deposited by water draining from the birds' dung, with which the rocks are covered. At Ascension, near a cavity in the rocks, which was filled with a laminated mass of infiltrated birds' dung, I found some irregularly-formed, stalactitical masses of apparently the same nature. These masses when broken had an earthy texture, but on their outsides, and especially at their extremities, they were formed of a pearly substance, generally in little globules, like the enamel of teeth, but more translucent, and so hard as just to scratch plate-glass. This substance slightly blackens under the blow-pipe, emits a bad smell, then becomes quite white, swelling a little, and fuses into a dull white enamel; it does not become alkaline; nor does it effervesce in acids. The whole mass had a collapsed appearance, as if in the formation of the hard glossy crust, the whole had shrunk much. At the Abrolhos Islands, on the coast of Brazil, where also there is much birds' dung, I found a great quantity of a brown, arborescent substance adhering to some trap-rock. In its arborescent form, this substance singularly resembles some of the branched species of Nullipora. Under the blow-pipe, it behaves like the specimens from Ascension; but it is less hard and glossy, and the surface has not the shrunk appearance."

In the third chapter, allotted to Ascension, we have an interesting notice of the volcanic bombs which are so numerous in that island, and which are probably connected in their origin with the explosions of aëriform matter that have covered the flanks of Green Mountain and the surrounding country with a mass some hundred feet in thickness, of loose fragments, consisting chiefly of tuffs and pumiceous breccia†. Here the author found bombs, the internal structure of

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* "In my Journal I have described this substance; I then believed that it was an impure phosphate of lime."

† On the northern side of Green Mountain a thin seam of compact oxide of iron extends over a considerable area, lying in the lower part of the mass of fragments. "This seam of compact stone," it is remarked, p. 39, "by intercepting the little rain-water which falls on the island, gives rise to a small dripping spring, first discovered by Dampier. It is the only fresh-water on the island, so that the possibility of its being inhabited has entirely depended on the occurrence of this ferruginous layer."

which presented a symmetrical and very curious appearance, of which he gives a figure and the subjoined description :—

> "The whole interior is coarsely cellular; the cells averaging in diameter about the tenth of an inch; but nearer the outside they gradually decrease in size. This part is succeeded by a well-defined shell of compact lava, having a nearly uniform thickness of about the third of an inch; and the shell is overlaid by a somewhat thicker coating of finely cellular lava (the cells varying from the fiftieth to the hundredth of an inch in diameter), which forms the external surface: the line separating the shell of compact lava from the outer scoriaceous crust is distinctly defined. This structure is very simply explained, if we suppose a mass of viscid scoriaceous matter to be projected with a rapid, rotatory motion through the air; for whilst the external crust, from cooling, became solidified (in the state we now see it), the centrifugal force, by relieving the pressure in the interior parts of the bomb, would allow the heated vapours to expand their cells; but these being driven by the same force against the already-hardened crust, would become, the nearer they were to this part, smaller and smaller or less expanded, until they became packed into a solid, concentric shell. As we know that chips from a grindstone* can be flirted off, when made to revolve with sufficient velocity, we need not doubt that the centrifugal force would have power to modify the structure of a softened bomb, in the manner here supposed. Geologists have remarked, that the external form of a bomb at once bespeaks the history of its aërial course, and we now see that the internal structure can speak, with almost equal plainness, of its rotatory movement."

M. Beudant has described in his Travels in Hungary, some singular little oval balls of obsidian, found strewed on the surface of the ground, their surface regularly marked with concentric ridges and furrows, all of which on the same ball are at right angles to one axis; and he supposes that these were produced by masses of lava, which when soft were shot into the air with a rotatory movement round the same axis. This leads Mr. Darwin to describe and figure a volcanic bomb of obsidian from Australia, which exhibits the external structure described by M. Beudant, and the internal cellular condition of the bombs from Ascension, described in the preceding extract. In this case, from certain parts of the existing structure, one is forced to suppose, as the author remarks, that the bomb burst during its rotatory course, before being quite solidified, and that the axis of rotation then changed.

The following observations relating also to some of the rocks of Ascension are important, as involving the history of a process partly related to that by which siliceous petrifactions have been produced, a subject as yet enveloped in total obscurity :—

> "*Siliceous sinter and jasper.*—The siliceous sinter is either quite white, of little specific gravity, and with a somewhat pearly fracture, passing into pinkish pearly quartz; or it is yellowish-white, with a harsh fracture, and it then contains an earthy powder in small cavities. Both varieties occur, either in large irregular masses in the altered trachyte, or in seams included in broad, vertical, tortuous, irregular veins of a compact, harsh stone of a dull red colour appearing like a sandstone. This stone, however, is only altered trachyte; and a nearly similar variety, but often honeycombed,

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* "Nichol's Architecture of the Heavens."

sometimes adheres to the projecting plate-like veins, described in the last paragraph. The jasper is of an ochre-yellow or red colour; it occurs in large irregular masses, and sometimes in veins, both in the altered trachyte and in an associated mass of scoriaceous basalt. The cells of the scoriaceous basalt are lined or filled with fine, concentric layers of chalcedony, coated and studded with bright-red oxide of iron. In this rock, especially in the rather more compact parts, irregular angular patches of the red jasper are included, the edges of which insensibly blend into the surrounding mass; other patches occur having an intermediate character between perfect jasper and the ferruginous, decomposed basaltic base. In these patches, and likewise in the large vein-like masses of jasper, there occur little rounded cavities, of exactly the same size and form with the air-cells, which in the scoriaceous basalt are filled and lined with layers of chalcedony. Small fragments of the jasper, examined under the microscope, seem to resemble the chalcedony with its colouring matter not separated into layers, but mingled in the siliceous paste, together with some impurities. I can understand these facts,—namely, the blending of the jasper into the semi-decomposed basalt,—its occurrence in angular patches, which clearly do not occupy pre-existing hollows in the rock,—and its containing little vesicles filled with chalcedony, like those in the scoriaceous lava,—only on the supposition that a fluid, probably the same fluid which deposited the chalcedony in the air-cells, removed in those parts where there were no cavities, the ingredients of the basaltic rock, and left in their place silica and iron, and thus produced the jasper. In some specimens of silicified wood, I have observed, that in the same manner as in the basalt, the solid parts were converted into a dark-coloured homogeneous stone, whereas the cavities formed by the larger sap-vessels (which may be compared with the air-vesicles in the basaltic lava) and other irregular hollows, apparently produced by decay, were filled with concentric layers of chalcedony; in this case there can be little doubt that the same fluid deposited the homogeneous base and the chalcedonic layers. After these considerations, I cannot doubt but that the jasper of Ascension may be viewed as a volcanic rock silicified, in precisely the same sense as this term is applied to wood when silicified: we are equally ignorant of the means by which every atom of wood, whilst in a perfect state, is removed and replaced by atoms of silica, as we are of the means by which the constituent parts of a volcanic rock could be thus acted on*. I was led to the careful examination of these rocks, and to the conclusion here given, from having heard the Rev. Professor Henslow express a similar opinion regarding the origin in trap-rocks of many chalcedonies and agates. Siliceous deposits seem to be very general, if not of universal occurrence, in partially decomposed trachytic tuffs†;

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* "Beudant (*Voyage en Hongrie*, tom. iii. p. 502, 504) describes kidney-shaped masses of jasper-opal, which either blend into the surrounding trachytic conglomerate, or are imbedded in it like chalk-flints; and he compares them with the fragments of opalized wood, which are abundant in this same formation. Beudant, however, appears to have viewed the process of their formation, rather as one of simple infiltration, than of molecular exchange; but the presence of a concretion, wholly different from the surrounding matter, if not formed in a pre-existing hollow, clearly seems to me to require, either a molecular or mechanical displacement of the atoms, which occupied the space afterwards filled by it. The jasper-opal of Hungary passes into chalcedony, and therefore in this case, as in that of Ascension, jasper seems to be intimately related in origin with chalcedony."

† "Beudant (*Voyage Min.* tom. iii. p. 507) enumerates cases in Hungary, Germany, Central France, Italy, Greece, and Mexico."

and as these hills, according to the view above given, consist of trachyte softened and altered *in situ*, the presence of free silica in this case may be added as one more instance to the list."

Mr. Darwin truly observes, as above, that we are ignorant of the means by which, in the silicification of wood, every atom of wood, whilst in a perfect state, is removed and replaced by atoms of silica. The recent discovery by Ebelmen, as noticed in our last Volume, p. 397, of the production of silicic æthers, by the substitution of silica in equivalent proportions for the water which in alcohol is united to the oxide of a certain hydro-carbon, induces us to offer a suggestion on this subject. Hydro-carbons, combinations of hydrogen and carbon, in various proportions, are the results, as Liebig has shown by a comprehensive induction from facts previously investigated by other chemists, of eremacausis, and of other changes, which either spontaneously occur or are readily induced, in natural operations, in ligneous and other vegetable matter, and some of them, as the turpentines, are the actual products of vegetable life. Now silicic æther, it is found, is soluble in all proportions, in alcohol and æther, which involves the fact of *the practical indefinite solubility of silica in the oxide and the hydrated oxide of that peculiar hydro-carbon which has been called by modern chemists, ethyl.* But if these products of vegetable matter have this kind of loose affinity for silica, causing them thus to unite with it in large proportions, from which it is again readily separable, is it not in the highest degree probable that other (natural) products of vegetation, other hydro-carbons or their oxides, may have a similar power of dissolving silica, readily to part with it again? If so, we have an indication of the line of research by which we may hope to solve the problem of the silicification of wood. A most interesting and extensive field for experiment and investigation, chemical, botanical, mineralogical and geological, is thus presented, and will be entered upon, we hope, by some competent inquirer. An accomplished man of science would be required to devote himself to the task, necessarily an arduous one, but its successful performance would gain or even enhance the highest reputation.

To return however to Mr. Darwin's researches. In the annexed account of the formation of calcareous rocks on the coast of Ascension, and in that of a peculiar incrustation on certain rocks of that island which immediately succeeds it, we have further examples of that particular class of existing causes in geology which have opened to him so fertile a field of inquiry.

"*Formation of calcareous rocks on the sea-coast.*—On several of the sea-beaches, there are immense accumulations of small, well-rounded particles of shells and corals, of white, yellowish and pink colours, interspersed with a few volcanic particles. At the depth of a few feet, these are found cemented together into stone, of which the softer varieties are used for building; there are other varieties, both coarse and fine-grained, too hard for this purpose: and I saw one mass, divided into even layers half an inch in thickness, which were so compact, that when struck with a hammer they rang like flint. It is believed by the inhabitants that the particles become united in the course of a single year. The union is effected by calcareous matter; and in the most compact varieties, each rounded particle of shell and volcanic rock can be distinctly seen to be enveloped in a husk of pellucid carbonate of lime. Extremely few perfect shells are imbedded in these agglutinated masses; and I have examined even a large fragment under a microscope, without being able to discover the least vestige of striæ or other marks of external form: this shows how long each particle must have been rolled about, before its turn came to be imbedded and cemented *. One of the most compact varieties, when placed in acid, was entirely dissolved, with the exception of some flocculent animal matter; its specific gravity was 2·63. The specific gravity of ordinary limestone varies from 2·6 to 2·75; pure Carrara marble was found by Sir H. De la Beche † to be 2·7. It is remarkable that these rocks of Ascension, formed close to the surface, should be nearly as compact as marble, which has undergone the action of heat and pressure in the plutonic regions.

"The great accumulation of loose calcareous particles, lying on the beach near the Settlement, commences in the month of October, moving towards the S.W., which, as I was informed by Lieut. Evans, is caused by a change in the prevailing direction of the currents. At this period the tidal rocks, at the S.W. end of the beach, where the calcareous sand is accumulating, and round which the currents sweep, become gradually coated with a calcareous incrustation, half an inch in thickness. It is quite white, compact, with some parts slightly spathose, and is firmly attached to the rock. After a short time it gradually disappears, being either redissolved, when the water is less charged with lime, or more probably is mechanically abraded. Lieut. Evans has observed these facts during the six years he has resided at Ascension. The incrustation varies in thickness in different years: in 1831 it was unusually thick. When I was there in July, there was no remnant of the incrustation; but on a point of basalt, from which the quarrymen had lately removed a mass of the calcareous freestone, the incrustation was perfectly preserved. Considering the position of the tidal rocks, and the period at which they become coated, there can be no doubt that the movement and disturbance of the vast accumulation of calcareous particles, many of them being partially agglutinated together, causes the waves of the sea to be so highly charged with carbonate of lime, that they deposit it on the first objects against which they impinge. I have been informed by Lieut. Holland, R.N., that this incrustation is formed on many parts of the coast, on most of which, I believe, there are likewise great masses of comminuted shells."

This is succeeded, as intimated above, by a minute description illustrated by a figure, of a frondescent incrustation of calcareous and animal matter, coating throughout the year the tidal volcanic rocks at Ascension, that project from the beaches composed of broken shells; the origin of this he shows, is due to the solution and subsequent deposition of matter derived from the rounded particles of corals and shells, which form the nuclei of the fronds or discs of which the incrustation is composed, mingled with minute lamellæ of selenite derived from the sea-water; and it is an interesting circumstance, he remarks, thus to find the waves of the ocean sufficiently charged with sulphate of lime, to deposit it on the rocks, against which they dash every tide. When the incrustation now described

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* "The eggs of the turtle being buried by the parent, sometimes become enclosed in the solid rock. Mr. Lyell has given a figure (Principles of Geology, book iii. ch. 17) of some eggs, containing the bones of young turtles, found thus entombed."

† "Researches in Theoretical Geology," p. 12.

has been deposited on the underside of ledges of rocks or in fissures, it appears always to be of a pale pearly-gray colour, whereas, in other situations, it has a dark gray or even a jet-black colour. This induces the author to suppose

> "that an abundance of light is necessary to the development of the dark colour, in the same manner as seems to be the case with the upper and exposed surfaces of the shells of living mollusca, which are always dark, compared with their under surfaces and with the parts habitually covered by the mantle of the animal. In this circumstance,—in the immediate loss of colour and in the odour emitted under the blow-pipe,—in the degree of hardness and translucency of the edges,—and in the beautiful polish of the surface, rivalling when in a fresh state that of the finest Oliva, there is a striking analogy between this inorganic incrustation and the shells of living molluscous animals*. This appears to me to be an interesting physiological fact."

In continuation of the geological history of Ascension are described some singular laminated beds, alternating with and passing into obsidian. These consist,—1, of a pale gray, irregularly and coarsely laminated harsh-feeling rock, resembling clay-slate which has been in contact with a trap-dike, and easily fusing, like the following varieties, into a pale glass; 2, a bluish-gray or pale brown, compact, heavy, apparently homogeneous stone, in which, however, under a lens, a crystalline fracture and even separate minerals can be observed; 3, a stone resembling the last, but streaked with crystalline white hairs of quartz and green transparent needles of diopside; 4, a compact crystalline rock banded with innumerable layers seeming to be composed chiefly of felspar, and containing numerous perfect crystals of glassy felspar placed lengthways, minute black specks of hornblende or augite, and red specks of oxide of iron; 5, a compact heavy rock, not laminated, with an irregular, angular, highly crystalline fracture, abounding with distinct crystals of glassy felspar, and having the crystalline felspathic base mottled with a black mineral, and the entire rock strikingly resembling, in aspect, a primitive greenstone.

We have been thus particular in describing after our author the characters of these rocks, because the knowledge of those characters is essential to the accurate comprehension of the history of their passage into obsidian, which, with its accompaniments, we deem one of the most instructive and suggestive portions of the work, and which, therefore, we will extract entire.

The five varieties of rock just described,

> "with many intermediate ones, pass and repass into each other. As the compact varieties are quite subordinate to the others, the whole may be considered as laminated or striped. The laminæ, to sum up their characteristics, are either quite straight, or slightly tortuous, or convoluted; they are all parallel to each other, and to the intercalating strata of obsidian; they

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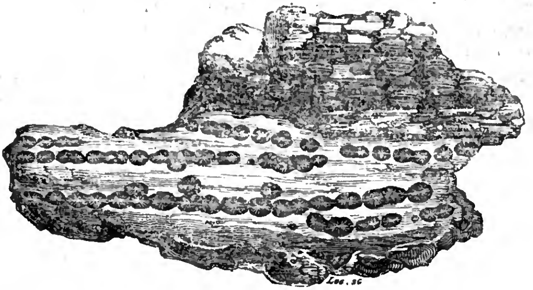
* "In the section descriptive of St. Paul's Rocks, I have described a glossy, pearly substance which coats the rocks, and an allied stalactitical incrustation from Ascension, the crust of which resembles the enamel of teeth, but is hard enough to scratch plate glass. Both these substances contain animal matter, and seem to have been derived from water infiltering through birds' dung."

are generally of extreme thinness; they consist either of an apparently homogeneous, compact rock, striped with different shades of gray and brown colours, or of crystalline felspathic layers in a more or less perfect state of purity, and of different thicknesses, with distinct crystals of glassy felspar placed lengthways, or of very thin layers chiefly composed of minute crystals of quartz and augite, or composed of black and red specks of an augitic mineral and of an oxide of iron, either not crystallized or imperfectly so. After having fully described the obsidian, I shall return to the subject of the lamination of rocks of the trachytic series.

"The passage of the foregoing beds into the strata of glassy obsidian is effected in several ways; first, angulo-nodular masses of obsidian, both large and small, abruptly appear disseminated in a slaty, or in an amorphous, pale-coloured felspathic rock, with a somewhat pearly fracture. Secondly, small irregular nodules of the obsidian, either standing separately, or united into thin layers, seldom more than the tenth of an inch in thickness, alternate repeatedly with very thin layers of a felspathic rock, which is striped with the finest parallel zones of colour, like an agate, and which sometimes passes into the nature of pitchstone; the interstices between the nodules of obsidian are generally filled by soft white matter, resembling pumiceous ashes. Thirdly, the whole substance of the bounding rock suddenly passes into an angulo-concretionary mass of obsidian. Such masses (as well as the small nodules) of obsidian are of a pale green colour, and are generally streaked with different shades of colour, parallel to the laminæ of the surrounding rock; they likewise generally contain minute white sphærulites, of which half is sometimes imbedded in a zone of one shade of colour, and half in a zone of another shade. The obsidian assumes its jet black colour and perfectly conchoidal fracture, only when in large masses; but even in these, on careful examination and on holding the specimens in different lights, I could generally distinguish parallel streaks of different shades of darkness.

"One of the commonest transitional rocks deserves in several respects a further description. It is of a very complicated nature, and consists of numerous thin, slightly tortuous layers of a pale-coloured felspathic stone, often passing into an imperfect pitchstone, alternating with layers formed of numberless little globules of two varieties of obsidian, and of two kinds of sphærulites, imbedded in a soft or in a hard pearly base. The sphærulites are either white and translucent, or dark brown and opake; the former are quite spherical, of small size, and distinctly radiated from their centre. The dark brown sphærulites are less perfectly round, and vary in diameter from the $\frac{1}{20}$ to $\frac{1}{30}$ of an inch; when broken they exhibit towards their centres, which are whitish, an obscure radiating structure; two of them, when united, sometimes have only one central point of radiation; there is occasionally a trace of a hollow or crevice in their centres. They stand either separately, or are united two or three or many together into irregular groups, or more commonly into layers, parallel to the stratification of the mass. This union in many cases is so perfect, that the two sides of the layer thus formed are quite even; and these layers, as they become less brown and opake, cannot be distinguished from the alternating layers of the pale-coloured felspathic stone. The sphærulites, when not united, are generally compressed in the plane of the lamination of the mass; and in this same plane they are often marked internally by zones of different shades of colour, and externally by small ridges and furrows. In the upper part of the accompanying woodcut, the sphærulites with the parallel ridges and furrows are represented on an enlarged scale, but they are not well executed; and in the lower part their usual manner of grouping is shown. In another specimen, a thin layer formed of the brown sphærulites closely

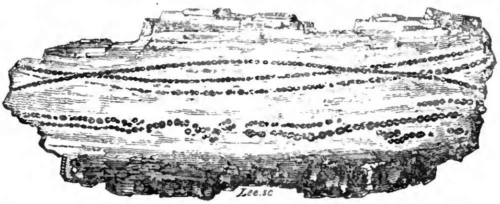
No. 6.



Opake brown sphærulites, drawn on an enlarged scale. The upper ones are externally marked with parallel ridges. The internal radiating structure of the lower ones is much too plainly represented.

united together, intersects, as represented in the woodcut, No. 7, a layer of similar composition; and after running for a short space in a slightly curved line, again intersects it, and likewise a second layer lying a little way beneath that first intersected. The small nodules also of obsidian are sometimes externally marked with ridges and furrows, parallel to the lamination of the mass, but always less plainly than the sphærulites. These obsidian nodules are generally angular, with their edges blunted; they are often impressed with the form of the adjoining sphærulites, than which they are always larger; the separate nodules seldom appear to have drawn each other out by exerting a mutual attractive force. Had I not found in some cases a distinct centre of attraction in these nodules of obsidian, I should have been led to have considered them as residuary matter, left during the formation of the pearlstone, in which they are imbedded, and of the sphærulitic globules.

No. 7.



A layer formed by the union of minute brown sphærulites, intersecting two other similar layers; the whole represented of nearly the natural size.

"The sphærulites and the little nodules of obsidian in these rocks so closely resemble in general form and structure concretions in sedimentary deposits, that one is at once tempted to attribute to them an analogous origin. They resemble ordinary concretions in the following respects,—in their external form,—in the union of two or three, or of several, into an irregular mass, or into an even-sided layer,—in the occasional intersection of one such layer by another, as in the case of chalk-flints,—in the presence of two or three kinds of nodules, often close together, in the same basis,—in their fibrous, radiating structure, with occasional hollows in their centres,—in the coexistence of a laminary, concretionary, and radiating structure, as is so well developed in the concretions of magnesian limestone described by Professor Sedgwick*. Concretions in sedimentary deposits, it is known, are due to

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* "Geological Transactions, vol. iii. part i. p. 37."

the separation from the surrounding mass of the whole or part of some mineral substance, and its aggregation round certain points of attraction. Guided by this fact, I have endeavoured to discover whether obsidian and the sphærulites (to which may be added marekanite and pearlstone, both of them occurring in nodular concretions in the trachytic series) differ in their constituent parts from the minerals generally composing trachytic rocks. It appears from three analyses, that obsidian contains on an average 76 per cent. of silica; from one analysis, that sphærulites contain 79·12; from two, that marekanite contains 79·25; and from two other analyses, that pearlstone contains 75·62 of silica*. Now, the constituent parts of trachyte, as far as they can be distinguished, consist of felspar, containing 65·21 of silica; or of albite, containing 69·09; of hornblende, containing 55·27†, and of oxide of iron: so that the foregoing glassy concretionary substance all contain a larger proportion of silica than that occurring in ordinary felspathic or trachytic rocks. D'Aubuisson‡, also, has remarked on the large proportion of silica compared with alumina, in six analyses of obsidian and pearlstone given in Brongniart's Mineralogy. Hence I conclude, that the foregoing concretions have been formed by a process of aggregation, strictly analogous to that which takes place in aqueous deposits, acting chiefly on the silica, but likewise on some of the other elements of the surrounding mass, and thus producing the different concretionary varieties. From the well-known effects of rapid cooling§ in giving glassiness of texture, it is probably necessary that the entire mass, in cases like that of Ascension, should have cooled at a certain rate; but considering the repeated and complicated alternations, of nodules and thin layers of a glassy texture with other layers quite stony or crystalline, all within the space of a few feet or even inches, it is hardly possible that they could have cooled at different rates, and thus have acquired their different textures.

"The natural sphærulites in these rocks||, very closely resemble those produced in glass when slowly cooled. In some fine specimens of partially devitrified glass, in the possession of Mr. Stokes, the sphærulites are united into straight layers with even sides, parallel to each other, and to one of the outer surfaces, exactly as in the obsidian. These layers sometimes interbranch and form loops; but I did not see any case of actual intersection. They form the passage from the perfectly glassy portions, to those nearly homogeneous and stony, with only an obscure concretionary

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* "The foregoing analyses are taken from Beudant, *Traité de Minéralogie*, tom. ii. p. 113; and one analysis of obsidian, from Phillips's Mineralogy."

† "These analyses are taken from Von Kobell's *Grundzüge der Mineralogie*, 1838."

‡ "*Traité de Géogn.* tom. ii. p. 535."

§ "This is seen in the manufactory of common glass, and in Gregory Watts's experiments on molten trap; also on the natural surfaces of lava-streams, and on the side-walls of dikes."

|| "I do not know whether it is generally known, that bodies having exactly the same appearance as sphærulites sometimes occur in agates. Mr. Robert Brown showed me in an agate, formed within a cavity in a piece of silicified wood, some little specks, which were only just visible to the naked eye: these specks, when placed by him under a lens of high power, presented a beautiful appearance: they were perfectly circular, and consisted of the finest fibres of a brown colour, radiating with great exactness from a common centre. These little radiating stars are occasionally intersected, and portions are quite cut off by the fine, ribbon-like zones of colour in the agate. In the obsidian of Ascension, the halves of a sphærulite often lie in different zones of colour, but they are not cut off by them as in the agate."

structure. In the same specimen, also, sphærulites differing slightly in colour and in structure occur imbedded close together. Considering these facts, it is some confirmation of the view above given of the concretionary origin of the obsidian and natural sphærulites, to find that M. Dartigues*, in his curious paper on this subject, attributes the production of sphærulites in glass to the different ingredients obeying their own laws of attraction and becoming aggregated. He is led to believe that this takes place, from the difficulty in remelting sphærulitic glass, without the whole be first thoroughly pounded and mixed together; and likewise from the fact, that the change takes place most readily in glass composed of many ingredients. In confirmation of M. Dartigues' view, I may remark, that M. Fleuriau de Bellevue † found that the sphærulitic portions of devitrified glass were acted on both by nitric acid and under the blow-pipe, in a different manner from the compact paste in which they were imbedded."

The author next points out how closely the description of the obsidian rocks of Hungary given by Beudant, that by Humboldt of the same formation in Mexico and Peru, and likewise the descriptions given by Scrope, Dolomieu, and D'Aubuisson, of the trachytic regions in the Italian Islands, agree with his own observations at Ascension. Many passages, he remarks, might have been transferred without alteration from the works of those authors, and would have been applicable to this island. He then proceeds to investigate the lamination of volcanic rocks of the trachytic series, and after referring to the frequently zoned structure of obsidian itself, and to the observations of the geologists named above, and the theoretical views on the subject enunciated by some of them, he finds, guided by Professor Forbes's clear description of the zoned structure of glacier ice, that

"far the most probable explanation of the laminated structure of these felspathic rocks appears to be, that they have been stretched whilst slowly flowing onwards in a pasty condition ‡, in precisely the same manner as Professor Forbes believes that the ice of moving glaciers is stretched and fissured. In both cases, the zones may be compared to those in the finest agates; in both, they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical: in the ice, the porous laminæ are rendered distinct by the subsequent congelation of infiltrated water, in the stony felspathic lavas, by subsequent crystalline and concretionary action. The fragment of glassy obsidian in Mr. Stokes' collection, which is zoned with minute air-cells, must strikingly resemble, judging from Professor Forbes' descriptions, a fragment of the zoned ice;

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* "*Journ. de Physique*, tom. lix. (1804), pp. 10, 12."

† "*Ibid.* tom. lx. (1805), p. 418."

‡ "I presume that this is nearly the same explanation which Mr. Scrope had in his mind, when he speaks (Geol. Trans. vol. ii. second series, p. 228) of the ribboned structure of his trachytic rocks having arisen from 'a linear extension of the mass, while in a state of imperfect liquidity, coupled with a concretionary process.'"—[On this subject we may refer for additional illustration to the observations by Professors Forbes and L. Gordon in our last Number, p. 206, on the motion of flowing pitch, as confirming the viscous theory of glaciers. Those who are familiar with the aspect of masses of glass, whether natural or artificial, which have retained their original surface, will immediately recognise its identity with that of flowing pitch, as represented in the lithograph illustrating the observations.]

and if the rate of cooling and nature of the mass had been favourable to its crystallization or to concretionary action, we should here have had the finest parallel zones of different composition and texture. In glaciers, the lines of porous ice and of minute crevices seem to be due to an incipient stretching, caused by the central parts of the frozen stream moving faster than the sides and bottom, which are retarded by friction: hence, in glaciers of certain forms and towards the lower end of most glaciers, the zones become horizontal. May we venture to suppose that in the felspathic lavas with horizontal laminæ we see an analogous case? All geologists who have examined trachytic regions have come to the conclusion, that the lavas of this series have possessed an exceedingly imperfect fluidity; and as it is evident that only matter thus characterized would be subject to become fissured and to be formed into zones of different tensions, in the manner here supposed, we probably see the reason why augitic lavas, which appear generally to have possessed a high degree of fluidity, are not*, like the felspathic lavas, divided into laminæ of different composition and texture. Moreover, in the augitic series, there never appears to be any tendency to concretionary action, which we have seen plays an important part in the lamination of rocks of the trachytic series, or at least in rendering that structure apparent."

In the fourth chapter, relating to St. Helena, we have by far the most definite and explicit account of the structure of that island which has yet been made public, and from which, if studied in conjunction with the views and local descriptions given in Seale's "Geognosy" of the island, the geologist may acquire positive and satisfactory notions of the phænomena it presents, such as will be comparable with those which he possesses of the volcanic regions of Naples or Central France. We would gladly follow our author in this place, but our limits warn us to conclude; and simply referring to the account, p. 87, of certain beds of soft calcareous sandstone in St. Helena, the particles composing which have been drifted into their present position by the wind, and subsequently consolidated by the percolation of rain-water, we pass to the end of the chapter, from which we extract the following instructive considerations on a difficult and much-debated subject.

"*Craters of Elevation.*—There is much resemblance in structure and in geological history between St. Helena, St. Jago, and Mauritius. All three islands are bounded (at least in the parts which I was able to examine) by a ring of basaltic mountains, now much broken but evidently once continuous. These mountains have, or apparently once had, their escarpments steep towards the interior of the island, and their strata dip outwards. I was able to ascertain, only in a few cases, the inclination of the beds; nor was this easy, for the stratification was generally obscure, except when viewed from a distance. I feel, however, little doubt that, according to the researches of M. Elie de Beaumont, their average inclination is greater than that which they could have acquired, considering their thickness and compactness, by flowing down a sloping surface. At St. Helena

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* "Basaltic lavas, and many other rocks, are not unfrequently divided into thick laminæ or plates of the same composition, which are either straight or curved; these being crossed by vertical lines of fissure, sometimes become united into columns. This structure seems related in its origin to that by which many rocks, both igneous and sedimentary, become traversed by parallel systems of fissures."

and St. Jago the basaltic strata rest on older and probably submarine beds, of different composition. At all three islands deluges of more recent lavas have flowed from the centre of the island, towards and between the basaltic mountains; and at St. Helena the central platform has been filled up by them. All three islands have been raised in mass. At Mauritius, the sea, within a late geological period, must have reached to the foot of the basaltic mountains, as it now does at St. Helena; and at St. Jago it is cutting back the intermediate plain towards them. In these three islands, but especially at St. Jago and at Mauritius, when standing on the summit of one of the old basaltic mountains, one looks in vain towards the centre of the island,—the point towards which the strata beneath one's feet and of the mountains on each side rudely converge,—for a source whence these strata could have been erupted; but one sees only a vast hollow platform stretched beneath, or piles of matter of more recent origin.

"These basaltic mountains come, I presume, into the class of Craters of elevation: it is immaterial whether the rings were ever completely formed, for the portions which now exist have so uniform a structure, that, if they do not form fragments of true craters, they cannot be classed with ordinary lines of elevation. With respect to their origin, after having read the works of Mr. Lyell* and of MM. C. Prevost and Virlet, I cannot believe that the great central hollows have been formed by a simple dome-shaped elevation, and the consequent arching of the strata. On the other hand, I have very great difficulty in admitting that these basaltic mountains are merely the basal fragments of great volcanos, of which the summits have either been blown off, or more probably swallowed up by subsidence. These rings are in some instances so immense, as at St. Jago and at Mauritius, and their occurrence is so frequent, that I can hardly persuade myself to adopt this explanation. Moreover, I suspect that the following circumstances, from their frequent concurrence, are someway connected together,—a connexion not implied in either of the above views; namely, first, the broken state of the ring, showing that the now detached portions have been exposed to great denudation, and in some cases, perhaps, rendering it probable that the ring never was entire; secondly, the great amount of matter erupted from the central area, after or during the formation of the ring; and thirdly, the elevation of the district in mass. As far as relates to the inclination of the strata being greater than that which the basal fragments of ordinary volcanos would naturally possess, I can readily believe that this inclination might have been slowly acquired by that amount of elevation of which, according to M. Elie de Beaumont, the numerous upfilled fissures or dikes are the evidence and the measure,—a view equally novel and important, which we owe to the researches of that geologist on Mount Etna.

"A conjecture, including the above circumstances, occurred to me when,—with my mind fully convinced from the phænomena of 1835 in South America†, that the forces which eject matter from volcanic orifices and

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* "Principles of Geology (fifth edition), vol. ii. p. 171."

† "I have given a detailed account of these phænomena, in a paper read before the Geological Society in March 1838. [See Phil. Mag. S. 3. vol. xii. p. 584.] At the instant of time when an immense area was convulsed and a large tract elevated, the districts immediately surrounding several of the great vents in the Cordillera remained quiescent; the subterranean forces being apparently relieved by the eruptions, which then recommenced with great violence. An event of somewhat the same kind, but on an infinitely smaller scale, appears to have taken place, according to Abich (Views of Vesuvius, plates i. and ix.), within the great crater of Vesuvius, where a platform on one side of a fissure was raised in mass twenty feet, whilst on the other side a train of small volcanos burst forth in eruption."

raise continents in mass are identical,—I viewed that part of the coast of St. Jago, where the horizontally upraised, calcareous stratum dips into the sea, directly beneath a cone of subsequently erupted lava. The conjecture is, that during the slow elevation of a volcanic district or island, in the centre of which one or more orifices continue open, and thus relieve the subterranean forces, the borders are elevated more than the central area; and that the portions thus upraised do not slope gently into the central, less elevated area, as does the calcareous stratum under the cone at St. Jago, and as does a large part of the circumference of Iceland*, but that they are separated from it by curved faults. We might expect, from what we see along ordinary faults, that the strata on the upraised side, already dipping outwards from their original formation as lava-streams, would be tilted from the line of fault, and thus have their inclination increased. According to this hypothesis, which I am tempted to extend only to some few cases, it is not probable that the ring would ever be formed quite perfect; and from the elevation being slow, the upraised portions would generally be exposed to much denudation, and hence the ring become broken; we might also expect to find occasional inequalities in the dip of the upraised masses, as is the case at St. Jago. By this hypothesis, the elevation of the districts in mass, and the flowing of deluges of lava from the central platforms, are likewise connected together. On this view, the marginal basaltic mountains of the three foregoing islands might still be considered as forming 'Craters of elevation;' the kind of elevation implied having been slow, and the central hollow or platform having been formed, not by the arching of the surface, but simply by that part having been upraised to a less height."

Other subjects scarcely inferior in interest are discussed in the chapter on the Galapagos Archipelago, as the fluidity of different

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* "It appears, from information communicated to me in the most obliging manner by M. E. Robert, that the circumferential parts of Iceland, which are composed of ancient basaltic strata alternating with tuff, dip inland, thus forming a gigantic saucer. M. Robert found that this was the case, with a few and quite local exceptions, for a space of coast several hundred miles in length. I find this statement corroborated, as far as regards one place, by Mackenzie, in his Travels (p. 377), and in another place by some MS. notes kindly lent me by Dr. Holland. The coast is deeply indented by creeks, at the head of which the land is generally low. M. Robert informs me, that the inwardly dipping strata appear to extend as far as this line, and that their inclination usually corresponds with the slope of the surface, from the high coast-mountains to the low land at the head of these creeks. In the section described by Sir G. Mackenzie the dip is 12°. The interior parts of the island chiefly consist, as far as is known, of recently erupted matter. The great size however of Iceland, equalling the bulkiest part of England, ought perhaps to exclude it from the class of islands we have been considering; but I cannot avoid suspecting that if the coast-mountains, instead of gently sloping into the less elevated central area, had been separated from it by irregularly curved faults, the strata would have been tilted seaward, and a 'crater of elevation,' like that of St. Jago, or that of Mauritius, but of much vaster dimensions, would have been formed. I will only further remark, that the frequent occurrence of extensive lakes at the foot of large volcanos, and the frequent association of volcanic and freshwater strata, seem to indicate that the areas around volcanos are apt to be depressed beneath the general level of the adjoining country, either from having been less elevated, or from the effects of subsidence."

lavas, craters of tuff*, direction of the fissures of eruption; and also in the sixth chapter, on trachyte and basalt†, the origin of trap-dikes (p. 123), and the distribution of volcanic vents. The subject of the last chapter we have already stated in detail, and we will here terminate this article, commending this "Second Part of the Geology of the Voyage of the Beagle" to the attention of every student of geology and philosophical mineralogy, and hoping that the publication of the Third Part, which will be devoted, we find, exclusively to South America, will follow at no distant period.

LII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 261.]

Dec. 13, 1844. THE following communications were read:—

I. Observations of the Moon and Moon-culminating Stars made at Port Essington, on the North Coast of Australia, in longitude 8h 48m 38s east, and latitude 11° 22′ south. By Owen Stanley, Esq., Commander R.N.

The observations give the times of transit of the moon and moon-culminating stars, together with the rates of the clock, and they are corrected for errors of level and azimuth. They extend from June 20 to September 22, of the year 1839. No description of the transit instrument, with which they were made, is given.

II. Announcement of the discovery of Mauvais' Second Comet, in a Letter from M. Mauvais, dated July 9, 1844, addressed to Mr. Baily.

The comet was discovered on the night of Sunday, July 7, and was observed again on July 8. Information of its discovery was immediately forwarded to M. Schumacher. Its positions are given for the times of observation, and the daily motion is deduced from them. The place of the comparison-star is also given.

III. Circular Letter from Professor Encke, dated Berlin, 1844, July 10, announcing the independent discovery of the Comet, known as Mauvais' Second Comet, on the night of July 9, by M. D'Arrest, a Student of the University at Berlin.

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* The structure and constitution of the craters of tuff in the Galapagos, as here described, confirm and illustrate what Dr. Daubeny has said (Description of Volcanos, p. 180) of the island of Ischia.

† Mr. Darwin explains, p. 117–124, the facts that trachytic lavas are in general antecedent to basaltic ones, and that in every trachytic district where obsidian has flowed as lava, it has issued from the upper or highest orifices, by supposing that the crystals of felspar which are the principal constituents of trachyte, being of inferior specific gravity to the other elements of (the stony) lavas, but of greater specific gravity than obsidian, have in the former case risen to the upper part of the molten lava, and in the latter sunk to the lower part. He remarks also, that quartz, on the contrary, would not from its specific gravity tend to sink with the basaltic bases, which would explain the frequent presence and the abundance of that mineral in the lavas of the trachytic series.

M. D'Arrest gave information of his discovery of the comet to Professor Encke, on the night of July 9, and furnished a position of it, from a good series of observations: he gave also its daily motion deduced approximately from a motion of 50′ in right ascension, and 20′ in declination.

IV. Astronomical Observations made at Hudson Observatory, United States, in longitude 5^{h} 25^{m} $39^{s}\cdot5$ west, and latitude 41° 14′ 42″·6 north. By Elias Loomis, Esq. Communicated by Lieutenant-Colonel Sabine.

The observations contained in this paper were principally made with a circular micrometer attached to the equatorial: they consist of—

1. Observations of Encke's Comet on nine days, from March 28 to April 11, 1842.
2. Observations of the Great Comet of 1843 on six days, from March 11 to April 6, 1843.
3. Observations of Mauvais' First Comet on twenty-five days, from July 30 to October 1, 1843.
4. Observations of Faye's Comet on three days, January 23, February 10 and 11, 1844.

For further information on these observations we refer to the Society's Monthly Notices, vol. vi. No. 11.

V. Two Communications from the late Professor Henderson, containing Elements and an Ephemeris of Mauvais' Second Comet.

VI. Observations of Mauvais' Second Comet, made at Starfield, by W. Lassell, Esq. Stated in the Monthly Notices, as already referred to.

VII. Observations of Mauvais' Second Comet and De Vico's Comet, and of an Occultation of a fixed Star by the Moon, made by C. Rumker, Esq. at Hamburg. Communicated by Dr. Lee. Given in the Monthly Notices.

From the observations made at Berlin, on September 5 and September 8, and the observation at Hamburg of September 13, M. Funk, assistant to M. Rumker, has computed the following elements:—

Perihelion passage, Sept. 2, 10^{h} 19^{m} 49^{s}, Greenwich mean solar time.

	°	′	″
Longitude of perihelion	342	56	30
Longitude of ascending node	62	8	44
Inclination	4	5	48
Logarithm of perihelion distance	0·1062216		

Motion direct.

Observation of the Occultation of ι Tauri by the Moon on September 4, 1844.

	h	m	s
Hamburg mean solar time of immersion	13	39	8·5
... ... emersion	14	36	15·3

VIII. Observations of De Vico's Comet, made at Aylesbury by Thomas Dell, Esq. Communicated by Dr. Lee. See Monthly Notices.

IX. Elliptical Elements of De Vico's Comet, with an Ephemeris. By J. R. Hind, Esq.

"These elliptical elements of the new comet discovered at Rome on August 22, have been computed from an observation made at Cambridge on September 15, one at Greenwich on October 3, and a third at Mr. Bishop's Observatory, in the Regent's Park, on October 22. The last position was obtained by comparison of the comet with a star in Prof. Santini's Catalogue, employing a wire micrometer. The observations were corrected for aberration and parallax, and the resulting elements are as follows:—

Epoch, 1844, Sept. 15·55896, mean time at Greenwich.

Mean anomaly $= 2° \, 22' \, 25''{\cdot}53$.

$$\begin{array}{rrrrl} & {}^{\circ} & {}' & {}'' & \\ \pi \ldots & 342 & 32 & 40{\cdot}1 & \left.\begin{array}{l} \\ \\ \end{array}\right\} \text{Apparent equinox,} \\ \Omega \ldots & 63 & 52 & 24{\cdot}1 & \quad \text{October 0.} \\ i \ldots & 2 & 54 & 27{\cdot}14 & \\ \varphi = \sin^{-1} e \ldots & 37 & 59 & 59{\cdot}65 & \end{array}$$

Log. semi-axis major	0·4893706
Log. mean daily sidereal motion in seconds	2·8159507
Period of sidereal revolution	1980 days.

Motion direct.

"An ephemeris computed from the above elements is given in the Monthly Notices.

"The following constants are adapted to this ellipse, and may be used for further calculation,

$$\begin{aligned} x &= [0{\cdot}4813600] \sin (\mathrm{E} + \overset{\circ}{76} \ \overset{\prime}{3} \ \overset{\prime\prime}{34}) - 1{\cdot}810161 \\ y &= [0{\cdot}3543699] \sin (\mathrm{E} + 339 \ 39 \ 41) + 0{\cdot}483894 \\ z &= [0{\cdot}0292555] \sin (\mathrm{E} + 331 \ 32 \ 24) + 0{\cdot}313834 \end{aligned}$$

where E is the comet's eccentric anomaly.

"It is to be remarked, that this comet is in some parts of the orbit liable to considerable perturbations from Jupiter's influence."

Mr. Bishop's Observatory, Regent's Park, J. R. Hind.
November 7, 1844.

X. Observations of De Vico's Comet, made at Ashurst by R. Snow, Esq.

The observations extend from September 24 to October 7. The right ascensions and declinations of the comet are given as deduced from the instrumental readings without any correction. In some instances, when the comet was immediately compared with a star, the position of the star is similarly given. On October 2, the instrumental right ascension and declination of θ Ceti are also given; but, in general, the corrected positions of the comet cannot be determined till the stars of comparison have been observed on the meridian, and their positions furnished.

XI. Observations of Altitude and Azimuth of the Great Comet of 1843, made at St. Helena. By G. Brand, Esq.

The observations were made with an altitude and azimuth instrument by Gilbert, and they extend from March 6 to March 23. On March 6, the comet was compared with the moon only; on every other day the altitudes and azimuths of some of the fundamental stars were observed. The length of the tail is stated to be, on March 6, 42° 55′; on March 7, 37° 23′; and, on March 17, 32° 47′. It is stated also, that on March 8, the colour of the tail had changed, and become more like the rays of the moon: on the 15th it appeared to be much brighter*.

XII. Extract from the Translation of a Letter from Professor Bessel, on the Variations of the Proper Motions of Procyon and Sirius. We have already given this communication in full, in our last Number, p. 256.

LIII. *Intelligence and Miscellaneous Articles.*

RECENT COMETS.

THE Great Comet of 1845, the discovery of which was announced last month, has since been observed in England; though, owing to its distance from the earth, it had become so faint an object as to escape any but a practised eye, aided by the most powerful instruments. Observations made at Ceylon, Bombay, Madras, and other places in the east, have already been received in this country: astronomers are yet expecting the arrival of those made at the Cape of Good Hope, which will doubtlessly furnish data for the satisfactory determination of the elements of its orbit.

A second telescopic comet was discovered at the Observatory of the Collegio Romano, on the evening of February 25. The positions given by Prof. De Vico are as follows:—

	Rome mean time.			Right ascension.			Declination.		
1845.	h	m	s	h	m	s	°	′	″
Feb. 25.	11	15	39·2	11	44	2·2	+55	5	8·5
26.	7	2	57·6	11	38	8·4	+54	46	43·3

It was observed by Prof. Schumacher on March 15, at $9^{h}\ 55^{m}\ 36^{s}$ (Altona mean time). Its right ascension was then $9^{h}\ 29^{m}\ 46^{s}{\cdot}3$, and its declination +37° 5′ 44″.

ON HUMOPINIC ACID. BY M. WÖHLER.

The author gives this name to an acid product analogous to humus, which is produced by the decomposition of narcotina by heat.

When narcotina is heated by an oil-bath in a platina vessel, to some degrees above its melting-point, it begins to become coloured, and eventually assumes a deep reddish-brown; at about 428° it suddenly swells and disengages a very large quantity of ammoniacal gas, nearly pure.

The residue solidifies into a very spongy mass, which, after powdering, is nearly of a pure brown colour; it is essentially composed of humopinic acid.

In order to purify it, the powder must be digested in hydrochloric acid, then washed, and afterwards dissolved in caustic potash, which

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* For other observations of this comet, see the preceding volume, p. 341.—Edit.

acquires a deep yellowish-red colour; this solution is to be supersaturated with hydrochloric acid, which precipitates the humopinic acid, resembling in appearance hydrated peroxide of iron. It is to be washed in alcohol, which leaves a light residue of a dull substance.

The alcoholic solution, when dropped gradually into water, forms with it an emulsion of a deep peach-red colour.

By evaporating the alcohol, the humopinic acid remains in the state of a deep brown mass.

The properties of humopinic acid are, that it is an amorphous substance of a deep brown colour. It melts when heated, and burns with flame, and emits an odour of narcotina. It is insoluble in weak acids and in water; the alcoholic solution is of a deep yellowish-red colour [illegible] alkalies dissolve it, forming a saffron-coloured solution. These solutions give deep brown gelatinous precipitates, with the salts of lead and barytes. When this acid is long boiled in water, it becomes insoluble in ammonia, the alkalies and alcohol merely dissolve it with more difficulty, and always leave a blackish-brown insoluble substance, which resembles *humin* perfectly.

It appears by analysis to be composed of,—

Carbon	64·4
Hydrogen	5·1
Oxygen......	30·5
	100·

Ann. de Ch. et de Phys., October 1844.

ANALYSIS OF THE METEORIC IRON OF GRASSE.

BY LE DUC DE LEVYNES.

The author states the following as the analyses hitherto published, which show the greatest quantity of nickel in various specimens of meteoric iron:—

Iron of Elbogen, by John, 8·75 per cent. Iron of Hradschina, [Agram] by Wehrle, 8·88; by Holger, 11·84. The Louisiana iron, by Shepard, 9·67. The Potosi, by Morren, 9·75. The Siberia, last analysis by Berzelius, 10·73. Iron from the Cape, by Tennant, 10; by Wehrle, 12·27. The iron from Claiborne, North America, by Jackson, 24·70 per cent.

The Grasse iron did not appear to contain either sulphur, silica or cobalt; it yielded,—

Iron	82·63
Nickel	17·37
	100·

It contained also traces of manganese and copper.—*Annales des Mines,* tome v. 161.

ON COTARNINA. BY M. WÖHLER.

This name is given by the author to a new organic base which is produced along with opianic acid; it contains azote and [illegible]rcotina. It occurs in the mother-waters from which opianic acid i[illegible]d, In order to separate it from the sulphate of manganese and undecoposed narcotina, the solution is to be heated to ebullition, and afterwards saturated with carbonate of soda, and the oxide of manganese precipitated is to be separated by filtration; the filtered liquid neutralized with hydrochloric acid is to be precipitated with chloride of platina, which forms a difficultly soluble compound with the new base, so that it may be washed with cold water; these precipitations must not, however, be made in solutions which are very dilute; the precipitation may also be effected by bichloride of mercury.

The author at first employed another method, but it is probable that a portion of the new base was decomposed by the excess of soda; it is, however, mentioned as probably serving to explain the formation of a body hereafter to be noticed.

The mother-waters above mentioned are to be much concentrated, so as to separate a large proportion of the salt of manganese; it is then, after filtration, to be treated with excess of carbonate of soda and evaporated to dryness. The dry mass treated with alcohol dissolves some substances which give it a deep brown colour, and the solution has a very bitter taste. The alcohol is afterwards evaporated, the syrupy residue is to be diluted with water and neutralized with a little hydrochloric acid, and then mixed hot with a solution of chloride of platina; on cooling the double salt is deposited in transparent mammillated masses of a reddish-yellow colour.

The double salt prepared in this manner could not contain any narcotina; excess of peroxide of manganese and sulphuric acid had been employed in the preparation; the ebullition of the mixture had been long continued, and the mixture had besides been treated with soda, so that ammonia did not precipitate any narcotina.

The narcotina [platina ?] was separated from this salt by powdering it and putting it into boiling water, into which sulphuretted hydrogen was passed. The yellow liquid which filters on separating the sulphuret of platina, retains hydrochlorate of cotarnina, and is afterwards treated with hydrate of barytes. It is then to be evaporated to dryness and treated with alcohol, which leaves carbonate of barytes and chloride of barium.

Cotarnina was obtained in the form of a radiated yellow mass. Water and alcohol dissolve it readily, and assume an intense yellow colour. It has a very bitter taste and a slightly alkaline reaction. When heated it fuses and carbonizes, emitting a disagreeable smell. The charcoal is difficult of incineration, but it burns without residue.

When dried after saturation with hydrochloric acid, it forms a yellow amorphous mass, in which crystalline nuclei are sometimes perceptible. The alkalies do not precipitate it from its solution in hydrochloric acid, but it is thrown down by the chlorides of mercury and platina, and by tannic acid.

The double salt of mercury is a bulky precipitate of a pale yellow colour, which soon becomes crystalline. In hot weak solutions no precipitation takes place, but on cooling, the salt is deposited in small pale yellow prisms. When attempts are made to recrystallize it, it appears to undergo some modification as to its composition.

The double salt of platina, formed in cold solutions, very much resembles the double chloride of ammonium and platina. When the solutions are mixed hot, it precipitates on the cooling of the mixture, and forms small transparent yellowish-red mammillated masses. This salt also seems to be modified when it is redissolved, and appears to contain ammonia. When it is boiled with barytes water, the base is decomposed and platina black is formed.

The author gives the following as the composition of cotarnina, reserving, however, a correction which may be required, as he was not quite sure that the substance was absolutely pure.

The platina salt gave,—

	I.	II.	Equivalents.	Calculation.
Chlorine ..	24·07		3	24·7
Carbon	35·04	34·35	26	36·3
Hydrogen ..	3·20	3·46	14	3·2
Azote......	4·44		1	3·3
Oxygen	10·26		5	9·6
Platina	22·97	22·64	1	22·9
	99·98			100·

The mercurial salt yielded—

	Experiment.	Equivalents.	Calculation.
Chlorine....	20·68	3	19·9
Azote	2·52	1	2·6
Carbon		26	
Hydrogen ..		14	
Oxygen		5	
Mercury....	37·95	2	

The composition of cotarnina will then be $= C^{26}\,\underline{H}^{13}\,\underline{N}O^{5}$;

The composition of the double salt of platina $= Pt\,\underline{Cl}^{2} + \underline{H}\,\underline{Cl} +$ cotarnina;

The composition of the double salt of mercury $= 2\underline{Hg}\underline{Cl} + \underline{H}\,\underline{Cl} +$ cotarnina.

The latter contains therefore one equivalent more metallic chloride than the former.—*Ann. de Ch. et de Phys.*, October 1844.

ON APOPHYLLENIC ACID. BY M. WÖHLER.

The author has been unable to produce this substance with certainty, and he has not obtained sufficient to determine its composition; but some of its properties are so characteristic, that he does not hesitate to consider it as a peculiar substance, and to bestow a name upon it.

It contains azote; it crystallizes in two different forms, when anhydrous and when hydrated; when it contains water of crystallization, it forms much-elongated octahedrons with a rhombic base, which cleave very readily parallel to their base, the cleavage face having a pearly lustre, exactly like the crystals of apophyllite, whence the name given to this acid. They readily lose their water, but preserve their form and become opake; this happens below 212°, and the loss amounts to 9 per cent.

Apophyllenic acid dissolves slowly and with difficulty in water; and, except when the solution is made in hot water, the octahedral crystals are obtained slowly. When the acid is made to crystallize by cooling a boiling solution, the crystals occur in long aggregated prisms, the form of which could not be determined; they do not effloresce, and are insoluble in alcohol and water.

The taste of this acid is slightly sour and astringent. When heated, it melts, carbonizes and disengages an oleaginous substance, which, to judge by its odour, must be quinolein; and it possesses a strong alkaline reaction.

Apophyllenic acid appears to form soluble salts with all bases; its ammoniacal salt crystallizes in tables; it is very soluble, and gives no precipitate with the salts of barytes or of lead. It does not immediately form a precipitate with a salt of silver, but after a few minutes have elapsed, small stellated crystals are produced, which soon form small groups of fine white needles. This salt of silver detonates with as much facility as the oxalate, when heated. The small residue of the detonation is composed of a coaly mass, which leaves spongy metallic silver. This acid was obtained during the preparation of cotarnina.—*Ann. de Ch. et de Phys.*, October 1844.

ANALYSIS OF HYPERSTHENE.

M. Damour has recently analysed a specimen of this mineral which belongs to the Ecole des Mines, and which is ticketed as coming from Labrador.

It is a lamellar mass, cleavable only in one direction; colour black, with a brilliant bronze reflection. Powder gray. Fractures readily into small laminæ and very minute needles. Specific gravity 3·392. Scratches glass feebly. Attracted slightly by the magnet. A thin portion heated by the blowpipe fused into a black enamel; gave no water when heated in a tube; fused with carbonate of potash and a little nitre; on platina foil gave a green-coloured substance, indicating manganese. Hydrochloric acid attacks it with difficulty.

It yielded by analysis,—

Silica	51·36
Protoxide of iron	21·27
Manganese	21·31
Lime	3·09
Protoxide of manganese	1·32
Alumina	0·37
	98·72

Annales des Mines, tome v. p. 157.

PROTOXIDE OF TIN AND THE ALKALIES. BY M. FREMY.

It is well known that protoxide of tin dissolves in the alkalies, the solution has, however, been hitherto but little examined. It is generally admitted, according to the experiments of Proust, that a solution of protoxide of tin in potash deposits metallic tin, and retains stannate of potash in solution. According to Berthollet, jun., the solution yields anhydrous protoxide of tin by evaporation.

M. Fremy states that anhydrous oxide of tin is not soluble in a weak solution of potash; and whenever the hydrate in solution can be deprived of its water, it quits the alkali and is precipitated; this curious property induced the author to examine the circumstances under which protoxide of tin can be dehydrated.

In the first place, it was found that the hydrate may lose its water in a solution of potash; if the hydrate be boiled in a very dilute solution of the alkali, it is in a very few minutes converted into small brilliant black crystals of anhydrous oxide of tin; the influence of the alkali in dehydrating the oxide is evident, for when equal quantities of hydrated protoxide of tin are boiled, one in pure and the other in alkaline water, the latter is very rapidly dehydrated; on the contrary, it requires a very long time to dehydrate the oxide in pure water.

Heat is not indispensable to cause an alkali to dehydrate oxide of tin; for if a solution of potash and hydrate of tin be put under the receiver of the air-pump, the oxide becomes anhydrous as soon as the potash is sufficiently concentrated.

These experiments explain the decomposition which a solution of oxide of tin in potash undergoes; this solution cannot be made without an excess of alkali, and as long as the solution is weak the oxide remains dissolved; but when the liquor is concentrated, the oxide is dehydrated, and precipitates in that state. On this account it is impossible to evaporate, even *in vacuo*, a solution of protoxide of tin in potash, without observing that at a certain point of the concentration the liquor deposits anhydrous oxide of tin. These facts agree with those observed by Berthollet, jun.; they show that protoxide of tin dissolved in a dilute solution of potash may be dehydrated and precipitated anhydrous.

In order to repeat Proust's experiment, protoxide of tin was dissolved in potash, and instead of evaporating the solution slowly, as in the preceding case, it was submitted to rapid evaporation; the protoxide of tin was then decomposed, the liquor deposited metallic tin and held stannate of potash in solution; it is therefore evident that the products of the decomposition vary with the concentration of the alkali; when the liquor is dilute and weakly alkaline, anhydrous protoxide of tin is precipitated, and it is only when the alkali is concentrated, that the protoxide is converted into tin and stannic acid.

These experiments show that dehydration takes place more readily in a hot solution of potash than a cold one. Must it then be admitted that a solution of potash, even when dilute, has an affinity for water and can take it from an hydrated oxide? or may not these effects be explained by the interesting experiments of M. Mitscherlich on ætherification, which prove that a certain quantity of sulphuric acid may convert alcohol into æther almost indefinitely? The author thinks it is difficult at present to explain the fact in a satisfactory manner; but he found that solution of sulphate of soda, chloride of potassium, chloride of sodium and hydrochlorate of ammonia, were capable when heated of dehydrating the oxide of tin rapidly. This action of the chlorides is the more remarkable, because they crystallize in the anhydrous state.

Protoxide of tin, when dehydrated under the circumstances described, does not always appear in the same state; it is sometimes black and crystalline, at other times brown, and often as red as minium.—*Ann. de Ch. et de Phys.*, December 1844.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1845.

Chiswick.—February 1. Sharp frost: snow-showers at night. 2. Clear and frosty: cloudy: clear and frosty. 3. Hazy and drizzly. 4. Overcast: cloudy: frosty. 5. Overcast: fine: clear. 6. Clear throughout: sharp frost at night. 7. Frosty: overcast: clear and frosty. 8. Overcast: frosty. 9. Clear and frosty: fine: slight frost. 10. Overcast: snowing. 11. Frosty: most intense frost at night, the lowest being 35° below the freezing-point. 12. Intense frost, only 4° above zero at 8 A.M.: dry air and frosty: bright sun: severe frost at night. 13. Overcast: snowing: sleet and rain. 14. Thawing: clear and fine: overcast. 15. Clear and frosty. 16. Fine: densely overcast: frosty. 17. Foggy: fine: clear and frosty. 18. Foggy: hazy clouds. 19. Overcast: clear: frosty. 20, 21. Clear and frosty throughout. 22. Snow-showers: foggy. 23. Snow: rain. 24. Cloudy and cold. 25. Frosty: fine: rain. 26. Clear: fine: densely overcast. 27, 28. Overcast.—Mean temperature of the month 8° below the average.

Boston.—Feb. 1. Cloudy: snow A.M. and P.M. 2. Fine. 3. Cloudy: rain A.M. and P.M. 4. Cloudy. 5. Fine: stormy night. 6. Windy. 7. Fine. 8. Cloudy. 9. Fine. 10. Snow. 11, 12. Fine. 13. Cloudy: rain and snow A.M. 14, 15. Fine. 16. Cloudy: rain early A.M. 17, 18. Fine. 19. Cloudy. 20. Fine. 21. Foggy. 22. Cloudy. 23. Snow: rain P.M. 24. Cloudy. 25. Fine: rain P.M. 26. Fine. 27. Cloudy. 28. Fine.

The above you will find a very cold month; I think you will find nothing like it since February 1838.

Sandwick Manse, Orkney.—Feb. 1. Frost: cloudy. 2. Rain. 3. Bright: cloudy. 4. Cloudy: sleet-showers. 5. Snow-showers. 6. Snow-showers: snow-drift. 7. Snow-showers. 8. Snow-showers: clear. 9, 10. Thaw: cloudy: drops. 11. Clear: showers. 12. Cloudy: showers. 13. Showers. 14. Snow: cloudy. 15. Showers: cloudy. 16. Fog: cloudy. 17. Bright: clear: fine. 18, 19. Fine: cloudy. 20. Showers. 21. Showers: clear: fine. 22. Showers: snow-showers. 23. Showers. 24. Cloudy: clear: aurora. 25. Clear: cloudy. 26. Cloudy: clear: frost. 27. Bright: cloudy. 28. Cloudy: clear.

Applegarth Manse, Dumfries-shire.—Feb. 1. Frost. 2. Severe frost: rain P.M. 3. Fine thaw. 4. Frost: thaw P.M. 5. Thaw A.M.: frost P.M. 6. Frost: clear and fine. 7, 8. Hard frost. 9. Snow. 10. Slight thaw. 11. Snow-showers. 12. Frost A.M.: rain P.M. 13. Heavy rain. 14. Frost: shower: snow and hail. 15. Frost: thaw P.M. 16, 17. Fine. 18. Fine spring day. 19. Frost: fine. 20. Slight frost: fine. 21. Fine: no frost. 22. Slight frost: snow. 23. Slight frost: fine. 24. Frost: rain P.M. 25. Fresh: fine. 26. Frost A.M.: fine. 27. Frost. 28. Frost: fine.

Mean temperature of the month	34°·5
Mean temperature of Feb. 1844	32 ·9
Mean temperature of Feb. for twenty years	36 ·0

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Feb.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	29·824	29·747	29·57	29·75	29·88	29·92	29·88	38	27	30	36	20	35	25½	n.	calm	nw.	s.	......	......	......	......
2.	29·984	29·362	29·71	29·89	29·80	29·67	29·70	40	21	31	37	10½	42	46	ne.	calm	nne.	w.	......	......	......	·70
3.	29·972	29·873	29·56	29·76	30·00	29·94	30·12	43	34	36	47	36	40	37	nw.	calm	nw.	nw.	·10	......	......	·18
4.	30·164	30·200	29·83	30·09	29·94	29·95	29·77	43	25	35	41	26	45	42	n.	calm	sw.	w.	......	·25	......	......
5.	30·017	29·871	29·65	29·87	29·70	29·73	29·77	48	31	36	44	36	32	32	nw.	calm	w.	n.	......	......	......	·28
● 6.	29·940	29·851	29·50	29·83	29·96	30·02	30·11	43	19	32	37	29	27	27	n.	nw.	wnw.	n.	......	......	......	......
7.	29·959	29·950	29·67	29·93	29·93	30·09	30·08	38	17	27·5	34	21	29	24½	n.	calm	ne.	s.	......	......	......	......
8.	30·065	30·053	29·80	29·94	29·90	30·06	29·95	35	18	26·5	44	24	24	30	ne.	calm	wnw.	se.	......	......	......	·13
9.	30·093	29·934	29·79	29·75	29·58	29·67	29·54	36	27	29	32	28	32½	35½	se.	calm	e.	sse.	......	......	......	......
10.	29·757	29·649	29·53	29·50	29·47	29·50	29·61	35	22	29	37	30	38	35½	se.	s.	e.	sse.	......	......	0·82	·08
11.	30·216	29·880	29·73	29·82	30·05	29·88	30·05	34	03	28	33	28	39	39	e.	se.	e.	sw.	......	......	......	·11
12.	30·409	30·400	30·11	30·12	29·99	30·05	29·76	32	12	23	35½	20	39½	39½	sw.	calm	e.	sse.	......	......	......	·10
13.	30·238	29·755	29·87	29·67	29·50	29·38	29·25	38	32	30	45	33	47½	41	sw.	sw.	sw.	w.	·30	......	......	·38
☽ 14.	29·796	29·696	29·40	29·51	29·75	29·56	29·80	45	27	35	43½	30	33½	36	sw.	calm	nw.	nw.	......	·25	......	·08
15.	29·966	29·876	29·62	29·82	29·80	29·80	29·87	45	33	33·5	43	28	39	36½	nw.	calm	w.	se.	·01	......	......	·04
16.	29·904	29·851	29·58	29·76	29·76	29·70	29·72	41	22	34	45	34½	40	41	ne.	calm	nne.	sse.	......	·11	......	......
17.	29·959	29·944	29·64	29·73	29·80	29·71	29·80	44	23	30	44	35½	43	41½	e.	calm	sse.	......	......	......	......	......
18.	30·021	30·007	29·72	29·84	29·95	29·87	29·98	41	21	30	45	34½	41½	42½	se.	calm	sw.	......	......	......	......	......
19.	30·139	30·063	29·79	29·98	29·97	29·99	29·91	35	16	34	40	25	39½	41	e.	calm	s.	ssw.	......	......	0·00	......
20.	30·164	30·029	29·86	29·92	29·85	29·78	29·79	37	22	27·5	45	31	41	39	se.	calm	ssw.	wnw.	......	......	......	·25
21.	29·946	29·757	29·60	29·74	29·53	29·73	29·63	44	20	30	46	35	38	34	se.	calm	s.	e.	......	......	......	·09
○22.	29·466	29·437	29·19	29·30	29·30	29·56	29·56	37	31	31	40	31	38	35	e.	calm	e.	e.	·10	......	......	......
23.	29·467	29·377	29·63	29·29	29·30	29·44	29·42	44	31	32·5	38½	30	38	36	sw.	alm	ne.	ne.	·16	......	......	·05
24.	30·018	29·665	29·29	29·49	29·85	29·77	30·00	48	28	33	44	28½	33	30	nw.	w.	n.	......	......	·05	......	......
25.	30·116	29·904	29·82	29·90	29·58	30·01	29·70	49	37	28	42	23½	29	35	s.	calm	nw.	se.	·26	......	......	......
26.	29·768	29·624	29·24	29·40	29·66	29·65	29·86	52	35	40	49	36½	29½	35	nw.	calm	ne.	n.	......	·24	......	·09
27.	29·945	29·918	29·59	29·83	29·84	30·02	30·02	50	35	35.5	46	33	36½	36	sw.	calm	ne.	e.	......	......	0·30	......
28.	29·933	29·917	29·67	29·80	29·80	29·98	29·88	45	29	34	40	32	36½	35½	e.	s.	ene.	se.	......	......	......	......
Mean.	29·973	29·861	29·64	29·750	29·765	29·801	29·804	41·43	24·71	31·4	41·1	28·9	36·66	36·37					0·93	0·90	1·12	2·56

THE

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[THIRD SERIES.]

MAY 1845.

LIV. *On the Changes of Temperature produced by the Rarefaction and Condensation of Air.* *By* J. P. JOULE, *Esq.*[*]

IN a paper[†] which was read before the Chemical Section of the British Association at Cork, I applied Dr. Faraday's fine discovery of magneto-electricity in order to establish definite relations between heat and the ordinary forms of mechanical power. In that paper it was demonstrated experimentally that the mechanical power exerted in turning a magneto-electrical machine is *converted into the heat* evolved by the passage of the currents of induction through its coils; and, on the other hand, that the motive power of the electro-magnetic engine is obtained at the expense of the heat due to the chemical reactions of the battery by which it is worked. I hope, at a future period, to be able to communicate some new and very delicate experiments, in order to ascertain the mechanical equivalent of heat with the accuracy which its importance to physical science demands. My present object is to relate an investigation in which I believe I have succeeded in successfully applying the principles before maintained to the changes of temperature arising from the alteration of the density of gaseous bodies,—an inquiry of great interest in a practical as well as theoretical point of view, owing to its bearing upon the theory of the steam-engine.

Dr. Cullen and Dr. Darwin appear to have been the first who observed that the temperature of air is decreased by rarefaction and increased by condensation. Other philosophers have subsequently directed their attention to the subject. Dalton was, however, the first who succeeded in measuring the change of temperature with some degree of accuracy. By the employment of an exceedingly ingenious contrivance, that illustrious philosopher ascertained that about 50° of heat are

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[*] Communicated by the Author.

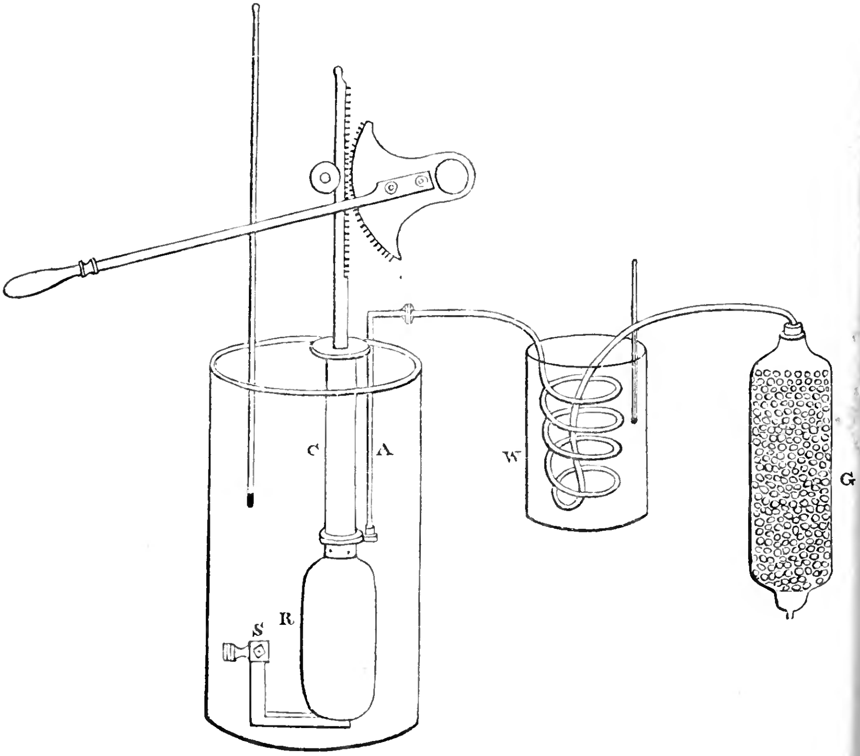
[†] Phil. Mag. S. 3. vol. xxiii. pp. 263, 347, 435.

evolved when air is compressed to one-half of its original bulk; and that, on the other hand, 50° are absorbed by a corresponding rarefaction*.

There is every reason for believing that Dalton's results are very near the truth, especially as they have been exactly confirmed by the experiments of Dr. Ure with the thermometer of Breguet. But our knowledge of the specific heat of elastic fluids is of such an uncertain character, that we should not be justified in attempting to deduce from them the absolute quantity of heat evolved or absorbed. I have succeeded in removing this difficulty by immersing my condensing pump and receiver into a large quantity of water, so as to transfer the calorific effect to a body which is universally received as the standard of capacity.

My apparatus will be understood on inspecting fig. 1. C

Fig. 1.



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* Memoirs of the Literary and Philosophical Society of Manchester, vol. v. part 2. p. 521–525.

represents the condensing pump, consisting of a cylinder of gun-metal, and of a piston fitted with a plug of oiled leather, which works easily, yet tightly, through a stroke of 8 inches. The cylinder is $10\frac{1}{2}$ inches long, $1\frac{3}{8}$ inch in interior diameter, and $\frac{1}{4}$ of an inch in thickness of metal. The pipe A, for the admission of air, is fitted to the lower part of the cylinder; at the bottom of this pipe there is a conical valve, constructed of horn, opening downwards. A copper receiver, R, which is 12 inches long, $4\frac{1}{2}$ inches in exterior diameter, $\frac{1}{4}$ of an inch thick, and has a capacity of $136\frac{1}{2}$ cubic inches, may be screwed upon the pump at pleasure. This receiver is furnished with a conical valve of horn opening downwards, and, at the bottom, with a piece of brass, B, along the centre of which there is a bore of $\frac{1}{8}$th of an inch diameter. There is a stop-cock at S which I shall describe more particularly in the sequel.

Anticipating that the changes of temperature of the large quantity of water which was necessary in order to surround the receiver and pump would be very minute, I was at great pains in providing a thermometer of extreme sensibility and very great accuracy. A tube of narrow bore having been selected, a column of mercury, 1 inch long, was introduced, and gradually advanced in such a manner that the end of the column in one position coincided with the beginning of the column in the next. In each position the length of the column was ascertained to the $\frac{1}{4000}$th part of an inch, by means of an instrument invented for the purpose by Mr. Dancer*. Afterwards the tube was covered with a film of bees'-wax, and each of the previously measured spaces was divided into twenty equal parts by means of a steel point carried by the dividing instrument; it was then etched by exposure to the vapour of fluoric acid. The scale thus formed was entirely arbitrary, and as it only extended between 30° and 90°, it was necessary to compare the thermometer with another, constructed in the same manner, but furnished with a scale including the boiling as well as the freezing point. When this was done, it was found that ten divisions of the sensible thermometer (occupying about $\frac{1}{2}$ an inch) were nearly equal to the degree of Fahrenheit; therefore, since by practice I can easily estimate with the naked eye $\frac{1}{20}$th of each of these divisions, I could with this instrument determine temperatures to the $\frac{1}{200}$th part of a degree. The scale being arbitrary, the indications of the

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* Of the firm of Abraham and Dancer, Cross Street, Manchester. I have great pleasure in acknowledging here the skill displayed by this gentleman in the construction of the different parts of my apparatus; to it I must, in a great measure, attribute whatever success has attended the experiments detailed in this paper.

thermometer had to be reduced in every instance, a circumstance which accounts for my having given the temperatures in the tables to three places of decimals.

It was important to employ, for the purpose of containing the water, a vessel as impermeable to heat as possible. With this view, two jars of tinned iron, one of them every way an inch smaller than the other, having been provided, the smaller jar was placed within the larger one, and the interstice between the two was closed hermetically. By this means a stratum of air of nearly the same temperature as the water was kept in contact with the sides and bottom of the inner jar. The jars used in the other experiments which I shall bring forward were constructed in a similar manner. Among other precautions to ensure accuracy, proper screens were placed between the vessels of water and the experimenter.

My first experiments were conducted in the following manner:—The pump and copper receiver were immersed into 45 lbs. 3 ozs. of water, into which the very sensible thermometer above described was then placed; whilst two other thermometers were employed in order to ascertain the temperature of the room and that of the water contained by the vessel W. Having stirred the water thoroughly, its temperature was carefully read off. The pump was then worked at a moderate degree of speed until about twenty-two atmospheres of air, dried by being passed through the vessel G full of small pieces of chloride of calcium, were compressed into the copper receiver. After this operation (which occupied from fifteen to twenty minutes) the water was stirred for five minutes so as to diffuse the heat equably through every part, and then its temperature was again read off.

The increase of temperature thus observed was owing partly to the condensation of the air, and partly also to the friction of the pump and the motion of the water during the process of stirring. To estimate the value of the latter sources of heat, the air-pipe A was closed, and the pump was worked at the same velocity and for the same time as before, and the water was afterwards stirred precisely as in the first instance. The consequent increase of temperature indicated heat due to friction, &c.

The jar was now removed, and the receiver having been immersed into a pneumatic trough, the quantity of air which had been compressed into it was measured in the usual manner, and then corrected for the force of vapour, &c. The result, added to 136·5 cubic inches, the quantity contained by the receiver at first, gave the whole quantity of compressed air.

TABLE I.

Source of heat.	Number of strokes of pump.	Barometrical pressure.	Quantity of air compressed in cubic inches.	Temp. of the air admitted.	Mean temp. of the room.	Difference.	Temp. of water.		Heat gained.
							Before expt.	After expt.	
Condensation, &c.	300	30·06	3047	56°·2	57°·5	2°·224—	54°·930	55°·622	0°·692
Friction, &c.	300	......	....	......	57·5	1·685—	55·652	55·979	0·327
Condensation, &c.	300	30·07	2924	54·8	53·5	0·817+	53·970	54·664	0 694
Friction, &c.	300	......	....	......	54·5	0·358+	54·675	55·042	0·367
Condensation, &c.	300	30·24	2870	53·7	52·5	0·380+	52·562	53·197	0·635
Friction, &c.	300	......	....	......	52·6	0·760+	53·197	53·524	0·327
Condensation, &c.	300	30·07	2939	58·8	57·5	1·794—	55·359	56·053	0·694
Friction, &c.	300	......	....	......	57·75	1·536—	56·053	56·375	0·322
Condensation, &c.	300	30·34	2924	55·7	53·5	2·184+	55·409	55·959	0·550
Friction, &c.	300	......	....	......	53·75	2·316+	55·962	56·170	0·208
Condensation, &c.	300	30·40	3033	58·1	60·0	0·174+	59·876	60·472	0·596
Friction, &c.	300	......	....	......	60·4	0·196+	60 478	60·713	0·235
Condensation mean	300	30·20	2956	56·2	......	0·078—	....	....	0·643
Friction, &c. mean	300	......	....	......	......	0·068+	....	....	0·297
Corrected result..	......	30·20	2956	......	......	....	....	....	0·344

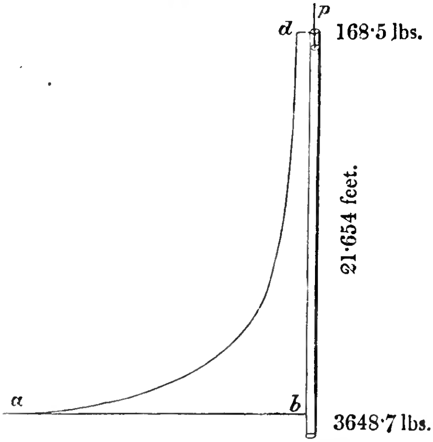
The result given in the above Table is the difference between the effects of condensation and friction alone, corrected for the slight superiority of the cooling influence of the atmosphere in the experiments on friction. We must now, however, proceed to apply a further correction, on account of the circumstance that the friction of the piston was considerably greater during the condensing experiments than during the experiments to ascertain the effect of friction. In the latter case the piston worked with a vacuum beneath it, whilst in the former the leather was pressed to the sides of the pump by a force of condensed air, averaging 32 lbs. per square inch. I endeavoured to estimate the difference between the friction in the two cases, by removing the valve of the receiver and working the pump with about 32 lbs. per square inch pressure below it. These experiments, alternated with others in which a vacuum was beneath the piston, showed that the heat given out in the two cases was, as nearly as possible, in the ratio of six to five. When the correction indicated in this manner has been applied to 0°·297 (see Table) and the result subtracted from 0°·643, we obtain 0°·285 as the effect of compressing 2956 cubic inches of dry air at a pressure of 30·2 inches of mercury, into the space of 136·5 cubic inches.

