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

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

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THE
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[THIRD SERIES.]

JULY 1848.

I. *On Alpha- and Beta-Orcine.*

By JOHN STENHOUSE, *Esq., Ph.D.**

A PAPER on the lichens, read before the Royal Society about three months ago, contained among other matters some observations upon orcine, and described an easy method of procuring that interesting substance in a perfectly colourless state. In the present notice, I purpose narrating a few observations I have subsequently made, and to describe a new species of orcine derived from usnic acid, to which I have given the provisional name of beta-orcine.

The usual mode of preparing alpha-orcine is by boiling lecanoric, erythric, or any of the similar principles of the lichens, with an excess of lime or baryta. The orcine obtained in this way is always more or less coloured. Without entering into the minute details of the process for preparing the colourless orcine, which are fully given in the paper already referred to, I only remark in passing, that it may be readily procured by boiling the alpha- and beta-orsellesic, or the erythrelesic acids, in pure water from half an hour to an hour, when a great deal of carbonic acid is given off; and on the solutions being concentrated and set aside, abundance of crystals of colourless orcine are deposited. If a little animal charcoal is kept in the solution while it is being concentrated, it will be found useful in preventing any tendency to oxidation.

A quantity of colourless orcine prepared in this way was dried at the ordinary temperature, and analysed with chromate of lead. 0·332 grm. substance dried at 60° F. gave 0·717 carbonic acid and 0·204 water.

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* Communicated by the Author.

Orcine dried at 60° F.

		Calculated numbers.	Found numbers.	Mr. Schunck found
21C	1605·135	59·61	58·90	58·98
15H	187·192	6·95	6·82	7·06
9O	900·000	33·44	34·28	33·96
	2692·327	100·00	100·00	100·00

It will be observed that this analysis agrees pretty closely with Mr. Schunck's, and that the small amount of colouring matter remaining in orcine prepared by the old method has no perceptible effect upon the result of the analysis.

Orcine is a body which retains its water of crystallization with great tenacity. It may be rendered anhydrous, however, in several ways, as when it is distilled, when it is heated to 212° F., and, as I have recently found, when it has been repeatedly crystallized out of æther, or even when it has been dried *in vacuo* over sulphuric acid.

The last-mentioned mode of rendering orcine anhydrous, though tedious, requiring from three to six weeks, is by far the best and safest, as the orcine remains perfectly colourless, and all risk of decomposition is avoided. The following are the results of several analyses of colourless orcine, prepared from alpha-orsellesic acid at four different times, and dried *in vacuo*.

I. 0·264 grm. substance gave 0·6610 CO^2 and 0·1570 water.

II. 0·349 grm. substance gave 0·8675 CO^2 and 0·205 water.

III. 0·324 grm. substance gave 0·8085 CO^2 and 0·1985 water.

IV. 0·2064 old orcine, also dried *in vacuo*, gave 0·515 CO^2 and 0·130 water.

	Calculated numbers.	Anhydrous orcine.	I.	II.	III.	IV.
21C	1605·135	68·16	68·28	67·80	68·05	68·02
12H	149·754	6·35	6·60	6·52	6·70	6·97
6O	600·000	25·49	25·12	25·68	25·25	25·01
	2354·889	100·00	100·00	100·00	100·00	100·00

I. and II. analyses were made with chromate of lead, and the heat strongly raised towards the close of the combustion. No. III. was burned with pure oxide of copper, assisted with a current of oxygen gas. No. IV. was orcine prepared by the old process, and also dried *in vacuo*: it too was burned with oxide of copper, assisted by a current of oxygen. It is observable, that the quantity of hydrogen in the two last analyses is too high: the reason of this is, that from fear of decomposing the orcine, I did not venture to heat the mixture of the substance and oxide of copper in the tube, and then to dry it with an exhausting syringe, as is usually done.

It is plain from the result of these analyses, that anhydrous orcine has lost three atoms of water, and that its probable formula is $C_{21}\ H_{12}\ O_6$, that of hydrated orcine being $C_{21}\ H_{15}O_9$.

I subjoin the results of previous experimenters on anhydrous orcine, with which it will be seen that these analyses agree pretty closely.

	Dumas. Distilled orcine.	Schunck. Dried at 212° F.	Robiquet. Distilled orcine.
Carbon . .	67·78	67·88	68·574
Hydrogen .	6·50	6·60	6·828
Oxygen . .	25·72	25·52	24·598
	100·00	100·00	100·000

In order to corroborate this determination of the atomic weight of orcine, which, it must be confessed, still remains somewhat doubtful, I made repeated attempts to prepare the lead salt previously described both by Dumas and Schunck, but I am sorry to say with by no means satisfactory results. This perhaps is not to be wondered at, when we consider how easily alterable orcine is. In my first trials, I treated an aqueous solution of orcine with a very slight excess of subacetate of lead. The precipitate in the course of a few minutes assumed a deep red colour, and the compound appeared so unstable, that much of the orcine was removed by the wash-water; so that the more the precipitate was washed, the greater was the amount of oxide of lead it contained. In some subsequent trials, when the quantity of subacetate of lead which was added was not sufficient to precipitate the whole of the orcine in the solution, the precipitate, though reddish-coloured, was not nearly so deeply coloured as when a slight excess of base had been employed. The amount of oxide of lead in these precipitates was however greatly diminished; and though frequently repeated, I never could get the results of the various trials to agree in a satisfactory manner. Mr. Schunck admits that his experiments were somewhat similar. I subjoin the results of a few of my experiments.

I. Orcine precipitated by a slight excess of subacetate of lead, 1·305 grm. salt gave 0·467 PbO and 0·525 Pb=1·081 PbO=82·83 PbO per cent.

II. Where part of the orcine remained in solution, 0·773 salt gave 0·1383 PbO and 0·371 Pb = 0·5379 PbO=69·58 PbO per cent.

III. Prepared in the same way, but at a different time, 0·667 salt gave 0·200 PbO and 0·231 Pb=0·4488 PbO=67·28 per cent.

Examination of the Crystalline Form of Orcine.

I transmitted a quantity of crystallized orcine to Professor W. H. Miller of Cambridge, who has kindly favoured me with the subjoined determination of its crystalline form.

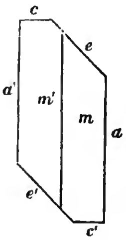
Oblique. Symbols. *a* 100, *c* 001, *e* 101, *m* 110.

Angles between normals to the faces.

	°	'
ae	43	44
ac	83	57
ma	51	12
mm'	77	36

Cleavage parallel to *a*, very perfect.

Orcine.



Beta-Orcine.

The substance to which I have given the name of beta-orcine is obtained from usnic acid, which is perhaps the most widely diffused of all the known proximate principles of the lichens. Knop found usnic acid in several species of *Usnea*, such as *Usnea florida*, *U. hirta*, and *U. plicata*. Messrs. Rochleder and Heldt extracted it from the lichen *Rangeferinus*, *Usnea barbata*, and *Ramalinea calicaris*. In addition to these sources, I have found it in the *Evernia Prunastri* and in *Ramalinea Fraxinia*. The best mode of extracting usnic acid, as I have fully detailed in the paper already referred to, is by macerating the lichens in milk of lime, and precipitating the filtered solution by muriatic acid.

Beta-orcine may be obtained from usnic acid by several processes. The method which I have found most convenient is by subjecting the crude usnic acid, previously dried, to destructive distillation. The beta-orcine sublimes, and is deposited in long yellow crystals, partly on the neck and sides of the retort; but by much the larger portion of it is dissolved in a brownish-coloured resinous liquid which passes into the receiver. A very large quantity of a bulky porous charcoal remains in the retort.

The resinous fluid which has passed into the receiver must be repeatedly treated with considerable quantities of boiling water to dissolve out the orcine, which is retained by the resin with great pertinacity. The solutions containing the impure orcine should be concentrated to the consistence of a syrup, and left standing for some time in a flat open vessel. In the course of a day or two the liquid becomes filled with a mass of brownish-coloured crystals, which must be collected and pressed between folds of blotting-paper to free them from adhering resin and colouring matter.

The crystals must be further purified by digestion with animal charcoal, and by being repeatedly crystallized out of water: they then form hard, slender, brittle prisms, which still retain a faintish yellow shade, and are about half an inch long. In order to obtain beta-orcine perfectly white, it must be crystallized out of weak spirits, assisted, as before, by a little animal charcoal. So soon as the crystals are all deposited, they should be removed from the solution and dried on blotting-paper, as the mother-liquor, on standing for some time, becomes reddish-coloured. Beta-orcine crystallizes out of weak spirits in larger and more regular crystals than those obtained from its aqueous solutions. It forms four-sided prisms, surmounted at either end by well-defined four-sided pyramids. These crystals are hard and brittle, have a brilliant lustre, and are from an inch to three-quarters of an inch long. I am indebted to Professor W. H. Miller of Cambridge for the subjoined measurement of their angles.

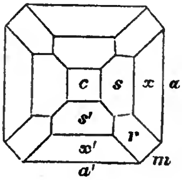
Pyramidal. Symbols. *a* 100, *c* 001, *m* 110, *s* 101, *x* 201, *r* 111.

Angles between normals to the faces.

	°	′
aa′	90	0
ac	90	0
ma	45	0
sc	49	3
xc	66	33
rc	58	29
xa	23	27
sa	40	57
rm	31	31
ra	52	56
rs	37	4
xr	40	27
xm	49	33

No cleavage observable.

Beta-orcine.



Beta-orcine, like ordinary orcine, to which in so many respects it bears a very close analogy, may be also obtained by boiling usnic acid with an excess either of caustic potash or of lime or baryta. By none of these methods, however, is it so readily procured as by destructive distillation. By much the larger portion of the usnic acid is converted by the potash into an acid resin, with which the alkali combines; the free alkali is also apt to destroy most of the orcine as it is formed.

When usnic acid is boiled with lime or baryta, the excess of base is removed by a current of carbonic acid gas, and the clear liquor evaporated to dryness on the water-bath. The dried mass is then to be agitated with æther; and on the evaporation of the æther, the beta-orcine is deposited in nearly colourless crystals.

Beta-orcine is tolerably soluble in cold water, though not nearly so much so as alpha-orcine, but very soluble in boiling water. It is very soluble in alcohol and in æther. It has a faint but distinctly sweetish taste. It has no reaction upon test-paper, and is a perfectly neutral body. When heated, it sublimes without leaving any residue. The smell of its vapours is suffocating. It inflames readily, and burns with a strong smoky flame.

In the course of a few minutes it assumes, with ammonia, a beautiful blood-red colour, which on standing becomes deeper. Beta-orcine is much more rapidly acted on by ammonia than alpha-orcine. The colour which alpha-orcine yields with ammonia is reddish-brown. When left in contact with caustic or carbonate of potash, beta-orcine yields a beautiful reddish-purple dye stuff. This appears to be the same amorphous red body which Knop observed when preparing usnate of potash, without, however, his remarking the source which produced it. The smallest portion of beta-orcine instantly strikes a bright blood-red colour, with a solution of hypochlorite of lime; just as alpha- and beta-orsellic and erythric acids do with the same reagent. Alpha-orcine, on the other hand, yields a purple violet colour with hypochlorite of lime.

The crystals of beta-orcine, when dried in the air and then *in vacuo* over sulphuric acid, do not lose any water. The four following analyses were made with beta-orcine prepared on four different occasions: they were all dried *in vacuo*:—

I. 0·317 grm. orcine burnt with chromate of lead, gave 0·8002 CO^2 and 0·209 water.

II. 0·391 grm. orcine burnt with chromate of lead, gave 0·985 CO^2 and 0·254 water.

III. 0·2608 grm. orcine burnt with chromate of lead, gave 0·657 CO^2 and 0·173 water.

IV. 0·3138 grm. orcine burnt with chromate of lead, gave 0·800 CO^2 and 0·212 water.

	Calculated numbers.		I.	II.	III.	IV.
38 C	2904·530	69·08	68·84	68·70	68·70	69·20
24 H	299·508	7·12	7·32	7·22	7·36	7·50
10 O	1000·000	23·80	23·84	24·08	23·94	23·30
	4204·038	100·00	100·00	100·00	100·00	100·00

These analyses give C_{38}, H_{24}, O_{10} as the formula for beta-orcine: it is, however, perfectly empirical. When beta-orcine, previously dried *in vacuo*, was heated in the water-bath to 212° F., it parted with a considerable amount of water. The orcine did not become in the least degree coloured, neither did it melt, though the temperature was raised to 230° F. Alpha-orcine melts and becomes coloured at 212° F. A quantity of beta-orcine, which had been kept four weeks in the water-bath at 212° F., had lost nearly 30 per cent. in weight, and was still continuing to lose weight. This however did not arise merely from the loss of water, but because a considerable portion of the orcine itself had volatilized even at the comparatively low temperature of 212° F. The quantity of water which beta-orcine contains cannot therefore be determined by drying it on the water-bath.

I have been unable to determine the atomic weight of beta-orcine. It gives no precipitate with nitrate or ammonio-nitrate of silver, with the salts of iron, baryta and copper, or with neutral acetate of lead. It yields a pretty abundant whitish precipitate with subacetate of lead, which is soluble in an excess of the precipitant. This precipitate is exceedingly alterable, and immediately becomes deep red-coloured, even more rapidly than is the case with alpha-orcine in similar circumstances.

Glasgow, May 22, 1848.

II. *On the Theory of* Nobili's *Coloured Rings.*

By E. DU BOIS-REYMOND *and* W. BEETZ[*].

I.

NOBILI first pointed out the possibility of applying the coloured films (*apparenze elettro-chimice*) which he discovered, to determine the diffusion of the current in nonprismatic conductors[†]. But with his unacquaintance of Ohm's law of the motion of electricity, even according to one dimension, he was unable to give proper evolution to this idea. In a treatise entitled *Note sur les anneaux produits par le dépôt des oxydes métalliques sur les métaux*[‡], M. Edmond Becquerel attempted to subject the simplest form of Nobili's rings to a rigid investigation, both in an optical and electrical point of view. The rings were obtained by a method given by M. Becquerel, senior, viz. by pouring a thin layer of a solution of oxide of

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[*] Read before the Physical Society of Berlin, on the 11th Dec. 1846, and communicated by the Authors.

[†] *Bibliothèque Universelle de Genève. Anc. Sér. Sc. et Arts,* 1835, t. lix. pp. 263, 416.

[‡] *Annales de Chimie et de Physique.* Janvier 1845, 3 ser. t. xviii. p. 342.

lead in caustic potash upon a plate of German silver (? "*Maillechort*"); the free extremity of an extremely fine platinum-wire drawn out by Wollaston's method, and fused in a glass tube, is dipped into the liquid opposite the middle of the plate, the wire is then connected with the negative and the plate with the positive pole of a battery composed of eight or ten pairs. A delicate film of peroxide of lead is deposited upon the plate, and as the current is stronger in the direct line from the point towards the plate than in the lateral directions, this film diminishes in thickness from the centre to the circumference, and hence exhibits Newton's rings most splendidly. M. Becquerel remarks, that as the index of refraction of the peroxide of lead lies between that of the air and that of the metal, the rings in the present instance should be compared to those appearing in transmitted light rather than to those obtained by reflexion. In fact it appears that the arrangement of the series of colours in Nobili's rings from without inwards, very accurately corresponds to that from within outwards in Newton's rings by transmitted light. The next point is to prove that the relative thicknesses of the film, which may be ascertained from the laws of diffusion of the electric current, in conjunction with the law of electrolysis, agree with those which, as regards the light and dark parts of the rings, are readily deduced from optical considerations.

Speaking of the law according to which the thickness of the rings varies, M. Becquerel says, "*It is easy to find the value of the curve formed by the upper surface of the film of oxide, in a plane perpendicular to the metallic plate and passing through its centre.* Let BX . . . (see Pl. I. fig. 1) *be the metallic plate,* B *its centre,* A *the extremity of the negative point of the pole, and let us suppose that the electricity escapes by that point only. Let* C *be any point; when the current passes, the thickness of the layer of oxide deposited at the point* C *will depend entirely upon the intensity of the current and the distance* AC. *Admitting that this intensity is in inverse proportion to the distance* AC, *we should have*

$$y=\frac{A}{\sqrt{m^2+x^2}}$$

as the equation of the curve which limits the upper part of the film of oxide, m *being the distance* A B, *and* A *a coefficient depending upon the conducting power of the solution and the power of the battery. If care be taken to place the point* A *very near* B, *within a half-millimetre,* m^2 *may be omitted before* x^2, *and the equation becomes*

$$y=\frac{A}{x},$$

i. e. *the thickness of the films of oxide is evidently in inverse proportion to the diameters of the rings.*"

If the thickness of the rings is inversely proportionate to their diameter, it is evident that if we multiply the relative thicknesses at certain spots derived from optical considerations, by the distances of these spots from the central point, we must obtain a constant product. Becquerel based his theory upon this test as applied to two selected plates, and found as perfect an agreement of the twofold results as could be expected in admeasurements of this kind.

On account of the beautiful idea of this experiment, I regret to be obliged to remark that the accordance of observation with calculation found by M. Becquerel can scarcely have depended upon anything more than a somewhat inexplicable result of chance.

First, it may be remarked, that in the present arrangement the electricity is not propagated in straight lines. In the case of the diffusion of the electric current in non-prismatic conductors, as is well known, we have first sought for *iso-electric* surfaces, or surfaces of equal tension. If the electrodes are metallic, but the conductor, in which the diffusion of the current takes place, one of Volta's second class, the tension at all points in it may be regarded as the same*; consequently the surface of every electrode represents the first iso-electric surface. At the margins of the conductor the iso-electric surfaces are situated perpendicularly. A system of curves intersecting them at right angles, which thus lie close to the margins of the conductor, and are at right angles to the surfaces of the electrodes, form the lines of the current, in which the motion of the electricity occurs.

If the curves of the current in the above arrangement coincided with the lines of conduction drawn from the point to the plate, the iso-electric surfaces would form spherical cups. Such of these spherical cups as were in contact with the plate when the projection of the point towards it was perpendicular, would then possess a degree of tension common to all points of the plate, and therefore all cause of further currents between the latter and the former would vanish.

It must remain for the superior analysis of those ingenious philosophers who have lately engaged in the solution of so many important problems in which the diffusion of the cur-

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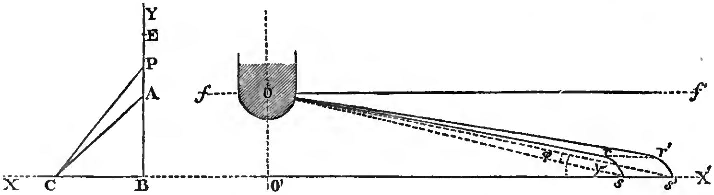
* G. S. Ohm, The Galvanic Circuit investigated mathematically. Taylor's Scientific Memoirs, vol. ii. p. 401. The procedure recommended by M. Becquerel, to conduct the current to the centre of the back of the plate, must therefore have arisen from misunderstanding. It could hardly produce the slightest perceptible distortion of the rings, if the current terminated at the circumference of the plate instead of the spot above mentioned.

rent in non-prismatic conductors is concerned, to ascertain the true course of the electricity under the present circumstances. Although M. Smaasen has already extended the fundamental equation of the dynamic equilibrium of electricity to the three dimensions, nevertheless we have no instance of one of these, however simple, being susceptible of mathematical treatment. But in this case, by means of mere inspection, we may ascertain with tolerable certainty, the probable manner in which the curves of the current are arranged. Let us first determine the terms of the phænomenon.

Imagine an indefinite flat plate XX′, the resistance of which vanishes in comparison with that of a uniform layer of a liquid conductor covering it everywhere of equal thickness. The other electrode in the form of a hemisphere dips into this to such an extent, that the surface *ff′* of the layer passes through its centre O. We have no concern with what takes place in the immediate vicinity of the centre O′ of the plate, perpendicularly beneath that of the electrode; for we cannot make any observations here, because those rings which are fit for investigation do not commence until some distance from the centre. It is thus easily seen that the curves of the current, which pass to the more distant points of the plate, must proceed almost rectilinearly for a very long time; they then curve inwards towards the plate, so as to reach it at right angles. Moreover, if we conceive two curves formed by the current lying near each other, in a plane falling perpendicularly upon the plate through the point from which the current issues, we also see, that in the rectilinear portion of their course they will remain very near each other; but that they tend to separate from each other, when, having arrived near the point at which they reach the plate, they curve inwards towards it (*Ors*, *Or′s′*).



The curves formed by the current at every point of the conductor through which it has passed, indicate the direction of the motion of the electricity; but the consideration of their mere length is by no means sufficient to determine the intensity of the current, *i. e.* the resistance divided by the electromotive force, for each spot of the conductor. M. Becquerel overlooked this in making his formula, when he regarded the intensity of the current for each point of the plate as inversely proportional simply to the length of the line of conduction.

To find the intensity of the current at a given infinitely small point of the conductor, we have first to calculate the resistance which the space between the curves of the current circularly bounding this spot offers to the current. In the present instance, however, the resistance of the curvilinearly bounded portion of this space in comparison with that of the part rectilinearly bounded may probably be neglected, because, as we have mentioned, in the former, which of itself is much shorter, the curves of the current also separate from each other, so that the section of the path of the current is here much greater. Thus, according to M. Becquerel's no longer justified process, the conducting rays Os, Os' may be replaced by Ors, Ors', the true curves of the current; or, neglecting the resistance of the portion rs, $r's'$, the triangle Oss' by the triangle Orr', with tolerable accuracy, but then, although starting from the same point of view, we must alter the expression of the resistance, which a partial current has to overcome.

We must then consider here as the partial path of the current, the space between two conical envelopes, the axis and apex of which coincide with each other, as also with the centre O of the point from which the current escapes, and its perpendicular distance OO′ from the plate, which latter forms the common basal surface of the cone, and the very obtuse angles of which are separated at the apex by an infinitely small angle 2ϕ. The resistance of such a space is easily determined. If we denote the distance of any point of the conical envelope from O by ξ, the angle at the surface of the base by γ, and the inverse value of the resistance for the unit, the length and the section of the path by ω, the element of this becomes

$$dw = \frac{1}{2\pi\omega \cos\gamma \, tg\,\phi} \cdot \frac{d\xi}{\xi^2}.$$

The integration may be completed between the limits $\xi = \Xi =$ the total length of the line of conduction, as the upper, and $\xi = \rho$, any small, but finally constant quantity, as the lower boundary. Thus by ρ we must understand the diameter of the hemispherical point from which the current escapes, for if we neglected it, as in M. Becquerel's process, the resistance would become infinite. We have

$$w = \frac{1}{2\pi\omega\rho} \cdot \frac{\Xi - \rho}{\Xi \cos\gamma \, tg\,\phi}.$$

Thus the partial current, when k represents the difference between the tensions of the electrodes, becomes

$$dI = 2\pi k\omega\rho . \frac{\Xi \cos\gamma \, tg\varphi}{\Xi - \rho}.$$

Ξ, $\cos\gamma$ and $tg\varphi$ must here be expressed in functions of x and dx. Retaining M. Becquerel's method of notation, we then obtain the determinations—

$$\Xi = \sqrt{x^2 + m^2}, \quad \cos\gamma = \frac{x}{\sqrt{x^2 + m^2}}$$

$$\Xi tg\varphi = dx \sin\gamma, \quad \sin\gamma = \frac{m}{\sqrt{x^2 + m^2}},$$

and thus the partial strength of the current becomes

$$dI = 2\pi\omega\rho k m . \frac{x dx}{(x^2 + m^2)(\sqrt{x^2 + m^2} - \rho)}.$$

If this expression be multiplied by a further constant quantity α, as the atomic weight of the product of decomposition precipitated, its density, &c., we shall obtain a measure of the absolute quantity of the product of decomposition which is precipitated in a unit of time by the partial current upon the ring of the plate from which the conical envelopes arise, but by no means, as Becquerel appears to think, of the relative thickness of the film, in which this quantity is deposited. To determine this, it is clear that we must take into consideration the extent of the surface upon which the deposition occurs. This is readily found by differentiating the expression of the basal surface of the cone as a function of the semidiameter, and consequently the approximative law of the diminishing thickness of the rings, when dI is divided by $2\pi . x dx$. This law therefore takes the form—

$$y = \omega\rho k m \alpha . \frac{1}{(x^2 + m^2)(\sqrt{x^2 + m^2} - \rho)}.$$

If ρ be neglected in comparison with Ξ, it becomes

$$y = \omega\rho k m \alpha . \frac{1}{(x^2 + m^2)^{\frac{3}{2}}};$$

if, again, as in Becquerel's example, m^2 be supposed to vanish in comparison with x^2, and the constants be included under the sign A, we get simply

$$y = \frac{A}{x^3}.$$

Hence the thickness of the ring would not be, as M. Becquerel thinks, inversely proportional to the *first power* of the radius, but what is partly evident, even from simple inspection, to its *cube.*

It is not easy to comprehend how M. Becquerel's observations correspond with considerable accuracy to the first power. It is however possible that a kind of polarization produced by the electromotive reaction of the products of decomposition may have been concerned in it. This cannot be supposed to be owing to the oxygen, because, even where the plate is difficultly oxidizable, it is applied to the higher oxidation of the oxide of lead; but the unequal film of the peroxide of lead itself may be taken into account, by which perhaps the nearer points of the plate become negative to the more remote parts, which may have produced an increased thickness of the film upon the latter in comparison to the former. And in fact, *e.g.* copper, which becomes oxidized when exposed to the influence of a powerful current, became negative to that which formed the positive electrode when a weaker current was used.

M. Becquerel also calculated the form of the curve which limits the film of the product of decomposition in that case in which, instead of the mere point, a shorter or longer piece AE of the wire formed the positive electrode. According to him, this form would be given by the definite integral

$$\int_m^n \frac{A dz}{\sqrt{z^2+x^2}},$$

in which m and x retain their former signification; z represents the perpendicular variable distance of the point P of the wire from the plate, the action of which upon the point C should be exactly observed, and n the distance EB (see the figure).

But this formula is not really based upon true notions, because, *e.g. dz*, in the manner in which it enters into it, cannot well represent anything more than a constant section perpendicular to the direction of the current, whilst, when measured at z, it forms a variable angle with this direction, which only becomes a right angle when $z=0$, and whilst partial currents flow in a four-sided pyramid from the surface of the base to the apex, yet every instant pass over a different section. Lastly, that in this case in determining the partial path of the current, the object has again been attempted in a perfectly arbitrary manner, and with the omission of the most essential conditions, after the above remarks, does not require further illustration.

Dr. E. du Bois-Reymond.

II.

The observations above communicated by Dr. du Bois-Reymond on the law according to which a substance separated by electrolysis is deposited upon an electrode to form Nobili's coloured rings, induced me to make some experiments upon this point, the results of which I shall detail in the following remarks.

In my experiments I generally used the same electrolyte as M. Becquerel, a solution of oxide of lead in a concentrated solution of potash, sometimes, but more rarely, a solution of the acetate of lead. A plate of one of the noble metals served as the anode to receive the film of peroxide of lead; the readily oxidizable metals appeared but little adapted to form receivers of the coloured rings, probably because they themselves are acted upon, partly by oxidation, partly by the transference of particles of their surface to the cathode. Even silver was not sufficiently free from this objection to allow of its being recommended for this purpose, so that I used either Daguerreotype plates covered with gold by voltaic electricity, or a plate of platinum. The cathode consisted of a thin platinum wire fused into a glass-tube, and was connected with Grove's zinco-platinum battery, consisting of from three to sixteen elements (each presenting about 1·5 square inches of active platinum surface). According to the composition of the solution, the surface of the plate, and the battery used, from three to eight systems of rings were formed, which were always of a paler colour towards the centre, and ultimately passed into the brown colour of the peroxide of lead. To determine the radii of these rings, the diameter of the darkest and of the lightest spots of the rings was measured by monochromatic illumination. When any ring was irregularly formed on one side, the radius of the other was directly measured. By the former procedure the errors of the admeasurements are halved, and thus slight errors in the determination of the central point are not of so much importance.

The next point was to determine whether the thickness of the films really diminishes from the centre according to the law of the cubes of the radius, as should occur according to the above calculation. It is evidently most advantageous for this investigation, to calculate the lengths of the undulations of the colours employed each time, from the radii of the same system of rings, measured with differently coloured illuminations, and to compare them with the known values for these lengths as ascertained by other experiments. We thus incur no risk of being deceived either by anomalies in the intensity of the current, as where the differences of the radii are great, or by a defective determination of the film of peroxide of lead, as when different systems of rings are used, which will be mentioned more in detail below. A line was drawn from the centre upon the plate covered with the coloured rings, and this radius successively placed in the portions of a prismatic solar spectrum, which are indicated by the lines B, D, E, and F. The following table contains the results of the experiments with two different plates. The second system of rings was used in each, this being the most perfectly formed. To render the table intelligible, it need only be remarked, that r denotes the radius in each case, and r_b the radius of the ring, which was measured when illuminated by the spot B of the spectrum.

TABLE I.

Line.	First experiment.			Second experiment.			Mean.	Length of undulations (millionth parts of a millimetre).		
	r.	r^3.	$\frac{r_b}{r^3}$.	r.	r^3.	$\frac{r_b^3}{r^3}$.	$\frac{r_b^3}{r^3}$.	From experiment.	According to Schwerd.	Δ.
B	13‴·1	2248·1	1	12‴·5	1933·1	1	1	688	688	
D	13 ·8	2628·1	0·855	13 ·1	2248·1	0·860	0·8575	590	589	+1
E	14 ·4	2983·1	0·753	13 ·6	2515·4	0·768	0·7605	523	526	−3
F	14 ·7	3186·5	0·705	14 ·0	2744·0	0·705	0·7050	485	486	−1

TABLE II.

Line.	$\frac{r_b}{r}$			Length of undulations.	
	First experiment.	Second experiment.	Mean.	According to Becquerel.	δ.
B	1	1	1	688	
D	0·949	0·954	0·9515	655	+66
E	0·910	0·919	0·9145	629	+103
F	0·891	0·893	0·8920	613	+127

The length of the waves of the various colours was determined by assuming as a basis for the red at the line B, the length given by Schwerd, viz. 0·000688 millimetres; this number was multiplied by the proportion of the thickness, *i. e.* by the inverse cubic proportion of the radii for the ring of the red, and that of the colour observed each time. The numbers used by M. E. Becquerel, deduced from Fresnel's experiments, differ considerably from those of Schwerd, as 0·000645 millim. is given as the length of the waves in the red at the line B, which however corresponds to a red between C and D. The results obtained show unequivocally, that the thickness of the ring is in inverse proportion to the cube of the radii; in the second table, for the sake of comparison, a calculation is given of the length of the undulations, as found from my admeasurements, but following Becquerel's principle, that the thicknesses are inversely proportional to the first powers of the radii. The difference is so great, that in the first series of experiments, in which the radius of the line B was $=13'''\cdot 1$, that for the line F would be $=18'''\cdot 1$ instead of $14'''\cdot 7$, to allow of 0·000486 being obtained as the required length of the undulations, a difference which cannot in any way be attributed to an error of observation.

To allow of our comparing the different systems of rings with each other, a question must first be answered which M. Becquerel considered as already determined. Thus, if the index of refraction of the peroxide of lead lies between that of the subjacent metallic plate and that of the air, the incident ray in each case is reflected from a denser into a rarer medium, and thus the minima of the effects of the light when monochromatic illumination is used, will occur when the thicknesses of the reflecting films are as the even numbers; if however the film of peroxide of lead is more highly refractive than the metallic plate, the relative thicknesses are as the uneven numbers. I have not succeeded in finding the peroxide of lead among those substances, the refractive indices of which have been given by any experimenters; and as M. Becquerel does not state whether he was led to the assumption that the refractive index of the peroxide of lead lies between that of the air and the metal, by earlier experiments, his own admeasurements, or mere suppositions, and as such an assumption can by no means be made *à priori*, I endeavoured to determine this value myself. A film of peroxide of lead was precipitated upon a gilt plate of silver until it had assumed a uniform brown colour. It was then capable of taking a tolerable polish. The substance polarized a ray of light reflected from its surface, so nearly rectilinearly, that the angle of polarization could be determined with tolerable accuracy. To control it, a beam polarized at an azimuth of 45° was made to fall upon such a surface of peroxide of lead, the reflected beam was again reflected by a second surface of the same kind, and the angle of incidence at which the elliptic polarization most completely passed into the linear sought for. Both methods gave very nearly the same result, *i. e.* the mean for the angle of incidence at which the maximum of polarization occurs was 63°. Hence the refractive index of the peroxide of lead is $=\text{tang } 63° = 1\cdot 964$, which is nearly equal to that of the sulphate and carbonate of lead, and is much less than that of the metal.

If therefore the radii of the dark and light rings, when monochromatic illumination is used, are measured, the even products of the cubes of the radii in the case of the dark rings, and the uneven products in the light rings should yield nearly constant values. To produce monochromatic illumination I used the flame of alcohol, into the wick of which common salt had been rubbed. This light is undoubtedly more pure than the red light which is produced by coloured glass. The following tables will show how far experiment approximates to calculation:—

Coloured rings upon silver gilt.

TABLE III.

Battery of Three Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	15'''·8	3944·31	1	3944·31	*	15·8
2.	13 ·2	2299·97	3	6899·91	*	39·6
3.	11 ·6	1560·70	5	7804·50	− 88 97	58·0
4.	10 ·6	1191·02	7	8337·14	+343·67	74·2
1. light	14 ·7	3186·52	2	6373·04	*	28·4
2.	12 ·4	1906·62	4	7626·48	−366·99	49·6
3.	11 ·1	1367·63	6	8205·78	+212·31	66·6
Mean ...	......	......	...	7993·47		

TABLE IV.

Battery of Four Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	16'''·4	4410·94	1	4410·94	*	16·4
2.	13 ·4	2406·10	3	7218·30	*	40·2
3.	12 ·0	1728·00	5	8640·00	−444·98	60·0
4.	11 ·0	1331·00	7	9317·00	+232·02	77·0
5.	10 ·2	1061·21	9	9550·89	+465·91	91·8
1. light	15 ·5	3723·87	2	7447·74	*	31·0
2.	12 ·8	2087·15	4	8348·60	−736·38	51·2
3.	11 ·5	1520·87	6	9125·22	+ 40·24	69·0
4.	10 ·6	1191·02	8	9528·16	+443·18	84·8
Mean ...	......	......	...	9084·98		

TABLE V.

Battery of Eight Elements.

Rings.	*r*.	r^3.	*n*.	$n.r^3$.	Δ.	*n.r*.
1. dark	19‴·6	7529·54	1	7529·54	*	19·6
2.	17 ·2	5088·45	3	15265·35	*	51·6
3.	15 ·7	3869·89	5	19349·45	+ 128·47	78·5
4.	14 ·2	2863·29	7	20043·03	+ 822·05	99·4
1. light	18 ·6	6434·86	2	12869·72	*	37·2
2.	16 ·4	4410·94	4	17643·76	−1577·22	65·6
3.	14 ·9	3307·95	6	19847·70	+ 626·72	89·4
Mean ...	......	......	...	19220·98		

TABLE VI.

Battery of Eight Elements.

Rings.	*r*.	r^3.	*n*.	$n.r^3$.	Δ.	*n.r*.
1. dark	15‴·6	3796·42	1	3796·42	*	15·6
2.	13 ·1	2248·09	3	6744·27	*	39·3
3.	11 ·6	1560·90	5	7804·50	− 31·51	58·0
4.	10 ·4	1124·86	7	7874·02	+ 38·01	72·8
5.	9 ·6	884·74	9	7962·66	+126·65	86·4
1. light	14 ·4	2983·10	2	5966·20	*	28·8
2.	12 ·4	1906·62	4	7626·48	−209·53	49·6
3.	11 ·0	1331·00	6	7986·00	+149·99	66·0
4.	9 ·9	970·30	8	7762·40	− 73·61	79·2
Mean	......	......	...	7836·01		

TABLE VII.

Battery of Eight Elements.

Rings.	*r*.	r^3.	*n*.	$n.r^3$.	Δ.	*n.r*.
1. dark	17‴·4	5268·02	1	5268·02	*	17·4
2.	14 ·4	2983·10	3	8949·30	*	43·2
3.	12 ·5	1933·12	5	9665·60	− 92·96	62·5
4.	11 ·2	1405·93	7	9841·51	+ 82·95	78·4
5.	10 ·3	1092·73	9	9834·57	+ 76·01	92·7
6.	9 ·65	898·63	11	9884·93	+126·37	106·15
7.	9 ·1	753·57	13	9796·41	+ 37·85	118·3
8.	8 ·65	647·21	15	9708·15	− 50·41	129·75
1. light	15 ·4	3652·26	2	7304·52	*	30·8
2.	13 ·4	2406·10	4	9624·40	−134·16	53·6
3.	11 ·7	1621·81	6	9730·86	− 27·70	70·2
4.	10 ·7	1225·04	8	9800·32	+ 41·76	85·6
5.	9 ·9	970·29	10	9702·99	− 55·57	99·0
6.	9 ·35	817·40	12	9808·80	+ 50·24	112·2
7.	8 ·85	693·15	14	9704·15	− 54·41	123·9
Mean ...	......	......	...	9758·56		

TABLE VIII.

Battery of Twelve Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	18‴·3	6128·49	1	6128·49	*	18·3
2. ……	16 ·2	4251·53	3	12754·59	*	48·6
3. ……	14 ·55	3080·27	5	15401·35	−171·54	72·75
4. ……	13 ·1	2248·09	7	15736·63	+163·74	91·7
5. ……	12 ·1	1771·56	9	15944·04	+371·15	108·9
6. ……	11 ·2	1405·93	11	15465·23	−107·66	123·2
1. light	17 ·1	5000·21	2	10000·42	*	34·2
2. ……	15 ·6	3796·41	4	15185·64	−387·25	62·4
3. ……	13 ·7	2571·25	6	15428·10	−144·79	82·2
4. ……	12 ·55	1976·65	8	15813·20	+240·31	100·4
5. ……	11 ·6	1560·90	10	15608·96	+ 36·07	116·0
Mean …	……	……	…	15572·89		

TABLE IX.

Battery of Twelve Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	21‴·5	9938·37	1	9938·37	*	21·5
2. ……	18 ·3	6128·48	3	18385·44	*	54·9
3. ……	16 ·3	4330·74	5	21653·70	+158·78	81·5
4. ……	14 ·5	3048·62	7	21340·34	−154·58	101·5
1. light	19 ·6	7529·54	2	15059·08	*	39·2
2. ……	17 ·4	5268·02	4	21079·08	−422·84	69·6
3. ……	15 ·4	3652·26	6	21913·56	+418·64	92·4
Mean …	……	……	…	21494·92		

TABLE X.

Battery of Twelve Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	19‴·4	7301·38	1	7301·38	*	19·4
2. ……	17 ·0	4913·00	3	14739·00	*	51·0
3. ……	15 ·1	3442·95	5	17214·75	+ 78·12	75·5
4. ……	13 ·5	2460·37	7	17222·59	+ 85·96	94·5
5. ……	12 ·4	1906·62	9	17159·58	+ 22·95	111·6
6. ……	11 ·6	1560·89	11	17169·79	+ 33·16	127·6
1. light	18 ·4	6229·50	2	12459·00	*	36·8
2. ……	16 ·1	4173·28	4	16693·12	−443·51	64·4
3. ……	14 ·2	2863·29	6	17179·74	+ 43·11	85·2
4. ……	12 ·9	2146·69	8	17173·52	+ 36·89	103·2
5. ……	12 ·0	1728·00	10	17280·00	+143·37	120·0
Mean …	……	……	…	17136·63		

Table XI.

Battery of Sixteen Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	15‴·8	3944·31	1	3944·31	*	15·8
2. ……	13 ·3	2352·77	3	7058·31	*	39·9
3. ……	11 ·8	1642·93	5	8214·65	− 19·94	59·0
4. ……	10 ·6	1191·02	7	8337·14	+102·55	74·2
5. ……	9 ·7	912·67	9	8214·03	− 20·56	87·3
1. light	14 ·0	2744·00	2	5488·00	*	28·0
2. ……	12 ·7	2048·38	4	8193·52	− 41·07	50·8
3. ……	11 ·1	1367·63	6	8205·78	− 28·81	66·6
4. ……	10 ·1	1030·30	8	8242·40	+ 7·81	80·8
Mean …	……	……	…	8234·59		

Coloured rings upon platinum.

Table XII.

Battery of Six Elements.

Rings.	r.	r^3.	n.	$n.r^3$.	Δ.	$n.r$.
1. dark	9‴·5	857·37	1	857·37	*	9·5
2. ……	7 ·6	438·98	3	1316·94	*	22·8
3. ……	6 ·6	287·50	5	1427·50	− 17·95	33·0
4. ……	6 ·0	216·00	7	1512·00	+ 56·55	42·0
1. light	8 ·6	636·06	2	1272·12	*	17·2
2. ……	7 ·0	343·00	4	1372·00	− 83·45	28·0
3. ……	6 ·3	250·05	6	1500·30	+ 44·85	37·8
Mean …	……	……	…	1455·45		

In these Tables we find an undoubted agreement of the values $n.r^3$; the radii of the external rings alone give different results, and are not therefore made use of in calculating the mean. These rings are denoted in the Tables by a star. Even at the second light ring complete confirmation of the law of the inverse cubic proportion of the radii commences. The cause of the difference in the breadth of the above most external rings is twofold. First, the entire calculation upon which it is based depends upon the assumption that the anode is an infinite plane. If this assumption is not satisfied, the curves of the current deviate further from the rectilinear form, their concave sides being turned towards the anode; thus the film of peroxide of lead must evidently be of sufficient thickness, even at a less distance than is expected from calculation, to correspond to the required multiple of the quarters of the length of the undulations of the yellow light, *i. e.* the radius of the ring will be less. Secondly, the charge which the plates acquire is not uniform throughout, but in the middle, where the intensity of the current is greatest, it also is greater than at the margins of the plate. Hence the precipitate is relatively more copious at the margins, and consequently the radii of the rings are, from this cause, smaller than we should anticipate from calculation. However, since the charges* are not in direct proportion to the intensities of the current, but increase asymptotically from a certain intensity, this influence may be made to disappear in proportion as the intensity of the battery used is increased. For this reason the experiments in the above Tables are arranged according to the number of elements of which the battery used on each occasion consisted. It is at once seen, that, with few exceptions, the greater the number of active elements the more the experiments approximate to calculation. In the first series of experiments, where the unessential resistance is somewhat considerable in proportion to the essential, the influence of the charge becomes so evident, that the differences of the values found for nr^3 from the mean, evidently follow a definite law. In the subsequent series of experiments this conformity to a law ceases to be perceptible.

M. du Bois-Reymond has already observed, that the charge upon the plates is less to be ascribed to a deposition of oxygen than to the greater or less thickness of the film of peroxide of lead. It might appear inconsistent with the principles of the contact-theory, that a greater electromotive activity should be ascribed to a thicker than to a thinner film. But this is not really the case. When, for instance, a plate of gold is connected, as the anode, with the platinum plate of a Grove's battery, and the deposition of the peroxide of lead commences, this substance will not form a coherent film, but its particles will be arranged in a more diffused manner the less the intensity of the current is at this part of the plate. Hence the electrolyte serves in some parts to connect the zinc and platinum circuit (which is composed of the zinc of the Grove's battery and the platinum point forming the cathode) with the gold and platinum circuit, sometimes to connect the former with a circuit of platinum and peroxide of lead. The latter action, which opposes the direction of the battery originally used in the same manner as a charge, will become more evident the nearer the particles of the peroxide of lead are situated as regards each other, *i. e.* the thicker the film is which is already deposited.

For the purpose of investigating which of the two sources

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* By charge (Ladung) the author evidently means the cause of that electromotive reaction to which the term polarization is more frequently applied.—Ed. *Phil. Mag.*

of error, the finiteness of the plate or the charge, exerts most influence upon the result of the experiment, I had a circular disc of brass, a foot in diameter, thickly gilt and polished. The rings which I obtained upon it extended so as to form a radius of more than three inches, but then exhibited almost the same irregularities in the proportions of the radii as those on the smaller plates. The influence of the charge is thus far greater than that of the finiteness.

As M. Becquerel in his experiments made use of German silver as the receptacle of his coloured rings, it appeared to me desirable to compare my admeasurements on plates of the noble metals with some made under exactly the same circumstances as those of the French philosopher; for in these plates of German silver we should not expect the same disturbing influence of the polarization as in surfaces of gold and platinum, which receive the charge to a far greater extent. The coloured rings which I procured upon plates of German silver, appeared at first of a very brilliant colour. This however, on account of the oxidizability of the metals, did not continue long; the surfaces became spotted, and the rings, on monochromatic illumination, were marked with curves, differing from the circular form by numerous indentations. The German silver plates, like the other ignoble metals, were not well-calculated for the present object, so that I made no further use of them; however, the experiments made with them convinced me that here also the law of the cubes as the proportion of the radii is correct, and on account of the slight influence exerted by the charge, it is attained in this case with greater accuracy than with plates of the noble metals. An example will serve as a proof of this:

TABLE XIII.

Coloured rings upon German silver.

Rings.	*r*.	r^3.	*n*.	$n.r^3$.	Δ.	*n.r*.
1. dark	21‴·9	10493·5	1	10493·5	*	21·9
2.	17 ·8	5639·7	3	16917·2	−145·6	53·4
3.	15 ·2	3511·8	5	17559·0	+496·2	76·0
4.	13 ·5	2460·4	7	17222·6	+159·8	121·5
1. light	20 ·2	8242·4	2	16484·8	−578·0	40·4
2.	16 ·2	4253·5	4	17014·0	− 48·8	97·2
3.	14 ·2	2863·3	6	17179·4	+116·6	113·6
Mean ...	......	......	...	17062·8		

Lastly, a third cause might urge the outermost rings nearer to the centre. Thus, when the point forming the cathode is not approximated so closely to the anode that this distance

(m) is infinitely small compared with the radii, we do not have $n.r^3=$ const., but $n(r^2+m^2)^{\frac{3}{2}}=$ const. In the above experiments the distance m is entirely neglected. I therefore made some experiments in which the point of the cathode was first placed in immediate contact with the anode, and then by a support, the clamp which supported the anode was moved, and the elevation measured. In these experiments the rings augmented in breadth as m increased, and consequently were less numerous. In the case of the value $m=\infty$ the plates obtained must naturally be of one perfectly uniform colour, and in fact, by removing the cathode quite away from them, we can cover small plates with a tolerably equal coating. These colours are still better obtained when the point is placed at a considerable distance above the plate; it is not advisable to replace it by a plate, because the precipitate is then formed very violently and with the colour of the peroxide of lead itself, whilst with a less intensity of the current used we can regulate its formation.

With regard to m I obtained the following results with two plates:—

TABLE XIV.

Rings.	$r.$	$m.$	$(r^2+m^2)^{\frac{3}{2}}.$	$n.$	$n(r^2+m^2)^{\frac{3}{2}}.$	Δ.	$n.r.$
1. dark	15‴·8	2·9	4145·30	1	4145·30	*	15·8
2.	12 ·4	...	1906·65	3	5719·95	−136·98	37·2
3.	10 ·2	...	1192·45	5	5962·25	+105·32	51·0
1. light	13 ·8	...	2804·07	2	5608·14	*	27·6
2.	11 ·2	...	1472·15	4	5888·60	+ 31·67	44·8
Mean ...	......	...	......	...	5856·93		

TABLE XV.

Rings.	$r.$	$m.$	$(r^2+m^2)^{\frac{3}{2}}.$	$n.$	$n(r^2+m^2)^{\frac{3}{2}}.$	Δ.	$n.r.$
1. dark	11‴·2	3·7	1641·09	1	1641·09	*	11·2
2.	8 ·6	...	820·59	3	2461·77	+ 8·65	25·8
3.	7 ·0	...	496·36	5	2481·80	+ 28·68	35·0
1. light	9 ·7	...	1118·94	2	2237·88	*	19·4
2.	7 5	...	603·95	4	2415·80	− 37·32	30·0
Mean ...	......	...	......	...	2453·12		

The approximation in the case of the second dark ring is thus perfect, and even in the first light one tolerably perceptible. Further distances of the point from the plate than those detailed, were not applicable, because the error which arises from its finiteness must then constantly become visible.

Both in these and in the previous series of experiments, I have detailed, in the last column, those values which would be equal to a constant quantity, if the radii were really in inverse proportion to the thickness of the deposited films. Each of the tables shows how little this law is confirmed by my experiments; the progress of one value to another is in fact so considerable, that *e.g.* in Table VII. the numbers vary from 17·4 to 129·75. To decide what circumstances could have exerted so remarkable an influence upon the experiments of M. E. Becquerel, that when taken into consideration his experiments might be made to accord with those just detailed, is a problem for the solution of which I do not feel myself adequate.

Dr. W. Beetz.

III. *On the Attraction of Spheroids.*

By Henry Hennessy, *Esq.*[*]

1. In the mathematical theory of the attractions of spheroids, a well-known theorem occurs, which was first pointed out by Laplace, and of which he has given demonstrations in the third book of the *Mécanique Céleste*, and in the *Connaissance des Temps* for 1820. The remarkable character, as well as the fundamental importance of this theorem, excited several eminent geometers to thoroughly examine the demonstrations given by its discoverer. The result was, that not only were these demonstrations considered defective, but also, according to Lagrange and Ivory, the generality of the theorem was thought incapable of being established.

Notwithstanding these objections, the truth of the theorem in question seems never to have been doubted by its illustrious discoverer; and similar views appear to be entertained by some of his successors. Among the rest, M. de Pontécoulant presents, in the twentieth article of the fifth book of his *Théorie Analytique du Système du Monde*, a demonstration of this theorem, which he considers free from any serious objection. On closely examining his investigation I perceived two errors, which in the end compensate each other; and it also appeared that some points in it were capable of being more rigorously established. I hope, therefore, that the following demonstration, which is free from the defects alluded to, will not be considered useless.

To render this improved form of M. De Pontécoulant's

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[*] Communicated by the Author.

demonstration more easily comparable with that which he has given, I shall adopt the same notation. Hence if

$$V=\int_0^{\pi}\int_0^{2\pi}\int_0^{R}\frac{\rho r'^2 dr' d\omega' \sin\theta' d\theta'}{\sqrt{r^2-2rr'[\cos\theta\cos\theta'+\sin\theta\sin\theta'\cos(\omega-\omega')]+r'^2}},$$

V is a function, the differential coefficient of which with respect to r, the radius drawn from the origin of the co-ordinates to an attracted point, would represent the attraction of any spheroidal mass on that point in the direction of r. The distance from the origin of an elementary parallelopiped of the spheroid is represented by r', and the density of this parallelopiped by ρ; θ' represents the angle between r' and any one of the co-ordinate axes, and ω' the angle comprised between the projection of r on the plane of the other two axes, and the direction of either of them. The angles θ and ω refer to r in the same way that θ' and ω' refer to r'. The limit π refers to θ', 2π to ω', and R, the radius of any point on the surface of the spheroid, to r'.

2. The theorem to be proved may be thus stated.

If a material point be situated on any part of the surface of a spheroid having the radius r at that point, and if the spheroid differ so little from a sphere having the radius a, that $r=a(1+ay)$, y being a rational function of μ and ω, and α a constant so small that any terms in the development of r multiplied by its second power may be neglected, then for the point in question,

$$V+2a\frac{dV}{dr}=-\frac{4\pi}{3}a^2. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1.)$$

But*

$$-V=\frac{4\pi a^3}{3r}+\frac{\alpha a^3}{r}\left(U_0+U_1\frac{a}{r}+U_2\frac{a^2}{r^2}+\&c.\right), \quad . \qquad (a.)$$

U_0, U_1, &c. satisfying the equation of Laplace's coefficients. If, for brevity, the term containing α as a factor be represented by u, we shall have, after differentiating and multiplying the result by $2r$,

$$2r\frac{dV}{dr}=-\frac{8\pi a^2}{3r}+2r\frac{du}{dr};$$

and adding (a.) to this,

$$V+2r\frac{dV}{dr}=-\frac{4\pi a^2}{3r}+u+2r\frac{du}{dr}. \quad . \quad . \quad . \qquad (2.)$$

In (a.), $\frac{4\pi a^3}{3r}$ is evidently the term in the attracting force of the spheroid depending upon the sphere with the radius a;

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* *Théorie Analytique*, &c., tom. ii. p. 372.

and therefore the term containing α or u must be that corresponding to the difference between the spheroid and the sphere.

If we make $\cos\theta=\mu$,

$$V=\int_{+1}^{-1}\int_0^{2\pi}\int_0^{R}\frac{\rho r'^2 dr' d\omega' d\mu'}{\sqrt{r^2-2rr'[\mu\mu'+\sqrt{1-\mu^2}\sqrt{1-\mu'^2}\cos(\omega-\omega')]+r'^2}};$$

and

$$u=\int\frac{\delta m'}{f},$$

δm being an element of the difference between the spheroid and the sphere, and f^2 the quantity under the radical.

After substituting for r its value,

$$u+2a\frac{du}{dr}=\int\left(\frac{1}{f}+2r\frac{d\frac{1}{f}}{dr}\right)\delta m'.$$

But if

$$\gamma=\mu\mu'+\sqrt{1-\mu^2}\sqrt{1-\mu'^2}\cos(\omega-\omega'),$$

$$\frac{d\frac{1}{f}}{dr}=\frac{a\gamma-r}{f^3}.$$

Hence

$$u+2a\frac{du}{dr}=\int\frac{(a^2-r^2)}{f^3}\delta m'.$$

But $\delta m'$ is a prism of homogeneous matter included in the prolongation of r' between the surfaces of the spheroid and sphere. Hence

$$\delta m'=\frac{1}{3}(R^3-a^3)d\mu' d\omega',$$

and therefore

$$u+2a\frac{du}{dr}=a^3\alpha\int_{+1}^{-1}\int_0^{2\pi}\frac{(a^2-r^2)y' d\mu' d\omega'}{f^3},$$

in which y' is the same function of μ' and ω' that y is of μ and ω, or so that when

$$y=F(\mu,\omega),\quad y'=F(\mu',\omega').$$

When the attracted point is at the surface of the spheroid $r=a(1+\alpha y)$, and unless $\gamma=1$,

$$u+2a\frac{du}{dr}=0.$$

Whence the theorem (1.) holds at least, unless

$$\mu\mu'+\sqrt{1-\mu^2}\sqrt{1-\mu'^2}\cos(\omega-\omega')=1.$$

Let

$$\mu' = \mu \pm h,\ \omega' = \omega \pm k,$$

then

$$1 - \mu^2 \pm \mu h = \sqrt{(1-\mu^2)(1-\mu^2 \pm 2\mu h - h^2)} \cos k;$$

or

$$\cos^2 k = 1 + \frac{h^2}{(1-\mu^2)[1-(\mu \pm h)^2]}.$$

h^2 is necessarily positive, μ must either be equal to or less than unity. If $\mu = 1$, the second term in the expression for $\cos^2 k$ would be infinite for any finite value of h. If $\mu < 1$, $\mu \pm h$ must either be equal to or less than unity. If $\mu \pm h = 1$, the second term would be again infinite for any finite value of h. If both μ and $\mu \pm h$ are less than unity, the denominator of the second term will be finite and positive, and therefore the above equation could not be then satisfied for any finite value of h. Hence in general $h=0$, and $\mu' = \mu$, and therefore $\cos k = \pm 1$.

The double sign, with which the value of $\cos k$ is affected, shows that, like $\cos \omega'$, it should be extended to both sides of the origin of the co-ordinates; and when thus interpreted, we may satisfy the above equation by making $k=0$, or $k = 2\pi$, and consequently $\omega' = \omega$. Therefore, when $\gamma = 1$,

$$y = y',$$

and

$$u + 2a\frac{du}{dr} = a^3\alpha(a^2 - r^2) y \int_{+1}^{-1}\int_0^{2\pi} \frac{d\mu' d\omega'}{f^3}.$$

3. If, which is permitted, the origin of the co-ordinates be so taken as to make $\mu = 1$,

$$f = \sqrt{a^2 - 2ar\mu' + r^2},\quad \frac{df}{d\mu'} = \frac{-ar}{f},$$

then

$$\int_{+1}^{-1}\int_0^{2\pi} \frac{d\mu' d\omega'}{f^3} = 2\pi \int_{+1}^{-1} \frac{d\mu'}{f^3} = \frac{2\pi}{ar}\left\{\frac{1}{\pm(a+r)} - \frac{1}{\pm(a-r)}\right\}.$$

Whence we have the two values

$$\int_{+1}^{-1} \frac{d\mu'}{f^3} = \frac{-2r}{a^2 - r^2},\quad \int_{+1}^{-1} \frac{d\mu'}{f^3} = \frac{-2a}{a^2 - r^2},$$

and therefore

$$u + 2a\frac{du}{dr} = -4\pi a^2 \alpha y,$$

or

$$u + 2a\frac{du}{dr} = -\frac{4\pi a^3 \alpha y}{r}.$$

The second of these expressions becomes identical with the first, when for r its value is substituted, and terms of the second order neglected. Substituting in (2.) it becomes

$$V+2a(1+\alpha y)\frac{dV}{dr}=-\frac{4}{3}\pi a^2(1+2\alpha y).$$

But

$$\frac{dV}{dr}=-\frac{4\pi a}{3}+\frac{8\pi}{3}a\alpha y+\frac{du}{dr};$$

consequently the preceding expression becomes

$$V+2a\frac{dV}{dr}=-\frac{4}{3}\pi a^2,$$

which is the theorem announced by Laplace.

This theorem may be easily verified with the aid of that demonstrated by Poisson in the *Théorie de la Chaleur*, pp. 213–221[*].

IV. *On a Function of the Red Corpuscles of the Blood, and on the Process of Arterialization. By* G. Owen Rees, *M.D., F.R.S.*[†]

ALTHOUGH it has been long known that the contact of oxygen is necessary in order to bring about the change from venous to arterial blood, we are not yet in possession of evidence calculated to explain satisfactorily the manner in which that change is effected.

The experiments of Dr. Stevens, published some years ago, proved that another cause than oxygenation was sufficient for the production of a bright arterial tint in venous blood; and it is now universally admitted, in accordance with those experiments, that neutral salts possess that power even when the contact of air is carefully avoided.

Notwithstanding the similarity of effect which they were capable of producing, it appeared doubtful that any connexion would ever be traced between these two causes apparently of so opposite a character.

The absorption of oxygen was known as necessary to the preservation of animal life; and it seemed, therefore, that neutral salts ought more properly to be regarded as merely simulating by their action an effect indispensable to existence, rather than as indicating a function necessary to its maintenance.

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[*] See Pratt's Mechanical Philosophy, pp. 166–171.

[†] Communicated by the Author, having been read before the Royal Society, June 3, 1847.

The experiments I am about to detail are, however, I believe, sufficient to prove that the two causes referred to bear the closest relation to each other; for the action of oxygen will be shown to be the more remote cause of the phænomenon, while the immediate cause for the production of the bright arterial colour consists in the action of a neutral salt, formed by the contact of oxygen during the process of respiration.

When experimenting on venous blood some few months ago, I was struck with a peculiar garlic odour evolved by the corpuscles when they were burst by the sudden addition of water. Reasoning on this fact, and being aware of the existence of phosphorized fat as a constituent of the blood, I resolved if possible to ascertain at some future period the more exact conditions of its presence, and the relation it might bear to the function of respiration. It was not difficult to imagine that the exposure of venous blood to the action of the atmosphere at a temperature of 98°, and, as occurs during respiration, over an enormously diffuse surface, would completely effect the combustion of any phosphorus it might contain; and, moreover, that this elementary body, if existing dissolved in fatty matters, would consume together with its solvent, and thus afford as the result of combustion three compounds, known both in the characters of excretions and constituents of animal bodies, viz. carbonic acid, water, and phosphoric acid.

Knowing that carbonic acid and water were excreted by the lungs, and also that it was now very generally admitted that more oxygen was absorbed during respiration than could be accounted for by the carbonic acid expired, I was greatly encouraged to prosecute my inquiries, and put the theory to the test.

Now if it were true that during respiration phosphorized fats are consumed in the venous blood-globules, the phosphoric acid formed by the contact of oxygen must unite with the blood, for none of this acid is expired; and as the globules or corpuscles float in an alkaline liquor, this result ought to be brought about with the formation of an alkaline phosphate in the liquor sanguinis of arterial blood.

Thus, while the venous blood-globules should contain the phosphorized fats in quantity, the serum of venous blood should contain far less alkaline phosphate than that from the blood of the arteries.

The type of the venous blood would then be—

1. A globule containing phosphorized fat.
2. A liquor sanguinis deficient in alkaline phosphate.

The type of the arterial blood would be—

1. A globule containing no phosphorized fat.

What does phosphorized fat mean? –

2. A liquor sanguinis containing a large proportion of alkaline phosphate.

The proof consisted therefore in the examination of these separate parts of the blood. The serum of course would suffice for the examination of the liquor sanguinis, as it would contain all the alkaline salts. The clot would serve for the examination of the fats.

Exp. 1. A quantity of venous blood was obtained from the external jugular vein of a cur dog, and subsequently arterial blood was drawn from the carotid artery of the same animal. These specimens were allowed to coagulate. The serum which separated was in both cases evaporated to dryness, and then incinerated in a platinum dish. Distilled water was poured upon the ashes (which were not completely decarbonized), and the solution so formed poured off and evaporated. Dilute nitric acid was next added to the dry residues, when lively effervescence occurred in both cases. The acid solution, after being gently warmed, was now tested for phosphoric acid as follows.

Nitrate of silver was added, which caused an immediate and copious precipitate of chloride of silver. When the solution was no longer affected by the further addition of this reagent, the precipitate was allowed to subside.

The supernatant clear liquid was next poured off and neutralized by the addition of a solution of caustic ammonia; when, in the case of the arterial blood, a plentiful precipitate of the yellow phosphate of silver was observed, while the solution obtained from venous blood yielded no evidence of the presence of phosphoric acid, though tested in precisely the same manner.

Exp. 2. Blood was drawn from the external jugular vein of a cat while under the influence of æther. Half this blood was set aside, the other half was arterialized by exposure over the surface of several dishes. On separately treating the serum of these two portions of blood, and testing them for phosphoric and carbonic acids, as described in Exp. 1, both proved to contain an alkaline carbonate; but the serum from the blood which had not been exposed to air gave no evidence of phosphate, while that which had been arterialized by exposure yielded a copious precipitate of yellow phosphate of silver on the addition of the nitrate and caustic ammonia.

Exp. 3. Blood was drawn from the median cephalic vein of a man, the subject of epilepsy. He had dined about half an hour. On coagulation, the serum of this blood was milky, owing to the presence of chyle. I obtained evidence of the existence of an alkaline phosphate in this serum. The carbonate was abundant in the ashes. I now digested a portion of the clot of this blood in æther, and found that the fats so extracted yielded an ash on incineration, having a very powerful acid reaction. The fats obtained from the *serum* by means of æther yielded an alkaline ash.

Exp. 4. Blood was drawn from the temporal artery of a man suffering from a disease of the eyes. It was brightly arterial, and did not buff or cup on coagulation. On examination, the serum of this blood gave no evidence of the presence of an alkaline carbonate, but the alkaline phosphate was in abundance.

The fats extracted from the clot by means of æther afforded an ash possessing a very alkaline reaction. The fats obtained from the serum also yielded an alkaline ash.

Exp. 5. A spaniel dog was bled from the jugular vein. Half this blood was immediately corked up in a bottle, the other half was arterialized by passing air through it, by means of a glass tube affixed to a pair of common bellows. On examining the serum from these specimens, that obtained from the portion of blood which had been arterialized, as above described, gave plentiful indications of an alkaline phosphate, and none whatever of an alkaline carbonate; while that from the other portion gave no indications of an alkaline phosphate, but very satisfactory evidence of the presence of an alkaline carbonate in the ashes. very probable !

Exp. 6. A portion of blood was drawn from the jugular vein of the same dog that had served for Exp. 5. Half this blood was immediately shaken up with an equal bulk of rectified æther.

The mixture was set aside for twenty-four hours, and the æther which had collected on the surface was then poured off. The mass of blood below was now subjected to the action of a current of air passed briskly through it. No brightening of colour took place now that the æther had removed the fatty matters.

Exp. 7. The serum was poured off from the other half of the blood drawn for Exp. 6, and the crassamentum broken up in a dry basin. Distilled water was now added; and a large quantity of corpuscles being thus burst, a solution of red colouring matter was obtained.

This mixture was filtered through blotting-paper, and the clear solution placed in a vessel containing plates of metallic zinc*. The metal precipitated the colouring matter after a

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* In order to effect this precipitation, it is necessary either largely to dilute the solution, or to use a very large surface of zinc, otherwise the precipitate will adhere to the zinc, and prevent further action. A piece of platinum affixed to the zinc assists the action greatly.

few hours in such a manner as to decolorize the fluid above, a precipitate falling composed of the hæmatosine combined with oxide of zinc. This precipitate was collected and digested with æther. On evaporating the æthereal solution, fatty matter was obtained in considerable quantity.

Exp. 8. Venous blood was drawn from the external jugular of a large poodle, and subsequently arterial blood from the carotid of the same animal. The serum was full of chyle in both specimens.

The presence of an alkaline carbonate in the ashes of the serum was ascertained in both cases, but in larger quantity in the venous than arterial serum. The reverse was the case with regard to the alkaline phosphate, which, though present, existed in smaller proportion in the venous than the arterial serum.

The fats of the arterial crassamentum yielded an alkaline ash. That obtained from the venous crassamentum gave an acid reaction.

In the experiments on serum which I have described above, I have generally used a very small quantity of the fluid. My principal object having been to show the difference in proportion of alkaline phosphate existing between venous and arterialized blood, I always took care to use a larger quantity of the former than of the latter in these comparative experiments, in order that my theory, which presumed an excess in arterial serum, might be the more severely tested, and that the alkaline phosphate might be prominently shown in the venous serum if it had happened to be present, even in proportion approaching that in which it existed in arterial serum. When larger quantities of venous serum are operated upon, an alkaline phosphate may be detected. I however obtained no evidence from the quantity I used, viz. from twenty-five to forty grains.

The results now stated appear sufficiently to prove the correctness of the theory I have advanced. Thus, not only does arterial serum prove to contain more alkaline phosphate than that from the venous blood, but we are able, by artificially arterializing a portion of blood, to remove the phosphorus of the corpuscles into the liquor sanguinis by forming phosphoric acid, which is absorbed by the alkali contained in the fluid.

The fatty matter from the corpuscles of venous blood proves to contain phosphorus, yielding an acid ash on incineration. *Vide* Exp. 3. The fatty matter from the corpuscles of arterial blood yields an alkaline ash on incineration. *Vide* Exp. 4. The fatty matter of the corpuscles exists in close combination with the colouring matter, and in all probability is its most essential adjunct (*vide* Exp. 7); which goes far to prove the intimate chemical union of the two substances, inasmuch as the combination of zinc with hæmatosine yielded fatty matter to æther.

Exp. 6 would seem to prove, that when the phosphorized fatty matters of the venous blood are removed, it no longer admits of being arterialized by contact with oxygen. I am not inclined however to argue much on this experiment, finding that æther, when agitated with blood, possesses the power of effecting its complete disintegration; so much so, that blood which has been thus treated appears under the microscope as a congeries of crystals of well-defined form, and is entirely divested of its organic and corpuscular character.

The change which takes place in order to produce the arterial tint in the dark-coloured blood propelled into the lungs, may then be stated thus.

The vesicular corpuscles of the dark venous blood contain within their cases, and in combination with hæmatosine, a fatty matter charged with phosphorus. The venous blood on reaching the air-cells is exposed to the action of atmospheric oxygen. This gas now combines with the phosphorized fat, forming carbonic acid and water, which are exhaled, and phosphoric acid, which unites with the alkaline liquor sanguinis.

The liquor sanguinis forms tribasic phosphate of soda with this acid, a salt possessing an alkaline reaction. This union most probably occurs principally at the expense of the albuminate of soda, and is attended with results which it is not my intention now to consider. Suffice it to say, that phosphoric acid may be added to venous serum in quantity insufficient quite to neutralize its alkaline reaction, without any immediate obvious effect being produced.

The formation of tribasic phosphate of soda as a substitute for a corresponding proportion of the alkali in combination either with albumen or lactic acid, is a sufficient explanation of the bright colour of arterial blood as compared with that of the veins, as that basic salt possesses the power of brightening blood in a marked degree; and it is not improbable that the destruction of the phosphorus in the venous globules also contributes to the effect.

I have not had recourse to any quantitative examinations on this occasion, for several reasons.

Thus it is obvious that venous and arterial blood, though differing in colour, owing to the action of tribasic phosphate formed during respiration, are always more or less approaching each other in quality under varying conditions of the organism. The flow of chyle into the blood introduces a portion of phosphate into the general circulation, which has not been produced by the respiratory act, and which will be only eliminated slowly from the blood; and thus, immediately after food, this salt will be readily found in the veins as well as the arteries, though not to the same amount (*vide* Exp. 3); while, on the other hand, the alkaline lactate and albuminate of chyle (yielding an excess of alkaline carbonate as the result of the incineration of the serum of arterial blood) will be only gradually converted into basic phosphate as the respiratory act proceeds. This approach in character on the part of the two kinds of blood may be observed after repletion, when the blood of the large arterial trunks is darker than usual, and becomes sensibly brightened by exposure to air. Again, in animals kept without food, the blood drawn from the veins will be preternaturally bright, for the reason that respiration has been long carried on without access of chyle, and the alkaline phosphate is affecting the venous blood as the result of respiration.

An approach to the arterial tint has been observed by physiologists in the venous blood of animals kept long without food. The darkened colour of arterial blood, as produced by repletion, I have myself remarked, and have been enabled to produce a very obvious brightening by exposing it to air.

April 28, 1847.

Note.—The ash obtained from the serum of venous blood contains an alkaline carbonate; but if the crassamentum be incinerated with the serum, a carbonate is not necessarily present, because the phosphorized fats produce phosphoric acid during their combustion, and this will form a phosphate with the alkali to the exclusion of carbonic acid. It was in consequence probably of these conditions that Enderlin did not observe an alkaline carbonate in the ashes of blood. The deduction from his observations, that no albuminate or lactate of an alkali exists in the blood, is erroneous.

V. *Additional Observations on the subject of the foregoing Memoir. By* G. Owen Rees, *M.D., F.R.S.*

Guildford Street, Russell Square,
June 8, 1848.

My dear Mr. Phillips,

THE novelty of the view which I have put forward in the foregoing paper with reference to the respiratory function, induces me to address these few lines to you, in order that they may be published should you consider them to contain matter of sufficient interest.

It is not my wish or intention to argue upon differences of result between my own observations and those of previous experimenters, but merely to refer to one or two points contained in my paper, in order to prevent the occurrence of such misconceptions as some of my friends have already fallen into with respect to my views.

In the first place, then, I do not consider the serum or any part of the blood to contain an alkaline carbonate, *but only to yield such salt by incineration.* My theory for the production of the arterial tint has no reference to the decomposition of a carbonate in the venous blood, but to the production of the phosphate of soda, *by the decomposition of animal organic salts.*

Secondly, the analyses which have been made of arterial and venous blood, showing identity, or nearly so, of the ash obtained, are in no way opposed to my observations; for the reason that, when the whole blood (*clot and serum*) is incinerated, the process of ignition does just what respiration would do, and produces phosphoric acid from the phosphorized fats of the venous blood-corpuscles. The difference between the two kinds of blood is shown by the *incineration of the serum only* of the two kinds of blood; for then no corpuscles are present to afford phosphorus to the venous serum for the production of phosphoric acid.

I took great care, when making my comparative experiments, to guard against all fallacy which might arise from resolution of precipitates by excess either of acid or ammonia, &c. in testing with the silver salt. It is an experiment which I have been much accustomed to make when examining animal matters, and with the uses and peculiarities of which I have been long familiarized.

The very marked degree of difference which I have detected between venous and arterial serum, leads me to believe that the quantity of blood circulating is far less than is generally supposed.

It is much to be regretted that we have as yet no correct means of ascertaining this interesting point, and that it is scarcely possible, in the present state of our knowledge, even to propose a plan which is not replete with practical difficulties and theoretical objections.

Believe me to be, my dear Mr. Phillips,
Yours most truly,
G. Owen Rees.

VI. *On certain Phænomena of Capillary Attraction exhibited by Chloroform, the Fixed Oils, and other Liquids: with an inquiry into some of the causes which modify the form of the mutual surface of two immiscible liquids in contact with the walls of the vessel in which they are contained. By* WILLIAM SWAN, *F.R.S.E., Teacher of Mathematics, Edinburgh**.

DR. GEORGE WILSON has recently noticed a curious class of phænomena depending upon capillary attraction, which he has described in a paper which will appear in the Quarterly Journal of the Chemical Society of London for July 1848. The following are the principal facts he has observed.

When chloroform, bisulphuret of carbon, Dutch liquid ($C_4 H_4 Cl_2$), bromine, and several of the volatile oils whose specific gravity is above 1·000, are placed in glass vessels under water or acids, they assume a highly convex surface; while, on the other hand, under solutions of potash, soda or ammonia, their surface is sensibly flat. The object of this paper is to describe some results of a similar nature which I have obtained with other liquids, and also to inquire into the probable cause of the phænomena which have been observed.

Before attempting any explanation of the flattened surface of chloroform under alkaline solutions, it was natural to ascertain whether its surface might be considered as absolutely flat, or only apparently so; and at Dr. Wilson's request I undertook the examination of this point. Perfect flatness was scarcely to be expected; for among all possible relations of the mutually attractive forces of the two liquids and the walls of the containing vessel, the chance was evidently small that the particular relation necessary to produce a perfectly flat surface should actually exist; and even supposing the requisite conditions to be fulfilled, the slightest particle of foreign matter adhering to the glass might vitiate the result.

In order to avoid any fallacious appearances which might arise from the irregular transmission of light through ordinary glass vessels, I ground the ends of a tube of glass, about an inch in length and half an inch in internal diameter, and cemented to them pieces of parallel plate glass by means of Canada balsam. The axis of the tube being placed horizontally, it was half-filled with chloroform, and solution of caustic potash was added. On looking through the glass plates, the surface of the chloroform was seen to be sensibly flat, except close to the glass, where it was convex. At the points of contact of the tube with the glass plates the surface was concave; but this reversal of its curvature was evidently due to

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* Communicated by the Author.

the presence of the Canada balsam, for which the chloroform manifests a strong affinity, and which it dissolves rapidly. I have since observed the surface of the chloroform in a tube, having plates of glass held in contact with its ends by means of vulcanized India-rubber bands. In this case the eye was assisted by a lens magnifying about eight times, and the surface of the chloroform was again seen to be slightly depressed at its contact with the tube; but as the plates were attached without cement, there was no reversal of the surface of the chloroform at their junction with the tube. From these experiments it would seem that the effect of the alkali is not so much to alter the form of the curved surface, as to produce a curve on a smaller scale, and thus to diminish the distance to which the curvature extends from the sides of the vessel: so that a deep depression at the sides is so much reduced as only to be perceptible by minute inspection; and consequently the curvature, which formerly extended to a distance over the liquid, is now confined to such narrow limits as to leave the greater part of the surface sensibly flat.

As potash has a chemical affinity for chloroform which is not possessed by hydrochloric acid, it occurred to me to try whether, in other cases besides those observed by Dr. Wilson, a flat surface would be formed by liquids having a strong affinity for each other, and a curved surface by liquids having a feeble affinity.

The following are the principal results I have obtained:—

1. Olive oil floating on the surface of water or of hydrochloric acid, has its under surface convex, but with solution of potash its surface is apparently flat. It has also, in contact with alcohol or sulphuric æther, a much flatter surface than with water.

2. Spermaceti oil also exhibits a convex surface in contact with water and hydrochloric acid, and a flat surface with solution of potash.

3. Oil of cloves sinks in water, and has a highly convex surface, which becomes nearly flat on the addition of alcohol to the water; but the liquids mingle so readily, that their mutual surface is not very well defined.

4. Oil of cassia sinks in water; and its surface, which is very convex, is considerably flattened by adding potash or alcohol to the water. The addition of acid in the former case, or of water in the latter, restores the convexity of the surface.

5. Oil of lavender floats on water, with its under surface convex. If alcohol is poured upon it, its upper surface is nearly flat.

In all the instances now enumerated, as well as in those given in Dr. Wilson's paper, it will be observed, that when the liquids have a decided chemical affinity for each other, their mutual surface is flat, as in the case of the fixed oils with solution of potash, and the essential oils with alcohol; while the curved surface is assumed by liquids whose chemical affinity is either extremely feeble or entirely wanting, as is the case with the oils in contact with water or acids. The experiments therefore confirm Dr. Wilson's opinion, that the flatness of the surface results from the chemical affinity of the liquids; and it is therefore probable that this effect is due to a strong mutual attraction of their particles.

But before coming to this conclusion, it will be proper to examine other explanations of the phænomenon which may suggest themselves. It might be supposed that the liquid which possesses the highest attraction for glass, or wets it most readily, will, in contact with a liquid whose attraction for glass is not so great, have the effect of rendering the surface of the less wetting fluid convex, by forcing itself between it and the walls of the containing vessel. On this supposition, the greater the difference between the attractive powers of the two liquids for glass, the greater will be the curvature of their mutual surface; while a surface nearly flat will indicate a nearly equal attraction of the two liquids for glass. As the addition of hydrochloric acid renders the flat surface of chloroform under an alkaline solution convex, the recession of the chloroform from the walls of the vessel, which is necessary to its becoming convex, would be attributed to the attraction of the acid for the glass enabling it to insinuate itself between the glass and the chloroform to a greater extent than the alkali; and it would necessarily follow, that the attraction of the acid for glass is stronger than that of the solution of potash. But it will be seen from the following experiments that the contrary is the case.

Having carefully ascertained the specific gravities of the solution of potash, the hydrochloric acid, and the chloroform I had used in my experiments, I observed the heights at which they stood in a thermometer-tube 0·014 of an inch in diameter. The following table exhibits the mean of six observations with each liquid.

	Specific gravity.	Height in tube.	Constant*.
Chloroform . .	1·493†	0·8 of an inch.	·011
Solution of potash	1·343	2·9 ...	·041
Hydrochloric acid	1·165	2·7 ...	·038

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* This number, obtained by multiplying the diameter of the tube by the height to which the liquid rises, is always the same for the same liquid, and is given here in order to check the accuracy of the experiments.

† As the specific gravity of chloroform has been generally stated at 1·480

It thus appears that the attraction of the alkaline solution for glass is greater than that of the acid; for it is capable of supporting a column of 2·9 inches, while that of the acid, although it is a lighter fluid than the solution of potash, can only support a column of 2·7 inches. In order to remove any doubt as to the superiority of the alkaline solution to the acid in attraction for glass, I made a weaker solution of potash, and repeated the experiments on chloroform with it and another sample of hydrochloric acid. The following table shows the heights at which these liquids stood in two different thermometer-tubes, as determined by the mean of three observations.

	Spec. grav.	Diam. of tube.	Height in tube.	Constant.	Diam. of tube.	Height in tube.	Constant.
		in.			in.		
Chloroform	1·493	0·026	0·46	0·012	0·010	1·07	0·011
Hydrochloric acid ...	1·167	0·026	1·51	0·040	0·010	3·73	0·037
Solution of potash...	1·105	0·026	1·77	0·047	0·010	4·61	0·046

It will be observed in the second set of experiments, that the alkaline solution is lighter than the acid, so that it is not obvious by inspection of the table which has the strongest attraction for glass. But supposing the attractions the same, the heights of the columns should be inversely as the specific gravities; so that, taking the acid as the standard, the height of the column of solution of potash in the tubes should be 1·60 and 3·94, instead of which the observed heights are 1·77 and 4·61, showing that in this case also the alkaline solution has a stronger attraction for glass than the acid. We have thus the remarkable fact, that solution of potash, which has a much stronger attraction for glass than that possessed by hydrochloric acid, is actually less able to wet it in the presence of chloroform. It follows also, that we cannot explain the flattened surface of the chloroform in the presence of the solution of potash, on the supposition of a nearly equal attraction of the liquids for glass; for those liquids obviously differ greatly in this respect, as the one rises in a capillary tube to nearly four times the height of the other. According to this view, as the attractions of chloroform and acid for glass differ less than those of chloroform and alkaline solution, the surface of chloroform in contact with acid in glass vessels should be less curved than when it is in contact with an alkaline solution; but precisely the reverse of this is the case.

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it may be proper to mention, that, having carefully ascertained the specific gravities of five different specimens of rectified chloroform, I have found them to vary from 1·493 to 1·497.

Another mode of accounting for the difference of the curvatures in the two cases would be, to suppose a strong cohesive attraction of the particles of the chloroform in comparison with that of the particles of water or acids, and a nearly equal cohesive attraction in the particles of chloroform and alkaline solutions. It is obvious that the more cohesive liquid would tend to assume a spherical form; but in the case of liquids of nearly equal cohesive power, it might be supposed that the attracting forces, being nearly balanced, the resulting action would be so small that their mutual surface would be nearly flat.

In order to estimate the cohesion of the liquids, I suspended a glass disc an inch in diameter from one arm of a balance capable of indicating 0·1 grain; and I found, by the mean of several experiments, that it required 34·3 grains placed in the opposite scale to detach the disc from the surface of hydrochloric acid; while 33·1 grains detached it from the surface of solution of potash, 38 grains from water, and 28·3 from chloroform.

As the disc separates from the surface wetted by the liquid, it is obvious the separation must have taken place, not at the surface of the glass, but in the body of the liquid, and hence the cohesion of its particles must have been overcome by the weight required to detach the disc. It would therefore appear that the cohesive force of the particles of solution of potash and of hydrochloric acid are nearly equal, while that of the particles of chloroform is inferior to that of either of these liquids. A similar result was also obtained by weighing a given number of drops of those liquids. Twenty drops of solution of potash, hydrochloric acid, and chloroform, weighed respectively 14·6, 16·9, and 11·3 grains. Since, then, the cohesive attraction of chloroform is inferior to that of hydrochloric acid, it will not account for its convexity in contact with that liquid; while the nearly equal cohesive powers of the acid and alkaline solutions will as little explain the diversity of their action in the presence of chloroform.

As the flatness of the surface of the chloroform seems to depend neither on the equality of the attractions of the liquids for glass nor upon the equality of their cohesive attractions, it next occurred to me to try whether the result was modified by altering the nature of the containing vessel. I have found that on immersing a slip of recently-ignited platina foil, or clean iron or brass wire in the liquids, the surface of the chloroform is highly convex in contact with them under water or hydrochloric acid, but apparently flat under solution of potash. If the metal is not perfectly clean, the surface of the chloroform is apt to become concave in contact with it under water or acid. On lining a glass tube with a thin coating of wax, I found that chloroform placed in it under hydrochloric acid had a very *concave* surface, while on supersaturating the acid with solution of potash the surface became sensibly flat. Similarly, chloroform under water in a quill has a very *concave* surface, which becomes sensibly flat on the addition of potash, and the concavity is restored by adding a sufficient quantity of hydrochloric acid. It will be observed that the coating of wax, or the substitution of a tube of quill for one of glass, while it completely reverses the curvature of the chloroform in contact with water and acids, exerts apparently no influence on the power of the alkali to flatten its surface; and the conclusion seems inevitable, that this effect depends upon a mutual action of the liquids, and is quite independent of the nature of the containing vessel, for it takes place equally in contact with surfaces of glass, platina, iron, wax, or quill.

Having thus endeavoured to show that the flatness of the surface of chloroform under alkaline solutions is independent of the equality of the attractions of the liquids for the walls of the vessel, of the equality of their cohesive powers, and of the nature of the containing vessel, it only remains to inquire how it may be explained on the supposition of an attractive force between the liquids themselves.

The form of the surface of the liquids in contact with the walls of the containing vessel will evidently depend,—1, on the mutual attractions of the particles of each fluid; 2, on the attractions of the particles of one fluid upon those of the other; and 3, upon the attraction of the walls of the vessel for both fluids. Considering the second of these forces, we may suppose it to act upon an indefinitely small element of the surface of the liquids, at its contact with the walls of the vessel, consisting of equal small contiguous volumes of solution of potash and chloroform. All the molecules of chloroform it contains will then be attracted by all the molecules of the solution of potash within the sphere of sensible attraction, while all the atoms of solution of potash it contains will be solicited by a similar attraction of the molecules of chloroform. Now since the attracting forces are identical in both cases, each individual attraction being that of a molecule of solution of potash to a molecule of chloroform, the sphere of sensible attraction will evidently extend to the same distance on both sides, and comprehend equal volumes of both liquids. But as the attracted particle also contains equal small volumes of chloroform and solution of potash, it is evident that the number of the attracting molecules of solution of potash will have, to that of the molecules of chloroform, the same ratio as the number of particles of potash in the elementary portion of the surface has to the number of particles of chloroform contained by it. Now the fluids are symmetrically situated in contact with the wall of the vessel, and the attracting molecules are therefore similarly situated on both sides; hence whatever law of attraction exists, the resultant force of all the individual attractions will in both cases be simply proportional to the products of the numbers of the mutually attracting molecules. But this product is evidently the same in both cases; hence the resultant attractions of both liquids upon their mutual surface will also be equal. This perhaps may be rendered more evident by the following considerations. Let r represent the ratio of the number of attracting molecules in a given volume of solution of potash to that of the attracting molecules in an equal volume of chloroform. Then, since the sphere of sensible attraction evidently extends to the same distance in both liquids, we may suppose it in both to be divided into an equal number of corresponding thin concentric spherical surfaces. If n be the number of attracting molecules in one of these attracting spherical layers of chloroform, rn will be the number in the corresponding layer of solution of potash; and, on the other hand, if n' be the number of molecules of chloroform in the small element of the mutual surface of the liquids, rn' will be the number of molecules of solution of potash which it contains. Now as the distances of the attracting molecules in the corresponding spherical layers from the attracted element of the surface is the same in both liquids, the attraction of a single pair of molecules will be the same in both cases; and therefore the ratio of the attractions of the two spherical surfaces to the elementary particle will be that of the products of the numbers of the mutually attracting molecules. The one force will therefore be that of rn molecules of potash attracting n' molecules of chloroform, and will be represented by rnn'; the other force will be that of n molecules of chloroform attracting rn' molecules of solution of potash, and will therefore be also represented by rnn'. Since then the attractions of the corresponding spherical layers of the liquids upon the elementary particle of the surface are equal, the whole attractions on either side will also be equal.

The resultants of those attractions will then evidently be two equal forces, inclined at an angle of 45° to the mutual surface of the liquid at the point of its junction with the wall of the containing vessel; and these again will have for their resultant a single horizontal force perpendicular to the wall of the vessel, and therefore tending directly to counteract its attraction for the liquids. Even although this reasoning be not considered sufficient to prove the perfect equality of the attractions of the liquids for each other, yet if they be only nearly equal, their resultant will still evidently act in opposition to the attraction of the containing wall. And it obviously follows from this, that the stronger the mutual attraction of the fluids, the less will be the effective attraction of the walls of the vessel. We have next then to consider the curvature of the surface of the liquids as depending upon the resultant of the attractions of the walls of the vessel, and the attractions of the particles of each liquid among themselves. Now Clairaut has shown, that when the attraction of the walls of the vessel is more than half that of the molecules of the liquid for each other, it will rise towards the solid; and therefore the greater the excess of the attractive power of the solid above this limit, the greater will be the rise of the liquid, and the extent of the curvature of its surface. The effect of the mutual attraction of the liquids in diminishing the attraction of the walls of the vessel will therefore evidently tend to reduce the curvature within narrower limits, and thus to produce a flat surface. In other words, the greater the mutual attraction of the liquids, the less will be the effective attraction of the walls of the vessel; and, consequently, the smaller will be the tendency to curvature due to the superior attraction of the solid for one liquid over the other, enabling it to insinuate itself between the walls of the vessel and the other liquid.

It was already observed, that the effect of potash in flattening the surface of chloroform does not consist in the absolute removal of its curvature, but only in reducing it within very narrow limits; and this is precisely what might be expected; for while the mutual attraction of the liquids counteracts their attractions to the walls of the vessel, it cannot remove any previous inequality between those attractions. The most wetting liquid will therefore still rise towards the solid; but the effective attraction being diminished, it will rise to a smaller extent, and the curvature will thus be reduced within much narrower limits than if there had been no force to counteract the attraction to the solid. The attraction of the liquids is also an obvious explanation of the flattening of the surface of chloroform, observed by Dr. Wilson, where it is not in contact with the walls of the vessel; and it likewise accounts for the diffuse and irregular form of the globules of chloroform in alkaline solutions observed by him. For it is evident that the attraction of the potash must counteract that of the particles of the chloroform for each other, and thus render it virtually a less cohesive liquid in contact with alkaline solutions than in contact with acids.

As the flattening of the surface seems to be sufficiently explained by the hypothesis of a powerful attraction between the molecules of the liquids, and as in all observed instances of a flat surface there has been a decided chemical affinity between them, it is difficult to avoid supposing some connexion between the hypothetical attracting force and the actually existing chemical affinity. If we suppose them to be identical, the attracting molecules of the liquids will then be their chemically equivalent atoms. When chloroform is decomposed by solution of potash, I understand that an atom of chloroform with three atoms of potash produces three equivalents of chloride of potassium and one equivalent of formic acid; which thereafter combines with a fourth atom of potash to form formiate of potash, as expressed by the following equation:

$$C_2 H Cl_3 + 4KO = 3K Cl + KO, C_2HO_3.$$

Supposing, then, the attractions of the liquids to be the same forces which ultimately tend to produce this decomposition, the attracting molecule of potash might be regarded as $4KO$, and that of chloroform as $C_2 H Cl_3$: or we may suppose two sets of attracting molecules; the one $3Cl$ attracting $3K$, and the other $C_2 H$ attracting $KO + 3O$. But I state this merely to point out what may perhaps have already suggested itself to the reader, that whatever view is taken of this molecular action, the ratio of the number of attracting molecules in the two liquids must still be inversely as the number of molecules in the infinitesimal element of the surface of the liquids subjected to that attraction; and thus the result which has already been arrived at will remain entirely unaffected.

In conclusion it may be remarked, that if the phænomenon of the flattened surface of two immiscible liquids has received a correct explanation on the hypothesis of a strong mutual attraction of their molecules, and if this phænomenon is found to appear only in cases where the liquids have a decided chemical affinity for each other, an interesting connexion is thereby established between chemical affinity and the mechanical force of adhesion, tending to prove that they are modifications of the same force; while the whole subject of the mutual attraction of two liquids opens up an interesting, and, so far as I am aware, a new field of inquiry in capillary attraction.

VII. *On some Forms of Quadratic Moduli. By* Prof. Young*. *In a Letter to* Prof. De Morgan, *&c.*

My dear Sir,

I HAVE read with much interest your paper from the Cambridge Transactions, On Triple Algebra. You are aware that I had no access to it till last week, and therefore may not as yet be fully in possession of all your views. It has occurred to me, however, that your system, or rather that particular system upon which you have more especially dwelt, may be arrived at in a manner somewhat more simple, by a process analogous to that which was employed by Euler and Lagrange for finding algebraic functions whose products are similar functions, and which process is nearly the inverse of that adopted in your paper. It is this: since

$$(x+\alpha y+\alpha^2 z)(x+\beta y+\beta^2 z)$$
$$=x^2+(\alpha+\beta)xy+(\alpha^2+\beta^2)xz+\alpha\beta y^2+(\alpha^2\beta+\beta^2\alpha)yz+\alpha^2\beta^2 z^2,$$

it follows that if a and b are the coefficients of v and v° in that quadratic equation, $v^2-av+b=0$, whose roots are α and β, we shall have

$$\left.\begin{aligned}(x+\alpha y+\alpha^2 z)(x+\beta y+\beta^2 z)=x^2+axy+(a^2-2b)\,xz\\ +by^2+abyz+b^2z^2\end{aligned}\right\}; \quad . \quad (1.)$$

and similarly for

$$(x'+\alpha y'+\alpha^2 z')(x'+\beta y'+\beta^2 z').$$

But, as well-known (see Additions to Euler's Algebra),

$$\left.\begin{aligned}(x+\alpha y+\alpha^2 z)\,(x'+\alpha y'+\alpha^2 z')=\mathrm{X}+\alpha\mathrm{Y}+\alpha^2\mathrm{Z}\\ (x+\beta y+\beta^2 z)(x'+\beta y'+\beta^2 z')=\mathrm{X}+\beta\mathrm{Y}+\beta^2\mathrm{Z}\end{aligned}\right\} \quad . \quad . \quad (2.)$$

These products being of the same form as the original factors, it follows that the product of the four factors on the left of (2.) will give a result of the form (1.); that is to say, we shall have

$$\left.\begin{aligned}&\{x^2+axy+(a^2-2b)xz+by^2+abyz+b^2z^2\}\\ &\times\{x'^2+ax'y'+(a^2-2b)x'z'+by'^2+aby'z'+b^2z'^2\}\\ &=\mathrm{X}^2+a\mathrm{XY}+(a^2-2b)\mathrm{XZ}+b\mathrm{Y}^2+ab\mathrm{YZ}+b^2\mathrm{Z}^2\end{aligned}\right\}. \quad (3.)$$

Lagrange, in discussing the quadratic forms of reproducing functions, confines himself to those involving only *two* arbitrary quantities, x and y; and when three enter, he considers them exclusively in reference to forms of three dimensions, and so on. From these the inferior forms may no doubt be

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* Communicated by the Author.

deduced; but it is much simpler to obtain them directly, as in the above instance.

If in the general formula (3.) we put $a=-1$, and $b=1$, the first of your modular equations will result, viz.

$$\begin{aligned}&(x^2+y^2+z^2-xy-xz-yz)\\ &\times(x'^2+y'^2+z'^2-x'y'-x'z'-y'z')\\ &=X^2+Y^2+Z^2-XY-XZ-YZ.\end{aligned}$$

Now with these values for a and b, our quadratic $v^2-av+b=0$ becomes

$$v^2+v+1=0\ \therefore\ v=\frac{-1\pm\sqrt{-3}}{2}.$$

Substituting these values of v, or either of them, for α or for β, we have, for our linear factors, the form

$$x+\left(\frac{-1+\sqrt{-3}}{2}\right)y+\left(\frac{-1+\sqrt{-3}}{2}\right)^2z,$$

or, which is the same thing,

$$x+\left(\frac{-1+\sqrt{-3}}{2}\right)y+\left(\frac{-1-\sqrt{-3}}{2}\right)z;$$

and the imaginary coefficients may be taken as interpretations of your η and ζ. Although only a *quadratic* has been employed to determine these, yet they are evidently the imaginary cube roots of *plus* unity. You have taken those of *minus* unity; so that, in multiplying two such forms together, the functions of x, y, z in our results would differ, as to signs, in some of the combinations. If, however, the signs of y and z in the factors here proposed be changed, which change is equivalent to changing the signs of their coefficients, these coefficients will then be converted into the imaginary cube roots of *minus* unity, and we shall both agree.

The second of your modular equations really requires a *cubic*. The general cubic formula Lagrange has investigated, and given in the Additions referred to. Each factor of this formula is of the form

$$\begin{aligned}&x^3+ax^2y+(a^2-2b)x^2z+bxy^2+(ab-3c)xyz\\ &+(b^2-2ac)xz^2+cy^3+acy^2z+bcyz^2+c^2z^3,\end{aligned}$$

the cubic which leads to it being

$$s^3-as^2+bs-c=0;$$

and from this form, if we suppose a and b to be zero, and c to be unit, your second equation results, viz.

$$\begin{aligned}&(x^3+y^3+z^3-3xyz)(x'^3+y'^3+z'^3-3x'y'z')\\ &=X^3+Y^3+Z^3-3XYZ;\end{aligned}$$

and, as before, we may, if we please, change the roots of $+1$ to those of -1, which is equivalent to changing the sign of c.

I remain, my dear Sir,
Very faithfully yours,
To Professor De Morgan. J. R. YOUNG.
Belfast, May 2, 1848.

P.S. At the close of my recent paper in the Irish Transactions, I have said, in reference to the expression $ax^2{}_3 + bx_3y_3 + cy_3{}^2$, that "no part of one term can be cancelled by a part of another." I should have said, that no part of $ax_3{}^2$ can be cancelled by a part of $bx_3y_3 + cy_3{}^2$; from which it follows that x_3 must be divisible by a, and $(bx_3 + cy_3)y_3$ by a^2; so that y_3 must be divisible by a.

In the paper here referred to, the following eight-square formula has been obtained. It differs from that previously arrived at by Mr. J. T. Graves, only by the introduction of the general coefficients; that is to say, it differs from Graves's formula only as that of Lagrange differs from the four-square formula of Euler:

$$\begin{aligned}&(s'^2 + bt'^2 + cu'^2 + bcv'^2 + abcw'^2 + acx'^2 + aby'^2 + az'^2)\\ &\times (s^2 + bt^2 + cu^2 + bcv^2 + abcw^2 + acx^2 + aby^2 + az^2)\\ &= s''^2 + bt''^2 + cu''^2 + bcv''^2 + abcw''^2 + acx''^2 + aby''^2 + az''^2.\end{aligned}$$

The expressions s'', $\sqrt{b}.t''$, $\sqrt{c}.u''$, &c., whose squares form the terms of this product, arise from multiplying the factors

$$(s' + \sqrt{b}.t' + \sqrt{c}.u' + \ldots), \; (s + \sqrt{b}.t + \sqrt{c}.u + \ldots)$$

in a peculiar way, and not in the manner in which we should proceed if our object were to find the *product* of those factors: yet all the items in the result severally agree with those which form the common algebraical product, with the exception of the *signs*. And if from this partial coincidence we were led to inquire what laws of combination, as respects signs, must be assumed for the coefficients $\sqrt{b}$, $\sqrt{c}$, &c., so as to render the coincidence complete, we should be conducted to those which Mr. Graves impressed upon his seven symbols l, m, n, o, i, j, k, in framing his theory of *octaves*, as explained in a Note, which Sir William R. Hamilton permitted me to add to my paper. I may perhaps be allowed to observe, in reference to the eight-square formula above, that if we make $a = \pm 1$, it may be otherwise written thus; from which several subordinate forms, not without interest, may be deduced:

$$\begin{aligned}&\{(s'^2 \pm z'^2) + b(t'^2 \pm y'^2) + c(u'^2 \pm x'^2) + bc(v'^2 \pm w'^2\}\\ &\times \{(s^2 \pm z^2) + b(t^2 \pm y^2) + c(u^2 \pm x^2) + bc(v^2 \pm w^2)\}\\ &= (s''^2 \pm z''^2) + b(t''^2 \pm y''^2) + c(u''^2 \pm x''^2) + bc(v''^2 \pm w''^2).\end{aligned}$$

J. R. Y.

VIII. *On Diamagnetism.* *By* M. Plucker. *In a Letter to* Mr. Faraday.

Sir,

I HAVE the honour to send you two small memoirs, which on account of political circumstances have reached me later than usual. I have already mentioned to you the first. The second relates especially to diamagnetic polarity, which is now placed beyond doubt. You will find, among others, the curious fact, that the intensity of the diamagnetic force increases more rapidly when the force of the electro-magnets is increased than that of the magnetic force. The increase of the force of the electro-magnet imparts to a piece of charcoal, having first the position of a magnetic body, that of a diamagnetic body. I have subsequently proved this law in different ways. The following experiment is striking. If by means of a counterpoise, any body containing at the same time magnetic and diamagnetic substances (for instance mercury in a brass vessel, this last being magnetic) is held in equilibrium, this body is repelled by the magnet when brought near it, and attracted when it is removed.

I have devised a method which allows of my comparing exactly the intensity of the diamagnetism of the different bodies, solid and liquid, and at the same time I arrived at a number of curious results concerning magnetic induction, and especially the relation between the chemical constitution of bodies and their magnetism. The difficulty of obtaining chemically pure substances presented the greatest obstacles. Thus, for example, $\frac{1}{3}$ per cent. of protoxide of iron mixed with a mass of peroxide, ought at least to *double* the magnetism. The magnetism of the oxides is *increased* by the acids which unite with them to form salts. Supposing the magnetism of the iron $=100000$, I find that of a similar weight of oxide of nickel $(\dot{\mathrm{Ni}})$ equal to 35, that of the same oxide in the state of hydrate $(\dot{\mathrm{Ni}}+\dot{\mathrm{H}})$ equal to 142. The yellow ferrocyanide of potash is diamagnetic; the red ferridcyanide, on the contrary (whether in crystal or in powder, or in solution), is decidedly magnetic. [A crystal of this last salt shows very clearly the repulsion of the optical axes by the magnet; without paying attention to it one might easily take it for a diamagnetic body, &c.] I have examined equally the influence of heat. For mercury, sulphur, stearine, I have not observed the least difference, either at a high or low temperature, whether in the liquid or solid state. But bismuth gave very different results. In one of my experiments, it required, at the ordinary temperature, 1·67 gramme to counterbalance the diamagnetic repulsion of a mass of 144 grammes. At an elevated temperature, 0·28 grm., that is to say, nearly a sixth, was necessary for this. I see in this a new analogy between magnetism and diamagnetism. They both diminish if the temperature augments, &c. It appears, moreover, that the diamagnetism of bismuth has its limit (its minimum) like the magnetism of iron and of nickel, &c.

I shall take the liberty to send you my new memoir, which is now in the hands of M. Poggendorff, as soon as it shall appear. Perhaps I shall have the pleasure of bringing it myself to London. Accept, Sir, the assurance of my deep respect.

PLUCKER.

Bonn, June 5, 1848.

IX. *On the Construction and Power of a new form of Galvanic Battery. By the* Rev. NICHOLAS CALLAN, *D.D., Professor of Natural Philosophy in Maynooth College*[*].

IN a paper published in the August Number of the London Philosophical Magazine, I described several experiments, which clearly prove that, as a negative element of the nitric acid battery, lead coated with chloride of gold or platina, or with borax dissolved in dilute acid, is superior to platina, and that cast iron is fully as powerful as platina. I have since compared, in various ways, the power of a cast-iron battery with that of a Grove's of equal size. The cast iron was excited by a mixture consisting of about four parts of sulphuric acid, two of nitric acid, and two of nitre dissolved in water. The platina was excited by equal parts of concentrated nitric and sulphuric acid. The zinc plates of both batteries were excited by dilute sulphuric acid of the same strength. The cast-iron battery was considerably superior to Grove's, in its magnetic power, in its heating power, and in its power of producing decomposition. The magnetic effects of the two batteries were compared by means of a galvanometer and of a small magnetic machine. Grove's produced a deflection of 82°; the cast iron caused a deflection of 85°. When the voltaic currents of the two batteries were sent simultaneously in opposite directions through the helix of the galvanometer, the current from the cast-iron battery destroyed the deflection caused by Grove's, and produced an opposite deflection of 60°. In the magnetic machine the cast-iron battery produced fifty revolutions in a minute; Grove's produced only thirty-five in the same time.

The superiority of the heating power of the cast-iron battery was shown by its fusing a steel wire, which Grove's only

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[*] Communicated by the Author.

raised to a dull red heat. I have been told by persons who tried the two batteries, that they found the heating power of the cast-iron battery to be twice as great as that of Grove's.

The decomposing powers of the two batteries were compared by the quantities of the mixed gases which they produced during the space of three minutes. The result clearly established the superiority of the cast-iron battery.

I have tried various kinds of cast iron, and have found them all to possess nearly equal power. I have got cast-iron plates containing oxide of chromium: they did not appear to have any advantage over common cast iron. Perhaps, by mixing with cast iron some of the more negative elements, an increase of power may be obtained.

Soon after I had discovered the great electromotive power of platinized lead and cast iron, when excited by nitric or nitro-sulphuric acid, I proposed to the trustees of the College to change our Wollaston batteries into a platinized lead or cast iron one. They readily authorised me to expend the sum required for the change. After weighing well the relative advantages of platinized lead and cast iron, I resolved on the latter, principally because I found that it did not require to be platinized. In one of our Wollaston batteries there were 300 zinc plates, each four inches square, and in the other 20 plates, each 2 feet square. In the two batteries the surface of the zinc plates was something more than 113 square feet; the copper surface was twice as great as the zinc surface. After mature reflection on the best form for the new battery, and on the most convenient size of the zinc plates, I resolved to get water-tight, cast-iron cells, rather than plates; to retain the 300 4-inch plates; and to divide the 20 large plates into 320 small ones, each 6 inches square. I therefore ordered 300 porous cells, each $4\frac{1}{2}$ inches high, $4\frac{1}{2}$ inches broad, and $\frac{1}{2}$ an inch wide, for the 4-inch plates; and 320 porous cells, each $6\frac{1}{2}$ inches high, $6\frac{1}{2}$ broad, and about an inch wide, for the 6-inch plates. I also ordered 300 cast-iron, water-tight cells, each about $4\frac{1}{8}$ inches high, 5 inches broad, and an inch wide, to hold the small porous cells; and 320 cast-iron cells, each about $6\frac{1}{8}$ inches high, $7\frac{1}{4}$ broad, and $1\frac{3}{4}$ wide, to contain the large porous cells. The new battery then was to consist of 620 voltaic circles, in which the entire zinc surface would be 113 square feet, and the surface of cast iron would exceed 226 square feet; but on account of several disappointments I have been obliged to be content for the present with 577 voltaic circles, containing 96 square feet of zinc, and about 200 square feet of cast iron. In this battery, which was exhibited in the College on the 7th of the last month, there were

300 cast-iron, water-tight cells, each containing a porous cell and zinc plate 4 inches square; 110 cast-iron cells, each holding a porous cell and zinc plate 6 inches by 4; and 177 cast-iron cells, each containing a porous cell and a zinc plate 6 inches square. The zinc plate of each circle was placed in a porous cell, and the latter in a cast-iron cell. The inside of each cast-iron cell was about a quarter of an inch wider than the exterior of its porous cell. Slips of sheet copper about an inch broad and 2½ inches long, were soldered to each cast-iron cell, and to each of the 320 6-inch zinc plates. The 4-inch plates were already furnished with screws and nuts. Each iron cell was connected by a binding-screw with the next zinc plate. The iron cells were kept in an upright position in nine wooden frames, which were placed on wooden supports nearly 3 feet high. The battery was charged by pouring into each cast-iron cell a mixture containing about twelve parts by measure of concentrated nitric acid, and eleven and a half parts of double rectified sulphuric acid; and by filling to the proper height each porous cell with dilute nitro-sulphuric acid, consisting of about five parts of sulphuric acid, two of nitric, and forty-five of water. In charging the entire battery we used about fourteen gallons of nitric and sixteen of sulphuric acid. I abstained from using the solution of nitre through an apprehension that it would cause the exciting mixture in the cast-iron cells to boil over. I know not whether this apprehension is well-founded; but I know that when ten or more cells are employed, the exciting fluid in the cast-iron cells will soon boil over, and produce nitrous fumes, if it does not contain one quarter of its bulk of nitric acid.

I have found by experiment that a cast-iron battery is about fifteen times as powerful as a Wollaston battery of the same size, and nearly as powerful and a half as Grove's. Hence our new cast-iron battery, in which there are 96 square feet of zinc, is equal in power to a Wollaston battery containing more than 1400 square feet of zinc, or more than 13,000 four-inch plates, and to a Grove's containing 140 square feet of platina. Now the battery made by order of Napoleon for the Polytechnic School, which was the largest zinc and copper battery ever constructed, contained only about 600 square feet of zinc; and the most powerful Grove's of which I have seen an account did not contain 20 square feet of platina. Hence the cast-iron battery belonging to the College is more than twice as powerfnl as the largest Wollaston, and seven times as powerful as the largest Grove's ever constructed.

I shall now describe a few of the experiments which were made with our large cast-iron battery on the 7th of the last month. The first experiment consisted in passing the voltaic current through a very large turkey, which was instantly killed by the shock. The craw of the turkey was burst, and the hay and oats contained within it fell to the ground. In order to give the shock, a piece of tin-foil, about four inches square, was placed under each wing along the sides of the turkey, which were previously stripped of their feathers, and moistened with dilute acid. The tin-foil was kept in close contact with the skin by pressing the wings against the sides. The person who held the turkey had a very thick cloth between each hand and the wing, in order to save him from the shock. As soon as the wire from the zinc end of the battery was put in contact with the tin-foil under one wing, sparks were given by the tin-foil, and shocks received by the turkey, before the connexion was made between the negative end of the battery and the tin-foil under the other wing, although the negative and positive ends of the battery were on tables nearly 3 feet high, and 3 feet asunder.

When a copper wire in connexion with the negative end was put in contact with a brass ring connected with the zinc end of the battery, a brilliant light was instantly produced. The copper wire was gradually separated from the brass ring until the arc of light was broken. The greatest length of the arc was about 5 inches. As soon as the connexion was made between the opposite ends of the battery by the copper wire, which was $\frac{1}{4}$ of an inch thick, and about 5 feet long, a loud noise was produced by the combustion of the solder which fastened some of the copper slips to the zinc plates. I immediately went to the part of the battery from which the noise proceeded, in order to try whether the connexion between the cast-iron cells and zinc plates was broken; I found one slip of copper detached from the zinc plate to which it had been soldered. There were probably others disconnected with their zinc plates, but I did not find them. The result of this experiment showed that the turkey conducted only a part of the current circulated by the battery, for the current which killed the turkey produced no combustion of the solder by which the copper slips were attached to the zinc plates.

We next tried the ignition of charcoal points. We were not able to determine the length of the arc of light between them; for before Sir Robert Kane had time to separate them, they were burned away. The light was, of course, most brilliant: .the charcoal scintillated like steel or iron. I never before observed these scintillations in the combustion of charcoal. Coke points were also ignited, and a most intense light produced; but during the experiments with the coke points the circuit was interrupted in consequence of the fracture of one of the porous cells, which caused the dilute and concentrated acids to mingle together, and, consequently, to boil over, until the porous and cast-iron cells were nearly emptied. Notwithstanding this interruption of the circuit, the arc of light between the coke points was about an inch long, and the heat of the flame deflagrated a file.

I had arrangements made for a long series of experiments on the decomposing power of the voltaic current, and of voltaic heat, and on the illuminating power of the various kinds of voltaic light; but these experiments I was obliged to omit, through fatigue, exhaustion, and bad health. I have since tried the illuminating power of the light produced by the ignition of coke points; and for the gas microscope and polariscope have found it far superior to the oxyhydrogen lime light. With good coke points, abundant light for the microscope and polariscope may be obtained from a battery containing 25 cast-iron cells, and as many zinc plates, each 2 inches by 4: if the coke be not very good, 40 plates will be required. When an iron cell, 2½ inches wide and 4 inches high, is large enough to contain between it and the porous cell nearly a wine-glassful of the concentrated acids, the battery will work with undiminished power for about three hours without any additional acid. If the cell containing the zinc plates be small, it will be necessary to pour in a little dilute acid every half-hour. I have got the lime light by igniting the mixed gases as they were produced by the decomposition of water, and throwing the flame on lime.

Maynooth College, April 6, 1848.

X. *On Chloropicrine.* *By* JOHN STENHOUSE, *Esq., Ph.D.*[*]

WHEN an aqueous solution of nitropicric acid is poured into a retort containing a great excess of hypochlorite of lime, the mixture heats spontaneously, and an aromatic pungent vapour, which affects the eyes very powerfully, is immediately evolved. If heat is applied to the retort so soon as the mixture begins to boil, a very large quantity of a colourless heavy oil comes over along with the vapours of water, and condenses in the receiver. To this oil I purpose giving the name of chloropicrine. When the mixture has boiled for a quarter of an hour or so, the whole of the oil has usually passed over; and if the liquid in the retort is perfectly colour-

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[*] Communicated by the Author.

less, the whole of the nitropicric acid has been decomposed; but if the solution retains a yellow colour, this indicates that it still contains undecomposed nitropicric acid; and on the addition of a fresh quantity of hypochlorite of lime, more chloropicrine is obtained. When nitropicric acid is boiled with a filtered solution of hypochlorite of lime, the oil is generated in the way already described, and it becomes apparent that a quantity of carbonate of lime is precipitated at the same time; and as the residue in the retort is found to contain not a trace of organic matter, carbonic acid, muriatic acid, and chloropicrine appear to be the only products of this decomposition. The oil which had passed into the receiver was separated from the acid liquid which surrounded it, and was repeatedly washed with water containing a little carbonate of magnesia. It was then deprived of any adhering moisture by being allowed to stand for some days over fused chloride of calcium. On being transferred into a small retort, it was cautiously rectified on the sand-bath, when pure chloropicrine distilled over as a colourless neutral oil possessing a very considerable refractive power.

When nitropicric acid is boiled with a mixture of chlorate of potassa and muriatic acid, it is converted into chloranile, which chiefly remains in the retort, and into chloropicrine, which passes into the receiver. The chloropicrine obtained by this process is always impure, and contains some crystals of chloranile which have been carried over into the receiver along with the oil and watery vapours; but from these the chloropicrine may be easily purified by careful rectification with water.

When nitropicric acid is boiled with aqua regia, it is also resolved into chloropicrine and chloranile; but the quantity of the oil is much larger, and that of the chloranile much smaller than when chlorate of potash and muriatic acid are employed. The same products, viz. chloropicrine and chloranile, are also formed when a stream of chlorine gas is sent through a hot aqueous solution of nitropicric acid. The nitropicric acid is but slowly acted on by the chlorine, and three or four days' treatment with chlorine is required to effect its complete decomposition. Chloropicrine is also generated when charbazotate of potash is heated with a solution of hypochlorite of lime. Chloranile cannot be converted into chloropicrine by being boiled either with nitric acid, aqua regia, or hypochlorite of lime.

The nitropicric acid which I employed for these experiments was prepared partly from indigo, but by far the larger portion of it from the yellow gum-resin of Botany Bay. This resin isthe produce of the *Xanthorœa hastilis*. It can be purchased in London for a shilling the pound. It yields half its weight of nitropicric acid: eight ounces gave four ounces of acid, which was much more easily purified than that prepared from indigo; it contained, however, some oxalic and a very little nitrobenzoic acid. This yellow gum-resin, as I showed in a paper published in this Journal nearly three years ago, is therefore by far the most œconomical source of nitropicric acid.

The resinous portion of gum-benzoin, after all the benzoic acid it contains has been extracted by boiling it with alkalies, when digested with nitric acid also yields a great deal of charbazotic acid. This, in fact, is the most advantageous way in which this otherwise useless product can be employed. I have found that the resinous portion of balsam of Peru is also converted into charbazotic acid when boiled with nitric acid; but I could not procure charbazotic acid from balsam of Tolu.

When the chrysammic acid of M. Schunck is boiled with hypochlorite of lime, it yields abundance of chloropicrine; and, as might almost have been expected, the same is the case with the styphnic acid of Messrs. Will and Boettger, or the oxypicric acid of Erdmann. With chlorate of potassa and muriatic acid these acids are also transformed into chloranile and chloropicrine.

Chloropicrine prepared from any of these sources has the following properties. It forms a transparent colourless oil which refracts the light pretty strongly. Its specific gravity is 1·6657. Itssmell when diluted is aromatic and very peculiar; when undiluted, it affects the nose and eyes as strongly as chloride of cyanogen or oil of mustard, only its effects are not so permanent. Chloropicrine is perfectly neutral to test-paper. It is nearly insoluble in water, but dissolves very readily in alcohol and in æther. It is not acted on by sulphuric, muriatic, or nitric acids in the cold; and when boiled along with these acids it distils over unchanged. When a small bit of potassium is dropt into a quantity of the oil and a gentle heat is applied, the chloropicrine is decomposed by a most violent explosion. If the potassium is put into the oil at the ordinary temperature no explosion takes place, and in the course of a day or two it is converted into a mixture of chloride of potassium and nitre.

These salts, when tested by sulphuric acid, gave no trace of nitrous gas. Aqueous solutions of the alkalies may be kept in contact with chloropicrine for a considerable time without producing any perceptible effect upon it; but when an alcoholic solution of potassa or soda is employed, the chloropicrine is slowly decomposed, and crystals of chloride of potassium and nitre are deposited. When the oil is saturated with dry ammoniacal gas or an alcoholic solution of ammonia, it is converted into sal-ammoniac and nitrate of ammonia; but an aqueous solution of ammonia produces scarcely any effect upon it.

Chloropicrine boils at 120° C., and it may be heated as high as 150° C. without being decomposed. It is not inflammable. When the oil is passed in vapour through a glass tube heated to considerably under low redness, it is completely decomposed. Much chlorine and deutoxide of nitrogen are evolved, and a quantity of the solid perchloride of carbon $C^4 Cl^6$ sublimes into the colder portion of the tube. Several precautions must be observed in subjecting chloropicrine to analysis. A very long tube must be employed, the fore part of which must be filled to the extent of seven or eight inches with a mixture of reduced oxide of copper and copper turnings. The combustion must also be conducted as slowly as possible, so that none of the deutoxide of azote may escape decomposition.

I. 0·2895 grm. oil gave 0·071 carbonic acid and 0·007 water = 0·26 per cent. H.

II. 0·227 grm. oil gave 0·0565 CO^2 and 0·005 water = 0·22 per cent. H.

III. 0·2719 grm. oil gave 0·0670 CO^2.

IV. 0·2628 grm. oil gave 0·0608 CO^2.

I. 0·3746 grm. oil burnt with lime gave 0·9841 chloride of silver = 0·2427 Cl.

II. 0·552 grm. oil gave 1·4515 Ag Cl = 0·357 Cl.

III. 0·5707 grm. oil gave 1·5920 AgCl = 0·373 Cl.

IV. 0·468 grm. oil gave 1·2245 Ag Cl = 0·302 Cl.

V. 0·3955 grm. oil gave 1·034 Ag Cl = 0·255 Cl.

VI. Analysis of the oil by combustion with oxide of copper and copper turnings yielded 350 volumes of gas, consisting of $116\frac{1}{2}$ volumes of nitrogen and 234 carbonic acid gas. This determination is almost exactly one volume of nitrogen to two volumes of carbonic acid, which is equivalent to 7·78 per cent. of nitrogen.

			Found numbers.				
Ats.	Calculated numbers.		I.	II.	III.	IV.	V.
4C	305·74	6·42	6·72	6·78	6·60	6·30	
7Cl	3098·55	65·11	64·80	64·83	64·53	64·47	65·34
2N	354·08	7·44	7·78	7·78	7·78	7·78	
10O	1000·00	21·03	20·70	20·61	21·09	21·45	
	4758·37	100·00	100·00	100·00	100·00	100·00	

These analyses agree pretty well with the formula 4C, 7Cl, 2N, 10O. The small quantity of hydrogen is omitted, being regarded as non-essential. The chloropicrine employed in these analyses was prepared at several different times. When these experiments on chloropicrine were pretty far advanced, I was struck with the resemblance which it presented in its elementary composition and properties to the nitro-chlorinated oil, obtained by Marignac by acting on hydrochlorate of chloronaphthalese with boiling nitric acid. The formula of Marignac's oil is C, Cl, N, O_4, and its per cent. composition—

One atom carbon . .	76·43	6·97
One atom chlorine . .	442·65	40·39
One atom nitrogen . .	177·04	16·15
Four atoms oxygen .	400·00	36·49
	1096·00	

It is obvious, therefore, that though the elements are the same, the per cent. composition is widely different. I prepared a small quantity of Marignac's oil by sending a stream of chlorine gas for three days over melted naphthaline, and then treating the chloro-naphthalese thus formed by nitric acid. The oil was very slowly generated, the chloronaphthalese requiring to be boiled with nitric acid for more than twelve hours. The quantity of the oil I obtained was very small. Its smell was quite different from that of chloropicrine, and not nearly so pungent. I also prepared a quantity of nitronaphthalese by treating naphthaline with hot nitric acid. When this nitronaphthalese was boiled with hypochlorite of lime, it yielded little more than a trace of chloropicrine, and with aqua regia no oily body whatever.

The most remarkable circumstance in regard to chloropicrine, is the very large number of organic bodies from which it may be procured.

1. All the substances which yield nitropicric acid. These are indigo and its derivatives, salicine and its derivatives, coumarine, hydrate of phenyle, creosote, Botany Bay yellow resin, liquid storax, gum-benzoin, balsam of Peru, &c.

2. All the substances yielding the styphnic acid of Will and Boettger, the oxypicric acid of Erdmann. These are gum assafœtida, galbanum, ammoniacum, sagapenum, purree or Indian yellow, the watery extracts of Brazil wood, red sanders wood, logwood, fustic, &c.

3. M. Schunck's chrysammic acid, and consequently aloes.

I have also procured chloropicrine by boiling Dammara resin with nitric acid and then treating it with hypochlorite of lime; and I have also obtained it by digesting in nitric acid the resinous compound which chlorine forms with usnic acid.

Glasgow, June 15, 1848.

XI. *On Quaternions; or on a New System of Imaginaries in Algebra.* *By* Sir WILLIAM ROWAN HAMILTON, *LL.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from vol. xxxii. p. 374.]

62. THE equations (85.), (90.), and (111.), of articles 56, 57, and 60, give

$$T(\rho-\lambda)=T(\rho-\mu')=b; \quad . \quad . \quad . \quad . \quad (113.)$$

and

$$T(\rho-\mu)=T(\rho-\lambda')=b; \quad . \quad . \quad . \quad . \quad (114.)$$

whence, by the meanings of the signs employed, the two following mutually connected constructions may be derived, for *geometrically generating an ellipsoid from a rhombus of constant perimeter*, or for geometrically describing an arbitrary curve on the surface of such an ellipsoid by the motion of a corner of such a rhombus, which the writer supposes to be new.

1st Generation. Let a rhombus LEM′E′, of which each side preserves constantly a fixed length $=b$, but of which the angles vary, move so that the two opposite corners L, M′ traverse two fixed and mutually intersecting straight lines AB, AB′, (the point L moving along the line AB, and the point M′ along AB′,) while the diagonal LM′, connecting these two opposite corners of the rhombus, remains constantly parallel to a third fixed right line AC (in the plane of the two former right lines); then, *according to whatever arbitrary law the plane of the rhombus may turn*, during this motion, *its two remaining corners* E, E′ *will describe curves upon the surface of a fixed ellipsoid*; which surface is thus the *locus of all the pairs of curves* that can be described by this first mode of generation.

2nd Generation. Let now *another rhombus*, L′E″ME‴, with the *same constant perimeter* $=4\,b$, move so that its opposite corners L′, M traverse the *same two fixed lines* AB, AB′, as before, but in such a manner that the diagonal L′M, connecting these two corners, remains parallel (not to the third fixed line AC, but) to a *fourth fixed line* AC′; then, whatever may be the arbitrary law according to which the plane of this new rhombus turns, provided that the angles BAB′, CAC′, between the first and second, and between the third and fourth fixed lines, have one *common bisector*, the *two remaining corners* E″, E‴ *of this second rhombus will describe curves upon the surface of the same fixed ellipsoid*, as that determined by the former generation: which surface is thus the *locus of all the new pairs of curves*, described in this second mode, as it was just now seen to be the locus of all the old pairs of curves, obtained in the first mode of description.

63. The ellipsoid (with three unequal axes), thus generated, is therefore the *common locus of the four curves*, described by the four points E E′ E″ E‴; of which four curves, the first and third may be made to coincide with *any arbitrary curves on that ellipsoid*; but the second and fourth become determined, when the first and third have been chosen. And in this new *system of two connected constructions for generating an ellipsoid*, as well as in that other construction* which was given in article 61 for a *system of two reciprocal ellipsoids*, the two former fixed lines, AB, AB′, are the *axes of two cylinders of revolution*, circumscribed about the ellipsoid which is the locus of the point E; while the two latter fixed lines, AC, AC′, are the *two cyclic normals* (or the normals to the two planes of circular section) of that ellipsoid. The common (internal and external) bisectors, at the centre A, of the angles BAB′, CAC′, made by the first and second, and by the third and fourth fixed lines, coincide in direction with the *greatest and least axes* of the ellipsoid; and the constant length b, of the side of either rhombus, is the length of the *mean semiaxis*. The diagonal LM′ of the first rhombus is the *axis of a first circle* on the ellipsoid, of which circle a diameter coincides with the second diagonal EE′ of the same rhombus; and, in like manner, the diagonal L′M of the second rhombus is the *axis of a second circle* on the same ellipsoid, belonging to the second (or *subcontrary*) system of circular sections of that surface: while the other diagonal E″E‴, of the same second rhombus, is a diameter of the same second circle. In the quaternion analysis employed, the first of these two circular sections of the ellipsoid corresponds to the equations (113.); and the second circular section is represented by the equations (114.), of the foregoing article.

64. We may also present the interpretation of those quaternion equations, or the recent double construction of the ellipsoid, in the following other way, which also appears to be new; although the writer is aware that there would be no difficulty in proving its correctness, or in deducing it anew, either by the method of co-ordinates, or in a more purely geometrical mode. *Conceive two equal spheres to slide within two cylinders* (of revolution, whose axes intersect each other, and of which each touches its own sphere along a great circle of contact), *in such a manner that the right line joining the centres of the spheres shall be parallel to a fixed right line*; then *the locus*

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* See Phil. Mag. for May 1848; or Proceedings of Royal Irish Academy for November 1847.

of the varying circle in which the two spheres intersect each other will be an ellipsoid, inscribed at once in both the cylinders, so as to touch one cylinder along one ellipse of contact, and the other cylinder along another such ellipse. And the *same* ellipsoid may be generated as the locus of *another* varying circle, which shall be the intersection of *two other equal spheres sliding within the same two cylinders of revolution,* but with a connecting line of centres which now moves parallel to *another fixed right line;* provided that the angle between these two fixed lines, and the angle between the axes of the two cylinders, have both one common pair of (internal and external) bisectors, which will then coincide in direction with the greatest and least axes of the ellipsoid, while the diameter of each of the *four sliding spheres* is equal to the mean axis. In fact, we have only to conceive (with the recent significations of the letters), that four spheres, with the same common radius $=b$, are described about the points L, M', and L', M, as centres; for then the first pair of spheres will cross each other in that circular section of the ellipsoid which has EE' for a diameter; and the second pair of spheres will cross in the circle of which the diameter is $E''E'''$; after which the other conclusions above stated will follow, from principles already laid down.

[To be continued.]

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxii. p. 541.]

March 23, 1848. "OBSERVATIONS on some Belemnites and other fossil remains of Cephalopoda, discovered by Mr. Reginald Neville Mantell, C.E., in the Oxford Clay, near Trowbridge in Wiltshire." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., Vice-President of the Geological Society.

The author states, that a line of railway now in progress of construction to connect the large manufacturing town of Trowbridge with the Great Western, being part of the Wilts, Somerset, and Weymouth line, traverses extensive beds of the Oxford clay of the same geological character as those at Christian-Malford in the same county, which furnished the remarkable fossil cephalopods described by Mr. Channing Pearce under the name of *Belemnoteuthis,* and by Professor Owen (in a memoir which received the award of a Royal Medal of this Society), as the animals to which the fossils commonly known by the name of *Belemnites* belong.

The son of the author, Mr. R. N. Mantell, being engaged in these works under the eminent engineer Mr. Brunel, availed himself of the opportunity to form an extensive and highly interesting collection of the fossils of the Oxford clay, and other oolitic deposits cut through or exposed by the engineering operations. Among those transmitted to the author are many illustrative examples of Belemnoteuthes and Belemnites; some of which confirm the opinions entertained by the late Mr. C. Pearce, Mr. Cunnington, and other competent observers, that the body and soft parts, with the cephalic uncinated arms, &c. of cephalopods, obtained from Christian-Malford by the Noble President and Mr. Pearce Pratt, and referred by Professor Owen in the memoir above-mentioned to the Belemnite, belong to a distinct genus—the Belemnoteuthis.

The author describes and figures several perfect examples of the phragmocone of the Belemnoteuthis, and institutes a comparison between them and a beautiful example of the phragmocone of a belemnite occupying the alveolus of the guard; and defines the essential differences observable in the form and structure of these chambered calcareous cones. He especially points out as distinctive characters of the phragmocone of the Belemnoteuthis, two flat longitudinal ridges which extend upwards from the apical extremity, and the granulated and striated external surface of the epidermis. The phragmocone of the Belemnite has a smooth surface, is destitute of any longitudinal ridges, and terminates at the apex in a very fine point, the axis being in an oblique direction.

The author next describes a remarkable specimen of a Belemnite, twenty-two inches in length, in which the osselet or guard, phragmocone, and capsule or receptacle, are preserved in connexion. In this fossil is demonstrated, for the first time, the upper or basal termination of the phragmocone, with two elongated calcareous processes extending upwards from the margin: these are analogous in form and position to the prolongations from the peristome of the outer chamber of certain Ammonites, as for example, in *A. Jasoni.* In the phragmocone of the Belemnoteuthis the peristome is entire.

Another interesting part of the structure of the Belemnite, not previously detected, is also shown in the same specimen, as well as in many other examples found in the Oxford clay near Trowbridge; namely, a calcareous shelly periosteum or capsule, which invests the guard, and expands upwards into a horny sheath or receptacle, that surrounds the basal chamber of the phragmocone in which the viscera were probably contained. This receptacle was formerly supposed to originate from within the alveolus of the guard. Mr. Miller, many years ago, inferred the existence of a vascular integument around the guard from the meandring impressions of blood-vessels observable on the surface of some specimens; but the presence of a calcareo-corneous capsule or sheath investing the guard, and expanding into a horny receptacle, has not till now been demonstrated.

The author considers the facts described as proving that the cephalopod of the Belemnite was entirely distinct from the Belemnoteuthis; and that the muscular mantle, cephalic arms, and other parts referred by Professor Owen to the former, exclusively belong to the latter genus.

He concludes that the remains of at least three genera of naked

Cephalopoda occur in the argillaceous deposits of the oolite in Wiltshire; namely, the first or true *Calamary*, with a horny dorsal gladius or pen; the second, the *Belemnoteuthis*, or a decapod with uncinated cephalic arms, ink-bag, pallial fins, and a corneo-calcareous phragmocone; and the third, the *Belemnite*, which possessed a phragmocone having the apical part implanted in the cavity or alveolus of a guard or osselet, which in its original state resembled in substance the sepiostaire of the Cuttle-fish, but is generally found mineralized by calcareous spar; and the peristome, possessing two or more elongated shelly processes; both the guard and the phragmocone being invested with a corneo-calcareous capsule or receptacle. He observes, lastly, that the body and other soft parts of the cephalopod of the Belemnite are at present unknown. The author's communication was illustrated by drawings, and accompanied by the specimens above described.

March 30.—"Chemical Researches on the Nature of Wax." By Benjamin Collins Brodie, Esq. Communicated by Sir Benjamin Collins Brodie, Bart., F.R.S.

It is known that bees'-wax is separable, by means of boiling alcohol, into two portions: to the one, which is more soluble in alcohol than the other portion, the name of *Cerine* has been given: the residuary portion, which does not dissolve, has been termed *Mycicine*. In this paper the author gives an account of his investigation of the properties of the former of these substances, namely Cerine.

This substance has been represented by certain chemists in France, M. Lewy and M. Gerhardt, as being convertible by oxidation into stearic acid, and as being a substance which stands with respect to that acid in the remarkable relation of an aldehyde. These views the author believes are incorrect; and he states that no pure chemical substance was procured by these chemists from cerine, and that the substance of which the greater part of the cerine consists is no aldehyde, but a hydrated acid, existing as such in bees'-wax.