This heat was distributed through 45 lbs. 3 ozs. of water,

$20\frac{1}{2}$ lbs. of brass and copper, and 6 lbs. of tinned iron. It was therefore equivalent to 13°·628 per lb. avoirdupois of water.

The force necessary to effect the above condensation may be easily deduced from the law of Boyle and Mariotte, which has been proved by the French academicians to hold good as far as the twenty-fifth atmosphere of pressure. Let fig. 2 represent a cylinder closed at one end, the length of which is 21·654 feet, and the sectional area 11·376 square inches. Then one foot of it will have exactly the same capacity as the copper receiver used in the experiments, and its whole capacity will be 2956 cubic inches. It is evident, therefore, that the force used in pumping (considered to be without friction) was exactly equal to that which would push the piston *p* to the distance of a foot from the bottom of the cylinder. Excluding exterior atmospheric pressure, the force upon the piston, when at the top of the cylinder, will be 168·5 lbs., the weight of a column ot mercury 30·2 inches long and of 11·376 square inches section; and at a foot from the bottom it will be 21·654 times as much, or 3648·7 lbs. The hyperbolic area, *a b c d*, will therefore represent the force employed in the condensation, including the assistance of the atmospheric pressure. Applying the formula for hyperbolic spaces, we have,

Fig. 2.



$$s = 3648{\cdot}7 \times 2{\cdot}302585 \times \log 21{\cdot}654 = 11220{\cdot}2.$$

The force expended in condensation was therefore equivalent to that which can raise 11220·2 lbs. to the perpendicular height of one foot.

Comparing this with the quantity of heat evolved, we have $\frac{11220{\cdot}2}{13^{\circ}{\cdot}628} = \frac{823}{1^{\circ}}$. So that a mechanical force capable of raising 823 lbs. to the height of one foot must be applied in the condensation of air, in order to increase the temperature of a pound of water by one degree of Fahrenheit's scale.

The following Table contains the results of experiments similar to the last, except in the extent to which the compression of the air was carried.

Table II.

Source of heat.	Number of strokes of pump.	Barometrical pressure.	Quantity of air compressed in cubic inches.	Temp. of the air admitted.	Mean temp. of the room.	Difference.	Temp. of water.		Heat gained.
							Before expt.	After expt.	
Condensation, &c.	120	30·40	1410	54°·0	54°·2	0°·010+	54°·099	54°·322	0°·223
Friction, &c.	120	......	....	......	54·6	0 224−	54·332	54·421	0·089
Condensation, &c.	120	30 50	1467	56·6	56·5	0·308+	56·693	56·923	0·230
Friction, &c.	120	......	....	......	56·7	0·281+	56·926	57·036	0·110
Condensation, &c.	120	30·50	1440	62·6	63·6	1·763−	61·703	61·971	0·268
Friction, &c.	120	......	....	......	64·0	1·960−	61·976	62·105	0·129
Condensation, &c.	120	30·57	1442	59·0	58·4	0·400+	58·680	58·921	0 241
Friction, &c.	120	......	....	......	58·5	0·477+	58·921	59·033	0·112
Condensation, &c.	120	29·94	1405	55·2	57·0	1·566−	55·310	55·558	0·248
Friction, &c.	120	......	....	......	57·2	1·573−	55·563	55·692	0·129
Condensation mean	120	30·38	1433	57 5	......	0·522−	....	....	0·242
Friction mean ...	120	......	....	......	......	0·600−	....	....	0·114
Corrected result...	......	30·38	1433	......	......	....	....	....	0·128

After applying the proper correction for the increase of friction during condensation, and reducing the result, as before, to the capacity of a lb. of water, I find 5°·26 to be the mean quantity of heat evolved by compression of air in the above series of experiments.

The mechanical force spent in the condensation is represented in this instance by

$$s = 1779{\cdot}3 \times 2{\cdot}302585 \times \log 10{\cdot}498 = 4183{\cdot}46.$$

Hence the equivalent of a degree of heat per lb. of water, as determined by the above series, is 795 lbs. raised to the height of one foot.

The mechanical equivalents of heat derived from the foregoing experiments were so near 838 lbs.*, the result of magnetical experiments in which "latent heat" could not be suspected to interfere in any way, as to convince me that the heat evolved was simply the manifestation, in another form, of the mechanical power expended in the act of condensation: I was still further confirmed in this view of the subject by the following experiments.

I provided another copper receiver (E, fig. 3) which had a capacity of 134 cubic inches. Like the former receiver, to

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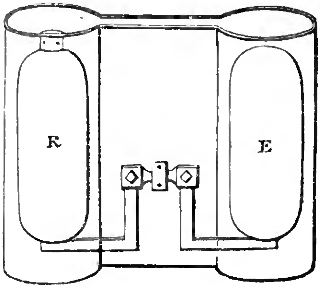
* Phil. Mag. S. 3. vol. xxiii. p. 441.

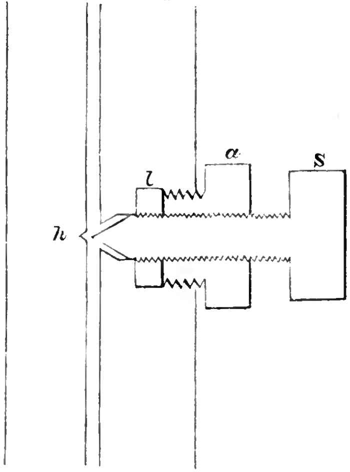
Fig. 3.



which it could be connected by a coupling nut, it had a piece D attached, in the centre of which there was a bore of $\frac{1}{8}$th of an inch diameter, which could be closed perfectly by means of a proper stop-cock.

I must here be permitted to make a short digression, in order to explain the construction of the stop-cocks, as it may save those who may in future attempt similar experiments, the useless trouble of trying to make the ordinary stop-cock perfectly air-tight under high pressures. The one I have used is the invention of Mr. Ash, of this town, a gentleman well known for his great mechanical genius; and he has in the most obliging manner allowed me to give a full description of it. Fig. 4 is a full-sized sectional view of the stop-cock. *a* is a brass screw, by means of which a thick collar of leather, *l*, is very tightly compressed. The centre of *a* is perforated with a female screw, in which a steel screw, *s*, works, the threads of which press so tightly against the leather collar as effectually to prevent any escape of air in that direction. The end of the steel screw is smooth and conical, and the conical hole *h* is plugged with tin. When the stop-cock is shut, the smooth end of the steel screw presses against the soft metal, so as to prevent the escape of the least particle of air; but when opened, as represented in the figure, it leaves a passage for the air round the conical point. I have tested this stop-cock in the most severe manner, and have found it to answer perfectly.

Fig. 4.



Having filled the receiver R (fig. 3) with about 22 atmospheres of dry air, and having exhausted the receiver E by means of an air-pump, I screwed them together, and then put them into a tin can containing 16½ lbs. of water. The water was first thoroughly stirred, and its temperature taken by the same delicate thermometer which was made use of in the former experiments. The stop-cocks were then opened by means of a proper key, and the air allowed to pass from the full into the empty receiver until equilibrium was established between the two. Lastly, the water was again stirred and its temperature carefully noted. The following Table contains the results of a series of experiments conducted in this way, interpolated with others to eliminate the effects of stirring, evaporation, &c.

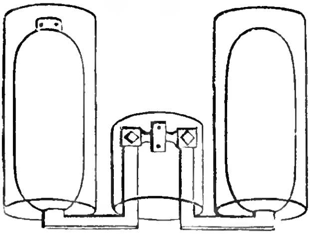
TABLE III.

Nature of experiment.	Barometrical pressure.	Quantity of air compressed in receiver R in cubic inches.	Mean temp. of the room.	Difference.	Temp. of water.		Gain or loss of heat.
					Before expt.	After expt.	
Expansion	30·20	2910	57° 4	0°·118+	57°·520	57°·517	0°·003 loss.
Interpolation	......	....	57·0	0·906—	56·085	56·103	0 018 gain.
Expansion	30·44	2920	57·0	0·885—	56·103	56·128	0·025 gain.
Interpolation	......	....	62·0	0·783—	61·217	61·217	0
Expansion	30·44	2910	62·1	0·873—	61·222	61·232	0·010 gain.
Interpolation	......	....	58·5	0·233+	58·732	58·735	0·003 gain.
Expansion	30·44	2915	58·6	0·132+	58·732	58·732	0
Interpolation	......	....	61·3	0·787—	60·508	60·518	0·010 gain.
Expansion	30·46	3200	61·3	0·780—	60·518	60·523	0·005 gain.
Interpolation	......	....	58 0	0·186+	58·184	58·187	0·003 gain.
Expansion	30·50	2880	58·3	0·110—	58·190	58·190	0
Mean of expts. of expansion	30·41	2956	......	0·400—	....	....	0·0062 gain.
Mean of interpolations	......	....	......	0·411—	....	....	0·0068 gain.
Corrected result...	30·41	2956	......	....	....	....	0

The difference between the means of the experiments and interpolations being exactly such as was found to be due to the increased effect of the temperature of the room in the latter case, we arrive at the conclusion, that *no change of temperature occurs when air is allowed to expand in such a manner as not to develope mechanical power*.

In order to analyse the above experiments, I inverted the receivers, as shown in fig. 5, and immersed them, as well as the connecting piece, into separate cans of water. One of the receivers had 2828 cubic inches of dry air condensed into it, while the other was vacuous. After equilibrium was restored by opening the cocks, I found that 2°·36 of cold per lb. of

Fig. 5.



water had been produced in the receiver from which the air had expanded, while 2°·38 of heat had been produced in the other receiver, and 0°·31 of heat also in the can in which the connecting piece was immersed; the sum of the whole amounting nearly to zero. The slight redundance of heat was owing to the loss of cold during the passage of the air from the charged receiver to the stop-cocks, through a part of the pipe which could not be immersed in water.

A series of experiments was now made in the following manner:—The receiver was filled with dry compressed air, and a coiled leaden pipe, $\frac{1}{4}$ of an inch in internal diameter and 12 yards long, was screwed tightly upon the nozle, as represented in fig. 6. The whole was then immersed into an oval can, which was constructed as before described, and was also covered at top as perfectly as possible. Having ascertained the temperature of the water by means of the sensible thermometer before used, the stop-cock was opened and the air made to pass from the receiver through a pneumatic trough into a jar, by which it was carefully measured. After the air in the receiver had been reduced to the atmospherical pressure, the water was again well-stirred and its temperature noted. An interpolation was made after each of these experiments, in order to eliminate the effects of stirring, &c.

Fig. 6.

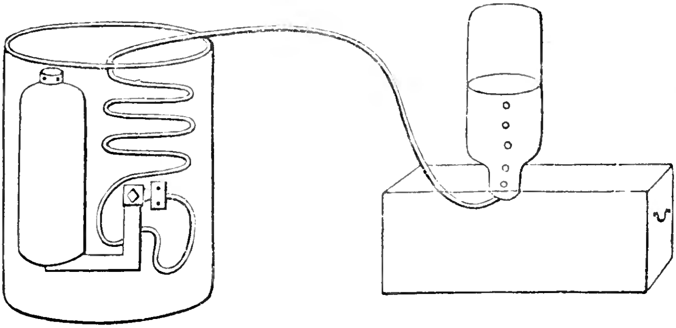


TABLE IV.

Nature of experiment.	Barometrical pressure.	Quantity of air compressed.	Quantity of air let out.	Mean temp. of the room.	Difference.	Temp. of water.		Gain or loss of heat.
						Before expt.	After expt.	
Expansion	30·04	2862	2726	55°·7	0°·405+	56°·207	56°·004	0°·203 loss.
Interpolation ...	......	......	......	55·4	0·579+	56·004	55·954	0·050 loss.
Expansion	30·10	2807	2670	54·6	0·022+	54·714	54·530	0·184 loss.
Interpolation ...	......	......	......	54·25	0·276+	54·536	54·516	0·020 loss.
Expansion	30·10	2723	2587	53·6	0·760+	54·460	54·259	0·201 loss.
Interpolation ...	......	......	......	53·4	0·839+	54·259	54·219	0·040 loss.
Expansion	30·10	2807	2670	49·05	0·307+	49·456	49·258	0·198 loss.
Interpolation ...	......	......	......	49·1	0·158+	49·258	49·258	0
Expansion	30·23	3039	2903	50·6	0·508—	50·176	50·008	0·168 loss.
Interpolation ...	......	......	......	51·1	1·063—	50·017	50·057	0·040 gain.
Expansion	30·20	2919	2782	49·0	0·355—	48·728	48·563	0·165 loss.
Interpolation ...	......	......	......	48·85	0·277—	48·573	48·573	0
Mean of expts. of expansion	30·13	2859	2723	......	0·105+	....	....	0·1865 loss.
Mean of interpolations ...	......	......	......	......	0·085+	....	....	0·0117 loss.
Corrected result.	30·13	2859	2723	......	....	....	....	0·1738 loss.

The cold produced was diffused through 21·17 lbs. of water, 14 lbs. of copper, 8 lbs. of lead, and 7 lbs. of tinned iron. Hence we find that a quantity of cold was produced in the experiments sufficient to cause the temperature of a lb. of water to decrease by 4°·085. At the same time a mechanical force was developed, which could raise a column of the atmosphere, of an inch square at the base, to the altitude of 2723 inches; or in other words, could raise 3352 lbs. to the height of one foot. For each degree of heat lost, there was therefore generated a force sufficient to raise 820 lbs. to the height of one foot.

In the two following series the experiments were varied by compressing and measuring out different volumes of air.

On reducing the results of these experiments in the manner before indicated, we find that in the experiments of Table V. 814 lbs., and in those of Table VI. 760 lbs. were raised to the height of a foot, for every degree of heat per lb. of water lost.

These results are inexplicable if heat be a substance. If that were the case, the same quantity of heat would have been absorbed by the rarefaction which took place in the experiments of Table IV., as was evolved by the corresponding

TABLE V.

Nature of experiment.	Barometrical pressure.	Quantity of air condensed.	Quantity of air let out.	Mean temp. of the room.	Difference.	Temp. of water.		Gain or loss of heat.
						Before expt.	After expt.	
Expansion ……	30·06	1336	1200	52°·5	1°·441—	51°·074	51°·044	0°·030 loss.
Interpolation …	……	……	……	52·55	1·460—	51·069	51·110	0·041 gain.
Expansion ……	30·20	1343	1206	53·5	1·385—	52·125	52·105	0·020 loss.
Interpolation …	……	……	……	53·6	1·457—	52·115	52·171	0·056 gain.
Expansion ……	30·28	1386	1250	52·4	0·419—	52·021	51·941	0·080 loss.
Interpolation …	……	……	……	52·55	0·588—	51·951	51·974	0 023 gain.
Expansion ……	30·28	1387	1250	52·95	0·778—	52·195	52·148	0·047 loss.
Interpolation …	……	……	……	53·2	1·017—	52·171	52·195	0·024 gain.
Expansion ……	30·30	1434	1298	59·0	0·610+	59·665	59·556	0·109 loss.
Interpolation …	……	……	……	58·65	0·888+	59·551	59·526	0·025 loss.
Expansion ……	30·27	1405	1268	55·35	0·227+	55·622	55·532	0·090 loss.
Interpolation …	……	……	……	55·1	0·534+	55·647	55·622	0·025 loss.
Expansion ……	30·14	1400	1264	55·2	0·313+	55·565	55·461	0·104 loss.
Interpolation …	……	……	……	55·3	0·158+	55·461	55·456	0·005 loss.
Mean of expts. of expansion	30·22	1384	1248	……	0·410—	………	………	0·0686 loss.
Mean of interpolations …	……	……	……	……	0·420—	………	………	0·0127 gain.
Corrected result.	30·22	1384	1248	……	………	………	………	0·081 loss.

TABLE VI.

Nature of experiment.	Barometrical pressure.	Quantity of air compressed.	Quantity of air let out.	Mean temp. of the room.	Difference.	Temp. of water.		Gain or loss of heat.
						Before expt.	After expt.	
Expansion ……	30·24	3116	1238	60°·1	0°·418—	59°·724	59°·641	0°·083 loss.
Interpolation …	……	……	……	60·2	0·552—	59·641	59·655	0·014 gain.
Expansion ……	30·20	3198	1238	56·1	0·041+	56·185	56·098	0·087 loss.
Interpolation …	……	……	……	56·2	0·090—	56·103	56·108	0·005 gain.
Expansion ……	30·15	3192	1238	61·15	1·117+	62·328	62·207	0·121 loss.
Interpolation …	……	……	……	60·9	1·275+	62·195	62·155	0·040 loss.
Expansion ……	30·15	3143	1238	60·15	0·863+	61·063	60·964	0·099 loss.
Interpolation …	……	……	……	60·05	0·896+	60·959	60·934	0·025 loss.
Expansion ……	30·20	2966	1238	55·45	0·343+	55·835	55·751	0·084 loss.
Interpolation …	……	……	……	55·45	0·298+	55·751	55·746	0·005 loss.
Expansion ……	30·30	3160	1238	58·1	0·285+	58·432	58·337	0·095 loss.
Interpolation …	……	……	……	57·95	0·384+	58·337	58·332	0·005 loss.
Expansion ……	30·14	3188	1238	55·0	0·678+	55·733	55·624	0·109 loss.
Interpolation …	……	……	……	55·1	0·515+	55·624	55·607	0·017 loss.
Mean of expts. of expansion	30·20	3137	1238	……	0·416+	………	………	0·0968 loss.
Mean of interpolations …	……	……	……	……	0·389+	………	………	0·0104 loss.
Corrected result.	30·20	3137	1238	……	………	………	………	0·0855 loss.

condensation in the experiments of Table I.: also a certain quantity of cold would have been produced in the experiments given in Table III. The results are, however, such as might have been deduced *à priori* from any theory in which heat is regarded as a state of motion among the constituent particles of bodies. It is easy to understand how the mechanical force expended in the condensation of air may be communicated to these particles so as to increase the rapidity of their motion, and thus may produce the phænomenon of increase of temperature. In the experiments of Table III. no cold was produced, because the momentum of these particles was not permanently converted into mechanical power; but had the motion of the air from one vessel to the other been opposed in such a manner as to develope power at the outside of the jar, which might have been accomplished by means of a cylinder and piston, *then* loss of heat would have occurred, just as in Tables IV., V. and VI., where the force was applied in lifting the atmosphere of the earth.

It is quite evident that the reason why the cold in the experiments of Table IV. was so much inferior in quantity to the heat evolved in those of Table I., is that all the force of the air, over and above that employed in lifting the atmosphere, was applied in overcoming the resistance of the stopcock, and was there converted back again into its equivalent of heat.

The discovery of Dulong*, that equal volumes of all elastic fluids, taken at the same temperature and under the same pressure, when suddenly compressed or dilated to the same fraction of their volume, disengage or absorb the same absolute quantity of heat, accords perfectly with these principles.

The mechanical equivalents of heat determined by the various series of experiments given in this paper are 823, 795, 820, 814, and 760. The mean of the last three, which I take as least liable to error, is 798 lbs., a result so near 838 lbs., the equivalent which I deduced from my magnetical experiments, as to confirm, in a remarkable manner, the above explanation of the phænomena described in this paper; and to afford a new, and to my mind, powerful argument in favour of the dynamical theory of heat which originated with Bacon, Newton, and Boyle, and has been at a later period so well supported by the experiments of Rumford, Davy and Forbes. With regard to the detail of the theory, much uncertainty at present exists. The beautiful idea of Davy, that the heat of elastic fluids depends partly upon a motion of particles round

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* *Annales de Chimie*, vol. xli. p. 156.

their axis*, has not, I think, hitherto received the attention it deserves. I believe that most phænomena may be explained by adapting it to the great electro-chemical discovery of Faraday, by which we know that each atomic element is associated with the same absolute quantity of electricity. Let us suppose that these atmospheres of electricity, endowed to a certain extent with the ordinary properties of matter, revolve with vast velocity round their respective atoms; and that the velocity of rotation determines what we call temperature. In an aeriform fluid we may suppose that the attraction of the atmospheres by their respective atoms, and that of the atoms towards one another, are inappreciable for all pressures to which the law of Boyle and Mariotte applies; and that, consequently, the centrifugal force of the revolving atmospheres is the sole cause of expansion on the removal of pressure. By this mode of reasoning, the law of Boyle and Mariotte receives an easy explanation, without recourse to the improbable hypothesis of a repulsion varying in a ratio different from that of the inverse square. The phænomena described in the present paper, as well as most of the facts of thermo-chemistry, agree with this theory; and in order to apply it to radiation, we have only to admit that the revolving atmospheres of electricity possess, in a greater or less degree, according to circumstances, the power of exciting isochronal undulations in the æther which is supposed to pervade space.

The principles I have adopted lead to a theory of the steam-engine very different from the one generally received, but at the same time much more accordant with facts. It is the opinion of many philosophers that the mechanical power of the steam-engine arises simply from the passage of heat from a hot to a cold body, no heat being necessarily lost during the transfer. This view has been adopted by Mr. E. Clapeyron in a very able theoretical paper, of which there is a translation in the 3rd part of Taylor's Scientific Memoirs. This philosopher agrees with Mr. Carnot in referring the power to *vis viva* developed by the caloric contained by the vapour, in its passage from the temperature of the boiler to that of the condenser. I conceive that this theory, however ingenious, is opposed to the recognised principles of philosophy, because it leads to the conclusion that *vis viva* may be destroyed by an improper disposition of the apparatus: thus Mr. Clapeyron draws the inference, that "the temperature of the fire being from 1000° (C.) to 2000° (C.) higher than that of the boiler, there is an enormous loss of *vis viva* in the passage of the heat from the furnace into the boiler." Believing that the power

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* Elements of Chemical Philosophy, vol. i. p. 94.

to destroy belongs to the Creator alone, I entirely coincide with Roget and Faraday in the opinion, that any theory which, when carried out, demands the annihilation of force, is necessarily erroneous. The principles, however, which I have advanced in this paper are free from this difficulty. From them we may infer that the steam, while expanding in the cylinder, loses heat in quantity exactly proportional to the mechanical force which it communicates by means of the piston; and that on the condensation of the steam, the heat thus converted into power is *not* given back. Supposing no loss of heat by radiation, &c., the theory here advanced demands that the heat given out in the condenser shall be less than that communicated to the boiler from the furnace, in exact proportion to the equivalent of mechanical power developed.

It would lengthen this paper to an undue extent were I now to introduce any direct proofs of these views, had I even leisure at present to make the experiments requisite for the purpose; I shall therefore reserve the further discussion of this interesting subject for a future communication, which I hope to have the honour of presenting to the Royal Society at no distant period.

Oak Field, near Manchester, June 1844.

LV. *Outline of a New and General Mode of Transforming and Resolving Algebraic Equations. By* James Cockle, *B.A., of the Middle Temple, Special Pleader*[*].

1. THE practical application of the following will be found in various papers which I have had the honour of publishing in the Mathematician. The method is, however, here presented in an entirely novel form. Considered generally, its characteristic is, the effecting the proposed reductions by modifying the *roots* of an equation directly. By way of commencement, I have, for this purpose, generalised the assumption of Mr. Murphy[†] (which is undoubtedly true for equations of the first four degrees), and assumed that the roots of the general equation of the nth degree, in y, are given by a set of expressions of which the type is

$$y_r = \beta_0 + \alpha^r \beta_1 + \alpha^{2r} \beta_2 + \ . \ . \ . \ + \alpha^{(n-1)r} \beta_{n-1}, \quad (1.)$$

where α denotes one of the nth roots of unity. It follows from this, that

$$y_1 + \alpha y_2 + \alpha^2 y_3 + \ . \ . \ . \ . \ + \alpha^{n-1} y_n = n \beta_{n-1}; \quad (2.)$$

and, denoting the left-hand side of (2.) by $\phi(y)$, if $\phi(y) = 0$, $\beta_{n-1} = 0$.

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[*] Communicated by T. S. Davies, Esq., F.R.S., F.S.A.

[†] Philosophical Transactions, 1837, part I.

2. If it be supposed that, in the roots of an equation of the nth degree in z, $\beta_{n-1} = 0$, then, by taking the roots in a proper order, we obtain, as before,

$$\phi(z) = n\beta_{n-2}, \text{ and, if } \phi(z) = 0,\ \beta_{n-2} = 0;$$

and, similarly, $\phi(w) = n\beta_{n-3}$, &c.

3. Next, x being the root of the general equation of the nth degree, let $y = \Lambda x^{\lambda} + \mathrm{M}x^{\mu}$, then, in order that, in the equation in y, β_{n-1} may $= 0$, we have

$$\phi(y) = 0 = \Lambda\phi(x^{\lambda}) + \mathrm{M}\phi(x^{\mu}), \quad \ldots\ldots \quad (3.)$$

since ϕ is a linear function. But ϕ has many values arising from the interchange of the roots one among another; let m of the values of (3.) arising from this circumstance be multiplied together, and we have

$$\Lambda^m\pi + \Lambda^{m-1}\mathrm{M}\pi' + \Lambda^{m-2}\mathrm{M}^2\pi'' + \ldots \mathrm{M}^m\pi^{(m)} = 0. \quad (4.)$$

Now the peculiarity of the quantities π, π', &c. is (see the work above mentioned*), that one is derivable from another by an easy process, and that when one consists of symmetric functions of x, all do; and if we select those forms of ϕ which are included in the expression $u_1 + \alpha^r(\phi(u) - u_1)$, giving r every value from 0 to $n - 2$, then, at least for the first four degrees, π is symmetric and (4.) becomes a homogeneous equation of the $(n-1)$th degree, whence $\frac{\Lambda}{\mathrm{M}}$ may be determined.

4. We have thus obtained equations of the 2nd, 3rd and 4th degrees, whose roots are respectively of the forms

$$a^{\frac{1}{2}}, \quad a + b^{\frac{1}{3}}, \quad a + b^{\frac{1}{2}} + c^{\frac{1}{4}},$$

whence a certain convenient relation among the coefficients is obtained (Mathemat. p. 83).

5. To take away another term of the expression for the roots, we must similarly assume $z = \Lambda' y^{\lambda'} + \mathrm{M} y^{\mu'}$; this gives us, *primâ facie* at least, the reduction of the biquadratic to the binomial form, and of the equation of the 5th degree to the solvable form of De Moivre, and may be found to throw some light on the difficulties attending those transformations.

6. The assumption indicated for taking away r terms of the root, is

$$y_r = \Lambda^{(r)} y_{r-1}^{\lambda^{(r)}} + \mathrm{M}^{(r)} y_{r-1}^{\mu^{(r)}}.$$

Devereux Court, March 4, 1845.

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* The formal proof will appear in the next July number of that work.

LVI. *On the Metamorphoses of Indigo*[*].

Production of Organic Bases which contain Chlorine and Bromine. By Dr. AUGUST WILHELM HOFMANN, *Assistant in the Giessen Laboratory*[†].

THE changes which result from the action of various bodies upon indigo have for a series of years engaged the attention of chemists. It may be even said that this subject has been selected with preference for investigation; and when we consider the rich harvests already obtained by a great number of celebrated chemists, there would appear, at the first glance, scarcely anything to be gleaned. It is however the character of every rapidly-advancing science, that each discovery becomes the source of numerous others, and that the solution of every question gives rise to others which must in their turn be answered. It was thus that the further investigation of the products from the decomposition of indigo, which had been already so thoroughly studied in many points by Laurent and Erdmann, led to results which appeared to me worthy of publication.

Before proceeding, however, to the description of these results, it may be advantageous to make a few preliminary observations for the purpose of showing the reasons for which these investigations have been undertaken.

Among the various theories which have of late years claimed the attention of chemists, that of substitution occupies a very prominent place. It is not my intention to examine the numerous phases of this discussion, for the most part carried on in France, nor to consider minutely the various consequences which had their rise in this originally extremely simple question. The problem which forms the basis of the present investigation is a more general experimental establishment of a single substitution theorem.

If we examine carefully the origin of the theory of substitutions, we shall be able to trace it to the endeavour to predetermine, by means of certain laws experimentally established, the changes to be produced upon a body through the action of chlorine, bromine, oxygen, &c. In the course of such investigations it was however perceived, that in order to

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[*] Communicated by the Chemical Society; having been read February 3, 1845.

[†] I have had the pleasure of witnessing the care and accuracy with which the author of this paper has proceeded in making his observations and establishing his facts.

It appears to me that he has produced a definite and irrefragable proof that the chemical character of a compound does not depend, as the electro-chemical theory supposes, upon the *nature* of the elements it contains, but solely on the manner of their grouping.—JUSTUS LIEBIG.

foretell these changes a complete knowledge of the constitution of the original body was necessary, and that this must be included in the argument. This discussion is not yet terminated, nor are we at present in a position to give an opinion as to its final result; this however is certain, that it has been the means of originating many excellent investigations, from which science has derived the greatest advantage.

It is now eight years since Laurent was led by a series of experiments to a peculiar mode of representing the action of chlorine on organic bodies, and among other views, of which we do not require here to speak, he laid down the following law:—

"In many organic compounds, by the action of chlorine, a certain number of equivalents of hydrogen are eliminated in the form of hydrochloric acid; the eliminated hydrogen being replaced by a like number of equivalents of chlorine, in such a manner, that the chemical character and the fundamental properties of the original compound are not sensibly altered. The chlorine molecules therefore must take the place of the hydrogen molecules, and play in the new compound the same part which was performed by the hydrogen molecules in the original."

The ideas of Laurent, which were opposed to the electro-chemical theory, and to the views generally received at that time, found, as might be expected, but an indifferent reception from chemists at the period of their publication. His views were reproached with being supported by too small a number of facts; even his facts were assailed, and the author required all his dexterity and steady perseverance to defend himself against his powerful opponents. He repeated many of his former experiments, and added by further investigations new proofs to those already given. Since that time a mass of facts has been observed which unequivocally support the ideas of Laurent. The discovery of chloracetic acid by Dumas, the investigations of Regnault on the action of chlorine on the *Liqueur des Hollandais*, of Malaguti on the action of the same body on the compound æthers, the extended works of Laurent on the naphthaline and phenyle series, the researches of the same chemist and of Erdmann on indigo, have given so many instances in which any other view is inadmissible, that it would appear scarcely necessary to undertake still further investigations in its support.

On examining however more closely the bodies in which hydrogen has hitherto been replaced by chlorine in the way mentioned, it is found that they consist of indifferent bodies, or acids, or their compounds with æther. Up to the present time the theory of substitution remains unrepresented by a large and important class of bodies. *No organic base* is known in which the hydrogen has been replaced by chlorine in the above-mentioned manner.* And yet it would be an argument of the greatest weight, were chlorine, an electro-negative element, to be introduced into a compound possessing electro-positive properties, without however changing its chemical character.

The above considerations induced me to undertake the following research.

The action of chemical agents in general upon the organic bases has been as yet but little studied. For the first accurate account of the special action of chlorine and bromine on these bodies, we are indebted to Pelletier†, who investigated the behaviour of strychnine toward chlorine, and at the same time communicated some details on the influence exercised by this body on quinine, cinchonine, brucine, morphine and narcotine. From his experiments he ascertained that chlorine withdrew from these bases hydrogen in the form of hydrochloric acid, and gave rise to neutral compounds difficult of solution in water, but soluble in alcohol. On submitting the crystalline product obtained from strychnine to analysis, the presence of chlorine in its composition was ascertained. Of the other bases, only quinine and cinchonine offered products of a somewhat crystalline structure. The physical properties of these substances however rendered their analysis so difficult, that we are still in the dark as to the relation which exists between them and the original compounds.

Lately Brande and Leben ‡ made some experiments on the action of chlorine on quinine, and described the various products—dalleiochine, rusiochine and melanochine, which all contain nitrogen but no chlorine. The properties of these bodies are not however well-defined, nor do their analyses bear the stamp of exactness, so that science has gained but little by their investigation. The same may be said of the researches of M. André § on the same subject.

All these details relate to solid crystalline bodies. As to

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* By organic base, in the strict sense of the word, I understand here, with Liebig, that class of compound, azotized bodies, which possess all the properties of basic metallic oxides. The oxides of ethyle, methyle, &c., are not included, as the most common character of salts is wanting in their compounds, viz. the replacement of their acids by other acids, and their bases by other bases.

† *Journal de Pharmacie,* Avr. 1838, and Liebig's *Ann.*, Bd. xxix. S. 48.

‡ *Archiv der Pharmacie,* Bd. xvi. S. 257, and Liebig's *Ann.*, Bd. xxxii. S. 270.

§ *Ann. de Chem. et de Phys.*, t. lxxi. p. 195, and Liebig's *Ann.*, Bd. xxxii. S. 273.

volatile bases, all that we know is, that nicotine and coneine are decomposed by the action of chlorine, but no investigation has as yet been made of these products of decomposition.

On the other hand, there exist some accounts of the products obtained by chlorine and bromine from aniline, that artificial base which has of late been met with under so many different circumstances. These bodies crystallize, but they no longer possess the character of basic compounds.

I have myself communicated some observations on the action of chlorine on aniline, made during an investigation of the organic bases of coal-gas naphtha *. I could not at that time obtain a chlorinated basic product of decomposition from aniline. Besides a neutral compound, which was not produced in sufficient quantity for analysis, there was always formed as the chief product chlorindoptinic acid (chlorophenissic acid of Laurent), whose relation to aniline is shown by the following formula:

$$\underbrace{C_{12}H_5, NH_2}_{\text{Aniline.}}+6\,Cl+2\,HO=\underbrace{C_{12}\left\{\begin{matrix}H_2\\ Cl_3\end{matrix}\right\}O+HO}_{\text{Chlorophenissic acid.}}+NH_3+3\,Cl\,H.$$

The action of bromine on aniline, investigated by Fritzsche†, is much less energetic. A simple substitution of bromine for hydrogen takes place, giving rise to the compound named by him bromaniloid,

$$\underbrace{C_{12}H_5, NH_2}_{\text{Aniline.}}+6\,Br=\underbrace{C_{12}\left\{\begin{matrix}H_2\\ Br_3\end{matrix}\right\},NH_2}_{\text{Bromaniloid.}}+3\,Br\,H.$$

But this compound too is no longer basic, but neutral, the positive properties of aniline being removed by the entrance of the electro-negative element into its composition. In these cases the action of chlorine and bromine has evidently gone too far for the aniline still to preserve its original character, and it remained therefore to try in some way to regulate the action of the above-named agents, so as to produce compounds in which only one equivalent of hydrogen was replaced, and which, like aniline, might possibly possess basic properties.

The remarkable facility with which the derived members of the phenyl family, viz. the chlorinated phenylic acid, as well as bromaniloid, crystallize, made it probable that the

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* Liebig's *Annalen*, Bd. xlvii. S. 68, and Phil. Mag. S. 3, vol. xxiv. p. 115.

† *Bullet. Scient. de St. Pétersb.*, 1843, t. i. p. 30, and Liebig's *Ann.*, Bd. xliv. S. 291.

desired compound would likewise be crystalline, by which the investigation would be rendered essentially easier. I resolved therefore to make my experiments in this direction.

ACTION OF CHLORINE ON ANILINE.

I began by carefully repeating my earlier experiments, but obtained exactly the same results.

As the action of chlorine on aniline immediately forms hydrochloric acid, I chose a solution of the base in this acid, and passed through it slowly a stream of the chlorine. After a few moments the fluid became violet, thickened and deposited a brown resinous matter, which fell to the bottom of the vessel. On submitting the whole fluid to distillation, a crystalline substance was deposited in the neck of the retort in the form of white needles, consisting of a mixture of two different bodies, which are easily separated by a second distillation with potash; an indifferent crystalline substance passes over, of which I shall immediately speak, and chlorophenissate of potash remains in the retort. The resinous matter submitted alone to distillation gave likewise a large quantity of chlorophenissic acid.

In no case could I obtain any other results than the above, however much I modified my experiments by employing a more or less diluted solution, and substituting a watery solution of chlorine instead of the gas. I soon found that the desired end could not be attained in this direct way, and had consequently to have recourse to an indirect method.

Aniline, originally discovered by Unverdorben[*] among the products of distillation of indigo, was afterwards produced by Fritzsche[†] on submitting anthranilic acid to the same process, and by Zinin[‡] through the action of sulphuretted hydrogen on nitrobenzid. The idea occurred to me to try the action of chlorine on anthranilic acid, and should it so happen that in this way out of the anthranilic acid $C_{14}\,H_7\,NO_4$ by the substitution of chlorine, a compound, $C_{14}\left\{\begin{matrix}H_{7-n}\\ Cl_n\end{matrix}\right\}NO_4$[§], could be obtained possessing the properties of anthranilic acid, it was highly probable that by its distillation the desired base would be obtained. It was also probable that by the action of chlorine on nitrobenzide, $C_{12}\,H_5\,NO_4$, it would like-

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[*] Poggend. *Ann.*, Bd. viii. S. 397.

[†] *Bul. Scient. de St. Pétersb.*, t. vii. No. 12, and t. viii., and Liebig's *Ann.*, Bd. xxxvi. p. 87.

[‡] *Bul. Scient. de St. Pétersb.*, t. x. No. 18, and Liebig's *Ann.*, Bd. xliv. p. 283.

[§] $n = 1$ or 2.

wise be transformed into a similar body, $C_{12}\left\{\begin{matrix}H_{5-n}\\Cl_n\end{matrix}\right\}NO_4$, and this body would require only to be treated with sulphuretted hydrogen to be changed into the desired compound.

Whilst occupied with the preparation of anthranilic acid, for the purpose of these experiments, another expedient offered itself, which, by changing completely the direction of my researches, led me quickly to the desired object.

It is well known that it is not necessary to employ pure anthranilic acid for the preparation of aniline, which may be obtained directly as a true product of oxidation by distilling indigo and hydrate of potash, as is shown in the following manner:—

$$\underbrace{C_{16}H_5NO_2}_{\text{Indigo.}} + 4\,HO = \underbrace{C_{12}H_7N}_{\text{Aniline.}} + 4\,CO_2 + 2\,H.$$

We may here take a short review of the substances obtained from indigo by other oxidizing means. By the action of nitric acid there is formed as the first product of oxidation a body named isatine, which was discovered by Laurent and Erdmann about the same time. This compound contains the elements of indigo, together with 2 equivalents of oxygen,

$$\underbrace{C_{16}H_5NO_2}_{\text{Indigo.}} + 2\,O = \underbrace{C_{16}H_5NO_4}_{\text{Isatine.}}$$

Aniline being, as was already remarked, a product of oxidation of indigo, it was in the highest degree probable, not to say certain, that it could also be obtained from isatine by fusing the latter with hydrate of potash. This question remained to be decided by experiment.

ACTION OF HYDRATE OF POTASH IN FUSION ON ISATINE*.

On pouring a solution of caustic potash on isatine in a tubulated retort, its colour becomes dark red, almost amounting to black, which, according to Laurent, is owing to a trans-

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* All the isatine employed in the following experiments was prepared from commercial indigo, according to the method of Laurent, with common nitric acid; a very considerable quantity is thus obtained. In its preparation care must be taken not to employ too large a quantity of nitric acid, otherwise there is formed the indigotic acid discovered by Buff (also called aniline, and nitrosalicylic acid), which is at once recognised by the yellow colour of its crystals and the blood-red reaction with perchloride of iron. Neither must the indigo be mixed with too much water. After each addition of nitric acid effervescence should take place, which must be allowed to finish before adding a fresh quantity. Should this effervescence not take place, and a large quantity of nitric acid be added gradually to the boiling mixture, a most powerful reaction takes place at a certain point of concen-

itory combination of isatine with potash. By warming the potash-ley, a solution of the dark red crystals is made of the same colour, but so soon as the fluid comes to the boiling-point the colour changes to orange-yellow, and contains now isatinate of potash. At the beginning of the distillation pure water passes over, but at a certain concentration of the fluid, which can be hastened by the addition of pieces of solid hydrate of potash, a lively reaction takes place, and there distils over with the water a body in the form of oily drops, which possesses all the properties of aniline. During the operation, and particularly at the end, free hydrogen gas is evolved, and carbonate of potash remains in the retort. The following formula exhibits the decomposition:—

$$\underbrace{C_{16}\,H_5\,NO_4}_{\text{Isatine.}} + 4\,(KO,\,HO) = \underbrace{C_{12}\,H_7\,N}_{\text{Aniline.}} + 4\,(CO_2,\,KO) + 2\,H.$$

After this experiment, the preparation of the desired compounds was no longer a matter of difficulty. They must be obtained by the action of alkaline bases on chlorisatine and bichlorisatine.

The experiments about to be detailed will show how far this conclusion was correct, supported as it was by the complete analogy existing between isatine, chlorisatine and bromisatine.

ACTION OF FUSED HYDRATE OF POTASH ON CHLORISATINE.

On distilling chlorisatine* with a mixture of potash-ley and solid hydrate of potash, exactly the same phænomena present themselves as in the case of isatine, with the single difference,

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tration of the fluid, and the mass flows over the largest vessels. The portions remaining contain generally only indigotic acid.

Isatine may be purified very easily in the following manner:—The crude product, still containing a quantity of resinous matter, is dissolved in potash-ley, and then carefully decomposed by the addition of hydrochloric acid so long as there is formed a black or brown precipitate. Should a small filtered portion possess a pure yellow colour, or the precipitate arising from the further addition of hydrochloric acid be of a pure lively red, the whole fluid is then to be separated by filtration from the resin and completely decomposed by hydrochloric acid. After washing with water, isatine obtained in this way is chemically pure.

* For the preparation of large quantities of chlorisatine I have always employed isatine, from the fact that chlorine in its action on indigo produces a mixture of chlorisatine and bichlorisatine, which can with difficulty be separated. Isatine is therefore much more convenient for this purpose. The change can be very much hastened by suspending isatine in boiling water, and directing a stream of chlorine through the fluid under the direct influence of the sun's rays. The transition is instantly recognised by the orange-yellow colour assumed by the crystals. As already stated by Erdmann (*Journal für Prakt. Chem.*, Bd. xxii. S. 272), not a trace of bichlorisatine is formed in this reaction, a fact also confirmed by my experiments.

that the oily fluid which distils over with the water condenses into a white crystalline mass in the neck of the retort, or, if this is too hot, in the cooled receiver. If the distillation is carefully conducted to prevent any of the fluid spirting over, the product collected in the receiver *does not react alkaline* so long as there remains a considerable quantity of water in the retort. When, however, the substance in the retort becomes nearly dry, a considerable quantity of ammonia, together with hydrogen, begins to be evolved, and the upper part of the retort becomes covered with a blue matter, which is sometimes carried over with the watery vapour into the receiver; at the same time the oily drops which have distilled over, from being colourless, now become brown and no longer solidify. At this period the distillation must be interrupted.

The crystalline substance which collects in the receiver can be purified with the greatest ease, by being thrown on a filter and washed with distilled water till all trace of ammonia is removed. To obtain it chemically pure, it is to be dissolved in boiling alcohol, from which it crystallizes in regular octahedrons. From the analysis it will be seen that this compound is aniline, in which one equivalent of hydrogen is replaced by one equivalent of chlorine. In short, it presents all the chemical characters and properties of aniline, and is like it a *base*. It may hence be denominated "chloraniline." If we employ the very excellent nomenclature of Laurent, who designates aniline by the name amaphenese (phenamide), then must the new chlorinated base be called amachlophenese.

A. *Chloraniline* (*Amachlophenese*).

a. Composition.

This substance was prepared and purified in the way above mentioned, and then dried in the air; in consequence of its containing chlorine, the combustion was made with chromate of lead.

I. 0·4495 grm. of substance gave 0·9210 grm. of carbonic acid and 0·1980 grm. of water.

II. 0·3220 grm. of substance gave 0·6670 grm. of carbonic acid and 0·1495 grm. of water.

III. 0·283 grm. of the substance well-mixed with powdered lime was placed in a tube, small pieces of lime added, and then submitted to a strong heat*. The residue, dissolved in

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* Strong heat is necessary, as the substance is decomposed with difficulty. In a previous operation a small quantity distilled over undecomposed, which made the amount of chlorine too small.

0·3925 grm. of substance gave 0·400 grm. chloride of silver = 25·14 per cent. chlorine.

nitric acid and precipitated by nitrate of silver, gave 0·315 grm. of chloride of silver.

IV. The determination of the nitrogen was made in an atmosphere of carbonic acid. 0·597 grm. of substance burned with peroxide of copper gave, at $334'''$ barom. and 11° centigrade, 58· cub. cent. of nitrogen. These numbers correspond to the following per-centage:—

	I.	II.	III.	IV.
Carbon* =	55·88	56·49		
Hydrogen =	4·89	5·15		
Chlorine =			27·45	
Nitrogen =				11·38

These numbers may be expressed in the following formula,—$C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N$, as is shown by comparison of the numbers calculated and found.

	Theory.	Per cent.	Mean of experiment.
12 Carbon =	900·00	56·43	56·18
6 Hydrogen =	75·00	4·70	5·02
1 Chlorine =	442·65	27·75	27·45
1 Nitrogen =	177·04	11·10	11·28
1 æq. Chloraniline =	1594·69	100·00	99·95

To control this formula, I prepared the platinum double salt, which crystallized beautifully, and, after drying at the temperature of the air, analysed it. 0·5095 grm. of this salt, after combustion, left 0·1495 grm. = 29·34 per cent. of platinum; the atomic weight found from this = 1626·85; theoretical atomic weight = 1594·69. The agreement between the two is sufficiently near.

By adopting the formula $C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N$, which is completely established by the analyses of the different salts and the products of decomposition of the base, the formation of this body can be explained in the most satisfactory manner. It is, as was to be expected, completely analogous to the formation of aniline from isatine, as is shown by the following formulæ:—

$$\underbrace{C_{16}\left\{\begin{matrix}H_4\\Cl\end{matrix}\right\}NO_4}_{\text{Chlorisatine.}}+4\,(KO, HO)=\underbrace{C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N}_{\text{Chloraniline.}}+4\,(CO_2\,KO)+2\,H.$$

I shall return afterwards to the origin of the ammonia in the last stage of the preparation.

b. Properties of Chloraniline.

I have already stated that this base is soluble in alcohol,

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* Carbon = 75. Hydrogen = 12·5.

from which it crystallizes on cooling or by evaporation in regular octahedrons, in which the axes, as in the crystals of alum, have the planes of the cubes developed in a very high degree. I have obtained completely formed crystals, the axes of which were about a centimetre in length, and could be therefore submitted to the most exact crystallographic examination; they are not altered by exposure to the air. By rapidly cooling a boiling alcoholic solution of this base, it is deposited in the form of small, regular octahedrons, which present the most striking resemblance to codeine. The facility with which this body crystallizes is indeed remarkable; its solution yields by evaporation regular crystals even to the last drop.

This base is also extremely soluble in æther, pyro-acetic spirit, acetone, bisulphuret of carbon, fatty and æthereal oils. Water dissolves it but sparingly. By rapid cooling the boiling saturated solution becomes milky; but by proceeding slowly the base is deposited in well-defined small octahedrons, possessing the lustre of the diamond. By agitation with æther the base is withdrawn from the aqueous solution. Its odour is pleasant, somewhat like wine; its taste aromatic and burning. In both respects it cannot absolutely be distinguished from aniline. The crystals melt by heat. On making the experiment with a considerable quantity, the whole mass fused at a temperature of 64° to 65° cent. into a yellow oily liquid, which solidified on cooling into a mass of large octahedrons, the thermometer sinking at the same time to 57° cent. This base is in the highest degree volatile. The alcoholic solution cannot be evaporated without considerable loss. Even at common temperatures the crystals volatilize, as may be seen by the white vapours which arise on bringing near them a glass rod moistened with hydrochloric acid. With water the base can easily be distilled. In an experiment to distil it alone, made with a small quantity, I always obtained, besides the oily drops, the blue product of decomposition which I have already stated is formed in the preparation of the base. This decomposition prevents the determination of the boiling-point, which at all events lies above 200° cent. The vapour of the base burns with a brilliant smoky flame, possessing the green-coloured border peculiar to organic compounds containing chlorine.

It is heavier than water, and neither alone nor in solution does it exhibit the least reaction on turmeric or reddened litmus paper. Dahlia paper is coloured of a feeble green.

From what has been stated, it appears that chloraniline almost completely corresponds in its physical properties to aniline. The chief difference is that aniline is fluid at common temperatures, whilst the chlorinated compound is solid.

In their relation to other bodies, both compounds also agree, but to a more limited extent. The intense yellow colour given to pine-wood and the pith of the elder tree by acid solutions of aniline, is also produced by the salts of the chlorinated compound. On the other hand, the characteristic violet colour communicated to aniline by chloride of lime, is not exhibited by the compound derived from it; a solution of this reagent by mixture with this body assuming only a slight shade of violet. The solution of the salts takes the same colour, which however rapidly passes into orange. Neither is a cold or hot watery solution of the base nor of its salts altered by chromic acid, whilst aniline under the same circumstances gives rise to dark or greenish-blue products. On adding chromic acid to the crystals of the chlorinated base, they become brown and resinify. A mixture of these crystals with solid chromic acid inflames at the fusing-point of the base.

With regard to its relation to other reagents, I have to remark the following:—Peroxide of iron is not precipitated by the watery or alcoholic solution of the base. In both cases the fluid becomes green, from a partial reduction of the persalt of iron. By employing a watery solution of the base or crystals of it, a violet product of oxidation separates at the boiling-point, which is soluble in alcohol. No change is produced in protosalts of iron, nor in sulphates of alumina or of zinc, even by boiling them with the crystals. These oxides, however, are precipitated by aniline, from which it is evident that by the entrance of chlorine into its composition, its basic properties are somewhat enfeebled. Sulphate of copper is not precipitated by a watery solution of the base, but on adding some of its crystals to a boiling copper solution, the latter is immediately decolorized, and then deposits a mass of crystals of a bronze colour, which are insoluble in water, but are partially dissolved in boiling alcohol, from which on cooling they crystallize in the form of scales. It appears to be a double salt of sulphate of copper and of the chlorinated base. Similar double salts are produced with the perchlorides of mercury, platinum and palladium; the first of these being white, the latter two of a splendid orange-yellow colour. The precipitate from a gold solution is reddish brown. Tincture of galls is not changed by a cold solution of the base, but on adding a hot saturated solution, a yellow flocky precipitate is produced on cooling. Other chemical reagents are not affected by its watery solution.

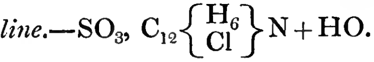
c. Compounds of Chloraniline.

The extraordinary facility of crystallizing which distinguishes the chlorinated base are found in nearly all its compounds with acids; most of them are precipitated as a crystalline mass by mixing its alcoholic solution with the respective acids. By recrystallization from boiling water or alcohol they are obtained quite pure; with the exception of the platinum and palladium double salts they are white, but the larger crystals have generally a shade of yellow. An excess of acid imparts to them a violet tint. An elevation of temperature is observed in the formation of these compounds, &c.; they possess the true character of salts by their capability of undergoing double decomposition. As this base has no alkaline reaction, all its salts, as a consequence, possess acid properties. A large excess of the base can be heated to fusion under dilute hydrochloric acid without removing the acid properties of the supernatant fluid.

The salts are decomposed with the greatest ease by the caustic and carbonated alkalies; in the latter case with evolution of the carbonic acid, which does not unite with the base. The base itself separates in the form of a crystalline mass. The same decomposition is also produced by hydrochlorate of ammonia in solution. By heating it, however, in a tube with hydrochlorate of ammonia, evolution of ammonia takes place, and the hydrochlorate of the base is deposited on the cool portion of the tube.

Its capacity of saturation is the same as that of aniline, although the atom of the latter is lower than that of the former by the difference of the equivalents of chlorine and hydrogen ($442{\cdot}65 - 12{\cdot}50 = 430{\cdot}15$).

The constitution of the salts in no respect differs from those of ammonia. They unite directly with the hydracids, and with the oxacids by the addition of an equivalent of water.



Sulphate of Chloraniline.—$SO_3,\ C_{12}\left\{\begin{matrix}H_6\\ Cl\end{matrix}\right\}N + HO.$

The alcoholic solution of chloraniline forms, by the addition of a few drops of sulphuric acid, a white crystalline mass, which by re-solution in boiling water separates on cooling as a confused mass of tabular crystals. From boiling alcohol, in which the salt is less soluble, it crystallizes in stellated tufts of fine silvery needles. Their crystalline form cannot however be determined. Even by slow evaporation of the alcoholic, as well as the watery solution, over sulphuric acid, I was unable to obtain large crystals. The salt is not volatile. By heating, a portion of the base escapes unchanged, while the remainder becomes black and evolves sulphurous acid.

Only one combustion of this salt was made. 0·3445 grm.

gave 0·5290 grm. of carbonic acid and 0·1365 grm. of water. These numbers correspond in the 100 parts to

Carbon . . . . = 41·87
Hydrogen . . . = 4·40

The formula is $SO_3, C_{12}\left\{\begin{matrix}H_6\\ Cl\end{matrix}\right\}N + HO$, which gives

Carbon . . . . = 40·75
Hydrogen . . . = 3·96

The excess of carbon arises from the formation of a small quantity of deutoxide of nitrogen, which almost always takes place in the combustion of such bodies, if a very considerable quantity of copper turnings is not placed at the fore part of the combustion tube. I did not think it necessary to repeat this analysis, as it was only intended to give a view of the constitution of the salt.

Binoxalate of Chloraniline, $2C_2O_3, C_{12}\left\{\begin{matrix}H_6\\ Cl\end{matrix}\right\}N + HO + 2\,aq.$

By dissolving crystals of chloraniline in a warm watery solution of oxalic acid, a salt separates on cooling, which by re-crystallization from hot water is obtained in long needles like saltpetre. These consist of closely agglomerated prisms, and hence could not be used for the determination of the crystalline form. The salt is difficultly soluble in cold water and alcohol. It is without smell, but possesses a sweetish burning taste. Its solution becomes coloured on exposure to the air, and deposits gradually a red powder. The following results were obtained on burning this compound with chromate of lead:—0·4475 grm. gave 0·6990 grm. of carbonic acid and 0·171 grm. of water, or in the 100 parts,

Carbon . . . . = 42·59
Hydrogen . . . = 4·24

These numbers give the following formula:—

$$2\,C_2\,O_3, C_{12}\left\{\begin{matrix}H_6\\ Cl\end{matrix}\right\}N + HO + 2\,aq.$$

		Theory.		Found.
16 equiv. of	Carbon . .	= 1200·00	42·37	42·59
9 ...	Hydrogen .	= 112·50	3·97	4·24
1 ...	Chlorine .	= 442·65		
1 ...	Nitrogen .	= 177·04		
9 ...	Oxygen . .	= 900·00		
		2832·19		

To control this formula I determined the amount of oxalic acid in the salt. 0·339 grm. of the salt, saturated with ammonia and precipitated by chloride of calcium, gave, after heating to redness, 0·153 grm. of carbonate of lime, which corresponds to 32·18 per cent. of anhydrous oxalic acid.

	Theory.		Found.
2 equiv. of Oxalic acid =	900·00	31·77	32·18
1 ... Base . . =	1594·69	56·30	
3 ... Water . =	337·50	11·93	
	2832·19	100·00	

The above salt corresponds in its constitution to the binoxalate of ammonia and to Zinin's binoxalate of naphtalidan. As a neutral oxalate of aniline is known, I thought I might obtain the neutral salt of the chlorinated base by mixing its watery solution with oxalic acid in such a way as to leave the former in great excess. From this mixture, on cooling, separated needle-form crystals, which were washed and recrystallized. The combustion with chromate of lead gave from 0·4215 grm., dried at the temperature of the air, 0·6565 grm. of carbonic acid and 0·1490 grm. of water, corresponding in the 100 parts to

Carbon	42·69
Hydrogen	3·92

hence proving that even under the conditions above-mentioned an acid salt had been formed.

An acid oxalate of aniline has not as yet been analysed. I endeavoured to form such a salt by saturating aniline with a large excess of oxalic acid. The precipitate formed was washed and frequently recrystallized from water. The needles obtained in this way possessed great similarity to the acid salt of the chlorinated base; they were, however, neutral oxalate of aniline, as was proved by the combustion. 0·3660 grm. of the salt gave 0·8188 grm. of carbonic acid and 0·2088 grm. of water. In 100 parts,

Carbon	61·01
Hydrogen	6·33

The formula is $C_2 O_3, C_{12} H_7 N + HO$, which gives

Carbon	60·79
Hydrogen	5·79

Nitrate of chloraniline is obtained by simply heating the base in dilute nitric acid. On cooling, the fluid becomes filled with tabular crystals, which generally possess a reddish tint. The salt is tolerably soluble in both alcohol and water. By heat it fuses into a dark mass, which forms with alcohol a beautiful rich blue solution, from which the nitrate, partly unchanged, again separates. I could not sublime this compound.

Phosphate of Chloraniline.

A solution of the base in alcohol forms, with phosphoric acid, a thick mass of crystalline plates, which are tolerably soluble in alcohol and water. I have not made any further investigation of this salt.

Hydrochlorate of Chloraniline, $Cl\,H,\ C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N.$

Forms large, colourless, transparent crystals, which are obtained on cooling from a hot saturated solution of the base in hydrochloric acid. By redissolving in water and slowly evaporating the dilute solution over sulphuric acid, large well-formed crystals are produced. In describing the corresponding bromine compound I shall speak more of their crystalline form. They remain unchanged on exposure to the air, but become white and opake by the application of heat. If the heat be carefully applied they can be sublimed in a tube like sal-ammoniac; by sudden elevation of the temperature, however, the salt is decomposed with evolution of a violet-blue vapour.

The analysis of the crystals, dried at the temperature of the air, gave the following results:—0·3155 grm. of the salt precipitated with nitrate of silver gave 0·271 grm. of chloride of silver, corresponding to 22·10 per cent. of hydrochloric acid, which leads to the formula $Cl\,H,\ C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N.$

	Theory.		Experiment.
1 eq. Chloraniline . .	=1594·69	77·80	77·90
1 eq. Hydrochloric acid	= 455·15	22·20	22·10
1 eq. Hydrochlorate of chloraniline .	=2049·84	100·	100·

Chloraniline and Bichloride of Platinum.

$$ClH,\ C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N + Cl_2\,Pt.$$

A solution of chloraniline in hydrochloric acid is precipitated of an orange-yellow colour by bichloride of platinum. If the two solutions are mixed hot, the whole fluid on cooling becomes a mass of crystalline plates. This double salt is very soluble in water, spirits of wine, and even in a mixture of alcohol and æther. To obtain it pure it is to be washed with cold water till the fluid, which continues to possess an evident acid reaction, passes colourless. On exposing the moist salt to the light, it becomes of a violet colour on the surface, which arises probably from the oxidizing action of the bichloride of platinum by the presence of water. Evaporated over sulphuric acid this salt forms little rounded lumps, which exhibit no facets. On burning with chromate of lead the following result was obtained:—

0·463 grm. of the platinum double salt gave 0·374 grm. of carbonic acid and 0·096 grm. of water; corresponding in the 100 parts to

Carbon . . . 22·03
Hydrogen . . 2·30

These numbers, with the determination of the platinum already mentioned, give the following formula:—

$$\text{Cl H, C}_{12}\left\{\begin{matrix}\text{H}_6\\ \text{Cl}\end{matrix}\right\}\text{N} + \text{Cl}_2\,\text{Pt}.$$

	Theory.		Experiment.
12 equiv. Carbon . .	= 900·00	21·53	22·03
7 ... Hydrogen .	= 87·50	2·09	2·30
4 ... Chlorine. .	=1770·61	42·47	
1 ... Nitrogen .	= 177·04	4·24	
1 ... Platinum .	=1233·50	29·46	29·34
1 eq. of the double salt	=4168·65		

Bichloride of Mercury and Chloraniline.

This salt can be obtained in beautiful fine needles by pouring an alcoholic solution of the base into a hot solution of corrosive sublimate. At first the fluid remains clear, but after a few moments begins to get turbid, and gradually thickens completely into a mass of needle-form crystals of the double salt. I have not investigated other salts of the base. I may, however, mention a very beautiful double salt formed with chloride of zinc: by the mixture of the two solutions the fluid remains at first clear, but after a short time becomes a mass of silvery crystals.

d. Products of the Decomposition of Chloraniline.

I have not as yet so fully studied the products of the decomposition of this base as they deserve. Indeed it appears to suffer in contact with other bodies the same changes as aniline. The presence of the chlorine seems to have no influence in the processes of transformation, but passes over into the new products just as the hydrogen of aniline which it has replaced. I intend to investigate this subject more fully, and shall mention here only those phænomena which came under my observation during the course of the present inquiry.

Action of the Oxygenated Compounds of Chlorine on Chloraniline.

On adding crystals of this substance to a mixture of chlorate of potash and hydrochloric acid, the fluid at first becomes violet-red and then gradually turbid and brown. The action is much hastened by the addition of alcohol, by which at the same time, however, acetic æther is formed. By long-continued action the fluid becomes again colourless, and a yellow crystalline matter is obtained, which can be easily purified by washing with alcohol and æther, in which it is but little soluble.

This substance crystallizes from boiling alcohol on cooling in small yellow plates. By slowly heating it sublimes without residue in the form of beautiful glittering scales of a golden colour. Potash forms with it a blood-red solution, from which, after some time, ruby-red prisms separate, which become of a vermilion colour by moistening with hydrochloric acid, and after removal of the acid dissolve in pure water with a beautiful violet colour. All these phænomena characterize in an undoubted manner the chloranil discovered by Erdmann in his investigations on indigo, a substance which, as I proved* a few months ago, is formed also in many other processes.

If the action of the oxidizing mixture on the chloraniline is not allowed to go so far, the chloranil is obtained mixed with a viscid matter, which is very soluble in alcohol and æther. On evaporating off the æther and submitting the resinous residue to distillation, oily drops pass over and crystallize in the neck of the retort. They possess the characteristic smell and peculiar reactions which distinguish the chlorophenissic or chlorophenussic acid of Laurent, namely the forming a violet precipitate with sulphate of copper, and a citron yellow with nitrate of silver.

The formation of all these bodies is easily explained. The nitrogen with a portion of the hydrogen forms ammonia, the presence of which can be proved in the mother-liquor. The remaining hydrogen is entirely or partially replaced by chlorine, chlorine and oxygen being called into action in the mixture of hydrochloric acid and chlorate of potash, whilst at the same time oxygen enters into the compound.

The following formulæ show the reactions:—

$$\underbrace{C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N}_{\text{Chloraniline.}} + 5Cl + 4O = \underbrace{C_{12}Cl_4O_4}_{\text{Chloranil.}} + NH_3 + 3Cl\,H.$$

$$\underbrace{C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N}_{\text{Chloraniline.}} + 2HO + 4Cl = \underbrace{C_{12}\left\{\begin{matrix}H_2\\Cl_3\end{matrix}\right\}O, HO}_{\text{Chlorophenissic acid.}} + NH_3 + 2Cl\,H.$$

$$\underbrace{C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N}_{\text{Chloraniline.}} + 2HO + 8Cl = \underbrace{Cl_{12}Cl_5O, HO}_{\text{Chlorophenussic acid.}} + NH_3 + 4Cl\,H.$$

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* Mem. Chem. Soc. vol. ii. p. 227.

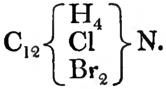
Chloraniline thus gives, under the same circumstances, as was to be expected, the same products of decomposition as aniline.

Action of Chlorine on Chloraniline.

At the commencement of this memoir are given the facts observed by the action of chlorine on aniline. The chlorinated base suffers precisely the same decompositions. There are formed chlorophenissic acid and a neutral body, the latter being trichloraniline, $C_{12}\left\{\begin{matrix}H_4\\Cl_3\end{matrix}\right\}N$. I shall return immediately to its preparation and its properties.

Action of Bromine on Chloraniline.

By mixing aniline with bromine, heat and hydrobromic acid are disengaged, and there is formed Fritzsche's bromaniloid (tribromaniline), $C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N$. The chlorinated base suffers the same decomposition with bromine. The product differs from the foregoing only in containing chlorine. It has the following composition, as we shall see afterwards:—



$$C_{12}\left\{\begin{matrix}H_4\\Cl\\Br_2\end{matrix}\right\}N.$$

Action of Nitric Acid on Chloraniline.

The decomposition produced in the chlorinated base by nitric acid I have unfortunately as yet but incompletely studied, although this point would have been of particular interest to have cleared up satisfactorily.

By the action of nitric acid aniline is transformed into carbazotic acid. It is in the highest degree probable that by the action of this oxidizing agent on chloraniline picric acid is formed, in which 1 equiv. of hydrogen is replaced by 1 of chlorine, having the formula $C_{12}\left\{\begin{matrix}H\\Cl\\3NO_4\end{matrix}\right\}O+HO$. By adding concentrated nitric acid to the chlorinated base, a lively reaction takes place, the fluid remains for some time boiling without the application of external heat, evolves nitrous acid, and becomes dark red, and finally black and opake. By continued action it becomes again transparent, and by allowing the scarlet red fluid to cool there sometimes separate golden yellow needles, which are very similar to the nitropicric acid. No trace of chlorine can be then detected in the fluid by nitrate of silver.

I made a great number of experiments to discover under what conditions this body was formed, but as yet I have not been able to produce it at will. In most cases I obtained instead a resinous body, which was precipitated in the form of yellow flakes from the nitric acid by addition of water, and which possessed the same property as nitropicric acid of tinging the skin. This substance is soluble in alcohol and æther, but cannot be obtained from the solution in a crystalline form. It is also soluble in the alkalies and ammonia, and is again precipitated by acids. By boiling the ammoniacal solution till the excess of ammonia is removed, and then adding nitrate of silver, a reddish-yellow precipitate is formed, which I was able to obtain only on one occasion in a crystalline form. My substance was, however, all expended ere I could discover a good method of preparation. Further experiments are necessary to decide the correctness of the forementioned hypothesis.

Action of Anhydrous Baryta and Lime on Chloraniline.

Though the examples are numerous in which the hydrogen of organic bodies has been replaced by chlorine, yet chemists have seldom been able to expel, without complete disorganization of the body, the assumed chlorine and refix the hydrogen in its old position. Indeed I know of no other instance than that of the regeneration of acetic acid from the chloracetic, performed by Melsens, by the employment of potassium amalgam.

From chloraniline aniline can likewise be reproduced. If a few crystals be placed in the end of a combustion-tube and lime in small pieces be laid before it, on heating the lime to a low red heat, there distils over a yellow oil which no longer becomes crystalline, and by all its reactions is at once recognised as aniline. At the same time a considerable quantity of ammonia is evolved, and there remains in the tube chloride of calcium and a large quantity of carbonaceous matter.

The regeneration of aniline is, as must be at once seen. essentially different from that of the acetic acid. In the reproduction of acetic acid from the chloracetic the hydrogen assumed is derived from the water present, its oxygen serving to oxidize the potassium; in the case of aniline the requisite hydrogen is furnished by that substance itself, which must consequently be partly decomposed. One equivalent of the chlorinated base contains just sufficient hydrogen to convert its nitrogen into ammonia, its own chlorine and that of another equivalent of base into hydrochloric acid, and finally, to furnish the required equivalent of hydrogen to the now regenerated aniline. Its carbon must, therefore, be completely separated. The following formula exhibits this change:—

$$\underbrace{2C_{12}\left\{\begin{matrix}H_6\\ Cl\end{matrix}\right\}N}_{\text{Chloraniline.}} + 2CaO = \left\{\begin{matrix}C_{12}N_7N + C_{12} + 2Cl\,Ca +\\ 2HO + NH_3.\end{matrix}\right.$$

This decomposition gives the key to some phænomena of which I have already made mention in the preparation of chloraniline.

I mentioned that in the last stage of the distillation, so soon as the mass in the retort commences to become dry, evolution of ammonia takes place, and aniline at the same time distils over. This reaction evidently depends on a transformation similar to that just mentioned. The vapours of the chlorinated base which deposit on the cooler parts of the apparatus meet with hot potash on the sides of the retort, and are decomposed into aniline, ammonia, and hydrochloric acid.

By carefully mixing a small portion of very pure chlorisatine with caustic baryta and slowly heating, a product of distillation was obtained which did not exhibit the slightest alkaline reaction, whilst, in the heated residue, not a trace of chlorine could be detected. All the nitrogen and chlorine of the chlorisatine had consequently distilled over in the form of chloraniline.

Action of Potassium on Chloraniline.

The decomposition experienced by the chlorinated base when its vapour is passed over fused potassium offers no great peculiarity. The potassium becomes incandescent and is converted into chloride and cyanide. A large quantity of carbon is also deposited.

[To be continued.]

LVII. *Reply to* Mr. Hopkins *on the Motion of Glaciers; with Reasons for avoiding further controversy. By* James D. Forbes, *Esq., F.R.S., Corresponding Member of the Institute, and Professor of Natural Philosophy in the University of Edinburgh* *.

TO enter into controversy with Mr. Hopkins would be to analyse all that he has written on the subject of glaciers, namely eighty-one octavo pages and thirty-six closely printed quarto ones, a majority of which are filled with mathematical reasonings intended to bear upon the subject.

As *replies* in controversy usually extend beyond the bounds of the reasonings replied to, it is easy to see that to do justice

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* Communicated by the Author.

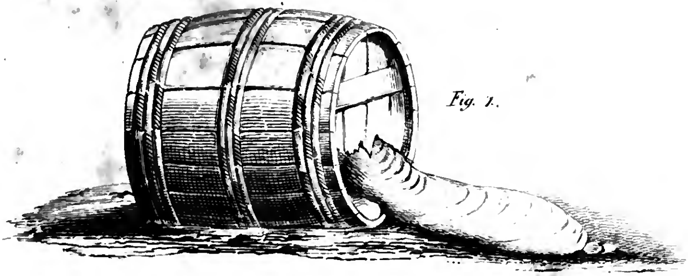


Fig. 1.

General View of the Broken Headed Barrel of Stockholm Pitch.

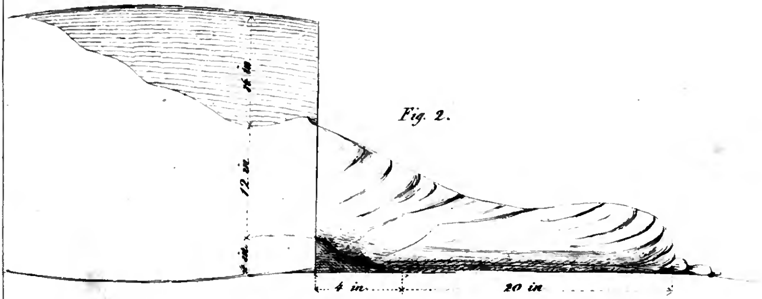


Fig. 2.

Elevation

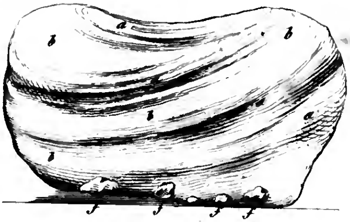
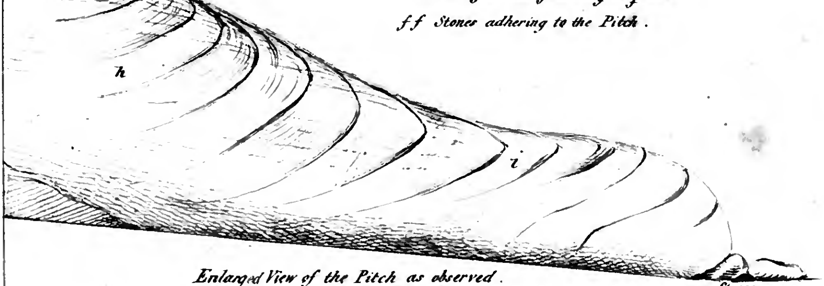


Fig. 4.

ff Stones adhering to the Pitch.

Fig. 3.



Enlarged View of the Pitch as observed.

Stones
Carried along by the
Stream or rather pushed.

Fr. Schenck Lith. Edinr.

to the subject would involve an extent of writing and printing not only incompatible with the limits of the Philosophical Magazine, but of the patience of the most determined reader.

But in point of fact such readers are few indeed. Mathematical and mechanical controversies (especially if either party has the dexterity to make them turn, as a last shift, upon the metaphysic of a term) are known, by sad experience, even to have outlived their originators, and to have been bequeathed to a second generation, without the world being a whit the wiser, or the combatants approaching mutual conviction. And in our own days of bustle and emulation it may be taken for granted that *such* controversies are absolutely unread, except by the parties and one or two partizans, at least when they have reached the first rejoinder.

I disclaim, therefore, the intention of entering into controversy with Mr. Hopkins, although far from saying that I will not defend my opinions against his, so far as they shall really be found to differ. But this I will do by an independent assertion of facts and observations and reasonings, which shall tend to the advancement of truth, as well as to the exposure of error on the part of others.

If we deduct from Mr. Hopkins's writings twenty-eight pages of a paper written on the Theory of Glaciers before he had made himself personally acquainted with the great *facts* about which he had to reason, there remain eighty-nine pages of matter before us. To enter into a controversy would, therefore, require an analysis of these. I will briefly state a few reasons, independent of the mere extent of the controversy, which seem to exempt me from a collision with Mr. Hopkins upon every point which he has thought it proper to discuss.

1. On account of the fluctuations of opinion they contain. If there be any, beside myself, who have read the whole of these eighty-nine pages, they know that, as regards many opinions and assertions, it would be easy to place Mr. Hopkins at issue with himself. Fortunately we are not altogether left to the interpretation of algebraical symbols and of English words, for both of these admit of much dexterity in their qualification, but the author has clothed his views in the earlier stage of authorship in diagrams sufficiently plain to admit of no dubiety. Of course, I only complain that Mr. Hopkins, whilst altering his opinions, does not allow that he is convinced by the arguments opposed to them. The first *rigour* of his views underwent some plastic change when my Travels appeared; and in proportion as his attention was forced to the mechanical theory there presented, they assumed successively a more and more ductile form; the very notions of plasticity

and the like, against which the author is nominally contending, being plentifully scattered over the whole of his later writings. Yet throughout, my work is quoted only as a repertory of facts, or as containing deductions fit only to be refuted.

2. Mr. Hopkins takes his stand on ground where no prudent theorist will follow him. Truly the problem of glacier motion is far from even approximate solution, if we are to wait until mathematicians shall have agreed upon the manner of acting of complicated forces upon all parts of a yielding mass, neither rigid nor absolutely fluid. Every one knows that such problems are beyond the compass of exact mathematics; but as in all such cases the great leading truths of mechanics enable us to point out with certainty whether phænomena of a palpable kind are at variance with them within reasonable limits of error or uncertainty. And still more, if, leaving conjecture and speculation, a theorist appeals to facts, and shows that viscous or plastic bodies do actually move in such and such a way, that they are torn asunder, or tend to slide over their own particles in certain directions; and then he draws an exact parallel in the case of the great problem in question, —such a theorist is entitled to claim for his speculations the character of a substantive discovery, although it must remain for a later stage of improvement to reduce the proximate laws inferred from *general* mechanical reasoning substantiated by careful experiments, to the accuracy which the later epochs of the exact sciences alone present.

I formally decline to rest the mechanical theory of glaciers upon such ingenious but tottering fabrics of argument as deductions from hypotheses respecting the constitution of matter and the effects of force on its integrant parts, which are as seductive by their apparent simplicity as they are known by experience to be inconclusive and erroneous when carried out into their legitimate consequences. That Mr. Hopkins may not think that in so expressing myself I mean to speak disparagingly of his talents, I would refer to the innumerable controversies of even the last forty or fifty years, and amongst mathematicians of the highest name, upon these same questions, of the mutual actions of bodies in certain states of aggregation (not rigid), the effects of distension and compression upon simple rods, and of the mutual attractions of fluids and solids; problems in themselves simple compared to the unravelling of the whole forces which a plastic body descending a trough exerts on its own molecules, and which yet were found sufficiently intricate, not merely to baffle mathematicians, for that is not the point, but to lead mathematicians of equal and superlative merit to results opposed to one another, although they professed to be reasoning from the same premises, and although in the merely deductive or mathematical part no error was supposed or admitted on either side.

Such investigations, though thorny and often unfruitful, are valuable as steps to better; but it is very rarely that the *foundation* or primary establishment of a great physical theory has been laid by them. It would be an inversion of the order of discovery were it so. It would be discovery by deduction instead of induction. The deductive part of the theory of glaciers will come, I trust, in time, but not until the cause of glacier motion has been generally admitted and understood.

I must do Mr. Hopkins the justice to state, that as he has remoulded the expression of his views several times*, in each leaving out something which rendered his views tangibly distinct from mine, and introducing expressions which might admit of an interpretation coincident with them, so he has also employed a simpler style of mathematical reasoning, and appears now to be convinced that any considerations of value employed in his earlier papers were reducible to most simple propositions of geometry and common mechanics. Were I to enter into a controversy point by point with Mr. Hopkins, I should have to show by what a formidable process he demonstrates the elementary truth, that a sliding body supported by one resisting point presses upon it with the force of its entire weight multiplied into the sine of the angle of inclination†. I should also have to show that the investigation of the temperature of the bottom of a glacier‡ is a ponderous display of dexterity, where the physical conditions of the problem are so entirely left out of sight that it conveys no positive information whatever on the subject. And as to the other "solutions" which Mr. Hopkins's two quarto papers contain—such as a mode of finding the curvature of a crevasse after any given time, and the actual inclination of its bed,—they can only be regarded as mathematical practice intended for his pupils, without any application, however remote, to the actual illustration of the theory of glaciers.

3. Mr. Hopkins, in his keenness to controvert the plastic theory, has been unfortunate enough to misstate that theory, and even the facts on which it, or any other, must be based.