The acid is best prepared by precipitation from the alcoholic solution of the cerine by an alcoholic solution of acetate of lead, and subsequent separation and precipitation of the acid by methods described in the present paper. When purified, the acid is a white brittle body, of a crystalline appearance, melting at from 79° to 80° C. The formula of the acid is $C_{54}\,H_{54}\,O_4$, a formula which was determined by the analysis of the silver salt having the constitution $C_{54}\,H_{53}\,O_3 + AgO$, and of the compound ether $C_{58}\,H_{58}\,O_4 = C_{54}\,H_{53}\,O_3 + C_4\,H_5\,O$. The acid is volatile: it was analysed after distillation; and it was also procured from the wax itself in a pure state by simple processes of crystallization. To this acid the author gives the name of *Cerotic acid*.

By the action of chlorine, the wax-acid is converted into a substance having all the appearance of a gum-resin; a change analogous to which may be effected in various other wax substances examined by the author. It has still the characters of an acid, and has the formula $C_{54} \left\{ \begin{matrix} H_{42} \\ Cl_{12} \end{matrix} \right. O_4$, a formula which is confirmed by that of the compound ether $C_{58}\left\{\begin{matrix}H_{46}\\ Cl_{12}\end{matrix}\right. O_4$. The analyses of these substances are given.

When distilled in a pure state, the cerotic acid is volatile. When mixed with other waxy matters, however, it passes by distillation entirely into volatile oils, a circumstance which accounts for the fact that it has never been found dissolved in the wax distillate. By precipitating a weighed quantity of wax by acetate of lead, the quantity per cent. of the cerotic acid in the bees'-wax, namely 22, was determined.

This acid was present in all the European bees'-wax examined by the author; but suspecting that its quantity might vary in other instances, he procured bees'-wax from Ceylon, formed under different conditions of climate and vegetation, and found on examination that there was a total absence of the acid in that specimen. The author draws attention to this curious variation in the nature of an animal secretion under different conditions of life, a variation of which we have another example in that of the volatile acid of butter, discovered by Lerch[*]; namely, that the butyric and caproic acid of one season were, in another, replaced by vaccinic acid, differing from the former acids in the amount of oxygen alone.

"A statement of the working of the Compasses on board the Honorable East India Company's Iron Steamer Pluto, from September 1841, on her passage from England to China, and during her service in those seas, until her arrival at Calcutta in January 1843." By John Tudor, Commander R.N. Communicated by S. Hunter Christie, Esq., Sec. R.S., &c.

The author states that the compasses of the Pluto were adjusted by Mr. Sims, of the firm of Troughton and Sims, by order of Mr. Pencoote of the East India House, under whose directions that ship was fitted out; and it is to the great pains taken by Mr. Sims in placing the magnets employed for counteracting the local attraction that the author attributes the undeviating accuracy of those compasses during the whole time the Pluto was under his command in both hemispheres. He observes that, in the first place, much care is required in securing the magnets, and protecting them from wet, after their proper position has been ascertained. In the case of the Pluto, two magnets were placed under the deck in the author's cabin; one of them eighteen inches below the deck, being, it is true, an eyesore, but one of trifling consideration, when compared with the great importance of the well-working of the compass. The next point to be attended to is that the cards, or needles, should be all of the same size, and exactly corresponding with that of the compass used at the placing of the magnets for counteracting the local attraction. The bittacles should all be of the same make and height, and the compass-boxes of the same size; so that whenever a new compass or a fresh bittacle is wanted, the circle in which the needle moves may remain at the same angle from the magnet as at the first adjustment. On a strict attention to these precautions will depend the well-working of the compass in all iron vessels, and also in wooden vessels when-

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[*] [Chem. Gazette, vol. ii. p. 377.]

ever the quantity of iron they contain creates the necessity of measures being taken for counteracting local attraction.

It has been alleged that the adjustments for local attraction made in northern latitudes are not correct when the ship is south of the equator; but the author states that, in the Pluto, he observed no difference; that ship having made, while under his command, passages of many thousand miles, comprising 94 degrees of latitude, namely from 51° North to 43° South, and 153 degrees of longitude, namely from 30° West to 123° East, during the whole of which he never found any other correction for the compasses necessary excepting that required for the magnetic variation, the local attraction having been completely neutralized.

A diagram is subjoined, showing the positions of the compensating magnets with relation to the compass.

"Practical Remarks on Annealing Flint-Glass." By Apsley Pellat, Esq., of the Falcon Glass Works, Holland Street. Communicated by Joshua Field, Esq., F.R.S.

This paper is entirely occupied with practical details relating to the art of annealing flint-glass and depriving it of colour, the author stating that he leaves the rationale of these facts to be explained by philosophers. He thinks, however, that they are reconcileable with the theory of Scheele, as explained by Bergman, and detailed by Murray in his work on Chemistry.

April 6.—"Determinations of the Magnetic Inclination and Force in the British Provinces of Nova Scotia and New Brunswick in the summer of 1847." By Professor George W. Keely, of Waterville College, Maine, United States. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

The observations recorded in this paper are of two kinds; first, those for the relative total force, which were made with a pair of Lloyd needles and an inclination-circle, seven inches in diameter, with two verniers reading to single minutes; and secondly, those for the absolute horizontal force made with a unifilar magnetometer. After a detailed description of these instruments, the results of the observations are given, occupying several pages of tables.

May 11.—"On the Chemical Nature of a Wax from China." By Benjamin Collins Brodie, Esq. Communicated by Sir Benjamin Collins Brodie, Bart., F.R.S.

The wax which is the subject of this investigation, is a substance imported into this country from China. It has the general appearance of spermaceti, but is harder than that body. The author gives reasons for believing that this wax, like bees'-wax, is a secretion from an insect.

The wax may be decomposed by fusion with hydrate of potash, by which process two substances are procured; namely, a wax acid, which, combined with the potash, forms a soap; and another body which is dissolved in the soap solution. By precipitation with chloride of barium and washing out the dried baryta salt with ether, or other suitable solvents, the two substances may be separated.

The substance dissolved in the ether has the appearance of a wax. By crystallization its melting-point may be raised to 79° C., at which point it is fixed. The body, when analysed, gave numbers agreeing with the formula $C_{54}H_{56}O_2$, the formula, namely, of the alcohol of cerotic acid, the acid which in a previous paper the author has shown to exist in a free condition in bees'-wax, and the constitution of which he there determined. To this alcohol the author gives the name of *cerotine*. By oxidation, by means of lime and potash, the alcohol is capable of being converted into cerotic acid, $C_{54}H_{54}O_4$. The analyses of the acid and of its silver salt are given. The formula of the alcohol is further confirmed by the analysis of its combination with sulphuric acid; and the process to be employed to procure this substance is detailed. Its formula is $SO_3, C_{54}H_{55}O + HO$; the sulphate of the oxide of cerotyle, using the usual chemical language to express the nature of the combination. By the action of chlorine on the alcohol, the alcohol-type is destroyed, and a body is formed, analogous to chloral, containing two equivalents of hydrogen less than the alcohol itself. The analyses lead to the formula $C_{54}\left\{\begin{matrix}H_{40\frac{3}{4}}\\Cl_{13\frac{1}{4}}\end{matrix}\right.O_2$. The substance has the appearance of a resin.

By decomposing the above-mentioned baryta salt, after the cerotine has been entirely removed by washing with suitable solvents, the same cerotic acid may be obtained as that into which the alcohol itself is converted by oxidation. The analysis of the acid and of its silver salt is given.

This Chinese wax cannot be distilled without decomposition. By its distillation two substances are procured; cerotic acid, $C_{54}H_{54}O_4$, and hydrocarbon. The hydrocarbon consists principally of a solid matter, one of those substances which, in the opinion of the author, have been indiscriminately classed together under the general name of *paraffine*. This substance, to which he gives the name of *cerotine*, contains equal equivalents of hydrogen and carbon, and has the formula $C_{54}H_{54}$. This formula is determined with precision by the action of chlorine on the substance, which gives rise to a series of products of substitution, of which several were analysed, namely the substances

$$C_{54}\left\{\begin{matrix}H_{35}\\Cl_{19}\end{matrix}\right., \quad C_{54}\left\{\begin{matrix}H_{33}\\Cl_{21}\end{matrix}\right., \quad C_{54}\left\{\begin{matrix}H_{32}\\Cl_{22}\end{matrix}\right..$$

The density of the vapour of cerotine cannot be taken, as, by distillation, it is decomposed. The experiment was made of distilling and redistilling the substance in a sealed tube, in which cases it passes entirely into fluid and gaseous hydrocarbon.

The analysis of the Chinese wax itself corresponds with the formula $C_{108}H_{108}O_4$, which admits of a simple explanation of the nature of its decompositions: its decomposition by potash being explained by the equation

$$C_{108}H_{108}O_4 + KO\,HO = (C_{54}H_{53}O_3 + KO) + C_{54}H_{56}O_2,$$

and its decomposition by heat by the equation

$$C_{108}H_{108}O_4 = C_{54}H_{54}O_4 + C_{54}H_{54},$$

the substance itself belonging to the class of compound ethers.

The author announces his intention of following up this paper by a third on the constitution of myricine.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxxii. p. 148.]

Jan. 14, 1848.—*Satellites of Saturn.*

Observations of Mimas, the closest and most interior satellite of Saturn. By Mr. Lassell.

It is to be regretted that, owing to the discovery of the closest two satellites of Saturn having been made at a period long after the five others became known, it is difficult, in referring to the individuals, to quote them by numerals which shall, explicitly and without ambiguity, point out which satellites are meant.

Sir John Herschel, seeing this difficulty, has invented and published in his Cape Observations a nomenclature which completely removes it, and moreover will retain its precision should any more satellites hereafter be discovered. He gives a proper name to each of them; and beginning with that nearest to Saturn, designates them thus: Mimas, Enceladus, Tethys, Dione Rhea, Titan, and Japetus. I cannot but think this nomenclature a great improvement, and worthy of general adoption.

Of Mimas, the satellite I now refer to, I have obtained in all five observations.

1846, Aug. 25. About 11½ P.M. mean time the satellite appeared at its greatest elongation westward. Aug. 30. The satellite appeared a little short of its greatest elongation eastward; and again, on the 1st of September at 10 P.M., the satellite appeared a little beyond its greatest eastern elongation.

During the late opposition of Saturn, although I have been very watchful of opportunities, I have been able to see it certainly but twice, viz. on the 16th of October and 18th of November.

On the 16th of October at $8^h 35^m$ mean time, Mimas was, as nearly as I could estimate, exactly at its greatest western elongation, appearing to be five or six-tenths of the length of the preceding arm of the ring distant from its extremity. This evening the satellite was better seen than on any other occasion.

On the 18th of November, at about 8½ hours, the satellite again appeared as nearly as possible in the same situation as on the 16th of October. The power with which Mimas was generally best seen is a Coddington lens magnifying 567 times. The difference of visibility between Mimas and Enceladus is almost incomparable; the latter being instantly seen in my telescope, under all tolerable circumstances, when within 40° or 50° of its greatest elongation; whilst in any but the very finest circumstances, Mimas is an object of great difficulty.

Satellites of Uranus[*].

Observations by Mr. Lassell.

These observations are principally of the two brightest, those first observed by Sir W. Herschel, or I. and II., with estimations of two others, presumed to be his inferior and middle satellites, or 1 and 2. I have not been able to obtain an undoubted observation of any satellite exterior to II.

The observations are made in position and distance, like those of a double star. The positions are reckoned from the north point, as zero, round by the *following* side.

The results here presented are generally the means of two or three measures. The observations of the I. and II., graphically projected, show *apparently* elliptic orbits, having their transverse axes very nearly perpendicular to the plane of the ecliptic and the proportions of the axes, roughly as 10 to 6.

The results marked *e* are careful *estimations* only.

As to the single observation of the satellite 2, there was unquestionably a point of light at the place indicated, which kept its relative position to the planet for about two hours. Moreover, that part of the sky in which Uranus was seen on November 6 was carefully scrutinized on November 8, without my being able to detect any stars in the places where the four satellites had been seen.

		1.		I.		2.		II.	
	G.M.T.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
1848.	h	°		°				°	
Sept. 14	13·6	10*e*		322 *e*				334·7	
27	9·8	326*e*		160 *e*				345 *e*	
29	11·5	336*e*		38 *e*				302·1	28″·98
Oct. 1	12·5	348*e*		334·6	32″·8			213·4	27·8
2	10·5			307·7				192·1	
11	11·2			294·6	20·57			332·7	41·39
13	10·0			182 *e*				283·0	
15	11·3			133·3	26·27			196·8	33·44
16	9·5			77·6				180	29·15
17	11·0			9·4	27·37			166·1	46·38
Nov. 1	9·4			150				144·1	33·33
6	10·3	349*e*	11″*e*	262	20 *e*	80*e*	10″*e*	349	45 *e*
8				187·2				316·5	
9	9·5			284·1				163·8	

Note on the Satellites of Uranus. By M. Otto Struve.

The satellites of Uranus were first seen by Sir W. Herschel on January 11, 1787, six years after his discovery of the primary planet. By a continued series of observations that year he established the undoubted existence and the approximate distance and periodic times of two *principal* satellites. In a second memoir (Phil. Trans. 1798) he announced the discovery of four new *supplementary* satellites; and in a third memoir (Phil. Trans., 1815) he presented the results of his observations to that date. The faintness of the objects ob-

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[*] As a temporary nomenclature, we call the two first discovered satellites I. and II., and the others 1, 2, 3, 4, reckoning from the planet.

served and the mounting of his telescope* sufficiently account for discrepancies in Sir William's results. The measures in distance were, with his means, *exceedingly* difficult: hence we have only two such measures of the interior of the two principal satellites, and none at all of the supplementary satellites. There are, however, several measures of the exterior principal satellite II.

In the memoir of 1788, Sir William gives $44''\cdot23$ as the greatest elongation of II. from Uranus at his mean distance from the earth, and calculates the similar greatest elongation of I. to be $33''$, *i. e.* from the periodic time and Kepler's law. He considers these to be approximate only. In the memoir of 1815 he uses in round numbers, and for numerical convenience, $48''$ and $36''$ for the elongations; and he adds a list of selected observations from which more certain values may be deduced; but it does not appear that he performed this calculation. The other elements of the orbits of the two satellites are pretty exact; the position of the planet was very favourable for these determinations. In 1797 the apparent orbits of the satellites were nearly right lines.

As to the supplementary satellites and their orbits, it would appear from both Memoirs that Sir William did not affect to speak of them with confidence. Though he had no doubt of their existence generally, yet he avowed that "to determine their numbers and situation would probably require an increase of illuminating power." The precision with which their periodic times, &c. are stated by various authorities must not be supposed to rest on any certain foundation, but on Sir William's approximations and shrewd guesses.

Sir John Herschel directed his attention to the satellites of Uranus in 1828–1832, and the results are to be found in our Memoirs, vol. viii. The situation of Uranus was unfavourable, the planet having considerable southern declination. From his own observations, combined with those of his father, Sir John deduced the following periodic times:—

$$\text{I.} = 8^{d}\ 16^{h}\ 56^{m}\ 31^{s}\cdot3 \qquad \text{II.} = 13^{d}\ 11^{h}\ 7^{m}\ 12^{s}\cdot6;$$

but "of other satellites," he says, "than these two I have no evidence."

M. Lamont, Director of the Observatory of Munich, commenced his observations of the satellites of Uranus in 1837. His memoir is printed in our Transactions, vol. xi. They relate to the principal satellites, I. and II. M. Lamont believes that he once saw what he takes to be 4, the most distant of the supplementary satellites.

The mass of Uranus has been found to be,—

$\frac{1}{20008}$, the Sun = 1, by Sir W. Herschel's observations.

$\frac{1}{24605}$ by M. Lamont.

$\frac{1}{17918}$ by Bouvard, from the perturbations of other planets by Uranus.

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* This was a 20-foot reflector with the *front view*, and Sir William's usual mounting.

M. Lamont does not consider his results as very certain, since I. and II. gave somewhat discordant results; but he is of opinion that Bouvard's mass is certainly too large.

The determination of the mass of Uranus has become of great and pressing interest, since the theoretical researches of MM. Le Verrier and Adams into the apparently irregular motion of the planet, have led to the memorable discovery of Neptune.

If we suppose the two planets to be equal, it is clear that the effect of Uranus to perturb the motion of Neptune will be more sensible than the reciprocal effect of Neptune upon Uranus, on account of the greater distance of Neptune from the sun. The theory of Neptune, therefore, must continue imperfect until a correct value of the mass of Uranus can be assigned. With this object in view, M. Otto Struve has undertaken a series of micrometrical measurements of the two principal satellites, which he hopes to complete about the beginning of February 1848. But though the planet is now 5° or 6° north of the equator, and therefore far more favourably placed than when Sir J. Herschel pursued his investigations, still the observations are very difficult, requiring steady images and a perfectly transparent sky. In this unfavourable autumn only fourteen nights have hitherto been sufficiently fine. Generally speaking, the distances of II. have been measured as satisfactorily as the angles of position, and only twice or thrice has the object been too faint to be well observed. The reduction is delayed until the series is completed.

When M. Struve had become familiarly acquainted with these two satellites, he turned his attention to their *supplementary* companions. After being twice deceived in measuring a small star instead of a satellite, he saw on October 8 an object, of which he made the following measures in position:—

	h	m		
Oct. 8	11	16	Pulkowa M. T.	Position = 178°·7
		18		186·5
		35		178·8
		36		182·3
		42		176·7

An estimate of comparative distance made the distance = 14″·2.

These observations do not, indeed, prove the object to have been a satellite; but the difference between the first and last angles of position, if it had been a star, would, from the motion alone of Uranus, have been 9°. The following observations leave no doubt that a satellite was observed:—

1847.	Pulkowa M. T. h m	Position. °	No. obs.	Pulkowa M. T. h m	Distance. ″	No. obs.
Nov. 1	9 52	190·5	3	10 4	17·8	4
	10 23	194·0	3			
28	8 31	203·6	6	8 37	17·0	2
	9 48	202·1	4	8 52	16·7	2
Dec. 9	11 28	218·6	4	11 28	13·7	Estim.
10	5 53	180·1	4	6 3	17·0	4
	6 16	182·2	5			

Nov. 1. The satellite better seen than I. and II., which are very near each other.
28. The satellite quite as visible as I.
Dec. 9. The images very good, but the observation not sure. The satellite seen by glimpses.
10. The satellite as well seen as II., but fainter than I.

The observations of Nov. 28 show clearly enough that the object was a satellite: the *position* angle of a fixed star would have been altered 10°.

It may at first sight seem odd, that *all* the observations are made on the *same side* of the planet. There are, however, many analogous cases among the satellites, if this should always occur. The satellites I. and II., for instance, are found to alternate in brightness. There has been, however, no really fine observing night after October 8, on which the satellite could have been observed on the other side.

The observations of this inferior satellite are not sufficiently numerous to yield an accurate orbit. It may, however, be assumed pretty safely, that it has been observed near its greatest elongation, *i. e.* that the apparent semiaxis is between 17″ and 18″; in which case it must have a period somewhere about three or four days.

Now the satellite has, approximately at least, always been observed at the same part of its orbit, and hence the intervals of twenty-four, twenty-seven, and twelve days must, approximately, be multiples of the periodic time. The hypothesis of a period of three days, which first presents itself (or more accurately, from the observations of October 8 and December 10, $2^d\ 23^h\ 45^m$), M. Struve finds wholly incompatible with the observation of December 9, and he prefers a period of four days (more accurately $3^d\ 22^h\ 10^m$), which, admitting a slight error on Nov. 1, agrees equally well with the observations. This result also accords better with the presumed mass of the planet; for the first period and greatest distance, 17″·5, gives the mass $\frac{1}{15480}$ larger than Bouvard's, while the second period assigns $\frac{1}{26860}$ to the mass, somewhat less than Lamont's, but yet a tolerable approximation to it.

The difference between M. O. Struve's period for this satellite, and that of $5^d\ 21^h\ 25^m$, assigned to Sir W. Herschel's, is considered by M. O. Struve to be due to an erroneous estimate of the semiaxis. He has no doubt of the identity of the two objects.

After remarking that the foregoing observations completely prove the existence of Herschel's first supplementary satellite, M. Struve remarks that the visibility of this satellite depends less upon the aperture of the telescope employed than on the sharpness of the image shown. All the telescopes, in his opinion, which will show I. and II., are *large* enough to show I. if they are *good* enough. It is not fainter than the other two, but, from its greater proximity to Uranus, requires better defining power and freedom from stray light.

M. O. Struve has not hitherto seen any other supplementary satellite, but he remarks that the position of Uranus is every year becoming more favourable for this inquiry; and there can be little doubt of their speedy rediscovery, so far as they actually exist.

A Method of Calculating the Orbit of a Planet or Comet from Three observed Places. By Professor Challis.

This method resembles in principle that of Laplace, of which it may be regarded an extension, the object of the author being to include in the calculation differential coefficients of the *third* and *fourth* orders, for the purpose of ensuring greater accuracy in the final results. The equations by which the problem is solved are formed as follows. If α and β be the observed right ascension and north polar distance of the body at one of the given times, corrected to a given equinox and given position of the earth's equator, and x, y, z be its co-ordinates at the same time, having their origin at the place of the observer, these quantities are related to each other by the two equations

$$x = y \cot\alpha, \quad x = z \cos\alpha \tan\beta.$$

Each observed place furnishes two such equations. To include parallax, the origin of co-ordinates is transferred to the earth's centre. If ρ be the distance of the body from the earth, and q be the aberration constant, the effect of aberration is taken into account by changing x, y, z respectively into

$$x - \frac{dx}{dt}q\rho, \quad y - \frac{dy}{dt}q\rho, \quad z - \frac{dz}{dt}q\rho.$$

The origin of co-ordinates is then transferred to the centre of the sun, by calculating exactly the sun's co-ordinates at the three times of observation. Thus six equations are formed in which the unknown quantities are the heliocentric co-ordinates of the body. The co-ordinates at the first and last times of observation are expressed in terms of the co-ordinates at the intermediate time, by series including differential coefficients of the *fourth* order of the latter co-ordinates. The six unknown quantities to be found are, then, the heliocentric co-ordinates x_2, y_2, z_2 at the middle time, and their first differential coefficients $\frac{dx_2}{dt}, \frac{dy_2}{dt}, \frac{dz_2}{dt}$. A first solution is obtained by including only differential coefficients of the second order, and neglecting the aberration terms. This conducts to the following values of the co-ordinates,—

$$x_2 = \text{M} + \frac{\text{N}}{r_2^3}, \quad y_2 = \text{M}' + \frac{\text{N}'}{r_2^3}, \quad z_2 = \text{M}'' + \frac{\text{N}''}{r_2^3},$$

r_2 being the body's heliocentric distance. Hence

$$r_2^2 = \left(\text{M} + \frac{\text{N}}{r_2^3}\right)^2 + \left(\text{M}' + \frac{\text{N}'}{r_2^3}\right)^2 + \left(\text{M}'' + \frac{\text{N}''}{r_2^3}\right)^2.$$

For solving this equation, a graphical method given by I. I. Waterston, Esq., in the Monthly Notice of the Royal Astronomical Society for December 1845, is recommended. The value of r_2 being found, those of x_2, y_2, z_2, and their first differential coefficients, are readily derived, the equations for determining them being linear.

By means of the first approximate values of the unknown quantities, the second order of approximation is proceeded with so as to include differential coefficients of the third order and the more important aberration terms. The third approximation included differential coefficients of the fourth order, and some small additional aberration terms. These approximations are so conducted, that the quantities obtained are *corrections* to the first obtained values, and it is consequently not necessary to calculate with seven-figure logarithms.

The values of x_2, y_2, z_2, $\frac{dx_2}{dt}$, $\frac{dy_2}{dt}$, $\frac{dz_2}{dt}$, being thus obtained as accurately as possible, the elements of the orbit are readily derived by known formulæ. As the observed right ascension and north polar distance were not corrected into latitudes and longitudes, the elements are by this calculation referred to a plane through the sun's centre parallel to the earth's equator in a given position. By a simple computation they may be transferred to the plane of the ecliptic. But the original form is the most convenient for obtaining geocentric co-ordinates in terms of the eccentric anomaly, for the purpose of calculating an ephemeris, and also for deriving equations of condition by which the elements may be corrected by future observations. The method of doing this the author proposes to describe at another opportunity.

A brief Notice of the Imperial Observatory of Poulkova. By the Astronomer Royal*.

The observatory of Poulkova was built on the plans furnished by its director, M. Struve; the instruments are, for the most part, constructed according to his special instructions. The peculiar scope of this noble establishment is *sidereal* astronomy in its widest sense; and Mr. Airy strongly expresses his admiration of the definiteness of the purpose which M. Struve had in his mind, and of the thorough manner in which it has been carried into effect. He says that " no astronomer can feel himself perfectly acquainted with modern astronomy in its most highly cultivated form, whether as regards the personal establishment, the preparation of the buildings, the selection or construction of the instruments, or the delicacy of using them, who has not well-studied the observatory of Poulkova. To this excellence many antecedent circumstances have materially contributed. The first of these is the personal character of M. Struve; his mature experience obtained before the intention of building the new observatory had been formed; his vigour in arranging the plans on a large scale, and in superintending constructions even to the minutest point, and his perseverance and skill in arranging the subsequent proceedings of the observatory." The liberality of the emperor, the freedom of choice as to site, instruments, &c., have been admirably employed by the director. The character of the buildings, in addition to their perfect suitability to their design, is that of splendour without extravagance. The foundations are most carefully laid, and solid beyond any former example†.

The large equatoreal, by Merz and Mahler, is the instrument which has principally extended the fame of the Poulkova Observatory.

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* The Astronomer Royal visited Poulkova last summer, and gave orally as account of the Observatory at the meeting of the Society in November, the substance of which was communicated in a letter to Professor Schumacher, and printed in the *Astronomische Nachrichten.*

† The Observatory and its instruments may be well understood from the

The optical part is doubtless admirable, though, perhaps, more control over the adjustment of the object-glass would be desirable; but Mr. Airy was far from satisfied with the rest of the instrument. It did not obey its slow motions in right ascension and declination with promptitude or accuracy, and the clock movement was deficient in power. In short, though in the delicate and experienced hands of M. Otto Struve the telescope is made to yield the utmost of its powers, Mr. Airy does not consider the instrument, as he saw it, a good specimen of mechanism.

The heliometer, by the same artists, met with much greater admiration; and, indeed, Mr. Airy seems to have been very much impressed with the beauty and excellence of all the other instruments. He goes so far as to say, that he considers the meridional observations at Poulkova to be greatly superior to those of any other observatory with which he is acquainted. The transit and vertical circle of Ertel, the meridian circle and prime vertical telescope of Repsold, are praised, with some critical remarks, but no serious drawback. Some doubt is expressed whether the prime vertical telescope would, in other hands than those of M. Struve himself, give quite such marvellous results as he obtains from it: still, in Mr. Airy's judgement, a prime vertical telescope is far superior to any zenith sector yet contrived for the investigation of aberration, parallax, &c., or small differences of latitude; and he considers the Poulkova results as leaving all competition far behind.

Mr. Airy "had the pleasure of witnessing complete observations made by M. Struve with the prime vertical telescope, and pays the tribute of his admiration to the caution, the delicacy, the steadily waiting till the proper time, the promptitude at the proper time, which distinguish the director's mode of observing."

The excellence of the levels attached to the instruments at Poulkova, and the successful precautions constantly practised there to avoid error in their use, are particularly noticed. The construction of the standard barometer is pronounced to be "admirable," and the noble library* considered "to be probably the most complete in the world in reference to its peculiar subjects."

On the Transit Instrument at the Bombay Observatory. By Captain Shortrede.

Captain Shortrede gives a short account of the instrument, its mounting, &c., before he became acquainted with it. Such mistakes as he relates can scarcely occur a second time, and therefore they may be passed over in silence.

When Captain Shortrede visited the Bombay Observatory during his trigonometrical operations, he learned that the instrument was considered incurably defective. A full consideration, however, led him to a different opinion; and as he is himself a workman, and

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beautiful work published by M. Struve, *Déscription de l'Observatoire Astronomique Central de Poulkova*, St. Pétersbourg, 1845, 4to, with a volume of plates, which has been distributed most liberally among astronomers.

* A Catalogue of this library has been recently published, *Librorum in Bibliotheca Speculæ Pulcovensis contentorum Catalogus Systematicus*, Petropoli Typis Acad. Scient. 1845, 8vo.

from his former connexion with the Mint as assistant-engineer, had great facilities for procuring the best assistance which the country afforded, he undertook the re-erection of the instrument, which, his own services being gratuitous, would not exceed 300 rupees. This offer being communicated to the governor, the sum specified was readily advanced.

The stone pillars are two feet square: the upper part of one had been greatly split and shaken by unskilful perforations, and Captain Shortrede directed the upper fifteen inches of each to be taken off. These he replaced by two blocks of Puna stone, which are eighteen inches square at bottom, sloping to twelve inches square at top. This size seemed sufficient for steadiness, and was as large as could well be procured at Puna or transported to Bombay. Due care was taken to attach the blocks perfectly to the piers.

The new Y plates were of bell-metal, cast at the Mint, and well and truly worked; particular care was taken that the plate bore truly on the stone near the screw-holes.

The pivots of the axis are supported throughout their length; the motions for every adjustment were found free and without shake. Instead of the usual Y, Captain Shortrede adopted a collar bearing, which he intended should exactly fit the pivots. The workman, however, made them somewhat large, and, as he finished them by grinding, not quite true. Having no time to get new collars made, Captain Shortrede had a groove cut out at the bottom of the bearing and at the top of the covering-piece, in each of which a slip of wash-leather was secured by lac-varnish. These do not interfere with the bearing, and they stop the dust before it comes to the bearing. The constant daily shower of sand from the sea-breeze and neighbouring shore would soon destroy any pivot working in the ordinary way; and this consideration suggested the collar mounting, which was intended to be as close fitting as possible. No iron was used in the work, the liability to rust in such a situation makes the material unsuitable.

The instrument thus fitted up continues to work satisfactorily. There are no counterpoises, Captain Shortrede (luckily perhaps) not having had time to procure them before quitting Bombay.

Self-luminous Spot in the Moon.

On the 11th of December last, at 6 P.M., while Mr. Hodgson, of Fir Grove, Eversley, was observing the dark body of the moon, " a bright spot, about one-fourth the angular diameter of Saturn, was perceived, which, though it varied in intensity like an intermitting light, was at all times visible." On this occasion Mr. H. used an achromatic telescope of five foot focal length, and powers 50 and 80. The bright spot was best seen by the higher power, probably, as he suggests, because the field was smaller and darker, but it was instantly visible to the most uninitiated eye when the bright part of the moon was excluded from the field.

On the following day glimpses of the same spot were caught between passing clouds with a Newtonian reflector; power 40.

From Mr. Hodgson's sketch, the bright spot is about 5′ below the real northern point and near the following limb.

XIII. *Intelligence and Miscellaneous Articles.*

ON THE FORMATION OF HYPONITRITE OF SILVER.

M. PERSOZ found that by heating nitrate of silver so as to expel a portion of its oxygen, the residue, when treated with sufficient water at 40° F. to dissolve the nitrate remaining undecomposed, yielded a gray residue, which was insoluble in cold but soluble in hot water; and the solution on cooling deposited long acicular crystals, possessing all the properties of hyponitrite of silver.

Being satisfied from this experiment that hyponitrite of silver results from the decomposition of the nitrate by heat, it appeared natural to conclude, that by cautiously heating the nitrate it might be totally converted into hyponitrite. All attempts to produce this effect were however unsuccessful. It was found not only to be impossible so to convert the whole of a given portion of the salt, but even to increase the quantity of hyponitrite beyond a certain proportion, because it decomposes much sooner than the nitrate. Its formation and existence, under the circumstances described, can be explained only by supposing that it acquires, by the presence of the nitrate of silver, a degree of stability which it does not possess when isolated. If this be the case, the nitrate of silver ought to be capable of being replaced by a more stable nitrate, and in point of fact this actually occurs; for by fusing and calcining a mixture of equal parts of nitrate of silver and nitrate of potash, a much larger quantity of hyponitrite is obtained and less silver is set free. This process of preparing the hyponitrite appears to be the most certain. The fine crystalline needles obtained by dissolving the salt in hot water, have a fatty and cotton-like appearance, and seem colourless in the liquid in which they form; whereas when taken from it, they always possess a greenish tint of greater or less intensity. When heated in a small glass tube by a spirit-lamp, these crystals are immediately decomposed into nitrous vapour and silver, which retains the form of the hyponitrite; when treated with concentrated sulphuric acid, they decompose instantly with the production of nitrous vapour and sulphate of silver.

One hundred parts of the crystallized hyponitrite yielded 69·87 of silver, showing that it is composed of—

One equivalent of hyponitrous acid	38
One equivalent of oxide of silver	116
	154

This result agrees with the previous determination of Mitscherlich, who found the salt to yield 70·082 per cent. of silver.

From the above-mentioned facts M. Persoz concludes,—

1. That nitrate of silver behaves up to a certain point similarly to the alkaline nitrates, being, like them, partially converted by heat into hyponitrite.

2. That this hyponitrite is formed only in the presence of nitrates which give it stability, such as the nitrates of silver, potash, soda and other alkaline nitrates.—*Ann. de Ch. et de Phys.*, Mai 1848.

ŒCONOMICAL PREPARATION OF OXIDE OF ANTIMONY.
BY M. E. G. HORNUNG.

The author states, that M. Froderking having published a process for the preparation of oxide of antimony by means of sulphuric acid, he tried whether the same process would not succeed with replacing antimony by the sulphuret. He mixed in an iron vessel fifteen parts of sulphuret of antimony, in very fine powder, with thirty-six parts of sulphuric acid, and exposed the mixture during a whole night to a gentle heat. The mixture at first thickened, and being frequently stirred, it afterwards became liquid on raising the temperature.

Eventually the mass became whitish, some sulphur fused and separated, and a large quantity of sulphurous acid was disengaged; the mixture was heated, and stirred frequently as long as the disengagement of sulphurous acid and the combustion of the sulphur continued. When no other vapour than that of sulphuric acid was formed, water was gradually added, and the mass was washed to remove the free sulphuric acid. The subsulphate of antimony was decomposed by carbonate of soda, and the oxide obtained washed: fifteen parts of sulphate [sulphuret?] of antimony yielded thirteen parts of oxide of a greenish-white colour, which, with the exception of some impurity, dissolved in tartaric acid. This process is the most œconomical for the oxide intended for the preparation of emetic tartar.—*Journ. de Ph. et de Ch.*, Mai 1848.

[I may remark, that the use of antimony and sulphuric acid for preparing the oxide of antimony was proposed and published by me in 1811, in my Experimental Examination of the *Pharmacopœia Londinensis*. The late Dr. Babington suggested soon afterwards to me to try as an improvement (and it is a great one) the use of sulphuret instead of metallic antimony. Although I did not at the time try the proposed alteration, I know it has since been adopted on a considerable scale, it having occurred as an improvement to two other parties with whom I am acquainted. I will only add, that there is no use in gently heating the mixture for a long time; it may be boiled to dryness at once, and the residue is fit for preparing tartarized antimony when merely washed with water, and without using any carbonate of soda.—R. P.]

ON THE CHRYSOTIL FROM THE VOSGES. BY M. DELESSE.

The serpentine of the Vosges, and especially that of Goujot near Eloyes, is intersected by a great number of veins which penetrate the rock in every direction. These veins are frequently microscopic, and generally are at most only from one to two centimetres in thickness; they are filled by an asbestiform substance, and which is generally called asbestus, but according to M. Delesse it ought to be referred to the chrysotil of M. Kobell.

It is formed of very fine parallel fibres, which may be easily separated. When in mass it is translucent, and the fibres are translucent when isolated; by exposure to the air they become opake and whitish. This substance is usually of a brightish oil-green colour, passing to olive-green; it has a characteristic pearly and silky lustre. Its density is 2·219. M. Delesse found it to consist of—

Silica	41·58
Alumina	0·42
Oxide of iron	1·69
Magnesia	42·61
Water	13·70
	100·00

The chemical composition of chrysotil is identical with that of the picrolite of Stromeyer, with the different noble serpentines recently analysed by M. Lychnell, and especially with the crystallized serpentine of Snarum. The density of chrysotil is, however, less than that of serpentine, which seems to indicate that it is a dimorphous variety of serpentine.—*Ibid.*

ON CHLORIDE OF GOLD AS A TEST OF ORGANIC MATTER IN COMMON WATER.

M. A. Dupasquier states, that after having found by numerous experiments that nitrate of silver was an uncertain test of the presence of organic matter in waters, he tried other processes, and at last found that chloride of gold might be successfully employed. The method of employing it is to put from twenty-five to thirty grammes of the water to be examined into a flask, and to add to it sufficient solution of perfectly neutral chloride of gold to render it slightly yellow, and afterwards to boil it. If the water contains only the usual quantity of organic matter found in drinkable waters, it retains its yellow colour; if, on the contrary, it contains an undue proportion of organic matter, it immediately becomes brown, then assumes a violet or bluish tint, which indicates the decomposition of the chloride of gold by the organic matter. By continuing the ebullition, the violet or bluish tint becomes deeper, if the proportion of organic matter be considerable; but if the liquid becomes slightly brownish or greenish, it is sufficient to prove that the water contains an unusual quantity of organic matter. The solution of gold must contain no excess of hydrochloric acid, as that would prevent the decomposing action of the organic matter.

It sometimes occurs, when the ebullition has been long continued, that the solution is rendered colourless by the precipitation of oxide of gold by the carbonate of lime in the water, unless the chloride has been used slightly in excess; and when it is so, the yellow tint may be modified by the liquid being rendered slightly turbid. To distinguish these effects from those of the decomposing reaction produced by the organic matter, a drop or two of hydrochloric acid is to be added, which will immediately dissolve the oxide of gold, and the liquid will assume its original appearance, unless the salt of gold has been decomposed by the organic matter. When, however, the chloride has been once reduced to the metallic state by the organic matter, it is not, like the oxide of gold, soluble in hydrochloric acid; and the liquor remains violet, bluish violet, or brownish or greenish violet, when there is great excess of chloride of gold. If, however, the water contains a trace of any nitrate, and it be again boiled, the gold becomes dissolved.

It has been objected, that all organic matters do not possess equal decomposing power with respect to the salt of gold. This objection is, however, groundless, when it is considered that the process is proposed for natural waters, the organic matter of which is always of the nature of ulmin or gein, and evidently derived from the washing of vegetable or animal matter, more or less decomposed, and frequently in the state of humus; and that these occur in lands into which rain-water filters, or in the soil of cities bathed by rivers which supply their wells and fountains. Under all these circumstances, and also in the cases of water which has filtered from cesspools, &c., the organic matter resulting from the spontaneous decomposition of animal and vegetable products, always decomposes the salt of gold.

M. Dupasquier made numerous experiments with distilled water, to which recent urine, gelatine, or crumb of bread had been added, and also some other organic matters which had not been altered by putrefaction. These substances generally decomposed the salt of gold, after long-continued ebullition; the effect was rapidly produced in water to which a small portion of peat or vegetable earth had been added, a violet or bluish colour appearing very quickly. Distilled waters which contain essential oils also decompose the chloride of gold.

It follows, then, from comparative trials, that chloride of gold may be decomposed by unaltered organic matter, but in general with greater difficulty than by that which results from the spontaneous decomposition of animal and vegetable remains, and consequently analogous to the organic matter held in solution by common or natural waters.—*Journ. de Ph. et de Ch.*, Mars 1848.

ANALYSIS OF THE ASHES OF TURNIP LEAVES.

M. Namur states that, deducting accidental admixtures, the leaves of the turnip (*Brassica Rufa*, L.) yielded 0·39 per cent. of ashes, consisting of—

Silica	6·144
Sulphuric acid	4·003
Phosphate of iron	1·332
Magnesia	7·447
Potash	29·529
Soda	2·107
Phosphoric acid	1·176
Chloride of sodium	3·251
Lime	25·510
Carbonic acid	19·501
	100·000

Journ. de Pharm. et de Ch., Janvier 1848.

NEW MINERALS—MEDJIDITE AND LIEBIGITE.

M. Laurence Smith has announced to the Academy of Sciences the discovery of two new minerals. The first, to which he has given the name *Medjidite,* is a double sulphate of lime and uranium, $U^2O^3 SO^3 + CaO SO^3, HO$; the other, which he calls *Liebigite,* consists of carbonate of lime and uranium, $U^2O^3, CO^2 + CaO CO^2 + 20 HO$.—*Comptes Rendus,* Fevrier 7, 1848.

METEOROLOGICAL OBSERVATIONS FOR MAY 1848.

Chiswick.—May 1. Fine: cloudless, with very dry air. 2. Dry haze. 3, 4. Slight fog: fine: clear. 5—7. Very fine. 8. Excessively dry air. 9, 10. Very fine. 11—13. Hot and very dry. 14. Fine. 15. Slight haze: cloudy and fine. 16. Cloudless and very fine. 17. Very fine. 18. Fine: large white clouds: thunder and hail-shower in afternoon: clear at night. 19. Cloudy: slight showers. 20. Showery. 21. Fine: slight rain. 22. Very fine. 23. Cloudless. 24. Fine. 25. Clear: cloudy: clear. 26. Foggy: fine, with slight haze: clear. 27. Overcast. 28. Very fine: slight haze. 29. Very fine: hot and dry: partially overcast at night. 30, 31. Fine.

Mean temperature of the month	*58°·12*
Mean temperature of May 1847	*56* ·83
Mean temperature of May for the last twenty years	*55* ·91
Average amount of rain in May	1·84 inch.

Boston.—May 1—8. Fine. 9. Cloudy. 10. Fine. 11. Fine: two o'clock thermometer 78°. 12. Fine. 13. Cloudy. 14. Fine. 15. Cloudy. 16. Fine. 17. Fine: rain, thunder and lightning P.M. 18, 19. Fine. 20. Cloudy. 21. Fine. 22. Cloudy. 23—26. Fine. 27. Cloudy. 28, 29. Fine. 30. Cloudy. 31. Fine: rain P.M.

Applegarth Manse, Dumfries-shire.—May 1. Slight frost A.M.: fine. 2. Fine, but cloudy. 3. Very fine. 4. Beautiful day. 5. Beautiful day: getting cloudy. 6. Beautiful day. 7. Slight rain P.M. 8. Heavy rain early. 9, 10. Fine summer days. 11. Fine summer day: overcast. 12. Fine summer day: still fair. 13. Fine summer day: fine: clear. 14. Fine summer day: slight shower: thunder. 15. Cloudy: rain P.M. 16. Cloudy A.M.: clear P.M. 17. Clear and fine. 18. Cloudy: cleared. 19. Wet A.M., but fine. 20. Cloudy A.M.: fine P.M. 21. Dull and drizzling: cleared. 22, 23. Beautiful summer day. 24. Beautiful summer day: distant thunder: shower. 25. Beautiful day. 26. Clear A.M.: shower P.M. 27. Warm and fine. 28. Rain A.M.: showery all day. 29, 30. Fair and fine. 31. Heavy rain all day.

Mean temperature of the month	*52°·9*
Mean temperature of May 1847	*51* ·1
Mean temperature of May for twenty-five years	*51* ·09
Rain in May for twenty years	1·69 inch.

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

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

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1848. May.	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.	30·139	30·067	29·80	30·02	29·98	30·13	30·12	60	36	54	60	31½	49	42	e.	calm	s.–se.	ese.	......	......	......	·05
2.	30·077	30·066	29·70	30·00	29·94	30·04	29·98	64	31	56	54	35½	51	47	e.	se.	se.	sse.	......	......	......	......
● 3.	30·101	30·074	29·69	29·92	29·95	29·90	29·96	72	33	58	63	39	50	48½	e.	e.	se.–s.	ne.	......	......	......	·19
4.	30·165	30·133	29·70	30·03	30·05	30·03	30·18	72	38	64	68	37½	59	51	e.	s.	w.	calm	......	......	......	......
5.	30·166	30·159	29·75	30·06	30·00	30·06	30·11	74	36	62·5	69	40½	58	48	e.	calm	ssw.	calm	......	......	......	......
6.	30·089	30·022	29·65	30·02	29·95	30·04	29·87	78	38	64	62½	43	50	46½	sw.	s.	w.	se.	......	......	......	......
7.	30·045	29·993	29·55	29·85	29·80	29·61	29·65	78	37	64	54½	42	52	46	sw.	sw.	s.	sw.	......	......	......	......
8.	30·112	30·036	29·54	29·79	30·00	29·71	29·95	76	36	62	58½	47½	48½	46	w.	calm	w.	wsw.	......	......	0·30	·14
9.	30·222	30·153	29·71	30·11	30·08	30·12	30·07	78	38	56	65	36	49	47	e.	calm	s.	calm	......	......	......	......
☽ 10.	30·304	30·260	29·80	30·15	30·19	30·07	30·20	76	36	64	67	38½	51	48½	e.	calm	w.	wsw.	......	......	......	......
11.	30·320	30·278	29·80	30·18	30·10	30·00	29·91	84	40	68	70	39½	64	51	s.	calm	w.	sw.	......	......	......	·09
12.	30·240	30·170	29·67	30·04	30·07	29·94	30·05	80	41	64	65	50	50	48	ne.	sw.	sw-se.	w.	......	......	......	·05
13.	30·175	30·137	29·66	30·09	30·07	30·05	30·10	81	41	73·5	73½	43½	52½	47	e.	calm	sw.	wnw.	......	......	......	·06
14.	30·126	30·091	29·65	30·10	30·05	30·21	30·13	81	48	72	70	42½	49	45	e.	ne.	e.	ese.	......	......	......	......
15.	29·996	29·781	29·54	29·83	29·53	29·56	29·37	85	43	59	60	43	52	47	s.	calm	ssw.	w.	......	......	......	......
16.	29·653	29·482	29·17	29·33	29·33	29·16	29·31	82	47	67	62	50	50	48	sw.	sw.	s.	sw.	......	......	0·08	·23
17.	29·404	29·296	28·96	29·32	29·20	29·37	29·35	79	52	67	62	50	52	48	s.	sw.	s.	se.	......	......	......	·09
○ 18.	29·680	29·382	28·90	29·18	29·32	29·23	29·26	80	39	61	53	48	52	47	sw.	sw.	sw.	w.	......	·18	......	......
19.	29·706	29·702	29·28	29·93	29·51	29·55	29·66	65	44	60	58	42	53	50	s.	s.	se.	se.	·01	......	......	·14
20.	30·160	29·783	29·43	29·79	30·05	29·87	30·06	66	37	59	62	50	49	50	nw.	calm	nnw.	calm	·06	......	......	·02
21.	30·253	30·215	29·74	30·10	30·15	30·07	30·16	69	54	63	62	38	55	50	sw.	calm	sw.-s.	wnw.	·07	......	......	......
22.	30·283	30·242	29·77	30·25	30·28	30·28	30·28	73	48	62	68	46	52	52	nw.	nw.	s.	w.	......	......	......	......
23.	30·314	30·261	29·86	30·28	30·25	30·30	30·30	72	38	62	73	45½	57	52	ne.	ne.	ne.	w.	......	......	0·10	......
24.	30·287	30·256	29·80	30·25	30·25	30·25	30·29	76	41	64	73	57	57	51	ne.	e.	s–nw.	wnw.	......	......	......	......
☾ 25.	30·266	30·158	29·75	30·22	30·10	30·26	30·18	83	44	70	72	46	51½	50	ne.	calm	se.	s.	......	......	......	......
26.	30·106	30·031	29·59	30·05	30·00	30·14	30·12	83	51	66	72½	52	53	47	nw.	calm	sw.ne.	ese.	......	......	......	·19
27.	30·130	30·101	29·65	30·01	29·99	30·09	29·99	67	34	69	70	53	54	50	e.	e.	ne.	w.	......	......	......	......
28.	30·107	30·014	29·63	29·98	29·92	30·06	30·03	77	37	61	68½	51½	51½	49	e.	calm	nw.	calm	......	......	......	·18
29.	30·009	29·963	29·50	29·87	29·90	29·94	29·94	81	53	68	60	51	55	50½	sw.	se.	nw.	sse.	......	......	......	......
30.	30·117	30·056	29·60	29·98	30·00	30·03	30·03	72	39	57·5	63½	45	53	48½	nw.	n.	sw.	w.	·14	......	1·50	......
31.	30·049	29·823	29·53	29·70	29·54	29·60	29·42	69	41	63	54½	46	48½	50	sw.	wnw.	sw.	se.	......	......	......	·35
Mean.	30·123	30·006	29·59	29·949	29·911	29·924	29·936	75·25	41·00	63·2	64·3	44·5	52·53	48·43					0·28	0·18	1·98	1·78

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[THIRD SERIES.]

AUGUST 1848.

XIV. *On Œnanthal, its Compounds, and the Products of its Decomposition.* *By* THOMAS GEORGE TILLEY, *Esq.*[*]

THE volatile liquid obtained when the oil of *Ricinus communis* is distilled, consists, as was shown by Bussy, of a substance analogous to aldehyde, bearing the same relation to œnanthylic acid which aldehyde bears to acetic acid. Bussy analysed œnanthal, and described the method employed by him in its preparation, but did not further investigate the matter. It is easily prepared in a pure state and in large quantities by simple distillation of castor-oil. The oil which passes over is mixed with a large quantity of water, and, with it, distilled again. This process separates the œnanthal from certain fatty acids not volatile with water; but it is still impure from admixture of acroleine and other substances of unknown nature. To expel the acroleine, which is exceedingly volatile, the fluid is heated in a water-bath to 100°, and a stream of carbonic acid passed through it until it has lost the well-known and intensely powerful odour of that substance. The temperature is then raised in an oil-bath to 155°, when the fluid commences to boil, and continues in ebullition until more than two-thirds are distilled. The product of this distillation is pure œnanthal, containing traces of water which it dissolves in considerable quantities, and from which it may be separated by chloride of calcium.

Thus obtained, œnanthal possessed all the properties ascribed to it by Bussy; and the analyses made by me agree with those of that chemist.

0·2570 gave carbonic acid 0·6795, and water 0·2940, giving per cent.,—

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[*] Communicated by the Author.

	Found.	Atoms.	Calculation.
Carbon .	73·81	14	73·68
Hydrogen	12·60	14	12·28
Oxygen .	13·59	2	14·04

Œnanthal boils at 155° Centigrade, but is somewhat decomposed by long ebullition, its colour becoming brown and its reaction acid, while its boiling-point rises. This takes place to a slight extent even in an atmosphere of carbonic acid gas. Bussy found the boiling-point to be 155°–158°, which difference is explained by the above observation.

ACTION OF NITRIC ACID ON ŒNANTHAL.

Nitracrol, Nitracholic Acid, Cholacrol, Œnanthylic Acid, Caproic Acid, and Oxalic Acid.

Œnanthal purified by distillation in a stream of carbonic acid, as above described, is allowed to fall drop by drop into a retort containing strong nitric acid. The action is very violent, and the oxidation quickly established. In the receiver is found a mixture of œnanthylic and caproic acids, and nitracrol, and there remain in the retort, dissolved in the strong nitric acid, œnanthylic, caproic and oxalic acids.

Nitracrol.—This substance was discovered by Redtenbacher among the products of the action of nitric acid on choloidinic acid. It seems to be frequently produced in actions of a similar nature, and to account for the yellow colour observed on neutralising by an alkali the acid products obtained. When œnanthal is distilled with nitric acid, nitracrol is found in the receiver mixed with the fatty acids and dissolved in nitric acid. When this is diluted with water, a stratum of oil separates which swims on the surface, and heavy oily drops sink to the bottom of the fluid. These heavy drops when washed constitute a colourless fluid very sparingly soluble in water, and possessing a most penetrating and powerful odour; so powerful indeed, that the mucous membrane of the nose is frequently inflamed after smelling it.

Heated to 100° with water it is decomposed, nitrous acid passing over with another body which Redtenbacher has called *cholacrol.* Mixed with caustic potash, it assumes immediately a yellow colour, and in a short time crystals appear, while an oil heavier than water remains undissolved. These crystals are beyond all doubt the *nitrocholate of potash* obtained by Redtenbacher, and found by him to have the following formula:—

$$C_2 H N_4 O_9 + KO.$$

Cholacrol.—This is a product of the decomposition of nitracrol by alkalies, and remains in the form of a heavy oil when the nitracrol is mixed with caustic potash. It is a clear, somewhat yellow, heavy fluid, with a smell, when concentrated, scarcely less intense than that of nitracrol; but when diluted, possessing an odour like cinnamon.

Its formula was found by Redtenbacher to be—

$$C_8 H_5 N_2 O_{13}.$$

Too small quantities of these substances were obtained by me to permit of my making analyses; but their properties are so marked, and their smell so characteristic, that no doubt remains in my mind as to their nature.

Œnanthylic Acid.—The oily acids which separate on dilution of the nitric acid used for oxidation with water, are a mixture of œnanthylic and caproic acids. They are purified by distillation with water, and neutralized with baryta. On neutralization the liquid becomes yellow, owing to the presence of the nitrocholate of the base used for saturation. The baryta salts are purified by repeated crystallizations in water, and then dissolved in that fluid, and evaporated until a slight film appears on the surface; they are then allowed to crystallize spontaneously. The œnanthylate of baryta appears in the form of lancet-shaped plates, sometimes thick and hard, and at others in groups of silky vegetations. As long as crystals of this character continue to be deposited, the liquid is allowed to evaporate; and as soon as small clusters appear, is decanted.

The œnanthylate of baryta is purified by several crystallizations; and thus obtained, the crystals were analysed, and found to have the composition assigned to this salt by me in a former memoir, and since authenticated by the analyses of Redtenbacher, Bussy and Williamson.

I. 0·2950 salt gave 0·145 carbonate of baryta.

II. 0·2255 salt gave 0·112 carbonate of baryta.

III. 0·4430 salt gave, when burned with chromate of lead, 0·6910 carbonic acid and 0·2680 water.

Therefore—

	Found.		Atoms.	Calculation.
Carbon . .	42·52		14	42·98
Hydrogen .	6·72		13	6·51
Oxygen . .	12·26		3	11·76
Baryta . .	38·50	38·23	1	38·75

It is therefore pure œnanthylate of baryta*.

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* *Œnanthylate of Silver.*—In a former publication on œnanthylic acid, it was stated that œnanthylate of silver, subjected to distillation, gave as products an oil and a crystalline substance. This statement must have been

Caproic Acid.—When œnanthylate of baryta is no longer deposited from the solution of the mixed salts, the clusters of prismatic crystals which appear are separated and purified by crystallization. They were analysed.

0·2790 salt gave 0·1500 carbonate of baryta.

0·4110 salt gave 0·5855 carbonic acid and 0·2180 water. Therefore,—

	Found.	Atoms.	Calculation.
Carbon . .	38·83	12	39·22
Hydrogen .	5·89	11	5·99
Oxygen . .	13·48	3	13·08
Baryta . . .	41·82	1	41·80

It is evident, then, that these crystals are caproate of baryta.

With regard to the formation of caproic acid in this oxidation, I at first thought that '*capronal*' was contained among the products of the distillation of castor-oil. I looked for it in vain among the products boiling at about 135° Cent.; and many analyses made only led to the result, that the more volatile portions of the oil were rendered so by admixtures of acroleine, and probably by the bicarburet of hydrogen of Faraday. The formation of one atom of oxalic acid and one atom of water explains the presence of caproic acid among the products of the oxidation of œnanthal:

$$C_{14}H_{14}O_2+7O-(C_2O_4H+HO)=C_{12}H_{12}O_4,$$

one atom of caproic acid.

Oxalic acid is found in the retort dissolved in the nitric acid liquid.

Action of melted Potash on Œnanthal.

Œnanthylic Acid.—Œnanthal is allowed to fall from a pipette into a retort containing fused hydrate of potash. Hydrogen gas is evolved, and œnanthylate of potash is formed. This is decomposed by an acid, the oily acid neutralized with baryta, and the salt formed is œnanthylate of baryta.

0·281 salt gave 0·1395 carbonate of baryta, which is equivalent to 38·57 per cent. of baryta, theory requiring 38·75.

Action of Caustic Potash on Œnanthal in the cold.

Hydruret of Œnanthyle.—It was observed by Bussy and Williamson, that œnanthal, when mixed with a strong solution of caustic potash, underwent a peculiar change; but they came to no results which explain the nature of this change.

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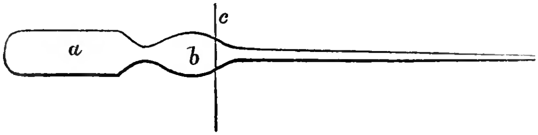
the result of error; for on carefully making a new trial, the products of the process were œnanthylic acid and a small quantity of a brown tarry matter.

One part of œnanthal is poured on five or six parts of hydrate of potash, and the mixture allowed to stand in a closed vessel. Heat up to 120° Cent. is produced, and the fluid becomes thick. After twenty-four hours a large quantity of water is added, and all that is soluble in that fluid removed. The oil which remains is mixed with a fresh quantity of water, and with it distilled: it passes over exceedingly slowly with the watery vapour; and only after several days, and the evaporation of large quantities of water, is the distillation completed. The oil which passes over with the water is colourless, fluid, and smells slightly like lemons. Its boiling-point is 220°, at which temperature, however, it is quickly decomposed, turning brown and smelling differently. It is slightly soluble in water, and dissolves small quantities of that liquid; it burns with a clear flame, giving little smoke. It is soluble in alcohol and æther in all proportions.

To determine its composition, several precautions are necessary to ensure its perfect conversion into carbonic acid and water. In the ordinary manner, with oxide of copper no results which agree with each other can be obtained; and only by means of free oxygen, and the simultaneous use of chlorate of potash in the bulb, can the whole of the carbon be obtained as carbonic acid. The same specimen analysed gave in three separate experiments incompatible results.

	I.	II.	III.
Carbon . .	77·69	77·09	78·37
Hydrogen .	12·69	12·71	13·11

numbers which are too diverse to permit of their use in calculating a formula. Traces of carbon are also found in the bulbs; and to obviate this occurrence, the substance is introduced into the combustion-tube in a double bulb having the annexed form.



Into *a* is introduced a small piece of melted chlorate of potash, and the bulb *b* being warmed, the air is driven out without heating the salt. When introduced into the combustion-tube, the bulb is filed at *c*, and thus the fluid allowed to escape into the oxide of copper before the combustion is commenced. Towards the end of the analysis a stream of dry oxygen is passed through the apparatus. By these means alone can results be obtained which agree with each other. The following analyses were made with different specimens of the substance.

I. 0·3340 substance gave 0·9750 carbonic acid and 0·3935 water.

II. 0·0265 substance gave 0·7720 carbonic acid and 0·3280 water.

	I.	II.	Atoms.	Sum.	Calculation.
Carbon .	79·28	79·43	14	1050	79·24
Hydrogen	13·34	13·38	14	175	13·21
Oxygen .	7·38	7·19	1	100	7·55
	100·00	100·00		1325	100·00

The formula for this new body is then $C_{14} H_{14} O$; and the nature of the change is explained by supposing three atoms of œnanthal decomposed by potash into one atom of œnanthylic acid and two atoms of the new body; as—

$$\begin{array}{rl} \text{3 atoms of œnanthal} \quad . \quad . & = C_{42} H_{42} O_6 \\ -\text{1 atom œnanthylic hydrate} & = \underline{C_{14} H_{14} O_4} \\ \text{2 atoms of the new substance} & = C_{28} H_{28} O_2 \end{array}$$

On examining the alkaline liquid washed from the mixture of potash with œnanthal, as described above, it will be found to contain œnanthylate of that base; and on the addition of one of the strong acids, œnanthylic acid is evolved, and in such proportion as would be expected from the above change in œnanthal, the œnanthylic acid amounting to about one-third of the œnanthal employed.

This is a parallel change to that which takes place when moist chlorine gas is passed through the oil of bitter almonds, and the compound $BzOHO + 2BzH$, or the benzoate of the hydruret of benzoyle formed. In the present case, however, the body formed is decomposed by the greater attraction of the potash for the œnanthylic acid, and the compound above described, which I shall now call hydruret of œnanthyle, $C_{14} H_{13} O + H$, is set free. Or œnanthal may be considered as a compound of œnanthylic acid and hydruret of œnanthyle.

When I had almost finished the examination of this substance, a memoir by Mr. Williamson (*Ann. der Chemie*, vol. lxi. p. 38) on the same subject fell into my hands. There is sufficient similarity between the results found by that gentleman and the numbers here published. The deficiency in carbon and the irregularity of the substance produced, arise partly from the difficulty of combustion of, and partly from the presence of potash in, the substance analysed by him. For it is almost impossible to separate entirely that body from the oil by washing; nay, even after many days' boiling with water, the oil still contained traces of potash.

The formation of the œnanthylate of potash was, however, also observed by Mr. Williamson.

Hydruret of Œnanthyle with oxidizing agents.

To render more certain the view as to the true nature of the hydruret of œnanthyle, it was oxidized in two several ways; and, as might have been supposed from the analogy of the hydruret of benzoyle, œnanthylic acid was the product of the operation. Mixed drop by drop with strong nitric acid, hydruret of œnanthyle is dissolved and oxidation proceeds rapidly, though more slowly than in the case of œnanthal; but œnanthylic acid is the sole product of the action.

Heated with potash, hydruret of œnanthyle is decomposed with the evolution of hydrogen, and the production of œnanthylic acid and a tarry substance. The action however proceeds slowly and with difficulty.

The acid formed in both cases was neutralized by baryta; and the properties of the salt formed agreed entirely with those of œnanthylate of baryta.

0·223 of the salt of the acid formed by oxidation with nitric acid gave 0·1105 carbonate of baryta; and it therefore contains 38·50 per cent. baryta, theory requiring 38·75.

The compounds of œnanthyle may probably be considered as thus constituted:—

Œnanthyle	$C_{14}H_{13}O$
Hydruret of œnanthyle .	$C_{14}H_{13}O+H$
Oxide of œnanthyle . .	$C_{14}H_{13}O+O$ (or anhydrous œnanthylous acid, that of Liebig and Pelouze).
Œnanthal	$C_{14}H_{13}O+HO$
Hydrated œnanthylic acid	$C_{14}H_{13}O+O_2+HO$

Or the nomenclature may be adopted until our views on this subject are rendered more clear by the addition of new facts.

Action of Ammonia on Œnanthal.

Œnanthylammon.—When ammoniacal gas is passed into œnanthal, as was observed by Bussy, it is absorbed in large quantities, heat is developed, and the liquid becomes thick; by the continued action of the gas it however resumes its former degree of fluidity.

Bisulphate of Œnanthylammon.—This substance has the composition of taurine, with the elements of œnanthal substituted for those of aldehyde. When œnanthylammon is dissolved in strong spirits of wine, and a stream of sulphurous acid passed through the solution, a crystalline powder falls in considerable quantities. If the alcoholic solution be not too concentrated, the salt is deposited more slowly and in a more decidedly crystalline form. These crystals are small prisms, which glitter in the sun and are brilliantly white. They are slightly soluble in alcohol and water, but more so in the former than in the latter; and may partly be obtained again by evaporation, but they are in a great measure decomposed. When mixed with water in the cold they are slowly decomposed, but when heated in this fluid the decomposition takes place more rapidly, œnanthal being evolved and the fluid containing bisulphite of ammonia. In the presence of strong acids decomposition proceeds still more quickly. Heated on a plate of platinum this salt melts, white fumes are given off, it blackens, and lastly burns with a flame, the vapours smelling of œnanthal.

To estimate the sulphur, the bisulphite of œnanthylammon was warmed in a large platinum vessel with strong caustic potash free from sulphate, the water evaporated, and the mass ignited with pure nitrate of potash. This method is attended with no difficulties, provided the platinum vessel be sufficiently large to prevent loss by particles being scattered on the application of heat. The nitrogen was determined as the double chloride of platinum and ammonia.

0·2770 substance gave 0·340 sulphate of baryta
0·2210 substance gave 0·2400 chlor. pt. amm.,

which numbers lead to the following rational formula :—

	Found.	Atoms.		
Carbon . .		14	=1050·0	43·08
Hydrogen .		17	212·5	8·7
Oxygen .		6	600·0	24·6
Nitrogen .	6·8	1	175·0	7·1
Sulphur .	16·8	2	400·0	16·4
			2437·5	

Or its rational formula is—

$$C_{14}H_{13}O, SO_2 + SO_2 NH_4 O;$$

and it may be considered as a double salt of the sulphite of œnanthal, and the sulphite of the oxide of ammonium.

The analogy between this substance and taurine is interesting; for the substance described in this section is the body isomeric with taurine found by Redtenbacher,

$$C_4 H_3 O SO_2 + SO_2 NH_4 O,$$

with the elements of œnanthal substituted for those of aldehyde.

I did not think it necessary to determine the carbon and hydrogen in this compound, as the sulphur calculated as sulphurous acid, and the nitrogen as the oxide of ammonium, immediately determine the nature of the substance formed, and the near approach of the quantities of these elements found by experiment to the numbers required by theory, leave no doubt as to the correctness of the above formula.

I was in the act of making further investigations on this subject, and had prepared compounds of a most interesting nature, by the action of sulphuretted hydrogen and sulphuret of carbon on œnanthylammon, when the town in which I was residing became the prey of one of those political convulsions which mark the present æra. The laboratory in which I worked was stormed, being in an exposed position, and everything it contained destroyed amidst the uproar. Instruments and the substances under investigation were alike scattered in fragments around,—only with difficulty I myself escaped. Such must be my excuse for the imperfectness of the present paper, but as soon as possible I purpose continuing this investigation.

XV. *Fifth Memoir on Induction.* *By* M. Elie Wartmann, *Professor of Natural Philosophy in the Academy of Geneva**.

[With a Plate.]

[Continued from vol. xxxi. p. 251.]

§ XVII. *On the non-propagation by radiation of Dynamic Electricity.*

146. RADIATION and conductibility are the only two modes known by which imponderable fluids are propagated. The first, possessed in common by heat and light, has furnished theorists with more than one occasion of discussing the relative value of the systems of emission and undulation. Conductibility is a second form of propagation peculiar to heat, and which light does not present. What is the true character of transmission of electricity? This is an important question, the solution of which requires direct experiments.

147. Analogy furnishes no data which can inspire any confidence. It is generally agreed that the imponderable agents form two distinct groups. Light and radiant caloric are

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* Communicated by the Author, having been read before the Société de Physique et d'Histoire Naturelle of Geneva, March 2, 1848.

governed by such similar laws, that M. Melloni* and other philosophers admit the identity of their nature. The same has been the case, since Ampère, with electricity and magnetism, the analogous properties of which are comprised in phænomena of attraction, repulsion, and induction. The only manifestations common to the four fluids are their imponderability, their property of mutually engendering themselves in matter, and of reacting in various cases upon one another, their incapacity of passing freely through certain bodies, (opake, athermanous, isolating); lastly, their extreme velocity of propagation. But these resemblances do not admit of our deciding whether electricity is transmitted by *radiation*, as might be conjectured from its more rapid motion than that of light, or by *conductibility*, in the manner of the caloric of contact, as has been supposed by a sort of universal convention, to which scientific language gives faith. The expressions of *conducting* bodies and *isolating* bodies have only acquired a theoretical importance from the remarkable works of Ampère on the propagation of electricity†, and of Mr. Faraday on the induction of contiguous particles‡.

148. In his *Recherches sur quelques points de l'Electricité voltaïque*, M. Vorsselman de Heer has given his opinion that the velocity of the current depends only on the *matter* of the conductor, just as the rapidity of sound is modified only by the *nature* of the medium in which it is transmitted§. If this was the case, the proposed hypotheses on the mode of propagation and on the nature of the electric fluid would be very much simplified. But the phænomenon is probably more complicated; and M. Poggendorff has recently shown that the rapidity of transmission of the current of the battery seems to be proportioned to the product of the conductibility of the medium by its section‖.

149. The object of this memoir is to establish the fact experimentally, that rectilinear propagation, a fundamental condition of all radiation, is not verified for dynamic electricity, which consequently does not possess the property of being

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* On the Identity of the various luminous, calorific and chemical radiations, vibrated by the Sun and the terrestrial sources. *Bibl. Univ.*, vol. xxxix. p. 168.

† On the mode of transmission of Electric currents, and on the Electro-chemical theory. This memoir, which had remained inedited, has been published in the Scientific Review of Quesneville, vol. xxxi. p. 171, Nov. 1847.

‡ Experimental Researches in Electricity, §§ 18 and 19, Phil. Trans. 1838.

§ *Bulletin des Sciences Physiques et Naturelles en Néerlande*, 1839, vol. i. p. 319.

‖ *Annalen der Physik und Chemie* (1848, No. 3), vol. lxxiii. p. 355.

reflected, refracted and polarized. The experiments which I proceed to describe were made at the invitation and with the assistance of my friend M. Ch. Cellérier. They are in a manner well-timed from the recent publication of Prof. Maas*.

150. The apparatus employed is represented in plan in Plate I. fig. 1, and a section of it in fig. 2. AB is a glass tube one metre in length, perforated by a small internal canal, and arranged horizontally. One end terminates at the bottom of a vertical chamber C, of the same substance, and the other at the centre of a cylindrical earthen vessel DD′, into which it penetrates by an aperture which is closed by means of a stopper *a*. The vessel is divided into two equal chambers, isolated by the plate of glass *bb*, cemented in a perpendicular position to the axis of the tube. All the sides of these two chambers were covered with several layers of varnish of gum-lac. On the middle of the plate was fixed a piece figured in detail in fig. 3. It is composed of a large copper cylinder, the foot of which is slit at *e*. The excentricity of this slit allowed of the vertical axis, to which it is parallel, being made to coincide exactly with the mouthpiece *v* of the tube AB. Higher up is seen a diametral aperture *f*, into which is fixed, by the screw *g*, the extremity of the wire *αα*. Lastly, the upper part of the cylinder *dd* is pierced in the direction of the axis for the length of $0^{m}\cdot035$, with a slightly conical aperture, in which the piece *h* turns with gentle friction. This piece is furnished with a support which regulates its insertion. At its extremity, this piece has a square hole which receives the stem *i* held tight by the screw *k*. The arm *i* is itself pierced so as to receive the copper wire *l*, the serviceable length of which is regulated by the screw *n*, and the lower part of which, isolated in a glass tube *m*, is exposed only at the base of the latter.

151. The experiment is made with a voltaic element P, the nature of which varies according to the sensibility of the rheometer G. The first experiments were performed with a pair formed of two square plates, copper and zinc, of $0^{m}\cdot035$ thickness; the others with a copper and an iron wire immersed in pure water. The polar extremities terminate in the mercury contained in the glasses *p* and *q*.

152. These glasses communicate with two other similar cups E, F, by means of the metallic branches *r* and *s*, in fig. 4. The first, *r*, is a large copper wire curved twice, and the horizontal portion of which is inclosed in a glass tube; the other, *s*, is a wire of the same diameter with three bends: its central

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* *Considérations sur le movement de la Dynamie Electrique: Bulletins de l'Académie Royale de Bruxelles*, July 10, 1847. *L'Institut* of Jan. 5, 1848.

portion is isolated in a coating of wax. These two pieces have their extremities amalgamated: they constitute a very simple rheotrope, which has proved very useful to me in a great number of researches*.

153. The reservoir C, the tube AB, and the two compartments D, D′ are filled with purified mercury up to the general level 0 0′ 0″ 0‴. C is then connected with the cup E by the metallic wire δ. The current then follows the mercury of the channel AB. To conduct it back from D into F, two ways are presented, the resistances of which are equal, or nearly so. One is the wire α, which terminates at the aperture *f* of the piece represented in figs. 2 and 3; an excellent Gourgon's multiplicating rheometer G, the needles of which only make a simple oscillation in twenty-six seconds, was interposed in its development. The other way is furnished by the wire β of determined dimensions, and which is immersed at *z* in the mercury of the chamber D: it communicates with this liquid only by its lower point, and is isolated for a sufficient length, by means of a covering of glass. This arrangement is analogous to those which I have described in employing the method of derivations (74 to 79.).

154. The principle on which this mode of experimenting is founded is easily understood. Let us suppose that the wires α and β exert an equal resistance, and that the second terminating in *z*, at a distance B*z* from the extremity of the channel AB, any suitable lengthening of the stem *i* causes the first to terminate in *z*′, the length B*z*′ being equal and symmetrical to B*z*. Each of them will then afford a passage for a current of the same intensity, and that intensity will be half that which flowed off by the channel AB. Now the pair P being of small dimensions and feebly excited, whilst the wires α and β are thick and good conductors, any one of the latter, taken singly, would suffice for the discharge. If the electricity is propagated by radiation, the portions of the total current transmitted by each of the circuits will vary with the respective positions of the points of contact of the wires with the mercury. The extremity of β being constantly immersed in *z*, let us bring that of α into *z*″. Thus placed in the prolongation of the axis of the tube, and in the course of the electric rays which issue from it in a *parallel bundle*, this wire α will be traversed by a stronger current, and the rheometric deviation will increase. The quantity of the fluid which will run off by the wire β will be proportionately diminished.

155. This inequality in the distribution of the parts of the

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* *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève*, vol. ix. p. 119. *Archives de l'Electricité*, vol. i. p. 74.

total current between the two circuits may be increased by shortening the arm $k\pi$, and thus approaching the point z'' to the opening v. We have even inserted the end of the wire α $0^{m}\cdot025$ into the channel AB, the diameter of which was larger, so that there still remained a thin ring of mercury around the wire.

156. The position of the extremity of the wire β may be changed at will, and all possible arcs be described around the point v, by the terminal surface of the wire α, giving any value to the ray of rotation $k\pi$: the deviation of the rheometer remains absolutely *constant.* This is due to the great conducting property of the mercury; for if a saturated solution of sulphate of copper is substituted for it, differences are found in the position of the index. But these differences are produced by the variations of resistance of the filaments of liquid interposed between the aperture of the tube AB and the extremities of the circuits.

157. The same constancy in the rheometric indication continues when the depths of immersion of the wires α and β are modified in a successive or simultaneous manner.

158. The result of these experiments cannot be attributed to a phænomenon of internal reflexions. Admitting a perfect cylindricity of the vessel DD′, all the electric radii would have been brought, by lateral reflexions, toward the aperture v of the tube AB which occupies its centre. Although the vessel does not present exactly this geometric form, it differs so little from it that we may admit that a very small number of reflected rays reached the wire β immersed in z at $0^{m}\cdot13$ of the aperture.

159. I also wished to demonstrate, that the relative distribution of electricity between the two wires α and β is not due to a total reflexion of the fluid on the surface of the mercury; and with this view I constructed the apparatus represented in fig. 5. A large copper wire *ttt* is curved so that its extremities become parallel and not far apart: they are moreover encased in glass tubes *uu*, which leave only the terminal surfaces of the wire exposed. The latter is fixed between two pieces of wood H, one of which is pressed against the other by means of a clamp x. A slit y permits of establishing this forked system on the glass plate *bb*, and extending it over the entire length right and left of the cylinder *d*. The extremity of the wire β is then introduced into the compartment D′. Experience proves that the deviation of the rheometer is in no degree affected by the diversities of position and immersion of the wires β and t, whether they are or are not accompanied by analogous changes in the place and depth of immersion of the other wire l, the extremity of α.

160. The tube AB was arranged in the centre of another, upon which was wound four metres of a copper wire covered with silk. In this wire the current of ten powerful Grove's elements was excited (139.), and rendered intermittent by means of the commutator described (122.). The rheometer was sheltered from the electro-magnetic influence of this long bobbin. The induction produced in the mercury contained in AB in no degree changed the constancy of indication of the instrument.

161. The same result was obtained by inserting into the inductor cylinder the portion of the wire *a* which joins the rheometer with the cup F. These two experiments confirm the conclusions detailed in § XIII.

162. Care was taken to alternate the direction of the current of the battery P in each of the preceding experiments, by placing the pieces *r* and *s* one while parallel, at another crossways; that is to say, by putting the cup *p* in communication with E, then with F, and joining the cup *q* with F, then with E. These changes in the direction of the current caused in it corresponding ones in the direction of the deviation of the needle of the rheometer, but did not alter the magnitude of that deviation.

163. Thus the fundamental fact is established, that electricity in the form of a current is not transmitted by rectilinear radiation. Does its propagation result from section to section in the bodies called conductors, as is admitted in the case of caloric by contact? This daily appears more probable, notwithstanding the extreme velocity with which it is effected. I propose very soon to examine the electricity of tension in the same point of view.

164. I may observe, that the conclusions of the present investigation confirm those of my second memoir. It may have been objected, that if electricity is propagated by undulations, comparable from their extreme minuteness to those which constitute light, none of the three methods which I have employed is sufficiently precise to testify for or against the interference. I shall not appeal to the probability that this latter would be shown, at least once, in the hundreds of repetitions which have been made of each experiment: it is sufficient for me to observe, that as rectilinear propagation does not exist, *interference is impossible.*

XVI. *On a particular Transformation of the Differential Equations of Motion in the Theory of Planetary Perturbation. By the* Rev. BRICE BRONWIN*.

THE transformation employed in this paper leads to results of the same form as Hansen's theory, with the exception that logarithms are not introduced. Its object is to dispense with his constant time. The known equations of motion, referred to the plane of the orbit, are

$$\frac{d^2r}{dt^2} - r\frac{dv^2}{dt^2} + \frac{\mu}{r^2} + \frac{d\mathrm{R}}{dr} = 0, \quad r^2\frac{dv}{dt} = k - \int\frac{d\mathrm{R}}{dv}\,dt. \quad . \quad (a.)$$

To transform these, suppose ρ and u determined by the equations

$$\frac{1}{\rho} = \frac{\mu}{h^2}\{1 + e\cos(u - \varpi\}, \quad \rho^2\frac{du}{d\tau} = h = \sqrt{\mu a(1-e^2)};$$

where a, e, and ϖ are constant, and τ is a function of t to be determined.

Now make $r = \rho\beta$, $v = mu$, and $k = mh$. Then

$$\frac{dv}{dt} = m\frac{du}{dt} = m\frac{du}{d\tau}\frac{d\tau}{dt} = \frac{mh}{\rho^2}\frac{d\tau}{dt};$$

and the second of (a.) becomes

$$\beta^2\frac{d\tau}{dt} = 1 - \frac{1}{mh}\int\frac{d\mathrm{R}}{dv}\,dt. \quad . \quad . \quad . \quad . \quad (1.)$$

Also

$$\frac{d^2r}{dt^2} = \beta\frac{d^2\rho}{dt^2} + 2\frac{d\rho}{dt}\frac{d\beta}{dt} + \rho\frac{d^2\beta}{dt^2} = \beta\frac{d^2\rho}{d\tau^2}\frac{d\tau^2}{dt^2} + \beta\frac{d\rho}{d\tau}\frac{d^2\tau}{dt^2}$$

$$+ 2\frac{d\rho}{d\tau}\frac{d\beta}{dt}\frac{d\tau}{dt} + \rho\frac{d^2\beta}{dt^2} = \beta\frac{d^2\rho}{d\tau^2}\frac{d\tau^2}{dt^2} + \frac{1}{\beta}\frac{d\rho}{d\tau}\frac{d}{dt}\left(\beta^2\frac{d\tau}{dt}\right)$$

$$+ \rho\frac{d^2\beta}{dt^2} = \beta\frac{d^2\rho}{d\tau^2}\frac{d\tau^2}{dt^2} + \rho\frac{d^2\beta}{dt^2} - \frac{1}{mh\beta}\frac{d\rho}{d\tau}\frac{d\mathrm{R}}{dv}.$$

If $m^2 = 1 + \epsilon$, we have

$$r\frac{dv^2}{dt^2} = m^2h^2\frac{\beta}{\rho^3}\frac{d\tau^2}{dt^2} = h^2\frac{\beta}{\rho^3}\frac{d\tau^2}{dt^2} + \epsilon h^2\frac{\beta}{\rho^3}\frac{d\tau^2}{dt^2};$$

and

$$\frac{\mu}{r^2} = \frac{\mu}{\rho^2\beta^2} = \beta\frac{\mu}{\rho^2}\frac{d\tau^2}{dt^2} + \frac{\mu}{\rho^2}\left(\frac{1}{\beta^2} - \beta\frac{d\tau^2}{dt^2}\right).$$

Substituting these values in the first of (a.), remembering that

$$\frac{d^2\rho}{d\tau^2} - \frac{h^2}{\rho^3} + \frac{\mu}{\rho^2} = 0,$$

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* Communicated by the Author.

we find

$$\rho\frac{d^2\beta}{dt^2}-\frac{1}{mh\beta}\frac{d\rho}{d\tau}\frac{d\text{R}}{dv}-ϐh^2\frac{\beta}{\rho^3}\frac{d\tau^2}{dt^2}+\frac{\mu}{\rho^2}\left(\frac{1}{\beta^2}-\beta\frac{d\tau^2}{dt^2}\right)+\frac{d\text{R}}{dr}=0. \quad (2.)$$

Make $\beta=1+p$, $\tau=t+z$. In substituting these values in (1.) and (2.), we will, for simplicity, neglect the second and higher powers of the disturbing force. And thus, observing that $ϐ$, p, and z are of the order of that force, we shall find

$$\frac{dz}{dt}=-2p-\frac{1}{mh}\int\frac{d\text{R}}{du}\,dt, \quad . \quad . \quad . \quad . \quad (3.)$$

and

$$\rho\frac{d^2p}{dt^2}-\frac{\mu}{\rho^2}\left(3p+2\frac{dz}{dt}\right)-\frac{ϐh^2}{\rho^3}-\frac{1}{m^2h}\frac{d\rho}{d\tau}\frac{d\text{R}}{du}+\frac{d\text{R}}{dr}=0.$$

Or

$$\left.\begin{aligned}&\frac{d^2p}{dt^2}+\frac{\mu}{\rho^3}p-\frac{ϐh^2}{\rho^4}+\frac{1}{\rho}\frac{d\text{R}}{dr}-\frac{1}{m^2h\rho}\frac{d\rho}{d\tau}\frac{d\text{R}}{du}\\&\quad+\frac{2\mu}{m^2h\rho^3}\int\frac{d\text{R}}{du}\,dt=0\,;\end{aligned}\right\} \quad . \quad (4.)$$

which will give z and p in terms of t.

In estimating the effect of the higher powers of the disturbing force, we must take account of the powers of p and z which have been neglected. And in expanding R, we must make $r=\rho(1+p)$, $v=mu$, and substitute for ρ and u their known values in functions of τ. But for the first power of the disturbing force, we must put t for τ, and make $p=0$. And for this first power, we may change $\frac{d\text{R}}{du}$ into $\frac{d\text{R}}{d\varepsilon}$, operating on ε, the epocha, only where it stands alone without ϖ. And we may also change $\frac{d\text{R}}{dr}$ into $\frac{a}{r}\,\frac{d\text{R}}{da}$, or into $\frac{1}{\rho}\,\frac{d\text{R}}{dp}$, afterwards making $p=0$. Then for the higher powers of the disturbing force, we shall only have to change t into $t+z$ by Taylor's theorem, and to put for p and z their approximate values as they arise.

We might have made $v=u+\alpha nt$ instead of $v=mu$, and have transformed (a.) in exactly the same manner; and this is Hansen's method. In the development, we shall easily perceive how to determine the constants $ϐ$ and m, or α. These would give the progression of the apse.

By the common method r and v are found in terms of t from the equations

$$\frac{d\delta v}{dt}=-\frac{2h}{r^3}\,\delta r-\frac{1}{r^2}\int\frac{d\text{R}}{dv}\,dt \quad . \quad . \quad . \quad . \quad (5.)$$

$$\frac{d^2(r\delta r)}{dt^2} + \frac{\mu}{r^3}(r\delta r) + r\frac{d\mathrm{R}}{dr} + 2\int d\mathrm{R} = 0, \quad . \quad (6.)$$

neglecting the powers of the disturbing force above the first, δr and δv being the variations in r and v produced by that force; but instead of (5.) another is sometimes employed.

To compare these with the equations before found, we may observe that (3.) is more simple than (5.), but (4.) is considerably more complex than (6). The higher powers of the disturbing force are more easy to develope (by the method suggested) in the former set, and are attended with some difficulty in the latter, on account of the $d\mathrm{R}$ in $2\int d\mathrm{R}$ relating only to the co-ordinates of the disturbed body. Upon the whole, therefore, perhaps (3.) and (4.) may be quite as easily solved as (5.) and (6).

I shall not enter upon the determination of the latitude and reduction, as these must be found by some of the ordinary methods.

Reply to Mr. Boole's *paper in the last Number of this Journal.*

Mr. Boole says that the example which I adduced by way of illustration is not analogous to his. I know it. It was given as one instance in which the supernumerary arbitraries must be determined by substitution. It appeared to me that he did not consider it necessary to retain them in any case. I might form this opinion on insufficient grounds, but I do not yet see how I could come to any other conclusion. In the Number of this Journal for January 1847, and in the seventh Number of the Cambridge Mathematical Journal, he made assertions which implied that the supernumerary arbitraries might be rejected. In neither case did he allege the peculiar nature of the example or process, or say that his assertions might be proved. And moreover these examples are not analogous. If in certain cases it can be proved that the arbitraries in question will go out, of course in those cases they may be at once rejected. And now he has only given the proof in one particular case or interpretation of his equation; and the completeness of the solution is only proved in this particular case. I pass over the various other points contained in his paper, believing that I may safely leave them to the consideration of the candid reader.

Gunthwaite Hall, June 15, 1848.

XVII. *Additional Analytical Considerations respecting the Velocity of Sound. By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

I PROPOSE in this communication to resume the inquiry respecting the mathematical determination of the velocity of sound, at the point where it was left (Phil. Mag. Supplementary number, p. 498) in my reply to the remarks made by the Astronomer Royal on my communication in the Number of the Philosophical Magazine for April.

After proving (Phil. Mag. for April, p. 277) that the expression $udx+vdy+wdz$ must be made integrable in a general manner, it was shown (Supplement, pp. 496 and 497) that on satisfying this condition by supposing the velocity to be a function of the distance either from a plane or from a centre, contradictory results were arrived at, which necessarily led to the inference that neither plane-waves nor spherical waves are physically possible. A third supposition, according to which the above expression is the differential of the product of a function (φ) of z and t, and a function (f) of x and y, involved no contradiction, the reasoning being confined to the first order of approximation. To complete the consideration of this third case, which for the sake of distinction may be called that of *non-divergent* waves, it will be necessary to inquire whether any contradiction is met with when the reasoning is conducted by exact equations, which I now proceed to do.

The reasoning by which it was concluded that $udx+vdy+wdz$ must be made integrable in a general manner, went on the supposition of small vibratory motions, and quantities of a higher order than the first were neglected. I have elsewhere urged (Camb. Phil. Trans., vol. viii. part 1, p. 35; and Phil. Mag. S. 3. vol. xxi. p. 426) that in this process of reasoning the equations

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

are approximate, whereas the inference of integrability requires that they should be identical equations. With reference to this point, Mr. Stokes, in his Report on Hydrodynamics (Report of the British Association for 1846, p. 2), has correctly remarked, that the reasoning proves the quantity in question to be *approximately* an exact differential, which implies that there exists an expression $u_1dx+v_1dy+w_1dz$ which is accurately an exact differential, u_1, v_1, w_1 differing from u, v, w

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[*] Communicated by the Author.

respectively by quantities of a higher order than the first. As I am about to treat of small vibratory motion by making use of exact equations, I shall assume, in accordance with the principle stated by Mr. Stokes, that $udx+vdy+wdz$ is accurately an exact differential. Let, therefore, $udx+vdy+wdz = (d.\varphi f)$, φ being a function of z and t only, and f a function of x and y only. This being premised, we may proceed to employ the exact equation (n) obtained by Lagrange in the *Mécanique Analytique* (part 2. section 12. art. 8). By substituting φf for φ that equation becomes,

$$\left.\begin{aligned} 0=a^2.\left\{\varphi\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}\right)+f\frac{d^2\varphi}{dz^2}\right\}-f\frac{d^2\varphi}{dt^2} \\ -2\varphi\frac{d\varphi}{dt}\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right)-2f^2\frac{d\varphi}{dz}\cdot\frac{d^2\varphi}{dzdt} \\ -\varphi^3\left(\frac{df^2}{dx^2}\cdot\frac{d^2f}{dx^2}+\frac{df^2}{dy^2}\cdot\frac{d^2f}{dy^2}\right)-f^3\frac{d\varphi^2}{dz^2}\cdot\frac{d^2\varphi}{dz^2} \\ -2\varphi^3\frac{df}{dx}\cdot\frac{df}{dy}\cdot\frac{d^2f}{dxdy}-2\varphi f\frac{d\varphi^2}{dz^2}\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right) \end{aligned}\right\}. \quad \text{(A.)}$$

The motion being by hypothesis vibratory, it may be assumed that the function f has a maximum value, which, since f is numerical, may be taken for the unit of value. Let therefore the values of x and y given by the equations $\frac{df}{dx}=0$ and $\frac{df}{dy}=0$ make $f=1$, and at the same time make

$$\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}=-\frac{b^2}{a^2},$$

the negative sign being a consequence of the supposition of a maximum. On account of the independence of the variables in f and φ, we obtain by substituting these values in the equation (A.), the following equation for determining φ:—

$$0=-b^2\varphi+a^2\frac{d^2\varphi}{dz^2}-\frac{d^2\varphi}{dt^2}-2\frac{d\varphi}{dz}\frac{d^2\varphi}{dzdt}-\frac{d\varphi^2}{dz^2}\cdot\frac{d^2\varphi}{dz^2}. \;. \quad \text{(B.)}$$

Again, to obtain the equation which determines f, let us suppose, on account of the independence of the variables in φ and f, that $\frac{d\varphi}{dz}=0$; and let the value of z given by this equation be substituted in φ, which will thus become φ_0, a function of t or a constant. Now substituting these values in (A.), and observing that equation (B.) gives

$$0=-b^2\varphi_0+a^2\frac{d^2\varphi_0}{dz^2}-\frac{d^2\varphi_0}{dt^2},$$

the resulting equation for determining f will be

$$\left.\begin{array}{l} 0=\dfrac{d^2f}{dx^2}+\dfrac{d^2f}{dy^2}+b^2f \\ \quad -\dfrac{\varphi_0^2}{a^2}\cdot\left(\dfrac{df^2}{dx^2}\cdot\dfrac{d^2f}{dx^2}+2\dfrac{df}{dx}\cdot\dfrac{df}{dy}\cdot\dfrac{d^2f}{dxdy}+\dfrac{df^2}{dy^2}\cdot\dfrac{d^2f}{dy^2}\right) \end{array}\right\}. \quad (C.)$$

Here it is to be remarked, that if φ_0 should contain t, f is not independent of t, and our result is at variance with the hypothesis by which $udx+vdy+wdz$ was rendered integrable.

To ascertain the actual value of φ_0, the equation (B.) must be integrated, which may be done by successive approximation. I have already shown (Phil. Mag. for April, p. 279), by taking account of the three first terms of (B.), which are the principal terms, that the only integral applicable to the present inquiry is

$$\varphi=m\cos\frac{2\pi}{\lambda}(z-a't).$$

Setting out with this for a first approximation, it may be readily shown that the equation (B.) is satisfied by a series of the form

$$\varphi=m_1\cos\frac{2\pi}{\lambda}(z-a't)+m_2\cos\frac{4\pi}{\lambda}(z-a't)+m_3\cos\frac{6\pi}{\lambda}(z-a't)+\&c.$$

Hence it is clear that the value of $z-a't$, which causes $\frac{d\varphi}{dz}$ to vanish, gives a value of φ_0 independent of z and t. Thus the hypothesis of non-divergent waves is entirely consistent with the exact hydrodynamical equations.

It is worthy of remark, that the above series accounts for the *harmonics* which are observed to accompany loud notes.

Another criterion may be applied to the hypothesis of non-divergent waves. Let V be the velocity at a certain point of the fluid at the time t, and V′ be the velocity at the time $t+\delta t$ at a point of the fluid in advance of the other by δs in the direction of the line of motion. Then

$$V'=V+\frac{dV}{dt}\delta t+\frac{dV}{ds}\delta s.$$

Hence if V′=V, we have

$$\frac{dV}{dt}+\frac{dV}{ds}\cdot\frac{\delta s}{\delta t}=0.$$

Here $\frac{\delta s}{\delta t}$ is the rate at which a *given* velocity is propagated through space; and if this be a constant rate a_1, we obtain the partial differential equation

$$\frac{dV}{dt}+a_1\frac{dV}{ds}=0,$$

the integral of which is $V=\psi(s-a_1t)$. In the same way it may be shown that the density (ρ) must be a function of $s-a_1t$ in the case of a constant rate of propagation.

Now these criteria of uniform propagation are not satisfied by the hypothesis of plane waves, because on that hypothesis we have

$$V=a \text{ Nap.} \log \rho = f(s-(a+V)t).$$

Still less are they satisfied by the hypothesis of spherical waves.

But they are fully satisfied on the hypothesis of non-divergent waves, because the above value of ϕ shows that in this case $\frac{d\phi}{dz}$ is a function of $z-a't$; and from the known general equation

$$a^2 \text{ Nap.} \log \rho + \frac{d\phi}{dt} + \frac{V^2}{2} = 0,$$

it follows that ρ is also a function of $z-a't$.

I have thus shown, by reasoning with exact equations, the entire compatibility of the hypothesis of non-divergent waves with the hydrodynamical equations. Having at the same time demonstrated the incompatibility of plane-waves and spherical waves, I consider that the theorems of capital importance, to which the reasoning in this and the two former communications has been directed, are established; viz. that non-divergence is the normal character of aërial waves, and that the velocity of propagation is greater than a. I propose in a future communication to draw some inferences from the equations (B.) and (C.)

Cambridge Observatory,
July 20, 1848.

XVIII. *On the Magic Square of the Knight's March.*

By WILLIAM BEVERLEY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I INCLOSE for insertion in the Philosophical Magazine a very interesting MAGIC SQUARE, formed by numbering consecutively the *moves of the* KNIGHT *in the grand tour* of the chess-board. The *knight's march* has engaged the ingenuity of many eminent philosophers and mathematicians; but I believe that Mr. W. Beverley is the first who has solved the difficult problem of converting it into a *magic square*. The principle upon which he has effected it, seems to be somewhat akin to that invented by Dr. Roget, S.R.S., as explained in his paper on the *Knight's Move* in vol. xvi. of the Philosophical Magazine.

Yours very faithfully,
H. PERIGAL, Jun.

5 Smith Street, Chelsea,
March 29, 1848.

Fig. 1.

Columns.	9	10	11	12	13	14	15	16	
			e		*a*		*g*		
1	1	30	47	52	5	28	43	54	=260
2	48	51	2	29	44	53	6	27	=260
i			**A**				**B**		*j*
3	31	46	49	4	25	8	55	42	=260
4	50	3	32	45	56	41	26	7	=260
c									*d*
5	33	62	15	20	9	24	39	58	=260
6	16	19	34	61	40	57	10	23	=260
k			**C**				**D**		*l*
7	63	14	17	36	21	12	59	38	=260
8	18	35	64	13	60	37	22	11	=260
			f		*b*		*h*		
	260	260	260	260	260	260	260	260	

Fig. 2.—The reverse.

64	35	18	13	60	37	22	11	= 260
17	14	63	36	21	12	59	38	= 260
34	19	16	61	40	57	10	23	= 260
15	62	33	20	9	24	39	58	= 260
32	3	50	45	56	41	26	7	= 260
49	46	31	4	25	8	55	42	= 260
2	51	48	29	44	53	6	27	= 260
47	30	1	52	5	28	43	54	= 260
260	260	260	260	260	260	260	260	

These figures show the successive moves of the knight over the chess-board, every square being numbered in the order of the knight's march; so that the numbers in each vertical and horizontal column, added together, shall be equal; the sum of the eight numbers in each column being =260.

These figures also present many other peculiarities.

1. The chess-board (see fig. 1) is divided into four equal parts by the lines *ab* and *cd*. These parts are marked A, B, C and D, and consist each of sixteen squares,—four squares in height and four in width; and if the columns of any of these parts be added up, both vertically and horizontally, each column will give the same addition, 130.

e. g. the part marked C.

33	62	15	20	130
16	19	34	61	130
63	14	17	36	130
18	35	64	13	130
130	130	130	130	

2. The figure is divided into sixteen equal parts by the lines *ab*, *ef*, *gh*, *cd*, *ij*, *kl*: each of these parts consists of four squares; and if the four numbers in the four squares which compose any one of these parts be added together, they will make 130.

e. g.

15	20	= 130
34	61	

or

55	42	= 130
26	7	

3. The figure is divided, by the vertical line *ab*, and the horizontal column 1 is also divided by the same line, into two equal parts. Each part consists of four squares, in two of which the numbers are even, and in the others odd. Take the even numbers in the left division of the column, and the odd numbers in the right division, and add them together, thus:

in column 1, 30 + 52 = 82
in column 1, 5 + 43 = 48
and they will make 130

Or take the odd numbers in the left division, 1 + 47 = 48
and the even numbers in the right division, 28 + 54 = 82
and they will make 130

This will be found the case with all the horizontal columns 1 to 8.

The figure is divided, by the horizontal line *cd*, and the vertical column 16 is also divided by the same line, into two equal parts. Each part consists of four squares, two of whose numbers are even and the others odd. Take the even numbers above the line *cd*, and add them to the odd numbers below the line, thus:

in column 16, 54 + 42 = 96
in column 16, 23 + 11 = 34
and they will produce 130

Or take the odd numbers above the line, 27 + 7 = 34
and the even numbers below the line, 58 + 38 = 96
and they will also produce 130

This will be found the case with the columns 10, 12, 13, 14, 15, 16; that is to say, all the vertical columns except Nos. 9 and 11, which are different in consequence of a peculiar movement of the knight in these columns.

Take the odd numbers in the upper half of column 9, 1 + 31 = 32
and the even numbers in the lower half of column 11, 34 + 64 = 98
and they will make 130

Or the even numbers in the upper half of column 9, 48 + 50 = 98
and the odd numbers in the lower half of column 11, 15 + 17 = 32
and they will make 130

Or the even numbers in the lower half of column 9
and the odd numbers in the upper half of column 11
Or the odd numbers in the lower half of column 9
and the even numbers in the upper half of column 11
will, added together, produce the same amount, 130

4. The top and bottom numbers of the same vertical column
the 2nd and 7th do. do.
the 3rd and 6th do. do.
the 4th and 5th do. do.
added together, will produce the same amount, 65.

Thus in column	16,	the top	No. 54	and the	bottom	No. 11	make	65
...	13,	... 2nd	... 44	...	7th	... 21	...	65
...	15,	... 3rd	... 55	...	6th	... 10	...	65
...	10,	... 4th	... 3	...	5th	... 62	...	65

But the columns 9 and 11 are different from the others in this respect, which arises from the peculiar movement of the knight in these columns already alluded to.

In these columns—

The top No. of the	col. 9	and the	bottom No.	of the col.	11,	1+64	make	65
... 2nd	...	9	...	7th	...	11, 48+17	...	65
... 3rd	...	9	...	6th	...	11, 31+34	...	65
... 4th	...	9	...	5th	...	11, 50+15	...	65
or the top	...	11	...	bottom	...	9, 47+18	...	65

and so with the rest.

5. It will be observed that every number in the figure is in apposition in six different ways. For example, No. 54 is

1st, in apposition in the same vertical column with No. 11, making	65
2nd, in conjunction with No. 28 it is in apposition with Nos. 1 and 47, making	130
3rd, in conjunction with No. 42 it is in apposition with Nos. 23 and 11, making	130
4th, in apposition with the numbers in the same subdivision 43, 6 and 27, making	130
5th, in apposition with Nos. 5, 28 and 43, forming a horizontal column in division B, making	130
6th, in apposition with Nos. 27, 42 and 7, forming a vertical column in the same division, making	130

6. The knight's march may be commenced at the square marked 64, in fig. 1, and so backwards, until the 64th move terminates on the square marked 1. This will produce fig. 2, which answers to the first figure in all its peculiarities.

It will be easily observed that the march can be commenced from any of the corner or rooks' squares of the board; from any of the bishops' squares, or bishops' eighths; or from either of the rooks' thirds, or rooks' sixths.

WILLIAM BEVERLEY.

9 Upper Terrace, Islington,
June 5, 1847.

XIX. *On the Determination of the Numerical Values of the Coefficients in any series consisting of Sines and Cosines of Multiples of a variable angle.* *By* Sir J. W. Lubbock, *Bart., F.R.S., &c.*[*]

IN the following methods of developing any function of sines and cosines by means of particular values of the function, the artifice by which the coefficients are successively eliminated is taken from Le Verrier's "*Dévéloppements sur plusieurs points de la Théorie des Perturbations des Planètes,*" No. I.[†] That illustrious philosopher has not yet given the values of the numerical constants which are requisite when the method is practically employed.

Let

$$\begin{aligned}
R_0&=B_0+B_1+B_2+B_3+\&c.\\
R_1&=B_0+B_1\cos 1+B_2\cos 2+B_3\cos 3+\&c.\\
R_2&=R_0+A_1\sin 1+A_2\sin 2+A_3\sin 3+\&c.\\
R_2&=B_0+B_1\cos 2+B_2\cos 4+B_3\cos 6+\&c.\\
&\quad+A_1\sin 2+A_2\sin 4+A_3\sin 6+\&c.\\
R_3&=B_0+B_1\cos 3+B_2\cos 6+B_3\cos 8+\&c.\\
&\quad+A_1\sin 3+A_2\sin 6+A_3\sin 8+\&c.
\end{aligned}$$

Let $R_j=B_i\cos i+A_i\sin i$

$$R_{j+1}=B_i\cos(i+\Delta i)+A_i\sin(i+\Delta i)$$

$$R_{j+2}=B_i\cos(i+2\,\Delta i)+A_i\sin(i+2\,\Delta i)$$

$$\begin{aligned}
&R_j+R_{j+2}-2\,R_{j+1}\cos\theta\\
&=B_i\{\cos i+\cos(i+2\,\Delta i)-2\cos(i+\Delta i)\cos\theta\}\\
&+A_i\{\sin i+\sin(i+2\,\Delta i)-2\sin(i+\Delta i)\sin\theta\}\\
&=2\,B_i\cos(i+\Delta i)\{\cos\Delta i-\cos\theta\}+2\,A_i\sin(i+\Delta i)\{\cos\Delta i-\cos\theta\}\\
&=\{2\,B_i\cos(i+\Delta i)+2\,A_i\sin(i+\Delta i)\}\{\cos\Delta i-\cos\theta\}\\
&=-2^2\{B_i\cos(i+\Delta i)+A_i\sin(i+\Delta i)\}\sin\frac{1}{2}(\Delta i+\theta)\sin\frac{1}{2}(\Delta i-\theta).
\end{aligned}$$

If this expression be compared with the expression for R_j, it will be seen that one of the new factors of B_i, which I call the *variable factor,* is $\cos(i+\Delta i)$, the coefficient of B_i in the expression for R_{j+i}; the other factor, which I call the *constant factor,*

$$\cos\Delta i-\cos\theta,$$

is independent of i; and if θ be assumed equal to Δ_{i}, B_i and A_i are eliminated, because $\sin\frac{1}{2}(\Delta i-\theta)=0$.

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[*] Communicated by the Author.

[†] A translation of this memoir appeared in Taylor's Scientific Memoirs, Part XVIII. p. 334.

Le Verrier has given expressions by means of which A_i, B_i are determined, the A's and B's having *lower* indices, being obtained successively from these, by means of the particular values which the series assumes when the arbitrary angle which I have taken equal to unity becomes successively 1, 2, 3, 4, &c.

The following table shows the order in which the indices recur.

TABLE I.

	B_0.	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.	B_8. A_8.	B_9. A_9.	B_{10}. A_{10}.
R_0	0	0	0	0	0	0	0	0	0	0	0
R_1	0	1	2	3	4	5	6	7	8	9	10
R_2	0	2	4	6	8	10	12	14	16	18	20
R_3	0	3	6	9	12	15	18	21	24	27	30
R_4	0	4	8	12	16	20	24	28	32	36	40
R_5	0	5	10	15	20	25	30	35	40	45	50
R_6	0	6	12	18	24	30	36	42	48	54	60
R_7	0	7	14	21	28	35	42	49	56	63	70
R_8	0	8	16	24	32	40	48	56	64	72	80
R_9	0	9	18	27	36	45	54	63	72	81	90
R_{10}	0	10	20	30	40	50	60	70	80	90	100
R_{11}	0	11	22	33	44	55	66	77	88	99	110

1st Operation. Elimination of B_0.

$$(1)_1 = R_0 - R_1 = B_1(1-\cos 1) + B_2(1-\cos 2) + B_3(1-\cos 3) \ldots\ldots$$
$$- A_1 \sin 1 - A_2 \sin 2 - A_3 \sin 3 + \&c.$$
$$(1)_2 = R_1 - R_2 = B_1(\cos 1 - \cos 2) + B_2(\cos 2 - \cos 4) + B_3(\cos 3 - \cos 6)$$
$$+ A_1(\sin 1 - \sin 2) + A_2(\sin 2 - \sin 4) + A_3(\sin 3 - \sin 6) + \&c.$$
$$(1)_3 = R_2 - R_3 = B_1(\cos 2 - \cos 3) + B_2(\cos 4 - \cos 6) + B_3(\cos 6 - \cos 9)$$
$$+ A_1(\sin 2 - \sin 3) + A_2(\sin 4 - \sin 6) + A_3(\sin 6 - \sin 9) + \&c.$$

The following table shows the order in which the indices recur.

TABLE II.—Indices of variable factor.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.
$(1)_1 =$	0−1	0−2	0−3	0−4	0−5	0−6	0−7
$(1)_2 =$	1−2	2−4	3−6	4−8	5−10	6−12	7−14
$(1)_3 =$	2−3	4−6	6−9	8−12	10−15	12−18	14−21
$(1)_4 =$	3−4	6−8	9−12	12−16	15−20	18−24	21−28
$(1)_5 =$	4−5	8−10	12−15	16−20	20−25	24−30	28−35

which table shows that in $(1)_3$, for example,

B_3 is multiplied by $\cos 6-\cos 9=2\sin\frac{15}{2}\sin\frac{3}{2}$

B_4 „ „ $\cos 8-\cos 12=2\sin 10\sin 2$

B_5 „ „ $\cos 10-\cos 15=2\sin\frac{25}{2}\sin\frac{5}{2}$

A_3 „ „ $\sin 6-\sin 9=-2\cos\frac{15}{2}\sin\frac{3}{2}$

A_4 „ „ $\sin 8-\sin 12=-2\cos 10\sin 2$

A_5 „ „ $\sin 10-\sin 15=-2\cos\frac{25}{2}\sin\frac{5}{2}$.

The following Table exhibits the indices of the arguments which are introduced when the difference of the sines and cosines are replaced by products.

Table III.—Indices of variable factor.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.	B_8. A_8.	B_9. A_9.	B_{10}. A_{10}.
$(1)_1=$	$\frac{1}{2}.\frac{1}{2}$	1.1	$\frac{3}{2}.\frac{3}{2}$	2.2	$\frac{5}{2}.\frac{5}{2}$	3.3	$\frac{7}{2}.\frac{7}{2}$	4.4	$\frac{9}{2}.\frac{9}{2}$	5.5
$(1)_2=$	$\frac{3}{2}.\frac{1}{2}$	3.1	$\frac{9}{2}.\frac{3}{2}$	6.2	$\frac{15}{2}.\frac{5}{2}$	9.3	$\frac{21}{2}.\frac{7}{2}$	12.4	$\frac{27}{2}.\frac{9}{2}$	15.5
$(1)_3=$	$\frac{5}{2}.\frac{1}{2}$	5.1	$\frac{15}{2}.\frac{3}{2}$	10.2	$\frac{25}{2}.\frac{5}{2}$	15.3	$\frac{35}{2}.\frac{7}{2}$	20.4	$\frac{45}{2}.\frac{9}{2}$	25.5
$(1)_4=$	$\frac{7}{2}.\frac{1}{2}$	7.1	$\frac{21}{2}.\frac{3}{2}$	14.2	$\frac{35}{2}.\frac{5}{2}$	21.3	$\frac{45}{2}.\frac{7}{2}$	28.4	$\frac{63}{2}.\frac{9}{2}$	35.5
$(1)_5=$	$\frac{9}{2}.\frac{1}{2}$	9.1	$\frac{27}{2}.\frac{3}{2}$	18.2	$\frac{45}{2}.\frac{5}{2}$	27.3	$\frac{63}{2}.\frac{7}{2}$	36.4	$\frac{81}{2}.\frac{9}{2}$	45.5
$(1)_6=$	$\frac{11}{2}.\frac{1}{2}$	11.1	$\frac{33}{2}.\frac{3}{2}$	20.2	$\frac{55}{2}.\frac{5}{2}$	33.3	$\frac{77}{2}.\frac{7}{2}$	44.4	$\frac{99}{2}.\frac{9}{2}$	55.5
$(1)_7=$	$\frac{13}{2}.\frac{1}{2}$	13.1	$\frac{39}{2}.\frac{3}{2}$	22.2	$\frac{65}{2}.\frac{5}{2}$	39.3	$\frac{91}{2}.\frac{7}{2}$	52.4	$\frac{117}{2}.\frac{9}{2}$	65.5

2nd Operation. Elimination of B_1 and A_1.

Let

$(1)_1+(1)_3-2\,(1)_2\cos 1=(2)_1$

$(1)_2+(1)_4-2\,(1)_3\cos 1=(2)_2$

$(1)_3+(1)_5-2\,(1)_4\cos 1=(2)_3$

……………………………………

3rd Operation. Elimination of B_2 and A_2.

$$(2)_1+(2)_3-2\,(2)_2\cos 2=(3)_1$$
$$(2)_2+(2)_4-2\,(2)_3\cos 2=(3)_2$$
$$(2)_3+(2)_5-2\,(2)_4\cos 2=(3)_3$$

……………………………………

4th Operation. Elimination of B_3 and A_3.

$$(3)_1-(3)_3-2\,(3)_2\cos 3=(4)_1$$
$$(3)_2+(3)_4-2\,(3)_3\cos 3=(4)_2$$
$$(3)_3+(3)_5-2\,(3)_4\cos 3=(4)_3$$

Taking any vertical column as that under B_{10}, the second operation by which B_1 and A_1 are eliminated is;

$$\begin{aligned}(1)_1+(1)_3-2\,(1)_2\cos 1&=2B_{10}\sin 5\,\{\sin 5+\sin 25-2\sin 15\cos 1\}\\&\quad-2\,A_{10}\sin 5\,\{\cos 5+\cos 25-2\cos 15\cos 1\}\\&=2^2\,B_{10}\sin 5\sin 15\,\{\cos 10-\cos 1\}\\&\quad-2^2\,A_{10}\sin 5\cos 15\,\{\cos 10-\cos 1\}\end{aligned}$$

$$\begin{aligned}(1)_2+(1)_4-2\,(1)_3\cos 1&=2\,B_{10}\sin 5\,\{\sin 15+\sin 35-2\sin 25\cos 1\}\\&\quad-2\,A_{10}\sin 5\,\{\cos 15+\cos 35-2\cos 25\cos 1\}\\&=2^2\,B_{10}\sin 5\sin 25\,\{\cos 10-\cos 1\}\\&\quad-2^2\,A_{10}\sin 5\cos 25\,\{\cos 10-\cos 1\}\end{aligned}$$

$$\begin{aligned}(1)_3+(1)_5-2\,(1)_4\cos 1&=2\,B_{10}\sin 5\,\{\sin 25+\sin 45-2\sin 35\cos 1\}\\&\quad-2\,A_{10}\sin 5\,\{\cos 25+\cos 45-2\cos 35\cos 1\}\\&=2^2B_{10}\sin 5\sin 35\,\{\cos 10-\cos 1\}\\&\quad-2^2\,A_{10}\sin 5\cos 35\,\{\cos 10-\cos 1\}.\end{aligned}$$

The factor between the brackets is the same throughout the same vertical column, it may therefore be called the *constant factor.*

If this operation is repeated as in

$$(3)_1=(2)_1+(2)_3-2\,(2)_2\cos 2,$$

the constant factor which is the same throughout the same vertical column is twice

$$\cos 10-\cos 2.$$

If the operation is again repeated as in

$$(4)_1=(3)_1+(3)_2-2\,(2)_3\cos 3,$$

the constant factor which is the same throughout the same vertical column is twice

$$\cos 10-\cos 3.$$

The following table shows the indices for each vertical column.

TABLE IV.—Indices of constant factor.

B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.	B_8. A_8.	B_9. A_9.	B_{10}. A_{10}.	B_{11}. A_{11}.	B_{12}. A_{12}.
2−1	3−1	4−1	5−1	6−1	7−1	8−1	9−1	10−1	11−1	12−1
	3−2	4−2	5−2	6−2	7−2	8−2	9−2	10−2	11−2	12−2
		4−3	5−3	6−3	7−3	8−3	9−3	10−3	11−3	12−3
			5−4	6−4	7−4	8−4	9−4	10−4	11−4	12−4
				6−5	7−5	8−5	9−5	10−5	11−5	12−5
					7−6	8−6	9−6	10−6	11−6	12−6
						8−7	9−7	10−7	11−7	12−7
							9−8	10−8	11−8	12−8
								10−9	11−9	12−9
									11−10	12−10
										12−11

Each operation moves the indices of the variable factor to the numbers which are in the next horizontal row beneath in Table III. The second operation in the preceding example gave sin 5 sin 15, sin 5 cos 15, the next sin 5 sin 25, sin 5 cos 25, and the fourth operation sin 5 sin 35, sin 5 cos 35, 5 and 35 being the indices which are found in that vertical column and in the fourth horizontal row.

$$\cos 10-\cos 1=-2\sin\frac{11}{2}\sin\frac{9}{2}$$

$$\cos 10-\cos 2=-2\sin\frac{12}{2}\sin\frac{8}{2}=-2\sin 6\sin 4$$

$$\cos 10-\cos 3=-2\sin\frac{13}{2}\sin\frac{7}{2}.$$

The following table gives the indices of the sines which are introduced by each operation when the products of the two sines are substituted for the difference of the two cosines.

TABLE V.—Indiccs of constant factor.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.	B_8. A_8.	B_9. A_9.	B_{10}. A_{10}.	B_{11}. A_{11}.	B_{12}. A_{12}.
$(1)_1$	......	......	......	......	......	......	......	......	......	......	......	......
$(2)_1$	......	$\frac{3}{2}\cdot\frac{1}{2}$	2.1	$\frac{5}{2}\cdot\frac{3}{2}$	3.2	$\frac{7}{2}\cdot\frac{5}{2}$	4.3	$\frac{9}{2}\cdot\frac{7}{2}$	5.4	$\frac{11}{2}\cdot\frac{9}{2}$	6.5	$\frac{13}{2}\cdot\frac{11}{2}$
$(3)_1$	......	......	$\frac{5}{2}\cdot\frac{1}{2}$	3.1	$\frac{7}{2}\cdot\frac{3}{2}$	4.2	$\frac{9}{2}\cdot\frac{5}{2}$	5.3	$\frac{11}{2}\cdot\frac{7}{2}$	6.4	$\frac{13}{2}\cdot\frac{9}{2}$	7.5
$(4)_1$	......	......	......	$\frac{7}{2}\cdot\frac{1}{2}$	4.1	$\frac{9}{2}\cdot\frac{3}{2}$	5.2	$\frac{11}{2}\cdot\frac{5}{2}$	6.3	$\frac{13}{2}\cdot\frac{7}{2}$	7.4	$\frac{15}{2}\cdot\frac{9}{2}$
$(5)_1$	......	......	......	......	$\frac{9}{2}\cdot\frac{1}{2}$	5.1	$\frac{11}{2}\cdot\frac{3}{2}$	6.2	$\frac{13}{2}\cdot\frac{5}{2}$	7.3	$\frac{15}{2}\cdot\frac{7}{2}$	8.4
$(6)_1$	......	......	......	......	......	$\frac{11}{2}\cdot\frac{1}{2}$	6.1	$\frac{13}{2}\cdot\frac{3}{2}$	7.2	$\frac{15}{2}\cdot\frac{5}{2}$	8.3	$\frac{17}{2}\cdot\frac{7}{2}$
$(7)_1$	......	......	......	......	......	......	$\frac{13}{2}\cdot\frac{1}{2}$	7.1	$\frac{15}{2}\cdot\frac{3}{2}$	8.2	$\frac{17}{2}\cdot\frac{5}{2}$	9.3
$(8)_1$	......	......	......	......	......	......	......	$\frac{15}{2}\cdot\frac{1}{2}$	8.1	$\frac{17}{2}\cdot\frac{3}{2}$	9.2	$\frac{19}{2}\cdot\frac{5}{2}$
$(9)_1$	......	......	......	......	......	......	......	......	$\frac{17}{2}\cdot\frac{1}{2}$	9.1	$\frac{19}{2}\cdot\frac{3}{2}$	10.2
$(10)_1$	......	......	......	......	......	......	......	......	......	$\frac{19}{2}\cdot\frac{1}{2}$	10.1	$\frac{21}{2}\cdot\frac{3}{2}$
$(11)_1$	......	......	......	......	......	......	......	......	......	......	$\frac{21}{2}\cdot\frac{1}{2}$	11.1
$(12)_1$	......	......	......	......	......	......	......	......	......	......	......	$\frac{23}{2}\cdot\frac{1}{2}$

The following table shows the indices of the entire factor, the indices of the variable factor being taken from Table III. and those of the constant factor from Table V.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.
$(1)_1$	$\frac{1}{2}\cdot\frac{1}{2}$	1.1	$\frac{3}{2}\cdot\frac{3}{2}$	2.2	$\frac{5}{2}\cdot\frac{5}{2}$	3.3
	$-\frac{1}{2}\cdot\frac{1}{2}$	-1.1	$-\frac{3}{2}\cdot\frac{3}{2}$	-2.2	$-\frac{5}{2}\cdot\frac{5}{2}$	-3.3
$(1)_2$	$\frac{3}{2}\cdot\frac{1}{2}$	3.1	$\frac{9}{2}\cdot\frac{3}{2}$	6.2	$\frac{15}{2}\cdot\frac{5}{2}$	9.3
	$-\frac{3}{2}\cdot\frac{1}{2}$	$-3\cdot1$	$-\frac{9}{2}\cdot\frac{3}{2}$	-6.2	$-\frac{15}{2}\cdot\frac{5}{2}$	-9.3
$(2)_1$	....	$3.1.\frac{3}{2}\cdot\frac{1}{2}$	$\frac{9}{2}\cdot\frac{3}{2}.2.1$	$6.2.\frac{5}{2}\cdot\frac{3}{2}$	$\frac{15}{2}\cdot\frac{5}{2}\cdot\frac{3}{2}.2$	$9.3.\frac{7}{2}\cdot\frac{5}{2}$
		$-3.1.\frac{3}{2}\cdot\frac{1}{2}$	$-\frac{9}{2}\cdot\frac{3}{2}.2.1$	$-6.2.\frac{5}{2}\cdot\frac{3}{2}$	$-\frac{15}{2}\cdot\frac{5}{2}\cdot\frac{3}{2}.2$	$-9.3.\frac{7}{2}\cdot\frac{5}{2}$
$(2)_2$	....	$5.1.\frac{3}{2}\cdot\frac{1}{2}$	$\frac{15}{2}\cdot\frac{3}{2}.2.1$	$10.2.\frac{5}{2}\cdot\frac{3}{2}$	$\frac{25}{2}\cdot\frac{5}{2}\cdot\frac{3}{2}.2$	$15.3.\frac{7}{2}\cdot\frac{5}{2}$
		$-5.1.\frac{3}{2}\cdot\frac{1}{2}$	$-\frac{15}{2}\cdot\frac{3}{2}.2.1$	$-10.2.\frac{5}{2}\cdot\frac{3}{2}$	$-\frac{25}{2}\cdot\frac{5}{2}\cdot\frac{3}{2}.2$	$-15.3.\frac{7}{2}\cdot\frac{5}{2}$

Finally, if $2^{\circ}=1$ be represented by 0

$2 \sin \frac{1}{2}$ be represented by . . . . $\frac{1}{2}$

$2^2 \sin \frac{1}{2} \sin 1$ be represented by 1

$2^3 \sin \frac{1}{2} \sin 1 \sin \frac{3}{2}$ be represented by . . . . . $\frac{3}{2}$

$2^i \sin \frac{1}{2} \sin 1 \ldots\ldots \sin \frac{i}{2}$ be represented by $\frac{i}{2}$.

The preceding table may be put in the following shape, omitting the alternate lines.

TABLE VI.—Indices of entire factor.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.
$+(1)_1$	$\frac{1}{2}$ $\frac{\frac{1}{2}}{0}$	1 $\frac{1}{\frac{1}{2}}$	$\frac{3}{2}$ $\frac{\frac{3}{2}}{1}$	2 $\frac{2}{\frac{3}{2}}$	$\frac{5}{2}$ $\frac{\frac{5}{2}}{2}$	3 $\frac{3}{\frac{5}{2}}$
$+(1)_2$	$\frac{3}{2}$	3	$\frac{9}{2}$	6	$\frac{15}{2}$	9
$-(2)_1$	……………	3 $\frac{\frac{3}{2}}{0}$	$\frac{9}{2}$ $\frac{2}{\frac{1}{2}}$	6 $\frac{\frac{5}{2}}{1}$	$\frac{15}{2}$ $\frac{3}{\frac{3}{2}}$	9 $\frac{\frac{7}{2}}{2}$
$-(2)_2$	……………	5	$\frac{15}{2}$	10	$\frac{25}{2}$	15
$+(3)_1$	……………	5	$\frac{15}{2}$ $\frac{\frac{5}{2}*}{0}$	10 $\frac{3}{\frac{1}{2}}$	$\frac{25}{2}$ $\frac{\frac{7}{2}}{1}$	15 $\frac{4}{\frac{3}{2}}$
$+(3)_2$	……………	……………	……………	14	$\frac{35}{2}$	21
$-(4)_1$	……………	……………	……………	14 $\frac{\frac{7}{2}}{0}$	$\frac{35}{2}$ $\frac{4}{\frac{1}{2}}$	21 $\frac{\frac{9}{2}}{1}$
$-(4)_2$	……………	……………	……………	18	$\frac{45}{2}$	27
$+(5)_1$	……………	……………	……………	……………	$\frac{45}{2}$ $\frac{\frac{9}{2}}{0}$	27 $\frac{5}{\frac{1}{2}}$
$+(5)_2$	……………	……………	……………	……………	$\frac{55}{0}$	33
$-(6)_1$	……………	……………	……………	……………	……………	33 $\frac{11}{2}$
$-(6)_2$	……………	……………	……………	……………	……………	39

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* $\frac{5}{2}$ here stands for $2^5 \sin \frac{1}{2} \sin 1 \sin \frac{3}{2} \sin 2 \sin \frac{5}{2}$.

The table reads thus, the coefficient of B_5 in $(4)_1$

$$=-2^8 \sin 1 \sin \frac{3}{2} \sin 2 \sin \frac{5}{2} \ldots\ldots\ldots \sin 4 \sin \frac{35}{2}$$

and the coefficient of A_5 in $(4)_1$

$$=2^8 \sin 1 \sin \frac{3}{2} \sin 2 \sin \frac{5}{2} \ldots\ldots\ldots \sin 4 \cos \frac{35}{2}.$$

Similar expressions may be obtained when the particular values are employed which the series assumes when the arbitrary angle becomes successively 1, 180°+1, 2, 180°+2, &c. This enables the separation into two categories, one containing the *even* and the other the *uneven* indices.

If

$$\begin{aligned}
R_1 &= B_0 + B_1 \cos 1 + B_2 \cos 2 + B_3 \cos 3 + \&c. \\
&\quad + A_1 \sin 1 + A_2 \sin 2 + A_3 \sin 3 + \&c. \\
R'_1 &= B_0 + B_1 \cos (180^\circ+1) + B_2 \cos 2\,(180^\circ+1) \\
&\quad + B_3 \cos 3(180^\circ+1) + \&c. \\
&\quad + A_1 \sin (180^\circ+1) + A_2 \sin 2\,(180^\circ+1) \\
&\qquad + A_3 \sin 3\,(180^\circ+1) + \&c. \\
&= B_0 - B_1 \cos 1 + B_2 \cos 2 - B_3 \cos 3 + \&c. \\
&\quad - A_1 \sin 1 + A_2 \sin 2 - A_3 \sin 3 + \&c.
\end{aligned}$$

$$\begin{aligned}
\frac{1}{2}(R_1 + R'_1) = \mathfrak{R}_1 &= B_0 + B_2 \cos 2 + B_4 \cos 4 + \&c. \\
&\quad + A_2 \sin 2 + A_4 \sin 4 + \&c. \\
\frac{1}{2}(R_1 - R'_1) = \mathfrak{R}'_1 &= B_1 \cos 1 + B_3 \cos 3 + B_5 \cos 5 + \&c. \\
&\quad + A_1 \sin 1 + A_3 \sin 3 + A_5 \sin 5 + \&c.
\end{aligned}$$

So that if R_1, R_2, R_3, R_4, &c. are the particular values of R for multiples of any angle which I have assumed equal to unity, and R'_1, R'_2, R'_3, R'_4, &c. are the particular values of R for the same multiples of the angle increased by 180°, $R_1+R'_1$, $R_2+R'_2$, $R_3+R'_3$, &c. will contain only A_i and B_i where i is even, and $R_1-R'_1$, $R_2-R'_2$, $R_3-R'_3$, &c. will contain only A_i and B_i where i is uneven.

1st Category, *even* indices.

1st Operation. Elimination of B_0.

$$\begin{aligned}
(1)_1 = \frac{1}{2}(\mathfrak{R}_1 - \mathfrak{R}_2) &= B_2 (\cos 2 - \cos 4) + B_4 (\cos 4 - \cos 8) + \&c. \\
&\quad + A_2 (\sin 2 - \sin 4) + A_4 (\sin 4 - \sin 8) + \&c.
\end{aligned}$$

$$(1)_2=\frac{1}{2}(\mathfrak{R}_2-\mathfrak{R}_3)=B_2(\cos 4-\cos 6)+B_4(\cos 8-\cos 12)+\&c.$$

$$+A_2(\sin 4-\sin 6)+A_4(\sin 8-\sin 12)+\&c.$$

The indices are given in the alternate columns of Table II., and the indices of the arguments which are introduced when the differences of the sines and cosines are replaced by products are given in the alternate columns of Table III.

2nd Operation. Elimination of B_2 and A_2.

Let

$$(1)_1+(1)_3-2(1)_2\cos 2=(2)_1$$

$$(1)_2+(1)_4-2(2)_2\cos 2=(2)_2$$

$$(1)_3+(1)_4-2(2)_3\cos 2=(2)_3$$

. . . .

3rd Operation. Elimination of B_4 and A_4.

$$(2)_1+(2)_3-2(2)_2\cos 4=(3)_1$$

$$(2)_2+(2)_4-2(2)_3\cos 4=(3)_2$$

$$(2)_3+(2)_5-2(2)_4\cos 4=(3)_3$$

. . . .

Taking any vertical column as that under B_{10}, the second operation by which B_2 and A_2 are eliminated is,

$$(1)_1+(1)_3-2(1)_2\cos 2=2B_{10}\sin 5\{\sin 15+\sin 35-2\sin 25\cos 2\}$$

$$-2A_{10}\sin 5\{\cos 15+\cos 35-2\cos 25\cos 2\}$$

$$=2B_{10}\sin 5\sin 25\{\cos 10-\cos 2\}$$

$$-2A_{10}\sin 5\cos 25\{\cos 10-\cos 2\}.$$

The indices of the constant factors are given in the alternate columns and the alternate horizontal rows of Table IV., and the indices of the sines which are introduced when the products of sines are substituted for the difference of the cosines, are given in the alternate columns and the alternate horizontal rows of Table V. Each operation as before moves the indices of the variable factor to the numbers which are in the next horizontal row beneath in Table III.

If

$2\sin 1$ be represented by 1

$2^2\sin 1\sin 2$ be represented by . . . 2

$2^3\sin 1\sin 2\sin 3$ be represented by 3,

&c.

The following table exhibits the entire factor.

TABLE VII.

	B_2. A_2.		B_4. A_4.		B_6. A_6.		B_8. A_8.		B_{10}. A_{10}.	
$+(1)_1$	3	1	6	2	9	3	12	4	15	5
$+(1)_2$	5		10		15		20		25	
$-(2)_1$	....		10	3	15	$\frac{4}{1}$	20	$\frac{5}{2}$	25	$\frac{6}{3}$
$-(2)_2$	....		14		21		28		35	
$+(3)_1$	....		....		21	5	28	$\frac{6}{1}$	35	$\frac{7}{2}$
$+(3)_2$	....		....		27		36		45	
$-(4)_1$	....		....		....		36	7	45	$\frac{8}{1}$
$-(4)_2$	....		....		....		44		55	
$+(5)_1$	....		....		....		....		55	9
$+(5)_2$	....		....		....		....		65	

of which the law is evident.

The table reads thus: the coefficient of B_8 in $(3)_1$

$$=2^6 \sin 2 \sin 3 \sin 4 \sin 5 \sin 6 \sin 28,$$

the coefficient of A_8 in $(3)_1$

$$=-2^6 \sin 2 \sin 3 \sin 4 \sin 5 \sin 6 \cos 28.$$

2nd Category, *uneven* indices.

1st Operation. Elimination of B_1 and A_1.

Let

$$(1)_1=\mathfrak{R}'_1=\frac{1}{2}(R_1-R'_1)$$

$$(2)_1=\mathfrak{R}'_2=\frac{1}{2}(R_2-R'_2)$$

$$(3)_1=\mathfrak{R}'_3=\frac{1}{2}(R_3-R'_3).$$

The indices are given in the alternate columns of Table I.

$$(1)_1+(1)_3-2(1)_2\cos 1=(2)_1$$

$$(1)_2+(1)_4-2(1)_3\cos 1=(2)_2$$

$$(1)_3+(1)_5-2(1)_4\cos 1=(2)_3$$

. . . .

2nd Operation. Elimination of B_3 and A_3.

$$(2)_1+(2)_3-2(1)_3\cos 3=(3)_1$$

$$(2)_2+(2)_4-2(2)_4\cos 3=(3)_2$$

$$(2)_3+(2)_5-2(2)_5\cos 3=(3)_3$$

. . . .

Taking any vertical column as that under B_9, the first operation by which A_1 and B_1 are eliminated, is—

$$\begin{aligned}(1)_1+(1)_3-2(1)_2\cos 1 &= 2\,B_9\{\cos 9+\cos 27-2\cos 18\cos 1\}\\ &\quad +2\,A_9\{\sin 9+\sin 27-2\sin 18\cos 1\}\\ &= 2\,B_9\sin 18\,\{\cos 9-\cos 1\}\\ &\quad -2\,A_9\cos 18\,\{\cos 9-\cos 1\}.\end{aligned}$$

If, as before,

$2\sin 1\sin 1$ be represented by 1

$2^2\sin 1\sin 2$ be represented by . . . 2

$2^3\sin 1\sin 2\sin 3$ be represented by 3,

&c.

The following table exhibits the entire factor.

TABLE VIII.

	B_1. A_1.	B_3. A_3.	B_5. A_5.	B_7. A_7.	B_9. A_9.
$+(1)_1$	1	3	5	7	9
$+(1)_2$	2	6	10	14	18
$-(2)_1$	....	6 2	10 $\frac{3}{1}$	14 $\frac{4}{2}$	18 $\frac{5}{3}$
$-(2)_2$	....	9	15	21	27
$+(3)_1$	....	....	15 4	21 $\frac{5}{1}$	27 $\frac{6}{2}$
$+(3)_2$	....	....	20	28	36
$-(4)_1$	....	....	....	28 6	36 $\frac{7}{1}$
$-(4)_2$	....	....	....	35	45
$+(5)_1$	....	....	....	....	45 8
$+(5)_2$	....	....	....	....	54

Expressions may also be obtained by means of which A_i and B_i can be obtained at once whatever i may be, without depending upon any others with different indices.