First, he has misrepresented my theory; not willingly, I assume; but because, until lately it is evident that he had not read it so as to understand it, even approximately. In his second memoir in the Cambridge Transactions, Mr. Hop-

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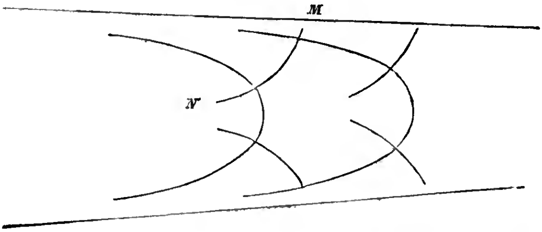
* In his *First* memoir in the Cambridge Transactions, in his *Second* memoir, and in his late papers in the Philosophical Magazine.

† First 4to memoir. ‡ Ibid. and Phil. Mag. January 1845.

kins thinks himself entitled to set aside a theory upon which I had spent so much time, exertion, and thought, *in three paragraphs* containing *two objections* (2nd mem. p. 4, 5). The one is the verticality of the crevasses, the other the want of evidence of the longitudinal compression of the glacier. The former has been fully explained by me in my Travels, p. 370, and more recently in my Seventh Letter on Glaciers; the latter will be found satisfactorily disposed of, on the evidence of facts not ascertained by me, in my Ninth Letter on Glaciers in the Edinburgh Philosophical Journal for April 1845, p. 333, &c. These were the *only* objections stated, on the bare statement of which Mr. Hopkins thought himself entitled to sweep away anything like a theory constructed by me, and, having cleared the field, to erect his own. These were the objections "easily refuted," to which I referred in my Eighth Letter on Glaciers; and though a less mild term might have been applied to them, Mr. Hopkins has thought my remark —intended to avoid offence in its expression—worthy of notice in his last communication.

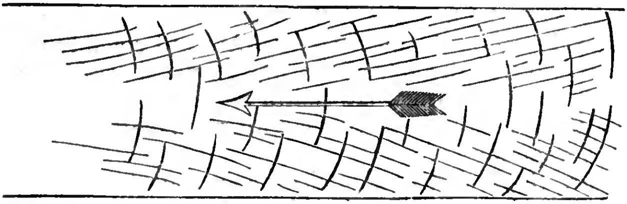
Another instance I must mention. Mr. Hopkins has employed the annexed figure in the Phil. Mag. for March,

Fig. 1.



p. 239, to prove that the structural veins cannot be perpendicular to the crevasses M N *throughout*, if the crevasses be convex upwards. Mr. Hopkins could hardly suppose me so dull as not to see that in the centre of the glacier the crevasse and the loop of the structure are *parallel*; but he might have recollected that the *loop* of the structure is formed by differential motion in a *vertical* plane, and that were he to take the directions of differential motion on my theory, estimated in a *horizontal plane*, they would be found to diverge towards the origin of the glacier; and the curve perpendicular to them would be convex in the same direction. This will be seen by constructing a *surface* everywhere normal to the *spoon-shaped* curves, which (as well as its section on the horizontal plane) will evidently be convex towards the origin. In the superficial motions of a common fluid, as determined by my experiments and models, the lines of separation are *almost mathematically perpendicular to the open crevasses*, which are convex towards the origin in fig. 2, which represents the lines of relative motion of a viscous fluid, reduced to the horizontal plane. Such I have also found to be the case in lava streams. Hence it is plain that when the horizontal forces are alone considered (as in all Mr. Hopkins's reasonings), the statement or empirical law is even more accurately true than I ever alleged it to be*.

Fig. 2.



In the *second place*, Mr. Hopkins has not had sufficient regard to accuracy in stating facts so as to support his own views, or to invalidate mine. I will not return to the question of the verticality of the transverse crevasses, which has been answered sufficiently; but I will take a more striking instance, lying at the very root of the matter. In the Phil. Mag. for March, p. 248, Mr. Hopkins affirms that "no experiments have been adduced to show that the plasticity of glacial ice is really greater than common inspection would lead us to suppose" (that is, quite insignificant); and yet in the next page he admits that the velocity of the centre of a glacier sometimes is *double that of its lateral parts*; and he does not deny that this is the result of plasticity, but he argues that it is *not sufficient* to explain the more rapid motion of the surface than the bottom. It comes therefore, as Mr. Hopkins has in more than one place admitted, to *a question of degree*; and if the degree of plasticity be sufficiently great, Mr. Hopkins will admit it to be the *cause of motion*†; but *half* the total motion is not a sufficient fraction in Mr. Hopkins's estimation. But could Mr. Hopkins be ignorant of the fact stated in the second paragraph of my Eighth Letter on Glaciers, that I had observed points in the transverse section of the glacier of Aletsch only 1000 feet apart, which moved with the relative velocities

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* There are circumstances in which the crevasses of glaciers approach the forms of figure 1, but in which the combination of several systems produces a general upward convexity. These I shall explain in another place.

† Phil. Mag., February 1845, p. 168.

of fourteen to three, or nearly FIVE TO ONE; and that at the side the motion had almost, if not entirely, vanished in that case? If it escaped his notice in reading the letter, does he not recollect that Dr. Whewell reminded him, at the last meeting of the British Association, that three to fourteen is not a small ratio? If Mr. Hopkins requires further proof than my assertions, let him see if the facts observed by my opponents in theory on the glacier of the Aar will come to his aid. He will find that the side of the Aar glacier moved regularly through one foot, whilst the centre moved through fourteen, or that the effect of plasticity was THIRTEEN-FOURTEENTHS of the entire motion*.

4. Mr. Hopkins does not object to an appeal to experiment to illustrate the complicated subject of the movement of glaciers. On the contrary, the foundation of his present speculations was an experiment to illustrate De Saussure's opinion, that the melting of the ice in contact with its bed by the heat of the earth greatly facilitates its movement. This fusion of the ice in contact with the soil is what Mr. Hopkins calls its "disintegration." It does certainly appear to me that this experiment was not more requisite to show the manner of progression of a mass of ice under these circumstances than experiments on semifluid or plastic bodies, and if possible on the mass of the glacier itself, were necessary to determine with certainty the manner in which their internal forces accommodate them to the circumstances of mechanical constraint under which they move. If calculation and estimation of forces was sufficient in the latter case, it appears to me to be not less so in the former; but if, on the other hand, the experiment of making ice-masses slide on a rough inclined plane was considered by Mr. Hopkins as the very touchstone of the true theory, and sufficient to put to silence any rival explanation, it would appear that the experiments which I have made upon plastic models and the directions of tearing and maximum distension of semifluid pastes, which are well-known to Mr. Hopkins and other readers of my writings on glaciers, deserved a deliberate consideration and reconcilement with the abstract investigations which Mr. Hopkins elaborated at leisure in his study, whilst others sought to test and correct theirs by a daily and hourly appeal to the multitude of phænomena which the glaciers present to those who live amongst them.

To explain the undoubted facts presented by these plastic models,—the external forms, concave at the origin, swelling

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* *Comptes Rendus*, Dec. 9, 1844; and my Ninth Letter on Glaciers, Edin. Phil. Journal, April 1845.

and convex at the termination, presenting at one view lines of veined structure in the direction of those existing on the glaciers, and in multitude and delicacy comparable to the slaty cleavage of the ice, with crevasses exactly at right angles to them, the radiating form of the crevasses at the termination, —in short all the leading facts which had been confidently predicted from general mechanical considerations before a single model had been made*; to explain this in a more precise and mathematical manner than had been attempted, would at least have been a problem suited to Mr. Hopkins's talents for such investigations, though it could add little force to the parallel already established between them and glaciers; but instead of this Mr. Hopkins never alludes to these models, except to find fault† with one deduction, in a note to the final chapter of my Travels. I willingly admit that the strict letter of the inference was incorrect, that "the direction of maximum distension of the particles must be, not parallel to the length of the glacier, but *in the direction of the branches of the elongated loops*‡." I admit that, though approximating, there is not a necessary coincidence in these directions; but the substantive fact, that the line of greatest force is not parallel to the axis of the glacier, but inclines towards its centre, remains established. It would plainly have been better if Mr. Hopkins, instead of dwelling on an oversight, common, as he states§, to himself and me, had attempted to explain something on which these models give indisputable evidence.

5. But as Mr. Hopkins does sometimes appeal to experiment, and has even quoted an experiment of Mr. Hodgkinson's in a note||, it were to be wished that he had distinguished more clearly cases in which an appeal to experiment is the *only* test of theory, such as those in which the undefined constitution of the mass is one of the most important elements of the problem. If a superficial foot of a body possessing a certain cohesion be wrenched by distorting the angles, I defy any analytical legerdemain to declare whether it will be torn in one line of fracture or a thousand. We know *by experience* that it depends upon the *degree* of cohesion and upon the *velocity* with which the distortion is effected, neither of which quantities enter into any of Mr. Hopkins's investigations. To take a familiar instance, a piece of sealing-wax in a warm room may be broken over sharply at one fracture, but if the

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* See my Third Letter on Glaciers, written in 1842, from the remote village of Zermatt, where my mechanical theory is stated as explicitly as I could do it now.—*Ed. Phil. Journal*, October 1842.

† Phil. Mag., March 1845, p. 237.

‡ Travels in the Alps, p. 378, *note*.

§ Phil. Mag., March, p. 238, *note*.

|| Ibid, p. 245.

distortion be gently effected, it takes place at an infinite number of points, and without visible discontinuity in the substance. It is no good to cite Mr. Hodgkinson's observation, that in the case of *cast iron* crushing force produced a single line of fracture, which, besides, was very far from being always the case. This shows only that under the circumstances of temperature and crushing force, iron was not a substance calculated to illustrate the analogy of glaciers, which might have been easily foreseen. It is evidently a begging of the question to say, that because iron did not yield at *many* points under a crushing force, *therefore* glacier-ice will not. The difference too is palpable; a cubic inch of ice may be *crushed* by a small weight, that of iron only by many tons. Then again, Mr. Hopkins, taking up an illustration of my own, says a sheet of paper whose edges are pulled in opposite directions tears but in one place*. This is true for the writing-paper which Mr. Hopkins uses, but let him trace the same paper in the successive stages of its manufacture until it becomes a pulp, he will find the lines of disseverment multiply, and at length become all but infinite, yet still dependent on the *slowness* of the action; the slower, the more numerous; yet in all Mr. Hopkins's mathematical investigations time never enters, and for obvious reasons could not be introduced. In one word, a rending pressure may produce in different bodies, all having the essential characters of solids in small masses, (1) an abrupt disseverment or fracture, (2) an imperceptible moulding or plasticity, or, (3) intermediate between these, a *contusion* or *bruise*, arising from the partial solution of continuity at innumerable points. Nay, all this may be seen in one and the same body at different temperatures, or in the same body at the same temperature, depending on the velocity or violence of the crushing force. The manner of the fracture therefore becomes "a question of degree," and cannot be specified without a specification of the viscosity or softness. It is not a "résultat de calcul;" it is a consequence of physical principles not yet compassed by algebra. Until Mr. Hopkins shall find the *equation to a bruise*, we must prefer our own conclusions to his.

I have shown that a glacier *does* move as a *bruised mass*,—by the yielding of different parts of its transverse section at a great, though finite number of points in its breadth †. Their

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* Phil. Mag., March, p. 242, *note*.

† See the experiments on the plasticity of glacier ice in my Eighth Letter on Glaciers. The angle which the lines of tearing make with the axis or side of a viscid stream will be found, I believe, to depend on its stiffness, or rather on the ratio of its viscosity to the rapidity of motion.

general appearance, and the unequal motion of the sides and centre which their existence alone renders *possible*, is the best evidence of their origin, especially as their general direction corresponds to what a "popular" view of the mechanism of the case would indicate, and most accurately to the observed results in the plastic models. This is the chief evidence which I have for the origin of the ribboned structure being "a forced separation of a half rigid mass*;" and it has this advantage over Mr. Hopkins's speculations, that he does not profess to offer *any* explanation† of this, the most elaborate and curious peculiarity of a glacier.

6. Yet something may be gleaned from Mr. Hopkins's writings, showing a disposition to yield even this point. Exactly in proportion as his statements have become more definite and tangible, and he has moderated the use of analysis, with which he at first attempted to carry his point by storm, his admissions to the viscous theory have become more frequent and explicit. The combination of this passage in the Second Letter (p. 166), "any internal constraint which might be superinduced by the peculiar motion of the glacier, would be relieved BOTH by transverse fracture and by the sliding of one part past another," with that in the Third Letter (p. 243), "I am far from supposing that the origin of this structure [the blue bands] is unconnected with the motion of the glacier," seems as nearly an admission of the theory which I have proposed of this structure as can well be imagined. Again, in the Second Letter, Mr. Hopkins admits plasticity as an attribute of glaciers with this qualification; "I mean that plasticity which shall require for its development the continuous application of force, possibly of great force, for a long period of time;" it is needless to say that it is of plasticity such as this that I have always spoken in the case of glaciers, where the forces are enormous and the motions slow. Yielding to the slightest force in the shortest time is not plasticity, but fluidity; and hence the term *secular* plasticity, by which Mr. Hopkins has attempted to raise a partition between his views and my own, is, as Dr. Whewell has justly observed in the letter in which he has generously defended my claims to this theory‡, not only superfluous, but improper, since the plasticity in question does not require *ages* or even *years* for its development, but is manifested (by the unequal motion of the centre and side of a glacier) in a few *days* or even *hours*.

7. Finally, Mr. Hopkins, admitting the plasticity, joins issue with me on the metaphysic of a word. According to him the

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* Travels, p. 377.

† Phil. Mag., March, p. 244.

‡ Ibid, p. 218.

plasticity is not sufficient in degree to constitute a *cause* of motion of the glacier; and here everything is a question of "degree." It is to this that all the rigour of mathematical demonstration comes at last! If the plasticity be "high" in degree, we are "justified in asserting that the effectiveness of gravity to put the mass in motion was due to that property;" but if the plasticity is small or "secular," we "have *no* right thence to conclude that the motion is effectively due to the plasticity of the mass*." Mr. Hopkins gives us no clue for finding, either by theory or experiment, where "high" plasticity ends, and "secular" plasticity begins. He is therefore plainly not in a state to affirm that the plasticity of glaciers is not sufficient to be a cause of motion. I leave it to metaphysicians to join issue with Mr. Hopkins on his idea of cause; here I will only state a *fact*.

It is this:—Lavas, every one admits to be more or less viscous fluids, and that their fluidity is indispensable to their motion. I have shown, in a paper lately communicated to the Royal Society, that whilst some lavas have been so fluid as to describe a space of *a thousand feet in a few seconds*, others have moved continuously and for a long period, even for *years*, at a rate not exceeding that of some glaciers, or *three feet a day*, and under. Since cases might be cited with every intermediate degree of speed, I shall leave it to Mr. Hopkins to define the effectiveness of plasticity in these instances.

When I speak of plasticity or viscosity, which is the same property in a higher degree, (I have used the term as it was originally suggested to me by Sir John Herschel for distinguishing my theory of glacier motion) as being the cause of the descent of glaciers, or effective in producing their descent, I mean *this* (and whatever my theory may be worth, I claim at least consistency in the interpretation of it, and definiteness in its statements), that *plasticity is a quality of glaciers without which they would remain stationary or descend in avalanches.* In this sense I understand it as a cause and not an accessory of motion.

Perhaps the following illustration will appear to the impartial reader almost a demonstration of this principle. I do not mean to offer it to Mr. Hopkins as such, because he knows of no demonstrations but those clothed in the language of analysis.

There is a glacier basin in the range of Mont Blanc called the Glacier du Talefre. Its outline is correctly represented in the annexed figure, as well as the relative dimension of the

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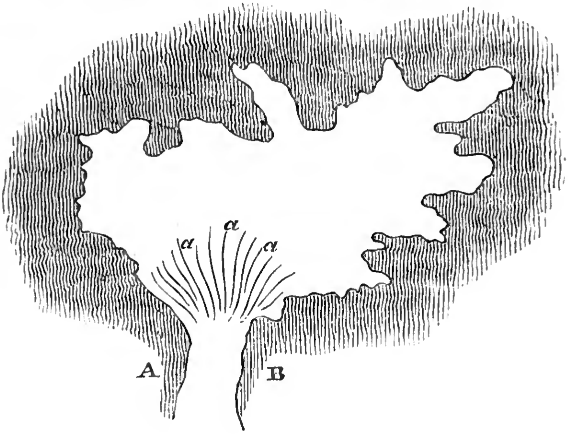
* Phil. Mag., February, p. 168.

mouth or outlet A B, by which it pours forth the mass of ice which it is annually unable to contain within its circuit. The breadth of the outlet is about 700 yards, whilst the greater diameter of the basin which it discharges is more than 4200 yards, or at least six times greater. Can it for one moment be imagined that any degree of *lubrication* of the bed of this cake of ice could drag it through the strait in question, even if its adhesion to the soil were absolutely nothing? The thing is impossible; it speaks for itself; the ice is compact and almost without fissures or open cracks; in the neighbourhood of the point of issue, structural bands of sliding discontinuity, *a a a*, are observed, pointing to the outlet, and corresponding to the directions in which the less retarded parts *must* slide over the lateral and most retarded; and which recall at once the analogous phænomena in a stream rushing through an orifice. The open crevasses, which commence a little above A B, are curved upwards towards the basin, or are perpendicular to the converging lines.



Glacier of Talefre. Scale $\frac{1}{75000}$.

It appears to me that from the point of view to which Mr. Hopkins has studiously confined himself in attempting to compare his theoretical deductions and the phænomena of glaciers, he has suffered himself to be led to results irreconcileable with such facts as those just mentioned, as well as with common and notorious experience. I have already declined the task of reconciling them. I shall be better pleased if Mr. Hopkins, leaving for awhile glaciers, and the peculiar set of ideas which by long cogitation have inseparably connected themselves in his mind along with them, will take up the consideration of the movement of fluids possessing viscosity and that of plastic bodies (which is an extension of the same case) under their own weight. Let him, for instance, consider the lines of greatest tension in a viscous fluid extending itself in a sloping trough when a sluice has been withdrawn, and the tendencies to separation by sliding of the parts over one another. Such I presume is the kind of mathematical investigations which Dr. Whewell means to recommend in his First Letter to Mr. Taylor*. The application to very viscous bodies will then not be difficult. It will at best be a "popular" view; nor will the mathematical expression of the forces and motions increase our knowledge (in the first instance) beyond what experiment, guided by general mechanical principles, has already unfolded or may unfold. The mathematician who values facts only, "pour donner prise au calcul," must be glad if his results can be shaped so as to represent what is already known to be experimentally true. But he must not hope to *predict* phænomena, or to consider that his symbolizing adds much weight to a mechanical theory. *That* must be the office of the inductive philosopher who can see his way in advance of the operations of his analytic tool. Nevertheless, by viewing the subject on more sides than one, something will be gained, and it may be hoped that the viscous theory will then be fairly treated and impartially compared with the facts of glaciers.

Thus Mr. Hopkins will satisfy both himself and me;—himself, by putting the glacier theory in a more mathematical form than I have given it; and me, by his becoming thenceforward (as I predict he will) a stanch supporter of the viscous theory, even should his support be coupled with the reservation that conviction being the result of his own proofs and not of mine, he claims in full the honours of a first discoverer.

Edinburgh, March 24, 1845.

Postscript.

When the preceding remarks were written, I believed that Mr. Hopkins had concluded, for the present, the exposition of his views; but as in the April Number of the Phil. Mag. he has taken special notice of the plastic models alluded to in the foregoing pages, I will state very shortly what this additional paper requires me to remark.

Mr. Hopkins has repeated my experiments with plastic bodies *urged by gravity* down narrow channels, in which, primarily, innumerable fine lines of separation due to the differ-

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* Phil. Mag., February 1845.

ential movements of points in the transverse section occur, and secondarily, lines of disruption or transverse crevasses perpendicular to the former. Now Mr. Hopkins denies that the former "ribboned structure" is produced by differential motion (p. 331, first paragraph). I have only to request him, when he next repeats the experiment, to watch the motions of the coloured particles on the surface, which is easily done, and he will perceive (with greatest distinctness in the higher parts of the model, where the tearing force is most violent) that the parts do visibly and mechanically slide past one another in such directions. No person who sees the model made, or has even been told how it was made, and inspects the "ribboned structure" on its surface, can, I think, unless influenced by previous theoretical views, entertain any other opinion.

The experiments, however, on which Mr. Hopkins lays most stress in his last paper, were made to corroborate "the conclusions at which he arrived by mathematical investigation," and which do not profess to imitate the physical conditions of a glacier, because the primary cause of its motion, *gravity producing fluid pressure*, is left out of account. The central part of a semi-solid mass is *mechanically* drawn away from the sides, without any provision being made for the continuity of the motion by a supply of the material from the source or origin of motion, as in my models and as in the real glacier. I cannot agree with Mr. Hopkins (p. 328), that "it is immaterial whether the motion be produced by gravity or any other cause." I hold it to be in the highest degree material. The results of such experiments, irrespective of the physical cause of the motion, may confirm Mr. Hopkins's merely abstract mechanical investigations, as no doubt on many points they do; but as to affording any illustration, whether positive or negative, of the actual problem before us, namely the *physical cause of glacier motion*, they are manifestly, and by his own admission, incompetent, since they display mere results of motion, independent of the cause.

Mr. Hopkins's experiment, however, illustrates two things; *first*, that forces exerted over large surfaces of even tolerably solid bodies, produce, not a *single* fissure or discontinuity, but a system of simultaneous parallel fissures (p. 329), whose number and distances must depend on the state of aggregation of the mass; *secondly*, that the phænomena about which Mr. Hopkins's reasonings and experiments are almost entirely conversant, are the effects of strains at the instant of the *commencement of motion* in a rigid mass; but they do not account for the *continuity* of the motion so as to satisfy the condition of the immense central, compared to the lateral, velocity. He is evidently reduced ultimately in theory (Phil. Mag., February, p. 166*), as in experiment (Phil. Mag., April, p. 331†), to admit that the central parts slide past the lateral parts, from whatever cause, and thence derive their superior rapidity of motion.

I have now entered more fully into this controversy than I at first intended; and as I do not think it likely that Mr. Hopkins can meet me on anything like new ground on this subject, I wish to decline any merely polemical writing. Any further explanations I have to give will, therefore, be contained in memoirs of the ordinary form and not controversial in their tendency.

Edinburgh, April 16, 1845.

LVIII. *On the Digestion of Vegetable Albumen, Fat and Starch.* By Robert D. Thomson, *M.D., Lecturer on Practical Chemistry in the University of Glasgow.*

[Continued from p. 328.]

BEING thus foiled in corroborating the deduction of Blondlot, that phosphoric is the free acid of the stomach, it was requisite to look out for another source of the acidity of the gastric fluid. It was therefore necessary to test the muriatic or hydrochloric acid theory, as propounded by the sagacity of Dr. Prout‡.

The contents of the stomach of a pig which had been fed on porridge two hours before being killed, were mixed with cold distilled water and filtered. Six fluid ounces of this fluid, which was quite clear and limpid, were introduced into a retort and distilled in a water-bath. The distillation occupied several hours; three ounces of fluid were distilled over, which possessed the peculiar smell of such fluids, and reddened infusion of litmus slightly. The three ounces of fluid remaining in the retort had a strongly acid reaction, and gave no appearance of acetic acid on the addition of chloride of iron. From twelve ounces of another portion of the same gastric fluid, four ounces of fluid were distilled over by the heat of the water-bath, which presented the same characters as those

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* "I have no doubt that the first fractures would be *transversal*, produced by the tension (R); and that after the mass had thus become considerably dislocated along its sides, the sliding of one finite portion past another would also take place."

† "When the central motion was continued long enough, the fissures along the flanks became more irregular and ran into each other, after which the central portion moved nearly as a continuous mass, sliding past the narrow lateral portions, from which it was severed on either side by the lateral fissures running into each other as just described."

‡ See Phil. Mag. S. 2. vol. iv. p. 3. and 120.

of the distilled fluid just described; but in neither instance could a trace of hydrochloric acid be detected by nitrate of silver.

With the liquor remaining in the retort, three experiments were made to determine the possible amount of free hydrochloric acid, on the supposition that the preceding experiment did not serve as evidence to prove the impossibility of its presence. Three equal portions of the fluid were measured out, to the extent of two fluid ounces in each portion.

1. To the first portion, a solution of nitrate of silver was added until a precipitate ceased to fall; pure nitric acid was then mixed with the liquid, and the temperature raised to the boiling-point. The precipitate was filtered, washed and weighed.

2. The second portion was evaporated to dryness and ignited; the residue was dissolved in water and precipitated by nitrate of silver, the solution being acidulated with nitric acid and brought to the boiling-point.

3. The third portion was exactly neutralized with caustic potash, evaporated and ignited; the residue dissolved in water, and the solution precipitated by nitrate of silver.

The results of these experiments are indicated in the following table in grains:—

Experiments.	Weight of Chloride of Silver.	Weight of Chlorine.	Weight of Hydrochloric Acid.
1st.	7·81	1·95	2·00
2nd.	7·17	1·79	1·84
3rd.	7·97	1·99	2·04

The atomic weights here employed are Dr. Thomson's:

Oxygen $=1\cdot$
Hydrogen $=\cdot125$
Chlorine $=4\frac{1}{2}$
Silver $=13\frac{1}{2}$

The correspondence between the first and third experiments shows that in the first no organic substance had been in union with the silver, which was precipitated by chlorine alone, and that there is no evidence from these results of free muriatic acid being present. The conclusion is the reverse, since if any free muriatic acid had been neutralized by the potash the third experiment ought to have given an inferior quantity of chloride of silver, because the sal-ammoniac ought to have been sublimed. The potash which was added, I conceive, in the third experiment, united with an organic acid; the salt formed was decomposed by the incineration, and the potash united to the chlorine previously in union with ammonia; for that sal-ammoniac or a volatile muriate was present is proved by the second result.

It is well known that Dr. Prout by similar experiments drew the conclusion that free hydrochloric acid was present in the stomach. That distinguished chemist however omitted the first experiment which I have described. Indeed he could not have made it successfully under the circumstances in which he operated, because the gastric juice in his experiments was not exposed to a heat that could coagulate and separate the albuminous matters, which would then have combined with the oxide of silver and have complicated the result. In the gastric juice employed in the preceding researches, however, nothing was present but soluble starch or dextrin and sugar, which formed obviously no compounds with silver, insoluble in boiling nitric acid. It is possible, therefore, in the experiments of Dr. Prout, who determined the total amount of chlorine in the gastric fluid by supersaturating with potash, igniting and precipitating with nitrate of silver, that he had formed cyanide of potassium, which would precipitate cyanide of silver along with the chloride, unless the precaution were taken not merely to *add* nitric acid, but to *boil* the solution after the addition of the acid, since cyanide of silver is insoluble, or at least not wholly decomposed by cold nitric acid. I offer this explanation, originally proposed by Leuret and Lassaigne, because from my knowledge of Dr. Prout, I am quite certain his experiments were most accurately made. Indeed, the testimony of all succeeding experimenters who have obtained the same results is sufficient to establish his accuracy without the addition of any confirmation on my part. It is even possible that, in cases where the food is different, the acid may be of the nature described by Dr. Prout.

The experiments which have been detailed seem to demonstrate that no free hydrochloric acid existed in the stomach of the animal, under the circumstances described, since no acid could be distilled over at a temperature greatly above that at which this acid boils when sufficiently concentrated, while the fluid in the retort became more intensely acid in proportion as the distillation proceeded. A portion of the liquid from the retort was evaporated to dryness and heated to a temperature exceeding 300° without giving out acid fumes; the residue was digested in water, and still retained an acid reaction. The solution of this residue was easily saturated by carbonate of lime, and was not precipitated by chloride of calcium, indicating the absence of biphosphate of lime (contrary to the views of Blondlot), and likewise of free sulphuric acid.

In another experiment the gastric juice was evaporated to dryness in the water-bath, and treated with alcohol and oxide of zinc with the necessary precautions; prismatic crystals were obtained corresponding with lactate of zinc, but in too minute quantity to admit of analysis, the only demonstrative argument. The preceding experiments appear to show, however, that the free acid of the stomach, in the digestion of vegetable matter at least, of all the known acids, alone corresponds with the lactic. To determine the nature of the volatile acid, which however appears to be present always in minute quantity, a portion of gastric fluid was distilled, and the product was obtained in three distinct receivers. Their characters, as determined by infusion of litmus, were as follows:—

		Infusion of litmus.
1st product of distillation amounted to ...	1 oz.,	bright red colour.
2nd	1½ oz.,	paler than preceding.
3rd	1 oz.,	slight red colour.

From these observations, it would therefore appear that the greatest amount of volatile acid was carried over at first, and that as the distillation proceeded its amount in the retort gradually diminished, indicating that the acidity was not due to the decomposition of lactic acid or its eduction by the vapour, but rather to the presence of acetic acid. The quantity present was however trifling, since the distilled product of a large amount of gastric fluid could never be detected in a state of effervescence on the addition of carbonate of soda.

Dextrin and Soluble Starch found in the Stomach in the Digestion of Starch.—I have already stated in a previous part of this paper that I was unable to detect any traces of starch in the serum of the blood. It was therefore necessary to return to the stomach, and to observe the chemical changes to which the starch was subjected in that viscus. When an animal is fed on porridge, if water be added to the contents of the stomach, the mixture well-stirred and then allowed to stand at rest, the supernatant liquor produces a blue colour with tincture of iodine; but if the liquid be filtered, the colour obtained by mixing the solution of iodine with the filtered liquor is red, indicative of the presence of dextrin, or one of the varieties of soluble starch. I have sometimes found, however, that starch has existed in solution in the gastric fluid even when neither a blue nor a red colour was indicated by iodine. This substance I have isolated by boiling the gastric fluid in order to coagulate the albumen, evaporating to dryness in the water-bath, and then removing the sugar and oil by means of alcohol. The substance thus obtained gave no decided indications with tincture of iodine previous to isolation when dissolved in water, and therefore corresponds with that variety of soluble starch which has frequently been described by chemical writers. It appeared a point of some importance to ascertain whether the transition of starch into dextrin takes place at once in the stomach, or whether the change commences before the food is swallowed. I accordingly prepared a quantity of porridge by boiling it for upwards of half an hour with distilled water, and on filtering it I obtained distinct evidence of the presence of dextrin in the filtered liquid. It may perhaps therefore be legitimate to infer that one of the important purposes to be acquired by cooking starch is to facilitate its conversion into soluble dextrin, and that other form of soluble starch already described; and hence the importance of the boiling being continued for a considerable space of time, when some of the harder species of amylaceous food, as rice, sago, tapioca, are used as articles of diet, and especially when they are administered to the delicate stomachs of the sick.

The soluble starch was separated in the manner already detailed, and was found to possess the following constitution when subjected to organic analysis:—

7·29	grains gave	9·86 CO_2
7·11	...	9·54 CO_2
13·93	...	7·97 HO
7·29	...	4·05 HO
7·11	...	4·03 HO

The result of these analyses per cent. is as follows:—

	1st.	2nd.	3rd.
Carbon		36·88	36·59
Hydrogen...	6·31	6·17	6·29
Oxygen......		56·95	57·12

This composition does not agree with that of starch, except with wheat starch, in the carbon before that substance has been dried; but according to the analyses of Prout, the hydrogen in wheat starch is greater in amount than in the present case. The carbon in the substance under consideration agrees with that of sugar of starch, but the hydrogen is much inferior; and the three experiments detailed agree so closely in reference to the hydrogen, that there can be little doubt of the accuracy of the results, more especially as the last analysis was made with oxide of copper and chlorate of potash, and the matter subjected to analysis appeared to be completely burned. The substance appears to have possessed some intermediate characters between those of sugar and starch.

Sugar in the Blood.—The preceding results show that a matter closely corresponding with sugar is found in the stomach. Experiment was scarcely necessary to prove that sugar exists in the stomach, since as all flour and meal contain sugar, it is obvious that when these articles of diet are swallowed, sugar must be present in that viscus. But as it exists in the stomach, it is natural to expect that it should pass into the blood-vessels, and be capable of being detected in the mass of the sanguineous circulation. That fermentation can be excited by yeast in many of the fluids of the body, was long ago shown by Tiedemann and Gmelin. Dr. A. Buchanan during the course of last winter obtained traces of carbonic acid from serum by fermentation, and I repeated the experiments with success; but I found it necessary to be careful in such a delicate experiment that no source of fallacy should be present to complicate the result, and that in order to arrive at a demonstration of the presence of sugar in the blood, it would be requisite to resort to weighing. For this purpose I employed what may be termed a saccharometer. It consists of two light phials or flasks connected by a bent tube, one or both of which possess a safety-tube. The serum or fluid to be examined is weighed out in one of the flasks, which is placed in a vacuum, or in hydrogen gas, to remove any carbonic acid in solution, while into the other is introduced a solution of barytes, the bent tube dipping considerably under the surface of the barytes solution. Yeast being mixed with the serum, the connexion between the flasks is established, and the whole apparatus placed in a warm atmosphere (70°). Fermentation speedily begins if sugar is present, and the carbonic acid as it passes over precipitates the barytes in the form of carbonate. In twelve hours the action will be at an end; the flask containing the serum is then to be heated to carry over any CO_2 which may remain in its atmosphere by means of vapour. I prefer this method to using a safety-tube in the second phial, because it is difficult to wash off the carbonate of barytes which is apt to adhere to the ends of the tubes.

The carbonate of barytes is then to be thrown on a small filter covered with a plate of glass, washed, ignited, and weighed. Every 49 grains of it are equivalent to 22·5 of sugar existing in the blood[*].

By means of this process, I found that the serum of a pig, which had been starved for at least twelve hours, then fed on

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[*] The results contained in this paper were communicated to Prof. Liebig in September 1844. He then suggested the mode of detecting sugar by means of bile and sulphuric acid. I found this test to be ambiguous, as sulphuric acid alone produced a red colour in diabetic urine and in other animal fluids.

porridge and killed three hours afterwards, afforded by the fermentation of 1000 grains ·80 carbonate of barytes, = 5·6 grains per pound, or to 2·57 grains of sugar.

In another experiment the serum of the pig was employed from whose stomach the soluble starch was extracted.

	520·27	grains of serum gave		
	·68	...	carbonate of barytes,	
equivalent to	9·01	...	...	per pound,
...	4·19	...	sugar	

and estimating the quantity of blood in the human body at 24 pounds, by the first experiment we should have 61·68 grains in the sanguineous circulation, and by the second 100·56 grains. A pound of urine in diabetes I have found to contain 14·361 grains of sugar. Whether the sugar is burned in the lungs and capillaries, or is previously converted into fat by the agency of the casein of the blood, it remains of course to determine; but the experiments of Pelouze have shown us how this may be effected; and the present determinations seem to form a connecting link between starch and fat, which render the views of Liebig, in reference to the supply of animal fat being derived from starch, still more plausible than was at one time suspected.

The experiments detailed in this paper appear to lead to the following conclusions:—

1. That vegetable albuminous matter and fat when digested can be detected in the blood.

2. That if any free hydrochloric acid exist in the stomach of animals during the digestion of starch, the quantity must be so minute as to be scarcely appreciable.

3. That an acid exists in the stomach of animals fed on starch, which corresponds more nearly with lactic acid than with any other known acid.

4. That dextrin and soluble starch exist in the stomachs of animals which have been fed on farinaceous diet during and for some time after digestion.

5. That sugar exists in the blood of animals which have been fed on starch. And

Lastly. It is hoped they have in some measure contributed to throw a ray of light upon the important function which starch fulfils in the animal œconomy.

LIX. *On the Necessity of Three Fundamental Equations for the general analytical determination of the Motion of Fluids. By the* Rev. J. Challis, *M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

THE communications which I have made from time to time to this Journal on the analytical theory of hydrodynamics have had particular reference to the mathematical *principles* of this department of science. In the course of my inquiries I have been led to the conclusion, that those principles, as commonly received, are essentially defective. I have pointed out wherein I conceive them to be defective, and what appears to be necessary to complete them. These views have not been refuted, nor, as far as I am aware, have they been assented to. I propose, therefore, in this communication to collect together, in as brief a compass as possible, the chief arguments I have employed, and to add others in support of them, for the purpose, more especially, of substantiating the main result I have arrived at, on which, in fact, all that I have advanced at variance with the views of other writers depends, viz. the necessity of a *third* general equation in hydrodynamics.

It will, I suppose, be granted, that in whatever manner a mass of fluid is in motion, an unlimited number of surfaces may be conceived to be drawn at each instant, cutting the directions of motion at right angles. Let two such surfaces be drawn at a given instant indefinitely near each other, and let one pass through a point P given in position. On this surface describe an indefinitely small rectangular area having P at its centre, and having its sides in planes of greatest and least curvature, and let normals be drawn at the four angular points. By a known property of curve surfaces these normals will meet, two and two, in the two focal lines situated in the planes of greatest and least curvature. Let the small area, of which P is the centre, be m^2, and at a given time, t, let r, r' be the distances of the focal lines from P, or the principal radii of curvature. Produce the normals to meet the other surface in four points, and join these points so as to form an area on the second surface corresponding to the area m^2 on the first. Then, if the two areas be supposed to be ultimately parallel, and to be separated by a given small interval δr, the second area is $m^2 . \frac{(r+\delta r)(r'+\delta r)}{r r'}$, at the given time. As in general the direction of motion through the fixed point P varies continually, the normal surface through that point will also vary its position with the time. The positions of the

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[*] Communicated by the Author.

focal lines will vary as well on this account as on account of any changes in the magnitudes of r and r'. Let α and β be the velocities of the focal lines estimated in the direction of the radii of curvature, and considered positive when their motions are towards P. Then, at the time $t+\delta t$, the values of r and r' become $r-\alpha\,\delta t$ and $r'-\beta\,\delta t$; and assuming the area m^2 to be constant, the elementary area on the second surface is

$$m^2 \,.\, \frac{(r+\delta r-\alpha\,\delta t)\,(r'+\delta r-\beta\,\delta t)}{(r-\alpha\,\delta t)\,(r'-\beta\,\delta t)},$$

or $$m^2 .\, \frac{(r+\delta r)\,(r'+\delta r)}{r\,r'} \,.\, \left(1+\frac{\alpha\delta t\delta r}{r^2}\right)\left(1+\frac{\beta\delta t\delta r}{r^2}\right) + \&c.$$

Hence, by omitting quantities of the order $\frac{\alpha\,\delta\,t}{r} \times \frac{\delta\,r}{r}$, the result is the same as if the positions of the focal lines and magnitudes of the radii of curvature had not varied. If now V and ϱ be the velocity and density of the fluid which passes the area m^2, and V' and ϱ' the velocity and density of the fluid which simultaneously passes the other area, and these quantities be supposed (as is allowable) to be uniform during the time $\delta\,t$, then considering the velocity positive when directed from the focal lines, the increment of matter between the areas in the time $\delta\,t$ is

$$-\,m^2 \,.\, \frac{(r+\delta r)\,(r'+\delta r)}{r\,r'}\,\varrho' V' \delta t + m^2 \varrho V \delta t,$$

or $$-\,m^2\,\delta\,t \left\{\frac{d.\varrho V}{d\,r} + \varrho\,V\left(\frac{1}{r}+\frac{1}{r'}\right)\right\}\delta\,r,$$

by neglecting quantities of an order already neglected. If $\delta\,\varrho$ be the increment of density in the time $\delta\,t$, the increment of matter is also $m^2 \delta\varrho\,\delta r$. Equating these two values and passing from differences to differentials, we have

$$\frac{d\varrho}{d\,t} + \frac{d.\varrho V}{d\,r} + \varrho\,V\left(\frac{1}{r}+\frac{1}{r'}\right) = 0. \quad . \quad . \quad . \quad (A.)$$

We have thus been led by elementary considerations to a general hydrodynamical equation. On reviewing the foregoing reasoning it will be seen to rest on two principles, on the constancy of mass of each elementary portion of the fluid, and on the existence of surfaces normal to the directions of motion and possessing the properties of surfaces of continuous curvature. It is also assumed in the course of the reasoning that two such surfaces indefinitely near each other are ultimately parallel, and (since α and β are finite velocities) that the radii of curvature of a surface passing through a given point do not vary *abruptly*, either in magnitude or direction.

These, however, are merely suppositions of continuity, necessary to be made that the subject may admit of the application of mathematical calculation, and virtually contained in the supposition of the existence of normal surfaces. Not being able to discover any objection that can be raised against the principles and the reasoning which have conducted to equation (A.), I shall assume this equation to be true.

Now the only general equations of hydrodynamics hitherto recognised are the two following:—

$$\frac{d\varrho}{dt}+\frac{d\,.\,\varrho u}{dx}+\frac{d\,.\,\varrho v}{dy}+\frac{d\,.\,\varrho w}{dz}=0, \quad . \quad . \quad . \quad (1.)$$

$$\left.\begin{array}{l}(dp)=\varrho\left\{\left(\mathrm{X}-\left(\frac{du}{dt}\right)\right)dx+\left(\mathrm{Y}-\left(\frac{dv}{dt}\right)\right)dy\right.\\ \qquad\qquad\left.+\left(\mathrm{Z}-\left(\frac{dw}{dt}\right)\right)dz\right\}\end{array}\right\}, \quad . \quad (2.)$$

which are so well known that I need not explain the signification of the symbols. These equations are perfectly general in their character, and if they are sufficient in number for the general consideration of fluid motion, the equation (A.) must either be identical with one of them, or must be derivable from the two combined. It cannot be identical with (2.), because it does not involve the impressed forces X, Y, Z. For the same reason it cannot be deduced from a combination of (1.) and (2.); and it is not identical with (1.) for the following reason:—In the differential coefficient $\frac{d\,.\,\varrho \mathrm{V}}{dr}$ of equation (A.), the variation of coordinates is necessarily from one point to another of a line of motion, while the differential coefficients of equation (1,) are subject to no such limitation.

It follows, therefore, that another general equation, independent of the impressed forces, must exist, by the combination of which with equation (1.), equation (A.) may result. The considerations we have gone through will guide us in the investigation of it. It is well known that if $\frac{1}{\lambda}$ be a factor which makes $udx+vdy+wdz$ integrable,

$$\frac{u}{\lambda}dx+\frac{v}{\lambda}dy+\frac{w}{\lambda}dz=0$$

is the general differential equation of surfaces normal to the directions of motion. Let the integral of this equation be $\psi(x,y,z,t)=0$. We thus have an equation embracing all such surfaces at all times. It must, however, be observed that the function ψ cannot have this comprehensive character in all cases of motion, unless it may be regarded as an arbitrary or discontinuous function. The same remark applies to the functions that u, v, w are of the coordinates and the time. To represent the velocities at all points and at all times, whatever motion be given to the fluid, they must be arbitrary or discontinuous functions. As, however, in the investigation of the equations (1.) and (2.) u, v, w were assumed to be functions of constant form for at least indefinitely small variations of the coordinates and the time (for they would not otherwise be subject to mathematical reasoning), the same supposition must be made with respect to the function ψ. Hence δx, δy, δz and δt, being indefinitely small variations, we have for a given form of the function,

$$\psi\,(x, y, z, t) = 0,$$

and $$\psi\,(x+\delta x,\ y+\delta y,\ z+\delta z,\ t+\delta t) = 0.$$

Expanding the second equation to first powers of the small quantities, and putting for shortness' sake, ψ for $\psi\,(x, y, z, t)$, the result is, by reason of the first equation,

$$\frac{d\psi}{dt}\delta t + \frac{d\psi}{dx}\delta x + \frac{d\psi}{dy}\delta y + \frac{d\psi}{dz}\delta z = 0.$$

It is plain that so far this equation is nothing more than an expression of geometrical continuity, and that the variables δx, δy, δz are in no way related to each other. We may, therefore, suppose that $\delta x = u\delta t$, $\delta y = v\delta t$ and $\delta z = w\delta t$; and the equation then becomes

$$\frac{d\psi}{dt} + \frac{d\psi}{dx}u + \frac{d\psi}{dy}v + \frac{d\psi}{dz}w = 0. \quad . \quad . \quad . \quad (3.)$$

It now expresses that the normal surfaces to the directions of the motion of the *same* elementary particle in successive instants are geometrically continuous, and consequently may be called the equation of continuity of the *motion*.

We have now arrived at the third general equation, and might proceed to test the truth of it by employing it with equation (1.) in the investigation of equation (A.). As I have already given the mathematical process for this purpose in the Cambridge Philosophical Transactions (vol. vii. part iii. p. 385) and in the Philosophical Magazine (S. 3. vol. xx. April 1842), it will be unnecessary to adduce it here.

The foregoing argument establishes the *truth* of equation (3.). Being true, it may be presumed to be as *necessary* for the general consideration of fluid motion as equations (1.) and (2.). This, however, will more distinctly appear by showing, as I proceed to do, the defective state of analytical hydrodynamics when only equations (1.) and (2.) are made use of.

Let the fluid be incompressible and the motion be parallel to the plane containing the rectangular axes of x and y; then by (1.), $\frac{du}{dx}+\frac{dv}{dy}=0$. Supposing $udx+vdy$ to be an exact differential ($d\phi$), equation (1.) becomes $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0$, which by integration gives

$$u=\mathrm{F}(x+y\sqrt{-1})+f(x-y\sqrt{-1}),$$
$$v=\sqrt{-1}\{\mathrm{F}(x+y\sqrt{-1})-f(x-y\sqrt{-1})\}.$$

In the same case, as is known, the integral of equation (2.) is $p=\phi(t)-\frac{d\phi}{dt}-\frac{u^2+v^2}{2}$. As no reason appears for limiting the arbitrariness of the functions in the above values of u and v, let us assume that

$$\mathrm{F}(x+y\sqrt{-1})=\frac{m}{2}(x+y\sqrt{-1}),$$

and $$f(x-y\sqrt{-1})=\frac{m}{2}(x-y\sqrt{-1}).$$

Then $$u=mx,\quad v=-my,\text{ and }\frac{d\phi}{dt}=0.$$

Hence $$p=\phi(t)-\frac{m^2}{2}(x^2+y^2).$$

It follows from this result, by putting $p=0$, that the boundary of the fluid may at *all* times be a cylindrical surface; but it is impossible this can be true, because the particles at the surface are all moving with the same velocity in directions making different angles with the surface. What then is the reason that we have been brought to an absurd result by a process all the steps of which are legitimate if two fundamental equations are sufficient? The answer simply is, that the third equation cannot be dispensed with, as will be made apparent from the argument that follows.

When the two recognised general equations are alone made use of, it is not possible to treat any instance of fluid motion for which $udx+vdy+wdz$ is not an exact differential. I know of no writer who has attempted to indicate generally the process to be followed when that condition is not fulfilled. Neither has any general rule been given to distinguish the cases in which it is allowable to assume $udx+vdy+wdz$ to be an exact differential. Lagrange asserts that the assumption may be made when the motion is small, and when it commences from rest. These rules I have proved to be without foundation (see Phil. Mag., S. 3. vol. xxi. pp. 106 and 426, and Number for February 1844, p. 94). They are not even mentioned in Poisson's Treatise on Hydrodynamics (2nd edition), which may be regarded as the standard work on this subject; yet they are made use of to the present day without scruple, apparently on the mere authority of Lagrange's assertion. Thus it has happened that an essential step has been taken in the application of the general equations without any reasons to justify it. The new equation which I have discovered is absolutely necessary for treating questions in hydrodynamics, for which $udx+vdy+wdz$ is not integrable without a factor; and, what is very important, it furnishes a criterion for distinguishing the cases in which that quantity is integrable of itself. For, since by hypothesis,

$$\frac{u}{\lambda}dx+\frac{v}{\lambda}dy+\frac{w}{\lambda}dz=(d\psi),$$

it follows that

$$u=\lambda\frac{d\psi}{dx},\quad v=\lambda\frac{d\psi}{dy},\quad w=\lambda\frac{d\psi}{dz}.$$

And by substituting in (3.), we have

$$\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)=0,$$

which is the additional equation necessary in consequence of the additional variable λ. Now if s be a line drawn at a given instant in the direction of the motion of the particles through which it passes, and V be the velocity at a point $x\,y\,z$ of this line, we shall have

$$\mathrm{V}^2=\lambda^2\frac{d\psi^2}{ds^2}=\lambda^2\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right),$$

and the foregoing equation becomes

$$\frac{d\psi}{dt}+\lambda\frac{d\psi^2}{ds^2}=0.$$

Consequently, if λ be a function of the time only, which is the case when $udx+vdy+wdz$ is integrable without a factor, this equation gives by integration ψ, a function of s and t; at the same time V or $\lambda\frac{d\psi}{ds}$ is a function of s and t. The criterion we thus arrive at is, that ψ and V are both functions of the same variables s and t. I have proved in the Cambridge Philosophical Transactions (vol. viii. part i. p. 36), that this condition is not fulfilled unless the motion be rectilinear.

The reason of the contradictory result in the instance adduced above will now be apparent. It is there first supposed that $udx+vdy$ is an exact differential, and then forms are given to the functions F and f, which, as they apply to curvilinear motion, are inconsistent with that supposition. The use of equation (3.) would have prevented falling into this inconsistency.

The objection urged against this instance applies in full force to another which I have frequently referred to in the pages of this Journal, viz. Poisson's solution of the problem of resistance of the air to the motion of a ball-pendulum. That solution, however admirable in some respects it may be, rests on the *gratuitous* assumption that $u\,dx + v\,dy + w\,dz$ is an exact differential. The reasoning I have gone through shows that the motion deduced is inconsistent with that assumption (see Cambridge Philosophical Transactions, vol. viii. part i. p. 41).

The existence and the necessity of a third general equation in hydrodynamics being, as it appears to me, established, I shall proceed at a future opportunity to make some applications of it.

Cambridge Observatory, April 10, 1845.

LX. *Remarks on* Mr. Hopkins's *Reply to the previous Remarks on Glacier Theories. By* Dr. WHEWELL.

To Richard Taylor, Esq.

DEAR SIR,

IN my remarks on glacier theories, printed in the Philosophical Magazine for February and for March, I did not mention Mr. Hopkins's name. I abstained from doing so, in order to avoid, as far as was possible, any personal controversy, such as he deprecates in a Reply to those remarks, printed in the Philosophical Magazine for April. Nevertheless, in that Reply, he speaks of a departure from the rules of strict courtesy, which charge he applies to me by name. Apparently he was led to make this charge, through a belief that, by those who had read his Letters on glaciers, my remarks could not fail to be understood as applicable to him. This is possible; but I am still unable to see how I could have made, in a more inoffensive manner, the remarks which justice to Mr. Forbes seemed to me to require. And I cannot but think Mr. Hopkins would have better promoted the object of avoiding personal controversy, if he had replied, supposing he thought reply necessary, in the same *impersonal* manner. This would have thrown no difficulty in the way of his showing my remarks to be inapplicable to him, if he could have done so. And if he had done this, I think it most likely that none of your readers would have discovered the "sarcasm" of which he complains.

In my remarks, I described four steps which a theorist might make, in passing gradually from the doctrine of a solid to that of a plastic glacier. The first step is, to suppose the solid glacier divided into longitudinal strips by arbitrary separations; the second is, to allow a possible separation by sliding at all points of the mass; the third is, to allow that the flanks of the glacier are plastic; the fourth is, to allow the glacier to have a *secular* plasticity. Each of these suppositions has been made, in order, by Mr. Hopkins; having been previously excluded by him: and in this manner he has brought his solid theory to agree quite, or nearly, with the plastic theory. In making these changes of his hypothesis, I conceived that he had been influenced by a growing perception of the evidence of Prof. Forbes's theory; and I thought that in his mode of introducing his hypothesis, he was likely, however unintentionally, to mislead his readers, as to the obligations which the theory of glaciers owes to Prof. Forbes. It appeared to me, therefore, that justice required some one to warn the readers of the Philosophical Magazine of this danger.

Having done this, I shall not enter further into the subject in your pages. Mr. Hopkins invites me to discuss his mathematical proofs on this subject. Such a discussion would have too much the character of a personal controversy between us. And I must add, that I do not see any probability that such a discussion would either throw light on the subject, interest the public, or bring us to agreement. Mathematicians will judge for themselves of Mr. Hopkins's demonstrations. They will also judge for themselves whether, when a plastic mass is drawn by a force in any direction, it appears by simple mechanical considerations, that the tendency of the parts to separate by sliding will be in the direction of the force, and the tendency to separate by rupture will be perpendicular to the direction of the force. They will further judge, whether such a motion of a plastic mass, as Prof. Forbes supposes in a glacier, will produce separations of the mass in directions, such as those of the structural bands which Prof. Forbes discovered.

I will make one remark. I had objected to the term *secular* plasticity, because the plasticity of glaciers produces a conspicuous effect in a few days. Mr. Hopkins says, that "no such application of the term was contemplated." I reply that such an application of the term describing the plasticity of glaciers *ought to have been* contemplated, after Prof. Forbes's decisive observations showing that the effect *is* conspicuous in a few days.

I will only add, with reference to my supposing that Prof.

Forbes included, in his views, the sliding of glaciers by melting, that I did so, on the same ground on which Mr. Hopkins denies that De Saussure regarded a glacier as a rigid mass; namely, that to suppose otherwise, would be to attribute an absurdity to a distinguished and philosophical traveller. With regard to Mr. Hopkins's assertion, that we may as justly include in De Saussure's theory the true part of Prof. Forbes's, as in Prof. Forbes's the true part of De Saussure's, I suggest, that Prof. Forbes wrote sixty years later than De Saussure, and had read and carefully studied De Saussure's writings; and that this makes a great difference in the chance of one having adopted the true part of the other's theory. Prof. Forbes's theory includes what was true in that of De Saussure, in the same way that Copernicus's theory included what was true in that of Ptolemy. Mr. Hopkins has used the word "appropriation" in reference to this subject; but nothing has been said which is any excuse for the introduction of such a term.

I am, dear Sir,
Your faithful and obedient Servant,
W. WHEWELL.

Trinity College, Cambridge,
April 12, 1845.

LXI. *Notice of a Tertiary Deposit lately discovered in the neighbourhood of Belfast.* *By* JAMES BRYCE, Jun., *M.A., F.G.S.*

IN the winter of 1842–43, a shelly deposit was discovered at Belfast in excavating the lower basin for the new waterworks. A short notice of it, drawn up by Mr. Hyndman and myself, with a list of the shells, was at Captain Portlock's request furnished to him for insertion in the appendix to his Report on the Geology of Londonderry, Tyrone and Fermanagh, then passing through the press. In the cuttings for the Cavehill railroad, made within the last few months, the deposit was again exposed through a considerable space, and in a direction somewhat transverse to the former cuttings, by which some new facts and additional species have been discovered.

The deposit is situated about one mile north of the town, at the height of 106 feet above low water in the bay, and consists in the lower part of a very tenacious compact gravelly clay, of a bluish-gray colour, from eight to twelve feet thick; and in the upper part of stratified red sand and red clay five or six feet thick; above this is a thick recent alluvium. The lower blue clay rests immediately on the gypseous marls of the new red sandstone, and is the chief repository of the shells, a few only being found in the upper sand and clay. The shells are in every state of preservation: some are rolled and polished; some, though strong shells, are broken into fragments, while others, though of a fragile nature, are in a state of perfect preservation. The same observation has been made by Captain Portlock respecting the tertiary clays of Londonderry (Report, chap. vi.); and he points out the conclusion to be drawn from it respecting the origin of these beds.

There have been identified in all forty-five species of shells, and there are fragments which may belong to others. From the occurrence of *Nucula oblonga*, which is now extinct, and found fossil in the crag and Sicilian pliocene beds, and of *Fusus contrarius*, also a crag fossil, and of which no living locality is known, the deposit was assigned by us to the newer pliocene epoch. In this view we were supported by Captain Portlock, who, prior to the discovery of the Belfast deposit, had assigned the same age to certain calcareous clays and gravels in Londonderry. These contain, however, a very limited number of species in comparison to those which occur with us. They extend inland about fifteen miles, and in elevation far surpass our deposits, ranging from near the sea-level to between 400 and 500 feet; yet they were found to contain but three species, *Nucula oblonga*, *Turritella terebra* and *Cyprina Islandica*, all of which occur also in the Belfast beds. In an excellent paper by Mr. Oldham on the state of our knowledge of these deposits in Ireland, an abstract of which was published last year in the Proceedings of the Geological Society of Dublin (vol. iii. part 1. p. 61), only sixteen species are enumerated as the result of his own researches, and those of Dr. Scouler and others in various parts of the country. The Belfast deposit alone thus contains about three times the number of species yet described as occurring in Ireland. This is, I suppose, to be explained less by any original difference in the conditions under which this deposit took place, than by the watchfulness of various collectors during the removal of the great quantity of matter, about 10,000 cubic yards, which was taken from these beds alone.

In the lower clay, in which the shells chiefly occur, rounded fragments of almost all the primary and secondary rocks of this country are found with many fossils of the latter class. The adjoining mountains, which are composed of the upper secondary and trap rocks, afford an obvious origin for some of the pebbles; for the others a more distant origin must be sought. The Mourne range is excluded, as containing but very few of these. They may have been transported from

North-western Donegal, a distance of seventy or eighty miles, as such rocks occur there *in situ*, but they bear a much closer resemblance to the pebbles of Scottish origin which strew the shores at the mouth of the bay; and thus they point back to that remote condition of things, when the whole trap district was still rising above the level of the sea, but when the elevated country at the base of the mountains, from Belfast to Carrickfergus, was yet under water, and the waves were beating against the base of the basaltic precipices.

The following is a list of the shells:—

Rostellaria pes-pelecani
Fusus antiquus
... ... var. containing several, rolled
... corneus
... lamellosus, or peruvianus, one, and fragments
... Bamffius, rare
Murex erinaceus
Buccinum undatum
... ... elongated, var. B. fusiforme, Humph., *Zool.* Journ.
Nassa reticulata
Purpura lapillus
Turritella terebra
Littorina littoreus
... retusa, one specimen
Trochus tumidus, one fragment
Natica clausa, rare
Emarginula fissura, one specimen
Ostrea edulis
Pecten maximus, fragments
Mytilus edulis
Pectunculus pilosus
Arca lactea

Nucula oblonga (Brown).
... margaritacea, fragments
Cardium echinatum
... edule
... nodosum
Tellina solidula
Mactra elliptica
... truncata
Cyprina Islandica
Venus ovata
... gallina
... decussata
... aurea
Astarte Damnonia
... multicostata, abundant
... Scotica
... Gairensis, abundant
... compressa (Smith), same as Clyde shell
... ... another, distinct
Amphidesma compressum
Saxicava rugosa
Serpula vermicularis
Balanus, many pieces with a few valves.

A comparison of this list with the shells of the Clyde deposits, described by Mr. Smith of Jordan Hill, and with those of other parts of Ireland, suggests many interesting reflections. One most abundant species, *Astarte Gairensis*, abounds also in the Clyde deposits, and is found near Dublin in several places, but does not inhabit Belfast Bay at present, and is rare in the Clyde. The occurrence of this species, of the *Nucula oblonga*, and some others, seems to connect, as of one age, the deposits of the west of England, the Isle of Man, the eastern and northern parts of Ireland, and the basin of the Clyde. I must, however, refer to Mr. Oldham's paper already mentioned for some very interesting remarks on this subject. See also Mr. Lyell's observations on the connexion of the Canadian, Swedish and Scottish deposits (Geological Proceedings, vol. iii. p. 119*).

Owing to the thick alluvial cover, the precise extent of this shelly deposit cannot be determined: it is certainly known to occupy several acres, but it may extend much farther. Traces of a clay deposit with shells have been noticed at several places on the northern shores of the bay, at heights reaching from near the sea-level to sixty feet. They have been passed through in well-borings, but have yielded only a few fragments. The intended cuttings on two projected lines of railway will give complete sections of the superficial clay beds, and it is to be hoped, will furnish some interesting facts.

LXII. *Queries regarding* Dr. Young's *Optometer.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HOWEVER eminent the abilities of the late Dr. Young, he certainly did not study the art of writing in such a style, that not only he *might possibly be understood* by those of his readers who comprehended the subject nearly as well as himself, but that he *could not possibly be misunderstood* by any one of ordinary capacity and attention—an invaluable art, too little attended to by English philosophers.

I should feel obliged to any of your readers, who understand Dr. Young's optometer, as described in his Natural Philosophy, vol. ii. p. 576, and the scales of that instrument as represented in fig. 72 of plate 9, who would give, through your pages, a plain and perspicuous explanation of them.

The following queries will direct attention to some of the particulars on which I wish information:—

1. Dr. Young says, "In order to adapt the instrument to the use of presbyopic eyes, the other end must be furnished with a lens." Now, which end is suited for examining the focal distance of normal eyes, the one with or that without the lens?

2. One line of the scale is a scale of inches; another is divided according to a "table here calculated, by means of which, not only diverging, but also parallel and converging rays from the lens are referred to their virtual focus." What is the calculation here referred to? In the series of numbers, the mark of infinity occurs, while some of the numbers have the negative sign before them, and others after them; what does this signify?

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* Inserted also in Phil. Mag., S. 3. vol. xv. p. 400.

3. What is the proportional effect of looking through the four-inch focus lens, and looking simply along a black line, some three feet long? For example, if looking through the slits and lens, an eye can make the lines cross at two inches and at four inches, and at any intermediate distance, what is the real average focus of that eye?

To use a homely but forcible comparison, I would say, Dr. Young, being himself "in the ship of science," seems to expect that "the disciple can arrive there without a boat." A few lucid paragraphs would furnish the boats on this occasion, and oblige,

Gentlemen, yours, &c.
E. D.

LXIII. *On the State of Oxidation of Iron in Soils.*

By RICHARD PHILLIPS, Jun.*

HAVING for some time past directed my attention to the analysis of soils, I have been somewhat surprised at the remark I have met with in the works on Agricultural Chemistry of Prof. Johnston and others, that the lowest state of oxidation of iron or the protoxide is when occurring in soils injurious to vegetation; my own experience having led me to the conclusion, that although it is not necessary to the fertility of a soil that it should exist in that state, nevertheless, that in all soils rich in humus it is so found. The grounds on which the statement I have alluded to has been brought forward, are the following:—Bog earths have been found to be unfit for vegetation; and the drainage water from them, when used for the purposes of irrigation, to be poisonous to plants. Now in these earths the iron exists as protoxide, and as there is much carbonic acid formed in them by the action of the oxygen of the atmosphere on the carbon of the large amount of humus they contain, the protoxide of iron, being soluble in this acid, is thus carried away by the drainage water. From the numerous examples I have had brought before me in the laboratory, I have selected the following six analyses as well illustrative of the fact I have before alluded to, that in most rich soils the iron is found principally in the lower state of oxidation.

The first two are analyses of the soil and subsoil of some of the best wheat land in the Lothians. The soils were brought from thence by my friend Mr. Wilson, and analysed by him; in the soil he found the iron to be protoxide, whilst in the subsoil it existed as peroxide.

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* Read before the Pharmaceutical Society, April 9, 1845.

Surface soil.	
Silica	77·
Alumina	8·8
Iron, *protoxide*	5·4
Sulphate of lime	·6
Magnesia, a trace	
Salts of potash and soda	1·
Organic matter	5·
Moisture	2·2
Phosphate of lime, a trace	
	100·

Subsoil.	
Silica	68·
Alumina	16·3
Iron, *peroxide*	7·4
Sulphate of lime	·4
Magnesia, a trace	
Organic matter	4·1
Moisture	3·8
Salts of potash and soda, traces	
	100·

The next is an analysis of a soil from Polder, in Belgium; it was likewise procured by the gentleman I have mentioned, and analysed by him, his reason for obtaining it being its exceeding richness, the farmer from whom it came assuring him that it had not been manured for more than fifty years; it consisted of—

Silica	61·5
Alumina	9·1
Iron, *protoxide*	3·1
Carbonate of lime	16·4
Carbonate of magnesia	·7
Salts of potash and soda	·8
Organic matter	6·2
Moisture	2·2
Phosphate of lime and humate, traces	
	100·

The following is the analysis of a soil brought to me from India by a gentleman, it being there considered as one of great fertility:—

Silica	40·
Alumina	29·7
Iron, *protoxide*	14·
Carbonate of lime	4·2
Carbonate of magnesia	·6
Sulphate of lime	·3
Salts of potash and soda	·4
Organic matter	8·9
Moisture	1·9
Phosphate of lime, a trace	
	100·

The next is the analysis of a soil from Berkshire, the farmer from whom I procured it assuring me that it was the richest wheat land on his estate. I found it contained—

Silica	68·
Alumina	12·4
Protoxide of iron	5·
Carbonate of lime	6·4
Sulphate of lime	1·1
Salts of potash and soda	·5
Organic matter	6·3
Moisture	·3
Phosphate of lime, a trace	
	100·

The last example is one of what is termed a virgin soil; it was sent to me from Yorkshire, and with it some experiments I shall afterwards detail, were made. Mr. Smith of Deanston, was kind enough to inform me that he considered it to be a soil of good quality, and the gentleman from whose estate it came, that it had been brought under very good cultivation. My friend Mr. Howard analysed it, and found it to consist of

Silica	72·4
Alumina	12·
Sulphate of lime	·4
Protoxide of iron	5·1
Salts of potash and soda	·7
Organic matter	7·4
Moisture	2·
	100·

Trusting that these results sufficiently prove the statement I have made, that the lower degree of oxidation of iron in soils is not injurious to vegetation, I shall proceed to consider the cause, notwithstanding the tendency of iron in the lower state of oxidizement to pass to the higher, when exposed to air and moisture, of its remaining unchanged. This fact may, I think, be accounted for, by the carbon of the organic matter or humus converting, by its affinity for oxygen, the peroxide into protoxide, itself becoming carbonic acid; and that the oxygen of the air having a greater tendency to combine with the carbon of the humus than to convert protoxide into peroxide of iron, the former is left unchanged.

In order to prove whether this idea was borne out by fact, I passed by an arrangement I shall presently describe—a gentle current of air for three days over about 200 grains of the soil I have last mentioned as having been analysed; it was operated on as when brought to me, and contained 18·5 per cent. of water. From the 200 grains I obtained six grains of carbonic acid, equal to a loss of 1·63 grain of carbon; and on examination I found the degree of oxidation of the iron to have remained the same.

The arrangement was the following, being a modification of the apparatus of Liebig for drying organic bodies:—The air was passed through bulbs containing solution of caustic potash, by which it was deprived of all carbonic acid; it then passed over the soil, and having, by passing through a long tube containing chloride of calcium, been deprived of any moisture it might have acquired, through some bulbs partly filled with caustic potash, these were connected with a Woulfe's bottle filled with water, and fitted with a siphon. Having ascertained the apparatus to be perfectly tight, the air was drawn through it by the action of the siphon, and the second set of bulbs filled with caustic potash, which had been previously weighed, were found, as I have before mentioned, to have increased six grains in weight.

I proved that the iron was uninfluenced by any soluble organic matter, by washing two samples, the one with cold, and the other with boiling water: the first gave 1·95 per cent. of soluble organic matter and inorganic salts, and the latter 3·27; but in both instances, although the residues were dried with exposure to the air, the iron remained unchanged. That it did not exist in the state of the black or magnetic oxide was shown by its being non-magnetic when dissolved and precipitated, as that oxide according to Liebig still retains its magnetic property when thus treated.

The non-fertility of bog-earths may, it appears to me, be perhaps accounted for from the organic acid they have been found to contain; supposed to be the suberic acid, but probably an acid peculiar to these earths. The action of the manures usually employed to bring them into a state of fertility, viz. lime and strong oil of vitriol, is made apparent if we take this view, for by uniting with the first the organic acid would be rendered innoxious, whilst the second would destroy it; but neither of these agents would have any influence on the further oxidation of the iron, and the second would render it soluble; and what makes this idea more probable is, that I have found in all rich soils a soluble organic salt of lime to exist, and that these soils never possessed any acid properties. The poisonous character of the drainage water from these earths is explained by this view; but, on the other supposition, that it results from the action of protoxide of iron, we can hardly imagine,—knowing as we do how immediately, when held in solution by carbonic acid, it is decomposed by exposure to the action of the atmosphere,—that these injurious effects could take place in the short time that would elapse before it became sesquioxide; and I have before pointed out that the soluble organic matter would have *no* effect in arresting this decomposition. The use of the red oxide of iron in a soil has been stated to be its power of retaining ammonia. I must confess, however, I am rather inclined to doubt that it is of any great value to it, on account of this property, as all soils containing much of it are found to be of poor quality; but, admitting this to be the fact, there does not appear to me to be any reason to doubt that this retentive power is equally possessed by the protoxide.

From the above observations I was led to the conclusion, that the preservative action of the humus on the protoxide of iron in soils was probably analogous to that of sugar in some pharmaceutical preparations, particularly in that of the saccharated carbonate of iron of the Edinburgh Pharmacopœia. I therefore prepared some of it by the process there given, and found, on passing a current of air over a portion of it by the apparatus I have before described, that a small amount of carbonic acid was given out from it. I do not, however, wish to be considered as speaking positively as to this being the action of the sugar, although the experiment shown above would appear to render it probable.

Note.

Since writing the above communication, I have had brought before my attention a remark occurring in several agricultural works, that some clays require either burning or long exposure to the atmosphere before they are fitted to be mixed with fertile soils; and as the iron in them is found to become peroxide during these operations, it has been brought forward as another proof of the injurious action of the protoxide of iron.

In my opinion, however, it is not because the iron is in the state of protoxide that clays require this treatment, but on account of its existing as sulphuret, which, as is well known, is hurtful to vegetation; and I believe that the appearance of oxidation assumed by them during exposure to the atmosphere, is nothing more than the decomposition of the sulphuret into peroxide of iron. That this decomposition would take place under these circumstances I may instance, in the case of an embankment of clay on the Croydon railway, where, accompanied by the formation of sulphate of lime, it occurred so extensively as to destroy part of the work.

I have, on analysis, usually found deep clays to contain sulphuret of iron; and another proof of its existence in them may be adduced from the slaty clay usually accompanying the coal formation, where it is found so extensively as to be employed for the manufactures of alum and copperas.

Craig's Court, April 9, 1845.

LXIV. *Reply to some Remarks of* Prof. A. Leymerie *on the Identification of certain Lower Greensand Fossils. By* Prof. Edward Forbes, *F.R.S., of King's College, London.*

To Richard Taylor, Esq.

Dear Sir,

In the last Number of the Philosophical Magazine appears a translation of a paper read before the Geological Society of France, by Prof. A. Leymerie, and entitled "Observations on a Communication made by Dr. Fitton to the Geological Society of France at the Meeting of May 20, 1844, on the Lower Greensand of the Isle of Wight," read November 4, 1844, and published in the *Bulletin* of the French Geological Society for January 1845.

In that paper M. Leymerie is rather severe upon me and my palæontological determinations, unreasonably, as I conceive, and I hope you will permit me to reply through the medium of the Philosophical Magazine. I am the more anxious to do so, in order to prove to my geological friends that I do not deserve the accusations of hastiness and carelessness preferred against me by M. Leymerie, and that I did not draw up the lists of Lower Greensand fossils for Dr. Fitton and other English geologists interested in the Neocomian question, without full and due consideration and good materials before me.

Fortunately the accusations are more special than general. M. Leymerie has fully stated the grounds of his doubts. At once, then, I will go over separately the list of species (eight in number) which forms the text of the lesson he would read me.

The first is "*Pholadomya Prevosti*, Deshayes," identified by me with *Mya plicata*, Sowerby—wrongly according to M. Leymerie, who adds, moreover, on M. Deshayes' authority, that the species of Sowerby is a *Panopæa*, whilst the French shell is a *Pholadomya*.

I identified the English fossil, which *certainly* is the species of the Mineral Conchology, with the French for the following reasons:—1st. One of the commonest varieties of the Lower Greensand, *Panopæa plicata*, exactly agrees with the figure and description given by M. Deshayes in M. Leymerie's paper on the Cretaceous Formation of l'Aube. 2nd. Specimens of the English shell sent to France were returned as *P. Prevosti* by M. d'Orbigny. 3rd. Specimens of the French shell brought to England were compared by myself with our own specimens, and found to be identical. As to the genus to which both English and French shells belong, there can now be no doubt on the subject; they are *certainly Panopææ.* This I know from the examinations of English specimens showing the dentition, which is identical with that of the French shells, as recently figured in the *Paléontologie Francaise*, so that any argument from supposed difference of genus must fall to the ground.

2nd. "*Astarte Beaumontii*, Leym."—This I identified with the *Astarte obovata* of the Mineral Conchology. That the English shell is identical with the French, I hardly think M. Leymerie himself would dispute. I identified it by comparison with French specimens, as well as by its agreement with the figures and descriptions given in M. Leymerie's paper, above referred to. English specimens sent to France were recognised as *Astarte Beaumontii* by M. d'Orbigny. The question is really, then, not whether I am right in calling the English and French species one, but whether I am correct in referring it to *Astarte obovata*, Sowerby. I did so with the concurrence of my friend Mr. Morris, on whose judgement I place great reliance. The figure in the Mineral Conchology is not very good, but does not differ materially from the figure given by M. Leymerie, nor does the description. Moreover, this *Astarte* is the only one which occurs, so far as English collectors know, at Sandown, where it is common, and whence the specimen figured in the Mineral Conchology came. Had M. Leymerie looked to the plate and text of the Mineral Conchology before he settled his species, he might, probably, have abstained from creating a new and useless synonym.

3rd. "*Cardium subhillanum*."—There can be no doubt that this name should be retained for the Lower Greensand form, which, as M. Leymerie says, is distinct from the true *striatulum*. It is not the less, however, the *Cardium striatulum* of the list of Lower Greensand fossils in the 4th vol. of the Geological Transactions, 2nd series, so that the argument of Dr. Fitton is not affected.

4th. "*Cucullæa Gabrielis*."—This I identified with the *Arca exaltata* of Nilson.

Nilson's figure (t. v. 1 A. 1 B) is very bad, and his ideas of the restoration of the species, as indicated by dotted lines, inadmissible. But the figure given by Goldfuss of the cast is excellent. The longitudinal ribs indicated in Goldfuss's figure are present in almost all British specimens, good examples of which being sent to France, were identified by M. d'Orbigny with *Cucullæa Gabrielis*, although the longitudinal ribs are not represented in his otherwise excellent figure (pl. 308, 1-2) in the *Paléontologie Francaise*. They are shown, however, in his figure of the young shell (same plate, f. 4, 5). My own impression is that M. d'Orbigny is right, and that the British and French forms are varieties of one species; and if so, they certainly should be referred to *C. exaltata*, rather than burden our lists with a new name.

5th. "*Pinna sulcifera.*"—The identification of *Pinna sulcifera* with *Pinna tetragona* of the Mineral Conchology is, no doubt, wrong. I was led to do so in consequence of M. d'Orbigny having identified a British example of the latter, forwarded through Mr. Pratt, with the French species above mentioned. Subsequently specimens of the English shell, laid before the same naturalist by Dr. Fitton, were named by him *Pinna Robinaldina*, a Neocomian species, which he has since described and figured; so that if *Pinna sulcifera* be not English (though I have reason to believe it has been found here also), our common lower greensand *Pinna* still turns out identical with a French Neocomian species. The name *tetragona* of Sowerby must be dropped, as it was adopted by that author from a mistaken reference of the shell to a species described by Brocchi; as also must the new name *Robinaldina*, and that of *restituta*, under which it had previously been described by Hœninghaus and figured by Goldfuss, adopted instead.

6th. "*Pecten interstriatus.*"—Specimens of *Pecten obliquus*, identified by comparison with the original types, were forwarded to France and returned as *Pecten interstriatus*; and French specimens, brought to England by Dr. Fitton, exactly agreed with our own. The figure of Sowerby is no doubt bad, not from faulty drawing, but in consequence of having been taken from the first specimen found, and that a bad one. But when M. Leymerie denies our identification of species with Sowerby's types, he should recollect that those types are on this side of the Channel, and a great number of them still accessible. The probability is certainly in favour of our knowing our own species best.

7th. "*Modiola Archiaci.*"—This I identified with *Modiola æqualis*, and M. Leymerie seems inclined to admit the identification. I am now convinced, from the examination of French specimens, that they are certainly identical. The species is a variable one, and varieties resembling the figures of M. Leymerie and of Sowerby are equally common, passing into each other by insensible gradations. I do not understand why M. d'Orbigny has lately separated his *M. Archiaci*, not only specifically, but generically from *M. æqualis*, calling it *Lithodomus Archiaci*, unless it be that it is sometimes a perforating species. That, however, is the case with some recent *Modiolæ*, which are sometimes free and sometimes perforators.

8th. The last fossil on the list is "*Ampullaria lævigata*, Desh.," which I had identified with the *Littorina rotundata* of Sowerby, referring the shell to the genus *Natica*. Dr. Fitton having taken his list from my unpublished catalogue of the Lower Greensand fossils in the collection of the Geological Society, adopted the revised name. Were French naturalists in the habit of referring specific appellations to the original authorities for them, and not of appending their own names after those of old species, whenever they remove them from one genus to another, M. Leymerie would probably have searched a little more diligently for a figure and description of this pretty shell, which is undoubtedly the *Ampullaria lævigata*, as I am convinced from comparison of British with French specimens. Moreover, the British shell was returned from France with its French name. If M. Deshayes persists in referring this species to *Ampullaria* instead of *Natica*, it is through a perversity unworthy of the first conchologist of France. Why he should refer a fossil abundant only in marine beds, associated only with species indicating a considerable depth of sea, and closely allied to several existing forms of *Natica*, to a freshwater genus, such as *Ampullaria*, is to me unaccountable, and would be so to M. Leymerie, were he naturalist enough to perceive the difficulty.

So much for the list of "*espèces contestées*," on account of my supposed errors in which, M. Leymerie afterwards expresses a doubt about *all* my determinations. His example in proof is an unfortunate one. With large suites of *Gryphæa sinuata* and *G. lævigata* before me, and after an examination of numerous specimens of those shells in their native beds, I came to the conclusion, that not only were they forms of one species, but that *Exogyra aquilina* and *Exogyra Couloni* (*subsinuata*, Leym.) were also only varieties of this changeable shell. I came to this conclusion, with the original English types before me, with specimens of the true *Couloni* brought from Neufchatel by Captain Ibbetson and named by M. Agassiz, specimens from the Crimea furnished by Mr. Murchison, and an extensive series collected in the Neocomian beds of France by Dr. Fitton and named by M. Cornueil. Such a way of going to work scarcely warrants the charge of hastiness. I feel confident that if M. Leymerie would honour us with his presence at this side of the channel, and look at the varieties of this *Gryphæa*, *intermingled* at Atherfield and elsewhere, he would no more think of making many species out of it, than of the far more distinct varieties of *Ostræa edulis* (a case exactly parallel) displayed in a London oyster-cellar.

As to the identity of *Trigonia caudata* of Agassiz with the

Trigonia scabra of Lamarck, M. Leymerie has misunderstood Dr. Fitton's list. That list was given to show that many of the supposed new Neocomian fossils of France were only well-known Lower Greensand fossils of England, some, no doubt, known under names to which they had no claim, but not the less *Neocomian* on that account; the *Trigonia* is an instance. The shell which has hitherto figured in British lists as *Trigonia scabra* is certainly the *Trigonia caudata* of Agassiz, a Neocomian species. The French conchologists say that the *T. scabra* of Lamarck is a different shell. Be it so; the case is not changed, though the name *scabra* in our lists must be replaced by that of *caudata*.

So much for the palæontological objections of M. Leymerie. Permit me now to add a word on the general question*.

It may be stated briefly as follows:—Dr. Fitton holds that the formation intermediate between the Wealden and the Gault, known in England as the Lower Greensand, is synchronic with the Neocomian of Neufchatel and l'Aube.

M. Leymerie holds that the Neocomian (if not all, the greater part thereof) is synchronic, not with the English Lower Greensand, but with the freshwater deposit of the Wealden.

Tested *palæontologically*, I maintain Dr. Fitton's view is right, not merely from a closer examination of specimens, but also from careful investigations made in the field in conjunction with my friend Captain Ibbetson. This view is also held on natural-history grounds in England by Mr. Morris, in France by M. A. d'Orbigny, and at Neufchatel by Prof. Agassiz and M. Dubois de Montperreaux. The doubts thrown on it by M. Leymerie, I conceive, have been removed by the statements put forth in this reply; for even were my names wrong (which I do not admit), the species to which those names were applied are undoubtedly identical with French Neocomian forms.

Tested geologically, the hypothesis of M. Leymerie (or rather of M. de Beaumont, whom he follows), that during the deposition of the Wealden there must necessarily have been a corresponding deposit in France, which, if not a freshwater deposit, must have been marine, and that the Neocomian is such deposit, cannot stand for two reasons:—

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* In the discussion which followed the reading of M. Leymerie's paper, M. d'Orbigny is reported as saying, that whilst he differed from M. Leymerie on the geological question and considered the Neocomian and Lower Greensand as identical, he agreed with the author as to the value of the determinations of Dr. Fitton's fossils. Yet it will be seen from what I have stated above, that out of the eight "espèces contestées," seven were identified with French species by M. d'Orbigny himself! There must be some mistake in the report, or M. d'Orbigny must have a very short memory.

1st. There is no necessity that there should have been in the French localities any synchronic deposits at all during the deposition of the Wealden. On the contrary, there is a necessity that in parts of France, where the Wealden is wanting, there should be *no* synchronic *marine* deposit; for the Wealden was a great freshwater area, which must have been walled in by a more or less extensive track of land, part of which wall must have been in or near the country where the Neocomian is now found.

2nd. The argument of the relation of the Wealden to the oolitic series in preference to the cretaceous, inferred from the determinations of Professors Owen and Agassiz respecting the forms of fishes and reptiles of the Wealden—determinations which M. Leymerie, with strange hardihood, denounces as "trop vague et trop incertaine pour qu'on puisse sérieusement s'y arrêter"—is further borne out by the fact, that Wealden mollusca are found in the freshwater beds, discovered by Mr. Robertson among the oolitic strata of Brora*.

London, April 1845. EDWARD FORBES.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 256.]

Feb. 6, 1845. "ON the Structure and Development of the Blood.—*First Series.* The development of the Blood-Corpuscle in Insects and other Invertebrata, and its comparison with that of Man and the Vertebrata." By George Newport, Esq., F.R.C.S., President of the Entomological Society, &c. Communicated by P. M. Roget, M.D., Sec. R.S.

The author commences his paper by remarking, that he was led to the present inquiry by some curious facts relating to the blood of insects, which attracted his notice while engaged on the last paper he presented to the Royal Society, on the reproduction of lost parts in insects and myriapoda. Some of these facts he is desirous of making known at once to the Society, preparatory to his offering them more extended researches on the blood of the invertebrata, and its comparison with that of the higher animals.

The chief purpose of the author in the present paper, is to show the analogy which exists between the different corpuscles in the blood of insects and of the vertebrata, to trace the changes which the former undergo as compared with those of the latter, and to show that in development and function they are analogous to secreting cells.

In pursuance of this object, he premises a brief notice of what little was already known respecting the corpuscle in the articulata,

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* See Geological Proceedings, vol. iv. part 1. p. 174 [or Phil. Mag., S. 3. vol. xxiv. p. 71].

and of the different descriptions given of it by Carus, Spence, Wagner, Bowerbank, Edwards, Baly and some later observers, all of whom have described it differently, one only, Mr. Bowerbank, having correctly indicated its form.

He then proceeds to state, that while engaged on other observations in June last, he found that the oat-shaped corpuscles, which are so abundant in the caterpillar state of the insect, almost entirely disappear before the insect has arrived at the perfect, or butterfly state, in which, a few days after the insect is fully developed, scarcely a single oat-shaped corpuscle is to be found; but in the place of these, there are numerous very minute rounded bodies, spherules, and also many flattened, obtusely oval or barrel-shaped, double concave discs. Both these forms of corpuscle have molecular movements, which are most energetic in the spherules.

He next makes some general observations on the composition of the blood of the invertebrata, and calls in question the accuracy of Professor Wagner's view in regarding the blood of these animals as analogous only to the chyle of the vertebrata, at the same time stating his belief that it is not only analogous to true blood, but that it undergoes a continued succession of changes through the agency of the corpuscles. These minute bodies first derive nourishment and the means of growth and increase from the fluid portion of the blood; and afterwards, when they have become fully developed, undergo dissolution, and help to supply the waste of the fluid that has been expended on the nourishment of the different structures, leaving other little bodies, which also undergo development, to assist in the further elaboration of this fluid. He states also, that the development of these latter bodies appears to have a certain relation to the type of each particular class of animals; and remarks that in the vertebrata the size of the corpuscle is perhaps in a ratio inverse to that of the activity and extent of the function of respiration.

The author states that he has been led to these views, which appear to him to apply to animals generally, by an examination of the corpuscles, and by watching the changes which take place in the blood in lepidopterous insects; and he points out their accordance with those of Wagner, Henle, and Wharton Jones, with regard to the function of the corpuscles; but proposes to give the details on which his own view respecting the size of the corpuscle is founded on a future occasion.

He then enters more particularly on the consideration of the forms of corpuscle in the blood in the Articulata, which he marks as four; although, he observes, they are in reality only so many stages of development of one ultimate structure. These forms are,—first, the *molecules,* which he regards as comparable to the molecules observed in the chyle of Vertebrata by Mr. Gulliver; secondly, the *nucleated* or *oat-shaped corpuscle,* which he believes with Wagner are analogous to the white, or chyle corpuscles of Vertebrata; thirdly, the *spherules,* or minute rounded bodies developed from the oat-shaped corpuscle, and which he believes are analogous to the free nucleoli of Valentin, and probably to the very minute white, opake granules constantly observed in the blood of vertebrata; and lastly, the *discs*, which are further developments of the spherules, and analogous to the true red blood-discs of the higher animals, and which, as he states in a subsequent part of his paper, in his examination of the blood of the human fœtus, he believes that he has also traced from the white, opake granules or spherules.

The author then proceeds to describe these forms of the corpuscle in insects more minutely, and enters into considerable detail with reference to the oat-shaped corpuscle, tracing it from its earliest distinct form, before any nucleus is perceptible in it; and shows that the nucleoli which constitute this body are gradually increased in number, until the corpuscle has attained its full size, when it first changes its form and becomes shorter, then rounded, and afterwards entirely breaks up and liberates the nucleoli that have been developed within it. This change of form he shows always takes place very rapidly in all the oat-shaped corpuscles, large and small, when out of the body, and to this circumstance he attributes the diversity in the descriptions that have been given by various observers of the form of the corpuscle. He shows also, that, with reference to the function of this body, the corpuscles are usually found in greatest number during the act of breaking up, immediately before the larva is preparing to change its skin, at which time the blood is extremely coagulable; and that there are fewest corpuscles, or that there is the greatest number of small corpuscles of this kind, soon after the caterpillar has again begun to feed. When the insect has assumed the pupa state, nearly the whole of these corpuscles are broken up. The greatest abundance of them is found in the act of changing on the third or fourth day of the pupa, after which the number of these corpuscles is gradually lessened, until, when the insect has entered the perfect state, very few remain. When the change to the perfect insect occurs, there is another opportunity of watching the function of this corpuscle. When the wings are being expanded and still soft, a few oat-shaped corpuscles circulate through them; but as the wings become consolidated, these corpuscles appear to be arrested, and break down in the circulatory passages, supplying directly the material for the consolidation of these structures; as appears from the entire arrest of circulation in these parts, and from the granular remains of the corpuscles which may be seen by transmitted light in a wing completely denuded of its scales on the upper and under surfaces. The spherules and discs of the perfect lepidopterous insect are then noticed; and some peculiar clavate or fiddle-shaped bodies, which appear to be the transition forms between spherules and discs, are pointed out as occurring in the blood of one of the night moths, *Xylophasia polyodon*, and also in the butterfly soon after it has left the pupa state. These facts are regarded as proofs, derived from direct observation, of the function of the corpuscle, and of its analogy, both in function and development, to the secreting cells of glands.

In the second division of his paper, the author draws some comparisons between the blood-corpuscles of insects and the vertebrata, and gives the details of a series of observations on the blood of a human fœtus that was born alive at the end of the sixth month. He examined the blood of the parent, and of the placenta, and also of different parts of the body of the fœtus a few hours after death, and found in general that the blood of the parent contained a very large quantity of white chyle corpuscles, and was extremely coagulable: the blood of the placenta contained, beside an abundance of chyle corpuscles, red blood-discs of extremely variable sizes, the largest being one-third or one-fourth larger than those of the mother, and the smallest scarcely more than one-fourth as large as the largest. There were also an immense abundance of molecules and nucleoli, from which latter the red blood-discs appeared to be developed. The blood of the vein and lungs presented a similar irregular condition as to size of the corpuscles, while that of the left auricle of the heart, aorta and arteries of the cord was more uniform in its character. From these observations the author concludes, that the blood of the vertebrata is analogous in its mode of development to that of the insects and other invertebrata, and that the red blood-discs are the ultimate developments of the opake white granules or nucleoli of the blood.

Drawings illustrating the subjects accompany the paper.

February 20.—"Additional Remarks respecting the Condensation of Gases." By Michael Faraday, Esq., F.R.S. &c.

The author, suspecting the presence of nitrogen in the nitrous oxide on which he had operated, repeated his experiments with this gas, very carefully prepared from pure nitrate of ammonia, but the results still indicated the presence of a more volatile gas mixed with another less volatile. He found that olefiant gas is readily soluble in strong alcohol, æther, oil of turpentine, and other bodies of the same kind; and that, like the former gas, it seems to be of a compound nature. His experiments confirm the prevalence of the law that the force of vapour increases in a geometrical ratio for equal increments of heat, commencing at a given amount of pressure. The more volatile a body is, the more rapidly does the force of its vapour increase by an augmentation of temperature, the increase of elasticity being directly as the volatility of the substance. By further and more accurate investigation, a general law may be established for deducing, from only a single observation of the force of any given vapour in contact with its fluid, its elasticity at any other temperature.

Postscript to the Paper by Sir John F. W. Herschel, Bart., F.R.S., read at the last meeting.

The author found that neither cinchonine nor salicine, in a state of great purity, possessed, in the smallest appreciable degree, the optical property which he has shown to belong to quinine.

February 27.—"An Account of Compact Aluminum," by Professor F. Wöhler of Göttingen, in a Letter to Thomas Graham, Esq. Communicated by Thomas Graham, Esq., F.R.S.

The author has lately found, contrary to the results of his former researches on aluminum made eighteen years ago, that this metal is readily fusible, and that in its reduction from the chloride of aluminum by means of potassium, it presents itself in the form of fused globules, generally so small that their shape is not distinguishable under the microscope, although occasionally they are met with having a sensible diameter. He effects the reduction at once in a clay crucible, the bottom of which he covers with pellets of pure potassium, and places upon these the chloride of ammonium, covering the whole with chloride of potassium in powder. The crucible being then closed up, and heated in a coal fire, the reduction is instantly effected.

Fused aluminum has the colour and lustre of polished tin; it continues perfectly white in the air; it is fully malleable, and the globules may be beaten out into the thinnest plates without cracking at the edges. It is entirely unmagnetic. In other respects the metal in this compact state has the properties which the author formerly ascribed to it.

March 6.—"Essays on Hygrometry and Barometry." By Captain Shortrede, F.R.A.S., First Assistant in the General Trigonometrical Survey of India. Communicated by Lieut.-Col. W. H. Sykes, F.R.S.

This paper commences with an account of the various investigations of the author on subjects relating to the elasticity of aqueous vapour at different temperatures and under different circumstances. He first discusses the tables given by different experimentalists of the force of vapour at various temperatures, and endeavours to deduce an analytical formula giving the nearest approximation to the results recorded. He then proceeds to the consideration of what he terms "the moist bulb problem," or the point of maximum depression attained by a thermometer with a moistened bulb exposed to evaporation in air: he deduces formulæ which he compares with the results of actual observation, and points out the probable sources of error in the cases in which he finds disagreements between them. In the miscellaneous remarks which form the next section of the paper, the author states his reasons for dissenting altogether from the views taken by Dalton of the constitution of mixed gases, or of mixtures of aqueous vapour with any of the gases, according to which, each gaseous body is uniformly diffused throughout the whole space, its particles repelling those of its own kind, but exerting no pressure on the particles of any other kind. He considers the fact that a given portion of air has its volume expanded by the addition of aqueous vapour, as being of itself a sufficient refutation of that theory. The author then takes occasion to discuss the question, whether aqueous vapour exists in the atmosphere in the state of mechanical mixture or of chemical solution, and argues in favour of the latter view of the subject.

In the concluding section, the author enters at large into the investigation of the method of ascertaining heights by barometric observations, and gives various tables to be used for that purpose.

Αμορφωτα, No. 2. "On the Epipolic Dispersion of Light; being a Supplement to a paper entitled 'On a case of Superficial Colour presented by a Homogeneous Liquid internally colourless.'" By Sir John Frederick William Herschel, Bart., F.R.S. &c.

The author inquires whether the peculiar coloured dispersion of white light intromitted into a solution of sulphate of quinine, is the result of an analysis of the incident light into two distinct species, or merely of a simple subdivision analogous to that which takes place in partial reflexion, as exemplified in the colours of thin plates. He endeavours to ascertain the laws which regulate this singular mode of dispersion, which for brevity he terms *epipolic*, on account of the proximity of the seat of dispersion to the intromitting surface of the fluid. It might have been expected that by passing the same incident beam successively through many such dispersive surfaces, the whole of the blue rays would at length be separated from it, and an orange, or red residual beam be left: but the author establishes, by numerous experiments, the general fact, that an *epipoloical beam of light*, meaning thereby a beam which has been once transmitted through a quiniferous solution, and undergone its dispersing action, is incapable of farther undergoing epipolic dispersion.

There were only two liquids, out of all those examined by the author, namely oil of turpentine and pyroxylic spirit, which, when interposed in the incident beam, act like the solutions of quinine in preventing the formation of the blue film: and the only solid in which the author discovered a similar power of epipolic dispersion, is the green fluor of Alston Moor, and which by this action exhibits at its surface a fine deep blue colour.

GEOLOGICAL SOCIETY.

Jan. 22, 1845.—The following communications were read:—

"Geological Features of the country round the Mines of the Taurus." By W. W. Smyth, Esq.

The mines described in this paper appear to be worked in great masses rather than beds or veins. They consist of two, one containing ores of copper, and the other argentiferous ores of lead worked for silver. The former at Arghaneh Maden is worked in igneous and altered rocks in the neighbourhood of Diarbekr, the average annual supply being about 3500 tons of ore, producing about 380 tons of copper, but it is thought that the return of metal from the ore might easily be doubled. There are several mines of silver and lead worked at Kiebban Maden, the proportion of silver being about an ounce or an ounce and a half per hundred pounds. About 900 lbs. weight of silver are produced annually, and a small quantity of lead. The geological date of the formations in the Taurus seems to be, in most cases, that of the cretaceous period, but there are also some metamorphic rocks of more ancient origin.

"On the newer Coal Formations of the Eastern part of Nova Scotia." By J. W. Dawson, Esq.

The paper was an appendix to a communication made last year before the Geological Society, and completed the account prepared by the author of the Carboniferous Formation. The paper also contained a notice of some footmarks observed in the sandstone, which were considered by the author to be those of a bird. In an appendix a notice was given of the junction of the Carboniferous and Silurian rocks at a locality called M'Cara's Brook.

Feb. 5.—A paper was read "On Raised Beaches and the Shells found in them, occurring on the coast of Essex near Walton." By J. Brown, Esq., of Stanway.

The object of this paper was to direct attention to the fact, that low raised beaches exist on this part of the eastern coast, and that they contain fossils, not only marine but freshwater, and confined to a small number of species, though individuals are very numerous. It was also the wish of Mr. Brown to bring these raised beaches into comparison with the beds called "Till" in the Clyde valley.

A paper was next read "On the Geology of the vicinity of the Wollondilly River, in Argyle County, in the colony of Sydney, New South Wales." By the Rev. W. B. Clarke.

The district described by the author is chiefly occupied by igneous rocks, upon which sedimentary rocks of the carboniferous period repose unconformably. The igneous rocks consist of granite and syenite, of porphyries, basalt and trachyte. They pass into and occasionally intersect one another, and are traversed by numerous dykes of igneous rocks of various kinds. The sedimentary rocks are not less violently disturbed, and have become greatly altered in every place where they have been brought into contact with the granite.

A communication was also made by Dr. Fitton, "On the Beds of the Lower Greensand of the Isle of Wight."

Dr. Fitton, after describing the general structure of the back of the Isle of Wight, alluded to the numerous fissures or *chines* found in these localities. He also described the different beds of the lower greensand, and mentioned the fossils most characteristic of each of them. He concluded by alluding to some of the fossils from the Neocomian beds of the Continent, and mentioned the fact that these foreign strata are strictly contemporaneous with the lower greensand of England.

LXVI. *Intelligence and Miscellaneous Articles.*

REMARKABLE DISCOVERY OF MASTODON BONES IN NEW JERSEY.

AT a meeting of the American Philosophical Society, December 6, 1844, Prof. Frazer read a letter from Mr. J. B. Maxwell, one of the trustees of Princeton College, to Prof. Henry, dated Belvidere, October 17, 1844, relating to the discovery of Mastodon bones on the farm of Mr. Abraham Ayers, near Hackettstown, New Jersey.

There are portions of the skeletons of five Mastodons—one pretty large, three of smaller size, and a calf: of the largest, only the grinders, portions of the tusks ($13\frac{1}{2}$ inches in circumference), and some fragments of the larger bones remain, the rest having fallen to pieces on being exposed to the air. The bones of the calf fell to pieces in like manner, and no parts have been preserved except the grinders and the tusks, which were five or six inches long, about three-quarters of an inch in diameter, and seem to have projected more than an inch from the bone. Of the other three, the skulls and most of the larger bones are in good preservation. We measured some parts of the largest of them, as follows:—

Skull—from top of head at junction of the muscles of the neck to end of bone between the tusks—3 feet 4 inches.
... Between the large cavities for the ears across the forehead—2 feet.
... Orbit of the eye—vertical diameter, 6 inches.
... Reniform orifice below the eyes (communication between trunk and brain, &c.), 10 inches across by 4 inches vertically.
... Oval orifice below this (communication between trunk and throat), 5½ inches vertically by 3 across.
... Tusk, from insertion, 2 feet 3 inches—whole length 3 feet 1 inch, and about 3 inches in diameter.
Pelvis, 4 feet 10 inches across, by 3 feet 8 inches.
Femoral bone, 3 feet long, 1 foot 1½ inch in circumference in the middle.
Scapula, 2 feet 5 inches long, 2 feet 1 inch wide, measuring over the projection.

There were three grinders on each side above, and the same number below. The two others and the calf had four teeth on each side, both above and below, but the forward ones were evidently "milk teeth," which would have been shed at a more advanced age. In none of these four did the back grinders appear to have cut through the gum. The largest of the five had had three grinders on each side in each jaw. All the grinders were of the same character, having the projecting conical points which distinguish the Mastodon. In one of the skulls, the tusks projected outwards and inclined upwards, while in the other two they were inclined downwards and nearly parallel. This difference, and that in the number of the teeth, had induced Mr. Ayers to believe that the remains belonged to more than one species. But Mr. Maxwell considered it to be evident that the only differences are those of age and sex. The bones which he measured, he supposed to be those of a female. The other two were younger animals, as was evident both from the teeth and the sutures of the skull; and as the skull with projecting tusks seemed to have a broader and rounder outline than the others, it probably belonged to a young male.

Mr. Ayers walked with Mr. Maxwell to the spot where they were dug out, and described the position in which they were discovered. North-west of the Musconetcong Valley, in which Hackettstown is situate, lies a range of highlands about two miles wide, rising perhaps 350 feet above the valley, and separating it from the valley of the Pequest. This ridge, which is of gneiss, and has, like all our mountains, a general course of about north-east, is cut into sections by transverse depressions, or hollows running generally about south of east. Through one of these depressions, which is probably 150 feet below the general level of the range, passes the road from Hackettstown to Vienna. By looking at Gordon's Map of New Jersey, a small stream is found to cross the road nearly half-way to Vienna. Mr. Ayers's house is about 100 yards beyond the stream, and the bones were found more than a quarter of a mile beyond his house in a northerly direction, and perhaps 300 yards from the road. The map does not represent the face of the country correctly. The road runs along the northern side of the valley or depression, most of which is occupied by a swampy meadow, through which the stream flows. From the road the ground rises regularly, but pretty rapidly, probably 120 feet in 200 yards, and then descends more gradually 25 or 30 feet into a smaller depression, which, however, does not cut through the ridge like the larger one, but descends very gradually from the general level on the east, and at its western end opens on the brow of the ridge by a kind of ravine. Near this western end is a depression or basin deeper than the outlet, and forming in wet weather a pond-hole. Mr. Ayers says, that formerly the water in it was at times four or five feet deep, but some years ago he drained it in part by a ditch four feet deep, so that now it is merely a wet swampy place, about forty yards in length by twenty-five wide. During the drought last summer it became quite dry, and he took the opportunity to dig out a portion of the earth for manure. In doing this he discovered the bones. The basin slopes gradually from the east to a depth of about twelve feet near the western side. On the top is about one foot of vegetable deposit formed of decayed leaves, &c., then about six inches of whitish sand mixed with vegetable matter, and below this a deposit, which Mr. Ayers says, when first opened, was of a yellowish colour, very much resembling in appearance the manure of a cow-yard when thrown up in heaps in the winter, and had a very strong smell of the same kind. Exposure to the weather has changed its colour to the dull, bluish-black of swamp earth, with which it seems to be mixed, and with great quantities of vegetable remains, principally of marsh plants, scattered fragments of branches of trees, &c. In this deposit the remains were found covered from four to six feet deep, except the largest, which lay near the south-east side of the basin, and were but slightly covered. A few feet to the north of this lay the next in size on its back, and a little to the north and west of this the other two, both as if in a standing position, and the calf was found in a similar position near the north side of the basin. From Mr. Ayers's description, the bones of the largest one must have been disturbed after its death, as the tusks were found reversed alongside of the neck. Between the ribs of two or three of them was a considerable quantity of what Mr. Ayers describes as resembling coarse chopped straw, mixed with fragments of sticks,—no doubt the contents of the stomach.

Not more than one-fourth of the basin has been examined. The openings in it have been made at random, and in each an animal has been found, so that there is probably a number more. "The question," says Mr. Maxwell, "very naturally occurs,—how and when did so many of these huge animals become imbedded in this narrow space? Questions more easily asked than answered. My first conjecture, before seeing the place, was that they had been mired in attempting to reach a spring or lick; but the small extent and shallowness of the basin, and the gradual descent and character of its bottom, (which, as far as has been examined, is perfectly solid, and like much of the ground around, closely paved with rolled stones of gneiss and limestone, generally six or eight inches in diameter,) all forbid such a supposition. It is possible that they may have been swept there by a deluge, which, from the configuration of the surrounding country, would, as it subsided, sweep through the larger depression, with a current to the east, and form an eddy through this one to the west. The whole depression has, in form, a close resemblance to such as we see formed on a smaller scale in the sand along the Delaware. But on the other hand, the number found together, most of them in a standing position, would seem rather to indicate that they had been overwhelmed in one of their native haunts, by some sudden catastrophe; and some circumstances seem to favour the supposition that this could not have been at a very remote period. This little basin receives the drainage of some fifteen acres of land, and seems to have had a considerable growth of grass and marsh plants around it. Under such circumstances, it would seem that the accumulation of vegetable matter indicates no very great antiquity. The holes were so filled with water that I could not ascertain whether the deposit below the sand showed anything like stratification, but as far as I could judge from what was thrown out, its character was pretty uniform throughout, exhibiting the appearance of a marsh much frequented by animals, which had trampled fragments of its plants all through it. I regretted very much that my knowledge was not sufficient to determine the species of the plants of which so many portions remain, but I thought I recognised some which are now growing in the neighbouring marshes, such as flags, cattails, &c. I hope, however, that you or some of your scientific friends will visit the place, and obtain more accurate information than I am competent to give."

ON STANNIC ACID. BY M. FREMY.

The author remarks that chemists have long considered the second degree of combination of tin with oxygen as a base susceptible of combining with acids to form salts: it is also well known that Guyton Morveau proposed to give this substance the name of stannic acid, in order to explain its solubility in the alkalies. M. Chevreul proved that it is a true acid, by showing that it reacted upon campeachy wood like an acid, whilst other metallic oxides, and even protoxide of tin, acted in this case like bases: the subject has since been considered by Berzelius and Gay-Lussac, and in 1835 Graham considered this peroxide as a base capable of combining with variable proportions of water, and forming with them different salts.

In the opinion of M. Fremy, peroxide of tin is in all cases to be considered as a true acid, and as incapable of combining with acids as a base to form salts.

The following are the reasons given by the author for adopting these opinions:—The action of peroxide of tin as an acid upon hematin, and on examining the compound which it forms with acids, no one of them possesses the properties of a salt; nitric acid dissolves but an inappreciable quantity; it combines with sulphuric acid, but the compound is decomposed by a few seconds' ebullition into sulphuric and stannic acids; and the same happens with the most distinctly marked metallic acids, as with antimonic acid: as to the compound of stannic with hydrochloric acid, may it not be compared to the chlorides of arsenic, sulphur, phosphorus and antimony, which have never been considered as true salts? Lastly, stannic acid does not combine with organic acids, or at any rate they form very unstable compounds, which are totally decomposed by water; under these circumstances M. Fremy is of opinion, that stannic acid combines with other acids to form double acids of little stability, like other compounds of this nature, instead of salts. Compounds of acids frequently occur in chemistry, and the complete decomposition of them which water effects is to a certain extent characteristic of them.

M. Fremy states an experiment, which in his opinion demonstrates positively that peroxide of tin is always an acid, even when separated from combination with hydrochloric acid:—if a solution of bichloride of tin be decomposed by carbonate of potash or soda, a precipitate is obtained, which is not peroxide of tin, but stannate of potash or of soda, which are insoluble in water containing a salt of potash; if, on the other hand, the bichloride of tin be treated with an insoluble carbonate, as that of lime or barytes, the stannic acid is then precipitated, possessing all the properties of an acid.—*Ann. de Ch. et de Phys.*, Decembre 1844.

METASTANNIC ACID.

M. Fremy observes, that the experiments of Berzelius and his own show that there are two modifications of stannic acid which possess properties entirely different. One of these is prepared by precipitating bichloride of tin, as already stated, by an alkali or by carbonate of lime or barytes; and to this M. Fremy proposes to restrict the name of stannic acid, whilst for the acid prepared by acting upon the metal with nitric acid, he proposes the name of metastannic acid.

This is always formed as a hydrate, and M. Fremy found, that after drying it by a current of air at the usual temperature, taking the mean of three experiments, it contained 19·8 per cent. of water. Now, taking 58 as the equivalent of tin, and 74 as that of the binoxide, it is evident that a compound of one equivalent of the metastannic acid and two of water must contain 19·5 per cent. of water.

When hydrated metastannic acid is exposed to a temperature of 284° Fahr., or to a vacuum, it loses very nearly 8 per cent. of water; if it parted with one equivalent, it would, of course, amount to 9·7.

When hydrated metastannic acid is dissolved in potash or soda, a salt is obtained, which, when decomposed by an acid, precipitates gelatinous metastannic acid, possessing the same characters as the acid before solution; it combines, for example, with hydrochloric acid, and forms a compound perfectly similar to that obtained by treating hydrated metastannic acid with hydrochloric acid.

Metastannic acid, however, which has been precipitated from potash or soda, differs from the acid which has not been dissolved in an alkali in a very important property, which is that of becoming soluble in ammonia in all proportions, whereas the acid before solution in the alkali is entirely insoluble in this alkali: these two modifications of metastannic acid evidently differ by a proportion of water; for when the metastannic acid which is soluble in ammonia is dried, even at a very gentle heat, it becomes totally insoluble; if also it be boiled for some time in water, it loses its solubility in ammonia: it was found impossible to determine the quantity of water retained by this acid, for the means required to get rid of the hygrometric water would have destroyed its solubility in ammonia; these acids, however, form similar salts with bases.

When metastannic acid is heated in concentrated sulphuric acid, a large quantity is dissolved, and a compound is formed which is very soluble in alcohol and in water; the aqueous solution is decomposed by boiling, and precipitates metastannic acid; this acid contains 19·6 per cent. of water; it is insoluble in ammonia.

The author concludes, both from the quantity of water and of bases with which metastannic acid combines, that it is composed of 3 equivalents of tin and 6 equivalents of oxygen, Sn^3O^6; in this case its number, hydrogen = 1, will be (tin 174 + oxygen 48) = 222.

FORMATION AND COMPOSITION OF CERTAIN METASTANNATES.

M. Fremy states that the metastannates of ammonia, potash and soda are soluble in water, whereas the other metastannates are insoluble therein; the alkaline metastannates are obtained by the direct solution of the acid in the bases; the others are prepared by double decomposition. The soluble metastannates do not crystallize, and are precipitated by alcohol; great difficulty was experienced by the author in obtaining these compounds of uniform composition, so as to determine their equivalent.

Metastannate of Potash.—To prepare this salt by dissolving the acid in the alkali, the solution was treated with alcohol, and a gummy deliquescent precipitate was obtained, which, after washing with alcohol, was quickly dried between folds of filtering paper, and immediately transferred to a vacuum.

This salt consisted of,—

Metastannic acid....	75·8
Potash	14·1
Water	10·1
	100·

The author considers this salt to be represented by Sn^3O^6, KO, 3HO, in which case its composition in 100 parts would be, acid 75·1, potash 15, water 9·9; this is, of course, a neutral salt, containing one equivalent each of acid and base.

In order to be still more certain as to the equivalent of metastannic acid, the author formed some acidulous metastannates, by dissolving the neutral metastannate above described in a large quantity of water and precipitating the solution by alcohol; the salt thus obtained is gelatinous, soluble in water, and has a strong alkaline reaction. It often becomes insoluble by drying, even at the temperature of the air. This salt yielded by analysis,—

Metastannic acid....	82·6
Potash	8·7

Representing this salt by $2Sn^3$, KO, 5HO, it would consist of acid 82·9, potash 8·7, water 8·4 in 100, or it is a bimetastannate; a corresponding soda salt was formed by the same means.

The author observes, that there exist but few examples in chemistry of salts undergoing decomposition by heat, when one of the elements composing them undergoes no change of state; but the salts now described when heated are decomposed and become insoluble, the acid separating from the base and water being disengaged during the calcination; when the calcined salt is treated with water, the alkali is dissolved, and the metastannic acid remaining insoluble contains mere traces of the base. The production of hydrate of potash when metastannate of potash is heated, prevents the direct determination of the quantity of water contained in this salt.—*Ann. de Ch. et de Phys.*, Decembre 1844.

ON DIGITALIC ACID. BY M. PYRAME MORIN.

To prepare this acid, the leaves of the plant are to be treated with hot water; the infusion is acid, and is to be evaporated by the water-bath to the consistence of a thick syrup; to the residue a large quantity of alcohol is to be added until it produces no further precipitation; it is to be suffered to remain for some days till a deposit is formed, and which ought not to be bitter, but which is so if the alcohol was too strong.

The liquid is then to be filtered, and distilled in a water-bath till it becomes a thick extract, which is to be treated with pure sulphuric æther, and kept at a moderate heat for an hour and eventually boiled; this operation is to be repeated several times.

The æthereal tinctures thus obtained are acid, and have a slightly greenish-yellow colour, especially the first portions. By this process the bitter principle, or digitalia, and the digitalic acid are dissolved; to this æthereal solution barytes in fragments is to be gradually added, which forms a yellowish precipitate, and when any remains insoluble, or the solution restores the blue colour of reddened litmus, enough barytes has been added.

The liquor, which contains only digitalia, but not perfectly pure, is then to be filtered. The precipitate is to be collected, which is to be washed with æther to remove all the bitterness, and then with alcohol, till it passes but slightly or not at all coloured.

The precipitate is to be diffused through cold water and mixed with sulphuric acid to precipitate the barytes; and it is better to leave some digitalate of barytes undecomposed than to add too much acid. By filtration a strongly acid, reddish-coloured solution is obtained; this is to be slowly distilled in a glass retort in order to separate much of the water, and care is to be taken that the heat is not too great. On cooling, a small quantity of a brown substance decomposes, the clear liquor is to be poured off, and sufficient alcohol is to be added to it to precipitate any digitalate of barytes which may have escaped decomposition; by evaporating the liquor *in vacuo*, crystals are abundantly formed.

In all these operations it is requisite to avoid the contact of air as much as possible, for the digitalic acid is converted by it into a brown substance: notwithstanding the evaporation *in vacuo*, the formation of this substance cannot be prevented, and the crystals are consequently formed in a liquid of a deep colour: the more the action of the air is prevented, the purer are the crystals obtained. The crystals are to be separated by and pressed between filtering paper, dissolved in a small quantity of alcohol, and again crystallized *in vacuo*, by which they are obtained white.

The properties of digitalic acid thus obtained are, that it crystallizes in long needleform crystals; it possesses a slight peculiar odour, which is increased by heat, and it yields a suffocating vapour, a small quantity of which occasions coughing; at a higher temperature it melts, becomes black, and burns with a white flame; it leaves a charcoal which readily burns without residue; no ammonia is produced during the decomposition by heat.

The acid when pure is perfectly white; but the action of light, heat and air upon it is very great, readily converting it into a brown product, which colours the solution, and from which it is difficult to free it, and on this account the greatest care must be taken during its preparation. When the acid is dissolved in a small quantity of water, it may be converted during evaporation by the water-bath into a deep-coloured substance: this is itself acid, insoluble in water, slightly soluble in æther, and readily so in alcohol.

Digitalic acid becomes still more readily brown by the action of alkaline bodies; thus in preparing it excess of barytes must be avoided.

Digitalic acid has a sour but not disagreeable taste; it immediately reddens litmus paper, expels carbonic acid from carbonates, and forms digitalates, and combines directly with basic oxides, and the soluble salts which it forms become readily yellow in the air.—*Journ. de Pharm. et de Ch.*, Avril 1845.

PREPARATION AND PROPERTIES OF CERTAIN DIGITALATES.

M. Morin states the properties of the following digitalates:—

Digitalate of Soda.—Obtained by exactly saturating the acid with carbonate of soda, and evaporating the solution under the air-pump vacuum. It is white, and crystallizes very well, but by exposure to the air it becomes quickly yellow, and so also does the solution. It is very soluble in water.

Digitalate of Potash.—Obtained by decomposing the carbonate in the same way as the preceding. It crystallizes with difficulty and is very soluble in water.

Digitalate of Magnesia.—Obtained from the acid and carbonate of magnesia; soluble in water.

Digitalate of Barytes.—Obtained by saturating the acid either with barytes or the carbonate. It is soluble in water, but insoluble in alcohol and in æther. Quite colourless, but by exposure to air and heat it becomes brown.

Digitalate of Lime is obtained in the same way as the barytic salt, with which it has great analogy; being insoluble in alcohol and in æther, it may be employed for the preparation of digitalic acid.

Digitalate of Zinc.—Obtained by treating the carbonate with digitalic acid; the filtered solution is to be evaporated by the air-pump vacuum.

This salt is formed notwithstanding an excess of carbonate. It is obtained at first as a transparent salt, having the appearance of a thick solution of gum; but after some days a number of small arborescent crystals are formed. This salt does not so quickly become yellow by exposure to the air as those previously described.

Digitalate of Lead.—A white, heavy, insoluble salt.

Digitalate of Copper.—A greenish, soluble salt.

Digitalate of Silver.—A white salt, insoluble in water, but soluble in nitric acid.

A solution of digitalate of soda gives, with protosulphate of iron, an abundant flocculent precipitate; with the peracetate of iron it produces no effect; these reactions prove, according to the author, that digitalic acid cannot be mistaken for gallic acid.

This acid exists, probably, uncombined with digitalia, for after having treated the watery extract with alcohol, an abundant brown precipitate is obtained, which is devoid of bitterness, but possesses an acid reaction, and digitalic acid may be obtained from it.

M. Morin observes, that the peculiar properties of digitalic acid, as such, appear to him to be sufficiently established; though it may, he states, be objected, that it is formed under the influence of the substances, especially of barytes, employed to obtain it; but to this objection it is replied, that the juice of the digitalis possesses an acid reaction as soon as it is procured; that the dry leaves treated with water, alcohol, or æther also yield acid solutions; if the alcoholic or æthereal solution, obtained during the preparation of the acid, be saturated with oxide of lead, the liquor remains bitter, and a precipitate is formed; if this be washed in the water and alcohol, and then treated with hydrosulphuric acid, the excess of which is to be expelled, a very acid solution is obtained, from which digitalic acid is obtained by crystallization. This process the author thinks may, perhaps, be better than that which he has described, unless it be wished to obtain the bitter principle at the same time; in this case the action of the base employed is not sufficiently energetic to allow of the formation of a secondary product; the same effect may be produced with carbonate of lead.—*Journ. de Pharm. et de Ch.*, Mars 1845.

ON PIAUZITE, A MINERAL RESIN. BY M. HAIDINGER.

Colour, blackish-brown; streak, yellowish-brown; massive; lustre, resinous; fracture, imperfect conchoidal; translucent on the thin edges; feeble lustre in cut places; hardness = 1·5; specific gravity, 1·220.

Chemical properties.—At about 590° Fahr. it inflames; it burns at a somewhat higher temperature with a peculiar aromatic smell, with much flame and soot, leaving an ash. It is completely soluble in æther and in potash; it is almost entirely soluble in anhydrous alcohol, but less soluble in alcohol containing water. Fuming nitric acid converts the colour of the dark brown resin into yellowish-brown. Heated in a glass tube, there is distilled from it a yellowish oily fluid, which has an acid reaction. In its common state it contains 3·5 per cent. of hygroscopic water; when dry it yields 5·96 per cent. of ash.

It occurs in veins from one inch to two inches wide, traversing brown coal and bituminous wood, in a brown coal deposit, in the neighbourhood of Piauze, north of Newstadtl, in Carniola.

Its easy inflammability, and the abundance of soot which it deposits during burning, cause it to be used for giving the black colour to cast-iron ware.—*Poggendorff's Annalen. Jameson's Journal.*

ON CRYSTALS IN THE CAVITIES OF TOPAZ. BY SIR D. BREWSTER.

The author gave a brief notice to the British Association of the discovery which he had made, about twenty years ago, of two new fluids in the cavities of topaz, and other minerals.

One of these fluids is very volatile, and so expansible, that it expands twenty times as much as water with some increase of temperature. When the vacuities in the cavity which it occupies are large, it passes into vapour; and in these different states he had succeeded in determining its refractive power, by measuring the angles at five feet. Total reflexion takes place at the common surface of the fluid of the topaz. The other fluid is of a denser kind, and occupies the angles and narrow necks of cavities. The cavities, however, in which the soluble crystals are contained, are of a different kind. They (viz. the cavities) are imperfectly crystallized, and thus they exist in specimens of topaz which contain the cavities with the two new fluids; they contain none of the volatile and expansible fluid, which is doubtless a condensed gas. The crystals which occupy them are flat and finely crystallized rhomboids. When heat is applied, they become rounded at their edges and angles, and soon disappear. After the topaz has cooled, they again appear, at first like a speck, and then recrystallize gradually, sometimes in their original place, but often in other parts of the cavity, their place being determined by the mode in which the cooling is applied.—*Jameson's Journal*, April 1845.

THE LATE PROFESSOR DANIELL.

Our readers will have heard, with sincere regret, of the sudden decease of Professor Daniell of King's College, which took place on the 13th of March, whilst he was attending a meeting of the Council of the Royal Society. Our space does not at present permit us to insert any biographical sketch of this distinguished philosopher and truly excellent man; but it is with pleasure that we have it in our power to state, that a subscription has already been entered into to do honour to his memory. On the Committee we see many of the personal and scientific friends of the late Professor. It is intended to execute a bust and establish a chemical prize in connection with King's College, the scene of his labours; and subscriptions for this purpose, we understand, are received at Lubbock's, at Coutts's, and at the Secretary's office in King's College. No one more truly deserved such a mark of respect than he whom it is thus intended to commemorate.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1845.

Chiswick.—March 1. Dry haze. 2. Fine: cloudy. 3. Rain. 4. Sharp frost: cloudy: clear and frosty. 5. Snowing: cloudy and cold: severe frost. 6. Severe frost: cloudy and cold: frosty. 7. Cloudy and cold: frosty. 8. Cloudy and cold: clear and frosty. 9. Cold and dry. 10. Overcast. 11. Fine, with clouds: clear and frosty. 12. Overcast: cloudy: sharp frost. 13. Frosty: cold and dry: severe frost at night. 14. Clear, with severe frost: cloudless, cold and dry. 15. Frosty: clear: cloudy: frosty. 16. Frosty: snowing. 17. Clear and frosty: bright sun: clear and frosty. 18. Clear and frosty: fine: overcast. 19. Overcast: fine. 20. Clear, cold and dry. 21. Clear: fine: overcast. 22. Overcast: slight rain. 23. Rain. 24. Cloudy: clear and fine. 25, 26. Cloudy and fine. 27. Overcast: boisterous. 28. Rain: boisterous. 29. Clear. 30. Clear: overcast: showery. 31. Cloudless and fine.—Mean temperature of the month 6° below the average; the coldest March since 1807.

Boston.—March 1. Fine. 2. Cloudy: snow early A.M. 3. Snow: large fall of snow. 4. Cloudy. 5. Snow. 6, 7. Fine. 8. Cloudy. 9. Fine. 10. Cloudy. 11, 12. Fine: snow P.M. 13. Windy. 14, 15. Fine. 16. Windy: large fall of snow. 17. Cloudy. 18. Cloudy: snow A.M. 19. Fine: snow P.M. 20, 21. Fine. 22. Rain: rain early A.M.: rain A.M. 23. Cloudy: rain P.M. 24. Fine: rain early A.M. 25. Cloudy: rain P.M. 26, 27. Fine. 28. Stormy: stormy all day. 29. Windy. 30. Fine. 31. Windy.—Not near so cold a March since March 1837.

Sandwick Manse, Orkney.—March 1. Cloudy: rain. 2. Cloudy: rain: clear. 3. Frost: showers. 4. Bright: frost: aurora. 5. Bright: frost: clear. 6—8. Cloudy. 9. Showers. 10. Showers: snow-showers. 11—14. Snow-showers. 15. Snow, deep: snow: clear. 16. Snow: cloudy: thaw. 17. Thaw: clear: frost: clear. 18. Thaw: bright: snow-showers. 19. Snow-drift. 20. Snow: bright: snow: cloudy. 21. Thaw: cloudy: drops. 22. Cloudy: rain. 23. Rain: clear. 24. Clear: aurora. 25. Cloudy: clear. 26. Bright: clear. 27. Bright: rain. 28. Rain: clear. 29. Clear. 30. Cloudy: rain. 31. Bright: cloudy.

Applegarth Manse, Dumfries-shire.—March 1. Showers of snow. 2. Clear: frost. 3. Dull. 4. Snow-showers. 5. Snow-showers: frost A.M. 6. Frost A.M.: slight snow. 7, 8. Slight frost. 9. No frost. 10. Rain P.M. 11. Clear: frost A.M. 12. Frost A.M. 13. Frost. 14, 15. Frost: snow-showers. 16, 17. Frost. 18. Frost: shower: snow. 19. Frost. 20. Frost: clear. 21. Frost A.M.: thaw: rain P.M. 22. Heavy rain. 23. Fine spring day. 24. Fine. 25. Rain: mild: growing. 26—28. Rain, and wind high. 29. Clear and bracing day. 30. Heavy rain and high wind. 31. Fair and fine.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. March.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
☾ 1.	29·929	29·904	29·81	29·79	29·75	29·79	29·82	42	28	30	36	30½	38	37	e.	sw.	e.	sse.	......	......	......	......
2.	29·858	29·853	29·60	29·78	29·70	29·82	29·72	42	33	33	46	30	43	38	n.	calm	ne.	wsw.	·10	......	......	·10
3.	29·841	29·545	29·36	29·65	29·85	29·88	29·90	44	25	31	41	37	39	37½	e.	se.	ne.	......	·28	......	......	·25
4.	29·908	29·900	29·65	29·80	29·85	29·97	30·05	45	19	29	35	30	39	31	ne.	calm	e.	se.	......	......	......	·06
5.	30·075	29·949	29·73	29·95	30·10	30·15	30·32	34	13	27	35½	22½	35	30	ne.	e.	ne.	se.	......	......	......	......
6.	30·229	30·205	30·00	30·22	30·23	30·38	30·41	31	23	27·5	36	22½	37	38½	ne.	calm	ne.	......	......	......	......	......
7.	30·232	30·210	30·02	30·24	30·22	30·42	30·38	39	26	23	42½	31½	42½	41	ne.	calm	e.	nw.	......	......	......	......
● 8.	30·161	30·102	29·82	30·19	30·25	30·38	30·40	41	30	36	47	34½	40	41	e.	e.	ne.	nw.	......	......	0·30	·10
9.	30·162	30·144	29·98	30·30	30·26	30·40	30·35	45	32	36	46	35	45	44½	ne.	calm	ne.	nw.	......	......	......	......
10.	30·129	30·058	29·83	30·19	30·02	30·26	30·24	45	32	38	48½	38	40	31	ne.	calm	n.	nne.	·02	·54	......	·12
11.	29·973	29·936	29·65	30·00	29·90	30·14	30·07	40	24	34	40½	30	30	31	n.	nw.	n.	n.	......	......	......	·14
12.	29·815	29·781	29·55	29·83	29·78	29·98	29·95	43	21	30·5	39	27	35	26	n.	calm	n.	se.	......	......	......	·07
13.	29·793	29·740	29·63	29·75	29·64	29·84	29·81	25	13	25	36	22	30	25	e.	se.	se.	nnw.	......	......	......	·11
14.	29·681	29·644	29·48	29·60	29·70	29·90	29·93	36	18	21·5	36	23	28½	27	e.	calm	e.	ne.	......	......	......	......
15.	29·745	29·705	29·50	29·78	29·85	30·06	30·12	36	29	31	34	21½	26½	19½	e.	se.	ene.	se.	......	......	......	·25
☽ 16.	29·572	29·500	29·52	29·80	29·71	30·03	29·82	34	19	30	34½	21	35	35½	ne.	e.	e ne.	w.	......	......	0·02	......
17.	29·770	29·746	29·46	29·65	29·62	29·74	29·70	42	16	28	40½	25½	39	35½	nw.	calm	n.	calm	·03	......	......	·17
18.	29·726	29·674	29·45	29·60	29·57	29·58	29·71	43	27	32	43½	24½	37½	31	n.	calm	w.	nnw.	......	......	......	......
19.	29·858	29·691	29·38	29·65	29·82	29·82	29·87	44	25	33·5	38½	25½	29	30	n.	w.	s.	nw.	......	·33	......	·05
20.	30·274	30·081	29·76	29·97	30·15	30·06	30·10	40	16	27	41½	23	34	32	ne.	calm	s.	n.	......	......	......	·12
21.	30·495	30·479	30·10	30·13	29·98	29·85	29·60	49	31	33·5	43½	23	39	43	w.	calm	sw.	s.	·08	......	......	......
22.	30·381	30·308	29·90	29·95	29·84	29·83	29·47	51	43	42·5	49	40½	46½	46	sw.	calm	nw.	ese.	·08	·23	......	......
○ 23.	30·197	29·957	29·68	29·88	29·88	29·50	29·83	51	42	51	55	45	45	42	sw.	calm	sw.	w.	·60	·14	......	·48
24.	30·121	30·022	29·54	29·97	29·95	29·92	29·92	58	29	45	52	36½	44	40½	nw.	w.	w.	w.	......	·64	0·50	......
25.	30·032	29·908	29·60	29·76	29·66	29·65	29·54	58	43	41	44	30½	43	36	s.	calm	w.	ssw.	·03	......	......	......
26.	29·899	29·803	29·38	29·58	29·27	29·44	29·05	58	42	45	49½	32½	44	40	nw.	calm	w.	sw.	......	·10	......	......
27.	29·874	29·803	29·25	29·45	29·30	29·25	29·09	60	47	51	52	44	44	40	w.	w.	wnw.	w.	......	......	......	·37
28.	29·813	29·672	29·05	29·06	29·50	28·60	29·33	58	37	50	49½	45½	43	41	w.	w.	w.	nw.	·01	......	......	·39
29.	30·270	30·035	29·50	29·90	30·11	29·90	30·11	58	27	47·5	52½	40	43½	40	nw.	w.	nw.	nnw.	......	......	......	·29
☾ 30.	30·273	29·997	29·88	30·00	29·43	30·00	29·72	56	36	46	45½	34½	43½	40	sw.	calm	e&sw.	ese.	·02	......	0·76	......
31.	30·227	30·151	29·69	30·00	30·08	29·96	30·00	63	30	48·5	55		42			w.	ssw.	sw.	......	......	......	·10
Mean.	30·010	29·919	29·63	29·85	[illegible]	·87	29·881	45·5	8·26	35·6	4								1·25	1·[illegible]8	1·58	3·17

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LXVII. *On the Interference Spectrum, and the Absorption of the Tithonic Rays.* *By* JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York*[*].

CHEMISTS are beginning to experience the want of a nomenclature applicable to the various facts which have lately been discovered in relation to the action of some of the imponderable principles. Of these several present themselves to us under the form of radiations, it being their function to group and arrange material particles into the various substances which the organic and inorganic kingdoms present. We may therefore conveniently class them, as Sir J. Herschel proposes, under the name of actinic forces, and entitle the science which treats of them actino-chemistry. For voltaic currents some other provision must be made.

But the progress of science further requires that a general nomenclature should be agreed upon, to mark out apparent subdivisions among these radiant forces. Even though the separate existence of some of them may not as yet be definitely proved, we must have provisional terms to designate distinctly the facts of which we speak. For want of better, I shall continue to use in this memoir those which I have heretofore employed, and speak of four agents existing in the solar beams:—1st, rays of light, or photic rays; 2nd, rays of heat, or thermic rays; 3rd, tithonic rays; 4th, phosphorogenic rays. The various phænomena and relations of these constitute the science of actino-chemistry.

In the solar spectrum, as formed by the action of a prism, if we consider any one of the coloured spaces, we cannot define with precision effects there taking place without the aid of an additional nomenclature. Throughout the spectrum each one of the various radiant principles occurs. In the blue space, for example, there is light which gives an impression to our

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[*] Communicated by the Author.

visual organs,—heat which disturbs a thermometer,—a tithonic ray which produces the decomposition of iodide of silver, and a phosphorogenic ray which causes the sulphuret of lime to glow. Even after the spectrum region is pointed out, we require for the distinct understanding of the case a further determination of the active principle involved; whether it has been light or heat, tithonicity or the phosphorogenic principle.

As is well known to the readers of this Journal, I have endeavoured for some time past to show that the rays occurring in the sunbeam, and passing now under the names of tithonic and phosphorogenic, are entitled to be regarded as distinct imponderable principles. The main object of the memoirs I have published, has been to bring these from their former ill-defined and indistinct position, and, by giving them specific designations, to have their title to the same rank as light and heat recognised by chemists. The names which I have suggested might, perhaps, have been amended; but after all it is with facts that we deal; and, provided our words are euphonious, and are connected with no erroneous theories, they may answer the purpose until the progress of discovery gives us a clearer view of the whole subject, and enables us to employ better. It is very probable that other imponderable principles will be recognised, and, for the present, trivial names are more appropriate for them than theoretical ones.

In this memoir I propose to confine my observations to the following propositions:—

I. To describe the photographic and tithonographic phænomena of the interference spectrum, and to suggest therefrom a suitable subnomenclature for actino-chemistry.

II. To determine the laws of absorption of the tithonic rays.

As the laws of absorption are at the foundation of actino-chemistry, I shall consider this latter proposition under two heads:—1st, to determine the changes which the ray itself undergoes; 2nd, to determine the changes which happen to the ponderable matter or sentient surface. These observations will, for the present, be limited to tithonic, photic, and thermic absorption. Of phosphorogenic absorption I shall speak hereafter.

I. *Phænomena of the Interference Spectrum.*—The prismatic spectrum, even when every precaution has been used to obtain it in a state of purity, its fixed lines being visible, is liable to lead us into many errors. As respects its luminous or photic properties, we cannot determine the distribution or intensity of the light, because the violet extremity is unduly dilated.

As respects its tithonic effects the same difficulty occurs, for these are necessarily controlled and disturbed by the law of distribution. All chemical actions occurring in the more refrangible regions, by being spread over a great space, appear to be more feeble than what they actually are.

In a perfect spectrum the most luminous portion of the yellow should be in the centre; and from this the intensity of the light should gradually decline, fading away on one side in the red, and on the other in the violet. At equal distances from the middle yellow point, on either hand, the intensity of the light should be equal. These beautiful results are due to Mossotti, who also shows that the length of the extreme red wave, is to that of the extreme violet, in the simple proportion of 2 : 1.

The prismatic spectrum does not exhibit these facts. The yellow is not in the centre; the blues are abnormally spread out, the spectrum having its own law of distribution. But the interference spectrum enables us to observe them. Its photographic action on the retina accords with the above-mentioned conditions; but its tithonographic action, as I shall presently show, appears not to correspond thereto.

By the aid of a heliostat, I arranged horizontally in a dark room a narrow riband of light, coming through a fissure $\frac{1}{30}$th of an inch wide. At the distance of twelve feet it fell perpendicularly on a piece of flat glass, the surface of which was ruled with equidistant parallel lines by a diamond, and having been silvered with tin foil, after the manner of a mirror, it served the purpose of a grating. The reflected beam went out through the aperture at which it entered; and on either side of it, to the right and left, the well-known double series of interference spectra made their appearance. I selected, for the obvious reason that it was not overlapped by its successor, the first of one of the series, and intercepting it by an achromatic object-glass, placed in the focus a frame capable of holding a ground glass or sensitive surfaces. This frame was adjusted until the fixed lines were distinctly depicted upon it.

For a further description of the reflected interference spectrum, I may refer to any of the elementary works on optics. It is sufficient for my purpose here that the reader should bear in mind, that the angular deviations of any two colours from the primitive incident ray are to one another as the lengths of their respective undulations.

But on the ground glass we see the fixed lines, and the length of waves corresponding to those lines has been rigorously determined by Fraunhofer. The following Table is extracted from Sir J. Herschel's treatise on Light; the Paris inch being divided into one hundred millions of equal parts:—

Length of wave corresponding to the fixed line	B	2541	parts
... ...	C	2422	...
... ...	D	2175	...
... ...	E	1945	...
... ...	F	1794	...
... ...	G	1587	...
... ...	H	1464	...

When, therefore, we have any tithonic, photic, thermic, phosphorogenic, or any other actinic effect under discussion, in relation to the spectrum, we have only to determine its place among the fixed lines, remembering in the interference spectrum the simple law that connects the deviations and wave-lengths. In the mode of operating here described, absolute exactitude is not reached, because our measures are obtained from a flat surface and not upon a circular arc.

To the interference spectrum thus formed, I exposed for half an hour a Daguerreotype plate, rendered sensitive by iodine and then by bromine. It resulted that the bromide of silver is decomposed at a maximum by a wave of tithonicity, which is 0·00001538 of a Paris inch in length. The action does not extend equally, as we might have supposed, towards the more and less refrangible regions.

I exposed a silver plate which had been prepared by iodine, bromine and chloride of iodine successively. The point of maximum fell, as before, at 0·00001538. The time of exposure one hour. The decomposition commenced by a wave in the green space, the length of which was 0·00002007, and was terminated by one in the violet, whose length was 0·00001257. The point of maximum action, therefore, inclined to the violet, and was not midway between the extremities of the tithonograph. The absolute length of the stain depends however on the time of exposure.

I need not multiply these results. It is sufficient to add, that in several trials I obtained, in these apparently delicate experiments, tithonographs of the interference spectrum, on different surfaces, in great perfection. The fixed lines, which are crowded close together, were beautifully distinct.

I would suggest, therefore, that when we wish to indicate spectrum regions with precision, we should use wave-lengths. By doing this we shall connect the various actinic phænomena, —whether they refer to light, heat, tithonic, or phosphorogenic rays,—with a great many of the numerical results of optics, and have fixed points of comparison.

And further, that when we design to speak less exactly, or merely to indicate in a general way the region to which we refer, we should use the well-known colour designations: thus we might speak of tithonic blue rays, phosphorogenic violet, photic yellow, or thermic red; pointing out at once the imponderable principle in action and the spectrum region it occupies.

II. *Laws of Absorption of the Tithonic Rays.*—There is a very great misconception respecting the mechanical force of the tithonic rays. Thus those which come from a candle or other artificial light, are regarded as being exceedingly feeble, and able only to produce an insignificant result. I took a nitric acid battery, of Prof. Grove's construction; it was sufficiently powerful to maintain a platina wire white hot, and decompose muriatic acid with great activity. It was evolving, as Dr. Faraday has made probable, as much electricity as could be found in a thunder-cloud, and yet the rays of a common wax candle could recompose the muriatic acid faster than the battery could decompose it.

When a beam of light has fallen on any changeable surface, such as a Daguerreotype plate, and is reflected by it, that beam in changing the surface suffers itself a change; for if received on a second similar sensitive surface, it will be found to have lost the quality of giving rise to the decomposition again. Two changes have therefore occurred,—a change in the ponderable body, and a change in the incident beam.

I passed the rays of a lamp through a mixture of equal volumes of chlorine and hydrogen contained in a plate-glass trough; the thickness through which the rays penetrated was 2·6 inches. The emergent ray was received on a tithonometer.

Two distinct phænomena now appeared:—1st, the chlorine and hydrogen began to unite; 2nd, the emergent rays had lost much of their power.

A beam of light, in producing a tithonic effect, becomes proportionally detithonized. Simultaneously the sensitive surface becomes tithonized. The beam undergoes a change in producing a change in the ponderable matter.

The investigation of actinic absorption, therefore, presents itself naturally to us under the two divisions I have already indicated:—1st, changes taking place in the incident ray; 2nd, changes taking place in the receiving body.

In this order I shall proceed to consider them, deducing the doctrine of tithonic absorption from one or two special cases, and then extending it to photic absorption.

CASE OF THE CHRYSOTYPE.—The sensitive material employed in this process is the ammonio-citrate of iron, a solution, which, when viewed through a small thickness, is of a yellow colour.

Prismatic analysis of the Chrysotype.—I projected a motionless spectrum on chrysotype paper, and speedily obtained an impression of a pale brown colour, which, when brought out by neutral chloride of gold, was found to extend from about the fixed line F, to beyond the violet extremity.

Having passed a beam from a heliostat through a trough with parallel sides, containing a solution of the ammonio-citrate of such a strength, and in a stratum of such a thickness, as to appear of a bright yellow colour, I received its spectrum on chrysotype paper. For a long time the paper remained unchanged, but after an hour's exposure I was able to bring out a very faint mark, the centre of which was nearly upon the line F, and the stain extended a little above and below that line.

On considering these two results, it will appear that the active chrysotype rays are absorbed by the ammonio-citrate of iron; or in other words, that this substance is sensitive, because it absorbs a peculiar class of rays. No change can take place in chrysotype paper by rays that have passed through a sufficient thickness of ammonio-citrate of iron, because the active ones have been absorbed, and are already expended in effecting the required decomposition.

The same conclusion was arrived at by experimenting in the following way:—I prepared a sensitive plate by exposure to iodine and bromine successively. This gives, as is well known, a very changeable surface. Such a plate may be called, for the sake of distinction, a test-plate.

On this test-plate I received a spectrum formed from a beam which had passed through the trough containing ammonio-citrate of iron. After a suitable exposure I found a tithonograph extending from the extreme red slightly beyond the fixed line F. But we have already seen that the active chrysotype rays extend from about F to the end of the violet. Consequently, those rays which affect the test-plate after absorption are complementary to those which affect the chrysotype. The following conclusions may therefore be drawn:—

1st. That the rays which escape absorption by the ammonio-citrate of iron are precisely those which do not affect it chemically.

2nd. That the rays which are absorbed by the ammonio-citrate of iron are the rays which produce chemical changes in it.

CASE OF THE BICHROMATE OF POTASH.—A piece of paper dipped in a solution of this salt turns brown rapidly on exposure to the sun's rays; but if there be placed before it a trough containing a solution of the salt, the change then goes on very slowly.

Prismatic analysis of this case.—On projecting a motionless spectrum on this paper, an impression was obtained in a quarter of an hour, reaching from a line a little beyond the fixed line D, which I will provisionally call x, to the violet extremity.

A trough with parallel sides filled with a solution of the salt was next interposed in the beam, and the resulting spectrum received on a bromoiodized plate. The tithonograph extended from the red extremity to the line x. The use here made of the bromide of silver as a test-surface was originally pointed out by Sir J. Herschel.

From this we may draw the same conclusions which we met in the case of the chrysotype; they are as follows:—

1st. *When a ray impinges on a sensitive surface, or passes through a changeable medium, with the chemical effect which takes place the constitution of the ray is correspondingly disturbed. A change in the composition of the medium involves a change in the ray. A specific detithonization of the ray is the necessary attendant on a chemical change in the medium.*

2nd. *Rays which thus disappear by absorption are occupied in disturbing the constitution of the ponderable medium.*

3rd. *Rays which have become detithonized or inactive, with respect to a given medium, and which are therefore not involved in changes going on in it, escape by being transmitted or reflected.*

Similar laws, I believe, will be found to apply to each instance of actino-decomposition, holding equally for the thermic, photic, and phosphorogenic rays, as for the tithonic. An illustration may render this matter plain.

Thus in THERMIC ABSORPTION. If the rays of the sun are converged on red oxide of lead by a burning lens, the heat will be absorbed by that substance and oxygen be given off, a lemon-coloured protoxide remaining. As soon as this is accomplished, no further absorption of heat takes place, and no further chemical changes ensue.

A piece of polished silver exposed to the focus of a burning mirror never melts, not because it is an infusible body, but because its optical constitution is such that it reflects the rays impinging on it. If the polish be taken off, it melts in an instant, because it can absorb the rays. So two different-coloured pieces of cloth exposed to the sunshine upon snow, sink to different depths, because the quality of coloration which they possess enables them to absorb heat more or less rapidly, and the calorific effect is determined by the optical constitution.

In the same manner, in another branch of actino-chemistry, PHOTIC ABSORPTION;—the law under which the discharge of vegetable colours in the solar spectrum takes place has not escaped the penetration of Sir J. Herschel, who has furnished us with so much that is new in this department of science. "The rays effective in destroying a given tint are in a great many cases those whose union produces a colour complementary to the tint destroyed, or at least one belonging to that class of colours to which such a complementary tint may be referred" (Phil. Trans., 1842, p. 189).

Now this is nothing more than an expression of a particular case of absorptive decomposition, in which light is the agent, and vegetable matter the substance involved. The reason that a yellow substance is bleached by the blue rays, *is because it absorbs those rays*; for the very same reason therefore that it looks yellow. A purple vegetable body is bleached by the yellow and green rays, *and because it absorbs those rays it looks purple.*

As respects light, the phænomena of coloration are obvious to our organs of vision; as respects the dark beam of heat, of tithonicity, and of phosphorescence, they must be hypothetical or ideal; but in the same manner that Melloni has found the admission of them for the calorific rays of such admirable advantage, so in this department of science similar and palpable advantages arise. Who could for a moment doubt that light and the tithonic rays were agents totally distinct, as soon as he came to understand that to one of them iodide of silver is yellow, to the other orange; to one chloride of silver is white, to the other red; to one bromide of silver is white, to the other it is black?

On this subject of ideal or imaginary coloration, I do not at present propose to enlarge; on a future occasion I shall return to it.

Let us now investigate the changes which take place in the sensitive body. The substance I have used is a mixture of chlorine and hydrogen, one of the most sensitive combinations known.

When a mixture of chlorine and hydrogen is exposed in a tithonometer to the rays of an Argand lamp, with due precautions to ensure a correct result, four distinct periods of action may be traced.

1st. For a brief space the mixture expands.

2nd. For a much longer period it then remains wholly stationary, neither expanding nor contracting, though the rays are constantly falling on it and it is absorbing them.

3rd. Contraction arising from the production of muriatic acid begins, commencing at first slowly, and then more and more rapidly.

4th. And, after that contraction has fairly set in, it goes on with uniformity; equal quantities of muriatic acid being produced in equal times by the action of equal quantities of the rays.

The following Table represents such a result:—

Spaces.	Time in seconds.	Spaces.	Time in seconds.
Expanded ½ degree in	60	13	20
Movement commenced..............	270	14	19
Reached ½ degree of contraction...	45	15	19
1	55	16	18
2	40	17	18
3	28	18	18
4	27	19	18
5	25	20	18
6	23	21	17
7	22	22	17
8	20	23	18
9	20	24	17
10	21	25	18
11	20	26	18
12	20	27	17
		28	16
		29	16
		30	16
		31	16
		32	16

If we project these observations, laying off the quantities of the gases that have united on the axis of abscissas, and representing the times by the ordinates, they will give us a curve the discussion of which exhibits the leading phænomena of absorption by this sentient mixture; for the ordinates of that curve represent the quantities of the tithonic rays, and its abscissas the corresponding chemical effects.

What now is the interpretation we are to give of the fact, that when a sensitive compound is exposed to a given ray, it does not change all at once, but a certain period must elapse, during which absorption is going forward without any corresponding apparent effect ensuing, and that once accomplished chemical change begins?

Is not this the same phænomenon which has been for a long time known in the case of radiant heat? When a ray of heat falls on a mass of ice at 32° F., in which a thermometer is imbedded, for a certain space of time no apparent rise of temperature takes place, but the radiation continuing long enough, a physical change is accomplished; the ice puts on a fluid form, and now the thermometer commences to ascend, equal quantities of heat producing for a certain period equal effects. Would not the table just given, or a curve projected from it, answer as well to express the phænomena of the action of caloric upon ice, as of the tithonic rays on a mixture of chlorine and hydrogen?

It was from the study of that phænomenon in the case of ice that the doctrine of latent heat arose; and do not these things teach us, that just as a calorific ray becomes latent under certain circumstances, so also does a tithonic ray, and consequently a photic ray? I regard the phænomenon of the pause which is seen before chlorine and hydrogen unite, and during which absorption is taking effect, as setting forth in a strong and clear and prominent manner, that as radiant heat may become latent, so also may tithonic rays, and also rays of light.

We have further to remark, that the consideration of the fourth period leads us to the important law, that for a given compound, equal quantities of the tithonic rays, after the preliminary latent absorption is over, give rise to equal effects.

In thus setting forth these two doctrines,—1st, of the latent condition of the rays which are first absorbed; and 2nd, of the definite action of those absorbed subsequently, I am again urging the same doctrine which four years ago I attempted to establish for iodide of silver.

Let us direct our attention in the next place to what has happened to the ray. We have already seen that when, through a gaseous sensitive mixture, the beams from a lamp are suffered to pass and fall on the tithonometer, they are found to have lost much of their chemical force. The beam has therefore become detithonized.

A glass trough, whose parallel sides were 2·6 inches apart, was filled with atmospheric air over a small pneumatic trough, and the chemical force of a ray passing through it from an Argand lamp was determined by the tithonometer. It was measured by the period required to cause the index to descend through one division, and represented by 12 seconds.

The vessel was now half-filled with chlorine, derived from a mixture of muriatic acid and peroxide of manganese; and the chemical force of the ray, after passing through it, determined as before. It was now represented by 25½ seconds.

To the chlorine an equal volume of hydrogen was now added, the vessel being consequently full of the united mixture. The force of the ray was again measured, and found to be represented by 19 seconds.

Lastly, the first of the preceding experiments was made again, with a view of ascertaining whether the intensity of the lamp had declined, or the apparatus remained in its former condition. It gave again 12 seconds.

Let us group these four results together, representing thus the intensity of the beam by the time it requires to produce a given effect.

A beam through	the glass vessel	and	atmospheric air	12	seconds
...	...	...	chlorine	25·5	...
...	...	...	chlorine and hydrogen	19	...
...	...	...	atmospheric air	12	...

We therefore gather from this, that the addition of hydrogen to the chlorine, far from increasing its absorptive power, actually diminishes it; that in the case before us, when to a given volume of chlorine an equal volume of hydrogen has been added, the absorptive power is diminished to one-half.

We further see that the action of the beam is expended primarily on the chlorine, giving to it a disposition to go into union with hydrogen, and that the functions discharged by the chlorine and hydrogen are wholly different.

The chemical forces of the ray are easily deduced from the foregoing measures, in which the times are given, for it is obvious they are inversely proportional to those times.

As atmospheric air exerts no absorptive action on the tithonic rays, the foregoing experiments give us the effect of chlorine, and of chlorine and hydrogen, compared with a vacuum. The absorptive action of the glasses being common to all the experiments, may be left out of the final estimate. The difference of the resulting numbers 25·5 and 19, from the probable numbers 25·5 and 18·7, may be accounted for from the disturbing causes which are encountered, such as the constant solution of the chlorine by the salt water of the pneumatic trough.

When therefore a ray falls upon this changeable mixture of chlorine and hydrogen, the primary action takes place upon the chlorine, which becomes tithonized, or has a disposition given to it to go into union with the hydrogen; the latter gas appears to be passive so far as the ray is concerned. In the mean time the ray itself becomes changed, undergoing absorptive action and being detithonized.

It is important that we should bear in mind this distinction of the functions which chlorine and hydrogen respectively discharge. The same thing occurs in the Daguerreotype, in which the iodide of silver acts as the absorbent, and the metallic silver behind it assumes the relation of the hydrogen of the foregoing experiment.

There is no reason to believe that oxygen, hydrogen, or nitrogen gases, in masses of ordinary magnitude, exert any perceptible absorptive effect on light, heat, tithonic or phosphorogenic rays. These bodies, therefore, and all others having the same relation, can exert no action on each other, even though they are under the influence of the most intense radiation.

A mixture of oxygen and hydrogen gases exposed to a brilliant light, can never produce water, because neither of its constituents has the power of absorbing the incident rays.

But a mixture of chlorine and hydrogen gas explodes in an instant, under the influence of light, because the chlorine can exert a powerful absorbent action.

If the reason that oxygen and hydrogen cannot form water, under the influence of the sunlight, be due to the circumstance that neither of those gases can absorb tithonic rays, but are perfectly transparent and colourless; and the reason that chlorine and hydrogen at once form muriatic acid, be due to the absorbent capacity of the chlorine; it results, that when a mixture of these latter gases intercepts a ray, the absorbent action upon that ray should not be greater than that of the chlorine alone, and even not more than one-half, because of the diluted state in which the chlorine is presented. But this is the same conclusion to which we have just arrived by experiment.

Although chemical action is the uniform result of absorption, the converse of the proposition does not hold good,—absorption is not necessarily attended by chemical action. Nevertheless it is attended with a certain effect. Even in the case of an elementary, and therefore unchangeable substance, like chlorine, a disposition or capacity for union is communicated. Chlorine, which has been exposed to the sun, unites with hydrogen more readily than chlorine which has been made and kept in the dark.

In some preceding memoirs I have shown that the decomposition of carbonic acid by the leaves of plants is brought about by the yellow photic ray. There seems to be a general relation, though the details of it have not yet been traced, between rays of a particular refrangibility and ponderable substances of a particular kind. Thus, in the case of most of the salts of silver, the point of maximum action falls among the blue rays. In the same way the question naturally arises, does the point for the maximum action on carbon compounds fall in the yellow space, and the yellow, for that reason, become the active ray in decomposing carbonic acid, and giving a green colour to leaves? Is it for this cause, also, that received into the eye, the yellow ray impresses us with the greatest illuminating power? It would be a beautiful result to coordinate phænomena apparently so widely apart as the formation of chlorophyll in a leaf, and the regulated destruction of the retina in the chamber of the human eye in producing the phænomena of vision. In nature there are many results which are apparently equally distinct, and which the progress of knowledge has shown are intimately allied. That to our organs of vision yellow light is the most brilliant, arises from the incidental circumstance, that it is a carbonaceous compound of which the changing nervous expansion is constructed. Had it been possible for nature to have formed a retina in which a salt of silver formed the basis, the maximum of brilliancy of light would have shifted, and the blues would have been among the brightest rays. Is it in the optical peculiarities of the carbon atom that all our ideas of harmony among colours and beauty of external objects have arisen?

Experimental science will probably before long trace a close connection between the physical properties of atoms and the physical properties of rays. It will show that molecules of a given weight can be moved most easily by æthereal waves of a given length, as a stretched string is thrown into vibration by atmospheric undulations of proper dimensions; that the transverse vibrations of the æthereal particles can agitate in a corresponding way ponderable atoms of a proper magnitude and constitution. We shall then have no difficulty in understanding how it was that among metallic substances, those first detected to be changed by light, such as silver, gold, mercury, lead, have all high atomic weights; and why such as sodium and potassium, the atomic weights of which are low, appear to be less changeable.

In this memoir the following facts have been brought forward:—The photographic and tithonographic peculiarities of the interference spectrum, and the propriety of using wave-lengths and colour designations for the purposes of actino-chemistry. We have obtained a rough estimate of the mechanical force of the tithonic rays, and have shown that a ray cannot produce chemical effects without itself becoming dethionized. From the decomposition of the chrysotype preparation and the bichromate of potash, we have deduced the general laws of tithonic absorption, and seen how these may be extended to thermic and photic absorption. From the synthesis of muriatic acid, we have deduced the fact of latent tithonicity, and the definite action of rays of that principle; we have also seen the totally distinct functions which chlorine and hydrogen respectively discharge; we have gathered the reason why water will not decompose under the most brilliant radiation, nor oxygen and hydrogen unite. And, lastly, we have alluded to the probable reason of the coincidence of the maximum point of decomposition of carbonic acid by the leaves of plants, and the maximum point of illumination for the human eye, in the yellow space of the spectrum;—that it originates in the physical peculiarities of the carbon atom.

LXVIII. *On certain Points of Mathematical Optics.*

By M. BIOT*.

AS the second volume of the third edition of my *Traité d'Astronomie* will appear in a few days, I request permission to lay before the Academy various applications of a new mathematical theory of optical instruments which are included in it.

The problem which this theory solves consists in determining, by general and explicit formulæ, the motion of luminous rays through any number of spherical surfaces, either refracting or reflecting, centred on the same axis, and separated by singly-refracting media, of whatever nature, when the inclinations of the rays to the central axis are very small. This enunciation comprises all the conditions to which optical instruments are subjected in the central part of their field of view, where perfection is most important: and the explicit formulæ at which I have arrived serve to establish them directly by simple substitutions of numbers for each given construction of the instrument under consideration.

In my first volume I presented these formulæ in all their generality, and I deduced from them the conditions common to all classes of instruments. But I remarked that, in the special application to purely dioptrical systems, they became susceptible of an abridgement, which, while leaving them the same form, rendered their numerical employment much more simple. It remained for me to prove the truth of this assertion, and to develope its consequences.

For this purpose, resuming the general formulæ which I had established, I limit the systems to which I apply them to any number of spherical lenses, centred on the same axis and surrounded by the same singly-refracting medium. The periodical return of each ray to the same velocity, at every second surface, then allows the general formulæ to be simplified, so

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* From the *Comptes Rendus de l'Acad. des Sciences*, Sept. 9, 1844.

that for a like total even number of surfaces the number of terms is reduced to one-half. Under this new form all the effects of any dioptric system are found still to depend solely on four principal coefficients, connected by one equation of condition. I deduce these four coefficients, both in the general and in the simple case, from one amongst them, by a simple process of differentiation; and I afterwards lay down a general analytical rule, by which we directly obtain this coefficient from which the three others are derived. The path of the rays in any optical system is thus completely determined by the explicit expressions of the four principal coefficients, in which we have only to substitute numbers for each system of given construction. I have collected these results, relative to systems purely dioptric, in a table which expresses explicitly all their effects, and in which may be introduced immediately all the particular conditions to which we may desire to subject them.

I avail myself first of these expressions to develope the conditions which would establish perfect achromatism in an astronomical object-glass of two lenses. As these conditions could not be completely fulfilled without fear of an excessive complexity, I discuss carefully their explicit form, and by means of it I analyse their physical effects, in order to distinguish the most influential of them, and to ascertain the degree of approximation to which they ought to be satisfied. We see, then, in the first place, that there is great danger in leaving a sensible interval between the two lenses of crown- and flint-glass, which was readily evident from physical considerations; so that it is desirable to make this interval nothing, or nearly so, as Fraunhofer always did. When this restriction is effected, we discover the possibility of establishing between the radii of curvature certain relations, which, leaving still a very great liberty of choice in the fixing of their values, have the effect of rendering the achromatism stable, when it shall have been established approximately; that is to say, it will be preserved sensibly exact to the eye, even when, in the practical execution, there may be some slight deviation from the precise values which these relations suppose in the radii of curvature. Having made this remark, I combine the conditions of approximate achromatism with those which destroy the first term of the spherical aberration, to form the final equation which compensates them simultaneously; and as it leaves still disposable the relation of the radii of curvature of the two opposite surfaces, I extract from them the real values of this relation, which approach the nearest possible to the relations previously found for the stability of the chromatic compensation. I thus find that it is confined within extremely narrow limits, between absolute equality of the two radii (which would put the posterior surface of the crown-glass in contact with the anterior of the flint), and a very small difference of length (which would separate by a very minute quantity the margins of the two surfaces). The combinations comprised between these two limits are therefore the only ones which it is suitable to choose, and it appears that they must be nearly equivalent in effectiveness, when we thus confine ourselves to the destruction of the first term of the two aberrations. All give the flint concave in the interior and convex on the exterior. This is precisely the configuration which Fraunhofer adopted, and which he always combined with the nullity of the interval between the two lenses. But the agreement of analytic theory with the practical combinations of this great artist, is seen to be still much more close when it is followed out in numbers. For, starting from the same physical data which he employed for the construction of an object-glass of this kind, all the peculiarities of which he himself indicated numerically, it is not only found to be comprised within the limits of the relations assigned above for the stability of achromatism, but, by adopting the proportion of inequality which Fraunhofer established between the radii of the opposite surfaces, the radii of the four curvatures calculated by my formulæ have been numerically almost identical with his. We may therefore hope that, by following the course which I point out, we shall obtain directly and surely, in all similar cases, the combinations of spherical curvatures which will apply with the greatest advantage to the physical data assigned for the execution.