Let

$$(1)_1 = B_1\{1-\cos 1\} + B_2\{1-\cos 2\} + B_3\{1-\cos 3\}$$
$$-A_1 \sin 1 - A_2 \sin 2 - A_3 \sin 3 \text{ \&c.}$$
$$(1)_2 = B_1\{\cos 1 - \cos 2\} + B_2\{\cos 2 - \cos 4\} + B_3\{\cos 3 - \cos 6\}$$
$$+A_1\{\sin 1 - \sin 2\} + B_2\{\sin 2 - \sin 4\} + A_3\{\sin 3 - \sin 6\} + \text{\&c.}$$

as before.

If any such series be treated thus:

$$(1)_1 + (1)_3 - 2(1)_2 \cos 1 = (2)_1$$

B_1 and A_1 are eliminated and are not found in $(2)_1$.

If the quantity

$$(1)_1 + (1)_3 - 2(1) \cos i = I \text{ is formed,}$$

B_i and A_i are eliminated and are not found in I.

If therefore it is desired to eliminate the A and B having any indices 2, 3, 4, 5, &c., similar operations must be performed with cos 2, cos 3, cos 4, cos 5.

2nd Operation. Elimination of B_2, A_2.

Let

$$(1)_1 + (1)_3 - 2(1)_2 \cos 2 = (2)_1$$
$$(1)_2 + (1)_4 - 2(1)_3 \cos 2 = (2)_2$$
$$(1)_3 + (1)_5 - 2(1)_4 \cos 2 = (2)_3.$$

The second operation by which A_2 and B_2 are eliminated is as regards B_1, A_1;

$$2\,B_1 \sin\frac{1}{2}\left\{\sin\frac{1}{2} + \sin\frac{5}{2} - 2\sin\frac{3}{2}\cos 2\right\}$$
$$-2\,A_1 \sin\frac{1}{2}\left\{\cos\frac{1}{2} + \cos\frac{5}{2} - 2\cos\frac{3}{2}\cos 2\right\}.$$
$$= 2^2 B_1 \sin\frac{3}{2}\sin\frac{1}{2}\left\{\cos 1 - \cos 2\right\} - 2^2 A_1 \cos\frac{3}{2}\sin\frac{1}{2}\left\{\cos 1 - \cos 2\right\}.$$

If this operation is repeated as in

$$(2)_1 + (2)_3 - 2(1)_2 \cos 3 = (3)_1,$$

as regards B_1, A_1, the constant factor introduced is

$$\cos 1 - \cos 3.$$

The following table gives the indices for each vertical column under B_i, A_i when the operations are conducted successively and separately, so as to eliminate all except B_i, A_i.

TABLE IX. Indices of constant factors.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.	B_8. A_8.	B_9. A_9.	B_{10}. A_{10}.	B_{11}. A_{11}.	B_{12}. A_{12}.
$(2)_1$	1−2	2−1	3−1	4−1	5−1	6−1	7−1	8−1	9−1	10−1	11−1	12−1
$(3)_1$	1−3	2−3	3−2	4−2	5−2	6−2	7−2	8−2	9−2	10−2	11−2	12−2
$(4)_1$	1−4	2−4	3−4	4−3	5−3	6−3	7−3	8−3	9−3	10−3	11−3	12−3
$(5)_1$	1−5	2−5	3−5	4−5	5−4	6−4	7−4	8−4	9−4	10−4	11−4	12−4
$(6)_1$	1−6	2−6	3−6	4−6	5−6	6−5	7−5	8−5	9−5	10−5	11−5	12−5

Each operation moves the indices of the variable factor to the numbers which are in the next horizontal row beneath in Table III. The following table gives the indices of the sines which are introduced by each operation when the products of two sines are substituted for the difference of two cosines.

TABLE X. Indices of constant factors.

	B_1. A_1.	B_2. A_2.	B_3. A_3.	B_4. A_4.	B_5. A_5.	B_6. A_6.	B_7. A_7.	B_8. A_8.	B_9. A_9.	B_{10}. A_{10}.	B_{11}. A_{11}.	B_{12}. A_{12}.
$(1)_1$	$\frac{3}{2}.\frac{1}{2}$	$\frac{3}{2}.\frac{1}{2}$	2. 1	$\frac{5}{2}.\frac{3}{2}$	3. 2	$\frac{7}{2}.\frac{5}{2}$	4. 3	$\frac{9}{2}.\frac{7}{2}$	5. 4	$\frac{11}{2}.\frac{9}{2}$	6. 5	$\frac{13}{2}.\frac{11}{2}$
$(2)_1$	2. 1	$\frac{5}{2}.\frac{1}{2}$	$\frac{5}{2}.\frac{1}{2}$	3. 1	$\frac{7}{2}.\frac{3}{2}$	4. 2	$\frac{9}{2}.\frac{5}{2}$	5. 3	$\frac{11}{2}.\frac{7}{2}$	6. 4	$\frac{13}{2}.\frac{9}{2}$	7. 5
$(3)_1$	$\frac{5}{2}.\frac{3}{2}$	3. 1	$\frac{7}{2}.\frac{1}{2}$	$\frac{7}{2}.\frac{1}{2}$	4. 1	$\frac{9}{2}.\frac{3}{2}$	5. 1	$\frac{11}{2}.\frac{5}{2}$	6. 3	$\frac{13}{2}.\frac{7}{2}$	7. 4	$\frac{15}{2}.\frac{9}{2}$
$(4)_1$	3. 2	$\frac{7}{2}.\frac{3}{2}$	4. 1	$\frac{9}{2}.\frac{1}{2}$	$\frac{9}{2}.\frac{1}{2}$	5. 1	$\frac{11}{2}.\frac{3}{2}$	6. 2	$\frac{13}{2}.\frac{9}{2}$	7. 3	$\frac{15}{2}.\frac{7}{2}$	8. 4
$(5)_1$	$\frac{7}{2}.\frac{5}{2}$	4. 2	$\frac{9}{2}.\frac{3}{2}$	5. 1	$\frac{1}{2}.\frac{1}{2}$	$\frac{11}{2}.\frac{1}{2}$	6. 1	$\frac{13}{2}.\frac{3}{2}$	7. 2	$\frac{15}{2}.\frac{5}{2}$	8. 3	$\frac{17}{2}.\frac{7}{2}$
$(6)_1$	4. 3	$\frac{9}{2}.\frac{5}{2}$	5. 2	$\frac{11}{2}.\frac{3}{2}$	6. 1	$\frac{13}{2}.\frac{1}{2}$	$\frac{13}{2}.\frac{1}{2}$	7. 1	$\frac{15}{2}.\frac{3}{2}$	8. 2	$\frac{17}{2}.\frac{5}{2}$	9. 3
$(7)_1$	$\frac{9}{2}.\frac{7}{2}$	5. 3	$\frac{11}{2}.\frac{5}{2}$	6. 2	$\frac{13}{2}.\frac{3}{2}$	7. 1	$\frac{15}{2}.\frac{1}{2}$	$\frac{15}{2}.\frac{1}{2}$	8. 1	$\frac{17}{2}.\frac{3}{2}$	9. 2	$\frac{19}{2}.\frac{5}{2}$
$(8)_1$	5. 4	$\frac{11}{2}.\frac{7}{2}$	6. 3	$\frac{13}{2}.\frac{5}{2}$	7. 2	$\frac{15}{2}.\frac{3}{2}$	8. 1	$\frac{17}{2}.\frac{1}{2}$	$\frac{17}{2}.\frac{1}{2}$	9. 1	$\frac{19}{2}.\frac{3}{2}$	10. 2
$(9)_1$	$\frac{11}{2}.\frac{9}{2}$	6. 4	$\frac{13}{2}.\frac{7}{2}$	7. 3	$\frac{15}{2}.\frac{5}{2}$	8. 2	$\frac{17}{2}.\frac{3}{2}$	9. 1	$\frac{19}{2}.\frac{1}{2}$	$\frac{19}{2}.\frac{1}{2}$	10. 1	$\frac{21}{2}.\frac{3}{2}$
$(10)_1$	6. 5	$\frac{13}{2}.\frac{9}{2}$	7. 4	$\frac{15}{2}.\frac{7}{2}$	8. 3	$\frac{17}{2}.\frac{5}{2}$	9. 2	$\frac{19}{2}.\frac{3}{2}$	10. 1	$\frac{21}{2}.\frac{3}{2}$	$\frac{21}{2}.\frac{1}{2}$	11. 1
$(11)_1$	$\frac{13}{2}.\frac{11}{2}$	7. 5	$\frac{15}{2}.\frac{9}{2}$	8. 4	$\frac{17}{2}.\frac{7}{2}$	9. 3	$\frac{19}{2}.\frac{5}{2}$	10. 2	$\frac{21}{2}.\frac{3}{2}$	11. 1	$\frac{23}{2}.\frac{1}{2}$	$\frac{23}{2}.\frac{1}{2}$
$(12)_1$	7. 6	$\frac{15}{2}.\frac{11}{2}$	8. 5	$\frac{17}{2}.\frac{9}{2}$	9. 4	$\frac{19}{2}.\frac{7}{2}$	10. 3	$\frac{21}{2}.\frac{5}{2}$	11. 2	$\frac{23}{2}.\frac{3}{2}$	12. 1	$\frac{25}{2}.\frac{1}{2}$

All the factors above the line which crosses the table are −, and all beneath it +.

So that, finally, for example, writing the indices of sines outside the bracket, if B_7, A_7 are neglected together with every B and A which has a higher index than 7.

$$(6)_1=2^{11}\frac{1}{2}.\frac{3}{2}.\frac{1}{2}.2.1.\frac{5}{2}.\frac{3}{2}.3.2.\frac{7}{2}.\frac{5}{2}\left\{B_1\sin\frac{11}{2}-A_1\cos\frac{11}{2}\right\}$$

$$(6)_2=2^{11}\frac{1}{2}.\frac{3}{2}.\frac{1}{2}.2.1.\frac{5}{2}.\frac{3}{2}.3.2.\frac{7}{2}.\frac{5}{2}\left\{B_1\sin\frac{13}{2}-A_1\cos\frac{13}{2}\right\}$$

$$(6)_2\cos\frac{11}{2}-(6)_1\cos\frac{13}{2}=2^{11}\frac{1}{2}.\frac{3}{2}.\frac{1}{2}.2.1.\frac{5}{2}.\frac{3}{2}.3.2.\frac{7}{2}.\frac{5}{2}.1\,B_1$$

$$(6)_2\sin\frac{11}{2}-(6)_1\sin\frac{13}{2}=2^{11}\frac{1}{2}.\frac{3}{2}.\frac{1}{2}.2.1.\frac{5}{2}.\frac{3}{2}.3.2.\frac{7}{2}.\frac{5}{2}.1\,A_1.$$

By means of the foregoing table and Table III., the values of any quantity B and A may be found upon any hypothesis as regards the terms which may safely be neglected.

Similar expressions may be obtained when the series are separated into two categories, the one containing only even and the other uneven indices.

1st Category, *even* indices.

2nd Operation. Elimination of B_4, A_4.

Let

$$(1)_1+(1)_3-2(1)_2\cos 4=(2)_1$$
$$(1)_2+(1)_4-2(1)_3\cos 4=(2)_2$$
$$(1)_2+(1)_5-2(1)_4\cos 4=(2)_3.$$

The second operation by which B_4 and A_4 are eliminated is as regards B_2, A_2.

$$2\,B_2\sin 1\{\sin 3+\sin 7-2\sin 5\cos 4\}$$
$$-2\,A_2\sin 1\{\cos 3+\cos 7-2\cos 5\cos 4\}$$
$$=2^2\,B_2\sin 5\sin 1\{\cos 2-\cos 4\}-2\,A_2\cos 5\sin 1\{\cos 2-\cos 4\}.$$

The following table gives the indices for each vertical column under B_i, A_i when the operations are conducted successively and separately so as to eliminate all except B_i, A_i.

TABLE XI. Indices of constant factors.

	B_2. A_2.	B_4. A_4.	B_6. A_6.	B_8. A_8.	B_{10}. A_{10}.	B_{12}. A_{12}.	B_{14}. A_{14}.	B_{16}. A_{16}.	B_{18}. A_{18}.	B_{20}. A_{20}.
$(2)_1$	2−4	4−2	6−2	8−2	10−2	12−2	14−2	16−2	18−2	20−2
$(3)_1$	2−6	4−6	6−4	8−4	10−4	12−4	14−4	16−4	18−4	20−4
$(4)_1$	2−8	4−8	6−8	8−6	10−6	12−6	14−6	16−6	18−6	20−6
$(5)_1$	2−10	4−10	6−10	8−10	10−8	12−8	14−8	16−8	18−8	20−8
$(6)_1$	2−12	4−12	6−12	8−12	10−12	12−10	14−10	16−10	18−10	20−10

Each operation moves the indices of the variable factor to the numbers which are in the next horizontal row beneath in Table III. The following table gives the indices of the sines which are introduced by each operation when the products of two sines are substituted for the difference of two cosines.

TABLE XII. Indices of constant factors.

	B_2. A_2.	B_4. A_4.	B_6. A_6.	B_8. A_8.	B_{10}. A_{10}.	B_{12}. A_{12}.	B_{14}. A_{14}.	B_{16}. A_{16}.	B_{18}. A_{18}.	B_{20}. A_{20}.
$(2)_1$	3. 1	3. 1	4. 2	5. 3	6. 4	7. 5	8. 6	9. 7	8. 10	10. 12
$(3)_1$	4. 2	5. 1	5. 1	6. 2	7. 3	8. 4	9. 5	10. 6	11. 7	12. 8
$(4)_1$	5. 3	6. 2	7. 1	7. 1	8. 2	9. 3	10. 4	11. 5	12. 6	13. 7
$(5)_1$	6. 4	7. 3	8. 2	9. 1	9. 1	10. 2	11. 3	12. 4	13. 5	14. 6
$(6)_1$	7. 5	8. 4	9. 3	10. 2	11. 1	11. 1	12. 2	13. 3	14. 4	15. 5
$(7)_1$	8. 6	9. 5	10. 4	11. 3	12. 2	13. 1	13. 1	14. 2	15. 3	16. 4
$(8)_1$	9. 7	10. 6	11. 5	12. 4	13. 3	14. 2	15. 1	15. 1	16. 2	17. 3
$(9)_1$	8. 10	11. 7	12. 6	13. 5	14. 4	15. 3	16. 2	17. 1	17. 1	18. 2
$(10)_1$	10. 12	12. 8	13. 7	14. 6	15. 5	16. 4	17. 3	18. 2	19. 1	19. 1
$(11)_1$	12. 14	13. 9	14. 8	15. 7	16. 6	17. 5	18. 4	19. 3	20. 2	20. 2
$(12)_1$	14. 6	14. 10	15. 9	16. 8	17. 7	18. 6	19. 5	20. 4	21. 3	21. 3

All the factors above the line which crosses the table are —, and all beneath it +.

So that, finally, for example, writing the indices of sines outside the bracket, if B_{14} and A_{14} are neglected together with every B and A which has a higher index than 14,

$$(6)_1=2^{11}1.3.1.4.2.5.3.6.4.7.5\{B_2\sin 11-A_2\cos 11\}$$

$$(6)_2=2^{11}1.3.1.4.2.5.3.6.4.7.5\{B_2\sin 13-A_2\cos 13\}$$

$$(6)_2\cos 11-(6)_1\cos 13=2^{11}.1.3.1.4.2.5.3.6.4.7.5.2.B_2$$

$$(6)_2\sin 11-(6)_1\sin 13=2^{11}.1.3.1.4.2.5.3.6.4.7.5.2\ A_2.$$

2nd Category, *uneven* indices.

2nd Operation. Elimination of B_3, A_3.

Let

$$(1)_1+(1)_3-2(1)_2\cos 3=(2)_1$$

$$(1)_1+(1)_4-2(1)_3\cos 3=(2)_2$$

$$(1)_1+(1)_5-2\ (1)_3\cos 3=(2)_2.$$

The operation by which B_3 and A_3 are eliminated is as regards B_1, A_1.

$$2\,B_1\{\cos 1+\cos 3-2\cos 2\cos 3\}+2\,A_1\{\sin 1+\sin 3-2\sin 2\cos 3\}$$
$$=2^2\,B_1\cos 2\,\{\cos 1-\cos 3\}+2^2 A_1\sin 2\,\{\cos 1-\cos 3\}.$$

The following table gives the indices for each vertical column under B_i, A_i, when the operations are conducted successively and separately so as to eliminate all except B_i, A_i.

TABLE XIII. Indices of constant factor.

B_1. A_1.	B_3. A_3.	B_5. A_5.	B_7. A_7.	B_9. A_9.	B_{11}. A_{11}.	B_{13}. A_{13}.	B_{15}. A_{15}.	B_{17}. A_{17}.	B_{19}. A_{19}.
1−3	3−1	5−1	7−1	9−1	11−1	13−1	15−1	17−1	19−1
1−5	3−5	5−3	7−3	9−3	11−3	13−3	15−3	17−3	19−3
1−7	3−7	5−7	7−5	9−5	11−5	13−5	15−5	17−5	19−5

Each operation moves the indices of the variable factor to the numbers which are in the next horizontal row beneath in Table I. The following table gives the indices of the sines which are introduced by each operation when the products of two sines are substituted for the difference of two cosines.

TABLE XIV. Indices of constant factor.

	B_1. A_1.	B_3. A_3.	B_5. A_5.	B_7. A_7.	B_9. A_9.	B_{11}. A_{11}.	B_{13}. A_{13}.	B_{15}. A_{15}.	B_{17}. A_{17}.	B_{19}. A_{19}.
$(2)_1$	2.1	2.1	3.2	4.3	5.4	6.5	7.6	8.7	9.8	10.9
$(3)_1$	3.2	4.1	4.1	5.2	6.3	7.4	8.5	9.6	10.7	11.8
$(4)_1$	4.3	5.2	6.1	6.1	7.2	8.3	9.4	10.5	11.6	12.7
$(5)_1$	5.4	6.3	7.2	8.1	8.1	9.2	10.3	11.4	12.5	13.6
$(6)_1$	6.5	7.4	8.3	9.2	10.1	10.1	11.2	12.3	13.4	14.5
$(7)_1$	7.6	8.5	9.4	10.3	11.2	12.1	12.1	13.2	14.7	15.4
$(8)_1$	8.7	9.6	10.5	11.4	12.3	13.2	14.1	14.1	15.2	16.3
$(9)_1$	9.8	10.7	11.6	12.5	13.4	14.7	15.2	16.1	16.1	17.2
$(10)_1$	10.9	11.8	12.7	13.6	14.5	15.4	16.3	17.2	18.1	18.1
$(11)_1$	11.10	12.9	13.8	14.7	15.6	16.5	17.4	18.3	19.2	20.1
$(12)_1$	13.11	13.10	14.9	15.8	16.7	17.6	18.5	19.4	20.3	21.2

All the factors above the line which crosses the table are −, and all beneath it +.

So that, finally, for example, writing the indices of sines outside the bracket if B_{13} and A_{13} are neglected together with every B and A which has a higher index than 13,

$$(6)_1 = 2^{10}\ 2.1.3.2.4.3.5.4.6.5\ \{B_1 \cos 6 + A_1 \sin 6\}$$

$$(6)_2 = 2^{10}\ 2.1.3.2.4.3.5.4.6.5\ \{B_1 \cos 7 + A_1 \sin 7\}$$

$$(6)_1 \sin 7 - (6)_2 \sin 6 = 2^{10}.2.1.3.2.4.3.5.4.6.5.1.B_1$$

$$(6)_2 \cos 6 - (6) \sin 6 = 2^{10}.2.1.3.2.4.3.5.4.6.5.1.A_1.$$

If the A's are missing, then if B_7 is neglected with every B which has a higher index than 7;

$$(3)_1 = 2^4.3.2.4.1 \cos 15\ B_5.$$

If B_9 is neglected with every B which has a higher index than 9;

$$(4)_1 = 2^6.3.2.4.1.6.1 \cos 20\ B_5.$$

If B_{11} is neglected with every B which has a higher index than 11;

$$(5)_1 = 2^8.3.2.4.1.6.1.7.2 \cos 25\ B_5.$$

The series may be separated into four categories by using the particular values of the series due to the angle 1, −1, 180°+1, 180°−1, 180°+2, 180°−2, &c.

The 1st category contains B's with *even* indices.
2nd . . . . . . B's with *uneven* indices.
3rd . . . . . . A's with *even* indices.
4th . . . . . . A's with *uneven* indices.

The expressions which are required in this case may be inferred from those which have already been given.

Let

$R_{1,1}$ denote the value of the series R due to angle 1,
$R_{1,2}$ . . . . -1,
$R_{1,3}$ . . . . $180°+1$,
$R_{1,4}$ . . . . $-180°-1$,

$$R_{1,1} = B_0 + B_1 \cos 1 + B_2 \cos 2 + \&c. + A_1 \sin 1 + A_2 \sin 2 + \&c.$$

$$R_{1,2} = B_0 + B_1 \cos 1 + B_2 \cos 2 + \&c. - A_1 \sin 1 - A_2 \sin 2 + \&c.$$

$$R_{1,3} = B_0 - B_1 \cos 1 + B_2 \cos 2 + \&c. - A_1 \sin 1 + A_2 \sin 2 + \&c.$$

$$R_{1,4} = B_0 - B_1 \cos 1 + B_2 \cos 2 + \&c. + A_1 \sin 1 - A_2 \sin 2 + \&c.$$

Let

$$\mathbf{R}_{1,1}=\frac{1}{4}(R_{1,1}+R_{1,2}+R_{1,3}+R_{1,4})$$

$$\mathbf{R}_{1,2}=\frac{1}{4}(R_{1,1}+R_{1,2}-R_{1,3}-R_{1,4})$$

$$\mathbf{R}_{1,3}=\frac{1}{4}(R_{1,1}-R_{1,2}+R_{1,3}-R_{1,4})$$

$$\mathbf{R}_{1,4}=\frac{1}{4}(R_{1,1}-R_{1,2}-R_{1,3}+R_{1,4})$$

$\mathbf{R}_{1,1}=B_0+B\cos 2+B_4\cos 4+\&c.$

$\mathbf{R}_{1,1}$ contains only B_0, B_2, B_4, &c., having *even indices.*

$\mathbf{R}_{1,2}=B_1\cos 1+B_3\cos 3+B_5\cos 5+\&c.$

$\mathbf{R}_{1,2}$ contains only B_1, B_3, B_5, &c., having *uneven indices.*

$\mathbf{R}_{1,3}=A_2\sin 2+A_4\sin 4+A_6\sin 6+\&c.$

$\mathbf{R}_{1,3}$ contains only A_2, A_4, A_6, &c., having *even indices.*

$\mathbf{R}_{1,4}=A_1\sin 1+A_3\sin 3+A_5\sin 5+\&c.$

$\mathbf{R}_{1,4}$ contains only A_1, A_3, A_5, &c., having *uneven indices.*

Similarly, if

$R_{i,1}$ denote the value of the series R due to the angle i,
$R_{i,2}$ . . . . $-i$,
$R_{i,3}$ . . . . $180^\circ+i$,
$R_{i,4}$ . . . . $-180^\circ+i$,

$$R_{i,1}=B_0+B_1\cos i+B_2\cos 2i+\&c.+A_1\sin i+A_2\sin 2i+\&c.$$

$$R_{i,2}=B_0+B_1\cos i+B_2\cos 2i+\&c.-A_1\sin i-A_2\sin 2i+\&c.$$

$$R_{i,3}=B_0-B_1\cos i+B_2\cos 2i+\&c.-A_1\sin i+A_2\sin 2i+\&c.$$

$$R_{i,4}=B_0-B_1\cos i+B_2\cos 2i+\&c.+A_1\sin i-A_2\sin 2i+\&c.$$

and if

$$\mathbf{R}_{i,1}=\frac{1}{4}(R_{i,1}+R_{i,2}+R_{i,3}+R_{i,4})$$

$$\mathbf{R}_{i,2}=\frac{1}{4}(R_{i,1}+R_{i,2}-R_{i,3}-R_{i,4})$$

$$\mathbf{R}_{i,3}=\frac{1}{4}\left(R_{i,1}-R_{i,2}+R_{i,3}-R_{i,4}\right)$$

$$\mathbf{R}_{i,4}=\frac{1}{4}\left(R_{i,1}-R_{i,2}-R_{i,3}+R_{i,4}\right)$$

$\mathbf{R}_{i,1}=B_0+B_2\cos 2i+B_4\cos 4i+\&c.$

$\mathbf{R}_{i,1}$ contains only B_0, B_2, B_4, &c., having *even indices.*

$\mathbf{R}_{i,2}=B\cos i+B_3\sin 3i+B_5\sin 5i+\&c.$

$\mathbf{R}_{i,2}$ contains only B_1, B_3, B_5, &c., having *uneven indices.*

$\mathbf{R}_{i,3}=A_2\sin 2i+A_4\sin 4i+A_6\sin 6i+\&c.$

$\mathbf{R}_{i,3}$ contains only A_2, A_4, A_6, &c., having *even indices.*

$\mathbf{R}_{i,4}=A_1\sin i+A_3\sin 3i+A_5\sin 5i+\&c.$

$\mathbf{R}_{i,4}$ contains only A_1, A_3, A_5, &c., having *uneven indices.*

I have thus endeavoured to furnish expressions for obtaining the coefficients in the development of any series of sines and cosines by eliminating successively all the coefficients of sufficient magnitude to be sensible except the one sought. I feel strongly impressed with the truth of the opinion* of Le Verrier, and I think that, if not altogether impracticable, it is at least exceedingly difficult in the case of the smaller planets and comets to obtain the coefficients in the functions which occur in the theory of the perturbations by any method which is not founded upon particular values of the function, that is either by the methods here treated, or by the method of mechanical quadratures. If the former are employed, the tables of logarithms of particular values of certain quantities which I have described elsewhere, due to particular values of the eccentric anomalies of the planets, would be useful, and would reduce somewhat the labour of calculation of the perturbations, the particular values of the eccentric anomaly being multiples of the arbitrary angle, which I have for simplicity made equal to unity; or if the method of quadratures is to be used, the particular values of the eccentric anomaly being multiples of 10°, or other part of the circumference $\frac{2\pi}{n}$, n being a whole number.

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* L'interpolation parait donc seule susceptible de fournir les coefficients correspondants à des multiples élevés des longitudes moyennes. Les calculs sont encore assurement très longs; mais ils ne sont pas impracticables comme ceux qui résultent des développements algébraiques.—*Le Verrier, Développements,* 1, p. 6.

This method of *mechanical quadratures* is also only, when applied to such series, a method of elimination, as Mr. Adams kindly pointed out to me, but carried out in a different manner.

Let

$$B_0+B_1\cos 1+B_2\cos 2+\&\text{c.}\quad +B_{n-1}\cos(n-1)$$
$$+B_n\cos n+B_{n+i}\cos(n+1)+\&\text{c.}$$

be the series in question, and let $\frac{2\pi}{n}$ be substituted for 1 in the series, it becomes

$$y_1=B_0+B_n+B_{2n}+\&\text{c.}$$
$$+\{B_1+B_{n+1}+B_{2n+2}+\&\text{c.}\}\cos\frac{2\pi}{n}$$
$$+\{B_2+B_{n+2}+B_{2n+2}+\&\text{c.}\}\cos\frac{4\pi}{n}$$
$$+\&\text{c.}$$

If

$$y_1=\text{Function}\,\frac{2\pi}{n}\text{ or }F\frac{2\pi}{n}\qquad y_2=F2\times\frac{2\pi}{n},\qquad y_i=Fi\times\frac{2\pi}{n},$$

$$\Sigma y=y_1+y_2+y_3+\;\ldots\ldots\;y_n$$
$$=n\{B_0+B_n+B_{2n}+\&\text{c.}\},$$

because all the other series vanish, and

$$B_0+B_n+B_{2n}+\&\text{c.}=\frac{1}{n}\Sigma y,$$

if the series converge and n is sufficiently large,

$$B_0=\frac{1}{n}\Sigma y.$$

In the same way it may be shown that

$$B_i+B_{n-i}+B_{n+i}+B_{2n-i}+B_{2n+i}+\&\text{c.}=\frac{2}{n}\Sigma y\cos i;$$

and if the series converge and n is sufficiently large,

$$B_i=\frac{2}{n}\Sigma y\cos i.$$

Similarly, if the series be mixed,

$$B_0=\frac{1}{n}\Sigma y,\qquad B_i=\frac{2}{n}\Sigma y\cos i,\qquad A_i=\frac{2}{n}\Sigma y\sin i\text{ nearly.}$$

Le Verrier has assumed the arbitrary angle $\alpha = 140° 56' 17''\cdot 6$, but as I do not know why he selected this angle, I have taken $\alpha = 57° 17' 44''\cdot 5$ sex., which is the angle of which arc = rad. The arbitrary angle must be incommensurable with the circumference.

TABLE I.—Showing multiples of α less even circumferences to be used in finding particular values of any function.

1.	2.	3.	4.	5.	6.
57° 17′ 44″·5	114° 35′ 29″·0	171° 53′ 13″·5	229° 10′ 58″·0	286° 28′ 42″·5	343° 46′ 27″·0
7.	8.	9.	10.	11.	12.
41 4 11·5	98 21 56·0	155 39 40·5	212 57 25·0	270 15 9·5	327 32 54·0
13.	14.	15.	16.	17.	18.
24 50 38·5	82 8 23·0	139 26 7·5	196 43 52·0	254 1 36·5	311 19 21·0
19.	20.	21.	22.	23.	24.
8 37 5·5	65 54 50·0	123 12 34·5	180 30 19·0	237 48 3·5	295 5 48·0
25.	26.	27.	28.	29.	30.
352 23 32·5	49 41 17·0	106 59 1·5	164 16 46·0	221 34 30·5	278 52 15·0

TABLE II.—Showing the logarithms of the sines and cosines of multiples of α to be used in finding particular values of any function.

	1.	2.	3.	4.	5.	6.
sin	9·9250387	9·9587066	9·1496023	−9·8789804	−9·9817852	−9·4462637
cos	9·7326378	−9·6192438	−9·9956316	−9·8153440	9·4527906	9·9823472
	7.	8.	9.	10.	11.	12.
sin	9·8175513	9·9953543	9·6150350	−9·7356058	−9·9999957	−9·7296409
cos	9·8773190	−9·1628281	−9·9595779	−9·9238032	7·6443760	9·9262624
	13.	14.	15.	16.	17.	18.
sin	9·6234039	9·9959003	9·8131169	−9·4592123	−9·9828999	−9·8756428
cos	9·9578251	9·1359521	−9·8806269	−9·9812143	−9·4396289	9·8197391
	19.	20.	21.	22.	23.	24.
sin	9·1756547	9·9604390	9·9225555	−7·9454019	−9·9274741	−9·9569332
cos	9·9950684	9·6107764	−9·7385452	−9·9999831	−9·7266147	9·6275161
	25.	26.	27.	28.	29.	30.
sin	−9·1218504	9·8822589	9·9806339	9·4328825	−9·8219074	−9·9947738
cos	9·9961603	9·8108699	−9·4655323	−9·9834434	−9·8739516	9·1881054

Calculation of B_5 or coefficient of $\cos 5\alpha$ in the development of $\{1-{\cdot}5\cos\alpha\}^{-\frac{3}{2}}$, by the method of p. 10.

$R_1 =$	1·60379	$R_2 =$	·75313	$R_3 =$	·54707	$R_4 =$	·65431
$R'_1 =$	·69858	$R'_2 =$	1·41896	$R'_3 =$	2·78644	$R'_4 =$	1·81052
$(1)_1 =$	·45260	$(1)_2 =$	− ·33292	$(1)_3 =$	−1·11968	$(1)_4 =$	− ·57810
$R_5 =$	1·25786	$R_6 =$	2·66744	$R_7 =$	2·03336	$R_8 =$	·90003
$R'_5 =$	·81960	$R'_6 =$	·55535	$R'_7 =$	·61890	$R'_8 =$	1·11995
$(1)_5 =$	·21913	$(1)_6 =$	1·05604	$(1)_7 =$	·70923	$(1)_8 =$	− ·10996
$R_9 =$	·56944	$(2)_1 =$	·30734	$(2)_2 =$	·29890	$(2)_3 =$	− ·27585
$R'_9 =$	2·48929	$(2)_4 =$	·24114	$(2)_5 =$	− ·21480	$(2)_6 =$	·18184
$(1)_9 =$	− ·95992	$(2)_7 =$	− ·13387				
$(3)_1 =$	·00863	$(3)_2 =$	− ·00612	$(3)_3 =$	− ·01321	$(3)_4 =$	− ·00232
						$(3)_5 =$	·01137
$(4)_1 =$	·00464	$(4)_2 =$	·01148	$(4)_3 =$	·00166	$(5)_1 =$	·02720

If B_7 is neglected with every B which has a higher index than 7;

$(3)_1=2^4.3$[*]$.2.4.1\cos 15\,B_5$ $B_5={\cdot}00869$ 1st approximation.

If B_9 is neglected with every B which has a higher index than 9;

$(4)_1=2^6.3.2.4.1.6.1\cos 20\,B_5$ $B_5={\cdot}00924$ 2nd approximation.

If B_{11} is neglected with every B which has a higher index than 11;

$(5)_1=2^6.3.2.4.1.6.1.7.2\cos 25\,B_5$ $B_5={\cdot}00934$ 3rd approximation.

The correct value of B_5 is ·0093675.

Calculation of the same coefficient by *mechanical quadratures.*

	$y\times\cos 5\alpha$	
2·82843		$\alpha=0$
	− ·44985	$\alpha=20$
	1·93901	$\alpha=40$
·76980		$\alpha=60$
·87785		$\alpha=80$
	·67611	$\alpha=100$
	·35777	$\alpha=120$
·57775		$\alpha=140$
·09745		$\alpha=160$
	·54433	$\alpha=180$
·09745		$\alpha=200$
·57775		$\alpha=220$
	·35777	$\alpha=240$
	·67611	$\alpha=260$
·87785		$\alpha=280$
·76980		$\alpha=300$
	1·93901	$\alpha=320$
	·44985	$\alpha=340$
7·47413	7·38981	
−7·38981		
9) ·08432		
·009368		

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[*] 3 is written here for sin 3, 2 for sin 2, &c.

XX. *On the existence of Lactic Acid in Living Bodies.*
By M. BERZELIUS*.

M. LIEBIG addressed a letter to the Academy of Sciences of Paris, which was read on the 17th of January 1847, in which he said, "It has been long known that the flesh of fresh-killed animals possesses, very appreciably, the reaction of a free acid. M. Berzelius has attributed this reaction to the presence of lactic acid, but without the fact having been hitherto ascertained by analytic results in an unquestionable manner. Many chemists have admitted that lactic acid exists in urine, the gastric juice, and in milk; but they have supported this opinion by uncertain reactions. The opinion, even, that lactic acid prevents the precipitation of oxide of copper by milk of lime is founded on error. M. Strecker has lately shown that pure lactate of copper is entirely decomposed by milk of lime, so completely that no reagent whatever indicates a trace of oxide of copper in the liquor after precipitation. It is true that pure lactate of lime dissolves a little oxide of copper, but a slight excess of lime precipitates it again. The intention of my experiments was to dispel the uncertainty which enveloped the non-volatile organic acid contained in the fluids of living bodies †."

M. Liebig afterwards announces, in the same letter, of which there will be occasion again to speak, that he has found this acid is the lactic acid. After having thus endeavoured to annihilate the labours of his predecessors on this acid which exists in the animal organization, and even the experiments on lactic acid by M. Pelouze, it is then to M. Liebig that science owes a debt of gratitude for having taught us that living bodies contain lactic acid.

I have myself too great a share in the discovery of lactic acid in living bodies, not to feel particular interest that the history of lactic acid in the animal organization should be stated with perfect truth.

In 1807 ‡ I analysed the flesh of animals recently killed, and I found lactic acid in the fluids which I extracted from it. The question as to the nature of this acid was to be determined by experiment. The fluid, coagulated by heat and filtered, was evaporated to the consistence of a thick syrup, which was treated with alcohol. The alcoholic solution was mixed with dilute sulphuric acid as long as a sulphate was precipitated. The precipitate was separated by the filter and the liquor was

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* Translated from Berzelius's *Annuaire*, 1848, p. 347.

† *L'Institut*, No. 683, p. 38.

‡ *Førelæsningar, &c.*, Lectures on Animal Chemistry, ii. 172.

digested with carbonate of lead, till it contained oxide of lead in solution. After having separated the sulphate and chloride of lead by filtration, the fluid was treated with sulphuretted hydrogen and evaporated: it was strongly acid, but yellow. Supposing it might contain foreign matters, it was again treated with water and digested with oxide of lead, which swelled and produced a bulky basic salt. After this the acid was again evaporated. It deposited no crystals, but remained in the form of an acid syrup, which I did not succeed in rendering colourless. I prepared some salts, and found that it produced, with lime and magnesia, granular crystalline masses. To satisfy myself as to the nature of the acid which I had obtained, I compared it with other acids known at this period; and I found that it most strongly resembled the acid found by Scheele in sour milk, and this I prepared on the occasion. I convinced myself of their perfect identity, and that the latter could not be better prepared in a state of perfect purity than that from flesh. I then asserted that the acid was lactic acid, precisely as M. Liebig now asserts, forty years afterwards. It appeared evident to me that lactic acid in the animal body ought to be considered as a product resulting from the use of the elements of flesh,—a product which circulated in the system, and which consequently would be found in the blood, and be finally evacuated with the urine. In my analysis of blood, which I undertook immediately afterwards, I also discovered the presence of lactic acid by an analogous process; but as the quantity which is found in it is but very small, I had some difficulty at first in acquiring complete certainty of its identity. The successive analyses which I performed, by degrees, of fresh milk, urine, tears, saliva, bile, &c., all furnished me with lactic acid: the alkaline fluids in minute quantity, the acid ones, such as urine, milk and sweat, in larger quantity.

Confidence was generally placed in my statements, until L. Gmelin and Tiedemann in 1826, in their excellent work on the act of digestion, asserted that this acid was the acetic. It could not have been difficult to distinguish a volatile acid from an acid which is not so; but M. Gmelin thought that the acetic acid lost its volatility by combination with animal matter. This property, described by this chemist, obtained confidence; and in all works which were published after this date, the acid in question was generally considered as acetic acid.

This event occasioned me to undertake a new series of researches on lactic acid, which was published in my Treatise on Chemistry, vol. iv. p. 577-585 (*Dresden*, 1831), in which I demonstrated that it was impossible to confound lactic with acetic acid, unless it be supposed that it is with respect to acetic acid, what sulphovinic acid is with respect to sulphuric acid; and that even on this supposition it ought to be considered as a peculiar acid.

I then employed tartaric acid to precipitate the alkali; and after having separated, by means of carbonate of lead, all the acids contained in the alcoholic solution, except the lactic acid, I precipitated the lead by sulphuretted hydrogen. The alcohol having been evaporated, and the solution decolorized by pure animal charcoal, I precipitated the lactic acid from it by digesting in it an excess of hydrate of tin. After having washed the precipitate and decomposed it in water by sulphuretted hydrogen, I obtained the acid in a much greater degree of purity.

The first article of the first volume of M. Liebig's *Annalen der Pharmacie*, 1832, is an extract from the above memoir. M. Mitscherlich discovered afterwards, in 1834, that the lactate of zinc is a slightly soluble salt, which crystallizes readily, and from which lactic acid may be obtained in a state of perfect purity.

During these discussions on lactic acid, which continued for several years, a scientific renown was acquired, founded on numerous brilliant discoveries, which was not slow in indicating that it would attain celebrity,—it was that of M. Liebig. In 1842 M. Liebig* openly proclaimed that he combined in his person so great an extent of knowledge acquired by innumerable experiments and practical results, that no person could in future, probably, amass so much (wie sie sich vielleicht nie in einem Individuum wieder vereinigen dürften); and he undertook to initiate the human race in the chemical phænomena of living nature. This essay was entirely premature; science did not then, nor does it yet possess, sufficient positive knowledge to lead to results worthy of reliance.

Since 1821 I have been called upon, in consequence of the duty which devolves upon me on account of the station which I occupy in the Academy of Sciences at Stockholm, to make an annual report respecting all the branches of chemistry, and I considered it as my duty to use every effort to exercise a sound and rigorous judgement. I have shown that in this essay M. Liebig stated probabilities, and often even some things which could not be so estimated, as demonstrated and incontestable truths. I therefore put myself in opposition to this unrestrained chemical authority. Considering the height to which M. Liebig thought himself raised, an apology would have been superfluous; any one who had the audacity to dare

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* *Ann. der Chem. und Pharm.*, xli. 373.

to make an objection was considered as guilty; and the punishment consisted in entirely censuring his labours on other subjects, of which the school of Giessen endeavoured to nullify the results, in order to diminish the reputation of the author. This is the motive which determined M. Liebig to cause several of my labours to be refuted by his pupils in the laboratory of Giessen.

The existence of lactic acid in the animal fluids was one of these labours. The refutation of it was committed to M. Enderlin, who stated formally, that it was on the invitation of M. Liebig that he had undertaken it. The following are the terms in which he discloses the fact that he had been called to demonstrate by his experiments[*]: "It is absolutely impossible to admit of the existence of lactic acid in the bodies of these animals (*Carnivora*), *for this acid has not yet been found*, and their food does not contain any substance which can give rise to it[†]."

It is evident that the results of his experiments were to agree with the commission which he had undertaken. He demonstrated that he found none. Up to this point the master might, apparently at least, defend himself, if the result should be found to be inexact, by saying that the pupil had committed a mistake; but he appears to have thought that M. Enderlin's process was not sufficiently decisive, and he undertook the demonstration himself. M. Liebig has declared[‡] that the experiments of M. Enderlin had proved that no animal fluid contained lactic acid, that he had himself sought to confirm and corroborate this result by the analysis of fresh and putrified urine, and that he had not succeeded in finding it. He took all possible pains to show to what extent my experiments were unsatisfactory, and he expresses himself in this respect as follows[§]: "Wenn man die von Berzelius angestellten Versuche,

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[*] *Ann. der Chem. und Pharm.*, xlvi. 166.

[†] "Es ist durchaus unmöglich das Vorhandensein der Milchsäure in dem Körper dieser Thiere (Carnivora) vorauszusetzen, *da sie bis jetzt noch nicht darin gefunden worden ist*, und die Nahrung keine Substanz enthält aus der sie enstehen könnte."

[‡] *Ann. der Chem. und Pharm.*, l. 163.

[§] "When the experiments by which Berzelius has ascertained the presence of lactic acid in urine are closely examined, we arrive at the conclusion that no one of them offers a proof that this acid is a constituent of fresh urine."

It is not uninteresting to give here, with respect to these data, the terms employed by M. Liebig on the 21st December, 1846, in announcing to the Chemical Society of London that he had discovered lactic acid in living bodies: "After overcoming more difficulties than I have ever experienced in any investigation, I have for the first time indisputably proved that free lactic and phosphoric acid exist in the whole organism wherever muscle is aus denen er die Gegenwart der Milchsäure im Harn erschlossen hat, näher beachtet, so ergiebt sich aus keinem derselben ein Beweis, dass sie einen Bestandtheil des frischen Harns ausmacht."

In the two papers of MM. Enderlin and Liebig which have been cited, lactic acid was merely a pretext. All the experiments were directed to the purpose of not finding lactic acid; and the principal intention was evidently that of showing that the data which I had published were inexact. Did they wish simply to revenge themselves for the opposition which I offered; or did they hope that by treating one of my labours in this way, that I should be induced to make a kind of confession in order to avoid the censure of the Giessen school with respect to my other labours? I shall not decide this question.

M. Liebig attained his end, that of causing it to be believed for some years that living bodies contained no lactic acid. And who could have imagined that this scientific *farce* would have terminated by M. Liebig himself discovering that lactic acid exists in the animal fluids, when I had proved it forty years before him?

M. Pelouze had indicated a reaction, by means of oxide of copper and hydrate of lime, by which the presence of lactic acid might be discovered. M. Boussingault employed this reaction to prove that an acid which he found in urine was lactic acid. But for the purpose of also destroying this reaction, before having ascertained the presence of lactic acid, M. Liebig caused it to be proved by M. Schlieper, one of his pupils in the Giessen laboratory, that M. Pelouze had deceived himself, and that his method was inapplicable. I have satisfied myself that the reaction described by M. Pelouze for the detection of lactic acid is perfectly correct; but I have at the same time found, that if proper precautions be not adopted, the end may be completely missed.

After M. Liebig was convinced by his own experiments that living bodies contain lactic acid, he did not endeavour to excuse the errors which he and his pupils had committed respecting the existence of this acid. Instead of this, he did all that he could to reduce the labours of his predecessors to mere attempts of insufficient reactions and unfounded hypotheses, in order to appropriate the whole of the discovery to himself. This mode of acting is unprecedented in the annals of science. M. Liebig has endeavoured not to be surpassed in this respect.

found. How curious, that in the absence of all proofs on the part of the opponents of lactic acid, I should now demonstrate to them its existence in the flesh of oxen, fowls, calves and sheep."—*Phil. Mag.*, vol. xxx. 1807, p. 412.

In former annual reports I have mentioned the attempts made by M. Liebig to annul the results at which I had arrived with regard to lactic acid by long and laborious researches, submitted to frequent fresh examinations. On these different occasions no expression of discontent escaped me, for I never felt it; and the less so, because I was thoroughly convinced that what is true remains so, although sometimes for a certain period success may attend attempts to prove that it is not so. The reasons for which I have now entered upon the question in a more serious point of view, are, on one hand, the circumstances which have occurred in stating the question by M. Liebig's manner of proceeding, and on the other hand, the position taken up by M. Mulder, for an analogous cause, against M. Liebig. After M. Mulder felt himself compelled to enter the lists against the adherent of peace in science, it would be pusillanimous to leave him alone in this noble combat, in which his arms were an ardent desire for the right and for truth,—arms which all those ought to bear seriously who cultivate science, and which they ought not to put off till respected by those against whom they were employed. It is my conviction that it is the duty of all true friends of science to unite their efforts for the extirpation of the root of this moral tare, which has begun to invade and domineer in the peaceful domain of science.

XXI. *On Colouring Matters.* *By* Dr. Schunck[*].

At the meeting of the British Association at Southampton I gave a short account of my experiments on the colouring matters of madder[†]. I have continued this investigation, and have found the extent of the subject too great to allow me to devote my attention to any other of the colouring matters. I shall therefore, without any further preface, state the new results which I have arrived at in regard to the chemical constituents of this root.

On treating finely-ground madder roots with boiling water, a brown fluid is obtained having a taste between bitter and sweet. In order to extract all the substances capable of solution in water, about sixteen quarts of water are required for every pound of madder. To this fluid any strong acid, such as sulphuric or muriatic acid, is added in slight excess. Nitric acid must not be used for the purpose. Oxalic acid is best adapted for the purpose, as it can afterwards be completely removed by chalk. The acid produces a dark brown precipitate, which is separated by filtration and washed with water until the excess of acid is removed. The percolating fluid is yellow. This brown precipitate consists of six vegetable substances, viz. two

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[*] From the Report of British Association for 1847.

[†] Phil. Mag. vol. xxxi. p. 46.

colouring matters, two kinds of fat, pectic acid and a substance of an intensely bitter taste, which I am as yet unable to refer to any known class of bodies. The whole quantity of colouring matter contained in the aqueous extract of madder is precipitated by the addition of a strong acid. In proof of this I took a quantity of the aqueous extract, added sulphuric acid, separated the brown precipitate by filtration and removed the excess of acid with cold water, and then boiled it with water, into which a small piece of mordanted cloth was introduced. The cloth assumed the same colours which it would have done with madder. To the fluid to which acid had been added I put chalk until all the acid was saturated, and I then found after filtering that it communicated no colour whatever to mordanted cloth.

Of the two colouring matters, one is Robiquet's *Alizarin*, and the other I shall call *Rubiacin*. Both are contained in the brown precipitate produced by acids in the aqueous extract of madder. In order to obtain them, this brown precipitate is first treated with boiling alcohol until nothing more is dissolved. A dark brownish-purple, somewhat gelatinous mass is left behind, consisting principally of pectic acid. The alcoholic fluid, which contains the two colouring matters together with the fats, has a dark brownish-yellow colour. After being distilled and then evaporated to dryness, a residue of a dirty orange colour remains. This residue is placed on a filter and washed with cold water, until the percolating fluid, which is at first yellow, becomes colourless. On evaporation this fluid leaves a transparent yellow substance with a bitter taste, mentioned above. This substance is soluble in pure water, but insoluble in water containing acids, and hence it is precipitated on adding acid to the aqueous extract of madder; but on washing the precipitate with water, after the acid has been removed it begins to dissolve. In order to obtain it, therefore, the brown precipitate must only be washed so long as it still contains free acid. I shall call this substance *Rubian*. The mass left undissolved by cold water is then treated with boiling water, and the fluid is filtered boiling hot. On cooling it deposits a quantity of red flocks, which consist of alizarin mixed with fat. This process must be repeated until the boiling fluid deposits nothing more on cooling. If any rubian be still left in the mass on treating with boiling water, the filtration of the boiling fluid is very much impeded, and it is therefore advisable to remove this substance completely with cold water previously to treating with hot water. The whole of the alizarin dissolved in the boiling water is deposited on cooling, but mingled with fat, which probably accompanies it through the filter in a state of suspension produced by the heat of the boiling fluid. This fat disguises its properties very much, and has been the cause, in all previous investigations, of alizarin never having been obtained in a state of purity except through the agency of heat, which has always left it doubtful whether it existed in the plant as such, or was formed by the action of heat from some other substance. I hope to establish satisfactorily its existence as a constituent of the madder root, and also the fact of its being a pure colouring matter, a circumstance which has likewise been doubted. In order to obtain it in a state of purity, the red flocks which are deposited by the boiling watery solution are separated by filtration and dissolved in boiling alcohol. To the boiling solution, which has a brownish-yellow colour, hydrate of alumina is added, and the boiling is continued for some time. The alizarin combines with the alumina, forming a dark-red compound, while the fluid loses its colour. Fresh alumina is added, until no more colouring matter can be thereby separated. A great part of the fat remains dissolved in the alcohol, while a part combines with the alumina. The coloured alumina is separated by filtration and washed with alcohol for some time. It is then treated with a weak boiling solution of caustic potash, which dissolves the excess of alumina and all the fat which may have combined with the alumina, but leaves the compound of alizarin and alumina undissolved, merely changing its colour from red to dark purple. This process is repeated several times, until the alkaline fluid is no longer red, but of a pure purple colour. The residue is treated with muriatic acid, which dissolves the alumina and leaves the alizarin behind in crystalline flocks of an orange colour, which are washed with water to remove the acid and then dissolved in alcohol. The alcoholic solution on evaporation gives shining, prismatic, orange-coloured crystals of alizarin, which may, if necessary, be purified by a second crystallization from alcohol.

The mass left undissolved by boiling water consists of rubiacin and two distinct kinds of fat. I have only been able to discover one method of extracting the rubiacin from this mixture. This method is founded on the solubility of rubiacin in perchloride and pernitrate of iron. It is immaterial which of these two salts be taken. Persulphate of iron would not answer the purpose. If the mixture of rubiacin with the two fats be treated with a somewhat concentrated boiling solution of perchloride or pernitrate of iron, a solution of a deep reddish-brown colour is obtained, while a brown residue remains insoluble in an excess of the iron salt solution, consisting of one of the two fats in combination with oxide of iron. The fluid is filtered, and on the addition to it of muriatic acid, a yellow flocculent precipitate is obtained, which is separated by filtration and washed until all the iron salt and the excess of acid are removed. This precipitate consists of rubiacin, the second of the two fats, and of a new body which I shall call *rubiacic acid.* The latter substance does not exist ready-formed in madder, but is produced by the action of the persalt of iron on rubiacin. This action consists in the rubiacin taking up a certain number of atoms of oxygen from the persalt of iron, and the acid which is thus formed combines with peroxide of iron, producing a compound soluble in water with a reddish-brown colour, and decomposable by any strong acid. Part of the rubiacin however escapes this action, and is precipitated together with the rubiacic acid and fat on the addition of muriatic acid. The precipitate is treated with boiling alcohol, which dissolves the rubiacin and the fat, and leaves behind the rubiacic acid in the shape of a yellow powder. This process is repeated until nothing more is dissolved by the alcohol. The boiling alcohol, after being filtered, deposits on cooling rubiacin in small shining yellow crystals, which must be purified by recrystallization. The fluid has a yellowish-red colour. On evaporation it gives a quantity of an easily fusible dark reddish-brown fat, which by repeated fusion in boiling water and agitation with the fluid while in a melted state, may be separated from the rubiacin which adheres to it. The rubiacic acid which is left by the boiling alcohol is treated with a weak boiling solution of carbonate of potash, in which it dissolves with a dark brown colour. The solution after cooling and standing some time deposits a mass of brick-red needle-shaped crystals of rubiacate of potash, which are easily purified by recrystallization. By dissolving this salt in boiling water and adding any strong acid, rubiacic acid in a state of purity is precipitated as a bright lemon-yellow powder. The dark solution from which the rubiacate of potash has crystallized contains a quantity of the brown fat dissolved in the potash.

The brown substance left undissolved by the perchloride or pernitrate of iron is, as I stated above, a compound of one of the two fats with oxide of iron. On being treated with muriatic acid the oxide of iron is dissolved, leaving behind the fat, which, after being washed with water, is dissolved in boiling alcohol. From this it is deposited on cooling as a light brown powder.

I mentioned above that the residue which is left on treating the brown precipitate with boiling alcohol, consists principally of pectic acid. It is treated with boiling water until nothing more is dissolved. The solution, which has a light brown colour, and is somewhat mucilaginous, gives a brown, transparent substance, which easily separates in scales from the sides and bottom of the vessel. After treating the residue with water until nothing more is dissolved, there still remains a dark brown substance undissolved, which I suppose to be a compound of some of the preceding substances with oxide of iron, as it leaves a considerable quantity of ash, consisting of peroxide of iron, on being burnt.

To the aqueous extract of madder, in which the brown precipitate had been produced by oxalic acid, I added chalk until all the excess of acid was saturated, and after filtration I evaporated it. It left a dark brown syrup, which may be separated by means of alcohol into two substances. That which is dissolved seems to be grape-sugar, and that which is left behind is a species of extractive matter, which agrees in its properties with *xanthin*, as described by Kuhlmann. It has a brown colour. During evaporation in contact with the air it deposits a brown substance or apothegma, like all extractive matters. But it is distinguished from all other extractive matters which I know, by being converted into a green insoluble substance when its solution is boiled with dilute sulphuric or muriatic acid. I have not examined it further, but I may mention that it is probably this body which exerts such a deleterious influence during the process of madder-dyeing, rendering the red and purple brown and unsightly.

I shall now proceed to give a more detailed account of the properties of some of the substances, the methods of obtaining which I have just described.

Alizarin.—It is necessary to state that the substance which I called alizarin in the paper read by me on this subject at the Southampton meeting of the British Association, I now call rubiacin. I am of opinion that the substance discovered by Robiquet, and called by him alizarin, does not agree in its properties with that which I call rubiacin; but after carefully comparing his account with the results obtained by myself, I have come to the conclusion that the substance to which I have applied the name of alizarin is identical with his.

Alizarin has the following properties:—When heated on platinum foil it melts and burns with a bright flame. When heated in a glass tube closed at one end it melts and gives yellow fumes, which condense on the colder parts of the glass, forming an oil, which soon congeals to a mass of orange-coloured crystals possessing considerable lustre, which are unchanged alizarin. A carbonaceous residue is usually left in this case, but I have no doubt that by carefully heating it might be entirely volatilized. Alizarin is slightly soluble in boiling water. The solution has a yellow colour, but so small is the quantity dissolved, that the alkali or earth usually contained in filtering paper is sufficient to render the solution pink, on being filtered to separate it from the excess of alizarin. Hence no doubt arises the statement found in books, that alizarin dissolves in water with a pink colour. The boiling solution deposits it on cooling in yellow crystalline flocks. It is soluble in boiling alcohol. The solution has a deep yellow colour and deposits nothing on cooling, but on evaporation the alizarin is left in long needle-shaped or prismatic orange-coloured crystals possessing a considerable lustre. The colour of the crystals very much resembles that of bichromate of potash. Concentrated sulphuric acid dissolves alizarin in the cold with a blood-red colour. It is precipitated from this solution by water in flocks of a dull orange colour. Dilute nitric acid decomposes it on boiling with an evolution of nitrous acid. If it is still mixed with fat, then the fat remains behind after the alizarin has been decomposed and dissolved by the nitric acid. It is not affected by muriatic or acetic acid. On passing chlorine into water in which alizarin is suspended, the colour of the latter is changed to yellow, but it is seemingly not destroyed. It is decomposed by bichromate of potash and sulphuric acid. A boiling solution of perchloride or pernitrate of iron decomposes it with a copious evolution of gas, a pungent smell resembling that of aldehyde being at the same time given off. The product of this decomposition I shall describe presently. Chloride of gold is not reduced by it on boiling, but on the addition of caustic potash metallic gold is deposited in shining scales. It is soluble in caustic and carbonated alkalies with a splendid purple colour, and is reprecipitated by acids in flocks of a dull orange colour. The solution in ammonia gives with the chlorides of barium and calcium precipitates of a splendid purple colour, with sugar of lead a light purple precipitate. The compound with alumina, produced by introducing hydrate of alumina into an alcoholic solution of alizarin, is not decomposed by a concentrated solution of caustic potash. The alcoholic solution gives with acetate of iron a dark purple precipitate, with acetate of copper a light purple precipitate, with protochloride of tin no precipitate, except on the addition of ammonia, when a light red precipitate is produced. If alizarin and a piece of mordanted cloth be introduced into boiling water and the boiling be continued for some time, the cloth becomes slowly dyed and the mordants assume the tints peculiar to the so-called madder colours. The alizarin slowly disappears in the same measure as the cloth becomes dyed, even though less water had been taken than was sufficient to dissolve the whole quantity. It is evident therefore that in the process of dyeing, the alizarin, which is dissolved in the first instance by the boiling water, is taken up by the mordants of the cloth, that then a fresh quantity is dissolved by the water, which is again absorbed, and so on, until all the alizarin has combined with the cloth, or until the mordants can take up no more. Hence the slowness with which madder dyeing is effected. No doubt can remain then, I think, that alizarin plays a great part in the production of madder colours. That it does not produce the whole effect in dyeing with madder I shall show afterwards.

The method of obtaining alizarin in a state of purity I have discovered so lately, that I have not yet been able to ascertain its composition and atomic weight.

Alizaric acid.—At one period of my investigation I imagined that alizarin and rubiacin might be separated by boiling the mixture with perchloride of iron, in which case I expected the rubiacin to dissolve in the solution of the iron salt and the alizarin to remain behind in combination with oxide of iron. I was not aware at that time that alizarin is decomposed by perchloride of iron. Before I had discovered this, however, I had taken about one hundred weight of madder, treated it with boiling water, added acid to the fluid, separated the brown precipitate by filtration, and treated the whole quantity of precipitate with perchloride of iron. The fluid was, after the addition of acid and filtration, evaporated to a syrup. After allowing the syrup to stand for some days, I found it filled with white crystals bearing a resemblance to oxalic acid. I added water to the syrup, filtered and washed until the perchloride of iron was removed. I found that the crystals bore washing with cold water without dissolving in any considerable degree. I then dissolved them in hot caustic alkali, filtered to separate some oxide of iron which remained behind, and added sulphuric acid to the fluid while still hot. On the fluid cooling there separated a quantity of long shining white crystals, which were separated by filtration and washed. I now found that they contained no oxalic acid, but that they bore a striking resemblance to benzoic acid. Indeed some of the reactions are the same as those of benzoic acid, and it was only after having submitted it to an elementary analysis that I became convinced that it was not benzoic acid. I shall call it *alizaric acid.* It has the following properties:—

Its taste is acid. When heated on platinum foil it melts and burns with a smoky flame, leaving no residue. When heated in a tube it is completely volatilized, without leaving the least residue. The vapours condense in the colder parts of the tube, forming long white needles. This sublimate differs however in composition from the acid itself, and I shall therefore call it *pyroalizaric acid.* When heated in a tube with caustic lime it is decomposed and a yellow oil distils over, having a smell very nearly resembling that of benzin. It is soluble in boiling water; the solution has an acid taste and reddens litmus paper. A concentrated boiling solution crystallizes on cooling. It is easily soluble in alcohol. Concentrated sulphuric acid dissolves it in the cold. On boiling the solution no blackening takes place, and the alizaric acid distils over and crystallizes on the cooler parts of the tube. It is easily soluble in alkalies. A concentrated alkaline solution gives crystals on the addition of a strong acid. The solution in ammonia gives no precipitates with the chlorides of barium and calcium. If to the solution in water chalk be added until all effervescence has ceased, the solution gives a crystallized lime salt on evaporation. This lime salt is decomposed on being strongly heated in a tube, becomes black, and gives an oil with an aromatic odour which on cooling crystallizes. The aqueous solution gives with perchloride of iron a yellowish-white precipitate, with sugar of lead a white precipitate, with nitrate of silver alone no precipitate, but on the addition of ammonia a white curdy precipitate, which after some time becomes crystalline. The reaction with sugar of lead distinguishes it from benzoic acid, which is not thereby precipitated. Its greater solubility in water and its acid taste are also distinguishing characteristics; but in most respects it will be seen that the resemblance between the two is very striking. It is very probable that this acid is also formed from alizarin by means of nitric acid, but the small quantity of pure alizarin hitherto at my disposal has prevented me from ascertaining whether this is the case.

From some analyses which I have made of this acid, I infer that its composition is expressed by the formula $C_{14} H_5 O_7$, and that the formula for pyro-alizaric acid is $C_{14} H_3 O_5$, in which case alizaric acid contains one equivalent of hydrogen less and three equivalents of oxygen more than benzoic acid, which is $C_{14} H_6 O_4$.

Rubiacin.—This is the substance which I called alizarin in the paper which I read on this subject last year. I have since discovered that it is not identical with Robiquet's alizarin, and have therefore given it a new name. Rubiacin is most easily obtained in a state of purity from rubiacic acid. Rubiacin and rubiacic acid are mutually convertible, as they only differ from one another by a certain number of atoms of oxygen. Rubiacin is converted into rubiacic acid by the action of the persalts of iron, and rubiacic acid may be reconverted into rubiacin by reducing agents, such as sulphuretted hydrogen. In order then to obtain pure rubiacin, it is best to take a solution of pure rubiacate of potash, to which a slight excess of caustic potash has been added, and to pass sulphuretted hydrogen gas through the solution for some time. Chloride of barium is then added, which produces a dark purple precipitate, consisting of rubiacin in combination with baryta. This is separated by filtration, and washed with cold water until the sulphuret of barium and hyposulphite of baryta are removed, and then decomposed with muriatic acid, which leaves the rubiacin behind. After being washed with water, it is dissolved in boiling alcohol, from which it crystallizes on the solution cooling. It is obtained by these means in beautiful yellow crystalline plates and needles, having a strong lustre. The colour is a little darker than that of neutral chromate of potash, and inclines more to green than to red. If there is a tinge of red in it, it is impure, and must be recrystallized. It has the following properties:—

It is a little soluble in boiling water, to which it communicates a reddish-yellow tinge. The solution deposits it on cooling in shining yellow scales. The alcoholic solution has a pure yellow colour, without a tinge of red. It is precipitated from its alcoholic solution by water in yellow flocks. When heated on platinum foil it melts, and burns with a smoky flame, leaving no fixed residue. When heated in a tube closed at one end, it melts and gives yellow fumes, which condense in the colder parts of the tube in yellow crystals. If it be heated gradually at a moderate temperature, it may be almost entirely volatilized, leaving only a film of charcoal. Concentrated sulphuric acid dissolves it with a yellow colour, and water precipitates it again in yellow flocks. The solution in sulphuric acid may be heated to the boiling-point without becoming in the least degree black, and water still precipitates it. Concentrated nitric acid slowly decomposes it on boiling with an evolution of nitrous acid. Dilute nitric acid dissolves it with a yellow colour on boiling, and on the solution cooling, it crystallizes out again in shining needles. It appears therefore that nitric acid does not convert it into rubiacic acid. Boiling acetic acid dissolves it and deposits it again on cooling in yellow flocks. It is not affected by chlorine. It dissolves in a concentrated solution of perchloride or pernitrate of iron, forming a dark brownish-red solution, which on the addition of acid becomes yellow, and deposits yellow flocks. These flocks are no longer unchanged rubiacin, but rubiacic acid. It is a remarkable circumstance, that though rubiacic acid differs from rubiacin merely in containing a few atoms more oxygen, it is not formed from the latter by the action of nitric acid either dilute or concentrated, but is easily produced by the agency of the persalts of iron, which seems to indicate that something besides the influence of oxygen is brought into play during the process. Rubiacin is soluble in carbonate of potash with a blood-red colour; the solution deposits after some time red flocks. It dissolves in caustic alkalies with a reddish-purple colour, and is reprecipitated by acids in yellow flocks. The solution in ammonia gives with the chlorides of calcium and barium dirty red precipitates. The alcoholic solution gives with an alcoholic solution of sugar of lead a dark red precipitate. If into the boiling alcoholic solution hydrate of alumina be introduced, the alumina acquires an orange colour, and the fluid is deprived of all its rubiacin. The compound with alumina thus formed dissolves entirely in caustic potash, forming a purple solution, a characteristic which distinguishes it from alizarin, the alumina compound of which, as mentioned above, is insoluble in caustic potash.

The aqueous and alcoholic solutions of rubiacin communicate a slight tinge to mordanted cloth, the latter more than the former. It seems to be incapable itself of dyeing, but in conjunction with alizarin it assists in the process of madder-dyeing in a very essential manner, as I shall afterwards show.

Rubiacic Acid.—I have before described the method of obtaining this acid through the instrumentality of the persalts of iron. There is no reason for supposing that it exists as such in the plant. It is doubtless a product of the oxidation of rubiacin. To obtain it in a state of purity, pure crystallized rubiacate of potash is dissolved in water, and muriatic acid or any strong acid is added, which precipitates the rubiacic acid in the shape of a yellow powder. It has a pure lemon-yellow colour. It cannot be obtained in a crystalline form. It is slightly soluble in boiling water, to which it imparts a yellow colour. It is also slightly soluble in boiling alcohol with a yellow colour. The solution deposits nothing on cooling, but water produces in it an iridescent appearance, caused by the deposition of minute crystals. When heated on platinum foil it melts and burns with a bright flame, leaving no residue. When heated in a tube it melts, and gives fumes which condense on the colder parts of the tube to an oil which solidifies, but does not crystallize. Concentrated sulphuric acid dissolves it with a yellow colour, and water precipitates it again in yellow flocks. On heating the solution in sulphuric acid it becomes darker, but no gas is given off; some decomposition seems however to have taken place in consequence of the heating, for water now produces no precipitate. Concentrated nitric acid dissolves it on boiling, and slowly decomposes it with an evolution of nitrous acid. It dissolves in a solution of perchloride of iron with a dark reddish-brown colour, and is reprecipitated by acids in yellow flocks. It is not affected by bichromate of potash and sulphuric acid even on boiling.

The potash salt is the only salt of this acid which I have as yet been able to examine. It crystallizes from a hot concentrated solution in water in needles and prisms of a light brick-red colour. On being heated it is instantly decomposed, giving rise to a slight detonation. The aqueous solution has a blood-red colour, but is quite transparent. The addition to it of caustic potash causes it to assume a beautiful dark purple colour, and a concentrated boiling solution does not crystallize on cooling after the addition of caustic potash. The watery solution gives with solutions of the earthy and metallic salts the following reactions. With chloride of calcium it gives an orange-coloured, crystalline precipitate; with chloride of barium a yellow precipitate; with sulphate of magnesia a yellow precipitate; with alum an orange-coloured precipitate; with protosulphate of iron a greenish-gray precipitate; with perchloride of iron a slight reddish-brown precipitate, which is soluble in the boiling fluid; with sugar of lead a red precipitate; with sulphate of copper a dull red precipitate; with nitrate of silver a yellow precipitate, which is not changed by boiling; with protonitrate of mercury a yellow precipitate; with perchloride of mercury a crystalline, yellow precipitate; with protochloride of tin a dirty yellow precipitate; with perchloride of tin a light yellow precipitate; with chloride of gold a yellow precipitate, not changed by boiling the fluid.

I have analysed rubiacin, rubiacic acid, and rubiacate of potash, and have obtained results which agree very well with one another. These analyses give for rubiacin the formula $C_{31}\,H_9\,O_{10}$, for rubiacic acid $C_{31}\,H_9\,O_{17}$, and for rubiacate of potash $C_{31}\,H_7\,O_{15} + KO$. It therefore appears that rubiacic acid contains seven atoms of oxygen more than rubiacin, and the facility with which they may be converted one into the other is easily conceivable. In rubiacate of potash two atoms of water existing in rubiacic acid are replaced by one atom of potash, which is not usually the case with potash salts. I do not, however, consider these formulæ as completely established, since I was obliged, from want of material, to operate on such extremely small quantities.

Rubian.—This substance is obtained, as I mentioned above, by treating the brown precipitate produced by an acid in an extract of madder with cold water, after having removed the excess of acid. It has the following properties:—

In thin layers it is perfectly transparent and of a yellow colour. When dry it is brittle. It is soluble in water; the solution has an extremely bitter taste. A concentrated boiling solution forms a jelly on cooling. It is precipitated from its aqueous solution by all acids, in yellow flocks. It is decomposed by nitric acid. In the watery solution lime and baryta water produce red flocculent precipitates, perchloride of iron a dark reddish-brown colour, but no precipitate, sugar of lead a brown flocculent precipitate, nitrate of silver a flocculent precipitate, corrosive sublimate no precipitate, tincture of galls and solution of glue no precipitates. The solution imparts a slight tinge to mordanted cloth, but so slight that this substance cannot be considered as a colouring matter. The solution deposits nothing during evaporation at all resembling apothem, and it therefore is not extractive matter. It dissolves in alcohol with a yellow colour, and in alkalies with a red colour. It dissolves in concentrated sulphuric acid with a red colour; the solution on being heated becomes black, and gives off sulphurous acid. When heated on platinum foil it melts, swells up immensely, and burns, leaving some ash. When heated in a tube it melts and gives yellow fumes, which condense and form a crystalline sublimate very much resembling rubiacin, so that I am induced to think that there is some relation subsisting between these two substances.

Pectic Acid.—There can hardly be a doubt, I think, that that part of the brown precipitate which is insoluble in alcohol, but soluble in water, is pectic acid, as will be seen from its behaviour towards reagents, which is as follows:—It is soluble in water; the solution has a light yellow colour, and reddens litmus paper slightly. In the watery solution acids produce white flocculent precipitates, alcohol a gelatinous white precipitate, lime and baryta water thick gelatinous pink precipitates, common salt a flocculent precipitate, nitrate of potash a flocculent precipitate, sugar of lead a gelatinous reddish precipitate, sulphate of copper a gelatinous greenish precipitate, corrosive sublimate no precipitate. On evaporating the watery solution, the substance separates on the surface of the fluid in the shape of a pellicle, and is left at last as a brownish extract, which may easily be detached from the sides of the vessel. In solutions of caustic and carbonated alkalies it first swells up, and on heating the fluid it dissolves with a light red colour, forming slimy fluids, from which it is precipitated by acids in flocks. Solutions of salts, even of alkaline salts, produce precipitates in the alkaline solutions. It is decomposed by boiling concentrated nitric acid. When heated on platinum foil it burns without melting, leaving a considerable ash. It seems that the pectic acid from madder retains in combination with it a portion of colouring matter, from which it cannot be separated. Hence the red colour with which it dissolves in alkalies.

Concerning the two fats which I mentioned above as constituents of the brown precipitate, I have little to say. They also retain in combination a quantity of colouring matter, from which I have found it impossible to separate them. They are both soluble in alcohol, but one more so than the other. One of them dissolves with rubiacin in perchloride of iron, the other not. The former is more easily fusible than the latter, but both melt below the temperature of boiling water.

I shall conclude this paper with some practical deductions which I have made from the experiments detailed in the preceding.

Few subjects connected with the arts have raised so much discussion as the nature of the process of madder-dyeing. The investigation of Robiquet on this subject, instead of clearing it up, seemed only to add to its complexity. He considered his alizarin as the substance mainly concerned in the production of madder colours. This has been denied by others, though I think on insufficient grounds. A remarkable discovery in regard to madder-dyeing, was the fact that lime is very essential in this process. It was found that madder, if not grown on calcareous soil, is incapable of producing fast colours, but that if in this case chalk be added to the madder during dyeing, or if calcareous water be employed, the desired effect is produced. This again has given rise to endless discussions. It was found by Persoz that the minutest quantity of lime added to alizarin impaired its colouring power during dyeing, and the effect of lime in madder-dyeing appeared to him an inexplicable mystery. I will not enter further into the disputes on this subject, but shall state at once my own views. It seems to me that former investigators have erred in supposing that madder contained only one colouring matter, whereas I think I have proved that there are two, perfectly distinct and definite, alizarin and rubiacin, which perform distinct functions during the process of dyeing. I have found, as I stated above, that of the two colouring matters, alizarin and rubiacin, the former is the only one that is capable of dyeing when in a free state, and further, that the brown precipitate produced by acids in a watery extract of madder contains the whole of these two colouring matters in a free state. If then a piece of mordanted cloth be dyed with this brown precipitate, after being freed from all excess of acid, the whole effect is produced by the alizarin contained in the brown precipitate. If, however, a small quantity of lime, chalk, soda, or any alkaline base, either caustic or carbonated, be added to the brown precipitate before dyeing, then its power of dyeing is very much increased. In order to prove this, I took six pieces of mordanted cloth, all of the same size. Nos. 1, 2 and 3 were mordanted in the usual way with acetate of alumina, and Nos. 4, 5 and 6 with acetate of iron. Nos. 1 and 4 were dyed with a certain quantity of the brown precipitate; Nos. 2 and 5 with the same quantity of the brown precipitate, to which, however, there had previously been added a very small quantity of lime water; Nos. 3 and 6, lastly, with the same quantity of brown precipitate, and a large excess of lime water. The dyeing was performed each time in the same vessel with the same quantity of water, and for the same length of time. Now I found at the conclusion that No. 2 exhibited a far darker, fuller, and more brilliant shade of red than No. 1, and No. 5 a much more intense purple colour than No. 4, whereas Nos. 3 and 6 showed hardly any colour at all. Now I can offer only one explanation of these differences. When a small quantity of lime is added to the brown precipitate, it combines exclusively with the rubiacin, or is transferred during the process of dyeing exclusively to the rubiacin. The first effect of the dyeing is the combination of the alizarin with the alumina and peroxide of iron of the mordants. These compounds then attract and combine with the lime compound of rubiacin contained in the fluid, by which means a greater intensity of colour is produced. I repeated this experiment with the pure colouring matters. I took two pieces of mordanted cloth of the same size, and dyed the one with pure alizarin, and the other with the same quantity of alizarin to which rubiacin combined with lime was added, and I found that the latter was much darker than the former. I therefore conclude that madder colours are always double compounds of alizarin, rubiacin, alumina, and an alkaline base, or of alizarin, rubiacin, peroxide of iron, and an alkaline base.

It follows from this that the maximum of tinctorial power in madder is produced when the alizarin is in a free state, and the rubiacin is in combination with lime or some alkaline base. If an excess of lime be added then the alizarin also combines with it, and is thus rendered incapable of attaching itself to the alumina and peroxide of iron of the mordants. A slight excess of lime exists in the root when grown on a calcareous soil; for if a quantity of madder which has dyed as much cloth as it is capable of doing and is seemingly quite exhausted of colouring matter, be treated with sulphuric acid, and the acid be carefully removed by washing, it is found that after being so treated it is capable of again dyeing almost as much mordanted cloth as it did before, a fact long known in practice. I may state in addition, that the colours produced by the brown precipitate to which a small quantity of lime has been added, resist the action of soap and acids, &c., to which all madder colours must be subjected in order to heighten them, much better than if no lime had been added. I therefore conclude, that though the possibility in general of dyeing with madder is due to alizarin, the solidity and brilliance of madder colours must be ascribed to rubiacin.

XXII. *Notices respecting New Books.*

Results of Astronomical Observations made during the years 1834, 1835, 1836, 1837, 1838, *at the Cape of Good Hope; being the Completion of a Telescopic Survey of the whole Surface of the visible Heavens, commenced in* 1825. *By* Sir JOHN F. W. HERSCHEL, *Bart., K.H., &c. &c.* London: Smith, Elder and Co. 1847. (*Third notice.*)

Chap. III. *Of Astrometry, or the Numerical Expression of the Apparent Magnitudes of the Stars.*

THE author commences the present chapter by remarking, that "a method of determining the relative magnitudes of the fixed stars, that is, of assigning to each, at a given epoch, a number expressing on a certain scale the intensity of its light, has long been and still remains a desideratum in astronomy." The inquiry is beset with numerous and great difficulties, which have only been partially met by any of the contrivances which have been applied to the purpose. Some of the difficulties, indeed, would seem to be insuperable; for instance those which arise from a diversity of colour in the light of the stars, since it appears hardly possible to assign any precise meaning to the proportion of total brightness of two stars differing sensibly in colour. Instances indeed occur where there would be no hesitation in assigning the order of precedence; but there are many other cases in which different observers would probably always come to opposite conclusions; and "nothing short of a separate and independent estimation of the total amount of the red, the yellow, and the blue rays in the spectrum of each star would suffice for the resolution of the problem of astrometry in the strictness of its numerical acceptation; and this the actual state of optical science leaves us destitute of the means even of attempting with the slightest prospect of success." We must therefore for the present, so far at least as stars differing in colour are concerned, rest contented with a somewhat rude and coarse approximation in assigning numerically their places on a photometric scale; and even with respect to those which offer no marked peculiarity of colour, the results obtained by the application of instrumental methods are in many instances so strongly at variance with the plain judgement of the unaided eye, that little is to be hoped for from such methods. It is very important, however, to establish, if possible, by direct and extensive observations, a scale of magnitudes which shall be applicable, with some reasonable degree of exactness, to the actual state of the heavens, so as to serve as a record to future ages sufficiently correct to ensure the detection of changes "such as there is abundant reason to believe a great many stars have undergone within the period of astronomical history." The subject was long ago urged on the attention of astronomers by Sir William Herschel, in his papers on the comparative brightness of the fixed stars published in the Philosophical Transactions; and if anything could add force to his arguments, it would be the extraordinary phænomena presented by the star η Argûs already mentioned, and of variable stars in general.

Sir John states, that after having made trial of various instrumental contrivances, he is disposed to rely mainly, for the formation of a real scale of magnitudes, on the unassisted judgement of the naked eye. The method which he adopted, and calls the *method of sequences*, is an extension of Sir William Herschel's method of naked eye comparisons, so modified as to afford a means of educing from it a *numerical* scale of values of the magnitudes compared. Sir William Herschel's object in forming his catalogues of comparative brightness, was to identify the lustre of each star so as to enable a future observer to satisfy himself whether any change had taken place; and to this end each star was compared with one or at most two others judged to be equal to it (or very nearly so), so as to establish a system of binary or ternary sequences or equalities. Supposing this to be done for every visible star, it is clear that no change could take place without being detected. For the purpose proposed by Sir John, namely, the formation of a numerical scale of magnitudes, it was necessary to establish more extended sequences, in each of which a considerable interval of the scale should be included; and when many such sequences were accumulated, to devise a means of combining them into general sequences, including *all* the stars observed. His method was, generally, as follows:—

"Choosing perfectly clear nights (which for this purpose are quite indispensable), a succession of stars was picked out by actual inspection of the heavens from the largest above the horizon down to some of considerably inferior magnitude, and noted down in a list, in a vertical column—leaving blank intervals more or less considerable, according as the steps of the skeleton scale so picked out were wider or closer—but taking care that between the skeleton stars arranged *seriatim*, there should always be an unequivocal descending step of apparent lustre. The business of the night, then, was to fill in as far as practicable the steps of this scale into an unbroken chain of downward gradation, placing each newly-added star by actual judgement and comparison with its immediate neighbours, in its proper order, until the scale became so gradual in its declension that it was no longer possible to insert fresh stars with certainty *between* its members, in which case they were set down as *equal* to some of those already noted down."—P. 306.

The charts referred to during the observations were those of Bode's Atlas, or working copies made from them. In consequence the nomenclature was Bode's, which, faulty as it is, it was then necessary to tolerate. On the publication of the Catalogue of the British Association, in which a systematic reformation of sidereal nomenclature was carried out, it became a matter of consideration whether the names used in the observed sequences should be altered to correspond with that Catalogue; but as it was found that this could not be done without causing great confusion, the nomenclature of Bode is retained. Sir John observes that he regrets this the less, because an entire remodelling of the whole system of constellations, both northern and southern, is now necessary for this especial purpose—without which the progress of this department of astronomy will of necessity be slow and uncertain.

The sequences or comparisons of apparent magnitude are given at length. Their number is forty-six. They were observed at Feldhausen between July 16, 1835, and Feb. 25, 1838, and on board the H.E.I. Company's ship Windsor, at sea, from March 28 to April 15, 1838. The number of stars included in each sequence is very unequal, but may be stated as varying from twenty or thirty to seventy or eighty.

The sequences observed in the manner above described formed, as it were, the raw material out of which a regular progression of magnitudes was to be elaborated. The author remarks that "if observations of this nature could be made with perfect exactness, all sequences in which the same stars occur should present them in the same order of succession, in so far as it may not have been disturbed by a real variability of the stars themselves. But as this is very far indeed from being the case, owing to the many disturbing influences, and the slight differences of magnitude between particular stars which have to be discriminated, it is necessary to fix upon some criterion by which the most probable order of arrangement can be discovered among a number of discordant ones. Such a criterion is supplied by an enumeration of cases." Thus if two stars, A and B, occur in a certain number of sequences, and A is set down sometimes as superior, sometimes as inferior to B, then the order of their arrangement will be determined by placing the one first which has been so placed the greatest number of times in the observed sequences. Again, if there be three stars, A, B, C, and the direct comparisons of B and C leave it doubtful which of those two is superior, and it be found on a revision of all the observations that A has been set down as decidedly superior to C but not to B, then there will be reason for placing B before C. It is needless to go into further details; the general method of proceeding will be obvious.

It might be supposed, both from the uncertainty which in a great number of cases must attend the judgement formed from a naked eye comparison, and the numerous discrepancies which occur in the recorded observations, that the final result must be open to very considerable doubt. This, however, does not seem to be by any means the case; on the contrary, to one who has not been in the habit of making such comparisons, the precision will appear to be really astonishing. "It might appear that so much is left to judgement in such cases, as to induce uncertainty in the final results. But, in fact, it is only in the case of stars which really are nearly equal that doubts on such points ever arise. Push observation and reasoning to what extent we will, there will always be shades of difference too slight for certain discrimination; and practically speaking, I am disposed (from my experience in these reductions) to consider a misarrangement to the extent of a tenth of a magnitude in one of our corrected sequences, *arising from such causes of uncertainty*, as a very unlikely occurrence."—P. 327.

The different steps of the process of arrangement are explained at considerable length; the author treating,—1st, "of the formation of corrected sequences by the collation of several observed sequences, and of the elimination of discordant and contradictory observations;" 2nd, "of the consilience of sequences, or the formation of normal sequences by the partial or entire combination of several corrected ones, and of the assignment of a series of provisional magnitudes to serve as a basis for those to be finally adopted;" and 3rd, "of the complete interpolation of the observed sequences, the conclusion of an independent numerical result from each observation of each star, and the assemblage of their results for the final determination of its magnitude." The whole number of stars included in the final arrangement, and to which magnitudes have now for the first time been assigned upon a systematic plan, is 452,—a number sufficiently considerable to serve as a fair specimen of the practical application of the method. The following intimation will be received with interest:—"Since my return to England I have been, and still am, engaged in extending its application to the northern stars with some perseverance, and I venture to hope not without some corresponding degree of success. Sixty-one sequences, in addition to those above recorded, have been observed and partially reduced, and where completely so, and brought into comparability with the southern sequences by means of the intertropical stars common to both series, will afford a uniform system of astrometrical nomenclature, on which I have every reason to believe considerable reliance may be placed. This work, however, besides that it is as yet incomplete, cannot properly find its place in a volume of Southern Observations."—P. 340.

A comparison of the magnitudes finally deduced by this process with those which were provisionally assumed, gives the following results:—out of 118 stars, the cases of exact agreement are 22; of disagreement under one-twentieth of a magnitude, 64; of disagreement above a twentieth but under a tenth, 26; of a tenth but under an eighth, 6. These results completely bear out the statement made above as to the precision with which the relative brightness of stars can be determined by the judgement of the naked eye.

One of the principal uses of the determination of relative magnitudes, as has been already stated, is the means it gives of ascertaining the existence of variable stars,—a class of objects probably more numerous than has been suspected. Several of those included in the sequences are marked as variable, or possibly so, and α Hydræ and β Ursæ Minoris are instanced as two of the most remarkable. Sir John concludes some interesting remarks on this subject with the following observations, which indicate a probability that the sun itself belongs to the class of variable stars.

"Ignorant as we are, however, both of the cause of solar and stellar light, and of the conditions which may influence its amount at different times, the law of regular periodicity is one which ought not to be too hastily generalised, and at all events there is enough of slow and gradual change of lustre in many stars, since the earlier ages of astronomy, to refute all *à priori* assumption as to the possible length of the cycle of variation of any particular star. The subject is one of the utmost physical interest. The grand phænomena of geology afford, as it appears to me, the highest presumptive evidence of changes in the *general* climate of the globe. I cannot otherwise understand alternations of heat and cold so extensive as at one epoch to have clothed high northern latitudes with a more than tropical luxuriance of vegetation; at another to have buried vast tracts of middle Europe, now enjoying a genial climate, and smiling with fertility, under a glacier crust of enormous thickness. Such changes seem to point to some cause more powerful than the mere local distribution of land and water (according to Mr. Lyell's views) can well be supposed to have been. In the slow secular variations of our supply of light and heat from the sun, which in the immensity of time past may have gone to any extent, and succeeded each other in any order, without violating the analogy of sidereal phænomena which we know to have taken place, we have a cause, not indeed established as a fact, but readily admissible as something beyond a bare possibility, fully adequate to the utmost requirements of geology*. A change of half a magnitude in the lustre of the sun, regarded as a fixed star, spread over successive geological epochs,—now progressive, now receding, now stationary, according to the evidence of warmer or colder *general* temperature which geological research has disclosed, or may hereafter reveal,—is what no astronomer would now hesitate to admit as in itself a perfectly reasonable and not improbable supposition. Such a supposition has assuredly far less of extravagance about it than the idea that the sun, by its own proper motion, may, in indefinite ages past, have traversed regions so crowded with stars as to affect the climate of our planet by the influence of *their* radiation. Nor can it be objected that the character of a *vera causa* is wanting in such an hypothesis. Of the exciting cause of the radiant emanations from the sun and stars we know nothing. It may consist, for aught we can tell, in vast currents of electricity traversing space (according to cosmical laws), and which, meeting in the higher regions of their atmospheres with matter properly attenuated, and otherwise disposed to electric phosphorescence, may render such matter radiant, after the manner of our own aurora borealis, under the influence of terrestrial electric streams. Or it

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* "Many phænomena in natural history seem to point out some past changes in our climates. Perhaps the easiest way of accounting for them may be to surmise that our sun has formerly been sometimes more and sometimes less bright than it is at present."—W. Herschel on the Changes that happen to the Fixed Stars. Phil. Trans., 1796, p. 186.

may result from actual combustion going on in the higher regions of their atmospheres, the elements of which, so united, may be in a constant course of separation and restoration to their actual state of mutual combustibility, by vital processes of extreme activity going on at their habitable surfaces, analogous to that by which vegetation on our earth separates carbonic acid (a product of combustion) into its elements, and so restores their combustibility. With specific hypotheses as to the causes of solar and sidereal light we have however no concern. It suffices that they must have a cause, and that this cause, inscrutable as it may be, does in several cases, and therefore may, in one more, determine the production of phænomena of the kind in question."—P. 352.

The last part of the chapter is devoted to an account of some attempts to compare the intensities of light of the stars one with another by the intervention of the moon, by the aid of an astrometer, or instrument adapted to that purpose. "The process by which these comparisons were made consists in deflecting the light of the moon by total internal reflexion at the base of a prism, so as to emerge in a direction nearly coincident with that of the undeflected light of one of the stars to be compared. It is then received upon a lens of short focus, by which an image of the sun is formed, which, viewed at a considerable distance by an observer placed in or near the axis of the lens, will appear to him as a star. This artificial star is then to be approached to or removed from the eye until its light is judged to be exactly equal to the light of the real star, which, lying in nearly the same direction from the observer, will be seen side by side with the artificial one with the same eye, or with both eyes at once, without the aid of a telescope in the ordinary mode of natural vision. The distance from the eye to the focus of the lens being then measured, the prism and lens are to be so placed as to form another similar artificial star, in a direction nearly coincident with that of the other star under comparison; and another equalisation being made, and distance measured, it is obvious that the intensities of the lights of the two stars, or at least their effects on the retina, under the circumstances of comparison, will be to each other in the inverse ratio of the squares of the distances so measured respectively. For no light being lost by total internal reflexion at the base of a prism, and the light lost at the ingress and egress of the moon's rays into and out of the prism, being at such moderate angles of incidence as it is ever necessary to employ in such comparisons, very nearly, indeed, in an invariable ratio to the total incident light, the artificial star, or lunar image, will be equally luminous in both cases, and its effect on the eye will therefore be in the ratio of its apparent angular diameter, or inversely as the square of its distance."—P. 353.

The application of the principle explained in the above extract is effected by a very simple construction. An upright pole about 20 feet high, firmly fixed in the ground as a *standard*, a *staff* consisting of a cylindrical rod about 2¾ inches in diameter and 12 feet long, and a *slider* consisting of a rectangular piece of deal about 15 inches long and 2¾ inches broad, form its principal parts. The slider carries the prism and lens, and the requisite motion is given by simple contrivances, into the details of which it is unnecessary to enter. In using the instrument, a star must be chosen not more than 102° 43′, nor less than 60° or 70° from the moon, which ought not to be too near the full. The staff being first directed to the star is turned on its axis till the prism is brought into the proper position; and when the proper angle of reflexion is attained, the artificial star (or moon) is seen side by side with the real star under comparison. The slider is then approached to or removed from the eye, along the staff, until a perfect equality between the real and artificial star is obtained, when the distance of the slider from the eye is measured by means of a measuring-tape connected with the apparatus. A sufficient number of equalisations ought to be made to destroy the numerous sources of illusion to which the judgement of equality is subject, and give confidence in a mean of the readings. This done, the other star is treated in the same manner.

Sir John states that the actual comparisons made with the instrument were neither very numerous nor performed under the most favourable circumstances. In several of them the moon was nearly full; and though he had expected this to influence the results, he was by no means prepared for the enormous extent of that influence. In the original conception of the process, he had hoped that the image of the moon, when reduced to that of a mean full moon, by allowing for the phase and apparent diameter, would afford a definite standard of light; but the effect of the illumination of the sky destroys all possibility of obtaining absolute results in this manner, and renders it necessary to regard them as comparable only with those taken on the same night and under the same circumstances. All these difficulties and objections, he adds, might be got over by using Jupiter instead of the moon as a standard luminary; but this idea did not occur to him when it might have been serviceable. He refers also to an instrument constructed on the same principle,—that is to say, in which a prism is made use of to deflect the light of a standard luminary by total reflexion at its base,—invented by M. Steinheil, and communicated to the Gottingen Academy in 1835; but he had no knowledge of the existence of M. Steinheil's memoir till his return to Europe in 1838.

The observations with the astrometer were made in the year 1836 on nineteen different days, between March 28 and December 26. In each case the numerical result is obtained from the following formula: "Let Δ represent the moon's *augmented semidiameter*, in seconds, at the place and moment of observation as given by the Nautical Almanac and a table of augmentations; e her elongation from the sun, R the earth's radius vector, the mean value of R being $=1$; $933''\cdot 5$ the moon's apparent diameter. Take

$$M = 1000 \left\{ \frac{\Delta}{R \times 933''\cdot 5} \sin \frac{e}{2} \right\}^2,$$

then will M express the absolute quantity of light in the moon's image (supposing all her illuminated surface equally bright) and

$M \div d^2$ the apparent intensity of our artificial star at the distance d from the eye."

In order to prepare the observations for reduction, all the measures of each star taken on the same night must be collected, and the mean taken. The results being then computed according to the formula $M \div d^2$, the influence exercised on the comparisons by the illumination of the general surface of the sky on which the stars are seen projected, becomes at once apparent on comparing the results for the same star obtained from observations on different nights. Taking α Centauri, for example, some of the results were as follows:—0·443, 0·259, 0·156, 0·505, 0·838, &c. "Such enormous differences show clearly, that, as a formula of reduction, the above is utterly inapplicable. Indeed, at first sight, it would almost appear from this that absolutely no conclusion can be drawn from the observations or the method in question. This, however, is very far from being a correct view of the subject, since, if instead of comparing the results in this manner for a single star, we take any two stars which occur together in more than one series, and compare, not the absolute numbers resulting from the formula in question, but the ratios of those numbers, we shall find, not, indeed, a precise, but, with a few exceptions (such as might be expected in the first trials of a new method) a reasonably good accordance."—P. 363.

Each pair of stars occurring in the different series being thus compared, and the mean of the results taken, a set of provisional or temporary values is obtained, expressing the intrinsic light of each star in a scale in which α Centauri is represented by 1. But these numbers, though a process of considerable labour has already been employed in obtaining them, are destined merely to afford a handle for a complete and impartial reduction of the whole, which is accomplished as follows:—Taking $\mu \div d^2$ for the expression of the light of the compared star, μ will be a coefficient which may be regarded as constant throughout any single series. The immediate object, then, is to assign for each series such a value of μ as shall give consistent results when the individual values of $\mu \div d^2$ for any one star deduced from different series are compared together. Now, calling the provisional values obtained as described above S, S', S'', &c., and denoting by d, d', d'', &c. the distances of equalization of any number of the stars observed in any series, we ought to have for that series

$$\mu = Sd^2 = S'd'^2 = S''d''^2 = \&c.$$

But this being only approximately true, a mean value of μ is determined for the series in question by taking a mean of all these values, and with the mean value thus obtained the values of S, S', S'', &c. are recomputed by the formula $S = \mu \div d^2$ for each star, and for every series in which it occurs. In this manner, as many different values are found for each star as there are series into which it enters, and the means of these values being again taken, the results are finally reduced so as to bring them again to a scale in which α Centauri is (as originally proposed) represented by 1·000. By these successive reductions and adjustments a table is ultimately obtained, showing the results of the photometric determination of the comparative intensities of the light of sixty-nine stars. The following are a few of the results:—

Sirius	4·052	Antares	0·404
Canopus	1·994	β Centauri	0·399
α Centauri	1·000	α Crucis	0·377
Arcturus	0·726	α Aquilæ	0·350
Rigel	0·654	Spica	0·309
Procyon	0·520	η Argûs	0·262
α Orionis	0·484	Fomalhaut	0·262
Lyra	0·446	γ Orionis	0·207

Sir John then enters into a comparison of the values of the coefficient μ, computed in the manner above explained, with those of M, which represents the absolute quantity of moonlight, and on which the former necessarily depend as their principal *argument*. The conclusion at which he arrives is, that the value of μ is as the cube of the quantity of moonlight. He also deduces this inference, namely, that "within the limits of the experiments the effective impression of a star on the retina is inversely as the square of the illumination of the ground of the sky on which it is seen projected."

The present chapter concludes with a comparison of the photometric results with the conventional scale of naked eye magnitudes adopted in the sequences. For this purpose the two series of values (representing the conventional magnitudes on the one hand and the light on the other) are projected as the abscissa and ordinates of an interpolating curve; then, calling x the conventional magnitude of any star in both series, and y its light, the values of y corresponding to every decimal of a magnitude from $x=1$ to $x=3{\cdot}6$ are read off from the curve. This curve is found to approach very nearly to a cubic hyperbola, the equation of which is accordingly assumed; and the two constants which enter into the equation are determined from particular values of x. Having thus an equation between the magnitude and the light, the magnitudes of the several stars whose light has been determined by the photometric observations are computed, and the resulting values compared with those assigned by the sequence observations. A table is given, showing the magnitudes, as determined by both methods, of sixty-eight stars; but of these, the five largest,—Sirius, Canopus, α Centauri, Arcturus, Lyra—should be excluded from the comparison, the sequence magnitudes assigned to them being purely arbitrary. Out of the remaining sixty-three there are eighteen in which the disagreement of the assigned values exceeds one-fifth of a magnitude (a quantity respecting which, it is observed, the judgement of a practised eye cannot be deceived); but on the other hand there are forty in respect of which the results agree within an eighth of a magnitude. In respect of the cases of considerable discordance, it is to be remarked, however, that in a majority of them the photometric determination rests on a single night's observation, and not unfrequently on a single equalisation. Subjoined is an extract from the table, containing the twenty stars next following Lyra, the five largest being omitted for the reason above stated.

Star.	By seq.	By astr.	Star.	By seq.	By astr.
Rigel	0·76	0·82	β Crucis	1·59	1·57
Procyon	0·85	0·97	α Gruis	1·69	2·02
α Eridani	0·93	1·09	γ Crucis	1·73	1·85
α Orionis	1·00	1·02	ε Orionis............	1·84	2·20
β Centauri	1·14	1·17	ε Canis	1·86	1·83
α Crucis	1·21	1·21	λ Scorpii	1·87	1·87
Antares	1·28	1·16	ζ Orionis	2·01	2·44
α Aquilæ	1·35	1·28	β Argûs	2·03	2·10
Spica	1·41	1·38	γ Argûs	2·08	1·98
Fomalhaut	1·47	1·54	γ Orionis	2·10	1·78

"The Messrs. Chambers's Edition of Euclid."

The Messrs. Chambers have published extensively, under the above title, an "advertisement" in reply to the strictures which were made in the Philosophical Magazine for June, upon Mr. A. Bell's edition of the Elements of Euclid.

Instead, however, of attempting to show that my censures were either *false* or *unjust*, or even more *severe* than the case actually demanded, they have attempted to divert public attention from those censures *upon the work itself*, by making it appear that it was not *originally* published in contemplation of its being adopted by the Committee of Council! What answer this may be to the charge of a *corrupted text* and *geometrically-absurd interpolations*, I leave to themselves to explain in a future advertisement: but I have to say, once for all, that till they offer some show of justification of their work against these charges, I cannot lend myself to the kind of bye-play in which they are attempting to entangle me.

As to the insinuation respecting the "animus" of the Review (obviously only another form for the imputation of unworthy motives), I can only say that I have always given the Messrs. Chambers credit for a degree of ingenuousness and discretion which could not stoop to so common-place an artifice. But it seems I misjudged them. I had no motives but those which I plainly avowed, and no personal interests whatever to serve. As a geometer, however, I could not stand by and see Geometry so mangled and mutilated as in the work which the Messrs. Chambers had published (and see it also taken under the patronage of the Government, and enforced upon us at the public expense), without attempting to prove that their work had no claims to such a distinction: and as far as yet appears, *my proofs are unassailable* on any legitimate principle. It will be time enough for me to think of justifying my strictures when they are met by *argument*, instead of being parried by *merely irrelevant statements.*

The Reviewer of
"The Messrs. Chambers's Edition of Euclid."

July 21, 1848.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 66.]

May 18, 1848. "ON a new case of the Interference of Light." By the Rev. Baden Powell, M.A., F.R.S. &c., Savilian Professor of Geometry in the University of Oxford.

The principal experiment exhibiting the newly-observed case of interference described by the author, is the following: into a hollow prism or trough containing oil of sassafras, anise, cassia, &c., a plate of glass is dipped so as to intercept the light passing through the upper or thicker half of the prism, and leave the lower half clear: the spectrum of a line of light or narrow slit, is then immediately seen to be covered with dark bands parallel to the slit or edge of the prism.

Some substances, as oil of turpentine, water, &c., give no bands with this arrangement, but will give them, if a complementary arrangement be adopted, viz. by placing a narrow slip of glass at the bottom of the prism, or so as to intercept the light passing through the thinner half.

The production of the bands in general, and the reason of the difference existing in the cases referred to, are fully accounted for on the theory of waves and interference: whence the following formula is deduced, giving the number of bands (n), when μ_p and μ_m, the indices for any ray for the plate and for the medium, are known, of which λ is the wave length; and taking these quantities for any two rays (1) and (2) of the spectrum, τ being the thickness of the plate, we have the number of bands between them expressed by

$$n=\left\{\left(\frac{\mu_p-\mu_m}{\lambda}\right)_1-\left(\frac{\mu_p-\mu_m}{\lambda}\right)_2\right\}\tau.$$

The numbers thus calculated agree with observation for many media. The distinction of the two cases alluded to depends on whether n result positive or negative: that is, on the relative values of the indices; also agreeing with observation.

If doubly refracting media are used as plates, two sets of bands are seen superimposed, which are easily shown to be due to the ordinary and extraordinary rays respectively. But for some of these crystals the data are as yet insufficient to give more than a general accordance with theory.

The method might be applied practically for determining the indices of many substances to which the ordinary method is inapplicable from the impossibility of forming them into prisms: n will also exhibit palpably the most insensible degree of double refraction, and may thus become useful to the mineralogist.

There is a close analogy between these phenomena and those observed by Baron von Wrede, and by Sir D. Brewster and Mr. Fox Talbot, of which Mr. Airy has given a theory. A similar theory is necessary for explaining some of the more minute details of the present phenomena; and on this subject some extensive researches have been pursued by Mr. Stokes of Pembroke College, Cambridge, which will soon appear.

"On the Meteorology of the Lake District of Cumberland and Westmoreland." By John Fletcher Miller, Esq. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

The author has devoted nearly four years to the investigation of the quantities of rain falling in the lake districts of Cumberland and Westmoreland; and he commenced, two years ago, a set of experiments specially directed to ascertain the amount of rain deposited at great elevations above the sea, such as the summits of our highest English mountains. As the investigation proceeded, some remarkable results were obtained, which coming to the knowledge of the Royal Society early in last year (1847), the Council contributed a sum of money from the Donation Fund towards the current expenses attending this inquiry, of which the results are given in the present communication, comprising extensive tables of observations relative to the quantity of rain in different situations within the above period of time.

May 25.—"On the structure of the Jaws and Teeth of the Iguanodon." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., Vice-President of the Geological Society, &c.

The recent discovery of the right dentary bone of the lower jaw of an adult Iguanodon with teeth, having enabled the author, with the aid afforded by other specimens, to determine the structure of the maxillary organs of that gigantic herbivorous reptile, the result of his investigations are embodied in the present communication.

The first memoir of the author on the teeth of the Iguanodon was published in the Philosophical Transactions for 1825; but owing to the fragmentary and water-worn condition in which the fossil remains of terrestrial vertebrated animals occur in fluviatile deposits, in consequence of these strata consisting of materials transported from far-distant lands, nearly a quarter of a century elapsed before any portion of the jaw with teeth was discovered.

The most important of the fossils described in this memoir consists of the anterior part of the right side of the lower jaw, which was discovered a few weeks since, in a quarry in Tilgate Forest, by Capt. Lambart Brickenden, F.G.S., who with great liberality placed it at the disposal of the author as the original investigator of the fossils of the Wealden.

This dentary bone, which is eighteen inches long, is perfect in the anterior part, but is broken at the hinder extremity, and retains five or six inches of the coronoid bone: the length of the jaw to which it belonged is estimated at four feet. It contains two successional teeth in place, the fang of a third, and the alveoli or sockets for eighteen or nineteen mature molars; the entire number of teeth on each side the lower jaw was about twenty.

The mature teeth, which, when abraded by use in mastication, resemble the worn molars of herbivorous mammalia, appear to have been arranged in a close-set series. The lower teeth had their enamelled striated face parallel with the alveolar plate, and fronting the inside of the mouth; but the upper were placed in a reverse position, that is, with the enamelled facet of the crown external; and the teeth in the upper and lower jaws were arranged subalternate or intermediate in relation to each other, as is the case in the ruminants.

But a still more remarkable character presented by this specimen is the peculiar construction of the anterior part of the lower jaw, which forms the symphysis. This process, instead of being continued round the front of the mouth and beset with teeth, as in all other saurians, is edentulous, and extends into a procumbent scoop-like expansion, very analogous to the symphysial portion of the lower jaw in the Sloths, and especially to that of the colossal extinct Edentata—the Mylodons. Along the external surface of this dentary bone there is a row of very large vascular foramina; and the symphysis also is perforated with numerous openings for the passage of blood-vessels and nerves sent off from the great dental canal. The unusual number and magnitude of these foramina indicate a great development of the integuments and soft parts with which the bone was invested, and also the large size of the under lip.

The upper jaw, of which a considerable portion discovered by the author is in the British Museum, confirms the inferences deduced from the teeth and dentary bone of the lower maxilla.

The author, with the able assistance of Dr. A. G. Melville, instituted a comparison between all the teeth of the Iguanodon to which he could obtain access, and those of recent saurians; and the result of the investigation is detailed. The new light shed on the structure and functions of the dental organs, confirms, in every essential particular, the inferences deduced by the author from the detached teeth alone, in his memoir of 1825; and it also reveals an extraordinary deviation from all known types of reptilian organization, and which could not have been predicated; namely, that this colossal reptile, which equalled in bulk the gigantic Edentata of South America, and like them was destined to obtain support from comminuted vegetable substances, was also furnished with a large prehensile tongue and fleshy lips, to serve as instruments for seizing and cropping the foliage and branches of trees; while the arrangement of the teeth as in the ruminants, and their internal structure, which resembles that of the molars of the Sloth tribe in the vascularity of the dentine, indicate adaptations for the same purpose.

Among the physiological phænomena revealed by Palæontology, there is not a more remarkable one than this modification of the type of organization peculiar to the class of reptiles, to meet the conditions required by the economy of a lizard placed under similar physical relations, and destined to effect the same general purpose in the scheme of nature, as the colossal Edentata of former ages, and the large herbivorous mammalia of our own times.

From the facts detailed, the author is led to consider the specimen described in his memoir of 1841, as being probably the lower jaw of a young Iguanodon (but the true nature of which, from the absence of the crowns of the teeth, was doubtful) belonging to the same family, but referable to a distinct genus or subgenus; and he proposes the name of *Regnosaurus Northamptoni* for that remarkable fossil saurian. The communication was illustrated by several drawings of the specimens described.

"An Account of some Observations made on the Depth of Rain which falls in the same Localities at different altitudes in the hilly districts of Lancashire, Cheshire and Derbyshire." By S. C. Homersham, C.E. Communicated by George Newport, Esq., F.R.S. &c.

The author states, that having been present at a meeting of the Royal Society when a paper was read on the Meteorology of the Lake districts of Westmoreland and Cumberland, by J. Miller, Esq. of Kendal, in which it was stated that the quantity of rain falling in mountainous districts appears to increase from the valley upwards, to the altitude of about 2000 feet, and then rapidly to decrease, he wishes to lay before the Royal Society the results of his own observations, which lead him to a different conclusion. After stating that he had been at some trouble to analyse Mr. Miller's observations which have been communicated to him by that gentleman, he is of opinion that they do not warrant the conclusion deduced from them, and are also at variance with the recorded observations of Daines Barrington, Dr. Dalton, Professor Daniell and others, as well as those of Capt. Lefroy and Col. Sabine.

The author then shows from observations very carefully made in Lancashire, Cheshire and Derbyshire, from January 1846 to March 1848, that more rain falls at the bottom than at the tops of hills of a less elevation than 2000 feet in the same locality, and that the quantity diminishes in a ratio almost precisely corresponding to the height. The details are given in tables of monthly observations, made near Whaley and Congleton in Cheshire, and Chapel-en-le-frith in Derbyshire, and also of other observations made for the Corporation of Liverpool at Rivington and in the valley of Roddlesworth near Preston in Lancashire, which have been communicated to him. The whole of these observations, carefully analysed and compared, have led the author to a conclusion opposite to that arrived at by Mr. Miller.

The author then proceeds to show that the details of Mr. Miller's own observations are in accordance with his, and that they fully bear out his views, and not those of that gentleman. Some apparent discrepancies in the results are pointed out and their cause explained by reference to peculiarities in the localities in which the observations were made, as shown by reference to a map accompanying this paper, and to the details given by Mr. Miller; so that the observations of this gentleman, when examined with reference to locality, fully confirm those of the author and of the authorities he has quoted, and establish the proposition, that as a general law, the quantity of rain deposited in the valleys and at the bottoms of hills is greater than in more elevated situations in the same locality.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 74.]

Account of the Annular Eclipse of the 9th November, 1847. By Major Lysaght.

Hingolee, at which station the following observations were made, is situated in lat. 19° 43′ 12″, long. 77° 7′ 5″ E. The former may be depended upon as correct, but the latter is open to correction.

The weather was very unpromising, and heavy clouds showed themselves in the forenoon; but a short time previous to the commencement of the eclipse they cleared off towards the western horizon, and left the required part of the heavens beautifully clear and serene, with a light wind.

The observations were made with a Dollond's 3½-foot refractor, 2·7 inches aperture and power 25, with a coloured glass, which gave the sun a greenish-yellow colour. A thermometer was placed against a brick wall with the sun shining directly on its bulb, and a barometer and a spirit thermometer in the shade of an open verandah: the times were noted by an assistant with a chronometer, adjusted to mean time by an observation of the sun in the morning.

The day was exceedingly hot for the season of the year, the exposed thermometer standing at 129°·5 a short time before the commencement of the eclipse, but it fell *immediately after the time of first contact was given* to 92°·2.

Before the commencement of the eclipse the spots on the sun appeared fainter than usual, or than I had observed them a day or two before with a smaller telescope; but the faculæ were very bright. As the moon advanced over the sun, the spots became much darker and well-defined, and round the largest (near the centre) was a dark fine ring, conforming to the shape of the spot, the intervening space being of the same dull colour as the rest of the sun.

The eye having been withdrawn for a little time from the telescope, the *exact* time of first contact was lost by a few seconds.

On nearing the solar spots, the moon, which up to this period was well-defined, became undulating, and this undulation increased until the annulus was about to form, when the lower limb again became steady and well-defined, and the upper, first much serrated, and then *hillocky*. At first this was attributed to the lunar mountains, but except in two or three places they all subsided. Immediately before the formation of the annulus, and on the western edge, a dark line was seen to connect the limbs of the sun and moon, and gave the idea of a post raised *gradually* from behind *on one* of the *hillocks*, and extending to the sun's limb. This was followed by another: both remained a few seconds, but the manner of their disappearance was not particularly observed; then appeared a thin thread of light, which was noted as the formation of the annulus.

As the ring increased in breadth, the southern limb of the moon became pinnacled, and an instantaneous blaze of light was seen, which had more the appearance of a fire suddenly breaking out on a windy night in dry grass on the summit of a jungly hill (as often seen in this country), than of exploded gunpowder, or anything else I can liken it to.

The moon's limb again became smooth, leaving a hillock on the eastern side, which, as she advanced, *gradually* elongated, and became pointed, when another post was erected like the former two, but more slowly (so much so that I was nearly turning my eye in another direction, not expecting to see the phænomenon), and remained somewhat longer; this broke in the centre, and the two parts shrunk slowly into the limbs of the sun and moon.

The clouds which had at the commencement settled down to the western horizon, had again risen, and now approached, and I gave up the hopes of seeing the breaking of the annulus, or end of the eclipse.

There was not much apparent diminution of light during the existence of the annulus, nor were any stars visible, but everything had a light blue colour.

At 3^{h} 7^{m} P.M. gunpowder was ignited in about two minutes with a lens of three inches diameter; but at 3^{h} 23^{m} P.M. it could not be exploded, although the lens was held over it for seven minutes, and care was previously taken to dry the powder.

Before the commencement of the eclipse.

Barometer	28·31 inches.	
Thermometer attached	93°	
Ditto exterior	129·5 exposed to the sun.	
Time of first contact		1^{h} 38^{m} 25^{s} ±
Formation of annulus		3 21 14

At the middle of the eclipse.

Barometer	28·27 inches.	
Thermometer attached	89°·7	
Thermometer exterior	92	exposed to the sun.
Spirit thermometer	84	minimum temperature of air during the eclipse.

The barometer and thermometers are corrected by the standards of the Surveyor-General of India.

Remarkable Appearances during the total Eclipse of the Moon on March 19, 1848.

Extract of a letter from Mr. Forster, Bruges.

"I wish to call your attention to the fact which I have clearly ascertained, that during the whole of the late lunar eclipse of March 19, the shaded surface presented a luminosity quite unusual, probably about three times the intensity of the mean illumination of an eclipsed lunar disc. The light was of a deep red colour. During the totality of the eclipse, the light and dark places on the face of the moon could be almost as well made out as in an ordinary dull moonlight night, and the deep red colour, when the sky was clearest, was very remarkable from the contrasted whiteness of the stars. My observations were made with different telescopes, but all presented the same appearance, and the remarkable luminosity struck every one. The British consul of Ghent, *who did not know that there was an* *eclipse*, wrote to me for an explanation of the blood-red colour of the moon at 9 o'clock.

"The sky was of unusual brilliancy, as often occurs between showers; there was a bright aurora in the north, and a most magnificent meteor descended obliquely towards the north-west horizon about the time of the central eclipse. The western margin of the disc presented a rough uneven appearance at this time. What would be the effect on the dark surface of the moon of extensive aurora borealis on our earth?"

Mr. Walkey, who observed the eclipse at Clyst St. Lawrence, near Collumpton, says the appearances were as usual till 20 minutes to 9. "At that period, and for the space of the next hour, instead of an eclipse, or the umbra of the earth being the cause of the *total* obscurity of the moon, the whole phase of that body became very quickly and *most beautifully illuminated*, and assumed the appearance of the glowing heat of fire from the furnace, rather tinged with a deep red. The above description I gave to the editor of one of the Exeter papers, and some one has attempted to solve the peculiarity of the appearance by speaking of the umbra and penumbra, which might have been the cause of it. But such a solution has nothing to do with the appearance, the whole disc of the moon being as perfect *with light* as if there had been *no eclipse whatever*.

"Having spoken of this appearance, I was informed by one or two individuals that they had seen, between twenty minutes to nine and twenty minutes to ten, a very luminous appearance of the aurora borealis. Now, it strikes me that the light reflected from this northern effulgence might have caused the *luminous appearance* of the moon in this part of the country at the time when it was under the perfect umbra of the earth in other portions of England.

"Many more than threescore years have passed with myself, and during that period I have several times beheld an eclipse of the moon, but never before did my eyes behold the moon positively giving *good* light from its disc during a total eclipse. The phases of the moon perfectly corresponded with the authorized diagram up to the period of 8^h 40^m, and after 9^h 40^m to the end of the eclipse."

In a letter to Captain Smyth, Mr. Vallack suggests the utility of adopting the binocular construction in astronomical telescopes, and especially in Newtonian reflectors. He has himself fitted up two mirrors on this principle, and finds a considerable superiority in the pair over a single mirror. Mr. Vallack, if we understand him correctly, proposes the following arrangement. The specula are fixed in parallel tubes, and by raising the further mirror higher up in its tube, and bringing the small mirror or prism nearer to the mirror, an image is formed considerably in advance of the tube, which may be brought so near the image formed by the nearer mirror, that each may be viewed at the same time by its proper eye. The partition between the tubes and also the outer tube must be pierced, to let the rays of the more distant mirror pass out; by a little adjustment, the distance between the images may be made to suit different eyes.

He remarks, as an objection to the binocular construction, that the observation will not be convenient, if objects of very different altitudes are to be viewed, but still thinks that it would be worthy of trial in certain cases.

XXIV. *Intelligence and Miscellaneous Articles.*

ON THE COMBINATIONS OF SILICIUM. BY I. PIERRE.

DOUBTS are still entertained as to the formula which should be assigned to silicic acid, and consequently to the chloride of silicium. Some chemists admit that silicic acid should be represented by the formula SiO^3, and adopt the number 266·82 as the equivalent of silicium; others prefer the formula SiO^2, taking $Si = 177{\cdot}88$. Lastly, some are of opinion that the rational formula of silicic acid is SiO, and the equivalent of silicium 88·94.

The first of these formulæ, admitted by the majority of mineralogists, has the imposing authority of Berzelius, Thenard, &c. The formula SiO^2, adopted by a certain number of German chemists and mineralogists, among whom may be mentioned M. Gmelin, possesses the advantage, according to M. Cahours, of making the volume of vapour representing the equivalent of the protosilicate of ethyle of M. Ebelmen enter within the ordinary conditions. Lastly, the formula SiO seems at present to be generally admitted by the majority of French chemists; it was proposed long since by M. Dumas, when he published his beautiful investigation upon the specific gravities of vapours. M. Ebelmen, in his interesting memoir upon silicic æthers, has adopted this latter opinion after a very learned and profound discussion.

As the curious facts of the ætherification of silicic acid do not appear to have resolved the question completely in the eyes of a certain number of chemists, I proposed in the present investigation to ascertain whether it might not be possible to obtain, either by some facts of substitution or by the production of some double chlorides analogous to the double fluorides already known, or lastly, by the production of some new æther or amidogen compounds, results of such a nature as would allow chemists to base their selection upon more explicit and varied data.

The difficulties encountered in the preparation of the majority of these various kinds of compounds, the long and tedious manipulations required by researches of this class, in which the chloride of silicium is the first indispensable material,—all these circumstances combined did not admit of my varying and multiplying the operations to the extent I desired; but imperfect and incomplete as they are, I think they deserve to fix for a moment the attention of chemists.

I. *Compounds derived by Substitution from the Chloride of Silicium.*—The facts exposed in this first part of my investigation may be resumed in the following propositions:—

1. The chloride of silicium may be deprived, at a high temperature, by the action of hydrosulphuric acid, of the whole of its chlorine, and the latter replaced by an equivalent quantity of sulphur,

passing through a series of intermediate compounds containing silicium, chlorine and sulphur.

2. The first of these intermediate compounds, the body $SiSCl^2$, is readily isolated on account of its great stability.

3. The sulphuret of silicium may likewise be obtained perfectly pure.

4. The existence of the compound SiS^2Cl is rendered highly probable from certain reactions which take place between alcohol or wood-spirit and the intermediate chlorosulphuretted products.

In concluding this short notice, it may perhaps not be uninteresting to arrange in a comparative table the expressions representing these different compounds, according to each of the three views of the molecular constitution of the chloride of silicium:

	Si = 266·82.	Si = 177·88.	Si = 88·94.
Chloride of silicium....	$SiCl^3$	$SiCl^2$	$SiCl$
Chlorosulphuret	$SiSCl^2$	$Si^3S^2Cl^4$	Si^3SCl^2
Chlorobisulphuret	SiS^2Cl?	$Si^3S^4Cl^2$?	Si^3S^2Cl?
Sulphuret............	SiS^3	SiS^2	SiS

If regularity and simplicity deserve to be taken into consideration, the preference will undoubtedly be given to the first series with Si = 266·82.—*Comptes Rendus*, May 15, 1848.

ON THE CONSTITUTION OF THE PHOSPHATES OF THE ORGANIC ALKALIES. BY DR. THOMAS ANDERSON.

The author had been led to investigate the phosphates of the organic alkalies, with the view of determining the accuracy of an analysis of the phosphate of strychnia by Regnault, which gave results incomparable with the known constitution of the organic phosphates. He alluded to the investigation of the phosphates of aniline by Nicholson, and proceeded to the statement of his own observations.

Phosphate of Strychnia, with one equivalent of strychnia, was obtained in long truncated needles, by digesting strychnia in tribasic phosphoric acid. It dissolved readily in water, and was acid to test-paper. By analysis it gave results corresponding to the formula $(C^{44}H^{23}N^2O^4HO)2HO\,PO^5$.

The crystallized salt was found to contain four equivalents of water of crystallization.

Phosphate of Strychnia, with two equivalents of strychnia.—By long-continued digestion of strychnia with the foregoing water in solution, an additional atom of the alkaloid is dissolved, and the solution on cooling deposits rectangular tables of a salt which is neutral to test-paper. It is less soluble in water than the acid phosphate, and its constitution was found to be represented by the formula $2(C^{44}H^{23}N^2O^4HO)\,HO\,PO^5$.

Phosphate of Brucia, with two equivalents of brucia, is obtained by the solution of brucia in phosphoric acid, and crystallizes from the concentrated solution in short prisms. The crystals are neutral to test-paper, and contain a large quantity of water of crystallization, which they lose by efflorescence. The formula of the salt is $2(C^{44}$

$H^{23}N^2O^7HO)HO\,PO^5$. A double phosphate of brucia and soda was also formed, but could not be obtained perfectly pure.

Phosphate of Quinine, with three equivalents of quinine.—By digesting quinine with phosphoric acid, a solution of this salt is obtained which becomes a solid mass of silky needles on cooling. They are extremely soluble in hot water, and are quite neutral to test-paper. They gave by analysis a result corresponding with $3(C^{20}H^{12}NO^2HO)PO^5$. These results the author considered sufficient to establish the fact, that the phosphates of the organic alkalies agree in their constitution with the inorganic salts of that acid; and he concluded his paper by observing, that the relation of these bases to phosphoric acid might be made use of as a means of classifying them. Thus quinine, which replaces three equivalents of water in phosphoric acid, might be compared to oxide of lead and the oxides of the heavy metals; brucia might represent the inorganic alkalies; while strychnia, which under ordinary circumstances replaces only one equivalent of water, belongs to a class which has no analogue among the series of inorganic bases.—R. S. Edinburgh. *Jameson's Journal,* July 1848.

ON THE PREPARATION OF CREATINE, &C. BY DR. GREGORY.

After some remarks on the present state of animal chemistry, the author commenced by giving a brief account of the recent discoveries of Liebig in regard to the constituents of the "juice of flesh," or the liquid contained in the substance of the muscles, which is distinguished from the blood by the large quantity of free acid which it contains. This remarkable animal fluid has been found by Liebig to contain phosphoric and lactic acids in large quantity, inosinic acid in small proportion, and some other acids not yet studied; also potash in large quantity, with a little soda, a considerable proportion of magnesia, and a little lime, chloride of potassium, with a little chloride of sodium, and, besides some compounds of animal origin not yet investigated, the new base Creatinine, and the very remarkable substance Creatine, first discovered by Chevreul, but in vain sought for by Berzelius and other chemists.

He then described the process, essentially that of Liebig, by which creatine is extracted from the flesh of quadrupeds, birds and fishes, in all of which hitherto tried it has been found, although in small and variable quantity. A table was exhibited, showing the per-centage obtained from different kinds of flesh and fish; and the result was, that this interesting substance may be most easily and cheaply prepared from fish, especially the cod, herring, salmon and mackerel, all of which yielded much more than beef or horse-flesh, and nearly as much as fowl, which was the most productive. The maximum proportion of creatine was 3·2 per 1000 parts of flesh; the average about 1·5 per 1000.

The author stated that he had found inosinic acid only in the flesh of fowl and turkey; and he is informed by Baron Liebig, that it is quite possible that this acid may also have been confined to the flesh of fowls in his experiments, as it was often absent, although he cannot now ascertain the cases in which it was present.

He concluded by stating, that as creatine is found in the urine along with creatinine, it appears to be, in part at least, a substance intended for excretion. Its crystalline character renders this probable; and at all events, if it has any function to perform in the body, that function is not yet known. It must be regarded, in the mean time, as one of the numerous series of less complex products derived from the decomposition, in the body, of the effete tissues; and although we cannot yet produce it artificially, yet from the rapid progress recently made in the study of the products of decomposition of the albuminous substances, we may hope soon, not only to do this, but also to discover, from these products, the true formulæ of the albuminous compound.—*Ibid.*

RESEARCHES ON THE CONSTITUTION OF THE ATMOSPHERE.

M. Doyère having had particular occasion to examine the phænomena of respiration of man and animals exposed to the influence of the vapour of æther, he was induced to try the protochloride of copper as an absorbent of oxygen in gaseous mixtures. The favourable results of the employment of this reagent having induced him to pursue the study of eudiometry, he succeeded in effecting a combination of instruments with simple means of correction and easy management, which gave the original volume of a gas, and that of the residue which any absorbent leaves, within a ten-thousandth part.

The author was surprised to find that his method indicated larger proportions of oxygen than those generally admitted, and that even among them considerable variations occurred. M. Doyère continued his operations for four months, and the results proved that the composition of the air varies incessantly. In general the variation is slight, and the proportions of oxygen varying between 208 and 210 parts in 1000; but this variation was found to go as low as 205, and as high as 212. These great differences never occurred suddenly; the quantity having diminished or increased as gradually as consists with such a description of facts. M. Doyère shows that his results harmonize perfectly with those of MM. Boussingault and Dumas obtained at Paris;—with those which were obtained by M. Stas at Brussels, and with the great work achieved by M. Lewy with respect to the air of the North Sea, and that of Guadaloupe. He also proves that Dr. Prout's experiments on the weight of the air, and those published by M. Regnault, agree with his view of the subject, and prove that the air is continually varying.

He shows also, that the densities of oxygen and nitrogen given by M. Regnault do not agree with the composition of the air when stated to contain only 209 of oxygen; and that they indicate 213 thousandths of oxygen if the mean density of nitrogen be adopted, and 212 to 215 if the extreme densities resulting from the experiments of M. Regnault be preferred.—*Comptes Rendus*, Février 14, 1848.

RESEARCHES ON ESSENTIAL OILS. BY M. C. GERHARDT.

Oil of Chamomile (*Anthemis nobilis*).—This oil, like the greater number of liquid essential oils, is a mixture of two principles, one oxygenated, the other not. It begins to boil at about 338° F.; the boiling remains long stationary at 356°, and then gradually rises to 410° F.

The oxygenated principle may be isolated by receiving only the last distilled portions of the oil. It undergoes a remarkable metamorphosis: when it is heated for a few minutes with an alcoholic solution of potash, it is converted into a saline mixture, from which the mineral acids separate an oily acid. This is a mixture of a liquid acid, the *valerianic* and a solid acid, which is the *angelicic.*

The identity of the liquid acid with the valerianic was rendered unquestionable by the analysis of the salts of silver and of barium, which gave $C^5 H^9 O^2(Ag)$ and $C^5 H^9 O^2(Ba)$. As to angelicic acid, it crystallizes from boiling water in magnificent crystals, which are volatile, without decomposing, and are very fusible: they contain $C^5 H^7 O^2(H)$. This composition places angelicic acid in the homologous group $nCH^2+O^2-H^2$, which contains anilic, pyroterebic, campbolic, moringic and elaïdic acids.

Angelicic acid was discovered by MM. Meyer and Zenner, who obtained it with valerianic acid by treating angelica root with milk of lime; it is therefore probable that angelica root contains the same essential oil as the chamomile.

M. Deville obtained by the dry distillation of the resin of guaiacum an oil $C^5 H^8 O$, which oxidizes by exposure to the air, and is converted into a substance which crystallizes in beautiful laminæ. This oil is unquestionably *angelicic aldehyde,* belonging to the homologous series nCH^2+O-H^2.

The author did not possess sufficient oil to determine the composition of the oxygenated principle which the two acids described yielded; it may, however, he thinks, be predicted that this substance belongs to the *échelon* C^{10}.

As to the carburetted hydrogen of the oil of chamomile, it remains in a pure state. After treating the oil with potash, it has a very agreeable odour of lemons, boils at 347° F., and appears to be identical with cumen $C^{40} H^{14}$, extracted by the author and M. Cahours from cummin.

Oil of Rue.—This substance was first analysed by M. Will; but M. Gerhardt states he showed three years since that the conclusions of M. Will were inadmissible, and that oil of rue must contain an isomeric of oil of mint, so well analysed by M. Walter. The author remarks that his recent analyses agree perfectly with his opinion.

Oil of rue contains but a very small quantity of carburetted hydrogen; it is composed almost entirely of an oil $C^{10} H^{20} O$, to which the author has given the name of *caprol*: it may be obtained pure by collecting the last distilled portions of the oil. Caprol boils at about 451° F.; it becomes resinous when heated with potashed lime, and reduces ammoniacal nitrate of silver at a boiling heat. These characters show that caprol is the *capric aldehyde* of the homologous series nCH^2+O.

The author has as yet been able so to regulate the oxidizement of caprol as to obtain capric acid only. In employing nitric acid of different degrees of concentration, a homologue of capric acid, *pelar-*

gonic acid, was always produced, the same which M. Pless has recently extracted from the leaves of geranium. Repeated analyses made of the salts of silver, barium and copper, of four different kinds, all agreed with the formula $C^9 H^{17} O^2(Ag)$, $C^9 H^{17} O^2(Ba)$, $C^9 H^{17} O^2(Cu)$.

The barytic salt is obtained in fine pearly laminæ, which are greasy to the touch, and resemble cholesterine. The author succeeded once only in obtaining a barytic salt, less soluble than the pelargonate, and whose composition approached that of the caprate. Oil of rue furnishes an excellent means of preparing pelargonic acid in large quantity. All that is requisite is to heat it for some minutes with nitric acid diluted with an equal volume of water; the reaction then goes on of itself; the product saponified by potash, decomposed by a mineral acid, then rectified by desiccation, yields pelargonic acid.

The author has also transformed caprol into a polymeric solid, *metacaprol,* resembling metænanthol and metaldehyde.—*Comptes Rendus,* Février 14, 1848.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1848.

Chiswick.—June 1. Cloudy and fine: rain. 2. Showery. 3. Showery: fine: clear. 4. Heavy showers: fine. 5. Fine. 6. Uniform haze: fine: clear. 7. Very fine. 8. Cloudy. 9. Very fine: rain. 10. Constant heavy rain. 11. Overcast: cloudy. 12. Cloudy: rain. 13. Heavy showers. 14—16. Very fine. 17. Cloudy and fine. 18. Densely overcast: rain at night. 19, 20. Overcast. 21. Cloudy and fine. 22. Very fine. 23. Hazy: rain. 24. Rain: fine: slight rain. 25. Overcast. 26. Fine. 27. Cloudy. 28, 29. Fine. 30. Rain.

Mean temperature of the month .... 59°·58
Mean temperature of June 1847 .... 58 ·46
Mean temperature of June for the last twenty years ...... 60 ·81
Average amount of rain in June .... 1·88 inch.

Boston.—June 1. Fine: rain P.M. 2. Cloudy. 3. Cloudy: rain P.M. 4. Fine: rain P.M. 5. Cloudy. 6. Fine. 7, 8. Fine: rain P.M. 9. Cloudy: rain P.M. 10. Fine: rain P.M. 11. Cloudy: rain early A.M. 12. Cloudy: rain A.M. and P.M. 13. Rain: rain A.M. 14, 15. Fine. 16. Cloudy: rain A.M. and P.M. 17. Rain: rain A.M. 18. Cloudy: rain A.M. and P.M. 19—21. Cloudy. 22. Fine. 23. Cloudy: rain P.M. 24. Cloudy: rain A.M. and P.M. 25. Cloudy. 26. Fine. 27. Cloudy: rain early A.M. 28. Cloudy: rain A.M. 29. Rain: rain A.M. 30. Fine: rain P.M.—This has been the wettest June for nine years, and May the driest for nineteen years.