The object-glass being thus completely calculated, it is necessary to be able to verify, by an exact calculation, whether, in fact, the spherical and chromatic aberrations are sufficiently destroyed in it, with the adopted combinations of thicknesses and curvatures, for the effective aperture which we propose to give it. For this purpose I propose a method of trigonometrical calculation, by which we obtain strictly the values of these aberrations in the different directions in which they have effect; and as the equation of condition which destroys the most sensible parts of it yet admits a slight inequality in the radii of the surfaces which face one another, we may, by varying these elements by a slow gradation, ascertain the direction as well as the extent of the modifications which must be made to render the final values of the aberrations insensible, or at least as small as possible. By these definitive corrections we ought to obtain from the spherical curvatures all the best effects which they can give.

After having developed this important application, I resume the general formulæ peculiar to dioptric systems, and I employ them to establish the theory of eye-pieces applied to achromatic object-glasses. I limit them to the usual case, in which the component lenses of these eye-pieces are made of glasses of the same kind, and I deduce from thence the exact rules for their construction, as well as all the peculiarities of their effects. I then apply the same formulæ to the analysis of the night glasses employed as finders, to that of the day or long-sight glasses, in which the objects are seen erect, and I deduce the best conditions for their construction. I finally employ them in the discussion of the heliometer, which has latterly acquired celebrity from the use which M. Bessel has made of the one constructed by Fraunhofer for the observatory at Kœnigsberg. I deduce from them the rigorous expression of its effects, as well as the modifications which they necessarily undergo under the influence of different temperatures,—a consideration essential for the appreciation of the degree of accuracy of the extremely delicate measures which it is intended to furnish. I then concurrently expound the process of duplication which M. Arago has devised for measuring small visual angles between celestial objects, and I give it with the latest improvements which he has recently introduced.

I am aware that so extended an exposition of the theory of optical instruments may appear too irrelevant to a special treatise on astronomy to be included in it; but I have been constrained to this necessity, when, desiring to present a succinct but exact analysis of the effects of these instruments, and of the principles by which we may regulate and rectify them, and appreciate their qualities or their defects, I have observed that, after so many mathematical investigations upon this subject undertaken by the most able geometricians, we did not yet possess, even in the case of very small inflexions, any analytical method which presented the definitive effects of optical spherical systems, under a general and explicit form, in which we had only to substitute numbers to appreciate their results. So that we were obliged to establish for each instrument a particular discussion, founded upon special simplifications, of which, for the most part, we could not appreciate the degree of exactness, still less justify the necessity. The only advances hitherto made toward this object, and which, without having completely attained it, offered at least an expectation of the possibility of arriving at it, were, I think, the beautiful theorems of Cotes, some generalized inductions of Euler, and, above all, the remarkable indications which Lagrange had given of the employment of finite differences to express generally the derivations of the effects produced by successive surfaces. Following the path which that enlightened mind had opened, and conducting it perhaps more practically through the difficulties of physical circumstances, the peculiarities of which were foreign to his genius, I have succeeded in ascertaining, as I have stated above, that, in the case of very small inflexions which he had treated, and which is the only one within our power, all the effects of any optical instruments whatsoever result in three independent coefficients, having each a distinct physical meaning, which can be made use of, and capable of being derived by the simple differentiation of a single one amongst them, whose general expression I have obtained under an explicit form. All the details peculiar to each instrument are then presented as deductions from this general form, with so much simplicity and evidence, that I could not refrain from presenting them thus. Those geometricians who will take the trouble to cast their eyes over the table of contents, in which I have enumerated the series of questions which this method has enabled me to solve, will perhaps excuse my having brought it forward in a work which at least rendered the results necessary.

Having thus established the details of the optical instruments which are constantly used in astronomy, I explain the other means of accuracy which are not less necessary to it, such as the processes which serve to subdivide the dimensions of measures laid down upon material instruments, the levels, and the plumb-lines, which serve to regulate the horizontality or the verticality of the axes of rotation and of the planes of divided limbs, the mechanical clocks which serve to measure time. Having described the principles of these instruments, and their application in the regulation of divided instruments, fixed or moveable, which are used in astronomy, I make them concur in the investigation of the diurnal motion of the heavens, whose exact circularity and uniformity I demonstrate, by observations strictly calculated. I thus succeed in introducing the use of this motion instead of that of mechanical clocks, as giving a much more perfect unit of time and its subdivisions. To have proceeded further in following up the logical plan of deduction which I prescribed to myself, it would have been necessary to enter on new questions which would have extended this volume too much, and I have thrown them into the succeeding one. Then, in order to complete this volume by results the preparatory notions of which were already comprised in it, I have annexed thereto, as an addition, the description of the measure of the seconds pendulum by the process of Borda, and the discussion of the experiments made as well by this process as by the comparative pendulums, to determine the laws according to which gravity varies throughout the whole extent of the terrestrial spheroid.

I have performed alone the numerous numerical calculations which are given in this volume and in the preceding one. Although I have made them with care, and most of them several times, I cannot be sure that some errors of detail have not escaped me. But the attentive reader, who must consider them only as examples, will, as he proceeds, himself find opportunities of correcting them. I have more reason to fear the errors which may have escaped me in the analytical formulæ, on account of the general influence which they might have upon the applications of them. But a person well-versed in analysis, and very skilful in discovering the errors of the expressions which it employs, M. Yvon Villarçeau, has had the kindness to revise attentively the formulæ which this volume contains, and to point out to me the inaccuracies which had escaped in the printing. I have rectified those which were reparable, and which might most easily have misled. I have indicated in an *erratum* others which, notwithstanding their obviousness, might embarrass the reader for a moment, requesting him to take the trouble to correct them before reading the work, and especially before making any applications of it.

LXIX. *On the Symbols* sin ∞ *and* cos ∞, *and on* Fourier's *Theorem. By* R. Moon, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society*[*].

THE remarkable symbols sin ∞ and cos ∞ have for some time attracted the attention of mathematicians, and very opposite opinions have been entertained as to their nature. It appears to have been taken for granted in the first instance, that when considered numerically they denote indefinite magnitudes, that indefiniteness being confined between certain limits, namely plus and minus unity; and this is certainly all that can be said of these symbols *à priori*. But recent writers have supposed that the above conclusion is erroneous, and that even considered numerically these symbols have definite values, or in fact that they denote evanescent quantities. To leave undecided a question of this nature would be a stigma on mathematical science. If we adopt the first opinion, we are met by certain analytical difficulties which have never yet

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[*] Communicated by the Author.

been clearly resolved; and if we take up with the second, we have the dissatisfaction of resting in a conclusion manifestly opposed to one's common sense notions, which it must be remembered may indeed fail to conduct us to the truth, but cannot mislead us.

A full investigation of the subject is therefore necessary; and if there be some to whom this discussion may appear of obscure interest, I would observe that we shall be led by it not merely to settle the point immediately at issue, but to expose the fallacy of principles and results now universally received and occasionally of the utmost importance, and to give to a great branch of mathematical inquiry a degree of truth and certainty which it has never hitherto possessed.

The conclusion that sin ∞ and cos ∞ represent indefinite quantities, is so obvious from the simple consideration of the symbols themselves, and is apparently so incontrovertible, that it is difficult to conceive how any person could have been brought to believe the contrary; yet of all the writers who have held the opposite opinion, Prof. De Morgan is the only one who has seen the propriety of attempting to obviate the force of the reasoning upon which the rival theory is grounded, though till that is done no other can be said to be established. I shall therefore give Mr. De Morgan's argument upon this point in his own words:—

"The continental mathematicians with one voice pronounce these symbols (sin ∞ and cos ∞) to be indeterminate in value, which is strictly true as far as *à priori* considerations are concerned; for a periodic function of x cannot be said to be in one part of its period rather than another, when x is infinite. If, however, we assume $\phi(x)$ to stand for x terms of the series $1-1+1-1+$&c., we might equally conclude that $\phi(x)$ is indeterminate when x is infinite, no reason existing to prefer 0 to 1 or 1 to 0; nevertheless there exists no doubt that this series represents half a unit" (vide Library of Useful Knowledge, The Differential and Integral Calculus, p. 640).

If the true nature of the above series had been distinctly recognised, we should not now have to discuss the meaning of sin ∞ and cos ∞. The true value of that series continued to infinity is not half a unit, but is either 1 or 0 indifferently.

If we put $\quad S=1-1+1-1+$ &c. in inf.,

we have $\quad S= \quad 1-1+1-$ &c. ......

and $\quad \therefore 2S= \quad 1.$

But if we adopt a different local arrangement of the two series, we have

$$S = 1 - 1 + 1 - 1 + \&c.,$$

$$S = 1 - 1 + 1 - 1 + \&c.;$$

whence we get

$$2\,S = 2 - 2 + 2 - 2 + \&c. \text{ in inf.}$$

$$= 2 \text{ or } 0 \text{ indifferently.}$$

Now there is no reason whatever for preferring the first of these methods to the second, and in fact the two results are equally admissible; whence it is plain that 2 S is actually more* indeterminate than S, having the three values 2 . 1 . 0, and does not by any means enable us to get rid of the ambiguity of value of the latter quantity.

I have entered into the consideration of the above method of investigating the value of the series $1 - 1 + 1 -$ &c., on account of its being somewhat difficult for the student to discover the fallacy of the common mode of conducting it. But another and more satisfactory method may be given as follows:—

We find by actual division, that

$$1 - x + x^2 - x^3 + \&c. + (-x)^n = \frac{1}{1+x} - \frac{(-x)^{x+1}}{1+x}.$$

If x be less than 1, the second member of the right side of the above equation $\frac{(-x)^{x+1}}{1+x}$ vanishes when n becomes infinite, in which case we have

$$1 - x + x^2 - x^3 + \&c. \text{ in inf.} = \frac{1}{1+x}.$$

If x be greater than unity, when n becomes infinite the same quantity becomes indefinitely great, but there is nothing in the circumstances of the problem to determine its sign (since it is impossible to say that an infinite number is either odd or even). The true conclusion therefore is, that in this case either sign will serve, or that

$$1 - x + x^2 - x^3 + \&c. \text{ in inf.} = \pm\,\infty \text{ indifferently.}$$

Lastly, if $x = 1$, we have

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* The rationale of this result is obvious. We may take 1 or 0 for the value of each series at pleasure, and there is nothing to restrict us to take the same value in the two cases, much less are we bound because we take the first value 1 in the first case to take the second 0 in the other, as is actually done in the common method. Hence to obtain all the values of the sum of the two series, we must combine each possible value of the one with each possible value of the other, which gives us the three values 2 . 1 . 0.

$$\begin{aligned} 1-1+1-1+\text{ \&c. in inf.} &= \frac{1}{2} - \frac{(-1)^{\infty}}{2} \\ &= \frac{1}{2} \pm \frac{1}{2} \text{ indifferently} \\ &= 1 \text{ or } 0 \text{ indifferently.} \end{aligned}$$

It is thus plain that Mr. De Morgan's argument derives no support from the example of the series $1-1+1-1+$ &c. in inf.

Mr. De Morgan goes on to say, "And in many different ways (some of which are shown in p. 571) sin ∞ and cos ∞ appear in formulæ which can only be made true by supposing them both to vanish. It must also be observed, that every instance in which the case can be clearly tried by an *à priori* method, confirms the conclusion that indeterminateness of value is to be removed by taking the mean of all the results.

"Two remarkable classes of instances are as follows:—

"1. Take for example $a+bx+cx^2+ax^3+bx^4+cx^5+$ &c., or $(a+bx+cx^2):(1-x^3)$. This, if $a+b+c=0$, becomes $0:0$ when $x=1$; and its value is $-\frac{1}{3}(b+2c)$, or $a+b+c-\frac{1}{3}(b+2c)$, or $\frac{1}{3}(3a+2b+c)$, the mean of a, $a+b$, $a+b+c$." In this example Mr. De Morgan assumes the whole point by supposing the series $a+bx+cx^2+ax^3+$ &c. to be $=(a+bx+cx^2):(1-x^3)$. It will be found by actual division, that

$$a+bx+cx^2+\text{ \&c. }+ax^{3n} = \frac{a+bx+cx^2}{1-x^3} - \frac{bx^{3n+1}+cx^{3n+2}+ax^{3n+3}}{1-x^3},$$

$$a+bx+cx^2+\text{ \&c. }+bx^{3n+1} = \frac{a+bx+cx^2}{1-x^3} - \frac{cx^{3n+2}+ax^{3n+3}+bx^{3n+4}}{1-x^3},$$

$$a+bx+cx^2+\text{ \&c. }+cx^{3n+2} = \frac{a+bx+cx^2}{1-x^3} - \frac{ax^{3n+3}+bx^{3n+4}+cx^{3n+5}}{1-x^3};$$

whence, representing the three last series by S_1 S_2 S_3 respectively, we have, when $x=1$ and $a+b+c=0$,

$$S_1 = \frac{b + 2cx}{-3x^2}$$

$$- \frac{(3n+1)bx^{3n} + (3n+2)cx^{3n+1} + (3n+3)ax^{3n+2}}{-3x^2}$$

$$= - \frac{b+2c-\{(3n+1)b+(3n+2)c+(3n+3)a\}}{3}$$

$$= a;$$

and, similarly,

$$S_2 = a + b, \qquad S_3 = a + b + c;$$

and this quite independently of n, whence it is clear that the proposed series has three values, a, $a+b$, $a+b+c$, and is not equivalent to the mean of those quantities.

It is plain, therefore, that this supposed illustration does not bear out the principle it is adduced to support.

Mr. De Morgan proceeds to say, "2. In applying Fourier's theorem (p. 629) to discontinuous functions, we find that at the point where the discontinuity takes place, and a function which generally can have but one value might be expected to take two, it takes neither, and gives only the mean between them."

It will take some trouble to unravel the maze of errors involved in the above short sentence.

Recurring to p. 629 of Mr. De Morgan's treatise, we find him there proposing to find, by means of Fourier's theorem, "a function of x which is $= x$ from $x = 0$ to $x = 1$ and $= 0$ everywhere else;" and he actually finds a function which he believes (most mistakenly) to satisfy the conditions in general, but which at the limit when $x=1$, instead of giving $x=1$, or $x = 0$, or both these values, gives $x = \frac{1}{2}$.

Before entering on the question of the analytical errors upon which this argument is based, I would say a few words upon its irrelevance to the subject in hand; and with this view I would observe, that there is no reason whatever to expect that in this case, when $x = 1$, the function should have two values. The conditions of the problem are, that the function shall be $= x$ from $x = 0$ to $x = 1$, and $= 0$ everywhere else. Because when $x = 1$ the function is to be $= 1$, and when x is greater than 1 it is to be $= 0$ it is rather an extraordinary conclusion, that when $x = 1$, the function should be 1 and 0 *unâ voce*; yet this Mr. De Morgan thinks "might be expected." But to cut this matter short I shall now show that the principles upon which Mr. De Morgan interprets his function are wholly erroneous, and that Fourier's theorem, by which the function itself was suggested, is untrue.

1. As to the function. The function which Mr. De Morgan finds is the following:—

$$\frac{1}{\pi}\int_0^\infty d\omega \int_0^1 \cos\omega(v-x) \,.\, v\,dv.$$

It is easy to show that this

$$=\frac{1}{\pi}\int_0^\infty d\omega\left\{\frac{\sin\omega(1-x)}{\omega}-\frac{(1-x)\sin\omega(1-x)-x\sin\omega x}{\omega}\right\}$$

$$=\frac{x}{\pi}\int_0^\infty d\omega\left\{\frac{\sin\omega(1-x)}{\omega}+\frac{\sin\omega x}{\omega}\right\}.$$

Mr. De Morgan assumes $\frac{1}{\pi}\int_0^\infty \frac{\sin k\omega}{\omega}d\omega=\frac{1}{2}$, which is not the case, as will be seen when we come to consider the way in which that result is deduced.

We have

$$\int_0^\infty \cos rx\,\varepsilon^{-qx}=\frac{q}{q^2+r^2}; \quad . \quad . \quad . \quad . \quad (\alpha.)$$

whence, integrating with respect to r, we have

$$\int_0^\infty \frac{\sin rx}{x}\varepsilon^{-qx}=\tan^{-1}\frac{r}{q}, \quad . \quad . \quad . \quad . \quad (\beta.)$$

a conclusion perfectly true so long as q is finite, but not otherwise, for when $q=0$ the equation (α.) does not hold; and instead of it we have

$$\int_0^\infty \cos rx\,\varepsilon^{-ox}=\frac{\sin\infty}{r},$$

where (for anything yet proved to the contrary) sin ∞ may be any quantity between $+1$ and -1;

$$\therefore\int_0^\infty \frac{\sin rx}{x}=\sin\infty\log_\varepsilon r+\mathrm{C},$$

from which we have

$$\mathrm{C}=\log_\varepsilon 0\,.\sin\infty=\infty\,.\sin\infty,$$
$$=\text{any quantity whatsoever},$$

which demonstrates the impossibility of assigning a definite value to the function $\int_0^\infty \frac{\sin k\omega}{\omega}d\omega$; and hence Mr. De Morgan's interpretation, which proceeds upon the contrary hypothesis, entirely fails*.

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* It might be argued, that as the equation (β.) holds generally for all finite and positive values of q, it must hold in the limit when $q=0$, but this is not true, as will be explained hereafter.

But in order that Mr. De Morgan may not be able to bolster up his verification of his function by the supposed authority of Fourier's theorem, by which it was originally suggested, it is necessary that I should show the fallacy of the theorem itself.

The truth of that theorem depends on the proposition

$$\int_{-l}^{+l}\left\{\frac{1}{2}+\cos\frac{\pi}{l}(x-v)+\cos\frac{2\,\pi}{l}(x-v)+\&\text{c.}\right\}\phi(v)dv=l\phi(x)$$

(vide Treatise on Differential and Integral Calculus, p. 616 *et seq.*), which in its turn depends on the fact that the limit of the series

$$\frac{1}{2}+\cos\frac{\pi}{l}(x-v)\,\mathrm{A}+\cos\frac{2\,\pi}{l}(x-v)\,\mathrm{A}^2+\&\text{c. is}$$

$$\frac{1}{2}\cdot\frac{1-\mathrm{A}^2}{1-2\,\mathrm{A}\cos\frac{\pi}{l}(x-v)+\theta^2},$$

when $\mathrm{A}=1$, which is not the case, for putting $\frac{\pi}{l}(x-v)=\theta$, we have

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

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

$$=\frac{1}{2}\cdot\frac{1-\mathrm{A}\,\varepsilon^{\theta\sqrt{-1}}+1-\mathrm{A}\,\varepsilon^{-\theta\sqrt{-1}}}{(1-\mathrm{A}\,\varepsilon^{\theta\sqrt{-1}})\,(1-\mathrm{A}\,\varepsilon^{-\theta\sqrt{-1}})}-\frac{1}{2}$$

$$=\frac{1}{2}\left(\frac{1}{1-\mathrm{A}\,\varepsilon^{\theta\sqrt{-1}}}+\frac{1}{1-\mathrm{A}\,\varepsilon^{-\theta\sqrt{-1}}}\right)-\frac{1}{2}$$

$$=\frac{1}{2}\left\{\begin{aligned}&1+\mathrm{A}\varepsilon^{\theta\sqrt{-1}}+\mathrm{A}^2\,\varepsilon^{2\theta\sqrt{-1}}+\&\text{c.}+\mathrm{A}^{n-1}\,\varepsilon^{n-1\theta\sqrt{-1}}\\&\quad+\frac{\mathrm{A}^n\,\varepsilon^{n\theta\sqrt{-1}}}{1-\mathrm{A}\varepsilon^{\theta\sqrt{-1}}}+\mathrm{A}\,\varepsilon^{-\theta\sqrt{-1}}+\mathrm{A}^2\,\varepsilon^{-2\theta\sqrt{-1}}\\&\quad+\&\text{c.}+\mathrm{A}^{n-1}\varepsilon^{-(n-1)\theta\sqrt{-1}}+\frac{\mathrm{A}^n\,\varepsilon^{-n\theta\sqrt{-1}}}{1-\mathrm{A}\varepsilon^{-\theta\sqrt{-1}}}\end{aligned}\right.$$

$$=\frac{1}{2}+\mathrm{A}\cos\theta+\mathrm{A}^2\cos 2\,\theta+\&\text{c.}+\mathrm{A}^{n-1}\cos(n-1)\,\theta$$

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

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

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

Now it might be supposed at first sight, that when n is infinite we should have $\cos \overline{n - 1} . \theta = \cos n\,\theta$, and that consequently the limit of $\cos n\,\theta - \mathrm{A} \cos \overline{n-1}\,\theta$ when $\mathrm{A} = 1$ is 0. But a very little consideration of the true nature of the function $\cos n\,\theta - \cos \overline{n-1}\,\theta$, will show that such cannot be the case. For that function simply expresses the difference of the cosines of two infinite or (which in considering the value of their cosines is the same thing) indeterminate arcs, all that can be said respecting which is, that the difference of their absolute magnitudes is the same constant quantity θ, and hence that function may have any of the series of values assumed by $\cos (\alpha + \theta) - \cos \alpha$, where α increases gradually from 0 up to ∞, *i. e.* may have any value between $-2 \sin \frac{\theta}{2}$ and $+2 \sin \frac{\theta}{2}$, whence it is evidently not true that when $\mathrm{A} = 1$, the limit of the series

$$\frac{1}{2} + \mathrm{A} \cos \frac{\pi}{l} (x - v) + \mathrm{A}^2 \cos \frac{2\,\pi}{l} (x - v) + \&c. \text{ in inf.}$$

is

$$\frac{1}{2} \frac{1 - \mathrm{A}^2}{1 - 2\,\mathrm{A} \cos \frac{\pi}{l} (x - v) + \mathrm{A}^2};$$

and therefore Fourier's theorem falls to the ground.

I might here go on to show that, assuming the general truth of Fourier's theorem, it might be made use of to prove the fallacy of Mr. De Morgan's interpretation of the above function, $\frac{1}{\pi} \int_0^\infty d\,\omega \int_0^1 \cos \omega\,(v - x) . v\,d\,v$. For in the common method of applying Fourier's theorem to discontinuous functions, it is a mistake to suppose that at the limits of discontinuity the true value of the function is the mean between the two adjacent values. For my present purpose, however, enough has been said; and as I believe Fourier's theorem to be unfounded in fact, it would be to little purpose to correct a mistake in its application*.

Having thus, as I hope, exposed the fallacy of the arguments adduced by Mr. De Morgan in order to impugn the validity of the direct method of ascertaining the values of sin ∞

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* As the error in question turns upon a curious point in analysis, I may on a future occasion revert to it.

and $\cos\infty$, by showing that the *à priori* considerations upon which that method rests sometimes conduct us to erroneous results, I shall now proceed to the examination of the specific proofs which have been brought forward to establish the contrary doctrine.

In 1781, as we are informed by M. Lacroix, (*Traité du Calc. Diff. et du Calc. Integ.* tom. iii. 1206), Euler presented to the Academy of St. Petersburg the following formulæ:

$$\left.\begin{aligned} \int \varepsilon^{-qy}\, y^{p-1}\, dy \cos ry &= \frac{(p-1)\dots 3.2.1}{f^p}\cos p\,\theta \\ \int \varepsilon^{-qy}\, y^{p-1}\, dy \sin ry &= \frac{(p-1)\dots 3.2.1}{f^p}\sin p\,\theta \end{aligned} \quad \begin{pmatrix} y=\infty \\ y=0 \end{pmatrix} \right\}, \text{(A.)}$$

where $q = f\cos\theta$, $r = f\sin\theta$. The same formulæ were subsequently obtained by M. Poisson by a different method. From these M. Lacroix proceeds (Art. 1210) to deduce the values of the definite integrals

$$\int dx \cos rx, \qquad \int dx \sin rx, \qquad \begin{pmatrix} x = \infty \\ x = 0 \end{pmatrix},$$

the first of which he proves to be $= 0$, and the second $= \frac{1}{r}$. Whether these last results are M. Lacroix's own, or are to be attributed to either of the eminent persons above mentioned, I am unable to learn; it is my present object to prove that they are erroneous.

Integrating by parts, we have

$$\int \varepsilon^{-ky}\, y^p\, dy = -\frac{\varepsilon^{-ky}\, y^p}{k} - \frac{p\,\varepsilon^{-ky}\, y^{p-1}}{k^2} - \frac{p.(p-1)\varepsilon^{-ky}\, y^{p-2}}{k^3}$$

$$- \&\text{c.} \dots - \frac{p\,.\,(p-1)\dots 3\,.\,2\,.\,1\;\varepsilon^{-ky}}{k^{p+1}},$$

a formula which is always true when p is either a positive integer or zero, and k is not $= 0$.

If $k = q + r\sqrt{-1} = f(\cos\theta + \sqrt{-1}\sin\theta)$, we have

$$\int y^p\, \varepsilon^{-qy}\, (\cos ry - \sqrt{-1}\sin ry)$$

$$= -\varepsilon^{-qy}(\cos ry - \sqrt{-1}\sin ry) \times$$

$$\left\{\begin{aligned} &\frac{y^p}{f(\cos\theta + \sqrt{-1}\sin\theta)} + \frac{p\,y^{p-1}}{f^2(\cos\theta + \sqrt{-1}\sin\theta)^2} \\ &\quad + \frac{p\,(p-1)\,y^{p-2}}{f^3(\cos\theta + \sqrt{-1}\sin\theta)^3} + \&\text{c.} \\ &\quad + \frac{p\,.\,(p-1)\dots 3\,.\,2\,.\,1}{f^{p+1}(\cos\theta + \sqrt{-1}\sin\theta)^{p+1}}. \end{aligned}\right.$$

$$= -\varepsilon^{-qy}(\cos ry - \sqrt{-1}\sin ry) \times \left\{ \begin{aligned} & \frac{y^p(\cos\theta - \sqrt{-1}\sin\theta)}{f} + \frac{py^{p-1}(\cos 2\theta - \sqrt{-1}\sin 2\theta)}{f^2} \\ & \quad + p(p-1)y^{p-2}\frac{(\cos 3\theta - \sqrt{-1}\sin 3\theta)}{f^3} \\ & \quad + \&c. \\ & \quad + \frac{p(p-1)\ldots 3.2.1(\cos\overline{p+1}.\theta - \sqrt{-1}\sin\overline{p+1}.\theta)}{f^{p+1}} \end{aligned} \right.;$$

therefore, equating together the possible and impossible parts of the above expression, and observing that

$$\begin{aligned} & (\cos ry - \sqrt{-1}\sin ry)(\cos\theta - \sqrt{-1}\sin\theta) \\ & \qquad = \cos(ry+\theta) - \sqrt{-1}\sin(ry+\theta), \end{aligned}$$

we find

$$\left. \begin{aligned} & \int y^p\varepsilon^{-qy}\cos ry = \\ & -\varepsilon^{-qy}\left\{ \begin{aligned} & \frac{y^p}{f}\cos(ry+\theta) + \frac{p.y^{p-1}}{f^2}\cos(ry+2\theta) \\ & \quad + \frac{p.(p-1)}{f^3}y^{p-2}\cos(ry+3\theta) \\ & \quad + \&c. \\ & \quad + \frac{p.(p-1)\ldots 3.2.1}{f^{p+1}}\cos(ry+\overline{p+1}.\theta) \end{aligned} \right. \\ & \int y^p\varepsilon^{-qy}\sin ry = \\ & -\varepsilon^{-qy}\left\{ \begin{aligned} & \frac{y^p\sin(ry+\theta)}{f} + \frac{p.y^{p-1}}{f^2}\cos(ry+\theta) \\ & \quad + \frac{p(p-1)}{f^3}y^{p-2}\sin(ry+2\theta) \\ & \quad + \&c. \\ & \quad + \frac{p.(p-1)\ldots 3.2.1}{f^{p+1}}\sin(ry+\overline{p+1}.\theta) \end{aligned} \right. \end{aligned} \right\} \text{(B.)}$$

formulæ which hold for all values of y, provided only the original conditions upon which they were deduced be still maintained,—1, that p be zero or a positive integer; and 2, that either q or r be finite, and not otherwise.

If q be finite and positive, every term of the above expressions vanishes when $y = \infty$, and it is easy to see that the values

$$\int_0^\infty y^p\varepsilon^{-qy}\cos ry \quad \text{and} \quad \int_0^\infty y^p\varepsilon^{-qy}\sin ry$$

deduced therefrom, will be identical with those given by the formulæ (A.). But if $q = 0$, we have, when $y = \infty$ $\left(\text{observing that since } q = 0 \, f \cos\theta = 0, \text{ and } \theta = \frac{m\pi}{2}\right)$,

$$\int y^p \cos r y = \sin r y \left\{ \frac{y^p}{f} + \frac{p\, y^{p-1}}{f^2} + \frac{p \,.\, (p-1)\, y^{p-2}}{f^3} + \&c. \right.$$

$$= \sin r y \,.\, \frac{y^p}{f}$$

$$= \frac{1}{f} \sin\infty\, (\infty)^p$$

$$= \infty\text{, if } p \text{ be finite.}$$

But if p be zero (as it may be), we have

$$\int \cos r y = \frac{1}{f} \sin\infty\, (\infty)^0 = \frac{1}{r} \sin\infty.$$

In the same manner we should find that when y is infinite, the above expression for the indefinite integral $\int \sin r y = -\frac{1}{r} \cos\infty$. Hence it is not true that the formulæ of Euler give definite values for $\sin\infty$ and $\cos\infty$; the fact being that they leave those functions just as they found them, *i. e.* indeterminate.

It would be easy to show that the process adopted by M. Poisson, when properly modified, as it must be to render it in any way applicable, leads to the same result.

It may be contended, that although we cannot follow out the process when $q = 0$, yet the results must still hold; for that as they hold for all finite and positive values of q, however small, they must also hold in the limit when $q = 0$. But to this it may be answered, that we might as well consider the definite integrals

$$\int_0^\infty \cos r y, \qquad \int_0^\infty \sin r y,$$

to be the limits of the integrals

$$\int_0^\infty \varepsilon^{-q y}\, y^p \cos r y, \qquad \int_0^\infty \varepsilon^{-q y}\, y^p \sin r y,$$

where q is negative, as where that quantity is positive. But when q is negative, it is evident from the formulæ (B.), which still hold, that

$$\int_0^\infty \varepsilon^{-q y}\, y^p \cos r y, \quad \text{and} \quad \int_0^\infty \varepsilon^{-q y}\, y^p \sin r y,$$

are infinite for all finite values of q, which shows that the above argument is wholly untenable. The fact is, that the principle that "what holds generally holds in the limit," is not true except in the case of continuously increasing or decreasing functions. Let X represent the equivalent for the indefinite integral

$$\int \varepsilon^{-qy}\, y^p \cos ry$$

in formulæ (B.), then the definite integral

$$\int_0^\infty \varepsilon^{-qy}\, y^p \cos ry$$

consists of two parts, $X_{y=\infty}$ and $X_{y=0}$; the first of which varies continuously with regard to q, and the latter does not. Now of the latter it is perfectly true that the equation

$$X_{y=0} = \frac{\varepsilon^{-qy} . p . (p-1) \ldots 3 . 2 . 1}{f^{p+1}} \cos (ry + \overline{p+1} . \theta),$$

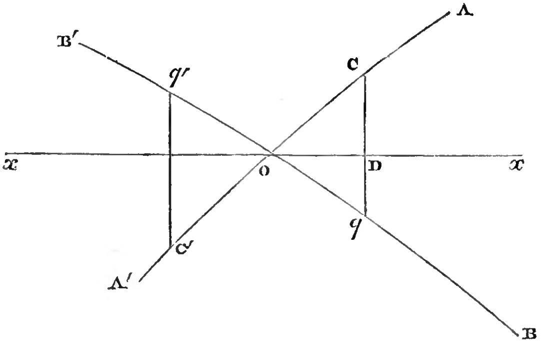
which holds for all finite values of q, holds also when $q = 0$. But it is not true that because when q is finite and positive $X_{y=\infty}$ vanishes, it therefore vanishes when $q = 0$. The principles of limits are here inapplicable. For the limit of a quantity or ratio is defined to be that quantity or ratio to which it continually approximates, but which, although its difference from it may be made less than any assignable quantity, it never actually reaches; and it is obvious that none of these characteristics obtain in the case of the function $X_{x=\infty}$, considered as a function of q.

In like manner the equation

$$\int_0^\infty \frac{\varepsilon^{-qx} \sin rx}{x} = \tan^{-1} \frac{r}{q},$$

which holds generally when q is finite and positive, does not hold when q is $= 0$.

Having thus, as I trust, established the position that the sine and cosine of an infinite arc are quantities which may assume any value between + 1 and − 1, it remains to say a few words on the geometrical interpretation of those symbols.



Draw A A′, B B′ intersecting X X′ and each other in O, so that A O X = B O X = $\tan^{-1}\alpha$; then it is clear that the equation

$$y = \alpha x \sin \infty$$

indicates that the point of which y is the ordinate is situate somewhere within one of the angles A O B′, A′ O B′ (the sides produced indefinitely); or in other words, $y = \alpha x \sin \infty$ is the equation to the *surface* included in those angles.

In like manner the two equations,

$$y = \alpha x \sin \infty, \qquad x = a \sin \infty,$$

(where O D = a) determine the triangle C O E.

The two equations, $y = \alpha x \sin \infty, \quad x = a,$ determine the *limited line* C D E drawn perpendicular to the axis of x.

The equations, $y = \alpha x, \quad x = a \sin \infty,$ determine the limited lines C C′, E E′, where C O = C′O and E O = E O′.

These examples are sufficient to elucidate the principle of interpretation to be adopted.

Cheltenham, March 17, 1845.

Postscript.

At the time of writing the above paper, the subject of which had been long before me, I was not aware that my friend Mr. Earnshaw had communicated to the Cambridge Philosophical Society a paper on the same subject, which has since been published. Our methods of treating the subject, however, are so different, that I have thought it not unadvisable to communicate the above. I cannot help observing, *en passant*, that Mr. Earnshaw seems to treat the crude notions which have been put forth on this subject, with far more respect than they deserve.

10 Maddox Street, Bond Street. May 24 1845.

LXX. *Suggestions relative to the Theory of the Movement of Glaciers.* *By* William Sutcliffe, *Esq.*[*]

THE theory of glaciers is at present an object of such deep and general interest in the scientific world, that a bystander, incompetent to the discussion of the entire subject, may perhaps be permitted to make a suggestion or two in reference to a particular point. If this indulgence is conceded, I would venture to ask,—

I. Whether the plasticity, so strongly indicated by the phænomena which attend the movement of glaciers, and yet so little in accordance with the properties of ice as exhibited under ordinary circumstances, may be due wholly or in part to the influence of heat developed by intense local pressure? That the pressure in the interior of a moving glacier must in many cases be not only very great but very unequally distributed is quite clear; and the friction, when disruption takes

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[*] Communicated by the Author.

place and the severed portion slides forward under the pressure which produced the fracture, is probably enormous.

Now the possibility of thawing ice by friction against itself is considered as established by the experiment of Davy: so that we have a *vera causa* actually present, the only question being as to the extent and mode of its operation. When fissures actually take place, and one portion of ice slides forward, the film of water produced by the mutual friction of the sides would, on the cessation of motion or its diminution beyond a certain rate, immediately freeze, thereby either obliterating the crack or filling it with a thin plate of half-crystallized ice running as a band through the substance of the glacier.

That such cases actually occur is highly probable, but my principal object is to suggest the probable effect of suddenly increased pressure, *without* friction, in producing heat and temporary fluidity at points and surfaces where the compressing force is a maximum, and thereby allowing the particles to slide into new positions, where, released from the excess of pressure, the mass would instantly resume its rigidity. This momentary development and instant reabsorption of heat would be strictly analogous to that which raises the velocity of sound in air by nearly one-fifth beyond the result deduced from the general theory of elasticity.

II. It deserves consideration how far pressure, *apart from* its temporary effect in developing heat, may tend to reduce ice to a state approaching fluidity. We know its influence in preventing the conversion of water into steam, and reducing steam to fluid water. It appears in this case to act simply by opposing obstacles to the separation of particles, and by forcibly bringing them nearer when separated. Now water is denser than ice, and may remain fluid independently of pressure at temperatures below the freezing-point; though in this case the equilibrium subsisting among the particles is *unstable*, and expansion with congelation takes place on the slightest disturbance.

But if water were cooled below the freezing-point while subjected to pressure, it might probably be found to remain permanently fluid; and in this case it would be fair to presume that a pressure sufficiently great would restore ice to the more compact form of fluid water.

It is particularly to be noticed, that the pressure requisite for condensing steam is very small compared with that which would be required for compressing an incondensible gas to the density of water; and it may be fairly supposed that the same rule would hold in the liquefaction of ice by pressure.

The suggestion last offered may be confirmed or disproved by direct experiment; but that respecting the momentary action of heat developed amongst the molecules must, I fear, remain as a theory, to be judged of by its ability to explain a difficulty not easily surmountable otherwise. It is however, as I have already stated, manifestly a *vera causa* actually present, and it would not perhaps be impossible to form some rough estimate as to the probable extent of its influence.

Bath, April 19, 1845.

LXXI. *On the Crania of the Laplanders and Finlanders, with observations on the differences they present from other European races.* By James C. Prichard, *M.D.*[*]

LITTLE has hitherto been done to elucidate the physical characters of the Ugrian or Ugorian races, under which term late writers have comprised the Finns and Lappes, the Magyars or Hungarians, and several nations of Siberia[†].

This is owing to the fact that but few specimens of the skulls of these nations exist in any of the collections in Europe, and few and by no means perfect descriptions of them have been published. Blumenbach has given in his 'Decades Craniorum' a representation of the skull of a Lappe, and he describes it as approaching altogether to the Mongolian variety. Dr. Hueck gives an account of the appearance and general physical characters of the Esthonian Finns, and sums up his observations by pointing out some very considerable differences which he finds between them and the Mongolian form; in fact he says that he can discover nothing common to the Mongolian and Esthonian skulls, except a certain squareness of figure, which is not constant.

From these statements we should be led to suppose that there is a great difference between the skulls of the Finns and Lappes, and we should be inclined to adopt the opinion maintained by Lehrberg, that they are two separate and distinct races, his argument being founded upon the moral as well as the physical diversities between them[‡].

On the other hand, the history of the people, and especially the great similarity of their languages, go far to prove a near relationship between the Finnish and Lappish nations; nor is a greater or less degree of civilization to be looked upon as a proof of diversity of origin, although it may be the cause of all the moral, and possibly of the physical differences also, which exist between the Finns and Lappes.

From this uncertainty it becomes much more important to ascertain, by the examination of their skulls, what the physical characteristics of each nation are, and whether they exhibit any points of

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[*] From the Proceedings of the Zoological Society for August 13, 1844.

[†] Der Ugrische Volkstamm von F. H. Müller.

[‡] Lehrberg, über die Wohnsitte der Jemen, ein Beitrag zur Geschichte Neu-Finnlands, in Untersuchungen zur Erlaüterung der alten Geschichte Russlands.

resemblance which may confirm the supposition that there is affinity between them, or whether, on the contrary, a sufficient degree of dissimilarity can be made out, from an accurate examination, to entitle us to set them down as separate races, and to class them with different grand divisions of the human species; whether, in short, these differences, if any such are found, are more than can be accounted for by the diversity of climate and modes of life which are well known long to have existed between them.

The examination of these skulls for the purpose of furnishing an accurate description of their appearance is interesting in another point of view. In Scandinavia and in Denmark there are numerous tumuli which contain osteological remains of former inhabitants, and it is a disputed point whether they are the remains of a Finnish aboriginal stock or of Cimbrian or some unknown race, since they differ from the old German remains. Now if we could establish a correct notion of the Finnish description of skull, we should have no difficulty in deciding whether the remains before mentioned belonged to this stock.

Having four specimens of these skulls, two of Finns and two of Laplanders, which my father has received through the kindness of Dr. Ilmoni and Mr. Daniel Wheeler, of Bristol, I have an opportunity of examining their peculiarities and of comparing them with each other and with the skulls of other Europeans, Chinese, American Indian, and the Esquimaux, the latter of which is a most remarkable specimen of the pyramidal and broad-faced skull.

Upon taking a general view of these skulls, there are no remarkable features which strike us so forcibly as those which we see in the conformation of the Esquimaux. In fact, the only point worthy of notice here, before we commence the particular description, is a degree of general breadth in the face superior to that which is seen in the European generally, which gives to the whole an appearance of squareness when the lower jaw is attached, and causes the actual shortness of the face, which is remarkable in these skulls, to become still more apparent. The general resemblance between the Finnish skulls and those of the Lappes is as strong as between four average European crania, even belonging to the same nation, and altogether their contour decidedly approaches what Blumenbach calls the Mongolian form of skull, the head appearing, as it has been noticed by an ocular observer, 'of the shape of a pent-house.'

It will be found, however, that it is more especially in a close and minute examination that differences are seen to exist between the Lappes and Finns on the one hand, and the European skulls on the other.

Viewed from above and behind, there is a slight difference observable between the Finn and the Lappe: the posterior part of the Lappe is larger than the anterior, while the form of the Finn is more regular and rounded; that is, the line between the parietal protuberances exceeds the transverse diameter of the forehead more in the Lappe than in the Finn. I find, however, that there is equal difference in this respect between two European skulls even of the same nation.

Again, from the same point of view the skulls of the Lappes present a central eminence or ridge, upon looking at the outline of the forehead (being the line of junction of the two halves of the frontal bone), which is much less marked, in fact scarcely discernible in the Finn, and altogether absent in the European, being on the contrary very strikingly prominent in the Esquimaux. Examined anteriorly, however, a general view of these skulls gives us exactly opposite results; for the sagittal suture, which is now the median line, and the continuation backwards of the frontal suture of early life, upon looking at the outline or horizon of the skull, is seen to project decidedly more in the Finn than in the Lappe; in both more than in other Europeans. Hence we may fairly lay down, that the skulls of the Finns and Lappes have (as far only as the vault of the cranium, exclusive of any effect produced by the width of the face, allows us to conclude,) more tendency to the pyramidal form than the European, but less than the Esquimaux.

Examining these skulls anteriorly, taking into consideration the face, the triangular form is very evident, partly in consequence of the fact above mentioned respecting the vault of the cranium, and partly in consequence of the great width between the external surfaces of the malar bones, which in actual measurement in the two Lappes and the two Finns exceeds the length of the same diameter in other Europeans by at least half an inch, and in one case by nearly an inch, being equal to the same diameter in the Esquimaux; in the latter, however, which exhibits the pyramidal shape in a remarkable degree, the form is owing as much to the shape of the forehead as to the lateral projection of the anterior roots of the zygomatic processes. This width across the face is, as has been correctly observed by Dr. Hueck, not owing to the increased breadth or altered shape in the malar bone, so much as to the altered width and direction of the malar process of the superior maxillary bone.

The outline of the external surface of this bone, viewed from a point exactly in front of the skull, that is to say, the line which runs from the furthest molar tooth that is visible from this point to the suture connecting the malar and superior maxillary bones, is, in the generality of European crania, either vertical, or sometimes even inclined inwards and upwards in the first part of its course, afterwards turning outwards to form the commencement of the zygoma. In the Esquimaux this line runs obliquely upwards and outwards, at an angle of 45° from its commencement; and in the skulls of the Finns and Lappes it is intermediate to the two directions, being however still inclined outwards. This obliquity is also decidedly more marked in the Finns than in the Lappes.

Upon this the anterior view, more of the lateral aspect of the lower jaw is seen than is ordinarily observed, in consequence partly of the greater distance between the condyles, which will be again noticed in the examination of the base of the skull, and partly from the fact that the angles project more in a lateral direction, the entire bone being apparently more developed than in other Europeans.

With respect to some more minute points regarding these skulls, the superciliary ridges are well-marked, the ossa nasi, and the ascending processes of the superior maxillary bones present a flatter and broader anterior surface than the European, and the cavities and foramina are well-marked. [In all these four skulls the supraorbital opening for the frontal nerve and artery is a complete foramen upon the left side, and merely a notch upon the right.]

In consequence of the greater width of the superior maxillary bone, the shape of the circumference of the orbit is not so round as in the generality of European skulls, where the external inferior angle is the lowest, but it is square, with the angles rounded; and for the same reason the space for the antrum is increased, while the depth of the infraorbital or canine fossa is very materially decreased: in one of the Finnish skulls this surface, from the inferior edge of the orbit to the alveolar processes, is almost plane. There is nothing remarkable in the nasal aperture. The shape of the orbit differs materially from that of the Esquimaux, where it is almost round, and from that in the skull of an Indian of the Sioux tribe, where it much resembles the European.

The distance from the inferior edge of the nasal aperture, that is, from the anterior nasal spine to the margin of the alveolar process, is in every specimen of these skulls of the Finns and Lappes decidedly less than in any other European with which I have compared them. The teeth are much ground.

A lateral view of these crania shows that the forehead is somewhat more receding than in the generality of Europeans, although the difference is not great, probably not more than is frequently seen between two specimens of the same tribe.

The general shape of the head resembles that of the European anteriorly, but the posterior part does not project so much. There is a marked difference between the posterior projection of the Finns and Lappes and that of the Esquimaux, the latter being much more prominent.

The line which represents the outline of the ossa nasi, &c., *i. e.* the profile of the face of the skull, presents much less marked irregularities than the European in general. Thus although, as I have before observed, the superciliary ridges are well-marked, the frontal bone does not overhang the ossa nasi, as in the latter, where a decided angle is formed. In the Esquimaux the line from the forehead to the nose is nearly straight, and in the skulls of an Indian of the Sioux tribe and a Chitamache Indian the curve is very regular and open. The junction of the nose and forehead in the Lappes and Finns is therefore more angular than either of the three last-mentioned crania, but much less so than the European.

Upon this the side view another remarkable fact is observed. The occipital bone being not so much developed downwards as in other Europeans (we observed just now that it had less posterior projection also), and the posterior edge of the lower jaw, from the condyle to the angle, being longer than in the latter, upon placing the skull upon a table or any plane horizontal surface, the inferior maxilla merely touches it by its angle, not resting upon the base of the jaw, as we observe in the English, Irish, ancient Irish (cast), Sioux, Italian and Mulatto skulls. The only ones which have this character in common with the Lappes and Finns are the Negro and the skull of a Hindu.

The angle of the lower jaw is certainly more obtuse, seen upon comparing skulls in which the molar teeth remain perfect. In the form and direction of the coronoid process there seems to be no great difference.

The temporal fossæ are well-marked, and in one of the Finnish skulls the anterior inferior angles of the parietal bones are connected to the great wings of the sphenoid by means of an os wormianum upon either side. This is not unfrequently the case in other crania.

The general shortness of the face which has been observed to exist in these skulls, is more plainly seen by viewing them from the side, when we find that the inferior edge of the malar bone is very little higher than the edge of the alveolar process. This is owing not so much to the want of development downwards of these processes, although I have already noticed the shortness of the space between the nose and the mouth, but to the great breadth (from above downwards) of the malar bone, measured from its free inferior border to its junction with the external orbital process of the frontal bone; and it is a remarkable fact, that this measurement, in all the specimens of the skulls of Finns and Lappes, considerably exceeds that of any of the other specimens of European nations, and is equal to that of the Esquimaux and American skulls. The breadth of this surface of the malar bone in one Finn much exceeds that of any which I have had an opportunity of measuring.

Thus the shortness of the face is more apparent upon the lateral view of the cranium, in consequence of the additional width of the malar bone.

The general shape of the basis cranii presents nothing very striking, with the exception of the zygomatic arches. The foramen magnum is of a more oval form than usual, and there appears to be scarcely as great a development of the occipital bone. This agrees with what we observed when considering the lateral aspect of these skulls, and with what has previously been noticed by Dr. Hueck respecting the space for the cerebellum, which, upon an examination of the interior of the cranium, is said to be small, in consequence of the slight concavity of the inferior occipital fossa. The condyles of the occipital bone are remarkably large, being, in three out of four of these skulls, an inch in the long axis, and in one of them (the Finn) longer. They are not unusually broad. This is not the case in any other European cranium which I have examined, but is seen in the Hindu, Chitamache Indian, and to a certain extent in the Esquimaux. There must doubtless have been a much greater freedom of motion backwards and forwards in these joints than is usually the case.

The zygomatic arches, which are best seen at the base, are much more curved than in the other Europeans, slightly less so than in the Esquimaux; and the anterior projection of the alveolar processes beyond the anterior termination of the zygoma is also intermediate between the European and the Esquimaux.

The glenoid cavities are flatter, more widely separated, and not so well-defined as in the European generally, and a difference corresponding to this is seen in the lower jaw, where the condyles, besides being more widely separated from one another, are also more rounded in form, allowing of a greater degree of lateral motion. In correspondence with this fact we also find that the pterygoid processes of the sphenoid bone, especially the external plates, are widened and enlarged, extending farther outwards, affording a greater space for the attachment of the pterygoid muscles, whose duty it is to perform the lateral or grinding motion in mastication. I mentioned above the corresponding fact of the teeth being much worn down.

The ridges for the attachment of the muscles on the palate bone are well-marked, and viewed from below it is seen that the alveolar processes do not project so much from the horizontal part of the palate; that is, that the entire hard palate presents a general curve throughout, instead of being at first plane with a sudden bend, or almost an angle, which is seen at the point where the alveolar processes are given off in the generality of European skulls.

These skulls of the Finns and Lappes are very solid and heavy.

Although this description of the Finnish skulls corresponds in very many respects with that given by Dr. Hueck, yet the examination leads us to an exactly opposite conclusion, viz. that there are very many points in common between the Finn and the races characterized by the pyramidal-shaped skull, and the conclusion with regard to the Lappe corresponds to that which was published by Professor Blumenbach. We are hence able to lay down, that there is no important difference between the skulls of the Finns and Lappes, but that, on the contrary, there is a very great resemblance between them; that altogether they are more nearly allied to the Hyperborean form than to the European; and that if any difference does exist between them, it is that the Finns approach more nearly to this conformation of skull than the Lappes.

LXXII. *On the Metamorphoses of Indigo.*

Production of Organic Bases which contain Chlorine and Bromine. By Dr. August Wilhelm Hofmann, *Assistant in the Giessen Laboratory.*

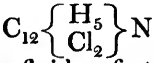
[Continued from p. 404.]

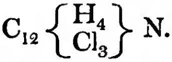
Action of the fused Hydrate of Potash on Dichlorisatine.

B. *Dichloraniline* (*Amachlophenise*).

Erdmann has proved by direct experiment that chlorisatine cannot be converted into dichlorisatine by the further action of chlorine on this substance suspended in water. I have had frequent opportunities of convincing myself likewise of this fact. If an alcoholic solution be employed the decomposition proceeds too far, and products of oxidation are obtained, viz. chloranil, chlorophenussic acid, &c., which no longer belong to the isatine series.

I endeavoured to prepare dichlorisatine by dissolving isatine or chlorisatine in potash and passing a stream of chlorine gas through the solution of isatinate or chlorisatinate of potash. The result was however unfavourable. No dichlorisatine was formed, but in its stead a tarry matter, which floated on the surface of the fluid; it was insoluble in water but soluble in alcohol. I was equally unsuccessful in dissolving chlorisatine in nitric acid and adding to the solution a few drops of hydrochloric acid. A violent reaction ensued, and the decomposition had evidently proceeded too far. There remained, therefore, only to fall back upon the method employed by Erdmann to prepare this substance, and of which Laurent had also made use in his investigation. It consists in treating indigo directly with chlorine. In this process, under every circumstance, there is formed a mixture of chlorisatine and dichlorisatine, the quantity of the latter, according to Erdmann, greatly preponderating. Chlorisatine and dichlorisatine can only be separated from each other by crystallization with extreme difficulty. The amount I had obtained was too small to think of trying to separate them in this way; I therefore at once submitted the mixture with hydrate of potash to distillation.

The distilled product contained, besides chloraniline, another basic substance, collecting on the surface of the fluid, which crystallized in long prisms. This substance was manifestly nothing but dichlorisatine,  $C_{12}\left\{\begin{matrix}H_5\\Cl_2\end{matrix}\right\}N$. The crystals which could be picked out of the fluid unfortunately were not sufficient in amount for a combustion. As I have, however, more completely studied the corresponding bromine compound, which is much more easily prepared, I considered it useless to occupy myself longer with this substance.

C. *Trichloraniline* (*Amachlophenose*).  $C_{12}\left\{\begin{matrix}H_4\\Cl_3\end{matrix}\right\}N$.

Isatine, in which 3 equivalents of hydrogen are replaced by three of chlorine, is as yet not known. The preparation of trichloraniline could not hence be made by persevering in the method hitherto used. It is however procured, as above mentioned, by the direct action of chlorine on aniline or the chlorinated base.

By submitting to distillation with hydrate of potash the crude product of the action of chlorine on these two bodies, which contains a mixture of chlorophenissic acid and trichloraniline, the first remains in the retort in combination with potash, while the latter passes over into the receiver with the watery vapour, and collects on the surface of the liquid in long needles. As this substance is very volatile, a cooling apparatus must always be employed in this distillation.

Trichloraniline is very little soluble in water, but dissolves readily in alcohol and æther. The solution has a neutral reaction. It does not unite with bases or acids, and can be distilled unchanged from water containing either acid or alkali.

This substance is obtained only in small quantities. My whole amount, after the qualitative determination of chlorine and nitrogen, was just sufficient for one analysis, which unfortunately did not turn out very good, but could not be repeated from the fact already mentioned. By burning with chromate of lead the following results were obtained :—

0·3020 grm. of the substance gave 0·4170 grm. of carbonic acid, 0·0665 grm. of water. In the 100 parts—

Carbon . . .	37·65
Hydrogen . .	2·44

The formula is $C_{12}\left\{\begin{matrix}H_4\\Cl_3\end{matrix}\right\}N$. The theoretical constitution is

12	equiv.	Carbon	= 900·00	36·66
4	...	Hydrogen	= 50·00	2·03
3	...	Chlorine	= 1327·95	54·09
1	...	Nitrogen	= 177·04	7·22
			2454·99	100·

It may perhaps appear bold to give an opinion as to the constitution of this body from a single determination of its carbon and hydrogen, which, besides, does not agree very well with the theory. When however we consider the complete analogy in the mode of formation of tribromaniline and chlorodibromaniline, to which I shall afterwards return, and the exact resemblance in the properties of these bodies, less doubt may perhaps be entertained.

It appeared to me also to be worth the trouble of endeavouring to form a base which contained bromine as well as chlorine. Such a body could with ease be obtained, were it possible to form a chlorobromisatine, $C_{16}\left\{\begin{matrix}H_3\\Cl\\Br\end{matrix}\right\}NO_4$. Distilled with hydrate of potash, this compound would yield chlorobrominated aniline, $C_{12}\left\{\begin{matrix}H_5\\Cl\\Br\end{matrix}\right\}N$. For this purpose, I poured on chlorisatine in a retort an excess of bromine, allowed the whole to remain for twelve hours, and then heated it in a water-bath to the boiling-point, until the excess of bromine was distilled off. The red crystals were separated by filtration, washed and crystallized from hot alcohol.

The prismatic crystals thus obtained produced on burning with chromate of lead the following result:—

0·2955 grm. gave 0·5697 grm. of carbonic acid and 0·0695 grm. of water; corresponding in the 100 parts to

Carbon . . .	52·57
Hydrogen . .	2·61

This substance was consequently nothing but pure undecomposed chlorisatine, which contains in 100 parts—

Carbon . . .	52·88
Hydrogen . .	2·25

This compound, $C_{16}\left\{\begin{matrix}H_3\\Cl\\Br\end{matrix}\right\}NO_4$, would have contained

Carbon . . .	36·90
Hydrogen . .	1·15

The small quantity of hydrobromic acid observed in this action of bromine on chlorisatine may have arisen from a small admixture of isatine, which would be converted into bromisatine. By continued action of bromine in sunshine, perhaps another result might be obtained.

D. *Chlorodibromaniline* (*Amachlobrophenose*). $C_{12}\left\{\begin{matrix}H_4\\Cl\\Br_2\end{matrix}\right\}N$.

It has been already stated that aniline is decomposed with facility by bromine. On adding bromine water to the solution of a salt of chloraniline, a white precipitate takes place, which dissolves in boiling alcohol, producing a slight violet colour, and crystallizes on cooling in needles. The same compound can be obtained by the direct action of bromine on chloraniline. On adding bromine to the crystals of the latter, a very violent reaction takes place, with great elevation of temperature and evolution of hydrobromic acid, the mixture at the same time assuming a violet colour. Bromine is to be added till the mass on being fused no longer absorbs any more. The crystalline product is then to be allowed to cool, and well-washed till all trace of hydrobromic acid is removed. By recrystallization from alcohol, white crystals are obtained, which have generally a shade of red.

Fritzsche, as is known, has discovered that by the action of bromine on aniline there is formed a body, $C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N$, which he called bromaniloid. From this decomposition we can already foresee the composition of the product which I obtained by the action of bromine on the chlorinated base.

This must stand in the same relation to the bromaniloid that the chlorinated base does to aniline, and consequently would be expressed by the formula $C_{12}\left\{\begin{matrix}H_4\\ Cl\\ Br_2\end{matrix}\right\}N$.

This conclusion is completely confirmed by the analysis.

The combustion with chromate of lead gave the following results:—

0·520 grm. crystals, dried at the temperature of the air, gave 0·485 grm. of carbonic acid and 0·070 grm. of water; corresponding in the 100 parts to

Carbon . . .	25·43
Hydrogen . .	1·52

which will give the calculated composition,—

			Theory.		Experiment.
12 equiv.	Carbon . . .	=	900·00	25·52	25·43
4 ...	Hydrogen . .	=	50·00	1·41	1·52
1 ...	Chlorine . . .	=	442·65		
2 ...	Bromine . . .	=	1956·61		
1 ...	Nitrogen . . .	=	177·04		
1 ...	Chlorodibromaniline	=	3526·30		

The formula of this compound is $C_{12}\left\{\begin{matrix}H_4\\ Br_3\end{matrix}\right\}N$, and it contains

Carbon	= 22·15
Hydrogen	= 1·22

This body is completely insoluble in water, but soluble in alcohol and æther. It melts under water into a brown oily fluid, which can be distilled over with the vapour of water; it crystallizes in shining needles on the sides of the receiver. Chlorodibromaniline, like the trichloraniline, no longer possesses basic properties. It is soluble in boiling hydrochloric acid, out of which however the greater part again recrystallizes unchanged on cooling. The portion soluble in hydrochloric acid at the ordinary temperature is precipitated by the addition of water. Strong sulphuric acid likewise forms with it a solution of a violet colour, from which it is also precipitated by the addition of water. Neither potash nor ammonia decomposes this substance, which by heat is slightly soluble in these fluids without the least change. It does not combine with bichloride of platinum or mercury, or with oxide of lead. Concentrated nitric acid decomposes the chlorodibromaniline.

ACTION OF FUSED HYDRATE OF POTASH ON BROMISATINE.

It was evidently to be expected that the brominated isatine would yield the same results as the chlorisatine. This was confirmed by experiment. Bromisatine has been as yet but little studied. Erdmann* obtained it in very small quantities by the direct action of bromine on indigo. Laurent did not prepare this subtance; by the action of bromine on isatine he obtained immediately dibromisatine (bromisatinese). The first compound can however easily be formed by employing a solution of bromine in water instead of anhydrous bromine. In two experiments I obtained bromisatine alone, as is shown by the following analysis. The compound is dissolved out by boiling spirits of wine, washed with water to remove all trace of hydrobromic acid, and recrystallized in boiling alcohol, from which it separates in shining prisms.

The following result was obtained on combustion with chromate of lead:—

I. 0·3270 grm. of bromisatine gave 0·5090 grm. of carbonic acid and 0·0625 grm. of water.

II. 0·3955 grm. of bromisatine gave 0·6165 grm. of carbonic acid and 0·0750 grm. of water.

These numbers give the following per-centages, which are placed in apposition with the calculated quantities:—

		Theory.		Experiment.	
				I.	II.
16 equiv.	Carbon .	=1200·0	42·41	42·45	42·51
4 ...	Hydrogen	= 50·0	1·78	2·12	2·10
1 ...	Bromine .	= 978·31			
1 ...	Nitrogen .	= 177·04			
4 ...	Oxygen .	= 400·00			
1 ...	Bromisatine	=2805·35			

The properties of this body, which I obtained in large quantities, agree completely with those of chlorisatine, to which on this account I refer in the memoirs of Erdmann and Laurent.

On submitting bromisatine to distillation with fused hydrate of potash, exactly the same phænomena are observed as those I have already described in the same process with chlorisatine, and there distils over a new base containing bromine.

E. *Bromaniline* (*Amabrophenese*).

a. Composition.

The purification of the crude product of distillation is made with the same ease as in the case of the corresponding chlorine compound. The analogy in the mode of formation of the two left no uncertainty as to the composition of bromani-

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* *Journal für Prakt. Chem.*, Bd. xix. S. 358.

line. To banish all doubt, however, I determined the carbon and hydrogen.

The following results were obtained on combustion with chromate of lead:—

0·4152 grm. of the brominated base gave 0·6463 grm. of carbonic acid and 0·1405 grm. of water. The corresponding numbers in the 100 parts found and calculated are as follows:—

12	equiv.	Carbon .	= 900·00	42·24	42·45
6	...	Hydrogen	= 75·00	3·52	3·75
1	...	Bromine .	= 978·31		
1	...	Nitrogen .	= 177·04		
1	...	Bromaniline	= 2130·35		

The formula is $C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N$. To control this formula the platinum double salt was prepared and analysed: 0·462 grm. gave 0·161 grm. = 26·19 per cent of platinum.

Atomic weight calculated from this . .	2125·80
Theoretical atomic weight	2130·35

This agreement is sufficiently close.

b. Properties of Bromaniline.

The bromine base resembles in so high a degree the chlorine, that I shall have little to say on this point. It crystallizes also with the same facility in large, colourless, regular octahedrons, which in appearance can be as little distinguished from the chlorine base as the bromide from the chloride of potassium. The same similarity extends to its smell, taste and other properties. Its point of fusion, however, lies somewhat lower than that of the chlorine compound. At 50° cent. it melts to a violet oily liquid. On solidifying, the thermometer descends to 46° cent.

The salts of this base, like those of aniline and chloraniline, impart a yellow colour to pine-wood, and its watery solution communicates to chloride of lime a violet tint, much weaker than that caused by aniline, but, on the other hand, much stronger than that produced by chloraniline.

c. Compounds of Bromisatine.

What I have said with regard to the formation and preparation of the compounds of the chlorine base applies for the most part to the salts of the corresponding bromine base. I have only investigated some of these fully.

Oxalate of Bromaniline. $C_2O_3, C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N, HO$.

On adding a watery solution of oxalic acid to this base dissolved in spirits of wine, a white crystalline precipitate is formed. This was separated by filtration, and recrystallized from boiling water. The crystals formed on cooling were indistinct in form, and with difficulty soluble in alcohol and water; insoluble in æther. The combustion of a portion dried at the ordinary temperature gave the following results:—

0·5635 grm. of the oxalate gave 0·8045 grm. of carbonic acid and 0·1695 grm. of water. These numbers correspond to the formula $C_2O_3, C_{12}\left\{\begin{matrix}H_6\\ Br\end{matrix}\right\}N+HO$, as is seen by a comparison of the theoretical and experimental numbers.

			Theory.		Experiment.
14	equiv.	Carbon .	=1050·0	38·99	38·93
7	...	Hydrogen	= 87·50	3·24	3·34
1	...	Bromine .	= 978·31		
1	...	Nitrogen .	= 177·04		
4	...	Oxygen .	= 400·00		
1	...	Oxalate of Bromaniline	=2692·85		

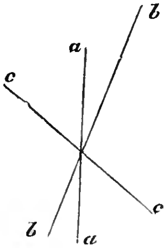
So that the brominated base forms, like aniline, the neutral oxalate under the same circumstances in which the acid salt of chloraniline is obtained.

Hydrochlorate of Bromaniline. $ClH, C_{12}\left\{\begin{matrix}H_6\\ Br\end{matrix}\right\}N$.

This salt, which is obtained directly by dissolving bromaniline in hydrochloric acid, crystallizes on cooling from its hot saturated solution in a radiating filamentous mass. By slow evaporation of the solution over sulphuric acid, large well-formed crystals can be procured.

According to the determination of Dr. T. Müller, these crystals belong to the monoclinometric system (system hemiorthotype of Mohs).

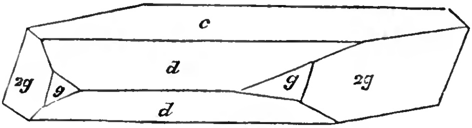
The angle which is formed by the horizontal axis b and a is $=90°$. The axis c forms with a a right, and with b an oblique angle.



Names of the planes :—

$c = c : \infty a : \infty b$
$d = a : c : \infty b$
$g = a : b : \infty c$
$2g = 2a : b : \infty c$

The oblique rhombic prism 2 *g* is terminated by the end planes *c* in such a manner, that the crystals present a tabular appearance. The acute edges of the prism are truncated by the planes *d* and *g*. The planes *g* do not present themselves at all, or if so, it is in the form of small triangles.



From the measurement of the angles result the following relation between the axes:—$b : a : c = 1 : 0{\cdot}989 : 0{\cdot}843$, and the angle between the axes *c* and $b = 72°\ 20'$.

This salt is isomorphous with the hydrochlorate of chloraniline. It belongs to the same crystalline system, and is formed by the same planes. Still the character is somewhat different, viz. the crystals are less extended in the direction of the axis *b*, so that the horizontal edge, in which the two planes *a* cut each other, disappear, and the two planes *d* with the two *g* form a four-sided summit: the angle of the prism 2 *g* : 2 *g* was found here = 127° 48′. The angles of this salt however can not be so well measured as in the analogous one.

The analysis of the hydrochlorate of bromaniline gave the following results:—

1·1425 grm. of the salt gave 0·798 grm. of chloride of silver.

This corresponds to the formula $\mathrm{Cl\,H,\ C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N}$, as is shown by the following calculation:—

		Theory.		Experiment.
1 equiv.	Base	=2130·35	82·40	
1 ...	Hydrochloric acid	= 455·15	17·60	17·71
1 ...	Hydrochlorate of Bromaniline	=2585·50	100·	

Bromaniline and Bichloride of Platinum,

$$\mathrm{Cl\,H,\ C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N + Cl_2\,Pt,}$$

is precipitated by mixing hydrochlorate of bromaniline with bichloride of platinum, and cannot be distinguished either in its relations or aspect from the platinum salt of the corresponding chlorine base. The analysis already given by the determination of the atomic weight furnished 26·19 per cent. platinum, from which is deduced the formula $Cl\,H, C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N + Cl_2\,Pt$, which requires 26·22 per cent.

ACTION OF FUSED HYDRATE OF POTASH ON DIBROMISATINE (*Bibromisatine*, Erdmann, *Bromisatinese*, Laurent).

F. *Dibromaniline* (*Amabrophenese*). $C_{12}\left\{\begin{matrix}H_5\\Br_2\end{matrix}\right\}N$.*

By the distillation of dibromisatine with hydrate of potash there passes into the receiver likewise an oily body, which solidifies into a crystalline mass. By washing these crystals with water till all trace of ammonia, which is formed in the last stage of the distillation, is removed, and dissolving in boiling alcohol, they are obtained of a shining white appearance.

a. *Composition of Dibromaniline.*

A very sure conclusion on the composition of this body may be drawn from analogy. I therefore contented myself with a determination of the carbon and hydrogen.

On burning with chromate of lead the following results were obtained:—

0·2795 grm. of the substance gave 0·2950 grm. of carbonic acid and 0·065 grm. of water. These numbers correspond in the 100 parts to

Carbon	28·77
Hydrogen . . .	2·40

Corresponding formula, $C_{12}\left\{\begin{matrix}H_5\\Br_2\end{matrix}\right\}N$, as is shown by the following:—

			Theory.		Experiment.
12 equiv.	Carbon . .	= 900·00		29·06	28·77
5 ...	Hydrogen .	= 62·50		2·01	2·40
2 ...	Bromine . .	= 1956·62			
1 ...	Nitrogen . .	= 177·05			
1 ...	Dibromaniline	= 3096·17.			

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* The dibromisatine employed was prepared by long digestion of bromisatine with bromine. The substance crystallized from alcohol and burned with chromate of lead gave the following results:—

0·406 grm. of substance gave 0·4715 grm. of carbonic acid and 0·047 grm. of water. These numbers correspond in the 100 parts to

Carbon	31·62
Hydrogen	1·28

and indicate the formula $C_{16}\left\{\begin{matrix}H_3\\Br_2\end{matrix}\right\}NO_4$, which requires

Carbon	31·83
Hydrogen	1·00

b. Properties of Dibromaniline.

These I have not as yet very carefully studied. They resemble in many respects those of bromaniline. The crystals of this body, however, have a totally different form. It does not crystallize in octahedrons as the bromaniline, but in flat, four-sided, rhombic prisms of considerable size, which unfortunately present no terminal planes.

The base is soluble in spirits of wine and yields successive crops of crystals to the last drop. It is only slightly soluble in boiling water, which becomes turbid on cooling, and deposits gradually fine white needles. The crystals of the base melt, between 50° and 60° cent., to a dark oily fluid, which frequently on cooling remains long liquid, but on agitation suddenly solidifies to a crystalline mass.

A solution of the base in acids communicates, like aniline, a yellow colour to fir-wood and the pith of the elder tree.

The compounds of dibromaniline have likewise as yet not been the object of particular study. I will only mention a few observations which appeared to me essential to the establishment of the chemical characters of dibromaniline.

It possesses the properties of a base; its basic character is however much less pronounced than that of bromaniline. It dissolves in acids, and is again precipitated by alkalies. Its solution in hydrochloric acid gives with perchloride of iron the crystalline orange-yellow precipitate, characteristic of organic bases. It forms with acids crystalline salts, but these compounds possess much less stability than the corresponding salts of bromaniline.

A solution of the base in boiling hydrochloric acid deposits on cooling a salt which crystallizes in plates.

0·2313 grm. of these crystals, precipitated with nitrate of silver, gave 0·1205 grm. of chloride of silver, corresponding to 13·21 per cent. of hydrochloric acid. This determination leads to the formula $\mathrm{Cl\,H,\ C_{12}\left\{\begin{matrix}H^{5}\\ Br_{2}\end{matrix}\right\}N}$, as is seen by the following:—

	Theory.		Experiment.
1 equiv. Base	=3096·17	87·19	
1 ... Cl H	= 435·15	12·81	13·21
1 ... Hydrochlorate of dibromaniline	=3551·32	100·00	

This substance is decomposed by the action of warm water. The base separates from the acid and swims on the surface of the fluid in the form of oily drops. This want of stability in the hydrochloric compound is also seen when the base is dissolved in a great excess of acid and placed under a bell-glass with caustic lime. After some time crystals are obtained, which are only slightly soluble in water, and consist almost entirely of pure base. The elasticity of the hydrochloric acid is thus sufficient to overcome the affinity between it and the base.

G. *Tribromaniline* (*Amabrophenose*). $C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N$.

This compound has been long since described by Fritzsche* and named by him bromaniloid. He obtained it by treating aniline with bromine. It forms with the same facility by the action of the same body on bromaniline or dibromaniline.

I have prepared this body from bromaniline. The hydrochlorate of this base gives with a solution of bromine in water a white precipitate with a tint of violet. As the violet colour did not disappear by re-crystallization from alcohol, I submitted the compound to distillation. At the commencement of the distillation there are condensed in the receiver beautiful snow-white crystals; towards the end however the violet accessory product also passed over, and could not be removed again by recrystallization from alcohol. Although the properties and mode of formation of this body left no doubt of its identity with bromaniloid, I however made a combustion of it. Unfortunately, as stated, the substance was not perfectly pure, hence the proportion of carbon is too high.

0·3305 grm. of substance gave 0·2800 grm. of carbonic acid and 0·0450 grm. of water; corresponding in the 100 parts to

Carbon	23·10
Hydrogen . . .	1·51

The formula will therefore be $C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N$, which requires

Carbon	22·15
Hydrogen . . .	1·22

Tribromaniline is not a base but an indifferent substance, and does not combine with bases or acids. I must refer to Fritzsche's memoir for the description of its properties.

The following table exhibits the formulæ of the different bodies which form the subject of this paper:—

$$\left.\begin{matrix}\text{Aniline}\ .\ \ .\ .\\ \text{Amaphenase}\ \ .\ .\end{matrix}\right\}=C_{12}H_7N$$

Substitution of the hydrogen by chlorine.

$$\left.\begin{matrix}\text{Chloraniline}\ \ .\ .\\ \text{Amachlophenese}\ .\end{matrix}\right\}=C_{12}\left\{\begin{matrix}H_6\\Cl\end{matrix}\right\}N$$

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* *Bullet. Scient. de St. Pétersb.*, 1843, t. i. p. 30.

$$\left.\begin{array}{l}\text{Dichloraniline . .}\\ \text{Amachlophenise .}\end{array}\right\} = C_{12}\left\{\begin{matrix}H_5\\ Cl_2\end{matrix}\right\}N$$

$$\left.\begin{array}{l}\text{Trichloraniline . .}\\ \text{Amachlophenose .}\end{array}\right\} = C_{12}\left\{\begin{matrix}H_4\\ Cl_3\end{matrix}\right\}N$$

Substitution of the hydrogen by chlorine and bromine.

$$\left.\begin{array}{l}\text{Chlorodibromaniline}\\ \text{Amachlobrophenose}\end{array}\right\} = C_{12}\left\{\begin{matrix}H_4\\ Cl\\ Br_2\end{matrix}\right\}N$$

Substitution of the hydrogen by bromine.

$$\left.\begin{array}{l}\text{Bromaniline . . . .}\\ \text{Amabrophenese . .}\end{array}\right\} = C_{12}\left\{\begin{matrix}H_6\\ Br\end{matrix}\right\}N$$

$$\left.\begin{array}{l}\text{Dibromaniline . .}\\ \text{Amabrophenise . .}\end{array}\right\} = C_{12}\left\{\begin{matrix}H^5\\ Br_2\end{matrix}\right\}N$$

$$\left.\begin{array}{l}\text{Tribromaniline . .}\\ \text{Amabrophenose . .}\end{array}\right\} = C_{12}\left\{\begin{matrix}H_4\\ Br_3\end{matrix}\right\}N.$$

We now return to the question, can chlorine play the part of hydrogen in organic compounds?

The isatine group is perhaps quite capable of affording some explanation of this question.

In isatine we have a body which, by the action of chlorine, loses one or more equivalents of hydrogen, which are replaced by the acting substance. These three compounds, isatine, chlorisatine and dichlorisatine, possess the same chemical and physical properties. By the substitution of chlorine for hydrogen the character of isatine has not been changed. Isatine, chlorisatine and dichlorisatine, by the action of other agents, suffered similar changes. By treatment with sulphuret of ammonium we see these three bodies assuming one equivalent of hydrogen and changing into isatyd, chlorisatyd and dichlorisatyd. By proper treatment with sulphuretted hydrogen the whole or part of the oxygen can be eliminated and replaced by sulphur. In contact with alkaline sulphates the sulphoisatinic and chlorisatinic salts are formed; peculiar compounds, in which the sulphurous acid is united in a singular manner with the organic substance. The behaviour of isatine and chlorisatine to ammonia and alkalies is also similar: particularly towards the latter the analogy in their action is in the highest degree characteristic. At the ordinary temperature they unite directly with the alkalies, but at a boiling heat they change into isatinic and chlorisatinic acid, whose strict analogy in all these salts never fails. Finally, by fusion with hydrate of potash, we see isatine converted into aniline; when chlorisatine is treated in the same manner, still under conditions which certainly are favourable to fix the chlorine, this element is observed to pass over into the new compound. From the fused hydrate of potash, which retains a portion of the carbon of chlorisatine in the form of carbonic acid, there distils over chloraniline, and with it the whole of the chlorine existing in the former compound. In the present memoir I have had frequent occasion to show the analogy which exists between aniline and the chlorinated base; it extends not only to the fundamental properties of both bodies, but even to the products of decomposition. By the action of bromine on aniline tribromaniline is formed, $C_{12}\left\{\begin{matrix}H_4\\ Br_3\end{matrix}\right\}N$; with the chlorinated base the same body gives chlorodibromaniline, $C_{12}\left\{\begin{matrix}H_4\\ Cl\\ Br\end{matrix}\right\}N$, which in no respect, either in its properties or characters, can be distinguished from the preceding. The only difference of the two compounds consists in the presence of chlorine in the second body.

On the preceding facts, when considered together, it appears to me that no two constructions can be entertained. They prove, in an undoubted manner, that in certain circumstances chlorine or bromine can perform the part of hydrogen in organic compounds.

It is quite another question, Is every organic body capable of assuming chlorine in the place of its hydrogen?

Further, in those organic compounds in which we see one or two equivalents of hydrogen replaced by chlorine, without any essential modification of their fundamental characters, is it possible to make a complete substitution of these elements without changing the properties of the compounds?