Applegarth Manse, Dumfries-shire.—June 1. Showery all day: A.M. clear. 2. Showery all day: thunder. 3. Fine A.M.: rain: thunder. 4. Showery: hail. 5. Fair, but cloudy. 6. Slight showers. 7. Showers: thunder: hail. 8. Showers: thunder. 9. Fine: some drops of rain. 10. Fair and fine. 11. Fine: warm: showers. 12. Rain all day. 13. Showery nearly all day. 14. Showers early: cleared: fine. 15. Fiery wind: fair. 16. Heavy showers P.M. 17. Fine: one slight shower. 18—20. Very fine summer days. 21. Warm: thunder: showers. 22. Fair: wind violent. 23. Slight shower P.M. 24. Fair and pleasant. 25. Fine A.M.: rain P.M. 26—30. Constant showers.

Mean temperature of the month .... 55°·7
Mean temperature of June 1847 .... 55 ·2
Mean temperature of June for the last twenty five years . 56 ·1
Average amount of rain in June for twenty years .... 3·16 inches.

Sandwick Manse, Orkney.—June 1. Showers: fine. 2. Cloudy. 3. Cloudy: rain. 4. Cloudy. 5. Rain: cloudy. 6. Bright: showers. 7. Clear: showers. 8. Clear: cloudy. 9—11. Clear. 12. Clear: cloudy. 13. Drops: rain. 14. Damp: rain. 15. Rain: drops. 16, 17. Fine. 18. Cloudy. 19, 20. Bright: cloudy. 21—25. Cloudy. 26. Bright: cloudy. 27. Rain: damp. 28. Rain. 29. Cloudy. 30. Bright: drops.

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.
1848. June.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
● 1.	29·854	29·656	29·33	29·58	29·34	29·46	29·42	60	49	55·5	52½	44	50	48½	w.	w.	sw.	calm	·08	·20	……	·10
2.	29·417	29·309	28·98	29·19	29·18	29·36	29·38	66	40	59·5	53½	46½	53	50	sw.	s.	sw.	ene.	·07	·15	……	……
3.	29·354	29·270	28·84	29·16	29·20	29·45	29·52	65	40	58	59	38	53½	50½	s.	s.	ne.	ene.	·03	·05	……	·14
4.	29·658	29·465	29·03	29·29	29·40	29·55	29·58	68	47	62	59	44½	51½	50½	sw.	s.	ne.	ne.	·07	·02	……	·11
5.	29·768	29·697	29·21	29·44	29·50	29·55	29·57	70	40	60	57½	46½	49	48½	sw.	w.	se-sw.	w.	……	……	……	·12
6.	29·869	29·787	29·30	29·56	29·56	29·53	29·44	73	46	62	57	43½	54	50	sw.	s.	sw.	s.	……	……	……	……
7.	29·861	29·788	29·35	29·50	29·54	29·46	29·49	73	47	63	58	48	54½	50	sw.	w.	sw.	s.	……	……	……	·11
☽ 8.	29·804	29·766	29·30	29·54	29·55	29·50	29·53	65	43	62	59	41½	55	50	sw.	w.	s-sse.	ssw.	·01	·10	1·40	……
9.	29·821	29·623	29·32	29·53	29·52	29·54	29·60	67	53	61	54	45½	56	49½	sw.	sw.	s.	wsw.	·27	·08	……	……
10.	29·627	29·576	29·14	29·42	29·52	29·57	29·65	63	53	61	61½	46	50	49	sw.	w.	s.	w.	·95	·43	……	……
11.	29·811	29·730	29·26	29·60	29·69	29·76	29·84	71	50	60	62½	44½	53	50	sw.	n.	sw.	e.	……	·06	……	……
12.	29·784	29·515	29·35	29·69	29·61	29·78	29·85	65	55	65	65½	40½	58	52	e.	w.	se.	e.	·78	……	……	·16
13.	29·919	29·499	29·03	29·30	29·27	29·75	29·49	65	42	60	57	49	51	48½	se.	sse.	se.–s.	ne.	·12	·52	……	·02
14.	29·974	29·934	29·48	29·55	29·69	29·42	29·69	73	50	60	59	46½	52	51	s.	se.	sw.	s.	……	·13	……	1·23
15.	29·833	29·770	29·45	29·78	29·83	29·95	30·02	80	56	68·5	68½	51	54	55	e.	se.	se.	se.	……	……	……	·24
○ 16.	29·822	29·748	29·36	29·84	29·83	30·05	30·06	82	54	63	70	49½	62	57	s.	e.	ese.	sse.	·09	·38	1·95	……
17.	29·800	29·754	29·35	29·80	29·75	30·06	30·01	72	54	64	65	55	60	56	sw.	ene.	ese.	e.	……	·40	……	……
18.	29·992	29·865	29·32	29·78	29·95	30·02	30·12	68	52	64	66	51½	59	52	sw.	w.	ese.	calm	·07	·04	……	……
19.	30·142	30·062	29·60	30·05	30·10	30·16	30·18	65	51	56·5	72	49	54	53	ne.	n.	sw-se.	w.	……	·17	……	……
20.	30·130	30·075	29·70	30·09	30·00	30·16	30·01	66	47	57	73	53	58	51½	ne.	……	ne.	w.	……	……	……	……
21.	30·065	30·063	29·60	29·99	29·99	30·10	30·09	77	52	61	73½	54	55	54	ne.	n.	se.	calm	……	……	……	……
22.	30·019	29·866	29·54	29·97	29·87	30·05	30·07	81	55	73	72½	55	54	51	e.	e.	se.	e.	……	……	……	……
23.	29·830	29·591	29·40	29·82	29·64	30·05	29·95	75	55	63	65	56	51½	51	e.	e.	e.	e.	·44	……	0·25	·05
☾ 24.	29·633	29·560	29·11	29·51	29·48	29·83	29·81	69	52	65	62	51½	54	50½	sw.	s.	ne.	ne.	·04	·69	……	……
25.	29·897	29·631	29·17	29·65	29·80	29·85	29·93	65	52	65	64½	49	50½	47½	sw.	wsw.	ne.	n.	·04	·09	……	……
26.	30·016	30·011	29·55	29·89	29·84	29·98	29·84	73	54	63	68½	45	54	48	sw.	n.	e—s.	ese.	·01	……	……	……
27.	29·923	29·776	29·40	29·60	29·43	29·54	29·38	70	57	62·5	60½	53½	54½	54	sw.	w.	s.	w.	·02	·64	……	·06
28.	29·859	29·807	29·25	29·48	29·56	29·35	29·44	69	50	65·5	61	52½	54½	52	sw.	w.	w.	w.	·02	……	……	·26
29.	29·758	29·619	29·10	29·42	29·47	29·52	29·59	71	48	57	63	52	54½	51	sw.	w.	w.	ne.	·01	·07	……	·07
● 30.	29·645	29·538	29·11	29·37	29·41	29·58	29·64	67	37	62	57½	50½	52	46	sw.	calm	ene.	n.	·08	……	2·50	……
Mean.	29·829	29·711	29·30	29·613	29·616	29·731	29·739	69·80	49·36	61·9	62·5	48·4	54·06	50·91					3·20	4·22	6·10	2·67

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XXV. *On the Influence of Temperature upon the Distribution of the Fauna of the Ægean Sea.* *By* Lieut. Spratt, *R.N., F.G.S.*[*]

THE publication by the British Association of the valuable and highly interesting report on the fauna of the Ægean by Professor Forbes, induced me to make some observations on the temperature of that Sea at different depths, so as to ascertain how far temperature might act as an influencing agent, in the distribution of the fauna, throughout the several regions into which he has divided it.

During the summer of 1844 and succeeding summers, I was employed on the survey of the shores of the Grecian Archipelago, and had opportunities of visiting several parts of the Ægean Sea. During favourable weather I made the observations alluded to. The following are some of the localities selected to show the comparative uniformity of the results at distant localities, as well as in different distances from the coast and sea-bottom.

	Region of depth.									
Locality.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Air.	Distance from shore.
Ægina Gulf, July 1845	82°	78°	69°	62°	57°	56°	…°	55½°	88°	1½ mile in 130 fathoms.
Ægina Gulf, July 1845	80	76	69	61	57	…	…	55½	84	3 miles in 210 fathoms.
Ægina Gulf, July 1845	80	78	70	62	57	…	56	…	86	½ mile in 96 fathoms.
Poros Harbour, July 1845	82	79	…	…	…	…	…	…	84	¼ mile in 10 fathoms.
Poros Passage, July 1845	82	…	…	62	…	…	…	…	86	$\frac{1}{10}$ in 30 fathoms.
Zea Passage, May 1846	79	76	68	60	58	…	55½	…	85	2½ miles in 90 fathoms.
Eubœa, June 1846 ……	80	79	…	…	…	…	…	…	87	¼ mile.
Eubœa, July 1846 ……	81	78	70	63	57	56	…	…	84	1½ mile in 70 fathoms.
Eubœa, July 1846 ……	79	74	…	64	…	…	…	…	82	½ mile.
Eubœa, July 1846 ……	84	73	68	62	58	56	…	…	84	6 miles in 70 fathoms.
Eubœa, July 1846 ……	80	75	66	61	57	…	55½	…	89	5 miles in 105 fathoms.
Off Nio, Aug. 1847……	…	…	…	…	…	…	…	55½	86	4 miles in 180 fathoms.
Off Andros ……………	…	…	…	…	…	…	…	55½	…	7 miles in 200 fathoms.
Smyrna Gulf, Sept. 1847	76	75	68	…	…	…	…	…	80	2 miles,
Depth in fathoms ……	2	10	20	35	55	75	105	230		

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[*] Read at the Swansea Meeting of the British Association, August 1848, and communicated by the Author.

It will be seen by this table that the temperatures of the upper zones are frequently two or three degrees higher near the coast than the average temperature of the corresponding depth at the distance of five or six miles, indicating apparently a terrestrial influence.

The average temperature of the air during summer in the Ægean is 86°; and the above observations show that the sea within the littoral zone, viz. under two fathoms, is from 73° to 78°. In the three lower regions the temperature decreases rapidly; so that in the fourth region, at the depth of 35 fathoms, the temperature of the sea is 20° lower than in the first region, being about 60° or 62°. In the sixth region the temperature is about its minimum, viz. 55° or 56°, only 4 or 5 degrees less than in the fourth region. I have not been able to prove the temperature at a lower depth than 230 fathoms, at which it stood $55\frac{1}{2}$°, as in the seventh region.

It is necessary for me to mention the difficulties I experienced in making the observations at great depths correctly, the labours of several hours having been often lost through the motion of the vessel; the twisting and jerking of the line whilst hauling it up again, by which means the steel bar of the Six's thermometer was shaken down the tube; thus showing a higher temperature than what the contents of the dredge showed when brought up with it. I finally found the plan of testing its temperature by the mud brought up more certain than that of lowering the thermometer, when the contents of the dredge was of a sufficient quantity to retain the lower temperature, and the sea-bottom a soft mud, as it almost always is in depths above 100 fathoms. Under that depth, and in corally, gravelly and sandy bottoms, the thermometer must be lowered.

It may thus be doubted whether the mud when brought up from such a depth really showed the temperature of the bottom; the question being, whether it is not lower than the mud indicated, owing to a gain of temperature by passing through regions of a higher grade. I however have no reason to suppose that the temperature is lower than 55° at any depth under 300 fathoms, having often tested the contents of a dredge after it had been some time standing on the deck of the vessel in a tub of water of the surface temperature. The certainty of there being a temperature as low as $55\frac{1}{2}$° in the seventh and eighth region of depth, is sufficient for the remarks I shall make upon its influence upon the distribution of marine fauna.

I will now endeavour to show, by analysing Prof. Forbes's report, that the key to the solution of that remarkable distribution of marine fauna into regions of depth is the decrease of temperature in those descending regions, corresponding to the temperature of the parallel of latitude in which the species are identical or analogous.

I have before pointed out that between the littoral and fourth or fifth region there is a difference of temperature amounting to 25°, and sometimes even to 30°; the upper division of the littoral zone ranging from 76° to 84° during seven or eight months in the year; whilst in the lowest regions, there appears to be a permanent temperature as low as $55\frac{1}{2}°$. Thus we have a region of depth of a temperature corresponding to the summer temperature of a high northern latitude. It seems natural, therefore, that in the fauna of one locality should be found identical or analogous species with the other, considering how great is the influence of temperature in governing the distribution of terrestrial fauna and flora. And I am of opinion, that increased density from depth is not so great an antagonist to the existence of animal life as is generally supposed. Air-breathing animals can soar to highly rarified regions without inconvenience of respiration. The inhabitants of 100 feet above the sea feel the same freedom of circulation that the inhabitants of 10,000 feet feel. It is temperature, and local conditions partially arising from it, that limits the ascent and existence of life. So does the same law appear applicable to marine animals which breathe the medium they inhabit. When we examine the members of the lower regions of depth, we find them as fragile and delicate, if not more so, than their brethren above. I may mention that the *Ophiuridæ* range into all regions yet explored, one species being a traveller from the highest to the lowest. At the depth of 320 fathoms at about forty miles east of Malta, I recently procured a small *Sipunculus*, which appeared however to be peculiarly adapted to the conditions of that depth; its density, it is said, tending to hold light bodies in suspension: the posterior extremity of the *Sipunculus* was truncated so as to form a circular disc, by which it secured itself to the bottom, or any solid body found there: whilst in my possession, it thus attached itself to the bottom of the glass of sea-water in which it was placed.

I shall now proceed briefly to investigate the fauna of each region of depth, as marked out by Professor Forbes.

The first or littoral zone he limits to two fathoms, or twelve feet below the surface: but this, although of so small an amount, the Professor shows "has two well-marked subdivisions; viz. first, the upper or tidal zone of about two feet, between air and water, the inhabitants of which are remarkable, as being such as have a wide geographic range: eight out of the eleven species peculiar to it are widely distributed in the

Atlantic; and one species, the *Littorina cœrulescens,* extending from Tristan d'Acunha to the shores of Norway." It is evident that the inhabitants of this region are subject to great vicissitudes of temperature during the summer and winter changes to which they are exposed. Nature having adapted them to these conditions, we thus find that they are wanderers through every clime; their geographic range corresponding apparently to the local vicissitudes of temperature to which they are subject.

The lower subdivision of this region, and the region below it reaching to the depth of 10 fathoms, is that in which the vicissitudes of seasons is less felt; and that, consequently, in which the average Mediterranean temperature has a more permanent influence, the lowest part of the second region being seldom at a lower temperature than 74° at the sea-bottom. It is in these two regions that Professor Forbes has shown distinctly the influence of local temperature on the fauna; for he remarks "that it is in this belt we have the characteristic species of the Mediterranean fauna."

The third region extends to the depth of 20 fathoms, at which the temperature has fallen to 68°; but it has no very characteristic fauna, Professor Forbes remarking that it may be regarded as a zone of transition between the Mediterranean fauna and that of the region below, where it presents well-marked characters.

The fourth region increases its range to 35 fathoms, at which the temperature has decreased five or six degrees, being generally 62°; and, being so far beyond the influence of atmospheric changes, is of a more permanent standard throughout the year; the consequence is, a more distinctly marked fauna, and one upon which the influence of the decreased temperature is decisive, for we find that the per-centage of Celtic forms in this region is nearly 50 per cent.

In the sixth region at the depth of 75 fathoms, the temperature has nearly reached its minimum of 55° or 56°, a difference of only five or six degrees below that of the fourth region. The Celtic forms are found rapidly to diminish in the lowest region to 20 per cent., whilst in the sixth and seventh regions it stands at 36 per cent. But Professor Forbes remarks, "that although we find fewer northern species in the fauna of the lower zone, the number of forms representative of northern species is so great, as to give them a much more boreal or sub-boreal character than is present in those regions where identical forms are more abundant." It must be remembered, that our knowledge of the northern fauna is not so perfect as that of the Ægean. This may partly be taken into account for the diminished number of identical species; or more probably, this poverty of identical northern species may arise from the peculiar condition of the sea-bottom at this depth. Fuci having become rare in and below the sixth region are replaced by occasional patches of Nullipora, upon which the living Testacea in the lower regions are mostly found, presenting prolific spots in the otherwise barren fields of yellow clay, like an oasis in the desert. These peculiar conditions develope necessarily a peculiar marine fauna, upon which the climatal influence nevertheless stamps its characteristic form throughout the species; and, if identical forms are not known, point to a far north clime for the locality in which they may be found.

The wide range in depth of the eighth region seems to be in unison with its pervading temperature and more uniform condition of the sea-bottom.

Among the Ægean fauna there are several species which have a considerable range in depth. Professor Forbes enumerates nine common to six regions, seventeen species to five regions, and two which ramble throughout the eight regions. "On inquiring into the history of the species," says the author of the report, "which have such a wide range in depth, it is found that more than one half are known to be wide geographic rangers; and from these facts we may fairly draw a general inference, that the extent of the range of a species in depth corresponds with its geographic range." The observation I made on the cosmopolite species of the littoral tidal zone, it will be remembered, is also equally applicable to these wide rangers in depth; for being adapted to climatal changes, they consequently become wanderers over wide geographic space, as they ramble into the representations of climate through regions of depth. Thus we have the climate of a parallel represented in marine depths as it is in terrestrial elevation.

It will be seen by Professor Forbes's report, that the species identical of a southern latitude are very few, these being confined to the characteristic Mediterranean zone, or are wide rangers. This is, consequently, in strict accordance with the climatal influence. As a law resulting from this influence, characteristic tropical and subtropical species will have a limited distribution in geographic space; whilst the boreal and sub-boreal characters will be found in every geographic position where corresponding regions of depth are found with animal life existing, the limit of which I believe to be much lower than 300 fathoms, having examples from 390 fathoms. But I must notice, that the Ægean deep dredges indicate generally a zero of animal life at 300 fathoms, as Professor Forbes was induced to assume. I believe, however, that in the deserts of yellow clay an occasional oasis of animal life may be found in much greater depths, depending upon some favourable local condition or accident.

I cannot close my remarks without noticing the singular fact of the decrease in temperature in marine depths in these parallels being about equal to the increase of temperature in terrestrial depths at 200 fathoms below the surface; in the one case decreasing from 78° and 80° to 55°, and in mines, it is said, increasing to 80° at the same depth. (See Ansted's Geology, vol. ii. p. 294.)

These reasonings and observations on the deeps of the Ægean Sea seem to point out the key to the distribution of the marine fauna generally; and as they are of evident geological importance, I am induced to present them to the notice of the British Association, although so imperfectly carried out, and attended with so little research as to what is already known of the temperature of the sea in other localities. The temperature of great depths in high latitudes was first ascertained by Sir John Ross in 1818, which he found to be 29 degrees at the depth of from 200 to 1000 fathoms. I hope, however, that these observations may lead to an extension of similar observations in each zone of depth, in connexion with the fauna of those zones in other and distant localities, by those members of my profession who have means at command, and more zeal in uniting science with duty.

XXVI. *On the Products of the Destructive Distillation of Animal Substances.* Part I. *By* THOMAS ANDERSON, *Esq., M.D.*[*]

IN April 1846 I communicated to the Royal Society a paper on a new organic base, to which I gave the name of picoline, and which occurs in coal-tar, associated with the pyrrol, kyanol, and leukol of Runge. In that paper I pointed out that the properties of picoline resembled, in many respects, those of a base which Unverdorben had previously extracted from Dippel's animal oil, and described under the name of odorine; and more especially mentioned their solubility in water, and property of forming crystallizable salts with chloride of gold, as characters in which these substances approximated very closely to one another. And further, I detailed a few experiments on the odorine of Unverdorben extracted from Dippel's oil, with the view of ascertaining whether or

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[*] From the Transactions of the Royal Society of Edinburgh, vol. xvi. part 4. Read April 3, 1848.

not they were actually identical, but on too small a scale to admit of a definite solution of the question.

These observations, coupled with the doubts which had been expressed by some chemists, and more especially by Reichenbach, as to the existence of the bases described by Unverdorben, induced me to take up the whole subject of the products of the destructive distillation of animal substances, which has not yet been investigated in a manner suited to the requirements of modern chemistry. In fact, Unverdorben is the only person who has examined them at all; and his experiments, contained in the eighth and eleventh volumes of Poggendorff's *Annalen*, constitute the whole amount of our knowledge on the subject; and his observations, though valuable, and containing perhaps as much as could easily be determined at the time he wrote, are crude and imperfect, when we come to compare them with those which the present state of the science demands. Since his time, the methods of investigation in organic chemistry have undergone an entire change: the simplifications of the process of organic analysis had not then been made, or at least had not come into daily use as the auxiliary of investigation; and Unverdorben, who belonged to the old school, and contented himself with the observation of reactions only, was necessarily led, as I shall afterwards more particularly show, to confound together substances, the reactions of which approximate so closely that it is impossible, or at least very difficult, to distinguish them by such means alone. The errors, however, lay with the method, and not with the observer; for Unverdorben's experiments, so far as they go, I have found to be correct in the main, notwithstanding their having been called in question by Reichenbach, whose numerous researches on the kindred subject of the products of the destructive distillation of vegetable substances gave weight to his opinion, and have indeed been the principal cause of the doubts expressed by others on the subject.

The investigation of these products has occupied me pretty continuously since the publication of the paper before alluded to; and my researches have now extended themselves over a large part of the subject, although, from its branching off into so many subdivisions, and embracing the consideration of so large a number of substances belonging to almost every class of organic compounds, some time must still elapse before it is complete in all its parts. It is my intention, therefore, as the subject naturally divides itself into several sections, to take up the consideration of these in a succession of papers, of which the present is the first, and in which I propose to consider the general properties of the crude product employed in my experiments, and those of certain of the organic bases contained in it.

The products of the destructive distillation of animal matters were long since employed in medicine, and were obtained from all parts of the body, and from almost every section of the animal kingdom; but these afterwards entirely gave way to the Oleum Cornu Cervi, which, as hartshorn is entirely free from fatty matters, must necessarily be the pure product of decomposition of the gelatinous tissues. The more volatile portions of this oil, separated by distillation with water, and purified by numerous rectifications, constituted the Oleum Animale Dippellii of the older pharmacopœias. These substances would in all probability have been the most convenient crude materials for my experiments; but as they have long since ceased to be employed, and cannot now be obtained except by going through the tedious and disagreeable process for their preparation, I have made use of the bone-oil of commerce, which is prepared on the large scale by the distillation of bones in iron cylinders, and can be had in any quantity from the manufacturers of ivory-black. This oil appears to differ in no respect from the true Oleum Cornu Cervi, and, like it, is the product of decomposition of the gelatinous tissues only; for previous to distillation the bones are boiled in a large quantity of water, by which means both the fatty matters and also a certain proportion of the gelatine are separated. They are then dried, packed in the cylinders, and distilled at a heat which is gradually raised to redness. The oily product of this distillation is separated from the watery portion, and, after rectification, forms the bone-oil of commerce; though in some instances this latter distillation is dispensed with, and the product of the first made use of without any further purification.

Bone-oil, as supplied by the manufacturer, has a dark brown, almost black colour, with a somewhat greenish shade, and perfectly opake in the mass; but when spread in a thin layer on a glass plate, it is seen to be brown by transmitted light. Its specific gravity is about 0·970. Its smell is peculiarly disagreeable, and is mixed with that of ammonia, which is always present, though sometimes in so small quantity that its odour is disguised by that of the oil itself, and is only rendered apparent by distillation. A piece of fir-wood moistened with hydrochloric acid, and held over the mouth of a vessel containing it, rapidly acquires the dark reddish-purple colour which is characteristic of the pyrrol of Runge. Acids agitated with the oil acquire a brown colour, especially on standing, and extract the bases contained in it; but if the quantity of the acid be large, and in a pretty concentrated state, a non-basic oil is also dissolved, which, on standing for some time, and more rapidly if heated, undergoes decomposition, and the fluid becomes filled with orange-yellow flocks of a resinous substance, which acquires a dark colour by exposure to the air: this change is produced by the stronger vegetable as well as the mineral acids. Alkalies extract an acid oil, and a considerable quantity of hydrocyanic acid, which, on the addition of an acid to the alkaline solution, and distillation, can be distinguished in the product by its smell, as well as by its reaction with the salts of iron.

Previous to the separation of the bases, the crude oil was again rectified in portions of about fifteen pounds each, in an iron retort,—an operation attended with some trouble, as the fluid is apt to froth up and boil over in the early part of the process, so that the retort must not be more than half-full, and the heat requires to be applied in a very gradual manner. At first a watery fluid distils, containing in solution ammonia, and a small quantity of the most volatile bases. This is accompanied by an oil of a pale yellow colour, limpid and very volatile, which after a time comes over without water, and with an increased though by no means dark colour. The distillation proceeds in a perfectly steady and gradual manner, until about two-fifths of the oil have passed over, when a point is attained at which the temperature requires to be considerably raised, in order that the distillation may continue uniformly, and the product becomes much thicker and more oily in its appearance. At this point the receiver was changed for the purpose of collecting the less volatile portion apart, and the distillation continued until the bottom of the retort reached a red heat. The latter portions of these products were obviously altered during the distillation, for a bulky porous charcoal remained in the retort; the oil which passed over smelt strongly of ammonia, crystals of carbonate of ammonia made their appearance in the neck of the retort, and a certain quantity of water collected in the receiver. The oil also became gradually darker in colour, and more viscid in its consistence. By collecting in a succession of receivers, I had an opportunity of observing a great number of curious optical phænomena at different epochs of the distillation. The oils frequently presented well-marked appearances of epipolic dispersion, and the very last portion exhibited a curious species of dichroism, its colour being dark reddish-brown by transmitted, and green, with the effect of opacity, by reflected light. All these appearances, however, were very evanescent, and are only seen in the newly-distilled oil, for after a few days it becomes very dark-coloured, and they are then no longer visible.

Both the more and the less volatile oils contain a variety of bases, and were separately treated for their extraction. In neither, however, is the quantity large. I obtained from the more volatile portion of three hundred pounds of bone-oil less than two pounds of the mixed bases; but as in the course of the various processes to which it was submitted, some small portions were lost, the whole may perhaps amount to about three-fourths per cent. of the total quantity of oil. The less volatile portion yields a larger quantity, which may be estimated at two or three per cent. of the crude oil. These, of course, are only rough estimates, but they may serve to give an idea of the quantity of the products.

Preparation of the Bases.

For the preparation of the bases precisely the same processes were followed throughout for both portions into which the oil was separated by distillation; and as the bases to be described in the present paper were contained in the more volatile portion, I shall detail the steps followed in reference to that quantity only. The oil was mixed in a cask with sulphuric acid diluted with about ten times its weight of water, and the fluids left in contact for a week or two, during which time they were frequently agitated. More water was then added, and the whole drawn off, and the process repeated with fresh quantities of sulphuric acid as long as any bases were extracted. The solution, which had a reddish and sometimes very dark brown colour, contained the bases, along with a quantity of non-basic oil and of pyrrol. It was mixed with an additional quantity of sulphuric acid, introduced into a glass distilling apparatus, and heat applied. As the fluid approached the boiling-point, a quantity of the red resinous matter before alluded to began to separate, and occasioned succussions of so violent a character as to endanger the safety of the vessel, and render it necessary to interrupt the process for the purpose of filtering it off, after which the distillation proceeded without difficulty. A small quantity of oil distilled over, and the water which accompanied it had exactly the smell of the water in a gas-meter, and contained pyrrol*, which continued to pass over for a long time, during the whole of which the distillation was continued. This distillation I had recourse to at first, from a suspicion that some of the bases were separated from the acid, and volatilized

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* These odours were so exactly alike, that I was induced to seek for pyrrol in the water of gas-meters; and I found that when mixed with sulphuric acid and distilled, the product gave the characteristic reaction of pyrrol with fir-wood. Ammonia remained in combination with the sulphuric acid.

during the process; but so soon as I had ascertained that this was not the case, it was dispensed with, and the fluid boiled down in porcelain or copper evaporating basins, water being added, and the distillation continued until, by taking a small quantity of the fluid and distilling off a few drops in a retort, they were found to be free from pyrrol. Even this precaution soon became unnecessary, for a little experience enabled me to know when the evaporation had been sufficiently prolonged.

The dark brown fluid which remained in the basins was once more strained, in order to get rid of such resinous matters as might have separated during the evaporation, and then distilled in a large glass balloon connected with a condenser, after the acid had been previously supersaturated by a base. For this purpose, potash, soda, and lime were indifferently made use of: the latter answers extremely well; but owing to the large quantity of sulphate of lime separated, the distillation requires to be carried on in the chloride of calcium or oil-bath. When the alkali is added in sufficient quantity, an oil floats up to the surface of the fluid, and a strong pungent odour is given off, in which that of ammonia is apparent, along with another which can be compared to nothing but the smell of stinking lobsters. At the first part of the distillation a transparent and colourless watery fluid passed over, which contained the bases in solution; but after this had continued for some time, an oil made its appearance running in globules down the tube of the condenser, and dissolving immediately in the fluid which had already distilled. When the bases ceased to distil in quantity, the receiver was changed, and a small quantity of oil heavier than water was obtained by continuing the distillation for some time. At the end of the process an oil remained floating upon the concentrated fluid in the balloon, the quantity of which is very variable, and depends on the distillation of the crude bone-oil, having been continued too long before changing the receiver. In fact, it contains some of the bases of the less volatile oil, and will come to be considered in an after part of the investigation.

To the product of the distillation sticks of caustic potash were added; and as these dissolved, the oily bases separated from the fluid in a manner exactly similar to that which was observed in the preparation of picoline, as detailed in the paper to which reference has already been made. The alkaline solution was drawn off by means of a syphon, and more potash added as long as water was separated. In this way the greater part of the base was obtained; but a small quantity of the most volatile of all still remains in the alkaline solution, and cannot be separated except by the addition of a very large quantity of potash. It was, however, readily obtained by distilling the fluid, and collecting only the first portion of the product, from which it was separated by a comparatively small expenditure of potash. The small quantity so obtained was preserved separately from the large mass.

The product of this operation was found to be extremely complex, and to consist of a mixture of four or five different bases, exclusive of ammonia. For the purpose of obtaining these in a separate state, a great variety of processes was attempted; but none were found to answer so well as fractionated distillation, although it is an extremely tedious method of separation, and occasions a considerable loss of substance, which is very annoying when the quantities obtained are so small. When the mixed bases were distilled with a thermometer, ammonia began to escape at a very low temperature; but at 160° Fahr. the fluid entered into steady ebullition, and a perfectly transparent and limpid oil began to distil. A small quantity of oil passed over between this temperature and 212°, which was received by itself, and the after products collected in a succession of receivers, which were changed at every ten degrees which the thermometer rose. The fluid continued in steady and rapid ebullition, and the thermometer ascended rapidly to 240°, and between that and 250° a considerable quantity was collected. It then again went up pretty rapidly, and another large quantity was obtained between 270° and 280°; after which the distillation proceeded more slowly until the temperature rose to 305°, at which point the characters of the products underwent a complete change. All the substances obtained at lower temperatures dissolved instantaneously in water; but that which now distilled floated on the surface, and only dissolved on agitation with a considerable quantity of water. Distillation now continued with somewhat greater rapidity, till the thermometer rose to about 355°, when a drop of the product allowed to fall into a solution of chloride of lime immediately gave the reaction of aniline. When this was observed, the whole remaining products, which formed only a small fraction of the whole, were collected together. They consisted chiefly of aniline.

The products of these different distillations were repeatedly rectified, and by this means bases were obtained corresponding to the points at which the thermometer was found to remain longest in the first distillation. Of these I have as yet examined only the most volatile, and that which boiled at about 270°.

Petinine.

The most volatile portion of the bases obtained by the fractionated distillation was mixed with the small portion which was separated with difficulty from the potash solution, and had been kept separate from the large quantity. The mixed fluid still contained a large quantity of ammonia, for the separation of which it was again rectified several times in succession, and fractionated in a small retort, the receiver being kept carefully cool. After this process has been repeated until it is properly purified, it constitutes the base to which I give the name of petinine (from πετεινὸς, *volatilis*), in allusion to its volatility, which is greater than that of any base yet known, with the exception, of course, of ammonia. The quantity of this substance contained in the bone-oil is excessively small, as I obtained from three hundred pounds no more than was sufficient for the determination of its constitution, and the leading characters of a few of its compounds. It is probable, however, that some loss was incurred in the preliminary processes, as, from not anticipating the presence of so volatile a base, I did not take any precautions for producing complete condensation of the products, by means of freezing mixtures or otherwise; and a considerable quantity was also lost owing to my anxiety to expel completely the ammonia which it might retain.

Constitution of Petinine.

The petinine employed for analysis was very carefully dried over caustic potash, the fluid poured off after some days' contact, and distilled in the water-bath at a very gentle heat; a precaution which is rendered necessary by its dissolving a certain quantity of potash. I did not possess a sufficient quantity to make a determination of the nitrogen, but took it for granted that oxygen was absent; an assumption which is justified by the analogy of all the other volatile bases, as well as by the perfect coincidence of the experimental results with the calculated formulæ. It was analysed with oxide of copper in a very long tube, and gave the following results:—

6·663 grains of petinine gave
16·286 ... carbonic acid, and
8·382 ... water;

corresponding exactly with the formula $C_8 H_{10} N$, as is shown by the following comparison:—

	Experiment.	Calculation.		
Carbon . .	66·66	66·66	C_8	600·0
Hydrogen .	13·97	13·88	H_{10}	125·0
Nitrogen .	19·37	19·44	N	175·0
	100·00	100·00		900·0

In order to ascertain the atomic weight of petinine, I prepared its compound with chloride of platinum, and made the following determinations of the platinum contained in it:—

I.	6·351	grains of	chloride of platinum and petinine gave
	2·245	...	platinum = 35·34 per cent.
II.	3·860	...	chloride of platinum and petinine gave
	1·372	...	platinum = 35·54 per cent.
III.	2·844	...	chloride of platinum and petinine gave
	1·010	...	platinum = 35·51 per cent.

The atomic weights deduced from which agree very closely with the calculated results.

I. Atomic weight, by experiment	910·3
II.	891·2
III.	894·2
Mean	898·5
Calculation	900·0

The mode in which this base is formed during the decomposition of gelatine, it is of course impossible at present to perceive. In its chemical relations it is, however, in all probability related to the butyric series; and it is even possible that we may obtain it by artificial processes. Some time since, Kolbe* published some researches on the galvanic decomposition of valerianic acid, among the products of which he discovered a carbo-hydrogen, having the formula $C_8 H_9$. Now by treating this substance in the same manner as benzine is acted upon for the preparation of aniline, we ought to obtain from it, if not petinine, at least an isomeric compound, as may be easily seen by comparing the formulæ of the different substances:—

Benzine . . .	$C_{12} H_6$	$C_8 H_9$	Kolbe's carbo-hydrogen.
Nitro-benzide .	$C_{12} H_5 (NO_4)$	$C_8 H_8 (NO_4)$	Action of nitric acid.
Aniline . . .	$C_{12} H_7 N$	$C_8 H_{10} N$	Petinine.

I have not yet had an opportunity of determining whether the change which theory would lead us to expect actually takes place, but there is every reason to suppose that it would.

Properties of Petinine.

Petinine is a transparent colourless fluid, limpid as æther, and possessing a high refracting power. It has an excessively pungent odour resembling that of ammonia, and yet quite distinct; for when the effect of its pungency has gone off, or it is smelt in a dilute state, its smell is disagreeable, and somewhat similar to that of decayed apples. Its taste is hot and very pungent. It boils at a temperature of about 175° F.;

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* Philosophical Magazine, vol. xxxi. p. 348.

but the quantity which I possessed was too small to admit of an accurate determination either of this point, or of its specific gravity, although the latter is certainly less than that of water. Petinine is a very powerful base, and immediately restores the blue colour of reddened litmus, and gives abundant fumes when a rod dipped in hydrochloric acid is held over it. It unites with the concentrated acids, with the evolution of much heat. It dissolves in all proportions in water, alcohol, æther, and the oils; and is also soluble in dilute solution of potash, but not in concentrated. Petinine gives double salts with bichloride of platinum and corrosive sublimate, both of which are soluble in water. With chloride of gold it gives a pale yellow precipitate, which does not dissolve on boiling the solution, and is not deposited in crystals. Petinine throws down peroxide of iron from its compounds. It also precipitates salts of copper, and the oxide thrown down dissolves in excess of the base with a fine blue colour.

These properties agree with those of none of the bases described by Unverdorben; and, in fact, it is certain that petinine could not have been present in the mixture to which he applies the name of odorine, for he expressly states that it commenced boiling at 212°. And it is easy to see why he did not obtain it; because in separating the bases from the acid by which they were extracted from the crude oil, he took care to add a quantity of potash just so great that the oily bases were liberated, and not the ammonia; and as his object in doing so was to get rid of the latter substance, and there being no means of doing this exactly, it is probable that he did not fully separate the bases, but the most volatile, which is also the most powerful, remained in combination with the acid along with the ammonia.

Compounds of Petinine.

The minute quantity of petinine which I obtained has necessitated a very cursory examination of its salts, which are interesting, both from the facility with which they crystallize, and their great stability. None of them undergo change in the air, but may be left exposed for any length of time without acquiring colour. They are all soluble in water, and those with the volatile acids sublime without decomposing, and are deposited in crystals upon cold surfaces.

Sulphate of Petinine is obtained by adding petinine to dilute sulphuric acid until the fluid is neutral. On evaporating, petinine is given off; and the solution, when concentrated to a syrup, concretes on cooling into a foliated mass of crystals of an acid sulphate. These crystals are strongly acid to test-paper, extremely soluble in water, and slightly deliquescent in moist air.

Nitrate of Petinine.—The solution of petinine in nitric acid, evaporated to dryness, and gently heated on the sand-bath, gives a sublimate of the nitrate in fine woolly crystals.

Hydrochlorate of Petinine.—Hydrochloric acid combines with dry petinine, with the evolution of much heat, and the formation of a salt which is extremely soluble in water, and sublimes in fine needle-shaped crystals.

Chloride of Platinum and Petinine.—If bichloride of platinum be added to a dilute solution of hydrochlorate of petinine, the salt formed remains in solution; but when both substances are concentrated, it falls as a pale yellow precipitate, which was purified by crystallization from hot water. On cooling, the fluid, if sufficiently concentrated, becomes entirely filled with exceedingly beautiful golden-yellow plates, resembling those of crystallized iodide of lead. It is pretty soluble in cold water, extremely so in hot, and is not decomposed by boiling the solution. It is also soluble in alcohol.

9·552 grains of chloride of platinum and petinine gave
5·930 ... carbonic acid, and
3·593 ... water.

By three determinations of platinum, the details of which have been already given, the mean per-centage of platinum was found to be = 35·46.

These results correspond with the formula $C_8 H_{10} N, H Cl, Pt Cl_2$.

	Experiment.	Calculation.		
Carbon .	16·93	17·26	C_8	600·0
Hydrogen	4·17	3·96	H_{11}	137·5
Nitrogen .	...	5·04	N	175·0
Chlorine .	...	38·29	Cl_3	1330·4
Platinum .	35·46	35·45	Pt	1232·0
		100·00		3474·9

Chloride of Mercury and Petinine.—A solution of petinine in water, added to a solution of corrosive sublimate, gives a white precipitate, which dissolves in a considerable quantity of hot water, from which it is again deposited in crystals. It is much more soluble in alchohol; and the boiling solution gives a deposit of beautiful silvery plates on cooling. It is decomposed by boiling its watery solution, petinine being driven off, and a white powder deposited. It is readily soluble in the cold in dilute hydrochloric acid, probably with the formation of another double salt.

Products of Decomposition of Petinine.

The want of substance, which prevented the full investigation of the salts, has likewise curtailed this branch of the subject to a very few observations; which is the more to be regretted, as the general properties and low atomic weight of petinine give promise of definite products, which might enable us fully to determine its position in the chemical system.

When treated with concentrated nitric acid, it dissolves without any remarkable phænomena, and on boiling, a feeble evolution of nitrous fumes takes place; but the petinine is attacked only to a very small extent; for after being kept boiling for a long time, and then supersaturated with potash, it evolved the smell of the base apparently unchanged. Solution of chloride of lime immediately acts upon it in the cold, and developes a highly irritating odour, and some compound is manifestly produced: the solution remains colourless. Bromine water dropped into an aqueous solution of petinine occasions the precipitation of a yellow oil heavier than water, and insoluble in acids: the solution contained hydrobromate of petinine. From the analogy of the other volatile bases, we should expect this to be tribromopetinine, $C_8 H_7 Br_3 N$. My material being exhausted, I was not able to extend these observations further.

Picoline.

Having determined the properties of petinine, I next turned my attention to that portion of the mixed bases which boiled between 270° and 280°, where I had every reason to expect the presence of picoline. After several rectifications, in each of which the first and last portions of the product were separated, I obtained a fine colourless transparent oil, possessed of all the properties of that substance. It dissolved readily in water; gave, with chloride of gold, a fine yellow compound depositing in needles from the hot solution, and with bichloride of platinum, a salt crystallizing in orange-yellow needles, analogous in all its properties to that of picoline. This identity was confirmed by analysis, which gave the following results:—

5·648 grains of picoline from bone-oil gave
15·990 ... carbonic acid, and
3·998 ... water.

Carbon .	77·21	77·41	C_{12}	900·0
Hydrogen	7·86	7·53	H_7	87·5
Nitrogen .	14·93	15·06	N	175·0
	100·00	100·00		1162·5

For still further security, a determination of the platinum in its double salt with the chloride was made:—

$$\begin{cases} 12{\cdot}784 \text{ grains of chloride of platinum and picoline gave} \\ 4{\cdot}204 \quad \ldots \quad \text{platinum.} \end{cases}$$

This corresponds to 32·88 per cent., and the calculation gives 32·94.

The suspicion, then, of the occurrence of picoline in the odorine of Unverdorben turns out to be perfectly correct; at the same time my experiments have clearly shown that odorine is a mixture of picoline, with at least one other base, the properties of which will be detailed in the second part of this investigation. The quantity of picoline contained in bone-oil is considerable, and it can be more readily prepared from that substance than from coal-tar naphtha; in fact, I obtained from three hundred pounds of bone-oil a larger quantity of picoline than that employed in my examination of it, which was obtained from some hundred gallons of coal-tar naphtha; and by means of it, I shall be enabled to trace out the products of its decomposition, which I was unable to pursue in my former communication.

The presence of aniline in bone-oil I have already alluded to; and its quantity, though small, is by no means inconsiderable, when compared with that of the other bases. I did not think it necessary to take any further means for its identification than its highly characteristic reactions with chloride of lime and nitric acid.

The investigation of the other bases is not yet in a sufficiently advanced state for publication. The sparingly soluble one has been especially troublesome, and its purification is attended by difficulties which I have not yet fully overcome. The consideration of these will be taken up in the second part of this investigation.

XXVII. *Remarks on the Weather during the Quarter ending June* 30, 1848. *By* James Glaisher, *Esq., of the Royal Observatory, Greenwich*[*].

THE meteorological returns for the past quarter furnished to the Registrar-General have been obtained from thirty-seven different places, situated between the latitudes of 50° and 55°, and between the longitudes of 5°·18′ W. and 0°·16′ E.

The results from every place have been examined and further reduced by myself. The following are the particulars of the weather during the quarter ending June 30, 1848.

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[*] Communicated by the Author.

The weather during the first month of this quarter was a continuance of the wet weather of the two preceding months; that during May was extremely fine; and that in the month of June was changeable, wet and dull. Till April 5, the daily temperatures of the air exceeded the averages of the same days of seven previous years by 11°·9, 12°·8, 15°·6, 16°·1 and 7°·2; on the 6th it was below the average, and for the most part continued below, till May 2, at times to a great extent; from this time till the 30th of May the daily temperatures exceeded their averages by quantities varying from 2° to 15°. From May 30 to the end of the quarter the daily temperatures were below their average values, with the exception of eight days only.

In pursuance of the arrangement I have hitherto followed, I will speak of each subject of investigation separately.

The mean temperature of the air at Greenwich—

For the month of April was 47°·6, which is 0°·6, 2°·4, 0°·5 *above* those of the years 1841, 1842, and 1843 respectively, 4°·1 *below* that in 1844; 1°·3, 0°·5, and 2°·3 *above* those of the years 1845, 1846 and 1847; or it is 0°·5 *above* the average of these seven years;

For the month of May was 59°·7, which is 2°·9, 6°·5, 7°·5, 6°·8, 10°·3, 5°·1, and 3°·3 *above* those of the years 1841 to 1847 respectively; or it is 6°·1 *above* the average of these seven years;

For the month of June was 58°·5, which is 2°·1, 2°·2, and 0°·5 *above* those of the years 1841, 1843, and 1847 respectively, 4°·4, 2°·2, 2°·2, and 6°·8 *below* those of the years 1842, 1844, 1845, and 1846, respectively; or it is 1°·6 *below* the average of these seven years.

The mean value for the quarter was 55°·3; that for 1841 was 53°·4; for 1842 was 55°·8; for 1843 was 51°·9; for 1844 was 55°·1; for 1845 was 52°·1; for 1846 was 55°·7; and for 1847 was 53°·2; so that the excess for this quarter *above* the corresponding quarter in the years 1841, 1842, 1843, 1844, 1845, and 1847, was 1°·9, 1°·5, 3°·4, 0°·2, 3°·2, and 2°·1; the only year between 1841 and 1847 whose mean temperature for this period exceeded that for the present year was 1846; the difference, however, is small, being 0°·4 only. The average value for this quarter from the seven preceding years was 53°·6; so that the mean temperature of the air for the quarter ending June 30, 1848, exceeds that of the corresponding quarter in the preceding seven years by 1°·7. In the quarter ending March 31, 1848, this value was found to be 1°·7 in excess; and in that ending Dec. 31, 1847, it was found to be 3°·4 in excess; so that the mean temperature of the air in the nine months ending June 30, 1848, exceeds the average value for the same period of time in the preceding seven years by 2°·3.

The mean temperature of the evaporation at Greenwich—

For the month of April was 44°·5, which is 0°·4 *above* that for the preceding seven years;

For the month of May was 53°·0, which is 2°·6 *above* that for the preceding seven years;

For the month of June was 54°·4, which is 1°·2 *below* that for the preceding seven years.

The mean value for the quarter was 50°·6, which is 0°·6 *above* the average for the seven preceding years.

The mean temperature of the dew-point at Greenwich—

For the month of April was 41°·4, which is 0°·7, 3°·1, 0°·8, and 4°·2 *above* those for the years 1841, 1842, 1845, and 1847. respectively; 1°·2, 2°·8, and 0°·9 *below* those of the years 1843, 1844 and 1846; or it is 0°·6 *above* the average of these seven years;

For the month of May was 48°·6, which is 1°·9, 2°·7, 4°·0, and 0°·6 *above* those for the years 1842, 1844, 1845, and 1846 respectively, 2°·2 and 0°·2 *below* those of the years 1841 and 1843, and is the same as that for 1847; or it is 0°·9 *above* the average for these seven years;

For the month of June was 51°·6, which is 2°·4, 0°·4, 1°·8 *above* those for the years 1841, 1843 and 1847 respectively; 2°·7, 3°·6, and 4°·4 *below* those of the years 1842, 1845 and 1846 respectively, and is the same as that for 1844, or it is 0°·8 *below* the average for these seven years.

The mean value for the quarter was 47°·2, which is 0°·2 *above* the average for the corresponding period of the preceding seven years.

The mean weight of water in a cubic foot of air for the quarter was 3·8 grains, which is 0·1 grain *less* than the average for the seven preceding years.

The additional weight of water required to saturate a cubic foot of air was 1·4 grain. The average for the seven preceding years was 1·2 grain. The value required in May was 2 grains, and the mean value for May from the preceding seven years is 0°·9 grain only.

The mean degree of humidity of the atmosphere for April was 0·794, for May was 0·664, and for June was 0·768; these values being *less* than their averages for the seven preceding years by 0·012, 0·154, and 0·012 respectively. The value for the quarter was 0·742, which is 0·059 *less* than the average for these years.

The mean elastic force of vapour for the quarter was 0·343 inch, which is of the same value as the average of the seven preceding years.

The mean reading of the barometer at Greenwich for April was 29·589 inches, for May was 29·926 inches, and for June was 29·642 inches; these values are 0·164 inch *below*, 0·158 inch *above*, and 0·167 inch *below* respectively the averages for the seven preceding years. The mean value for the quarter was 29·719 inches, which is 0·058 inch below the average for these years.

The mean reading in February was 29·517 inches, in March was 29·505 inches, and in April, as above, 29·589 inches. There is no similar instance in this century of the mean reading of the barometer for any three consecutive months being so small as this; the nearest approach to it was in the months of November and December, 1803, and January, 1804.

The average weight of a cubic foot of air under the average temperature, humidity, and pressure, was 531 grains; the average for the seven preceding years was 533 grains.

The *rain fallen at Greenwich* in April was 3·4 inches; in May was 0·4 inch; and in June was 3·5 inches; the average values for the seven preceding years were 1·3 inch, 1·6 inch, and 1·5 inch respectively. The amount fallen in the quarter was 7·3 inches, which is 2·9 inches above the average of the corresponding quarters of seven previous years. The total amount fallen in this year till June 30 was 15·2 inches, which is nearly six inches *above* the average fall in this period as deduced from the above-mentioned years. So large a fall of rain as 7·3 inches has not occurred at the Observatory within the corresponding quarter since the year 1824; and so large a fall as 15 inches within the first six months of the year has not taken place within the previous thirty-three years,—probably not within this century.

The temperature of the water of the Thames was 60°·7 by day, and 59°·6 by night. The water, on an average, was 3°·6 warmer than the air.

The horizontal movement of the air was about 114 miles daily, being about its average value.

The highest and lowest readings of the thermometer in Air at the height of four feet above the ground, and protected as much as possible from the effects of radiation and rain, were 80°·0 and 32°·0.

The average daily range of the readings of the thermometer in Air at the height of four feet, were 16°·7, 30°·5, and 17°·7 in the months of April, May, and June respectively. The average ranges for these months from the observations of the seven preceding years were 16°·7, 17°·6, and 19°·4. The range in the month of May exceeded the average value for that month by 12°·9, and it was larger than the mean daily range in any month in the preceding seven years. The next largest mean daily range was 22°·5, which took place in the month of June 1846. The average for the quarter was 21°·6, being 3°·4 in excess over the average for the seven years ending 1847.

In April the *readings of the thermometer on grass* were at or below 32° on twelve nights, and the lowest reading was 25°. In May they were at or below 32° on fourteen nights, and on eleven other nights the readings were below 40°. In June the lowest reading was 31°·5: on six nights the readings were between 32° and 40°. The amount of heat radiated at night from the earth in the month of May was very great indeed. The observer at Leeds says, that white frosts were almost of nightly occurrence during this month. The observer at Beckington speaks of the severe frost of the 30th of June, and which was general over the south of England.

The mean amount of cloud for April was 7·3, for May was 3·0, and for June was 7·4. The month of May presented this remarkable peculiarity,—that the sky was absolutely cloudless both day and night during the first eight days, and almost free from cloud till the 15th day, the atmosphere being free from haze during this time. These circumstances are without a parallel on record. The sky during the months of April and June was more clouded than usual, so that the mean amount for the quarter, viz. 5·9, is only 0·2 *less* than the average for the corresponding quarter of the seven previous years.

There were three exhibitions of the *aurora borealis* during the quarter, which occurred on April 3, 7, and 29.

The *electricity* in the atmosphere during the month of April was generally in an active state, and rather more than the average amount. It was frequently negative, which circumstances always preceded or occurred during the fall of rain. In the month of May the amount of electricity was small, and particularly during the first half of the month, excepting on the 3rd, 4th, and 5th, on which days very active positive electricity was shown. Till the last week in June the electricity was frequently active, being mostly positive, and at times negative. Generally the electricity was positive at all times when rain was not falling, and at times after rain had been falling for some time.

Thunder-storms at different parts of the country occurred on April 1, 2, 7, 17, 19, May 10, 14, 15, 18, 20 and 26, June 12 and 22. The heaviest of these storms occurred on June 12, and extended over all the southern parts of the country, including latitude 52°; north of this parallel very heavy rain fell. At many places on this day more than an inch of rain fell in a few hours. Generally, however, the storms during this quarter, and more particularly in the month of April, were local, in many cases not extending beyond a radius of a mile. The observer at Cardington says, "On April 2, at 4 P.M., an exceedingly heavy storm of hail and rain fell; within twenty minutes water to the depth of 0·64 inch was collected." By inquiries it appeared that this storm was confined within a circumference of three miles. The observer at Whitehaven says, "that on May 14 a violent thunder-storm occurred, accompanied by a heavy fall of triangular pieces of ice; near Grasmere, garden plants, shrubs, and vegetables were completely riddled, and eighty panes of glass were broken in a conservatory by the ice-shower. Though the ground was previously quite warm, the hail, or rather ice, remained on the ground for several hours, and in some places till the following morning."

This storm continued for about forty minutes, and was confined to a radius of less than two miles. Many other storms of a similar character took place in different parts of the country.

Snow fell at Saffron Walden on April 9, at Greenwich*, Lewisham, and Stone on April 10, and at Leeds on April 11. The flakes at Stone measured three inches by two inches.

The mean *monthly temperatures* of the places in Cornwall and Devonshire have not been very different from those at other places during this quarter; usually in the summer months they are below those of other places.

The reading of the barometer during the month of April was fluctuating, and exhibited a continuance of those oscillations and low readings mentioned in last report in February and March. On April 1, at 9^h A.M., the reading was 29·969 inches; slight variations only took place till the 5th, on this day the reading decreased 0·3 inch, and on the 6th, at 6^h P.M., was 29·516 inches, and still decreasing; on the 8th, at 6^h P.M., it was 29·198 inches, it then turned to increase, and was 29·330 inches at midnight. On the 9th, at noon, it was 29·430 inches, when it again turned to decrease, and was 29·183 inches on the 10th at midnight; shortly after this the reading increased, and was 29·639 inches on the 11th at 3^h P.M.; it again decreased, and was 29·301 inches on the 12th at 6^h A.M.; it then turned to increase, at first slowly, and then quickly. The reading on the 12th at midnight was

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* The fact of snow having fallen on April 10 at Greenwich and Lewisham, was inadvertently omitted in the remarks accompanying the Registrar-General's report.

29·928 inches; it then turned to decrease, and was 29·179 inches on the 18th at midnight; it continued at about this value on the 19th and 20th; it then began to increase slowly, and reached 29·8 inches on the 26th at midnight; it then alternately decreased and increased by small quantities till the last day, when for the first time in the month it reached the point 30 inches.

From May 1 to 14 its readings were above 30 inches, the highest being 30·217 inches on the 11th at 6^h^ A.M; this value decreased to 29·155 inches on the 17th at 6^h^ P.M.; it increased to 30 inches on the 20th, and to 30·169 inches on the 24th; it passed below 30 inches on the 26th, and to the end of the month the changes were small.

During June the reading was generally low; its extreme readings were 29·143 inches on the 3rd, and 30·015 inches on the 20th.

The heavy rains in April, following the wet weather of February and March, caused the land to be in a soddened state, and rivers generally to be much swollen. The thunder-storms in many places did much damage. The months of February, March, and April were so wet that the spring corn was sown with much difficulty. The month of May was distinguished by high temperatures, cloudless skies both day and night for a long period, very small falls of rain, with only the average amount of water mixed with the air, notwithstanding the high temperature, so that the degree of humidity was small. The earth became sun-baked, and so hard as to be almost unbreakable; vegetation was greatly checked. During the month of June the earth became again saturated; the crops improved, and at the end of the quarter there was every prospect of a full average produce.

The observer at Leeds says, "that in April the diseases in the lungs affecting cattle and sheep were extremely frequent and generally fatal. In May, notwithstanding the great heat during the days, the almost nightly occurrence of white frosts checked the growing vegetation greatly; the disease among cattle and sheep was in a great measure stopped. In June, with the return of wet and cold weather, the disease among cattle again appeared. So fatal a season to milch cows has not occurred within my remembrance in this neighbourhood. The potato crop is free from disease, and this vegetable is so abundant, that I have never known it so cheap before at this season."

The observer at Beckington says, "I have heard a good deal of the potato disease in this parish, but my own are in as healthy a state as they have ever been. The severe frost on Friday night, the 30th of June, struck the potato haulm, and did much damage to the peas."

The approximate mean *monthly values* of the several subjects of investigation are printed in the Registrar-General's quarterly report, for the time or times of the day that the observations have been made. These numbers have been reduced as follows:—First, for diurnal variations to deduce true monthly values for each element; secondly, the reduced monthly mean "elastic force of vapour" was taken from the reduced "barometer readings;" thirdly, the mean of these reduced monthly values were then taken; reducing that for the barometer to the level of the sea, and in this way the subjoined quarterly table was formed.

From the numbers in the first column it seems that the volume of dry air was the same at all parts of the country. The mean of all these results is 29·554 inches, and this value may be considered as the pressure of dry air for England during the quarter ending June 30, 1848.

From the numbers in the second column it seems that the mean temperature of the air for the quarter ending June 30, 1848, in the counties of Cornwall and Devonshire, was 54°·1; at places situated south of latitude 52° was 54°·0; between the latitudes of 52° and 53° was 53°·6; between the latitudes of 53° and 54° was 52°·0; and of Durham and Newcastle was 50°·7.

The average daily range of the temperature of the air in Cornwall and Devonshire was 15°·3; at Brighton, Liverpool and Whitehaven, was 14°·9; south of the latitude of 52° was 21°·3; between the latitudes of 52° and 53° was 20°·3; between the latitudes of 53° and 54° was 19°·5; and of Durham and Newcastle was 14°·9.

The greatest mean daily ranges took place at Latimer, Hartwell, Aylesbury, and Beckington respectively; and the least occurred at Liverpool, Brighton, Whitehaven, and Newcastle respectively.

The highest thermometer reading during the quarter was at Leeds, which was 88°; and the lowest was also at Leeds, viz. 23°. The extreme range of temperature in England during the quarter was therefore 65°, but this is probably somewhat too great.

The average quarterly range of the reading of the thermometer in Cornwall and Devonshire was 42°·5; at Brighton, Liverpool, and Whitehaven, was 37°·7; at all other places, except Beckington, Hartwell, Leeds and Wakefield, was 51°·5.

The direction of the wind has been so variable, that it is not possible to determine its mean direction. Observers in

Meteorological Table for the Quarter ending June 30, 1848.

Names of the places.	Mean pressure of the atmosphere of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Range of the thermometer.	Wind. Mean estimated strength 0—6.	Wind. General direction.	Mean amount of cloud 0—10.	Rain. Number of days on which it fell.	Rain. Amount collected.	Mean weight of vapour in a cubic foot of air.	Mean additional weight required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
	in.	°	°	°	°	°					in.	grs.	gr.		in.	grs.	feet.
Helston	29·539	53·5	78·0	30·0	15·8	48·0	1·6	....	4·3	35	8·3	4·0	1·0	0·804	4·7	531	106
Falmouth	....	53·4	77·0	30·0	18·4	47·0	1·5	n.e. & s.w.	5·6	40	8·5						
Truro	....	52·1	70·0	35·0	11·8	35·0	0·9	Variable.	5·3	45	9·4						
Torquay	....	55·1	72·0	37·0	12·3	35·0	2·3	n.e.	4·3	38	9·1	3·8	1·1	0·767	......	......	120
Exeter	29·549	56·3	77·5	30·0	18·0	47·5	2·0	n.e.	2·4	42	8·4	3·7	1·4	0·725	4·6	530	140
Brighton	29·588	54·2	77·5	30·0	12·8	47·5	......	n.e.	4·3	39	......	4·0	1·1	0·778	4·1	533	60
Chichester	....	52·6	77·0	30·0	16·2	47·0	......	n.	......	......	7·6						
Southampton	....	54·5	81·5	28·0	21·2	53·5	0·7	s.w.	5·6	37	10·6	4·0	0·9	0·813	4·9	533	
Uckfield	29·556	55·9	82·0	29·0	21·6	53·0	......	Variable.	......	41	7·8	3·9	1·7	0·684	4·7	530	180
Beckington	29·572	53·1	83·0	24·0	22·4	59·0	1·3	s.w.	5·4	46	9·2	3·8	1·0	0·819	4·6	532	265
Royal Observatory, Greenwich	29·570	54·3	80·0	30·2	21·6	49·8	......	e.	5·9	46	7·3	3·8	1·4	0·775	4·7	531	159
Maidenstone Hill, Greenwich	29·563	54·2	78·6	30·7	18·9	47·9	......	e.	6·0	41	7·2	3·9	1·1	0·781	4·7	531	107
Walworth	29·618	54·8	80·0	35·0	19·0	45·0	3·3	s.w.	5·7	45	6·7	3·8	1·2	0·742	4·5	532	32
Latimer Rectory	29·588	53·2	82·0	23·5	26·7	58·5	1·5	n.	5·3	46	7·9	3·8	1·2	0·763	4·6	527	335
Aylesbury	29·482	55·0	82·0	28·0	23·3	54·0	0·8	s.	5·8	41	8·0	3·8	1·5	0·718	4·7	529	280
Stone Observatory	29·596	53·3	77·9	27·7	19·6	50·2	0·8	Variable.	5·3	43	5·8	3·7	1·2	0·754	4·6	529	300
Hartwell House	29·581	53·7	84·0	24·0	26·0	60·0	0·8	s.	5·3	......	......	3·8	1·2	0·773	4·6	529	300
Cardington	29·594	54·2	81·0	29·0	21·8	52·0	......	s.	5·7	48	7·5	4·0	1·2	0·793	4·9	531	150
Thwaite	....	......	84·0	33·0	......	51·0	......	e.	......	33	7·8	......	......	....	......	......	200
Cambridge Observatory	29·628	53·6	79·5	30·7	18·7	48·8	0·6	s.w.	6·4	39	5·5	3·8	1·1	0·776	4·6	532	88
Norwich	29·482	54·9	84·0	31·0	20·8	53·0	......	Variable.	......	......	7·3	4·1	1·1	0·781	5·0	531	39
Derby	29·544	52·5	77·0	26·0	18·1	51·0	......	Variable.	......	47	8·5	3·8	1·0	0·798	4·6	531	39
Highfield House, Notts	29·541	53·7	83·0	27·0	21·9	56·0	......	Variable.	6·2	54	8·3	3·8	1·1	0·778	4·6	533	103
Liverpool Observatory	29·561	52·2	71·9	34·7	11·3	37·2	1·0	n.w.	5·9	47	7·0	3·6	0·8	0·835	4·3	537	37
Leeds	29·513	50·5	88·0	23·0	21·7	65·0	1·4	Variable.	......	59	9·4	3·8	0·9	0·801	4·6	532	148
Wakefield	29·452	52·8	85·0	25·0	21·0	60·0	......	w.	......	48	9·4	3·5	1·2	0·739	4·2	......	113
Stonyhurst Observatory	....	51·1	75·5	27·0	18·1	48·0	0·8	w.s.w.	6·8	51	11·6	3·5	0·9	0·784	4·2	......	381
York	....	53·4	79·5	27·0	17·3	52·5	......	s.e.	......	48	9·9	......	......	....	......	......	50
Scarva, Ireland	....	53·1	77·6	28·1	16·9	49·5	1·9	s.	6·0	34	6·1	3·9	0·9	0·805	4·7	......	162
Whitehaven	....	52·2	73·0	31·0	13·1	42·1	1·9	s.w.	......	41	6·2	3·7	1·1	0·784	4·6	532	
Durham	29·565	50·7	76·4	24·5	15·6	51·9	1·4	s.w.	6·0	41	6·2	3·5	1·0	0·779	4·3	530	340
Newcastle	29·506	50·6	79·5	26·5	14·2	53·0	......	s.w.	......	36	5·6	3·9	1·2	0·887	4·8	531	121

adjacent localities have estimated it differently. At all places its strength seems to have been unusually small.

From the numbers in the ninth column, the distribution of cloud seems to have been the same at all places, and such as to cover about one-half of the sky. This value is much less than the average amount of cloud.

The fall of rain during the quarter has greatly exceeded the average amount for the season. The amount in May was much below the average for that month. In the months of April and June the amount was unusually large, particularly in the latter month. The places at which rain fell on the greatest number of days were Leeds, Nottingham, Stonyhurst, Saffron Walden, &c.; and on the smallest number of days were Thwaite, Scarva, Helston, Newcastle, &c. The places at which the largest falls have taken place were Hereford, Stonyhurst, Southampton, York, Leeds, Wakefield, &c.; and the places where the falls have been the least in amount, are Saffron Walden, Cambridge, Newcastle, Stone, &c.; but it would seem that the amount at the last-mentioned place is wrong (see the amounts at Hartwell and Aylesbury). Generally the largest falls have been in Yorkshire, and the smallest in the counties north of Yorkshire.

The numbers in column 12 to 16 contain the mean values of the hygrometrical results at every station; from which we find that—

The mean weight of vapour in a cubic foot of air for England (excepting Cornwall and Devonshire) in the quarter ending June 30, 1848, was 3·8 grains.

The mean additional weight required to saturate a cubic foot of air in the quarter ending June 30, 1848, was 1·1 grain.

The mean degree of humidity in the quarter ending June 30, 1848, was 0·778.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth, to the depth of 4·6 inches.

The mean weight of a cubic foot of air at the level of the sea, under the mean temperature, humidity and pressure, was 534 grains.

And these values for Cornwall and Devonshire were 3·8 grains; 1·2 grain; 0·765; 4·7 inches; and 534 grains respectively.

The results from the station in Ireland agree very closely with those in England in the same parallel of latitude, excepting those depending on the water mixed with the air, and in these respects an excess of humidity is shown at this station.

XXVIII. *On the Application of Quaternions to the Theory of Rotation.* *By* ARTHUR CAYLEY, *Esq.**

IN a paper published in the Philosophical Magazine, February 1845, I showed how some formulæ of M. Olinde Rodrigues relating to the rotation of a solid body might be expressed in a very simple form by means of Sir W. Hamilton's theory of quaternions. The property in question may be thus stated. Suppose a solid body which revolves through an angle θ round an axis passing through the origin and inclined to the axes of co-ordinates at angles a, b, c. Let

$$\lambda = \tan\frac{1}{2}\theta\cos a, \quad \mu = \tan\frac{1}{2}\theta\cos b, \quad \nu = \tan\frac{1}{2}\theta\cos c,$$

and write

$$\Lambda = 1 + i\lambda + j\mu + k\nu.$$

Let x, y, z be the co-ordinates of a point in the body previous to the rotation, x_1, y_1, z_1 those of the same point after the rotation, and suppose

$$\Pi = ix + jy + kz$$
$$\Pi_1 = ix_1 + jy_1 + kz_1.$$

The co-ordinates after the rotation may be determined by the formula

$$\Pi_1 = \Lambda\Pi\Lambda^{-1};$$

viz. developing the second side of this equation,

$$\begin{aligned}\Pi_1 = i.&(\alpha x + \beta y + \gamma z)\\ +j&(\alpha' x + \beta' y + \gamma' z)\\ +k&(\alpha'' x + \beta'' y + \gamma'' z),\end{aligned}$$

where, putting to abbreviate $\varkappa = 1 + \lambda^2 + \mu^2 + \nu^2$, we have

$$\begin{aligned}&\varkappa\alpha = 1 + \lambda^2 - \mu^2 - \nu^2, && \varkappa\alpha' = 2(\lambda\mu + \nu), && \varkappa\alpha'' = 2(\lambda\nu - \mu),\\ &\varkappa\beta = 2(\lambda\mu - \nu), && \varkappa\beta' = 1 - \lambda^2 + \mu^2 - \nu^2, && \varkappa\beta'' = 2(\mu\nu + \lambda),\\ &\varkappa\gamma = 2(\lambda\nu + \mu), && \varkappa\gamma' = 2(\mu\nu - \lambda), && \varkappa\gamma'' = 1 - \lambda^2 - \mu^2 + \nu^2;\end{aligned}$$

these values satisfying identically the well-known system of equations connecting the quantities α, β, γ, α', β', γ', α'', β'', γ''.

The quantities a, b, c, θ being immediately known when λ, μ, ν are known, these last quantities completely determine the direction and magnitude of the rotation, and may therefore be termed the co-ordinates of the rotation—Λ will be the quaternion of the rotation. I propose here to develope a few of the consequences which may be deduced from the preceding formulæ.

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* Communicated by the Author.

Suppose, in the first place, $\Pi = \Lambda - 1$, then $\Pi_1 = \Lambda - 1$, which evidently implies that the point is on the axis of rotation. The equation $\Pi_1 = \Pi$ gives the identical equations

$$\lambda(\alpha - 1) + \mu\beta + \nu\gamma = 0$$
$$\lambda\alpha' + \mu(\beta' - 1) + \nu\gamma' = 0$$
$$\lambda\alpha'' + \mu\beta'' + \nu\gamma'' = 0.$$

From which, by changing the signs of λ, μ, ν, we derive

$$\lambda(\alpha - 1) + \mu\alpha' + \nu\alpha'' = 0$$
$$\lambda\beta + \mu(\beta' - 1) + \nu\beta'' = 0$$
$$\lambda\gamma + \mu\gamma' + \nu(\gamma'' - 1) = 0.$$

Whence evidently, whatever be the value of Π,

$$\Lambda\Pi\Lambda^{-1} - \Pi = 0,$$

if after the multiplication i, j, k are changed into λ, μ, ν, a property which will be required in the sequel.

By changing the signs of λ, μ, ν, we also deduce

$$\begin{aligned}\Lambda^{-1}\Pi\Lambda = i.&(\alpha x + \alpha' y + \alpha'' z)\\ +j&(\beta x + \beta' y + \beta'' z)\\ +k.&(\gamma x + \gamma' y + \gamma'' z),\end{aligned}$$

where α, β, γ, α', β', γ', α'', β'', γ'' are the same as before.

Let the question be proposed to compound two rotations (both axes of rotation being supposed to pass through the origin). Let L be the first axis, Λ the quaternion of rotation, L′ the second axis, which is supposed to be fixed in space, so as not to alter its direction by reason of the first rotation, Λ' the corresponding quaternion of rotation. The combined effect is given at once by

$$\Pi_1 = \Lambda'(\Lambda\Pi\Lambda^{-1})\Lambda'^{-1}, \textit{ i. e.}$$
$$\Pi_1 = \Lambda'\Lambda.\Pi.(\Lambda'\Lambda)^{-1}.$$

Or since if Λ_1 be the quaternion for the combined rotations $\Pi_1 = \Lambda_1 \Pi \Lambda_1^{-1}$, we have clearly

$$\Lambda_1 = M_1 \Lambda' \Lambda,$$

M_1 denoting the reciprocal of the real part of $\Lambda'\Lambda$, so that

$$M_1^{-1} = 1 - \lambda\lambda' - \mu\mu' - \nu\nu'.$$

Retaining this value, the coefficients of the combined rotations are given by

$$\lambda_1 = M_1(\lambda + \lambda' + \mu'\nu - \mu\nu')$$
$$\mu_1 = M_1(\mu + \mu' + \nu'\lambda - \nu\lambda')$$
$$\nu_1 = M_1(\nu + \nu_1 + \lambda'\mu - \lambda\mu');$$

to which may be joined

$$x_1 = M_1^2 . xx',$$

x, x', x_1 as before. Λ_1 or Λ' may be determined with equal facility in terms of Λ', Λ_1, or Λ, Λ_1. These formulæ are given in my paper on the rotation of a solid body (Camb. Math. Journal, vol. iii. p. 226).

If the axis L$'$ be fixed in the body and moveable with it, its position after the first rotation is obtained from the formula $\Pi_1 = \Lambda \Pi \Lambda^{-1}$ by writing $\Pi = \Lambda' - 1$. Representing by $\Lambda'' - 1$ the corresponding value of Π_1, we have $\Lambda'' = \Lambda\Lambda'\Lambda^{-1}$, which is the value to be used instead of Λ' in the preceding formula for the combined rotations, *i. e.* the quaternion of rotation is proportional to $\Lambda\Lambda'\Lambda^{-1}.\Lambda$, that is to $\Lambda\Lambda'$. So that here

$$\Lambda_1 = M_1 \Lambda\Lambda',$$

which only differs from the preceding in the order of the quaternion factors. If the fixed and moveable axes be mixed together in any order whatever, the fixed axes taken in order being L, L$'$,.. and the moveable axes taken in order being L$_0$, L$'_0$.. then the combined effect of the rotation is given by

$$\Lambda_1 = M \, .. \, \Lambda''\Lambda'\Lambda\Lambda_0\Lambda'_0 \, ..,$$

M being the reciprocal of the real term of the product of all the quaternions.

Suppose next the axes do not pass through the same point. If α, β, γ be the co-ordinates of a point in L, and

$$\Gamma = \alpha i + \beta j + \gamma k,$$

the formula for the rotation is

$$\Pi_1 - \Gamma = \Lambda(\Pi - \Gamma)\Lambda^{-1},$$

or

$$\Pi_1 = \Lambda\Pi\Lambda^{-1}. - (\Lambda\Gamma\Lambda^{-1} - \Gamma),$$

where the first term indicates a rotation round a parallel axis through the origin, and the second term a translation.

For two axes L, L$'$ fixed in space,

$$\begin{aligned}\Pi_1 = {} & \Lambda'\Lambda . \Pi . (\Lambda'\Lambda)^{-1}. \\ & - (\Lambda'\Gamma'\Lambda'^{-1} - \Gamma') - \Lambda'(\Lambda\Gamma\Lambda^{-1} - \Gamma)\Lambda'^{-1};\end{aligned}$$

and so on for any number, the last terms being always a translation. If the two axes are parallel, and the rotations equal and opposite,

$$\Lambda = \Lambda'^{-1}.,$$

whence

$$\Pi_1 = \Pi + \Lambda'(\Gamma - \Gamma')\Lambda'^{-1}(\Gamma - \Gamma');$$

or there is only a translation. The constant term vanishes if

i, j, k are changed into λ', μ', ν', which proves that the translation is in a plane perpendicular to the axes.

Any motion of a solid body being represented by a rotation and a translation, it may be required to resolve this into two rotations. We have

$$\Pi_1 = \Lambda_1 \Pi \Lambda_1^{-1} + \mathrm{T},$$

where T is a given quaternion whose constant term vanishes. Whence, comparing this with the general formula just given for the combination of two rotations,

$$\Lambda_1 = \Lambda'\Lambda . \mathrm{M}_1$$

$$\mathrm{T} = -(\Lambda'\Gamma'\Lambda'^{-1} - \Gamma') - \Lambda'(\Lambda\Gamma\Lambda^{-1} - \Gamma)\Lambda'^{-1},$$

the second of which equations may be simplified by putting $\Lambda'^{-1}\mathrm{T}\Lambda' = \mathrm{S}$, by which it may be reduced to

$$\mathrm{S} = (\Lambda'^{-1}\Gamma'\Lambda' - \Gamma') - (\Lambda\Gamma\Lambda^{-1} - \Gamma),$$

which, with the preceding equation $\Lambda_1 = \mathrm{M}_1\Lambda'\Lambda$, contains the solution of the problem. Thus if Λ or Λ' be given, the other is immediately known; hence also S is known. If in the last equation, after the multiplication is completely effected, we change i, j, k into λ, μ, ν, or λ', μ', ν', we have respectively,

$$\mathrm{S} = \Lambda'^{-1}\Gamma'\Lambda' - \Gamma', \quad \mathrm{S} = -(\Lambda\Gamma\Lambda^{-1} - \Gamma),$$

which are equations which must be satisfied by the coefficients of Γ' and Γ respectively. Thus if the direction of one axis is given, that of the other is known, and the axes must lie in certain known planes. If the position of one of the axes in its plane be assumed, the equation containing S divides itself into three others (equivalent to two independent equations) for the determination of the position in its plane of the other axis. If the axes are parallel, λ, μ, ν are proportional to λ', μ', ν'; or changing i, j, k into λ, μ, ν, or λ', μ', ν', we have $\mathrm{S}=0$; or what is the same thing, $\mathrm{T}=0$, which shows that the translation must be perpendicular to the plane of the two axes.

If p, q, r have their ordinary signification in the theory of rotation, then from the values in the paper in the Cambridge Mathematical Journal already quoted,

$$\varkappa(ip + jq + kr) = 2\frac{d\Lambda}{dt}\Lambda + \frac{d\varkappa}{dt};$$

but I have not ascertained whether this formula leads to any results of importance. It may, however, be made use of to deduce the following property of quaternions, viz. if $\Lambda_1 = \mathrm{M}_1\Lambda'\Lambda$, M_1 as before, then

$$\frac{1}{\varkappa_1}\left(2\frac{d\Lambda_1}{dt}{}^1\Lambda_1 + \frac{d\varkappa_1}{dt}\right) = \frac{1}{\varkappa}\left(2\frac{d\Lambda}{dt}\Lambda + \frac{d\varkappa}{dt}\right),$$

in which the coefficients of Λ' are considered constant.

To verify this *à posteriori*, if in the first place we substitute for $\varkappa_1$ its value $M_1^2 . \varkappa\varkappa'$, we have

$$\frac{d\varkappa_1}{dt} = M_1^2 \varkappa' \frac{d\varkappa}{dt} + \frac{2}{M_1}\frac{dM_1}{dt}\varkappa_1,$$

and thence

$$\frac{d\Lambda_1}{dt}\Lambda_1 + \frac{1}{M_1}\frac{dM_1}{dt}\varkappa_1 = M^2\varkappa'\frac{d\Lambda}{dt}\Lambda.$$

Also

$$\frac{d\Lambda_1}{dt}\Lambda_1 = \left(\frac{1}{M_1}\frac{dM_1}{dt}\Lambda_1 + M_1\Lambda'\frac{d\Lambda}{dt}\right)\Lambda_1 = \frac{1}{M_1}\frac{dM_1}{dt}\Lambda_1^2$$
$$+ M_1^2\Lambda'\frac{d\Lambda}{dt}\Lambda'\Lambda,$$

which reduces the equation to

$$\frac{1}{M_1}\frac{dM_1}{dt}(\Lambda_1^2 + \varkappa_1) + M_1^2 . \Lambda'\frac{d\Lambda}{dt}\Lambda'\Lambda = M_1^2\varkappa'\frac{d\Lambda}{dt}\Lambda.$$

Or observing that

$$\Lambda_1^2 + \varkappa_1 = 2\Lambda_1 = 2M_1\Lambda'\Lambda,$$

and omitting the factor Λ from the resulting equation,

$$\frac{2}{M_1^2}\frac{dM_1}{dt} . \Lambda' + \Lambda'\frac{d\Lambda}{dt}\Lambda' = \varkappa'\frac{d\Lambda}{dt}.$$

Or since

$$\frac{1}{M_1} = 1 - \lambda\lambda' - \mu\mu' - \nu\nu'.$$

Substituting and dividing by Λ'

$$2\left(\lambda'\frac{d\lambda}{dt} + \mu'\frac{d\mu}{dt} + \nu'\frac{d\nu}{dt}\right) = \varkappa' . \Lambda'^{-1} . \frac{d\Lambda}{dt} - \frac{d\Lambda}{dt}\Lambda'.$$

Or finally,

$$2\left(\lambda'\frac{d\lambda}{dt} + \mu'\frac{d\mu}{dt} + \nu'\frac{d\nu}{dt}\right) = (1 - i\lambda' - j\mu' - k\nu')\left(i\frac{d\lambda}{dt} + j\frac{d\mu}{dt} + k\frac{d\nu}{dt}\right)$$
$$- \left(i\frac{d\lambda}{dt} + j\frac{d\mu}{dt} + k\frac{d\nu}{dt}\right)(1 + i\lambda' + j\mu' + k\nu')$$
$$= -(i\lambda' + j\mu' + k\nu')\left(i\frac{d\lambda}{dt} + j\frac{d\mu}{dt} + k\frac{d\nu}{dt}\right)$$
$$- \left(i\frac{d\lambda}{dt} + j\frac{d\mu}{dt} + k\frac{d\nu}{dt}\right)(i\lambda' + j\mu' + k\nu'),$$

which is obviously true.

58 Chancery Lane,
July 1, 1848.

XXIX. *Geometry and Geometers. Collected by* T. S. Davies, *Esq., F.R.S. and F.S.A.*[*]

No. II.

I HAVE already mentioned that there are twenty-seven letters of Dr. Simson's to John Nourse in the collection before me. There are also two from Simson's pupil, Dr. James Moor; and three notes from Dr. Robert Smith of Cambridge (the founder of the "Smith's prizes," to obtain which is now quite as high a distinction as to be senior wrangler): all of which relate to Simson. To this series the present and the next communications will be confined.

Dr. Trail, in his Life of Simson, p. 26, has a passage which will throw light on this correspondence. "The hesitation about making additions to the Treatise of Apollonius [on Plane Loci] probably contributed to the delay in printing the work, which was not executed till 1749. He then met with some unexpected difficulties in treating with a bookseller for the sale of the whole impression, which alone prevented the publication at that time; and except a few copies distributed amongst his friends in 1750, the book remained unpublished till after his death."

This information is founded on a letter of Dr. Simson to Earl Stanhope (at least all but the statement of the actual time of publication being after Simson's death; which is a mere inference, and an erroneous one), dated Sept. 10, 1750. Now it appears in the course of these letters that Nourse was in personal intercourse with the Earl at a period shortly afterwards, and that this intercourse did not appear to be of recent origin. Nothing is more likely than that the Earl at once brought the matter under the notice of Nourse; for the first letter of this series is dated Glasgow, Jan. 1, 1751, in answer to one referred to in it, from Nourse of the "25th Oct. last." It will be seen from the first three of these letters, that the *Loci Plani* was actually published in London almost immediately, instead of being delayed till after Simson's death, as Dr. Trail infers. A subsequent letter will also show that the work "did not sell,"—even in an age in which it has been represented that geometry was more ardently cultivated than it is in our own.

"Glasgow, 1st Jan. 1751.

"Sir,—I had the favour of Your letter of the 25th Octr. last in due time, and am much obliged to you, who are so good a Judge as several have informed me, for the kind Opinion you entertain of any of my

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[*] Communicated by the Author.

slender performances. I would by no means have so long delayed to write in answer to your civil letter, but that I every post expected to have heard of the receipt of Nine copies I sent more than half a year ago to Stationers' Hall, that I might inform you of the books (I mean the Loci Plani) being to be speedily published, but notwithstanding I have dunned the person to whose care I committed the Sending of these copies from Edinburgh, ever since he told me of their being ready to be shipt for London, I have not yet account of their being got there. I beg you will forgive my incivility in not writing sooner, and that you will be so good as to write me how to direct a letter to the keeper of Stationers' Hall whose name I know not, or, if it were not too much trouble that you would, as the speedier way, acquaint me if he has received these 9 copies, and if he has not already sent a receipt of them to be given me, that you would get one and transmit it to me, upon which I should immediately order the book to be published in the Lon. Gazette and some other of the publick papers.

"The price of it to the publick, bound, I am advised to make 11 sh: the finest paper, and 9 shill: the other, which is not much coarser; and to Booksellers who take 100 or more copies 5 sh: in sheets, one third of the best and $\frac{2}{3}$ds of the other paper. It consists of 31 sheets and $\frac{1}{2}$ sheet over in 4to, and there are about 700 copies printed by Mr. Foulis here.

"As for the Conick Sections, Mr. Alexander Kincaid Bookseller in Edinburgh bought the entire right from me more than a year ago, nay it is a year since he reprinted them at Edinburgh; but they are not yet published, and I heard last week that all the Copper plates were not cast off as yet, for what reason I know not; so it is to him you are to apply if you incline to have any of them. I have corrected a good many things and changed some Demonstrations for the better. Some few things have been added by my friend Mr. Matth. Stewart and my self, an account of which you will see at the end of the Preface. I wish you a happy new year, and am

"Sir, Your most humble
"and much obliged Servant,
"ROB: SIMSON."

"Glasgow, 21 Jan. 1751.

"SIR,—The favour of Your letter I had in due time, and return you hearty thanks for the trouble you have taken in Searching at Stationers' Hall for the 9 copies of the Loci Plani. Tho' a good time ago I was informed from Edinburgh that these copies had, long before that, been Sent up to London, I find now by a letter from Mr. Kincaid that they are not yet Sent, which is a neglect I cannot explain the reason of. As soon as I knew this, which was only last week, I wrote to send them hither immediately.

"I have this day directed Mr. Foulis to put up one hundred copies, $\frac{1}{3}$d of the best and $\frac{2}{3}$ds of the other paper, but shall wait a few days that I may get the 9 for Stationers' Hall to send along with them. Since you are so good to offer to deliver them and see them registered. There are some few Errata which I caused print after the book came from the press, which I shall send along that they may be pasted to the leaf on which some are already printed.

"I thank you most kindly for this favour and your design of taking some more copies; You may be sure I would be glad you took the whole impression, if it consisted with your Interest.

"When you get leisure to look over the book, it will be a great favour to have your censures and remarks upon it, and the more impartial and free they are they will be so much the more acceptable to me. I am, Sir, Your most obedient humble Servant,

"ROB: SIMSON.

"be pleased to turn over.

"In your former letter you wrote that you would either remit the money or pay it by your friend Mr. Barry. I know he is a very honest good man, and my particular acquaintance, but I choose rather you would do it the former way: I shall take care to deliver him the parcell, and cause it to be delivered at the sight of one of my friends to the Cariers, in case he should be slow as you justly remark he sometimes is. Adieu."

"Glasgow, 8th March, 1751.

"SIR,—Sometime ago I informed you of the books being sent off which you wrote for, viz: 100 copies of the Loci Plani $\frac{1}{3}$d of the best and $\frac{2}{3}$ds of the other paper. Mr. Jo. Barry gave me notice 3 weeks ago that you had writ to him to pay me £25 upon your Account, and this day, which was the first time I saw him since his writing, he told me I should have it next week, which I shall reckon very Seasonable payment.

"This day I had a letter from Mr. Alex^{r}: Kincaid in which he writes of the 9 copies of the book being received at Stationers' Hall, but that they give no receipts. I desire you may do me the favour to enquire if they are delivered in such a manner as the book may be published, which Mr. Kincaid Sayes they are, and if it be so I desire you may cause the book to be published once or twice in the London Gazette, and as often at least in some of the most proper London Newspapers, which I leave to your choice, and Charge the Expenses to my account. I design soon to write to Mr. Foulkes, President of the Royal Societie, which I have not time to do by this post, and shall commit the Letter to your care, which you will be pleased to deliver, and Along with it a Copy of the Loci for their Library, and one for Mr. Foulkes himself, which in the mean time you may cause to be bound handsomely and have ready against the time you get the letter. I am, Sir,

"Your most humble and Obliged Servant,

"ROB: SIMSON.

"turn over.

"I have lately read 3 letters concerning Royal Societies &c. which I guess have been published by Mr. H—l. and dayly expect a larger book which he has put his name to, containing remarks upon the Phil. Transactions, I shall be much Obliged to you, if you be pleased to give me a Short account of the affair, and the reason of his quarrell with the Societie. Whether has the Scribbleriad any connexion with this affair, and against whom is it intended? for I have seen only the first part of it. adieu."

These three letters fully prove the period of the publication of the *Loci Plani*; whilst the quaint stile and the indications of Simson's methodical habits, give independent interest to them. I find amongst these letters no further reference to the work in question till near the close of the correspondence, namely in a letter dated June 29, 1767, a few months before his death, and when he had nearly completed his eightieth year. He says:—

"I am sorry the Loci plani do not Sell, but indeed the taste for the Antient Geometry or indeed any Geometry seems to be quite worn out, and for want of acquaintance with it not a few blunders appear in the modern books; I know not if you have observed one in Kirkby's book of the doctrine of ultimators, which even a tolerable knowledge in the Algebraick doctrine of Loci might have prevented; in page 115 at the bottom you see there and in next page he makes the curve which he calls Pyriformis to be of a different kind from the Ellipsis, but in page 125 where he gives you the Æquation of it, viz: $\overline{2cv-vv}^{\frac{1}{2}}-v=s$ it appears to be formed by two arches of two equal and Similar Ellipses meeting one another. I could not prevail with myself to read his doctrine of Ultimators tho Mr. Will. Trail lent me the book to consider Something about that Doctrine; Pray let me know what you or any of your friends that deal in these things think of his performance, and if he sufficiently makes out what he designed.

"Could I think I were able, both in respect of memory and eye Sight, to print Apollonius two books de Sectione Determinata, which Mr. Trail who is here, and is to stay till October, has from me for some weeks past to read over and observe any errors, I would when I published them put the Loci plani after them; perhaps the Novelty of the first might help off the other. Mr. Trail is very urgent with me to make the wooden cuts, which I believe I shall do even tho' they should never be used by me; If I come the length of resolving to print, I shall write to you for paper.

"I wish your health and business, both which I heartily pray may encrease, could allow you to write to me oftener, it would refresh me much. Pray let me know how much I am in your debt, and let our Accounts be Settled and cleared. I believe I wrote to you formerly that, if I can do no better, I design to send you the copies of the Loci plani and the Latin 4^{to} Euclide of which I have a good number, for any price you think fit, or for no price at all rather than let them lie useless beside me."

The last letter of this series is dated Sept. 11, 1767 (probably his last letter to Nourse), a few months before his death. As I have intentionally left the correspondence respecting the publication of his eight books of Euclid and the data to form a series of itself, I extract from this letter only what relates to subjects mooted in the preceding one.

"Your opinion about Kirkby's Ultimators I believe is very just; If Sir Isaac Newton had made use of the limits of ratios he had done much better than he has done by bringing in Ultimate ratios which I think is a very improper form, as in most cases there is no such thing as an Ultimate ratio.

* * * *

"I was once thinking this Summer to have written for paper to print the Sectio Determinata, which as you observed might have helped the sale of the Loci plani, but all I have gotten done is to revise the first of these books by the assistance of my good friend Mr. William Trail who is Professor of Mathematics in the new College of Aberdeen, but who has been staying here all this summer with his uncle Dr. Trail Professor of Divinity, he has copied over for me a few sheets, I believe but one or two at most, concerning the limits of ratios from which the rules for finding fluxions are geometrically deduced, and he also copied as short a paper concerning the elementary properties of Logarithms which I think have not yet been accurately treated of; if I find myself in tolerable health next Spring, for I am afraid how I shall stand thro' the winter, I design to annex these two small things to the Sectio Determinata and print them; but if God see fit to determine otherwise by my want of health, I design to leave the care of printing them to Mr. Trail who is a worthy good young man and an Excellent Mathematician for his age; and I design to do the same with respect to the Porisms if I can get them ready to be put into his hands."

This letter is marked on the back with a memorandum in Nourse's hand, "answered April 16, 1768;" and as Simson lived only till the 1st of October, it is probable that he never wrote in reply.

The complaint which Simson makes of the ancient geometry being "worn out," was then only too true in respect to academic history: yet amongst non-academic mathematicians in this country, of whom Dr. Simson seems to have known little, nothing could be less accurate. I find that even in Cambridge, a considerable degree of patronage was afforded to Simson's Euclid; for in the letters of Dr. Robert Smith, there is notice of a remittance of 28*l*. 15*s*., dated April 4, 1756, and another respecting 10*l*. 16*s*. on account of this work. We ought also to recollect how very extensively the geometrical writings of Thomas Simpson had been diffused; and the periodicals devoted to mathematics (as the Lady's Diary, the Gentleman's Diary, the Mathematician, and others) will show how strongly geometry prevailed amongst non-academic men in those days, and the eminent powers of investigation possessed by those geometers. It cannot be denied, however, that as a general rule, geometry has held a very subordinate and precarious position in our universities since the commencement of the preceding century; whilst non-academic men, till very recent times, have manifested extraordinary skill in such researches. It is certainly a curious fact, that since our non-academic men have devoted so much more of their attention to analysis, (for I believe that Messrs. Lowry and Whitley remain the only two representatives of the old English school of geometers now left us,) the tide is setting, in one of our universities at least, in the direction of the ancient geometry.

It would appear from the last of these letters, that the editing of the posthumous works of Simson was not a work of considerable difficulty; and indeed they are probably printed nearly in the state that they were left by their distinguished author. There is no doubt that with due attention there might have been selected from his papers things much more worthy of publication than those printed in the appendix,—the last proposition, however, excepted. This one is important in respect to a problem of great celebrity—that of the "tactions" of Apollonius: but on this head I must refer to a paper in the Mathematician, vol. iii. p. 77, where its importance is established.

There still exists (as I am informed by a friend to whom I refer in the paper just quoted) in the library of the Escurial, an Arabic translation of the *Sectio Determinata*. If it were possible for another Halley to compare that translation with the restoration of the work by Simson, it would be a subject of the greatest interest; inasmuch as it would fully decide the degree of confidence which we should place in any "restorations" whatever, this being the one upon which Simson appears to have bestowed more care than upon any of his works except the Porisms.

I think the conclusion of Kirkby's preface ought to disarm criticism with respect to his "Doctrine of Ultimators." The work itself, indeed, never appears to have attracted much attention; and it is now altogether forgotten, except by a few collectors of mathematical curiosities. The remark of Simson, however, is correct; for the equation which Kirkby brings out (which is unexceptionable except as to the omission of the sign $\pm$ from the radical) is that of the ellipse. As a general rule, however, curves generated by means of ordinates to the circle assume more convenient forms of equation when polar co-ordinates are employed, though there occasionally occur exceptions to it—of which the *pyriformis* is one.

Royal Military Academy,
April 28, 1848.

XXX. *On a new and practical Voltaic Battery of the highest powers, in which Potassium forms the positive element. By* JOHN GOODMAN, *Esq.**

POTASSIUM is well known in science as one of the most powerful chemical substances, possessing chemical affinities and powers of the highest order. This substance appears to have been long known also in an electrical point of view, for we find it arranged by Sir H. Davy and others at the head of a list of positive and negative metals, in which it forms the principal oxidizable metal, being, with its amalgams, electro-positive to all other metals arranged in electrical relations with it.

It was with this knowledge of the high powers of potassium, that the author, whilst pursuing his researches into the analogy of light, heat, electricity, &c., devised several experiments with the intention of constructing a voltaic arrangement, in which this metal should form *one element*. At first, the potassium, amalgamated, was suspended by a copper wire in mineral naphtha, floating upon dilute sulphuric acid. In the acid liquid a plate of platinum was arranged, in connexion with one extremity of the galvanometer, to the opposite extremity of which the potassium wire was attached. On lowering the potassium down to the surface of the acidulated water, a current of 60° or 70° was observed, and a strong action ensued, which after a few moments generally dislodged the metal from its connexions, and put an end to the experiment.

After many fruitless attempts permanently to fasten the metal upon its wire of communication, it was remembered that in one instance a very efficient voltaic pair had been formed, in which a simple copper wire of a spiral form merely surrounded the wet bladder in which the zinc element was immersed in dilute acid, being only in contact with, and acting through, the substance of the membrane, although unimmersed in any fluid. Advantage was taken of this knowledge, and it was contemplated that potassium might, as possessing such extraordinary affinities, also act upon water through an intervening membrane. Thus might its excessive action be moderated, and itself rendered probably quiet and manageable.

On the 5th of May last the following plan was ultimately adopted after much consideration, and proved successful beyond my most sanguine expectations. A wine-glass was filled with dilute sulphuric acid, and in this was immersed a plate of platinum just below the surface of the liquid. At the ex-

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* From the Manchester Literary and Philosophical Society's Memoirs, vol. viii. Communicated by the Author.

tremity of a short length of glass tubing a piece of membrane was tied, so as to close up its lower end, which was by an appropriate stand so fixed that the membrane or diaphragm should come in contact with the surface of the acidulated water, immediately above the immersed plate of platinum. Into this tube was dropped a globule of mercury, which, lying upon the membrane, would serve to amalgamate and keep in that condition the piece of potassium destined for this situation. The tube was then filled with mineral naphtha, so that the metal could be raised at pleasure into a medium in which it would remain perfectly quiescent, and would only suffer loss when required so to do.

The potassium, weighing about half a grain, was now screwed upon the "tapped" extremity of the copper wire, upon which a shoulder or button of wood was also screwed, about one-sixteenth of an inch from its extremity, to prevent the wire perforating the potassium too far, and coming itself in contact with the diaphragm. This wire was (as is usual) in metallic communication with the immersed platinum, and for the purpose of raising or depressing the potassium in its cell, a moveable mercury cup formed the medium of communication. From this the potassium hung suspended by its wire, upon which a small weight was affixed, to ensure the continuous contact and close application of this metal to the membrane.

With the apparatus thus arranged, it was found that potassium became a very *manageable element* in a voltaic battery; and on lowering it into contact with the diaphragm, a continuous current of 45° to 50° was observed by the aid of an intervening galvanometer. To test the comparative power of this arrangement with an ordinary pair, two plates, one of zinc the other of copper, 2¼ inches by 1¾ inch, were employed in deflecting the same galvanometer. And it was found that in very fierce action in water, strongly acidulated with sulphuric acid, they only deflected the galvanometer to 65°, which in a few minutes fell to 61°, although at that period in fair brisk action.

Potassium Experiment.—Aqueous Decomposition.

May 19, 1846.—Exp. 1. Again arranged this potassium battery as before, substituting however *solution of sulphate of copper acidulated with sulphuric acid* in the wine-glass. The arrangement produced a remarkably quiet battery. The deflection of the galvanometer was but 10° to 30° when the decomposition apparatus was connected. Two platina wire poles, arranged as usual in acidulated distilled water in a convenient glass tube for showing the development of the gas, formed the decomposition apparatus. The decomposition by *this single pair* was beautiful and *energetic*; and what was remarkable, continued some time (decreasingly) after the platina plate was disconnected, the electric energy of the potassium still inducing current.

Exp. 2. With sulphate of copper solution, the next very small piece of potassium gave only 10° of deflection, and 5° during decomposition, which was tolerably rapid. Afterwards a deflection of 50° in another experiment, which fell to 5° in decomposition.

Exp. 3. With *dilute acid* only, galvanometer 55°; but during *decomposition*, which was *very faint*, only $2\frac{1}{2}$°.

Exp. 4. *In neither case could decomposition be effected in a second decomposing cell.*

Exp. 5. With a copper plate instead of platina, a deflection from 45° to 50° fell during decomposition, which was not powerful, to 5°. Decomposition could not be effected in two cells.

There is a peculiar property of electricity, exhibited in all its various modifications, which is considered by the author as the *distinguishing quality* of electric force. It is first perceived in the tendency of all substances exposed to the action of electric force, to assume what is termed *a polar condition.* It has been shown by Dr. Faraday to exist in the molecules of the various substances which are interposed between the decomposing poles, or electrodes of a voltaic arrangement. The author suggests that it is this polar condition which causes, by the simple law of attraction and repulsion, the elements of any fluid under decomposition to progress towards, and ultimately to be deposited upon, or chemically combined with, that pole of the apparatus to which they severally belong, by a law as certain as the force of gravitation itself*.

This quality of electric force *does not depend upon the quantity of fluid existing in a circuit*; for the ordinary electricity manifests it in a much higher degree than does voltaic fluid, although the latter is found at all times incomparably greater in quantity. The author of this paper has frequently decomposed water in four successive cells, by the current from a single electrical machine; and yet to effect decomposition in one cell by voltaic power, several pairs are required.

It is found that this state of polarization is at all times accompanied by other characteristics in proportion to its inten-

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* As conduction destroys polarity, this polar condition could have no existence if the battery-current were *conducted* by the electrolyte, as suggested by Dr. Faraday, and upon which basis the whole of his electric nomenclature is founded. (See experiments 46, 48 and 49.)

sity. It is observable, that the higher the polar condition, the greater are its powers of transmission, or conduction, through various known aëriform media, liquids and solid bodies. And when at its maximum, as seen in ordinary electricity and lightning, it is found to be accompanied by, and capable of inducing attraction and repulsion of whole substances of a light nature, as gold-leaf, pith-balls, &c. The power of *chemical* combination and decomposition appears to be also dependent upon this quality, *or polar condition* of bodies.

Mr. Gassiot has shown that deflection of gold-leaf may be produced or developed by chemical substances possessing the *lowest chemical powers* in voltaic arrangements, if a *sufficient number* of well-insulated pairs only be employed, which he has also beautifully illustrated by a water battery of 3500 pairs. So it is shown by the following experiments, that a *chemical substance* of the *highest powers of affinity* will develope, *by a very few pairs, the same phænomena.*

An attempt was now made to construct twelve pairs of plates of the description aforementioned. By means of glass pillars and an appropriate framework of wood, each pair was kept insulated from the other, and twelve wine-glasses formed the jars for this beautiful little battery. The whole being completed, an attempt was made to produce the deflection of gold-leaf. A micrometer-screw was arranged so as to cause a copper disc to recede or approach the lower extremity of a suspended slip of gold-leaf, and on connecting the poles of the battery with the same, a deflection was readily produced of above one-tenth of an inch with twelve pairs. The solution of sulphate of copper was here employed *unacidulated.*

I believe Mr. Grove has succeeded in affecting gold leaves by a small number of pairs of his gas battery; and I am not yet aware how small a number of my arrangement will produce sensible action upon the electroscope, but shall shortly attempt this inquiry, when time permits*.

These experiments with potassium tend to show, that there is a *very intimate relation* (if not a complete analogy) between electrical and chemical phænomena, as shown by Sir H. Davy. For the substance which possesses the *highest chemical affinity* is here shown to manifest also the *most exalted electrical energy or tension, and vice versâ*; and *this electrical energy is at all*

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* Since this paper was read, the author has attempted the deflection of gold-leaf by six pairs, and succeeded very satisfactorily. Five pairs, four, three, and two were tried, and a measured deflection took place in each instance; and ultimately *one pair alone produced a sensible and measured deflection.*

times proportional to the measure of the chemical forces employed. The battery was on one occasion *kept in continuous action for two hours*, and, by a little contrivance, the potassium was in each cell simultaneously raised from its membrane into the supernatant naphtha, and remained there in a quiescent state until the following evening, when it was again used with facility; no loss having occurred of any consequence, except in the giving way of three membranes. It was found that, for delicate experiments with one pair, goldbeaters' skin or turkey's craw is considerably more efficient than bladder: decomposition of water is scarcely perceptible when the latter is employed.

XXXI. *Remarks on a Paper by the* Rev. Brice Bronwin, *On the Solution of a particular differential Equation.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THERE is a single explanation which, after reading Mr. Bronwin's remarks published in the last Number of your Journal, I deem it right to make.

Mr. Bronwin observes that I have in various papers made assertions without alleging any proof, or entering into any sufficient explanation, and that he was led to conclude from this, that I held it to be always lawful to reject the supernumerary constants of the solution of a differential equation.

I think it probable that in the instances to which Mr. Bronwin refers, sufficient detail was not given. Having clearly stated in one paper the general principles which decide whether the elements in question are to be rejected or determined, I have in subsequent applications of the same method, contented myself with simply referring to the original paper, and, in one instance, with applying the principle without reference. Mr. Bronwin will find upon examination that the results are in each instance correct, and that the cases are mutually analogous,—the real point of analogy being the relative position of the direct and the inverse factors of operation. But inasmuch as there may really have been a deficiency of needful explanation, I feel it just to make the present acknowledgement; and had the same view of the case presented itself to me before, I would have made it sooner.

I am, Gentlemen,
Your obedient Servant,
George Boole.

August 4, 1848.

XXXII. *An Account of a Method of determining the Total Intensity of the Earth's Magnetic Force in Absolute Measure. By the* Rev. H. Lloyd, *D.D., President of the Royal Irish Academy*[*].

THE ordinary process for the determination of the earth's magnetic force, it is well known, consists in observing the time of vibration of a freely-suspended horizontal magnet, whose moment of inertia is known; and then employing the same magnet to deflect another, similarly suspended, and observing the angle of deflection at a given distance. From these two observations the *horizontal* component of the earth's magnetic force is deduced; and the *total* force is thence inferred, by multiplying by the secant of the inclination.

This method is inapplicable in the high magnetic latitudes. The relative error of the force, arising from a given error of inclination, varies as the tangent of that angle; and where the inclination approaches 90°, it becomes so great as to render the result valueless. I was induced to consider the means of supplying this defect, upon the occasion of the expedition of Sir John Franklin to the Polar Sea in 1845; and I have been recently led to re-examine the problem, on account of the two Arctic expeditions, under Sir James Ross and Sir John Richardson, which are now in course of preparation.

The object to be attained is to determine the total force *directly*, without the intervention of its horizontal component. The ordinary inclinometer will serve for this purpose. The *statical* method, in which the position of the dipping-needle is observed under the combined action of magnetism and gravity[†], will enable us to determine the product of the earth's total magnetic force into the moment of free magnetism of the needle; and the ratio of the same quantities may be obtained (as in the case of the horizontal component) by removing this needle, and employing it to deflect another substituted in its place.

Let us suppose, for generality, that the needle moves in any vertical plane, inclined to the plane of the magnetic meridian by the angle α; and let R denote the earth's magnetic force, X and Y its horizontal and vertical components, and m the magnetic moment of the needle. Then the effective mag-

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[*] From the Proceedings of the Royal Irish Academy, January 24, 1848.

[†] The principle of this method appears to have been first suggested by Mr. Christie, for the *relative* determination of the intensity; and it has been since applied, under different modifications, by Mr. Fox and myself, to the same purpose. Mr. Fox's mode of applying it, although not the simplest in practice, is undoubtedly the best.

netic forces are $mX \cos \alpha$, mY; and their moment to turn the needle is

$$m(Y \cos \eta - X \cos \alpha \sin \eta);$$

in which η denotes the actual inclination of the needle to the horizon. This moment is opposed by that of the weight. Let this be applied in the manner adopted by Mr. Fox, namely, at the circumference of a light pulley, whose centre is on the axis of the cylindrical axle. Its moment is in this case independent of the position of the needle, and is equal to the weight W, multiplied by the radius r, of the pulley at whose circumference it is applied. Accordingly, the equation of equilibrium is

$$m(Y \cos \eta - X \sin \eta \cos \alpha) = Wr. \quad . \quad . \quad . \quad (1.)$$

There are two cases which deserve consideration, namely, that in which the plane of motion of the needle coincides with the magnetic meridian, and that in which it is perpendicular to it. In the former case $\alpha = 0$; and substituting for X and Y their values, $R \cos \theta$ and $R \sin \theta$ (θ being the inclination), the preceding equation becomes

$$mR \sin (\theta - \eta) = Wr; \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

from which we obtain mR, the product of the earth's magnetic force into the moment of free magnetism of the needle, when W and r are known, and the angles θ and η given by observation. In the latter case, $\alpha = 90°$, and (1.) becomes

$$mY \cos \eta = Wr; \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

which gives the similar product in the case of the vertical component of the force.

Now let the needle be removed, and applied to deflect another which is substituted in its place; and let the deflecting needle be placed so that its axis passes through the centre of the supported needle, and is perpendicular to its axis. Then the moment of its force to turn the needle is $mm'U$, in which m' is the moment of free magnetism of the second needle, and U a function of D, the distance of the centres of the two needles, of the form

$$U = \frac{2}{D^3}\left(1 + \frac{p}{D^2} + \frac{q}{D^4}\right).$$

The moment of the earth's magnetic force, opposed to this, is of the form already assigned, in which we have only to substitute m' and η' for m and η. Hence the equation of equilibrium is

$$Y \cos \eta' - X \sin \eta' \cos \alpha = mU. \quad . \quad . \quad . \quad (4.)$$

When the plane of motion of the needle coincides with the magnetic meridian, or $\alpha = 0$, this becomes

$$R \sin(\theta - \eta') = mU; \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

which gives the ratio of the earth's magnetic force to the magnetic moment of the needle, when U is known, and the angles θ and η' given by observation. The coefficients p and q, in the value of U, may be obtained (as in the ordinary method) by observing the angles of deflection, $\theta - \eta'$, at different distances; it is probable, however, that their values may be inferred, *à priori*, from the lengths of the needles, with as much accuracy as is attainable in observations of this nature. When the plane of motion is perpendicular to the magnetic meridian, or $\alpha = 90°$,

$$Y \cos \eta' = mU; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

which gives, in like manner, the ratio of the vertical component to the magnetic moment of the needle.

The total force is determined *absolutely* by means of the two observations in the plane of the meridian: for, multiplying the equations (2.) (5.), m disappears, and we have

$$R^2 = \frac{WrU}{\sin u \sin u'}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

in which the angles of deflection, $\theta - \eta$, $\theta - \eta'$, are denoted for abridgement by u and u'. Again, dividing the former of these equations by the latter,

$$m^2 = \frac{Wr}{U} \cdot \frac{\sin u'}{\sin u}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

The equations (3.) (6.) furnish, in like manner, a similar value of the vertical component of the force.

In order to determine the probable error in the resulting value of the force, arising from the errors of the observed angles, u and u', we have to observe that the moveable needle is acted on, in each case, by two forces, one of which is the moment of the earth's magnetic force $mR \sin u$, while the other is constant. Hence, in any position, the directive force is

$$F = mR \sin u - G.$$

Let u_0 denote the value of u, corresponding to $F = 0$, or to the case of equilibrium; then $mR \sin u_0 = G$, and

$$F = mR(\sin u - \sin u_0).$$

Let $u = u_0 + \Delta u_0$, Δu_0 being a small angle,—or, in other words, let the needle be displaced by a small amount from the position of equilibrium,—and let the force brought into play by the displacement be just balanced by friction; then

$$f = mR \cos u_0 \Delta u_0,$$

f denoting the moment of friction. Now, this being constant for a given instrument, $\cos u_0 \Delta u_0$ is so likewise: and we have

$$\cos u_0 \Delta u_0 = \epsilon,$$

ϵ denoting the value of Δu_0 corresponding to $u_0 = 0$, or the limit of the error due to friction in the natural position of the needle, under the influence of the earth's magnetic force alone.

To find the error in the value of R, corresponding to Δu_0, we have only to differentiate the equation of equilibrium with respect to R and u_0, and we have

$$\Delta \mathrm{R} \sin u_0 + \mathrm{R} \cos u_0 \Delta u_0 = 0\,;$$

and substituting for $\cos u_0 \Delta u_0$, its value above given,

$$\frac{\Delta \mathrm{R}}{\mathrm{R}} = \frac{-\epsilon}{\sin u_0}.$$

We see, then, that the relative error in the value of the force resulting from friction, in either part of the process, is inversely as the sine of the angle of deflection; and that it is therefore requisite for accuracy that these angles should be considerable. The angle of deflection may obviously be as large as we please in the first part of the process, where the deflection is caused by a weight; but in the second, a large deflection can only be produced by a massive magnet, and such a magnet cannot be employed in the first part without impairing the accuracy of the result by the increased friction. The conditions of accuracy required in the two parts of the process are therefore incompatible.

We evade this difficulty by employing the inclinometer for one only (namely, the second) of the two observations, and completing the process by the determination of the magnetic moment of the bar in the ordinary method. This method is applicable to the determination of $m\mathrm{X}$ and $\frac{m}{\mathrm{X}}$ (and, therefore, also to that of m) in the high magnetic latitudes; and we have only to substitute the value so obtained in the formula derived from (5.),

$$\mathrm{R} = \frac{m\mathrm{U}}{\sin u}.$$

In this manner the *relative* determination of R, obtained by the deflection of the dipping-needle, is rendered *absolute*[*].

To compare the probable error of R, found in this way, with that of the same quantity deduced by the ordinary me-

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[*] The deflection of a dipping-needle by a pair of magnets has already been applied by Mr. Fox, in another manner, to the *relative* determination of the total intensity.

thod, we may neglect the errors in the values of mX and $\frac{m}{X}$, common to both processes, as they are small in the high latitudes in comparison with those which arise from the friction of the needle on its supports. Now in the ordinary method, R is deduced from the equation $R\cos\theta = X$; and differentiating this with respect to R and θ, and denoting by ε, as before, the limit of the error of position due to friction,

$$\frac{\Delta R}{R} = \varepsilon\tan\theta.$$

But, in the proposed method, the corresponding error is

$$\frac{\Delta R}{R} = \frac{\varepsilon}{\sin u};$$

which is to the former as $\tan(90^\circ - \theta) : \sin u$. This method is therefore to be preferred to the old in the high magnetic latitudes, provided that the angle of deflection be sufficiently great; and the relative accuracy increases indefinitely as the observer approaches the magnetic pole.

It should be observed that the two observations for the determination of m may be made in a room, where the magnets are under the action of local disturbing forces; it is only necessary that these forces should not be so great as to alter the magnetic distribution in the deflecting bar, and that they should remain unchanged during the observation. This circumstance of course will contribute to the facility of the observation, and to the exactitude of the result. It will probably not be necessary to repeat these observations on every occasion on which the value of R is sought by deflection; the repetition being, in fact, unnecessary so long as the moment of the deflecting bar continues unchanged.

For the observation of deflection it is only required that the inclinometer should be provided with a revolving arm, moveable round the centre of the divided circle, for the support of the deflecting magnet; while a second arm, connected with the former, and at right angles to it, carries the microscopes by which the position of the needle is observed. The general plan of the instruments, now in course of preparation for the Arctic expeditions, is similar to that of one made for me by Mr. Barrow in 1846 (see Proceedings of the Royal Irish Academy, vol. iii. No. 56). The plane of the divided circle is separate from that in which the needle moves, but parallel to it; and there is an adjustment, by which the axle of the needle is brought to coincide in direction with the axis of the divided circle. The circle is six inches in diameter; it is divided to

10′, and read, by verniers, to one minute. The numbering of the graduation commences at each extremity of the horizontal diameter, and extends to 180°. The needle is three inches and a half long, and is enclosed (together with its supports) in a rectangular wooden box with glazed sides. The microscopes by which its position is observed carry each a line in the focus, in the direction of the radius of the circle; and the position of these lines is adjusted by the same means as those employed in the former adjustment.

The plane of the instrument being made to coincide with the magnetic meridian, and facing the east, the deflecting magnet is to be fixed on its support at a given distance, with its north pole *towards* the needle; and the angles of position of the deflected needle, a_1 and a_2, with its north pole towards the north and towards the south, respectively, are to be observed. The deflecting magnet is then to be reversed on its supports, so as to have its north pole turned *from* the needle, its distance being unchanged. Then a_3 and a_4 being the corresponding angles of position, the magnetic inclination is

$$\theta = \frac{1}{4}(a_1 + a_2 + a_3 + a_4);$$

and the angle of deflection is

$$u = \frac{1}{4}(a_1 - a_2 + a_3 - a_4).$$

The observations are to be repeated with the face of the instrument towards the west, and will give new values of θ and u, which are to be combined with the former. We have only to observe that, in this latter case, the arithmetical mean of the four observed angles is the *supplement of the inclination*, instead of the inclination itself.

XXXIII. *An Investigation on the Chemical Nature of Wax.*

By Benjamin Collins Brodie, *Esq.**

I. *On Cerotic Acid, a new Acid contained in Bees'-Wax.*

IN the summer of 1845, while studying at Giessen, in the laboratory of Professor von Liebig, I undertook, at the request of that distinguished chemist, the analysis of certain waxes which were the results of an experiment made by M. Gundlach of Cassel, of feeding bees upon different kinds of sugar. It is not my intention to give those analyses here, and I mention them now only for the purpose of stating that it was this circumstance which first turned my attention to the inquiry of which I now offer the results to the Royal Society,

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* From the Philosophical Transactions, part i.; having been received by the Royal Society March 2, and read March 30, 1848.

and that it was in Professor von Liebig's laboratory that this investigation was begun.

Various chemists have before me undertaken a similar inquiry. The chemical history of a substance so abundant in nature and so useful to man as wax was always a curious question. Of late it has acquired a peculiar interest from our knowledge derived from repeated experiments, that wax is formed in the organs of the bee, and that in the body of that insect that remarkable change of sugar into wax takes place, the knowledge of the true conditions of which would, we may hope, throw light upon the formation of fatty bodies, and on the way by which out of vegetable products the continual repair of the animal structure is effected. The first step to such a knowledge must be the accurate study of the chemical nature of those substances which are thus produced.

But little progress however has been made in this inquiry. I may sum up in a few words those results already known which, by my own experiments, I am able to confirm as true. It has been ascertained that wax is separable by alcohol into two portions, which have been called cerine and myricine; that, by the action of potash upon wax, an acid or acids may be obtained, and also an unsaponifiable body, ceraine; and that by the distillation of wax we obtain volatile oils, solid hydrocarbon, and an acid which has been surmised to be margaric acid, from its resemblance to that substance.

I say that these are the ascertained facts. The high atomic weight of these bodies, and the unavoidable errors of analysis, have rendered it easy to find formulæ for them, and to speculate as to their nature. If, however, the views which, in the following pages, I offer to the Royal Society are correct, their true chemical relations and constitution have been undiscovered.

It would be useless, and it is by no means my intention, to comment upon all the ideas which other chemists have entertained upon this matter. There is however one theory, which has been advanced by certain chemists in France, of which it would be unbecoming in me to take no notice, both because it has a certain apparent amount of fact to support it, and because the originators of it, and others also, believe that they have finally settled the question of the true place which wax should hold in our classification of chemical substances*. M. Lewy has stated that cerine, that portion of the wax which is the more soluble in alcohol, is converted by oxidation by means of lime and potash into stearic acid. The method he

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* *Annales de Chimie*, vol. xiii. p. 439; and Berzelius's *Jahresbericht*, vol. xxiv. p. 468.

pursued was that used by Dumas with such success for the conversion of alcohol into acetic acid; of potatoe oil into valerianic acid; and for other similar transformations. He has explained the reaction by giving to the cerine the formula $C_{68}H_{68}O_4$. On this hypothesis, the cerine, by conversion into stearic acid, loses three equivalents of hydrogen and takes up three equivalents of oxygen into its constitution, the reaction being expressed by the equation

$$C_{68}H_{68}O_4+3HO=C_{68}H_{68}O_7+H_3=\left[\begin{array}{c}\text{stearic acid}\\+H_3\end{array}\right],$$

the cerine being considered the aldehyde of stearic acid. M. Gerhardt has taken up and extended this idea. Proceeding on the belief that the myricine and cerine are isomeric—a belief, I may observe, not justified by experiment, although almost universally adopted by chemists—he has stated that the myricine, of which by far the larger portion of the wax consists, corresponds to the metaldehyde of the same acid, and in a paper which he entitles, "Faits pour servir à l'histoire de la cire des abeilles*," has explained on this hypothesis the origin of the products of the dry distillation of that substance. The theory of M. Lewy agrees sufficiently well with his analyses of the substances in question and with the relations of his formulæ. I must however observe, that any person who will take the trouble of reckoning out those analyses on which the formula of stearic acid depends, according to the atomic weight of carbon now almost universally adopted by chemists, carbon 6, hydrogen 1, will see that this formula itself demands a new inquiry before we can accept it. These results of M. Lewy are in many ways at variance with my own experiments; but I confess that if the agreement of the analysis and of the melting-point of the substance he obtained by oxidation of the cerine with the analysis and the melting-point of stearic acid were in truth a sufficient proof of the identity of the bodies, this difference in our results would be to me very difficult to explain. Wax certainly stands in a remarkable relation to fat, but I do not believe that relation to be of the nature M. Lewy has conjectured, and I cannot but think that, in his desire to establish this relation, he has rested content with insufficient proofs of his theory. Any person who has had a little experience in these inquiries, must know how deceptive this melting-point and analysis are as criteria of the purity or identity of bodies. The separation of this class of substances by crystallization is difficult, and often the composition per cent. of two substances of entirely different chemical formulæ will agree within the unavoidable errors of the method of

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* *Annales de Chimie*, vol. xv. p. 236.

analysis. The reasons will hereafter appear why I am justified in saying that, in the cerine which M. Lewy analysed, he did not work on a pure chemical substance; and that consequently any theory founded on the reactions of this body must fall to the ground; I believe that by more careful inquiry he might have procured that substance of a different melting-point and constitution to that which he found. I have also in vain searched the papers which M. Lewy and M. Gerhardt have published upon this matter, to find some account of the preparation and the analysis of a salt of this so-called stearic acid, an easy and satisfactory experiment, and one which alone in the case of acids of such high atomic weight, can justify the chemist in pronouncing on their constitution. Until such a salt is made, I cannot but consider that the evidence is insufficient, that the remarkable oxidation in question has ever been effected.

I propose to give to the Society, in three papers, the results of an investigation on the nature of wax. The present paper will contain an inquiry as to the constitution of the so-called cerine; I mean that portion of the bees'-wax which is the more soluble in boiling alcohol. The second paper will treat of the chemical constitution of a wax from China, a substance which, although it considerably differs in its appearance and properties from bees'-wax, in the form in which it comes before us in nature, is nevertheless, chemically speaking, closely analogous to that body. In a third paper I propose to consider the nature of myricine, the other constituent of the bees'-wax itself. I may here state, that to ensure the purity of the wax used in the following experiments, I prepared it myself from the comb. It was made by bees in the county of Surrey in the years 1845 and 1846. This wax I have always used for the first preparation of a substance. For further experiment I have sometimes used wax procured in other ways.

Cerotic Acid.

If wax melting at about 62° or 63° Centigrade be treated with boiling alcohol, a considerable portion will be dissolved. If this operation be repeated, the quantity of substance dissolved the fifth or sixth time will be evidently less than that dissolved in the first operation. But however often this operation be repeated, there will always be a portion of wax dissolved. This fact alone might lead us to suspect that any absolute separation of these two portions of the wax, by boiling with alcohol and subsequent crystallization out of that liquid, was impossible.

A partial separation can, however, be readily effected, and by this method a substance can be obtained melting at 70° and even 72° Centigrade, much harder than the residue of the wax, brittle, and slightly crystalline in its structure. If this substance be boiled with a solution of caustic potash, it is saponified with the greatest facility. If the soap be decomposed by an acid, a baryta salt formed of the acid produced, and this salt be dried and washed out with æther, a certain portion of a substance will be taken up by the æther, which is but very slightly acted on by potash. This is that substance which has been called ceraine. I remarked that while from the residue of the wax this matter could be procured in considerable quantities, but a very small portion was obtained from the cerine; a portion very small in relation to the acids formed, and the quantity of which diminished with the purification of the substance. The analysis also of the unsaponified cerine agreed closely with that of the acid as separated from the baryta salt, and the melting-points only differed by four or five degrees Centigrade. Further experiment showed me that the same cerine, when boiled with caustic baryta, entered with the greatest facility into combination with that substance. These facts led me to suspect that the formation of the unsaponifiable body was due to the presence of a certain portion of the other substances of the wax, and was no true product of the decomposition of the cerine itself; but that this substance was in truth no other than the acid itself existing, in a free state, in the wax. The experiments which follow are inconsistent with any other hypothesis.

To prepare this acid bees'-wax is to be boiled out with strong alcohol, and the hot solution poured off from the undissolved residue. This operation may be repeated with advantage three or four times. The precipitates which are formed on the cooling of the alcohol are to be collected together and treated repeatedly with alcohol in the same manner as the wax itself, until the melting-point of the precipitate is raised to about 70° C., after which point the further purification of the body by this method of crystallization is extremely difficult. This substance is now to be dissolved in a large quantity of boiling alcohol, care being taken perfectly to effect the solution. To this solution is to be added a boiling solution of acetate of lead in alcohol, by which a voluminous precipitate is produced, which does not redissolve in the boiling mixture. The solution is to be boiled and filtered hot, by means of a hot water-apparatus, from the precipitate. The precipitate while moist is to be taken from the filter, placed again in the flask, and boiled out with strong alcohol, and afterwards with æther. This operation is to be repeated several times until the fluid which passes through the filter carries no substance with it. The first portions of alcohol will contain considerable portions of a wax matter having a lower melting-point than the cerine, and having the appearance of the residue undissolved by the hot alcohol.

The lead salt is to be decomposed by very strong acetic acid. The substance which separates, after having been well-washed with boiling water, is to be dissolved in absolute alcohol, and the solution, while hot, filtered. On the cooling of the alcohol a substance will crystallize out in fine granular crystals. This substance melts at about 78° C. It is highly crystalline on cooling from the melted state. Very carefully prepared, it gave on analysis the following numbers. The analysis was made with chromate of lead.

0·26 grm. of substance gave 0·753 grm. of carbonic acid and 0·307 grm. of water.

These analyses give in 100 parts—

Carbon	78·98
Hydrogen . . .	13·12
Oxygen	7·90
	100·00

This corresponds to the formula $C_{54} H_{54} O_4$.

	Atomic weight.	Calculated in 100 parts.
C_{54} . . .	324	79·02
H_{54} . . .	54	13·17
O_4 . . .	32	7·81
	410	100·00

It is very difficult to wash this lead salt perfectly out. Even in the present case, after the greatest care, I obtained a trace of a substance, soluble in æther, on attempting further to purify the substance by combining it with baryta and washing out its baryta salt. This makes no difference in the numbers or the melting-point. But it is advisable to prepare the pure acid by boiling the acid as separated from the lead salt with caustic potash, precipitating the baryta salt by a large excess of chloride of barium, and carbonate of soda; washing this well out with æther; decomposing with an acid, and crystallizing the resulting substance repeatedly out of alcohol and æther. The acid prepared in this manner melted at 78°, 79° C., being the same melting-point as the acid separated from the lead salt; it gave to analysis the following numbers:—

I. 0·263 grm. gave 0·7583 carbonic acid and 0·3085 water.

II. Another preparation. 0·2615 grm. gave 0·7558 carbonic acid and 0·307 water.

III. 0·2612 grm. gave 0·7562 carbonic acid and 0·308 water.
These analyses correspond in parts per cent. to—

	I.	II.	III.
Carbon . .	78·63	78·82	78·95
Hydrogen .	13·04	13·04	13·10
Oxygen . .	8·33	8·14	7·95
	100·00	100·00	100·00

The silver salt of this acid was made by precipitating the ammoniacal solution of the acid in alcohol with nitrate of silver. It is desirable to take an excess of ammonia, and the solution must be precipitated boiling by a hot alcoholic solution of the nitrate. The salt is to be washed with water and dried, first *in vacuo*, then in a water-bath.

I. 0·469 grm. of this salt gave 1·067 carbonic acid and 0·4315 water.

II. 0·4663 grm. of this salt gave 1·0675 carbonic acid and 0·4273 water, giving in 100 parts,—

	I.	II.
Carbon	62·04	62·43
Hydrogen	10·22	10·18
Oxygen and silver .	27·74	27·39
	100·00	100·00

I. 0·6566 grm. of this salt gave 0·1413 grm. of silver.
II. 0·6508 grm. of this salt gave 0·1388 grm. of silver.
III. 0·6147 grm. of this salt gave 0·1292 grm. of silver.
IV. 0·6641 grm. of another preparation gave 0·1396 grm. of silver.
V. 0·665 grm. of the same preparation gave 0·1396 grm. of silver.

These determinations correspond in 100 parts to—

	I.	II.	III.	IV.	V.
Silver	21·52	21·33	21·02	21·02	20·99

The above analyses agree with the formula $C_{54}H_{53}O_3,AgO$.

	Atomic weight.	Calculated in 100 parts.
C_{54}	324·0	62·66
H_{53}	53·0	10·25
O_4	32·0	6·19
Ag	108·1	20·90
	517·1	100·00

Cerotic Æther.

The combination of this acid with the oxide of ethyle is readily obtained by dissolving the acid in absolute alcohol, and passing hydrochloric acid gas through the solution. This substance has the appearance of a soft wax, and melts at 59°–60° C.

I. 0·2628 grm. gave 0·765 carbonic acid and 0·3094 water.

II. 0·2638 grm. gave 0·7695 carbonic acid and 0·3105 water, giving in 100 parts—

	I.	II.
Carbon . .	79·38	79·55
Hydrogen .	13·08	13·08
Oxygen . .	7·54	7·37
	100·00	100·00

To obtain these numbers the action must be long continued. They correspond to the formula of the æther,

$$C_{58}H_{58}O_4 = C_{54}H_{53}O_3 + C_4H_5O.$$

	Atomic weight.	Calculated in 100 parts.
C_{58}	348	79·45
H_{58}	58	13·24
O_4	32	7·31
	438	100·00

Chlor-Cerotic Acid.

Cerotic acid is readily acted on by chlorine if melted in that gas. Hydrochloric acid is formed, and a substitution of chlorine for hydrogen in the substance takes place. The following substance was made by passing a current of chlorine over the acid, melted in a small flask in a water-bath. The action must be continued for several days. I considered the action as terminated when no more vapour of hydrochloric acid could be perceived. The substance had now undergone a remarkable transformation in appearance, a transformation similar to which may be effected in all the wax substances with which I have experimented.

It was perfectly transparent, slightly yellow, and of the consistency of a very thick gum, more than viscid, and yet capable of being drawn out in threads.

I. 0·382 grm. of this substance gave 0·5581 carbonic acid and 0·1752 water.

II. 0·401 grm. of this substance gave 0·5848 carbonic acid and 0·184 water.

These analyses give in 100 parts—

Carbon	39·82	39·77
Hydrogen	5·10	5·09
Chlorine and oxygen . .	55·08	55·14
	100·00	100·00

I. 0·486 grm. by the usual method of analysis gave 1·012 grm. chloride of silver, containing 0·249 chlorine.

II. 0·6715 grm. by the usual method of analysis gave 1·3915 grm. chloride of silver, containing 0·344 chlorine.

These analyses give as the per-centage of chlorine—

	I.	II.
Chlorine . . .	51·40	51·28

These analyses lead us to the formula

$$C_{54}\left\{\begin{matrix}H_{42}\\ Cl_{12}\end{matrix}\right. O_4.$$

	Calculated in 100 parts.
C_{54}	39·45
H_{42}	5·10
Cl_{12}	51·50
O_4	3·95
	100·00

Chlor-Cerotic Æther.

The last substance has the properties of an acid, and gives with soda a salt almost insoluble in water. I prepared this salt but did not analyse it, as the substance was lost in attempting to dry it on a water-bath, in which case it is decomposed. The compound æther, however, of the acid may be obtained in a similar way to that in which is obtained the compound æther of the cerotic acid itself. The numbers which this æther gave on analysis, taken with those of the chlor-cerotic acid, and of the cerotic acid, are conclusive as to the formulæ of these bodies. The appearance of this æther is similar to that of the acid from which it is derived.

0·454 grm. gave 0·6815 carbonic acid and 0·214 water, which analysis gives in 100 parts—

Carbon	40·94
Hydrogen	5·24
Oxygen and chlorine .	53·82
	100·00

0·650 grm. of the substance gave 1·332 grm. of chloride of silver, which contains 0·3294 grm. of chlorine, and corresponds to chlorine 50·68 parts per cent.

The formula of the æther $C_{58}\left\{\begin{matrix}H_{46}\\ Cl_{12}\end{matrix}\right. O_4$ requires in 100 parts

	Calculated.
C_{58}	41·11
H_{46}	5·40
Cl_{12}	49·99
O_4	3·50
	100·00

which agree remarkably with the numbers found; it being remembered that the body cannot be crystallized or indeed in any way purified.

Distillation of Cerotic Acid.

Cerotic acid is volatile. If the acid as separated from the lead salt by acetic acid be distilled, the acid separated from the distillate and purified by boiling with potash and washing the baryta salt with æther, it will again be procured with a melting-point nearly corresponding to that of the original acid.

0·251 grm of this substance gave 0·7254 carbonic acid and 0·2977 water, giving in 100 parts—

Carbon . . .	78·80
Hydrogen . .	13·19
Oxygen . . .	8·01
	100·00

being precisely the same numbers as the substance gave before distillation.

A question suggests itself, if it be true that this acid is volatile, and it be also true that it exists in the wax in an uncombined state, how has it happened that it has never been discovered among the products of the distillation of wax which have been so often examined?

In truth, it is a remarkable fact, that while this acid, when heated in a nearly pure state, as separated from the lead salt, distils over nearly unaltered, the same substance, when distilled in an impure state, as separated, namely, by alcohol from the wax, and mixed, as in this case, with other wax matter, which is decomposed by heat, itself suffers decomposition. In the course of this investigation other examples have occurred to me of similar facts.

About three ounces of the cerine, melting at 70° C., from which this acid can be precipitated by acetate of lead, was distilled. The first portions of the distillate consisted entirely of oil. Towards the end a small quantity of solid matter appeared. The oil, on standing, separated into two portions: the lower part was withdrawn by a pipette, being but a small portion, say one-twentieth of the whole. The upper portion was boiled with potash, only a very small portion of acid however was separated in this manner. The soap was drawn off by a syphon, and the oil washed out with water. On being washed with strong alcohol the oil dissolved, almost entirely, leaving only a small portion of solid matter of the appearance of paraffine. This oil consists of a mixture of oils of different boiling-points, which were separated as far as possible by rectification, but during the distillation the boiling-point kept constantly rising, and I could obtain no oil in which it was absolutely constant.

Three of these oils, of the respective boiling-points of 210° to 220° C., 230° to 250° C., and 250° to 274° C., were analysed. By far the greater portion of this whole distillate went over between 230° and 250° C. The results of these analyses were—

In the case of the first oil,

(1.) 0·2676 grm. gave 0·826 carbonic acid and 0·3307 water.

In the case of the second oil,

(2.) 0·2629 grm. gave 0·8127 carbonic acid and 0·3325 water.

In the case of the third oil,

(3.) 0·2697 grm. gave 0·8443 carbonic acid and 0·341 water, giving in 100 parts—

	I.	II.	III.
Carbon . .	84·17	84·30	85·37
Hydrogen .	13·73	14·05	14·05
Oxygen . .	2·10	1·65	0·58
	100·00	100·00	100·00

The amount of oxygen therefore diminishes as the boiling-point rises in these oils, the last oil being nearly pure hydrocarbon. It is useless to calculate formulæ for them: I give however the analyses to point out the source of the oils in the wax distillate, and to account for the disappearance of the cerotic acid in that product.

The products of this distillation prove also that we must look to some other body than the cerine for the source of the margaric acid, or the acid resembling that substance which has been found in the wax distillate, and for the origin also of the paraffine, of which only traces can be found even on the distillation of the impure cerine.

The existence of a free acid in wax is a fact to which we have no parallel in the constitution of any known fat, and although the reactions, which I have given, left little doubt upon my mind that in the wax the acid was in this condition, it yet appeared to me desirable to procure it from the wax by simple crystallization. By patience this may be accomplished. The cerine analysed by M. Lewy melted at 62°·5 C. I have stated that by means of alcohol this substance may be procured of a melting-point of 72°. If this substance of 72° melting-point be repeatedly dissolved and crystallized out of a large quantity, not of alcohol, but of æther, the melting-point can be raised to 78°, in which state the substance is highly crystalline, and has all the appearance of the acid as procured by other means.

0·25625 grm. of this gave 0·7435 CO_2 and 0·3005 HO, which gives in 100 parts—

Carbon	79·13
Hydrogen . . .	13·20
Oxygen	7·67
	100·00

Both the melting-point and the analysis perfectly agree with the melting-point and analysis of cerotic acid, which have been already given, and with its formula.

It is certainly a strange fact that the presence of this body in the wax should so long have escaped the notice of chemists. The wax however is a complex substance, and the cerotic acid to be obtained in purity has to be separated from many other bodies which disguise its nature and reactions.

Should any chemist be induced to verify the results which I have given, I must beg him also rigidly to observe the methods I have laid down for the preparation and purification of the substances, for errors which are slight in the analysis of the substance and which neither the analysis nor melting-point detect, become of great importance when the transformations of the substance are investigated, and its atomic weight is to be determined.

The alcoholic extract, out of which the cerotic acid has crystallized, contains yet another acid, although in very small quantity. If to the solution an alcoholic solution of acetate of lead be added, a precipitate of a lead salt is produced: this salt is readily distinguished from the salt of the cerotic acid as it is dissolved on boiling the alcoholic solution, out of which, on cooling, it will separate in crystalline grains. This substance is contained in very small quantity in the wax. It resembles in appearance margaric acid. I have analysed this acid and one of its salts. These experiments showed that it approached margaric acid in its constitution: but I have never been able to procure the quantity necessary to its sufficient purification, and the analyses led to no conclusions as to its formula which deserve to be recorded.

It is a matter of interest to know in what proportions, relative to its other constituents, the cerotic acid is contained in the wax. This may readily be determined by precipitating by acetate of lead the solution of a known quantity of wax and determining the quantity of the lead salt produced. It is necessary however, in order to know to what quantity of acid in the wax the found quantity of lead salt corresponds, to know the atomic weight of the lead salt itself. There is great difficulty in washing out this lead salt so as perfectly to remove from it the myricine. In order therefore to effect any satisfactory purification of it, I was obliged to operate on a smaller quantity of the salt than I should otherwise have chosen. My experiment, however, was sufficient to show that the salt is the neutral salt containing one equivalent of lead.

0·1445 grm. of the lead salt gave when ignited 0·032 grm. of solid residue. This residue, extracted with acetic acid and dried, left 0·0065 grm. of residue, and lost 0·0255 grm., which, reckoned as oxide, contains 0·0236 grm. lead. This gives 0·0296 as the whole quantity of lead in the salt. Reduced to parts per cent. this gives 20·48 as the per-centage of lead. The formula $C_{54}\ H_{53}\ O_3 + PbO$ requires 20·26 per cent.; this therefore is the formula of the salt.

To determine the proportion of the acid in the wax itself, a portion of pure yellow wax was dissolved in æther and filtered from adhering impurities, then dissolved in naphtha-æther and precipitated by acetate of lead dissolved in alcohol; an additional portion of æther was afterwards added to ensure the entire precipitation of the salt, the solution was filtered hot, and the lead salt was carefully washed on the filter.

1·0905 grm. of wax treated in this manner gave 0·3015 grm. of the lead salt, which, assuming it to be the neutral salt, is equivalent to 0·24 grm. of the acid $C_{54}\ H_{54}\ O_4$. This reduced to parts per cent. gives 22·0 of the cerotic acid in every 100 parts of bees'-wax.

Although this was the proportion of cerotic acid in the bees'-wax I had prepared, it by no means followed that other specimens of wax when examined would give a similar result. Various chemists have stated that the proportions of the cerine varied in different specimens of wax; and although the estimate of the cerine was a mere matter of guess, no criterion of its presence or absence being known, yet nevertheless the great difference in the results of the different chemists who have attempted to determine this point led me to suspect that the quantity of the cerotic acid in the wax did in truth vary; and that it might be possible, if so, to find a wax which even did not contain this body. I have found this acid in all the bees'-wax made in this part of the world, bleached or otherwise, which I have examined [*]. It occurred to me however that wax made under very different conditions of climate and vegetation to ours might possibly have a different constitution. With this view I procured, through the kindness of a friend who was resident in Ceylon, some bees'-wax from that island. The wax as I received it was white, having in all respects the appearance of English wax, and melting at 63° C. In other points also, as I shall show in another paper, its chemical nature was the same as that of English wax. The cerotic acid however was entirely absent. When extracted in the boiling alcohol a portion of the wax dissolves; but on the addition of acetate of lead to the alcoholic solution hardly a trace of precipitate is formed.

Such a variation in the nature of an insect-secretion under different conditions of life is a remarkable fact, which we may place by the side of the curious difference in the nature of the constituents of butter in different years, discovered by Lerch. In his excellent investigation on the nature of the volatile acids of butter, this chemist discovered [†] that the butyric and caproic acids of one year were in another year replaced by vaccinic acid, an acid from which, by deoxidation, the two former acids might readily be formed.

$$\overbrace{C_{20}\,H_{20}\,O_7}^{\text{Vaccinic acid.}} = \overbrace{C_8\,H_8\,O_4}^{\text{Butyric acid.}} + \overbrace{C_{12}\,H_{12}\,O_4}^{\text{Caproic acid.}} - O.$$

The cerotic acid, $C_{54}\,H_{54}\,O_4$, although far removed in the series of fatty acids from these volatile acids of butter, yet nevertheless belongs to the same chemical series of bodies, to that series, namely, which contain carbon and hydrogen in equal equivalent proportions, and which, theoretically at least, are capable of being produced by deoxidation from sugar or from starch.

Any fact is of importance which can throw light upon the law by which these substances are truly convertible, one into the other, and it would be highly interesting to investigate, in those waxes where the cerotic acid is not found, by what bodies, if any, it can be replaced.

13 Albert Road, Regent's Park,
February 28th, 1848.

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[*] I should except a wax made by wild bees in Wiltshire, which I tested with acetate of lead for this acid, but could find none. The quantity of the wax however was so small that I was unable to make many experiments with it.

[†] *Annalen d. Ch. und Pharm.* vol. xlix. p. 230.

XXXIV. *On the Action of Chlorine on Anilic Acid.*

By JOHN STENHOUSE, *Esq., Ph.D.*[*]

IT is stated in most of the systems of chemistry that anilic acid is not acted on by chlorine. This however is by no means correct; for when a solution of anilic acid is digested with a mixture of chlorate of potash and muriatic acid, no chloropicrine is formed, but the anilic acid is rapidly converted into chloranile.

When a stream of chlorine gas is sent through a hot saturated solution of anilic acid, the acid is speedily attacked; and if the action of the chlorine is continued for a couple of days, the anilic acid is wholly changed into chloranile. When anilic acid is boiled with an excess of hypochlorite of lime no chloropicrine is evolved, and the solution assumes a dark brown colour. When this solution is saturated with muriatic acid, a flocculent matter precipitates, forming a yellowish uncrystallizable resin.

The action of chlorine is therefore a very easy mode of distinguishing anilic acid from chrysammic, oxypicric or nitropicric acids, and of detecting any mixture of these acids in anilic acid, as the presence of the smallest portion of any of these acids is immediately indicated by the formation of chloropicrine.

XXXV. *Notices respecting New Books.*

Results of Astronomical Observations made during the years 1834, 1835, 1836, 1837, 1838, *at the Cape of Good Hope; being the Completion of a Telescopic Survey of the whole Surface of the visible Heavens, commenced in* 1825. *By* Sir JOHN F. W. HERSCHEL, *Bart., K.H., &c. &c.* London: Smith, Elder and Co. 1847. (*Fourth notice.*)

Chap. IV. *Of the Distribution of Stars, and of the Constitution of the Galaxy in the Southern Hemisphere.*

THE present chapter is divided into three sections, which treat—1st, of the statistical distribution of stars; 2nd, of the general appearance and telescopic constitution of the Milky Way in the southern hemisphere; and 3rd, of some indications of very remote telescopic branches of the Milky Way, or of an independent sidereal system, or systems, bearing a resemblance to such branches.

The subject of the distribution of the stars over the surface of the visible heavens had received little attention previous to the time of Sir William Herschel, whose researches, and their results, are thus briefly alluded to:—

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[*] Communicated by the Author.

"That the comparative abundance or paucity of stars in any particular region of the northern hemisphere, so far as visible in our latitudes, has reference to its situation in respect of the Milky Way, is a proposition which the researches of Sir William Herschel have sufficiently established, not merely by the general aspect of the heavens when viewed with the naked eye, but also when examined with powerful telescopes in a mode which may properly be called statistical,—that is to say, by counting the number of stars in the field of view of one and the same telescope at a great number of given points in the concave of the heavens, and so estimating their comparative populousness in stars, in their different districts. The results of a system of observation of this nature have, as is well known, conducted him to the highly remarkable and interesting conclusion above-mentioned, and to this further consequence, that all the stars visible to us, whether by unassisted vision, or through the best telescopes (such at least as are scattered, or not congregated in resolvable nebulæ, or globular or similar highly condensed clusters), belong to and form a part of a vast *stratum*, or considerably *flattened* and unsymmetrical congeries of stars in which our system is deeply, though excentrically plunged, and, moreover, situated near a point where the stratum bifurcates or spreads itself out into two sheets."

It may be readily supposed that the opportunity of carrying out this great induction by similar observations in the southern hemisphere was not neglected; and accordingly, so soon as a knowledge of the regions where nebulæ might more especially be expected had been acquired, or where, without too much risk of missing such objects the sweeps might be interrupted for the purpose of gauging,—*i. e.* of counting the number of visible stars in determinate fields of view,—a system of star-gauges was set on foot, "so as to dot over the surface of the heavens as it were with a regular tesseration of gauged or counted fields, disposed at regular and equal intervals of right ascension and polar distance." According to the plan laid down, the gauges were taken when the telescope, in the regular course of sweeping, arrived at either extremity and at the middle of the zone under examination, so that the intervals were 10^{m} of time in R.A., and $1^{\circ} 30'$ of arc in P.D. It was not indeed expected, or even considered possible, that so great a multitude of fields as this plan supposes could be actually counted; but a system of some sort was absolutely necessary to prevent confusion and unequal observing, and more especially to ensure an absolute impartiality in the selection of the gauge-points; and it was also foreseen that many of the fields would inevitably be passed over from the interference of the regular and more important business of the sweep. On the whole about 2300 gauges were obtained in this manner, which, though leaving some blanks, afford ample materials "for testing the validity of the induction in question, and for estimating the comparative richness of every considerable district in the southern heavens."

After some further details relative to the mode of observing and arranging the results, the author gives a "synoptic table of southern star-gauges, arranged on meridians and parallels." He then proceeds to examine how far the induction before alluded to is borne out by the observations, for which purpose the following process was instituted. The poles of the great circle marking the medial line of the Milky Way were first found, their places being ascertained to be in $0^h\ 47^m$ R.A. and 116° 0′ N.P.D. for the southern pole, and $12^h\ 47^m$ R.A. and 64° 0′ N.P.D. for the northern. To this great circle, which Sir John happily designates the *Galactic circle*, a series of parallels were drawn on an 18-inch globe, dividing the heavens into zones of 15° each in breadth, and the points of intersection of these with the meridians, taken at intervals of twenty minutes, were read off in N.P.D. by the aid of the brass meridian arc, and transferred to a chart containing the actual projections of all the observed gauges. These points were then connected by regular curves, which are consequently the projections of the series of parallels abovementioned, or the galactic parallels, the spaces between them being the projections of the zones into which they divide the surface of the sphere. This done, the next step consisted in counting the number of fields of view actually laid down on the charts, and in adding into one sum the number of stars observed. The following are the results obtained :—

"In the oval representing the circle of 15° radius surrounding the southern pole of the galactic circle were found to have been observed 43 fields, containing in all 260 stars, which gives for the average density of stars in this region of the heavens, 6·05 stars to a field of 15′ in diameter, visible in the 20-foot reflector with the usual sweeping power 180.

"In the zone comprised between the parallels of 15° and 30° galactic south polar distance 144 gauges were found to occur, containing an aggregate of 953 stars, or 6·62 stars to a field.

"In the zone comprised between 30° and 45° were observed 218 gauges, containing 1980 stars, which gives an average of 9·08 stars to a field.

"In the zone from 45° to 60° were found 375 gauges containing 5060 stars, being 13·49 stars to a field.

"From 60° to 75° were found to have been observed 472 gauges containing 12409 counted stars, whence the average of 26·29 stars to a field.

"From 75° to 90°, being the zone immediately adjacent to the Milky Way on the south side, and containing half its breadth, 442 gauges are registered containing 26105 stars. Among these occur some gauges in which the stars are set down as 'innumerable' or so crowded as to be impossible to count them without bestowing more time and care than the nature of the object in view was judged to require. These are taken when they occur at 200 stars to the field, which is certainly not an excessive estimate. There are, however, not above five or six gauges in this predicament. The average density of stars in this zone, according to the numbers above stated, is 59·06 to a field.

"A zone of 3° in breadth bisected throughout by the galactic circle, or extending 1½° on its north and as much on its south side, was found to include 84 gauges containing 6258 stars, giving an average of 74·50 stars to a field. The average would have been much higher if, instead of following the course of this circle, a zone of equal breadth pursuing the irregular line of maximum intensity of the Milky Way had been chosen, which in some places deviates by several degrees from the great circle which expresses its general situation. Judging from the course of the counted gauges only, the mean density of stars in the medial line of the actual galaxy in that part I have observed would be somewhere about 90 stars to the field, but this must be considered as exclusive of the more densely clustering masses."—P. 380.

On the northern side of the galactic circle the average number of stars to a field was found to be as follows:—In the zone extending from 0° to 15°, 51·28; from 15° to 30°, 23·47; from 30° to 45°, 14·46; from 45° to 60°, 7·71. The number of gauges taken in those zones were respectively 321, 195, 68 and 21.

"Nothing," says Sir John Herschel, "can be more striking than the gradual but rapid increase of density on either side of the Milky Way as we approach its course, and the reproduction of nearly the same law of graduation on the north side which holds good on the south, so far as the comparative paucity of the gauges taken in that direction allow us to judge. On the whole, this induction, founded as it is on the actual enumeration of 68948 stars contained in 2299 fields, must be admitted as decisive of the specific point in question, and as completing the evidence to the same effect afforded by Sir William Herschel's observations in the northern hemisphere."

Calculating upon the above averages the number of stars *visible enough to be distinctly counted in the 20-foot reflector* over the whole sphere, it will be found to be 5,331,572, or somewhat less than five and a half millions. "That the actual number is much greater there can be little doubt, when we consider that large tracts of the Milky Way exist so crowded as to defy counting the gauges, not by reason of the smallness of the stars, but their number."

It will be understood that great local departures from the law of distribution above indicated occur in all regions, and nowhere more remarkably than in the Milky Way itself, whose irregularities of breadth and structure are most conspicuous and singular; but with the exception of portions of the galaxy, nothing was found in any part of the heavens meriting in the smallest degree to be regarded as *systematic*, as respects those deviations from perfect regularity. So purely local are they, that on a careful revision of the whole chart Sir John found it difficult to specify any considerable areas over which an average density of stars prevails materially differing from what might be expected from the law above indicated, regarded as a function of the galactic polar distance.

Another interesting question remains, namely, whether the increased frequency of stars in approaching the Milky Way is observable in respect of stars of all classes of magnitude indifferently. In order to ascertain this point, not only the total numbers of stars were set down in counting the gauges, but those of all the several magnitudes down to the eleventh inclusive; and a table is given in which are exhibited the numbers of all the stars of the several magnitudes occurring in the registered gauges, distributed in the respective zones of galactic polar distance. The following are the results :—

"On a general view of the table it appears that the tendency to greater frequency, or the increase of density in respect of statistical distribution, in approaching the Milky Way, is quite imperceptible among stars of a higher magnitude than the 8th, and except on the very verge of the Milky Way itself, stars of the 8th magnitude can hardly be said to participate in the general law of increase. For the 9th and 10th, the increase, though unequivocally indicated over a zone extending at least 30° on either side of the Milky Way, is by no means striking. It is with the 11th magnitude that it first becomes conspicuous, though still of small amount when compared with that which prevails among the mass of stars of magnitudes inferior to the 11th, which constitute sixteen-seventeenths of the totality of stars within 30° on either side of the galactic circle.

"Two conclusions seem to follow inevitably from this; viz.—1st, that the large stars are really nearer to us (taken *en masse*, and without denying individual exceptions) than the smaller ones . . . . ; 2nd, that the depth at which our system is plunged in the sidereal stratum constituting the galaxy, reckoning from the southern surface or limit of that stratum, is about equal to that distance which, on a general average, corresponds to the light of a star of the 9th or 10th magnitude, and certainly does not exceed that corresponding to the 11th."—P. 383.

The 2nd Section of this chapter contains a minute and exceedingly interesting description of the general and telescopic appearance of the Milky Way—too long, however, to be extracted here *in extenso*, and scarcely susceptible of abridgement. We must make room for two or three paragraphs.

"Immediately after the contraction between λ *Centauri* and δ *Crucis* the Milky Way suddenly expands so as to include the southern half of the cross and the northern portion of *Musca*, whence it proceeds to β *Centauri*, embracing in this wide expansion that singular vacuity on the south following side of the cross, called the 'Coal-sack,' a pear-shaped oval, whose greatest length is about 8°, and breadth 5°, the longer axis being nearly parallel to the line joining α and β Crucis, which line is very nearly a tangent to the north-preceding portion of its circumference. As this is always regarded by voyagers and travellers as one of the most conspicuous features of the southern sky, it may not be irrelevant to state a few particulars as to its telescopic constitution. It is by no means entirely devoid of stars, the lowest gauges being 9 and 7, and no blank fields being specified as occurring in it. The cluster of telescopic stars h. 3407 is actually contained within its area; and even in the middle of its extent gauges of 29 and 48 stars are noted. Its striking blackness is, therefore, by no means owing to an absolute want of telescopic stars, but rather to its contrast with the very rich portion of the Milky Way adjacent, where the gauges run up to 98, 100, 120, and even

200 on the preceding side—to 65, 103, 108, on the following; 68 on the south, and 90 on the north. The contrast is enhanced by the suddenness of the transition."—P. 384.

"It is about this region, or perhaps somewhat earlier, in the interval between *η* Argûs and *α* Crucis, that the galactic circle, or medial line of the Milky Way, may be considered as crossed by that of the zone of large stars which is marked out by the brilliant constellation of Orion, the bright stars of Canis Major, and almost all the more conspicuous stars of Argo, the Cross, the Centaur, Lupus, and Scorpio. A great circle passing through *ε* Orionis and *α* Crucis will mark out the axis of the zone in question, whose inclination to the galactic circle is therefore about 20°, and whose appearance would lead us to suspect that our nearest neighbours in the sidereal system (if really such), form part of a subordinate sheet or stratum deviating to that extent from parallelism to the general mass which, seen projected on the heavens, forms the Milky Way."—P. 385.

"Bode's, and most other celestial charts, make the Milky Way bifurcate, in Cygnus and the tail of Scorpio, into two great streams, both of which, the preceding and the following, preserve their continuity, from point to point, unbroken. This, however, is the case only with the following, or main stream, whose course we have just traced. The preceding is discontinuous. Its northern portion (from Cygnus southwards) terminates precisely at the equator, just beyond the bisection of a line *η* Serpentis and *β* Ophiuchi; and from this to the nearest point of its southern continuation (if it can be so considered) there is a break of 14° in extent to the star *o* Serpentis, totally devoid of all appearance of it."

"If we now consider the telescopic structure of the region spread over by, and inclosed between the effusions of the Milky Way in the body and tail of Scorpio, the hand and bow of Sagittarius, and the following leg of Ophiuchus, we shall find it, in the highest degree, interesting and complex. No region of the heavens, in fact, is fuller of objects beautiful and remarkable in themselves, and rendered still more so by their mode of association, and by the peculiar features assumed by the Milky Way, which are without a parallel in any other part of its course."—P. 386.

The description is wound up with the following remarks:—

"From the foregoing analysis of the telescopic aspect of the Milky Way in this interesting region, I think it can hardly be doubted that it consists of portions differing exceedingly in distance, but brought by the effect of projection into the same or nearly the same visual line; in particular, that at the anterior edge of what we have called the main stream, we see, foreshortened, a vast and illimitable area scattered over with discontinuous masses and aggregates of stars in the manner of the cumuli of a mackerel sky, rather than of a stratum of regular thickness and homogeneous formation; and that in the inclosed spaces insulated from the rest of the heavens by the preceding and following streams, and the 'bridges' above spoken of as connecting them (as, for instance, in that which includes λ Scorpii), we are, in fact, looking out into space through vast chimney-form or tubular vacancies whose terminations are rendered nebulous by the effect of their exceeding distance, and at the same time are brought by that of perspective to constitute the interior borders of the apparent vacuities. It is possible that the globular clusters we see scattered over it are nothing more than such masses in a higher state of aggregation, to which perhaps the others are by slow degrees advancing. Yet in that case we should certainly be prepared to expect specimens of an intermediate character to occur in considerable numbers, scattered among them, whereas, in fact, it would be difficult to particularize any objects in the region in question which can be quite fairly so considered. The intermediate stages of central condensation between the highly compressed globular cluster, and the dilute and nearly uniform nebuloid patch, if not altogether wanting, are, at all events, feebly represented."—P. 390.

In the 3rd Section mention is made of a phænomenon sometimes observed in the course of sweeping, which seems to indicate the existence of starry regions of great extent and excessive remoteness, not in traceable connexion with the Milky Way, except in some particular localities, yet possibly outlying portions of that system. It consists in "an exceedingly delicate and uniform dotting or stippling of the field of view by points of light too small to admit of any one being steadily and fixedly viewed, and too numerous for counting were it possible so to view them." Sir John states that he always felt satisfied of the reality of the phænomenon at the moment of observation; but the conviction was not permanent, the idea of illusion continually arose subsequently, partly from the extreme delicacy of the points of light, but chiefly from the circumstance, that when noticed it was *almost* invariably soon after the commencement of the sweeping. But he thought it right, notwithstanding the doubt, not to suppress all mention of the phænomenon, "which, if it arise from physiological causes, is at least curious and remarkable as a case of optical illusion." A list is given of the places in the heavens where it was remarked, and another of places where the ground of the sky was recorded as particularly black, and certainly devoid of any such stippling or nebulous appearance; and some details are added with the view of throwing further light on the subject.

The chapter closes with a reference to one of the plates, representing the course and aspect of the Milky Way from Antinous to Monoceros, delineated with as much precision as the nature of such work admits, that is, with the naked eye by faint lamp-light in the open air. The differences in some very obvious features between this and former representations of the same portion of the heavens are sufficiently remarkable.

XXXVI. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 162.]

March 10, 1848, ON the Interior Satellites of Uranus. By the Rev. W. R. Dawes.

In the Monthly Notices for January last were printed some observations of an interior satellite of Uranus, which had been made in the autumn of last year by Mr. Lassell and M. Otto Struve. The results are in several respects interesting and remarkable. The fact that one observer always saw the close satellite on the *northern* side of the planet only, while the other as uniformly observed it only on the southern side, is sufficiently curious to invite further investigation.

It is however obvious, that the observations at Starfield and at Poulkova are utterly incompatible with each other. While the latter point to an approximate period of $3^d\ 22^h\ 10^m$, the period indicated by the former is only about $2^d\ 2^h\ 43^m{\cdot}6$. The *distance* also of the satellite carefully estimated by Mr. Lassell on Nov. 6, 1847, under favourable circumstances and with great probability of considerable accuracy, was only 11″; the position-angle being estimated 349°. Now, assuming the direction of the major axis of the projected orbit to be from 10° to 190°, as determined by M. O. Struve for the satellite observed by him; and assuming also that the apparent ellipticity of the orbit does not greatly differ from that of the orbits of the bright satellites I. and II.; we find that the distance of the satellite, at its greatest elongation, would be 12″·2, on the supposition that the distance was correctly estimated at 11″ when the position-angle was 349°. But this is almost precisely the greatest elongation theoretically due to a satellite revolving about Uranus in the period indicated by Mr. Lassell's observations. We are thus led to the conclusion, that there are at least *two* satellites interior to the nearest bright one: and to avoid the confusion which might arise from applying numbers of any kind to the smaller satellites, I beg permission to denominate them for the present, *a*, *b*, *c*, &c. in the order of distance from the primary;—*a* being the satellite observed by Mr. Lassell, and *c* the satellite observed by M. O. Struve.

M. O. Struve suggests that the satellite observed by him may lose much of its light when in the northern portion of its orbit; and this may be the reason why Mr. Lassell did not see it on Nov. 6, 1847, on which favourable night *c* must have been near its greatest northern elongation. On all the other nights when *a* was observed by Mr. Lassell, *c* was very close to the planet, with one exception only, on Sept. 14, at which time *c* was near its greatest elongation southwards, and might perhaps have been seen if the night had been sufficiently good. Neither Mr. Lassell, however, nor myself then observing with him, perceived any such object in that place.

It should here be stated, that the estimated position for Sept. 14, as given in the printed table, appears to be erroneous. It is inconsistent with the diagrams independently made at the time by Mr. Lassell and myself, which, taking the *measured* position of II. as a guide, show that the position of *a* was about 80° north *preceding*;—whereas the angle as printed is 80° north *following*. It seems clear, therefore, that the angle should be 350° instead of 10°. This being rectified, and the position-angles computed for the times of observation, by reckoning back from the peculiarly valuable observation of Nov. 6 as an epoch, and assuming a period of $2^d\ 2^h\ 43^m{\cdot}6$, the estimated angle *minus* the calculated angle comes out, for Sept. 14 $= -2°$; for Sept. 27 $= -2°$; for Sept. 29 $= +1°$; for Oct. 1 $= +2°$.

That the satellite *a* should never have been seen by M. O. Struve may arise from its becoming faint in the southern portion of its orbit, as *c* probably does in the *northern*. It appears, from calculation on the assumptions before mentioned, that on every night when M. O. Struve saw *c*, with the only exception of Dec. 10, *a* was in the southern portion of its orbit; yet sufficiently distant from his observed position of *c* to render any confusion between them impossible. On Nov. 28, indeed, the position-angles of the two satellites must have been almost precisely the same: but the measured distance of *c* was 16″·85; while the distance of *a* could not have exceeded 11″ in that part of its orbit. It should be noticed, with reference to the non-observation at Poulkova of the satellite *a* on Dec. 10, that though on that night near its greatest northern elongation, it was at a smaller distance from the primary than *c* ever was when it was observed. Unless, therefore, the night were unusually fine, so faint an object might easily be overlooked.

So long ago as the autumn of 1845, Mr. Lassell occasionally saw a faint object, supposed to be a satellite, at about the same distance from the planet as *a* was observed to have been last year; and, with only one exception, it was always seen on the *northern* side of the primary, and usually in the north *preceding* quadrant, in which it was uniformly seen in 1847. On Oct. 5, 1845, at $12^h\ 26^m$ Greenwich mean time, being on a visit at Starfield, I had the gratification of seeing this satellite in the 20-foot reflector. When the light of the planet was hidden from the eye by a bar, the satellite became steadily visible; and a careful diagram being made, both by Mr. Lassell and myself independently, the position-angles deduced from them agreed within 3°, the mean being 324°·1. This is very nearly the same with the estimated position on Sept. 27, 1847; and assuming, that in the interval of 721·8896 days, 342 complete revolutions had been performed, the period comes out $2^d\ 2^h\ 39^m\ 36^s$. It seems probable, therefore, that the period of this satellite does not differ much from that quantity.

Supposing that those nights on which the satellites I. and II. were measured at Starfield in 1847 were probably favourable, though *a* was not noticed, it becomes interesting to know whether *a* were then at such a distance from the planet as might allow it to be visible in the 20-foot reflector. I have therefore computed the places of *a* for each of those nights; and the results show, that only on Oct. 16 was it in the northern portion of its orbit, and not very close to its primary. On every other occasion, therefore, it was probably either invisible in the southern portion of its orbit, or overpowered by its vicinity to the planet.

By a similar computation, it becomes evident, that on none of those nights was the satellite *c* near its greatest southern elongation; and that, therefore, it was probably invisible on the northern side of the planet, or too near it to be discerned.

It is singular, that in the whole series of observations at Starfield and Poulkova, only one night, Nov. 1, is common to them both; nd that was of so indifferent a quality at Starfield, as to render it improbable that so difficult an object as the satellite *c* could have been detected; *a* was then in the south following quadrant, and rather near the planet, so that it was not seen at either place of observation.

On the whole, therefore, it seems highly probable that the interior satellites observed by Mr. Lassell and M. O. Struve are entirely distinct; that the one becomes invisible in the southern part of its orbit, and the other in the northern; and that, during the last apparition of Uranus, it has so happened, that each satellite has been seen only by its own observer. If either of them is the same as was discovered by Sir W. Herschel, it seems most likely to be that observed at Poulkova.

The object observed at Starfield on Nov. 6, 1847, and supposed to be an intermediate satellite between I. and II., was estimated to be only 10″ distant from the planet. But its position was then almost precisely *opposite* to that of I., whose distance was estimated at 20″. It is therefore obvious, that if this were a satellite, its orbit must be *interior* to that of I. It could not however have been *c*, which must at that time have been near its greatest *northern* elongation, and therefore probably invisible. Moreover, its greatest distance could be only about $15''\frac{1}{4}$; and it would therefore be nearly intermediate between *a* and *c*, and for the present it may be distinguished as *b*. Though it may seem premature to attempt any conclusions from a single observation of so difficult an object, yet, on the other hand, the observation of Nov. 6 was peculiarly worthy of reliance. The night was unusually fine. The planet was viewed for two hours, during which time the supposed satellites were carried along with it. The distance also of *b* was estimated just half that of I., which was almost exactly opposite to it. And that the estimated distance of I. (20″) was nearly correct, appears from comparing it with the *measured* distance of the same satellite (20″·57) on Oct. 11, when it was in almost precisely the same part of its orbit. If therefore it were really a satellite, it appears probable that there are *three* satellites revolving within the orbit of I., at apparent mean distances of about 12″, 15″, and 18″.

Mr. Lassell having favoured me with a communication of all his observations of a close satellite of Uranus, with the position-angles estimated at the time, and copies of the diagrams, it appears from them, that only on one occasion has such an attendant been undoubtedly seen on the *southern* side of the planet. This occurred on Sept. 27, 1845, at $12^{h}\ 6^{m}$ Greenwich mean time. The estimated angle was 160°, the distance was three diameters of the planet from the edge of the disc, which gives 14″ for the central distance. This does not at all agree with any probable period of the satellite *a*. But on calculating the period of *c* from this observation, compared with each set obtained by M. O. Struve in 1847, employing as a guide to the number of entire revolutions the approximate period deduced by him, viz. $3^{d}\ 22^{h}\ 10^{m}$, and giving a weight to the Poulkova observations proportional to the number from which each result is derived, the period comes out from the mean of the whole,—3^{d}

$22^h\ 8^m\ 35^s$. Mr. Lassell's estimation of distance corresponds to a greatest elongation of about 17″; which, as well as the period, agrees so nearly with the result of the Poulkova observations, as to render it highly probable that the satellite observed by Mr. Lassell on Sept. 27, 1845, is the same as that observed by M. O. Struve.

Extract of Letter from M. Otto von Struve to the Astronomer Royal.

"You will see from the *Astronomische Nachrichten*, &c., that we have not been idle at Poulkova since your visit. In addition to the published accounts I have little to say, except that my father's calculations of the great Russian meridian arc give a considerably larger value to the difference between the two axes of the earth than has been hitherto found. I cannot tell you the exact quantity, as the calculations are not completed.

"I have finished my observations of the satellites of Uranus for this season. As soon as I have a little leisure, I shall deduce their motions from the observations. I have seen the third satellite, distinctly, only once since my communication to Sir John Herschel, viz. at 6 P.M. on January 25th. Its position was 202°, its distance about 18″. I am now inclined to think that the differences of the light of this satellite in different parts of its orbit are so great, that it cannot be seen by our refractor when it is in the opposite direction to that in which I have hitherto observed it. The atmosphere was very favourable on two occasions, when the satellite was supposed to be to the north of the planet, but I could not see the least trace of it. The period of its revolution is, however, somewhat uncertain, for these *negative* observations are far from conclusive.

"M. Döllen has finished his calculations on Bessel's fundamental catalogue for 1820. The result, as respects Procyon, is that the irregularity supposed by Bessel in its proper motion, vanishes altogether."

May 12, 1848.—On an easy Method of approximating to the distance of a Planet from the Sun by means of two observations only, made near the Planet's opposition. By Professor Chevallier.

If v be the linear velocity of the earth, and r the distance of the planet from the sun, the earth's distance being 1, the linear velocity of the planet $= \dfrac{v}{\sqrt{r}}$.

Also the angular retrograde velocity of the planet at or near opposition

$$= \frac{1}{r-1}\left(v - \frac{v}{\sqrt{r}}\right) = \frac{v}{\sqrt{r}(\sqrt{r}+1)}.$$

Again, let L, L′ be the heliocentric longitudes of the earth at the times of observation, and l, l' the *geocentric* longitudes of the planet, and let

$$n = \frac{L'-L}{l-l'} = \frac{\text{heliocentric velocity of the earth}}{\text{geocentric velocity of the planet}},$$

since the heliocentric velocity of the earth $=v$, we have

$$n=\sqrt{r}(\sqrt{r}+1) \text{ and } r=\tfrac{1}{2}(1+2n-\sqrt{1+4n}).$$

Using this formula, Mr. Chevallier deduces from the Durham observations, May 6 and 10, a mean radius vector which agrees very nearly with that computed by Mr. Graham.

On a formula for reducing Observations in Azimuth of Circumpolar Stars near Elongation, to the Azimuth at the greatest Elongation. By Captain Shortrede.

In trigonometrical surveys the direction of a chain of triangles is often deduced from the observed azimuth of a circumpolar star at its greatest elongation. The method is convenient, as an accurate knowledge of the time is not requisite, and the latitude is generally sufficiently well-known. It is indeed necessary to have the polar distance of the star with the utmost precision. In the northern hemisphere Polaris and δ Ursæ Minoris are usually employed.

When the time is known with tolerable certainty, it is much more satisfactory to observe the star frequently, both before and after its greatest elongation; and Captain Shortrede in this memoir gives a demonstration of the formula which he has found most convenient for reducing such a series of observations of azimuth to the azimuth at the greatest elongation.

He deduces an *exact* expression for the tangent of the difference of each azimuth from the greatest azimuth, in which the only variable quantities are the time from the greatest elongation, and the arc joining the position of the star at its greatest elongation with its position at the time of observation. The formula is easy of computation; and when many observations have been made, would greatly facilitate their reduction.

Captain Shortrede shows, that in most cases an approximate computation of the principal variable, viz. of the secant of the arc abovementioned, would be sufficient, and that this may be very readily effected by a formula involving $\log \frac{\text{ver. sin}}{\sin 1''}$. A table of this function for all arcs up to 1^{h} is added to the memoir.

On a Regulated Time-ball. By Professor Chevallier.

"The usual method of indicating the time by a ball is by permitting the ball to fall freely, the motion being a little accelerated at first by a spring. It is evident that this method is subject to some uncertainty as to the particular instant of time which is to be observed. There is also some inconvenience arising from the derangement to which the apparatus is liable by the sudden stoppage of the motion of the ponderous ball.

"It is proposed to remedy these disadvantages by regulating the descent of the ball, so that its motion may be uniform, and causing it to pass through three or five horizontal hoops. The motion may be so regulated that the ball may pass through the distance between one hoop and another in a determinate interval, as about 20^{s}; and the mean of the times at which the ball is observed to pass the successive hoops may be taken as in the observation of the transit of a star.

"If the ball is spherical, the time of its bisection by the hoops may be noticed.

"The observer is supposed to be at some distance from the apparatus, so that his eye may not be very far distant from the plane of any of the hoops."

Colonel Batty sent for inspection a curious dial, of the workmanship of Nicholas Kratzer, horologier to King Henry VIII., and the friend of Holbein.

This consists of a block cut into various faces and hollows, each serving for a dial, mounted on a foot, and furnished with a plumb-line for rectification. There is a hollow on the top, which it is conjectured was meant to receive a compass. The date is 1542, and the arrangement and ornamental work very elegant.

Colonel Batty has supplied the following biographical notice of Kratzer, extracted from Bliss's edition of Wood's *Athenæ Oxonienses*:

"Nicholas Kratz was born at Munich, and educated at the universities of Cologne and Wittenburg. He came to England with the degree of B.A., was made Fellow of Corpus Christi, Oxford, by Bishop Fox, in 1517, gave lectures in astronomy in that university on the king's order, and was appointed mathematical reader by Cardinal Wolsey.

"He seems to have written several mathematical treatises, which are in still in MSS., chiefly in the Bodleian Library. One of these, which is reputed to be his, is *De Compositione Horologiorum.*

"He made the old dial which is now in the garden of Corpus, and another standing on a pillar in the churchyard of St. Mary's. He was living in the year 1550, and after his death many of his books came into the hands of Dr. John Dee, and some into those of Dr. Richard Forster."

Lunar Eclipse of March 19, 1848.

Extract of a letter from the Rev. Charles Mayne, Killaloe.

"The eclipse was observed with an excellent thirty-inch telescope of two inches aperture, fixed pretty firmly to the window-sash. Nothing particular was noted at first. The moon was seen well at intervals between clouds for an hour and a half, and then was completely covered. Some considerable time after, one of the family going to the window exclaimed, 'The eclipse is over!' I went to the window and saw the *whole* of the moon, the colour much like that of tarnished copper, *i. e.* of a dullish red, some parts being darker than others. After looking at it for some time, I perceived with great surprise that the eclipsed part was marked, but (from the general effect produced on the moon) only indistinctly. Clouds soon after covered the whole sky, and the moon was not again visible till about a quarter of an hour before the end, when the appearance was as usual, the eclipsed part nearly black and the rest perfectly bright. I am told that aurora was visible the same night."

XXXVII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOSITION OF THE PHOSPHATES OF URANIUM. BY M. WERTHER.

THE author obtained two phosphates of uranium in a state of purity. When oxide of uranium is diffused in a small quantity of phosphoric acid, a saline mass of a bright yellow is formed, part of which is dissolved by ebullition, and part remains insoluble. The yellow solution, when sufficiently concentrated, deposits after long exposure over sulphuric acid, a lemon-coloured salt in irregular crystals, collected in groups. These crystals gave the following results by analysis:—at 212° F. they lost 7·5 per cent.; at 248°, 10·2 per cent.; after long exposure to 320°, they lost 13·11; and by calcination 17·9 per cent. of water. Another portion lost by calcination 16·8 per cent. of water, gave 26·7 of phosphoric acid, and 55·96 of oxide of uranium; a third portion lost 12·1 per cent. of water at 230°, 17·2 by calcination, and gave 54·6 per cent. of oxide of uranium; lastly, a fourth portion gave 27·49 of phosphoric acid.

The mode of estimating adopted by the author was the following:—He fused some potassio-tartrate of soda in a platina crucible, and carbonized it with a sufficient degree of heat, out of contact of the air, and added to it a noted weight of phosphate of uranium. The heat must not be continued longer than is requisite to fuse the black mass: by this all the sesquioxide is reduced to protoxide, and from it water will dissolve all the phosphate of soda, and the excess of carbonate, unmixed with even a trace of uranium, the protoxide of which remains on the filter, mixed with charcoal.

M. Werther recommends two precautions in this operation to avoid decomposing the phosphoric acid, and consequently to prevent the perforation of the crucible: the phosphate of uranium must not be placed at the bottom of the crucible, and then merely introduce the potassio-tartrate of soda; again, the heat must not be raised above the degree of redness requisite to effect the decomposition.

Sometimes small flames appear during the fusion; they are occasioned merely by oxide of carbon. The filtered portion containing the phosphoric acid is saturated by hydrochloric acid, mixed with excess of ammonia and precipitated by sulphate of magnesia. As to the protoxide remaining on the filter, it is to be dissolved in nitric acid, precipitated by ammonia, and the precipitate is to be calcined.

The preceding results lead to the formula $[P^2O^5, U^2O^3+5H^2O]$; or reckoning $2H^2O$ as constituting an integrant part of the salt $[P^2O^5, U^2O^2O, 2H^2O+3\text{ aq}]$. It is, however, correct to state that all the water is expelled between 338° and 356° F.

This phosphate of uranium is decomposed by water into phosphoric acid, holding a little oxide of uranium in solution, and an insoluble subphosphate of uranium. If the latter be dissolved in phosphoric acid and precipitated by ammonia, a yellow salt is deposited which retains ammonia after repeated washings.

The phosphate of uranium, corresponding to common phosphate of soda, is obtained by different processes; either by treating oxide of uranium by phosphoric acid, or acetate of uranium with this acid; or lastly, by precipitating nitrate of uranium by diphosphate of soda. The salts obtained by these different processes do not, however, contain the same quantity of water. They are of a bright yellow colour, and usually crystalline, at least when seen by the microscope; they are insoluble in water and acetic acid, but soluble in the mineral acids and excess of carbonate of ammonia.

a. The salt obtained by treating oxide of uranium with dilute phosphoric acid and submitted to washing, is not crystalline, becomes deeper coloured by calcination, and on cooling reassumes its bright colour. Analysis gave,—oxide of uranium, 72·77 to 72·16; phosphoric acid, 18·11 to 17·96; water between 248° and 356° F., 7·03; and by calcination, 9·12 to 9·7 per cent. The formula deduced is $[P^2O^5, 2U^2O^2O, H^2O+3\,\text{aq}]$. The calcined salt, moistened with water and dried over sulphuric acid, regains 7 per cent. of water, which may be expelled at 347° F.

b. The salt obtained by adding phosphoric acid to a solution of acetate of uranium, as long as a precipitate is formed, constitutes, after washing, a crystalline powder, a little deeper than the foregoing salt. Dried at 140° F. it yielded,—oxide of uranium, 67·93; water at 248°, 12·9 per cent.; and from 500° to calcination 15·74 to 15·2. The calcined salt gave 80·1 of oxide of uranium and 19·9 of phosphoric acid. In another experiment, in which the salt had been dried merely by exposure to the air, by heating to 248° F. it lost 2·19 per cent. of water, the same quantity at 320°, and by calcination such a further quantity as to give 18·46 as the whole quantity of water; the same salt gave 64·78 of oxide of uranium. From these numbers M. Werther deduces the formula

$$[P^2O^5, 2U^2O^2O, H^2O+8\,\text{aq}].$$

c. The salt obtained by the mixture of diphosphate of soda and nitrate of uranium has the same appearance as the foregoing; it lost 12 91 per cent. of water at 248° F., and by calcination 15·08; it gave 67·97 per cent. of oxide of uranium, and 16·51 of phosphoric acid. M. Werther obtained the same salt by precipitating incompletely the trisphosphate of soda by nitrate of uranium, separating the precipitate by a filter, and adding excess of nitrate of uranium to the filtered liquid.

When to nitrate of uranium an excess of trisphosphate of soda is added, a deep yellow salt is precipitated, which redissolves almost entirely on a further addition of the phosphate. If this excess be avoided, and the precipitate be perfectly washed, an agglomerated powder of a deep yellow colour is obtained, which, like all the other phosphates, is insoluble in water, but which is decomposed by acetic acid. This salt contains more uranium than the preceding; the author was, however, unable to obtain it of a composition exactly corresponding to a trismetallic phosphate.—*Journ. de Ph. et de Ch.*, Juillet 1848.

ON THE COMPOSITION OF THE ARSENIATES OF URANIUM.

BY M. WERTHER.

The author observes that arsenic acid acts like phosphoric acid on the oxide of uranium. It is easy to obtain salts with one or two equivalents of metal; as to trisalt, M. Werther cannot positively affirm that it exists.

To analyse these arseniates, the author dissolved them in hydrochloric acid, then boiled with sulphurous acid, and precipitated the arsenic by sulphuretted hydrogen; the filtered solution was afterwards precipitated by ammonia, as in other analyses. He always obtained more exact results by estimating the arsenic acid by difference.

The arseniate with one equivalent of uranium is obtained by evaporating the oxide of the nitrate or acetate of uranium with an excess of arsenic acid, and allowing it to remain over sulphuric acid. The salt then appears in small crystals grouped together, but which are not measurable, It is soluble in mineral acids and carbonate of ammonia, insoluble in acetic acid and in water. When strongly calcined it yields anhydrous arsenic acid, oxygen and a subsalt, the composition of which has not been determined.

If the solution of the salt in arsenic acid be treated with ammonia, a bright yellow precipitate is obtained which contains ammonia.

This salt yielded by analysis,—oxide of uranium, 48·17; water, 13·7. Heated to 302° F. the salt lost 9·2 to 10·8 per cent. of water, which agrees with the formula $[As^2 O^5, 2H^2 O, U^2 O^2 O, + 3\,aq]$.

The diarseniate of uranium, like the corresponding phosphate, may be obtained by different processes.

a. By adding arsenic acid to acetate of uranium, washing the pale yellow precipitate, and drying it over sulphuric acid. The product is insoluble in water and in acetic acid. Analysis:—oxide of uranium, 59·05; water, 17·2; it loses at 248° F. 15·07 per cent. of water. These results agree with the formula

$$[As^2 O^5, H^2 O, 2U^2 O^2 O + 8\,aq].$$

b. When nitrate of uranium is boiled with arsenic acid till a good part of the nitric acid is expelled, on water being added to the liquid, a yellow powder separates, which, when completely washed and analysed, gives the same composition as the preceding.

c. If a solution of biarseniate of potash be added to one of nitrate of uranium, a pale yellow crystalline powder separates. This salt always contains potash, but not as much as an equivalent. Analysis:—water by calcination, 11·3; at 266°, 10·5; oxide of uranium, 60·85; potash, 2·7. The calcined salt contains 67·84 per cent. of oxide of uranium, and 3·6 per cent. of potash.

If an excess of trisarseniate of soda be added to a solution of nitrate of uranium, a pale yellow powder separates, and the liquid contains no uranium. The precipitate appears viscid, and passes through the filter during washing. It must therefore be washed with a solution of sal-ammoniac, and then with weak alcohol.

This salt contains—soda, 5·91; oxide of uranium, 60·21 to 61·13; water, 9·91; that is to say $[As^2 O^5, Na^2O, 2U^2 O^2 O + 5\,aq]$. It is therefore a sodio-arseniate of uranium.—*Journ. de Ph. et de Ch.*, Juillet 1848.

ON THE MAGIC SQUARE OF THE KNIGHT'S MARCH.

In Mr. Beverley's Chess-Knight's March (Phil. Mag. Aug. 1848), Mr. Beverley might have added that the squares whose diagonals are 31. 3. 15. 61, 25. 41. 39. 23, and the other halves 1. 51. 17. 13, 5. 53. 59. 11, are likewise magic squares, whose sides form the sum 130. Imagine a prism on a square base, having the rows 1, 2 : 3, 4 : 5, 6 : 7, 8 successively written on the sides; then any two consecutive sides present a double square, each of which is the magic square of 130. Further, $65=64+1$, $130=2(64+1)$, $260=4(64+1)$.

S. M. Drach.

August 10, 1848.

METEOROLOGICAL OBSERVATIONS FOR JULY 1848.

Chiswick.—July 1. Clear: cloudy and cold: clear. 2. Fine: overcast. 3. Drizzly: overcast. 4. Overcast. 5, 6. Very fine. 7. Sultry: cloudy. 8. Fine: heavy showers. 9. Constant rain. 10—13. Very fine. 14. Very fine: thunder, lightning and rain at night. 15. Cloudy and fine: clear at night. 16—18. Very fine. 19. Cloudy. 20. Cloudy and boisterous. 21. Clear: cloudy: rain. 22. Clear: cloudy. 23. Cloudy: fine: rain. 24. Cloudy and fine. 25. Rain. 26. Overcast: boisterous, with slight rain. 27. Cloudy. 28, 29. Very fine. 30. Overcast: rain. 31. Rain: overcast.

Mean temperature of the month	62°·9
Mean temperature of July 1847	65 ·84
Mean temperature of July for the last twenty years	63 ·14
Average amount of rain in July	2·36 inches.

Boston.—July 1. Rain: rain A.M. and P.M. 2. Fine: rain P.M. 3. Fine. 4. Cloudy. 5. Fine. 6. Fine: thermometer 84° 2 P.M. 7. Cloudy. 8. Windy. 9. Rain. 10. Cloudy. 11. Fine. 12. Cloudy. 13. Fine. 14. Fine: rain and thunder P.M. 15. Cloudy. 16—18. Fine. 19. Cloudy. 20. Cloudy: rain, with thunder and lightning P.M. 21. Fine: rain P.M. 22. Fine. 23, 24. Cloudy: rain P.M. 25. Cloudy: rain A.M. 26. Rain: rain A.M. and P.M. 27. Cloudy. 28. Fine. 29, 30. Cloudy. 31. Cloudy: rain A.M.

Applegarth Manse, Dumfries-shire.—July 1. Dull A.M.: cleared and fine. 2. Heavy showers. 3, 4. Fair and fine, though sharp. 5. Cloudy P.M., but fair. 6. Heavy rain A.M.: cleared. 7. Heavy shower. 8. Very heavy showers. 9. Rain A.M.: cleared P.M. 10. Fine, but cloudy. 11. Fine: warm. 12. Very fine. 13. Very sultry: thunder. 14. Fine soft shower P.M. 15. Fine: thunder. 16. Fine bracing air. 17. Fine, but cool. 18. Drizzling day. 19. Rain all day. 20. Frequent showers. 21. Bright and dry: shower P.M. 22. Wet nearly all day. 23. Occasional showers. 24. Fine: slight shower. 25. Wet. 26. Fine throughout. 27. Heavy shower: thunder. 28. Showery: fine P.M. 29. Fair and fine. 30. Rain all day. 31. Fine, though cloudy.

Mean temperature of the month	56°·5
Mean temperature of July 1847	61 ·5
Mean temperature of July for the last twenty-five years	58 ·1
Average amount of rain in July for twenty years	3·91 inches.

Sandwick Manse, Orkney.—July 1. Bright: drops. 2. Bright: clear. 3. Clear. 4. Cloudy: damp. 5. Bright: cloudy. 6. Rain: cloudy. 7. Bright: thunder and rain. 8. Bright: fog. 9. Fog. 10. Cloudy: clear. 11. Bright: rain. 12. Bright: damp. 13. Cloudy. 14. Cloudy: clear. 15. Bright: drizzle. 16, 17. Damp: drizzle. 18. Showers: cloudy. 19. Rain: damp. 20. Damp: rain. 21. Bright: drops. 22. Bright: rain. 23. Showers: clear. 24. Bright: clear: showers. 25. Bright: heavy showers. 26. Bright: drops. 27. Rain: damp. 28. Clear. 29. Clear: fine. 30. Showers: rain. 31. Clear: cloudy.

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.
1848. July.	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.	29·783	29·530	29·07	29·54	29·60	29·58	29·63	65	40	53	58½	46	47½	46	nw.	w.	w.	wnw.	·04	·12	......	·06
2.	29·876	29·838	29·36	29·60	29·59	29·63	29·59	68	50	62	57½	38½	50	50	w.	calm	w.	ese.	·07	·33	......	......
3.	29·813	29·728	29·32	29·57	29·69	29·61	29·68	67	52	61	61	40½	52½	53	sw.	calm	w.	w.	·29	·01	......	......
4.	30·137	29·940	29·50	29·80	30·04	29·75	29·97	73	48	59·5	60	41	55	52	e.	w.	w.	nw.	......	......	......	......
5.	30·186	30·121	29·71	30·08	30·02	30·08	30·06	82	50	66	66	38	54	52	s.	calm	s.	ese.	......	......	......	·02
6.	30·085	29·969	29·56	29·94	29·99	29·89	29·93	88	59	73·5	60	54	55	59	s.	calm	sw.	s.	......	......	......	·28
7.	29·876	29·849	29·23	29·69	29·58	29·73	29·58	74	51	74	70	55	55½	58	sw.	s.	se.–s.	se.	·03	......	......	......
☽ 8.	30·063	29·983	29·35	29·60	29·72	29·43	29·51	70	51	63	60	52	63	54	sw.	wsw.	sw-w.	w.	·22	......	0·72	·22
9.	29·989	29·742	29·34	29·68	29·68	29·61	29·77	68	52	56	60	50	53	53½	sw.	s.	w–nw	ne.	·36	·24	......	......
10.	30·283	30·003	29·54	29·98	30·18	30·11	30·24	72	47	61	67	46½	54½	51	n.	nw.	nw-n.	e.	......	·65	......	......
11.	30·417	30·380	29·90	30·29	30·33	30·26	30·22	72	48	63	68½	47½	62	55	n.	nw.	nw.	w.	......	......	......	......
12.	30·448	30·439	29·93	30·38	30·39	30·30	30·35	81	48	65	73	54½	57	55	e.	calm	sw.	w.	......	......	......	·31
13.	30·423	30·358	29·90	30·37	30·31	30·34	30·32	82	52	72·5	76	53½	55	53	ne.	e.	sw.	w.	......	......	......	·04
14.	30·310	30·198	29·70	30·28	30·24	30·28	30·30	87	60	75	72½	56	53½	54	e.	calm	ne-e.	wnw.	·04	......	......	......
15.	30·260	30·225	29·70	30·24	30·20	30·20	30·09	71	41	65	68½	54½	61	52½	ne.	ne.	w.	w.	......	·05	......	......
○16.	30·296	30·230	29·66	30·19	30·15	30·10	30·06	78	49	69	71½	50	54½	51	ne.	calm	n.	nw.	......	......	0·47	......
17.	30·171	30·122	29·60	30·09	30·07	30·02	29·92	76	47	67	64½	53	53½	52	w.	calm	nw.	wnw.	......	......	......	......
18.	30·088	29·913	29·50	29·92	29·72	29·77	29·64	80	51	65	61	52½	53	50	w.	calm	sw.	calm	......	......	......	·22
19.	29·776	29·499	29·19	29·37	29·10	29·21	29·07	74	59	59	61	53½	51	51	sw.	w.	sw.	ne.	......	......	......	·32
20.	29·512	29·299	28·83	28·98	29·16	28·98	28·95	72	44	66·5	60½	54	58	49	sw.	w.	s–nw.	w.	·07	......	......	·15
21.	29·758	29·677	29·23	29·38	29·44	29·11	29·20	71	51	63	60½	45½	53½	51	sw.	w.	s–sw.	sw.	·21	·20	......	·29
22.	29·836	29·812	29·25	29·49	29·44	29·43	29·44	73	65	67	61	49½	57½	53½	sw.	w.	sw.	se.	......	·34	......	·05
☾23.	29·854	29·819	29·26	29·58	29·62	29·29	29·53	76	53	69	63	49½	55	52	sw.	w.	wsw.	s.	·38	......	1·78	·23
24.	29·903	29·863	29·30	29·65	29·60	29·57	29·54	73	51	64	61½	45½	58	53	sw.	calm	s-ssw.	calm	·08	·06	......	......
25.	29·794	29·753	29·20	29·44	29·45	29·37	29·33	68	53	61·5	61½	54	58	51	sw.	w.	ssw.	calm	·11	·11	......	·10
26.	29·720	29·695	29·16	29·50	29·48	29·31	29·36	67	51	62·5	65	51½	56	54	sw.	w.	sw.	calm	·08	·21	......	·42
27.	29·950	29·823	29·25	29·45	29·66	29·22	29·49	70	53	63	60½	52½	53	52	sw.	w.	sw.	w.	·01	·21	......	·40
28.	30·084	30·006	29·50	29·82	29·95	29·78	29·93	73	56	63·5	64	49½	53	51	w.	calm	w.	w.	......	......	......	·02
29.	29·096	29·005	29·60	30·00	29·92	30·04	29·99	78	48	62	68	42½	51	48½	sw.	w.	w.	ese.	......	......	......	......
●30.	29·927	29·654	29·40	29·70	29·40	29·66	29·48	71	58	65	68	55½	58	55	sw.	calm	sw.	se.	·18	......	2·00	......
31.	29·512	29·360	29·40	29·38	29·29	29·37	29·30	72	51	64	61½	50	55	54	sw.	calm	sw.	se.	·04	·36	......	·24
Mean.	29·974	29·865	29·43	29·773	29·776	29·710	29·724	73·93	50·25	64·6	61·3	49·5	55·05	52·45					2·21	2·89	4·97	3·37

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XXXVIII. *Some Remarks on the Substances discovered by* Mr. Stenhouse *in the* Roccella tinctoria *and* Evernia Prunastri.

By EDWARD SCHUNCK*.

IN a paper read some months ago before the Royal Society, Mr. Stenhouse has communicated the results of an elaborate investigation of the proximate principles of some of the lichens, especially the *Roccella tinctoria* and the *Evernia Prunastri.* The multitude of new observations recorded by Mr. Stenhouse in his valuable paper, and the accuracy with which he has examined the composition of the various substances obtained by him, would seem at length to confer on this part of organic chemistry the degree of completeness required in the present state of the science. Nevertheless, on comparing his results with those of his predecessors on this field of inquiry, it may be asked how it is possible for so many persons to examine the same subject and arrive at such widely different results; and some doubt may arise in the mind either as to the skill of some of the investigators, or as to the accuracy of the methods at present employed in organic chemistry. The *Roccella tinctoria* has now been examined four times, and the discrepancies between the several investigations are very striking. Heeren discovered in it a substance which he called *erythrine.* Kane obtained from it a substance differing in its properties from Heeren's erythrine, and which he named *erythryline.* In my examination of the plant I found a body corresponding to Heeren's erythrine and Kane's erythryline, but differing in properties from both, and in composition from the latter. To this substance I gave the name of *erythric acid.* Lastly, Mr. Stenhouse has obtained from different varieties of the *Roccella tinctoria* no less than three different substances, similar to, but not identical with, those discovered by Heeren, Kane and myself. If, therefore, we are to believe all the

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* Communicated by the Author.

statements of those who have examined the subject, the same plant, according to its varieties and the places of its growth, produces sometimes one, sometimes another, of six different bodies, each of which may entirely supply the place of any one of the others. This circumstance is contrary to analogy; since it is more common to find plants differing totally from one another produce the same substance, than to find the same plant in different circumstances secrete different substances. From the *Evernia Prunastri*, again, Messrs. Rochleder and Heldt obtained lecanoric acid, while to Mr. Stenhouse the same plant afforded evernic acid. There can of course be no doubt that the *Roccella tinctoria* does in reality produce substances very different from one another according to the place in which it grows. I shall, however, endeavour to show in the following pages that it is possible, by comparing these substances, to find a link by which to bind them together and bring them into harmony with one another.

Now on comparing together the various colour-giving substances found at different times in the *Roccella tinctoria*, we shall find that all or several of them have certain properties in common. They all give red colouring matters when dissolved in ammonia and exposed to the air. In the next place, erythrine, erythryline and erythric acid, all produce, under certain circumstances, a bitter substance; but these circumstances are not the same with all three, and the bitter substance itself is also different in each case. Erythric acid and the two orsellic acids of Mr. Stenhouse are decomposed by alkalies into carbonic acid and orcine, accompanied in the case of erythric acid, according to Mr. Stenhouse, by another body, viz. pseudo-orcine. Lastly, all these substances agree with one another and with lecanoric acid, in giving, when treated with boiling alcohol, crystallized bodies soluble in boiling water, which have been proved to be æthers.

It is to the latter circumstance that I wish in the first place to draw attention. Lecanoric acid, erythrine, erythryline, erythric acid, alpha-orsellic and beta-orsellic acid, all give, when treated with boiling alcohol, solid crystallizable æthers. The æther produced in this way from erythrine was called by Heeren, who was ignorant of its true nature, *pseudo-erythrine*. Let us now compare together the composition of these various æthers, as determined by Kane, Rochleder and Heldt, Stenhouse and myself.

The æther from erythrine consists, according to Kane, of—

	I.	II.
Carbon . .	60·33	60·43
Hydrogen .	6·20	6·31
Oxygen . .	33·47	33·26

Lecanoric æther:—

	From *Lecanora Parella*. S.			From *Evernia Prunastri*.
	I.	II.	III.	Rochleder and Heldt.
Carbon . .	60·74	60·73	60·57	61·14
Hydrogen .	6·25	6·13	6·39	6·38
Oxygen . .	33·01	33·14	33·04	32·48

Æther from erythric acid:—

	S.			Stenhouse.	
	I.	II.	III.	I.	II.
Carbon . .	60·65	60·72	60·57	60·65	60·74
Hydrogen .	6·13	6·14	5·97	6·33	6·31
Oxygen . .	33·22	33·14	33·46	33·02	32·95

Æther from alpha-orsellic acid (orsellesic æther of Stenhouse):—

	I.	II.
Carbon . .	61·24	61·13
Hydrogen .	6·26	6·15
Oxygen .	32·50	32·72

Æther from beta-orsellic acid (Stenhouse):—

	I.	II.	III.
Carbon . .	60·82	60·75	60·83
Hydrogen .	6·27	6·15	6·27
Oxygen . .	32·91	33·10	33·00

On comparing the preceding analyses with one another, any one conversant with organic chemistry will immediately perceive that the differences are not greater than those usually found in analysing the same substance from different sources. The properties of these æthers are also identical as far as they are known. I found lecanoric æther to have exactly the same properties as those ascribed to pseudo-erythrine by Heeren and Kane. I was also unable to discover any characteristic difference between lecanoric æther and the æther from erythric acid. Mr. Stenhouse says of orsellesic æther, "This æther cannot be distinguished in its external properties from the lecanoric and erythric æthers;" and the æther from beta-orsellic acid, according to him, "cannot be distinguished in appearance from the orsellesic, lecanoric or erythric æthers, with which bodies in its properties and reactions it very closely corresponds." I therefore maintain that all these æthers are nothing more nor less than lecanoric æther. Now lecanoric æther consists of $C_{18} H_8 O_8 + C_4 H_5 O = C_{22} H_{13} O_9$, and its calculated composition is as follows:—

22 eqs.	Carbon . .	1650	60·83
13 ...	Hydrogen .	162·2	5·98
9 ...	Oxygen . .	900	33·19
		2712·2	100·00

It will be seen that the numbers of almost all of the above analyses closely correspond with those of the calculated composition of lecanoric æther. There is therefore at present nothing to oppose the hypothesis that pseudo-erythrine, erythric æther, and the orsellic æthers, are all identical with lecanoric æther*.

If this conclusion be accepted, it follows necessarily that erythrine, erythryline, erythric acid, and the two orsellic acids, must contain lecanoric acid; and not only the elements of that acid, but the acid as such. For if by acting on a certain substance with boiling alcohol we obtain the æther of a certain acid, it follows that this acid must have been contained in the original substance as such; since it would be contrary to all analogy to suppose that boiling alcohol is capable of exerting such a powerful influence on any substance as to make the elements of the latter arrange themselves in such a way as to form an acid, with which the basis of the alcohol might combine to produce an æther, the affinity of oxide of ethyle for organic acids being in general so weak as to render the intervention of a strong acid necessary to cause the decomposition of the alcohol before the combination can take place.

Having now obtained a basis for comparing the various colour-giving substances of the *Roccella tinctoria*, I shall in the first place proceed to the consideration of erythric acid.

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* I have supposed that the formula given above for lecanoric acid, viz. $C_{18} H_8 O_8$, is firmly established, and I am not aware that any experiments have been made tending to establish a better. Mr. Stenhouse, however, in a recent paper on Alpha-orcine and Beta-orcine (Phil. Mag., vol. xxxiii. p. 1), has given a new formula for orcine, called by him alpha-orcine, viz. $C_{21} H_{15} O_9$. Dry orcine, according to him, is $C_{21} H_{12} O_6$. This change he has made apparently in order to obtain a formula corresponding with the analyses of dry orcine; since the composition of crystallized orcine, as determined by him, agrees much better with the old formula $C_{16} H_{11} O_7$ than with the new one. I have myself never been able to obtain dry orcine of uniform composition; and even if obtained of uniform composition, as stated by Mr. Stenhouse, it may be questioned whether it is not a mixture of various hydrates in some proportion determined by the temperature and the length of time occupied in drying. At all events, if we change the formula for orcine, the formula for lecanoric acid must also necessarily be changed, since lecanoric acid minus carbonic acid plus water is orcine. It will, however, be found impossible to find a formula which will express the composition of lecanoric acid, and will at the same time be in accordance with Mr. Stenhouse's new formula for orcine. I therefore prefer retaining the old formulæ for orcine and lecanoric acid, the more so as these also harmonise with those of lecanoric æther and orceine.

The first point to be determined is, whether the substance to which I gave the name of erythric acid is the same as the erythric acid of Mr. Stenhouse. Both give when treated with boiling water picro-erythrine. Mr. Stenhouse, however, states that picro-erythrine, when decomposed with alkalies, gives carbonic acid, and a new substance called by him *pseudo-orcine*, while I was of opinion that it is decomposed into carbonic acid and orcine. Being still in possession of a sufficient quantity of picro-erythrine, I treated some of it with boiling caustic baryta, and after filtering, precipitating the excess of baryta by a stream of carbonic acid gas and evaporating, I obtained a mass of crystals, which, after being treated with æther and crystallized over again, possessed the properties and composition ascribed by Mr. Stenhouse to pseudo-orcine. The æther on evaporation gave a quantity of orcine. This property of picro-erythrine, the discovery of which is due to Mr. Stenhouse, is so characteristic as to prove at once that the erythric acid examined by him is identical with that described by me. As Mr. Stenhouse seems to have obtained the acid in a state of much greater purity than I did, I shall lay no stress on the differences found by us in its composition, but shall at once accept the numbers given by him as expressing the true composition.

Mr. Stenhouse found erythric acid to consist in 100 parts of—

	I.	II.	III.
Carbon . .	56·85	56·94	57·14
Hydrogen .	5·56	5·33	5·63
Oxygen . .	37·59	37·73	37·23

The formula given by him is $C_{20}\,H_{11}\,O_{10}$.

Now I propose to change this formula into $C_{42}\,H_{23}\,O_{21}$. The calculated composition will then be as follows:—

42 eqs. Carbon . .	3150	56·89
23 ... Hydrogen .	287	5·18
21 ... Oxygen . .	2100	37·93
	5537	100·00

It will be seen that Mr. Stenhouse's numbers agree sufficiently well with this formula; and I shall now show how, by means of it, we may explain the various changes which the acid undergoes.

If from 1 eq. erythric acid . . $= C_{42}\,H_{23}\,O_{21}$
we subtract 1 eq. lecanoric acid $= C_{18}\,H_{8}\,O_{8}$
$C_{24}\,H_{15}\,O_{13}$
and add 1 eq. water . . . . . $=$ H O
we obtain . . . . $C_{24}\,H_{16}\,O_{14}$

On calculating the formula $C_{24}H_{16}O_{14}$ for 100 parts, the following results are obtained:—

24 eqs.	Carbon . .	1800	52·94
16 ...	Hydrogen .	199·6	5·87
14 ...	Oxygen . .	1400	41·19
		3399·6	100·00

Picro-erythrine contains in 100 parts—

	Stenhouse.		
	I.	II.	S.
Carbon . .	53·07	53·23	52·86
Hydrogen .	6·08	5·95	6·22
Oxygen . .	40·85	40·82	40·92

The formula $C_{24}H_{16}O_{14}$ therefore expresses the composition of picro-erythrine. The formation of pseudo-orcine still remains to be explained in harmony with the preceding formulæ.

If from 1 eq. picro-erythrine . . $=C_{24}H_{16}O_{14}$
we subtract 2 eqs. carbonic acid $=C_{2}\quad O_{4}$
$C_{22}H_{16}O_{10}$
and add 12 eqs. water . . . . . $=H_{12}O_{12}$
we obtain . . . . $C_{22}H_{28}O_{22}$

The formula $C_{22}H_{28}O_{22}$, calculated for 100 parts, gives—

22 eqs.	Carbon . .	1650	39·29
28 ...	Hydrogen .	349·4	8·32
22 ...	Oxygen . .	2200	52·39
		4199·4	100·00

On subjecting pseudo-orcine, prepared according to Mr. Stenhouse's method, to analysis, I obtained the following results:—

I. 0·5695 grm. substance gave 0·8270 carbonic acid and 0·4355 water.

II. 0·4130 grm. gave 0·6020 carbonic acid and 0·3240 water.

It therefore consists in 100 parts of—

			Stenhouse.		
	I.	II.	I.	II.	III.
Carbon . .	39·60	39·75	39·46	39·42	39·36
Hydrogen .	8·49	8·71	8·55	8·30	8·60
Oxygen .	51·91	51·54	51·99	52·28	52·04

It will be seen that the formulæ here adopted for erythric acid, picro-erythrine and pseudo-orcine, agree very well with the composition of these substances as determined by Mr. Stenhouse; while the formulæ given by that gentleman for picro-erythrine, $C_{34} H_{23} O_{20}$, and pseudo-orcine, $C_{10} H_{13} O_{10}$, are not reconcilable with one another, nor with that given by him for erythric acid, viz. $C_{20} H_{11} O_{10}$. By adopting the above formulæ, the changes which erythric acid undergoes admit of an easy explanation. According to my view, erythric acid may be considered as a compound of lecanoric acid and picro-erythrine. It is a coupled acid, in which the saturating power of the simple acid is not destroyed. When erythric acid is treated with boiling water, the lecanoric acid is decomposed into orcine and carbonic acid, so that on evaporating the solution, a mixture of picro-erythrine and orcine is obtained. When erythric acid is treated with boiling alcohol, the lecanoric acid contained in it is changed into lecanoric æther*, and the liquid contains picro-erythrine. When picro-erythrine is treated with alkalies, it loses carbonic acid, and taking up several equivalents of water, is changed into pseudo-orcine†. When, however, erythric acid is acted on by alkalies, the lecanoric acid contained in it is changed into orcine and carbonic acid, and the picro-erythrine is changed into pseudo-orcine and carbonic acid, so that a mixture of orcine and pseudo-orcine is obtained.

As the names picro-erythrine and pseudo-orcine have a barbarous sound, and the latter is also inappropriate, I propose to change the former into *Erypicrine* and the latter into *Eryglucine*. Erythric acid might therefore be called erypicrine-lecanoric acid.

Mr. Stenhouse has discovered in a South American variety of the *Roccella tinctoria* an acid, which he calls *alpha-orsellic acid*; and in a Cape of Good Hope variety of the same plant, a very similar acid, called by him *beta-orsellic acid*. He has

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* It follows that Mr. Stenhouse's erythric-methylic æther is, in fact, lecanoric-methylic æther. Its formula must therefore be $C_{18} H_8 O_8 + C_2 H_3 O = C_{20} H_{11} O_9$. Its composition would then be as follows:—

			Stenhouse.	
			I.	II.
20 eqs. Carbon .	1500	59·12	59·30	59·36
11 ... Hydrogen	137·2	5·40	5·52	5·69
9 ... Oxygen .	900	35·48	35·18	34·95
	2537·2	100·00	100·00	100·00

† I am inclined to think that the orcine, which is found in small quantity accompanying the pseudo-orcine derived from the decomposition of picro-erythrine, was produced from erythric acid left undecomposed among the picro-erythrine.

given for the former the formula $C_{32}H_{16}O_{14}$, and for the latter $C_{34}H_{18}O_{15}$.

They consist in 100 parts of—

	Alpha-orsellic acid.			Beta-orsellic acid.	
	I.	II.	III.	IV.	V.
Carbon . .	60·00	60·98	60·78	60·07	60·20
Hydrogen .	5·03	5·00	4·98	5·06	5·26
Oxygen .	34·97	34·02	34·24	34·87	34·54

Now if these two acids are distinct, they are certainly isomeric, since the differences in the preceding analyses are not sufficiently great to justify the establishment of distinct formulæ. I propose for these acids, therefore, the formula $C_{34}H_{18}O_{14}$. The calculated composition will then be as follows:—

34 eqs. Carbon . .	2550	61·08
18 ... Hydrogen .	224·6	5·38
14 ... Oxygen . .	1400	33·54
	4174·6	100·00

If this be the true composition of orsellic acid, orsellate of baryta must be $C_{34}H_{18}O_{14}+BaO$, and must have the following composition:—

			Alpha-orsellate. Stenhouse.		
			I.	II.	Beta-orsellate.
34 eqs. Carbon .	2550	49·69	49·36	49·18	49·46
18 ... Hydrogen	224·6	4·37	3·83	3·79	4·12
14 ... Oxygen .	1400	27·30	27·32	27·54	27·90
1 ... Baryta .	956·8	18·64	19·49	19·49	18·52
	5131·4	100·00	100·00	100·00	100·00

Assuming, then, that the above formula is the correct one, it follows that—

if from 1 eq. orsellic acid . . $=C_{34}H_{18}O_{14}$
we subtract 1 eq. lecanoric acid $=C_{18}H_{8}O_{8}$
$C_{16}H_{10}O_{6}$
and add 1 eq. water . . . . . $= HO$
we obtain 1 eq. crystallized orcine $=C_{16}H_{11}O_{7}$

According to this view, then, orsellic acid is a coupled acid, consisting of orcine and lecanoric acid; or it is orcine-lecanoric acid, just as erythric acid is erypicrine-lecanoric acid. It is therefore obvious how the formation of lecanoric æther by means of boiling alcohol, and of orcine by means of alkalies, takes place.

The formation of erythrelesic and orsellesic acids still remains to be explained. If the view here taken be the correct one, it follows that these two acids must be identical with lecanoric acid combined with one equivalent of water, *i. e.* $C_{18}H_8O_8 + HO = C_{18}H_9O_9$. Its composition would then be as follows:—

			Orsellesic acid.	
			I.	II.
18 eqs. Carbon .	1350	57·14	57·99	57·90
9 ... Hydrogen	112·3	4·75	5·25	5·08
9 ... Oxygen .	900	38·11	36·76	37·02
	2362·3	100·00	100·00	100·00

The composition of erythrelesic acid not having been examined, it is impossible to arrive at any conclusion in regard to it.

I may here mention, that I some time ago discovered that if lecanoric acid be dissolved in caustic baryta and again precitated, it is found to have taken up one equivalent of water, and been changed into $C_{18}H_9O_9$, and this water it does not again lose. This equivalent of water is replaced in the æther compound by C_4H_5O; so that it appears that this æther forms no exception to the general rule, according to which the oxide of ethyle in the æthers always replaces the water of the hydrated acid. The body $C_{18}H_8O_8$ might still retain the old name of lecanorine.

Mr. Stenhouse's evernic acid may also be shown to belong to the same class of bodies as the preceding. Mr. Stenhouse gives for evernic acid the formula $C_{34}H_{16}O_{14}$. I propose to change this to $C_{36}H_{17}O_{15}$. It would then be composed as follows:—

			Stenhouse.	
			I.	II.
36 eqs. Carbon .	2700	61·19	61·63	61·61
17 ... Hydrogen	212·1	4·78	5·00	5·16
15 ... Oxygen .	1500	34·03	33·37	33·23
	4412·1	100·00	100·00	100·00

The potash and baryta salts, when calculated according to this new formula, also give numbers which agree sufficiently well with those found by Mr. Stenhouse.

Evernate of potash:—

				Stenhouse.	
				I.	II.
36 eqs.	Carbon .	2700	55·22	55·82	55·28
16 ...	Hydrogen	199·6	4·08	4·39	4·30
14 ...	Oxygen .	1400	28·64	27·49	28·12
1 ...	Potash .	589·9	12·06	12·30	12·30
		4889·5	100·00	100·00	100·00

Evernate of baryta:—

				Stenhouse.	
				I.	II.
36 eqs.	Carbon .	2700	51·36	50·30	50·31
16 ...	Hydrogen	199·6	3·79	4·10	3·96
14 ...	Oxygen .	1400	26·65	27·08	27·38
1 ...	Baryta .	956·8	18·20	18·52	18·35
		5256·4	100·00	100·00	100·00

If from 1 eq. evernic acid . . . $= C_{36} H_{17} O_{15}$
we subtract 1 eq. lecanoric acid . $= C_{18} H_{8} O_{8}$
$C_{18} H_{9} O_{7}$
and add 1 eq. water . . . . . $= \quad H \; O$
we obtain 1 eq. evernesic acid . $= C_{18} H_{10} O_{8}$

Evernic acid may therefore be considered as a coupled acid, consisting of lecanoric and evernesic acids. It consequently appears probable that the *Evernia Prunastri* may sometimes contain lecanoric acid only, as found by Rochleder and Heldt, and sometimes lecanoric and evernesic acids combined to evernic acid, as found by Mr. Stenhouse. The orcine which is produced on treating evernic acid with alkalies, must be derived from the lecanoric acid. Evernesic acid being a more stable body than lecanoric, resists the action of alkalies; and on supersaturating with a mineral acid, it is precipitated. It seems probable that if evernic acid were dissolved in caustic alkali without adding an excess of the latter, and the solution just heated to the boiling-point, as in the case of orsellic acid, then, on adding a strong acid, a mixture of lecanoric and evernesic acids would be precipitated. But as Mr. Stenhouse treated his evernic acid with an excess of caustic potash or baryta, and boiled for some time, the lecanoric acid would of course be decomposed. On treating evernic acid with boiling alcohol also, I should suppose that lecanoric æther would be generated, as well as evernesic æther.

Mr. Stenhouse, however, added caustic potash to the alcohol, so that any lecanoric acid or lecanoric æther present must necessarily have been decomposed.

I have still a few words to say in regard to Kane's erythryline. Kane has given this name to the colour-producing substance found by him in the *Roccella tinctoria*. It is analogous to erythric acid; but in composition it differs so widely from the latter, as to preclude the supposition of their being identical. Since erythryline gives among the products of its decomposition a body of the same composition as lecanoric æther, I infer that it contains lecanoric acid in a similar state of combination as in erythric, orsellic and evernic acids. Kane gives as the formula of erythryline, $C_{22}H_{16}O_6$. I propose to change this into $C_{42}H_{29}O_{12}$. The composition would then be as follows:—

			Kane.	
			I.	II.
42 eqs. Carbon .	3150	66·85	66·88	65·53
29 ... Hydrogen	361·9	7·68	8·13	8·38
12 ... Oxygen .	1200	25·47	24·99	26·09
	4711·9	100·00	100·00	100·00

If this formula be adopted, it follows that —

$$\begin{array}{ll} \text{if from 1 eq. erythryline . . . .} & = C_{42}H_{29}O_{12} \\ \text{we subtract 1 eq. lecanoric acid} & = \underline{C_{18}H_8\,O_8} \\ & \quad C_{24}H_{21}O_4 \\ \text{and add 2 eqs. water . . . . .} & = \underline{\quad\;\; H_2\,O_2} \\ \text{the resulting formula is . . .} & \quad C_{24}H_{23}O_6 \end{array}$$

= Rosellic aci

Now $C_{24}H_{23}O_6$ is the formula which I have given in my paper on the *Roccella tinctoria* for roccellic acid. It would appear, therefore, that erythryline is equal to lecanoric acid plus roccellic acid minus two equivalents of water. As Kane has mentioned no experiments on its products of decomposition, no further reasons can be given for or against this view of its composition.

In my opinion, therefore, erythryline, erythric acid, orsellic acid and evernic acid, are coupled acids, in which lecanoric acid forms the acid ingredient, and they might be called respectively I roccell-lecanoric, II erypicrine-lecanoric, III orcine-lecanoric and IV evernesine-lecanoric acids. They resemble those coupled acids, which consist of simple acids united with neutral bodies, and which saturate the same quantity of any base as the simple acids contained in them. They are similar to hippuric acid, which may be considered as a compound of

(Lecanoric acid) $C_{18}H_8O_8$ + (Rocellic acid) $C_{24}H_{23}O_6 - 2HO = C_{42}H_{29}O_{12}$ I

(- d° d°) $C_{18}H_8O_8$ + (Eripicrine) $C_{24}H_{16}O_{15} - HO = C_{42}H_{23}O_{21}$ II benzoic acid and glycocoll minus two equivalents of water. Before this view, however, can be considered as established, it will be necessary to examine carefully the æthers derived from lecanoric, erythric and orsellic acids, in regard to their crystalline form, specific gravity, action on light, &c., and also to compare accurately together orsellesic, erythrelesic and lecanoric acids. If the æthers derived from various sources should possess the same physical properties, and if orsellesic and erythrelesic acids should prove to be identical in properties and composition with lecanoric acid which has been dissolved in alkali and reprecipitated, then I contend that my view of the composition of these bodies will best explain the manifold changes which they undergo.

Rochdale, August 26, 1848.

XXXIX. *An Account of some Experiments on Volta-Electric Induction.* By Reuben Phillips, *Esq.*[*]

THE apparatus I employed consisted of three similar coils of covered copper wire, number 16, each coil being about 3·5 inches diameter, and containing eleven yards of wire; the ends of the wire were left exposed, so that by binding-screws the coils could be united in any order. I call these coils respectively A, B and C. To ascertain the force and direction of the induced current, I generally used an indicating helix containing a needle; this helix was made by winding covered copper wire, No. 24, on a piece of glass tube about $\frac{1}{13}$th of an inch in diameter; the wire was wound on the glass until it had acquired the depth of nine layers. This helix, which I call the *indicator*, was maintained in the same relative position with regard to an induced current, and the needles were always inserted with their points in the same relative direction; consequently the direction of the magnetism imparted by a current traversing the indicator was always the same for currents in that direction. I call a needle after it has been placed in the indicator, the *indicating needle*; the needles employed were of one size, and by the same maker. The magnetism imparted to them by the indicator was discovered by presenting them to one of two sewing-needles, which had been mag-

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[*] When I had completed this paper, which I thought was altogether new, I acquainted Dr. Faraday with it, who informed me Dr. Henry had previously given the same results in a paper entitled "Contributions to Electricity and Magnetism" (Transactions of the American Philosophical Society, vol. vi.); however, Dr. Faraday thought very well of my paper and advised its publication. It is proper to remark, the paper has been a little altered since Dr. Faraday expressed this good opinion of it.

netized and suspended so as to form a partially astatic arrangement. The battery was one on Smee's construction; it consisted of six repetitions, and the immersed portion of each plate was about 4 inches by 3·5 inches. Throughout these experiments the battery current always passed in the same direction with regard to the convolutions of the indicator.

One of the coils A was laid horizontally on a table, and the coil B upon A; the ends of A were in communication with the indicator, one end of B with a wire from the battery, and the other end of B dipped into some mercury into which the other galvanic pole could be plunged. A needle was now placed in the indicator, the battery circuit completed, and the needle removed; the point of the needle was found as the marked end of a magnet.

The battery current was now sent through B, then the needle was placed in the indicator and the battery circuit broken, when the eye end of the needle was the marked magnetic end.

A needle was now placed in the indicator, and the battery circuit made and broken, when the eye end of the needle became as the marked end of a magnet. In these three cases the magnetism was very strong and apparently equal.

These induced currents circulate in the same direction as pointed out by Dr. Faraday, but their law of relative force is entirely different; for he found that "when the unmagnetized needle was put into the indicating helix, before contact of the inducing wire with the battery, and remained there until the contact was broken, it exhibited little or no magnetism; the first effect having been nearly neutralized by the second. The force of the induced current upon making contact was found always to exceed that of the induced current at breaking of contact*."

When the coil A was connected with a galvanometer instead of the indicator, then closing the circuit through B, caused the ends of the galvanometric needles to start through a space of about $\frac{1}{20}$th of an inch, and the same on breaking the circuit; but when the end of the wire was rapidly plunged into and removed from the mercury, the needle appeared to make only an abrupt and barely perceptible motion on the circuit being completed. This agrees with the equality which Dr. Faraday and all who have followed him have found; so that although different helices give the two induced currents as various forces measured by the indicator, yet, as measured by the galvanometer, they are respectively very equal. This can only arise from the two induced currents being about equal in

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* Experimental Researches in Electricity, vol. i. p. 4.

quantity but differing in velocity; for if a shock of electricity is sent through the wire of a galvanometer in a given time, imparting motion to the needle, then if a shock of the same quantity of electricity passes through the wire in any less time, it will impart, making up in force what it wants in time, the same motion to the needle, while it would develope a much higher magnetism in the indicator. I thus conclude, that when induced currents are produced by the discharge of a Leyden jar, the magnetized needle principally registers the variable difference of velocity between the two currents.

It will be convenient to call the current induced in the neighbouring coil by completing the battery circuit, the *first induced current*; and that generated while the battery current ceases, the *second induced current.*

It seemed to me very improbable that the first and second induced currents should travel the copper wires with such unequal velocities, and yet be exactly equal in quantity; as it would involve a strange relation between the electromotive force of the induced currents and metallic electric conduction.

By proper supports and by bending it, I arranged the wire which dipped into the mercury, so that striking the wire, as one would a key of a piano played staccato, sufficed to close and open the battery quickly whenever I pleased. The galvanometer being connected with the coil A as before, the needles were made to vibrate through an arc about $\frac{1}{10}$th of an inch long; I then struck the wire with my finger at such times as should tend to increase the motion of the needles, on the supposition that the first induced current is the more powerful; the needles were soon reduced to rest. I again made the needles vibrate, and struck the wire at those times when so doing should add motion to the needles according to the notion that the second induced is the more powerful current, and now the needles could be kept oscillating in a small arc as long as I pleased. Therefore the second induced current is the larger quantity.

The third coil C was placed on B, one end of both A and C were united to the indicator, and the other ends bound together; A and C were so placed that on causing an electric current to circulate through B, the current induced in A should act against that induced in C; I found they effectually neutralized each other.

I next placed a copper plate 4·5 inches square (which was the size of all the plates employed in these experiments) and $\frac{1}{40}$th of an inch thick, between B and C, and made and broke the battery circuit once through B, which rendered the eye end of the needle as the marked magnetic pole.

The battery circuit was completed through B, and then the needle was placed in the indicator and the circuit broken, which made the eye end the marked pole.

Finally, a needle was placed in the indicator, the battery circuit closed and the needle removed; the point of the needle was the marked end. In these two last experiments the magnetism was much stronger than in the one preceding them.

The copper plate was now removed, and a plate of zinc the $\frac{1}{100}$dth of an inch thick was substituted for it; on closing and opening the battery circuit once, the eye end of the needle became as the marked magnetic pole. I have thought it rather unnecessary to examine every plate as I did the copper, and I have therefore taken the most easily obtained case as a representative of the others. I should observe, too, that in all these experiments care was taken to place only unmagnetized needles in the indicator.

A plate of lead $\frac{1}{20}$th of an inch thick being substituted for the zinc, produced the same effect; so also did a plate of tin of similar thickness; but when a glass plate $\frac{1}{20}$th of an inch thick was used instead of the metal plates, I could obtain no effect on the indicating needle, although I closed and opened the battery circuit through B many times.

When a plate of iron $\frac{1}{20}$th of an inch thick was placed between B and C, the same effect as if it was a copper plate was produced on the indicating needle, but with the iron plate the induced current was evidently much more powerful.

I now proceeded to examine if a specific force belongs to each metal, or whether they all similarly effect the volta-electric induction.

Between A and B I placed a plate of lead $\frac{1}{20}$th of an inch thick, and between B and C a plate of copper of similar thickness; the battery was then closed and opened once through B as before; the indicating needle had its eye end as the marked end of a magnet.

A needle was now placed in the indicator, the circuit through B completed, and the needle removed; in this case the point was the marked end.

The battery was now closed through B, the needle placed in the indicator, and the circuit broken; this gave the eye end of the indicating needle as the marked magnetic pole. In these two last experiments the magnetism was stronger than in the experiment next going before.

When a plate of copper was substituted for the lead, no effect could be obtained in the indicator, even when this plate of copper was only $\frac{1}{40}$th of an inch thick.

From the last experiment it follows that lead is much inferior to copper in cutting off the inductric influence of a conductor; and this difference is perceptible with either the first or second induced current; also the difference of magnetizing power between the first and second induced current still remains.

Having reversed the plates, that is, having placed the copper where the lead was, and the lead where the copper was, other circumstances being the same as before, the magnetism of the indicating needle announced a current of electricity the reverse of that obtained when the plates were in their former position. In this, and generally in all the following experiments, the mercury was not employed to complete the battery circuit, which was effected by bringing together two copper wires; I have found the results identical with either method.

A plate of tin $\frac{1}{20}$th of an inch thick and the usual size of 4·5 inches square, was placed between A and B, and the similar plate of copper between B and C; no effect was produced on the needle by completing the battery circuit twelve times; but when the circuit was closed and opened by drawing one of the terminal wires a few times over a file, the eye end of the needle became as the marked end of a magnet.

On placing the copper plate between A and B, and the tin plate between B and C, and interrupting the battery current as before, the point of the needle became as the marked end of a magnet. Hence tin acts with regard to copper as lead does, but the difference between tin and copper is less than between lead and copper.

I now placed between A and B a plate of zinc $\frac{1}{20}$th of an inch thick, and between B and C the similar plate of copper; but although I employed the file I could obtain no magnetism, even when another similar copper plate was laid on that previously used.

It occurred to me that perhaps such thick plates of metal were unsuited to develope the effects sought; for denoting by P, P′ those portions of the magnetic force which found their way respectively to A and C, then, although $\frac{P}{P'}$ might be very great and go on increasing with the thickness of the metal plates, yet P—P′, which must be the magnetizing force, might be very small and capable of increase by a somewhat diminished thickness of the plates. This proved to be true.

I placed between A and B a plate of zinc $\frac{1}{100}$dth of an inch thick, and between B and C a plate of copper $\frac{1}{80}$th of an inch thick; the battery circuit was closed and opened ten times, when the eye end of the indicating needle was its marked magnetic end. I now reversed the order of the plates, and closed and opened the circuit ten times, when the point of the needle was its marked magnetic end.

I substituted for the zinc in the two last experiments another plate of copper $\frac{1}{113}$th of an inch thick; in one experiment there was obtained a scarcely appreciable amount of magnetism and in the other none; in both instances the circuit was made ten times.

I could produce no effect in the indicating helix even with the aid of the file when a plate of glass $\frac{1}{20}$th of an inch thick was placed on one side of B, and a corresponding interval filled of course with air, maintained on the other side.

I thought it interesting to ascertain what relation two similar plates of one metal, but having different thicknesses, bear to each other in limiting the action of the primary coil upon the secondary coils. I therefore placed a plate of zinc $\frac{1}{100}$dth of an inch thick, together with a plate of glass $\frac{1}{20}$th of an inch thick, between A and B, and between B and C a plate of zinc $\frac{1}{20}$th of an inch thick; the glass was merely employed to produce a suitable space between A and B; the battery circuit was closed and opened twenty times, which made the eye end of the indicating needle to become as the marked end of a magnet. When the thin zinc and glass were placed between B and C, and the thick zinc between A and B, then on completing and breaking the battery circuit as before, the indicating needle showed a reverse magnetism. I made some experiments to ascertain if any difference arose by putting the glass plate on different sides of the thin metal plate, but could obtain no result to countenance such a notion.

Like results were obtained by using for the two zinc plates of the last experiments two plates of copper, one $\frac{1}{113}$th of an inch and the other $\frac{1}{20}$th of an inch thick. Nor did it appear to occasion any difference if the place of the thicker metal plate was supplied by two thinner ones.

From these experiments I conclude, that a thick mass of metal plate stands to a thinner mass of the same metal as copper does to lead.

The question now arose in my mind, Does a metallic plate effect similarly, as to degree, the first and second induced currents? This was answered negatively by the following experiments.

Having removed the upper coil C, I united that end of the indicator, with which C had previously communicated, to the end of A which had been joined to C, and placed two plates of copper, each $\frac{1}{20}$th of an inch thick, between A and B; after having completed and broken the battery circuit once through B, the point of the indicating needle was as the marked end of a magnet. One only of the copper plates was insufficient to produce this effect.

I now used instead of the copper plates two zinc plates each $\frac{1}{20}$th of an inch thick; the magnetism of the needle, after the completion and disruption of the battery circuit as before, was much weaker than if no plates had been interposed between the coils, and in the same direction, that is, the eye end of the needle was as the marked magnetic end. Having removed these zinc plates, and substituted a pair of zinc plates each 8 inches by 4·5 inches square, their combined thickness being ·3 of an inch, and having closed and opened the battery circuit once, I found the point of the needle strongly as the marked end of a magnet; these plates had however been amalgamated and a little used for galvanic purposes. Two plates of zinc and two of tin, each $\frac{1}{20}$th of an inch thick and of the usual size, acted the same as the two thick zinc plates in the last experiment; the tin plates alone were quite ineffective. In these experiments the induced current always passed in the directions pointed out by Dr. Faraday.

Here, then, we have the first induced current the more powerful, which is the contrary to that obtained in any of the foregoing experiments. The superiority of copper in producing this effect evidently arises from its great power of electric conduction. When a mass of glass was used instead of the copper, the eye end of the indicating needle became of course as the marked end of a magnet.

I made some experiments on the transmission of the magnetic force by the metals with the aid of two small electro-magnets, the coil of one being connected with the indicator and that of the other with the battery; the poles of each were placed near to each other, so that the battery current being sent through, or checked in the coil of one magnet, induced a current in that of the other. The interposition of masses of metal between the magnetic poles occasioned no difference in the intensity of the magnetism imparted to the indicating needle when the battery circuit was closed or opened. I account for this by considering the slowness with which iron varies its magnetic state, and by supposing the action of such a metal as copper, when placed in the lines of magnetic force, retards their transmission more than air, and my apparatus not being sufficiently delicate to distinguish these two effects. Dr. Faraday first supposed such might be the action of interposed substances, and his experiments on this subject much confirm this view*.

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* See Experimental Researches in Electricity, vol.i.p.545 and following.

In concluding I cannot but observe, that the power possessed by the different metals I have yet employed to shut off the circumferential influence of an electric current varies as their electric conduction; for we have them in the order of copper, zinc, tin and lead, which is that of their relative powers of electric conduction according to Harris. This view is further confirmed by the difference produced by thick and thin plates of the same metal. Indeed the following experiment clearly shows that the office of a metal, in thus intercepting volta-electric induction, is that of an electric conductor.

The former arrangement of A, B and C was restored; and of two similar plates of zinc, each $\frac{1}{100}$dth of an inch thick, one was placed between A and B and the other between B and C; then by the aid of a file I rapidly made and broke the battery current through B, but could obtain no magnetism in the indicating needle. I now removed the plate from between B and C, and commencing at the middle point of one of its edges, I cut a slit at a right angle to the edge 3·5 inches long and ·1 of an inch wide; the plate was then replaced in its former position between B and C, and the battery closed and opened once through B, which made the point of the indicating needle as the marked end of a magnet. When the position of the plates was reversed, the magnetism of the indicator was also reversed. Therefore a plate is to its fellow which has been slit as copper is to lead; and, moreover, the only assignable use of the slit is to prevent the circulation of electric currents.

With regard to constructing the three coils with which these foregoing results are to be obtained, I would remark they should not be much more than half an inch thick each; they may however contain any quantity of wire, and be of any diameter consistently with the power of the battery. I propose to call this instrument a *differential volta-inductometer.*

By means of my three small coils it is easy to obtain a shock from the specific induction of a metal, by removing the indicator from the ends of the coils A and C, these coils being of course set so as to tend to circulate the induced current in opposite directions through each other; then having placed a plate of copper next B and brought the unattached ends of A and C to one's tongue, on closing and opening the battery circuit through B the shock is obtained.

7 Prospect Place, Ball's Pond Road,
August 8, 1848.

XL. *On some Properties derivable from the development of a Binomial; with a simplified proof of a remarkable Theorem of* Abel. *By* J. R. Young, *Professor of Mathematics in Belfast College*[*].

THE following properties of the binomial development, although for the most part new to me, yet lie so near the surface, that I can scarcely suppose that they have hitherto been overlooked. It is probable, however, that there may at least be something novel in the method of investigation I have employed; and this, added to the circumstance that the proof of Abel's remarkable generalization of the binomial theorem, and of the development of the function $\varphi\,(x+\alpha)$, is a little facilitated by this method, may perhaps justify the publication of the paper in these pages.

If we apply successive differentiation to the formula

$$(1-x)^n=1-nx+\frac{n(n-1)}{2}x^2-\frac{n(n-1)(n-2)}{2.3}x^3+\&c.,$$

regarding n as a positive integer, and always multiply by x previously to each differentiation after the first; then, upon making x equal to 1, we shall obtain these results, viz.

$$0=-n+\frac{n(n-1)}{2}2-\frac{n(n-1)(n-2)}{2.3}3+\&c.\quad .\quad .\quad (1.)$$

$$0=-n+\frac{n(n-1)}{2}2^2-\frac{n(n-1)(n-2)}{2.3}3^2+\&c.\quad .\quad (2.)$$

$$0=-n+\frac{n(n-1)}{2}2^3-\frac{n(n-1)(n-2)}{2.3}3^3+\&c.\quad .\quad (3.)$$

$$\vdots$$

$$0=-n+\frac{n(n-1)}{2}2^{n-1}-\frac{n(n-1)(n-2)}{2.3}3^{n-1}+\&c.\quad (4.)\dagger$$

as also

$$(-1)^n1.2.3.4\ldots n=-n+\frac{n(n-1)}{2}2^n-\frac{n(n-1)(n-2)}{2.3}3^n+\&c.\quad (5.)$$

In this last formula n may be any positive integer without

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[*] Communicated by the Author.

[†] Certain expressions analogous to these are well-known, viz.

$$0=1-n.2^m+\frac{n(n-1)}{2}3^m-\&c.,$$

where, as above, m is less than n; but whether the forms in the text have also been given or not I am unable to say. By differentiating oftener than once, before multiplying by x, different zero-expressions may be obtained, as also by multiplying by powers of x, instead of by x simply.

exception; but from the others the case of $n=1$ is excluded: it is comprehended in the well-known form

$$0=1-n+\frac{n(n-1)}{2}-\frac{n(n-1)(n-2)}{2.3}+\&c. \quad . \quad . \quad (6.)$$

supplied at once by the original development.

As in each of the above forms the final coefficient is equal to unit, it follows that, by transposing the final term of (4.), we shall have the expression

$$(-n)^{n-1}=-n+\frac{n(n-1)}{2}2^{n-1}-\frac{n(n-1)(n-2)}{2.3}3^{n-1}+\&c. \quad (7.)$$

to $n-1$ terms; and similarly for the forms which precede (4.).

Now all these formulæ may be converted into others of greater generality by aid of the following theorem, viz.

If it be true that

$$0=-n+\frac{n(n-1)}{2}2^{m}-\frac{n(n-1)(n-2)}{2.3}3^{m}+\&c. \quad (8.)$$

throughout a certain range of consecutive positive integer values of m, then it is equally true that, throughout the same range, the following more general form will have place:

$$\left.\begin{array}{l}0=x^{m}-n(x+\beta)^{m}+\frac{n(n-1)}{2}(x+2\beta)^{m}-\frac{n(n-1)(n-2)}{2.3} \\ \qquad (x+3\beta)^{m}+\&c;\end{array}\right\} \quad (9.)$$

and also that if (5.) be true, then will

$$\left.\begin{array}{l}(-1)^{n}\,1.2.3\ldots n=x^{n}-n(x+1)^{n}+\frac{n(n-1)}{2}(x+2)^{n} \\ \qquad -\frac{n(n-1)(n-2)}{2.3}(x+3)^{n}+\&c.\end{array}\right\} \quad (10.)$$

be equally true, x and β being any values whatever.

This theorem may be established either by common algebra, or by the first principles of the integral calculus; the latter being the shorter method, I shall employ it here.

Suppose the formula (9.) to be true for some preceding value of m, as $m=p$, so that we may have

$$\begin{array}{l}0=x^{p}-n(x+\beta)^{p}+\frac{n(n-1)}{2}(x+2\beta)^{p}-\frac{n(n-1)(n-2)}{2.3} \\ \qquad (x+3\beta)^{p}+\&c.;\end{array}$$

then, multiplying by $(p+1)\,dx$ and integrating, we have

$$\begin{array}{l}C=x^{p+1}-n(x+\beta)^{p+1}+\frac{n(n-1)}{2}(x+2\beta)^{p+1}-\frac{n(n-1)(n-2)}{2.3} \\ \qquad (x+3\beta)^{p+1}+\&c.\end{array}$$

To determine the constant C, put $x=0$; and we have

$$C=\beta^{p+1}\left\{-n+\frac{n(n-1)}{2}2^{p+1}-\frac{n(n-1)(n-2)}{2.3}3^{p+1}+\&c.\right\},$$

which, by (8.), is equal to zero, p being by hypothesis less than m: hence if the formula hold for a value less than m, it holds also for the next value, and so on for all values up to m; and as it obviously holds for $p=0$ by (6.), the formula is general.

If, however, we suppose p to be so great as $n-1$, then the expression for C will be

$$C=\beta^{n}\left\{-n+\frac{n(n-1)}{2}2^{n}-\frac{n(n-1)(n-2)}{2.3}3^{n}+\&c.\right\}, \quad (11.)$$

in which, if we make $\beta=1$, we have for C, as shown in (5.), the value

$$C=(-1)^{n}\,1.2.3.4\ldots n,$$

and therefore the general formula (10.) is also true.

It thus appears that we may give to m in (9.) any positive integral value up to $n-1$; for this latter value the formula is

$$\left.\begin{array}{l} 0=x^{n-1}-n(x+\beta)^{n-1}+\dfrac{n(n-1)}{2}(x+2\beta)^{n-1}- \\ \dfrac{n(n-1)(n-2)}{2.3}(x+3\beta)^{n-1}+\&c.; \end{array}\right\} . \quad (12.)$$

and therefore, by transposing the $(n+1)$th term,

$$\left.\begin{array}{l} (-1)^{n-1}(x+n\beta)^{n-1}=x^{n-1}-n(x+\beta)^{n-1}+\dfrac{n(n-1)}{2} \\ (x+2\beta)^{n-1}-\ldots \text{ to } n \text{ terms.} \end{array}\right\} \quad (13.)$$

This expression will be useful in simplifying the investigation of the theorem of Abel already alluded to*. There is only one step in Abel's process in which the need for any such simplification can be felt; it is that in which the constant in the result of his integration is to be determined; and where, by the analytical artifice employed by Abel, there seems to be something like an anticipation of the value sought.

Abel's generalization of the binomial theorem is thus expressed:

$$\left.\begin{array}{l} (x+\alpha)^{n}=x^{n}+n\alpha(x+\beta)^{n-1}+\dfrac{n(n-1)}{2}\alpha(\alpha-2\beta)(x+2\beta)^{n-2} \\ +\dfrac{n(n-1)(n-2)}{2.3}\alpha(\alpha-3\beta)(x+3\beta)^{n-3}+\ldots. \\ n\alpha(\alpha-(n-1)\beta)^{n-2}(x+(n-1)\beta)+\alpha(\alpha-n\beta)^{n-1} \end{array}\right\} (14.)$$

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* *Œuvres Complètes*, vol. i. p. 31.

where x, α, and β are any quantities whatever, and n any positive integer.

That this is true when $n=0$, is obvious; and it may be shown, as follows, that if it be true for $n=m$, it must be true for $n=m+1$; and thence that it is true generally*.

Let

$$(x+\alpha)^m = x^m + m\alpha(x+\beta)^{m-1} + \frac{m(m-1)}{2}\alpha(\alpha-2\beta)(x+2\beta)^{m-2}$$

$$+\frac{m(m-1)(m-2)}{2.3}\alpha(\alpha-3\beta)(x+3\beta)^{m-3}+\ldots\ldots$$

$$m\alpha(\alpha-(m-1)\beta)^{m-2}(x+(m-1)\beta)+\alpha(\alpha-m\beta)^{m-1};$$

then, multiplying by $(m+1)dx$, and integrating, we have

$$(x+\alpha)^{m+1}=x^{m+1}+(m+1)\alpha(x+\beta)^m+\frac{(m+1)m}{2}\alpha(\alpha-2\beta)$$

$$(x+2\beta)^{m-1}\ldots\ldots(m+1)\alpha(\alpha-m\beta)^{m-1}(x+m\beta)+\mathrm{C}.$$

To determine C, put $x=-\alpha$; then, dividing by $-\alpha$,

$$0=(-\alpha)^m-(m+1)(-\alpha+\beta)^m+\frac{(m+1)m}{2}(-\alpha+2\beta)^m-\ldots\ldots$$

$$(m+1)(-\alpha+m\beta)^m-\frac{\mathrm{C}}{\alpha};$$

$$\therefore\frac{\mathrm{C}}{\alpha}=(-\alpha)^m-(m+1)(-\alpha+\beta)^m+\frac{(m+1)m}{2}(-\alpha+2\beta)^m-$$

$$\ldots\ldots\text{to } m+1 \text{ terms.}$$

But, by (13.), the right-hand member of this is the development of

$$(-1)^m(-\alpha+(m+1)\beta)^m,\text{ or }(\alpha-(m+1)\beta)^m;$$

$$\therefore\ \mathrm{C}=\alpha(\alpha-(m+1)\beta)^m..$$

Hence the theorem has place for $n=m+1$, and thence generally.

The theorem (12.) is included in this of Abel, as that illustrious analyst has noticed; but the other analogous theorems in this paper, where the exponents are different from $n-1$, are not implied in the theorem of Abel, nor have I ever met with them.

It is probable, however, that some works on finite differences may contain these expressions; but this is a point about which I have not thought much inquiry necessary, as my chief object is to show, in detail, how a remarkable theorem in the higher analysis may be arrived at by only the simplest principles of the calculus, combined with common algebra.

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* The process, up to the determination of C, is Abel's.

If we interchange x and α, the foregoing development will become

$$(x+\alpha)^n=\alpha^n+n(\alpha+\beta)^{n-1}x+\frac{n(n-1)}{2}(\alpha+2\beta)^{n-2}x(x-2\beta)$$
$$+\frac{n(n-1)(n-2)}{2.3}(\alpha+3\beta)^{n-3}x(x-3\beta)^2+\&c.$$

Having obtained these results for the expanded binomial, in the case of n a whole number, it is natural to inquire whether the theorem does not admit of a wider application; whether indeed it is not perfectly general for every function of $(x+\alpha)$.

Let us then assume, from the law thus suggested, that

$$\phi(x+\alpha)=\mathrm{A}+\mathrm{B}x+\mathrm{C}x(x-2\beta)+\mathrm{D}x(x-3\beta)^2$$
$$+\mathrm{E}x(x-4\beta)^3+\&c.$$

$$\therefore\ \frac{d\phi(x+\alpha)}{dx}=\mathrm{B}+2\mathrm{C}(x-\beta)+\mathrm{D}\{(x-3\beta)^2+2x(x-3\beta)\}$$
$$+\mathrm{E}\{(x-4\beta)^3+3x(x-4\beta)^2\}+\&c.$$

$$\frac{d^2\phi(x+\alpha)}{dx^2}=2\mathrm{C}+2.3\mathrm{D}(x-2\beta)+2.3\mathrm{E}$$
$$\{(x-4\beta)^2+x(x-4\beta)\}+\&c.$$

$$\frac{d^3\phi(x+\alpha)}{dx^3}=2.3\mathrm{D}+2.3.4\mathrm{E}(x-3\beta)+\&c.$$

$$\frac{d^4\phi(x+\alpha)}{dx^4}=2.3.4\mathrm{E}+\&c.$$

Now the right-hand members of these equations become each reduced to its first term by putting $x=\beta$, $x=2\beta$, $x=3\beta$, and so on in succession. Hence

$$\phi(x+\alpha)=\phi(\alpha)+\frac{d\phi(\beta+\alpha)}{d\beta}x+\frac{d^2\phi(2\beta+\alpha)}{(d2\beta)^2}\frac{x(x-2\beta)}{2}$$
$$+\frac{d^3\phi(3\beta+\alpha)}{(d3\beta)^3}\frac{x(x-3\beta)^2}{2.3}+\frac{d^4\phi(4\beta+\alpha)}{(d4\beta)^4}\frac{x(x-4\beta)^3}{2.3.4}+\&c.;$$

or, interchanging α and x, and remembering that

$$\frac{d^n\phi(p+x)}{(dp)^n}=\frac{d^n\phi(x+p)}{(dx)^n},$$

we have

$$\phi(x+\alpha)=\phi(x)+\alpha\frac{d\phi(x+\beta)}{dx}+\frac{\alpha(\alpha-2\beta)}{2}\frac{d^2\phi(x+2\beta)}{dx^2}$$
$$+\frac{\alpha(\alpha-3\beta)^2}{2.3}\frac{d^3\phi(x+3\beta)}{dx^3}+\&c.,$$

which theorem is established by Abel in an elaborate memoir at page 82 of his second volume.

If we put $x-h$ for x, and make $\alpha=h$, and $\beta=-h$, we shall have

$$\phi(x)=\phi(x-h)+\frac{d\phi(x-2h)}{dx}\frac{2h}{2}+\frac{d^2\phi(x-3h)}{dx^2}\frac{(3h)^2}{2.3}+\&c.,^*$$

a theorem given by Murphy in the Philosophical Transactions for 1837, and which is usually referred to as "Murphy's theorem." It appears, however, from the above method of obtaining it, that it is a particular case of the general theorem of Abel. But as Abel's posthumous papers—among which this theorem was found—were not published till 1839, ten years after the author's death, Murphy's claim is quite independent, although not, as hitherto supposed, exclusive.

The preceding method of establishing this general theorem of Abel, by assuming a series of a particular form, and then, as in the investigation of Maclaurin's theorem, putting, in the successive differential coefficients, those values for x which cause all the terms containing this variable to vanish, is very short and simple; but without the previous proof of the particular case of the binomial theorem, we should have had nothing to suggest to us the peculiar form selected. This suggestion, however, once supplied, we may, instead of examining each differential coefficient in succession, as above, and thence inferring the general uniformity of the law of the coefficients A, B, C, &c. by induction,—instead of this, we may proceed at once to demonstrate the general property that

$$\frac{d^n x(x-m\beta)^{m-1}}{dx^n} \quad . \; . \; . \; . \; . \; . \; . \qquad \text{(A.)}$$

is zero for $x=n\beta$, provided $n<m$; and that it is $1.2.3....n$, for $n=m$; which property will establish the above values of A, B, C, &c. rigorously and universally.

To prove it, conceive x in (A.) to be changed into $x+h$, the binomial to be developed, and the series multiplied by $x+h$: the coefficient of h^n, in the result, will, of course, when multiplied by $1.2.3...n$, be the nth differential coefficient (A.). It is easy to see that this coefficient, that is the coefficient of h^n, is

$$\frac{(m-1)(m-2)(m-3)....(m-n)}{1.2.3....n}x(x-m\beta)^{m-n-1}$$

$$+\frac{(m-1)(m-2)....(m-n+1)}{1.2....(n-1)}(x-m\beta)^{m-n},$$

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* The more general form of the theorem is got by putting

$$x=x-\alpha,\quad \alpha=nh,\quad \beta=-h.$$

of which the factors involving x, when $n\beta$ is put for x, are

$$n(n-m)^{m-n-1}\beta^{m-n}, \text{ and } (n-m)^{m-n}\beta^{m-n};$$

so that the expression for the nth differential coefficient, still omitting the numerical factor $1.2.3 \ldots n$, is

$$\frac{(m-1)(m-2)\ldots.(m-n+1)}{1.2\ldots.(n-1)}\{-(n-m)^{m-n}+(n-m)^{m-n}\}\beta^{m-n},$$

which, when $n<m$, is zero. If $n=m$, the first term of the general coefficient of h^n above, is simply unit: hence, introducing the absent numerical factor, we have in this case

$$(\text{A.})=1.2.3\ldots n,$$

as was to be proved.

The expression under the sign of differentiation in (A.), if developed, and then the series differentiated n times, $n\beta$ being afterwards put for x, will furnish new properties of the binomial coefficients, which need not however be here formally exhibited.

There are probably other expressions, analogous to (A.), that might suggest developments of like generality with that of Abel; but I question whether they are worth searching for. In fact, and the remark is of some importance, all such general theorems must be received with caution and qualification: they are strictly true only when the terms of the series become at length actually zero, or at least tend to zero. Without this qualification, the binomial theorem itself, even in its ordinary form, would be fallacious. Analysis, unfortunately, abounds with fallacies of this kind. Theorems are sometimes presented to us with an aspect of unlimited generality, which are not true even in a single individual case! The following is an instance: viz.

$$\phi(x)=\phi(x+\alpha)+\phi(x-\alpha)-\phi(x+2\alpha)-\phi(x-2\alpha)+\ldots.$$

the simplest case of which, viz.

$$x=x+x-x-x+x+\ldots.$$

is repugnant to common sense.

This theorem was, I believe, first given by Français in the *Annales des Math.*, vol. iii. It was again obtained by Abel, in vol. ii. of his *Œuvres*, p. 85, and has been reproduced by Gregory in his Examples, p. 243. It is the tendency of analysis, if left unrestrained, to spread, in some directions, into unprofitable luxuriance, by which it is encumbered, but not advanced. A little in the way of correcting this fault, I have myself attempted in the pages of this Journal and elsewhere; but much more is required. This, however, is a task which I leave to those whose position may be less inimical to the cultivation of science than mine happens to be. I shall merely add, in conclusion, that in order not to encroach unreasonably on these pages, I have transferred two articles connected with the present paper to the Mechanics' Magazine; they probably appear in the numbers for Sept. 16 and Sept. 23; in the latter of which I have given a short and easy investigation of the theorem of Leibnitz for the advanced differential coefficient of the product of two functions.

Belfast, Aug. 26, 1848.

XLI. *Sixth Memoir on Induction. By* M. ELIE WARTMANN, *Professor of Natural Philosophy in the Academy of Geneva*[*].

[Continued from p. 94.]

§ XVIII. *Does Induction affect the acoustic properties of Elastic Bodies?*

165. I PUBLISHED two years ago[†] some experimental researches on the *causes* of the sounds produced by discontinuous electric currents in metallic wires. This phænomenon may be viewed under other aspects, and it may be asked whether a *permanent* electric induction determines in the molecules of sonorous bodies a change of elasticity which is rendered evident by appreciable modifications in their acoustic properties. The following experiments have been undertaken with a view to solve this question.

166. A disc was selected of $0^{m}\cdot198$ diameter, and $0^{m}\cdot0018$ thickness, forming part of a Marloye's set of plates. This disc was made with the greatest possible care: its texture is remarkably homogeneous, and the acoustic figures, formed of diameters or concentric circumferences, are produced upon it with extreme precision. Its lower surface was covered with a thick layer of gum-lac varnish; and after strongly electrizing this sort of electrophore, the charge was sustained by means of a good machine. It was however impossible to discover the least difference in its sonorous state, whether it was electrified or not.

167. A glass disc, $0^{m}\cdot135$ in diameter and $0^{m}\cdot002$ in thickness, was furnished on both its surfaces with a circular armature of tinfoil, $0^{m}\cdot117$ in diameter. This flat condenser was

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[*] Communicated by the Author.

[†] *Archives des Sciences Physiques et Naturelles*, vol. i. p. 419. In the sitting of the 8th of May last, M. Wertheim presented to the Academy of Sciences of Paris some further researches on this subject, the conclusions of which are identical with those which I then enunciated with respect to the effects of discontinuous currents. See the *Institut* of the 10th of May, No. 749.

held by its centre between two isolating clamps. Its fundamental tone was then ascertained, and the tones which correspond to different modes of nodal subdivisions. The same experiments were then repeated, after electrifying the lower surface, the other being put in communication with the ground. The fluid distributed over the latter exercised no influence on the musical qualities of the plate. The only effect which indicated its presence was a repulsion of the particles of sand at the fixed point toward the common intersection of the diameters.

168. An iron wire, one metre long, was stretched on a monochord. It occupied the axis of a glass tube, on which was rolled a thick copper wire covered with silk, and forming part of the circuit of a Grove's battery of five pairs (139.). The transversal and longitudinal sounds of the wire remained the same as before the action of the current.

169. A brass wire, arranged on the sonometer parallel to the first, was tuned exactly in unison with it. An electro-magnet excited by the five pairs was placed near the iron wire. Every time the instrument was arranged so that the attraction of its poles could not make the wire bend, the latter comported itself like the brass wire. This was proved by the absence of beats when they were made to sound together.

170. In the preceding experiment the induction was distributed over a great length. It might thence be feared that its effect would be diminished on the nodal or ventral portions of the wire. I therefore repeated these experiments with wires of only $0^{m}\cdot20$ of copper and iron. In order to prevent any temporary or permanent disfigurement resulting from the different tensions to which they were subjected, I employed in stretching them a mechanical artifice used in the suspension of the threads of rheometers. It consists of a moveable nut acting upon a screw terminated by a square prism, which slips into a fixed hollow piece of the same section. The metallic cord was attached at one end to an immoveable pin, and the other to the extremity of the prism opposite to the screw. It coincided with the axis of a bobbin $0^{m}\cdot1$ in length, and $0^{m}\cdot032$ external diameter, and which is perforated by an aperture of $0^{m}\cdot011$, in which were placed eleven concentric tin cylinders, cut according to a generatrice and isolated from one another. Each wire was made to vibrate transversely by means of the bow, first in the natural state and then under the intense induction of Grove's battery, without ever finding a difference in the musical sound. The experiment was repeated with different degrees of tension of the wires, and making them give a numerous series of harmonics. The result never varied.

171. I afterwards operated on metallic plates placed near a magnetic apparatus, the induction of which could make itself felt successively in all their sections. With this view I procured three discs, one of tempered steel, another of untempered steel, and the third of soft iron, $0^{m}\cdot198$ in diameter and $0^{m}\cdot0018$ in thickness: they were fixed in turn on a glass bar, kept vertical by a convenient support. The magnet, which is provided with a vertical steel rod situated in the prolongation of the geometrical axis of the bar, and parallel to the branches of the horseshoe, is arranged above the plate, and at a distance variable at will. This rod is terminated by an endless screw in which a toothed-wheel catches, which is worked by a winch. One of the extremities of the wire of the electro-magnet is fastened to the rod, whilst the other is soldered to a copper ring which surrounds an isolating wooden disc carried by the axis. This simple arrangement permits the current of Grove's battery to pass into this wire without interruption, whatever be the velocity of rotation given to the magnet. It suffices for this to employ as voltaic poles two copper springs, one of which presses against the steel axis, and the other against the ring.

172. The three discs, being of different elasticity, gave a different fundamental sound, notwithstanding the equality of their dimensions. The electro-magnet exerted on them, at a distance of two to six millimetres, so energetic an attraction, that it was necessary to recur to an enormous pressure against their support to overcome it entirely.

173. After having sprinkled the plate under observation with very dry sand, and determined the corresponding sounds by different modes of vibration indicated by acoustic figures, I endeavoured to ascertain whether the magnetic influence is capable of modifying these sounds. Here, again, all the results were negative. The only difference of action of the magnet, according as it was immovable or turned with any rapidity, was limited to a slight irregularity of the nodal lines, solely produced by the currents of air.

174. When iron-filings were substituted for the sand, the sonorous phænomena were still the same, although more suppressed. The filings spread in the vicinity of the poles accumulated against them as soon as the electric current was established, and soon rendered the rotatory motion of the magnet almost impossible.

175. These experiments were repeated, rendering the passage of the electricity discontinuous. The molecular shocks investigated by several physicists were then heard. With a mercurial commutator in the circuit of the battery exciting

the magnet, the plates of soft iron become the seat of sufficiently intense sounds for a great number of persons to be able to hear them simultaneously.

176. Finally, I tried to induce at the same time electricity of tension and magnetism in the steel and iron discs. For this purpose, the bar which supports them was placed in the centre of a glass tube $0^{m}\cdot02$ in diameter, to which was fixed a horizontal circle of wood, $0^{m}\cdot18$ in diameter by $0^{m}\cdot018$ in thickness. This circle, entirely covered with tinfoil, communicates with a good electric machine. Parallel to the surface which is made to vibrate, it is thus brought as near as possible to it without the spark being emitted to it. The acoustic properties of the three discs remained indifferent to this new action. The plate of tempered steel had acquired a permanent magnetism which did not at all interfere with its musical properties.

177. It results from these experiments, that electric or magnetic induction has no *appreciable* action on the elasticity of different sonorous bodies, such as glass, copper, brass, soft iron, and steel tempered or untempered. The number of vibrations executed by them in the unity of time remains the same. But this conclusion must probably not be accepted in too absolute a manner. It might be that extremely energetic and very durable causes of induction determine an action which, in my experiments, has been too weak to be observed*.

Geneva, April 15, 1848.

XLII. *Notes on Quaternions. By* GEORGE BOOLE, *Esq.*†

Interpretation of Quaternions.

MR. CAYLEY'S ingenious researches, published in the last Number of the Philosophical Magazine, have recalled to my mind some speculations of my own upon the same subject. To the purely mathematical treatment of it I have indeed little to add. What I shall say will rather have reference to its philosophy.

It were much to be desired that the general principles which govern the use of signs, as instruments of reasoning, were re-

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* M. G. Wertheim has found that no modification of elasticity is perceptible in an iron or steel wire occupying the centre of an electro-magnetic bobbin, when the current has only traversed it for a short time. According to that ingenious experimentalist, the magnetization does not act directly upon the elasticity, but produces a new molecular arrangement.—*Annales de Chimie et de Physique*, December 1844, vol. xii. p. 623.

† Communicated by the Author.

duced to a consistent theory; for there undoubtedly exists a theory of signs applicable as well to the signs of common discourse as to the signs of mathematics. Without attempting to exhibit any complete doctrine on the subject, I will venture to state one or two views which I have been led to form, and apply them to the subject of quaternions.

Signs employed as instruments of reasoning may, in one point of view, be considered as the representatives of operations. This is not indeed the interpretation that we *necessarily* attach to them, but it is one which it is probable that in all cases we *may* attach, and from which their laws may in all cases be deduced. If we employ a particular symbol or combination of symbols A to represent a given operation, and another symbol or combination of symbols B to represent another given operation of the same kind, then, according to the Arabic order of writing, AB will represent the successive performance of the operations denoted by B and A. In order that such signs may be really available as instruments of deduction, it is necessary that the laws of the symbols entering into B and A should be such, that the expression AB developed according to those laws may, *in agreement with the conventions* established for the interpretation of A and B, represent an operation, the effect of which is equivalent to the combined effects of the operation A and B performed in the order (proceeding from right to left) AB.

In strict accordance with this principle, we may assign an interpretation to a quaternion $w+ix+jy+kz$, *subject to the condition*

$$w^2+x^2+y^2+z^2=1 \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

For if we write

$$w=\cos\frac{\theta}{2} \quad x=\sin\frac{\theta}{2}\cos\phi \quad y=\sin\frac{\theta}{2}\cos\psi \quad z=\sin\frac{\theta}{2}\cos\chi,$$

and assume the quaternion $w+ix+jy+kz$ to represent a rotation through an angle θ round an axis whose direction cosines are ϕ, ψ, χ, then representing this quaternion by Λ and any other quaternion subject to a similar condition by Λ', we shall have

$$\Lambda\Lambda'=\Lambda'',$$

Λ'' being a quaternion which, according to the same conventions, will represent a single rotation equivalent in effect to the two rotations represented by Λ' and Λ performed in the given succession.

A quaternion which does not satisfy the condition (1.) cannot be directly interpreted in geometry. Such expressions may nevertheless be employed, with the understanding that certain factors shall be rejected or other reductions performed, and thus they may lead to correct results. But the employment of such a form of the process seems to involve a departure from the principle, that the laws of the sign shall constitute in every respect an exact counterpart to the laws of the thing signified.

Sir William Hamilton's theory of the application of quaternions appears to be based upon the relations which their elements bear to the angles and angular points of spherical polygons; and similar to this is the basis which Prof. Graves has adopted for the not less interesting theory of triplets and multiplets. In all these systems we meet with such theorems as the following, viz. *that the product of two given points is a certain third point*, &c., by which it is meant that a certain expression, having a determinate reference to the latter point, is the product of two expressions having a similar determinate reference to the two original points. I believe that upon examination it will be found that these systems of interpretation are founded upon a principle of Naming, as the one which I have proposed is founded upon a principle of Operation. And I think it not foreign to the subject to remark, that the symbolical forms of common language as exhibited in the calculus of logic may indifferently be referred to the one or the other of these modes of conception.

Laws of Quaternions.

The laws of quaternion multiplication are founded, as is well known, upon the following relation:

$$i^2=-1 \quad j^2=-1 \quad k^2=-1 \quad ij=k \quad jk=i \quad ki=j$$
$$ji=-k \quad kj=-i \quad ik=-j;$$

but it may be shown that the three last of these laws are consequences of the former ones considered as of universal application.

For if $ij=k$ universally, let the subject be jy, then

$$ij\,jy=kjy,$$

or

$$ij^2y=kjy;$$

but $j^2=-1$, therefore

$$-iy=kjy,$$

and similarly for the others.

Lincoln, September 3, 1848.

XLIII. *On the Existence of the Colour Brown.*
By Ernst Brücke*.

BROWN is wanting in the prismatic spectrum, and its relation to the colours of the spectrum is as yet unknown. Any one may, however, easily convince himself that brown is nothing more than the complementary colour to that of Herschel's lavender-gray rays, *i. e.* white light from which these rays have been removed.

For this purpose, separate plates should be split from crystallized gypsum in such a manner that on one side they are as thin as possible, and from it gradually increase in thickness in broad terraces. One of these plates is placed under the microscope, which must be furnished with two Nichol's prisms, one beneath the object-glass, and one in the eye-piece, and so arranged, the prisms being parallel, and the linear magnifying power being about twenty diameters (at a distance of eight French inches), that the above-mentioned thin side is in the field. If it is sufficiently thin, no colour is perceived immediately at the side; but as we proceed towards the thicker part, at first a pale brown tint becomes visible, as if we were looking through a very thin plate of horn, and as the thickness of the plate gradually increases in broad and low terraces, the brown continues to become darker until it assumes a deep and pure nut-brown colour, without the intervention of any of the prismatic colours which the thicker parts of the plate exhibit.

It is evident that the plate at the margin where it appears colourless is so thin, that the difference of the path of the ordinary and extraordinary ray on their exit does not amount to half the length of a wave for any colour. Thus interference of the most refractive rays does not occur until the thickness is greater, and the brown colour must therefore be produced by the disappearance of the lavender-gray rays from the compound light.

The correctness of this conclusion is readily tested. On crossing the prisms, it is seen that whilst in the case of all the other colours of the plate the well-known complementary colours appear, that portion which was previously brown becomes coloured lavender-gray, and the intensity of this colour is in proportion to the depth of the brown previously observed at the same spot.

Königsberg, June 11, 1848.

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* From Poggendorff's *Annalen*, No. 7, 1848, having been read before the Physical Society of Berlin on the 23rd of June 1848.

XLIV. *On the Colouring Matters of Madder.*
By James Higgin, *Esq., Manchester*[*].

THE various analyses I have made of madder have led me to the same conclusion as Dr. Schunck, in his recent valuable paper "On the Substances contained in Madder," viz. that there are three colouring matters capable of isolation—xanthine, a yellow; an orange, which he names rubiacine; and alizarine, a red. To this latter alone he ascribes all the tinctorial power of madder, an opinion from which I am compelled to differ; and the object of my present paper is to show that, under proper circumstances, the other two colouring matters have a great influence on its dyeing properties.

I believe the difference in our results to have arisen from the use, in one case, of boiling water to extract the principles, and in the other of cold or tepid water; the temperature having a remarkable influence on the products of extraction.

I will here state the method I employ to separate from each other the three colouring matters; observing that, when separated, they differ little in their properties from those described by Dr. Schunck. I will afterwards show that, as existing in madder, and in the condition in which they are applied in dyeing, peculiar reactions take place, which, as far as I know, have never been described before.

The same method was employed for various sorts of madder, viz. Dutch, French, Turkey, &c., with the same results, varied of course in the amount and proportion of colouring matters.

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Madder
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I put the madder on a conical calico filter, and wash well with boiling water till the liquor comes away only faintly coloured and has no particular taste. I object altogether to boiling madder in water, as I find that substances are obtained by so doing which have no existence in normal madder, being the result of decomposition; and consider the action ought to be as brief as possible. The extract by hot water, when cold, is acidulated with sulphuric acid, which causes a flocculent precipitate, which is collected on a filter. The precipitate contains rubiacine and alizarine, with some pectine. The deep yellow filtrate contains all the xanthine, slight traces of alizarine and rubiacine, some sugar, and the various organic and inorganic salts.