For the definite solution of these questions many further investigations must still be made. From the foregoing experiments it appears, however, that the chlorine preserves, to a certain extent, its electro-negative character in those compounds in which it replaces hydrogen, and that in proportion to the increase in the number of equivalents of the latter for which chlorine or bromine are substituted, so is this character the more impressed on the resulting compounds. The compound atom aniline, $C_{12}\ H_7\ N$, in consequence of the peculiar arrangement of its elements, possesses the property of uniting with an acid; replace one equivalent of its hydrogen by bromine, and we obtain bromaniline; a body possessing likewise basic properties, but in a feebler degree. A number of metallic oxides, which can be precipitated from their solutions by aniline, are not affected by the brominated base. Bromaniline is a less powerful base than aniline. By the assumption of the equivalent of the electro-negative body bromine, the basic properties of the original system is enfeebled; substitute now another equivalent of bromine for hydrogen, and we have dibromaniline. The facility with which, as we mentioned, all its salts are decomposed, evidently indicates that the basic character of aniline is further enfeebled by the repeated assumption of bromine. Finally, in tribromaniline the electro-negative properties of the assumed bromine equivalents have placed themselves in equilibrium with the electro-positive character of the original system. Possibly, by the further addition of bromine, a body may be formed possessing acid properties.

We have in inorganic chemistry the compounds of many metals with oxygen which exhibit in a distant degree the same character; for distant I must call it, since it is not a question of substitution. Protoxide of manganese is a strong base, whilst in the sesquioxide, richer in oxygen, the basic properties are lessened; peroxide of manganese unites with neither base nor acid, and by the addition of more oxygen we have manganic and hypermanganic acid generated.

It is only with the greatest diffidence I venture to express an opinion on this subject: nothing is more dangerous than to wish to draw general conclusions without having a mass of facts at command.

Bromaniline and dibromaniline possess, though in a lesser degree, the fundamental properties of aniline—they are bases. Tribromaniline is neutral; in it the original character of aniline has entirely disappeared. This body was however produced in a totally different way from that of the two brominated bases. It may be objected, that in it the grouping of the atoms, as they prevailed in the original system of aniline, no longer remained. Possibly there may exist a body of the same composition having the formula $C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N$, which still possesses basic properties. Such a body would be formed by distilling tribromisatine, $C_{16}\left\{\begin{matrix}H_2\\Br_3\end{matrix}\right\}NO_4$, with hydrate of potash. This compound is as yet unknown. Further experiments must therefore be made to solve the question.

Before concluding this memoir I shall mention another compound which appears worthy of the attention of chemists.

It is formed by passing into aniline free from water a stream of cyanic acid obtained by distilling cyanuric acid. The fluid becomes considerably heated, and solidifies on cooling into a crystalline mass. These crystals are insoluble in water, but soluble in boiling alcohol, from which they are deposited in white shining plates. It contains neither cyanic acid nor aniline. It may be heated with hydrochloric acid or potash without decomposition. It is a kind of urea, in which the ammonia is replaced by aniline. I was in hopes that this body would possess basic properties, and like urea form crystalline salts with nitric and oxalic acid; its properties are however totally different.

I shall in a separate memoir return more fully to this product of the decomposition of aniline.

LXXIII. *On the almost total Disappearance of the earliest Trigonometrical Canon. By* A. DE MORGAN, *Esq.*[*]

I LATELY found in a second-hand book-shop a trigonometrical canon, by the celebrated Rheticus, which was totally new to me, and, as I afterwards found, would have been just as new to any of the historians of astronomy and mathematics. I am therefore induced to make a communication on the subject to the Astronomical Society, which, more than any other, has a right to take interest in the history of tables for the facilitation of the application of arithmetic to geometry.

The paper which I now read is a remote consequence of the proscription of Copernican opinions by the Inquisition. No follower of Copernicus was more zealous or more plain-spoken than Rheticus or George Joachim of Rhætia. The master, whether from conviction or policy, called his theory no more than an hypothesis for the explanation of the planetary motions: the pupil strongly insisted on the absolute physical truth of the motion of the earth. Both were gathered to their fathers long before the storm arose: but long before that time arrived there was a distinction drawn between the treatment of the two. In the *Index Expurgatorius*, it is not Copernicus who is forbidden to be read generally; the prohibition only extends to the work *De Revolutionibus*, and is accompanied with a *nisi corrigatur*. But Rheticus is wholly forbidden to be read in any of his works: nay, in Sotomaier's folio edition of the *Index* (Madrid, 1667), even the *Opus Palatinum*, though all but the very table is the work of Valentine Otho, is an unlawful book, unless on the condition that the praises of Rheticus in the preface be erased or passed over. And Riccioli tells us that the condemnation of the books of Rheticus took place in 1550, which is the year before the canon I intend to describe was published. The extreme severity with which his writings were treated will appear less remark-

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[*] From the Monthly Notices of the Royal Astronomical Society, vol. vi. p. 221 (having been read before the Society April 11, 1845); with an addition by the Author.

able, if we remember that he held the chair of mathematics at Wittemberg, in which Luther had very lately taught theology, and burned the pope's bull, and in which Melancthon actually taught Greek. Riccioli very gravely informs us that Rheticus received a supernatural punishment for his presumption. While he was puzzling himself about the motion of Mars, he invoked his genius or guardian angel to help him out of the difficulty: the angel accordingly lifted him up by the hair of his head to the roof and threw him down upon the pavement, saying with a bitter laugh, "That's the way Mars moves." Kepler, it seems, had heard this story with such evidence as induced him to suppose that Rheticus must have knocked his head against the wall in the agitation of his spirits. Riccioli does not feel himself at liberty to make any other mention of him, and adds *damnatus auctor* to his name, in a list in which Copernicus himself figures without remark: and Clavius, in the diffuse trigonometry attached to the edition of Theodosius, does not even introduce his name. The Jesuit Blancanus, also, in his list of mathematicians, excludes Rheticus altogether, though he was a contemporary of the *Opus Palatinum*, and though he admits Copernicus.

The works of Rheticus are accordingly very scarce; and it can be no matter of surprise that one of them, and perhaps more, should have entirely slipped out of notice. Even the second edition of Copernicus, Basle, 1566, which was edited by Rheticus, is much more scarce than the first.

The early history of trigonometrical tables, as given, may be thus summed up. Tables of sines, which were substituted for Ptolemy's chords by Albategnius, were published, to every minute, from Regiomontanus* (who died in 1476), and also by Apian, both in 1533. Regiomontanus used tangents, as the Arabs had done before him, and there is a table, to degrees only, in a work of his published by Reinhold in 1561. This same Erasmus Reinhold had printed tangents of every

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* Such is the statement: but on examining the work on triangles by Regiomontanus, in which these sines are said to occur, and which was published in 1533, I cannot find any table at all. But Apian published *two* tables of sines: one in the *Introductio Geographica*, 1533, and another in the *Instrumentum primi mobilis*, 1534. There is a table of sines in the *Tabulæ Directionum*, 1552, and another in the *second* edition of the work *De Triangulis*, Basle, 1561, folio. And in the catalogue of De Thou's library, and in Niceron's list, Murhard's catalogue, and Kästner's history, is mentioned the following work, "Joh. Regiomontani compositio tabularum Sinuum, cum tabulis duplicibus Sinuum ejusdem," folio, Nuremberg, 1541. All the writers who assert that sines of Regiomontanus were published before those of Apian, go on the assumption that what is in the second edition of the work on triangles is also in the first, which assumption is not true. Lalande gives a wrong date, place and title, to this first edition.

minute, in his *Liber Tabularum Directionum*, published in 1554, three years after the Prutenic tables, on which his fame principally rests. Regiomontanus had called the table of tangents *canon fœcundus*, which name Reinhold adopted. As to secants, the table of which was afterwards called *canon fœcundissimus*, a name which I think first occurs in Vieta, the first table mentioned is that of Maurolycus in the spherical treatise which accompanies his edition of Theodosius and Menelaus, published in 1558; this table goes only to degrees, and is called *tabula benefica*. The *Canon Mathematicus* of Vieta, published in 1579, is said to have been the first work in which sines, tangents, and secants were joined together; that is, the first complete canon printed. But all admit that Rheticus, who died in 1576, had very nearly completed the enormous table which Valentine Otho published in 1596, under the name of *Opus Palatinum*. Still, the rigid rule is, that first publication gives a right which nothing but unquestionable proof of fraud can impugn; and accordingly Vieta has been justly considered, up to this time, as the first author of a complete canon. I intend to show, however, that under the same rule, Rheticus is not only the first who published either tangents or secants*, but the first who joined the three into a complete canon, and also the first who adopted the now universal semi-quadrantal form. Before, however, I come to the description of the table which establishes these things, I shall show that it once had a recognised existence. It is not enough, if better may be, to produce a printed book as the sole evidence of the fact of publication. There may have been a suppressed edition, or one accidentally destroyed by fire, and of which only a few copies escaped: the forgery of a work is neither impossible nor unexampled, and more than one big catalogue consists entirely of pseudonymous works.

Lansberg attributes the first publication of tangents, and Bossut the first publication of secants, to Rheticus: on what authorities I do not know. Moestlinus, in a letter written to Kepler in 1594, and published in the folio correspondence of

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* Montucla gives the secants to Rheticus, and Delambre (*Astron. Mod.* ii. 34) seems to assent, because Maurolycus only published them to every degree. This is hardly fair: a person who points out the uses of a given function, and tabulates it to a certain extent, is the inventor, and must not lose his right because another gives more and better tabulation. But we now see that Rheticus has a claim absolutely prior to that of Maurolycus. Again, Delambre says that a certain Finckius gave secants to minutes, in 1583, referring to Rheticus: he seems to imply that Finckius had access to the materials of the *Opus Palatinum*, not then published. Perhaps it may now be held that the ten-minute canon described in the present article was the original, the intermediate minutes being supplied by interpolation.

the latter, expresses a great desire to see a certain little book of Rheticus, the title of which he does not mention. In a subsequent letter of 1595, he reports that he has seen this book, that it treats only of plane triangles, and that it strives at (*nititur*) the canon of triangles which Rheticus afterwards (*olim*) published. What can this *canon of triangles* be? At the date of this last letter, the *Opus Palatinum* was not published. It can refer to nothing but the work which I now bring forward, and which, it thus appears, was known. The next witness I shall cite is Thomas Digges, who is likely to have taken a particular interest in the writings of his fellow Copernican. In the *Alæ seu Scalæ Mathematicæ*, published in 1573, during the life of Rheticus, Digges says that those who dislike labour should consult the tables of the proportions of right-angled triangles by Rheticus, of which tables he further states that they go to every ten minutes.

Valentine Otho, in the preface to the *Opus Palatinum*, says that "when Rheticus gave to the public a specimen of his method of enriching the canon and doctrine of triangles, he excited a wonderful degree of hope and expectation about it in the minds of the learned, especially when in the dialogue which he prefixed to his ten-minute canon, he brought forward extraordinary and almost incredible things concerning its use." I produce to the Society the ten-minute canon with the dialogue, which however is not prefixed to the table, but follows it. Otho afterwards mentions this dialogue again, and says that on reading it he was so struck with the pretensions of its author that he sought out Rheticus in Hungary, and commenced the acquaintance which led to his becoming the editor of his friend's posthumous work. He further adds, that at their first meeting, when he had just stated that he was come to acquire knowledge on the properties of triangles, Rheticus interrupted him with, "You are just as old as I was when I went on the same errand to Copernicus." Thus it appears that this canon was the indirect cause of the publication of the *Opus Palatinum*.

But nevertheless, this work is not mentioned in the catalogues of Lipenius, Dechâles, or Murhard; it is not alluded to either by Riccioli, Clavius, Gassendi, Weidler, Heilbronner, Delambre, Montucla, Hutton, or Kästner. Delambre distinctly says he never heard of any canon containing sines, tangents and secants, previous to that of Vieta: though elsewhere he describes the preceding passage from Otho by saying that Rheticus had published a programme of the *Opus Palatinum*, and even an extract for every ten minutes. But he then quotes the account of Lalande, to which I shall immediately come, and drops the matter, as if declining to decide the point. Kästner quotes the passage from Otho which I have translated above, and makes a separate head of it, in his abstract of the preface of the *Opus Palatinum*; he does not give his usual short comment, and evidently leaves it to the reader without knowing what to make of it.

That Weidler should have been utterly ignorant of this work is rather a striking proof of the complete oblivion into which it had fallen. For Rheticus (with Reinhold) professed mathematics in the University of Wittemberg, after having taken degrees there; and Weidler was not only of this university, but wrote his history of astronomy, and printed it, at Wittemberg. Consequently he had access to the *matricula*, or register-book, and to all other records; so that he is able to give several minute particulars of the literary life of Rheticus, which another writer could hardly have obtained. But not even at Wittemberg did any tradition exist of the work on which this paper is written. It was printed, certainly, at Leipsic, and on the residence of Rheticus in this latter university, Weidler can say nothing more than that Rheticus is reported to have taught there. It is worth noting that, next to Wittemberg, Leipsic was the university most obnoxious to the adherents of the old church.

Lalande actually possessed a copy of a reprint of this same canon, published (he says) in 1580, and has given the title-page with perfect correctness, in his short description of the *Opus Palatinum*, contained in the *Bibliographie Astronomique.* But, with a negligence which is unusual in his bibliographical accounts, he represents it as a canon for the first forty-five minutes only, to every ten seconds; a kind of extract from the forthcoming *Opus Palatinum.* Had he looked more closely, he would have seen that his *minutes are degrees and his seconds minutes*; and he would have seen the remaining forty-five degrees rising in the reverse direction on the opposite side of the page. Perhaps Lalande could not imagine the possibility of the calculator of the immense *Opus Palatinum* publishing a table to every ten minutes only.

Murhard has this reprint, Basle, 1580, in his list, but not the original work, and it is also in the catalogue of De Thou's library. The title is to be found in Teissier's *Eloges des Hommes Savans*, from whence it is copied into a work in which we should hardly have looked for mathematical treatises which are unknown to mathematicians, Gorton's *Biographical Dictionary.* The library of the British Museum, which is unusually rich in the mathematical works of the sixteenth century, has both the original work and the reprint. But I do not find any date to the reprint, nor do I know from whence Murhard and Lalande got theirs. The copy of the original edition now in my possession probably escaped the Inquisition, from the accident of its being bound up with the *editio princeps* of the Greek text of the optical writings of Euclid, published six years after it.

We thus see that the existence of a work may be forgotten, and the fair claims of its author reduced in amount, by the neglect of a biographer in stating precisely its title, date, place, and form; and also that it may be possible, partially at least, to repair the neglect, by collection of scattered notices. We also see that the publication of proper catalogues of our libraries would tend to promote historical knowledge. And while on this subject, I trust it is not out of place to make the following remark. I very much fear that the publication of a good alphabetical catalogue of the splendid library in the British Museum is retarded, by the demand which has often been made for a *classed catalogue*, or one arranged in order of subjects. From much, almost daily, use of catalogues for many years, I am perfectly satisfied that a classed catalogue is more difficult to use than to make. It is one man's theory of the subdivision of knowledge, and the chances are against its suiting any other man. Even if all doubtful works were entered under several different heads, the frontier of the dubious region would itself be a mere matter of doubt. I never turn from a classed catalogue to an alphabetical one without a feeling of relief and security. With the latter I can always, by taking proper pains, make a library yield its utmost: with the former I can never be satisfied that I have taken proper pains, until I have made it, in fact, as many different catalogues as there are different headings, with separate trouble for each. Those to whom bibliographical research is familiar, know that they have much more frequently to hunt an author than a subject: they know also, that in searching for a subject, it is never safe to take another person's view, however good, of the limits of that subject, with reference to their own particular purposes.

I now proceed to the description of the canon.

The title is "Canon doctrinæ triangulorum. Nunc primum a Georgio Joachimo Rhetico, in lucem editus, cum privilegio imperiali, Ne quis hæc intra decennium, quacunq; forma ac compositione, edere, neue sibi uendicare aut operibus suis inserere ausit. Lipsiæ ex officina Wolphgangi Gunteri. Anno M.D.LI." quarto. The title is followed by one page of verses, fourteen of tables, and six of dialogue. In the title-page is an obelisk, with a man drawing a diagram on the base.

The degrees and the sines, &c. are in black ink, the minutes and the differences are in red ink. The tables extend across the open book, the $\genfrac{}{}{0pt}{}{\text{sine}}{\text{cosine}}$, $\genfrac{}{}{0pt}{}{\text{cosine}}{\text{sine}}$, and $\genfrac{}{}{0pt}{}{\text{secant}}{\text{cosecant}}$, being on the left; the $\genfrac{}{}{0pt}{}{\text{tangent}}{\text{cotangent}}$, $\genfrac{}{}{0pt}{}{\text{cosecant}}{\text{secant}}$, and $\genfrac{}{}{0pt}{}{\text{cotangent}}{\text{tangent}}$, on the right. Thus at 30° 0′, we read along the double page, as follows, all in one line, differences coming before the semicolon which is here substituted for the double line in the work.

30 0 5000000 25171; 8660254 14581;
11547005 19474; 5773503 38851;
20000000 100180; 17320508 115774; 0 60.

The words sine, tangent, &c., are not used. The running title is, "Canon doctrinæ triangulorum in quo triquetri cum angulo recto in planitie partium 10000000 ponitur." This running title is not a complete sentence, the nominative of the last verb being wanted: it is completed in three different ways in the three great compartments of the table; the three nominatives are "Subtendens angulum rectum,"—"Majus latus includentium angulum rectum,"—"Minus latus includentium angulum rectum." Under the first heading come four columns, the *perpendiculum* and its *differentia*, and the *basis* and its *differentia*. Under the second come the *hypotenusa* and *perpendiculum*; under the third the *hypotenusa* and *basis*: all with differences. The words *perpendiculum* and *basis* are of course interchanged in the running titles at the bottom of the page, where also no other part of the running title is repeated. This system of headings is faithfully preserved in the *Opus Palatinum*.

Modern teachers of trigonometry have pretty generally abandoned the system of independent lines, which used to be called sines, tangents, &c.; and have substituted, for the meaning of these words, the ratios of the sides of right-angled triangles. It appears that they have antiquity in their favour; indeed so completely has the idea of representing the ratios of the sides of triangles taken possession of the mind of Rheticus, that he abandons the use of the word *sine*. He dwells on the importance of the right-angled triangle, without any reference to the circle: his maxim, expressed in the dialogue, is *Triquetrum in planicie cum angulo recto, est magister Matheseos.* It would also seem as if his choice of the semi-quadrantal arrangement with double descriptions was dictated merely by the convenience of heading one division with *majus latus*, and the other with *minus latus*. This is worth noting: most persons suppose that this disposition must have arisen from the circumstance of the sines and cosines of the latter half of the quadrant being only repetitions of the cosines and sines of the first half, and so on. But the very reverse is the fact; the names cosine, cotangent and cosecant, are the consequence, not the cause, of this duplicate system of arguments. Rheticus made his arrangement with a view to separate from each other all the cases in which the greater side and the less side were data. This involved the bringing into the same line the tangent of each angle with the tangent of its complement, and the same for the secant. Completeness then required that the same should be done with the sine. The introduction of the terms sine of the complement, complemental sine, and cosine, &c., followed after an interval of more than half a century, and was a consequence of the semi-quadrantal arrangement.

The dialogue at the end is between Philomathes, a supposed friend of Rheticus, and Hospes, his pupil. The pupil asks what the intention of the book is, and is answered at length. He suggests that, perhaps, the intention may be to complete the system of Copernicus, by publishing tables from it resembling those then in use. But he is answered, that Rheticus would rather that Copernicus himself had not done so much in this line, as he thereby diminished the geometrical practice of the learner. The modern astronomer, if such a one there be, whose luxurious means render him discontented whenever he has to go to the common trigonometrical tables, even of logarithms, should think of Rheticus, so well content with his intervals of ten minutes, and their differences, that he asked for nothing more, and regretted that any further help should be interposed between the observer and his wholesome exercise. I think I might have hoped for his sanction to an opinion which has been for a long time my own, namely, that the only way of learning to use a table thoroughly well, is to learn to do without it.

There is much examination yet wanted into the history of the sixteenth century. The era of logarithms, of literal algebra, and of sound mechanics, has naturally diverted attention from the day of smaller things. One question has never been properly considered: what were the immediate producing causes of that burst of successful energy which marks the first half of the seventeenth century?

The biography of Rheticus may be collected from the preface to the *Opus Palatinum*, Teissier's *Eloges des Savans*, with the references therein given, &c. He led a wandering life for a calculator of such a mass of tables, being successively with Copernicus at Wittemberg, at Leipsic, and in Hungary. And his friend and correspondent, Peter Ramus, informs us (*Schol. Mathem.*), that he taught at Cracow, and would have taught at Paris, but was prevented by being obliged to learn and practise medicine in the stead of a certain patron, *Mæcenatis cujusdam loco.* What this means I do not know: perhaps *loco* is a misprint for *domo.* Though Otho does not mention that Rheticus practised medicine, he to a certain extent confirms Ramus, by stating that his friend died at Cassau, in Hungary, on his way home, after being called out by a certain baron. I mention these things, because it is never stated that Rheticus was a physician. He died in 1576, in the sixty-first year of his age.

With regard to the choice of intervals of ten minutes, it may have been dictated by the existing state of astronomy; but it is more than likely that Copernicus was the suggester of the arrangement. Rheticus has preserved it as a saying of Copernicus, that if he could only succeed in giving planetary tables which should be true within ten minutes, he should feel as much gratified as Pythagoras, when he discovered the great property of the hypothenuse of a right-angled triangle.

In this communication I have confined myself to points which are either new, or very little known. I will add one more circumstance of the latter kind.

I have noted that Rheticus stands in the *Index* as *damnatus auctor*, while Copernicus is *damnati libri auctor*: a material difference. But perhaps it may suggest itself to some that Copernicus is only the writer of one work, which, being condemned, makes him a condemned author; and that it would not be thought necessary to condemn, in general terms, a writer all whose works can be prohibited under one title. But, not to dwell here upon such a supposition really implying an ignorance of the usage, it is not true that Copernicus wrote and published only one work. Though it be but little known, and not mentioned by any of the French school of historians, it is certain that Rheticus himself published (Wittemberg, 1542) in 4to the "De lateribus et angulis triangulorum tum planorum rectilineorum, tum sphæricorum libellus," containing a table of sines to every minute, and to a radius of ten millions, or as we should now say, to seven decimals.

This work is mentioned by Weidler, who, though of Wittemberg, had no knowledge of it till he published the supplements of his history: it is catalogued by Murhard, and described by Kästner. The great work *De Revolutionibus*, published the next year, contains a probably abridged treatise on triangles, and a certainly abridged table of sines.

In speaking of the great work of Copernicus, it should be remembered that Rheticus procured its publication, or extracted the author's consent to its appearance, as much as

Halley did that of Newton in the case of the Principia; and nothing but circumstances which made it more convenient to print it at Nuremberg than at Wittemberg, prevented the name of Rheticus from appearing in the title-page as editor.

⁂ Mr. De Morgan requests us to append the following remark to our reprint of his paper.

The mode of defining the sine, cosine, &c., which is now becoming common, and which, as appears above, was preferred by Rheticus, was first advocated in modern times by Dr. Peacock. In the second volume of his Algebra, just published (page 157, note), Dr. Peacock remarks "the definition in the text, which is now exclusively used (see Hymer's Trigonometry, Cambridge, 1837), was first formally introduced into my Algebra (Cambridge, 1830), and afterwards into a Syllabus of a course of Lectures on Trigonometry (Cambridge, 1833)."

The fact, however, as to *printed* publication, is, that the system in question first appeared in Lardner's Trigonometry, though Dr. Peacock is not the less the reviver of it. In the first edition of Dr. Lardner's work (1826), the definition by *ratios* is *given*, but not *used*, the old ones being also given. Thus the formulæ for the sines and cosines of sums, &c. of angles are deduced from the old definitions. Thus it is said (p. 42), that "from the definition of the sine of an angle or arc it appears that twice the sine of any arc is the chord of double that arc." But in the second edition (1828) the formulæ are consistently deduced from the definition by ratios. This was done at my suggestion, as noted in the preface, and I was indebted for the idea to Dr. Peacock, who had taught it at Cambridge for some years previously.—A. De Morgan.

LXXIV. *Note on a recent Fossil Freshwater Deposit in Southern India, with a few remarks on the Origin and Age of the Kunker, and on the supposed decrease of Thermal Temperature in India. By* Capt. Newbold, *M.N.I. Assistant Resident, Kurnool, Madras Territory*[*].

THE geographical locality of this deposit is in the Kurnool territory, about a mile easterly from the village of Lunjabunda, in about latitude N. 15° 30′ and longitude E. 78° 3′.

It lies in a jungly defile, or transverse valley, crossing the range of hills which, running nearly north by east and south by west, divides Kurnool into two portions. This range commences about five miles south of the city, and after traversing

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[*] From the Journal of the Asiatic Society of Bengal, N. S., 1844. No. 64.

the whole length of the Kurnool territory, passes below the Zurairoo valley to the east of Gooty, where it is connected with the Cuddapah chains, which, curving easterly, terminate near the coast in the Naggery ranges.

These ranges consist principally of the diamond-sandstone and limestone, and comprise within their area the diamond mines of Condapetta, Chinnoor, Ovalumpully, Munimudgoo, Banganpilly, Ramulacota, and others of less note. The fossil deposit rests on this sandstone conglomerate, which at no great distance is seen reposing on granite, with a dip of 10° to the south of east.

A little to the east of this, the diamond limestone intervenes between the granite and sandstone, underlying the latter in conformable dip and stratification.

A spring rising from the foot of a mound of conglomerate, composed of fragments of the sandstone rocks cemented by kunker, marks the site of the fossil bed, which lies in a slight depression above this mound, and considerably out of the reach of the spring in its present state. It is only a few yards in extent, and has evidently been deposited by the spring under former conditions, to which I shall allude presently. The imbedding matter is also a kunker, but one of a much harder, compact, and siliceous nature than that at present seen around the margin of the spring and below the mud at the bottom. Portions of it are sometimes so siliceous as to give fire with steel and scratch glass; other portions of the rock contain more lime, are less compact, and effervesce freely with acids. The colour is a light brownish-gray; fracture varying from flat-conchoidal to earthy.

The shells imbedded are freshwater, principally *Melania*, with a few small *Planorbes*, and are all of existing *genera*. The number of the former is so proportionally great, as to excite surprise in persons who have not studied the segregarious habits of the inhabitants of freshwater and terrestrial shells. Besides the shells there are impressions and casts of the stems of grasses, reeds, &c. perfectly fossilized by carbonate of lime.

The shells afford instructive examples of the various stages of fossilization. Some of their coats have been completely converted into sparry carbonate of lime; others have been filled with the imbedding paste, which, when the shell is broken off, exhibit a cast with a highly polished exterior. Others again are lined with drusy crystals of quartz; in some, this siliceous crystallization is just beginning to roughen the surface of the interior, and is hardly perceptible without the aid of a lens; thus exhibiting interesting examples of the processes by which fissures in rocks are lined and filled up with minerals which we look in vain for in the enclosing walls; geodes of calcedony and agate, with calc spar and crystals of quartz and zeolite in the midst of calc spar. I have seen a solitary and beautiful pyramidal hexagon of rock crystal, glittering like a diamond in the whitest snow, in a mass of the saccharine marble of Carrara.

None of the shells have lost their carbonic acid, although they have parted with most of their colour; and some are quite empty as if imbedded but yesterday; most have been evidently entombed in a dead state.

As no trap or other volcanic rock was at hand to account for the silicification of this freshwater limestone, I proceeded to examine the present deposit of the spring a few yards west of the fossil bed. Its water I discovered to be slightly thermal, having a temperature of 85° 3′ Fahr., which is a few degrees above the mean temperature of the spot, isothermally calculated; the height above the sea, as roughly approximated by the boiling-point of water, is about 1250 feet, and the average temperature of the ordinary wells about 80° Fahr.

The present deposit of these waters is a brownish-gray calcareous mud, about six inches thick, mingled with sand, imbedding similar freshwater shells and a minute specimen of *Paludina*. Stems of grasses and leaves were also found in it; some of the latter apparently just decayed, while others are blackened by carbonization; none were fossilized.

Below the mud lay a deposit of nodular kunker, quite distinct in character from that of the fossil bed, being white and earthy, externally pulverulent or chalky, but internally compact and hard. I did not observe any shells or plants in it. The depth of this layer could not be ascertained for want of leisure, and better instruments for digging under water than a geological hammer.

The water of the spring is tasteless, inodorous, and free from gaseous bubbles; and, instead of any free carbonic acid gas, is slightly alkaline, turning reddened litmus paper into a faint greenish-blue; oxalate of ammonia and muriate of baryta produced a considerable white precipitate. That from the muriate of baryta effervesced with dilute nitric acid, showing the precipitate to be carbonate of lime. A thin slice of gall-nut, suspended in the water, detected a trace of iron. A minute portion of silica remained after evaporation.

The present layer of mud then, as we have just seen, is more of a mechanical deposit than the subjacent white kunker, which is evidently a chemical precipitate, and concretionary in character, while that which has fossilized the shells and plants is far more siliceous; so much so, indeed, as to resemble in some parts the siliceous tufa deposited by the hot springs of Iceland, more than the common calcareous kunker of India.

The natives declare, that both the volume and heat of these thermal springs (of which I have within the last two years discovered several in the diamond formation) are on the decrease. There is no question that the spring under description is fast drying up, for the *Reddy* of the village pointed out to me land, now waste, which within the last forty years had been irrigated by it. It now yields but a scant supply to the cattle and the beasts of the forest. The traditionary accounts of diminishing temperature are by no means so satisfactory, since the Hindoos had no means of measuring warmth or cold.

However, the examination of the deposits in and around the mouth of this spring goes to support what the natives say, assuming that the more siliceous deposit containing the fossils is of an older date than the two at the bottom of the spring, and formed when the spring was more abundant, and its water hot enough to hold a considerable quantity of silica as well as lime in solution, possibly combined. As the heat decreased, the water would lose most of its silica, but still retain the lime; at this period it may be inferred that the kunker was precipitated as the water cooled on the earth's surface. As the heat still diminished, the portion of lime brought up in solution decreased to the state in which we now see it. That such is the fact, appears from the circumstance of the water of two other and warmer springs, which I have since discovered in the same formation, holding considerably more lime in solution than this.

The waters of two other thermal springs in the same formation still deposit lime as a kunkrous incrustation on their sides and on the rocks in their course.

These had a higher temperature, viz. 90° and 91° 3′; the minerals held in solution are similar, but the proportion of lime is greater. One fact is worthy of note, that they were all slightly alkaline, and contained no perceptible free carbonic acid.

In order to ascertain the interesting problem, as indicated by the traditions of natives, and the difference between the quantity and quality of the present and ancient deposits, viz. that the heat of this part of the interior of the globe is decreasing, it would be desirable to keep a register not only of the thermal springs of Southern India, but of those far hotter fountains that gush from the great southern line of dislocation of the Himalayan strata, and the trap hills of Central and Western India.

The heat of the springs might be annually or triennially noted with compared thermometers. After many experiments, I find existing meteorological causes generally affect the temperature of such springs in a sensible degree; and great care should be taken, in making successive experiments, that the meteorological conditions be as far as possible similar. The time of the year should be the dry season, and the time of day sun-set and sun-rise.

The plains and valleys of India are often covered with sheets of kunker, sometimes upwards of seventy feet deep, overspreading places where it could never have been deposited by rivers or rivulets, and where now not a spring or drop of water is to be seen. Along the edges of trap dykes, we occasionally observe mounds of kunker precisely resembling those around the mouths of existing kunker-depositing springs, but we look in vain for the springs that deposited the former.

Still these dwindled remains of springs are generally to be found where kunker prevails at no great depth from the surface, deprived of their heat and of the greater part of their mineral character, which renders the water better adapted for the use of man and animals. Most of the native diggers in boring for a well, consider kunker as almost a sure sign of the vicinity of water. If there can be any doubt after what has been said of the certainty of the vast sheets of kunker at present seen covering waterless plains, and the arid summits of hills of Southern India having been formed by springs, many of which are now dried up or diverted, it will be removed on an inspection of a vertical section of the rocks which underlie it. These, whether trap, granite, the hypogene schists, sandstone or limestone, will be found invariably to be penetrated by nearly vertical fissures, through which the kunker appears to rise like trap in a dyke and to overflow the surrounding surface, and like trap, to introduce itself into any horizontal or other seams, imparting the appearance of beds of kunker alternating with gneiss, &c. On a more minute inspection, it will be found that the kunker has in reality been precipitated chemically from the water of springs that now, or formerly, found vent to the surface through these fissures. The thermal waters holding the lime in solution as they cooled in approaching the surface deposited the lime as they ascended.

I had an opportunity in 1840 of studying the formation of travertine in the old volcanic area around Rome, and found it to assimilate that of the Indian kunker in all the leading facts. The calcareous conglomerates at present forming along the shores of the Red Sea and Mediterranean, are little different from the present kunkrous conglomerates of India.

It may also be added, that the surface soils of Southern India, whether of the red alluvial or the black regur, are frequently so strongly impregnated with muriate and carbonate of soda, as to be utterly unfit for the purposes of agriculture. Many of the springs in such situations are still brackish, holding a portion of these salts in solution, but are quite inadequate to have caused their diffusion in the superincumbent soil to the present immense amount.

It is difficult to classify a formation still going on, and to fix the period, geologically, when it commenced, as it is seen in all rocks from the granite to alluvium. We have sufficient evidence, however, to divide it into two periods, viz. that immediately prior to the deposition of the regur, which it often underlies in thick beds, and the present formation, going on. The kunker characterized by the remains of the mastodon at Hingoli, and the kunker conglomerate imbedding the mammoth near Nursingapore, like the travertine of Rome, which imbeds the remains of this animal and of existing species of freshwater shells, may be referred to the post pleiocene period.

Since the discovery of the first fossil bed I have found another near the temple of Hoodelaity on the same range, of considerably greater extent, being more than ten feet thick, resting on the ledge of a precipice thirty feet above the present level of a stream formed by a thermal spring. But not a vestige of the spring that deposited this bed is to be seen. The stems and plants it fossilizes are in a much more distinct and perfect form, and in addition to *Melania* and *Planorbis*, I found fragments of *Unio*, and a shell having the suborbicular shape of *Cyrena* with the thinness of *Cyclas*; two forms of freshwater Conchifer that often pass into each other; the hinge was not visible. A very perfect impression of a leaf, and a number of curious cylindrical bulbiform and reniform bodies, probably vegetable forms, were found. The vertical surface of this cliff presents in its layers all the curved and geodic forms seen in oriental agate, and imbeds solid fragments of a more ancient kunker. The height of the sandstone cliffs forming the sides of the fissure (probably a fault), I found, by a trigonometrical observation, to be seventy-five feet from the bed of the stream.

Specimens of some of the fossil shells and supposed petrified vegetable forms have been forwarded to the Museum of the Asiatic Society. I have little doubt of the longer and thinner cylindrical bodies being stems of grasses. They are seen in the rock fossilized in clusters upright as they grew, with freshwater shells half-entangled about their roots.

LXXV. *On certain Fallacies in* Enderlin's *Researches on the Constitution of the saline ingredients of Animal Fluids. By* Golding Bird, *A.M., M.D., F.L.S., Lecturer on Materia Medica at, and Assistant-Physician to,* Guy's *Hospital. In a Letter to Richard Phillips, Esq., F.R.S.*

My dear Sir,

THE opinion lately announced by Prof. Liebig, regarding the non-existence of salts of organic acids in the urine and other animal secretions, has, as you are aware, appeared to receive full confirmation from the researches of Enderlin, who, in a long communication (Liebig and Wöhler's *Annalen*, 1844, 3 and 4, and Heller's *Archiv*, 1844, s. 144) on the chemical constitution of the incombustible residue of the blood and other fluids, has stated that in no instance, except in the case of bile, is an alkaline carbonate to be detected in the ashes left after the incineration of the extract of the fluid under examination.

This remark, on a *prima facie* view, appears most conclusive as to the accuracy of Prof. Liebig's statement; for if an alkaline acetate, lactate, or any other salt of an organic acid existed in a secretion, we should of course expect to find it in the residue of incineration in the state of carbonate. The almost constant alkaline reaction of the ashes of blood and other fluids is of course readily explained by the existence of the sub-tribasic phosphate of soda ($3NaO+P_2O_5$), which constitutes an important ingredient in the residue of combustion, the absence of a carbonate in the alkaline ash being demonstrated by effervescence not being excited on the addition of a mineral acid.

On repeating the experiments of Enderlin with every precaution to avoid error, I obtained precisely similar results in the case of serum of blood and mucous secretions. The urine however, in most instances, afforded an ash which not only was alkaline, like that obtained from serum, but effervesced with acids and consequently contained alkaline carbonates.

Admitting, then, the correctness of the statement made by Enderlin, that the alkalinity of the ashes of blood, saliva, &c. is owing to a tribasic phosphate of soda, and not to an alkaline carbonate, it may be asked, is the induction drawn by this chemist from these facts equally correct? *From the ab-*

sence of alkaline carbonates in the ash, he has considered that no combination of an organic acid with an alkali could have existed in the secretion yielding it.

I confess, I always doubted the correctness of this opinion, and hope now to adduce satisfactory evidence that a combination of *an organic acid with an alkali may really exist in a solution of phosphate of soda in considerable quantity, and yet yield no carbonate by ignition.*

If nine grains of dry tribasic phosphate of soda (HO, NaO, P_2O_5) be mixed carefully with four of dry acetate of soda, and the mixture exposed for a quarter of an hour to a full red heat in a covered crucible, a snow-white spongy mass will be obtained. This will be found to dissolve readily in water, to exert a powerfully alkaline action on litmus-paper, and does not evolve any bubbles of gas when dilute sulphuric acid is added to its solution, or to a portion of the substance after being covered with water. The addition of nitrate of silver to a solution of the salt produced a copious precipitate of *yellow* tribasic phosphate of silver. It is hence obvious that the soda left by the ignition of the acetate of soda was sufficient to replace the water in the phosphate, which thus became converted into the sub-salt, the acid being, notwithstanding the heat to which it had been submitted, in the same state as that existing in the common rhombic phosphate, in consequence of the presence of three atoms of base. The reaction occurring is simple enough.

$$NaO, C_4H_3O_3 + HO, 2NaO, P_2O_5 = 3NaO, P_2O_5 + CO_2HO + C_3H_3O.$$

The evolution and combustion of the acetone may be observed when the mixed acetate and phosphate are ignited in a platinum spoon.

It is assumed by Enderlin that the sub-tribasic phosphate pre-exists in a secretion, when it is found in the ash obtained from the incineration of its extract. Of this I venture to assert we do not possess a single proof. The ordinary rhombic phosphate is in all probability the compound which exists in secretions; indeed in the urine very satisfactory evidence can be obtained on this head. If urine be evaporated to an extract and digested in alcohol, an insoluble residue containing chloride of sodium and rhombic tribasic phosphate will be left: on allowing a drop of a watery solution of this salt to evaporate on a plate of glass, penniform groups of crystals of the rhombic phosphate mixed with common salt will be left.

Without occupying further space by alluding to other less important points of Enderlin's paper, I venture to hope I have proved that the absence of a carbonate in an ash is by no means a proof of the non-existence of salt of an organic acid in the substance yielding it, providing phosphate of soda be present. Therefore the researches I now take the liberty of criticising do not in any way invalidate the correctness of the generally-received opinion of the presence of alkaline lactates in the different fluids of the body.

I remain, my dear Sir,
Very sincerely yours,
Guy's Hospital, May 6, 1845. GOLDING BIRD.

LXXVI. *Further Researches on Animal Electricity: Of the muscular current and the proper current.* (*Extract from a letter from* M. Matteucci *to* M. de Humboldt.)*

Pisa, March 27, 1845.

IN order to complete all that relates to the muscular current, I shall first observe that I have obtained very distinctly signs of tension on the condenser, at the two extremities of my muscular piles. In the same manner I have obtained the signs of *electro-chemical decomposition* by the muscular current. I have been especially interested in these new researches in studying, in a much more complete manner than I had done in my preceding investigations, the relation between the intensity and the duration, after death, of the muscular current, and the activity of the respiration and of the circulation of the blood, the temperature of the medium in which the animal lives, its rank in the animal scale. I have worked at this for five months, submitting to experiment every day a certain number of frogs taken from the same pond. Some of these frogs were immediately killed, to obtain a measure of the muscular current; others were placed, at the temperature of the external air, in an apparatus by the aid of which I could ascertain the quantity of carbonic acid emitted by a frog in a given time: others again were placed in an ambient medium the temperature of which was constantly at +16° C. I have operated thus on frogs which had lived from −4° up to +16°. The result of so great a number of experiments leaves me not the least doubt on this conclusion: the intensity of the muscular current is proportional to the activity of the respiration. I have also operated upon frogs kept for a longer or shorter time in water *deprived of air*, and which were by this means in a more or less decided state of asphyxia. The same result has always been arrived at.

On operating upon several warm-blooded animals, I have verified in a more complete manner the result at which I had already arrived, that is to say, that the intensity of the mus-

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* From the *Comptes Rendus*, April 14, 1845.

cular current is proportional to the rank of the animal in the series of beings, whilst the duration of this current after death varies in an opposite relation.

I wished to study the influence of the different gases on the intensity and the duration of the muscular current; for this purpose I arranged an apparatus which allowed me to have a muscular pile in a certain gaseous medium, and to open and close at will the circuit of this pile with the galvanometer. I operated in atmospheric air, in oxygen, in highly rarefied air, in carbonic acid, and in hydrogen. In these different media the muscular pile performed equally, both as to intensity and duration. Hydrogen gas alone presented a singularity which could not have been foreseen previous to the experiment. This peculiarity does not depend on any action of the gas on the muscles, but on a phænomenon of secondary polarity which manifests itself, whatever be the source of the current. The fact is, that in operating in this gas with a muscular pile, the deviation remains constant for several hours. This nullity of action of the different gases abovenamed on the intensity and the duration of the muscular current, proves that the origin of this current is in the muscle itself even living, or taken from an animal soon after its death. This same consequence is proved by another experiment. I prepared with some very fine intestinal membrane a great number of small conical cavities; I filled these cavities with some fibrine separated from the blood of an ox which had just been killed; I rapidly prepared with these elements a pile, which, in appearance, was perfectly similar to my piles of half thighs (*de demicuisses*). I obtained no signs of any current from this pile. This pile performed with the same effect in hydrogen and in oxygen. The cause of the current therefore exists in the muscle, and consequently in its organization and in the chemical actions which take place in its substance when belonging to a living animal or one recently killed. The most curious results which I have obtained in these last experiments relate to the proper current of the frog. I can now affirm *that this current does not appertain exclusively to the frog*, but that it is manifested in all the muscles of all animals, provided that these muscles present at their extremities an unequal tendinous termination. All the muscles which have on one side the tendinous extremity more close, more condensed than on the other, give the current directed in the muscle from the tendinous extremity to the surface of the muscle. I have verified this result in all the muscles of the frog,—those of the upper limbs as well as the muscles of the lower limbs; in muscular masses of the pigeon, rabbit and dog. If I have rightly understood the latest anatomical labours on the structure of muscles, on their relations with the tendons and the sarcolema, I cannot hesitate to regard the proper current, or that from the tendon to the surface of the muscle, as the most simple case of the muscular current. The tendinous fibres are in connexion with the muscular fibres, whilst the sarcolema envelopes the muscular fibres only. This result is rendered still more probable when we recollect, that the same laws regulate the proper current and the muscular current.

LXXVII. *On Sillimanite.* *By* THOMAS THOMSON, *M.D., F.R.S., &c., Professor of Chemistry, Glasgow*[*].

THE name *Sillimanite* (from Prof. Silliman) was imposed by Mr. Bowen on a mineral discovered in 1817 at Saybrook in Connecticut. An account of it, together with an analysis by Mr. Bowen, was published in the 8th volume of Silliman's Journal (p. 114), in 1824. A few years afterwards I received a specimen of this mineral from Mr. Nutall, and gave it for analysis to Dr. Thomas Muir, who was at that time a practical student of chemistry in my laboratory. He had devoted several years of his life to mineral analysis, and had acquired much skill and was remarkable for his accuracy. He found sillimanite composed of

Silica	38·670
Alumina	35·106
Zirconia	18·510
Peroxide of iron	7·216
	99·502

In consequence of this analysis I placed sillimanite among the zirconia minerals in my Outlines of Mineralogy. The specimen of sillimanite from which Dr. Muir made his analyses was small; so much so, that only 5·64 grains could be obtained for taking its specific gravity. This put it out of my power to draw up a correct description of the mineral, or to compare it with others to which it seemed to bear an affinity. But five or six years ago I got from a friend in America a very fine collection of specimens, partly from Saybrook and partly from Petty Pang, both in Connecticut. The mineral was in crystals, several inches long, running through granular quartz in a gneiss rock. I was immediately struck with their resemblance to bucholzite, a specimen of which, from Chester on the Delaware, I had also got from Mr. Nutall. This induced me to subject it to analysis. I was surprised to find

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[*] Communicated by the Author.

it quite free from zirconia, and identical in its composition with bucholzite, which is a *silicate of alumina.* How Dr. Muir fell into the mistake I do not know. I looked over all my specimens, and found in one small crystals which had the aspect of zircons. It might be that the same crystals existed in the specimen which he analysed. That it was impure is obvious from the quantity of iron which he obtained.

The mistake of Dr. Muir had escaped my memory till it was recalled by the notices in the new edition of Phillips's Mineralogy by Mr. Alger. To set mineralogists right I repeated the analyses of sillimanite which I had made several years ago. This I shall give at the end of this notice, after drawing up a short description of the mineral.

Sillimanite, *bucholzite* and *fibrolite* are all synonyms of the same mineral, the chemical constitution of which is Al S, or a simple silicate of alumina.

In all the specimens which I have seen the mineral is in long crystals, consisting of oblique prisms, with angles of about 80° to 100°.

The colour is gray or rather mottled, in some places nearly white, in others gray, more or less dark. The texture is foliated, or in some crystals fibrous. The lustre vitreous. The hardness 5, or about that of phosphate of lime. It is opake, or only slightly translucent when in thin plates. It is easily frangible, especially in the direction perpendicular to the axis of the prism.

The specific gravity in very pure crystals I found to vary from 3·200 to 3·357.

Before the blowpipe it becomes white but does not fuse. With soda it forms a bead, transparent and red while hot, but becoming white and opake when cold. With microcosmic salt it forms a colourless bead.

Being subjected to analysis, I found it composed of—

Silica	45·65
Alumina	49·50
Protoxide of iron	4·10
	99·25

This is exactly

20·75 atoms of silica,
20 atoms of alumina,
0·827 atom of protoxide of iron.

From the colour of the mineral there could be no doubt that the iron was in the state of protoxide; while from its mottled appearance it was evident that the iron was not combined but mixed unequally with the pure sillimanite. If we suppose the protoxide of iron to be combined with silica in the form of silicate of iron, and this silicate scattered through the crystal, it is obvious that the mineral, if pure, would be an anhydrous silicate of alumina, having an atomic weight of 4·25.

I have compared the specimens of bucholzite from America with sillimanite, and find them to agree, excepting that the bucholzite is not so pure as the sillimanite. The shape of the crystals, the hardness and lustre, and the specific gravity agree very nearly.

I have never seen the *fibrolite* of Bournon and Chenevix, but the descriptions and analysis correspond with those of sillimanite.

Henceforward the terms *sillimanite*, *bucholzite* and *fibrolite* may be dropt, and the species distinguished by the name of *silicate of alumina*, which indicates the chemical constitution of the mineral.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 452.]

April 17, 1845. "DESCRIPTION of a Self-registering Thermometer." By Mr. Mansfield Harrison. Communicated by James Alderson, M.D., F.R.S.

The instrument here described is composed of two parallel bars, the one of iron and the other of copper, united at their lower end, and registering their differences of expansion by heat, by means of a series of multiplying levers, carrying a pencil which is made to press on paper wound round a cylinder moved by clock-work.

"On the Viscous Theory of Glacier Motion. Part I. containing Experiments on the Flow of Plastic Bodies, and Observations on the Phenomena of Lava Streams." By James D. Forbes, Esq., F.R.S. Lond. and Edin., Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh.

The author adduces some new experiments in confirmation of his theory of the nature and causes of the motion of glaciers, and which present an analogy with the phenomena exhibited by the flow of masses of semifluid or viscous matter contained in a narrow channel, along which they move by the force of gravity; and also with the ripple marks on the surface of a stream of water when its course is impeded by obstacles. These latter phenomena, he remarks, were noticed and accurately described by Leonardo da Vinci. Analogies of a still more striking nature are presented by the appearance of streams of lava in their flow from volcanos and in the progress of their descent, which illustrate a great number of the phenomena of glacier motion, and corroborate the views of the author as to their nature and as to the laws they obey. Various quotations are given from authors who have been struck with this analogy, and who have pointed it out more or less circumstantially in the narratives of their travels in the neighbourhood of Etna and Vesuvius; to which the author adds some of his own observations on the lava from these mountains, which throw further light on the subject.

LXXIX. *Intelligence and Miscellaneous Articles.*

TERRESTRIAL MAGNETISM.

A COMMUNICATION on Terrestrial Magnetism, by Professor Locke, the result of several years' observation in various parts of the United States, was read at the American Philosophical Society, April 5, 1844, and referred to a Committee, consisting of Prof. Frazer, Mr. M'Euen and Prof. Bache.

Prof. Locke gave a brief history of the circumstances which induced him to undertake these researches, in which he made a grateful acknowledgement for the kind and able instruction, especially in the practical manipulations, received from Prof. Bache of this Society, while in London and Paris. He read a part of the correspondence between himself and Col. Sabine on the subject of the point of maximum intensity at or near to Lake Superior, and suggested, as the probable result of his researches and of those of Lieut. Lefroy, briefly alluded to by Col. Sabine, that the region of maximum intensity is either at Kewenon peninsula, south side of Lake Superior, or along a line extending thence to Rainy Lake—the last opinion having been expressed by Col. Sabine in his correspondence. To determine this point more precisely, Prof. Locke suggested that observations should be multiplied in various parts of the region of the lake. The observations were prefaced by a popular explanation of what is meant by the four elements of terrestrial magnetism, declination ("variation") dip or inclination, horizontal intensity of magnetic force, and total intensity of the same force. These explanations were followed by the observations of six successive years, commencing in 1838, and extending more or less over the region included between the middle of Kentucky on the south, and the north side of Lake Superior on the north; and between Cambridge, Mass., on the east, to the middle of Iowa on the west. His observations had been accompanied throughout by remarks on the geology at each station, and he expressed the opinion that those observations justified the conclusion, that the general character of the rocks, especially as regards the distinction between the aqueous and the igneous formations, could be ascertained by the elements, as indicated by lines of observations extended along any given region. He exhibited a chart illustrative of this subject, on which the two quantities of dip and intensity were represented by curves passing through ordinates standing on horizontal distances, generally along lines of latitude. These curves, along regions of aqueous rocks, such as those along the Mississippi, were remarkably uniform, while those over igneous rocks were extremely irregular and undulating, sometimes ascending and descending with a needle-like sharpness. He remarked, that if these distinctive characters obtain so generally as to constitute a *law*, the magnetical instruments will form a kind of "*mineral* or *divining rod*" to the geologist.

There was exhibited, also, a chart of the United States, showing chiefly, the isodynamic lines consistent with the observations. These lines form ovals around Lake Superior as an axis, the longer or pointed ends pointing towards New York, and in the opposite direction. The outer oval descending along the Hudson river, passing through the city of New York, along the coast, to near Baltimore, and, turning westward, traverses Maryland, Virginia, and Kentucky, and crosses the Mississippi into Missouri about thirty miles below St. Louis.

The force along this line is 994, that at Cincinnati, in Ohio, being 1000; and an oval is delineated at every increase of 10, until, at the axis at Lake Superior, it rises to from 1054 to 1060. But Prof. Locke stated, more observations are needed to fix more precisely those lines at remote points, and he expressed a desire that the observations of Major Graham, Prof. Bache, and Prof. Loomis, together with his own, might be united in a suitable chart, such as might be produced by a convention of observers. Two other charts, explained in the communication, were also exhibited; the one a copy of a part of Col. Sabine's north polar chart, with additions, and with a delineation of the relative positions of the astronomical poles, the pole of dip and convergence discovered by Ross, and the pole of intensity at Lake Superior; and the other a particular delineation of Copper Harbour and of Porter's Island, where, in Prof. Locke's line of observations, he found the greatest intensity of force.

Prof. Locke, in a second paper, continues his observations upon the magnetic dip and intensity, made at different points in the United States during the year 1844, and includes thirty-five sets of observations made at twenty-four different stations. He then gives a series of thirteen observations made at three stations, viz. Fort Lee, N. Y., Snake Hill, and Patterson, N. J., intended to show the remarkable changes which take place both in the dip and intensity, in passing from rocks of the usual kind, to those belonging to the trappean family; which changes, so far as they regard the magnetic intensity, he announces in the following terms:—"The intensity, which ordinarily has a value along a line of moderate length not varying beyond certain moderate limits, becomes at the base of a trappean pinnacle, extraordinarily diminished, and at the top of the same still more extraordinarily increased." These changes, as well as those of the dip which follow the same law, are illustrated by diagrams, in which the values of the dip and intensity are represented as the ordinates of a curve, the distances between the stations being taken upon the line of abscissas. Prof. Locke believes these changes to be due to the assumption of magnetic properties by the trappean rocks, the axis of the magnet coinciding with the axis of figure of the hill, which is generally vertical.

Prof. Locke strongly urges the propriety of multiplying observations upon these phænomena, in order to develope the general law; and concludes by returning his thanks to Col. Sabine and the British Association for the kindness and liberality with which they have tendered their assistance in his labours.—*Proceedings of the Amer. Phil. Soc.*, vol. iv., p. 63.

COHESION OF LIQUIDS.

Prof. Henry made a verbal communication to the American Philosophical Society, on April 5th, 1844, relative to the cohesion of liquids; in which he stated that very erroneous ideas are given as to the constitution of matter in the ordinary books on Natural Philosophy. The passage of a body from a solid to a liquid state is generally attributed to the neutralization of the attraction of cohesion by the repulsion of the increased quantity of heat; the liquid being supposed to retain a small portion of its original attraction, which is shown by the force necessary to separate a surface of water from water in the well-known experiment of a plate suspended from a scale beam over a vessel of the liquid. It is, however, more in accordance with all the phænomena of cohesion to suppose, instead of the attraction of the liquid being neutralized by the heat, that the effect of this agent is merely to neutralize the polarity of the molecules so as to give them perfect freedom of motion around every imaginable axis. The small amount of cohesion (53 grains to the square inch) exhibited in the experiment above alluded to, is due, according to the theory of capillarity of Young and Poisson, to the tension of the exterior film of the surface of water drawn up by the elevation of the plate. This film gives way first, and the strain is thrown on an inner film, which, in turn, is ruptured; and so on until the plate is entirely separated; the whole effect being similar to that of tearing the water apart atom by atom.

Reflecting on this subject, Prof. Henry had thought that a more correct idea of the magnitude of the molecular attraction might be obtained by studying the tenacity of a more viscid liquid than water. For this purpose he had recourse to soap water, and attempted to measure the tenacity of this liquid by means of weighing the quantity of water which adhered to a bubble of this substance just before it burst, and by determining the thickness of the film from an observation of the colour it exhibited in comparison with Newton's scale of thin plates. Although experiments of this kind could only furnish approximate results, yet they showed that the molecular attraction of water for water, instead of being only about 53 grains to the square inch, is really several hundred pounds, and is probably equal to that of the attraction of ice for ice. The effect of dissolving the soap in the water is not, as might at first appear, to increase the molecular attraction, but to diminish the mobility of the molecules, and thus to render the liquid more viscid.

Prof. Henry, in a second communication, (made on the 17th of

May,) states that he had prosecuted his experiments on the soap-bubble to a greater extent, and had arrived at a number of results which appeared to him of some interest in reference to capillarity, a subject which had given rise to a greater diversity of opinion than any other part of natural philosophy. As an evidence of its present unsettled state, he mentioned the fact that the last edition of the *Encyclopædia Britannica* contained two articles on this subject, under different names; one by Dr. Young, and the other by Mr. Ivory, which explain the phænomena on entirely different physical principles.

According to the theory of Young and Poisson, many of the phænomena of liquid cohesion, and all those of capillarity, are due to a contractile force existing at the free surface of the liquid, and which tends in all cases to urge the liquid in the direction of the radius of curvature towards the centre, with a force inversely as this radius. According to this theory, the spherical form of a dew-drop is not the effect of the attraction of each molecule of the water on every other, as in the action of gravitation in producing the globular form of the planets (since the attraction of cohesion only extends to an unappreciable distance), but it is due to the contractile force which tends constantly to enclose the given quantity of water within the smallest surface, namely, that of a sphere. Prof. Henry finds a contractile force perfectly similar to that assumed by this theory in the surface of the soap-bubble; indeed, the bubble may be considered a drop of water with the internal liquid removed, and its place supplied by air. The spherical force in the two cases is produced by the operation of the same cause. The contractile force in the surface of the bubble is easily shown by blowing a large bubble on the end of a wide tube, say an inch in diameter; as soon as the mouth is removed, the bubble will be seen to diminish rapidly, and at the same time quite a forcible current of air will be blown through the tube against the face. This effect is not due to the ascent of the heated air from the lungs with which the bubble was inflated, for the same effect is produced by inflating with cold air, and also when the bubble is held perpendicularly above the face, so that the current is downwards.

Many experiments were made to determine the amount of this force, by blowing a bubble on the larger end of a glass tube in the form of the letter U, and partially filled with water; the contractile force of the bubble, transmitted through the enclosed air, forced down the water in the larger leg of the tube, and caused it to rise in the smaller. The difference of level observed by means of a microscope, gave the force in grains per square inch, derived from the known pressure of a given height of water. The thickness of the film of soap water which formed the envelope of the bubble, was estimated as before by the colour exhibited just before bursting. The results of these experiments agree with those of weighing the bubble, in giving a great intensity to the molecular attraction of the liquid; equal at least to several hundred pounds to the square inch. Several other methods were employed to measure the tenacity of the film, the general results of which were the same: the numerical details of these are reserved, however, until the experiments can be repeated with a more delicate balance.

The comparative cohesion of pure water and soap water was determined by the weight necessary to detach the same plate from each; and in all cases the pure water required the greater force. The want of permanency in the bubble of pure water is therefore not due to feeble attraction, but to the perfect mobility of the molecules, which causes the equilibrium, as in the case of the arch without friction of parts, to be destroyed by the slightest extraneous force.

Several other experiments with films of soap water were also described, which afford striking illustrations of the principles of capillarity, and which apparently have an important bearing on the whole subject of cohesion.—*Proceedings of the Amer. Phil. Soc.*, vol. iv. pp. 56, 84.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1845.

Chiswick.—April 1. Slight haze: clear. 2, 3. Foggy: fine: clear. 4. Foggy: cloudless. 5. Thick haze: fine. 6. Clear and fine: sharp frost. 7. Clear and frosty: very dry air: slight frost. 8. Densely overcast: cloudy: sharp frost. 9. Foggy: overcast. 10. Showery throughout. 11. Rain: dense clouds and cold: frosty at night. 12. Cloudy and fine: rain. 13. Showery: clear and fine. 14. Boisterous. 15. Boisterous, with slight rain. 16. Overcast: fine. 17. Fine. 18. Overcast: fine. 19. Overcast and cold: very fine. 20. Slight haze: very fine. 21, 22. Very fine. 23. Sultry. 24—26. Very fine. 27. Showery. 28. Overcast. 29. Very fine. 30. Overcast.—Mean temperature of the month 1° above the average.

Boston.—April 1. Cloudy. 2—5. Fine. 6. Fine: ice this morning. 7. Cloudy. 8. Cloudy: rain A.M. 9. Fine. 10. Windy: rain P.M. 11. Windy: rain A.M. 12. Cloudy. 13. Cloudy: rain P.M. 14. Windy: rain P.M. 15. Cloudy and stormy: rain early A.M. 16. Cloudy. 17. Fine. 18. Cloudy. 19—21. Fine. 22. Cloudy. 23—25. Fine. 26. Rain. 27. Cloudy. 28—30. Fine.

Sandwick Manse, Orkney.—April 1. Bright: damp. 2. Bright: clear. 3, 4. Clear: cloudy. 5. Clear. 6. Fog: clear. 7, 8. Bright: clear. 9. Rain: cloudy. 10, 11. Bright: cloudy. 12, 13. Cloudy: clear. 14. Rain: clear. 15. Clear. 16. Cloudy: drizzle. 17. Drizzle: fog. 18. Bright: fog. 19. Clear. 20, 21. Fine: clear: fine. 22. Fine. 23. Fine: fog. 24. Cloudy. 25. Bright: cloudy. 26. Rain: damp. 27. Bright: cloudy. 28. Showers. 29. Bright: clear. 30. Cloudy.

Applegarth Manse, Dumfries-shire.—April 1, 2. Fine: dry: hoar frost. 3. Remarkably fine. 4. Very droughty: raw frost. 5. Very droughty, but milder. 6. Hoar frost. 7. Slight frost. 8. Rain and hail. 9. Slight showers. 10. Frequent showers. 11. Clear: fair, but cold. 12. Cloudy: drops: rain. 13. Hail and rain. 14. Slight showers. 15. Dry and cold. 16. Fair: fine: slight frost. 17. Very fine. 18. Fine. 19—22. Fine: droughty. 23, 24. Fine. 25. A few drops: rain. 26. Heavy rain. 27. Heavy rain and flood. 28. Fair, but unsettled. 29. Shower P.M. 30. Heavy rain P.M.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. April.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
1.	30·237	30·194	29·87	30·03	30·00	29·95	30·01	59	29	38	54½	33	45	48	e.	calm	ssw.	sw.	......	......	......	......
2.	30·086	30·061	29·74	29·95	29·94	30·10	30·06	60	33	46·5	62	36½	52	40	e.	calm	e.	se.	......	......	......	......
3.	29·968	29·882	29·62	29·90	29·80	30·02	30·02	69	30	49	63½	34½	44½	42	se.	calm	e.	e.	......	......	......	......
4.	30·053	29·886	29·53	29·89	29·98	30·04	30·08	69	35	50	51	35½	44½	40	e.	calm	ene.	se.	......	......	......	......
5.	30·041	29·992	29·61	29·99	29·95	30·10	30·06	58	32	43	58	38½	46	40	e.	calm	ene.	se.	......	......	......	......
● 6.	29·914	29·891	29·62	29·95	29·85	30·04	29·96	60	22	45	59½	31½	43	40	ne.	calm	w.	n.	......	......	......	......
7.	29·915	29·813	29·57	29·82	29·57	29·88	29·63	60	26	40	57	36	44	39½	ne.	calm	e.	se.	......	......	......	......
8.	29·606	29·416	29·18	29·39	29·20	29·26	29·20	57	23	41	51	40	45	40	w.	w.	ssw.	se.	·04	......	0·15	......
9.	29·068	28·972	28·76	29·02	28·99	29·19	29·26	47	35	44	52	34	40½	39	ne.	w.	ne.	ene.	·09	·06	......	·27
10.	29·118	29·020	28·60	29·04	29·28	29·42	29·64	48	34	42	46½	37	42	39½	w.	nw.	nne.	ne.	·19	......	......	......
11.	29·636	29·285	29·00	29·47	29·60	29·74	29·73	49	30	41	49	38½	42½	39½	ne.	ne.	n.	......	·16	·34	......	......
12.	29·770	29·607	29·37	29·60	29·56	29·62	29·54	54	34	42·5	51½	30	44	38½	w.	calm	ssw.	s.	·03	·04	......	......
13.	29·787	29·354	29·33	29·30	29·00	29·22	29·17	54	41	47	48	36½	45½	41	w.	w.	sw.	n.	·10	......	......	......
☽ 14.	29·558	29·401	28·90	29·22	29·61	29·50	29·97	55	39	48	52	38	44	42½	nw.	nw.	wnw.	nne.	·10	·25	......	·10
15.	30·126	29·694	29·42	30·00	30·25	30·30	30·40	49	40	43	51½	41	44	40	ne.	ne.	nne.	......	·02	·20	......	......
16.	30·275	30·265	29·97	30·35	30·35	30·32	30·40	54	39	43	54	34½	46	44	ne.	calm	nne.	sw.	......	......	0·78	......
17.	30·328	30·190	29·98	30·36	30·25	30·34	30·34	62	38	49	65½	37	47	47	ne.	ne.	sw.	w.	......	......	......	·05
18.	30·157	30·101	29·80	30·20	30·18	30·32	30·30	58	37	42	59	48	48	40	ne.	calm	ne.	......	......	......	......	......
19.	30·081	30·032	29·72	30·15	30·10	30·28	30·21	60	36	49	61	40½	50	44	ne.	ne.	ne.	sse.	......	......	......	......
20.	30·139	30·078	29·74	30·12	30·10	30·20	30·20	68	36	51	66	37½	55	48	e.	e.	e.	sse.	......	......	......	......
21.	30·108	30·014	29·60	30·10	30·00	30·19	30·14	66	38	53	68	38½	52½	48½	ne.	e.	se.	......	......	......	......	......
○ 22.	29·982	29·924	29·58	30·00	29·90	30·14	30·08	63	31	43	65½	43	56	46	ne.	calm	e.	ese.	......	......	......	......
23.	29·851	29·727	29·47	29·84	29·75	29·98	29·90	72	34	48	65	43½	48½	42	e.	calm	e.	ese.	......	......	......	......
24.	29·747	29·707	29·30	29·68	29·66	29·86	29·79	71	34	54	68	39	44½	43	sw.	calm	se.	ese.	......	......	0·00	......
25.	29·786	29·632	29·32	29·65	29·31	29·73	29·55	67	52	53	65	41	45	46	s.	calm	se.	ese.	·18	......	......	......
26.	29·583	29·414	28·83	29·05	29·11	29·30	29·25	63	47	55	58	50	50	45	sw.	sw.	s.	ese.	·01	·23	......	·20
27.	29·654	29·620	29·08	29·07	29·18	29·30	29·23	61	44	52	50½	46½	49	46	sw.	w.	se & s.	ese.	·02	......	......	......
☾ 28.	29·682	29·627	29·20	29·39	29·50	29·29	29·51	64	50	56	59	47½	49½	49	s.	calm	s.	sse.	......	......	......	·18
29.	29·982	29·875	29·35	29·67	29·72	29·64	29·73	66	49	57·5	59½	47½	56	48	w.	calm	s.	ese.	......	......	2·00	·14
30.	30·041	29·975	29·44	29·70	29·47	29·66	29·30	63	51	59	60½	48½	54	48	sw.	calm	s.	se.	·01	......	......	......
Mean.	29·875	29·754	29·41	29·746	29·692	29·831	29·822	60·20	36·63	47·4	57·7	39·4	47·24	43·13					0·95	1·12	2·93	0·94

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SUPPLEMENT TO VOL. XXVI. THIRD SERIES.

LXXX. *Reflections on the Resolution of Algebraic Equations of the Fifth Degree.* By G. B. JERRARD, *Examiner in Mathematics and Natural Philosophy at the University of London*[*].

SOME years ago, while reflecting on the possibility of discovering an expression which, consisting of a finite combination of radicals and rational functions, would satisfy the general equation of the fifth degree,

$$x^5 + A_1 x^4 + A_2 x^3 + A_3 x^2 + A_4 x + A_5 = 0,$$

I was led to a result which seems to indicate the possibility of assigning, in a subsidiary equation of the third degree with respect to x,

$$x^3 + p_1 x^2 + p_2 x + p_3 = y,$$

such finite expressions to p_1, p_2, p_3, that the equation for y may take the known form

$$y^5 + A'_2 y^3 + \frac{1}{5} A'^2_2 y + A'_5 = 0.$$

If p_1, p_2, p_3 admit of having the requisite expressions assigned to them, it must, in opposition to what has been advanced by Abel[†], be possible to discover five finite algebraic expressions capable of satisfying the equation

$$x^5 + A_1 x^4 + A_2 x^3 + A_3 x^2 + A_4 x + A_5 = 0.$$

In effect, we might obtain, as we shall see in another place,

$$x = q_4 + q_3 y + q_2 y^2 + q_1 y^3 + q_0 y^4,$$

or rather the system of equations

$$\begin{array}{l} x_1 = q_4 + q_3 y_1 + \cdot\cdot + q_0 y_1^4, \\ x_2 = q_4 + q_3 y_2 + \cdot\cdot + q_0 y_2^4, \\ \quad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \\ \quad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \\ x_5 = q_4 + q_3 y_5 + \cdot\cdot + q_0 y_5^4, \end{array}$$

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[*] Communicated by the Author.

[†] A complete exposition of the argument of Abel is given by Sir William R. Hamilton in vol. xviii. part 2 of the Transactions of the Royal Irish Academy.

where $x_1, x_2, \ldots x_5$ are the roots of $x^5 + A_1 x^4 + \ldots = 0$; $y_1, y_2, \ldots y_5$ of $y^5 + A'_2 y^3 + \ldots = 0$; and where $q_4, q_3, \ldots q_0, y_1, y_2, \ldots y_5$ are all of them expressible as known functions of p_1, p_2, p_3.

Can then p_1, p_2, p_3 be found? To bring this question to a decision, we must enter upon an inquiry of great length and intricacy. But it is in the nature of such discussions to unfold in their progress theories which bear upon remote and seemingly unconnected departments of knowledge: and of this truth we shall find in what follows some remarkable exemplifications.

SECTION I.

1. Now p_1, p_2, p_3 must be such that $x^3 + p_1 x^2 + p_2 x + p_3$ may become a root of an equation of the form

$$y^5 + A'_2 y^3 + \frac{1}{5} A'^2_2 y + A'_5 = 0;$$

or that the expressions

$$\begin{gathered} x_1^3 + p_1 x_1^2 + p_2 x_1 + p_3, \\ x_2^3 + p_1 x_2^2 + p_2 x_2 + p_3, \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ x_5^3 + p_1 x_5^2 + p_2 x_5 + p_3 \end{gathered}$$

may become the five roots of that equation. For $x_1, x_2, \ldots x_5$ enter symmetrically into the calculus, and there is consequently nothing to connect one of them rather than another with the x of the expression $x^3 + p_1 x^2 + p_2 x + p_3$.

If then we consider that the roots of the equation for y must, as De Moivre has shown, be generally expressible by

$$t + u, \quad \iota t + \iota^4 u, \quad \iota^2 t + \iota^3 u, \quad \iota^3 t + \iota^2 u, \quad \iota^4 t + \iota u,$$

$\iota, \iota^2, \iota^3, \iota^4$ denoting the imaginary roots of the binomial equation $\rho^5 - 1 = 0$, we shall be conducted to a system of equations

$$\left.\begin{aligned} x_\alpha^3 + p_1 x_\alpha^2 + p_2 x_\alpha + p_3 &= t + u, \\ x_\beta^3 + p_1 x_\beta^2 + p_2 x_\beta + p_3 &= \iota t + \iota^4 u, \\ x_\gamma^3 + p_1 x_\gamma^2 + p_2 x_\gamma + p_3 &= \iota^2 t + \iota^3 u, \\ x_\delta^3 + p_1 x_\delta^2 + p_2 x_\delta + p_3 &= \iota^3 t + \iota^2 u, \\ x_\varepsilon^3 + p_1 x_\varepsilon^2 + p_2 x_\varepsilon + p_3 &= \iota^4 t + \iota u; \end{aligned}\right\} \quad . \quad . \quad \text{(a.)}$$

in which $\alpha, \beta, \gamma, \delta, \varepsilon$ represent, in an undetermined or arbitrary order of succession, the five indices 1, 2, 3, 4, 5.

2. From this system there will arise, as we know from the theory of permutations, $1 . 2 . 3 . 4 . 5$ systems; if instead of

α, β, γ, δ, ε, we substitute 1, 2, 3, 4, 5, in all the different arrangements which they can assume. But these 120 systems will be found to furnish only twelve different sets of values for p_1, p_2, p_3. Our first object will be to express p_1, p_2, p_3 as functions of $x_1, x_2, \ldots x_5$ without t and u.

3. By combining any three of the five equations of the system (a.), we see that we may eliminate t and u; and that therefore, if we replace $x_\alpha^3 + p_1 x_\alpha^2 + \ldots$, $x_\beta^3 + p_1 x_\beta^2 + \ldots$, $\ldots x_\varepsilon^3 + p_1 x_\varepsilon^2 + \ldots$, by $y_\alpha, y_\beta, \ldots y_\varepsilon$, we may arrive at a final equation of the first degree with respect to $y_\alpha, y_\beta, \ldots y_\varepsilon$,

$$\mu_\alpha y_\alpha + \mu_\beta y_\beta + \mu_\gamma y_\gamma + \mu_\delta y_\delta + \mu_\varepsilon y_\varepsilon = 0:$$

where $\mu_\alpha, \mu_\beta, \ldots \mu_\varepsilon$ are functions of ι which have no common factor different from 1, and are such that two of them must admit of being equated to zero.

4. It is clear that $\mu_\alpha, \mu_\beta, \ldots \mu_\varepsilon$ will be susceptible of $\frac{5.4.3}{1.2.3}$ or ten differently derived sets of values. The ten equations which may thus arise I shall for the moment represent by

$$\begin{array}{c} {}^1\mu_\alpha y_\alpha + {}^1\mu_\beta y_\beta + \ldots + {}^1\mu_\varepsilon y_\varepsilon = 0, \\ {}^2\mu_\alpha y_\alpha + {}^2\mu_\beta y_\beta + \ldots + {}^2\mu_\varepsilon y_\varepsilon = 0, \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ {}^{10}\mu_\alpha y_\alpha + {}^{10}\mu_\beta y_\beta + \ldots + {}^{10}\mu_\varepsilon y_\varepsilon = 0. \end{array}$$

All these will belong to the same system.

5. Further, if we consider that $y_\alpha + y_\beta + y_\gamma + y_\delta + y_\varepsilon$, which must be equated to zero, will give

$$x_\alpha^3 + x_\beta^3 + \ldots + x_\varepsilon^3 + (x_\alpha^2 + x_\beta^2 + \ldots + x_\varepsilon^2) p_1 + (x_\alpha + x_\beta + \ldots + x_\varepsilon) p_2 + 5 p_3 = 0;$$

and that consequently, if denoting

$$x_\alpha^n + x_\beta^n + \ldots + x_\varepsilon^n \text{ by } S_n$$

we eliminate p_3 from the equation

$$x_\tau^3 + p_1 x_\tau^2 + p_2 x_\tau + p_3 = y_\tau,$$

there will result

$$y_\tau = x_\tau^3 - \frac{1}{5} S_3 + \left(x_\tau^2 - \frac{1}{5} S_2\right) p_1 + \left(x_\tau - \frac{1}{5} S_1\right) p_2:$$

we shall perceive that any two of those ten equations will, if incapable of being made coincident by any transformation and reduction, be sufficient for enabling us to express p_1 and p_2 as rational functions of the roots of the original equation.

6. It is not difficult actually to obtain such a pair of equa-

tions, and to show that those values of p_1, p_2, which satisfy them, will necessarily satisfy every other pair belonging to the same system.

In effect, if introducing an indeterminate multiplier λ we unite

$$0 = \lambda\,(y_\alpha + y_\beta + y_\gamma + y_\delta + y_\varepsilon)$$

with $$\Phi = \mu_\alpha y_\alpha + \mu_\beta y_\beta + \mu_\gamma y_\gamma + \mu_\delta y_\delta + \mu_\varepsilon y_\varepsilon,$$

on dividing the result by $\mu_\alpha + \lambda$, and designating

$$\frac{\mu_\tau + \lambda}{\mu_\alpha + \lambda} \text{ by } \nu_\tau,$$

we shall find

$$\frac{\Phi}{\mu_\alpha + \lambda} = y_\alpha + \nu_\beta y_\beta + \nu_\gamma y_\gamma + \nu_\delta y_\delta + \nu_\varepsilon y_\varepsilon.$$

Now in order that Φ may be different from

$$\mu_\alpha\,(y_\alpha + y_\beta + y_\gamma + y_\delta + y_\varepsilon),$$

λ must admit of being determined so as to satisfy at least one equation of the form $\mu_\upsilon + \lambda = 0$, without causing $\mu_\alpha + \lambda$ to vanish. If, therefore, we reflect that the system of equations on which Φ depends will remain unaltered if, while we substitute another imaginary root ι' instead of ι, we make certain substitutions among $y_\beta, y_\gamma, y_\delta, y_\varepsilon$; and that consequently Φ may be deduced from

$$\frac{\Phi}{\mu_{\alpha'} + \lambda'} = y_\alpha + \nu_{\beta'} y_{\beta'} + \nu_{\gamma'} y_{\gamma'} + \nu_{\delta'} y_{\delta'} + \nu_{\varepsilon'} y_{\varepsilon'},$$

$\nu_{\upsilon'}$ being the same function of ι' and λ' as ν_υ is of ι and λ, but $y_{\upsilon'}$ being a different root from y_υ: we shall readily perceive that the coefficient of *any one indifferently* of the four roots $y_\beta, y_\gamma, y_\delta, y_\varepsilon$ may be equated to zero, when the coefficient of y_α is equal to 1.

Accordingly let us suppose that

$$\nu_\varepsilon = 0,$$

and, on expressing $y_\alpha, y_\beta, \ldots y_\delta$ in terms of t and u, there will arise

$$\frac{\Phi}{\mu_\alpha - \mu_\varepsilon} = (1 + \iota\,\nu_\beta + \iota^2\nu_\gamma + \iota^3\nu_\delta)\,t + (1 + \iota^4\nu_\beta + \iota^3\nu_\gamma + \iota^2\nu_\delta)u.$$

This expression for $\dfrac{\Phi}{\mu_\alpha - \mu_\varepsilon}$ must in vanishing assume the form $0\,t + 0\,u$. For $\dfrac{t}{u}$, which is not independent of A'_2, A'_5, cannot generally be equal to $-\frac{1+\iota^4\nu_\beta+\,..}{1+\iota\,\nu_\beta+\,..}$. We must therefore have separately

$$1+\iota\,\nu_\beta+\iota^2\nu_\gamma+\iota^3\nu_\delta=0,$$
$$1+\iota^4\nu_\beta+\iota^3\nu_\gamma+\iota^2\nu_\delta=0.$$

If now we multiply the first of these equations by ι^ζ, and from the product subtract the second; we shall find

$$\iota^\zeta-1+(\iota^{\zeta+1}-\iota^4)\,\nu_\beta+(\iota^{\zeta+2}-\iota^3)\,\nu_\gamma+(\iota^{\zeta+3}-\iota^2)\,\nu_\delta=0:$$

where ζ may have an unlimited number of different values assigned to it.