The filtrate is neutralized with carbonate of soda, and a very small quantity of hydrate of alumina added; the mixture is digested at a temperature of 130° F. for about half an hour,

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[*] Communicated by the Author, having been read at the Meeting of the British Association held at Swansea, August 9, 1848.

and then filtered; by this means the traces of rubiacine and alizarine are removed, and the xanthine left, if not too much alumina has been used. To the filtrate is added barytic water to throw down sulphuric and phosphoric acids; and after the separation of the baryta salts, sufficient subacetate of lead to throw down all the xanthine: some chloride and an organic salt are precipitated at the same time. The red lake is washed with a little cold water to remove some chloride of lead, sugar, &c.; it is then diffused in water and sulphuretted hydrogen passed through; the xanthine remains attached to the sulphuret of lead. This is thrown on a filter, and washed with cold water to remove the organic acid; it is then repeatedly boiled in water and filtered. Xanthine dissolves with a rich yellow colour. The solution is evaporated to a syrup in a water-bath, and, if necessary, neutralized with baryta, the evaporation completed, and the dry mass treated with absolute alcohol. Pure xanthine dissolves, and may be again evaporated to dryness.

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As thus obtained it is a dark brown, gummy and deliquescent mass, perfectly soluble in water; the solution is a beautiful yellow when sufficiently dilute. It has a peculiar disagreeable bitter taste, without astringency or sweetness; it is very soluble in alcohol, and but sparingly in æther; soluble in alkalies, with a purplish red colour. Alum throws down from this a dull red lake, which may also be produced by adding a large quantity of hydrate of alumina to the watery solution. Acids make the solution in water lighter in colour, but do not cause any precipitate. When the solution in water is boiled with a little sulphuric or muriatic acid, a green powder falls; this is characteristic of xanthine. When the solution is strong and rendered very acid with either of those acids, it slowly becomes green in the cold. When dry, xanthine dissolves in concentrated sulphuric acid with a fine orange colour; when heated, this becomes more crimson; and on the addition of water all the colouring matter is deposited in yellow flocks, which dissolve easily in ammonia, and give a beautiful crimson colour, much more brilliant than that produced by ammonia with the original xanthine. The flocks, as far as I have examined them, have the properties of rubiacine. The solution in sulphuric acid, heated an hour or so, becomes brown; and on addition of water a brown powder falls, which does not dissolve in ammonia and is not coloured thereby. Xanthine is not precipitated by neutral acetate of lead, but entirely by subacetate; the compound is sparingly soluble in cold water, more so in hot, and very soluble in acetic acid. When heated, xanthine fuses, blackens and swells up into a bulky coal, which burns away without leaving a residue. Tried with mordanted cloth in the usual manner, pure xanthine has little or no effect, merely giving an orange tinge to the alumina.

The precipitate by sulphuric acid from the watery extract of madder is washed, first with water acidulated with sulphuric acid, and afterwards with a little pure water. About an equal bulk of fine chalk is then mixed up with it, and the mixture repeatedly boiled in water and filtered, till the solution, at first dark-coloured, becomes a faint pink only; the mixed solutions are acidulated with sulphuric acid, and the greenish-yellow precipitate collected, washed to remove acid, and dissolved in alcohol; the solution evaporated down to one-fourth of its volume, and an equal measure of water added. The precipitated rubiacine may be again dissolved in hot alcohol, and crystallized therefrom. It has the following properties:—Sparingly soluble in cold water, but more so in hot,—the solution is amber-coloured; very soluble in alcohol and æther. Boiling alum solution dissolves it, and forms a clear orange solution with no shade of pink; it does not precipitate from this solution when cold, unless a large quantity of alizarine be also in solution, when almost all the rubiacine precipitates. Sulphuric acid precipitates it from the alum solution perfectly in greenish-yellow flocks. Its solution in alkalies is a fine crimson, similar to archil. Concentrated sulphuric acid dissolves it readily, and gives a fine orange solution; water precipitates it unchanged. The solution, if heated some time, becomes brown, and water now precipitates a fine brown powder, which has no colouring properties. Rubiacine is less soluble in acidulated than in pure water; it forms a compound with lime of considerable solubility. In solution of sulphate of ammonia made alkaline by ammonia, rubiacine is soluble when heated, a property offering one means of separation from alizarine, which is totally insoluble in that menstruum at any temperature. Rubiacine may be boiled a long time in dilute sulphuric acid without change. Boiling solution of chlorate of potash has no action on it. When heated, rubiacine fuses, blackens, and gives off orange vapours, which condense in a crystalline mass. Mordanted cloth is not dyed, when treated in the usual manner, with rubiacine.

The alizarine is contained in the insoluble chalky matter left after the separation of rubiacine. This is digested with dilute hydrochloric acid at a gentle heat, cooled, filtered, and again treated with dilute acid, then washed on a filter till free from acid; alizarine remains, and may be crystallized from alcohol. It is obtained in much greater quantity from the madder which had been treated with boiling water at first.

This washed madder contains principally alizarine, with a small quantity of rubiacine. It is boiled three or four times with a weak solution of alum to separate the rubiacine; it is next boiled with a saturated solution of alum for a quarter of an hour and filtered, and this process repeated till the madder is perfectly exhausted, and has become an ash-gray; the mixed liquors are cooled to 90° F., and made sharply acid with sulphuric acid; after several hours the alizarine will have separated; the separation may be assisted by violent stirring occasionally. The filtered precipitate may be purified in the same manner as rubiacine.

Alizarine thus obtained is sparingly soluble in cold water, much more so in hot; very soluble in alcohol and æther; very soluble in boiling alum liquor, the solution is a splendid and pure pink; it does not separate much from this solution when cooled, but sulphuric acid precipitates it wholly in a few hours in orange flocks; an alkali added to the alum solution throws down a delicate pink lake. Alizarine is less soluble in water acidulated than in pure; concentrated sulphuric acid dissolves it with a fine red colour; the solution may be strongly heated without decomposition; addition of water precipitates it wholly from the solution. Alizarine dissolved in alcohol, and lime-water added, is thrown down as a dark red compound of alizarine and lime, which is soluble to a small extent in pure water, and may be entirely dissolved by continued washing; but the compound is totally insoluble in water containing lime. Carbonate of soda dissolves alizarine; but the solution, on standing, deposits purple flocks which dissolve in more water; the solution is crimson; ammonia dissolves it very plentifully, and forms a splendid pink solution; carbonate of potash only dissolves it on the application of heat. Alizarine boiled with chalk dissolves some lime, and dyes the chalk red. Alizarine is insoluble in any liquid containing sulphate of ammonia or sulphate of potash, even if strongly alkaline by ammonia; it merely becomes a deep purple powder. This insolubility in boiling alkaline sulphate of ammonia is characteristic of alizarine. When heated, alizarine fuses and sublimes. Mordanted cloth is dyed the ordinary madder shades by alizarine, but much finer than those obtained by madder.

Having described the isolated colouring matters, I will endeavour to show that the action of cold or tepid water on madder is attended with peculiar effects, and that by proper treatment all the xanthine and the greatest part of the rubiacine may be made to disappear, the change being accompanied by an increase in the tinctorial power; and since, of the three colouring matters, alizarine only can be made to dye colours, proving that alizarine and rubiacine result from changes in the xanthine.

When madder is mixed with cold water, stirred three or four minutes, and then filtered through fine calico, a deep reddish-brown liquid is obtained, tasting sweet at first and then disagreeably bitter; a drop let fall upon white calico stains it a deep yellow, in the same way as a solution of pure xanthine does. When the liquid is allowed to stand an hour or two, it becomes gelatinous, if the solution has been strong, and an orange red flocculent precipitate falls; when this is filtered out, the liquor appears to have undergone a change, and is much diminished in intensity of colour. It is now sweet only to the taste, without a trace of the bitterness so remarkable before; and a drop applied to calico stains it only a pale reddish colour, with no shade of yellow. The powder filtered out has a very intense tinctorial power. The filtrate, on acidulation with sulphuric acid, gives more precipitate of the same nature, and remains a pale yellow, similar to the saturated solution of rubiacine and alizarine in a dilute acid. From some of the freshly-filtered solution of madder, xanthine was separated in the following manner:—Subacetate of lead was added to throw down all colouring matter; the lake washed and decomposed by sulphuretted hydrogen, the sulphuret boiled in water several times, the mixed liquors neutralized with ammonia, and a small quantity of hydrate of alumina added; the mixture digested half an hour and filtered from the lake of alizarine and rubiacine; on evaporation to dryness and treatment with alcohol, abundance of xanthine was obtained of the usual properties. The filtrate from the madder liquor which had stood till changed, was examined in like manner for xanthine, but without any being found. The red powder which had separated was dissolved in alcohol, precipitated by subacetate of lead, and the lake decomposed by sulphuretted hydrogen. On boiling the sulphuret in water, an orange liquor was obtained, from which, on cooling, flocks of rubiacine and alizarine fell: the pale orange liquor then evidently contained no xanthine. The sulphuret boiled in alcohol yielded abundance of alizarine and rubiacine.

It was evident from this experiment, that in the madder liquor left to spontaneous action, all the xanthine had become changed into some colouring matter. To see what was the nature of this change, I took some madder liquor quite freshly made in the cold, and added some solution of acetate of lime, which precipitates alizarine. The dark red precipitate of alizarine-lime having been separated, a dark reddish-brown solution remained: part of this was acidulated with hydrochloric acid, when a few yellow flocks fell, which when washed were found to be pure rubiacine. When mordanted cloth was attempted to be dyed with this powder, only the faint shades of rubiacine were obtained. The deep yellow filtrate from the rubiacine, when left to stand several hours, deposited no more flocks, and ultimately became green, as does solution of pure xanthine under the same circumstances.

Another portion of the filtrate from precipitation by acetate of lime was diluted a little with water, and allowed to stand about four hours; it had then lost the intense bitterness of taste, and retained only that of acetate of lime. On acidulation by hydrochloric acid, copious yellow flocks fell, which when filtered out left the filtrate a pale yellow; they were proved to be rubiacine, and had no tinctorial power. The yellow filtrate was examined for xanthine, but only a very small quantity was found.

Another portion of the original filtrate was set aside for about eight hours, when a quantity of reddish-orange powder had subsided, and the supernatant liquor was much diminished in intensity of colour. The filtered precipitate, after treatment with dilute acid and washing, had a very strong tinctorial power; it was dissolved in alcohol, diluted a little with water, and lime-water added; a dark red precipitate appeared, suspended in a red liquid. The filtered precipitate, washed with boiling water and decomposed by hydrochloric acid, was found to be pure alizarine; the filtrate from it, acidulated with hydrochloric acid, gave yellow flocks of rubiacine. The quantity of alizarine in the powder was greater than that of rubiacine; the liquor from which it had subsided was acidulated with hydrochloric acid, when a further quantity of powder was obtained, consisting of both rubiacine and alizarine, but more of the former. The filtrate from it was a pale yellow, and contained no traces of xanthine.

In these experiments, the madder liquor, from which all alizarine had been taken and which had then no power to dye, by spontaneous action acquired a further portion of alizarine, and consequently recovered its power of dyeing. As the xanthine had totally disappeared during the process, the formation of alizarine must have been at its expense. The action is not however direct, as by stopping the process before any precipitation has occurred, only rubiacine is obtained, whilst, as before, xanthine has disappeared. It is evident, then, that the xanthine has become first rubiacine and afterwards alizarine.

This reaction does not result from oxidation, since I have found it to take place equally well *in vacuo*; and as numerous

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experiments on the pure substance with various oxidizing agents failed totally to produce any alizarine, I am induced to consider it the result of fermentation produced by a peculiar azotized matter found in madder, and to be similar to the reaction between starch and diastase, where the starch becomes first dextrine and afterwards sugar.

This action becomes very probable from various experiments, in which it is found that the madder changes are best produced under circumstances favourable to the ordinary action of ferments, and are stopped by such substances as destroy their action.

I have, in the proximate analysis before-mentioned, used boiling water to extract the principles, as by this means all ulterior action is stopped, and the colouring matters are obtained in the same proportion in which they existed in the madder; were cold water used, and the solution allowed to stand a certain time, very little if any xanthine would be obtained, and more than the due proportion of alizarine and rubiacine.

If boiling water be added to madder, the mixture may be allowed to stand any length of time without losing its deep yellow colour and bitter taste. When madder is washed with cold water and the clear filtered solution brought to boil, it becomes turbid, froths up, and is found to be full of small flocculent particles, which when filtered and washed burn with a smell like burning feathers. Boiled with caustic alkali, they evolve abundance of ammonia; the filtrate from them undergoes no change on standing.

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When to a cold solution of madder any acid or any acid salt is added, a flocculent precipitate falls, which is composed of alizarine and rubiacine, pectine and the azotized principle, whilst the xanthine remains in solution, and never changes on standing.

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If to a very strong solution of madder, made in the cold, alcohol be added in sufficient quantity, a gelatinous substance separates, which is partly pectine and partly the nitrogenous matter; if this be filtered out, the liquor never changes; but if left in, and the liquor gently heated to drive off alcohol, as the flocks redissolve, the changes take place as usual.

My endeavours to obtain the pure nitrogenous substance, and prove its power of exciting a change in xanthine and rubiacine, have not yet met with success. I have not been able to obtain it in a soluble, and consequently active condition. My experiments are still in progress, and I give them as far as I can. I made madder into a paste with cold water, expressed strongly, and then added a considerable quantity of alcohol, which threw down a flocculent substance; I washed this well with alcohol to free it from colouring matter which it carries down with it. It is now a brown matter, which, when burnt, emits the smell of burning flesh, and leaves a large quantity of ash; it probably also contains pectine and woody matter, which must inevitably be pressed through the cloth. It is insoluble in water. Boiled with caustic soda, it gives the characteristic smell that all proteine compounds do with an alkali. Nitric acid heated with it forms a yellow mass, which gives to ammonia a red colour; an acid precipitates therefrom a yellow powder, which has the properties of xanthoproteic acid. The mass is easily soluble in slightly alkaline liquors. It appears in every respect to be similar to coagulated vegetable albumen. I have not been able to extract any nitrogenized substance soluble in alcohol from madder, and believe this substance to be the only one containing nitrogen.

I have in the foregoing experiments described the changes undergone in cold water; but, as in all other fermentations, a proper increase of temperature is attended with more rapid results. The temperature I find best is from 120° to 130° F. Water added to madder and the mixture kept at that temperature, rapidly changes in appearance and taste; it gradually becomes redder, loses its bitter taste, and becomes more sweet: the yellow disappears, and the liquid, tested from time to time by dropping on white calico, is found constantly diminishing in intensity of colour. In about half an hour all xanthine has disappeared; and the madder, if examined, is found to contain abundance of rubiacine and alizarine: if it was rich in xanthine, there will be at this stage a preponderance of rubiacine, so much so as to give an orange solution with boiling alum liquor, without much shade of pink. If the fermenting action be continued, the rubiacine is found gradually diminishing, and the alizarine proportionably increasing: the action is complete in about two hours and a half; and if the madder be now tried, only a small quantity of rubiacine is found; and the colour of the alum solution is pink, but not quite so pure as that produced by pure alizarine, as I have never been able to get rid of the last portions of rubiacine by this process; a small fraction of the quantity originally found still remains.

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The quantity of water required for this reaction is small, though it will take place in however dilute a solution. I have frequently done it with perfect success by making madder into a thin paste with water, either heating or not, and adding water to replace that evaporated. If this process be continued too long, the alizarine formed attaches itself to the fibre, and the madder will now give out little colour to water; treatment with hot acid brings it again into a soluble condition.

Good madder contains sufficient ferment to convert an excess of xanthine into available colouring matter. I made several experiments with different quantities, and found 20 per cent. additional to be the greatest quantity madder would convert.

With a view to place the utility of xanthine in dyeing beyond all doubt, I made the following experiments:—

No. 1. Two equal portions of the same madder were taken, and an equal quantity of water at 120° F. added to each; to one, one-tenth of its weight of dry xanthine was added: I kept them at the same heat for half an hour, then put in each an equal quantity of mordanted cloth of the same pattern, and heated gradually on a sand-bath, observing that the heating of the two dyeings progressed equally. At the end of an hour and a half, when the heat was 180° F., I removed the dyed cloth, washed and dried it: the piece dyed with the 10 per cent. additional xanthine was much darker and better dyed than that with madder alone; the whites were equally good.

No. 2. The same experiment was repeated with 20 per cent. additional xanthine with increased advantage.

No. 3. Two equal weights of madder were taken: to one a certain quantity of boiling water was added, and the mixture allowed to cool; to the other portion the same quantity of cold water was added. Two equal quantities of mordanted cloth were then introduced, the temperature being the same in each, and the dyeings made with the same precautions as before. That dyed with the portion treated with cold water was immeasurably superior to the one treated with boiling water, having fine solid colours; those of the other being pale, meagre and dirty-looking. The whites of the cold one were also good; those of the hot very bad. The liquors after dyeing were saved and examined, being filtered from the woody matter; that from the hot portion contained abundance of xanthine, whilst that from the other contained none.

No. 4. The preceding experiment was repeated with a little variation. Lest by the action of boiling water it might have happened that the alizarine was merely thrown into an insoluble condition, and consequently the madder weakened in power, I resolved to convert the residue of each dyeing into garancine, and dye with it some more mordanted cloth; I therefore, after taking out the dyed cloth, added to each the same quantity of sulphuric acid and boiled half an hour, then filtered, and washed each with the same quantity of water. The two garancines were then put into an equal quantity of water, and mordanted cloth dyed in them as before. Now if the alizarine had been merely thrown into an inactive condition by the boiling water, the converting of the residue into garancine would certainly have restored its activity, and the dyeing made with that garancine would have been superior to the other; but, on the contrary, the second results were not even equal, the same remarkable difference occurring as in the first instance. This shows that in the one case the tinctorial power was dependent on the alizarine existing normally in the madder; in the other, that the xanthine had contributed a further portion.

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No. 5. Two equal portions of madder were again taken: to one a certain quantity of water at 120° F. was added, and the heat continued for half an hour; a certain quantity of dilute sulphuric acid then added, the mixture filtered, and the mass washed with a known quantity of dilute sulphuric acid, and afterwards with a known quantity of water.

The other portion was mixed with the same quantity of water and dilute acid as the former, filtered, and washed with the same quantity of dilute acid and water. The two portions thus treated were used to dye equal quantities of mordanted cloth, with the usual precautions. The one which was subjected to spontaneous action previous to washing with acid, dyed full and rich shades; that from which the xanthine had been washed out unchanged, dyed shades very many times weaker.

In this experiment the two madders were under exactly equal circumstances, except that one of them was allowed to ferment the xanthine; yet the difference in result was very great, and could only arise from the xanthine becoming available colouring matter. I consider these five experiments as clearly demonstrating the value of xanthine in madder-dyeing.

The madder used in this case was "ombro," a variety very rich in xanthine, and therefore the best to test its value.

With this theory of madder, the operation of dyeing becomes much more intelligible. It is known that, to dye well with madder, the process must be begun at a low temperature and the heat gradually raised. The application of the theory will be this. The xanthine, on immersing the madder in water, immediately begins to decompose, becoming rubiacine; this in its turn becomes alizarine, and then the combination between it and the mordanted cloth takes place. The dyeing is begun by the alizarine already existing in the madder, and continued by the quantity continually formed. The slow heating of the liquor is very favourable to the change taking place. Generally the temperature is about 130° at the end of the first hour; long before that, however, the xanthine has become rubiacine and part of this alizarine, the remainder of the time being occupied in completing the change; the alizarine, being removed from solution as fast as formed, has probably some influence in accelerating the change of rubiacine. This will be the final action; and when the mordants are saturated, there will always be a small residue of rubiacine, which has a tendency to dull the shades; and hence the superior brightness of those dyed with garancine, which contains no rubiacine. Examination of the residual dye liquor generally shows a small quantity of rubiacine, which gives it an opaline appearance; there is also always alizarine united to woody fibre and lime. Examination of the dyed cloth, of whatever colour, gives only alizarine, as was proved by Schunck.

It is found advantageous sometimes to mix a lower quality of madder with a superior; the effect produced not being a mean of those resulting separately, but not much inferior to that produced by the superior madder alone. The low quality abounding in xanthine, has probably more than can be readily converted in the required time; but as we have seen that good madder can readily convert more than its own quantity, the change goes on nearly as uniformly as if good madder alone were used.

The fermentation and improvement of madder, when kept in the cask, is readily explained by supposing the xanthine gradually to change into alizarine: this is particularly noticed in Dutch madders, which abound in xanthine. Many substances and salts, added to a madder dyeing, weaken its influence very much. An examination of the residual liquor uniformly shows the xanthine remaining unchanged, and the dyed effect has only been produced by the quantity of alizarine originally in the madder.

Madder contains a varying quantity of sugar of an uncrystallizable species, which may be isolated in the following manner:—To the madder solution left after throwing down xanthine, in the proximate analysis before-mentioned, is added sufficient dilute sulphuric acid to throw down any lead that may be in excess, then evaporated to dryness, the sugar dissolved in alcohol, decolorized by animal charcoal, and again evaporated to dryness: if the evaporation be too fast, the resulting sugar is coloured slightly; but if a gentle water-bath heat has been used, it is quite colourless.

I do not think the resins found by Dr. Schunck are originally contained in madder, but have been formed during the boiling. I have never in my experiments on the products obtained at a low temperature, or brief boiling temperature, met with any resinous matter; with a view further to test this, I boiled madder in alcohol repeatedly, till it became an ash-gray, and no more colouring matter was given to the alcohol: the solutions were mixed. Dilution with water in any proportion did not produce any milkiness, even after standing some time, as must have been the case had any resins been dissolved by the alcohol. The solution, added to alum liquor and boiled, gave a perfectly clear solution, without any resinous matter being rejected. The alcoholic solution, evaporated to dryness in contact with air, and again treated with alcohol, left a brown substance, insoluble in boiling alcohol, ammonia, and caustic soda; some more was left on evaporating the second solution to dryness, and treating with alcohol. The powder seemed formed at the expense of the xanthine, as on examination very little could be found in the third solution; this was acidulated with sulphuric acid and diluted with water, and the resulting flocks washed and boiled in alum liquor. This time a small quantity of resinous matter was rejected, which could not have been in the original solution.

This experiment also proves that the colouring matter of madder is all free, none being united to lime, as it is capable of being so perfectly exhausted by alcohol as to leave only an ash-gray powder. The compound of alizarine with lime is perfectly insoluble in boiling alcohol. Boiling alum solution also decolorizes madder more rapidly and better than alcohol. If, however, madder be boiled in water, the colouring matter cannot afterwards be wholly separated by alcohol; a large quantity remains in a state not soluble in water or alcohol; this results partly from alizarine becoming united to lime during the boiling, and partly from the woody fibre acting as a mordant, and withdrawing alizarine from solution; woody fibre having a great affinity for that colouring matter, and when introduced into its boiling solution becoming dyed a dark red. I have sometimes found that, after all lime-salts have been removed from madder by an acid, if dried at too high a temperature, very little colouring matter is given to water, and mordanted cloth receives very faint shades of colour; the madder in this condition is a deep red; hot acid separates alizarine again from the fibre. This fact explains why garancine is spoilt by too high drying.

Having thus detailed my experiments on normal madder, I will briefly describe those which I made on that preparation of it called garancine.

I find it to contain only one colouring matter, alizarine, and to be remarkably free from rubiacine, xanthine, pectine, and the nitrogenous matter. In no way have I been able to separate any of these substances from the specimens I have tried. The peculiar utility of garancine consists in all substances being removed which have an injurious influence on the action of alizarine. From the experiments of Schunck it appears, that any of these substances, when used in conjunction with pure alizarine, injure the dyed colour and stain the whites. In the making of garancine, they are rendered inert, as the following experiments will attest.

1. I took xanthine, dissolved it in a little sulphuric acid, and heated the solution for some time. From being a bright orange, it gradually became a dark brown; when diluted with water, a brown powder separated, which, when dry, was insoluble in any menstruum except sulphuric acid.

2. I treated rubiacine in the same way, and obtained exactly the same results.

3. Alizarine treated the same way was not decomposed even by long heating, but remained as a rich red solution, from which water threw down all colouring matter.

4. I then made a mixture of alizarine, rubiacine, and xanthine, dissolved in sulphuric acid, and heated some time. The solution gradually became dark brown; and on addition of water a brown powder fell, which when washed and dried, was a soft velvety substance, possessing an intense tinctorial power; hot alcohol separated all the alizarine and left the dark brown powder obtained from xanthine and rubiacine. This compound of alizarine and the brown matter may be considered as pure garancine freed from woody fibre.

As to the greater proportional dyeing power of garancine over madder, I coincide in the opinion of Dr. Schunck, that the lime-salts being removed, no alizarine can remain combined with lime, and that in garancine dyeing all colouring matter is used up, whilst in madder only two-thirds are used.

The foregoing series of experiments, though perhaps not possessing the completeness they might have, warrant us, I think, in drawing the conclusion, that all the colouring matter in madder springs primarily from xanthine, an opinion held by Decaisne, who observed that fresh madder roots contained only a deep yellow fluid, and acquired a granular appearance only during the process of drying.

XLV. *Notice of a colourless Atmospheric Arch seen in the interior of Brazil.* *By* William Jory Henwood, *Esq., F.R.S., F.G.S., Chief Commissioner of the Gongo Soco and Bananal Gold Mines, &c. &c.*

To Sir David Brewster, K.H., LL.D., F.R.S.L. & E., &c.

My dear Sir David,

THE village of Cattas Altas is situated in long. 43° 15′ west, and lat. 19° 58′ 30″ south, on an open undulating expanse of pasture land, about 3500 feet above the sea, bounded on the south-west by the mountain-chain of the Caraças, which rises from 4000 to 5000 feet above it, a deep narrow glen intervening between them.

At about seven o'clock in the morning of the 12th of May last*, I had occasion to cross this open country towards the north-west, almost on the edge of the ravine. A dense mist covered all the lower grounds, whilst the little hills shone in unclouded sun-light; in the fog a light air from the westward was perceptible, but a gentle breeze in an opposite direction prevailed on the hills.

During my journey I passed several times from sunshine into mist, and *vice versâ*.

Whilst immersed in fog on the verge of the vale, and some 400 feet above its bottom, an arch of about forty-five degrees in altitude became visible. In width, and indeed in every other respect, it exactly resembled a rainbow, except that the whole of its upper part was entirely colourless, being, as it were, a bow of denser mist surrounded by the ordinary fog. For an altitude of about ten degrees, however, of the lower portion, which was beneath the horizon of my station, it had the faintest possible tint of violet colour, which was rather more perceptible in the south-eastern extremity than in the other.

On entering the mist a second time, a few minutes later, I observed a similar, but fainter and less perfect arch.

My isolated situation affords me access to but few books of reference; and as but one of these mentions a colourless rainbow†, and as I have never before seen one myself, I venture to trouble you with this brief notice of a phænomenon which perhaps may not be of frequent occurrence.

I have the honour to remain,
My dear Sir David,
Your most faithful, humble Servant,
W. J. Henwood.

Gongo Soco Gold Mines, Brazil,
June 3, 1848.

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* At Gongo Soco, about sixteen miles off, the temperature at 6 a.m. was 51°, and at 9 a.m. 63°.

† *Encyclop. Brit.*, 6th edit. (1823), article "*rainbow.*"

XLVI. *Notices respecting New Books.*

Results of Astronomical Observations made during the years 1834, 1835, 1836, 1837, 1838, *at the Cape of Good Hope; being the Completion of a Telescopic Survey of the whole Surface of the visible Heavens, commenced in* 1825. *By* Sir JOHN F. W. HERSCHEL, *Bart., K.H., &c. &c.* London: Smith, Elder and Co. 1847. (*Fifth and concluding notice.*)

Chap. V. *Observations of Halley's Comet, with Remarks on its physical condition, and that of Comets in general.*

THE comet was first observed at the Royal Observatory at the Cape on the morning of the 1st of September (1835), but it was not before the 28th of October that Sir John Herschel obtained a view of it. About the end of March, previously, he had carefully but without success examined the places set down in M. Rumker's ephemeris, and the whole adjacent region of the heaven; nor was he more fortunate when he resumed the search about the end of August, by sweeping over the places given in the Nautical Almanac for that year. On these occasions his efforts were uniformly baffled by a drift of light vapour hurried rapidly along by the south-east wind of the season, but constantly forming anew nearly over the spot to which the telescope was directed. At length, despairing of success at home, and impatient of repeated disappointments, he dismounted the equatorial telescope, and having constructed for it a temporary stand, transported it to a station on the Cape Flats, five or six miles to the eastward of Feldhausen, where he was sheltered by a range of low sand-hills from the violence of the wind, and freed by distance from the obstruction produced by the Table Mountain with its superposed mass of cloud. The instrument was erected about sunset on the 28th of October, and before the termination of the twilight he had obtained an excellent view of the comet.

The appearance of the comet in the 7-foot achromatic on this night was "that of a nebula very suddenly and highly condensed in the centre, to so great a degree indeed that I could not have ventured from this observation alone to have denied the existence of a solid nucleus. No phase, however, was perceptible on that central mass which might have been so regarded. Neither was any other remarkable peculiarity about the head, nucleus, or tail, noticed on this occasion." On the following evening, which proved very clear at Feldhausen, the 20-foot reflector, armed with an excellent mirror recently and brilliantly polished for the purpose, was brought to bear on the comet. "Under these circumstances its appearance was most singular. . . . . . Its nucleus, small, bright, and highly condensed, was shielded or capped on the side next the sun by a vivid but narrow crescent of nebulous light, the front of which presented an outline nearly circular, having an amplitude of somewhat more than 90° from horn to horn." Within this was the nucleus, the horns of the crescent extending a good way behind it on either side. "The nucleus was decidedly not planetary, and as decidedly exhibited no phase. As the evening advanced, the exterior strata of the crescent —the coma and the tail which were before obliterated by the twilight —became visible. The tail, however, was never very apparent." On the 30th, no observation was made at Feldhausen. On the 31st the general appearance of the comet was not materially different, but the crescent was less remarkable than on the 29th, and on the following evening still less so. After this the comet was not again seen in the 20-foot reflector till its return from the sun after the perihelion passage.

After the passage the comet was first seen at the Cape on the 25th of January, and from this time till the 5th of May it was very frequently observed. On its reappearance it offered to the naked eye "the aspect of a star, a very little dim and hazy, but no tail was visible, neither was any seen in the night glass, in which its appearance was that of a highly condensed globular nebula. In the equatorial it appeared as a bright, round, and very nearly uniform nebulous disc, little condensed towards the centre, but decidedly more sharply defined on the following than on the preceding side. . . . No trace of a tail was observed in this instrument, but a considerable coma enveloped the disc, extending to nearly an equal distance on all sides." For some days trees prevented its being taken up by the great reflector; but from the time this instrument could be brought to bear on it, the observations assume an extraordinary interest. The following extract conveys a vivid impression of its general appearance:—

"Viewed through the 20-foot reflector (which on this as on the former occasion I had taken care to provide with a mirror freshly polished and in the highest possible condition), the comet was now indeed a most singular and remarkable object. Certainly nothing could be more surprising than the total change which had taken place in its appearance since the observations of October. Not to speak of the extraordinary sharpness of termination of the head, especially towards the following side, for which I can find no comparison so apt as the ground glass of an Argand lamp, a new and perfectly unexpected phænomenon had presented itself, a phænomenon, I believe, quite unique in the history of comets. Within the well-defined head, and somewhat excentrically placed (being rather nearer to its northern than to its southern edge), was seen a vividly luminous nucleus, or rather an object which I know no better way to describe than by calling it a miniature comet having a nucleus, head, and tail of its own, perfectly distinct and considerably exceeding in intensity of light, the nebulous disc or envelope which I have above called the 'head.' As the comet gained altitude, a minute bright point like a very small star, or rather like the nucleus of the nebula in Andromeda, only smaller, came to be distinctly perceived. It is this point which I shall henceforward term the nucleus, and which, according to the state of the atmosphere at the time of the observation (and perhaps also that of the comet itself), was subsequently seen sometimes more, sometimes less sharply concentrated, and stellar—but always of excessively minute dimension, certainly on no occasion subtending an angle of 4″, and never quite so well-defined as to give a positive assurance of the existence of a solid sphere—much less could any phase at any time be discerned on it. The whole was encircled with a strong *coma* which nearly filled the field of view (15″ diameter), dying away insensibly but rapidly at the edges. When the nucleus was brought to any part of the circumference of the field, the border of the coma fell little short of the centre. On the south preceding side, however, its extent was somewhat greater, projecting in that quarter into a feeble and irregular appendage, which might be regarded as a rudimentary tail."—P. 397.

The changes which the comet underwent in its figure and dimensions, as also in respect of the relative degrees of illumination of its different parts, were extremely remarkable, and are of great importance in reference to the study of its physical constitution. On the 25th of January some measures were taken with the equatorial. The diameter of the head in declination was found to be 237″·3; and upon the measure being repeated about two hours and a quarter after it was found to be 252″·0, or about 14″·7 greater. Such a difference in so short an interval of time, and in opposition to the tendency of the morning twilight, " seemed inconceivable, and threw a momentary doubt on the exactness of the earlier observation. That measure had, however, been taken and read off with all possible care, and in short nothing remained but the conclusion, *that the change was real, and that the comet was actually increasing in dimensions with such rapidity that it might almost be said to be seen to grow*!

"On the next night, the 26th, the increase of dimension of the comet was evident at the first glance. Its form, too, had undergone a change, being more elongated in proportion in the direction of its axis, and materially less definite in its posterior region. . . . . . The coma had also undergone a great and remarkable change, not so much in dimension as in brightness; being very much enfeebled. . . . The head or envelope had begun to assume an evident tendency to a paraboloidal form, the posterior portion being terminated, however, by an outline of an oval or nearly semicircular figure. The nucleus appeared as a star about the 11th magnitude, not, however, sharp and sparkling like a true star, but furred and nebulous, so that, with its ray, it presented exactly the appearance of a miniature comet, having a head and tail of its own."—P. 399.

On the 27th of January, the head or envelope, with its enclosed nucleus or ray, was still more dilated than on the preceding night, but the coma was grown extremely thin and faint. On the 28th the coma was all but gone, but there were long irregular nebulous tails in various directions. The nucleus was no longer a dim misty speck but a brilliant point. The observations of this night conclude with the following memorandum:—" so ended a most memorable night. I can hardly doubt that the comet was fairly evaporated in perihelio by the heat, and resolved into transparent vapour, and is now in process of rapid condensation and re-precipitation on the nucleus."

The 29th and 30th proved cloudy. On the 31st the comet was so much dilated as to more than fill the field of view in length, and nearly so in breadth, of the great reflector. Every trace of coma had disappeared. "The clear parabolic outline of the head was seen free from any mistiness or vaporous appearance, in its full beauty; and certainly a more delicately formed and soft, yet well-defined, outline than it presented on this night could not be imagined. . . . . . All idea of vagueness or confusion connected with an ill-defined and fluctuating coma being now absent, an impression of regularity of structure, and of precise and definite laws obeyed in its constitution, was no less strongly conveyed by this view of it, than by that of the body of Jupiter or Saturn. From this time may be dated the commencement of the development of the true tail. . . . . The coma from this time appeared no more, but in the progress of the comet towards its final extinction the semblance of a new coma arose from the dilatation of the mass of internal light immediately surrounding the nucleus, after the final dissipation of the envelope, and which, from being at first a subordinate and inconspicuous part of the phænomenon, became by degrees a leading feature, and at last may be said to have constituted the whole visible comet, the infinitely minute and hardly perceptible nucleus excepted."—P. 401.

With respect to the small density of the comet, as indicated by its almost inappreciable effect in extinguishing the light of small stars seen through it, we have the following remark:—"I may here mention, once for all, that among the innumerable stars of all magnitudes, from the ninth downwards, which at various times were seen through it, and some extremely near to the nucleus (though none *exactly on it*), there never appeared the least ground for presuming any extinction of their light in traversing it. Very minute stars, indeed, on entering its brighter portions, were obliterated, as they would have been by an equal illumination of the field of view; but stars which before their entry appeared bright enough to bear that illumination, were in no case, so far as I could judge, affected to a greater extent than they would have been by so much lamp-light artificially introduced."

From the 1st to the 11th of February the observations with the reflector were suspended by reason of the moon approaching and passing the place of the comet. On the latter day the comet was a superb object, but grown much too great for the grasp of the telescope with its ordinary sweeping power. The nucleus was then the most conspicuous part. On the 17th it was "dilated to a vast size and no longer *sharply* defined, but yet the outline well made out, the nucleus pretty sharp, all but stellar, and much *brighter than ever seen before*, but the interior cometic tail hardly perceptibly brighter than the general tail. On the 19th the nucleus and *its* coma very bright and highly condensed. The parabolic envelope hardly to be made out, and evidently distorted. . . . . From this time forward the envelope continued still to dilate, being, however, worse defined and fainter, till at length, on the 18th of March, all trace of a visible outline had disappeared."—P. 402.

The last recorded observation of the comet was made on the 5th of May, on which day it was still visible, though with great difficulty, in the equatorial. Cloudy nights prevented further operations till the 11th, when the comet was searched for but not found. On the 12th another attempt was made, and a faint object was discovered which was supposed to be the comet, and consequently prevented further search; but on the next night the object was again seen in the same precise situation, and proved to be a small nebula not previously known. "Clouds prevented all further search on that and several subsequent nights, and when at length the weather became favourable, no trace of the comet could be found after a long and persevering search. Thus," says Sir John, "by a most inopportune discovery of a nebula, otherwise interesting enough, I found myself deprived of the opportunity I should doubtless have otherwise enjoyed, of accompanying this remote denizen of our system a few steps further on its journey."—P. 398.

After describing the successive appearances presented by the comet (of which he has given a series of beautiful drawings), Sir John proceeds to make some observations on its physical state as indicated by the changes it underwent. "Four things," he remarks, "in the post-perihelion history of this comet are especially remarkable:—1st, the astonishingly rapid dilatation of the visible dimensions; 2nd, the preservation of the same geometrical form of the dilated and dilating envelope; 3rd, the rapid disappearance of the coma; and, lastly, the increase in density and relative brightness of the nucleus." With respect to the first of these, a synoptic table is given of the measures of its several parts, taken from January 25 to February 11, upon which he remarks, "The only element in these measurements which is sufficiently definite and comparable throughout the series and at the same time little affected by the unequal lights of the telescope used, is the distance of the nucleus from the vertex of the paraboloid." Calculating from this element, a series of values was found of "the apparent and real bulk of that definite segment of the paraboloid which may be conceived cut off at its vertex by a plane at right angles to its axis passing through its nucleus." The real volumes were as follows:—

Jan. 25	1·000	Jan. 28	5·179	Feb. 1	12·242
... 26	1·789	... 30	9·314	... 2	16·953
... 27	4·089	... 31	10·672	... 4	22·343
				... 12	74·303

From this it appears that the definite segment in question had actually enlarged to seventy-four times its original bulk in the interval of seventeen days between the extreme observations. The total volume, however, of the whole visible paraboloid had increased in a much greater proportion. By comparison of the different measurements, it was found that *the apparent linear dilatation* of the envelope was nearly uniform, and at the rate of 21″ a day on the vertical distance. Calculating backwards on this hypothesis, the singular conclusion is obtained that "on the 21st of January at 0^{h} 10^{m} P.M. *the envelope had no magnitude*; that in short at that moment a most important physical change commenced in the comet's state. Previous to that instant it must have consisted of a mere nucleus, a stellar point more or less bright, and a coma more or less dense and extensive. At that instant the formation of the envelope commenced, and continued in the manner and at the rate above described. The *Ray*, or internal comet, no doubt, also commenced its existence at the same instant." This result was corroborated by an observation made at Breslau by Prof. Boguslawski, who on the 22nd of January observed the comet, as a star of the 6th magnitude, a bright concentrated point which showed no disc with a magnifying power of 140.

Sir John next proceeds to examine the hypotheses respecting the physical constitution of the comet which the phænomena render probable. He observes, "It will not be necessary to enter into any calculation of the comet's distance from the sun at the moment of these observations to warrant our rejection of an hypothesis such as that of M. Valz, which makes the volume depend *directly* on the distance. Undoubtedly its recess from the sun is *ultimately*, but indirectly, the cause of the observed increase. The question is only as to the *modus operandi*. The perihelion passage, it will be borne in mind, took place on the 15th of November, and it was not therefore till the 83rd day after that event that the formation of the envelope commenced. Now during these 83 days it is certain that a process of refrigeration had been going on. . . . . The surface of the nucleus must be supposed to have cooled down, at this precise epoch, to *the dew-point*. . . . At this instant, therefore, must have commenced the formation of a mist, first precisely in contact with the radiating surface, and by degrees more or less rapid ascending above it, and limited by a definite upper surface, the seat of a fresh process of radiation. As this *superior vapour plane* attains a higher and higher level, corresponding to a less and less actual density both of the atmosphere and vapour, its surface, without losing its geometrical form, necessarily becomes less and less visible and conspicuous. . . . . It is this superior vapour plane which constitutes, in our conception of the matter, the visible surface of the envelope."

In reference to the form of the envelope, he says, "If the condensible vapour were everywhere distributed spherically around the nucleus, so as to have equal density at equal distances, the form of the envelope would necessarily be spherical, because on account of the exceeding tenuity of the cometic atmosphere, its posterior portion is in no way shaded by its anterior, but every part, interior and exterior, equally exposed to the sun's rays, or very nearly so. That the form is in fact not spherical, but paraboloidal, or very eccentrically elliptic, instructs us in an important and truly wonderful fact, viz. that *the surfaces of equilibrium of the vapour in its transparent state* are so; that, in fact, *although not seen the envelope existed* as a transparent atmosphere—a tail *in posse*, though not to our eyes *in esse*, and of its full extent, prior to the epoch of its commencing visibility, and probably prior to the parhelion passage."

He next proceeds to inquire by what forces it is possible that such a form of equilibrium can be maintained, and remarks that the laws of gravitation, as at present recognised, are altogether insufficient to account for it. "Such a form, as one of equilibrium, is inconceivable without the admission of repulsive as well as of attractive forces. But, if we admit the matter of the tail to be at once repelled by the sun, and attracted by the nucleus, it no longer presents any difficulty."

After some further remarks in corroboration of this view, and citing the opinions of Newton and Kepler, he adds—"Supposing the approach of a comet to the sun to be such as to enable the repulsive force to overcome the attractive in those portions of its tail remote from the nucleus, they would, of course, be driven off irrecoverably. But what would now be the state of the remaining mass? Inertia and repulsion have been subtracted; of course, what is left has become *pro quantitate materiæ*, on both accounts more *attractively disposed* as a whole; the dimensions of the orbit must therefore contract. The periodic time will diminish. We have here presented to us a series of consequences, identical in some of their leading features with those which, observed in the case of Encke's comet, have been attributed to resistance of the æther. Pursued into all their consequences no doubt the two explanations diverge. . . . But it may be doubted whether observation of a body so ill-defined is yet precise enough to decide between them."—P. 410.

We can only make room for two further extracts:—

"The preservation of the geometrical form of the envelope of our comet is undoubtedly indicative of a high degree of tranquillity, the definite action of perfectly regular forces, and at least a near approach to a state of equilibrium in the strata of which the mass consists. It proves also, if taken in conjunction with what has been said respecting its apparent dilatation, similarity of external and internal strata, precisely such as the laws of equilibrium would lead us to expect in an elastic fluid mass subjected to such forces.

"The rapid disappearance of the coma would seem to be referable neither to dissipation nor absorption into the head, but rather to its being swept off into the tail by the sun's action, and there deposited as part thereof. . . . . The ray or tail of the nucleus is also a highly instructive phænomenon. As the envelope dilated and grew fainter, this, on the contrary, while also dilating at the same rate, grew more intense up to the 1st of February, after which time it faded, while the nucleus with *its* coma rapidly increased in comparative brightness. It seems hardly possible not to recognise in these changes the effect of the gradual deposition of the matter of the envelope; choosing the line of its axis as the course of the deposited particles in their progress to rejoin the nucleus."—P. 411.

From the above extracts the reader will be enabled to form an opinion of the manner in which this very interesting portion of the work is treated. At the next return of the comet, it will be regarded as a fortunate event for astronomy that the time of Sir John Herschel's sojourn at the Cape embraced that of its last appearance, and that the most powerful optical means then in existence, in the hands of the first astronomer and philosopher of the age, were applied to the observation of phænomena so interesting in themselves, and so important in reference to the light which they throw on the constitution of the solar system.

Chap. VI. *Observations of the Satellites of Saturn.*

"During the years 1835, 1836 and 1837, the ring of Saturn and the orbits of the satellites were sufficiently open to permit the accumulation of a considerably extensive series of their angles of position with the meridian, or with the longer axis of the ring, so disposed as to be available towards the more exact determination of their orbits; of which little is known with the exception of that of the sixth, which is the largest and most easily observable, and whose elements and perturbations have been made the subject of elaborate investigation by Bessel." The author therefore considers that his observations of those bodies, "though confessedly imperfect, owing to their extreme faintness in the equatorial, and the difficulty of obtaining any measures at all of position with the 20-foot reflector out of the meridian, may not be wholly without interest as a contribution to their theory."

The number of known satellites belonging to Saturn is seven,—of which one was discovered by Huygens, four by D. Cassini, and the remaining two by Sir William Herschel. The two last are extremely small and faint; and so long was it before the observations of the discoverer were confirmed, that some astronomers began even to entertain doubts of their existence. Such, indeed, is their extreme minuteness, that it is only under the most favourable circumstances, and with telescopes of extraordinary power, that they can be perceived. The three which are most remote from the planet may be seen with telescopes of moderate power. "Measures of the fourth satellite could rarely be procured so as to be in any degree satisfactory with the equatorial—of the third, never. . . . . As to the two interior satellites, it is needless to state that no glimpse of either of them was ever obtained with this instrument." In fact, the one nearest the planet was only observed once in the 20-foot reflector.

The satellites of Saturn had hitherto been distinguished merely by the ordinal numbers, first, second, third, and so on; and it is the usual, though not uniform practice, to begin with the satellite nearest the central body and reckon outwards. But the discovery of the two interior satellites necessarily deranged the nomenclature, the one which had been previously reckoned the first now becoming the third. Some astronomers have also adopted the contrary order, and begun the numeration with the most remote satellite. Sir John states, that having found the equivoque practically annoying, and a source of frequent error and mistake, he had recourse to the practice of distinguishing them by individual names, adopting a mythological nomenclature, which, however, he does not "pretend to recommend to others, though persuaded that *some* nomenclature other than the equivocal one in actual use will be found necessary by all who observe these bodies." The names which he selected are the following, beginning with the most remote, and proceeding *downwards* in order:

Iapetus, Titan, Rhea, Dione, Tethys, Enceladus, Mimas. The reasons for the selection are set forth in a note which we transcribe at length.

"As Saturn devoured his children, his family could not be assembled round him, so that the choice lay among his brothers and sisters, the Titans and Titanesses. The name Iapetus seemed indicated by the obscurity and remoteness of the exterior satellite, Titan by the superior size of the Huygenian, while the three female appellatives class together the three intermediate Cassinian satellites. The minute interior ones seemed appropriately characterized by a return to male appellatives, chosen from a younger and inferior (though still superhuman) brood. Should an eighth satellite exist, the confusion of the old nomenclature will become quite intolerable. I am not aware that a distant satellite of Jupiter (analogous to Iapetus and our moon) has ever been *looked for*. Would it not be worth a search?*"—P. 415.

The contents of the present chapter may be briefly described. The author first gives some explanations respecting the mode in which the observations were made. A table follows, exhibiting a synopsis of all the measured (and some of the estimated) angles of position of the satellites as taken both with the equatorial and reflector. In order to render the observations available for the purpose of affording epochs by which to correct the received periodic times, the observed angles of position must be converted into Saturnicentric longitudes. Formulæ and elements for this purpose are given; but as the use of the elements supposes the orbits of the satellites to be coincident with the plane of the ring, they are not applicable to the reduction of the observations of Iapetus, the orbit of which deviates from that plane very considerably. Of Mimas, there being only a single observation, no conclusion could be deduced respecting the orbit. In respect of the remaining five satellites, tables are given exhibiting the Saturnicentric longitudes from the descending node of the ring, calculated from the elements and formulæ above referred to for each of the times of observation, and for each satellite drawn out *seriatim*. These *observed* longitudes are then compared with the corresponding *mean* longitudes, calculated from certain assumed epochs of longitude and mean motions, and the differences given. In most cases, where the differences are considerable, the observations were found to have been recorded as doubtful or unsatisfactory, and some of them are therefore rejected. In order to deduce elements, the observations were treated by a method of graphical projection, and the results gave values of the eccentricity, the equation of centre, the perisaturnium, and correction of epochs for four of the satellites, viz. Titan, Rhea, Dione, Tethys. For Enceladus the correction of the epoch only was obtained.

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* It may not be out of place to remind the astronomical reader that mythological names were long ago proposed for the satellites of Jupiter by Simon Marius—the same who disputed with Galileo the honour of their discovery. The names proposed by Marius were those of Jupiter's cupbearers—*Io*, *Europa*, *Ganymede* and *Callisto*.

For the determination of the mean distances or major semiaxes of the orbits, measures at or near the greatest elongations can only be advantageously employed. In the course of the observations a good many measures of distance were taken; but by reason of the want of symmetry of the upper and lower portions of the outline of the visible part of the planet, which renders it extremely difficult to fix upon the position of the apparent centre, they were found (excepting in situations very near the line of the *ansæ*) not only to present discordances among themselves, but to be affected by a general cause of error. The measures of distance are therefore made use of in the case of Titan only, and the resulting value of the semiaxis of the orbit is $A = 177''{\cdot}53$.

The author states that he had no confidence in any of the distances of Rhea measured with the equatorial micrometer, the faintness of the object not permitting it to be satisfactorily covered with the wire. They are accordingly not stated. In the case of Iapetus there was less difficulty; and though the measures were not numerous, and were for the most part taken merely for the purpose of identifying the satellite, it was thought they might be usefully recorded. One of them appears to have been an observation of the greatest elongation, and amounted to $553''{\cdot}93$, which, reduced to the mean distance of Saturn, corresponds to a radius vector of $8'\ 38''{\cdot}07$. Considering that we have no knowledge of the eccentricity of the orbit, this agrees sufficiently well with the mean elongation ($8'\ 34''{\cdot}8$) calculated from the distance of Titan by comparison of their mean motions.

Chap. VII. *Observations of the Solar Spots.*

"At the latter end of 1836, and during the first half of 1837, the spots on the surface of the sun were extremely remarkable, not only for their number and size, but also in their arrangement and forms. In consequence, during the interval above-mentioned, a great number of drawings were made of the sun's disc by projecting the image formed in the focus of an achromatic finder attached to the equatorial, for which, after a few trials, a perspective day telescope of 20 inches focus, and 1·4 inch aperture, was substituted as more convenient. The image was received on paper pinned on a screen of wood, and traced with pencil with one hand, the other managing the right ascension handle so as to keep the preceding limb of the sun on a fiducial line, previously drawn on the paper, and the centre of some small and well-defined spot on a fiducial dot. When the right ascension motion was allowed to rest, the image of this spot of course travelled away from the dot, and after allowing it to do so till near the edge of the paper, another dot was made, marking its new place, and these two dots being joined by a straight line, gave the direction of the diurnal parallel on the paper. The minuter details necessary to effect a complete resemblance of the projection to the actual appearance of the spots, with their penumbras, &c., were then worked in with the aid of the telescope, by hand, as an eye-draft; as well as magnified representations of remarkable spots, faculæ, and other particulars."

The above description may be found useful to some who are desirous of making projections of the solar spots. Sir John states that he did not consider it worth while to engrave and publish all the drawings made by him, but he gives a list of the days on which they were made, "in case it should ever be considered necessary to appeal to any of them as records of the state of the sun's surface at that time;" and he observes, it is much to be wished that all who habitually make such drawings would at least place the days for which they possess them on record, in order that it may be rendered *possible* to ascertain the state of the sun by actual observation on any given day.

After giving some further particulars respecting the mode of delineating the spots, he calls attention to the following points shown by the drawings as characteristic or remarkable:—1st, to certain forms frequently reproduced on the sun's surface; 2ndly, to the remarkable radiated or striated apparent structure of the penumbra in certain cases; 3rdly, to the total absence of all penumbra in some spots; 4thly, to the occurrence of distinct shades in certain parts of some penumbræ; and lastly, to the immense area occupied by some of the spots with their penumbræ represented in the drawings. As instances of enormous magnitude, he mentions that the spot observed on March 29 occupies an area of nearly five square minutes; and as a square minute on the sun corresponds to 756,000,000 square miles, "we have here an area of 3,780,000,000 square miles included in one vast region of disturbance, and this requires to be increased for the effect of foreshortening. The black spot of May 25 would have allowed the globe of the earth to drop through it, leaving a thousand miles clear of contact on all sides of that tremendous gulph."—P. 432.

He next proceeds to consider a question which may be regarded as the most interesting which can be proposed in connexion with the subject, namely, "to inquire for an efficient cause—for a *vis motrix*—to give rise to such enormous dynamical phænomena, for such they undoubtedly are;" and after remarking that the cause of the movements which we observe must reside within the sun itself, and must be there sought for, he thus continues:—

"Whatever may be the physical cause of the spots, one thing is certain, that they have an intimate connexion with the rotation of the sun on its axis. The absence of spots in the polar regions of the sun, and their confinement to two zones extending to about 35° latitude on either side, with an intermediate equatorial belt much more rarely visited by spots, is a fact notorious in their history, and which at once refers their cause to fluid circulations, modified, if not produced, by that rotation, by reasoning of the very same kind whereby we connect our own system of trade and anti-trade winds with the earth's rotation. Having given any exciting cause for the circulation of atmospheric fluids from the poles to the equator, and back again, or *vice versâ*, the effect of rotation will necessarily be to modify those currents as our trade-winds and monsoons are modified, and to dispose all their meteorological phænomena on a great scale which accompany them as their visible manifestations in zones parallel to the equator, with a calm equatorial zone interposed. It only remains, therefore, to inquire, Whether any such cause of circulation can be found in the œconomy of the sun, so far as we know and can understand it?"

In answer to this inquiry he observes, that if any physical difference in the constitution or circumferences of the sun's polar and equatorial regions tends to repress the escape of heat in the one, and to favour it in the other of these regions, the effect will be the same as if those regions were unequally heated from without, and all the phænomena of trade-winds, *mutatis mutandis,* must arise. That the sun is surrounded by a transparent atmosphere, extending beyond its luminous surface, Sir John holds to be conclusively established by various phænomena, and he instances in particular the striking deficiency of light at the borders of the visible disc, and the extraordinary phænomenon of the rose-coloured clouds witnessed during the total eclipse of July 8, 1842, which must have floated in and been sustained by an exterior transparent atmosphere. He also thinks that the distance to which this atmosphere extends beyond the visible disc must be considerable, not merely in absolute measure, *but as an aliquot part of the sun's radius.* "Now, granting the existence of such an atmosphere, its form in obedience to the laws of equilibrium must be that of an oblate spheroid, the ellipticities of whose strata differ from each other and from that of the nucleus. Consequently, the equatorial portions of this envelope must be of a thickness different from that of the polar, *density for density,* so that a different obstacle must be thereby opposed to the escape of heat from the equatorial and the polar regions of the sun. The former, therefore, ought, according to this reasoning, to be habitually maintained at a different temperature from the latter."

According to this view of the subject, the spots come to be "assimilated to those regions on the earth's surface in which, for the moment, hurricanes and tornadoes prevail—the upper stratum being temporarily carried downwards, displacing by its impetus the two strata of luminous matter beneath, ... the upper of course to a greater extent than the lower, and thus wholly or partially denuding the opaque surface of the sun below. Such processes cannot be unaccompanied with vorticose motions, which, left to themselves, die away by degrees and dissipate, with this peculiarity, that their lower portions come to rest more speedily than their upper, by reason of the greater resistance below, as well as the remoteness from the point of action, which lies in a higher region, so that their centre (as seen in our water-spouts, which are nothing but small tornadoes) appears to retreat upwards. Now this agrees perfectly with what is observed during the obliteration of the solar spots, which appear as if filled in by the collapse of their sides, the penumbra closing in upon the spot and disappearing after it."—P. 434.

Some appearances which were frequently noticed in the course of these observations might seem at first sight to militate against this idea. Lines of spots of more or less extent, or connected with penumbral trains more or less beset with spots, were frequently noticed oblique to the direction of the parallels of latitude on the sun's surface, and converging on both sides of the equator to the *preceding* side of the disc. Penumbræ of great spots also were sometimes observed to be rounded and well-terminated on the preceding side, but rugged and extensively diffused on the following, where they would often take the form of great trains, and ultimately affect the form of pretty long, straight parallel bands, having the same obliquity to the parallels of latitude as the lines and spots above-mentioned. Sir John is of opinion that these bands and lines, having a transverse direction to the course of the currents which the dynamical theory supposes, "must be assimilated rather to ripple-marks which are transverse to the direction of the general movement in which they have their origin than to trails drawn out by a current in its own direction, if indeed the appearances in question be anything more than accidental."

A very remarkable character of the solar spots is the intense blackness of the *spot* "*nucleus,*" or "*opening,*" as it has been termed, as contrasted with the *penumbra* or "shallow" surrounding it. "This want of graduation—this sharply marked suddenness of transition, is altogether opposed to the conception of a susceptibility of indefinite and easy mixture in the luminous, non-luminous, and semi-luminous constituents of the solar envelope. . . . . There is no gradual melting of the one shade into the other—spot into penumbra —penumbra into full light. The idea conveyed is more that of the successive withdrawal of veils,—the partial removal of definite films, than the melting away of a mist, or the mutual dilution of gaseous media. Films of immiscible liquids having a certain cohesion, floating on a dark or transparent ocean and liable to temporary removal by winds, would rather seem suggested by the general tenour of the appearances, though they are far from being wholly explicable by this conception, at least if any considerable degree of transparency be allowed to the luminous matter."—P. 436.

The chapter concludes with the remark, that during the months embraced by the observations the state of the sun seemed evidently undergoing a gradual alteration in the nature of a subsidence from violent agitation to comparative tranquillity.

There are five appendices to the work, the contents of which are as follows:—

A. "On the numerical magnitudes of certain stars, as obtained by subsequent observation on the principle of sequences, in the northern hemisphere, compared and combined with the same stars as observed in the southern; and of some others whose places in the scale of magnitudes are directly deducible from these by interpolation of the sequences."

B. "On the difference of level between the Royal Observatory, Cape of Good Hope, and Feldhausen." The result is 112·23 feet for the difference of level between the cisterns of the barometers, that at Feldhausen being the higher.

C. "On the temperature required by the surface-soil under exposure to clear sunshine, and some other effects of accumulated radiation; and on the quantity of heat given out by the sun. Suggestion of an improvement on the ice calorimeter."

D. "Approximate places of seventy-six ruby-coloured, insulated stars, noticed in the course of observation, in either hemisphere."

E. "Geodesical determination of the site of the 20-foot reflector at Feldhausen, with respect to the Royal Observatory at the Cape of Good Hope." The co-ordinates of the site were ascertained to be 17,595 feet in the direction of the meridian to the south, and 5190 feet in that of a parallel to the west, from the centre of transit instrument at the Observatory, corresponding to $2' \, 53''{\cdot}55$ of latitude and $4^{s}{\cdot}11$ of longitude.

It would be unpardonable to conclude our long extracts without transcribing the sentence with which the author closes his laborious undertaking. "The record of its site (*i. e.* of the great reflector) is preserved on the spot by a granite column erected after our departure by the kindness of friends, to whom, as to the locality itself, and the colony, every member of my family had become, and will remain, attached by a thousand grateful and pleasing recollections of years spent in agreeable society, cheerful occupation, and unalloyed happiness."

XLVII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxxii. p. 144.]

Dec. 6, 1847. ON the Critical Values of the sums of Periodic Series. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

There are a great many problems in heat, fluid motion, &c., the solution of which requires the development of an arbitrary function of x, $f(x)$, between certain limits as o and a of x, by means of functions of known form. The form of the expansion is determined, at least in part, by the conditions to be satisfied at the limits; and it is usually considered that these conditions are satisfied by adopting the form of expansion to which they lead. Thus, if the problem requires that $f(o)$ and $f(a)$ vanish, it is considered that this condition is satisfied by developing $f(x)$ in a series of sines of $\frac{\pi x}{a}$ and its multiples. But since an arbitrary function admits of expansion in such a series, the expanded function is not restricted to vanish at the limits o and a. It becomes then a question, how shall we know when the expanded function does really vanish at the limits, and if it does not, how are such expansions to be treated, and are they of any practical importance?

In considering the logic of such developments, the author was led to perceive in what manner the evanescence of $f(x)$ at the limits can be ascertained, or else the values of $f(o)$ and $f(a)$ obtained, from the development itself, even when the series cannot be summed, by examining the coefficient of $\sin \frac{n\pi x}{a}$ in the nth term. In a similar manner the discontinuity of $f(x)$ or any of its derivatives may be ascertained, and the amount of the sudden change of the function determined. In such cases the expansions of the derivatives of $f(x)$ cannot be obtained by differentiating under the sign of summation, but are given by formulæ which the author has considered.

The most important case in considering a series of sines, is that in which $f(x)$ is continuous; but $f(o)$ and $f(a)$, instead of being equal to zero, are given quantities, and the coefficients in the expansion are indeterminate. In this case the coefficients in the expansions of $f'(x)$ and $f''(x)$ contain, in addition to the indeterminate coefficients which enter into the expansion of $f(x)$, the given quantities $f(o)$ and $f(a)$. Thus the expansion in a series of sines is useful, not only when $f(o)$ and $f(a)$ vanish, but also when they are given quantities. In the same way the expansion of $f(x)$ in a series of cosines is useful when $f'(o)$ and $f'(a)$ are given, as well as when they vanish. Thus, to take an example, the permanent temperature in a rectangular parallelepiped, when the temperatures of the faces are any arbitrary functions of the co-ordinates, can be expressed in a double series of sines involving *any two* of the three co-ordinates.

The author has only considered a series of sines and a series of cosines, with the corresponding integrals; but the methods which he has employed are of very general application. The comparison of different expressions of the same function of two or more independent variables often leads to very remarkable formulæ. The development of arbitrary functions in the way considered by the author is, however, not only curious but useful; for the expressions thus obtained are often much better adapted to numerical computation than those which would be obtained by the developments usually employed.

In connexion with these investigations, the author was led to consider the discontinuity of the sums of infinite series, or of the values of integrals between infinite limits, which sometimes takes place even when the series or integral remains convergent, and the general term of the series, or the quantity under the integral sine, is a continuous function of some quantity which is regarded as variable. The author has shown that in all such cases the convergency of the series or integral becomes infinitely slow.

The problem of determining the potential due to a given electrical point within a hollow conducting rectangular parallelepiped, and to the electricity included on the surface, is solved by a method which leads very readily to the result. The author thinks that a similar method may sometimes be advantageously employed in other questions. The electricity is first supposed to be diffused over a finite space: this allows of the expansion of the potential V in a triple series of sines. Instead of the equation $\nabla V=0$, where ∇ means the same as

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2},$$

it becomes of course necessary to employ the equation $\nabla V = -4\pi\rho$. The solution having been obtained, the electricity may now be supposed to be condensed into a point, and one of the summations may be effected. The potential is thus expressed in a double series, which appears to be the simplest form that it admits of.

May 8, 1848.—Supplement to a paper "On the Intensity of Light in the neighbourhood of a Caustic." By G. B. Airy, Esq., Astronomer Royal.

The author, after referring to the paper printed in a former volume of the Society's Memoirs, in which he has shown that the expression for the intensity depends on the integral $\int_w \cos(w^3 - mw)$ between the limits $w=0$, $w =$ infinity, where m is proportional to the distance of the point at which the intensity is required, from the geometrical caustic, and in which he has calculated by quadratures the value of the definite integral for different values of m as far as $m = \pm 4\cdot0$, states that he was induced to have recourse to the method of quadratures only because every expansion which he attempted made it necessary to rely (for some of the terms) upon definite integrals equivalent to the integral $\int_\theta \cos\theta$ from $\theta=0$ to $\theta =$ infinity, and that he was not satisfied with the reasoning upon which some mathematicians had given a determinate value to that integral. Professor De Morgan, however, who felt no doubts upon it, had furnished him with a series proceeding by ascending powers of m, and had also explained in detail (in a letter embodied in this paper) his views on the evidence for the value of the series, and on the method of determining it. From this series, the values of the definite integral are computed for all the values of m for which the computation had been made by quadratures, and the result is that the two sets of computed numbers are entirely accordant. The computations are also extended to the limit $m = \pm 5\cdot6$, which is the greatest value to which it is possible to extend the calculations by the use of 10-figure logarithms.

June 5.—On some new Fossil Fish of the Carboniferous Period. By Frederic M'Coy, M.G.S., N.H.S.D.

The author having premised that the species of fish of the carboniferous limestone enumerated in the third volume of the *Poissons Fossiles* of M. Agassiz are for the most part still unpublished, being without definitions or figures, states that through the kindness of Capt. Jones, R.N., M.P., &c. he was enabled to study the original specimens of twenty-eight out of the thirty unpublished species from Armagh in M. Agassiz's list, and is therefore certain of the species described by him being so far distinct from those alluded to. The greater number of the examples here described are in the cabinets of the University of Cambridge (principally collected by the Rev. W. Stokes, of Caius College), and of Captain Jones; a few from the lower carboniferous shales of Ireland are only known in that of Mr. Griffith of Dublin. The descriptions are accompanied by drawings of all the species of the natural size, and illustrations of the microscopic structures; and acknowledgements are made of the kind co-operation of the Rev. Prof. Clark and Mr. Anthony of Caius College, Cambridge, in this part of the investigation, by allowing the use of their large microscopes, and assisting to prepare the transparent fragments for examination.

Twelve new genera are proposed :—1st. *Isodus*, for a fish of the yellow sandstone, having very numerous teeth with a simple conical pulp-cavity in their upper part, which divides into branches below as in *Rhizodus* (Owen); but the section is circular, and the teeth are all equal in size. 2nd. *Centrodus*, for curved conical teeth with a wide simple pulp-cavity, reducing the base to a sharp edge, and having not only the form but the microscopic structure of a reptile tooth, that is, from the simple pulp-cavity minute calcigerous tubes radiate to the circumference, terminating near the surface in a layer of small calcigerous cells, covered by a layer of true glass-like enamel, presenting no trace of structure with a power of 300 diameters, and quite distinct from that dense modification of *dentine*, which, forming the polished surface of most fish-teeth, has been confounded with true enamel, but which it is here proposed to call *ganoine* in future descriptions. 3rd. *Colonodus*, for very long simple teeth with simple pulp-cavity, and their sides indented by transverse wrinkles. 4th. *Osteoplax*, for large, flat, polygonal dermal plates, minutely wrinkled on the surface, and allied to *Psammosteus* (Ag.) of the Old Red Sandstone; but while the latter plates are composed of horizontal layers of large cells, the present genus has a very singular microscopic structure, being traversed by vertical branched (Haversian ?) canals terminating in the pores of the surface; and in the intervening blastema are numerous oval Purkinian cells, the radiating tubuli of which do not anastomose. 5th. *Erismacanthus*, for a singular Ichthyodorulite not uncommon in the Armagh limestone, which, arising from a large compressed base, branches into two portions, one long anterior closely tuberculated prop-like portion, and another extruding backwards, short, and resembling a small *Ctenacanthus*, but with smooth ridges. 6th. *Platycanthus*, for small spines, extremely wide and compressed, resembling small Oracanths, but arched and with posterior rows of teeth. 7th. *Dipriacanthus*, for small, curved dorsal spines, which have two rows of denticles pointing downwards on the posterior face, and two rows pointing upwards on the anterior face, reminding us of the recent *Pimelodus* and *Synodontus* of the Nile. 8th. *Polyrhizodus*, an extraordinary genus of Psammodontoid teeth not uncommon in the Armagh limestone, having the root divided into numerous fang-like lobes, as in a mammalian tooth. 9th. *Glossodus*, for certain tongue-shaped teeth allied to *Helodus* (Ag.). 10th. *Climaxodus*, for some palates allied to *Pœcilodus* (Ag.), but instead of being transversely trigonal and obliquely ridged, they are equilateral, and have the ridges transverse and parallel (like a flight of steps). 11th. *Chirodus*, for little hand-shaped teeth allied to the *Ceratodi*, but distinguished by the thumb-like lobe projecting from the middle of the long side, and which would prevent the union of the teeth in pairs in the mouth, in the manner of *Ceratodus*. 12th. *Petrodus*, small conical ridged teeth resembling limpets, common in the Derbyshire limestone, but presenting, of all known fossil fish, the nearest approach to the microscopic structure of the recent *Cestracion*. It is also proposed to divide the genus *Holoptychius* of M. Agassiz; and instead of considering it and *Rhizodus* of Owen as synonymous, to limit the latter to those great teeth with an elliptical section so common in some parts of the Carboniferous series, accompanied by large, thin, quadrate scales, marked with concentric lines of growth, and having a fine cancellated structure internally, the *Holoptychius Hibberti* (Ag.) (*Rhizodus ferox*, Owen) and *H. Portlocki* (Ag.) being the types; thus retaining the name *Holoptychius* for those fish so abundant in the Old Red Sandstone with thick, bony, ovate, longitudinally wrinkled scales, and minute teeth with a circular section, having the *H. nobilissimus*, *H. giganteus*, &c. as the type.

The number of new species described and figured in this paper is forty-one, of which several belong to genera not previously known in rocks of the carboniferous period, many showing a strong affinity to the Devonian type of form. Thus we have two species of *Psammosteus*, one of *Chelyophorus*, one (doubtful) of *Coccosteus*, one of *Asterolepis*, two of *Homacanthus*, and one of *Cosmacanthus*, genera hitherto only found in the Old Red Sandstone.

On an Absolute Thermometric Scale founded on Carnot's Theory of the Motive Power of Heat*, and calculated from Regnault's observations†. By Prof. W. Thomson, Fellow of St. Peter's College.

The determination of temperature has long been recognized as a problem of the greatest importance in physical science. It has accordingly been made a subject of most careful attention, and, especially in late years, of very elaborate and refined experimental researches‡; and we are thus at present in possession of as complete a practical solution of the problem as can be desired, even for the most accurate investigations. The theory of thermometry is however as yet far from being in so satisfactory a state. The principle to be followed in constructing a thermometric scale might at first sight seem to be obvious, as it might appear that a perfect thermometer would indicate equal additions of heat, as corresponding to equal elevations of temperature, estimated by the numbered divisions of its scale. It is however now recognized (from the variations in the specific heats of bodies) as an experimentally demonstrated fact that

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* Published in 1824 in a work entitled *Réflexions sur la Puissance Motrice du Feu*, by M. S. Carnot. Having never met with the original work, it is only through a paper by M. Clapeyron, on the same subject, published in the *Journal de l'Ecole Polytechnique*, vol. xiv. 1834, and translated in the first volume of Taylor's Scientific Memoirs, that the author has become acquainted with Carnot's theory.—W. T.

† An account of the first part of a series of researches undertaken by M. Regnault by order of the French Government, for ascertaining the various physical data of importance in the Theory of the Steam-Engine, is just published in the *Mémoires de l'Institut*, of which it constitutes the twenty-first volume (1847). The second part of the researches has not yet been published.

‡ A very important section of Regnault's work is devoted to this object.

thermometry under this condition is impossible, and we are left without any principle on which to found an absolute thermometric scale.

Next in importance to the primary establishment of an absolute scale, independently of the properties of any particular kind of matter, is the fixing upon an arbitrary system of thermometry, according to which results of observations made by different experimenters, in various positions and circumstances, may be exactly compared. This object is very fully attained by means of thermometers constructed and graduated according to the clearly defined methods adopted by the best instrument-makers of the present day, when the rigorous experimental processes which have been indicated, especially by Regnault, for interpreting their indications in a comparable way, are followed. The particular kind of thermometer which is least liable to uncertain variations of any kind is that founded on the expansion of air, and this is therefore generally adopted as the standard for the comparison of thermometers of all constructions. Hence the scale which is at present employed for estimating temperature is that of the air-thermometer; and in accurate researches care is always taken to reduce to this scale the indications of the instrument actually used, whatever may be its specific construction and graduation.

The principle according to which the scale of the air-thermometer is graduated, is simply that equal absolute expansions of the mass of air or gas in the instrument, under a constant pressure, shall indicate equal differences of the numbers on the scale; the length of a "degree" being determined by allowing a given number for the interval between the freezing- and the boiling-points. Now it is found by Regnault that various thermometers, constructed with air under different pressures, or with different gases, give indications which coincide so closely, that, unless when certain gases, such as sulphurous acid, which approach the physical condition of vapours at saturation, are made use of, the variations are inappreciable*. This remarkable circumstance enhances very much the practical value of the air-thermometer; but still a rigorous standard can only be defined by fixing upon a certain gas at a determinate pressure, as the thermometric substance. Although we have thus a strict principle for constructing a *definite* system for the estimation of temperature, yet as reference is essentially made to a specific body as the standard thermometric substance, we cannot consider that we have arrived at an *absolute* scale, and we can only regard, in strictness, the scale actually adopted as *an arbitrary series of numbered points of reference sufficiently close for the requirements of practical thermometry.*

In the present state of physical science, therefore, a question of extreme interest arises: *Is there any principle on which an absolute thermometric scale can be founded?* It appears to me that Carnot's

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* Regnault, *Relation des Expériences*, &c., Fourth Memoir, First Part. The differences, it is remarked by Regnault, would be much more sensible if the graduation were effected on the supposition that the coefficients of expansion of the different gases are equal, instead of being founded on the principle laid down in the text, according to which the freezing- and boiling-points are experimentally determined for each thermometer.

theory of the motive power of heat enables us to give an affirmative answer.

The relation between motive power and heat, as established by Carnot, is such that *quantities of heat*, and *intervals of temperature*, are involved as the sole elements in the expression for the amount of mechanical effect to be obtained through the agency of heat; and since we have, independently, a definite system for the measurement of quantities of heat, we are thus furnished with a measure for intervals according to which absolute differences of temperature may be estimated. To make this intelligible, a few words in explanation of Carnot's theory must be given; but for a full account of this most valuable contribution to physical science, the reader is referred to either of the works mentioned above (the original treatise by Carnot, and Clapeyron's paper on the same subject).

In the present state of science no operation is known by which heat can be absorbed, without either elevating the temperature of matter, or becoming latent and producing some alteration in the physical condition of the body into which it is absorbed; and the conversion of heat (or *caloric*) into mechanical effect is probably impossible*, certainly undiscovered. In actual engines for obtaining mechanical effect through the agency of heat, we must consequently look for the source of power, not in any absorption and conversion, but merely in a transmission of heat. Now Carnot, starting from universally acknowledged physical principles, demonstrates that it is by the *letting down* of heat from a hot body to a cold body, through the medium of an engine (a steam-engine, or an air-engine for instance), that mechanical effect is to be obtained; and conversely, he proves that the same amount of heat may, by the expenditure of an equal amount of labouring force, be *raised* from the cold to the hot body (the engine being in this case *worked backwards*); just as mechanical effect may be obtained by the descent of water let down by a water-wheel, and by spending labouring force in turning the wheel backwards, or in working a pump, water may be elevated to a higher level. The amount of mechanical effect to be obtained by the transmission of a given quantity of heat, through the medium of any kind of engine in which the œconomy is perfect, will depend, as Carnot demonstrates, not on the specific nature of the substance employed as the medium of transmission of heat in the engine, but solely on the interval between the temperatures of the two bodies between which the heat is transferred.

Carnot examines in detail the ideal construction of an air-engine and of a steam-engine, in which, besides the condition of perfect

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* This opinion seems to be nearly universally held by those who have written on the subject. A contrary opinion however has been advocated by Mr. Joule of Manchester; some very remarkable discoveries which he has made with reference to the *generation* of heat by the friction of fluids in motion, and some known experiments with magneto-electric machines, seeming to indicate an actual conversion of mechanical effect into caloric. No experiment however is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of natural philosophy.

œconomy being satisfied, the machine is so arranged that at the close of a complete operation the substance (air in one case and water in the other) employed is restored to precisely the same physical condition as at the commencement. He thus shows on what elements, capable of experimental determination, either with reference to air, or with reference to a liquid and its vapour, the absolute amount of mechanical effect due to the transmission of a unit of heat from a hot body to a cold body, through any given interval of the thermometric scale, may be ascertained. In M. Clapeyron's paper various experimental data, confessedly very imperfect, are brought forward, and the amounts of mechanical effect due to a unit of heat descending a degree of the air-thermometer, in various parts of the scale, are calculated from them, according to Carnot's expressions. The results so obtained indicate very decidedly, that what we may with much propriety call *the value of a degree* (estimated by the mechanical effect to be obtained from the descent of a unit of heat through it) of the air-thermometer depends on the part of the scale in which it is taken, being less for high than for low temperatures*.

The characteristic property of the scale which I now propose is, that all degrees have the same value; that is, that a unit of heat descending from a body A at the temperature T° of this scale, to a body B at the temperature $(T-1)^\circ$, would give out the same mechanical effect, whatever be the number T. This may justly be termed an absolute scale, since its characteristic is quite independent of the physical properties of any specific substance.

To compare this scale with that of the air-thermometer, the *values* (according to the principle of estimation stated above) of degrees of the air-thermometer must be known. Now an expression, obtained by Carnot from the consideration of his ideal steam-engine, enables us to calculate these values, when the latent heat of a given volume and the pressure of saturated vapour at any temperature are experimentally determined. The determination of these elements is the principal object of Regnault's great work, already referred to, but at present his researches are not complete. In the first part, which alone has been as yet published, the latent heats of a given *weight*, and the pressures of saturated vapour at all temperatures between 0° and 230° (Cent. of the air-thermometer), have been ascertained; but it would be necessary in addition to know the densities of saturated vapour at different temperatures, to enable us to determine the latent heat of a given *volume* at any temperature. M. Regnault announces his intention of instituting researches for this object; but till the results are made known, we have no way of completing the data necessary

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* This is what we might anticipate, when we reflect that infinite cold must correspond to a finite number of degrees of the air-thermometer below zero; since, if we push the strict principle of graduation, stated above, sufficiently far, we should arrive at a point corresponding to the volume of air being reduced to nothing, which would be marked as -273° ($-\frac{100}{\cdot 366}$, if ·366 be the coefficient of expansion) of the scale; and therefore -273° of the air-thermometer is a point which cannot be reached at any finite temperature, however low.

for the present problem, except by estimating the density of saturated vapour at any temperature (the corresponding pressure being known by Regnault's researches already published) according to the approximate laws of compressibility and expansion (the laws of Mariotte and Gay-Lussac, or Boyle and Dalton). Within the limits of natural temperature in ordinary climates, the density of saturated vapour is actually found by Regnault (*Etudes Hygrométriques* in the *Annales de Chimie*) to verify very closely these laws; and we have reason to believe from experiments which have been made by Gay-Lussac and others, that as high as the temperature 100° there can be no considerable deviation; but our estimate of the density of saturated vapour, founded on these laws, may be very erroneous at such high temperatures as 230°. Hence a completely satisfactory calculation of the proposed scale cannot be made till after the additional experimental data shall have been obtained; but with the data which we actually possess, we may make an approximate comparison of the new scale with that of the air-thermometer, which at least between 0° and 100° will be tolerably satisfactory.

The labour of performing the necessary calculations for effecting a comparison of the proposed scale with that of the air-thermometer, between the limits 0° and 230° of the latter, has been kindly undertaken by Mr. William Steele, lately of Glasgow College, now of St. Peter's College, Cambridge. His results in tabulated forms were laid before the Society, with a diagram, in which the comparison between the two scales is represented graphically.

In the first table, the amounts of mechanical effect due to the descent of a unit of heat through the successive degrees of the air-thermometer are exhibited. The unit of heat adopted is the quantity necessary to elevate the temperature of a kilogramme of water from 0° to 1° of the air-thermometer; and the unit of mechanical effect is a metre-kilogramme; that is, a kilogramme raised a metre high.

In the second table, the temperatures according to the proposed scale, which correspond to the different degrees of the air-thermometer from 0° to 230°, are exhibited. [The arbitrary points which coincide on the two scales are 0° and 100°.]

Note.—If we add together the first hundred numbers given in the first table, we find 135·7 for the amount of work due to a unit of heat descending from a body A at 100° to B at 0°. Now 79 such units of heat would, according to Dr. Black (his result being very slightly corrected by Regnault), melt a kilogramme of ice. Hence if the heat necessary to melt a pound of ice be now taken as unity, and if a *metre-pound* be taken as the unit of mechanical effect, the amount of work to be obtained by the descent of a unit of heat from 100° to 0° is $79 \times 135{\cdot}7$, or 10,700 nearly. This is the same as 35,100 foot-pounds, which is a little more than the work of a one-horse-power engine (33,000 foot-pounds) in a minute; and consequently, if we had a steam-engine working with perfect œconomy at one-horse-power, the boiler being at the temperature 100°, and the condenser kept at 0° by a constant supply of ice, rather less than a pound of ice would be melted in a minute.

XLVIII. *Intelligence and Miscellaneous Articles.*

ON A SINGULAR IRREGULARITY OF VISION.

BY N. S. HEINEKEN, ESQ.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Sidmouth, Sept. 2, 1848.

CHANCE has thrown in my way an extract from the Medico-Chirurgical Review for 1834, in which it is stated that M. Prevost of Geneva and Mr. Babbage have themselves experienced a singular irregularity of vision, viz. that of double images with one eye. The details are given in the work above-named, and also in the Arcana of Science for 1834, p. 184.

I am induced to trouble you with this communication in order to make known a somewhat similar irregularity in my own case; though as mine was only temporary, it may perhaps, from the circumstances under which it occurred, afford some clue to the cause. I therefore forward the following note, made at the time, for your perusal.

"Having occasion to divide a yard into a thousand parts, I used an eye-lens of six inches focus to assist the RIGHT eye while dividing—the *left* was kept *closed*. I had been employed about two hours in making 400 divisions, and then left off. I then found that upon looking at a window on the opposite side of the street with the LEFT (*unemployed*) eye, the bars were TRIPLE, while with the *right* (*employed*) they were SINGLE. This effect lasted (gradually decreasing) for at least two hours; the bars were also surrounded by a strong penumbra. At first, objects—such as people on horseback—were so distorted that I had great difficulty in deciding what they were."

On another occasion I have noticed a defect of vision of a somewhat different character, of which I also send you the note made at the time. "Upon rising one morning I observed in the right eye, as it were, innumerable faint scintillations or lucid points, the whole field of vision being covered by them. Upon going to a looking-glass, I found that I could not see one half of the face with that eye—it appeared perfectly dark. The effect lasted perhaps half or three quarters of an hour." Dr. Kitchener mentions having been alarmed by a somewhat similar appearance; but this seems to have arisen from over-exertion of the eye from minute examination of the powers, &c. of several telescopes. In my own case, I am not aware that the eye had been at all *overworked* on the *previous* day, or *for some length of time*; nor had it any connexion with the *previous* cause of multiple vision, this having occurred more than twelve months before. If you deem the above at all likely to interest your readers, I hope you will make use of it.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

ON THE DETECTION OF LEAD IN THE PRESENCE OF BISMUTH IN BLOWPIPE EXPERIMENTS. BY E. J. CHAPMAN, ESQ.

It is well known that lead, bismuth and cadmium are the only metals which, when heated before the blowpipe, deposit a yellow coating of oxide on the support. The areola formed by oxide of cadmium is of a much deeper colour than that deposited by either lead or bismuth; and the metal itself is so rapidly volatilized, that in these experiments it is never obtained in the metallic state. When, therefore, in the examination of any substance, a yellow sublimate takes place upon the charcoal with the production of a metallic globule, we have to determine whether this globule consist of lead or of bismuth, or of the two combined. A malleable "button" is usually presumed to consist of lead, and a brittle one of bismuth; nevertheless a perfectly malleable globule may be obtained, containing an admixture of bismuth; and a brittle one, on the other hand, containing a very considerable quantity of lead. For this reason the assay should be examined further; and in the reaction of bismuth with microcosmic salt, as first noted by Berzelius, we possess a ready method of detecting that metal, whether it be combined with lead or not. For this purpose it must be treated with the reagent on charcoal in the reducing-flame, with the addition of a minute particle of tin, when the glass, which is colourless and quite transparent whilst hot, becomes on cooling, if bismuth be present, grayish-black and opake. The only metal which produces a similar reaction is antimony; but this, if it chance to be present, can easily be got rid of by Plattner's process, which consists in fusing the assay matter with vitrified boracic acid in the oxidating flame, taking care not to allow the globule to be entirely surrounded by the flux, by which means the antimony is entirely driven off, whilst the lead and bismuth are retained, as oxides, by the melted acid, and may be again brought to the metallic state by fusion in the reducing-flame with carbonate of soda.

Thus far then the presence of bismuth is easily ascertained even in a malleable globule consisting almost wholly of lead; but the detection of this latter metal in a globule rendered brittle by a large proportion of bismuth, has been hitherto, in experiments with the blowpipe, a much more troublesome affair. Plattner recommends for this purpose a modification of one of the methods given in the Manual of Professor Rose for the separation of lead from bismuth; but this method—which consists, as modified by Plattner, in fusing the mixed metals with an excess of bisulphate of potash, and subsequently treating the fused mass, first with water, and afterwards with nitric acid, to dissolve the sulphate of bismuth—is scarcely admissible in these rapid experiments, in which the use of liquid acids and other bulky reagents should, if possible, be avoided, in order not only to render the operations as simple as accuracy will admit of, but also to prevent the dimensions of the blowpipe-case from affecting its portability. On this account I have endeavoured to discover some other method for the detection of lead when combined in small quantity with bismuth; and the following very simple process for that purpose has occurred to me, founded on the known reduction and precipitation of salts of bismuth by metallic lead—a method which I have found to succeed perfectly with brittle alloys containing upwards of 85 per cent. of bismuth. A small fragment or little crystal of nitrate of bismuth is placed in a porcelain capsule, and moistened with a few drops of water, the greater part of which is afterwards poured off; and the metallic button of the mixed metals, as obtained by the blowpipe, having been slightly flattened on the anvil until it begins to crack at the sides, is then placed in the midst of the subsalt of bismuth formed by the action of the water, when in the course of a minute, or even less, according to the amount of lead present, minute arborescent crystals of metallic bismuth form and collect around the assay. Copper does not affect this reaction; but if either zinc or iron were present, the precipitation would ensue from that cause alone. Zinc, however, if originally contained in the assay-matter, would be volatilized by the action of the blowpipe, especially if a little carbonate of soda were added to it before subjecting the globule to the above test; and iron might be easily separated by treating the assay in the reducing-flame with a mixture of carbonate of soda and borax—the latter reagent serving to dissolve the iron, and to prevent its reduction to the metallic state. If a single operation do not effect this, the globule must be removed from the saturated dark green glass, and treated with a further supply of the mixture on another piece of charcoal until the resulting glass be no longer coloured.—*From the Chemical Gazette for September* 15, 1848.

ON THE SEPARATION OF ANTIMONY FROM ARSENIC.

BY C. MEYER.

The following method is founded on the insolubility of the antimoniate of soda, and the conversion of arseniferous antimony into arseniate and antimoniate of soda. The author first convinced himself of the perfect insolubility of calcined anhydrous antimoniate of soda. When antimony is deflagrated with nitrate of soda and the mass exhausted with cold water, no antimony can be found in the liquid. The residuous antimoniate of soda is $NaO, Sb\,O^5$, and anhydrous. When a solution of antimoniate of potash is precipitated with sulphate of soda, and the liquid after some time filtered off clear, no antimony can be detected in it. The crystalline precipitate was proved by analysis to be also NaO, SbO^5, but it contains 6 atoms or 21·23 per cent. of water; experiment gave 21·5. The antimony was estimated in some cases by precipitating it with sulphuretted hydrogen from a solution of the salt in a mixture of muriatic and tartaric acids; in others by the method recently proposed by Rose*, igniting the salt with chloride of ammonium, which gave

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* Chem. Gaz., vol. vi. p. 166.

the most accurate result. This hydrated salt is not quite insoluble in boiling water.

To test the applicability of the insolubility of the calcined antimoniate of soda to its separation from arsenic, a known weight of pure antimony was mixed with arsenic, and the mixture deflagrated with three times its weight of a mixture of nitrate and carbonate of soda. After washing the mass with cold water and ignition, a quantity of antimoniate of soda was obtained, which almost exactly corresponded with the weight which should have been obtained according to theory. On examination before the blowpipe, the salt proved to be perfectly free from arsenic. The arsenic was precipitated from the solution with sulphuretted hydrogen, and determined in the usual manner; and its quantity likewise agreed perfectly well with the amount employed.

0·10 grm. tartar-emetic and as much arsenious acid were dissolved in a thick soup, the organic matters destroyed as much as possible by a long-continued current of chlorine, the heated liquid filtered, the mass upon the filter well-washed, the solution saturated with sulphuretted hydrogen and left for some time in contact with it; the precipitate filtered, washed, and then dissolved together with the filter in hot nitric acid, the solution saturated with carbonate of soda, some nitrate of soda added to it, the whole evaporated, and the residue heated in a porcelain crucible until the sulphurets and organic matter were completely oxidized, and the mass finally fused. On exhausting the mass with water, 0·057 grm. antimoniate of soda was obtained, while according to theory 0·058 should have been obtained. The solution which contained the arseniate of soda was evaporated to dryness; all the carbonic, nitric and nitrous acids expelled by concentrated sulphuric acid; the salt dissolved in water, mixed with sulphurous acid; and, after expelling the latter, the arsenic precipitated by sulphuretted hydrogen. To separate the free sulphur, the precipitate was dissolved in very dilute ammonia, and reprecipitated with sulphuric acid; it weighed 0·120 grm., which corresponds to 0·096 arsenious acid, which also is very near to the amount employed.

The author applied this insolubility of the antimoniate of soda likewise to the preparation of antimony free from arsenic acid with complete success. Commercial antimony was fused together with $\frac{1}{4}$th its weight of arsenic, the powdered regulus mixed with $1\frac{1}{4}$th part of crude nitrate of soda (cubic nitre), and $\frac{1}{2}$ part carbonate of soda, heated to faint redness, and the mass exhausted with water. The residue of antimoniate of soda was, when dry, fused with half its weight of cream of tartar, when a very beautiful white regulus was obtained, which gave off not the slightest odour of arsenic before the blowpipe, and was characterized by the ease with which it continued to burn away. Moreover the regulus neither contained sodium nor potassium, which latter it generally contains when reduced from the antimoniate of potash.—Liebig's *Annalen*, May 1848.

ON THE COMPOSITION OF ORCINE AND ITS DERIVATIVES.

BY MM. LAURENT AND GERHARDT.

Chemists generally admit the formulæ proposed some years since by M. Liebig, for dry and crystallized orcine. According to this chemist, they are—

Dry orcine	$C^{16}H^{16}O^{4}$
Crystallized orcine..	$C^{16}H^{16}O^{4}+3Aq.$

These formulæ seem at first to agree perfectly with the metamorphoses by which orcine originates, and especially with the production of orcine by lecanorine. But if the formulæ of M. Liebig be compared with the analyses of M. Dumas, and with the more recent ones of MM. Will and Schunck, whose results are almost identical with those of M. Dumas, it will be seen that these formulæ suppose an error of nearly 3 per cent. in the carbon found by experiment in dry orcine.

Struck with this circumstance, one of the authors proposed two years since to reject the formulæ of M. Liebig, and to replace them by the following:—

Dry orcine............	$C^{7}H^{8}O^{2}$
Crystallized orcine	$C^{7}H^{8}O^{2}+Aq.$

These not only agree very well with the analyses of orcine, but also with those of lecanorine and other crystallizable derivatives.

MM. Laurent and Gerhardt state that they have lately obtained with orcine a new derivative, the composition of which confirms the latter formulæ. This compound is *bromorcine*, a substance which crystallizes in fine silky needles, and is easily produced by the action of bromine on orcine. The formula of bromorcine is $C^{7}H^{5}B^{3}O^{2}$, and it consequently represents orcine, in which three equivalents of hydrogen are replaced by three equivalents of bromine. It is remarkable that potash dissolves it immediately, and becomes of a very deep violet-brown colour. It was found to contain 23·1 per cent. of carbon, the authors' formula requiring 23·2. If the proportions of M. Liebig were supposed to be correct, the authors state that they must have committed an error of nearly 2 per cent. in the same way as M. Dumas in his experiments on normal orcine.

The authors' formulæ place orcine in relation with the salicylic series; they form an isomeric with the saligenine of M. Piria. The following are the relations which exist, according to MM. Laurent and Gerhardt, between orcine and its crystallizable derivatives.

Lecanorine: by ebullition with barytes water, it gives carbonate and orcine:—

$$\underset{\text{lecanorine}}{C^{16}H^{14}O^{7}}+H^{2}O=2[CO^{2}+\underset{\text{orcine.}}{C^{7}H^{8}O^{2}}].$$

Pseuderythrine: by boiling with alcohol and an alkali, lecanorine gives carbonate, orcine and pseuderythrine:

$$\underset{\text{lecanorine}}{C^{16}H^{14}O^{7}}+\underset{\text{alcohol}}{C^{2}H^{6}O}=CO^{2}+\underset{\text{orcine}}{C^{7}H^{8}O^{2}}+\underset{\text{pseuderythrine.}}{C^{10}H^{12}O^{4}}.$$

The alkalies when boiling transform pseuderythrine into carbonate, alcohol and orcine:

$$\underset{\text{pseuderythrine}}{C^{10}H^{12}O^{4}}+H^{2}O=CO^{2}+\underset{\text{alcohol}}{C^{2}H^{6}O}+\underset{\text{orcine.}}{C^{7}H^{8}O^{2}}.$$

The formulæ by which the authors represent lecanorine and pseuderythrine agree perfectly with the analyses of MM. Schunck, Liebig, Kane, Rochleder and Heldt.

As to orceine, which is a red uncrystallizable colouring matter, the following relations agree very well with the analysis of M. Dumas:

$$\underset{\text{orcine}}{C^{7}H^{8}O^{2}}+O^{3}+NH^{3}=2H^{2}O+\underset{\text{orceine.}}{C^{7}H^{2}NO^{3}}.$$

Comptes Rendus, Août 1848.

ON PSEUDOQUINA—A NEW ALKALOID. BY M. MENGARDUQUE.

M. Pelouze had in his laboratory an extract of cinchona, the source of which was uncertain; he gave it to the author for examination as an exercise. This substance was of a deep brown colour, friable, very bitter, slightly soluble in water, soluble in acids, which it saturated like the alkaloids, and formed true saline solutions, from which water precipitated it as a pitchy mass. This matter, treated by the processes for the extraction of quina and cinchonia, did not yield the least trace of either of these alkaloids; nor was the cinchovatine of M. Manzine met with in it; but the author was so fortunate as to discover an alkaloid which he believes to be new, and which he so described as to leave no doubt in the mind of M. Pelouze, who witnessed his experiments.

This alkaloid differs from the substances by which it is accompanied in the extract, in saturating acids more perfectly, insomuch that it expels ammonia from its compounds, like lime or barytes; it scarcely dissolves even in boiling æther, and of these properties advantage was taken in order to effect its separation.

The extract was boiled with an equal weight of hydrochlorate of ammonia till ammonia ceased to be evolved. On cooling, a very abundant brown matter was deposited of a syrupy consistence, upon which floated a limpid liquid of a light amber colour. This liquor, poured off and filtered, was precipitated by ammonia.

The product thus obtained was yellowish and flocculent, susceptible of softening, and agglutinating by heat. It was dried and treated with cold æther, which dissolved the greater part of it, and left a pulverulent white matter, which was the new alkaloid in a state of purity.

This product, thus purified, possessed the following characters: subjected to heat on platina foil it fuses, and then burns with a blue flame without leaving any residue. It is insoluble in water and insipid, soluble in alcohol, and much more so when hot than cold; its alcoholic solution readily crystallizes in irregular prisms; it is soluble in the mineral and in organic acids, even diluted; it is insoluble in æther.

Ammonia, potash, and soda precipitate it from its saline solutions; water precipitates it from solution in alcohol. Lastly, if it be dissolved in aqueous solution of chlorine, and ammonia be added, the liquor assumes a reddish-yellow colour. It is well known that quina similarly treated yields a green solution.

Its solution in sulphuric acid may be rendered neutral to litmus paper; it is but slightly bitter. By evaporation it yields fine crystals, which are flattened prisms beveled at the summits.

The solution in hydrochloric acid had all the properties of an hydrochlorate, but it could not be made to crystallize.

By analysis it yielded—

	I.	II.
Carbon..........	76·5	76·7
Hydrogen........	8·1	8·2
Nitrogen	10·2	10·4
Oxygen	5·2	4·7
	100·0	100·0

The author concludes from the chemical and physical properties of this substance, and especially from its composition, that it is a new alkaloid.—*Comptes Rendus,* Août 1848.

PREPARATION OF META-ANTIMONIATE OF POTASH AS A TEST FOR SODA.

M. Fremy observes, that since the publication of his first memoir on the antimoniates, the meta-antimoniate of potash has been generally employed in laboratories as a test of the salts of soda; and as the preparation of this salt has been found difficult by several chemists, the author states the following to be the process which he now employs, and by which he obtains in a few hours nearly two pounds of meta-antimoniate of potash.

He begins by acting upon one part of antimony with four parts of nitre in a red-hot earthen crucible; insoluble anhydrous antimoniate of potash is formed, which is washed with cold water to remove the nitrite and nitrate of potash, an excess of which it usually retains.

The antimoniate of potash is then boiled for two or three hours in water, in order to convert it into the gummy soluble antimoniate; water is to be added to supply the loss by evaporation. During ebullition the greater part of the antimoniate dissolves, there remaining but a small quantity of bi-antimoniate of potash, which is separated by the filter.

The solution of the gummy antimoniate of potash is then evaporated, adding to it several fragments of pure hydrate of potash so as to render it very caustic. A few drops of the solution are tried from time to time to see whether on cooling they become crystalline; and when this takes place the evaporation is to be discontinued, the meta-antimoniate of potash then crystallizing abundantly; the alkaline solution is to be poured off, and the salt is to be dried on porcelain plates.

This salt always contains an excess of alkali; it ought to be washed two or three times before it is used as a reagent. As the meta-antimoniate of potash decomposes in solution in water, it is proper to keep it in the dry state, and to dissolve it at the moment in which the trial is to be made.

To make an examination, which scarcely requires ten minutes, about fifteen grains of the potash to be tried should be dissolved in a small quantity of water and supersaturated with hydrochloric acid, and the solution is to be evaporated to dryness in a porcelain or platina capsule. The chloride of potassium, being then perfectly neutral, is redissolved in water and treated with the solution of meta-antimoniate of potash. If the potash contains 2 or 3 per cent. of soda, a precipitate is almost instantly formed; but if the quantity be smaller, time and agitation will be necessary to effect precipitation.

M. Fremy states the sensibility of this reagent to be so great, that he found by synthetic experiments he could detect a half per cent. of carbonate of soda in commercial potash.—*Ann. de Ch. et de Phys.*, Août 1848.

SOLUBILITY OF THE HYDRATES OF COPPER AND CHROMIUM, ETC. IN POTASH AND SODA.

M. Fremy observes, that when hydrate of copper, precipitated from the sulphate by great excess of potash, is washed and dried *in vacuo*, it is represented, according to his analysis, by the formula CuO, 2HO. In this state it is perfectly soluble in concentrated solutions of potash and soda, as stated by Proust, forming solutions of a fine blue tint, the hydrate of copper thus consequently acting as an acid. If these solutions be subjected to ebullition, the hydrated oxide of copper is decomposed; the oxide, becoming anhydrous, completely loses its solubility in the alkalies, and precipitates.

The influence which water appears to exercise on the acidity of certain hydrates, is shown more remarkably with the sesquioxide of chromium. If a salt of this oxide be precipitated by potash, and the precipitate be washed with cold water and then dried in a current of dry air at common temperatures, a hydrate is obtained which is totally soluble in the alkalies, forming soluble green salts which have been properly called *chromites*. This hydrate is composed of—

Hydrated oxide of chromium	0·519
Water	0·268

It contains therefore 51·6 per cent. of water. Representing this hydrate by Cr^2O^3, 9HO, theory would give 51·4 per cent. of water.

When a solution of chromite of potash is boiled for a few minutes, it is decomposed; a green precipitate of sesquioxide of chromium is formed, which is entirely insoluble in the alkalies. This precipitate was by repeatedly washing freed from potash, and yielded by analysis—

Hydrated oxide	0·729
Water	0·360

It contained therefore 49·3 per cent. of water. Representing this hydrate by the formula Cr^2O^3, 8HO, theory would give 48·4 per cent. of water.

It appears, then, that the elimination of only one equivalent of water is sufficient to completely modify the hydrate of sesquioxide of chromium, and to render it insoluble in the alkalies. This hydrate becomes anhydrous by heating to 266° F.

This example will suffice to show the influence which water can exert over the properties of a metallic oxide; since, according to its proportion, it determines its solubility or non-solubility in the alkalies; it shows also the difficulties attendant upon the study of hydrates. M. Fremy states that he has often seen hydrates lose a certain quantity of their water at common temperatures during washing, or even by the action of light alone, and thus acquire properties entirely new. The instability of certain hydrates may be compared to that of some metallic peroxides.

The preceding hydrates are not the only ones which owe their acid character to the presence of the water which they contain. The hydrate of protoxide of tin, which dissolves, as well known, so readily in potash and soda to form *stannites*, loses its solubility in the alkalies when it becomes anhydrous.

The hydrates of the oxides of antimony, zinc and lead, which dissolve so readily even in very dilute alkaline solutions, can be dissolved by them only when concentrated, if rendered anhydrous.

It must then be admitted that hydrates often lose their acid character on becoming anhydrous. As this modification of the properties of a hydrate may be effected at common temperatures, and as it is always accompanied with the disengagement of water, it cannot be attributed to those isomeric changes which certain oxides undergo when they are calcined, and which M. Chevreul has with reason compared to the coagulation of albumen, and describes under the general denomination of *phénomènes de cuisson.*

When a hydrated oxide dissolves in potash or soda, is it to be understood that it combines with the base in the state of hydrate to form a ternary group composed of water, of base and of acid? In order to resolve this question, it is requisite to obtain crystalline compounds of the preceding hydrates with alkaline bases, and this M. Fremy states he has not yet been able to accomplish.

When a solution of an hydrated oxide in an alkali is suffered to evaporate even *in vacuo*, a period arrives at which the hydrate loses its water and precipitates. The author states that other combinations which he has described furnish incontestable proofs of the existence of saline groups, which would be impossible without the presence of water, and in which the oxide appears to be combined in the state of hydrate.—*Ann de Ch. et de Phys.*, Août 1848.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1848.

Chiswick.—August 1. Heavy showers. 2. Very fine. 3. Rain. 4. Cloudy and fine. 5. Heavy showers. 6. Fine: cloudy: showery. 7. Very fine. 8. Fine: rain. 9. Clear: showers: clear. 10. Very fine: rain at night. 11. Overcast: rain. 12. Cloudy. 13. Overcast: rain. 14. Heavy rain. 15. Densely clouded. 16. Foggy: overcast: clear. 17. Hazy and damp: overcast. 18. Very fine. 19. Rain. 20. Very fine. 21. Heavy rain: showery. 22. Heavy shower: clear. 23. Fine. 24. Showery: clear, with lightning at night. 25. Hazy: rain: cloudy and fine. 26. Uniformly overcast: rain. 27. Rain. 28. Cloudy: slight rain. 29. Fine. 30. Very fine. 31. Very fine till 5 P.M., when a very heavy thunder-storm commenced and continued upwards of two hours—an inch of rain falling in the time.

Mean temperature of the month	58°·74
Mean temperature of Aug. 1847	62 ·47
Mean temperature of Aug. for the last twenty years	62 ·32
Average amount of rain in Aug.	2·41 inches.

Boston.—Aug. 1. Cloudy: rain early A.M. 2. Fine. 3. Rain: rain A.M. 4. Fine. 5. Rain: rain A.M. 6. Fine: rain P.M. 7, 8. Fine. 9. Fine: hail-storm and rain A.M. 10, 11. Fine. 12. Fine: rain P.M. 13. Fine. 14. Rain. 15. Fine. 16, 17. Cloudy. 18. Fine: rain P.M. 19. Cloudy: rain A.M. and P.M. 20. Fine. 21. Rain: rain A.M. and P.M.: very stormy. 22. Cloudy: rain A.M. and P.M. 23, 24. Fine. 25. Fine: rain P.M. 26. Cloudy: rain A.M. 27. Cloudy: rain P.M. 28. Cloudy. 29—31. Fine.

Applegarth Manse, Dumfries-shire.—Aug. 1. Fine: one slight shower. 2. One shower. 3. Showery. 4. Slight showers. 5. Fine throughout: thunder. 6. Fine: shower P.M. 7. Fine and fair. 8. Heavy rain all day: thunder: flood. 9. Frequent showers. 10. Frequent showers: thunder. 11, 12. Fair and fine. 13. A few drops: fine. 14, 15. Fair and fine. 16. Fair and fine: a few drops P.M. 17. Heavy rain A.M.: cleared and fine. 18. Fine A.M.: heavy rain P.M. 19. Heavy rain A.M.: very high flood. 20. Fine, but cloudy A.M.: shower P.M. 21. Dull A.M.: rain P.M. 22. Showers. 23. Gentle showers: thunder. 24. Showers. 25. Fair and fine. 26. Complete day of rain: flood. 27. Fair and fine. 28. Rain: cleared P.M. 29. Fine till 6 P.M.: then heavy rain. 30. Fair till 11 A.M.: rain and hail P.M. 31. Very fine day throughout.

Mean temperature of the month	53°·7
Mean temperature of Aug. 1847	57 ·1
Mean temperature of Aug. for the last twenty-five years .	57 ·1
Rain in Aug. 1847	2·23 inches.
Average amount of rain in Aug. for twenty years	3·60 „

Sandwick Manse, Orkney.—Aug. 1, 2. Bright: clear. 3. Cloudy: clear. 4—6. Bright: clear. 7. Cloudy: clear. 8. Bright: clear: aurora. 9. Bright: rain. 10. Showers. 11. Showers: clear: cloudy. 12. Cloudy: clear. 13. Clear. 14, 15. Fine. 16. Bright: frost: clear. 17. Rain: clear. 18. Clear: cloudy. 19. Rain: showers. 20. Showers: clear. 21. Clear: aurora. 22. Clear: cloudy. 23. Showers. 24. Clear. 25. Clear: frost: fine. 26. Cloudy: showers. 27. Clear. 28. Cloudy: clear: drops. 29. Clear: drops. 30. Clear; showers. 31. Bright: cloudy.

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.
1848. Aug.	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.	29·669	29·369	28·82	29·19	29·41	29·36	29·44	69	50	64	64½	50	56	56	sw.	w.	wnw.	calm	·34	·52	......	......
2.	29·949	29·880	29·33	29·62	29·69	29·57	29·60	67	44	61	60½	49½	56	56	sw.	w.	w.	sw.	·10	·08	......	·06
3.	29·856	29·707	29·34	29·64	29·58	29·57	29·60	68	43	59	60	47	58	54	sw.	w.	w.	calm	·15	·02	......	......
4.	29·671	29·652	29·17	29·53	29·50	29·53	29·55	73	48	62	63	40	58	54	sw.	w.	w.	calm	·05	......	......	......
5.	29·532	29·429	29·03	29·38	29·27	29·50	29·44	71	50	57	67	42½	58	54	sw.	s.	ne–se.	calm	·11	·38	......	·04
6.	29·775	29·639	29·12	29·45	29·48	29·50	29·56	72	46	63	66½	44	60	54	sw.	w.	sw.	e.	·12	·77	......	·09
☽ 7.	29·972	29·924	29·40	29·72	29·78	29·70	29·80	73	40	60·5	62½	47½	58	56	sw.	w.	sw.	ne.	......	·13	......	......
8.	29·896	29·763	29·42	29·75	29·68	29·85	29·87	68	45	63	54	48½	58	54	s.	w.	sw.	ne.	·13	......	1·50	......
9.	29·851	29·803	29·26	29·65	29·70	29·80	29·83	69	40	62·5	61½	46½	54	50	sw.	w.	w.	n.	·23	......	......	......
10.	30·022	29·952	29·40	29·80	29·83	29·80	29·89	74	43	61	62½	45½	53	50	sw.	w.	w.	nw.	·15	·16	......	·38
11.	30·054	29·994	29·53	29·90	29·91	29·94	29·95	70	52	62	63	41½	53	52	sw.	w.	w.	w.	·19	......	......	·09
12.	30·018	29·963	29·48	29·91	29·89	29·88	29·91	73	54	63	63	43½	54	47½	n.	w.	var.	n.	......	......	......	·05
13.	29·990	29·951	29·47	29·90	29·94	29·97	30·05	66	53	61	60	43	51	47½	n.	w.	ene.	nnw.	·21	·08	......	·03
○ 14.	29·872	29·711	29·48	29·92	29·85	30·05	30·04	69	53	55	60	40	52½	48	e.	ne.	ene.	ne.	·68	......	......	......
15.	29·896	29·846	29·45	29·88	29·87	30·04	30·04	68	55	60	61	43½	53½	47	ne.	ene.	ene.	se.	......	·76	......	......
16.	29·896	29·759	29·38	29·80	29·74	29·92	29·82	70	51	61	63	45½	54½	52	e.	e.	ene.	se.	·12	......	0·45	......
17.	29·956	29·881	29·38	29·60	29·79	29·49	29·71	65	44	59	61½	55	54½	52½	sw.	e.	wnw.	w.	·02	......	......	·24
18.	30·016	29·884	29·58	29·91	29·50	29·87	29·73	75	54	57	62	40	57	53	s.	w.	s–sse.	se.	......	......	......	·03
19.	29·841	29·718	29·18	29·36	29·53	29·09	29·23	69	42	62	60½	50	59	52	sw.	s.	var.	sw.	·06	·07	......	·50
20.	29·960	29·882	29·37	29·66	29·68	29·43	29·58	71	45	59	60½	46½	55½	54	sw.	wsw.	w.	sw.	·26	·09	......	·16
☾ 21.	29·718	29·478	29·21	29·39	29·29	29·53	29·44	69	51	53·5	54	47½	54½	50½	sw.	s.	sw.	calm	·17	·15	......	·04
22.	29·643	29·577	29·05	29·30	29·32	29·38	29·43	65	44	56	56	48½	54	49	sw.	sw.	w.	wnw.	·06	·09	......	......
23.	29·797	29·527	29·14	29·45	29·62	29·48	29·69	70	42	57	60	45½	49	49	sw.	w.	sw.	w.	·04	·25	1·70	·10
24.	30·020	29·887	29·39	29·74	29·82	29·75	29·84	64	41	55	58½	41	53	50½	w.	nw.	w.	w.	·03	......	......	......
25.	30·034	30·005	29·55	29·88	29·80	29·85	29·83	66	50	56	61½	35½	55	51½	sw.	w.	s.	se.	·01	·05	......	......
26.	29·837	29·815	29·34	29·54	29·40	29·57	29·32	70	54	58	60	51½	56½	57½	sw.	s.	se.	s.	·04	·03	......	......
27.	29·839	29·816	29·24	29·60	29·56	29·53	29·54	69	63	68	63	51½	56	51	sw.	wsw.	sw.	s.	·01	......	......	·07
● 28.	29·956	29·821	29·20	29·47	29·63	29·36	29·41	74	56	68	61½	53	56½	54	sw.	wsw.	sw.-s.	s.	·10	·07	......	......
29.	29·995	29·970	29·44	29·70	29·68	29·49	29·56	70	47	62	61½	54	57	54	s.	sw.	sw.	ssw.	·01	......	......	·10
30.	30·050	30·022	29·48	29·78	29·89	29·69	29·90	72	45	59·5	60	49	54	51	sw.	sw.	wsw.	nw.	......	......	1·25	......
31.	30·058	30·029	29·57	30·00	30·05	30·03	30·03	69	48	57	61	41	52	52	n.	sw.	e–sw.	ssw.	1·31	......	......	·06
Mean.	29·891	29·795	29·33	29·658	29·376	29·661	29·697	69·32	48·16	60·0	61·0	46·0	55·19	52·04					4·70	3·70	4·90	2·04

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XLIX. *Description of the Photographometer, an instrument for measuring the Intensity of the Chemical Action of the Rays of Light on all the Photographic preparations, and for comparing with each other the sensitiveness of these different preparations. Invented by* A. Claudet, *London, September* 1848*.

[With a Plate.]

We are not yet in possession of an instrument capable of measuring the variations in intensity of the sun's light with the same precision that we appreciate those of its heat. In photographic operations the want of a photometer has long been felt; and it was soon perceived that it was not sufficient to measure the light, properly so called, but certain accompanying rays, which exert on different bodies a chemical action without producing on the retina the sensation by means of which objects appear to us visible. All photographers know by experience that the chemical action is not exactly in proportion to the intensity of the visible light.

Many ingenious contrivances have been resorted to for indicating the intensity of the chemical or actinic rays existing at a given moment; but as the object was to study more particularly the variations of the direct solar rays, it was found necessary to have recourse to complicated instruments furnished with heliostats and clock-work movements.

In 1839 Mr. T. B. Jordan of Falmouth invented an instrument which he called a heliograph. This instrument was imperfect. It was modified and improved by Mr. R. Hunt, who has devoted much time to photographic processes and researches on the properties of the different rays of light.

The apparatus of Mr. Hunt, which he called an actinograph, consists of a cylinder placed parallel to the axis of the

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* This paper has been communicated to the Académie des Sciences, October 9.

ecliptic, having a triangular opening and turning upon its axis, following the sun's path, clock-work movement being used for the purpose.

The photogenic paper is rolled upon another cylinder placed in the interior of the first, and turning in a contrary direction. The sun's rays, passing through the triangular opening, fall successively upon all that portion of the paper exposed by the rotation of the exterior cylinder.

The photogenic effect produced upon the paper is in direct proportion to the intensity of the light present during the motion of the triangular opening, and also in direct proportion to the length of the lines parallel to the base of the triangle at every part of its opening; so that as the intensity of the light increases or diminishes, the commencement of the photogenic effect is indicated at a point more or less distant from the base. At the end of the day, on removing the paper we find a long figure, the height of the ordinates of which is the measure of the intensity of the chemical action of the light.

Mr. Hunt described his actinograph at the meeting of the British Association at Cambridge in June 1845.

Sir John Herschel in 1840 constructed an instrument for the same purpose. He places photogenic paper under an immoveable disc, having a narrow opening in the direction of its radius for the passage of the sun's light reflected by a heliostat: this light falls on the paper fixed upon another disc, turning on its axis by clock-work.

At the end of the day every part of the photogenic paper has passed this opening, and the effect produced shows the intensity of the light during the various hours from sunrise to sunset.

All these several ingenious contrivances had no other object than that of measuring the photogenic intensity of the direct rays of the sun during the different hours of the day. Besides, they were applicable only to photography on paper; and the results were uncertain, because the paper does not long preserve the same degree of sensitiveness: then the heliostats and clock-work movements rendered the apparatus complicated, expensive and difficult to use.

It may then be asserted, that up to the present time there does not exist an instrument for indicating to the photographer the intensity of the chemical rays, and at the same time the sensitiveness of his preparation.

I have endeavoured to supply this want, and offer the result of my labours in publishing an instrument which, I believe, fulfils all the conditions necessary to render it useful, as it measures at the same time the intensity of the chemical rays and the sensitiveness of the preparation. I have called it a *photographometer*, because it indicates the combined results of the photographic operation.

My apparatus is very simple, and serves equally for processes on paper or on metallic plates. It indicates the intensity of the chemical rays at all moments of the day during atmospheric variations, and at the instant we may wish to operate. It serves also to compare the degree of sensitiveness of the different photographic preparations.

For an instrument of this kind (see Plate II. fig. 1), it is important in the first place to have a motion always uniform, without complicated or expensive mechanism. This I have obtained by a means founded upon the principle of the fall of bodies sliding down an inclined plane. The sensitive surface is exposed to the light by the rapid and uniform passage of a metal plate AB, fig. 2, having openings of different lengths which follow a geometric progression. It is evident that the exposure to light will be the same for each experiment, because the plate furnished with the proportional openings falls always with the same rapidity, the height of the fall being constant, and the angle of the inclined plane the same. Each opening of this moveable plate allows the light to pass during the same space of time, and the effect upon the sensitive surface indicates exactly the intensity of the chemical rays. The rapidity of the fall may be augmented or diminished by altering the inclination of the plane by means of a graduated arc CD, fig. 1, furnished with a screw E, by which it may be fixed at any angle. The same result may be obtained by modifying the height of the fall or the weight of the moveable plate. The photogenic surface, whether it be the Daguerreotype plate, the Talbotype paper, or any other preparation sensitive to light, is placed near the bottom of the inclined plane F, figs. 1 & 2. It is covered by a thin plate of metal pierced with circular holes, which correspond to the openings of the moveable plate at the moment of the passage of the latter, during which the sensitive surface receives the light wherever the circular holes leave it exposed.

The part of the apparatus which contains the sensitive surface is an independent frame, and it slides from a dark box into an opening on the side of the inclined plane. The fig. 3 represents the frame and the dark box, and by its inspection the manner will be understood in which the sensitive surface is placed in it before the experiment and carried to the mercury-box after the operation.

A covering of black cloth impermeable to light is attached to the sides of the moveable plate enveloping the whole inclined plane, rolling freely over two rollers R, R′ placed, the one at the upper and the other at the lower part of the inclined plane. This cloth prevents the light striking the sensitive surface before and after the passage of the moveable plate.

The openings of the moveable plate are parallel to each other, and in the direction of the inclined plane. They are seven in number. The first is 1 millimetre; the second, 2 millim.; the third, 4 millim.; the fourth, 8 millim.; the fifth, 16 millim.; the sixth, 32 millim.; and the seventh, 64 millim. Each opening is then the half of the following, and double of the preceding one. After the operation we have seven (or less, according to the intensity of light) separate images, the different intensities of which represent the action of the light during intervals of time in the geometric progression of :: 1 : 2 : 4 : 8 : 16 : 32 : 64. These numbers are quite sufficient for general observation; we can multiply the effects by allowing the plate to fall twice, thrice, or a greater number of times. I adopted this progression because I found an arithmetic progression did not give an appreciable difference in the intensity of the photogenic effect, a doubling of the action being required to produce a marked difference of effect. If we wish to compare the photogenic intensity of the sun's rays with those of the moon, we allow the moveable plate to fall a number of times sufficient for the moon's rays to produce an action, which may be rendered apparent by the mercury. A few trials suffice to obtain an effect from which may be calculated the proportion of the intensities.

The moveable plate containing the parallel proportionate openings is furnished with a flap which opens and shuts freely, and by which the openings may be protected from light when the plate is to be raised again in order to fall a second time.

A simple apparatus thus constructed will be a photographometer certain for every preparation. It will be an index and an infallible guide for the operator. It will indicate the intensity of the photogenic light, and also the sensitiveness of the preparation. But it becomes still more useful when constructed double; that is to say, when the moveable plate is furnished with two equal series of proportional openings placed in a line perpendicular to the direction of the inclined plane, and the fixed plate pierced with two series of holes corresponding to the openings in the moveable plate.

By placing beneath each series of holes a different sensitive surface, each of these surfaces will, during the fall of the moveable plate, receive the same proportion of the same light, and thus their different degrees of sensitiveness may be compared. In this manner we learn the comparative sensitiveness of different preparations of the iodide, of the bromo-iodide and chloro-iodide of silver, and of the various photogenic papers; for it is indispensable, in making an exact comparison, to operate with the same light and during strictly the same space of time, as it is known that the light varies from one minute to another.

When we only wish to make experiments of comparison with different preparations, a much more simple and portable apparatus will serve as well. In this the plate furnished with parallel openings is immoveable, and fixed in the centre of a frame having grooves underneath to keep it in contact with another frame, in which we place the two sensitive surfaces we wish to compare. Two plates, having round holes for the passage of the light, cover the sensitive surfaces, which are held by a shutter fastened at the back by a button. This part of the apparatus resembles a Daguerreotype "*passe partout*," with this difference, that in the place of the glass we have a metallic plate furnished with holes.

When the apparatus has been charged in a room shaded from the daylight, it is carried out, and the frame containing the sensitive surfaces is made to slide along by the hand, while the frame containing the proportional openings is exposed to the daylight or to the sun. With a little practice, a movement may be obtained regular enough to measure by this simple apparatus, with sufficient exactness, the intensity of the light.

By the double apparatus, whether it has the uniform motion given by the inclined plane, or that less regular given by the hand, we shall decide a very interesting question; which is, to know if each variation in the quantity and quality of light affects equally the different photogenic preparations. We shall find, perhaps, that certain lights require different preparations or proportions to obtain the maximum of sensitiveness.

In place of a single series of round holes upon each plate, we can have two, three, four or five, which enable us to make several experiments upon the same surface. On the paper, for instance, we can prepare a zone for each series of holes with a different substance; and on the Daguerreotype plate we may vary the coatings of iodine, bromine or chlorine, so that the same surface will contain two, three, four, or five experiments of comparison, serving to indicate the best method of preparation.

The plate, fig. 4, shows the result of an experiment of this kind, and is the exact representation of the plate itself after it has received the mercurial vapour. The plate had been exposed to the vapour of iodine in such a manner that one zone had attained the first coating of yellow colour, a second zone had reached the red, a third the blue-green, and the fourth, after having passed through all these tints, had obtained the second yellow coating. The number of white circular spots on each vertical zone indicates the degree of sensitiveness of these various coatings; the less sensitive being the first coating of yellow, and the most sensitive the second coating of the same colour, which I call the double coating of iodine.

Therefore, we have an instrument by which we can decide, in an incontestable manner, upon the merits of different accelerating liquids used in the Daguerreotype, resolve a great number of problems, and make experiments, from which will infallibly result the simplification and improvement of the art of photography, which up to the present time has suffered too much from empiricism and charlatanism.

Appended to this description are a drawing of the apparatus, and the plans necessary to explain its action and construction. (See Plate II.)

I am still engaged in making experiments with this apparatus; as soon as they are completed I shall hasten to communicate the result of my labours. I content myself for the present with announcing a very extraordinary fact which my apparatus has furnished me with.

I do not give it as the result of a calculation mathematically correct; but I cannot be far from the truth in stating, that the pure light of the sun modifies the bromo-iodized silver plate, communicating to it an affinity for mercurial vapour which produces the white image in the Daguerreotype, in a space of time which cannot be much more than the thousandth part of a second. I made the experiment in the following manner:—I let the light of the sun fall upon the plate through an opening of a millimetre, whilst this opening passed over a space of 250 millimetres in one quarter of a second, as near as I could judge; this light could not therefore have acted on the plate during much more than the $\frac{1}{1000}$dth part of a second, and nevertheless this inconceivably short space of time sufficed to produce a decided effect.

It is not necessary that I should indicate all the applications of this photographometer, and the experiments which may be made with it. Without doubt many will be discovered which have not occurred to me; but I will enumerate a few which appear to me important enough to merit the attention of philosophers:—What is the effect of the compound light, and that of the different separated rays of the solar spectrum? How much photogenic light is lost by reflexion from parallel mirrors, prisms and other substances, and by refraction through lenses? The proportion of photogenic rays in the lights obtained from various sources, including that produced by electricity? If the photogenic light varies with the height of the atmosphere and with the changes of temperature? If it is affected by the electrical state of the atmosphere? In fine, what is the proportion of the photogenic rays at each hour of the day, and at different points in space at a given moment?

L. *On the Extension of the Theorem of* Leibnitz *to Integration.*
By J. R. Young, *Professor of Mathematics, Belfast*[*].

THE following method of showing the applicability of the theorem of Leibnitz to successive integration, though new to me, may possibly be found in some works on the integral calculus which I have not seen. I venture however to give it here, chiefly because it suggests a consideration, in reference to the "Calculus of Operations," which is perhaps deserving of notice.

If we put du_1 for udx, du_2 for u_1dx, du_3 for u_2dx, and so on, and apply "integration by parts" to the differential $vudx$, we shall have

$$\begin{aligned}\int vudx &= vu_1 - \int \frac{dv}{dx}u_1dx\\ &= vu_1 - \frac{dv}{dx}u_2 + \int \frac{d^2v}{dx^2}u_2dx\\ &= vu_1 - \frac{dv}{dx}u_2 + \frac{d^2v}{dx^2}u_3 - \int \frac{d^3v}{dx^3}u_3dx\\ &= vu_1 - \frac{dv}{dx}u_2 + \frac{d^2v}{dx^2}u_3 - \frac{d^3v}{dx^3}u_4 + \ldots\ldots \frac{d^{m-1}v}{dx^{m-1}}u_m\\ &\qquad \pm \int \frac{d^mv}{dx^m}u_m dx,\end{aligned}$$

the sign of this last quantity being opposite to that of the term immediately preceding it, and which term, here regarded as the mth term, may be any whatever in the series. Thus far there is nothing new.

Integrating again, and proceeding on the same principles, omitting however the supplementary integral, which, as above, should terminate each of the following rows, we shall have

$$\begin{aligned}\int^2 vudx^2 = vu_2 - \frac{dv}{dx}u_3 + \frac{d^2v}{dx^2}u_4 - \frac{d^3v}{dx^3}u_5 + \ldots\ldots\\ -\frac{dv}{dx}u_3 + \frac{d^2v}{dx^2}u_4 - \frac{d^3v}{dx^3}u_5 + \ldots\ldots\\ + \frac{d^2v}{dx^2}u_4 - \frac{d^3v}{dx^3}u_5 + \ldots\ldots\\ - \frac{d^3v}{dx^3}u_5 + \ldots\ldots\\ \&c. \qquad \&c.\end{aligned}$$

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[*] Communicated by the Author.

that is, still omitting the supplementary integrals,

$$\int^2 vudx^2 = vu_2 - 2\frac{dv}{dx}u_3 + 3\frac{d^2v}{dx^2}u_4 - 4\frac{d^3v}{dx^3}u_5 + \&c.$$

And by repeating this process for a third integration, a fourth, and so on, it is obvious, from the law of their formation, that the numerical coefficients will be those furnished by the corresponding negative powers of a binomial; so that, by restoring the original symbols for u_1, u_2, u_3, &c., we shall have

$$\left.\begin{aligned}\int^n vudx^n = v\int^n udx^n - n\frac{dv}{dx}\int^{n+1} udx^{n+1} + \frac{n(n+1)}{2}\frac{d^2v}{dx^2} \\ \int^{n+2} udx^{n+2} - \frac{n(n+1)(n+2)}{2.3}\frac{d^3v}{dx^3}\int^{n+3} udx^{n+3} + \&c.\ldots\ldots\end{aligned}\right\}\text{(A.)}$$

which is the form in which the theorem is usually given.

From the suppression of the supplementary integrals, it is plain that this theorem cannot be generally true. In fact, it is strictly accurate only when v is such a function that one of its differential coefficients, and therefore every one that follows, becomes zero.

If we supply the omissions which have been made, and employ the former notation for brevity, we shall have

$$\int^2 vudx^2 = vu_2 - 2\frac{dv}{dx}u_3 + 3\frac{d^2v}{dx^2}u_4 - 4\frac{d^3v}{dx^3}u_5 + \ldots\ldots m\frac{d^{m-1}v}{dx^{m-1}}u_{m+1}$$

$$\pm m\int\frac{d^mv}{dx^m}u_{m+1}dx \mp \int^2\frac{d^mv}{dx^m}u_m dx^2$$

$$\int^3 vudx^3 = vu_3 - 3\frac{dv}{dx}u_4 + 6\frac{d^2v}{dx^2}u_5 \ldots\ldots \text{ to } m \text{ terms}$$

$$\pm\frac{m(m+1)}{2}\int\frac{d^mv}{dx^m}u_{m+2}dx \mp m\int^2\frac{d^mv}{dx^m}u_{m+1}dx^2 \pm \int^3\frac{d^mv}{dx^m}u_m dx^3,$$

and so on; the leading sign in the lower series being the contrary sign to that with which the upper terminates, that is, contrary to the sign of the mth term, which may be any term arbitrarily chosen. In both series the signs are alternately plus and minus; and it is to be observed, that the coefficient with which the second or supplementary series commences is the same as that with which the first series terminates; for

$$\frac{m(m+1)}{2} = \frac{n(n+1)(n+2)\ldots.(n+m-2)}{1.2.3\ldots..(m-1)}.$$

If we call this mth coefficient M, the supplementary series for the nth integral will be

$$\left.\begin{aligned}\int^n\frac{d^mv}{dx^m}u_m dx^n \pm m\int^{n-1}\frac{d^mv}{dx^m}u_{m+1}dx^{n-1} \mp \frac{m(m+1)}{2} \\ \int^{n-2}\frac{d^mv}{dx^m}u_{m+2}dx^{n-2} \pm \ldots\ldots \text{M}\int\frac{d^mv}{dx^m}u_{m+n-1}dx\end{aligned}\right\}.\ .\ \text{(B.)}$$

The theorem here considered is usually treated by the "Calculus of Operations," a method which has deservedly received much attention of late. The employment of this refined principle of investigation requires, however, more than ordinary caution and circumspection: among other things it must be observed, that the theorems to which it leads cannot be generally true when they assume the form of series, whose character is such that, when the symbols of operation are replaced by those of quantity, divergency takes place. The present short paper illustrates this: the calculus of operations gives the theorem here discussed under the form (A.), and makes no provision for the important correction (B.).

Belfast, October 9, 1848.

LI. *On the absence of any Traces of the fall of Aërolites and of Glacial Action in the Strata formed before the last great modification of the Earth's surface. By* Lieut.-Colonel PORTLOCK, *R.E., F.R.S. &c.**

IN no science have the results of a close adherence to the sound principles of inductive reasoning been more striking than in geology during the last fifty years. So long as the mind was allowed to ramble unrestrained in the field of speculation, hypothesis supplied the place of facts; and the vision occasionally obtained of truth was like that of a dreamer, so confused and distorted as no longer to resemble any distinct reality; but when geologists abandoned this pursuit of an airy image, and began to search cautiously and laboriously for facts as the only sure basis on which any system of human knowledge could be founded, a new æra commenced, and a science has now been established which in beauty, interest and utility, is second to none. To this great end the work of Mr. Lyell, Principles of Geology, most powerfully contributed, by its advocacy in the strongest and most uncompromising manner, of the advantage of seeking only in existing causes (that is, in forces still in action) an explanation of past as well as of present phænomena. It is thus that not merely the true structure and the mode of existence of the strangest forms of extinct animals have been elucidated by a reference to the laws of anatomical structure and of organic functions, as exhibited in the types still existing, but the ripple-mark on the sea-shore or on the sandy bottom, the scar of the pattering rain-drop, and the action of the sea wave on the rocky cliff, have been carried back by various observers to the remotest epochs, so that the mind of the geo-

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* Being the substance of remarks made in the Geological Section of the British Association at Swansea.

logist is now familiarized with the fact that sea and land existed contemporaneously even at the period of the now metamorphic schists, and at each succeeding geological epoch, and is accustomed to strip off, as it were, the accumulations of more recent deposits, in order to study the condition of the earth's surface at a period antecedent to them. Whilst, however, we thus rightly and wisely adhere to the system of reasoning from those facts which we are enabled to study and estimate in all their bearings, we must not forget that there are also facts which we can only partially study and imperfectly estimate: for example, astronomy gives us strong reasons for believing that it is more than probable that whilst some sidereal and planetary systems may be coming into existence others are passing away even at the present moment, and that an exact balance in the universe has not been yet attained. It teaches us, therefore, that we cannot always measure the possible extent of action of any given force by the effects it exhibits at any one period, and that interfering or co-operating causes may often tend to diminish or to augment its energy. I have been led to make these remarks by the acccidentally simultaneous notice of the two subjects of this article, at the Geological Section of the British Association at Swansea; the first by the Marquis of Northampton, in describing some recently fallen aërolites, and the second by the Dean of Westminster, in again bringing forward his discovery of the traces of ancient glaciers in the Snowdonian chain of North Wales. It had been long familiar to my mind that geological investigation had hitherto discovered no traces either of the fall of aërolites or of glacial action in the ancient strata of the earth's surface, and I therefore made a few observations on the papers read, with a view to direct the general attention of geologists to the fact, or to draw from them any statements of observations tending to invalidate the conclusion, and to carry back to past systems those remarkable phænomena of the present. It appeared to me that none of the eminent geologists present were prepared to doubt the accuracy of my conclusion, so far as it depends on facts hitherto observed; but there seemed to be an impression that the number of falling aërolites is even now so small, that the chances of meeting with them are few, and that such chances must be still rarer in the examination of ancient strata. This opinion I do not consider to be well-founded, as it must be remembered that the study of these interesting bodies is comparatively recent, and that there is now every reason to believe that the number falling is very considerable, though from many natural circumstances a large portion of them may fall unobserved; and so far from the chances of observation being fewer in the examination of the ancient strata, I consider the probability of finding such bodies (had they fallen) in the numerous quarries opened in all parts of the world, and in the natural sections examined by so many geologists, much greater than when they are sought for on the present surface, obscured as it is by vegetation, or occupied by tillage. When I made the remark as regards the aërolites, I thought it original; but a note to Humboldt's Kosmos has shown me that the illustrious Olbers had made it before me, and I am even more gratified by finding that I had thus unconsciously dropped into the same train of thought with that acute philosopher. The passage occurs in his paper on falling stars in Schumacher's *Jahrbuch* for 1838; and as it is one of great interest, I shall extract it at length from the original, and not from the abbreviation by Humboldt.

"Finally, I may be permitted to mention a circumstance which has not hitherto, so far as I know, been taken into consideration by any one, and respecting which I would gladly learn the opinions of well-informed philosophers and be instructed by them. It is this, that neither fossil meteorite nor fossil meteoric iron has as yet been found. May we not, nay, must we not conclude from this fact, that before the last modification of the surface of our earth no meteorites had ever fallen upon it? Would they not indeed be somewhat frequently found in the secondary and tertiary strata if they had been projected downwards on the earth for thousands of years previously, when in the present time several hundreds of such falls of stones take place yearly? And although such aërolites may not have retained so perfectly their original form as the shells of mollusca and the bones and teeth of saurians and of other vertebrata, yet must they have continued sufficiently recognizable. I must confess that this fact appears to me of great importance as regards geology and cosmology, and deserves especial consideration in any inquiry into the nature, formation, and source of aërolites."

Such, then, is the opinion of Olbers; and though it is stated in a note to his paper that some fragments of aërolites had, by long exposure to moisture whilst imbedded in damp earth, been totally decomposed and reduced to a gray-looking substance mixed with peroxide of iron, it must be remembered that many shells and other organic bodies have been also destroyed and removed by decomposition, and that the total absence of aërolites in the ancient strata cannot therefore be accounted for by so partial a cause. Humboldt quotes two examples of bodies supposed to have been aërolites; but it does not appear from his statement whether they were found in gravel, or whether there was any proof that they had been previously imbedded in rocks; and I think, therefore, it must be admitted, that as yet there is no satisfactory proof of the fall of aërolites on the earth's surface prior to its last modification, or to the glacial period.

Let us now refer to my second subject, namely, the absence of any traces of glacial action in the ancient strata. Here it is only necessary to observe, that one effect of glacial action, namely, the transport of large angular fragments of rock by floating ice, is still exhibited to us by the regular progression of large masses of polar ice loaded with fragments of rocks from the arctic towards the tropical regions; and there cannot be a doubt that many such blocks of large size must be every year dropped by the melting ice on the bottom of the ocean. From the greater extension of glaciers, which also still exist on the mountains, in the epoch preceding the present, it has been reasonably inferred that all glacial phænomena were in like manner more extended; and a sufficient cause has been thus acquired, in the floating ice of that epoch, for the production of the effects exhibited in many of our gravel beds, and in the remarkable phænomenon of the huge angular erratics scattered over the surface, irrespectively of the nature of the rock on which they rest. But when we pass this boundary, our evidence fails us; and although in the vicinity of a basaltic flow lumps of that subaqueous lava may be occasionally found imbedded in the strata close to it, or at the very base of the cliff where it was forming, some large fragments of rock may be found in a sandstone or conglomerate; the general character of the secondary formations is not such as to warrant us in concluding that ice had co-operated in their production, whilst the absence of 'erratics' or large angular blocks in such formations is a strong negative proof that ice did not then exist. Is not then this curious negative fact a remarkable confirmation of the positive evidence afforded by organic remains, that prior to the glacial period the whole earth enjoyed a temperature in some measure assimilated to that of its now tropical regions? It is true that in the more recent tertiary formations there is an approximation to the now existing conditions as to temperature; but it was only an approximation, and widely different in degree from that extraordinary state of depressed temperature which followed in the glacial period. It may then be asked, can the extraordinary change which produced the glacial period be explained or its cause assigned? and, in like manner, can the resumption of a comparatively genial temperature by the earth's surface be explained? Few philosophers, I believe, are satisfied by the explanation, however ingenious, of such changes, as given by Mr. Lyell, having been produced by alterations of the relative position of sea and land; and most philosophers look beyond the globe, and seek in great astronomical causes, that is in general physical causes, for its explanation. It is not, however, my intention to enter rashly on any such speculation, though I think myself justified in at least drawing attention to the simultaneity of the two great phænomena here alluded to, namely, the first fall of aërolites and the commencement of glacial action, leaving it to astronomers to decide how far it is possible, under the present most generally received theory of aërolites, as being either condensed nebulous matter, or planetary fragments moving regularly in an orbit, that either the one may have been the cause of the other, or both the effects of some one great common cause.

LII. *An Inquiry into the Amount of Inorganic Constituents contained in Ale and Porter. By* THOMAS DICKSON, *Manchester, late Assistant in the University Laboratory, Edinburgh*[*].

THE object in view in commencing the series of analyses given below, was to ascertain if a standard could be formed of the nutritive powers of the ales and porters in general use, in order that a correct estimate of their real value as an article of diet might be deduced.

The analyses show, as might have been expected, a very great variety in the amount of their respective constituents, partly arising from the various modifications each brewer finds himself under the necessity of adopting, from local circumstances over which he has no control, or from personal experience, though the main steps of the process are essentially the same throughout.

The ash consists of that portion of the grain, and other substances employed in the process, which is soluble in water; whilst owing to the various proportions of malt and water employed, to the length of time the infusion is continued, to the temperature at which the infusion is made, to the concentration the liquor undergoes in the boiling to which it is subsequently subjected, and to the process of fermentation, the proportional amount of the constituents varies considerably.

The method given by Fresenius and Will for the analysis of Ash was that followed in the present instance. The amount of iron has been omitted, as in most of the cases it was altogether wanting; and in the few instances in which it did occur, it was so minute as to be inappreciable, and apparently was only an accidental impurity.

In order to save needless repetition, the analyses have been calculated after the deduction of the carbon and moisture, and are as follows:—

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[*] Communicated by the Author.

No. I. Scotch ale at 3*s*. per gallon.

Potash . . .	24·547
Soda	34·429
Lime	1·203
Magnesia . .	0·399
Chlorine . .	5·095
Sulphuric acid .	2·131
Phosphoric acid	25·657
Silica	6·539
	100·000

No. II. Another variety of Scotch ale at 3*s*. per gallon.

Potash	26·603
Soda	35·715
Lime	0·555
Magnesia . . .	0·232
Chlorine . . .	9·754
Sulphuric acid .	2·272
Phosphoric acid .	20·005
Silica	4·864
	100·000

No. III. Another variety of Scotch ale at 3*s*. per gallon.

Potash . . .	20·925
Soda	53·083
Lime	0·172
Magnesia . .	0·331
Chlorine . . .	4·283
Sulphuric acid .	1·615
Phosphoric acid	12·513
Silica	7·078
	100·000

No. IV. Scotch ale at 2*s*. 6*d*. per gallon.

Potash	22·119
Soda	33·480
Lime	0·862
Magnesia . . .	4·642
Chlorine . . .	6·925
Sulphuric acid .	3·214
Phosphoric acid .	24·172
Silica	4·586
	100·000

No. V. Another variety of Scotch ale at 2*s*. 6*d*. per gallon.

Potash . . .	15·913
Soda	42·758
Lime	0·707
Magnesia . .	0·963
Chlorine . . .	10·085
Sulphuric acid .	4·657
Phosphoric acid	16·346
Silica	8·571
	100·000

No. VI. Scotch ale at 2*s*. per gallon.

Potash	23·710
Soda	46·085
Lime	1·979
Magnesia . . .	5·650
Chlorine . . .	3·052
Sulphuric acid .	0·130
Phosphoric acid .	12·049
Silica	7·345
	100·000

No. VII. Another variety at 2*s*. per gallon.

Potash . . .	10·484
Soda	48·394
Lime	0·257
Magnesia . .	2·269
Chlorine . . .	6·649
Sulphuric acid .	5·562
Phosphoric acid	15·333
Silica	11·052
	100·000

No. VIII. Scotch ale at 1*s*. 9*d*. per gallon.

Potash	27·564
Soda	40·520
Lime	1·341
Magnesia . . .	1·633
Chlorine . . .	8·121
Sulphuric acid .	7·014
Phosphoric acid .	7·923
Silica	5·884
	100·000

No. IX. Scotch ale at 1*s*. 6*d*. per gallon.

Potash	3·162
Soda	58·508
Lime	0·874
Magnesia	1·032
Chlorine	6·450
Sulphuric acid	6·277
Phosphoric acid	17·622
Silica	6·075
	100·000

No. X. Another variety at 1*s*. 6*d*. per gallon.

Potash	29·828
Soda	38·390
Lime	0·740
Magnesia	0·505
Chlorine	5·557
Sulphuric acid	2·773
Phosphoric acid	16·827
Silica	5·380
	100·000

No. XI. Scotch ale at 1*s*. 3*d*. per gallon.

Potash	31·907
Soda	20·871
Lime	6·694
Magnesia	1·558
Chlorine	9·454
Sulphuric acid	2·665
Phosphoric acid	16·851
Silica	10·000
	100·000

No. XII. Scotch ale at 1*s*. per gallon.

Potash	17·570
Soda	31·285
Lime	1·536
Magnesia	2·163
Chlorine	8·802
Sulphuric acid	7·072
Phosphoric acid	16·657
Silica	14·915
	100·000

No. XIII. Another variety at 1*s*. per gallon.

Potash	11·635
Soda	24·862
Lime	0·646
Magnesia	0·064
Chlorine	18·255
Sulphuric acid	14·566
Phosphoric acid	10·925
Silica	19·047
	100·000

No. XIV. Export ale at 3*s*. per gallon.

Potash	19·418
Soda	37·131
Lime	1·242
Magnesia	0·528
Chlorine	6·559
Sulphuric acid	19·160
Phosphoric acid	5·984
Silica	9·978
	100·000

No. XV. London porter at 3*s*. per gallon.

Potash	26·357
Soda	21·777
Lime	6·893
Magnesia	0·394
Chlorine	9·133
Sulphuric acid	6·615
Phosphoric acid	20·576
Silica	8·255
	100·000

No. XVI. Another variety at 3*s*. per gallon.

Potash	31·140
Soda	32·933
Lime	1·514
Magnesia	0·122
Chlorine	6·459
Sulphuric acid	4·637
Phosphoric acid	9·261
Silica	13·934
	100·000

No. XVII. London porter at 2*s*.6*d*. per gallon.

Potash	11·938
Soda	24·330
Lime	3·868
Magnesia	0·659
Chlorine	14·528
Sulphuric acid	12·198
Phosphoric acid	19·545
Silica	12·934
	100·000

No. XVIII. Dublin porter at 2*s*.6*d*. per gallon.

Potash	32·042
Soda	42·722
Lime	1·543
Magnesia	0·512
Chlorine	6·777
Sulphuric acid	1·574
Phosphoric acid	7·893
Silica	6·937
	100·000

No. XIX. Scotch porter at 2*s*. per gallon.

Potash	20·883
Soda	38·766
Lime	1·623
Magnesia	0·166
Chlorine	0·151
Sulphuric acid	6·358
Phosphoric acid	18·773
Silica	13·280
	100·000

No. XX. London porter at 2*s*. per gallon.

Potash	16·032
Soda	50·820
Lime	1·306
Magnesia	0·097
Chlorine	7·420
Sulphuric acid	4·526
Phosphoric acid	10·279
Silica	9·520
	100·000

No. XXI. Another variety at 2*s*. per gallon.

Potash	22·879
Soda	30·521
Lime	1·335
Magnesia	1·272
Chlorine	10·919
Sulphuric acid	4·973
Phosphoric acid	12·859
Silica	15·242
	100·000

No. XXII. Dublin porter at 2*s*. per gallon.

Potash	21·382
Soda	24·008
Lime	0·833
Magnesia	1·187
Chlorine	10·097
Sulphuric acid	2·763
Phosphoric acid	19·987
Silica	19·743
	100·000

No. XXIII. Scotch porter at 1*s*. per gallon.

Potash	18·861
Soda	33·765
Lime	1·344
Magnesia	1·386
Chlorine	11·386
Sulphuric acid	2·177
Phosphoric acid	12·480
Silica	18·601
	100·000

The dried residue of No.	I.	yielded	4·800	per cent. of ash.
... ...	II.	...	3·395	...
... ...	III.	...	3·797	...
... ...	IV.	...	5·475	...
... ...	V.	...	3·500	...
... ...	VI.	...	4·621	...
... ...	VII.	...	3·595	...
... ...	VIII.	...	4·673	...
... ...	IX.	...	4·152	...
... ...	X.	...	3·885	...
... ...	XI.	...	8·186	...
... ...	XII.	...	7·626	...
... ...	XIII.	...	12·035	...
... ...	XIV.	...	8·666	...
... ...	XV.	...	7·863	...
... ...	XVI.	...	8·822	...
... ...	XVII.	...	6·864	...
... ...	XVIII.	...	5·720	...
... ...	XIX.	...	6·050	...
... ...	XX.	...	9·001	...
... ...	XXI.	...	14·579	...
... ...	XXII.	...	11·113	...
... ...	XXIII.	...	6·054	...

LIII. *On the Apparent Motion of the Figures in certain Patterns of Blue and Red Worsted. By* HENRY TAYLOR, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Guildford, Oct. 12, 1848.

THE apparent motion of the figures in certain patterns of blue and red worsted is sufficiently well-known to have excited some attention among the learned as well as the unlearned, but, so far as I know, without receiving any satisfactory explanation. After having examined the subject for some time, I believe I have succeeded in making out the cause of this singular phænomenon; and if the following observations are deemed of sufficient importance, I should be glad to have them inserted in the pages of your Journal.

If certain patterns of blue and red worsted-work are examined by candle-light, it is found that when they are moved about, the figures have a tremulous appearance as if they were moving faster than the ground of the pattern—and blue and red are the only colours exhibiting this remarkable property—although it must be observed that some persons are so constituted as not to perceive the illusion under any circumstances.

The usual patterns consist of blue and red figures on a ground of the opposite colour, several of them being arranged together on the same piece; but for the purpose of experiment, it is better to have patterns of the simplest kind, and I shall suppose them now to consist of single spots, of red on a blue ground and blue on a red ground.

If the pattern is moved up and down before the eye, and attention is directed to the red spot, we may observe a change of colour taking place at its edge, like a dark border or shadow flitting from one side to the other. On close examination, it will be found that as the pattern is moved upward, this dark border appears only at the upper edge of the spot, and but for an instant; as the pattern descends, it is seen again at the lower edge, vanishing as before. When the movement is made from side to side, instead of up and down, a similar appearance may be seen on the right and left of the spot.

If the blue spot is now examined in the same manner, it will present an appearance somewhat different, the borders that form on its edge being of a lighter colour than the rest of the spot, and having the effect of lights instead of shadows, as in the former instance.

These changes of tint are so momentary, that they require very close attention in order to be seen at all; and the best way of discovering them is to hold the pattern at some distance from the lamp or candle, moving it at first rather slowly. I have described these appearances thus minutely because they serve, according to my observation, to produce the illusion in question: in them lies the whole mystery.

I conceive that the light and dark borders in the two figures have the same effect on the mind of the observer as the ordinary light and shadow seen on any prominent object that is illuminated from one side; and if such an object could be presented to our view with its light and shade shifting rapidly from one side of it to the other, though without its being moved, I believe we should have an impression that the object really was moving, because we know from experience that such an effect is only to be produced by shifting the position of the candle, or of the object illuminated by it*.

And so it is with the worsted patterns. When we see these spots with their light and dark borders shifting from side to

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* I have often convinced myself of the truth of this by a simple experiment. Some prominent body, as a white ball or cylinder, is suspended before an observer seated with his back to a table, on which two lighted candles are placed at some distance apart. By covering up each candle alternately, the light and shadow on the object may be reversed; and if this is done rapidly, it will give it the appearance of moving from side to side.

side, we are led to infer that, as the lamp or candle is stationary, the spots themselves actually shift their position, and move faster than the ground of the pattern. Such at least is the illusion; and in some instances the spots will even appear like protuberant bodies rolling from side to side.

In order to prove that this change of tint at the edges of the figures is an efficient cause of the illusion, I prepared a small instrument on this principle. A blue and a red spot were affixed to a white card in such a manner that moveable shadows might be introduced behind them, which were made to slide from side to side in imitation of the borders of the worsted spots, and to persons not acquainted with the nature of the contrivance the effect was the same, the coloured spots appeared to move when their shadows only were being moved.

On the other hand, when the formation of the light and dark borders is prevented, as may be done by inserting a row of white stitches round each of the spots, so as to separate the two colours, then the illusion ceases—the figures are no longer seen to move. The reason of this is obvious, that the presence of the white stitches is an obstacle to the blending of the colours on which the light and dark borders depend for their production. The difference of tint in the two cases may require some further explanation,—why the red spot has a dark border, and the blue a light one. The apparent blending of colours in a state of rapid motion is generally accounted for by the physiological fact, that the eye retains impressions for an appreciable time after the objects themselves are withdrawn from view; and when a number of objects are presented to the eye in quick succession, the image left on the retina by one object is liable to be confused by that of the succeeding object; and when these are of different colours, a compound colour is perceived which is intermediate between the two; but as the spectrum left by the first object is fainter than the image of that which is actually present, it follows that the colour of the latter will always predominate. Hence in the blending of the two colours which takes place at the junction of the spot and the ground of the pattern, the resulting tints are different in each case; for the red spot it is purple, that is, the margin of the red spot is seen through a blue spectrum, that of the ground,—while the border of the blue spot appears of a lavender tint, the blue being seen through the spectrum of the red ground. From experiments made with glass of different colours, I venture to suggest this as the probable explanation; for I find that red becomes darker by being seen through a medium of light blue, while the blue is rendered lighter by the aid of a red glass.

These effects are not to be confounded with the phænomenon of "accidental colours," in which the spectrum of an object is of the opposite or complementary colour, these colours being produced only when the eye has been exerted upon an object for some considerable time; and, moreover, the complementary colours of blue and red are not perceived at all in these experiments. I mention this distinction, because an attempt has been made to explain the apparent motion of these figures by a reference to the theory of accidental colours.

It remains to be shown why blue and red are the only colours to exhibit this peculiarity. The only suggestion I have to make is, that the intermediate tints formed by the admixture of blue and red contrast strongly with both of the original colours, so as to produce the required effects of light and shade; and certainly both purple and lavender form decided contrasts with blue and red. And this is not the case with any other combination of the primary colours. For example, the orange tints produced by blending red and yellow contrast too feebly with either of these colours for any effect of light or shade, and the same objection applies to blue and yellow.

Finally, why is the illusion seen only by artificial light? Not from the blue being seen as green (because some degree of motion is observable in blue and red patterns by daylight if they are examined in obscure corners), but from the indistinctness of artificial light as compared with solar light; for it will be found that if the colours are held close to a lamp or candle so as to be strongly illuminated, the effect is destroyed; and, on the contrary, it becomes most striking in indistinct lights, as when the patterns are held under cover of the table on which the lamp stands, the shadows on the red spot then become exceedingly dark, while the light tints on the blue spot have almost a phosphorescent appearance. For the same reason, when the pattern is viewed indirectly, the effect is better than when the eye is directed full upon it; and in patterns composed of several spots or figures on their appropriate ground, it is remarked that the apparent motion is greatest in those parts on which the eye is not directed point blank, because our vision is then indistinct.

In connexion with this subject, I may mention another phænomenon equally curious and perplexing, to be observed with these same patterns. The blue spot, when viewed indirectly, always appears lighter than it really is; and when the eye turns full upon it, it resumes its proper colour with a kind of start: the red spot again appears much darker by indirect vision, and becomes as suddenly light again when the eye is directed straight towards it. There is no doubt that these changes of tint serve to heighten the illusion of apparent motion when the eye is allowed to wander over the different parts of a complicated pattern. This phænomenon may perhaps be explained by the fact of the sight being most perfect in the axis of vision, or, as Sir David Brewster has expressed it, "the eye has the power of seeing objects with perfect distinctness only when it is directed straight upon them, so that all objects seen indirectly are seen indistinctly;" and it may be supposed that impressions received in those parts of the retina used in oblique vision are, as it were, diffused. Thus the red and blue spots, when viewed indirectly, appear tinged with the prevailing colour of the ground of the pattern,—the red spot becoming darker by the influence of the blue round it, and the blue spot lighter by the vicinity of the red; for it is remarkable that this illusion is not produced with single colours, only with spots of one colour surrounded by a field of the other.

In concluding these observations, I have only to add, that there cannot be much doubt of the correctness of the view which ascribes the illusory appearance of motion to the change of tint at the edges of the figures. These are matters of fact: but whether the theories offered in explanation of these facts are correct or not, I must leave to more competent observers to determine.

I have the honour to be, Gentlemen,
Yours faithfully,
HENRY TAYLOR.

LIV. *On a difficulty in the Theory of Sound.* *By* G. G. STOKES, *M.A., Fellow of Pembroke College, Cambridge*[*].

THE theoretical determination of the velocity of sound has recently been the occasion of a discussion between Professor Challis and the Astronomer Royal. It is not my intention to enter into the controversy, but merely to consider a very remarkable difficulty which Professor Challis has noticed in connexion with a known first integral of the accurate equations of motion for the case of plane waves.

I would first however observe, that I do not think that we are *obliged*, in treating the subject to a first approximation, to enter into the consideration of any difficulty which may arise when we come to employ exact equations. In neglecting the squares of small quantities, we adopt a consistent system of approximation, and we arrive at a precise result, namely, the

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[*] Communicated by the Author.

laws which the actual motion tends indefinitely to obey when the magnitude of the velocity is indefinitely diminished. And if, moreover, we have every reason to believe that the first order of small quantities contains all that are sensible in practical cases, we have arrived, not merely at abstract mathematical results, but at important physical laws. Nevertheless it will be admitted on all hands, that any apparent contradiction arrived at by employing exact equations is deserving of serious consideration.

The difficulty already alluded to is to be found at page 496 of the preceding volume of this Magazine. In what follows I shall use Professor Challis's notation.

Without entering into the consideration of the mode in which Poisson obtained the particular integral

$$\varpi=f\{z-(a+\varpi)t\}, \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

it may be easily shown, by actual differentiation and substitution, that the integral does satisfy our equations. The function f being arbitrary, we may assign to it any form we please, as representing a particular possible motion, and may employ the result, *so long as no step tacitly assumed in the course of our reasoning fails.* The interpretation of the integral (1.) will be rendered more easy by the consideration of a curve. In fig. 1 let oz be the axis of z, and let the ordinates of the curve represent the values of ϖ for $t=0$. The equation (1.) merely

Fig. 1.

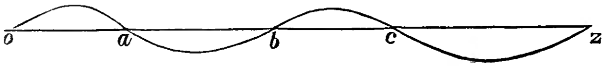
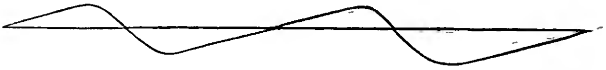


Fig. 2.



asserts that whatever value the velocity ϖ may have at any particular point when $t=0$, the same value will it have at the time t at a point in advance of the former by the space $(a+\varpi)t$. Take any point P in the curve of fig. 1, and from it draw, in the positive direction, the right line PP′ parallel to the axis of z, and equal to $(a+\varpi)t$. The locus of all the points P′ will be the velocity-curve for the time t. This curve is represented in fig. 2, except that the displacement at common to all points of the original curve is omitted, in order that the modification in the form of the curve may be more easily perceived. This comes to the same thing as drawing PP′ equal to ϖt instead of $(a+\varpi)t$. Of course in this way P′ will lie on the positive or negative side of P, according as P lies above or below the axis of z. It is evident that in the neighbourhood of the points a, c the curve becomes more and more steep as t increases, while in the neighbourhood of the points o, b, z its inclination becomes more and more gentle.

The same result may easily be obtained mathematically. In fig. 1, take two points, infinitely close to each other, whose abscissæ are z and $z+dz$; the ordinates will be w and

$$w+\frac{dw}{dz}dz.$$

After the time t these same ordinates will belong to points whose abscissæ will have become (in fig. 2) $z+wt$ and

$$z+dz+\left(w+\frac{dw}{dz}dz\right)t.$$

Hence the horizontal distance between the points, which was dz, will have become

$$\left(1+\frac{dw}{dz}t\right)dz;$$

and therefore the tangent of the inclination, which was $\frac{dw}{dz}$, will have become

$$\frac{\frac{dw}{dz}}{1+\frac{dw}{dz}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

At those points of the original curve at which the tangent is horizontal, $\frac{dw}{dz}=0$, and therefore the tangent will constantly remain horizontal at the corresponding points of the altered curve. For the points for which $\frac{dw}{dz}$ is positive, the denominator of the expression (A.) increases with t, and therefore the inclination of the curve continually decreases. But when $\frac{dw}{dz}$ is negative, the denominator of (A.) decreases as t increases, so that the curve becomes steeper and steeper. At last, for a sufficiently large value of t, the denominator of (A.) becomes infinite for some value of z. Now the very formation of the differential equations of motion with which we start, tacitly supposes that we have to deal with finite and continuous functions; and therefore in the case under consideration we must not, without limitation, push our results beyond the least value of t which renders (A.) infinite. This value is evidently the reciprocal, taken positively, of the greatest negative value of $\frac{dw}{dz}$; w here, as in the whole of this paragraph, denoting the velocity when $t=0$.

By the term *continuous function*, I here understand a function whose value does not alter *per saltum*, and not (as the term is sometimes used) a function which preserves the same algebraical expression. Indeed, it seems to me to be of the utmost importance, in considering the application of partial differential equations to physical, and even to geometrical problems, to contemplate functions apart from all idea of algebraical expression.

In the example considered by Professor Challis,

$$w=m\sin\frac{2\pi}{\lambda}\{z-(a+w)t\},$$

where m may be supposed positive; and we get by differentiating and putting $t=0$,

$$\frac{dw}{dz}=\frac{2\pi m}{\lambda}\cos\frac{2\pi z}{\lambda},$$

the greatest negative value of which is $-\frac{2\pi m}{\lambda}$; so that the greatest value of t for which we are at liberty to use our results without limitation is $\frac{\lambda}{2\pi m}$, whereas the contradiction arrived at by Professor Challis is obtained by extending the result to a larger value of t, namely $\frac{\lambda}{4m}$.

Of course, after the instant at which the expression (A.) becomes infinite, some motion or other will go on, and we might wish to know what the nature of that motion was. Perhaps the most natural supposition to make for trial is, that a surface of discontinuity is formed, in passing across which there is an abrupt change of density and velocity. The existence of such a surface will presently be shown to be possible, on the two suppositions that the pressure is equal in all directions about the same point, and that it varies as the density. I have however convinced myself, by a train of reasoning which I do not think it worth while to give, inasmuch as the result is merely negative, that even on the supposition of the existence of a surface of discontinuity, it is not possible to satisfy all the conditions of the problem by means of a single function of the form $f\{z-(a+w)t\}$. Apparently, something like reflexion must take place. Be that as it may, it is evident that the change which now takes place in the nature of the motion, beginning with the particle (or rather plane of particles) for which (A.) first becomes infinite, cannot influence a particle at a finite distance from the former until after the expiration of a finite time. Consequently, even after the change in the nature of the motion, our original expressions are applicable, at least for a certain time, to a certain portion of the fluid. It was for this reason that I inserted the words "without limitation," in saying that we are not at liberty to use our original results without limitation beyond a certain value of t. The full discussion of the motion which would take place after the change above alluded to, if possible at all, would probably require more pains than the result would be worth.

I proceed now to consider the possibility of the existence of a surface of discontinuity, and the conditions which must be satisfied at such a surface. Although I was led to the subject by considering the interpretation of the integral (1.), the consideration of a discontinuous motion is not here introduced in connexion with that interpretation, but simply for its own sake; and I wish the two subjects to be considered as quite distinct.

Suppose that in passing across a point Q in the axis of z the velocity and density change suddenly from ϖ, ρ to ϖ', ρ', and let ȣ be the velocity of propagation of the surface of discontinuity. Let us first investigate the equation which expresses that there is no generation or destruction of mass at the surface of discontinuity, the equation in fact which takes the place of the equation of continuity, which has to be satisfied elsewhere.

Take two points A, B in the axis of z, the first on the negative and the second on the positive side of Q, and let $\mathrm{QA}=h$, $\mathrm{QB}=h'$. Take also $\mathrm{QQ}'=ȣ\,dt$, so that Q$'$ is the point where the surface of discontinuity cuts the axis of z at the time $t+dt$. The quantities h, h' are supposed to be very small, and will be made to vanish after QQ$'$. Consider the portion of space comprised within a cylinder whose ends consist of two planes, of area unity, drawn through the points A, B perpendicular to the axis. In the time dt, the mass of fluid which flows in at the plane A is ultimately $\rho\varpi dt$, and that which flows out at the plane B is ultimately $\rho'\varpi' dt$. Hence the gain of mass within the cylinder is ultimately $(\rho\varpi-\rho'\varpi')dt$. Now the mass at the time t is ultimately $\rho h+\rho' h'$, and that at the time $t+dt$ is

$$\left(\rho+\frac{d\rho}{dt}\,dt\right)(h+ȣ\,dt)+\left(\rho'+\frac{d\rho'}{dt}\,dt\right)(h'-ȣ\,dt);$$

and therefore the gain of mass is $ȣ(\rho-\rho')dt$, h and h' being omitted, since they are to be made to vanish in the end. Equating the two expressions for the gain of mass, we get

$$\rho\varpi-\rho'\varpi'=(\rho-\rho')ȣ. \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

It remains to form the equation of motion. Now we have

moving force on plane A$=a^2\rho$, ultimately;
moving force on plane B$=a^2\rho'$, ultimately;
$\therefore$ resultant moving force $=a^2(\rho-\rho')$,
$\therefore$ momentum generated in time $dt=a^2(\rho-\rho')dt$.

Now the momentum of the mass contained within the cylinder at the time t is ultimately $\rho\varpi h+\rho'\varpi' h'$, and the momentum of the same set of particles at the time $t+dt$ is

$$\left(\rho w+\frac{d\rho\varpi}{dt}dt\right)(h+\overline{\mho-\varpi}\,dt)+\left(\rho'\varpi'+\frac{d\rho'\varpi'}{dt}dt\right)(h'+\overline{\varpi'-\mho}\,dt);$$

and therefore the gain of momentum is ultimately

$$\{(\rho\varpi-\rho' w')\mho-\rho\varpi^2+\rho'\varpi'^2\}\,dt;$$

whence we have

$$(\rho\varpi-\rho'\varpi')\mho-(\rho\varpi^2-\rho'\varpi'^2)=a^2(\rho-\rho')\,.\,.\quad .\quad .\quad (3.)$$

By eliminating $\mho$ between (2.) and (3.), we get

$$(\varpi'-\varpi)^2\rho\rho'=a^2(\rho-\rho')^2,\quad .\quad .\quad .\quad .\quad (4.)$$

an equation which we may if we please employ instead of (3.).

The equations (2.), (3.) being satisfied, it appears that the discontinuous motion is dynamically possible. This result, however, is so strange, that it may be well to consider more in detail the simplest possible case of such a motion.

Conceive then an infinitely long straight tube filled with air, of which the portion to the left of a certain section s is of a uniform density ρ, and at rest, while the portion to the right is of a uniform smaller density ρ', and is moving in the positive direction with a uniform velocity ϖ', the surface of separation s at the same time travelling backwards into the first portion with the uniform velocity $\mho$. The conception of such a motion having been formed, consider next whether the motion is possible or impossible; that is to say, not whether it is possible or impossible in the actual state of elastic fluids, but whether it would or would not be consistent with dynamical principles in the case of an ideal elastic fluid, in which the pressure was equal in all directions about the same point, and varied as the density.

In the case under consideration, the fluid to the left of s is in equilibrium in the simplest way. The fluid to the right is of uniform pressure, and there is no generation or destruction of velocity. The only question, then, can be as to the possibility of the passage from the one state into the other taking place in the way supposed. In the first place, it is evident that, independently of any consideration of force, there must be a relation between ρ, ρ', ϖ', and $\mho$; for a length $\mho\,t$ of condensed air comes to occupy, in the rarefied state, a length $(ȣ+\varpi')t$, so that we must have $\rho ȣ=\rho'(ȣ+\varpi')$. Next, if we take two sections s_1, s_2, the first to the left and the second to the right of s, and suppose the first to remain at rest, with the fluid in which it is situated, while the second moves, along with the fluid in which it is situated, with the velocity ϖ', since the pressure on s_1 exceeds that on s_2 by $a^2(\rho-\rho')$, the surface of s_1 or s_2 being for simplicity's sake supposed equal to unity, there must in the time t be generated a momentum $a^2(\rho-\rho')t$ in the fluid lying between s_1 and s_2. But this will be the case in consequence of the velocity ϖ' being communicated to a volume $ȣt$ of air which was previously at rest and of density ρ, while the state of rest or motion of the remainder of the air between s_1 and s_2 has been unaltered, provided $a^2(\rho-\rho')=\varpi' ȣ\rho$. These two relations being satisfied, it appears that the motion is dynamically possible. The two equations might have been obtained at once from (2.) and (3.) by writing $-ȣ$ for $ȣ$ and putting $\varpi=0$, but I have preferred deducing them afresh from first principles in consequence of the novelty of the subject, and the reluctance with which the conclusions that I have arrived at are likely to be received by mathematicians.

These conclusions certainly seem sufficiently startling; yet a result still more extraordinary remains behind. By solving the two equations of the preceding paragraph with respect to ϖ' and $ȣ$, we get

$$\varpi'=\frac{\rho-\rho'}{\sqrt{\rho\rho'}}\,a, \qquad ȣ=\sqrt{\frac{\rho'}{\rho}}\,a^{*}.$$

Now let ρ' vanish; then ϖ' becomes infinite and $ȣ$ vanishes. Hence the rate at which the condensed air (which remains packed like the combustible matter in a rocket) is discharged decreases indefinitely as the space into which the discharge takes place approaches indefinitely to a vacuum. Of course the velocity of discharge becomes infinite, without which the requisite momentum could not be furnished. The quantity of air which passes in a unit of time across a plane, of area unity, taken at the positive side of the tube, is $w'\rho'$, which is easily seen to be a maximum, for a given value of ρ, when $\rho'=\frac{1}{3}\rho$.

A similar paradox is fully considered by MM. Barré de

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* It is worthy of remark, that when ρ' is very nearly equal to ρ, and consequently w' very small, the velocity of propagation $ȣ$ is very nearly equal to a, to which it approaches indefinitely when w' is indefinitely diminished. Thus even this discontinuous motion offers no exception to the theorem, at once proved by neglecting the squares of small quantities, that in very small motions any disturbance is propagated in the fluid with the velocity a.

Saint-Venant and Wantzel, in the 27th *Cahier* of the *Journal de l'Ecole Polytechnique.*

The strange results at which I have arrived appear to be fairly deducible from the two hypotheses already mentioned. It does not follow that the discontinuous motion considered can ever take place in nature, for we have all along been reasoning on an ideal elastic fluid which does not exist in nature. In the first place, it is not true that the pressure varies as the density, in consequence of the heat and cold produced by condensation and rarefaction respectively. But it will be easily seen that the discontinuous motion remains possible when we take account of the variation of temperature due to condensation and rarefaction, neglecting, however, the communication of heat from one part of the fluid to another. Indeed, so far as the possibility of discontinuity is concerned, it is immaterial according to what law the pressure may increase with the density.

Of course the communication of heat from one particle of the fluid to another would affect the result, though whether to the extent of preventing the possibility of discontinuity I am unable to say. But there is another supposition that we have made which is at variance with the actual state of elastic fluids. It is not true that one portion of an elastic fluid is incapable of exerting any tangential force on another portion on which it slides, even though the variation of velocity from the one portion to the other be not abrupt but continuous. In consequence of this tangential force, analogous in some respects to friction in the case of solids, the mutual pressure of two adjacent elements of a fluid is not accurately normal to the surface of separation, nor equal in all directions about the same point. In many cases the influence of this internal friction is insensible, while in other cases it is very important. Its general effect is to check the relative motion of the parts of a fluid. Suppose now that a surface of discontinuity is very nearly formed, that is to say, that in the neighbourhood of a certain surface there is a very rapid change of density and velocity. It may be easily shown, that in such a case the rapid condensation or rarefaction implies a rapid sliding motion of the fluid; and this rapid sliding motion would call into play a considerable tangential force, the effect of which would be to check the relative motion of the parts of the fluid. It appears, then, almost certain that the internal friction would effectually prevent the formation of a surface of discontinuity, and even render the motion continuous again if it were for an instant discontinuous.

LV. *On the Spontaneous Cohesion of the Particles of Alumina.* *By* Richard Phillips, *F.R.S.**

IT has been observed by M. Wittstein (Chemical Gazette, April 1847), that the precipitate which is obtained from the persulphate or perchloride of iron, if kept for a great length of time in water, loses almost entirely the property of being soluble in acetic acid. This statement recalled an observation which I made several years since with respect to the similar cohering power of alumina; the two cases differing, however, in these respects, that whereas the sesquioxide of iron requires one or probably two years for the production of the effect, alumina undergoes the change partially in a very short time: the precipitated alumina does not, however, assume a crystalline appearance, as stated by M. Wittstein to be the case with the cohering sesquioxide of iron.

It is well known, both with respect to recently precipitated sesquioxide of iron and alumina, that they are soluble even in acetic acid, and consequently in the more powerful acids; but if the precipitated alumina be kept for two days even moist, and in contact with the solution from which it was precipitated, I find that sulphuric acid does not immediately dissolve it.

The following experiment will perhaps elucidate the fact which I have now stated: I precipitated the alumina from a large quantity of alum by excess of ammonia, and after keeping it moist for fourteen days, I washed it to get rid of the excess of ammonia and the salts existing in solution; to a quantity of this precipitated alumina I added dilute sulphuric acid in excess, and almost immediately separated by the filter the insoluble from the dissolved portion; the latter, being washed and ignited, weighed twenty and a half grains. The sulphuric solution of the soluble portion was decomposed by ammonia, and weighed, after washing and igniting, eight grains; so that in about a fortnight, nearly 72 per cent. of the particles of the alumina had so cohered as to resist the immediate chemical action of the sulphuric acid. I did not try whether solution would have been effected by long cold digestion in this case, but I have observed that time greatly increases the solvent power of the acid; I found, however, in the present instance, that the alumina which cold dilute acid would not immediately dissolve was entirely soluble in it when boiling; but if my recollection serves, I have found that, by long keeping, the alumina has become insoluble even in boiling sulphuric acid.

When, however, a solution of alum is decomposed by the

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* Read before the British Association held at Swansea, August 9, 1848, and communicated by the Author.

carbonate of soda or the sesquicarbonate of ammonia, the result as to the solubility of the precipitated alumina is totally different; the precipitate in the latter case is more dense, and instead of being semitransparent and gelatinous, it is opake and pulverulent, these differences evidently denoting some variation of constitution. Accordingly, I find that whatever may have been the length of time which I have kept this precipitate moist, I have always found it totally and immediately soluble in cold dilute sulphuric acid.

If it were not almost universally admitted that carbonic acid does not combine with alumina, the most obvious conclusion to arrive at would be that carbonate of alumina is actually formed, and that the carbonic acid preventing the cohesion of the particles of alumina, it remained soluble in acid.

I shall now, however, state some experiments which seem to me to prove that carbonate of alumina may be formed. I precipitated 200 grains of alum by excess of sesquicarbonate of ammonia, and washed the precipitate long after it ceased to render the water alkaline. After the alumina had been precipitated about fourteen days, I dissolved it in a counterpoised vessel in dilute sulphuric acid; solution took place with effervescence, and twenty grains of carbonic acid gas were evolved. It follows therefore, I think, that carbonate of alumina was formed, and that the carbonic acid interposed to prevent the cohesion of the alumina.

A very moderate heat seems, however, sufficient to decompose carbonate of alumina. A quantity of precipitate, prepared as above stated by sesquicarbonate of ammonia, was dried by the heat of steam; a portion of this was added to dilute sulphuric acid, and although effervescence occurred and some alumina was dissolved, the greater portion had lost its carbonic acid and become insoluble.

I have further found, that when hot solutions of alum and carbonate of soda are mixed, the precipitate formed is not totally and immediately soluble in dilute sulphuric acid.

That other substances have the power of intervening to prevent the cohesion of precipitated alumina is shown by the following experiment:—To a solution of alum I added one of sulphate of magnesia, and by ammonia precipitated a mixture of the two earths; and on keeping the mixed precipitate for such a length of time as would have rendered only one-fifth of alumina precipitated *per se* soluble, I found that the mixed earths were totally and almost immediately dissolved by cold dilute sulphuric acid.

The experiments which I have now detailed prove, I think, the following facts:—

1. That the particles of alumina, like those of peroxide of

iron, even when kept moist, have the power of cohering so as to prevent chemical action.

2. That carbonate of alumina may be formed, though decomposable at a comparatively low temperature.

3. That owing to the presence of carbonic acid or magnesia, the particles of alumina are prevented from cohering.

It will also, I think, be admitted, that the cohering property which I have now described may have led to errors in analysis, in which precipitated alumina may have been kept without any such intervening substances as to render it soluble. I may remark, in conclusion, that the alumina insoluble in dilute acids was not taken up by soda, at any rate when used cold.

Although not connected with the present subject, I will take the opportunity of mentioning an unexpected case which lately occurred to me. In examining a soil, I treated one portion of it simply with muriatic acid and another with nitro-muriatic acid. On the addition of ammonia to both solutions, I found the muriatic solution yield a bulky precipitate, evidently containing, if not consisting of protoxide of iron, whereas the nitro-muriatic solution gave a comparatively small quantity of precipitate evidently of sesquioxide of iron.

On investigation, I found that the protoxide of iron had carried down with it a considerable quantity of magnesia; whether in a state of mixture or combination remains to be proved.

To try the experiment on a considerable scale, I mixed an equivalent of sulphate of magnesia with half an equivalent of protosulphate of iron, adding an equivalent of sal-ammoniac to prevent the partial precipitation of the magnesia by the ammonia, which I added in excess to precipitate all that it was capable of throwing down. Having carefully washed the precipitate, I redissolved it in nitro-muriatic acid, so as to peroxidize the iron, and I again added sal-ammoniac and ammonia. By this peroxide of iron was thrown down; and on adding phosphate of soda to the filtered solution, I obtained so abundant a precipitate of the ammonio-magnesian phosphate, as to render it probable that at least one-fourth of the magnesia had been precipitated with the protoxide of iron.

I may also notice another circumstance connected with the precipitation of the magnesia with the protoxide of iron. It is well known that excess of ammonia dissolves a portion of protoxide of iron, which by oxidizement of the air is precipitated. This solution however does not happen in the case above alluded to, the whole of the protoxide of iron being at once precipitated with the magnesia.

LVI. *On the Vibrations of an Elastic Fluid.* *By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

IN accordance with the intention expressed at the close of my communication to the Philosophical Magazine for August last, I proceed to draw some inferences from the equations (B.) and (C.). But, first, it will be proper to point out two inaccuracies contained in that communication. The third term of equation (C.) in p. 100 should be $\frac{b^2f}{a^2}$ instead of b^2f; and in the same page, in the series for the form of φ, the quantities in brackets should be $z-a't+c_1$, $z-a't+c_2$, $z-a't+c_3$, &c., c_1, c_2, c_3, &c. being certain constants. No inferences are in the least degree affected by these alterations.

Let us now, for the purpose of testing equation (C.), suppose that f is a function of x^2+y^2. By this supposition the equation becomes, on putting r^2 for x^2+y^2,

$$0=\frac{d^2f}{dr^2}\left(1-\frac{\varphi^2{}_0}{a^2}\cdot\frac{df^2}{dr^2}\right)+\frac{1}{r}\cdot\frac{df}{dr}+\frac{b^2}{a^2}\cdot f,$$

showing that the supposition is allowable. In any case of vibrations not very large, the second term in the brackets is too small to have any appreciable effect. Omitting this term, and putting $4e$ for $\frac{b^2}{a^2}$, the integral of the resulting equation in a series is

$$f=1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c.$$

I have already stated (Phil. Mag. for April, p. 281) that according to this result f and $\frac{df}{dr}$ cannot vanish together. Hence, since the velocity of the vibrating particle in any direction perpendicular to the axis of z is $\varphi.\frac{df}{dr}$, and parallel to the axis of z, $f.\frac{d\varphi}{dz}$, it follows that the fluid cannot be constantly quiescent at a certain distance from that axis. This conclusion is inconsistent with the non-divergence of the vibrations, and would be enough to condemn the whole of the previous investigation, if the reasoning by which it was arrived at be good.

It is now proper for me to state the grounds on which that conclusion is to be rejected. I have maintained, and still

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* Communicated by the Author.

maintain, that to obtain true and consistent results in hydrodynamics another general equation, distinct from those commonly recognised, is absolutely necessary. In the foregoing investigation no reference has been made to that equation, and the reasoning is on that account defective. Before stating the modification which the reasoning must undergo, I will briefly advert to the evidence for the new equation which I have given in a communication to the Philosophical Magazine for May 1845 (p. 425). I have there obtained, by elementary considerations which admit of no dispute, the following equation,

$$\frac{d\rho}{dt}+\frac{d.\rho\mathrm{V}}{ds}+\rho\mathrm{V}\left(\frac{1}{\mathrm{R}}+\frac{1}{\mathrm{R}'}\right)=0, \quad . \quad . \quad . \quad \text{(I.)}$$

in which ρ is the density and V the velocity at the time t, at a point where the principal radii of curvature of the surface cutting at right angles the directions of motion are R and R′; and ds is the increment of a line drawn always in the direction of the motion of the particles through which it passes. This equation being *true* and *general*, is also *necessary* in the solution of every hydrodynamical problem. It is not identical with the recognised equation,

$$\frac{d\rho}{dt}+\frac{d.\rho u}{dx}+\frac{d.\rho v}{dy}+\frac{d.\rho w}{dz}=0, \quad . \quad . \quad . \quad \text{(II.)}$$

because it rests on *two* principles, that of constancy of mass, and the principle that the directions of motion are normals to a surface of continued curvature; whilst equation (II.) rests only on the principle of the constancy of mass. As however equation (II.) is true, there must exist another equation, which, combined with it, conducts to (I.). That additional equation is the one I contend for. I derive it from the single principle, that the directions of motion in a *given* element are at each instant normals to a surface of continued curvature,—a condition necessary for the continuity of the motion,—and I have shown how, by its combination with (II.), the equation (I.) is arrived at (see Camb. Phil. Trans., vol. vii. part 3, p. 385; and Phil. Mag., S. 3, vol. xx. April 1842). Lastly, in a particular instance I have obtained an absurd result by employing legitimately only the commonly received equations (Phil. Mag. for May 1845, p. 429). The evidence for the necessity of the new equation seems therefore to be in every respect complete. I am not, however, at present aware of any other use required to be made of it than that of deducing equation (I.). That deduction being once made, it follows that in every hydrodynamical problem equation (I.) must *supersede* equation (II.).

I proceed now to state the modification which the reasoning in the question under consideration receives by using equation (I.). The equation (A.), cited in my last communication from the *Mécanique Analytique*, was derived from equation (II.), and from the following three equations,

$$\frac{a^2.d\rho}{\rho dx}+\left(\frac{du}{dt}\right)=0,\quad \frac{a^2.d\rho}{\rho dy}+\left(\frac{dv}{dt}\right)=0,\quad \frac{a^2.d\rho}{\rho dz}+\left(\frac{dw}{dt}\right),$$

the supposition being at the same time made that $(d.\phi f)=udx+vdy+wdz$, ϕ being a function of z and t only, and f a function of x and y only. The analogous equation obtained by using (I.) in the place of (II.) differs from (A.) in only one, but that a very important particular; viz. *that the variation of the co-ordinates must be from point to point in the direction of motion*, whilst in (A.) x, y, z are independent variables. Introducing that limitation into (A.), and putting ds for the increment of space along the line of motion, the equation becomes

$$\left.\begin{aligned}&\frac{d^2.f\phi}{dt^2}-\left(a^2-\left(\frac{d.f\phi}{ds}\right)^2\right)\frac{d^2.f\phi}{ds^2}+2.\frac{d.f\phi}{ds}.\frac{d^2.f\phi}{dsdt}\\&\qquad-a^2.\frac{d.f\phi}{ds}\left(\frac{1}{\mathrm{R}}+\frac{1}{\mathrm{R'}}\right)=0;\end{aligned}\right\}\quad.\quad(\mathrm{A'}.)$$

and at the same time, by analytical geometry,

$$\begin{aligned}\left(\frac{1}{\mathrm{R}}+\frac{1}{\mathrm{R'}}\right)\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}+\frac{f^2}{\phi^2}.\frac{d\phi^2}{dz^2}\right)^{\frac{3}{2}}&=\frac{d^2f}{dy^2}.\frac{df^2}{dx^2}+\frac{d^2f}{dx^2}.\frac{df^2}{dy^2}\\&-2\,\frac{d^2f}{dxdy}.\frac{df}{dx}.\frac{df}{dy}+\frac{f^2}{\phi^2}.\frac{d\phi^2}{dz^2}\left(\frac{d\,f}{dx^2}+\frac{d^2f}{dy^2}\right)\\&+\left(\frac{f}{\phi}.\frac{d^2\phi}{dz^2}-\frac{2f}{\phi^2}.\frac{d\phi^2}{dz^2}\right)\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right).\end{aligned}$$

Now since

$$u=\phi.\frac{df}{dx},\quad v=\phi.\frac{df}{dy},\quad\text{and } w=f.\frac{d\phi}{dz},$$

it follows that, corresponding to the maximum value of f (which we have supposed to be unity),

$$u=0,\quad v=0,\quad\text{and } w=\frac{d\phi}{dz}.$$

The motion is consequently parallel to the axis of z, and for this line of motion the above equations give

$$\frac{d^2\phi}{dt^2}-\left(a^2-\frac{d\phi^2}{dz^2}\right)\frac{d^2\phi}{dz^2}+2\frac{d\phi}{dz}.\frac{d^2\phi}{dzdt}-a^2.\frac{d\phi}{dz}\left(\frac{1}{\mathrm{R}}+\frac{1}{\mathrm{R'}}\right)=0,$$

$$\left(\frac{1}{\mathrm{R}}+\frac{1}{\mathrm{R'}}\right).\frac{d\phi}{\phi dz}=\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}.$$

For the right-hand side of the second equation we have already substituted $-\frac{b^2}{a^2}$, so that the first equation becomes

$$\frac{d^2\varphi}{dt^2} - \left(a^2 - \frac{d\varphi^2}{dz^2}\right)\frac{d^2\varphi}{dz^2} + 2\frac{d\varphi}{dz}\cdot\frac{d^2\varphi}{dz\,dt} + b^2\varphi = 0. \quad (\text{B}'.)$$

This is the general equation for determining φ. It is identical, as might have been anticipated, with equation (B.).

By integrating (B′.) to the first order of approximation (see Phil. Mag. for April, p. 279), I found the only value of φ expressible in finite terms to be

$$\varphi = m\cos q(z - a't + c),$$

the constant a' being such that

$$a'^2 = a^2 + \frac{b^2}{q^2}.$$

Setting out with this first approximation, and including terms which contain the third power of m, I find the following expressions for φ and a'^2:

$$\left.\begin{aligned} \varphi = {} & m\cos q(z - a't + c) \\ & - \frac{m^2q^3a'}{3b^2}\sin 2q(z - a't + c) \\ & - \frac{m^3q^4}{4b^2}\left(\frac{q^2a^2}{b^2} + \frac{7}{8}\right)\cos 3q(z - a't + c) \end{aligned}\right\} \quad . \quad . \quad . \quad (1.)$$

$$a'^2 = a^2 + \frac{b^2}{q^2} + m^2q^2\left(\frac{2q^2a^2}{3b^2} + \frac{5}{12}\right) \quad . \quad . \quad . \quad . \quad (2.)$$

The equation (1.) serves to define the motion parallel to the axis of z, and the equation (2.) determines the rate of propagation (a'), which, it thus appears, is not altogether independent of m. I have before stated, that the different terms of the expression for φ account for *harmonics*.

Let us now investigate the equation which defines the motion transverse to the axis of z. For this purpose, on account of the independence of the variables in φ and f, suppose $\varphi = 0$. Then $u = 0$ and $v = 0$. Hence the motion at each point of the plane corresponding to $\varphi = 0$ is parallel to the axis of z. For any point of this plane R and R′ each become infinitely great. But $\left(\frac{1}{R} + \frac{1}{R'}\right)\cdot\frac{1}{\varphi}$ may have a finite value. Hence the general expression for $\frac{1}{R} + \frac{1}{R'}$ gives for this particular case,

$$\left(\frac{1}{R} + \frac{1}{R'}\right)\frac{f}{\varphi}\cdot\frac{d\varphi}{dz} = \frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} - \frac{2}{f}\cdot\left(\frac{df^2}{dx^2} + \frac{df^2}{dy^2}\right);$$

at the same time the equation (A'.) becomes

$$f.\frac{d^2\phi}{dt^2}-\left(a^2-f^2.\frac{d\phi^2}{dz^2}\right)f.\frac{d^2\phi}{dz^2}+2f^2.\frac{d\phi}{dz}.\frac{d^2\phi}{dzdt}$$
$$-a^2.f.\frac{d\phi}{dz}\left(\frac{1}{R}+\frac{1}{R'}\right)=0.$$

Hence substituting the above value of $\frac{1}{R}+\frac{1}{R'}$, we have

$$f.\frac{d^2\phi}{\phi dt^2}-\left(a^2-f^2.\frac{d\phi^2}{dz^2}\right)f.\frac{d^2\phi}{\phi dz^2}+2f^2.\frac{d\phi}{dz}.\frac{d^2\phi}{\phi dzdt}$$
$$=a^2.\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}\right)-\frac{2a^2}{f}.\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right).$$

In this equation must be substituted for the factors involving ϕ, the several values which they have when $\phi=0$. These may be obtained from equation (1.), which clearly shows that when $\phi=0$,

$$q(z-a't+c)=\frac{\pi}{2}.$$

This process having been gone through, the following is the resulting equation for determining f:—

$$\left.\begin{aligned}&\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}-\frac{2}{f}.\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right)+\frac{b^2}{a^2}f+\frac{2ma'q^3}{a^2}.f(f-1)\\&\quad-\frac{m^2}{a^2}.\left\{\frac{16q^6a'^2}{3b^2}.f(f-1)-q^4f(f^2-1)\right\}=0\end{aligned}\right\}\quad(3.)$$

It is now clear that where $f=0$, we have also $\frac{df}{dx}=0$ and $\frac{df}{dy}=0$. The equations are consequently consistent with non-divergence of the vibrations.

The equations (1.), (2.) and (3.), define the motion and propagation of the motion of the vibrating particles. To complete the theory of non-divergent vibrations, an expression for the density remains to be obtained. This I reserve for a future opportunity.

Cambridge Observatory,
Oct. 21, 1848.

LVII. *Remarks on the Weather during the Quarter ending September 30, 1848. By* James Glaisher, *Esq., of the Royal Observatory, Greenwich**.

THE meteorological returns for the past quarter furnished to the Registrar-General have been obtained from the usual places. These have been all examined and discussed by myself in the same manner as detailed in the previous Numbers of the Philosophical Magazine. The following are my remarks upon the weather of the past quarter.

With the exception of a few days in July, and the period between the 9th and 23rd of September, the weather during the quarter ending September 30, 1848, was wet, with very little sunshine. The month of August was extremely wet, and in many places the falls of rain both in July and September were unusually great. So much rain falling in a period immediately following the previous bad weather, renders the season and the year very remarkable. On July 1 the mean temperature of the air was 8°·4 below the average value from the seven preceding years, and on the 6th it was 12°·2 in excess above the average; on the former day the mean temperature was 46°·7, and on the latter day it was 74°·0. On the 9th it was 3° below the average, and on the 14th it was 9°·4 above the average; and on the 15th it was again below the average. These changes were large and abrupt. From July 11 to September 19 the temperature of the air was almost always below the average value, and particularly so between the 11th and the 15th of September; on the 12th the departure from the average was 12°·6. From the 20th of September to the end of the quarter, the temperature of the air ranged somewhat above the average value.

The hottest day in this year was July 6, and this day was the hottest all over the country. On an average of seven years, the hottest day is July 5.

In pursuance of the arrangement hitherto followed, I will speak of each subject of investigation separately.

The mean temperature of the air at Greenwich—

For the month of July was 61°·5, which is 3°·7, 1°·3, 0°·6, 0°·1, and 1°·7 *above* those of the years 1841 to 1845 respectively, 3°·0 and 3°·9 *below* those of the years 1846 and 1847; or it is 0°·1 *above* the average of these seven years;

For the month of August was 58°·5, which is 2°·0, 6°·9, 3°·6, 4°·7, and 3°·6 *below* those of the years 1841, 1842, 1843, 1846, and 1847 respectively, 0°·8 and 1°·2 *above* those of the years 1844 and 1845 respectively; or it is 2°·7 *below* the average of these seven years;

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* Communicated by the Author.

For the month of September was 55°·8, which is 2°·3, 0°·6, 3°·7, 1°·1, and 4°·3 *below* those of the years 1841, 1842, 1843, 1844, and 1847 respectively, 1°·2 and 1°·5 *above* those of the years 1845 and 1846 respectively; or it is 1°·2 *below* the average of these seven years.

The mean value for the quarter was 58°·6; that for 1841 was 58°·8; for 1842 was 60°·7; for 1843 was 60°·8; for 1844 was 58°·7; for 1845 was 56°·9; for 1846 was 62°·6; and for 1847 was 60°·3; so that the defect for this quarter *below* the corresponding quarter in the years 1841, 1842, 1843, 1844, 1846, and 1847, was 0°·2, 2°·1, 2°·2, 0°·1, 4°·0, and 1°·7 respectively; the only year between 1841 and 1847 whose mean temperature for this period was less than that for the present year was 1845, and the difference is 1°·7. The average value for this quarter from the seven preceding years was 59°·8; so that the mean temperature of the air for the quarter ending September 30, 1848, was *below* that of the corresponding quarter in the preceding seven years by 1°·2.

The mean temperature of evaporation at Greenwich—

For the month of July was 57°·6, which is 0°·1 *above* that for the preceding seven years;

For the month of August was 55°·2, which is 2°·9 *below* that for the preceding seven years;

For the month of September was 53°·2, which is 1°·5 *below* that for the preceding seven years.

The mean value for the quarter was 55°·3, which is 1°·4 *below* the average for the seven preceding years.

The mean temperature of the dew-point at Greenwich—

For the month of April was 54°·6, which is 3°·0, 1°·4, 0°·2, 1°·9, and 1°·8 *below* those for the years 1841, 1842, 1845, 1846, and 1847 respectively, 1°·7 and 0°·1 *above* those for the years 1843 and 1844 respectively; or it is 0°·9 *above* the average of these seven years;

For the month of August was 52°·8, which is 2°·2, 6°·1, 5°·0, 4°·7, and 3°·3 *below* those for the years 1841, 1842, 1843, 1846, and 1847 respectively, 0°·5 and 0°·2 *above* those for the years 1844 and 1845; or it is 2°·9 *below* the average for these seven years;

For the month of September was 50°·9, which is 2°·8, 2°·6, 4°·0, 2°·3, and 4°·0 *below* those for the years 1841, 1842, 1843, 1844, and 1846 respectively, 1°·2 *above* those of the years 1845 and 1847; or it is 1°·9 *below* the average of these seven years.

The mean value for the quarter was 52°·8, which is 1°·3 *below* the average for the corresponding period of the preceding seven years.

The mean weight of water in a cubic foot of air for the quarter was 4·5 grains, which is 0·2 grain *less* than the average for the seven preceding years.

The additional weight of water required to saturate a cubic foot of air was 1·1 grain. The average value for the seven preceding years was 1·0 grain.

The mean degree of humidity of the atmosphere for July was 0·762, for August was 0·797, and for September was 0·795. The averages for the seven preceding years were 0·780, 0·804, and 0·842 respectively. The value for the quarter was 0·785, which is 0·024 *less* than the average for these years.

The mean elastic force of vapour for the quarter was 0·411 inch, which is 0·026 *less* than the average for these years.

The mean reading of the barometer at Greenwich for July was 29·836 inches, for August was 29·732 inches, and for September was 29·832 inches; these values are 0·041 inch *above*, 0·065 inch *below*, and 0·021 inch *above* respectively the averages for the seven preceding years. The mean value for the quarter was 29·797 inches, which is of the same value as the average for the seven preceding years.

The average weight of a cubic foot of air under the average temperature, humidity, and pressure, was 527 grains; the average for the seven preceding years was 526 grains.

The rain fallen at Greenwich in July was 2·1 inches; in August was 4·6 inches; and in September was 2·4 inches; the average amount for the seven preceding years was 2·3 inches in July, 2·7 inches in August, and 2·2 inches in September. The amount fallen in the quarter was 9·1 inches, which is 1·9 inch greater than the average for the seven preceding years. The average fall of rain during this quarter, as derived from the observations since the year 1815, is 7 inches. In the year 1824 the fall of rain in the quarter ending September 30 was 9 inches, in 1828 it was 12·5 inches, in 1829 it was 11 inches, and in 1839 it was 10·5 inches. The total amount of rain fallen this year till September 30 was 24·3 inches; in 1841 it was 21·2 inches; in 1842 it was 14·2 inches; in 1843 it was 17·5 inches; in 1844 it was 16·2 inches; in 1845 it was 16·6 inches; in 1846 it was 17·5 inches; and in 1847 it was 11·6 inches. So that the fall of rain this year exceeds that in 1841 by 2·7 inches; in 1842 by 9·7 inches; in 1843 by 6·4 inches; in 1844 by 7·7 inches; in 1845 by 7·3 inches; in 1846 by 6·4 inches; and in 1847 by 12·3 inches. The *excess* of the fall of rain this year over the average for the seven preceding years is 7·5 inches.

In the years 1824 and 1828 the depth of rain fallen to the end of September exceeded 23 inches; and in the years 1829 and 1839 the amount collected exceeded 20 inches. So large a fall as 24·3 inches within the first nine months of the year has probably not been exceeded within this century.

The temperature of the water of the Thames was 63°·0 by day, and 62°·0 by night. The water, on an average, was 3°·9 *warmer* than the air.

The horizontal movement of the air was about 130 miles daily; during the period of time between July 19 and July 27 it amounted to 233 miles per day; from July 31 to August 6 its average daily value was 240 miles; and on August 21 it exceeded 300 miles.

The highest and lowest readings of the thermometer in Air at the height of four feet above the ground, and protected as much as possible from the effects of radiation and rain, were 85°·3 and 32°·8.

The average daily ranges of the readings of the thermometer in Air at the height of four feet, were 22°·5 in July, 18°·5 in August, and 20°·9 in September. The average ranges for these months from the observations of the seven preceding years were 17°·2, 17°·1, and 16°·2 respectively.

In July the *readings of the thermometer on grass* were 29°·5 on one night; between 32° and 40° on nine nights; between 40° and 50° on twelve nights; and above 50° on nine nights.

In August the lowest reading was 36°: and the readings were below 40° on four nights; between 40° and 50° on eighteen nights; and above 50° on nine nights.

In September the readings were below 32° on nine nights, and the lowest was 23°; they were between 32° and 40° on seven nights; between 40° and 50° on nine nights; and above 50° on four nights.

The mean amount of cloud for July was 6·6, for August was 7·6, and for September was 5·6. The average values for the seven preceding years were 6·7, 6·3, and 6·0 respectively.

There were five exhibitions of the *aurora borealis* during the quarter, which occurred on July 11, August 28, September 4, 8 and 18.

Thunder-storms at different parts of the country occurred on July 14, 26, August 1, 3, 4, 5, 6, 7, 8, 9, 10, 11, 22, 23, 31, and September 5; and lightning unaccompanied by thunder was seen on July 24, August 23, 24, 25, September 22 and 25. The storms of July 14 were observed at Greenwich, Latimer Rectory, Cardington, Saffron Walden and Leicester. At Greenwich, thunder-clouds were observed first in the west and north-west at 8^{h} 30^{m} P.M.; and from this time till near midnight the thunder followed the lightning at intervals varying from 5^{s} to 40^{s}; and lightning was visible during the whole of the night. At Latimer the storm is described as awful, and of five hours' duration. On July 24 lightning was seen at Saffron Walden; on July 26 there was a thunder-storm at Leicester; on August 1 thunder was heard at Greenwich during the afternoon; on August 3 and 4 at Saffron Walden; on August 5 there were thunder-storms at Greenwich, Stone, Saffron Walden, Leicester and Empingham. Near the last-mentioned place a tree was struck by the lightning.

On August 6 and 7 there were thunder-storms at Leicester; on August 8 at Leicester and at Exeter; on August 9 at Greenwich, but no lightning was seen; on August 10 at Stone; on August 11 at Greenwich, between the hours of 6 and 8 P.M.; the storm began in the east, and many of the flashes of lightning were vivid, and preceded the thunder by two or three seconds only. On August 22 there were storms at Exeter, Stone, Saffron Walden, Cardington, and at Leicester. At Greenwich, on August 23 and 24, between the hours of 8 P.M. and midnight, on both days many flashes of lightning were seen. On August 31, at Greenwich and at Stone, there were thunder-storms; at the former place the thunder followed the lightning at intervals varying from 1^s to 12^s. On September 5 there were storms at Greenwich, Stone and Leicester; on September 22 lightning was seen at Greenwich and at Stone, and again at Stone on the 25th.

Hail fell at Stone and Cardington on August 22, and at Empingham on August 5; the hailstones at Cardington were of very large dimensions.

Gales of wind took place all over the country on August 20, 21 and 22, but more particularly on the 21st. At many places trees were blown down, and a great deal of injury was done; coasting-vessels and fishing-boats generally suffered very much.

Large and continuous falls of rain.—In July, at Greenwich, rain fell to the depth of 0·3 inch on the 15th, 23rd and 31st. On the 14th, at Latimer Rectory, rain to the depth of half an inch fell in half an hour. In August rain was falling more or less at every part of the country on every day. At Greenwich the amount collected exceeded 0·3 inch on the 1st, 3rd, 8th, 10th and 21st; and the falls exceeded 0·7 inch on the 14th and 31st; on the 14th a large fall occurred at all places. In September, on the 24th, at Thwaite, between 4^h A.M. and 9^h A.M., rain fell to the depth of 2·12 inches, a greater fall within the same interval of time than has occurred at Thwaite within the preceding forty years; and on this day, at Leeds, the fall within nine hours was 2 inches. On the 28th, 29th and 30th days, rain was falling almost continuously over all parts of the country. At Cardington the fall within sixty hours was 2·6 inches. At Leicester the amount within seventy-two hours was 2·25 inches, and this was the amount which fell on these days at most places. This large fall, extending over so large a portion of the country, was most unusual.

The approximate mean monthly values of the several subjects of research are shown in extensive tables in the report of the Registrar-General.

The mean monthly temperatures of the places in Cornwall and Devonshire in these three months are about the same values as those of other places, but the extremes of daily and monthly temperatures are much less than elsewhere.

The reading of the barometer was low at the beginning of July, being 29·403 inches at 6ʰ A.M. on the 1st; this reading increased to 29·740 by 6ʰ P.M. on the 2nd; decreased to 29·611 on the 3rd, and increased quickly on the 4th, and reached 30 inches before midnight on this day. The reading ranged above 30 inches on the 5th, and decreased to 29·635 by midnight on the 9th. During the 10th the value increased 0·539 inch, having passed the point 30 inches at about 1ʰ P.M. on this day. The reading continued above 30 inches till the 17th; the highest value was 30·344 on the 12th. From the 17th there was a gradual decrease to 29·146 on the 20th at 3ʰ P.M.; at midnight on this day the reading was 29·467, and increased to 29·781 on the 24th; during the 25th the decrease was 0·300 inch, and the reading was 29·480 at midnight; it then gradually increased to 30 inches by the 29th. On the 30th the change was considerable, amounting to half an inch during the day; and at the end of the month the reading was 29·313 inches, and still decreasing.

On August 1st, at 6ʰ A.M., the reading was 29·244 inches; after this time it turned to increase, and was 29·581 at midnight, and reached 29·817 on the 2nd day. On the 3rd it decreased, and was 29·582 at midnight; during the 4th the changes were small; on the 5th the decrease was 0·190 inch, and the reading at midnight was 29·335 inches. During the 6th and 7th there was a slight increase. From this time to the 20th the changes were small, and at midnight on the 20th the reading was 29·846 inches. The decrease on the 21st was 0·334 inch, and at noon on the 22nd the reading was 29·423 inches, when it turned to increase, and was 29·528 at midnight; the increase continued till the 25th at noon, the reading at this time being 29·947, when it turned to decrease, and the changes after this time to the end of the month were small.

In September, till the 4th, the reading was above 30 inches; during the 5th it decreased 0·2 inch, and was 29·638 at midnight; from the 6th to the 9th the changes were small; on the 10th, by P.M., the reading decreased 0·219 inch, and the reading was 29·496 inches; it then turned to increase quickly; on the 11th the reading at midnight was 30·091 inches. From this time till the 18th the reading was always above 30 inches; the highest value was 30·345 on the 16th. On the 19th it began to decrease, and on the 24th the reading was 29·223 inches; and from this time to the end of the month the changes were small, and chiefly about the mean reading of 29·6 inches.

The great prevalence of rain during the quarter, together with the very short periods of sunshine, have harassed the farmer in gathering in the crops. The month of July was about its usual character, but the constant rain in August impeded the farmer in his operations, and in many of the southern counties injured the crops considerably, causing the corn to sprout and seed-leaves to appear, of fully an inch in length, by the middle of August; the greater coldness of the northern counties prevented the sprouting of the corn, but it otherwise was seriously injured.

Between the 9th and the 23rd of September the weather was generally fine, and this period was the only good interval of time for harvest work during the quarter, and the wheat gathered within this time was in good condition. The heavy falls of rain at the end of this month flooded many parts of the country, and the roads in some places were deeply trenched.

John Fletcher Miller, Esq., of Whitehaven, says, "The harvest in this neighbourhood was completed by September 20, somewhat earlier than usual. The crops were abundant, and secured in excellent condition."

Charles Charnock, Esq., of Leeds, says, "The harvest has been most protracted, and there is yet much both of barley and oats out in the fields, which I fear is seriously injured. There is in fact much more corn out in the northern districts than is generally supposed. On September 25 I observed in the East Riding of Yorkshire hundreds of acres of corn and potatoes with water to the depth of a foot upon them. The wheat and barley, which were housed between the 9th and the 23rd of September, were without damage and in good condition; that which was housed before this time was in a bad state from not being dry. Wheat in this neighbourhood has not sprouted generally. Wheat and barley yield badly, and both crops are under an average. Both oats and beans are average crops. Potatoes are getting worse; the winter or late varieties of this vegetable are now attacked and rotting very fast; *wherever the tops have been very luxuriant they are the worst*. I have observed that the disease seems to have been immediately preceded by a white frost, which disease I have no doubt is attributable to meteorological causes.

"Sheep stock has not been healthy; the deaths among the lambs have been very numerous even in the driest districts."

Samuel Charles Whitbread, Esq., says, "My harvest began on the 28th of July; it continued forty-two days, and rain fell on twenty-eight of these days, depositing 4·75 inches of water; on the whole the crops suffered no damage." This remark has reference to the crops in Bedfordshire.

The observer at Stone says, speaking of the crops in the Vale of Aylesbury, "that those of hay and clover were abundant and good, but were not well-gathered; that wheat was below an average of many years, but was well-housed; that both barley and oats were average crops, and they were well-housed." The observer speaks of the potato crop as being generally bad.

John Drew, Esq., F.R.A.S., of Southampton, has kindly procured me an agricultural report from John Clark, Esq. of Finsbury Farm, near Romsey, Hampshire, and which I have condensed into the following few lines.

The South Hampshire farmer has been subjected during the past quarter to more trying dispensations of Providence than Mr. Clark has experienced within a period of twenty-five years' practice. This gentleman further observes that the almost constant wet weather has injured every kind of crop, and that but little has come to maturity. The occasional short periods of sunshine have frequently induced the hope that a season would come for haymaking and harvest work, but which expectation unfortunately has never been realized. A large quantity of hay has been consequently rendered useless, and turnips have been very much injured. The wide range of prices of new wheat affords the best evidence of the extent to which this crop has been harassed. On those lands which have been well-farmed and well-drained, or on those which would readily part with an excess of moisture, the crops are an average (with the exception of potatoes, which are almost a failure). On heavy cold soils the crops are below an average. In consequence of the exceeding wetness cattle have done badly.

The recent heavy rains (now Oct. 6) excite great anxiety with respect to seed-time. On wet lands a great deal is needed to be done to prepare them for sowing, which operation must be late, and therefore to a certain extent more precarious than if performed under the more favourable circumstances of a good seed season.

The mean of the numbers in the first column of the subjoined quarterly meteorological table is 29·541 inches, and this value may be considered as that of the pressure of dry air for England during the quarter ending September 30, 1848. The differences between this number and the separate results contained in the first column, show the probable sums of the errors of observation and reduction; the latter arising partly from erroneously assumed altitudes, and partly in consequence of the index error of the instruments not having been determined. In most cases, however, the sums of these errors are small.

The mean of the numbers in the second column, for those places situated in the counties of Cornwall and Devonshire, is 58°·1; for those places situated south of latitude 52°, including Chichester and Hartwell, is 57°·8; for those places situated between the latitudes of 52° and 53°, including Saffron Walden and Highfield House, was 56°·8; for those places situated between the latitudes of 53° and 54°, including Liverpool and Whitehaven, but not Stonyhurst, whose mean temperature, from its greater elevation, is lower than that due to its latitude alone, was 56°·2; and for Durham and Newcastle was 55°·8. This value, however, is somewhat too high for the former place and too low for the latter, on account of the difference of elevation of these places. These values may be considered as those of the mean temperature of the air for these different parallels of latitude during the quarter ending September 30, 1848.

The average daily range of the temperature of the air in Cornwall and Devonshire was 14°·2; at Liverpool and Whitehaven was 10°·2; south of latitude 52° was 19°·5; between the latitudes of 52° and 53° was 15°·8; between the latitudes of 53° and 54° was 15°·9; and of Durham and Newcastle was 14°·4.

The greatest mean daily ranges of the temperature of the air took place at Hartwell, Latimer, Aylesbury and Leicester respectively; and the least occurred at Liverpool, Whitehaven, Torquay and Truro respectively.

The highest thermometer readings in air during the quarter were 95° at Leicester, 90° at Wakefield, and 89° at Hartwell; but it seems highly probable that these readings are greater than the temperature of the air really reached. The reading 88° seems to be confirmed, and this value may be considered as the highest during the quarter. The lowest values of the thermometer readings in air were 29° at Hartwell, 31° at Latimer and Cardington. The extreme range of temperature of the air during the quarter was therefore about 59°.

The average quarterly range of the reading of the thermometer in Cornwall and Devonshire was 33°·2; at Liverpool and Whitehaven was 32°·5; and the mean of the numbers at all the other places is 49°·2. The highest and lowest readings

Meteorological Table for the Quarter ending September 30, 1848.

Names of the places.	Mean pressure of the atmosphere of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Range of the thermometer.	Wind.		Mean amount of cloud 0—10.	Rain.		Mean weight of vapour in a cubic foot of air.	Mean additional weight required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
							Mean estimated strength 0—6.	General direction.		Number of days on which it fell.	Amount collected.						
	in.	°	°	°	°	°					in.	grs.	gr.		in.	grs.	feet.
Helston	29·535	58·5	76·0	41·0	15·0	35·0	1·5	s.w.	5·2	57	11·3	4·8	0·9	0·839	5·9	527	106
Falmouth	......	58·0	79·0	42·0	18·4	37·0	1·4	s.w.	6·3	50	11·6						
Truro	......	57·3	72·0	42·0	10·9	30·0	0·9	s.w.	6·3	45	12·5						
Torquay	......	58·0	75·0	46·0	10·4	22·3	2·2	s.w.	......	49	10·4	......	......	......	......	......	120
Exeter	29·656	58·6	80·7	39·0	16·5	41·7	1·4	w.	4·3	52	8·9	4·9	0·9	0·847	6·1	528	140
Chichester	......	55·9	78·0	39·0	15·0	39·0	......	s.w.	......	......	10·2						
Southampton Observatory	29·531	58·7	85·0	38·0	17·9	47·0	0·8	Variable.	6·7	50	11·0	5·0	0·7	0·906	6·2	528	55
Uckfield	29·558	59·6	83·0	36·0	19·0	47·0	......	s.w.	......	45	11·7	4·5	1·8	0·706	5·6	526	180
Beckington	29·498	57·1	88·0	35·0	19·5	53·0	0·8	s.w.	5·8	59	12·7	4·4	0·9	0·827	5·7	529	265
Royal Observatory, Greenwich	29·581	58·6	85·3	32·8	20·6	52·7	......	....	......	61	8·7	4·5	1·1	0·785	5·7	526	159
Maidenstone Hill, Greenwich	29·571	58·4	85·5	35·8	16·4	49·7	......	s.w.	6·4	45	10·2	4·7	1·1	0·810	5·7	528	107
Walworth	29·534	59·4	85·0	34·0	19·2	51·0	3·1	s.w.	4·0	50	8·0	4·6	1·4	0·759	5·6	525	32
Latimer Rectory	29·571	56·1	84·0	31·0	23·4	53·0	1·5	Variable.	6·6	52	10·5	4·5	1·1	0·817	5·5	524	335
Aylesbury	29·561	58·3	88·0	33·0	22·9	55·0	0·5	s.w.	6·1	47	10·4	4·4	1·2	0·781	5·5	524	280
Stone Observatory	29·527	56·2	76·1	37·0	15·3	39·1	0·9	s.w.	6·7	53	8·1	4·4	0·9	0·813	5·4	524	300
Hartwell House	29·515	57·7	89·0	29·0	25·8	60·0	0·8	s.w.	6·3	......	10·5	4·9	0·6	0·876	5·9	525	300
Saffron Walden	......	56·8	82·0	32·0	17·0	50·0	2·5	s.w.	4·7	56	7·8						
Cardington	29·591	57·6	85·0	31·0	20·2	54·0	......	s.w.	6·1	51	9·9	4·7	1·0	0·835	5·8	527	70
Thwaite	......	......	87·0	38·0	......	49·0	......	s.w.	......	40	9·8	......	......	......	......	......	200
Norwich	29·522	57·7	84·0	38·0	16·3	46·0	......	....	......	38	8·1	4·7	0·8	0·870	5·8	530	39
Leicester	29·603	57·2	95·0	35·0	21·9	60·0	2·0	s.w.	5·3	50	11·3	3·9	1·5	0·718	4·7	529	150
Derby	29·502	55·7	81·0	35·0	16·6	46·0	......	....	......	52	11·8	4·6	0·5	0·900	5·7	528	39
Highfield House, Notts	29·516	57·2	84·8	37·2	16·6	47·6	......	s.w.	6·3	58	11·7	4·4	1·0	0·813	5·4	529	103
Liverpool Observatory	29·466	56·3	76·8	45·2	10·2	31·6	0·9	n.w.	6·7	54	8·9	4·2	0·8	0·842	5·1	530	37
Leeds	29·519	55·2	84·0	35·0	17·7	49·0	1·5	Variable.	6·3	59	12·6	4·5	0·6	0·882	5·5	529	148
Wakefield	29·517	57·2	90·0	32·5	20·0	57·5	......	s.w.	......	57	9·5	4·1	1·3	0·762	5·0	527	113
Stonyhurst Observatory	......	53·9	83·5	32·6	16·0	50·9	0·9	s.w.	7·4	62	15·2	4·2	0·7	0·860	5·1	528	381
York	......	55·2	84·0	35·0	15·9	49·0	......	w.	......	45	9·0	......	......	......	......	......	50
Whitehaven	......	57·2	72·0	38·5	10·2	33·5	2·5	n.w.	......	50	11·0	4·3	1·2	0·780	5·3	529	
Durham	29·544	54·7	79·6	34·8	15·0	44·8	1·3	s.w.	5·8	39	6·8	4·1	1·1	0·798	5·0	525	340
Newcastle	29·488	56·8	79·5	34·5	13·7	45·0	......	s.w.	......	31	7·9	4·5	1·2	0·790	5·5	526	121
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17

at Stone, and all depending upon them, are evidently erroneous.

From the numbers in this quarter, as well as those of preceding quarters, it would seem that the Vale of Aylesbury is subject to greater extremes of temperature than any other part of the country of equal extent.

The great mass of air has passed from the south-west in all places except Liverpool and Whitehaven, at both of which places it seems to have passed from the north-west. By reference to the Monthly Table, it will be seen that this was particularly the case in July and August; but in September the direction of the wind was frequently from the east, and its compounds.

From the numbers in the ninth column, the distribution of cloud seems to have been nearly the same in amount at most places, and such as to have covered about three-fifths of the whole sky.

The fall of rain during the quarter has greatly exceeded the average amount for the season, and this was particularly the case in the month of August. The places at which rain has fallen on the greatest number of days were Stonyhurst, Greenwich, Beckington, Leeds, Highfield House, Helston and Wakefield, and the average number of these places was 59; and the places at which rain fell on the least number of days are those situated near the eastern coast. The places at which the largest falls have taken place were Stonyhurst, Hereford, Beckington, Leeds and Torquay. The places where the falls have been the least in amount are Durham, Newcastle, Saffron Walden, Walworth, Stone and Norwich. The amount at Stone being so much less than the fall at adjacent places, seems to be strange; and this was the case in the preceding quarter, in consequence of which the Rev. J. B. Reade, on August 17, wrote to me, stating that "there had been many electrical clouds giving copious showers around us, especially on the Chiltern Hills and in the neighbourhood of Aylesbury, while we were in sunshine. This was particularly the case yesterday afternoon."

The numbers in column 12 to 16 show the mean values of the hygrometrical results at every station; from which we find that—

The mean weight of vapour in a cubic foot of air for England (excepting Cornwall and Devonshire) in the quarter ending September 30, 1848, was 4·5 grains.

The mean additional weight required to saturate a cubic foot of air in the quarter ending Sept. 30, 1848, was 1·0 grain.

The mean degree of humidity (complete saturation =1) in the quarter ending September 30, 1848, was 0·815.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth, to the depth of 5·5 inches.

The mean weight of a cubic foot of air at the level of the sea, under the mean pressure, temperature and humidity, was 529·3 grains.

And these values for Cornwall and Devonshire were 4·8 grains; 0·9 grain; 0·843; 6·0 inches; and 530 grains respectively.

LVIII. *Account of the Aurora Borealis as seen at Stonyhurst Observatory, October* 1848. *By the* Rev. ALFRED WELD, *B.A., of Stonyhurst College*[*].

ON the evening of this day I was fortunate enough to witness the most magnificent display of aurora borealis which has been known in this part of the country for many years. The following are the principal features as they were noticed at the Observatory of Stonyhurst.

At about 6^{h} 45^{m} P.M. it was observed that the sky in the N.E. was tinged with crimson, and at the same time an arc of light was seen stretching from N.N.E. to a point about the N.W. From this arc great numbers of faint rays were emitted in both directions. By degrees the whole of that portion of the heavens became filled with rays of light, which were spread in confusion over the sky, some arising immediately from the horizon, others having their origin at different altitudes: none of these were remarkable for length or intensity of light. In the meantime the red in the N.E. was increasing to a brilliant crimson, remaining perfectly fixed, except that it extended itself northward like a great cloud of fire, and assumed a very magnificent appearance, while the streamers and sheets of light between the N. and N.W. underwent rapid changes, exhibiting now a somewhat irregular inverted arch, and now wound into a wreath so as to enclose an almost perfect circle. Shortly after seven the spectacle was very grand; from N.E. to S.W. the whole sky, to the altitude of 30° or 40°, was filled with sheets and streamers of light, all nearly steady (if we except a certain tremulous motion which seemed to animate the whole mass), and chiefly of a brilliant crimson hue.

During this time the cloud of red light continued stationary in the N.E., in the constellation Auriga, the principal

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[*] Communicated by the Author.

stars of which shone through with a red light, and there were occasional streamers of great brilliancy. About the same time there seemed to be three principal centres of light, one in the N.E., another in the N.W., and a third in the S.W. An ill-defined arch was still stretched over the north, and two distinct but faint arches arose from the same point in the N.E.; one passing South of the Zenith through the Pleiades, the constellations Aries, Pisces and Aquila, and the other rather north of the Pleiades, through the constellations of Cassiopæa, Draco and Lyra, and terminating with the other about the S.W. At 7h 30m P.M. clouds were formed in the north, and passed in small detached portions over the red mass in Auriga, which continued to shine with brilliancy between them.

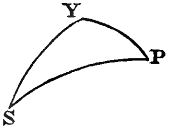
At 7h 40m P.M. the observations were interrupted until about 8h P.M. Between 8h P.M. and 8h 15m P.M. nothing was observed, except that the sky near the horizon in the N.E. and W. was lighter than the rest. At 8h 15m P.M. an auroral arch again appeared, terminating in these points; the red colour again appeared in the N.E., whilst sheets of light began to arise successively from every portion of the horizon from W. to E. These rays, which presented many shades of red mingled with white, began now to mount to a great altitude, and presented the appearance of vast truncated cones, tending to some common apex not very far from the zenith.

The splendour of the scene increased with great rapidity. At 9h P.M. the rays of light had shot past the zenith and began to meet round a point, corresponding about that hour, exactly with the position of α Andromedæ, which star was then about one hour distant from its culminating point. Between 9h P.M. and 9h 15m P.M. the heavens presented the magnificent appearance of a mighty fan of crimson light stretching out from α Andromedæ, and presenting over two-thirds of the heavens, from S.W. to E., one unbroken sheet of light, varied only by its different shades of crimson mingled with white, which served to distinguish the rays, and the vibratory motion which pervaded it, while waves of paler light shot up from time to time with the rapidity of lightning.

At this time the heavens were divided by a distinct and clearly defined line into two portions, the greater part being brilliant to the extreme, while the remainder, though cloudless, appeared so black, that but for the stars which shone with great distinctness, I should have imagined it overcast with a very heavy cloud; the rays which formed the boundary inclined over like the sides of a spherical triangle, whilst those in the more northern regions appeared vertical.

At 9h 15m P.M. the splendour of the spectacle reached its height. The appearance could no longer be compared to that of a fan but to that of a dome of fire; the unillumined portion of the horizon became confined to about one-fourth part of the circle, extending more or less between S.S.W. and E.: γ Pegasi became enveloped in the light which shot southwards from the summit, passing several degrees below Saturn, so that in the S.E. not more than a space of about 30° in altitude remained dark, while all the rest of the heavens seemed on fire. The summit of the dome presented several remarkable changes of appearance: at one time the rays met as at a point, with great regularity; at another there were seen great irregular masses of light gathered round it, and at another there was a distinct circle round the centre, which was dark. As the evening advanced, this crown changed its position among the stars to a considerable extent, retaining however the same altitude and azimuth as the stars moved westward. After $9^{h}\ 30^{m}$ P.M. the grandeur of the scene diminished, and at $10^{h}\ 30^{m}$ P.M. the crimson colour had disappeared; still the streamers continued with great vigour till October 19, at $1^{h}\ 30^{m}$ A.M., at which time the observations were discontinued.

The altitude and azimuth of the corona were determined from the spherical triangle S P Y, in which S P represents the north-polar distance, and S P Y the hour-angle of α Andromedæ at 9^{h} P.M.



$YP = 36°\ 10'$
$SP = 61\ \ 46$
$SPY = 15$

From this the zenith distance of the corona appears to have been about 18° south, and its azimuth to have been about 41° east.

On the 19th and 20th there were also slight exhibitions of aurora borealis. On these days the magnets at Greenwich Observatory were affected, as I learn both from Mr. Glaisher and from the Registrar-General's Report for the week ending October 21st.

LIX. *On the Chemical Nature of a Wax from China.*

By BENJAMIN COLLINS BRODIE, *Esq.*[*]

THE wax which is the subject of the following investigation, is a substance which was imported into this country from China as an article of commerce. Its appearance closely resembles that of spermaceti. It is, like spermaceti, white and, in large masses, highly crystalline, but differs from it by being

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[*] From the Philosophical Transactions, part i.; having been received by the Royal Society March 30, and read May 11, 1848.

harder, more brittle, and of a more fibrous character of crystallization. The melting-point of the wax is about 83° C. It is but very slightly soluble in either alcohol or æther, but dissolves with great facility in naphtha, out of which fluid it may be crystallized. This substance is generally spoken of as a vegetable wax: on looking however into such facts as I can gather which throw any light on its origin, it seems more than probable that, like bees'-wax, it is the secretion of an insect. Sir George Staunton, in his Embassy to China*, gives an account of a wax of insect origin, which there formed an article of trade, and in his work may be seen a drawing of the insect and of the tree on which the insect lives. Other writers on China give a similar account. In the *Comptes Rendus* for 1840† is a paper by M. Stanislas Julien, who gives an account of this tree wax, and states it to be the work of an insect: where may be found also a great number of extracts from Chinese writers on agriculture, giving an account of the insect itself and of the trees suitable for its food; one of these trees is the *Rhus succedaneum.* This same gentleman, M. Stanislas Julien, gave to M. Lewy, who was engaged in an investigation on these wax substances, a specimen of the wax from this very plant, which is therefore in all probability this insect-wax. The melting-point, the appearance and the analysis of this wax, as given by M. Lewy, agree so exactly with those of the wax which I have examined, that I cannot but believe them to be the same chemical substance, and that this wax also is of insect origin.

The existence of any other wax-making insect, such as this *Coccus ceriferus,* besides the bee, is a point of considerable interest in relation to the question as to the origin of the wax in that insect, and the possibility of the chemical transformations by which it is produced.

The Chinese wax, as it appears in commerce, is a substance nearly in a state of chemical purity. By alcohol small portions of a greasy matter may be separated from it, and on distillation it affords traces of acroleine, which is not a product of the distillation of the pure wax. The impurities however are unimportant.

I have spoken of this substance as a wax; and in truth, although to the eye it more nearly resembles spermaceti or stearine than ordinary bees'-wax, the substance, nevertheless,

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* Vol. i. p. 352, edition 1797.

† Vol. x. p. 619. The title is "*Nouveaux renseignements sur la cire d'arbre, et sur les insectes qui la produisent.*" See also in the same volume, M. Virey, *Sur les insectes qui produisent la substance appelée par les Chinois, "Cire d'arbre."*

which, even in appearance, it more nearly resembles than any other, is the purified cerine, that is, that cerotic acid of which, mixed with certain other waxy matter, in a former paper, I have shown the bees'-wax to consist. The accurate investigation of the chemical nature of the Chinese wax has brought to light certain curious chemical relations which exist between these bodies, and led to the discovery of the alcohol of cerotic acid.

Cerotine.

Chinese wax may be boiled for a long time either with dilute or with concentrated potash with hardly any signs of saponification. If, however, it be melted with the hydrate of potash, it is readily decomposed. This decomposition is best effected in an iron basin over a large gas flame or gentle fire. The mass, after the action, is soluble in boiling water, giving a milky solution. From this solution two substances may be procured; an acid which may be combined with baryta by precipitation of the soap with chloride of barium; and a wax-substance of another nature which is obtained by washing out the baryta salt with any suitable solvent, such as alcohol, æther, or naphtha. The soap, after precipitation by chloride of barium, becomes perfectly clear, and, to obtain the wax-substance which is not combined with the baryta, the baryta salt is first to be separated by filtration from the fluid, then dried and pulverized. It is convenient to effect, at first, a partial separation of the salt from the other matter by washing it out with a large quantity of boiling alcohol, and filtering the solution hot through linen. After this operation has been two or three times repeated, the substance, which passes through the filter, is to be redissolved in the same alcohol and the solutions filtered, in a similar manner, through paper, so as to separate the small portions of the baryta salt which unavoidably pass through the linen. The solution is much facilitated by the addition of a small quantity of naphtha to the alcohol. If the substance be purified by crystallization out of æther and absolute alcohol, its melting-point will be raised to 79° C. Previous to analysis the substance is to be dissolved in absolute alcohol and æther, and the solution filtered. This is a necessary precaution, as the naphtha dissolves traces of the baryta salt. The substance was analysed.

I. 0·258 grm. gave 0·7725 CO_2 and 0·327 HO.

II. The numbers have been mislaid.

III. 0·2602 grm., another preparation, gave 0·7785 CO_2 and 0·334 HO, which give in 100 parts,—

	I.	II.	III.
Carbon	81·55	81·76	81·59
Hydrogen	14·08	14·25	14·26
Oxygen	4·37	3·99	4·15
	100·00	100·00	100·00

These numbers give the formula $C_{54} H_{56} O_2$.

	Atomic weight.	Calculated in 100 parts.
C_{54}	324	81·81
H_{56}	56	14·14
O_2	16	4·05
	396	100·00

This is the formula of an alcohol. Other experiments decided that this was in truth the class of bodies to which this substance belonged, and led me to adopt the particular formula for it which I have given. This alcohol I call cerotine. If this substance be heated with lime and potash according to the method of Dumas, hydrogen gas is given off, and if the experiment be conducted with care there will hardly be traces of any other volatile product. In the tube is found an acid. The experiment requires considerable heat, and I have found that it is best made in a long combustion-tube, suspended by means of two corks in a large tube of porcelain, which forms an air-bath. The apparatus is heated by charcoal in a combustion-trough. In this manner the heat can be regulated with the greatest precision. The acid having been purified in the usual manner, which it is unnecessary again to refer to, is a substance highly crystalline in its texture and perfectly resembling in its sensible properties the cerotic acid from bees'-wax, with which acid analysis shows it to be identical. The melting-point of this preparation was about a degree higher than that of the cerotic acid from wax, namely 81° C.

0·259 grm. gave 0·754 CO_2 and 0·309 HO, which correspond in 100 parts to—

Carbon	79·39
Hydrogen	13·28
Oxygen	7·33
	100·00

This agrees with the formula $C_{54} H_{54} O_4$.

	Calculated in 100 parts.
C_{54}	79·02
H_{54}	13·17
O_4	7·81
	100·00

I prepared also the silver salt of this acid. The method used for its preparation was the same as that used in the case of the cerotic acid from bees'-wax.

I. 0·3775 grm. of this salt gave 0·862 CO_2 and 0·349 HO.

II. 0·3625 grm. of this salt gave 0·833 CO_2 and 0·3385 HO, which correspond in 100 parts to—

	I.	II.
Carbon . . .	62·27	62·74
Hydrogen . .	10·27	10·38
Oxygen and silver	27·46	26·88
	100·00	100·00

I. 0·654 grm. of the same gave 0·135 grm. of silver.

II. 0·629 grm. of the same gave 0·1305 grm. of silver.

These correspond in parts per cent. to—

	I.	II.
Silver . . .	20·64	20·74

The formula of the silver salt of cerotic acid requires—

	In 100 parts.
C_{54}	62·66
H_{53}	10·55
O_4	6·19
Ag	20·90
	100·00

Sulphate of the Oxide of Cerotyle.

When cerotine is treated in the cold with concentrated sulphuric acid, it is only acted on by the acid partially and at the surface. If the action be increased by heat, a reddening of the mass and decomposition take place. If however the cerotine be taken in a state of fine granular division, as obtained by crystallization out of æther, and in this condition acted upon by the acid, all decomposition is avoided and a perfect combination takes place of the acid with the cerotine. The granular crystals are to be dried between blotting-paper, and then digested with the sulphuric acid, in the cold, for two or three hours, sufficient acid being added to render the mixture a rather fluid paste. The mass is to be thrown into cold water, and washed out with the same on a filter. As long as the solution is acid the wash-water will go through perfectly clear, but when the acid is washed out it becomes slightly turbid. After the adhering acid has been thus removed, the substance may be dried *in vacuo*, and, when perfectly dry, dissolved in æther and crystallized out of that fluid. In this condition it is perfectly soluble even in water, and dissolves with the greatest facility in water to which the smallest quantity of alcohol has been added. When the solution in water has been evaporated to dryness at a low temperature, it remains in the form of a soft wax.

I. 0·333 grm. of this substance gave 0·912 CO_2, and 0·3915 HO.

II. 0·3317 grm. of this substance gave 0·9025 CO_2 and 0·3865 HO, corresponding in 100 parts to—

	I.	II.
Carbon	74·67	74·20
Hydrogen	13·06	12·95
Oxygen and sulphur	12·27	12·85
	100·00	100·00

Owing to the loss of a portion of the substance I was not able to make a separate determination of the sulphur. These numbers, however, so correspond to the formula SO_3, $C_{54}H_{55}O + HO$, and so exclude all other probable formulæ, as to determine the constitution of the compound.

	Atomic weight.	In 100 parts.
C_{54}	324	74·31
H_{56}	56	12·84
O_5	40	7·81
S	16	
	436	100·00

Adopting therefore the usual chemical language as to such compounds, this substance is to be regarded as the sulphate of the oxide of cerotyle, containing one equivalent of water, cerotyle being $C_{54}H_{55}$, the hypothetical radical of the alcohol.

Chlor-cerotic Aldehyde—Chlor-cerotal.

The action of chlorine on cerotine gives us a proof of the strong analogy of chemical constitution between alcohol and that substance, notwithstanding the wide interval by which they are separated in the alcoholic series. A body is formed analogous to chloral; two equivalents of hydrogen are removed without substitution, the substance passing, apparently, through the condition of an aldehyde, before chlorine is substituted. The chlorine produces a similar change in the appearance of the substance to that which is produced by the action of chlorine on cerotic acid. The character of wax is entirely lost, and the substance converted into a perfectly transparent slightly yellow body, possessing the appearance and consistency of a gum-resin, and which becomes highly electric by friction. The experiment was made by passing the chlorine over the substance kept melted in a flask by means of a water-bath. The gas was dried. The action is slow, and the experiment takes several days. When no more action was perceived, the substance was boiled with water, dried in a water-bath, and analysed.

I. 0·394 grm. of the substance gave 0·5435 CO_2, and 0·169 HO.

II. 0·4404 grm. of the substance gave 0·6120 CO_2 and 0·186 HO, giving in 100 parts—

Carbon	37·62	37·89
Hydrogen	4·77	4·70
Oxygen and chlorine .	57·61	57·41
	100·00	100·00

I. 0·51075 grm. of the substance gave 1·141 grm. of chloride of silver, equivalent to 0·2814 chlorine.

II. 0·7035 grm. of the substance gave 1·574 grm. of chloride of silver, equivalent to 0·388 chlorine.

III. 0·667 grm. of the substance gave 1·489 grm. of chloride of silver, equivalent to 0·3673 chlorine, giving in 100 parts—

	I.	II.	III.
Chlorine . . .	55·11	55·10	55·07

From these data we may calculate for the substance the formula $C_{54}\left\{\begin{matrix}Cl_{13\frac{1}{4}}\\H_{40\frac{3}{4}}\end{matrix}\right.O_2$.

Calculated in 100 parts—

C_{54}	38·12
$H_{40\frac{3}{4}}$ . . .	4·79
$Cl_{13\frac{1}{4}}$ . . .	55·20
O_2	1·89
	100·00

It is very difficult to tell with certainty when the action of the chlorine on the substance ceases, and I have therefore written the fractional equivalents, which agree rather more closely with the analyses than the whole numbers, $C_{54}\left\{\begin{matrix}Cl_{13}\\H_{41}\end{matrix}\right.O_2$.

The analysis determines with certainty that the substance no longer belongs to the alcohol type; for the addition of two equivalents of hydrogen to the formula would require above 0·3 per cent. more hydrogen than the quantity found, a kind of error which is highly improbable.

Cerotic Acid from the Saponification of the Wax.

The perfect washing out of the baryta salt from which the cerotine has to be separated, is attended with considerable difficulty. It is best effected by naphtha-alcohol or naphtha-æther. The wax acid, after separation from the baryta salt, is to be purified, first by long boiling with water in an open vessel, to get rid of all traces of naphtha, and then by repeated crystallization out of æther. In this way the melting-point may be raised to 78°, 79° C. In appearance the acid perfectly resembles the cerotic acid, with which it has also the same melting-point. The substance, after long-repeated crystallizations, was analysed.

I. 0·2632 grm. gave 0·765 CO_2 and 0·3035 HO.
II. 0·2631 grm. gave 0·7626 CO_2 and 0·3095 HO.
III. 0·232 grm. gave 0·6695 CO_2 and 0·274 HO.

These analyses give in parts per cent.—

	I.	II.	III.
Carbon . .	79·26	79·04	78·70
Hydrogen .	12·81	13·07	13·12
Oxygen . .	7·93	7·89	8·18
	100·00	100·00	100·00

The formula $C_{54} H_{54} O_4$ requires—

C_{54}	79·02
H_{54}	13·17
O_4	7·81
	100·00

I prepared the silver salt of this acid.

I. 0·656 grm. of substance gave 0·1335 silver.

II. 0·6635 grm. of substance gave 0·1355 silver, which gives in 100 parts—

	I.	II.
Silver . .	20·35	20·42

I. 0·4675 grm. of the salt gave 1·072 CO_2 and 0·431 HO.

II. 0·4655 grm. of the salt gave 1·0767 CO_2 and 0·4295 HO, which give per cent.—

	I.	II.
Carbon	62·53	63·08
Hydrogen	10·24	10·25
Oxygen and silver .	27·23	26·67
	100·00	100·00

The formula $C_{54}H_{53}O_3+AgO$ requires—

C_{54}	62·66
H_{53}	10·25
O_4	6·19
Ag	20·90
	100·00

There is a difference between the calculated and found amount of silver of about 0·5 per cent. which I cannot doubt arises from traces of cerotine still adhering to the acid, notwithstanding all the pains I took to wash out the salt: for I found that the amount of silver increased with the purification of the acid by crystallization, even after I could find not the slightest variation in the melting-point. The silver salt made from an acid which was a part of the same original preparation as the above and of exactly the same melting-point, but which had not been so often crystallized, gave in two determinations 20·07 and 20·09 per cent. silver. It will be seen that the formula I have given for the acid is confirmed by the analysis of the acid obtained from distillation of the wax.

Distillation of Cerotine.

Cerotine requires a high temperature for its distillation. The operation is accompanied with little explosions, owing to the formation of a small quantity of water. The distillate is perfectly colourless to the last, and solid, giving, when pressed with blotting-paper, hardly a trace of oil; it resembles in its general appearance the cerotine itself, but has a lower melting-point, about 70° C.

The distillate also contains a larger per-centage of carbon than the substance. A portion, melting at 73° C. and purified by crystallization, gave to analysis carbon 83·20 and hydrogen 14·22 per cent.; but it is very difficult to raise the melting-point much beyond 73° C. At first, from the constancy of the melting-point, I was led to think that a new oxygen combination had been formed. I found, however, afterwards that the melting-point could be raised to 79° C.; that is, the melting-point of the alcohol, a substance of a lower melting-point remaining behind; and there can be little doubt but that part of the cerotine distils over unaltered, while another portion decomposes into solid hydrocarbon and water. I found, in fact, that the amount of carbon diminished as the melting-point was raised.

Distillation of Chinese Wax.

The Chinese wax is decomposed by heat. When distilled, the distillate consists of two portions; a wax acid which forms the earlier portions of the distillate, and a portion which is not acted on by potash, and from which the acid portion may be separated by saponification. The soap requires to be drawn off by a siphon. I will first give the analysis of the acid, which is undoubtedly the same acid as that procured by saponification of the wax itself. The melting-point of the acid may, by the methods pursued in other cases of separation from the soap, purification and crystallization out of æther, be raised to 80°, 81° C., which is probably the true melting-point of the cerotic acid.

0·2613 grm. of the acid gave 0·7555 CO_2 and 0·3075 HO, which corresponds in parts per cent. to—

Carbon	78·85
Hydrogen . . .	13·08
Oxygen	8·07
	100·00

I give here again, for the sake of comparison, the numbers of the formula.

	Calculated.
C_{54}	79·02
H_{54}	13·17
O_4	7·81
	100·00

The siver salt of the acid, prepared as in other cases, gave to analysis the following results:—

0·3625 grm. of the salt gave 0·827 CO_2 and 0·332 HO, giving in 100 parts,—

Carbon	62·21
Hydrogen	10·18
Oxygen and silver . .	27·61
	100·00

I. 0·649 grm. of the salt gave 0·139 grm. of silver.
II. 0·626 grm. of the same gave 0·13375 grm. of silver.
These determinations correspond in parts per cent. to—

	I.	II.
Silver . .	21·42	21·19

The formula $C_{54}H_{53}O_3+AgO$ requires—

	Calculated.
C_{54}	62·66
H_{53}	10·25
O_4	6·19
Ag	20·90
	100·00

Cerotene.

The portion of the distillate from which the soap has been separated by decantation and by repeated washings and boiling out with water, consists chiefly of a solid hydrocarbon, one of those substances which have been comprehended and confounded under the general name of paraffine. It is mixed with a certain quantity of oil, from which it may be almost entirely separated by pressure in a press between folds of blotting-paper. If this substance be crystallized out of naphtha-alcohol and then out of æther, it may be obtained of a melting-point of 57°, 58° C. In this state it is highly crystalline on cooling, and presents the general appearance of the substance called paraffine.

I. 0·2555 grm. gave 0·802 CO_2 and 0·331 HO.

II. 0·2593 grm. gave 0·810 CO_2 and 0·332 HO, which give in 100 parts,—

	I.	II.
Carbon . . .	85·60	85·20
Hydrogen . .	14·39	14·23
	99·99	99·43

These analyses correspond to the formula

		Calculated.
C_{54} . . .	324	85·71
H_{54} . . .	54	14·28
	378	99·99

This substance may be called cerotene. After the discovery of the alcohol, there was a strong presumption that the hydrocarbon and the cerotine would be related in the manner expressed by the formula, the hydrocarbon being the olefiant gas of the wax alcohol. It was, however, very desirable to find some method for the determination of its formula. I investigated, with this view, the action of chlorine on the substance.

Chlor-Cerotene.

If moist chlorine be passed over the melted cerotene in the manner before described in the case of the other wax substances, it is readily acted upon by the gas. It loses its wax character, becomes gum-like, and is ultimately converted into a transparent resin; the substance becoming harder and harder with the increase of the chlorine substituted, at length becoming extremely hard, and cracking in all directions, on cooling, on the surface of the glass on which it has been melted. The action goes on very slowly, but more rapidly with moist than with dry gas. It was continued for several weeks, until no more traces of hydrochloric acid formed could be observed.

During the operation portions of the substance were taken out at intervals and analysed.

0·303 grm. of the first portion thus examined gave 0·3495 CO_2 and 0·0965 HO, corresponding in 100 parts to—

Carbon . . .	31·46
Hydrogen . .	3·54
Chlorine . . .	65·00
	100·00

These numbers agree with the formula $C_{54}\left\{\begin{matrix}H_{35}\\Cl_{19}\end{matrix}\right.$, which requires in 100 parts,—

C_{54}	31·31
H_{35}	3·39
Cl_{19}	65·30
	100·00

After an interval of about four days the substance was again analysed.

I. 0·376 grm. of the substance gave 0·4060 CO_2 and 0·9085 HO.

II. 0·362 grm. of the substance gave 0·3915 CO_2 and 0·1005 HO.

These analyses give in 100 parts,—

	I.	II.
Carbon . . .	29·45	29·49
Hydrogen . .	2·91	3·09
Chlorine . .	68·64	67·42
	100·00	100·00

The formula $C_{54}\left\{\begin{matrix}H_{33}\\Cl_{21}\end{matrix}\right.$ requires

C_{54}	29·43
H_{33}	2·99
Cl_{21}	67·58
	100·00

After a further interval of several days the substance was again analysed.

I. 0·4434 grm of the substance gave 0·464 CO_2 and 0·115 HO.

II. 0·309 grm. of the substance gave 0·323 CO_2 and 0·08 HO, which give in 100 parts,—

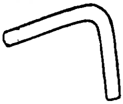
	I.	II.
Carbon . . .	28·54	28·51
Hydrogen . .	2·79	2·88
Chlorine . . .	68·67	68·61
	100·00	100·00

Notwithstanding that between these and the last analyses the action of the chlorine had been prolonged for a considerable time, the formula shows a difference of only one equivalent of chlorine.

The formula $C_{54}\left\{\begin{matrix}H_{32}\\ Cl_{22}\end{matrix}\right.$ requires

C_{54}	28·76
H_{32}	2·84
Cl_{22}	68·40
	100·00

These analyses determine with certainty the ratio of the hydrogen to the carbon in the cerotene, and leave no doubt as to the nature of the hydrocarbon. M. Lewy attempted to take the density of the vapour of paraffine from bees'-wax. He found, however, that this could not be effected, as the substance was altered, in process of conversion into vapour, with the formation of a small quantity of hydrocarbon gas; the paraffine however remaining white, and the analysis showing no variation in composition*. It has been also remarked by others that if bees'-wax be repeatedly distilled, the solid hydrocarbon disappears from the distillate. These observations point to the source of the oil in the distillation of the Chinese wax, viz. the transformation of the cerotene itself into isomeric hydrocarbons. In fact, I found, if the cerotene be distilled and redistilled in a closed tube of the form annexed, that by effecting the distillation in this manner under pressure, after about two distillations the distillate becomes liquid and the solid matter entirely disappears. The experiment after about six distillations was put an end to by the bursting of the heated end of the tube, when a large quantity of combustible vapour was given off. The oil which had



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* *Ann. de Chimie*, Series III. vol. v. p. 398.

collected at the other end of the tube was a mixture of hydrocarbons of various boiling-points, from 75° C. to above 260° C. No trace of solid matter was to be seen.

If from the products of decomposition we turn to the analysis of the Chinese wax itself, we find numbers which are perfectly consistent with the idea that the chemical position of this body is among the class of compound æthers, where its reactions also would lead us to place it.

To purify the substance, it is to be crystallized out of naphtha and alcohol; washed with æther to remove the naphtha; boiled with water and crystallized again out of absolute alcohol, in which it is soluble, although with difficulty. Its melting-point is 82° C.

I. 0·2644 grm. gave 0·798 CO_2 and 0·323 HO.

II. 0·2622 grm. gave 0·79 CO_2 and 0·3205 HO, which give in 100 parts—

	I.	II.
Carbon . . .	82·31	82·16
Hydrogen . .	13·57	13·58
Oxygen . . .	4·12	4·26
	100·00	100·00

These numbers agree with the formula

C_{108} . . .	648	82·23
H_{108} . . .	108	13·71
O_4 . . .	32	4·06
	788	100·00

This formula affords us a simple solution of the decompositions of this substance by saponification and by heat.

In the former case,

$$C_{108}H_{108}O_4 + KO, HO = \overbrace{C_{54}H_{53}O_3, KO} + C_{54}H_{56}O_2.$$

In the latter,

$$C_{108}H_{108}O_4 = C_{54}H_{54}O_4 + C_{54}H_{54}.$$

It is my intention shortly to offer to the Society another communication, on the nature of myricine from bees'-wax; but I will now take the opportunity of stating that I have discovered in the investigation two wax substances of the formulæ $C_{60}H_{62}O_2$ and $C_{92}H_{92}O_4$.

LX. *Notices respecting New Books.*

The Physical Atlas. Parts VI. to X.

NEARLY twelve months ago we directed attention to the earlier portions of this work, and the completing Parts have been some time in our hands. In these we find the high character fully sustained, and we congratulate Mr. Johnston on the successful conclusion of his labours, which have given to the public one of the most striking of modern standard works. Of its value it is no longer necessary to speak, since it has now an European reputation. In pursuance of the course before adopted, we present a brief synopsis of the contents of the parts published since our former notice.

Part VI. contains—1. A Chart of the Geological Structure of the Globe according to Ami Boué, corrected up to Sept. 1846; accompanied by notes on the general principles of Geology, and their application, by Prof. Nichol. 2. A Physical Chart of the Indian Ocean. 3. Two maps showing the distribution of the Rodentia and the Ruminantia, constructed from the notes of Mr. Waterhouse, accompanied by elaborate tables drawn up by that gentleman.

Part VII.—1. A map of the lines of equal Polarization of the Atmosphere, with some account of the subject, by Sir David Brewster. 2. Comparative Views of remarkable Geological phænomena. This is a highly interesting sheet, exhibiting maps and views of a number of volcanoes, regions of volcanic origin, &c., after Humboldt, Von Buch and others, an exact relative proportion being preserved, so that it is easy to estimate comparative extent of surface. 3. A map of the River Systems of America, with an enlarged sketch of the Niagara, plan of the Delta of the Mississippi and of the singular bifurcation of the Orinoco.

Part VIII.—Sheets 1 and 2 are devoted to a Palæontological Map of the British Islands, by Prof. E. Forbes, which with the accompanying text presents a mass of information on this subject, illustrating it in every point of view and exhibiting as much originality as industry. The author's own researches into the distribution of organized beings "in time" rendered him peculiarly fitted to the execution of this chart, and its elaborate character at once makes it a striking feature of the Atlas, and reflects credit on the condition of Geological science in Britain. 3. A Tidal Chart of the British Seas, showing the progress of the Wave of High water, the line of High-water mark in Greenwich time at new and full moon, and the depth of the Sea; in the text is also incorporated a smaller Tidal Chart of the World; these are by Mr. Scott Russell.

Part IX.—1. A map of the Mountain chains of South America. 2. A Chart exhibiting Humboldt's Isothermal lines and the lines of equal Barometric pressure at the level of the sea. 3. Charts exhibiting the distribution of the most important Plants yielding food for Man, with indications of the Isotheral and Isochimenal lines. These lines are at present so imperfectly known, that as here given, they are to be taken as mere indications. A wide field lies here for investigation; it is to be desired that not only the mean summer and winter lines should be completed, but even those of months, which, together with the observation of periodic phænomena of animal and vegetable life, may lead to many important generalizations.

Part X.—1. The Mountain chains of Asia and Europe. 2. A Chart of the Geographical distribution of the Currents of Air, showing the course of the perennial, periodical and variable Winds, also the various regions of prevalent Hurricanes. 3. An Ethnographic Map of Europe. With this Part, which concludes the work, are given useful Indices, in which the principal subjects illustrated are arranged under the heads of the four great divisions of the Atlas, viz. Meteorology, Hydrology, Geology, and Zoology-Phytology.

Before taking leave of the Physical Atlas, we cannot but express a sincere hope that the support which it may receive will be as liberal as the spirit in which it has been executed.

LXI. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 317.]

May 22, 1848. SOME Remarks on the Theory of Matter. By R. L. Ellis, Esq., M.A., Fellow of Trinity College, Cambridge.

The question to which these remarks principally relate is this: Can all phænomena, *e. g.* those of chemistry, be explained mechanically? The writer, assuming that this question is to be answered negatively, endeavours to determine what principles of causation, beside mechanical force, may be introduced into physical theories, consistently with the doctrine that the secondary qualities of bodies are to be explained by means of the primary. His conclusion is, that we are at liberty, in constructing an hypothesis as to the mode of action of matter on matter, to introduce a new principle of causation (which he calls $(\text{force})^2$), bearing the same relation to force that force does to velocity; and further, that following the analogy here suggested, we may introduce an indefinite number of such principles, viz. $(\text{force})^3 \ldots (\text{force})^n$, &c., all essentially distinct from one another, and from those previously recognised.

But, on the other hand, he conceives that it is necessary to reject any modification of *qualitative* action; and that consequently physical science, though it may cease to be wholly *mechanical*, will yet always continue to be *cinematical*, in the largest sense of which this word (so far as relates to local motion) can admit.

June 5.—Methods of Integrating Partial Differential Equations. By Prof. De Morgan.

This paper contains a sketch of two distinct methods. In the first (x, y, z, p, q, r, s, t, having their usual significations) the given equation is supposed to be of the form $\varphi(x, y, p, q)=0$, and this is made the result of elimination between two equations involving a new variable v. From these two, and their four differentials of the first order, p, q, r, s, t are eliminated, and an equation of the first order results between x, y, v. This last equation is often more manageable than the original one.

The process is rendered very simple when the given equation can be reduced to depend on two of the form

$$p=\phi(x,y,v) \quad q=\psi(x,y,v).$$

The second method was completed, Mr. De Morgan states, and out of his hands for transmission to the Society, when he discovered that Mouge had communicated it to the Institute, by which body it was never published. But M. Chasles found it among the manuscripts of the Institute, and stated it a few years ago in one of the notes to his *Aperçu Historique .... des Méthodes en Géométrie.* Its occurrence in the voluminous additions made to a work which itself treats only of geometry, seems to have prevented it from becoming known to any writer on the differential calculus. Certain particular cases appear in the writings of Legendre and Lacroix.

Let the equation be $\phi(x,y,z,p,q,r,s,t)=0$. Change x into p, y into q, z into $px+qy-z$, p into x, q into y, r into $\frac{t}{rt-s^2}$, s into $\frac{-s}{rt-s^2}$, t into $\frac{r}{rt-s^2}$. If the equation thus resulting can be integrated, let its solution be $\mathrm{Z}=\psi(\mathrm{X}, \mathrm{Y})$. Then the solution of the original equation can be obtained by eliminating X, Y, Z from

$$\mathrm{Z}=\psi(\mathrm{X},\mathrm{Y}) \quad x=\frac{d\mathrm{Z}}{d\mathrm{X}} \quad y=\frac{d\mathrm{Z}}{d\mathrm{Y}} \quad z=x\mathrm{X}+y\mathrm{Y}-\mathrm{Z}.$$

In both methods the most effective mode of proceeding is to find what Lagrange calls a *primary solution,* containing two arbitrary constants, and then to use that primary solution.

LXII. *Intelligence and Miscellaneous Articles.*

ON THE RIPENING OF FRUITS AND THE GELATINOUS BODIES OF VEGETABLES. BY M. E. FREMY.

THE author gives the following summary of the facts detailed in his memoir on the above-named subjects:—

1. There exists in the tissues of vegetables, and principally in the pulps of fruits and of roots, a substance insoluble in water, which he has named *pectose*; its characteristic property is that of being converted into pectin, by the influence of the weakest acids. It differs essentially from cellulose in all its properties.

2. Pectin exists in the juices of ripe fruits; it may be artificially obtained by causing boiling weakly acid liquors to act upon pectose. Pectin ought to be considered as a weak acid; it does not precipitate the neutral acetate of lead, and changes into pectic acid under the influence of soluble bases.

3. Pectin, submitted for some time to the action of boiling water, acquires the property of precipitating neutral acetate of lead, and is converted into a new substance which M. Fremy calls *parapectin*;

it is neutral to test-papers, and occurs in the juices of perfectly ripe fruits.

4. Parapectin is transformed, under the influence of acids, into a substance which the author calls *metapectin*; it has the properties of a weak acid, reddens tincture of litmus, and precipitates chloride of barium; it may be named *metapectinic acid.*

5. The preceding substances form compounds which are soluble in a certain number of acids, and principally with sulphuric and oxalic acids. These compounds are crystallizable, and form gelatinous precipitates with alcohol.

6. There accompanies pectose in vegetable tissues, a peculiar ferment called by M. Fremy *pectase*; this has the property of transforming pectin successively into two gelatinous acids, which are the pectosic and pectic acids; this change occurs without the presence of air or the disengagement of gas, and constitutes the *pectic fermentation,* which may be compared to the *lactic fermentation.* Pectase exists in vegetables in two states, one soluble and the other insoluble.

7. When pectin is submitted to the action of pectase, the acid first formed is a new acid, the *pectosic*; it differs from pectic acid in being completely soluble in boiling water.

8. Pectosic acid is transformed into pectic acid by the prolonged action of pectase; the pectosic and pectic acids are also formed when pectin is added to an alkali either free or carbonated, or under the influence of lime, barytes, or strontia.

9. Pectic acid dissolves in considerable quantity in neutral alkaline salts, and especially in ammoniacal salts, which contain an organic acid; gelatinous double salts with an acid reaction are then formed, which are precipitated in a gelatinous state by alcohol.

10. Pectic acid, kept for several hours in boiling water, completely dissolves, and is transformed into a new acid, the *parapectic acid.*

11. Parapectic acid changes, under the long-continued influence of water, into a powerful acid, the *metapectic acid.*

These two last acids arise under several circumstances, and principally by the reaction of acids, alkalies, or of pectase, pectin and pectic acid; they possess the property of decomposing by ebullition the double tartrate of potash and copper, like glucose.

12. Gelatinous substances exposed to a temperature of 392° Fahr. disengage water and carbonic acid, and are converted into a black pyrogenous acid, which the author calls *pyropectic acid.*

13. Gelatinous substances exhibit all the generic characters of acids, the capacity of saturation and their power augmenting in proportion as they recede from pectose; they appear to be all derived from a ternary molecule $C^8 H^5 O^7$, and differ from each other only as to water.

14. The properties of the gelatinous substances of vegetables afford an explanation of the alterations which a fruit undergoes when submitted to the action of heat, as well as of the formation *vegetable jellies*. Vegetable jellies may be produced—1st, by the conversion of pectin into pectosic and pectic acids under the influence

of pectase; 2ndly, by the combination of pectic acid with the organic acids contained in fruits.

15. The pectose contained in green fruits is successively transformed, during ripening, into pectin, metapectin, and metapectic acids. These changes are determined by the influence of acids and pectase.

It will appear from this summary, in the opinion of the author, that after having ascertained the nature of the principal properties of the substances which constitute the pulp of certain fruits, he was led to observe that the gelatinous substances of vegetables undergo modifications by the influence of reagents entirely comparable to those to which they are subject during vegetation.—*Ann. de Ch. et de Phys.*, Septembre 1848.

ON SULPHOMORPHIDE AND SULPHONARCOTIDE, DERIVATIVES FROM MORPHIA AND NARCOTINA. BY MM. LAURENT AND GERHARDT.

In 1845 M. Arppe described a peculiar substance which he obtained by treating morphia with an excess of sulphuric acid. To this compound he assigned the formula $4(C^{35}H^{40}N^2O^6)+5SO$, which is not analogous to any organic compound.

On considering the manner in which this compound is obtained, the authors thought that its composition would resemble that of the amides and anilides; and their experiments supported this supposition: they obtained a similar compound with narcotina.

Sulphomorphide.—This substance was prepared according to M. Arppe's process, by heating morphia with a slight excess of sulphuric acid. When recently prepared it is white, but eventually it becomes green, even in closed tubes; this colour is especially deepened by drying the product at 266° to 302° Fahr.; it is permanent, and does not appear to be owing to the action of the air, for the corresponding product, prepared with narcotina, is immediately obtained of a deep green colour.

Sulphomorphide is a fixed body; heated on platina foil it yields a bulky charcoal, which is extremely difficult to burn. By analysis 100 parts yield—

Carbon	63·0
Hydrogen	5·8
Sulphur	5·4

These results lead to the formula $C^{34}H^{36}N^2O^8S$, that is to say, to the formula of neutral sulphate of morphia, minus two equivs. of water, $SO^4(H^2, C^{17}H^{19}NO^3)-2OH^2$; these proportions require

C^{34}	408	64·5
H^{36}	36	5·7
N^2	28	
S	32	5·1
O^8	128	
	632	

It is to be observed, that the quantity of carbon obtained by analysis is rather too small; but similar errors are inevitable with bodies which are uncrystallizable and difficult to burn. M. Arppe obtained still less carbon (61·2) than the authors.

Boiling nitric acid attacks sulphomorphide and forms sulphuric acid; water added to the solution precipitates a yellow flocculent matter, soluble in ammonia. As no gas is liberated during the formation of sulphomorphide, and as the sulphuric liquid contains no other substance, the formula given seems sufficiently controlled by the preceding analysis.

Sulphonarcotide.—When narcotina moistened with water is heated with a slight excess of sulphuric acid, a solution is obtained which becomes of a deep brown colour when more strongly heated, and eventually thickens. No gas is liberated during this reaction; the mixture is to be diluted with water and boiled, thrown on a filter and washed with cold water, in which it appears to be insoluble. It dissolves in alcohol, but it is not deposited from it in crystals.

This product behaves like sulphomorphide; when calcined on platina foil, it yields much charcoal very difficult to burn; subjected to distillation, it yields water and oily matter of a fœtid odour. By analysis 100 parts yielded

Carbon	59·1
Hydrogen	5·3
Sulphur......	3·6

These correspond to a formula represented by neutral sulphate of narcotina, minus two equivs. of water, $SO^4(H^2, 2C^{23}H^{25}NO^7)—2OH^2$. This formula requires—

C^{46}........	552	60·2
H^{48}........	48	5·2
N^2	28	
S	32	3·5
O^{16}........	256	
	916	

Ammonia does not act upon sulphonarcotide, potash dissolves it with a brown colour, and acids separate in the green state. When boiled with nitric acid, sulphonarcotide gives sulphuric acid, and also a yellow matter soluble in ammonia.

Sulphomorphide and sulphonarcotide evidently belong to the same class of bodies as the amides and the anilides; they are to the sulphates of morphia and narcotina, what sulphamide and sulphanilide are to the sulphates of ammonia and aniline. Certainly it has not been possible to separate morphia and narcotina from them; but it must be remembered, that with respect to the anilides this regeneration requires the intervention of a high temperature, and that this necessarily acts destructively on non-volatile alkaloids, like morphia and narcotina.—*Ann. de Ch. et de Phys.*, Septembre 1848.

COMPOSITION OF URANITE AND CHALKOLITE.

BY M. WERTHER.

The specimen analysed by M. Werther was from Gunnislake Mine, Cornwall. He operated on a well-crystallized specimen. His results were as follows:—

	I.	II.	III.
Phosphoric acid	15·01	13·52	14·40
Oxide of uranium	58·45	57·20	60·80
Oxide of copper			8·27
Water..............	15·22	15·55	
Silica		0·49	
Earthy matter........		0·61	0·22

The formula deduced from these analyses is

$$[P^2O^5,\ Cu^2O,\ 2U^2O^2O+8\ aq].$$

The uranite of Autun gave the following results:—

Phosphoric acid............	14·00
Oxide of uranium	63·28
Lime	5·86
Barytes	1·03
Water	14·30

Uranite has therefore a composition similar to that of chalkolite, its formula being $[P^2O^5, Ca^2O, 2U^2O^2O+8\ aq]$. It is to be observed that the formula long since proposed by M. Berzelius agrees exactly with that of M. Werther.

M. Werther obtained chalkolite artificially by the following process:—He took the salt of uranium $[P^2O^5, H^2O, 2U^2O^2O+8\ aq]$, and boiled it for some time with a solution of subacetate of copper, decanted the liquid, digested the residue with acetic acid, and washed it perfectly: he thus obtained a greenish powder, which presented by the microscope the unaltered crystals of the phosphate of uranium which he had employed. This product contained 14·6 per cent. of water, and 8·7 per cent. of oxide of copper.—*Journ. de Ph. et de Ch.*, Juillet 1848.

SOCIETY OF THE FRIENDS OF THE NATURAL SCIENCES OF VIENNA.

The *Wiener Zeitung* of the 28th September last contains a report of the proceedings of a Meeting held at the Imperial Mint of Vienna, on the 22nd of the same month, at which Bergrath Haidinger presided.

The President communicated to the Meeting that his application to the Government respecting the formation of a Society of the Natural Sciences had received the following favourable answer:—"According to an announcement made by the Minister of the Interior on the 22nd of July of the present year, His Majesty the Emperor and King, by a Decree of the 18th of July, has been graciously pleased to sanction the formation of the Society proposed by you, under the appellation of *The Society of the Friends of the Natural Sciences of Vienna* (Gesellschaft der Freunde der Naturwissenschaften in Wien)." Circumstances inevitably resulting from the existing state of affairs, and the summer-months during which so many scientific men and others interested are absent from the capital, render it unavoidable to postpone for a time the General Meeting which is necessary to be held for the formal constitution of the Society. Notice of this Meeting will be duly given.

This communication was preceded by several others of a scientific nature, one of which was from Dr. Friedrich Bialloblotzky of Hanover, giving an outline of his intended journey into a totally unknown part of Africa. The position of the sources of the Nile is a problem, which, from the earliest ages down to the present time, has given rise to numerous geographical inquiries. Hypotheses of various kinds have been formed and rejected; and all that is certain is that nothing positive is known on the subject. The recent expeditions up the Nile have reached only the 4th degree of north latitude, beyond which, according to the statements of the natives, the river extends a distance of thirty days' journey further to the south. In a valuable communication made by Dr. Beke to the British Association for the Advancement of Science at the late Meeting at Swansea*, the information collected by himself in Abyssinia was compared with the results arrived at by other travellers; and it was clearly shown that the essential point to be determined is, whether the Upper Basin of the Nile is not bounded towards the east by a continuous mountain-range, stretching from north-east to south-west, as far as the fourth degree of south latitude, or even further; the waters from which mountain range are received into the main stream of the river flowing from south to north. At the instance of Dr. Beke, Dr. Bialloblotzky has now undertaken the task of attempting the solution of this problem by a method directly opposite to that hitherto adopted. For this purpose, he will proceed by the way of Aden to Mombás, on the east coast of Africa, in about 4° S. lat.; which place will be regarded as the starting-point of his journey. Having hastily crossed the unhealthy districts near the coast, the traveller will penetrate more slowly into the upper country, making friends with the native tribes as he proceeds, so as to ensure a more favourable reception from those residing further in the interior. This is expected to occupy him a twelvemonth: he then calculates on being engaged two years more in exploring the mountain country; and should circumstances prove favourable, the enterprise may possibly be completed by a voyage down the Nile to the Mediterranean.

ON THE DISTILLED WATERS OF CHERRY-LAUREL AND BITTER ALMONDS. BY M. LEPAGE.

The author having made numerous experiments on the above preparations, the following is a summary of the inferences deducible from them.

1. Volatile oil and hydrocyanic acid pre-exist in large quantity in the green leaves of the cherry-laurel; these two products are readily separated by means of æther.

2. Exsiccation completely dissipates the volatile oil and hydrocyanic acid, and consequently deprives them of their medicinal and poisonous properties.

3. There occurs, however, a principle in the dry leaves which cold water is unfit to remove, but which is taken up by boiling water, or still better by alcohol. This principle, put into contact with milk of sweet almonds, acts like a solution of amygdalin.