Hence, if we cause ν_γ to disappear by making $\zeta+2=3$, there will result

$$\nu^\beta=\nu_\delta+a_\iota,$$

a_ι denoting $-\frac{\iota-1}{\iota^2-\iota^4}$; and if $\zeta+1=4$, we shall have

$$\nu_\gamma=a_\iota\,\nu_\delta+1,$$

$-\frac{\iota-\iota^2}{\iota-\iota^3}$ the coefficient of ν_δ being evidently equal to $\frac{\iota}{\iota}\,a_\iota$.

Finally, on returning to the expression for $\frac{\Phi}{\mu_\alpha+\nu}$ which involves y_α, y_β, .., and making the requisite substitutions in it, we shall obtain

$$\frac{\Phi}{\mu_\alpha-\mu_\iota}=y_\alpha+y_\gamma+a_\iota\,y_\beta+(y_\beta+y_\delta+a_\iota\,y_\gamma)\,\nu_\delta\,;$$

which, if $\Phi=0$, will give, independently of ν_δ,

$$\left.\begin{array}{l} y_\alpha+y_\gamma+a_\iota y_\beta=0, \\ y_\beta+y_\delta+a_\iota y_\gamma=0. \end{array}\right\} \;.\;.\;.\;.\;.\;.\;.\quad \text{(b.)}$$

And we see that the values of p_1 and p_2 which satisfy this pair of equations must be such as to fulfil the condition $\Phi=0$, or $\mu_\alpha y_\alpha+\mu_\beta y_\beta+\mu_\gamma y_\gamma+\mu_\delta y_\delta+\mu_\iota y_\iota=0$, and consequently to satisfy every one of the ten equations belonging to the same system.

7. We might now by means of these two equations, which involve p_1, p_2, x_α, x_β, x_γ, x_δ, and which are both of them of the first degree with respect to p_1 and p_2, express p_1 and p_2 as rational functions of x_α, x_β, x_γ, x_δ; and then, from discovering the number of different values which the expression for p_n (either p_1 or p_2) would assume if the five indices 1, 2, 3, 4, 5

were made to enter into it four at a time in every order of succession, determine the degree of the final equation $\chi_n (p_n, A_1, A_2, \ldots A_5) = 0$; χ_n representing a rational function. Certain properties of the roots of this equation would also become known. But we shall arrive far more rapidly at the same results, from considering the ten equations in question; the remaining eight of which are connected by a remarkable law with those already found.

8. It is obvious that we should have obtained a different pair of equations if, instead of supposing that $\nu_\varepsilon = 0$, we had equated another of the coefficients $\nu_\beta, \nu_\gamma, \nu_\delta, \nu_\varepsilon$, to zero. It will not, however, be necessary for us to retrace our steps in order to complete the system. From either of the equations (b.) we may discover all the rest. Thus if we take the first of them, and represent by

$$y_{\alpha'} + y_{\gamma'} + a_{\iota'} y_{\beta'} = 0$$

what that equation will become if ι' be substituted for ι, and the system (a.) remain unaltered; we shall see that, ι' being different from ι, there will arise a new equation belonging to the system. A difficulty here indeed presents itself. For if we write $\iota^n t'$ for t and $\iota^{4n} u'$ for u, it will be evident that we may obtain, corresponding to each of the four expressions $a_\iota, a_{\iota^2}, a_{\iota^3}, a_{\iota^4}$, five equations of the form in question. We appear therefore at first view to be conducted to twenty and not ten equations in the system. But an examination of the function $a_{\iota'}$ will, as I proceed to show, point out the relation

$$a_{\iota^n} = a_{\iota^{4n}};$$

which includes these two conditions,

$$a_\iota = a_{\iota^4}, \qquad a_{\iota^2} = a_{\iota^3}.$$

9. Reverting to the expression for a_ι, we see that

$$(\iota^2 - \iota^4)\, a_\iota = 1 - \iota.$$

Now a_ι, considered generally as a rational function of ι, may evidently be included under the form

$$a_\iota = c_4 + c_3 \iota + c_2 \iota^2 + c_1 \iota^3 + c_0 \iota^4;$$

where $c_4, c_3, \ldots c_0$ do not involve ι.

Hence we find

$$1 - \iota = c_1 - c_3 + (c_0 - c_2)\iota + (c_4 - c_1)\iota^2 + (c_3 - c_0)\iota^3 + (c_2 - c_4)\iota^4;$$

which will be satisfied independently of ι, if

$$c_1 - c_3 = 1, \quad c_0 - c_2 = -1,$$
$$c_4 - c_1 = 0, \quad c_3 - c_0 = 0, \quad c_2 - c_4 = 0.$$

We thus perceive that

$$\begin{aligned} a_{,} &= c_1\,(1+\iota^2+\iota^3) + c_0\,(\iota+\iota^4) \\ &= 1+\iota^2+\iota^3+c_0\,(1+\iota+\iota^2+\iota^3+\iota^4) = -(\iota+\iota^4). \end{aligned}$$

From which there will result, on writing ι^n instead of ι,

$$a_{,n} = a_{,4n}.$$

We might have arrived at the expression for $a_{,}$ from considering that $y_\alpha + y_\gamma + a_{,} y_\beta$, which expressed as a function of t and u would become $\iota^4(\iota^4+\iota+a_{,})\,t + \iota\,(\iota+\iota^4+a_{,})\,u$, must in vanishing assume the form $0\,t + 0\,u$.

It appears then that $a_{,n}$ has only two different values, $a_{,}$ and $a_{,2}$. Hence all functions symmetric relatively to $a_{,}$ and $a_{,2}$ will remain unaltered when we substitute any one of the three roots, ι^2, ι^3, ι^4, instead of ι.

And in accordance with this we find

$$\begin{aligned} a_{,} + a_{,2} &= -\,(1+\iota+\iota^2+\iota^3+\iota^4) = -\,(-1), \\ a_{,} \times a_{,2} &= (-1)^2\,(\iota+\iota^4)\,(\iota^2+\iota^3) = (-1)^2\,(-1). \end{aligned}$$

$a_{,}$ and $a_{,2}$ are in fact the roots of the equation

$$a^2 - a - 1 = 0;$$

which solved as a quadratic equation will give

$$a = \frac{1 \pm \sqrt{5}}{2}.$$

10. Another consequence of the properties of $a_{,}$ must here be pointed out.

Representing any one of the ten equations of the system by

$$y_{\mathrm{a}} + y_{\mathrm{c}} + a_{,n}\,y_{\mathrm{b}} = 0,$$

and observing that $a_{,n}$ is equal to $1 - a_{,2n}$, we see that

$$\begin{aligned} y_{\mathrm{a}} + y_{\mathrm{c}} + a_{,n}\,y_{\mathrm{b}} &= y_{\mathrm{a}} + y_{\mathrm{b}} + y_{\mathrm{c}} - a_{,2n}\,y_{\mathrm{b}} \\ &= -\,(y_{\mathrm{d}} + y_{\mathrm{e}} + a_{,2n}\,y_{\mathrm{b}}); \end{aligned}$$

a, b, .. e having, for greater simplicity, been introduced instead of the accented indices $\grave{\alpha}$, $\grave{\beta}$, .. $\grave{\varepsilon}$.

Thus it appears that p_1 and p_2 cannot be determined by means of any two equations expressible by

$$\left.\begin{aligned} y_{\mathrm{a}} + y_{\mathrm{c}} + a_{,n}\,y_{\mathrm{b}} &= 0, \\ y_{\mathrm{d}} + y_{\mathrm{e}} + a_{,2n}\,y_{\mathrm{b}} &= 0, \end{aligned}\right\} \quad . \;\; . \;\; . \;\; . \;\; . \;\; . \quad \text{(c.)}$$

which, although seemingly independent of each other, are in reality reducible to a single equation.

11. In discussing equations of the class (c.), the following definition will be found useful:—

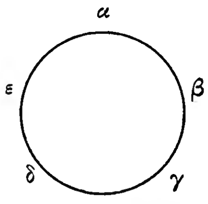
Of the two functions $y_{\mathrm{a}} + y_{\mathrm{c}} + a_{,n}\,y_{\mathrm{b}}$, $y_{\mathrm{d}} + y_{\mathrm{e}} + a_{,n}\,y_{\mathrm{b}}$, which are of the same form, and which taken together include all the roots of the equation for y, $y_{\mathfrak{b}}$ remaining fixed; the one will be said to be the *complement* of the other: and either of these functions with the letter $\mathfrak{c}$ prefixed to it as a characteristic will express symbolically the complement of that function.

12. It may now be seen, either from multiplying y_α, y_β, .. y_ε successively by a_{ι}, or from the law of the indices in the equations (b.), that the ten equations,

$$\left.\begin{aligned} y_\varepsilon + y_\beta - (\iota + \iota^4)\, y_\alpha &= 0, \\ y_\alpha + y_\gamma - (\iota + \iota^4)\, y_\beta &= 0, \\ y_\beta + y_\delta - (\iota + \iota^4)\, y_\gamma &= 0, \\ y_\gamma + y_\varepsilon - (\iota + \iota^4)\, y_\delta &= 0, \\ y_\delta + y_\alpha - (\iota + \iota^4)\, y_\varepsilon &= 0, \end{aligned}\right\} \quad .\;.\;.\;.\;\; \text{(d.)}$$

$$\left.\begin{aligned} \mathfrak{c}\,(y_\varepsilon + y_\beta - (\iota^2 + \iota^3)\, y_\alpha) &= 0, \\ \mathfrak{c}\,(y_\alpha + y_\gamma - (\iota^2 + \iota^3)\, y_\beta) &= 0, \\ \mathfrak{c}\,(y_\beta + y_\delta - (\iota^2 + \iota^3)\, y_\gamma) &= 0, \\ \mathfrak{c}\,(y_\gamma + y_\varepsilon - (\iota^2 + \iota^3)\, y_\delta) &= 0, \\ \mathfrak{c}\,(y_\delta + y_\alpha - (\iota^2 + \iota^3)\, y_\varepsilon) &= 0, \end{aligned}\right\} \quad .\;.\;.\;\; \text{(e.)}$$

where the indices in each vertical column follow the same order of succession as in the cycle



cannot, whilst α, β, .. ε remain unaltered, conduct to more than one set of values for p_1 and p_2.

But before discussing the equations (d.) and (e.), I proceed to consider properties of functions in which the elements are supposed to change places among themselves.

SECTION II.

13. Let X represent a function of n independent quantities $x_{\mathfrak{a}}$, $x_{\mathfrak{b}}$, $x_{\mathfrak{c}}$, ..; then $X\begin{pmatrix} x_{\mathfrak{a}}\, x_{\mathfrak{b}}\, x_{\mathfrak{c}} & \\ x_\alpha\, x_\beta\, x_\gamma & .. \end{pmatrix}$ or simply $X\begin{pmatrix} \mathfrak{a}\,\mathfrak{b}\,\mathfrak{c} & \\ \alpha\,\beta\,\gamma & .. \end{pmatrix}$ will express, according to a known notation, that in the function X the quantities $x_{\mathfrak{a}}$, $x_{\mathfrak{b}}$, $x_{\mathfrak{c}}$, .. have been changed into

x_α, x_β, x_γ, . . respectively. I shall term $\begin{pmatrix} a\,b\,c \\ \alpha\,\beta\,\gamma \,.. \end{pmatrix}$ the affix of substitution.

14. The number of different values which X can receive when we change the order of the elements on which it depends cannot exceed the product $1 . 2 . 3 .. n$; but the affix of substitution will admit of $1 . 2 . 3 .. n \times 1 . 2 . 3 .. n$ differently derived expressions.

Thus if we denote by Λ_1, Λ_2, . . $\Lambda_{1.2.3..n}$ the different forms or states which the five indices (1, 2, 3, 4, 5) are capable of assuming from the several changes of arrangement to which they are supposed to be subjected, the values of X may all of them be expressed by

$$X\begin{pmatrix}\Lambda_1\\\Lambda_1\end{pmatrix}, X\begin{pmatrix}\Lambda_1\\\Lambda_2\end{pmatrix}, X\begin{pmatrix}\Lambda_1\\\Lambda_3\end{pmatrix}, .. X\begin{pmatrix}\Lambda_1\\\Lambda_\nu\end{pmatrix},$$

ν denoting the product $1 . 2 . 3 .. n$; but in this system we may successively substitute Λ_2, Λ_3, . . Λ_ν instead of Λ_1: whence will result $(\nu - 1)$ other systems, each of them consisting of ν terms.

15. Suppose X to be such that the number of different values of which it is susceptible shall be less than ν.

Here certain terms in the system

$$X\begin{pmatrix}\Lambda_1\\\Lambda_1\end{pmatrix}, X\begin{pmatrix}\Lambda_1\\\Lambda_2\end{pmatrix}, .. X\begin{pmatrix}\Lambda_1\\\Lambda_\nu\end{pmatrix},$$

must be equal to each other.

Let therefore

$$X\begin{pmatrix}\Lambda_1\\\Lambda_1\end{pmatrix} = X\begin{pmatrix}\Lambda_1\\\Lambda_2\end{pmatrix} = .. = X\begin{pmatrix}\Lambda_1\\\Lambda_\mu\end{pmatrix}.$$

On submitting each of these μ expression to the substitution denoted by $\begin{pmatrix}\Lambda_1\\\Lambda_{\mu+1}\end{pmatrix}$, and observing that instead of an expression of the form

$$X\begin{pmatrix}\Lambda_1\\\Lambda_\tau\end{pmatrix}\begin{pmatrix}\Lambda_1\\\Lambda_{\mu+1}\end{pmatrix},$$

where X has been subjected to two successive substitutions, we may write

$$X\begin{pmatrix}\Lambda_1\\\Lambda_v\end{pmatrix};$$

we shall have no difficulty in perceiving that the new set of equal quantities which will arise may be represented by

$$X\begin{pmatrix}\Lambda_1\\\Lambda_{\mu+1}\end{pmatrix} = X\begin{pmatrix}\Lambda_1\\\Lambda_{\mu+2}\end{pmatrix} = .. = X\begin{pmatrix}\Lambda_1\\\Lambda_{2\mu}\end{pmatrix}:$$

which quantities are different from the former, but equal to them in number.

If we operate in the same manner with $\binom{\Lambda_1}{\Lambda_{2\mu+1}}$, $\binom{\Lambda_1}{\Lambda_{3\mu+1}}$, .. $\binom{\Lambda_1}{\Lambda_{(\omega-1)\mu+1}}$ until we have exhausted all the substitutions, we shall find that the ν values of X will be separated into ω groups composed each of them of μ terms.

Hence the number of different values which a function of n quantities may receive from all the possible substitutions of these quantities among themselves, is necessarily a submultiple of the product $1 \cdot 2 \cdot 3 \, .. \, n$, as is well known.

16. If X be affected by a series of *contiguous** substitutions,

$$\binom{\Lambda_1}{\Lambda_2}, \ \binom{\Lambda_2}{\Lambda_3}, \ \binom{\Lambda_3}{\Lambda_4}, \ .. \ \binom{\Lambda_{\mu-1}}{\Lambda_\mu},$$

we shall have generally

$$\mathrm{X}\binom{\Lambda_1}{\Lambda_2}\binom{\Lambda_2}{\Lambda_3}\binom{\Lambda_3}{\Lambda_4} .. \binom{\Lambda_{\mu-1}}{\Lambda_\mu} = \mathrm{X}\binom{\Lambda_1}{\Lambda_\mu}.$$

This is evident.

17. Let us now consider

$$\mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}\binom{\Lambda_1}{\Lambda_\nu}\binom{\Lambda_1}{\Lambda_\nu} ..$$

where the same substitution is supposed to be applied any number of times in succession to the function X.

It is obvious that a limited number, p, of such operations must bring us to an expression equal to X; and that all the expressions previously obtained will then reappear in a periodical manner.

If, in effect, we denote by $\mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}^r$ the value of X which will arise when the substitution designated by $\binom{\Lambda_1}{\Lambda_\nu}$ has been applied r times; we shall have

$$\mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}^0, \ \mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}^1, \ \mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}^2, \ .. \ \mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}^{p-1},$$

after which we shall come to the term $\mathrm{X}\binom{\Lambda^1}{\Lambda_\nu}^p$ which, by hypothesis, is equal to X or $\mathrm{X}\binom{\Lambda_1}{\Lambda_\nu}^0$; and consequently if

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* A term made use of in connexion with substitutions by M. Cauchy, to whom we are indebted for the results in (17.) and (18.), as well as for the theorem given above in (16.).

we continue to operate with $\binom{\Lambda_1}{\Lambda_v}$ we shall merely reproduce the same series of p terms disposed in the same order as before.

Thus we shall obtain, in the form of an equation,

$$\mathrm{X}\binom{\Lambda_1}{\Lambda_v}^{ap+r} = \mathrm{X}\binom{\Lambda_1}{\Lambda_v}^{r};$$

a and r representing any integers, zero included. What is termed the *degree* of $\binom{\Lambda_1}{\Lambda_v}$ is indicated by p, which is, as might easily be shown, equal to the number of the indices $\alpha, \beta, \gamma, ..$ contained in the affix when reduced to its most simple expression*.

18. An important theorem on the decomposition of substitutions here presents itself.

Observing that if

$$\binom{\Lambda_1}{\Lambda_{v'}} = \binom{\alpha\,\beta\,\gamma\,..\,\zeta\,\eta}{\beta\,\alpha\,\gamma\,..\,\zeta\,\eta}$$

we shall have

$$\mathrm{X}\binom{\Lambda_1}{\Lambda_{v'}} = \mathrm{X}\binom{\alpha\,\beta}{\beta\,\alpha},$$

where $\binom{\alpha\,\beta}{\beta\,\alpha}$ indicates an *interchange* or *transposition* of the elements α and β; and that if

$$\binom{\Lambda_1}{\Lambda_{v''}} = \binom{\alpha\,\beta\,\gamma\,\delta\,..\,\zeta\,\eta}{\gamma\,\alpha\,\beta\,\delta\,..\,\zeta\,\eta},$$

we shall have

$$\mathrm{X}\binom{\Lambda_1}{\Lambda_{v''}} = \mathrm{X}\binom{\alpha\,\beta\,\gamma}{\gamma\,\alpha\,\beta}$$

$$= \mathrm{X}\binom{\alpha\,\beta\,\gamma}{\beta\,\alpha\,\gamma}\binom{\beta\,\alpha\,\gamma}{\gamma\,\alpha\,\beta} = \mathrm{X}\binom{\alpha\,\beta}{\beta\,\alpha}\binom{\beta\,\gamma}{\gamma\,\beta}, \quad \text{(see 16.)}$$

the operation denoted by $\binom{\Lambda_1}{\Lambda_{v''}}$ being in this case equivalent to the two interchanges $\binom{\alpha\,\beta}{\beta\,\alpha}$ and $\binom{\beta\,\gamma}{\gamma\,\beta}$ taken in succession; we are at once led to infer that every substitution may be represented by a succession of interchanges.

And in effect if

$$\binom{\Lambda_1}{\Lambda_v} = \binom{\alpha\,\beta\,\gamma\,..\,\zeta\,\eta}{\eta\,\alpha\,\beta\,..\,\varepsilon\,\zeta},$$

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* Examples of such a reduction will appear in (18).

there will arise

$$X\begin{pmatrix}\Lambda_1\\ \Lambda_\upsilon\end{pmatrix} = X\begin{pmatrix}\alpha\beta\\ \beta\alpha\end{pmatrix}\begin{pmatrix}\beta\gamma\\ \gamma\beta\end{pmatrix}\cdot\cdot\begin{pmatrix}\zeta\eta\\ \eta\zeta\end{pmatrix};$$

as will be evident on reflecting that

$$X\begin{pmatrix}\Lambda_1\\ \Lambda_\upsilon\end{pmatrix} = X\begin{pmatrix}\alpha\beta\gamma\cdot\cdot\zeta\eta\\ \zeta\alpha\beta\cdot\cdot\varepsilon\eta\end{pmatrix}\begin{pmatrix}\zeta\alpha\beta\cdot\cdot\varepsilon\eta\\ \eta\alpha\beta\cdot\cdot\varepsilon\zeta\end{pmatrix}$$

$$= X\begin{pmatrix}\alpha\beta\gamma\cdot\cdot\zeta\\ \zeta\alpha\beta\cdot\cdot\varepsilon\end{pmatrix}\begin{pmatrix}\zeta\eta\\ \eta\zeta\end{pmatrix},$$

or that every substitution of the *n*th degree, *n* being any number, may be represented by a substitution of the $(n-1)$th degree followed by an interchange.

19. I proceed to consider some properties of the function

$$X\begin{pmatrix}\alpha\beta\\ \beta\alpha\end{pmatrix}\begin{pmatrix}\gamma\delta\\ \delta\gamma\end{pmatrix}\cdot\cdot;$$

which in accordance with the meaning usually attached to the symbol $(\cdot\cdot)$, I shall express by

$$X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta})\cdot\cdot;$$

$(\underset{\cdot\cdot}{\mathrm{a\,b}})$ thus denoting the same thing as $\begin{pmatrix}\mathrm{a\,b}\\ \mathrm{b\,a}\end{pmatrix}$.

20. Beginning with the function $X(\underset{\cdot\cdot}{\alpha\beta})$, it will at once be seen that

$$X(\underset{\cdot\cdot}{\alpha\beta}) = X(\underset{\cdot\cdot}{\beta\alpha});$$

$(\underset{\cdot\cdot}{\alpha\beta})$ and $(\underset{\cdot\cdot}{\beta\alpha})$ being equally expressive of an interchange of the elements α and β.

21. Passing to the function $X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta})$, we see that, if α, β, γ, δ be unequal, we must have

$$X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta}) = X(\underset{\cdot\cdot}{\gamma\delta})(\underset{\cdot\cdot}{\alpha\beta}). \quad . \quad . \quad . \quad . \qquad (\alpha.)$$

For the interchanges being, according to this hypothesis, independent of each other, it must be indifferent in what order they are taken.

But if the function in question were of the form $X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\beta\gamma})$, in which the element β is common to the interchanges, we should, exclusively of particular cases, alter the value of the function by inverting the order of the interchanges.

Thus if $\quad X = \Psi(\alpha, \beta, \gamma)$,

we shall have $\quad X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\beta\gamma}) = \Psi(\gamma, \alpha, \beta)$,

and $\quad X(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\alpha\beta'}) = \Psi(\alpha, \gamma, \beta)(\underset{\cdot\cdot}{\alpha\beta'})$,

β' being arbitrary.

Now, if $\Psi(\gamma, \alpha, \beta) = \Psi(\alpha, \gamma, \beta)(\underset{\cdot\cdot}{\alpha\beta'})$, we must take $\beta' = \gamma$. There will consequently result

$$X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\beta\gamma}) = X(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\alpha\gamma}); \quad . \quad . \quad . \qquad (\beta.)$$

in the second member of which equation the element γ, and not β, is common to the interchanges. α, β, γ are supposed to be unequal. In $X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\alpha\beta})$ an inversion of the interchanges can take place without disturbing the value of that function.

Further, it is clear from the equation (β.) that

$$X(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\alpha\gamma}) = X(\underset{\cdot\cdot}{\alpha\gamma})(\underset{\cdot\cdot}{\alpha\beta}),$$

and

$$X(\underset{\cdot\cdot}{\alpha\gamma})(\underset{\cdot\cdot}{\alpha\beta}) = X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\beta\gamma});$$

so that we shall have the three equal expressions

$$X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\beta\gamma}), \quad X(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\alpha\gamma}), \quad X(\underset{\cdot\cdot}{\alpha\gamma})(\underset{\cdot\cdot}{\alpha\beta}); \qquad (\beta'.)$$

which, if we continue to apply the equation (β.), will reappear periodically.

22. With respect to $X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta})(\underset{\cdot\cdot}{\varepsilon\zeta})$, on denoting it by Y we shall find, since $X_u(\underset{\cdot\cdot}{\varepsilon\zeta})^2 = X_u$,

$$X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta}) = Y(\underset{\cdot\cdot}{\varepsilon\zeta}).$$

If therefore we substitute* $X(\underset{\cdot\cdot}{\gamma\delta})(\underset{\cdot\cdot}{\alpha\beta'})$ instead of $X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta})$, and affect with $(\underset{\cdot\cdot}{\varepsilon\zeta})$ both members of the equation which will thence arise, we shall have

$$X(\underset{\cdot\cdot}{\gamma\delta})(\underset{\cdot\cdot}{\alpha\beta'})(\underset{\cdot\cdot}{\varepsilon\zeta}) = Y = X(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta})(\underset{\cdot\cdot}{\varepsilon\zeta});$$

that is, we can operate with the first and second of the interchanges as if the third did not exist. In like manner it might be shown that in operating with the second and third we may neglect the first.

And analogous results will be obtainable whatever may be the number of the interchanges with which X is affected.

SECTION III.

23. Returning now to the equations (d.), and designating them, in the order in which they occur, by $f_\alpha = 0$, $f_\beta = 0$, $\ldots f_\iota = 0$, we find, on inspection,

$$f_\alpha(\underset{\cdot\cdot}{\alpha\beta}) = f_\beta(\underset{\cdot\cdot}{\varepsilon\gamma}),$$

$$f_\gamma(\underset{\cdot\cdot}{\alpha\beta}) = f_\iota(\underset{\cdot\cdot}{\varepsilon\gamma}).$$

We also find, as might have been foreseen,

$$f_\beta(\underset{\cdot\cdot}{\alpha\beta}) = f_\alpha(\underset{\cdot\cdot}{\varepsilon\gamma}),$$

$$f_\iota(\underset{\cdot\cdot}{\alpha\beta}) = f_\gamma(\underset{\cdot\cdot}{\varepsilon\gamma}).$$

And it will have been observed that the two functions in-

---

* See (α.) and (β.).

volved in any of these four equations are either f_α, f_β, or f_ε, f_γ; the indices being in the former case equal to the elements of the given affix $(\underset{..}{\alpha\beta})$; and in the latter to those of $(\underset{..}{\varepsilon\gamma})$. Further, we obtain

$$f_\delta(\underset{..}{\alpha\beta}) = f_\delta(\underset{..}{\varepsilon\gamma});$$

which involves the single function f_δ, the index of which, δ, does not enter into either $(\underset{..}{\alpha\beta})$ or $(\underset{..}{\varepsilon\gamma})$.

We are thus conducted to the equation (20.),

$$f_{\mathrm{a}}(\underset{..}{\alpha\beta}) = f_{\mathrm{b}}(\underset{..}{\gamma\varepsilon}), \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{f.})$$

where a and b are such as, abstractedly of the order in which they are arranged, to be restricted to the three sets of values

$$\left.\begin{matrix}\alpha\\ \beta\end{matrix}\right\}, \quad \left.\begin{matrix}\gamma\\ \varepsilon\end{matrix}\right\}, \quad \left.\begin{matrix}\delta\\ \delta\end{matrix}\right\}.$$

24. When α instead of occurring among the elements to be interchanged remains fixed, we have

$$f_{\mathrm{b}}(\underset{..}{\beta\varepsilon}) = f_{\mathrm{e}}(\underset{..}{\gamma\delta}), \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{g.})$$

b and e here depending on

$$\left.\begin{matrix}\beta\\ \varepsilon\end{matrix}\right\}, \quad \left.\begin{matrix}\gamma\\ \delta\end{matrix}\right\}, \quad \left.\begin{matrix}\alpha\\ \alpha\end{matrix}\right\}.$$

This theorem may either be derived from the preceding one, or obtained directly from the equations (d.).

25. Finally, we obtain

$$f_{\mathrm{b}}(\underset{..}{\beta\gamma})(\underset{..}{\delta\varepsilon}) = (f_{\mathrm{c}})(\underset{..}{\beta\varepsilon})_n; \quad . \quad . \quad . \quad . \quad (\text{h.})$$

where $(\underset{..}{\beta\varepsilon})_n$ must coincide either with $(\underset{..}{\beta\varepsilon})$ or with $(\underset{..}{\gamma\delta})$, the complementary interchange relatively to f_α. With respect to b and c, if we take b successively equal to

$$\alpha, \ \beta, \ \gamma, \ \delta, \ \varepsilon,$$

the corresponding values of c will be, if $(\underset{..}{\beta\varepsilon})_n = (\underset{..}{\beta\varepsilon})$,

$$\alpha, \ \gamma, \ \varepsilon, \ \beta, \ \delta;$$

but if $(\underset{..}{\beta\varepsilon})_n = (\underset{..}{\gamma\delta})$, they will be

$$\alpha, \ \delta, \ \beta, \ \varepsilon, \ \gamma;$$

the successive values of c being in each case arranged at equal intervals in the cycle formed with the indices of the equations (d.) or (e.) taken in order.

And a similar theorem will exist for $f_{\mathrm{b}}(\underset{..}{\beta\delta})(\underset{..}{\gamma\varepsilon})$.

SECTION IV.

26. The final equation on which p_1, p_2, p_3 depend, is of the $(1 . 3 . 4)$th degree. This result, which will have been fore-

seen independently of the form of that equation, or of the nature of its roots*, may be deduced anew, in conjunction with some very remarkable properties of the roots in question, from the theorems given in the last section.

Designating by P one of the quantities p_1, p_2, we perceive from the equation (f.), or rather from

$$f_{\mathrm{a}'}\left(\underset{..}{\alpha\beta'}\right) = f_{\mathrm{b}'}\left(\underset{..}{\gamma'\varepsilon'}\right)$$

(in which β' may be supposed to be equal to any one of the four indices, β, γ, δ, ε; and γ', ε' to be each of them different from α), that the expression for P, considered as a function of $x_\alpha, x_\beta, \ldots x_\varepsilon$, will be such as to assume all its values whilst one of the roots x_α remains fixed; and $x_\beta, x_\gamma, x_\delta, x_\varepsilon$ undergo among themselves all the different changes of arrangement to which they can be subjected. It appears therefore already that the equation on which P depends cannot rise above the (1.2.3.4)th degree.

Again, it is evident from the next theorem (g.) that of the four pairs of equations to which all those which include f_α, or $y_\varepsilon + y_\beta - (\iota + \iota^4)\,y_\alpha$, are reducible,

$$\left.\begin{array}{r} f_\alpha = 0, \\ f_{\mathrm{b}} = 0, \end{array}\right\} \left.\begin{array}{r} f_\alpha = 0, \\ f_{\mathrm{b}}\left(\underset{..}{\beta\varepsilon}\right) = 0, \end{array}\right\} \left.\begin{array}{r} f_\alpha = 0, \\ f_{\mathrm{e}}\left(\underset{..}{\gamma\delta}\right) = 0, \end{array}\right\} \left.\begin{array}{r} f_\alpha = 0, \\ f_{\mathrm{e}}\left(\underset{..}{\beta\varepsilon}\right)\left(\underset{..}{\gamma\delta}\right) = 0, \end{array}\right\}$$

the first will furnish the same expression for P as the fourth; and the second as the third.

And since f_α will not, whilst α remains fixed, admit of more than $\frac{4 \times 3}{1 \times 2}$ different expressions, the number of different values which may be assigned to P cannot exceed $\frac{4}{2} \times \frac{4 \times 3}{1 \times 2}$.

P therefore will depend on an equation of the 12th degree, or rather on an equation of the form

$$(\mathrm{P}^{12} + \mathrm{B}_1\,\mathrm{P}^{11} + \mathrm{B}_2\,\mathrm{P}^{10} + \ldots + \mathrm{B}_{12})^{10} = 0;$$

in which $\mathrm{B}_1, \mathrm{B}_2, \ldots \mathrm{B}_{12}$ are symmetric relatively to $x_1, x_2, \ldots x_5$, and may consequently, as is well known, be expressed as rational functions of $\mathrm{A}_1, \mathrm{A}_2, \ldots \mathrm{A}_5$, the coefficients of the original equation.

27. To obtain the roots of this equation in terms of $x_1, x_2, \ldots x_5$, let us suppose that

$$\alpha = 1;$$

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* From considering that the equations

$$\mathrm{A}'_1 = 0, \quad \mathrm{A}'_3 = 0, \quad \mathrm{A}'_4 - \frac{1}{5}\,\mathrm{A}'^2_2 = 0$$

are of the first, third, and fourth degrees relatively to p_1, p_2, p_3.

we shall then, by making the following substitutions with β and ε,

$$\begin{pmatrix}\beta & \varepsilon\\ 2 & 5\end{pmatrix}, \qquad \begin{pmatrix}\beta & \varepsilon\\ 3 & 4\end{pmatrix},$$
$$\begin{pmatrix}\beta & \varepsilon\\ 3 & 5\end{pmatrix}, \qquad \begin{pmatrix}\beta & \varepsilon\\ 2 & 4\end{pmatrix},$$
$$\begin{pmatrix}\beta & \varepsilon\\ 4 & 5\end{pmatrix}, \qquad \begin{pmatrix}\beta & \varepsilon\\ 2 & 3\end{pmatrix},$$

find the six expressions of which f_1 is susceptible,

$$y_2 + y_5 - (\iota + \iota^4)\, y_1, \quad y_3 + y_4 - (\iota + \iota^4)\, y_1,$$
$$y_3 + y_5 - (\iota + \iota^4)\, y_1, \quad y_2 + y_4 - (\iota + \iota^4)\, y_1,$$
$$y_4 + y_5 - (\iota + \iota^4)\, y_1, \quad y_2 + y_3 - (\iota + \iota^4)\, y_1\,;$$

or, if we designate the expressions in the first of these columns by i_1, k_1, l_1, respectively,

$$i_1, \qquad \mathrm{c}\, i_1,$$
$$k_1, \qquad \mathrm{c}\, k_1,$$
$$l_1, \qquad \mathrm{c}\, l_1\,;$$

each expression in one column being the complement of the corresponding expression in the other.

By applying $\begin{pmatrix}\alpha & \beta & \gamma & \delta & \varepsilon\\ 1 & 2 & 3 & 4 & 5\end{pmatrix}$ to each equation in the system (d.), we obtain $i_1 = 0$ along with four equations which, from analogy, we shall indicate by $i_2 = 0$, $i_3 = 0$, $i_4 = 0$, $i_5 = 0$, taken in order.

Again, by applying $\begin{pmatrix}\alpha & \beta & \gamma & \delta & \varepsilon\\ 1 & 3 & 2 & 4 & 5\end{pmatrix}$ to each equation in the same system, we obtain $k_1 = 0$ followed by four equations, which abstractedly of the order in which they present themselves, will be indicated by $k_2 = 0$, $k_3 = 0$, $k_4 = 0$, $k_5 = 0$; the index of k being made to correspond with the index of the term multiplied by a_{ι}.

Lastly, by applying $\begin{pmatrix}\alpha & \beta & \gamma & \delta & \varepsilon\\ 1 & 4 & 3 & 2 & 5\end{pmatrix}$, we obtain $l_1 = 0$, and $l_2 = 0$, $l_3 = 0$, $l_4 = 0$, $l_5 = 0$.

Now, from the theorem (h.) there will result

$$i_{\mathrm{b}'} \left(\underset{\cdot\cdot}{23}\right) \left(\underset{\cdot\cdot}{45}\right) = \left(\mathrm{c}\, i_{\mathrm{c}'}\right) \left(\underset{\cdot\cdot}{25}\right)_n,$$
$$k_{\mathrm{b}''} \left(\underset{\cdot\cdot}{32}\right) \left(\underset{\cdot\cdot}{45}\right) = \left(\mathrm{c}\, k_{\mathrm{c}''}\right) \left(\underset{\cdot\cdot}{35}\right)_n,$$
$$l_{\mathrm{b}'''} \left(\underset{\cdot\cdot}{43}\right) \left(\underset{\cdot\cdot}{25}\right) = \left(\mathrm{c}\, l_{\mathrm{c}'''}\right) \left(\underset{\cdot\cdot}{45}\right)_n,$$

the second members of which will, if $\mathrm{b}' = \mathrm{b}'' = \mathrm{b}''' = 1$, reduce themselves to $\mathrm{c}\, i_1$, $\mathrm{c}\, k_1$, $\mathrm{c}\, l_1$.

If then we observe that generally (10.)

$$\mathrm{c} f_{\tau} = -f_{\tau}'\,;$$

f'_τ denoting what f_τ becomes when a_ι is changed into $a_{\iota 2}$; we shall readily perceive that the six groups of equations on which, as we have seen, all the different expressions for P depend will be reducible to the three following groups,

$$\left.\begin{array}{l} i_1 = 0, \\ i_\upsilon = 0, \end{array}\right\} \quad \left.\begin{array}{r} i_1 = 0, \\ i_\upsilon (\underset{\cdot\cdot}{25})_n = 0, \end{array}\right\} \quad \left.\begin{array}{l} i'_1 = 0, \\ i'_\upsilon = 0, \end{array}\right\} \quad \left.\begin{array}{r} i'_1 = 0, \\ i'_\upsilon (\underset{\cdot\cdot}{25})_n = 0, \end{array}\right\} \quad \text{(i.)}$$

$$\left.\begin{array}{l} k_1 = 0, \\ k_\upsilon = 0, \end{array}\right\} \quad \left.\begin{array}{r} k_1 = 0, \\ k_\upsilon (\underset{\cdot\cdot}{35})_n = 0, \end{array}\right\} \quad \left.\begin{array}{l} k'_1 = 0, \\ k'_\upsilon = 0, \end{array}\right\} \quad \left.\begin{array}{r} k'_1 = 0, \\ k'_\upsilon (\underset{\cdot\cdot}{35})_n = 0, \end{array}\right\} \quad \text{(k.)}$$

$$\left.\begin{array}{l} l_1 = 0, \\ l_\upsilon = 0, \end{array}\right\} \quad \left.\begin{array}{r} l_1 = 0, \\ l_\upsilon (\underset{\cdot\cdot}{45})_n = 0, \end{array}\right\} \quad \left.\begin{array}{l} l'_1 = 0, \\ l'_\upsilon = 0, \end{array}\right\} \quad \left.\begin{array}{r} l'_1 = 0, \\ l'_\upsilon (\underset{\cdot\cdot}{45})_n = 0, \end{array}\right\} \quad \text{(l.)}$$

$(\underset{\cdot\cdot}{25})_n$, $(\underset{\cdot\cdot}{35})_n$, $(\underset{\cdot\cdot}{45})_n$ may denote either the three interchanges $(\underset{\cdot\cdot}{25})$, $(\underset{\cdot\cdot}{35})$, $(\underset{\cdot\cdot}{45})$, or $(\underset{\cdot\cdot}{34})$, $(\underset{\cdot\cdot}{24})$, $(\underset{\cdot\cdot}{23})$, the complementary interchanges relatively to i_1, k_1, l_1. For greater uniformity the same index υ has been retained throughout the groups; the expression for P being in every case unaffected in value by writing 2, 3, 4, 5 successively instead of υ.

28. But for our purpose it will not be necessary from each pair of these equations actually to find an expression for P in terms of x_1, x_2, .. x_5. In effect, if we denote by $\mathrm{P}_{f(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\,..}$ that value of P which is derived from the pair of equations $f_\alpha (\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd}) \,.. = 0$, $f_\upsilon (\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd}) \,.. = 0$; the twelve values of which P is susceptible will be represented by

$$\left.\begin{array}{llll} \mathrm{P}_i\,, & \mathrm{P}_{i(\underset{\cdot\cdot}{25})_n}\,, & \mathrm{P}_{i'}, & \mathrm{P}_{i'(\underset{\cdot\cdot}{25})_n}\,, \\ \mathrm{P}_k, & \mathrm{P}_{k(\underset{\cdot\cdot}{35})_n}\,, & \mathrm{P}_{k'}, & \mathrm{P}_{k'(\underset{\cdot\cdot}{35})_n}\,, \\ \mathrm{P}_l\,, & \mathrm{P}_{l(\underset{\cdot\cdot}{45})_n}\,, & \mathrm{P}_{l'}, & \mathrm{P}_{l'(\underset{\cdot\cdot}{45})_n}: \end{array}\right\} \quad . \quad . \quad . \quad \text{(m.)}$$

so that without proceeding any further we may perceive that the roots of the equation $\mathrm{P}^{12} + \mathrm{B}_1 \mathrm{P}^{11} + \,.. = 0$ must be such as to admit of being distributed into three groups which are related to each other in a very remarkable manner; the second pair of roots in each group being derivable from the first pair by merely introducing $a_{\iota 2}$ instead of a_ι. Indeed the groups themselves may all of them be derived from

$$\mathrm{P}_f, \quad \mathrm{P}_{f(\underset{\cdot\cdot}{\beta\varepsilon})_n}\,, \quad \mathrm{P}_{f'}, \quad \mathrm{P}_{f'(\underset{\cdot\cdot}{\beta\varepsilon})_n}\,,$$

which will represent four roots of the equation for P.

SECTION V.

29. At first view it might be imagined that, if

$$\mathrm{V_F} = \mathrm{P}_f + \mathrm{P}_{f(\underset{\cdot\cdot}{\beta\varepsilon})_n} + \mathrm{P}_{f'} + \mathrm{P}_{f'(\underset{\cdot\cdot}{\beta\varepsilon})_n}\,,$$

the equation for V_F would not rise above the third degree. But although the eight functions,

$$\begin{array}{ll} V_F, & V_{F'}, \\ V_{F(\underset{..}{\beta\varepsilon})}, & V_{F'(\underset{..}{\beta\varepsilon})}, \\ V_{F(\underset{..}{\gamma\delta})}, & V_{F'(\underset{..}{\gamma\delta})}, \end{array}$$

$V_{F'}$ denoting what V_F becomes when f is changed into f', will be necessarily equal to each other; and consequently if f be changed successively into i, k, l, and F into I, K, L, there will arise, the index 1 remaining fixed, eight functions equal to V_I; eight to V_K; and eight to V_L; we must not conclude that $V_{F(\underset{..}{1\beta})}$, in which the index 1 is supposed not to be fixed, will be capable of coinciding with one of the three functions V_I, V_K, V_L. It is true that all the roots of the equation $P^{12} + B_I P^{11} + \ldots = 0$ may be evolved separately from a single expression whilst the index 1 remains fixed. But the question here relates to the possibility of evolving them four at a time in a certain definite order. And in effect if we examine the function $V_{F(\underset{..}{\alpha\beta})}$, we shall find that

$$V_{F(\underset{..}{\alpha\beta})} = P_{h'(\underset{..}{\delta\varepsilon})_n} + P_{g'} + P_{h(\underset{..}{\delta\varepsilon})_n} + P_g; \quad . \quad . \quad \text{(n.)}$$

wherein I suppose $f_\alpha(\underset{..}{\beta\gamma})$, $f_\beta(\underset{..}{\beta\gamma})$, . . to be denoted by g_α, g_γ, . .; $f_\alpha(\underset{..}{\beta\delta})$, $f_\beta(\underset{..}{\beta\delta})$, . . by h_α, h_δ, . .; and the accent attached to the g and h to indicate, as before, a change of $a_{,}$ into $a_{,2}$*. Whence it is clear that the four roots which com-

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* It may be easily shown that

$$P_{h'(\underset{..}{\delta\varepsilon})_n} = P_{f(\underset{..}{\alpha\beta})}, \quad \text{and} \quad P_{g'} = P_{f(\underset{..}{\beta\varepsilon})_n(\underset{..}{\alpha\beta})}.$$

Observing that $h_\alpha = y_\varepsilon + y_\delta + a_{,}y_\alpha$, and that consequently $h_\alpha(\underset{..}{\beta\delta})(\underset{..}{\gamma\varepsilon}) = \varsigma h_\alpha$, we immediately find from the theorem (h.)

$$\begin{aligned} h_\sigma(\underset{..}{\beta\delta})(\underset{..}{\gamma\varepsilon}) &= (\varsigma h_\tau)(\underset{..}{\delta\varepsilon})_n \\ &= h'_\tau(\underset{..}{\delta\varepsilon})_n; \end{aligned}$$

where σ and τ may have more than one set of values assigned to them. Hence the same expression for P may be evolved from

$$\left.\begin{array}{l} h'_{\tau'}(\underset{..}{\delta\varepsilon})_n = 0 \\ h'_{\tau''}(\underset{..}{\delta\varepsilon})_n = 0 \end{array}\right\} \text{ as from } \left.\begin{array}{l} h_{\sigma'}(\underset{..}{\beta\delta})(\underset{..}{\gamma\varepsilon}) = 0 \\ h_{\sigma''}(\underset{..}{\beta\delta})(\underset{..}{\gamma\varepsilon}) = 0 \end{array}\right\}.$$

We must accordingly have

$$P_{h'(\underset{..}{\delta\varepsilon})_n} = P_{h(\underset{..}{\beta\delta})(\underset{..}{\gamma\varepsilon})}$$

pose the function $V_{F(\underset{\cdot\cdot}{\alpha\beta})}$ do not all of them belong to one group, but must have come in pairs from two of the three groups (i.), (k.), (l.). Thus by the method of continuous substitutions we shall be conducted to an equation for V_F of the form $$(V^{15} + C_1 V^{14} + C_2 V^{13} + \,..\, + C_{15})^8 = 0,$$ in which the coefficients C_1, C_2, .. C_{15} will be rational functions of A_1, A_2, .. A_5. And the roots of the equation $V^{15} + C_1 V^{14} + \,..\, = 0$ will be expressible by

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$$= P_{f(\underset{\cdot\cdot}{\gamma\varepsilon})}$$
$$= P_{f(\underset{\cdot\cdot}{\alpha\beta})}. \qquad \text{See theorem (f.)}$$

Again, observing that $g_\alpha = y_\varepsilon + y_\gamma + a_1 y_\alpha$, and therefore $g_\alpha (\underset{\cdot\cdot}{\beta\gamma}) (\underset{\cdot\cdot}{\delta\varepsilon}) = \varsigma g_\alpha$; we find, as in the preceding case,

$$g_\tau (\underset{\cdot\cdot}{\beta\gamma}) (\underset{\cdot\cdot}{\delta\varepsilon}) = (\varsigma g_\upsilon) (\underset{\cdot\cdot}{\gamma\varepsilon})_n$$
$$= g'_\upsilon (\underset{\cdot\cdot}{\gamma\varepsilon})_n;$$

and thence

$$P_{g'(\underset{\cdot\cdot}{\gamma\varepsilon})_n} = P_{g(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\delta\varepsilon})}$$
$$= P_{f(\underset{\cdot\cdot}{\delta\varepsilon})}$$

Now if we apply the interchange $(\underset{\cdot\cdot}{\gamma\varepsilon})$ to the equations thus obtained, reflecting that, since the index n may be suppressed, there will result $P_{g'(\underset{\cdot\cdot}{\gamma\varepsilon})_n (\underset{\cdot\cdot}{\gamma\varepsilon})} = P_{g'(\underset{\cdot\cdot}{\gamma\varepsilon})^2} = P_{g'}$; we shall have

$$P_{g'} = P_{f(\underset{\cdot\cdot}{\delta\varepsilon})(\underset{\cdot\cdot}{\gamma\varepsilon})}$$
$$= P_{f(\underset{\cdot\cdot}{\gamma\varepsilon})(\underset{\cdot\cdot}{\gamma\delta})} \qquad \text{eq. } (\beta.)$$
$$= P_{f(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\delta})};$$

the last of which functions will manifestly be equal to $P_{f(\underset{\cdot\cdot}{\beta\varepsilon})_n (\underset{\cdot\cdot}{\alpha\beta})}$; since $(\underset{\cdot\cdot}{\beta\varepsilon})$ and $(\underset{\cdot\cdot}{\gamma\delta})$ are complementary relatively to f_α.

That $P_{h'(\underset{\cdot\cdot}{\delta\varepsilon})_n}$ and $P_{g'}$ do not belong to the same group, we may at once convince ourselves from considering that

$$P_f \binom{\Lambda_\alpha}{\Lambda_1} = P_i, \quad P_g \binom{\Lambda_\alpha}{\Lambda_1} = P_k, \quad P_h \binom{\Lambda_\alpha}{\Lambda_1} = P_l;$$

where $$\binom{\Lambda_\alpha}{\Lambda_1} = \begin{pmatrix} \alpha\,\beta\,\gamma\,\delta\,\varepsilon \\ 1\,2\,3\,4\,5 \end{pmatrix}.$$

And in fact if we suppose f to be changed into i,

$P_{h'(\underset{\cdot\cdot}{\delta\varepsilon})_n}$ will become $P_{l'(\underset{\cdot\cdot}{45})_n}$

and $P_{g'}$ ... ... $P_{k'}$

$$\left.\begin{array}{l} V_{I},\ V_{I(\underset{\cdot\cdot}{12})},\ \ldots\ V_{I(\underset{\cdot\cdot}{15})}, \\ V_{K},\ V_{K(\underset{\cdot\cdot}{12})},\ \ldots\ V_{K(\underset{\cdot\cdot}{15})}, \\ V_{L},\ V_{L(\underset{\cdot\cdot}{12})},\ \ldots\ V_{L(\underset{\cdot\cdot}{15})}; \end{array}\right\} \quad \ldots\ldots \quad (o.)$$

which, except in particular cases, will be distinct one from another.

30. But if we designate by $W_{f^{\backprime}(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\ldots}$ the function

$$(P_f + P_{f'})\,(\underset{\cdot\cdot}{a\,b})\,(\underset{\cdot\cdot}{c\,d})\ldots,$$

which evidently does not admit of more than six different expressions, and observe that

$$V_F = W_{f^{\backprime}} + W_{f^{\backprime}(\underset{\cdot\cdot}{\beta\iota})_n};$$

we shall see that the resolution of the equation $V^{15} + C_1 V^{14} + \ldots = 0$ may be reduced to that of a determinate equation of six dimensions,

$$W^6 + D_1 W^5 + D_2 W^4 + \ldots + D_6 = 0,$$

the roots of which will be

$$\left.\begin{array}{ll} W_{i^{\backprime}}, & W_{i^{\backprime}(\underset{\cdot\cdot}{25})_n}, \\ W_{k^{\backprime}}, & W_{k^{\backprime}(\underset{\cdot\cdot}{35})_n}, \\ W_{l^{\backprime}}, & W_{l^{\backprime}(\underset{\cdot\cdot}{45})_n}. \end{array}\right\} \quad \ldots\ldots \quad (p.)$$

31. Could we solve this equation for W, the roots of the general equation of the fifth degree might indeed be easily obtained. For from the expression for $P_f + P_{f'}$ we might deduce that for P_f or P. p_1, p_2, p_3 would thus become known. And by combining $x^3 + p_1 x^2 + p_2 x + p_3 = y$ with $x^5 + A_1 x^4 + A_2 x^3 + \ldots + A_5 = 0$, we should be conducted to

$$x = q_4 + q_3 y + q_2 y^2 + q_1 y^3 + q_0 y^4; \quad \ldots \quad (q.)$$

where $q_4, q_3, \ldots q_0$ are rational functions of p_1, p_2, p_3, or simply of P; and where

$$y = \rho t + \rho^4 u,$$

$(\rho t)^5$ and $(\rho^4 u)^5$ being, as is well known, the roots of the equation

$$(t^5)^2 + A^{\backprime}_5 (t^5) - \left(\frac{A^{\backprime}_2}{5}\right)^5 = 0,$$

and consequently admitting of being expressed in terms of $A^{\backprime}_2$ and $A^{\backprime}_5$, which also are rational functions of p_1, p_2, p_3.

SECTION VI.

32. We have not hitherto taken into consideration the forms of the functions denoted by t and u.

Now from the preceding number we perceive that

$$(\rho\, t)^5 = \mu + \alpha' \sqrt{\nu},$$
$$(\rho^4 u)^5 = \mu + \alpha'' \sqrt{\nu};$$

in which α' and α'' are the roots of the equation $\alpha^2 - 1 = 0$, and

$$\mu = -\frac{A^{\backprime}_5}{2}, \quad \nu = \left(\frac{A^{\backprime}_5}{2}\right)^2 + \left(\frac{A^{\backprime}_2}{5}\right)^5.$$

Hence

$$(\rho\, t) \times (\rho^4 u) = \rho^5 \sqrt[5]{\mu^2 - \nu} = -\frac{A^{\backprime}_2}{5}.$$

Again,

$$y = \rho\, t + \rho^4 u = \rho\, t - \frac{A^{\backprime}_2}{5\,\rho\, t} \cdot \frac{(\rho\, t)^4}{(\rho\, t)^4}$$
$$= (\rho\, t) - \frac{A^{\backprime}_2}{5\,(\mu + \alpha' \sqrt{\nu})}\,(\rho\, t)^4;$$

and, as might be foreseen,

$$y = \rho^4 u + \rho\, t = \rho^4 u - \frac{A^{\backprime}_2}{5\,\rho^4 u} \cdot \frac{(\rho^4 u)^4}{(\rho^4 u)^4}$$
$$= (\rho^4 u) - \frac{A^{\backprime}_2}{5\,(\mu + \alpha'' \sqrt{\nu})}\,(\rho^4 u)^4.$$

The equation (q.) may therefore, without altering the root x, be resolved into the two following equations,

$$\left.\begin{aligned} x &= r'_4 + r'_3 (\rho\, t) + r'_2 (\rho\, t)^2 + r'_1 (\rho\, t)^3 + r'_0 (\rho\, t)^4, \\ x &= r''_4 + r''_3 (\rho^4 u) + r''_2 (\rho^4 u)^2 + r''_1 (\rho^4 u)^3 + r''_0 (\rho^4 u)^4; \end{aligned}\right\} \quad (\text{r.})$$

$r'_4, r'_3, \ldots r'_0$ being rational with respect to $q_4, q_3, \ldots q_0, \mu, \sqrt{\nu}, A^{\backprime}_2$; and r' becoming r'' when α' is changed into α''.

33. If now in the equation

$$x = r_4 + r_3 v + r_2 v^2 + r_1 v^3 + r_0 v^4,$$

which may represent either of the equations (r.), we write $\iota^\zeta v$ instead of v; taking ζ successively equal to 0, 1, 2, 3, 4, we shall, if

$$r_n v^{4-n} = \Theta_{4-n},$$

arrive at the system of equations

$$\left.\begin{aligned} x_\alpha &= \Theta_0 + \Theta_1 + \Theta_2 + \Theta_3 + \Theta_4, \\ x_\beta &= \Theta_0 + \iota\,\Theta_1 + \iota^2\,\Theta_2 + \iota^3\,\Theta_3 + \iota^4\,\Theta_4, \\ x_\gamma &= \Theta_0 + \iota^2\,\Theta_1 + \iota^4\,\Theta_2 + \iota\,\Theta_3 + \iota^3\,\Theta_4, \\ x_\delta &= \Theta_0 + \iota^3\,\Theta_1 + \iota\,\Theta_2 + \iota^4\,\Theta_3 + \iota^2\,\Theta_4, \\ x_\varepsilon &= \Theta_0 + \iota^4\,\Theta_1 + \iota^3\,\Theta_2 + \iota^2\,\Theta_3 + \iota\,\Theta_4; \end{aligned}\right\} \quad . \quad . \quad (\theta.)$$

from which there will result

$$\begin{aligned}
\iota^{5n} x_\alpha &= \ldots + \iota^{(5+0)n}\,\Theta_n + \ldots,\\
\iota^{4n} x_\beta &= \ldots + \iota^{(4+1)n}\,\Theta_n + \ldots,\\
\iota^{3n} x_\gamma &= \ldots + \iota^{(3+2)n}\,\Theta_n + \ldots,\\
\iota^{2n} x_\delta &= \ldots + \iota^{(2+3)n}\,\Theta_n + \ldots,\\
\iota^{n}\ x_\epsilon &= \ldots + \iota^{(1+4)n}\,\Theta_n + \ldots;
\end{aligned}$$

the successive coefficients of Θ_{n+h} being ι^{5n}, ι^{5n+h}, ι^{5n+2h}, ι^{5n+3h}, ι^{5n+4h}, or $(\iota^h)^0$, $(\iota^h)^1$, $(\iota^h)^2$, $(\iota^h)^3$, $(\iota^h)^4$.

And if we reflect that the system (θ.) will remain unaltered if, substituting $\iota^a\Theta_1$, $\iota^{2a}\,\Theta_2$, $\iota^{3a}\,\Theta_3$, $\iota^{4a}\,\Theta_4$ instead of Θ_1, Θ_2, Θ_3, Θ_4 respectively, we make suitable substitutions among x_α, x_β, x_γ, x_δ, x_ϵ; and that Θ_n, or $r_{4-n}v^n$, must be such as to admit of being equated either to $r'_{4-n}(\rho\,t)^n$ or to $r''_{4-n}(\rho^4 u)^n$; we shall have little difficulty in perceiving that

$$\left.\begin{aligned}
&\left\{\Theta_n^5 - \frac{1}{5^5}(x_\alpha + \iota^{4n}x_\beta + \iota^{3n}x_\gamma + \iota^{2n}x_\delta + \iota^n x_\epsilon)^5\right\}\times\\
&\left\{\Theta_n^5 - \frac{1}{5^5}(x_\alpha + \iota^{n}x_\beta + \iota^{2n}x_\gamma + \iota^{3n}x_\delta + \iota^{4n} x_\epsilon)^5\right\} = 0.
\end{aligned}\right\}\ (\text{s.})$$

34. We are thus permitted to assume

$$\left.\begin{aligned}
\Theta'^{\,5}_n &= \frac{1}{5^5}(x_\alpha + \iota^{4n}x_\beta + \iota^{3n}x_\gamma + \iota^{2n}x_\delta + \iota^n x_\epsilon)^5,\\
\Theta''^{\,5}_n &= \frac{1}{5^5}(x_\alpha + \iota^{n}x_\beta + \iota^{2n}x_\gamma + \iota^{3n}x_\delta + \iota^{4n} x_\epsilon)^5;
\end{aligned}\right\}\ .\ .\ (\text{t.})$$

$\Theta'^{\,5}_n$, $\Theta''^{\,5}_n$ being respectively equal to $r'_{4-n}(\rho\,t)^n$, $r''_{4-n}(\rho^4 u)^n$.

35. It follows therefore that

$$\Theta'^{\,5}_n = \Theta''^{\,5}_{4n}.\ .\ .\ .\ .\ .\ .\ .\ (\text{u.})$$

Whence we deduce

$$\begin{aligned}
\Theta'^{\,5}_1 &= \Theta''^{\,5}_4, & \Theta'^{\,5}_2 &= \Theta''^{\,5}_3,\\
\Theta'^{\,5}_4 &= \Theta''^{\,5}_1, & \Theta'^{\,5}_3 &= \Theta''^{\,5}_2;
\end{aligned}$$

observing that $\Theta^5_{5a+b} = \Theta_b^5$.

SECTION VII.

36. Let us now consider Θ_n in relation to the different values of P. To indicate that the expression which may take the place of Θ_n is a function of $P_{f(\underset{..}{ab})(\underset{..}{cd})\ldots}$, I annex to Θ_n the index of P. Thus

$$\Theta_{n,f(\underset{..}{ab})(\underset{..}{cd})\ldots}$$

will denote a function of that value of P, the index of which is $f(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{c\,d})\ldots$

37. It is evident from the equations (s.) and (t.) that we may have

$$\left.\begin{aligned}\Theta_{n,f(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\,..} &= \frac{\iota^{\zeta}}{5}(x_\alpha + \iota^{4n}x_\beta + \iota^{3n}x_\gamma + \iota^{2n}x_\delta + \iota^{n}x_\varepsilon)(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{c\,d})\,..\\ \text{if}\qquad \Theta_{n,f(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\,..} &= \iota^{\zeta}\,(\Theta'_{n,f})(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{c\,d})\,..\end{aligned}\right\}\quad\text{(v.)}$$

or

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

wherein ζ and η are not independent of $(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{c\,d})\ldots$ This theorem is remarkable not only for its hypothetical character, but also for being composed of two branches.

38. If we suppose the operation denoted by $(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{c\,d})\,..$ to take the particular form $(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{a\,b})$, there will result from the first branch,

$$\left.\begin{aligned}\Theta_{n,f} &= \frac{\iota^{\zeta_1}}{5}(x_\alpha + \iota^{4n}x_\beta + \iota^{3n}x_\gamma + \iota^{2n}x_\delta + \iota^{n}x_\varepsilon)\\ \text{if}\qquad \Theta_{n,f} &= \iota^{\zeta_1}\,\Theta'_{n,f}\end{aligned}\right\}.\quad\text{(v}_1\text{.)}$$

Further, if, observing that $P_{f(\underset{\cdot\cdot}{\beta\varepsilon})(\underset{\cdot\cdot}{\gamma\delta})} = P_f$, and consequently $\Theta_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})(\underset{\cdot\cdot}{\gamma\delta})} = \Theta_{n,f}$, we suppose $(\underset{\cdot\cdot}{a\,b})(\underset{\cdot\cdot}{c\,d})\,..$ to become $(\underset{\cdot\cdot}{\beta\,\varepsilon})(\underset{\cdot\cdot}{\gamma\,\delta})$, we shall have

$$\left.\begin{aligned}\Theta_{n,f} &= \frac{\iota^{\zeta_2}}{5}(x_\alpha + \iota^{n}x_\beta + \iota^{2n}x_\gamma + \iota^{3n}x_\delta + \iota^{4n}x_\varepsilon)\\ \text{if}\qquad \Theta_{n,f} &= \iota^{\zeta_2}\,(\Theta'_{n,f})\,(\underset{\cdot\cdot}{\beta\,\varepsilon})(\underset{\cdot\cdot}{\gamma\,\delta})\end{aligned}\right\}.\quad\text{(v}_2\text{.)}$$

Lastly, since $P_{f(\underset{\cdot\cdot}{\alpha\beta})(\underset{\cdot\cdot}{\gamma\varepsilon})} = P_f$, the same branch will give

$$\left.\begin{aligned}\Theta_{n,f} &= \frac{\iota^{\zeta_3+4n}}{5}(x_\alpha + \iota^{n}x_\beta + \iota^{2n}x_\gamma + \iota^{3n}x_\delta + \iota^{4n}x_\varepsilon)\\ \text{if}\qquad \Theta_{n,f} &= \iota^{\zeta_3}\,(\Theta'_{n,f})\,(\underset{\cdot\cdot}{\alpha\,\beta})(\underset{\cdot\cdot}{\gamma\,\varepsilon})\end{aligned}\right\}.\quad\text{(v}_3\text{)}$$

And analogous results will be obtainable from the other branch (w.).

Now we see that (v$_1$.) cannot generally apply to $\Theta''_{n,f}$ (since the condition $\Theta''_{n,f} = \iota^{\zeta}\,\Theta'_{n,f}$ cannot be satisfied without inducing certain relations among $x_1, x_2, \ldots x_5$*); but that it will

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* See the equation (u.).

apply to $\Theta'_{n,f}$. On the contrary, (v_2.) and (v_3.) will be clearly applicable to $\Theta''_{n,f}$, and not to $\Theta'_{n,f}$.

If we take $\zeta_1 = \zeta_2 = 0$, we shall have

$$\left.\begin{aligned} \Theta'_{n,f} &= \frac{1}{5}\left(x_\alpha + \iota^{4n} x_\beta + \iota^{3n} x_\gamma + \iota^{2n} x_\delta + \iota^{n} x_\varepsilon\right), \\ \Theta''_{n,f} &= \frac{1}{5}\left(x_\alpha + \iota^{n} x_\beta + \iota^{2n} x_\gamma + \iota^{3n} x_\delta + \iota^{4n} x_\varepsilon\right), \end{aligned}\right\}. \quad \text{(x.)}$$

But having decided upon thus fixing the meanings of $\Theta'_{n,f}$ and $\Theta''_{n,f}$, we must be careful, in evolving particular forms either of $\Theta_{n,f(\underset{..}{\mathrm{ab}})(\underset{..}{\mathrm{cd}})\,..}$ or of $(\Theta_{n,f})\,(\underset{..}{\mathrm{a\,b}})\,(\underset{..}{\mathrm{c\,d}})\,..$, to attend to the equations (x.) as well as to (v.) or (w.).

There may also subsist

$$\left.\begin{aligned} \Theta'_{n,f(\underset{..}{\beta\varepsilon})} &= (\Theta''_{n,f})\,(\underset{..}{\gamma\delta}), \\ \Theta''_{n,f(\underset{..}{\beta\varepsilon})} &= (\Theta''_{n,f})\,(\underset{..}{\beta\varepsilon}), \end{aligned}\right\}. \quad . \quad . \quad . \quad . \quad \text{(y.)}$$

and

$$\left.\begin{aligned} \Theta'_{n,f(\underset{..}{\alpha\beta})} &= (\Theta''_{n,f})\,(\underset{..}{\gamma\varepsilon}), \\ \Theta''_{n,f(\underset{..}{\alpha\beta})} &= (\Theta''_{n,f})\,(\underset{..}{\alpha\beta}), \end{aligned}\right\}. \quad . \quad . \quad . \quad . \quad \text{(z.)}$$

$\Theta_{n,f(\underset{..}{\beta\varepsilon})}$ being equal to $\Theta_{n,f(\underset{..}{\gamma\delta})}$, and $\Theta_{n,f(\underset{..}{\alpha\beta})}$ to $\Theta_{n,f(\underset{..}{\gamma\varepsilon})}$.

SECTION VIII.

39. Again, if we examine the equation

$$\Theta_n\,(\underset{..}{\beta\varepsilon}) = \frac{1}{5}\left(x_\alpha + \iota^{n} x_\beta + \iota^{3n} x_\gamma + \iota^{2n} x_\delta + \iota^{4n} x_\varepsilon\right),$$

on designating, for the moment, $\Theta_n\,(\underset{..}{\beta\varepsilon})$ by I_n, we shall perceive that

$$\begin{aligned} \mathrm{I}_0 + \mathrm{I}_1 + \mathrm{I}_2 + \mathrm{I}_3 + \mathrm{I}_4 &= x_\alpha = \Theta_0 + \Theta_1 + \Theta_2 + \Theta_3 + \Theta_4, \\ \mathrm{I}_0 + \iota^4\mathrm{I}_1 + \iota^3\mathrm{I}_2 + \iota^2\mathrm{I}_3 + \iota\mathrm{I}_4 &= x_\beta = \Theta_0 + \iota\Theta_1 + \iota^2\Theta_2 + \iota^3\Theta_3 + \iota^4\Theta_4, \\ \mathrm{I}_0 + \iota^2\mathrm{I}_1 + \iota^4\mathrm{I}_2 + \iota\mathrm{I}_3 + \iota^3\mathrm{I}_4 &= x_\gamma = \Theta_0 + \iota^2\Theta_1 + \iota^4\Theta_2 + \iota\Theta_3 + \iota^3\Theta_4, \\ \mathrm{I}_0 + \iota^3\mathrm{I}_1 + \iota\mathrm{I}_2 + \iota^4\mathrm{I}_3 + \iota^2\mathrm{I}_4 &= x_\delta = \Theta_0 + \iota^3\Theta_1 + \iota\Theta_2 + \iota^4\Theta_3 + \iota^2\Theta_4, \\ \mathrm{I}_0 + \iota\mathrm{I}_1 + \iota^2\mathrm{I}_2 + \iota^3\mathrm{I}_3 + \iota^4\mathrm{I}_4 &= x_\varepsilon = \Theta_0 + \iota^4\Theta_1 + \iota^3\Theta_2 + \iota^2\Theta_3 + \iota\Theta_4; \end{aligned}$$

from which will result

$$\begin{aligned} 5\,\Theta_n = (3 + \iota^{2n} + \iota^{3n})\,\mathrm{I}_n &+ \{1 + 2\,(\iota^{2n} + \iota^{3n})\}\,\mathrm{I}_{2n} \\ &+ \{1 + 2\,(\iota^{n} + \iota^{4n})\}\,\mathrm{I}_{3n} + (3 + \iota^{n} + \iota^{4n})\,\mathrm{I}_{4n}; \end{aligned}$$

which connects one system with the other.

Hence if we denote

$$\begin{array}{lllll} & 3 + \iota^2 + \iota^3, & 1 + 2\,(\iota^2 + \iota^3), & 1 + 2\,(\iota + \iota^4), & 3 + \iota + \iota^4, \\ \text{by} & a, & b, & c, & d, \end{array}$$

respectively, we shall have

$$\Theta_1 = \frac{1}{5}(a\,\mathrm{I}_1 + b\,\mathrm{I}_2 + c\,\mathrm{I}_3 + d\,\mathrm{I}_4),$$

$$\Theta_2 = \frac{1}{5}(d\,\mathrm{I}_2 + c\,\mathrm{I}_4 + b\,\mathrm{I}_1 + a\,\mathrm{I}_3),$$

$$\Theta_3 = \frac{1}{5}(d\,\mathrm{I}_3 + c\,\mathrm{I}_1 + b\,\mathrm{I}_4 + a\,\mathrm{I}_2),$$

$$\Theta_4 = \frac{1}{5}(a\,\mathrm{I}_4 + b\,\mathrm{I}_3 + c\,\mathrm{I}_2 + d\,\mathrm{I}_1).$$

Elevating each of these eight functions to the fifth power, I now express, as before, Θ_n as a function of P_f; and I_n as a function of $\mathrm{P}_{f(\underset{\cdot\cdot}{\beta\varepsilon})}$; and observing that $\Theta'_{n,f} = \Theta''_{4n,f}$, and that $(\Theta'_{n,f})\left(\underset{\cdot\cdot}{\beta\,\varepsilon}\right) = \Theta'_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})}$*, $(\Theta''_{n,f})\left(\underset{\cdot\cdot}{\beta\,\varepsilon}\right) = \Theta''_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})}$, I find $\Theta'^5_{1,f} + \Theta'^5_{2,f} + \Theta''^5_{2,f} + \Theta''^5_{1,f} =$

$$\left.\begin{aligned}
&\frac{1}{5^5}\left(a\Theta'_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})} + b\Theta'_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})} + c\Theta''_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})} + d\Theta''_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})}\right)^5\\
&+\frac{1}{5^5}\left(d\Theta'_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})} + c\Theta''_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})} + b\Theta'_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})} + a\Theta''_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})}\right)^5\\
&+\frac{1}{5^5}\left(d\Theta''_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})} + c\Theta'_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})} + b\Theta''_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})} + a\Theta'_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})}\right)^5\\
&+\frac{1}{5^5}\left(a\Theta''_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})} + b\Theta''_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})} + c\Theta'_{2,f(\underset{\cdot\cdot}{\beta\varepsilon})} + d\Theta'_{1,f(\underset{\cdot\cdot}{\beta\varepsilon})}\right)^5;
\end{aligned}\right\}\text{(aa.)}$$

of which the first member is a rational function of P_f†; and the second is a function, but not a rational one, of $\mathrm{P}_{f(\underset{\cdot\cdot}{\beta\varepsilon})}$ another root of the equation for P.

In this theorem we may evidently change f successively into i, k, l; $f\left(\underset{\cdot\cdot}{\beta\,\varepsilon}\right)$ successively becoming $i\left(\underset{\cdot\cdot}{2\,5}\right)$, $k\left(\underset{\cdot\cdot}{3\,5}\right)$, $l\left(\underset{\cdot\cdot}{4\,5}\right)$. We may also write $f\left(\underset{\cdot\cdot}{\beta\,\varepsilon}\right)$ and f' instead of f. But for certain values of the letters in $\left(\underset{\cdot\cdot}{\mathrm{a\,b}}\right)\left(\underset{\cdot\cdot}{\mathrm{c\,d}}\right)$ .. the equation (aa.) will be discontinuous in consequence of the hypothetical character of the fundamental theorem (v, w.).

40. Writing in the expression for $\Theta'^5_{1,f} + \Theta'^5_{2,f} + \Theta''^5_{2,f} + \Theta''^5_{1,f}$, ${}_{\iota}\zeta^{n}\,\Theta'_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})}$ instead of $\Theta'_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})}$, and ${}_{\iota}\zeta^{4n}\,\Theta''_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})}$ instead of $\Theta''_{n,f(\underset{\cdot\cdot}{\beta\varepsilon})}$; and denoting the function which will

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* Compare the first of the equations (y.) with the equations (x.) and (v_2.).

† For $\Theta'^5_{1,f} + \ldots$ is evidently included in the form

$$\mathrm{M}+\sqrt{\mathrm{N}}+\mathrm{M}'+\sqrt{\mathrm{N}'}+\mathrm{M}'-\sqrt{\mathrm{N}'}+\mathrm{M}-\sqrt{\mathrm{N}},$$

or

$$2(\mathrm{M}+\mathrm{M}');$$

where M and M′ are rational functions of P_f.

thence arise by ${}_{\zeta}\Xi$, we shall, on taking ζ successively equal to 0, 1, 2, 3, 4, obtain the five functions ${}_0\Xi$, ${}_1\Xi$, ${}_2\Xi$, ${}_3\Xi$, ${}_4\Xi$; the first of which, ${}_0\Xi$, will be equal to $\Theta'^5{}_{1,f} + \Theta'^5{}_{2,f} + \Theta''^5{}_{2,f} + \Theta''^5{}_{1,f}$.

We may therefore form an equation of five dimensions,

$$(\Xi - {}_0\Xi)\,(\Xi - {}_1\Xi)\,..\,(\Xi - {}_4\Xi) = 0; \quad . \quad . \quad . \quad \text{(ab.)}$$

the coefficients of which, when arranged according to the powers of Ξ, shall be rational functions of $P_{f(\underset{\cdot\cdot}{\beta\varepsilon})}$. For, in consequence of the symmetric manner in which α' and α'' will enter into these coefficients, the symbol $\sqrt[2]{\;}$, as well as $\sqrt[5]{\;}$, will disappear from the calculus*.

41. Now $\Theta'^5{}_{1,f} + .. + \Theta''^5{}_{1,f}$ is, as we have already stated, a rational function of P_f, and cannot therefore, when the roots $x_1, x_2, .. x_5$ change places among themselves, receive more than twelve different values. If, indeed, we consider that $(\Theta'^5{}_{1,f} + .. + \Theta''^5{}_{1,f})(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\delta\varepsilon}) = (\Theta'^5{}_{1,f} + .. + \Theta''^5{}_{1,f})(\underset{\cdot\cdot}{\gamma\delta})$, while $P_{f(\underset{\cdot\cdot}{\beta\gamma})(\underset{\cdot\cdot}{\delta\varepsilon})}$ is not equal to $P_{f(\underset{\cdot\cdot}{\gamma\delta})}$, but to $P_{f'(\underset{\cdot\cdot}{\gamma\delta})}$; we shall instantly perceive that $\Theta'^5{}_{1,f} + .. + \Theta''^5{}_{1,f}$ must admit of becoming a root of a determinate equation of the $\frac{12}{2}$th degree expressible by

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* We see at once that we are permitted to suppose

$${}_0\Xi = \psi_1(\alpha', \alpha'') + \psi_2(\alpha', \alpha'') + \psi_2(\alpha'', \alpha') + \psi_1(\alpha'', \alpha'),$$

${}_0\Xi$ will therefore belong to the class of expressions

$${}_0\Psi(\alpha', \alpha'') + {}_0\Psi(\alpha'', \alpha').$$

There may also subsist

$$\left.\begin{aligned} {}_1\Xi &= {}^1\psi_1(\alpha', \alpha'') + {}^1\psi_2(\alpha', \alpha'') + {}^4\psi_2(\alpha'', \alpha') + {}^4\psi_1(\alpha'', \alpha'), \\ {}_4\Xi &= {}^4\psi_1(\alpha', \alpha'') + {}^4\psi_2(\alpha', \alpha'') + {}^1\psi_2(\alpha'', \alpha') + {}^1\psi_1(\alpha'', \alpha'). \end{aligned}\right\}$$

and

$$\left.\begin{aligned} {}_2\Xi &= {}^2\psi_1(\alpha', \alpha'') + {}^2\psi_2(\alpha', x'') + {}^3\psi_2(\alpha'', \alpha') + {}^3\psi_1(\alpha'', \alpha'), \\ {}_3\Xi &= {}^3\psi_1(\alpha', \alpha'') + {}^3\psi_2(\alpha', \alpha'') + {}^2\psi_2(\alpha'', \alpha') + {}^2\psi_1(\alpha'', \alpha'); \end{aligned}\right\}$$

as is evident. If, therefore, we suppose ${}_1\Xi$ and ${}_2\Xi$ to be denoted by

$${}_1\Psi(\alpha', \alpha''), \quad {}_2\Psi(\alpha', \alpha'')$$

respectively, ${}_4\Xi$ and ${}_3\Xi$ must take the forms

$${}_1\Psi(\alpha'', \alpha'), \quad {}_2\Psi(\alpha'', \alpha').$$

Whence it appears that α' and α'' are involved symmetrically in the equation (a b.).

There are some other results connected with the equation (a b.) which I should like to verify, but I have no time to do so now.

$$\left.\begin{array}{l}(\Xi - \Xi_f)\ (\Xi - \Xi_g)\ (\Xi - \Xi_h) \\ \times\, (\Xi - \Xi_{f(\underset{..}{\beta}\varepsilon)})\ (\Xi - \Xi_{g(\underset{..}{\gamma}\varepsilon)})\ (\Xi - \Xi_{h(\underset{..}{\delta}\varepsilon)}) = 0\,^*;\end{array}\right\}\ \text{(a c.)}$$

$\Xi_f, \Xi_g, \ldots \Xi_{h(\underset{..}{\delta}\varepsilon)}$ representing rational functions of $P_f, P_g, \ldots P_{h(\underset{..}{\delta}\varepsilon)}$.

42. Comparing the equations (a b.) and (a c.), we are now conducted to an equation of the form

$$\Xi_f - \mathrm{r}\,(P_{f(\underset{..}{\beta}\varepsilon)}) = 0\,;$$

in which r is expressive of a rational function; that is, we find

$$a_{11} + a_{10}\,P_f + a_9\,P_f^2 + \ldots + a_0\,P_f^{11} = b_{11} + b_{10}\,P_{f(\underset{..}{\beta}\varepsilon)} + b_9\,P^2_{f(\underset{..}{\beta}\varepsilon)} + \ldots + b_0\,P^{11}_{f(\underset{..}{\beta}\varepsilon)}\,;$$

$a_{11}, a_{10}, \ldots a_0, b_{11}, b_{10}, \ldots b_0$ being symmetric functions of $x_1, x_2, \ldots x_5$.

And comparing this equation with

$$P_f^{12} + B_1\,P_f^{11} + B_2\,P_f^{10} + \ldots + B_{12} = 0,$$

there will result

$$P_f = {}^1\mathrm{r}\,(P_{f(\underset{..}{\beta}\varepsilon)})\,;$$

where ${}^1\mathrm{r}$ will represent a rational function.

We must also have, since in the theorem (a a.) we are permitted to write $f(\underset{..}{\beta}\varepsilon)$ instead of f,

$$\Xi_{f(\underset{..}{\beta}\varepsilon)} - \mathrm{r}\,(P_f) = 0,$$

$$P_{f(\underset{..}{\beta}\varepsilon)} = {}^1\mathrm{r}\,(P_f)\,;$$

and therefore

$$P_f = {}^1\mathrm{r}\,({}^1\mathrm{r}\,(P_f)).$$

43. Similarly, on considering that $P_{f'}$ may be expressed as a rational function of P_f, and $P_{f'(\underset{..}{\beta}\varepsilon)}$ of $P_{f(\underset{..}{\beta}\varepsilon)}$ (41.), we shall see that

$$P_f = {}_1R\,(P_f + P_{f'}) = {}_1R\,(W_{f'}),$$

$$P_f = {}_2R\,(P_{f(\underset{..}{\beta}\varepsilon)} + P_{f'(\underset{..}{\beta}\varepsilon)}) = {}_2R\,(W_{f'(\underset{..}{\beta}\varepsilon)})\,;$$

and thence

$${}_1R\,(W_{f'}) = {}_2R\,(W_{f'(\underset{..}{\beta}\varepsilon)})\,;$$

${}_1R$ and ${}_2R$ representing rational functions.

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[*] The equation (a c.) must, in fact, be capable of coinciding with the celebrated equation of the sixth degree, by which Vandermonde and Lagrange were stopped in their researches on the solution of algebraic equations of the fifth degree.

And combining this equation with

$$W_{f^{\backprime}}{}^{6} + D_1\, W_{f^{\backprime}}{}^{5} + \,..\, + D_6 = 0,$$

we shall ultimately obtain

$$W_{f^{\backprime}} = {}^{1}R\,(W_{f^{\backprime}(\underset{\cdot\cdot}{\beta\,\varepsilon})})^{*}:$$

${}^{1}R$ also being expressive of a rational function.

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 by a finite combination of radicals and rational functions the general equation of the fifth degree. (31.)

44. The equation of which

$$W_{f^{\backprime}} + {}^{1}R\,(W_{f^{\backprime}})$$

is a root will evidently be of the third degree. For omitting the parentheses connected with ${}^{1}R$, we see that

$${}^{1}R\, W_{f^{\backprime}} = {}^{1}R^{2}\, W_{f^{\backprime}(\underset{\cdot\cdot}{\beta\,\varepsilon})} = W_{f^{\backprime}(\underset{\cdot\cdot}{\beta\,\varepsilon})},$$

the exponent, as is usual, indicating a repetition of an operation; and that consequently the root in question will not be affected by writing $f^{\backprime}\,(\underset{\cdot\cdot}{\beta\,\varepsilon})$ instead of $f^{\backprime}$.

We must also have

$$(W_{f^{\backprime}} + {}^{1}R\, W_{f^{\backprime}})\,(\underset{\cdot\cdot}{a\,b})\,(\underset{\cdot\cdot}{c\,d})\,.. = (V_F)\,(\underset{\cdot\cdot}{a\,b})\,(\underset{\cdot\cdot}{c\,d})\,..\,,$$

when $(\underset{\cdot\cdot}{a\,b})\,(\underset{\cdot\cdot}{c\,d})\,..$ takes the form $(\underset{\cdot\cdot}{a\,b})\,(\underset{\cdot\cdot}{a\,b})$; but not for all values of a, b, c, d, ..: since the method of continuous substitutions will not generally be applicable to processes based upon the theorem (v, w.), which is, as we must remember, hypothetical in itself.

Hence I conclude that there will be an equation of the third degree with given coefficients simultaneous with the equation $V^{15} + C_1\, V^{14} + \,..\, = 0$, which cannot be depressed below the 15th degree without inducing certain relations among A_1, A_2, .. A_5‡.

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[*] We shall have, as before,

$$W_{f^{\backprime}} = {}^{1}R\,({}^{1}R\,(W_{f^{\backprime}})).$$

[†] In a memoir "Sur une classe particulière d'Equations résolubles algébriquement." Crelle's Journal, vol. iv. p. 131.

[‡] All this will be more readily understood from considering that, in expanding any function whatever of $x+h$ two independent quantities, we do not necessarily obtain an expression symmetric with respect to x and h, and such as to admit of an interchange between them. In fact, instead of the

45. Again, if we consider that $P_{f'(\underset{..}{\beta_5})}$, $P_{f'}$ are rational functions of $P_{f(\underset{..}{\beta_5})}$, P_f, using R as the characteristic, we shall have

$$P_{f'(\underset{..}{\beta_5})} = R\,P_{f(\underset{..}{\beta_5})} = R\,{}^1\mathfrak{r}\,P_f,$$

and

$$P_{f'(\underset{..}{\beta_5})} = {}^1\mathfrak{r}\,P_{f'} = {}^1\mathfrak{r}\,R\,P_f.$$

Hence the equation for P will be such that

$$R\,{}^1\mathfrak{r}\,P_f = {}^1\mathfrak{r}\,R\,P_f;$$

and if we further consider that there must subsist an equation analogous to (aa.) when each of the remaining roots is combined with P_f, we shall find ourselves conducted to another class of equations solved by Abel in the memoir just alluded to.

46. Lastly, $\Xi_{f(\underset{..}{\beta_5})}$ being (41.) a rational function of $P_{f(\underset{..}{\beta_5})}$, we obtain (42.)

$$\Xi_f = \mathfrak{r}^{\backprime}\,\Xi_{f(\underset{..}{\beta_5})},$$

$$\Xi_{f(\underset{..}{\beta_5})} = \mathfrak{r}^{\backprime}\,\Xi_f;$$

$\mathfrak{r}^{\backprime}$ indicating a rational function.

The equation for the celebrated function Ξ will therefore belong to the same class as that for W, and must consequently admit of a similar solution. Meyer Hirsch, in endeavouring to solve this equation, alighted upon the equation of the 15th degree analogous to $V^{15} + C_1 V^{14} + \ldots = 0$.

At some future time I hope to return to this subject, and to discuss the resolution of the trinomial equation

$$x^5 + A_4 x + A_5 = 0;$$

to which very simple form the general equation of the fifth

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absolute or unconditional theorem of Taylor, we ought to have, as the discussion in the text first led me to perceive,

$$\left.\begin{aligned} &f(x+h) = f(x) + f'(x)\frac{h}{1} + f'^{2}(x)\frac{h^2}{1.2} + f'^{3}(x)\frac{h^3}{1.2.3} + \ldots \\ \text{if}\quad &\left(f'^{i+1}(x) + f'^{i+2}(x)\frac{h}{1.2} + f'^{i+3}(x)\frac{h^2}{1.2.3} + \ldots\right) \times h\binom{h}{0} = 0, \end{aligned}\right\} \text{(A.)}$$

where i may denote any term of the series 0, 1, 2, 3, ...

And thus, to take the first example which suggests itself, we see that there cannot subsist

$$\left(\frac{1}{(a-b)^2}\right)\left(\underset{..}{a\,b'}\right) = \left(\frac{1}{a^2}\left\{1 + \frac{2b}{a} + \frac{3b^2}{a^2} + \ldots\right\}\right)\left(\underset{..}{a\,b'}\right)$$

both when $b' = a$, and when $b' = b$. For the series which constitutes a factor of the equation of condition will be essentially divergent in one of these two cases: so that this equation will no longer be satisfied in virtue of the other factor $h\binom{h}{0}$.

degree may be reduced, as I showed some years ago in my *Mathematical Researches*[*].

Long Stratton, Norfolk, May 1, 1845.

LXXXI. *On the Distilled Waters of our Pharmacopœias.*

By ROBERT WARINGTON, *Esq.*[†]

THE subject of flavoured waters, prepared by distillation or extemporaneously, through the medium of carbonate of magnesia, has for some time past engaged my attention; and as it is a question of considerable importance to the pharmaceutical chemist, I am induced to lay the detailed investigation before the Society.

The formulæ given for the preparation of distilled waters in the London Pharmacopœia of 1836 is as follows:—To a specified quantity of material, whether it be essential oil, flowers, herbs, bark or berries, let 2 gallons of water and 7 oz. of proof spirit, having a specific gravity 0·920, be added, and submit the mixture to the process of distillation until one gallon shall have passed over. Presuming that the entire amount of spirit employed passes over in this operation, the resulting product will contain a quantity that will be equivalent to about 4½ oz. of rectified spirit of spec. grav. 0·838.

The Edinburgh form has rather less spirit to the proportion of materials, 2 gallons of water and 3 oz. of rectified spirit are so employed, and a gallon distilled.

The Dublin formula differs from the foregoing, in ordering half an ounce of rectified spirit to be added to each pound of the water after distillation, or in the ratio of 5 oz. to the gallon.

In the course of other investigations, some years since, I found that when a very small quantity of alcohol was added to distilled water, and the mixture kept exposed to the air for a length of time, the containing vessel being carefully covered with paper to exclude the dust, it became gradually converted into acetic acid, and, by analogy, I was led to believe that the same effect would take place in these distilled waters; the following experiments were therefore put in operation to test the accuracy of these ideas.

In the distillation of essential oils, it is well known that the water which passes over is opalescent from its being highly charged with the oil, and that when thus impregnated, it will keep perfectly sound for a great length of time. Distilled waters were prepared from various of the essential oils without

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[*] See Sir W. R. Hamilton's Inquiry on the subject in the Sixth Report of the British Association for the Advancement of Science.

[†] Communicated by the Chemical Society; having been read January 20, 1845.

the addition of any spirit, and these were taken as the basis of the following experiments.

Equal portions of the waters obtained from the oils of dill, carraway, pimento, spearmint and cinnamon were taken, and to one-half the proportion of spirit ordered by the London College was added, the other half remaining in its original state; these were all loosely corked, marked, and placed aside. After remaining six months they were examined, when the spearmint and carraway waters, with the spirit, were found to be distinctly acid, reddening litmus paper and causing an effervescence with carbonated alkalies; all the others remained perfectly sound. They were again examined after a lapse of twelve months; the acidity had increased in the previous cases, while the same waters without the spirit, remained perfectly unchanged, and evidenced no signs of acidity. On examination after another twelve months they did not appear to have undergone any further alteration.

I need scarcely mention, that in selecting the specimens specified, the object was to try the general action of ordinary exposure to the atmosphere on waters varying in their characters and sources, as other samples might have been chosen which would doubtless have become acid as soon, or even sooner than the carraway and spearmint, as for instance, rose-water, elder-flower, orange-flower, peppermint, and penny-royal waters.

Another point in favour of these distilled waters keeping, was their having been prepared from their respective essential oils, and not from the herb, seed, or bark. It is stated that these waters are liable to become mucilaginous and sour when long kept, and that the small quantity of spirit is added to prevent this; from the foregoing experiments, however, it will be evidently seen to produce a directly contrary effect, and the formulæ of the Edinburgh and Dublin Pharmacopœias will be equally in error on this point.

It is curious that of the European Pharmacopœias, that I have looked into during this investigation, not one labours under this error, their instructions being simply to distil the materials with water.

On searching back through former editions of the London Pharmacopœia, to ascertain, if possible, whether the formulæ had always been the same, and if not, when this erroneous alteration had been effected, I found that in the Pharmacopœia of 1624 two classes of distilled waters are specified, entitled simple, and compound or spirituous; the former made without the addition of any spirit, either in the process or afterwards, the latter similar to the preparations entitled spirits of the present day; after this date the error of adding small portions of spirit seems to have originated, and to have been regularly copied, with very little deviation, from edition to edition, down to the year 1836, the general directions being to distil a certain quantity of the water from a given weight of material, and then to add five ounces of proof spirit to each gallon, *that it may be preserved*, or rather I may say in many cases, to spoil it. In the edition of 1836, the proportion of spirit was increased to the formula that has been already given, and this was also the form prescribed in some of the various dispensatories about the middle of the eighteenth century.

On examining into the quantity of acid generated in the foregoing experiments, I found that half a pound of the carraway water required 4 grs. of dry carbonate of soda to effect perfect neutralization, which is equivalent to 4·45 grs. of real acetic acid. This acid was also isolated by distillation, and its identity proved by various tests.

We now pass on to the extemporaneous preparation of flavoured waters from their respective essential oils. The general formula given in the Pharmacopœia of 1836, in which it is introduced for the first time, is to triturate carefully together one drachm of the essential oil with one drachm of carbonate of magnesia, and afterwards with four pints of distilled water, and then strain. Now on examining the waters thus prepared, they are all found, without exception, to contain a notable quantity of magnesia, the proportions taken up seeming to vary with the various essential oils employed, all other circumstances remaining the same. Dr. Pereira, in his valuable 'Elements of Materia Medica,' vol. i. p. 258, speaks thus of these waters:—"The magnesia (carbonate of magnesia) effects the minute division of the oil. Moreover, when the oils possess acid properties, as the old oils of pimento, cloves and cinnamon, it probably serves to saturate them. Prepared in this way the medicated waters usually contain a minute portion of magnesia in solution: hence, by exposure to the air, they attract carbonic acid, and let fall flocculi of carbonate of magnesia. Moreover, the magnesia unfits them for the preparation of solutions of some of the metallic salts, as bichloride of mercury and nitrate of silver."

The method of examination adopted in the following experiments was to take a measured volume of the water, say 1000 grains, and evaporate it to dryness at a temperature not exceeding 212°, then to ascertain carefully the weight of the residue, redissolve it in dilute acid, and test the resulting solution. Magnesia in the form of carbonate was invariably found, together frequently, with resinous matter from the oil; this was more especially the case where oils of considerable age had been employed. By this mode of operating, the results obtained range from 0·50 gr., in the 1000 gr. measures of the water, as in peppermint-water, to 1·20 gr. in dill and pimento waters; many others were tried, which ranged between these two points. It may perhaps be as well here to mention, that it was observed very soon after the application of the heat in evaporating, that flocculi of carbonate of magnesia invariably separated.

The general opinion has been that these waters were formed in consequence of the magnesia as a base favouring the union of the oil and water, and thus implying some combination of these materials; but I was led to believe, from various circumstances presenting themselves during these experiments, that this was not in any respect the true cause, and that the carbonate of magnesia acted simply as a mechanical subdivider of the oil, if I may be allowed the expression, so that the water acted as a solvent with greatly increased effect upon an enormously extended surface of the essential oil, as is the case in distillation, where the cohesive attraction of the molecules being overcome by the influence of heat, the vapours of the oil and water rise together, are mutually diffused and pass over into the refrigerator or worm to be condensed, and thus solution is to a certain extent effected. Under this impression the same experiments were repeated, substituting for the carbonate of magnesia the kaolin or porcelain clay of Cornwall, or finely-divided silica in the form of powdered flints, and with perfect success, the resulting medicated waters being perfectly colourless, transparent, and having a full flavour of their respective oils. On evaporation no residue was obtained, except in a few instances a small quantity of resinous matter, where, as before mentioned, oils had been used which had been made a long time: this amounted in the case of pimento water to 0·50 gr.

Two questions remain now to be settled arising out of the foregoing experiments; first, does the magnesia enter into combination with the acid oils existent in some of the essential oils used, as pimento, &c.? or, secondly, is the oil simply taken into solution by the distilled water? The fact of the carbonate of magnesia separating from the solution by the application of heat, and remaining always in the state of carbonate on evaporation to dryness at a temperature of 212° F., is a strong argument in opposition to the first of these questions, as I do not think the view taken by Dr. Pereira is possible, namely, that the magnesia of the carbonate of magnesia first combines with the acid of the oil, which as a consequence, implies the evolution of its carbonic acid from its weaker affinity, and that then, by the absorption of carbonic acid from the atmosphere it is again deposited as carbonate of magnesia, thus making its affinity stronger; the second position being directly contradictory to the first.

On trying a repetition of the same experiments with the same materials, but omitting the essential oil, and going through precisely the same routine, I obtained from 0·50 to 0·70 gr. of carbonate of magnesia by evaporation as the average results of many trials; so that I consider from these data we may fairly conclude no combination of the magnesia and oily acids had taken place, but that it was simply a solution of the earthy carbonate in the distilled water; nevertheless, in many of the experiments, the presence of the different oils evidently affected the solvent power of the water and thus directly influenced the quantity of carbonate of magnesia taken up; and the resin existent in some of the old oils will account for the occasional increase of weight over this quantity, for it has been shown that this resin is soluble to some extent in water.

There can be no doubt, from the above experiments, that any insoluble substance in a fine state of division will answer the same purpose as the carbonate of magnesia much better, as, for instance, very fine porcelain clay, finely-divided silica, powdered glass, pumice-stone, &c. I am not aware how the use of carbonate of magnesia for the extemporaneous formation of these waters originated, except with the pharmaceutist as being a material generally at hand, and it is well known to have been the substance usually employed for this purpose for a great number of years, and was, I am informed, used by the perfumers to produce the same results long before its introduction for medicinal preparations.

LXXXII. *On some commercial Specimens of Green Glass.*

By Robert Warington, *Esq.*[*]

SOME short time since some green glass wine bottles were put into my hands for examination, the inner surface of which was covered with a thin film, having a dull and slightly opake appearance. The question to be solved was, the possibility of removing this so as to restore the glass to its original transparency. My first impression, from the general appearance of the bottles, was that this dullness was to be attributed to some matter mechanically adherent to the surface, and that it probably arose from their having been washed with

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[*] Communicated by the Chemical Society; having been read December 16, 1844.

foul, greasy, or soapy water; but on examination this was found not to be the case, the surface of the glass having been evidently affected by the action of some corroding agent. Portions of the glass were therefore submitted to the action of a weak solution of tartaric acid, as the acid most likely to be present in wines; and in the course of twenty-four hours, the vessel, in which the digestion was conducted, was one-fourth part full of beautiful crystals of bitartrate of lime. It was also strongly acted upon by diluted hydrochloric acid, forming a solution of chloride of calcium, and a gelatinous mass from the hydration of the silicic acid. From these preliminary experiments it was evident that a great excess of lime had been employed in the manufacture of the glass, and this had rendered it totally unfit for any of the ordinary purposes to which such bottles are applied. The glass was then submitted to analysis, and as the routine followed, in this and the subsequent case, was similar to that adopted in the examination of all such compounds, I shall not detain the Society by going through the detail; suffice it to say, that the alkalies, soda and potash were separated by the fluoric acid process, and the potash estimated from the weighed quantity of the mixed sulphates thus obtained, by means of the double chloride of platinum and potassium.

The results, calculated to the 100 parts, are as follows:—

Silica	49·00
Lime	24·75
Soda	7·25
Potash	2·00
Oxide of iron	10·10
Alumina	4·10
Magnesia	2·00
Oxides of copper and manganese	a trace
	99·20

On making additional inquiries concerning these bottles, I found that they were offered to the consumer, the wine-merchant, at a lower price than the ordinary wine bottles, and that the vendors urged strongly the superiority of their goods under the recommendation that they caused port wine to deposit its crust much sooner and firmer, that is, adhering more tightly to the bottle, than would be the case where the common glass was employed. One gentleman had been a loser to a considerable amount from bottling a large quantity of white wine in these vessels, the bottles having become clouded, and the flavour of the wine, as a matter of consequence, being materially affected. As it was a question of scientific interest to ascertain to what extent the foregoing glass differed from our ordinary green glass of commerce, as occurring in the form of wine bottles, an analysis of this was undertaken, and the following are the results, calculated as before to the 100 parts:—

Silica	59·00
Lime	19·90
Soda	10·00
Potash	1·70
Oxide of iron	7·00
Alumina	1·20
Magnesia	0·50
Oxide of manganese .	a trace
	99·30

On comparing these results with the former, it will be seen that the total amount of bases present is far less, and calculating them out as silicates, the first analysis will show a deficiency of silicic acid amounting to about 20 parts on the 100.

It was about the period of this examination being terminated, that Professor Faraday, in a lecture at the Royal Institution on the manufacture of glass mirrors, exhibited a French glass bottle which had been subjected accidentally by Mr. Pepys, jun. to the action of diluted sulphuric acid, in the proportions of 1 of acid to 10 of water; in a short time this had, by its action on the glass, produced a most extraordinary and beautiful crystallization of sulphate of lime in small, detached, and rounded pyramidal masses, tightly adherent to the surface of the bottle. In consequence of this curious action, I was induced to submit the bottles under notice to a similar mixture, and after having been filled and loosely corked they were put aside in a place of safety. In the course of two or three weeks I was surprised one morning to find the floor of the laboratory covered with wet, which had apparently run from the direction where these bottles had been deposited, and on examination they were found to be cracked in all directions, the fissures being covered with a deposit of gelatinous silica: on breaking one of these, the whole internal surface was found lined, for the thickness of about one-fourth of an inch, with a mixture of sulphate of lime and silica, the crystallization of which had evidently caused the fracture of the bottles; in some places the glass was corroded completely through its substance.

LXXXIII. *On certain Processes in which Aniline is formed.* *By* Drs. J. S. Muspratt and A. W. Hofmann[*].

NOTWITHSTANDING the laborious endeavours that have been made of late years to enable us to foretell the metamorphoses which a given organic compound should sustain under the influence of different chemical agents, still it must be admitted that we have by no means succeeded in establishing antecedently, with absolute certainty, the *modus operandi* of a decomposition; we have only arrived at some main features, giving us an insight as to the probable result of an experiment we are about to perform.

It is known, for example, that by the action of chlorine or bromine a number of equivalents of hydrogen is replaced; that phosphoric acid eliminates the elements of water; and that by treating a compound with caustic barytes, carbon and oxygen are abstracted in the form of carbonic acid. There are many instances, however, where no change occurs, or if so, it is at least quite dissimilar.

The fundamental cause of this uncertainty lies in the deficiency of our present knowledge respecting the true constitution of organic bodies.

Hitherto the simplest decompositions known are those effected by powerful agents and with the cooperation of very high temperatures. In such cases the chemical constitution of the substance decomposed influences the nature of the products far less than the composition and affinities of the elements of the decomposing body.

In regard to this some observations deserving of attention have been recently made which particularly gave origin to the researches described in the present treatise.

It has been ascertained, namely, that bodies possessing *the same composition but very different constitutions*, when submitted to the influence of strong agents, suffer frequently *the same decomposition.*

One of the most remarkable examples of this kind was lately remarked by Cahours.

Some years since Laurent[†] obtained by the action of ordinary nitric acid upon the oil of *Artemisia dracunculus*, a body containing no nitrogen, the *draconic acid*, which is expressed by the annexed formula:

$$C_{16}\,H_8\,O_6.$$

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[*] Communicated by the Chemical Society; having been read December 16, 1844.

[†] *Revue Scient.*, t. x. p. 6.

Previous to this Cahours [*] had converted in a similar manner, oil of anise (from *Pimpinella anisum*) into *anisic acid*, for the composition of which he gave the formula $C_{16}\,H_7\,O_6$.

In consequence of a discussion between Gerhardt [†] and Laurent [‡], this acid was afterwards recognised as identical with the draconic.

When distilled with an excess of barytes, the draconic or anisic acid yields a colourless liquid, named *dracole* by Laurent and *anisole* by Cahours. The transformation is very simple, two atoms of carbonic acid being removed by the oxide of barium.

$$\underbrace{C_{16}\,H_8\,O_6}_{\substack{\text{Draconic acid.}\\ \text{Anisic acid.}}} + 2\text{ BaO, giving } \underbrace{C_{14}\,H_8\,O_2}_{\substack{\text{Dracole.}\\ \text{Anisole.}}} \text{ and } 2\text{ BaO} + 2CO_2.$$

Through the beautiful investigation which Cahours [§] has lately published on the oil of *Gaultheria procumbens*, we have become acquainted with the methyle compound of salicylic acid, which is remarkable for possessing the same composition as draconic acid.

$$\underbrace{C_{14}\,H_5\,O_5\,,\;C_2\,H_3\,O}_{\text{Salicylate of oxide of methyle.}} = \underbrace{C_{16}\,H_8\,O_6}_{\text{Draconic acid.}}.$$

It was worth while to investigate in a comparative manner the action of barytes upon this body. The experiment was performed by Cahours, who ascertained that the oil of *Gaultheria procumbens* undergoes exactly the same metamorphosis as draconic acid.

The draconic acid and the salicylate of oxide of methyle are two compounds whose constitutions could not be imagined more at variance with each other; nevertheless we see that under the influence of a high temperature and the powerful cooperation of barytes, they yield exactly the same product.

After the foregoing, it appeared to us to involve sufficient interest to compare the products of decomposition which other *isomeric bodies* furnish under the same circumstances; and, indeed, some experiments made in the laboratory of Giessen have led to results which perhaps are not unworthy of communication.

Fritzsche [||], in his investigation upon the action of potash on

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[*] *Ann. de Chim. et de Phys.*, 3 ser. t. ii. p. 274, and *Annal. der Chem. und Pharm.*, bd. xli. s. 66.

[†] *Revue Scient.*, t. x. p. 184.

[‡] Ibid. p. 362.

[§] *Ann. de Chim. et de Phys.*, 3 ser. t. x. p. 327, and *Annal. der Chem. und Pharm.*, bd. xlviii. s. 6.

[||] *Bullet. Scient. de St. Pétersb.*, t. viii., and *Ann. der Chem. und Pharm.*, bd. xxxix. s. 96.

indigo, discovered, as is well known, the anthranilic acid, having the formula $C_{14}\,H_7\,NO_4$, which when distilled rapidly *per se*, or with lime, furnishes aniline and carbonic acid.

$$\underbrace{C_{14}\,H_7\,NO_4}_{\text{Anthranilic acid.}} + 2\,CaO, \text{ yielding } \underbrace{C_{12}\,H_7\,N}_{\text{Aniline.}} + 2\,CaO + 2\,CO_2.$$

Investigations recently made, in quite another department of organic chemistry, have presented us with two other bodies whose composition agrees also with the formula of anthranilic acid.

Cahours once observed, during his researches upon the oil of gaultheria, that the salicylate of oxide of methyle by the action of ammonia is converted into salicylanide, a compound *isomeric* with anthranilic acid.

$$\underbrace{C_{14}\,H_5\,O_4 + NH^2}_{\text{Salicylanide.}} = \underbrace{C_{14}\,H_7\,NO_4}_{\text{Anthranilic acid.}}.$$

Deville * had beforehand discovered, as one of the products of the distillation of tolu balsam, a carbo-hydrogen possessing the greatest similarity with Faraday's tri-carburet of hydrogen (benzin-benzole), which he called *benzoene*; and for which Berzelius † afterwards proposed the name *toluine*. This body, which according to Deville's analysis is represented by the formula $C_{14}\,H_8$, when heated with fuming nitric acid, loses, similarly to benzin, one equivalent of hydrogen, which is replaced by the elements of peroxide of nitrogen; the resulting compound is *protonitrobenzoene*,—Laurent's nitrobenzoenase and Berzelius's nitrotoluid,—and possesses likewise the same composition as anthranilic acid.

$$\underbrace{C_{14}\,H_8}_{\text{Benzoene.}} + NO_5 \text{ giving } \underbrace{C_{14}\,H_7\,NO_4}_{\text{Protonitrobenzoene.}} + HO.$$

In relation to salicylamide, it is requisite to mention here some researches antecedently performed by Gerhardt ‡. Before this substance was discovered, the products of the distillation of salicylate of oxide of ammonium were investigated by that chemist, with the view to obtain anthranilic acid, or at least aniline.

$$\underbrace{C_{14}\,H_5\,O_5 + NH_4\,O}_{\text{Salicylate of oxide of ammonium.}} = \underbrace{C_{14}\,H_7\,NO_4}_{\text{Anthranilic acid.}} + 2HO.$$

A transformation of the above kind would have possessed some similarity with the conversion of oxalate of ammonia into

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* *Annal. de Chim. et de Phys.*, 3 ser. t. iii. p. 151, and *Annal. der Chem. und Pharm.*, bd. xliv. s. 304.

† *Jahresbericht*, xxii. s. 254.

‡ *Annal. der Chem. und Pharm.*, bd. xlv. s. 24.

oxamic acid; the decomposition, however, takes place in quite another way; the salt evolves first all its ammonia, then the acid undergoes a metamorphosis, which Cahours previously foretold when working upon oil of anise, namely, that the salicylic acid separates into carbonic acid and hydrate of phenyle (phenol).

$$\underbrace{C_{14}H_5O_5, NH_4O}_{\text{Salicylate of oxide of ammonium.}} = NH_3 + 2CO_2 + \underbrace{C_{12}H_5O + HO.}_{\text{Hydrate of phenyle.}}$$

The possibility of salicylamide yielding aniline was much more likely; for if we regard the latter as the amide of phenyle, it was to be expected that, by decomposition, it would afford the amide compound of the same radical as we get by the distillation of the acid, the oxygen compound of phenyle.

$$\underbrace{C_{14}H_5O_5, HO}_{\text{Hydrated salicylic acid.}} = \underbrace{C_{12}H_5O, HO}_{\text{Hydrated oxide of phenyle.}} + 2CO_2.$$

$$\underbrace{C_{14}H_5O_4, Ad}_{\text{Salicylamide.}} = \underbrace{C_{12}H_5 Ad}_{\text{Phenamide.}} + 2CO_2.$$

We prepared, therefore, a large quantity of salicylamide, according to the method prescribed by Cahours. The crystalline substance, fused at a low heat to expel any adhering water, was introduced into the closed end of a combustion tube, the fore part of which we filled with caustic lime. After heating the lime to incipient redness, the vapour of the salicylamide was allowed to permeate it. Some moisture from the lime condensed in the recipient, upon which floated brownish drops of oil; there was besides given off a large quantity of ammonia at the same time that a considerable portion of carbon deposited in the tube.

The distillate, when mixed with an excess of hypochlorite of lime, afforded immediately the blue colour so characteristic of aniline, while a chip of fir-wood moistened with an acid and then immersed in the oil partook of a deep orange-yellow tinge.

When a portion of the distillate, however, was supersaturated with hydrochloric acid, only a minute quantity of the oil dissolved; what remained intact, from its smell and its solubility in potash, &c., manifested itself to be hydrate of phenyle. The mixture was allowed to percolate a moist filter and the filtrate distilled with hydrate of potash. During this process a fluid trickled from the retort, which gave with hypochlorate of lime the *evident* reaction of aniline. The quantity of the base obtained from this source is, however, extremely small. We have repeated the experiment in many ways, *e.g.*

instead of lime caustic barytes was taken, or the vapour of the salicylamide was conducted through a heated tube filled with pounded glass only. In nearly all these cases nothing but hydrate of phenyle was obtained; ammonia being formed at the expense of a portion of the decomposed substance, whilst the superfluous hydrogen passed off, most likely, in the state of a hydrocarburet.

Had it not been for the extreme delicacy of the hypochlorite of lime test, it would have been utterly impossible to detect aniline among the products of the distillation of salicylamide. Quite another result took place when we submitted protonitrobenzoene to the same treatment. This substance, which we had prepared in large quantities for the purpose of another investigation, soon to be communicated to your Society, was conducted over glowing lime. A dark yellow oil distilled over, which was almost completely soluble in hydrochloric acid. The undissolved portion was some protonitrobenzoene which had escaped decomposition. In order to remove it we agitated the acid solution with æther, in which medium the uncombined oil dissolved, and after taking off the æthereal solution with a pipette, we boiled the remainder to expel the last traces of nitrobenzoene.

We next distilled the purified liquid with hydrate of potash, and obtained a colourless mobile oil which collected in a homogeneous layer upon the surface of the water. This fluid was soon recognised as *pure aniline.* It would have been very easy for us to have produced a sufficient quantity of the base for a combustion, but we concluded that an analysis would be quite superfluous, as the properties of this body are so characteristically marked.

The oil obtained in the above manner was readily soluble in mineral and organic acids, and on the addition of an alkali reappeared; perchloride of platinum produced, in the hydrochloric acid solution of the base, a beautiful yellow crystalline deposit; the oil, when brought in contact with concentrated hydrochloric, sulphuric, or nitric acids, inspissated, forming a dense crystalline mass, which, when redissolved, coloured fir-wood and elder-pith intensely yellow; with hypochlorite of lime the characteristic violet colouring appeared, which was also the case with fuming nitric acid; and lastly, with a solution of chromic acid it afforded the bluish-green deposit, which Fritzsche gives as a sure indication of aniline.

All these reactions showed infallibly that the nitrobenzoene, when subjected to the united influence of lime and a high temperature, affords the same decomposition as anthranilic acid under the same circumstances.

We could not succeed in obtaining aniline by passing the vapour of nitrobenzoene through a heated tube, finding that when the temperature is not very high, the oil distils over unchanged, while, if the heat is great, the whole is consumed, forming a dense brownish-black cloud. We recognised, nevertheless, the formation of aniline in an unquestionable manner, by distilling nitrobenzoene with an alcoholic solution of potash, in order to procure the substance corresponding to Mitscherlich's azobenzide, for on adding hypochlorite of lime to the distillate which contained the red crystals of azobenzoéne (?), a beautiful purple reaction ensued.

The formation of aniline in this manner affords, perhaps, a new mode for obtaining bodies possessing basic properties, which we intend studying in conjunction with the products of decomposition of *nitrobenzide* and a few other analogous compounds.

LXXXIV. *On the Brittleness and Non-plasticity of Glacier Ice. By* ROBERT MALLET, *Esq., M.R.I.A.*[*]

IN the course of the discussion upon the mechanics and conditions of motion of glaciers, which has continued since the publication of Prof. Forbes's first papers on the subject, very much stress has been laid by most authors, and especially by Prof. Forbes himself, on what he has called the plasticity of the glacier ice. Indeed, so important a part does this assumed property play in his views, that he almost appears to rest his claims to discovery, or at least to enunciate his discovery of the nature of glacier motions, in the assertion of this plasticity of mass. A great deal of ink has been wasted in controversy respecting this assumed property, from the want of forming distinct ideas as to what this quality in question really is; and, having fixed its conditions, determining whether such were compatible with the known properties of ice. Nor have the verbal definitions of Dr. Whewell recently published in the Philosophical Magazine at all helped the matter.

It is not my present object to enter upon the question of glacier motion generally. I have for some years abstained from joining in the controversy which I was the first British author to raise, confident that ultimately the views I originally promulgated as to the great motion of translation of glaciers would be acknowledged true; and having already seen one opposing theory (that of Charpentier and Agassiz) consigned to merited oblivion, after having carried away the

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[*] Communicated by the Author, having been read before the Geological Society of Dublin.

whole of British geologists, I have little doubt of yet seeing the same fate meet the plasticity theory. In saying this, I beg not to be understood as slighting the undoubted merits of Prof. Forbes, who has been the first to demonstrate by well-devised measures and experiments the actual motion of the glacier, but who has not, that I can perceive, thrown any fresh light upon the causes of these motions, by the assertion of an ill-defined plasticity in the mass of ice, and its illustration, in common with Prof. Gordon and others, by small experiments upon pitch* and other admittedly plastic bodies, whose molecular properties are totally distinct from those of ice in any known state.

My present intention is merely to call the attention of those interested in these questions to certain ascertained facts as to the molecular constitution of bodies, which appear to me to preclude the possibility of such a property as plasticity existing in ice, and thence to show that the power of moulding its mighty mass to the sinuous, or unequal, or occasionally narrowed and wire-drawn portions of the glacier's bed is due, not to any such imaginary property, but simply to the breaking up of the rigid mass, and its continual subdivision by the various forces to which it is exposed.

And first, by plasticity is meant, I presume, "the intermobility of particles, without discontinuity, of a body beyond the range of its elasticity," the extreme case of which is fluidity. This property may be accompanied with great elasticity and elastic range, or these additional properties may be wholly wanting. Indian rubber is an example of the first, tempered clay of the second.

Now I assert that no known case can be quoted of a crystallized body possessing these properties. The very essential idea of crystallization is mutual, rigid fixation of particles, except within the elastic range, upon which the external form of any individual crystal depends, and which, break it up as you will, causes any fragment to be of the form of the original or derived from it. A single crystal or a crystalline mass may, by the application of external force, be bent or twisted or compressed within certain limits due to its particular elastic modulus; but increase the force beyond those limits, and rupture or fracture instantly takes place. This is as true of the most imperfectly crystallized bodies as of those most perfectly so. Thus a plume of amianthus or a plate of mica or selenite may be bent almost double, or a plate of elastic marble may suffer a large amount of flexure, but increase this beyond a given point and fracture results. So in the metals

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* See present volume, p. 206.—Edit.

which are so imperfectly crystallized, a load which produces a "permanent set as it is called by mechanics," produces incipient rupture, for flexure continues to increase until at last the bar breaks; and in general, the more perfect the crystallization of any body is, whether metallic, saline or simple, the more brittle it is, however great its modulus of cohesion, and irrespective thereof.

I affirm, therefore, that if glacier ice possess a crystalline structure it is impossible for it to possess any plasticity whatever; and that it *does* possess a crystalline structure optical phænomena prove, as well as those presented by the disintegration of glacier and iceberg ice. Indeed, it scarcely admits of a doubt, that ice is not capable of existing in any other than a crystallized form. I am not aware of any instance that can be given of any solid body composed of two elements, one or both of which are non-metallic, where the atomic constitution is in the same ratio as that of water, having any other structure.

But with respect to ice. Dr. Brewster has proved it to have a crystallized structure in plates as removed from the surface of frozen water; we see it shoot into crystals upon a freezing pool; it forms in crystals as hoar frost, and we see it fall in crystals as snow, and the mass of the glacier is made up of snow crystals cemented into a mass by freezing together; and are we to suppose that in this act of consolidation these minute snow crystals do not continue and impart their own crystalline forms to the water frozen between them, but contrary to all analogy assume them to lose their own crystalline structure without being liquefied, and not aggregate to themselves fresh matter of their own kind, to fill with crystals similar to themselves the interspaces amongst them?

I conceive, then, that what I have thus briefly and impartially stated destroys the possibility of assuming glacier ice to possess plasticity of any kind, without violation of all sound reasoning upon the known molecular properties of ice and all other crystallized bodies.

Nor is it necessary to call in the aid of such a baseless hypothesis to account for the most extreme and distorted cases of glacier motion. Sufficient subdivision of the whole mass to give enough of intermobility to its integrant masses is all that is requisite to enable the enormous procession of colossal fragments to be pushed, or slide and stagger, or be lifted through the narrowest gorge of the glacier valley.

Enormous as is the magnitude of each block of ice between the crevasses at any given locality, it is a small piece compared with the whole moving mass, with the forces engaged, and with the cavity in which it moves; and thus the congregation of these huge and rigid blocks moves forward and adapts itself more or less completely to the twistings and changings of size and shape of the glacier valley, with much of the outward character of a fluid or a plastic mass; but there is no more real plasticity than there is in a cartload of road metal when shot from the cart, which, notwithstanding the rigidity of every broken angular stone, assumes the general character and outline which a mass of plastic mud would if shot from the same cart.

This power, then, of adapting itself to its bed, viz. by being broken up into fragments of such a size as to be small in proportion to the whole mass and the cavity in which it moves, it is due to myself to remark that I clearly perceived, and have distinctly enunciated in my original paper on the mechanism of glaciers, published in the Transactions of the Geological Society of Dublin in 1838. The passage will be found in pages 10 and 11 of that paper, which, having been published fully nowhere else, and that journal having a very limited circulation, has I am aware become comparatively little known.

It appears strange how Prof. Forbes, after remarking upon the annual re-appearance of the same crevasses in the same spots of the glacier, should have conceived any such thing as plasticity necessary to account for the motion of translation; and equally strange does it appear, how obstinately both he and Mr. Hopkins reject the idea of any essential aid being had from the lifting of the masses by hydrostatic pressure, although both admitting (I believe) that there may be, and most probably are, great cavities in the bottom of glacier valleys by which the ice becomes hooked on to its bed, and from which neither of these gentlemen's views seem capable of detaching it, without the aid of this, which I believe to be not the sole but one of the most important causes of the motion of translation of glaciers.

It is quite true, as has been said by Prof. Forbes and others, that Saussure noticed this cause of motion; so he did, incidentally, in one single sentence in his three quarto volumes; but it is perfectly plain that Saussure looked upon the lifting of the ice by hydrostatic pressure solely as a possible and contingent event, and not as an efficient cause of glacier motion in daily and hourly action.

While, on reviewing my paper of 1838, I can now mark some mistakes, and that I perhaps attached an undue importance to some minor phænomena,—an error into which those who, like myself, have no opportunity of obtaining measures of the phænomenon which they wish to explain are prone to fall,—I am nevertheless impressed with the belief that the main views I then enunciated as to glacier motions are true; they appear to be supported by Mr. Hopkins's researches, and I am not aware that they conflict with any truth that Mr. Forbes has advanced.

To the latter philosopher will ever belong the merit and the good fortune of having been enabled first to apply accurate measures to glacier motion; and by fixing our data and removing a mass of rubbish, done more than any living man to advance our knowledge of this subject.

Dublin, April 9, 1845. ROBERT MALLET.

Note on the preceding Paper.

Since the above paper was written and read, it has been urged as a forcible argument against the views I therein put forward, as to the impossibility of plasticity existing in a crystallized mass, that the mass of glacier ice may be viewed as a sort of crystalline sponge, penetrated by water held between its interstices in all directions and at all depths, and that, although a perfectly crystallized mass cannot be also plastic, such a compound structure may admit of plasticity. This I believe is the most that can be said against my views above, and upon this I would remark, that it is mere matter of assumption that the mass of glacier ice is throughout penetrated in any such way by watery spaces. Hand specimens show nothing of the sort at whatever depth taken, nor have the experiments made as to capillary fissures at all proved either their universality as to depth or even surface, much less their size being sufficient to permit plastic motion. Thawing ice has never been found in other circumstances in any analogous state; on the contrary, it is well known that when floating icebergs descend into southern latitudes, and are in a thawing condition, in place of being plastic they are throughout their whole masses so pre-eminently brittle, that the firing of a gun is often sufficient totally to dislocate and shatter their scarcely coherent crystals. Nor does the unsound and *slushy* surface ice found in our own or in Arctic climates, when in a thawing condition and greatly penetrated with water, present any trace of plasticity.

Again, it is confessed on all hands that the whole glacier mass must be viewed as full of dislocations, and that the great fragments are more or less insulated masses. These are often 100 or even 500 feet in depth. Is it then conceivable, that if such masses were really plastic they should show their plasticity only in the direction of horizontal or inclined motion?

rather, is it not certain that they would be pressed down more or less under their own enormous weight? that their sides, where free, would bulge outward, filling up the crevasses between, and presenting to the eye all the outline of plastic masses, in place of the keen, cutting, cliff-like, and often overhanging sides which these invariably show?

It does appear singular to me, how one of Prof. Forbes's great perspicacity can lay any stress, or ground any argument upon such experiments, *en petit*, as he and others have made on plastic pitch, &c. in artificial troughs of a few inches or feet in length. These bodies are confessedly plastic, which it remains to be shown that ice is; and all these experiments only prove (if they prove anything) that it is possible to simulate and represent in some degree to the eye the general form and motion of a glacier by that of a small plastic body; but this is only to show that all bodies which flow, viz. which move in a continuous train or stream, no matter how, do present certain characters in common: this is true of a cartload of broken stone or road metal, when shot out, as much as of a mass of plastic mud. In a word, such experiments are illustrations in which there may be accidental and superficial resemblances, but in which the conditions are so entirely different, that there is no analogy and therefore no base for argument.

If an advocate of the plasticity theory will crystallize a mass of soda or of alum, or some other cheap salt, in a form to be interpenetrated with water or with water and foreign matter, —as clay, sand, &c.—which can easily be done, and show that this mass, in pieces of a few feet in length even, has any traces of plasticity, the experiment would bear more upon the question. It would remain however to show that glacier ice was in an analogous condition.

In conclusion, I cannot help remarking two instances of approximation to my previously enunciated views in the most recent papers of Prof. Forbes and of Mr. Hopkins which I have seen, viz. in the Phil. Mag. for this month, and in Jameson's Journal, last number.

In treating of the motion of translation of secondary glaciers, Prof. Forbes for the first time admits hydrostatic pressure to a prominent place as a cause of their motion, or rather the want of it, by want of water, as the cause of their slow motion; while Mr. Hopkins urges in a forcible manner the internal freedom of motion of the whole glacier's mass as due to the dislocation of its parts and the reproduction of crevasses.

It seems to me that much light might possibly be thrown upon the subject of the coloured bands of glacier ice (which I am strongly impressed have a crystalline origin) by experiments made on a large scale with such masses of crystallized salts as I have above alluded to, subjected for some time to alternations of temperature, pressure, &c. The analogy would be closer if salts belonging to the same crystalline system as water were chosen for experiment. In a paper of mine lately published by the Geological Society of Dublin, I have drawn attention to a fact of crystallization which seems to bear directly on this point, viz. that in all crystallized or crystallizable bodies, if they are suddenly cooled, or *chilled* as it is technically called, from a state of fusion or solution, by a plane surface of low temperature, the crystals in forming arrange themselves perpendicularly to the refrigerating plane: thus, if speculum metal, or cast iron, or indeed any crystallizing metal be cast in a thick metallic mould, so as suddenly to chill them, their crystals are all found or fractured perpendicular to the faces of the mould.

The same has been remarked of the crystals of trap rock in dykes, and generally of vein stones. It is also true of highly concentrated solutions of salts, which on crystallizing form one mass, like water in freezing into ice; and conversely, if a crystallizable body be heated near to but not up to its fusing-point, by the application of heat in one plane, a crystalline structure perpendicular to the plane is immediately developed. Thus, if a cube of lead be laid on or held against a hot plate of any substance until it is heated to within a few degrees of fusion, it becomes brittle (not *plastic*, observe), and on breaking it is found to have a crystalline structure, the direction of the crystals being perpendicular to the heating surface; so that in general *change of temperature beyond certain limits developes in crystallizable bodies a crystalline structure in the direction of transmission of the wave of heat, whether into or out of the mass of the body.*

This it seems to me will probably be ultimately found to be concerned in the formation of the coloured bands (viz. bands of variable crystallization) in glacier ice; and if this be true, it renders the plasticity of the mass the less likely, in proportion as its perfect crystallization becomes more certain.

The perfect brittleness and looseness of cohesion of crystalline bodies when heated nearly to their melting points, as above illustrated in the case of metals, and as observed in thawing icebergs, suggests also one circumstance of rapid degradation, and of motion of translation in glaciers, which connects itself with Mr. Hopkins's views, but has not, that I am aware of, been noticed by him; namely, that the mass of glacier ice at the bottom, either in contact with the glacier valley, with the subglacial streams, or hanging cavernous above them, must be in this brittle, loose and dissolving condition, and hence must crumble away under pressure from above with great rapidity, while the crystalline particles thus pulverized (so to speak) must be washed away and melted in the subglacial water, and thus a source of very rapid descent added to all others.

Dublin, April 25, 1845. ROBERT MALLET.

LXXXV. *Remarks on* Professor Forbes's *Reply*. *By* W. HOPKINS, *Esq*., *M.A.*, *F.R.S.*, *&c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I MUST request of you permission to offer a few remarks on Prof. Forbes's reply to my letters on glaciers.

The Professor says that I have misrepresented his theory. The first proof he offers of it is, that I have dismissed it somewhat too summarily in my second memoir on glacial motion. This may imply an inadequate appreciation of the theory, in the estimation of its author, but ought not assuredly to be considered as a misrepresentation of it.

Again, the Professor cites, as another instance of misrepresentation, my observations respecting the inadequacy of his theory, even if his mechanical reasoning were admitted, to account for the observed convexity of lines of fissure. His curves of structure have been uniformly represented as elongated loops, of which the convexity is turned towards the lower end of the glacier; and he has said (I have quoted the words in my last letter) that "crevasses will naturally occur, *crossing the structure at right angles*," conceiving that he has previously demonstrated that the directions of the loops will be those of maximum tension*. In my remarks on these statements, I asserted that according to this theory the inclination of the crevasses to the axis of the glacier must be *less for points nearer than for those more remote from the axis*, as represented in figure 1 in my Third Letter, and copied by Prof. Forbes in his reply. That such is the case is too obvious to require a moment's consideration, and yet the Professor exclaims, How could I suppose him so dull as not to be aware that, in the centre of the glacier, the crevasses and the loops must be *parallel*? My reply is, that I had his own explicit declaration for it, that the loops and crevasses were

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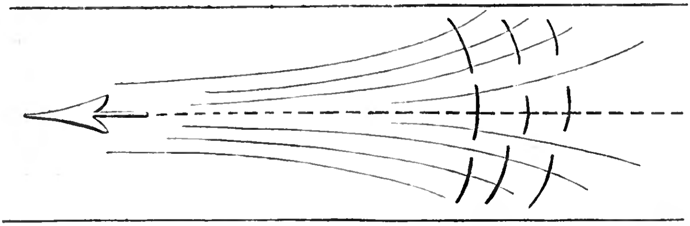
* There is a manifest oversight in the note at the bottom of page 378 of the Travels, where it is stated that the structural bands are *perpendicular* to the lines of greatest tension.

perpendicular to each other. He has given a diagram to illustrate his views, but I never saw lines intended to establish a geometrical proposition so singularly ambiguous. I allude to the lines in fig. 2 of the Professor's reply, representing the lines of structure. To preserve their consistency with all those of the same family which have preceded them elsewhere, they ought to turn gradually towards the axis and finally cross it at right angles; but they would betray the cause for which they are here called into requisition, unless they curved themselves the contrary way, as in the annexed diagram, so as never to cross the axis at all. In what direction the lines in the Professor's diagram are intended to curve I know not, but it is only when they curve as in the above diagram that the crevasses perpendicular to them can be such as represented in this or Prof. Forbes's figure. It is not for me to reconcile this form of the structural curves with that which the Professor has always assigned to them. But it was scarcely worth while perhaps to dwell at all on this point, since the Professor has given up his explanation of the formation of the crevasses, by the admission of the error he had made in concluding that the direction of maximum tension coincided with that of the loops. It was in controverting his opinion on this point that I was led to make the remarks which the Professor has incorrectly represented as a misstatement of his theory, except so far as he may himself have misstated it.



Again, the Professor asserts that I have misrepresented facts, because I have asserted that no experiments have been made to prove that the plasticity of glacial ice is really greater than common inspection might lead us to suppose. I mean by that assertion that no such experiments have been made independent of the phænomena to be accounted for. Different explanations are given of those phænomena by two different theories. According to one, they indicate a property of glacial ice which is alone sufficient to account for glacial motion; according to the other, they are only the necessary consequences of a totally independent cause of motion. How then, in discussing the contending claims of the two theories, can these phænomena be appealed to as experimental tests of the truth of either? I refer your readers to the whole passage in my Third Letter (Phil. Mag. for March, p. 248)*; I reassert the truth of what is there stated.

So much for my misstatements. The first is no statement at all; if the second be a misstatement, it is due to the Professor himself; and the third I reassert as a correct statement.

The general problem of glacial motion as considered by Prof. Forbes and myself, naturally resolves itself into two parts—the cause of motion, and the internal tensions, pressures, &c. which result from it. In considering this latter part of the problem, Prof. Forbes arrived at the conclusion that the direction of maximum tension at any point coincided with that of the *loop* through that point, whence also he deduced another conclusion, viz. that the same direction also coincided with that in which there is the greatest tendency in one particle to slide past another. On these two conclusions his whole theory of the laminar structure entirely rests; if they be untenable his theory ceases to exist. How then stands the matter at present? The first conclusion is given up as an oversight, and the correctness of my investigation on this point is tacitly admitted. The second conclusion is not explicitly surrendered, though not retained with any explicit declaration in its favour. In fact, if the correctness of my results on the first point be allowed (and they are too obvious to be controverted), I defy any ingenuity to disprove the accuracy of my conclusions on the second point. We might as well deny the proposition of the lever, while we allow the parallelogram of forces. No hypothesis as to the properties of matter is made in one part of the investigation which is not involved in the other; so that if Prof. Forbes would be consistent with himself, he must either show that I have committed some mathematical blunder, or he must admit a second oversight in his own reasoning as obvious as the first.

But, says the Professor, my writings on the subject are so *voluminous* that he cannot undertake to go through them. Now the little pamphlet comprising the four letters on glacial motion which you have done me the honour to publish, and containing the full development of my views on the subject, is now lying before me in juxtaposition with the ROYAL OCTAVO of Prof. Forbes, and when I compare the lean and jejune look of my own volume with the portly and royal bearing of its neighbour, I cannot but think the Professor's taunt of my having written so much either a piece of the most obvious irony, or as one of the most unreasonable charges that con-

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* See also the Introduction to my first memoir.

troversy ever provoked. More especially is it unreasonable when considered with immediate reference to the two primary conclusions above-mentioned of the Professor's mechanical reasoning; for after having satisfied himself of his having made one oversight, the single paragraph following fig. 2 of my Third Letter, could not fail to convince him that he had also made a second. Having admitted the one, he must necessarily admit the other.

The Professor, in the commencement of his reply, makes a formal disclaimer against all deductions from hypotheses respecting the constitution of matter and forces acting on its integrant parts. But on what are the forces we are here concerned with to act if not on the integrant parts of the mass? and what are these hypotheses against which the Professor lifts his voice of warning? It was his duty, I think, to have informed your readers. As he has not done so, I will state them. It has been assumed, then, that matter may be compressed or extended, and that the greater the extension or compression, the greater *cæteris paribus* will be the compressing or extending force. Such are the properties on which it would seem nothing but "tottering fabrics" can be raised. Does Prof. Forbes intend to intimate that his own mechanical reasoning does not involve the same hypotheses, or that the internal forces called into action, according to his views, do *not* act on the integrant molecules of the mass? It must be a remarkable case of mechanical action if they do not. Or does he mean to assert that my investigations involve, more than his own, vague and uncertain hypotheses respecting the nature and laws of molecular attractions? If he does, why did he not point out where such hypotheses enter? and if he does not, how will he justify his classing my mechanical investigations on this subject with that large class of researches which do professedly involve doubtful suppositions respecting the ultimate constitution of matter? It may be that the Professor has written under the unconscious influence of controversy, or possibly with the want of that entire familiarity with the mathematical investigations of mechanical problems, which can only be acquired by an almost incessant application to the subject, scarcely consistent perhaps with his devotion to those experimental researches by which he has made such valuable contributions to science. Be that as it may, I consider that he has endeavoured to get rid of investigations which he has not ventured to approach in detail, by a general representation respecting the basis on which they rest, unsupported by the slightest evidence; and therefore it is that I speak strongly on this point. When men uniting little modesty with little knowledge set up their own vague conclusions against the demonstrative results of the mathematician, we pass them in silence; but when the applicability of such results is denied by men of totally different character, men of high scientific position and acquirement, we expect from them not vague and general assertion, but explicit reasons for their objections. This is what Professor Forbes has not even attempted to do with reference to my investigations on the subject before us. Still had he insisted on the inadmissibility of all theoretical investigations, he might have been at least consistent; but while he denies the admissibility of my solutions, he would call upon us to repose our faith in his own, involving as they do the same properties of matter, and leading to conclusions demonstrably erroneous. I assert that Prof. Forbes has given no mechanical solution of the problem before us as regards the internal pressures and tensions called into action by the peculiar motion of a glacier, and therefore no physical explanation of the laminar structure. He may assert, that in allowing the probability of that structure being due *in some way* to the motion, I am really admitting the correctness of his theory; I can only reply, that if our views are mechanically the same, I should be at a loss to say what constitutes a real difference of mechanical views on the subject.

Hitherto I have spoken with reference to the mechanism of glacial motion independently of its cause. In my first memoir my object was to explain how the motion of a glacier might take place according to the sliding theory, hypothetically regarding glacial ice as having only a small degree of plasticity, and a glacier to be a dislocated mass. In my second memoir I endeavoured to point out certain characters which would distinguish the motion, under other hypotheses respecting the constitution of the glacial mass. I made four different hypotheses, and my conclusion was, that none of them could be correct, and yet this memoir has been cited as a proof that I had altogether modified my views on the subject and almost adopted those of Professor Forbes, though the Professor at the same time complains of my having, in the same memoir, so summarily dismissed his whole theory. I could not however both adopt his views and summarily reject them, and I might as well have been charged with the simultaneous adoption of all the four hypotheses I had made, as with that of any one of them in particular. They were made with the view of testing their admissibility, but rejected as leading to conclusions inconsistent as I believed with observation. Thus I considered the hypothesis of the whole or much the greater part of the motion of a glacier being due to its plasticity (no part, or a very small part of the motion being due to its sliding) as inadmissible, because if that hypothesis were true, it would follow that the general state of a glacier would be that of longitudinal compression, *especially during those seasons of the year when its motion is greatest.* This I consider inconsistent with the *general* existence of transverse fissures in the *central portion* of a glacier. With respect to those in the lateral portions, I have distinctly shown (Art. 20. Second Letter), that they are not inconsistent with the existence of longitudinal compression. Local causes may doubtless produce transverse fissures in the central portion, but they must so act as to produce *extension* there, for the formation or continued existence of a transverse fissure under longitudinal compression is a mechanical contradiction. Professor Forbes has endeavoured to meet this argument by a reference to M. Agassiz's admeasurements on the glacier of the Aar, which undoubtedly give a smaller velocity at the lower extremity compared with that at higher points than I should have anticipated. But still let us take the evidence of this glacier in conjunction with that of the Mer de Glace. In the former, it appears, the *mean** state is that of compression, and if there be not an entire absence of transverse fissures in its central portion, they are I believe fewer in number and smaller than those in any other large glacier, and must be referred to local causes. But who would venture to refer the crevasses of the Mer de Glace to local causes? And how will Professor Forbes's theory account for them by any general cause, or for the elongation of that glacier during the summer, which his measurements and observations have so clearly established†? He has in fact assigned no *general* cause whatever for the very general phænomena, in most glaciers, of transverse fissures in their central portion.

Prof. Forbes asserts that "plasticity is a quality of glaciers without which they would remain stationary, or descend in avalanches." I shall not renew any argument on this point. I will merely state, that whether we consider the question with

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* The evidence actually required is the relative motions of the upper and lower extremities of the glacier when the absolute motion is greatest.

† I have never been able to understand how Professor Forbes reconciles his theory, which always implies that the mass is urged by a *pushing* force, with his impressions respecting the *elongation* of glaciers, as expressed in the following and other passages: ". . . . . a circumstance mentioned by M. Elie de Beaumont, which is so true that one wonders it has not been more insisted on, viz. that a glacier, where it descends into a valley, is like a body pulled asunder or stretched, and not like a body forced on by superior pressure alone."

reference to experimental facts or mechanical reasoning, I deny altogether the validity of the Professor's conclusion.

I have already stated in my Third Letter that the ultimate test of the sliding and plastic theories must be sought in observations on the relative motions of the upper and lower surfaces of a glacier. The claims of the two theories would thus be decided beyond dispute. Accurate observations are also required to ascertain the form which a *continuous* straight line drawn on the surface of a glacier and perpendicular to its axis, will assume by the more rapid motion of its central portion. Will it be deformed into a *continuous* loop, like fig. 1, or into a *discontinuous* one, like fig. 2.? Such observations would decide the degree in which the greater central motion is due to the flexibility or plasticity of glacial ice, and the degree in which it is attributable to the dislocation of the general mass. Observations of this latter kind continued only for a few days would be of no value; they ought to be continued for as long a time as possible. Observations of both kinds are become essential in the present state of glacial theories, and would do much more towards settling the questions at issue respecting the cause of glacial motion, than any further controversy, which on my part will terminate with this letter. With respect to the mechanics of the problem, there can be little need of further controversy, since nearly every proposition maintained on one side has been proved erroneous, and almost every one on the other remains unquestioned.

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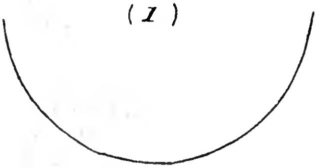

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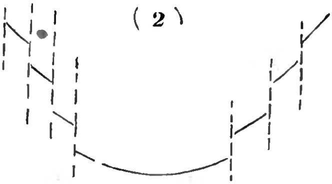

I would make one additional remark. It has been intimated that I have not been ready to do justice to Prof. Forbes with respect to his glacial theory. Is it meant that I ought to have written a eulogy on a theory which I believed to be erroneous? While I dissent from his theory, no one has ever been more ready to allow the merit of the Professor's researches on this subject as well as on many others which have engaged his attention; and if in the course of this controversy a word has escaped me inconsistent with the high respect due to him both as a man and a philosopher, it will be to me a matter of deep regret.

Your obedient Servant,

Cambridge, May 24th, 1845.

W. Hopkins.

LXXXVI. *On the rule for obtaining the Atomic Volume of Elementary Substances.* *By* BENJAMIN BOUGHEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I FIND it stated in several works on the subject, that by dividing the atomic weight of any element by its specific gravity, the result will be the relative volume of its ultimate atom or its atomic volume. But let us examine whether this rule in its present state is constructed on sound principles. If we take for example the case of oxygen gas, its specific gravity is (water 1·000) about ·00134; now if we divide this by 8 (its atomic weight), the result will be 5970. Again, take the same oxygen at the temperature of 32°, and also at 212°, and let its specific gravity at these temperatures be treated as before, and we obtain for the atomic volume at 32°, 5500, and at 212°, 7700. From these results it would appear that an atom of oxygen fills a space at 32° equal to 5500; at 60°, 5970; and at 212°, 7700. But the most generally-received opinion of an atom is, that it is incapable of expansion or contraction, and that when heated the atoms themselves do not undergo expansion, but are only further separated from each other. But how must we reconcile the results just given with this opinion? It thus appears that the rule just stated is fallacious in its present state, but it is quite evident that the fault lies, not with the rule itself, but with the specific gravity.

The question then arises, how are we to avoid these objections? It may be stated that they may be avoided by taking the specific gravities all at the same temperature; but are all bodies in the state at the same temperature? Are not some solid, some liquid, and others gaseous? and are not even these states very irregular in their degree? If the specific gravity of all the elements were taken at 212°, or at 32°, would that of all be lowered or raised in an equal degree? Most decidedly not. Can any reason be given why we should take the specific gravities of bodies just as we find them in nature? What relation do they bear to each other in that condition?

What I have to propose is, that instead of taking the specific gravities of bodies all at the same temperature, it should be taken with all in the same state; either all solid, all liquid, or all gaseous; all heated or cooled, as may be required to bring the atoms of all to the same repulsion and distance. Either of these three states might be taken, but the liquid appears to me to be most convenient; of course they should all be brought to the same degree of liquidity, *i. e.* they should not be heated or cooled above or below the point of liquidity, or in the same relative state as water at 32°.

Let us again take the rule before alluded to, and ascertain the relative volumes of the atoms of oxygen and of mercury; the specific gravities of these two elements at the same temperature will be, oxygen ·00134, and mercury 13·545; if we divide their atomic weights by these specific gravities, we obtain for the atomic volume of oxygen 5970, and for that of mercury 14·91: now are we to conclude from this that the ultimate atom of oxygen fills a space 400 times the size of one of mercury? I think not; for if we could liquefy the oxygen gas and also reduce the mercury to $-39°$, and then take their specific gravities in this state, it is not altogether improbable to suppose that their atomic volumes might approach equality.

Again, if we take the two isomorphous elements, iodine and chlorine, we find one in the solid, and the other in the gaseous state; if we take their specific gravities in these states at the same temperature, that of iodine will be 4·948, and that of chlorine ·00305; divide their atomic weights by these specific gravities, and we obtain 25·58 for the atomic volume of iodine, and 11·629 for that of chlorine. What analogies or laws can be deduced from this result? But if we liquefy the chlorine and then take its specific gravity as 1·380, then its atomic volume will be 25·70, approaching to within 0·12 of the other; and is it unreasonable to suppose that if we reduced the iodine also to the liquid state, that the atomic volumes might approach still nearer, if not exactly coincide? and yet what reason can be assigned why, if we liquefy the chlorine, we should not also liquefy the iodine?

Several ways have occurred to me of manipulation to procure the specific gravities on the principle I have proposed. For those bodies liquefiable in glass vessels, the common specific gravity bottle might be employed; of course the water used as the standard must be at 32°; having obtained this standard in the usual manner, the bottle may then be filled with the substance operating on and heated to liquefaction, adjusted while liquid so as to occupy the same bulk as the standard water, allowed to cool, and then weighed. For substances that cannot be melted in glass vessels some other means must be adopted; perhaps a metallic hydrometer might be constructed to insert into the liquid metal.

The process would require variation to meet all the variety of the elementary bodies, and would be both tedious and laborious, but of course it would have only to be *once correctly* performed.

And will a series of specific gravities thus obtained serve only to construct tables of relative atomic volumes? Will it not then be reasonable to look for analogies between the specific gravity, the conduction of heat, specific heat, conduction and development of electricity, &c.*, hitherto looked for in vain, on which to base new laws, and to explain phænomena hitherto inexplicable?

Submitting these suggestions to your notice,

I am, Gentlemen,

Your obedient Servant,

Benjamin Boughey.

Mayfield, Manchester,
May 10th, 1845.

LXXXVII. *Remarks made by* Dr. Hare, *at a late meeting of the American Philosophical Society, on a recent speculation by* Faraday *on Electric Conduction and the Nature of Matter*†.

Messrs. Editors, Philadelphia, Nov. 30, 1844.

At the last meeting of the American Philosophical Society, I made some verbal remarks on a recent "speculation" of the celebrated Faraday, published in the London and Edinburgh Philosophical Magazine for February last (vol. xxiv. p. 136.). Of course a brief notice will be given of those remarks in the bulletin of the Proceedings. I send you for publication a statement of my reasoning on the questions at issue, hoping that it will not be found unworthy of the attention of philosophical chemists.

Your friend, Robert Hare.

Faraday objects to the Newtonian idea of an atom being associated with combining ratios. These he conceives to have been more advantageously designated as chemical equivalents‡.

This sagacious investigator adverts to the fact, that after each atom in a mass of the metal potassium has combined with an atom of oxygen and an atom of water, forming thus a hydrated oxide, the resulting aggregate occupies much less space than its metallic ingredient previously occupied; so

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* I would here also merely ask the question, would it not be on more philosophical principles if these tables were likewise obtained on the same principle as I have proposed for the specific gravity? *i. e.* with all the bodies liquid, and not taken promiscuously as they are found, as is the case at present.

† Communicated by the Author, from Silliman's American Journal of Science.

‡ See his speculations touching electric conduction and the nature of matter, vol. xxiv. 3rd series, Philosophical Magazine and Journal, February 1844.

that taking equal bulks of the hydrate and of potassium, there will be in the metal only 430 metallic atoms, while in the hydrate there will be 700 such atoms. Yet in the latter, besides the 700 atoms, there will be an equal number of aqueous and oxygenous atoms, in all 2800 ponderable atoms. It follows that if the atoms of potassium are to be considered as minute impenetrable particles, kept at certain distances by an equilibrium of forces, there must be, in a mass of potassium, vastly more space than matter. Moreover, it is the space alone that can be continuous. The non-contiguous material atoms cannot form a continuous mass. Consequently the well-known power of potassium to conduct electricity must be a quality of the continuous empty space which it comprises, not of the discontinuous particles of matter with which that space is regularly interspersed. It is in the next place urged, that while, agreeably to these considerations, space is shown to be a conductor, there are considerations equally tending to prove it to be a non-conductor; since in certain non-conducting bodies, such as resins, there must be nearly as much vacant space as in potassium. Hence the supposition that atoms are minute impenetrable particles, involves the necessity of considering empty space as a conductor in metals and as a non-conductor in resins, and of course in sulphur and other electrics. This is considered as a *reductio ad absurdum.* To avoid this contradiction, Faraday supposes that atoms are not minute impenetrable bodies, but, existing throughout the whole space in which their properties are observed, may penetrate each other. Consistently, although the atoms of potassium pervade the whole space which they apparently occupy, the entrance into that space of an equivalent number of atoms of oxygen and water, in consequence of some reciprocal reaction, causes a contraction in the boundaries by which the combination thus formed is inclosed. This is an original and interesting view of this subject, well worthy of the contemplation of chemical philosophers.

But upon these premises Faraday has ventured on some inferences which, upon various accounts, appear to me unwarrantable. I agree that "*a*" representing a particle of matter, and "*m*" representing its properties, it is only with "*m*" that we have any acquaintance, the existence of "*a*" resting merely on an inference. Heretofore I have often appealed to this fact, in order to show that the evidence both of ponderable and imponderable matter is of the same kind precisely: the existence of properties which can only be accounted for by inferring the existence of an appropriate matter to which those properties appertain. Yet I cannot concur in the idea, that because it is only with "*m*" that we are acquainted, the existence of "*a*" must not be inferred; so that bodies are to be considered as constituted of their materialized powers. I use the word materialized, because it is fully admitted by Faraday, that by dispensing with an impenetrable atom "*a*," we do not get rid of the idea of matter, but have to imagine each atom as existing throughout the whole sphere of its force, instead of being condensed about the centre. This seems to follow from the following language:—

"*The view now stated of the constitution of matter would seem to involve necessarily the conclusion that matter fills all space, or at least the space to which gravitation extends, including the sun and its system, for gravitation is a property of matter dependent on a certain force, and it is this force which constitutes matter.*"

Literally this paragraph seems to convey the impression, that agreeably to the new idea of matter, the sun and his planets are not distinct bodies, but consist of certain material powers reciprocally penetrating each other, and pervading a space larger than that comprised within the orbit of Uranus. We do not live upon, but within the matter of which the earth is constituted, or rather within a mixture of all the solar and planetary matter belonging to our solar system. I cannot conceive that the sagacious author seriously intended to sanction any notion involving these consequences. I shall assume, therefore, that excepting the case of gravitation, his new idea of matter was intended to be restricted to those powers which display themselves within masses at insensible distances, and shall proceed to state the objections which seem to exist against the new idea as associated with those powers.

Evidently the arguments of Faraday against the existence, in potassium and other masses of matter, of impenetrable atoms endowed with cohesion, chemical affinity, momentum, and gravitation, rest upon the inference that in metals there is nothing to perform the part of an electrical conductor besides continuous empty space. This illustrious philosopher has heretofore appeared to be disinclined to admit the existence of any matter devoid of ponderability. The main object of certain letters which I addressed to him, was to prove that the phænomena of induction could not, as he had represented, be an "*action*" of ponderable atoms, but, on the contrary, must be considered as an *affection* of them consequent to the intervention of an imponderable matter, without which the phænomena of electricity would be inexplicable. This disinclination to the admission of an imponderable electrical cause has been the more remarkable, as his researches have not only proved the existence of prodigious electrical power in metals, but likewise that it is evolved during chemico-electric reaction, in equivalent proportion to the quantity of ponderable matter decomposed or combined.

According to his researches, a grain of water by electrolytic reaction with four grains of zinc, evolves as much electricity as would charge fifteen millions of square feet of coated glass. But in addition to the proofs of the existence of electrical powers in metals thus furnished, it is demonstrated that this power must be inseparably associated with metals, by the well-known fact, that in the magneto-electric machine, an apparatus which we owe to his genius and the mechanical ingenuity of Pixii and Saxton, a coil of wire being subjected to the inductive influence of a magnet, is capable of furnishing, within the circuit which it forms, all the phænomena of an electrical current, whether of ignition, shock or electrolysis.

The existence in metals of an enormous calorific power must be evident from the heat evolved by mere hammering. It is well known, that by a skilful application of the hammer, a piece of iron may be ignited. To what other cause than their inherent calorific power can the ignition of metals by a discharge of statical electricity be ascribed?

It follows that the existence of an immense calorific and electrical power is undeniable. The materiality of these powers, or of their cause, is all that has been questionable. But, according to the speculations of Faraday, all the powers of matter are material; not only the calorific and electrical powers are thus to be considered, but likewise the powers of cohesion, chemical affinity, inertia and gravitation, while *of all these material powers only the latter can be ponderable!!!*

Thus a disinclination on the part of this distinguished investigator to admit the existence of one or two imponderable principles, has led him into speculations involving the existence of a much greater number. But if the calorific and electrical powers of matter be material, and if such enormous quantities exist in potassium, as well as in zinc and all other metals, so much of the reasoning in question as is founded on the vacuity of the space between the metallic atoms, is really groundless.

Although the space occupied by the hydrated oxide of potassium comprises 2800 ponderable atoms, while that occupied by an equal mass of the metal comprises only 430, there may be in the latter proportionably as much more of the material powers of heat and electricity as there is less of matter endowed with ponderability.

Thus while assuming the existence of fewer imponderable causes than the celebrated author of the speculation has himself proposed, we explain the conducting power of metals, without being under the necessity of attributing to void space the property of electrical conduction. Moreover, I consider it quite consistent to suppose that the presence of the material power of electricity is indispensable to electrical conduction, and that diversities in this faculty are due to the proportion of that material power present, and the mode of its association with other matter. The immense superiority of metals, as conductors, will be explained by referring it to their being peculiarly replete with the material powers of heat and electricity.

Hence Faraday's suggestions respecting the materiality of what has heretofore been designated as the properties of bodies, furnish the means of refuting his arguments against the existence of ponderable impenetrable atoms as the basis of cohesion, chemical affinity, momentum and gravitation.

But I will in the next place prove, that his suggestions not only furnish an answer to his objections to the views in this respect heretofore entertained, but are likewise pregnant with consequences directly inconsistent with the view of the subject which he has recently presented.

I have said that of all the powers of matter which are, according to Faraday's speculations, to be deemed material, gravitation alone can be ponderable. Since gravitation, in common with every power heretofore attributed to impenetrable particles, must be a matter independently pervading the space throughout which it is perceived, by what tie is it indissolubly attached to the rest? It cannot be pretended that either of the powers is the property of another. Each of them is an *m*, and cannot play the part of an *a*, not only because an *m* cannot be an *a*, but because no *a* can exist. Nor can it be advanced that they are the same power, since chemical affinity and cohesion act only at insensible distances, while gravitation acts at any and every distance, with forces inversely as their squares: and moreover, the power of chemical affinity is not commensurate with that of gravitation. One part by weight of hydrogen has a greater affinity universally for any other element, than 200 parts of gold. By what means then are cohesion, chemical affinity, and gravitation, inseparably associated, in all the ponderable elements of matter? Is it not fatal to the validity of the highly ingenious and interesting deductions of Faraday, that they are thus shown to be utterly incompetent to explain the inseparable association of cohesion, chemical affinity and inertia with gravitation; while the existence of a vacuity between Newtonian atoms, mainly relied upon as the basis of an argument against their existence, is shown to be inconsistent both with the ingenious speculation, which has called forth these remarks, and those Herculean "researches" which must perpetuate his fame?

LXXXVIII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 79.]

Dec. 11, 1843. ON the Motion of Glaciers. By William Hopkins, M.A., F.R.S., Fellow of the Society (Second Memoir).

In a previous memoir Mr. Hopkins had given the details of certain experiments, by which it was proved that ice will descend with a very slow unaccelerated motion down an inclined plane, presenting a surface like that of a common slab of paving-stone, at an angle scarcely exceeding half a degree (and probably also at still smaller angles), provided the lower surface of the ice in immediate contact with the inclined plane be in a state of constant but slow disintegration. This experimental conclusion was brought forward in support of the *sliding theory* of De Saussure, and the author endeavoured to explain, according to that theory, different phænomena connected with the motion of glaciers. He there considered glacial ice as a *solid* substance, having a certain degree of *plasticity* and *flexibility*, and the general mass of the glacier as a *dislocated* mass, the greater motion of the central portion of the glacier being much facilitated by these dislocations, though due partly, but in a comparatively small degree, to the plasticity of the general mass. In the present memoir Mr. Hopkins considers what would be the nature of the motion under other hypotheses respecting the constitution of glaciers. (1.) The lower part of a glacier may be conceived to be crushed, and consequently disintegrated, by the superincumbent weight, each component particle still retaining its solidity; or (2.) the whole mass may be conceived to be *plastic*, and to move by a change of form, produced by gravity, in each component element. The author contends, if either of these hypotheses were true, that, *cæteris paribus*, the more superficial portion of the mass must tend to move the faster as the depth of the glacier should be greater; and that, consequently, the part of the glacier near the upper extremity must generally tend to move much faster than that near the lower extremity, assuming always the whole, or much the greater part of glacial motion, to be due to the plasticity of the mass, and to be independent of *sliding* over its bed. But in such case it is manifest that the general state of a glacier must be one of *longitudinal compression*, more particularly during the summer months, when the motion is greatest. Now the author contends that the general existenee of transverse fissures (at least during summer) is a conclusive proof against the existence of general longitudinal compression; and he observes that no observer ventured to assert the fact of such compression to be deducible from actual observation. He conceives this to be a serious objection to the hypothesis here considered.

In this memoir Mr. Hopkins has also investigated the directions in which transverse fissures must be formed when referrible to the internal tensions superinduced by the conditions to which glaciers in general are subjected, and more especially by the more rapid motion of their central portions.

Assuming the velocity of each particle of the glacier to be the same in any vertical line (which is at least true at points not remote from the surface), the glacier may, in this investigation, be considered as a *lamina*. In this lamina take a rectangular element having two of its sides parallel to the axis of the glacier, and, therefore, the remaining sides perpendicular to it. Let X denote the intensity of the force acting *normally* to these latter sides of the element, Y that of the force acting normally to the two former sides. Also let f denote the intensity of the force acting *tangentially* on the sides on which X acts normally. It is proved that f will also be the intensity of the tangential force on the other two sides. Then, if θ be the angle which the line of maximum tension through the proposed element makes with the axis of the glacier, it is proved that

$$\tan 2\theta = \frac{2f}{X-Y},$$

where X and Y are *tensions*. If either be a *pressure*, it must be made negative.

If the maximum tension become greater than the cohesion of the ice, a fissure will be formed in a direction perpendicular to that of the tension at each point, or at least approximately so. Consequently, the line whose direction is defined by the angle θ, will be a normal to the curve of fracture. Now, taking the case in which the glacial valley contracts in descending (which is the more frequent case), Y is doubtless most frequently a *pressure*, in which case

$$\tan 2\theta = \frac{2f}{X+Y};$$

also f will be greatest at the sides (where the velocities of particles in a transverse line vary most rapidly), and will vanish at the centre. Hence θ will vanish at the centre of the glacier, and will increase towards the sides, since the change in the value of the denominator cannot be great. Consequently, if a fissure were continued across the glacier it would form a curve, meeting the axis of the glacier at right angles; and its convexity will be turned towards the upper extremity of the glacier, for the line defined by the angle θ, or the normal to the curve, meets the axis of the glacier when produced towards its *lower* extremity. This is the well-known character of transverse fissures, which the author conceives to be thus completely accounted for. In the previous memoir above referred to, this curious character had been very imperfectly explained by referring it to the action of the longitudinal tension (X) alone.

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