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* Since printed in the Edinburgh New Philosophical Journal, No 90, for October 1848, vol. xlv. pp. 221–251.

4. There is no advantage in allowing before distillation the green leaves of the cherry-laurel to macerate in water, even with the addition of the milk of sweet almonds, for the purpose of preparing a stronger distilled water; for the cold water does not dissolve the substance contained in these leaves analogous to amygdalin, and susceptible of being transformed by emulsion into hydruret of benzoyle and prussic acid.

5. In the decoction remaining after distilling the leaves of the cherry-laurel, the bitter matter described by Winkler always occurs; this is susceptible of being transformed into essential oil and hydrocyanic acid by almond emulsion.

6. Cherry-laurel water distilled from dry leaves, first macerated in boiling water, to which milk of sweet almonds is added when cold, contains hydrocyanic acid and volatile oil, but in much smaller quantity than that prepared with fresh leaves; the dry leaves ought, therefore, never to be substituted for them.

7. The water prepared with the dry leaves macerated in cold water, before distillation, contains also a quantity of hydrocyanic acid, but no quantity of volatile oil appreciable by the minutest tests.

8. The various reagents which it is convenient to employ to appreciate the quantity of distilled bitter almond and cherry-laurel waters may be divided into two classes:

First Class.—Reagents which serve to prove the presence of hydrocyanic acid; these are nitrate of silver, acid sulphate of ferroso-ferric oxide of iron and potash, and protosalts of mercury:

Second Class.—Reagents which serve to prove the presence of the volatile oil; these are, ammonia, ammoniuret of copper, iodine and bromine: chlorine, which was not tried, might probably act like the two last.

9. Ammonia, the sulphate and the sulphotartrate of quina, cannot be employed to determine the strength of distilled cherry-laurel and bitter almond waters; but chloride of gold employed cold and in proper proportion, appears suitable for this purpose. When this salt reacts upon the hot distilled waters, hydrochloric acid is formed and cyanide of gold is deposited by concentrating the liquors.

10. The distilled waters above-named, when exposed to the action of the air in wide open vessels, simply covered with paper, lose, after a certain time, all the oil and acid which they contain; the bitter almond water requiring about a month, and the cherry-laurel water, which is more stable, ten weeks to three months.

11. These distilled waters also undergo the same alteration in well-closed vessels if partly empty; but when the vessel is small, and not too frequently opened, the waters scarcely lose their active principles in four or five months.

12. At the expiration of twelve months these waters, kept in vessels perfectly filled and with glass stoppers, lost none of their active principles. In this case, as also when kept in partially full bottles, there occurs, after a certain time, a deposit of a light yellowish sediment.

13. Lastly, a small quantity of an ammoniacal salt always occurs in these distilled waters when they have undergone change.—*Journ. de Pharm. et de Ch.*, Septembre 1848

ON METALLIC CARBONATES. BY M. J. LEFORT.

The author obtained the metallic carbonates which he examined by treating the salts with neutral alkaline carbonates and bicarbonates, both cold and hot.

Carbonate of Manganese obtained by the above methods is always represented by $CO^2+MnO+HO$. When heated it begins to lose water at about 194° F; it may be rendered perfectly anhydrous without any notable change of colour; it does not absorb oxygen below 576° F.; it is a white powder with a rose tint.

Carbonate of Cadmium.—Almost all treatises on Chemistry gives CO^2+CdO as the formula of this compound; the author's analyses prove that it always contains 4 to 5 per cent. of water, or half an equivalent; this it loses between 180° and 248° F. Its composition is then $2CO^2+CdO+HO$.

Carbonate of Nickel.—Oxide of nickel forms three perfectly definite compounds with carbonic acid:—

1. A basic carbonate whenever cold solutions of the salts of nickel are precipitated by cold solutions of neutral carbonates. It is of an apple-green colour, and represented by $2CO^2+5NiO+8HO$.

2. A sesquibasic carbonate; the colour very nearly resembles that of the preceding; it is formed when the salts of nickel are treated with bicarbonates. Its formula is $2CO^2+3NiO+6HO$.

3. A pentabasic carbonate, which may be prepared by boiling the two preceding salts, or still better precipitating the hot solutions of the salts of nickel by neutral carbonate of potash or soda. Its colour is meadow-green, and its formula $CO^2+5NiO+5HO$.

Carbonate of Chromium.—The salts of sesquioxide of chromium give with the alkaline carbonates and bicarbonates, sometimes a peculiar hydrate, and sometimes a definite compound represented by $CO^2+Cr^2O^3+4HO$. When sulphuret of chromium of the green variety is treated with a neutral or alkaline bicarbonate, all the carbonic acid is disengaged, and hydrate of sesquioxide of chromium is at the same time precipitated; but if the operation be conducted with a salt of the violet blue modification, the salt formed is always that of the above-stated composition.

Carbonate of sesquioxide of chromium, exposed to the action of heat, loses at about 167° F., 19·58 per cent. of water, which corresponds to three equivalents; it does not lose its last equivalent of water and its carbonic acid till heated to above 572° F.

Carbonate of Bismuth exists in the anhydrous and hydrated state.

It is anhydrous whenever a salt of bismuth, as neutral as possible, is treated with an alkaline carbonate either cold or hot; no carbonic acid is evolved, and the precipitate formed has always the formula $CO^2 Bi^2 O^3$. But if instead of a neutral carbonate a bicarbonate is employed, much acid is disengaged, and a white precipitate, much lighter than the preceding, is obtained, represented by $CO^2 Bi^2 O^3+HO$; it loses its equivalent of water at 212° to 248° F.

Carbonate of Lead.—Carbonate of lead obtained by treating a salt of lead with a neutral or bicarbonated alkali, possesses, as has been long known, the formula CO^2+PbO; but when hot solutions are used, the composition is very different, and represented by $2CO^2+3PbO+HO$.

This composition is the same as that which MM. Mulder, Link and Hochstetter have found to exist in the different ceruses of commerce.

The author states that some comparative trials made by artists, have satisfied him that this new ceruse covers as well as those of the best manufactures in France, and it is less hurtful to workmen than the common operation.—*Comptes Rendus*, September 4, 1848.

META-ANTIMONIATE OF AMMONIA. BY M. E. FREMY.

The author observes that this salt has not hitherto been described by any chemist. It was prepared by first precipitating, with a great quantity of water, very pure perchloride of antimony, in order to obtain hydrated meta-antimonic acid; this acid was allowed to remain during several weeks in a concentrated solution of ammonia.

The meta-antimonic acid dissolved slowly in the ammonia, and produced a meta-antimoniate of ammonia, containing one equivalent of acid and two equivalents of ammonia, corresponding to the bibasic salt of potash, $Sb^2 O^3, 2KO$.

The bibasic meta-antimoniate of ammonia appears difficult to separate; but when a few drops of alcohol are added to the aqueous solution, a crystalline salt is precipitated containing one equivalent of acid and one of base, and corresponding to the acid meta-antimoniate of potash.

It yielded by analysis 68·1 per cent. of meta-antimonic acid; representing the meta-antimoniate of ammonia by the formula $Sb^2 O^5, Az H^3, HO + 6HO$, theory gives 5·6 per cent. of azote, 68·2 of meta-antimonic acid, and 4·0 of hydrogen.

This salt contains one equivalent of water less than the meta-antimoniate of potash, $Sb^2 O^5, KO, 7HO$. This partial loss of water is probably owing to the alcohol employed to precipitate the ammoniacal salt.

The meta-antimoniate of ammonia may be employed to characterise the salts of soda; for it precipitates this base from its solutions with the same facility as the meta-antimoniate of potash.

The author observes, that he has before stated that the antimoniate of potash completely loses its solubility in water by being rendered anhydrous; the meta-antimoniate of ammonia undergoes a similar transformation, becoming also almost insoluble by losing a part of its water; this effect is produced by subjecting it to a very slight increase of temperature.

When this salt is boiled in water, it loses its crystalline appearance and becomes a white powder, which is the insoluble antimoniate of ammonia; in this case no trace of ammonia is given out.

When antimonic or meta-antimonic acid is dissolved with heat in ammonia, it is always the insoluble antimoniate of ammonia which separates on cooling: the crystalline meta-antimoniate of ammonia can be obtained only by the cold digestion of the meta-antimonic acid in ammonia; yet it sometimes happens that a large proportion of the meta-antimoniate changes into amorphous antimoniate of ammonia. This loss of water appears to be produced when too great an excess of ammonia is employed. The meta-antimoniate of ammonia has the curious property of being spontaneously convertible, at common temperatures, into insoluble antimoniate; all the specimens which M. Fremy prepared underwent this change.

Crystalline and perfectly dry meta-antimoniate of ammonia was put into well-corked bottles: after a lapse of some days, the crystals had become slightly moist and farinaceous; they had completely lost their solubility in water, and were converted into antimoniate.

The formula representing this salt is $Sb^2O^5, AzH^3, HO, 4HO$; it differs from the meta-antimoniate of ammonia by containing two equivalents less of water.

The author concludes from his experiments that there exist two antimonic acids, requiring different quantities of base to form neutral salts.

The bi-meta-antimoniates are isomeric with the neutral antimoniates; these two classes of salts are readily distinguished from each other; the first precipitates the salts of soda and the second does not.—*Ann. de Ch. et de Phys.*, Août 1848.

GRESHAM COLLEGE.

"Of Gresham College it may be truly said that no foundation of the present day is based upon more liberal and comprehensive principles. The first of these is, that instruction in different sciences should be given gratuitously to all who wish to receive it: the second, that the professors be chosen with a sole regard to their character and attainments, and without any reference to their attachment to, or dissent from, the established church. The boundaries of science have been largely widened since Gresham's time; but there is nothing in his will to limit the range of his professors, or to prevent any addition to their number. The professor of geometry may embrace the entire subject of practical mechanics, or the professor of physic may lecture on chemistry, botany, or physiology.

"Up to the year 1768, the professors resided and lectured at the spacious mansion of Sir Thomas Gresham in Broad Street. There Briggs, Barrow, Hook, Gunter, Sir William Petty, and Sir Christopher Wren, gave their lectures as professors of the College: there Newton, Boyle, Halley, and other eminent men of science joined them, and formed the Royal Society, which continued to meet for fifty years under its roof. The rents and profits of the Royal Exchange were bequeathed by Gresham for the support of his College, the trustees being the Corporation of London, and the members of the Mercers' Company.

"It will hardly be believed that such an institution, beneficial to all, burdensome to none, should have been destroyed by an act of parliament. But so it was. The means employed to effect this barbarous and nefarious transaction are not known, and can only be conjectured. The result is, that the government of the present day possesses a site in the most valuable part of London, equal in size to that covered by the Bank of England, for about 150*l*. per year. Meanwhile the professors were driven to lecture in a small room in the Royal Exchange. Every motive to exertion was destroyed, since any endeavour to assemble an audience in a room of such scanty dimensions would have been absurd. In such circumstances, the lectures ceased to excite any interest or attention, those for whom they were designed being practically excluded from them.

"In the year 1837 the Exchange was burnt down, and the cost of erecting the new one devolved on the trustees, to whom, as a temporary lecture-hall, was offered the theatre of the City of London School; a room capable of holding 500 persons. It now remained to be seen whether Gresham College was a worn-out institution, and unsuited to the present state of science and of society, or whether it was still able to realize the intentions of its founder.

"The trial exhibited a regular increase in the number of hearers, varying according to the general interest of the subjects, but always sufficient to show that the public attention, and especially that of the citizens of London, was directed to the re-establishment of the College. Several years elapsed before the building of the new Exchange began; and by this time the rebuilding of the College was no longer regarded as a doubtful or uncertain affair. A piece of ground belonging to the Corporation, at the junction of Cateaton (now Gresham) and Basinghall Street, was fixed on for the site, and there Gresham College stands. It was opened on the 2nd of November 1843. Since that time, the number of hearers has gradually increased; having been in Michaelmas term of that year 2451, and in the corresponding term of last year 2940: so that the four terms give an aggregate attendance of from 10,000 to 12,000 persons per annum,

"When Gresham College was razed to the ground by a decree of the legislature, had the ground on which it stood, and by which it was surrounded (reaching from Broad Street to Bishopsgate), been let out on building leases, the income arising from it would now have been nearly 10,000*l.* per annum, instead of the pitiful sum for which it was bartered away. It might have been anticipated that the present government of the country, having professed so much zeal for popular education, would have gladly done an act of tardy justice to an institution especially founded for, and dedicated to, the service of the people, without distinction of rank, sex, or sect—an institution fettered by no obsolete usages, but in active and useful operation, as far as its means allowed. These, at present, are very slender, owing to the heavy debt which the Gresham trust incurred by building the Royal Exchange and the College.

"That the munificent design of its founder has been but partially carried out, is true; but this has arisen from events which he could not foresee. He left, in the Royal Exchange, what he regarded as an ample revenue for his College. And such it was, till its destruction in the great fire of London brought on the trust the heavy charge of rebuilding it: and before this debt was liquidated, the second Exchange shared the fate of the first, and occasioned a renewal of the debt. These were casualties which he did not contemplate; but still less would he have imagined that the government of England would, by an act of the legislature, have compelled his trustees to expend 1800*l.* of the revenues of the College in its destruction, and thus deprive London of his munificent bequest.

"It is, however, satisfactory to reflect that the germ of the institution yet remains; that its advantages, even with its present limited means, are extending; and that Gresham's generous wish of *instruction for all* is, as far as it can be, realized."—Chambers' *Edinburgh Journal*, Sept. 30, 1848.

Views of an entirely opposite kind to those expressed in the above extract, in favour of the liberal and comprehensive character of the Gresham trust, have been put forth in another publication.

In a series of articles in the Mechanics' Magazine (July 22, &c.), commenced on the recent occurrence of a vacancy in the Gresham Professorship of Geometry, and obviously written for the express purpose of urging the *exclusive* claims of one particular candidate, the narrowest possible limitation of the duties of the chair of Geometry is insisted upon, under the pretence that "by Geometry Sir Thomas Gresham *meant* GEOMETRY;" that as there was an old ordinance that the lecturers should "read their lectures in their hoods, according to their degrees in the Universities," therefore, "none but members of the Universities were eligible*"; and then furthermore that "Mr. Potts was the *only man in either University now fairly before the world with any legitimate claims*," &c.

Professor De Morgan's testimonial in favour of the same candidate, is merely *conditional*, and commences with a very important "if," which involves the question of the justice of this limitation. "IF," says he, "the word Geometry be used in the limited sense [the ancient Geometry], then Mr. Potts has very high claims indeed; for he is one of the very few persons who have paid attention to that now rather neglected subject. In fact, *the word being thus used*, there are but two or three persons on whom the choice would properly fall."—A testimonial thus qualified, and resting merely upon great proficiency in Geometry in the narrowest possible sense of the term, may probably have been considered as any thing but a recommendation, such a choice being likely to restrict the lectures in an institution essentially popular to a branch of the subject which engages the attention of "but two or three persons."

That the term Geometry, as connected with the Gresham College, was to be taken in no such narrow sense, appears certainly to have been the opinion of its distinguished ornament Dr. Barrow, who seems especially to have had in view the relations and applications of Geometry to various purposes of science and of art, as Optics, Architecture, Navigation, Fortification, Geodesy, Mechanics, Geography, Astronomy, as well as Logic, and mental culture and delight. The conclusion of his inaugural discourse in Gresham College, 1662, is well deserving the attention of those who desire that these lectures may now be made extensively useful.

"Quinimo institutum hoc nostrum quo lubentius aggrediamur, et persequamur laetius, animos addat timidis, et tardis subdat aculeos, illius cui operam addicemus disciplinae ingens utilitas insignisque praestantia. Neque enim spinosis tenellas mentes subtilitatibus

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* Nothing can be more ridiculous than to assume on such frivolous grounds that the choice is limited to members of the Universities. Woodward, who founded the professorship at Cambridge which bears his name, had already held the office of Gresham Professor of Physic for some years before he entered the University.—He is said to have been apprenticed to a linen-draper.

excruciare, aut futilium tricas argutiarum innectere; non steriles conserere rixas, ludicroque cum larvis certamine velitari; non volaticas insequi chimaeras, evanidasque passim per lubricos calles venari conjecturas: sed manifesta adstruere principia, certas conclusiones elicere, utiles regulas condere, jucundasque nobis incumbet quaestiones expedire. Curae siquidem nostrae fundus committitur excolendus, tot artium foecunda parens, tot scientiarum inconcussa basis, tot in rem humanam commoditatum perennis scaturigo, Geometria; cui scilicet uni aequum est, ut praecipua vitae oblectamenta, praesidia salutis, incrementa fortunae, operaeque nostrae compendia accepta referamus; quod eleganter et commode habitamus; quod tutis ab hostili incursione vallis protegimur; quod per infidos fluctus secura commercia celebramus; quod agrorum pacifice fines dispescimus; quod momenta ponderum aequa lance expendimus, justaque suum cuique mensura dispensamus; quod vastas susque deque, quo volumus, levi digito moles versamus, immanemque rerum perpusilla vi resistentiam profligamus, quod speciosis spectaculis et concinnis ocellos simulachris oblectamus, quodque harmonicis auriculas concentibus delinimus; quod terreni faciem orbis delineamus accurate, remque mundi publicam nostro universam conspectui subjicimus, quod temporis fluxam seriem apte digerimus, et rerum vices agendarum debitis intervallis distinguimus, quod coelestium radiorum in usus nostros subtilem efficaciam derivamus; quod mente demum superos accedimus, imo superos admovemus nobis, nostraque neutiquam a tellure disjuncti per aethereas libere regiones spatiamur, leges ipsis praescribimus inviolabiles caecis, et vagos syderum circuitus certos intra cancellos coercemus. Ut praeteream in palaestra rationem nostram geometrica cum ad valide intorquenda argumentorum tela, tum ad caute declinandos sophismatum ictus; cum ad nervose disserendum, tum ad solide dijudicandum, ad prompte inveniendum, ad recte disponendum, ad dilucide explicandum utilissime exerceri; nec non ad attentae meditationis perferendum taedium, ad alacrem cum objectis difficultatibus conflictum, ad pertinacem in studiis solertiam usu componi mentem, et robore confirmari: instabilem hac et inaequalem phantasiam, quasi saburra, librari; hac fluctuantem anchora figi; hac desultoriam orbita contineri; luxurians hac ingenium, ceu falce, castigari, obtusum ac cote exacui, praefervidum hoc fraeno reprimi, torpidum hoc stimulo excitari; nulla clarius lampade per caecas naturae ambages, nullo certius filo per tortuosos philosophici labyrinthi anfractus vestigia regi incedentis, nec alia demum bolide veritatis fundum facilius explorari. Ne dicam, hinc quam varia rerum cognitione ditetur, quam multiplice perpoliatur ornatu, quam salubri pabulo nutriatur animus, et quam sincera voluptate perfundatur. Nam illius, quae coelos, quae terras, quae maria permetitur, scientiae nulla juste limites describat, nulla plene complectatur utilitates, nulla penitus laudes exhauriat oratio. Et alioquin in materia decantata ab omnibus, et vobis intimius perspecta, improbe stolidus sim, si aut meam frustra abutar operam, aut vestram ulterius violem patientiam. Quanquam haud mediocrem jure meo possim in geometriae auditoribus patientiam exigere, facilemque debeam utcumque a vobis veniam impetrare, si (quod felici cedat augurio) optimum me hodie praestiterim geometram, hoc est pessimum oratorem. Dixi."

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1848.

Chiswick.—September 1. Clear and fine. 2. Overcast and fine. 3. Slight fog: very fine. 4, 5. Slight fog: sultry: clear: rain. 6. Cloudy: clear at night. 7. Overcast. 8. Overcast: fine. 9. Very fine. 10. Cloudy and fine: overcast: rain. 11. Very clear: fine: clear. 12. Fine. 13. Slight fog: fine. 14. Partially overcast: dusky clouds: fine. 15. Clear: fine: clear. 16. Slight fog: very fine: clear. 17. Slight fog: fine: light clouds. 18. Very fine. 19. Foggy: cloudy: clear. 20. Clear: exceedingly fine: clear. 21. Slight fog: very fine, with hot sun. 22. Fine: slight haze: clear. 23. Foggy: heavy rain: clear. 24. Foggy and drizzly: overcast. 25. Fine. 26. Rain. 27. Overcast. 28. Heavy rain. 29. Rain. 30. Cloudy.

Mean temperature of the month .... 56°·34
Mean temperature of Sept. 1847 .... 53 ·40
Mean temperature of Sept. for the last twenty years .... 52 ·80
Average amount of rain in Sept. .... 2·68 inches.

Boston.—Sept. 1—4. Fine. 5. Fine: rain P.M. 6, 7. Fine. 8. Cloudy. 9. Fine. 10. Rain: rain A.M. and P.M. 11—21. Fine. 22. Cloudy. 23. Fine. 24. Rain: rain early A.M.: rain A.M. and P.M.* 25, 26. Cloudy. 27. Rain. 28. Cloudy. 29. Rain. 30. Rain: rain early A.M.

Remarkable rainy days at Boston, Lincolnshire:—

1837.	inches.
August 17	2·34
October 25	1·06
1839.	
June 15	1·28
July 31	0·96
1841.	
June 25	1·08
July 6	1·27
1842.	
January 27	1·08
July 1	1·38
September 24	1·15
1843.	inches.
June 5	1·10
August 10	1·22
November 1	0·95
1844.	
February 26	1·16
1846.	
October 29	1·0
December 22	0·89 melted snow.
1847.	
May 29	1·75
October 24	0·97
1848.	
September 24	2·34

Applegarth Manse, Dumfries-shire.—Sept. 1—4. Fine harvest day. 5. Fair A.M.: rain P.M. 6. Showery: cleared. 7. Fair, but stormy. 8. Showery. 9. Damp, though no rain. 10. Fine A.M.: showers P.M. 11. Beautiful harvest day. 12. Beautiful harvest day: after frosty evening. 13. Beautiful harvest day, though getting cloudy. 14. Beautiful harvest day: fine. 15. Beautiful harvest day: dull. 16. Heavy dew: thunder. 17. Heavy dew: clear and sharp. 18. Fine harvest day. 19. Fine harvest day, but looking dull. 20. Cloudy: rain P.M. 21. Sunny and warm. 22. Still and cloudy: threatening. 23. Fiery wind: sultry. 24. Dull A.M.: rain P.M. 25. Fair, but blustering. 26, 27. Fair: cloudy. 28. Fair and clear. 29. Slight drizzle: cleared. 30. Drizzly day.

Mean temperature of the month .... 53°·1
Mean temperature of Sept. 1847 .... 50 ·9
Mean temperature of Sept. for twenty-five years ...... 53 ·2
Mean rain in Sept. for twenty years .... 3·13 inches.

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

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* Not so great a rain since 17th August 1837.

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.
1848. Sept.	Max.	Min.		9 a.m.	9 p.m.	9¾ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¾ a.m.	8½ p.m.								
1.	30·284	30·140	29·70	30·08	30·10	29·91	29·95	66	42	54	60	39	55½	55	n.	nw.	w.	w.	......	......	......	·04
2.	30·352	30·340	29·81	30·18	30·19	30·03	29·96	70	45	61	61	51	55	55	nw.	calm	w.	w.	......	......	......	·10
3.	30·362	30·254	29·80	30·20	30·12	30·01	29·95	75	46	62	59	51½	56	55	w.	w.	w.	sw.	......	......	......	·20
4.	30·174	29·929	29·64	30·00	29·86	29·85	29·83	80	47	64	66	52	57	50	sw.	calm	sw.	calm	......	......	......	......
☽ 5.	29·835	29·754	29·28	29·64	29·51	29·62	29·50	83	57	68	68½	46	58	56	s.	calm	s.	se.	·10	......	......	......
6.	29·984	29·791	29·20	29·47	29·70	29·30	29·40	68	43	62	63	53½	57	54	sw.	w.	sw.	sw.	·01	·40	......	......
7.	30·055	30·036	29·47	29·70	29·70	29·43	29·62	64	55	61	...	55	54	50	sw.	w.	w.	w.	......	......	......	·40
8.	30·085	29·864	29·43	......	29·50	29·50	29·40	66	49	60	63	...	56	52	sw.	sw.	sw.	calm	......	......	0·20	·11
9.	29·919	29·829	29·38	29·60	29·51	29·30	29·33	67	54	58	57½	47½	56	50	sw.	w.	sw.	w.	......	......	......	·11
10.	29·740	29·594	29·17	29·55	29·69	29·51	29·69	69	43	59	58	47	48	45	sw.	w.	nne.	nw.	·33	·12	......	·06
11.	30·228	29·937	29·44	29·90	30·11	29·93	30·10	60	36	48	56½	38	43	41	nw.	n.	ne.	nw.	......	·76	......	·18
12.	30·312	30·272	29·78	30·20	30·20	30·20	30·18	58	31	49·5	57	32	45	49	ne.	nnw.	nne.	wnw.	......	......	......	·09
○ 13.	30·280	30·210	29·84	30·19	30·09	30·00	29·94	64	37	50	55½	35½	50½	49	n.	n.	w.	w.	......	......	......	·12
14.	30·300	30·219	29·71	30·11	30·18	30·11	30·23	63	37	55	59	47	50	49	nw.	nw.	wnw.	w.	......	......	......	·04
15.	30·409	30·359	29·90	30·25	30·24	30·21	30·15	64	32	51	60½	37½	52½	54	ne.	nnw.	w.	s.	......	......	......	......
16.	30·433	30·377	29·86	30·20	30·19	30·09	29·95	69	34	56	61½	53½	59	55	s.	wsw.	sw.	ssw.	......	......	0·00	......
17.	30·361	30·280	29·80	30·20	30·22	30·10	30·14	71	39	56	62	52	54	51½	nw.	calm	wsw.	wnw.	......	......	......	......
18.	30·260	30·122	29·73	30·20	30·06	30·16	30·01	68	36	55	60	35	54	50	nw.	calm	ne.–s.	sse.	......	......	......	·03
☾ 19.	29·989	29·792	29·55	29·78	29·56	29·68	29·51	68	37	52·5	61	38	53½	54½	sw.	calm	s.–se.	sse.	......	......	......	......
20.	29·768	29·617	29·29	29·52	29·63	29·47	29·60	77	37	57	60½	51½	54½	50½	·	calm	s.	sw.	......	......	......	·04
21.	29·798	29·764	29·32	29·73	29·73	29·78	29·86	75	44	58·5	62½	45	52	52	se.	s.	s.–sse.	se.	·03	......	......	......
22.	29·795	29·767	29·30	29·71	29·78	29·90	29·85	75	52	62	65	49½	55½	54	e.	calm	e.	se.	......	......	......	......
23.	29·664	29·531	29·23	29·63	29·40	29·66	29·53	65	51	62	68½	51	55½	54	sw.	e.	e.	sse.	·22	......	0·20	......
24.	29·341	29·326	28·86	29·33	29·14	29·59	29·74	67	53	59	60½	54	55	54	sw.	calm	ene.	ese.	......	·82	......	......
25.	29·476	29·381	28·99	29·48	29·61	29·82	29·86	69	55	61	58	48½	54½	53	se.	calm	e.	e.	·10	1·52	......	·04
26.	29·650	29·514	29·16	29·61	29·71	29·88	29·94	62	54	59	59	49	55	49	e.	e.	ne.	e.	·24	......	......	......
● 27.	29·702	29·666	29·20	29·75	29·81	29·96	30·05	64	52	57·5	55½	48½	51	49	ne.	ne.	nne.	e.	·01	·06	......	......
28.	29·738	29·723	29·29	29·89	29·90	30·17	30·16	58	54	58	56	49	51	48	ne.	calm	nne.	ne.	·80	......	......	......
29.	29·677	29·669	29·23	29·80	29·70	30·01	29·97	58	51	56·5	54	47½	50	53	n.	calm	nnw.	ese.	·27	·10	......	......
30.	29·790	29·691	29·26	29·68	29·51	29·91	29·73	64	51	58	58	52½	54	51½	sw.	calm	nnw.	ese.	·09	·83	0·10	......
Mean.	29·992	29·891	29·45	29·847	29·750	29·836	29·837	67·56	45·13	57·6	60·2	46·7	53·40	51·10					2·20	4·61	0·50	1·56

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LXIII. *On the Atomic Weight of Molybdenum and some of its Compounds.* By L. SVANBERG *and* H. STRUVE*.

[From the Transactions of the Royal Academy of Sciences of Stockholm for the year 1848.]

Introduction.

FORMERLY all those minerals occurring in nature which possessed a leaden hue and rubbed off were designated by the name of molybdena. Under this name were therefore comprised, not only certain plombiferous minerals, but others widely different, as graphite, the native sulphuret of molybdenum, and manganese ore. With the progress of science these minerals were more carefully examined and distinguished from each other. Cronstedt was the first who separated plumbago from the sulphuret of molybdena as distinct, although closely allied minerals. The views respecting these minerals were however long divided. Many persons, from the similar action of heat upon them, both being volatilized, and their similar outward appearance, asserted them to be of the same nature. It was naturally impossible to decide this question by mere external characters; and it consequently remained doubtful until chemistry made an advance, or in other words, until it became evident that all questions could not be correctly solved by the mere examination in the dry way.

It was reserved for Scheele†, who may justly be termed the founder of chemistry in the humid way, and who showed what conclusions might be drawn from the combinations obtained, to establish, in a memoir which he laid before the Royal Academy of Stockholm in 1778, the differences between graphite and the native sulphuret of molybdenum, by the disco-

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* Translated by Dr. W. Francis from Erdmann and Marchand's *Journal für Praktische Chemie* for August 15, 1848.

† *Kongl. Vet. Akad. Handlingar* för 1778, S. 247.

very of a peculiar metallic calx. This metallic calx, so termed by Scheele according to the then prevailing language in chemistry, was said to occur in and characterise the sulphuret of molybdenum. Scheele prepared the molybdena (*molybdenjerd*) by treating the mineral with nitric acid, and showed that this earth possessed the properties of an acid. It combines with potash to form a salt which separates in small crystals, which expels carbonic acid when treated with carbonate of potash, and yields the neutral salt. In like manner it unites with the volatile alkali, and leaves a gray powder when this compound is heated, the alkali being volatilized. Scheele showed that the acid was volatile, and entered into easily soluble combinations with sulphuric and muriatic acids, and that these solutions readily assumed a blue colour when kept for some time, and also when placed in contact with metals. This colour again disappears upon the addition of some nitric acid, and Scheele thence explained the phænomenon of the blue colour to be a reduction, or as he expressed it, "this is owing to the circumstance that the molybdenum calx readily absorbs phlogiston." Scheele showed that the molybdenum calx, which is obtained by deflagrating the mineral with nitre and precipitation with an acid, behaves differently from the pure acid, and proved that this was owing to the retention of a small quantity of alkali. He finally asserts that the mineral molybdenum consists of a new metallic acid mineralized by sulphur.

This subject engaged the attention of several chemists after Scheele; however, they added very little to our knowledge of the new body, but were principally engaged in confirming the discoveries made by Scheele. Among these Pelletier[*] stands first, in 1785. He could not explain the nature of graphite, which mineral he likewise examined, but he showed that it was perfectly distinct from molybdenum in its properties. Pelletier prepared the calx by roasting the molybdenum ore, and drew from his experiments the conclusion that the mineral was a combination of a metal with sulphur. He was likewise the first who attempted to produce molybdenum in the metallic state, and some alloys of it with copper, iron, silver, &c. Ilsemann[†] worked upon the same subject in 1787, and contradicted the statements of all former chemists respecting the volatility of the molybdic acid and the presence of sulphur in the mineral. This induced Pelletier to return to the subject[‡], and so to substantiate still further his former observations.

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[*] *Observations sur la Physique, sur l'Histoire Naturelle*, &c., par Rozier, Mongez et De la Metherie. Paris, 1785, vol. xxvii. pp. 343, 434.

[†] Crell's *Chemische Annalen*, 1787, vol. i. p. 407.

[‡] *Observations sur la Physique*, &c., 1789, vol. xxxiv. p. 127.

About the same time Heger[*] prepared the combinations of molybdic acid with potash and soda, and stated that a portion of the potash salt might be obtained in small, colourless crystals of the form of six-sided prisms having their terminal edges truncated, but that the greater portion consisted of flattened prisms. He asserted, in opposition to the earlier statements, that a pure acid might be prepared by precipitating the potash salt with nitric acid. Ruprecht[†] made some experiments upon molybdenum in 1790 without adding in the least to our stock of knowledge.

During this period, from 1788 to 1792, Hjelm[‡] published his experiments on molybdenum. His principal object was the reduction of the acid, and the obtaining a fused regulus of metal, and also its alloys with other metals; but at the same time he describes the behaviour of the acid towards several metallic oxides. Hjelm's experiments clearly proved the metallic nature of molybdenum; and it may be said of these experiments, that they were the last which were made with this substance, according to the phlogistic theory, to decide as to the distinctness of the new metal. The ease with which molybdic acid is reduced, the fusion to a button of the metal obtained, the metallic lustre, the specific gravity and brittleness, and the properties of the alloys were the objects of investigation. It will be seen from this how slow the path pointed out by Scheele was generally adopted. Scheele taught us the moist way, to render the metals soluble, and to ascertain and determine from the phænomena then presented the chemical behaviour of bodies.

In 1799 Richter[§] fixed his attention especially upon the blue compound of molybdenum, and showed that it might be produced most easily by the action of tin upon a solution of molybdic acid in muriatic acid. He prepared the compounds of molybdic acid with the oxides of lead and silver, ascertained their sparing solubility in water, and their ready solubility in dilute nitric acid; he moreover determined the saturating capacity of molybdic acid with regard to magnesia and muriatic acid, and so prepared the way for those researches which were subsequently to be carried out quantitatively.

Thus for nearly a quarter of a century several chemists had experimented upon this new metal, and nevertheless at the end of this time little more was known of it than what its discoverer

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[*] Crell's *Chemische Annalen*, 1787, vol. ii. pp. 21, 124.

[†] Ibid. 1790, vol. i. p. 483; vol. ii. p. 3.

[‡] *Kongl. Vet. Akad. Handl.* för 1788, sid 280; 1789, sid 131, 241; 1790, sid 50, 81; 1791, sid 65, 213; 1792, sid 115.

[§] *Ueber die neuen Gegenstände der Chemie*, I. Stück, p. 61; II. p. 97; X. p. 86.

had described. An important contribution to a more accurate knowledge of this metal was now published by Buchholz*. He directs the same method for the preparation of the molybdic acid as that employed by Hjelm, without, apparently being aware that it had been described by him. He likewise states that the acid may be obtained pure and free from alkali when the soda salt is treated with nitric acid. Two years later Buchholz† published a more complete investigation of molybdenum, in which his attention was principally drawn to its several oxides. He admits six oxides, but is not able to give good methods for preparing them pure. He also analysed molybdic acid and the native sulphuret with tolerable accuracy. He likewise describes a higher sulphuret than the native mineral, but has no correct idea of the peculiar nature of this compound; he was acquainted with its solubility in caustic alkalies and in alkaline sulphurets. We see in him how a man of science opens a new path in experimenting in order to explain all the phænomena, even to the minutest details.

The metallic nature of molybdenum was now ascertained beyond all doubt; but there was wanting the man who should lead us out of the labyrinth of white, blue, gray, yellow, brown and red compounds which Buchholz had produced under different circumstances. The exploring of this labyrinth was connected with the theory which, under the name of the theory of definite chemical proportions, will hand down the name of Berzelius to a late posterity. By experiments which Berzelius‡ made in the year 1818, he showed that molybdenum required a certain quantity of oxygen in order to form molybdic acid; and by the analysis of the neutral molybdate of lead, a subject to which we shall return in the third section of our memoir, he ascertained the number representing the atomic weight of molybdenum, assuming 100 as the atomic weight of oxygen.

After this was fixed, Brandes§ published his analyses of some molybdates. He showed that molybdic acid forms with ammonia two distinct crystalline salts, although he gives merely the analysis of one. He also analysed the neutral soda salt, and prepared several insoluble salts by double decomposition.

In 1825 Berzelius‖ published an investigation of molyb-

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* Scherer's *Journal der Chem.*, 1802, vol. ix. p. 485.

† Gehlen's *Allgemeines Journal der Chem.*, 1805, vol. iv. p. 598.

‡ *Afhandlingar in Fysik, Kemi och Mineralogie,* 5te delen. Stockholm, 1818, sid 475.

§ Schweigger's *Journal der Chem. und Phys.*, 1820, vol. xxix. p. 325.

‖ *Kongl. Vet. Akad. Handl.* för 1825, sid 145.

denum, in which he showed that not more than three oxides of molybdenum should be admitted, which however, by combining with one another, were capable of producing several intermediate combinations. These combinations, which have very peculiar and distinct characters, had confused those chemists who had previously experimented with this metal. The method of preparing these oxides, their characteristic combinations with acids, formed the chief object of the memoir. Soon after Berzelius* examined the behaviour of molybdenum towards sulphur, and that of the sulphuret of molybdenum towards electro-positive sulphurets, when he discovered and accurately examined all that we have hitherto known on this subject.

Part I.

Behaviour of Molybdic Acid to Bases.

After this historical introduction, we will now pass on to our experiments, the principal object of which was the most accurate determination possible of the atomic weight of molybdenum. In the present state of chemistry, neither pains nor time should be spared to determine the atomic weights of the simple bodies as accurately as possible, for it is connected with the important question, what elements have such numbers for equivalents as may be expressed by multiples of the equivalent of hydrogen, and how far can this be admitted as a general law. Now if we find, in fact, that the atomic weights of several simple bodies are multiples of that of hydrogen, this must some day lead to a number of other conclusions, the vast importance of which to the whole science of chemistry is not now apparent. If, on the other hand, experiments have indicated deviations from this law, these also lead to important considerations.

But in order to determine the atomic weight of a body, it is first requisite to become acquainted with its properties and some of its combinations. We shall therefore, to avoid frequent repetition, first describe those combinations of molybdenum which we have examined.

The following atomic weights for the metal have been used in the theoretical calculations:—

$$\text{Mo} = 588{\cdot}966 \ldots . (a.) \qquad \text{Mo} = 575{\cdot}829 \ldots . (b.)$$

These numbers will be justified in the third part of this paper. For the other bodies, we have employed the atomic weights given by Berzelius in his Manual of Chemistry, vol. iii. Leipzig, 1845.

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* *Kongl. Vet. Akad. Handl.* för 1825, p. 300.

We employed in these researches a sulphuret of molybdenum from Lindås in Småland, in Sweden, which was very pure, and of which an abundant stock was at our disposal. This mineral, which was mixed with but small portions of gangue, was reduced to a fine powder, and then roasted on a shallow earthenware dish over a charcoal fire, constantly stirring it. From 100 to 200 grammes of the mineral were roasted at a time, and the operation lasted a couple of hours, when so much molybdic acid had formed, that the entire mass, while hot, was throughout yellow and adhered to the dish. When it had cooled the mass was treated in a closed flask with ammonia, which took up all the molybdic acid and left a considerable portion of the mineral and other impurities undissolved. These operations had to be repeated several times with the insoluble portion. The solution of the molybdic acid in ammonia was evaporated to a small bulk, and the following process adopted to obtain a perfectly pure acid; the solution contains besides molybdic acid, minute quantities of alumina and oxide of copper, and what is more inconvenient, phosphoric acid. To remove these impurities, the ammoniacal solution of the molybdic acid is mixed with an excess of carbonate of potash and evaporated; this separates a portion of the alumina, which is removed by filtration. The clear solution is evaporated to dryness and heated to redness in a platinum crucible. The ignited mass is treated with water, by which a solution of molybdate, carbonate with some sulphate and phosphate of potash is obtained, while alumina and oxide of copper remain undissolved. The filtered solution of the potash salts is again evaporated to dryness, the dry mass mixed with twice its weight of sulphur, and the whole heated to redness either in a porcelain crucible or in a glass flask, the mouth of which can be easily closed with a glass stopper. We used a glass flask, placed it in a Hessian crucible, and surrounded it with sand: the temperature is gradually increased and continued until no more sulphur burns away, when the entire mass is at a low red heat. After cooling, the whole is treated with hot water, which separates it into a blackish green mass and a red solution. It is filtered as hot as possible, pouring the solution only upon the filter and adding more hot water to the residue. When this water is no longer coloured yellow, a few drops of carbonate of potash are added, and the whole heated with constant agitation until it boils. This treatment with water, carbonate of potash, and filtration, is continued until a little of the filtered liquid, on being mixed with muriatic acid, neither deposits sulphur nor smells of sulphuretted hydrogen. By this means all the phosphoric acid and excess of sulphur are removed from the sulphuret of molybdenum, which gradually acquires a pure black colour, and when pressed with a glass rod immediately exhibits the metallic lustre peculiar to the sulphuret. The sulphuret is now treated with water containing muriatic acid, and washed upon the filter with hot water. This sulphuret of molybdenum is pure; and to prepare molybdic acid from it, it may either be roasted or treated with nitric acid; the oxidation by nitric acid is readily effected, as the sulphuret is in a very finely divided state.

In most cases this circuitous process is not requisite, especially when the object is to prepare combinations with other bases; a more simple plan may then be adopted, as will be stated under the different compounds.

Molybdic acid is easily tested as to its freedom from fixed impurities; a portion exposed to a strong red heat in a crucible with free access of air, should be entirely volatilized.

Salts of Potash.

As stated in the introduction, the potash salts have in part been briefly described by various chemists, but many of them have only been used for the preparation of some of the insoluble compounds. All the statements respecting them are very imperfect, and founded upon mere test experiments without any analysis. The following salts have been examined by us.

Neutral Molybdate of Potash.—The formula for the salt dried over sulphuric acid is $KaO, MoO^3 + \frac{1}{2}HO$. We have prepared this salt in different ways.

A. Either of the ammonia salts is treated with an excess of carbonate of potash, the solution evaporated by heat to the consistence of a syrup, when the salt gradually separates in the cold. The salt however cannot be thus obtained free from carbonate of potash, as will be shown by the analysis.

B. Caustic potash is dissolved in alcohol of 0·815 spec. grav. and small portions of moist trimolybdate of potash added, and the whole shaken in a well-corked bottle. The neutral salt separates as an oily mass at the bottom of the bottle; the supernatant alcoholic solution of potash is removed with a pipette, the oily mass washed with alcohol, then poured into a dish and left to crystallize over caustic lime and sulphuric acid.

The neutral molybdate of potash crystallizes in four-sided prisms having two truncations on the smaller lateral faces of the prism. It is readily soluble in water after ignition. When heated it parts with its water and falls to a white powder; it requires the highest temperature which can be produced with an Argand lamp to fuse it; it then flows readily, but solidifies into a crystalline mass as soon as the temperature is a little lowered. When it has become cold it falls to a white powder. This peculiarity is very striking, and merits every attention. The salt deliquesces when exposed for any length of time to the air, greedily absorbs carbonic acid, and gradually passes into other salts. Although insoluble in alcohol, it is only separated in the form of an oil by alcohol from the most concentrated solution in water.

Analysis.—I. Salt obtained according to the method A. 0·2179 grm. gave after ignition 0·1825 sulphate of potash, or 0·0986 potash.

II. Salt prepared according to the method B. 1·500 grm. lost on ignition 0·043 and yielded 0·844 sulphate of potash = 0·456 potash.

III. The same salt as in II. 0·7793 grm. anhydrous salt gave 0·587 sulphate of potash = 0·317 potash.

According to analysis II. the crystalline salt contains in 100 parts—

Potash	39·663
Molybdic acid . . .	56·598
Water	3·739

100 parts of the anhydrous salt contain—

	I.*	II.	III.
Potash . . .	45·363	41·203	40·707
Molybdic acid	54·637	58·797	59·293

The calculated theoretical composition of the crystalline salt is—

	Atomic weight, *a*†.	Calculated.	Atomic weight, *b*.	Calculated.
1 at. Potash . . .	588·856	38·385	588·856	38·717
1 at. Molybdic acid	888·966	57·949	875·829	57·585
½ at. Water . . .	56·239	3·666	56·239	3·698
	1534·061	100·000	1520·924	100·000

The theoretical composition of the anhydrous salt is—

	a.	Calculated.	*b*.	Calculated.
1 at. Potash . . .	588·856	39·840	588·856	40·204
1 at. Molybdic acid	888·966	60·160	875·829	59·796
	1477·822	100·000	1464·685	100·000

It will be seen on comparing the found quantities of potash

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* The excess of 5½ per cent. in this analysis is partly owing to the salt having crystallized from a strongly alkaline solution, and partly to the molybdic acid not having been entirely freed from every trace of phosphoric acid and alumina.

† Throughout this paper we intend, by the designation atomic weight, *a*, that the atomic weight of molybdenum is 588·966, and by *b*, the atomic weight 575·829.

with those calculated, that we have in each case obtained an excess of potash. We nevertheless believe that this salt is the neutral molybdate of potash, but the reason why no perfectly pure product can be obtained is the mode of preparation of this salt from a very alkaline liquid.

In these analyses, and in all the subsequent ones, the potash was determined according to the following method:—After the salt had been heated to redness in a platinum crucible to determine the amount of water of crystallization, it was dissolved in water; but if the salt after ignition was insoluble or sparingly soluble in water, we added to it a few drops of ammonia. The solution was then mixed with an excess of hydrosulphate of ammonia and afterwards with muriatic acid, which instantly precipitates tersulphuret of molybdenum, which, according to the concentration of the liquid, had sometimes a brown, and at other times a black colour. If care had not been taken to add a sufficient quantity of hydrosulphate of ammonia at the commencement, and the whole of the molybdenum has not been precipitated (which is indicated by the colour of the solution), this fault can only be made good by redissolving the whole in ammonia and hydrosulphate of ammonia, and again precipitating it. The precipitated sulphuret of molybdenum, which very quickly subsides, is collected upon a filter and washed with water containing sulphuretted hydrogen and muriatic acid. The filtrate is then evaporated in a counterpoised platinum crucible, the excess of chloride of ammonium driven off at a gentle heat, the residue dissolved in water and sulphuric acid, again evaporated, and the potash determined with the usual precautions as neutral sulphate of potash. In most of the analyses an excess of potash will be found, which is owing to the readiness with which the sulphuret of molybdenum becomes oxidized.

Bimolybdate of Potash.—We did not succeed in preparing this salt, for on fusing together a weighed quantity of trimolybdate of potash with the requisite quantity of carbonate of potash to produce the bimolybdate, we obtained after ignition, the carbonic acid being expelled with effervescence, a white mass which solidified in a crystalline state on cooling, and on pouring water over the fused mass a small quantity dissolved at first, but in a few minutes the liquid grew turbid, and the whole salt was gradually decomposed into the neutral and the tribasic molybdate of potash. We do not mean however, by this experiment, to deny the existence of this salt, for it is highly probable that it will be more stable when produced in liquids of a certain degree of concentration, or in which other salts are contained. This view is moreover confirmed by the fact, that we have succeeded in preparing the bimolybdate of soda.

Double salt of the Bimolybdate of Potash and Trimolybdate of Potash.—The formula of the crystallized salt is $4KaO, 9MoO^3 + 6HO$, or $3KaO, 2MoO^3 + 3KaO, MoO^3 + 6HO$, or $3KaO, MoO^3 + 5KaO, 3MoO^3 + 12HO$.

When concentrated nitric or muriatic acid is added with constant agitation to a solution of molybdic acid in carbonate of potash, on the addition of each drop a precipitate results, which however soon redissolves. The addition of acid is continued until the precipitate no longer entirely dissolves, and the liquid is slightly opalescent; it is then set aside, when this salt gradually crystallizes; but if the solution is very concentrated it is precipitated in a few minutes. The separation of this salt from concentrated solutions occurs even though the liquid is strongly alkaline. The salt is collected on a filter, not washed with water, but as soon as the mother-ley is drained off dried as quickly as possible between folds of bibulous paper.

On slow crystallization it separates in six-sided prisms, four faces of which are very predominant, with truncations upon the two narrow faces. When the salt separates rapidly, it appears under the microscope in the form of rhombohedrons, which mutually intercept each other in the centre, and so form minute stars. The salt contains water of crystallization, which it loses when heated. It melts at a red heat, and solidifies into a crystalline mass on cooling, which dissolves with difficulty in water. It is soluble in cold water, but is very readily decomposed, so that it can only exist in the perfectly dry state, or in those liquids from which it separated. It is decomposed by water into the trimolybdate of potash and the neutral molybdate of potash. We shall return again to this phænomenon.

Analysis.—I. 1·5385 grm. salt obtained by precipitating with nitric acid, lost 0·0885 water on ignition, and gave 0·6083 sulphate of potash, or 0·328 potash.

II. 1·1175 grm. salt of the same preparation gave 0·0635 water and 0·4422 sulphate of potash = 0·2390 potash.

III. 0·949 grm. salt of a different preparation with nitric acid gave 0·058 water and 0·3655 sulphate of potash = 0·1975 potash.

IV. The salt for this analysis was obtained by evaporating a solution of molybdic acid in carbonate of potash with a large excess of alkali. A portion of the salt separated from the hot solution, the other portion on cooling, when the entire mass solidified. On treatment with water this salt was left as a white crystalline powder very sparingly soluble in water; it was dried in the air; 1·0973 grm. afforded 0·0408 water and 0·4474 sulphate of potash = 0·2418 potash. We have also given the per-centage composition of this salt; we leave it however to other chemists to decide in how far this salt is distinct from the others, especially in respect to the amount of water. It appears to contain only 3 atoms of water.

The per-centage composition of the crystallized salt is, according to analysis,—

	I.	II.	III.	Mean.	IV.
Potash . . .	21·267	21·385	20·814	21·188	22·035
Molybdic acid	72·881	72·932	73·074	72·962	74·247
Water . . .	5·752	5·683	6·114	5·849	3·718

The anhydrous salt contains—

	I.	II.	III.	IV.	Mean.
Potash	22·672	22·673	22·169	22·886	22·600

The theoretical composition of the crystallized salt is—

	a.	Calculated.	*b.*	Calculated.
4 at. Potash . . .	2355·424	21·353	2355·424	21·584
9 at. Molybdic acid	8000·694	72·529	7882·461	72·232
6 at. Water . . .	674·874	6·118	674·874	6·184
	11030·992	100·000	10912·759	100·000

and of the anhydrous salt—

4 at. Potash . . .	2355·424	22·744	2355·424	23·007
9 at. Molybdic acid	8000·694	77·256	7882·461	76·993
	10356·118	100·000	10237·885	100·000

Trimolybdate of Potash.—Formula of the salt dried in the air $KaO, 3MoO^3 + 3HO$. This salt can be very readily prepared in large quantities and in the purest state. Indeed, the combinations of molybdic acid with potash exhibit a great tendency to pass into this salt. It is prepared by adding water to the preceding double salt, and stirring frequently; complete decomposition then takes place, and the trimolybdate separates, being very sparingly soluble in water. This decomposition requires some days, but it is only requisite to warm the liquid to hasten it. When large quantities of the double salt are used, the liquid thickens to such an extent on the separation of the trimolybdate of potash, that the whole becomes a thick paste. When the precipitate no longer increases, the entire mass is thrown upon a filter and washed carefully with water. The pure salt is best dried at first in the air and then at a gentle heat, for if exposed while moist to a temperature of 212° a portion dissolves in hot water. The precipitate, which is very bulky in the moist state, shrinks considerably in drying. This salt is likewise always obtained when an excess of nitric acid is added to a solution of molybdic acid in carbonate of potash, and the whole set aside for some time. Besides the trimolybdate of potash some other molybdates separate, which are easily recognized by their crystalline form, and also some salts which are not crystalline, and which we shall subsequently describe. As soon as the separation has taken place, the whole is brought upon a filter and washed well with cold water; this dissolves the other crystalline salts, a portion of which is even converted into trimolybdate. The precipitate is then treated with boiling water, which dissolves the trimolybdate, and the solution filters. The salt subsequently separates gradually from the solution.

When this salt has separated quickly it forms a bulky white precipitate, which, under the compound microscope, is seen to be made up of innumerable white needles. When however the separation has been very slow, it is acicular and with a beautiful silky lustre, especially when stirred under water with a glass rod; the salt retains this lustre when dry. When once separated it is almost insoluble in cold water, but is dissolved in large quantity by boiling water, and from this solution it separates very slowly, sometimes requiring several weeks. In liquids containing other salts, as for instance nitre, it is almost insoluble even when boiling. It contains water of crystallization, which it parts with on calcination, then melts, and solidifies on cooling into a crystalline mass. It is then very sparingly soluble even in boiling water.

Analysis.—I. 0·6282 grm. of the fused salt obtained by decomposition of the double salt and dried in the air gave 0·219 sulphate of potash = 0·1189 potash.

II. 1·0479 grm. salt of the same preparation lost on calcination 0·1009 water.

III. 0·7255 grm. of the same preparation lost on calcination 0·0709 water.

IV. 2·6185 grms. fused salt gave 0·8810 sulphate of potash, or 0·4761 potash.

V. 1·408 grm. salt precipitated by nitric acid and dried in the air lost at a red heat 0·632 water, and gave 0·4395 sulphate of potash = 0·2375 potash.

The per-centage composition of the air-dried salt is accordingly—

	II.	III.	V.	Mean.
Potash . . .	...	...	16·869	16·869
Molybdic acid	...	...	73·756	73·756
Water . . .	9·628	9·773	9·375	9·592

and of the anhydrous salt—

	I.	IV.	V.	Mean.
Potash . .	18·840	18·183	18·614	18·546
Molybdic acid	81·160	81·817	81·386	81·454

The composition of the air-dried salt, according to theory, is—

	a.	Calculated.	*b.*	Calculated.
1 at. Potash . . .	588·856	16·388	588·856	16·570
3 at. Molybdic acid	2666·898	74·221	2627·487	73·935
3 at. Water . . .	337·437	9·391	337·437	9·495
	8593·191	100·000	3553·780	100·000

and of the anhydrous salt—

1 at. Potash . . .	588·856	18·087	588·856	18·308
3 at. Molybdic acid	2666·898	81·913	2627·487	81·692
	3255·754	100·000	3216·343	100·000

As above stated, this salt is obtained by the decomposition of the previously-described double salt; and as the trimolybdate separates in an almost insoluble state, we made the following quantitative experiment.

5·7178 grms. of the crystalline double salt were dissolved in cold water and set aside in a covered vessel; after two days the first flakes began to separate, and the decomposition proceeded slowly. After twelve days the trimolybdate of potash which had separated was collected upon a weighed filter, and after being dried in the air, weighed 4·6087 grms. The filtered liquid was evaporated in the water-bath to dryness; on re-solution in a little water it left 0·0053 grm. trimolybdate. The filtered liquid was neutral, but absorbed carbonic acid from the air, acquired an alkaline reaction, and gradually deposited still more trimolybdate.

100 parts of the crystalline double salt give, according to this experiment, 80·695 parts trimolybdate of potash; the decomposition then takes place according to the following formula:—

$$\left.\begin{array}{l}\text{2 at. } (4\mathrm{KaO},\ 9\mathrm{MoO}^3+6\mathrm{HO})\\ \quad\text{are decomposed into . .}\end{array}\right\}\left\{\begin{array}{l}5(\mathrm{KaO},\ 3\mathrm{MoO}^3+3\mathrm{HO})\\ 3(\mathrm{KaO},\ \mathrm{MoO}^3+x\mathrm{HO})\end{array}\right.$$

According to this formula 100 parts of the double salt should yield 81·438 parts trimolybdate, and this result agrees as accurately as could be expected with the experiment.

When trimolybdate of potash is exposed to a current of hydrogen, there is no reaction until the salt is fused; reduction then occurs, water is formed, and the fused mass gradually becomes solid, acquiring a shining brown colour.

0·5525 grm. fused and pulverized trimolybdate of potash gave after reduction 0·5195 grm. When the reduction was complete, which was indicated by the weight remaining constant, the residue was exhausted with water, which dissolved a portion, forming a colourless liquid; the insoluble portion was collected on a filter. To the naked eye some portions of the residue present a strong lustre and a tomback-brown colour; this is still more distinct under the microscope. The filtered solution was evaporated in a counterpoised crucible, ignited and weighed. The salt forms a white powder, behaves like the neutral molybdate of potash, and weighed 0·2496.

100 parts of the trimolybdate of potash consequently lose on reduction—

5·973 oxygen, and yield
45·176 neutral molybdate of potash and
48·851 insoluble oxide (estimated from the loss).

If we compare this with theory, we find that

$$\begin{array}{r}\text{1 at. KaO, 3MoO}^3 + \text{2 at. HO is} \\ \text{decomposed into}\end{array}\left\{\begin{array}{l}\text{1 at. KaO, MoO}^3 \\ \text{1 at. MoO, MoO}^3 \\ \text{9 at. HO,}\end{array}\right.$$

which gives in 100 parts—

	a.	*b.*
Oxygen	6·143 . .	6·218
KaO, MoO^3	45·391 . .	45·539
MoO, MoO^3	48·466 . .	48·243
	100·000	100·000

When the insoluble oxide which has been freed by water from the neutral molybdate of potash is heated with a solution of caustic potash, a large quantity of molybdic acid dissolves. The same occurred on boiling it with muriatic acid; however, it was far less violently acted upon by the latter. These experiments indicate that molybdic acid exists in the insoluble oxide, and we have on that account admitted the formula MoO, MoO^3, and not the formula $2MoO^2$, both which formulæ express the same relation between the oxygen and molybdenum; it is however possible that by the action of the potash, the insoluble residue may have passed into molybdic acid, either by re-arrangement of the elements, or from having absorbed oxygen from the air. Nitric acid readily converts it into molybdic acid.

By the careful addition of nitric acid to a solution of molybdic acid in carbonate of potash, the previously-described double salt is precipitated; on using a large excess of nitric acid, however, the phænomena are totally different. In this case a very bulky precipitate is instantly produced, which gradually increases. But in this precipitation different crystalline and amorphous substances can be distinguished, as was stated when treating of the trimolybdate of potash. All the crystalline salts may be extracted with hot water, which leaves an amorphous residue, that may sometimes be collected on a filter and washed, but at other times it passes through the pores of the filter. It is likewise a chemical combination of molybdic acid with potash. It appears however that a tetramolybdate or pentamolybdate of potash is precipitated according to the quantity of nitric acid. It is not a mixture of molybdic acid with some molybdic acid containing potash, for when heated in a platinum crucible it fuses, but not a trace of molybdic acid is volatilized even with the strongest heat. We have analysed two such precipitates.

Tetramolybdate of Potash, KaO, $4MoO^3$, forms a white crystalline powder, which is anhydrous, insoluble in water, readily fusible, and crystallizes on cooling.

Analysis.—1·612 grm. salt gave 0·4162 sulphate of potash = 0·2249 potash.

On comparing the numbers found with those resulting from theory, we have—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Potash..............	588·856	14·207	588·856	14·39	13·953
4 at. Molybdic acid ...	3555·864	85·793	3503·316	85·61	86·047
	4144·720	100·000	4092·172	100·00	100·000

Pentamolybdate of Potash, KaO, $5MoO^3$, also forms a white powder, but of a much finer grain, and very readily passes through the filter. It is anhydrous, fuses and solidifies into a crystalline mass on cooling.

Analysis.—2·1815 grms. fused salt gave 0·5033 sulphate of potash = 0·274 potash.

Theory and analysis give the following per-centage composition:—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Potash..............	588·856	11·698	588·856	11·853	12·468
5 at. Molybdic acid ...	4444·830	88·302	4379·145	88·147	87·532
	5033·686	100·000	4968·001	100·000	100·000

The series of combinations of potash and molybdic acid is certainly not closed with the salts here described. In the course of this investigation we have observed several other salts differing in their crystalline form, but did not pursue the subject further, as it was beyond the object of this paper; we will however mention three other salts, in order that should any person take up the examination of the potash salts he may direct his especial attention to these. One salt crystallizes in small six-sided prisms, which when stirred under water exhibit a strong silvery lustre. It is readily soluble in water.

Another salt crystallizes in beautiful acute rhombohedrons, which have an adamantine lustre, are very sparingly soluble in cold, more readily soluble in hot water; on evaporating its solution it is converted for the greater part into the trimolybdate. It fuses at a red heat, and solidifies on cooling with a steel-gray colour and a strong lustre.

A third salt sometimes separates when the mother-ley obtained after precipitating with nitric acid is somewhat warmed; it forms shining four-sided prisms.

Soda Salts.

Neutral Molybdate of Soda.—Formula of the crystallized salt NaO, $MoO^3 + 2HO$.

To prepare this salt 5·491 grms. MoO^3 were fused with 4·074 grms. anhydrous carbonate of soda, being exactly the proportion required for the production of the neutral salt. On cautiously raising the temperature, mutual reaction takes place with violent evolution of carbonic acid, and at a red heat the whole mass forms a perfect liquid; the crucible was placed over sulphuric acid to cool, when the whole solidified to a crystalline mass. It dissolved easily and entirely in water, and on evaporating the solution by heat, the greater portion of the salt separated in a crystalline state. The crystals were collected on a filter, washed with very little water, dried as quickly as possible between folds of paper, and then left for twenty-four hours over caustic lime and sulphuric acid. They form small rhombohedrons with very acute angles, fuse readily, giving off their water of crystallization, and solidify on cooling to a crystalline mass. The salt is readily soluble in water.

Analysis.—1·280 grm. salt gave 0·1927 water and 0·7542 sulphate of soda = 0·3301 soda.

	a.	Calc.	*b.*	Calc.	Found.
1 at. Soda	389·729	25·919	389·729	26·147	25·788
1 at. Molybdic acid ..	888·966	59·120	875·829	58·760	59·207
2 at. Water	224·958	14·961	224·958	15·093	15·005
	1503·653	100·000	1490·516	100·000	100·000

and in the anhydrous salt—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Soda	389·279	30·479	389·729	30·795	30·358
1 at. Molybdic acid ..	888·966	69·521	875·829	69·205	69·642
	1278·965	100·000	1265·558	100·000	100·000

Bimolybdate of Soda.—Formula of the crystallized salt NaO, $2MoO^3 + HO$. To prepare this salt 2·4325 grms. molybdic acid were fused with 0·919 grm. of dry carbonate of soda, which is exactly requisite to form the salt; the mixture fused and solidified on cooling into a white crystalline mass, which, on being pressed or agitated under water, fell into a number of acicular crystals. The saline mass dissolved with great difficulty in cold water, and only after a long time in hot. The liquid was evaporated to a very small bulk, but no crystals separated either by heat, nor after standing for three days. The concentrated solution was further evaporated in a platinum crucible in the water-bath, when gradually a white crystalline mass separated, whilst another portion was left as a syrupy liquid. The crystals were collected on a filter and washed with a little water, when the greater portion of the salt redissolved; it was then dried as quickly as possible by pressure between folds of paper, and finally at a temperature of 176° F.

The salt appeared to consist, under the microscope, of four-sided prisms; it dissolves readily in water, but with great difficulty after fusion. When heated, it parts with its water of crystallization, melts and solidifies on cooling to a yellowish white crystalline mass.

Analysis.—0·4473 grm. salt lost on ignition 0·0203 water, and gave 0·1819 sulphate of soda = 0·0796 soda.

The crystalline salt consists therefore in 100 parts of—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Soda …………	389·729	17·092	389·729	17·292	17·798
2 at. Molybdic acid	1777·932	77·975	1751·658	77·718	77·552
1 at. Water…………	112·479	4·933	112·478	4·990	4·650
	2280·140	100·000	2253·865	100·000	100·000

and the anhydrous salt of—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Soda …………	389·729	17·979	389·729	18·20	18·666
2 at. Molybdic acid	1777·932	82·021	1751·658	81·80	81·334
	2167·661	100·000	2141·387	100·00	100·000

Trimolybdate of Soda.—Formula of the crystalline salt $NaO, MoO^3 + 7HO$. When nitric acid is added gradually to a concentrated solution of molybdic acid in carbonate of soda, no double salt separates, as is the case with the potash salt. When so much acid is added that the liquid exhibits an acid reaction, trimolybdate of soda separates after some time as a bulky precipitate. It crystallizes like the corresponding salt of potash, but is more soluble in water. The salt dried in the air contains more water of crystallization than the potash salt, which it parts with on being heated to redness; it melts and solidifies on cooling into a crystalline mass.

Analysis.—I. 0·618 grm. salt lost at a red heat 0·1253 water and gave 0·1490 sulphate of soda = 0·065 soda.

II. 0·8467 lost on ignition 0·1725 water.

The crystalline salt is accordingly composed of—

					Found.	
	a.	Calc.	*b.*	Calc.	I.	II.
1 at. Soda	389·729	10·139	389·722	10·244	10·552	
3 at. Molybdic acid	2666·966	69·378	2627·487	69·061	69·173	
7 at. Water	787·353	20·483	787·353	20·695	20·275	20·350
	3844·048	100·000	3804·569	100·000	100·000	

and the anhydrous salt of—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Soda	389·729	12·747	389·729	12·917	13·235
3 at. Molybdic acid	2666·966	87·253	2627·487	87·083	86·765
	3056·695	100·000	3017·216	100·000	100·000

Reduction of the Trimolybdate of Soda by Hydrogen.—Wöhler first observed and described the peculiar behaviour of the bitungstate of soda towards hydrogen, it became therefore interesting to ascertain how this salt of molybdic acid would behave under the same treatment. With the tungstate, a curious compound of oxide of tungstenium and soda was formed by the reduction; no such thing however happens in this case. The molybdate of soda behaves precisely like the corresponding potash salt.

0·6534 grm. of fused trimolybdate of soda lost on reduction 0·043 grm., and the residue yielded on exhaustion with water, evaporation, ignition and weighing, 0·266 grm. neutral molybdate of soda.

100 parts of the anhydrous trimolybdate of soda consequently afford—

6·581 oxygen
40·710 neutral molybdate of soda, and
52·709 insoluble oxide (estimated by the loss).

On comparing these values with theory, we find that

$$\text{1 at. } NaO,\ 3MoO^3 + 2HO \text{ is decomposed into} \left\{\begin{array}{l} \text{1 at. } NaO,\ MoO^3 \\ \text{1 at. } MoO,\ MoO^3\text{, and} \\ \text{2 at. } HO. \end{array}\right.$$

which gives in 100 parts—

	a.	*b.*
Oxygen . .	6·565	6·625
NaO, MoO^3	41·823	41·393
MoO, MoO^3	51·612	51·981
	100·000	100·000

The amount of neutral molybdate of soda obtained does not agree accurately with the quantity required by theory, but this is owing to the glass being very much acted upon by the neutral salt in the operation. In this experiment the reduction was continued for some hours uninterruptedly, the entire mass being kept at a red heat in order to ascertain whether the action might not gradually proceed further; this however was by no means the case, as the results obtained show.

We found on treating a soluble molybdate of potash with an excess of concentrated nitric acid in the cold, that salts instantly separated with a greater amount of molybdic acid; this is not the case with the soda salt. When an excess of nitric acid is added to a cold solution of molybdate of soda, no precipitate results even after several days, but on heating the liquid it instantly becomes turbid, and gradually deposits a bulky yellow precipitate. On collecting this upon the filter, it easily passes through the paper on being washed with water, which may however be prevented by adding a few drops of nitric acid to the water. The washing is very slowly and imperfectly effected, as the precipitate adheres closely to the filter. When dry it appears amorphous and yellow, except at the edges and on the surface, where the colour is bluish-green, produced by the escape of the nitric acid in drying. When a portion of the precipitate is heated in a platinum crucible, the greater part is volatilized at a strong red heat. This had been observed by Buchholz, and may in some cases serve for the preparation of pure molybdic acid.

Salts of the Oxide of Ammonium.

The series of these salts is quite as numerous as that of the salts of potash. We have not examined these combinations so accurately, as in all the analyses the amount of molybdic acid only has been determined by carefully heating the salt in a counterpoised platina crucible, and in no instance that of the ammonia or of the water; by these determinations, and partly by the preparation of the corresponding salts of baryta, we are enabled to establish the following definite compounds.

Neutral Molybdate of the Oxide of Ammonium, NH^4O, MoO^3.—This salt was prepared by dissolving molybdic acid in an excess of highly concentrated solution of ammonia in a stoppered vessel, and precipitating with alcohol of 0·942 spec. grav. The salt so precipitated is collected as quickly as possible upon a filter and dried over caustic lime.

When seen under the microscope, it has the form of small four-sided prisms with two truncations. When a little of this salt moistened with water is placed under the microscope, it is quickly seen to change into the acid salt. It is anhydrous.

Analysis.—0·8095 grm. salt gave 0·594 molybdic acid; the per-centage composition of the salt is therefore—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Oxide of ammonium	324·98	26·771	324·98	27·063	
1 at. Molybdic acid	888·966	73·229	875·829	72·937	73·379
	1213·946	100·000	1200·809	100·000	

Bimolybdate of the Oxide of Ammonium, $NH^4O, 2MoO^3$.—When an ammoniacal solution of molybdic acid is strongly concentrated, always however smelling of ammonia, this salt gradually separates as a white crystalline powder.

Analysis.—1·0448 grm. salt left on ignition 0·8824 molybdic acid.

	a.	Calc.	*b.*	Calc.	Found.
1 at. Oxide of ammonium...	324·98	15·544	324·98	15·649	
2 at. Molybdic acid	1777·932	84·456	1751·658	84·351	84·456
	2102·912	100·000	2076·638	100·000	

Double salt between the Bimolybdate and the Trimolybdate of the Oxide of Ammonium, $NH^4O, 2MoO^3 + NH^4O, 3MoO^3 + 3HO$.—This salt is formed when a solution of molybdic acid in ammonia is evaporated by heat to crystallization, or the solution left to spontaneous evaporation. It forms large six-sided prisms with two truncations; the crystals are perfectly transparent, and are not altered by exposure to the air.

Analysis.—I. 3·0623 grms. salt lost 0·2001 by desiccation in a current of dry air at 212°, the water however carried with it traces of ammonia. The residue left on ignition 2·503 molybdic acid.

II. 3·550 grms. salt gave 2·8998 molybdic acid.

III. 0·381 grm. salt gave 0·3095 molybdic acid.

100 parts of the salt therefore contain—

	I.	II.	III.	Mean.
Molybdic acid	81·725	81·685	81·234	81·548
Water	6·534			

On comparing these numbers with the theoretical composition, we have—

	a.	Calc.	*b.*	Calc.	Found.
2 at. Oxide of ammonium	649·960	11·965	649·960	12·210	
5 at. Molybdic acid.........	4444·830	81·823	4379·145	81·501	81·548
3 at. Water	337·437	6·212	337·437	6·289	6·534
	5432·227	100·000	5366·542	100·000	

Several other salts may be prepared by the action of nitric acid upon solutions of molybdic acid in ammonia; among others there exists, as with potash, a double salt which on solution in water is decomposed into a readily soluble salt, and an insoluble one, which separates in minute acicular crystals.

By the partial precipitation of a solution of crude molybdic acid, *i. e.* as obtained by exhausting the roasted mineral in ammonia, a nearly pure molybdic acid may be obtained; it contains mere traces of phosphoric acid. This method is very simple, and may be employed in many cases.

Salts of Baryta.

Molybdic acid forms a long series of salts with baryta, which are crystalline, amorphous, soluble and insoluble. Their preparation is in most cases easy, the corresponding baryta salt being precipitated from a solution of any salt of potash or ammonia. But a remarkable circumstance occurs in this reaction, viz. the precipitated salt is always contaminated with small quantities of the potash or ammonia salt employed, which admixture cannot be removed by washing with boiling water. The amount of these foreign admixtures is so small, that were we to consider these salts as double salts, we should be compelled to admit a most peculiar class of combinations, in which one atom of the one salt would be combined with a very large number of atoms of the other salt.

This circumstance is of serious inconvenience in the analyses, as the numbers found never agree accurately with the calculated composition of the pure salt. Besides this uncertainty there is another evil, arising from the great difficulty of completely decomposing the baryta salt. We adopted the following method for the analysis of these compounds:—We first determined the loss which the salt experiences at a red heat. This loss is water, but if an ammonia salt had been employed for the precipitation, water and oxide of ammonium. The residue was then treated in a beaker with an excess of dilute sulphuric acid and evaporated, so that, eventually, the highly concentrated sulphuric acid acts upon the salt. It was left in this state from two to three days upon the sand-bath, then diluted with water, filtered, the precipitate treated several times with hot water and a few drops of sulphuric acid, the precipitate placed upon the filter and washed with hot water. In the calculations no attention has been paid to the small admixture of foreign salt, and on that account less baryta will be found in all the analyses than theory requires.

Neutral Molybdate of Baryta, BaO, MoO^3.—When a solution of chloride of barium is added to any solution of molybdic acid in an excess of ammonia, this salt instantly separates. It should be filtered immediately, in order that the free baryta in the solution may not absorb carbonic acid from the air and accompany the precipitate. We proceeded as follows to be certain of obtaining a pure salt:—

I. A solution of the neutral molybdate of ammonia was precipitated with chloride of barium, the precipitate collected upon a filter and washed for a very long time, although the salt is somewhat soluble in water.

II. A solution of the above-described double salt was mixed with an excess of ammonia, and then so much chloride of barium added that a portion of the salt of ammonia was left undecomposed. It was immediately collected upon a filter, access of air being prevented as much as possible.

This salt forms a fine crystalline powder, which is anhydrous and sparingly soluble in water; on calcination it acquires a bluish-green colour, owing to the reducing action of the traces of ammonia which escape. It does not fuse; adheres very firmly to the glass.

Analysis.—I. 0·864 grm. of salt obtained by method I. lost on ignition 0·014, and gave 0·634 sulphate of baryta = 0·4262 baryta.

II. 1·686 grm. salt, according to method II., lost on ignition 0·0273, and gave 1·2982 sulphate of baryta = 0·846 baryta.

Assuming the loss by ignition to consist of ammonia, 100 parts of the salt contain—

	I.	II.
Baryta	49·323	50·177
Oxide of ammonium .	1·620	1·619
Molybdic acid . . .	49·057	48·204
and the ignited salt—		
Baryta	50·137	51·003
Molybdic acid . . .	49·863	48·997

Although these analyses do not agree accurately in the amount of baryta, we mention them because they show that a certain amount of the oxide of ammonium salt always accompanies them. The loss by ignition does not result either from hygrometric or chemically combined water, as we have convinced ourselves of the presence of ammonia, not by quantitative but by qualitative experiments. Were we to attempt to deduce a formula from these numbers, we should find that 1 at. of the ammonia salt is combined with about 12 ats. of the salt of baryta. The composition of the neutral molybdate of baryta, according to theory, is—

	a.	Calc.	*b.*	Calc.
1 at. Baryta . .	955·29	51·789	955·29	52·17
1 at. Molybdic acid	888·966	48·202	875·829	47·83
	1844·256	100·000	1831·119	100·00

Heine* has described a basic molybdate of baryta, of which he gives two analyses, which however were made with the same preparation. On comparing the characters which he gives of his salt with those of our neutral molybdate of baryta, they agree very accurately. We have also endeavoured to prepare the basic salt according to Heine's description, but

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* *Journ. für Prakt. Chem.* 1836. vol. ix. p. 204.

have always obtained the neutral salt only, and are therefore led to doubt the existence of this basic salt.

Trimolybdate of Baryta, $BaO, 3MoO^3 + 3HO$.—A solution of the trimolybdate of potash was mixed with chloride of barium, when a flocculent precipitate subsided. It is somewhat soluble in pure water. After washing and drying, it forms a yellowish white horny mass, which adheres very firmly to the filter and is brittle. On ignition it parts with water, fuses, and solidifies on cooling to a yellowish crystalline mass.

Analysis.—0·9854 grm. salt lost on ignition 0·0829, and gave 0·347 sulphate of baryta = 0·2261 baryta.

The hydrated salt has, according to theory and analysis, the following composition:—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Baryta.........	955·290	24·125	955·290	24·368	22·946
3 at. Molybdic acid	2666·966	67·353	2627·487	67·024	68·641
3 at. Water	337·437	8·522	337·437	8·608	8·413
	3959·693	100·000	3920·214	100·000	100·000

and the anhydous salt

	a.	Calc.	*b.*	Calc.	Found.
1 at. Baryta.........	955·290	26·373	955·290	26·663	25·054
3 at. Molybdic acid	2666·966	73·627	2627·487	73·337	74·946
	3622·256	100·000	3582·777	100·000	100·000

Double salt of Baryta, $BaO, 2MoO^3 + BaO, 3MoO^3 + 6HO$.—This salt is obtained by decomposing the corresponding ammonia salt with chloride of barium as a flocculent white precipitate. It is amorphous and somewhat easily soluble in water; on ignition it parts with its water with traces of ammonia, fuses, and solidifies into a crystalline mass on cooling.

Analysis.—0·872 grm. salt lost by ignition 0·0793, and gave 0·3575 sulphate of baryta = 0·233 baryta. Theory and analysis give the following composition for the hydrated salt.

	a.	Calc.	*b.*	Calc.	Found.
2 at. Baryta.........	1910·580	27·176	1910·580	27·433	26·714
5 at. Molybdic acid	4444·830	63·224	4379·145	62·877	64·192
6 at. Water	674·874	9·600	674·874	9·690	9·094
	7030·284	100·000	6964·599	100·000	100·000

and for the anhydrous salt—

	a.	Calc.	*b.*	Calc.	Found.
2 at. Baryta	1910·58	30·062	1910·580	30·376	29·393
5 at. Molybdic acid	4444·83	69·938	4379·145	69·624	70·607
	6355·41	100·000	6289·725	100·000	100·000

On attempting to decompose the above-described double salt of potash, in order to obtain the corresponding baryta salt, by adding the dry potash salt gradually and with constant agitation to a dilute solution of chloride of barium, a bulky precipitate quickly forms, which appears under the microscope to consist entirely of six-sided plates; but this salt is not very stable, it soon loses its bulky appearance, sinks and forms a mixture of two salts, which may be distinguished by the microscope. One salt is amorphous, whilst the other crystallizes in minute distinct six-sided prisms. Whether in this case a similar decomposition occurs as with the potash salt, we are unable to decide, and we have not made any further experiments with it, as the mixture of salts cannot be separated.

The above salts were all prepared by double decomposition; another long series of baryta salts may be obtained by treating any one of the former with nitric or muriatic acid. Various acid salts are formed according to the amount of acid, some of which are soluble, while others are perfectly insoluble. All these compounds are remarkable on account of their being crystalline. We have analysed only one of these salts.

Molybdate of Baryta with 9 atoms of Acid, $BaO, 9MoO^3 + 4HO$.—We obtained this salt by treating the neutral salt with nitric acid. It crystallizes in minute six-sided prisms. This salt is insoluble in cold and in hot water, and in dilute nitric acid. On ignition it parts with water, fuses, and solidifies on cooling into a crystalline mass. As this salt is but imperfectly acted upon by sulphuric acid or a mixture of sulphuric with nitro-muriatic acid, it was fused in a platinum crucible with bisulphate of potash.

Analysis.—0·782 grm. salt dried at 212° lost at a red heat 0·0393 water, and gave 0·120 sulphate of baryta = 0·078 baryta, or in 100 parts—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Baryta	955·290	10·156	955·290	10·256	9·999
9 at. Molybdic acid ..	8000·694	85·061	7882·461	84·900	84·975
4 at. Water	449·916	4·783	449·916	4·844	5·026
	9405·900	100·000	9287·667	100·000	100·000

The anhydrous salt has the following composition:—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Baryta	955·290	10·667	955·290	10·808	10·528
9 at. Molybdic acid ...	8000·694	89·333	7882·461	89·192	89·472
	8955·984	100·000	8837·751	100·000	100·000

Salts of Lead.

Neutral Molybdate of Lead, PbO, MoO^3.—Oxide of lead does not appear to combine in more than one proportion with molybdic acid; in all cases the neutral salt separates. We prepared this salt by precipitating a solution of the trimolybdate of potash with a solution of the nitrate of lead, when a white flocculent precipitate immediately separates, which is very sparingly soluble in water. After washing and drying it forms a white powder, which fuses only at a very high temperature. It appears to contain no chemically combined water, for on ignition it lost only 3·380 per cent. after having been dried at 212°, and this quantity only corresponds to ¾ atom of water. The analysis of this salt, if it is desired not to employ hydrosulphate of ammonia, is attended with several difficulties, as a perfect separation of the molybdic acid from the oxide of lead is impossible. In both analyses we have obtained an excess of lead. We effected the separation by treating the ignited salt in a beaker with sulphuric acid and the application of heat. In the first analysis we attempted to determine directly the molybdic acid by evaporating the sulphatic solution, and subsequent ignition with the addition of a few drops of nitric acid. In this determination it is impossible to avoid a considerable loss.

Analysis.—I. 0·7367 grm. lost on ignition 0·023, and gave 0·6082 sulphate of lead, or 0·4475 oxide of lead and 0·2612 molybdic acid.

II. 1·5436 grm. salt lost on ignition 0·0446, and gave 1·2609 sulphate of lead = 0·9278 oxide of lead; or in 100 parts—

	I.	II.
Water . . .	3·122	3·638
Oxide of lead .	60·730	60·106
Molybdic acid	36·148	36·256

Admitting the water to be hygrometric, the salt will have the following composition:—

	a.	Calc.	*b.*	Calc.	Found.	
1 at. Oxide of lead	1394·645	61·072	1394·645	61·425	62·704	61·893
1 at. Molybdic acid	888·966	38·928	875·829	38·575	35·449	
	2283·611	100·000	2270·474	100·000	98·153	

Salts of Silver.

Neutral Molybdate of Silver, AgO, MoO^3.—When a solution of neutral molybdate of potash is mixed with nitrate of silver, a yellowish flocculent precipitate is obtained, which gradually becomes darker under the influence of light. It is slightly soluble in water, but readily so in water containing nitric acid. After being washed and dried at 212°, it was analysed by dissolving it in ammonia, and then precipitating the silver as chloride, taking care to add an excess of muriatic acid, in order to hold the whole of the molybdic acid in solution. We attempted to determine the molybdic acid directly, by evaporating the filtered solution, expelling the chloride of ammonium, and treating the residue with a few drops of nitric acid; but this determination is far more uncertain than in the case of the lead salt, as a considerable quantity of molybdenum is volatilized in the form of some chlorine-compound, which was very perceptible, from the paper covering the beaker being coloured blue.

Analysis.—0·2431 grm. of salt gave 0·1862 chloride of silver = 0·1505 oxide of silver. The evaporated liquid afforded 0·051 molybdic acid, corresponding to 20·971 per cent., so that, compared with the theoretical composition, we experienced a loss of 17·2 per cent. of molybdic acid.

The following is the composition in 100 parts:—

	a.	Calc.	*b.*	Calc.	Found.
1 at. Oxide of silver	1449·660	61·988	1449·660	62·338	61·929
1 at. Molybdic acid	888·966	38·012	875·829	37·662	
	2338·626	100·000	2325·489	100·000	

Acid Molybdate of Silver.—When a solution of the trimolybdate of potash is mixed with nitrate of silver, a flocculent white precipitate is immediately produced. If this is collected upon a filter and well washed, although a portion is dissolved, various compounds appear to be obtained under different circumstances, as will be seen from the analyses. These salts are anhydrous.

Analysis.—I. 0·7694 grm. salt gave 0·3783 chloride of silver = 0·305 oxide of silver.

II[*]. 0·8353 grm. gave 0·4195 chloride of silver = 0·3392 oxide of silver.

III. 1·025 grm. gave 0·5148 chloride of silver = 0·4162 oxide of silver.

IV. 2·0109 grms. salt gave 1·0235 chloride of silver = 0·8274 oxide of silver.

Or, in 100 parts,—

	I.	II.	III.	IV.
Oxide of silver	39·754	40·605	40·608	41·143

These numbers agree best with the formula $AgO, 3MoO^3 + AgO, 2MoO^3$:—

	a.	Calc.	*b.*	Calc.
2 at. Oxide of silver	2899·320	39·478	2899·320	39·384
5 at. Molybdic acid	4444·830	60·522	4379·145	60·166
	7344·150	100·000	7278·465	100·000

[To be continued.]

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[*] The salt employed for the analyses II. and III. was taken from the same portion.

LXIV. *On certain Functions resembling Quaternions, and on a new Imaginary in Algebra. By* JAMES COCKLE, *Esq., M.A., Barrister-at-Law. In a Letter to* T. S. DAVIES, *Esq., F.R.S.L. & E. &c.*[*]

To Richard Taylor, Esq., F.S.A. &c.

Royal Military Academy, Woolwich,
October 27, 1848.

MY DEAR SIR,

I BELIEVE that Mr. Horner's attention was first directed by me to the consideration of what he has happily termed "congeneric surd equations." This was done in consequence of a difficulty that I had found in discussing such an equation which was accidentally brought before me; a difficulty which, from the friendship that had long subsisted between us, I very naturally referred to him. His reply was printed in the Philosophical Magazine about a dozen years ago; and it has received proper attention from those algebraists who look with due care to the fundamental principles of their science.

Almost every new difficulty in algebra is the precursor of more extended views respecting its principles; and often out of a case which may, at first sight, appear to be little more than a conundrum, very important improvements in our general theories often arise. All efforts to generalize our views, so as to include the "conundrum," deserve, therefore, attentive consideration; and for this reason I send you for publication the accompanying letter, which I have just received from your correspondent and my friend, Mr. James Cockle, of the Middle Temple.

I hope you will be able to give Mr. Cockle's letter an early place in your valuable Magazine, as it contains much that is suggestive of inquiry.

I remain,
Yours very truly,
T. S. DAVIES.

2 Church-Yard Court, Temple,
October 26, 1848.

MY DEAR SIR,

YOU are aware that the new algebraical symbol, which I have recently discovered, was suggested to my mind by reflecting on the structure of congeneric surd equations—a subject on which I have lately been much engaged. Did I require any excuse for throwing the present investigations into the form of a letter to you, I should find it in the fact that the remarks of Horner on congenerics were put into a similar shape. (See Phil. Mag., S. 3, vol. viii. p. 43–50.)

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[*] Communicated by Mr. Davies.

In what follows, I propose to confine the term *unreal* to the imaginary quantities of ordinary algebra. When the word *impossible* is used, it must be understood as referring exclusively to expressions involving the new symbol. So that imaginary quantities constitute a genus which includes two species, viz. unreal quantities and impossible quantities.

Let

$$1+i^2=0,$$

then i is the simplest representation of unreal quantity; and, if

$$1+\sqrt{j}=0,$$

then j is the simplest representation of impossible quantity. It is to be borne in mind that, in the latter equation, the radical is to be considered as essentially affected with the sign $+$.

Let w, x, y, z be any real quantities, positive, negative, or zero; also let

$$w+ix+jy+ijz=t,$$

then t I call a *tessarine*, and w, x, y, z its *constituents.* The latter term I have adopted from the quaternion theory of Sir W. R. Hamilton.

This being premised, I shall first proceed to prove, that, when a tessarine vanishes, its constituents are simultaneously zero.

Suppose then that $t=0$, and solve this last equation with respect to j; the result is

$$j=-(w+ix)\div(y+iz);$$

that is to say, j is capable of being linearly expressed in terms of i and of real quantities, for we may readily reduce the above expression for j to the form

$$j=\alpha+i\beta.$$

But, as may be proved from the principles of my theory of congeneric surd equations*, there is no linear relation between i and j. Hence α and β must each be of the form $0\div0$; and we see, in consequence, that when $t=0$, we have, necessarily,

$$w=0, \quad x=0, \quad y=0, \text{ and } z=0.$$

The symbol j is not capable of being identified with any of the imaginaries of the quaternion theory. It is a deduction from the principles of algebra rather than an invention. I shall not repeat my argument in favour of its admission as a new and independent symbol, because I think that I have put the question of the propriety of such admission beyond dispute at

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* Mechanics' Magazine, vol. xlvii. pp. 151, 307, 331, 409; vol. xlviii. p. 181; vol. xlix. p. 364.

pp. 364–367 of the current (49th) volume of the Mechanics' Magazine; I shall rather go on with the development of the properties of expressions which involve it.

For convenience, I shall denote the product of i and j by k; I shall assume that j^2 is equal to unity. I say *assume* because, although I have reasons (not, however, free from objection,) for making such a supposition, yet I wish to reserve to myself full liberty to modify the assumption, and to discuss *ab initio* the symbol j^n and its symbolic relation to j. Then the following system of relations will, together with the second equation given in this letter, furnish us with all the conditions requisite for the formation of a theory of these three imaginaries. That system is—

$$i^2=-j^2=k^2=-1,$$

$$ij=k, \quad jk=i, \quad ki=-j.$$

Almost the first thing that strikes us, in examining the expression for a tessarine, is, that, under the above system, the product of two or more tessarines is a tessarine. Let us confine our attention to the case in which two only are multiplied together, and let the factors be t and t', where

$$t=w+ix+jy+kz,$$

and

$$t'=w'+ix'+jy'+kz'.$$

By actual multiplication, we obtain

$$\left.\begin{aligned} tt'=ww'+i^2xx'+j^2yy'+k^2zz' \\ +i(wx'+xw')+jk(yz'+zy') \\ +j(wy'+yw')+ik(xz'+zx') \\ +k(wz'+zw')+ij(xy'+yx'). \end{aligned}\right\}$$

Suppose that

$$\left.\begin{aligned} ww'-xx'+yy'-zz'=w'', \\ wx'+xw'+yz'+zy'=x'', \\ wy'+yw'-xz'-zx'=y'', \\ wz'+zw'+xy'+yx'=z'', \end{aligned}\right\}$$

and also that

$$w''+ix''+jy''+kz''=t'';$$

then we have, under the foregoing system of relations,

$$tt'=t'';$$

and, if we had three or more such factors as t, the result would still be a tessarine.

The next point that arises is,—what is the tessarine *law of moduli*? or, more generally, what modular or quasi-modular relation or relations exist between the constituents of the factors and those of the product? Seeing that the operations of algebra necessarily and inevitably conduct us to tessarine formulæ, I cannot doubt but that some relations may be found, such as to enable us to extend the boundaries of analysis. I have not, however, time to pursue this inquiry further now —more particularly as I wish to make one or two remarks upon other parts of the subject of this new imaginary j.

For instance, I would observe that the product of two or more factors of the form

$$w+jy$$

is of the same form; thus

$$(w+jy)(w'+jy')=w''+jy'',$$

where

$$w''=ww'+yy',$$

and

$$y''=wy'+yw'.$$

There are cases in which the impossible quantity j altogether disappears from a result. Thus, if ε be the base of the Napierian logarithms, we have

$$\varepsilon^{jy}=1+jy+\frac{y^2}{2}+j\frac{y^3}{2.3}+\frac{y^4}{2.3.4}+\&\text{c.},$$

and

$$\varepsilon^{-jy}=1-jy+\frac{y^2}{2}-j\frac{y^3}{2.3}+\frac{y^4}{2.3.4}-\&\text{c.};$$

hence

$$\varepsilon^{jy}+\varepsilon^{-jy}=2\left(1+\frac{y^2}{2}+\frac{y^4}{2.3.4}+\&\text{c.}\right),$$

and

$$\frac{1}{2j}(\varepsilon^{jy}-\varepsilon^{-jy})=y+\frac{y^3}{2.3}+\&\text{c.}.$$

This last is a very simple case; but the nature of such a letter as this obliges me to have recourse to illustration rather than extensive investigation.

I need not tell *you* how to read the second equation which occurs in this communication; still, I cannot help saying—to use words adopted on another occasion, by our friend Professor J. R. Young (Alg. 4th edit. p. 131),—that we must read that equation as meaning that *unity together with* the *plus* [*square-*] root of j is equal to *zero*, and NOT that *unity together with* a *square-root of j* is equal to *zero*. The whole discussion is involved in the correct reading of that fundamental equation.

By way of conclusion, I will add that, if we had three *inde-*

pendent imaginaries i, j, k, we should have to deal, not with tessarines, but with what might be called *octrines.* These last-mentioned expressions would be of the form

$$w+ix+jy+kz+ijp+ikq+jkr+ijks,$$

where w, x, y, z, p, q, r, s are any real quantities. These quantities might be termed the constituents of the octrine, which could not vanish unless all its constituents were zero. So if we had four independent imaginaries (that is to say, imaginaries unconnected by any linear relation), the corresponding expression would consist of *sixteen* terms, and would have properties analogous to those of the tessarine and the octrine. I must here break off, but I remain, always,

My dear Sir,
Yours most truly,
JAMES COCKLE.

To T. S. Davies, Esq., F.R.S. L. & E.,
&c. &c. &c., Woolwich.

LXV. *Seventh Memoir on Induction. By* M. ELIE WARTMANN, *Professor of Natural Philosophy in the Academy of Geneva**.

[Continued from p. 278.]

§ XIX. *On the relations of Electricity with bodies in the spheroidal state, and on some properties of those bodies.*

178. IT is a well-known fact that liquids are incapable of moistening solid surfaces sufficiently heated, or of mixing, in certain cases, with other liquids raised to a high temperature. The first accurate observation of this fact is attributed to Eller: it dates from 1746†. Ten years later, this phænomenon was studied by Leidenfrost, by whose name it is still designated‡. Since then a number of experiments little known have been made in Germany§; afterwards Italy ||

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* Communicated by the Author.

† *Histoire de l'Académie de Berlin*, 1746, p. 42.

‡ *De aquæ communis nonnullis qualitatibus*: Duisburg, 1756.

§ Among others we may cite Ziegler, *Specimen de digestore Papini*, 1765.—Link, *Beiträge zur Physik und Chemie*, No. 2, p. 11.—Kastner, Tromsdorff's *Journal*, vol. ix. p. 270.—Œrsted, Gehlen's *N. Allgem. Journ.*, vol. iii. p. 324.—Klaproth, Scherer's *Allg. Journal de Chemie*, vol. vii. p. 646; and *Journal de Physique*, 1802, p. 62.—Döbereiner, Schweig. *Journ.*, vol. xxix. p. 43.—N. W. Fischer, Poggend. *Annalen*, vol. xix. p. 514, and vol. xxi. p. 163.—Buff, Pogg. *Ann.*, vol. xxv. p. 591.—A. H. Emsmann, Pogg. *Ann.*, vol. li. p. 444.—Frei, Kastner's *Archiv*, vol. iv. p. 57.—Erdmann, *Journal für Praktische Chemie*, vol. x. p. 334.—Böllger, *Eine Sammlung eigener Erfahrungen, Versuche und Beobachtungen*, No. 18, Francfort-sur-Mein, 1838.

|| Bellani, *Giornale di Fisica di Brugnatelli*, 1808, p. 779; *Giornale di*

and France* have taken part in these researches, which have recently been renewed and much extended by M. Boutigny†.

179. I propose to examine in this memoir the relations of the electric fluid with the spheroidal state,—relations which appear to me not to have hitherto attracted the attention of observers. In the face of incomplete and often contradictory theories, by which this singular condition of bodies is attempted to be explained, it is necessary to accumulate new facts in order to determine with certainty the part taken by the different forces which produce it.

180. My first experiments were made with electricity of tension. The apparatus employed is very simple. It consists of a support furnished with a moveable stem terminated by a ring, on which rests a tolerably thick platina crucible, almost flat, and of a diameter of $0^{m}{\cdot}06$. Another support carries, at the extremity of a metallic arm, a platina wire twisted on itself, and which bends back below in a sort of eye, or elongated slightly open loop, to $0^{m}{\cdot}001$ of the capsule. A chain attached to the arm serves to connect this wire with the external armature of a Leyden jar.

181. After heating the capsule, some portions of liquid are projected on to it, which collect in a spheroidalized drop around the eye. The lamp is then taken away, and the knob of the jar well-charged is rapidly approached to the lower surface of the crucible. The explosion determines different effects, according to the nature of the liquid and the temperature of the vessel containing it.

182. Let us first raise the capsule to a degree of heat which much exceeds the limit indispensable to the spheroidal state. If we operate on pure water, this is not traversed by the discharge, and its close or radiating form experiences no alteration. The spark, of a purplish colour, does not penetrate the immersed portion of the platina wire, but glides over the convex surface of the drop.

Fisica di Pavia, 1816, p. 255; *Giornale dell' I. R. Istituto Lombardo delle Scienze, Lettere ed Arti, e Biblioteca Italiana*, 1844, p. 193.—Belli, *Corso di Fisica*, vol. i. p. 96.—Belli e de Kramer, *Giornale dell' I. R. Istituto Lombardo*, 1844, p. 192.

* Lambert, *Pyrométrie*, p. 130.—Rumford, *Mémoires sur la Chaleur*, p. 93.—Pouillet, *Annales de Chimie et de Physique*, vol. xxxvi. p. 5.—Beaudrimont, *Ann. de Chim. et de Phys.*, vol. lxi. p. 319.—Laurent, *Ann. de Chim. et de Phys.*, vol. lxii. p. 327.—Le Chevallier, *Journal de Chimie Médicale*, vol. vi. p. 539; and *Journal de Pharmacie*, vol. xvi. p. 666.—Dulong, *Journal de Pharmacie*, 1830.—Pelouze, *Journal de Pharmacie*, 1840, p. 779.—Dumas, *Traité de Chimie appliquée aux Arts*, vol. i. p. 32.—Person, *Comptes Rendus*, vol. xv. p. 492.

† *Nouvelle branche de Physique, ou études sur les corps à l'état sphéroïdal.* 2nd edition. Paris, 1847.

183. When the quantity of liquid has been greatly reduced by evaporation, so that there no longer remains more than a small drop lodged in the lower part of the eye and suspended above the capsule, the fluid projects it mechanically toward the sides, and it is seen to re-descend immediately along the concavity of the vase to resume its first position on the wire.

184. But if the discharge is excited through the capsule cooled to about 200° C., it acts in quite a different manner. The cause which keeps the distilled water in the spheroidal state having become very weak, the drop passes suddenly to the state of a moistening liquid and disappears entirely, or at least in great part, with all the characters of a brisk evaporization.

185. The same experiments were repeated on water rendered conducting by the addition of a little nitric acid. If the temperature of the capsule is much raised (without however attaining to a dull red), the spark is emitted through the drop which is not sensibly affected by it, either in its state of gyration or of repose, or in its limpid or mamelonnated, ellipsoidal or stellate appearance.

186. On the contrary, if the heating is near the minimum necessary to the spheroidal state, the electricity destroys that state, and the drop is converted into gas diffusing acid vapours.

187. The second case to be examined was that of dynamic electricity. I employed a Grove's battery of five large pairs (139.), moderately excited and provided with platina rheophores.

188. With distilled water, we find that in the spheroidal state, as in the cold, there is no decomposition.

189. If we substitute acidulated water, this is electrolyzed when the two polar wires enter the drop without touching the capsule. The bubbles of gas, being produced at a great heat, are more dilated than at the ordinary temperature, but their mass remains probably the same.

190. When either one of the electrodes is put in contact with the vessel and the other is immersed in the drop, there is no decomposition, because *the current does not pass**. A

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* Since the compilation of these notes, I have found that M. Poggendorff made some similar experiments in 1841. He showed that no current is established when a lamina of zinc is immersed in spheroidalized sulphuric acid, or a lamina of amalgamated zinc and a platina wire heated red-hot, is plunged simultaneously into cold acidulated water. That these results might not be attributed to unipolar phænomena, that skilful experimenter reproduced them with a Saxton's magneto-electric machine provided with an inversor; the poles of the magnet were connected with platina wires, one of which was heated to redness by a lamp (Pogg. *Ann. der Physik und Chemie,* vol. lii. p. 539). MM. Peltier, De Kramer and Belli, subsequently observed the same fact (Boutigny, *ouvr. cité,* p. 37).

rheometer placed in the circuit shows that this is the case. This is an excellent proof of the character essential to the spheroidal state, that the drop does not touch the heated surface. The excessively thin and very hot layer which separates it is not conducting. It would be interesting to ascertain the relation which exists between its isolating power and its temperature, with various liquids placed on supports of different natures.

191. When one of the rheophores touches any portion of the capsule externally to the liquid, and the other is immersed in the liquid and in contact with the metal, the passage of the current is established, and the drop evaporates rapidly producing abundant bubbles accompanied by a certain sonorous vibration. This phænomenon ceases as soon as the voltaic circuit is opened. It is presented not only when the extremity of the rheophore rests on the capsule below the liquid which it traverses, but also when the contact takes place outside the lower surface of the drop. In both cases the two somewhat thin polar wires are heated for a great length till they cannot be longer handled. The heat thus disengaged is the cause of the bubbles of vapour. This may readily be observed by immersing in pure spheroidalized water a very massive silver wire. A few instants after its contact with the capsule ebullition is perceptible, resulting from its conductibility for heat*. This phænomenon has considerable analogy with that of the combustion of albumen under the influence of very powerful currents (139.).

192. Acidulated water was replaced by mercury, the conductibility of which is still greater. Contrary to the assertion of Fischer, this body passes into the spheroidal state provided it is reduced to very small masses. A rather large drop which falls on a platina capsule at a dull red heat, boils at first in a very remarkable manner. Its surface is furrowed with a thousand wrinkles which deprive it of its mirroring aspect, and it rapidly evaporates with a sort of crepitation. While it is in this state, it touches the platina metallically, for it conducts the current of the pile. All at once the small quantity of mercury which remains recovers its brilliant polish: it becomes round in a nearly geometric sphere, the noise ceases, and the evaporation continues with very great slowness: the drop is then electrically isolated from its support.

193. The discontinuous currents remained to be examined. To study their influence on the spheroidal state, I produced with the five Grove's pairs instantaneous currents of very great

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* An analogous observation is mentioned by M. Boutigny, *loc. cit.* pp. 66, 70 and 105.

energy in an electro-electric machine with an electro-electric rheotome*. The extremities of the induced circuit abut on platina wires immersed in pure or acidulated water in the spheroidal state. It has undergone no change in its united or radiated form. The same experiments, repeated by means of my commutator (122.), gave similar results, whatever was the velocity with which the inductor current was interrupted†.

194. I pass to the description of some experiments in which electricity does not intervene, and which appear not to have been before made. They are given here only on account of their connection with the preceding details, and some practical applications of which they will perhaps be susceptible.

195. Five circumstances at least have an influence on the general phænomena of the spheroidal state: the nature and the mass of the liquid, the nature and the thickness of the solid which supports it, lastly the degree of heat at which it is kept.

196. At an equal temperature, pure water spheroidalized and reduced to little drops, skips in a silver spoon with more energy than on a leaf of platina. The syrup of sugar, also in small spheres, rebounds more on the platina than pure water.

197. It is known that a great mass of water may be brought to the spheroidal state in a thick capsule of platina. When the temperature is very elevated, the star-form is soon pro-

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* This apparatus, constructed by M. Bonijol, demonstrates the influence of the state of closing or opening of the circuit induced on the action of the inductor current on itself (§ IV.). While the induced circuit is open, the spring of the rheotome makes a sharp sound; but this sound falls immediately a semitone, a fifth, and even an interval of a twelfth, as soon as the induced circuit augments by its opening the electro-magnetic attraction of the iron cylinders, which occupy the hole of the bobbin on the disc of soft iron fixed above the spring. This variation in the musical sound merits the attention of medical men who employ the machine for shocks. The nervous sensibility may be so much weakened in certain paralytic patients, that they may not perceive distinctly the passage of the instantaneous induced currents. Before modifying the conditions of the experiment, or augmenting the power of the battery (which might in certain cases (143.) entail the most fatal consequences), the practitioner should assure himself of the real closing of the induced circuit. For this purpose he will consult the tone of the sound of the rheotome. If the machine works with a mercurial commutator, he will examine the diminution of the brilliancy of the sparks at the time of the rupture of the inductor circuit (36.).

† The want of apparatus sufficiently energetic has hitherto prevented my trying an experiment which would be interesting in many respects. Its object would be to ascertain whether, in the spheroidal state, liquids are subject to the forces which emanate from the poles of a powerful magnet. The beautiful discoveries of Prof. Plücker of Bonn, on the axial or equatorial elongations which these forces impress on the primitive forms of equilibrium of the liquids exposed to their action in ordinary circumstances, are well known.—Poggend. *Ann. der Physik und Chemie*, vol. lxxiii. p. 549.

duced and persists for a long time. If the wick of the lamp is lowered a little, the drop is seen to rise centrally and give passage to a large bubble of vapour, which is immediately followed by several others. Each of these bubbles is unique in the mass. The form of the liquid is no longer stellate, but elongated. The drop turns as around a vertical axis, passing through one of the focuses of the ellipsoid. A further decrease of temperature sometimes causes the cessation of this gyratory motion; then the drop presents itself as a beautiful biconvex lens, which, by its immobility and its perfect transparency, most completely resembles an object-glass.

198. This remarkable form suggested to me the idea of submitting to a decisive and new proof a question still controverted. The question has been often asked, whether the caloric which reaches the drop traverses it by radiation without raising its temperature, or whether it is reflected by it. In the first case, the refraction in the centre of the lens must produce a focus at which the temperature will be certainly superior to that of the neighbouring points. In the second, this lens will form a circular screen, behind which the temperature will be lower than in the space submitted to the direct radiations of the capsule. To decide between these alternatives, I operated with small thermometers open at the top and furnished with a small ivory scale with arbitrary graduations. The stem passes through a thick cork disc fixed into the hollow of a glass funnel, so that their bulb makes a slight projection at the extremity of the beak. The instrument thus clothed, and placed above a drop $0^{m}{\cdot}028$ in breadth, demonstrates that the latter is not the seat of any refraction. Not only is there no calorific focus, but the temperature increases in proportion as the thermometer is raised above it, on account of the radiation caused by the parts of the metal which are not concealed by the liquid. This radiation explains why the heat near the surface of the lens is much higher than in its interior.

199. The influence of a musical sound suitably chosen on the constitution of a fluid vein is well known: that influence is explained by the periodicity of the variations which are established in the diameter of the vein at its origin. A body in the spheroidal state is often exposed to regular and periodical movements, which give to it in particular a stellate form. Numerous experiments have proved to me that this form is not at all changed by the sonorous pulsations of a diapason communicating with the support of the capsule, although those pulsations be energetic enough to derange and alter the position of the capsule.

200. I shall conclude by relating some experiments which appear to me to have a certain practical importance. M. Thury, my assistant during the winter session, remarked that water let fall upon a heated copper capsule only assumes the spheroidal state for some seconds, and generally not at all when the surface is coated with deutoxide of copper. I immediately suspected that this was not a property peculiar to the oxide, but an effect attributable to the rough state of the surface, as M. Boutigny has remarked*. Direct experiments have shown that this opinion is well-founded.

201. A platina capsule was half coated with a paste formed of oxide of zinc and water, and raised to a dull red. It then presented the curious appearance of two surfaces, one of which, being metallic and brilliant, spheroidalized instantaneously the pure water which now only evaporated with extreme slowness; whilst the latter, rough and of a beautiful canary-yellow, converted no less suddenly into vapour the drops of water which were projected on to it.

202. The carbonate of protoxide of iron, reduced to the state of peroxide by calcination, behaves like the oxide of zinc. The colour and the chemical nature of the deposit do not therefore take any part in the phænomenon.

203. English rouge mixed up with water covers platina, but it does not absolutely prevent the spheroidal state from being produced: its action seems to be limited to diminishing considerably the duration of the evaporation†.

204. The red oxide of manganese, obtained by the prolonged calcination of the peroxide, is still less destructive of the spheroidal state than the colcothar: moistened with cold water it adheres much less to metallic surfaces.

205. The colourless syrup of sugar readily assumes the spheroidal state on platina. The drops appear at first opake, especially if they are large, from a number of small bubbles of gas traversing them; they subsequently become beautifully transparent, and remain frequently immoveable, similar to nearly spherical glass lenses. Lastly, when the syrup has attained the maximum of concentration, it enters into a violent state of ebullition, without however spreading over the capsule, or moistening it; it changes into caramel, passing from a yellow to a brown colour, and finally leaves a bulky ball of porous carbon.

206. If the heat is diminished at the moment when chemical

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* *Loc. cit.* pp. 48, 56 and 76.

† M. Muncke has already observed, that a plate of iron oxidized by contact with air at a white red heat ceases to spheroidalize water which is poured on it.—Gehler's *Phys. Wörterbuch*, 2nd edit., vol. x. p. 490, 1841.

decomposition commences, the liquid touches the metal and covers it, at the end of the operation, with a slightly adherent coating, which is of a beautiful glossy black. This crust of carbon, heated even until the naked parts of the capsule are red, absolutely prevents the production of the spheroidal state; but pure or sugared water which is let fall on the platina, and is coloured of a deep black colour by some particles of the deposited carbon, behaves as if it had remained transparent and colourless.

207. M. Fechner states* that water no longer becomes spheroidal when it has been tinged with ink, or rendered opake by charcoal suspended in it. I repeated these experiments with care, and have found them inaccurate. In the two cases the blackened liquid readily assumed the spheroidal state on a plate of platina, and remained so for several minutes, until the solid particles from which it separates on evaporating unite into a small spongy ball†.

208. Moist crystallized sugar spheroidalizes whilst dissolving in its water of crystallization. It presents the same phases as the syrup from the moment of the caramelization.

209. Butter and suet pass into the spheroidal state in melting: they soon take fire and give off a light soot which disappears, being converted into carbonaceous gases.

210. It is very generally thought at the present day that one of the principal causes of explosion of boilers is owing to the sudden conversion into gas, at a very high temperature, of water first spheroidalized by contact with over-heated surfaces. If this opinion is proved, this terrible danger will certainly be remedied by clothing with an appropriate coating the internal surface of the generators, or by giving it a rough surface, which is unfavourable to the production of the spheroidal state. This would be less costly, and would present much less danger than the set of points proposed by M. Boutigny. I have not had the means of making experiments on this subject, which I must postpone till a more propitious moment.

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* *Repertorium der Physik*, vol. ii. p. 401. German translation of the Treatise of Experimental and Mathematical Physics of M. Biot, vol. v. p. 367.

† M. Boutigny has made an analogous observation on water containing soot (work cited, p. 25).

LXVI. *On Pluquaternions, and Homoid Products of Sums of* n *Squares. By the* Rev. THOMAS P. KIRKMAN, *A.B., Rector of Croft with Southworth, Lancashire**.

THE following analysis is the fruit of my meditations on Professor Sir W. R. Hamilton's elegant theory of quaternions, and on a pregnant hint kindly communicated to me, without proof, by Arthur Cayley, Esq., Fellow of Trinity College, Cambridge, about the connexion between a system of triplets having no duad in common, and the property that the product of two sums of $2n$ squares shall be a sum of $2n$ squares. In a note with which the latter gentleman recently favoured me, he writes, "The complete test of the possibility of the product of 2^n squares by 2^n squares reducing itself to a sum of 2^n squares, is the following:—Forming the complete system of triplets for (2^n-1) things, if *eab ecd fac fdb* be any four of them, we must have, paying attention to the signs only,

$$(\pm eab)(\pm ecd)=(\pm fac)(\pm fdb);$$

that is, if the first two are of the same sign, the last two must be so also, and *vice versâ*. I believe that, for a system of seven, two conditions of this kind, being satisfied, would imply the satisfaction of all the others. It remains to be shown, that the complete system of conditions cannot be satisfied for a system of fifteen things."

In the following investigation, the truth and completeness of Mr. Cayley's test of the property in question for sums of 2^n squares are established, and the contradiction anticipated by him for a system of fifteen is deduced; while ulterior results about homoid products of sums of n squares are obtained.

Let $a_0\, b_0\, c_0 \ldots r_0$ be $2n-1$ imaginary units, among which no linear relation exists, and let

$$a_0^2=b_0^2=\ldots=r_0^2=-1.$$

Let

$$Q_a=w+a_0 a+b_0 b+\ldots+r_0 r,$$

$$Q_{a_\prime}=w_\prime+a_0 a_\prime+b_0 b_\prime+\ldots+r_0 r_\prime,$$

$ww_\prime\, aa_\prime \ldots rr_\prime$ being any real quantities: it is proposed to consider under what conditions it is possible that the product of two such functions shall be of the same form. Suppose, then, that, if possible,

$$Q_a Q_{a_\prime}=Q_{a_{\prime\prime}}=w_{\prime\prime}+a_0 a_{\prime\prime}+b_0 b_{\prime\prime}+\ldots+r_0 r_{\prime\prime},$$

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* Communicated by the Author.

where $w_{\prime\prime} a_{\prime\prime} \dots r_{\prime\prime}$ are certain functions of the arbitrary numbers $ww_{\prime}\, aa_{\prime} \dots rr_{\prime}$.

In this last equation, it is plainly implied, that every duad, $a_{\circ} b_{\circ}$, that can be made with two imaginaries, is a linear function of those units; *i. e.* we must have

$$a_{\circ} b_{\circ} = \mathrm{A} a_{\circ} + \mathrm{B} b_{\circ} + \mathrm{C} c_{\circ} + \dots + \mathrm{R} r_{\circ};$$

whence, since $a_{\circ}^{2} = -1 = b^{2}$,

$$-b_{\circ} = -\mathrm{A} + \mathrm{B} a_{\circ} b_{\circ} + \mathrm{C} a_{\circ} c_{\circ} + \dots + \mathrm{R} a_{\circ} r_{\circ},$$
$$-a_{\circ} = \mathrm{A} a_{\circ} b_{\circ} - \mathrm{B} + \mathrm{C} c_{\circ} b_{\circ} + \dots\dots + \mathrm{R} r_{\circ} b_{\circ}.$$

Therefore $\mathrm{A} = 0 = \mathrm{B}$; and

$$-b_{\circ}^{2} = 1 = \mathrm{C} b_{\circ} a_{\circ} c_{\circ} + \mathrm{D} b_{\circ} a_{\circ} d_{\circ} + \mathrm{E} b_{\circ} a_{\circ} e_{\circ} + \dots + \mathrm{R} b_{\circ} a_{\circ} r_{\circ}.$$

As $b_{\circ}$, $a_{\circ}$, $c_{\circ}$, &c. are pure imaginaries, we cannot suppose that any term of the last expression, as $\mathrm{C} b_{\circ} a_{\circ} c_{\circ}$, is of the form $\mathrm{V} + \mathrm{W}$, where V is real and W imaginary. Some one or more, then, of these terms must be real; say the first three of them. We have then

$$\mathrm{C} b_{\circ} a_{\circ} c_{\circ} = \mathrm{C}',$$
$$\mathrm{D} b_{\circ} a_{\circ} d_{\circ} = \mathrm{D}',$$
$$\mathrm{E} b_{\circ} a_{\circ} e_{\circ} = 1 - \mathrm{C}' - \mathrm{D}'.$$

It follows that

$$\frac{\mathrm{C}}{\mathrm{C}'} c_{\circ} = \frac{\mathrm{D}}{\mathrm{D}'} d_{\circ},$$

which is impossible by hypothesis. There can then be only one of the terms real in the value of $-b^{2}$, and the equation before us is of the form

$$-b^{2} = 1 = \mathrm{C} b_{\circ} a_{\circ} c_{\circ},$$

or assuming $\mathrm{C} = \pm 1$,

$$1 = \pm b_{\circ} a_{\circ} c_{\circ};$$

which gives

$$b_{\circ} = \mp a_{\circ} c_{\circ}, \quad a_{\circ} b_{\circ} = \pm c_{\circ},$$

as the form of the linear function in question, equivalent to any duad $a_{\circ} b_{\circ}$ or $a_{\circ} c_{\circ}$.

One condition, therefore, of the possibility of the equation

$$\mathrm{Q}_{a} \mathrm{Q}_{a_{\prime}} = \mathrm{Q}_{a_{\prime\prime}}$$

is, that all the imaginaries can be combined into triplets, of which no two shall have a duad in common; since if $b_{\circ} a_{\circ} c_{\circ}$ and $d_{\circ} a_{\circ} c_{\circ}$ were both real, there would exist a linear relation between $b_{\circ}$ and $d_{\circ}$, contrary to hypothesis. The number $(2n-1)$ of the imaginaries is hereby restricted to the two forms $6m+1$ and $6m+3$, in the case of either of which the triplets can be formed so as to have no duad in common, and that generally in various ways (vide Camb. and Dub. Math. Journ., N.S. vol. ii. p. 191), upon the comparison of which it is not necessary here to enter, although the restriction might thus be further narrowed.

When $n=2$, or when there are only three imaginaries, Q_a is the quaternion invented by Sir W. R. Hamilton, and profoundly discussed by him in the twenty-fifth volume of the Philosophical Magazine. When $n=4$, it is the biquaternion or octad, of which Professor John T. Graves has made mention at the end of a paper On the Connexion between the General Theory of Normal Couples, and the Theory of Quadratic Functions of two Variables, ibid. vol. xxvi. As it will be useful to have a name for Q_a in cases where $n>4$, I venture to propose the appellation Pluquaternion, as generally applicable to Q_a when there are more than three imaginaries; not dreading here the pluperfect criticism of grammarians, since the convenient barbarism is their own.

Further, if c_0, a_0, b_0 be any three imaginaries forming one of the triplets, $c_0 a_0 b_0$ cannot differ in sign from $a_0 b_0 c_0$; for if it does differ,

$$c_0 a_0 b_0 = -a_0 b_0 c_0 = +b_0 c_0 a_0 = -c_0 a_0 b_0 ; \text{ Q.E.A.}$$

$$\therefore c_0 a_0 b_0 = a_0 b_0 c_0 = b_0 c_0 a_0 = \mp 1 ;$$

which is the property of every triplet of the system.

From these we deduce

$$a_0 b_0 = \pm c_0,$$
$$c_0 a_0 = \pm b_0,$$
$$b_0 c_0 = \pm a_0 ;$$

whence also

$$a_0 b_0 c_0 a_0 = c_0 b_0 = a_0 a_0 b_0 c_0 = -b_0 c_0,$$
$$c_0 a_0 b_0 c_0 = b_0 a_0 = c_0 c_0 a_0 b_0 = -a_0 b_0,$$
$$b_0 c_0 a_0 b_0 = a_0 c_0 = b_0 b_0 c_0 a_0 = -c_0 a_0 ;$$

for, because $b_0 c_0 a_0$, $= a_0 b_0 c_0$, is real,

$$a_0 b_0 . c_0 a_0 = a_0 . b_0 . c_0 a_0 = a_0 . b_0 c_0 a_0 = a_0 . a_0 b_0 c_0 = a_0 . a_0 . b_0 c_0 :$$

and so the other two of these three assertions may be established likewise. Wherefore

$$m_0 n_0 + n_0 m_0 = 0$$

is true of every duad $m_0 n_0$ that can be made with the $(2n-1)$ imaginaries in the equation $Q_a Q_{a,} = Q_{a,,}$, by virtue of our definitions.

Let Q_a $Q_{a,}$ and $Q_{a,,}$ be biquaternions, that is, let there be seven imaginaries $a_0 b_0 c_0 d_0 e_0 f_0 g_0$. The number of triplets that can be formed with seven things, so as to have no duad in common, is seven. Of these, there will be three in which a_0 is found; let the three be made all of one sign, by the inversion of a duad if necessary, a change which can alter no value, since $-a_0 b_0 c_0 = +a_0 c_0 b_0$. Neglecting subindices, let the three triplets be *abc*, *ade*, *afg*. We shall consider the order *abcdefg* to be alphabetical, and shall inquire what the remaining four triplets will be, when written out in the same order. Because

$$abc = ade = \mp 1;$$

$$bc = de,$$

and

$$-c = b.de.$$

Now $b.de$, whether real or not, if b, d, e be any three imaginaries, cannot differ in sign from $e.db$; for if it does,

$$b.de = -e.bd = d.eb = -b.de, \text{ Q.E.A.}$$

Therefore

$$\begin{aligned} -\ c &= b.de = e.bd, \\ -ec &\quad\ \ = -bd, \end{aligned}$$

or

$$bd = ec;$$

and for the same reasons,

$$be = cd.$$

Hence, in each of the following lines, any two equations are implied in the third:—

$$\left.\begin{array}{lll} bc = de, & be = cd, & bd = -ce; \\ bc = fg, & bg = cf, & bf = -cg; \\ de = fg, & dg = ef, & df = -eg. \end{array}\right\} \quad . \quad . \quad . \quad (\text{A.})$$

It is necessary that three similar lines deduced from any three triplets of the system, beginning with another imaginary than a, should be consistent with these. Now it is readily seen, that the system of seven triplets, of which three are *abc*, *ade*, *afg*, must perforce contain either *bdf* or *ceg*; and the reader will convince himself with less pains than the perusal of a demonstration would cost him, that the above conditions can, and thus only can, be fulfilled, by giving to that triplet of these two, which is found in the system of seven, a sign contrary to that of the remaining three. Hence all possible arrangements, if *abc*, *ade*, *afg* are in the system, are comprised in the two following types, each of four systems:—

$$\pm\left\{\begin{array}{l} +abc \\ +ade \\ +afg \end{array}\right\} \pm \left\{\begin{array}{l} -bdf+cdg \\ +beg+cef \end{array}\right\}; \quad \pm\left\{\begin{array}{l} +abc \\ +ade \\ +afg \end{array}\right\} \pm \left\{\begin{array}{l} +bdg+cdf \\ +bef-ceg \end{array}\right\};$$

of which eight systems any one will satisfy the test invented by Mr. Cayley, of which mention has been made, and which is proved by the preceding considerations to be both necessary and complete for a system of seven triplets.

The congruity of any of these, *e.g.* of those in the first type, may be tested by taking some other order for alphabetical; for instance,

$$\pm\left\{\begin{matrix}+fga\\+fce\\+fdb\end{matrix}\right\}\pm\left\{\begin{matrix}-gcd+acb\\+geb+aed\end{matrix}\right\},$$

which is still of the same type as before.

One of these eight systems, being adopted, will furnish the non-linear conditions among the seven imaginaries, by the aid of which the product of two biquaternions is reduced to a biquaternion. The system of the first type, having the two upper signs, for example, expresses that

$$a_0=b_0\,c_0=d_0\,e_0=f_0\,g_0,$$

and

$$d_0=e_0\,a_0=-f_0\,b_0=g_0\,c_0.$$

Hence in the biquaternion $Q_{a_{,,}}$, formed by this system,

$$a_{,,}=aw_{,}+wa_{,}+(bc_{,}-cb_{,})+(de_{,}-ed_{,})+(fg_{,}-gf_{,}),$$

and

$$d_{,,}=dw_{,}+wd_{,}+(ea_{,}-ae_{,})-(fb_{,}-bf_{,})+(gc_{,}-cg_{,});$$

for these are exactly the terms affected by a_0 and d_0 in the product $Q_a\,Q_{a_{,}}$. But if we adopt the system of the second type which has the two lower signs, we should form the same functions thus,

$$a_{,,}=aw_{,}+wa_{,}-(bc_{,}-cb_{,})-(de_{,}-ed_{,})-(fg_{,}-gf_{,})$$
$$d_{,,}=dw_{,}+wd_{,}-(ea_{,}-ae_{,})-(gb_{,}-bg_{,})-(fc_{,}-cf_{,});$$

where the given numbers $ww_{,}\;aa_{,}\ldots gg_{,}$ are in both cases the same.

It thus appears, that a biquaternion $Q_{a_{,,}}$ may be formed, that shall be equal to the product of any two Q_a and $Q_{a_{,}}$, in at least eight different ways; and the eight different values of the product shall be equally congruous and consistent with our definitions; the real quantities in Q_a and $Q_{a_{,}}$ being any numbers, and remaining unchanged in value, sign, or order.

Let Q_a and $Q_{a_{,}}$ now be pluquaternions of more than seven imaginaries. If $Q_a\,Q_{a_{,}}=Q_{a_{,,}}$, there must be some complete system of triplets into which the $(6m+1)$ or $(6m+3)$ imaginaries are thrown. Let $a_0\,d_0\,e_0$ and $a_0 f_0\,g_0$, both of one sign, be any two of these; then, neglecting subindices, $\pm dg$ will be equal to some sixth, and $\pm eg$ to some seventh imaginary, say

$$\pm dg=c, \qquad \pm eg=b.$$

It follows that

$$cd=\pm be$$
$$cb=\pm ed=\mp a.$$

If then cdg and beg are of the same sign, abc is a triplet in the pluquaternion system, of the same sign with ade and afg; and since by equations (A.), which here apply,

$$dg=ef \text{ and } eg=fd,$$

it is plain that the imaginaries $abcdefg$ form some one of eight complete systems of seven triplets. If cdg is of a different sign from beg, acb is a triplet in the system, of the same sign with ade and afg; so that, if we put b' for c, and c' for b, we know that $ab'c'defg$ form one of the same eight systems.

Let it be either of these two,

$$\begin{matrix}+abc\\+ade\\+afg\end{matrix}\pm\left\{\begin{matrix}-bdf+cdg\\+beg+cef\end{matrix}\right\};$$

and let $+ahi$ be any fourth triplet in the system beginning with a. Then the following are proved to coexist, like the equations (A.), being deduced from the equal values of a:

$$\left.\begin{matrix} bc=hi, & bi=ch, & bh=-ci;\\ de=hi, & di=eh, & dh=-ei,\\ fg=hi, & fi=gh, & fh=-gi.\end{matrix}\right\}\quad .\ .\ .\ .\quad \text{(B.)}$$

Let now k be the imaginary equivalent to bi in the system. We have then, since $bi=ch$,

$$\mp bdf=bik,$$
$$\mp df=ik,$$
$$\mp dk=fi=gh;$$

and

$$\pm cdg=chk,$$
$$\pm dg=hk,$$
$$\pm dk=gh;$$

or

$$dk=-dk; \text{ Q.E.A.}$$

If the system of seven triplets be of the other type, or one of these,

$$\begin{matrix}+abc\\+ade\\+afg\end{matrix}\pm\left\{\begin{matrix}+bdg+cdf\\+bef-ceg\end{matrix}\right\},$$

we have

$$\pm bef = bik,$$

$$\pm\ ek = fi = gh;$$

and

$$\mp ceg = chk,$$

$$\mp\ ek = gh;$$

or

$$ek = -ek; \text{ Q.E.A.}$$

It is therefore impossible that $b_0 i_0$ or $c_0 h_0$ should be equal to k_0, or to any other ninth imaginary; and the same contradiction is involved in the supposition that $c_0 i_0$, $d_0 i_0$, $e_0 i_0$, $f_0 i_0$ or $g_0 i_0$, or their equivalent duads in (B.), are any one of them equal to a single imaginary. Each of these must therefore of necessity appear, by itself or by its equivalent duad, in the product of the pluquaternions Q_a and $Q_{a_{,}}$. Let these, *e. g.* be pluquaternions of nine imaginaries; then their product must be of the form

$$\begin{aligned}&(w + a_0 a + b_0 b + e_0 e + f_0 f + g_0 g + h_0 h + i_0 i)\\ &\times (w_{,} + a_0 a_{,} + b_0 b_{,} + c_0 c_{,} + d_0 d_{,} + e_0 e_{,} + f_0 f_{,} + g_0 g_{,} + h_0 h_{,} + i_0 i_{,})\\ &= w_{,,} + a_0 a_{,,} + b_0 b_{,,} + c_0 c_{,,} + d_0 d_{,,} + e_0 e_{,,} + f_0 f_{,,} + g_0 g_{,,} + h_0 h_{,,} + i_0 i_{,,}\\ &\quad + b_0 i_0 \mathrm{B}_i + c_0 i_0 \mathrm{C}_i + d_0 i_0 \mathrm{D}_i + e_0 i_0 \mathrm{E}_i + f_0 i_0 \mathrm{F}_i + g_0 i_0 \mathrm{G}_i;\end{aligned}$$

i. e. $Q_a Q_{a_{,}} = Q_{a_{,,}} + R_1$, or the product is a pluquaternion of $(2 \cdot 1 + 7)$ imaginaries, and an additional function of imaginaries of a different form, which, for distinction's sake, we may call the condition-function R_1, the number of its terms being the number of conditions to be satisfied among the real quantities of Q_a and $Q_{a_{,}}$, in order that the product $Q_a Q_{a_{,}}$ may be a pluquaternion. In the case of nine imaginaries, there are six of these conditions, viz. $R_1 = 0$, imposed upon the sixteen numbers $bb_{,} cc_{,} \ldots ii_{,}$. The terms $h_{,,}$ and $i_{,,}$ of the pluquaternion $Q_{a_{,,}}$, are formed with the aid of the triplet $a_0 h_0 i_0$; and B_i, C_i, &c. are given by the equations (B.), thus:

$$\mathrm{B}_i = bi_{,} - ib_{,} + ch_{,} - hc_{,}, \qquad \mathrm{C}_i = ci_{,} - ic_{,} + hb_{,} - bh_{,},$$

$$h_{,,} = hw_{,} + wh_{,} + (ia_{,} - ai_{,}), \quad i_{,,} = iw_{,} + wi_{,} - (ah_{,} - ha_{,}).$$

Since $+a_0 b_0 = c_0$, and $+a_0 h_0 = i_0$, $c_0 h_0 = -i_0 b_0$ gives

or

whence

$$\left.\begin{aligned} a_0 b_0 h_0 &= -a_0 h_0 b_0,\\ b_0 h_0 &= -h_0 b_0;\\ c_0 i_0 &= -i_0 c_0;\end{aligned}\right\} \quad \ldots\ldots \quad \text{(C.)}$$

and the like property can be shown to belong to all the duads containing h_0 or i_0, and one of the seven imaginaries $a_0 b_0 c_0 d_0 e_0 f_0 g_0$, none of which duads are in the system of seven triplets.

From what has preceded, we conclude that it is impossible for pluquaternions of more than seven imaginaries to have a product of the same form, if there are no limitations upon the values of the real quantities, ww_i, $aa_i \ldots . rr_i$.

If we now suppose that there are eleven imaginaries, we can form a fifth triplet beginning with a_0, viz. $a_0 k_0 l_0$; and it can be proved, as in the case of nine imaginaries, that $b_0 l_0$, $c_0 l_0$, $d_0 l_0$, $e_0 l_0$, $f_0 l_0$, $g_0 l_0$, have each of them an equivalent duad, but that they cannot any of them be put equal to any single imaginary, without a contradiction. The equal values of a_0, $h_0 i_0$ and $k_0 l_0$, give also

$$h_0 i_0 = k_0 l_0, \quad h_0 l_0 = i_0 k_0, \quad h_0 k_0 = -i_0 l_0; \quad . \quad \text{(D.)}$$

of which neither $h_0 l_0$ nor $h_0 k_0$ can be equal to any of the seven $a_0 b_0 c_0 d_0 e_0 f_0 g_0$; for this equality is what we have just shown to be impossible; nor can $h_0 l_0$ be equal to either i_0 or k_0, nor $h_0 k_0$ to either i_0 or l_0. Wherefore the product of two pluquaternions of eleven $(=2 \cdot 2 + 7)$ imaginaries must of necessity be of the form

$$Q_a Q_{a_i} = Q_{a_{ii}} + R_2,$$

where the condition-function R_2 contains, besides the six terms of R_1, the six following,

$$+b_0 l_0 B_l + c_0 l_0 C_l + d_0 l_0 D_l + e_0 l_0 E_l + f_0 l_0 F_{l_0} + g_0 l_0 G_{l_0},$$

with the additional pair of terms $+h_0 l_0 H_l + i_0 l_0 I_l$.

The six B_l, &c. differ from the six B_i, &c. only in this, that for the real quantity h or i in the latter, is substituted k or l in the former; and, in the same way, for h_i or i_i is put k_i or l_i. H_l and I_l are formed from equations (D.), and k_{ii} and l_{ii}, the two additional terms in $Q_{a_{ii}}$, with the aid of the triplet $a_0 k_0 l_0$.

It can also be shown, as in (C.), that the additional duads in R_2, $b_0 l_0$, &c., have the property that $b_0 l_0 + l_0 b_0 = 0$.

Suppose next that we have thirteen $(=2 \cdot 3 + 7)$ imaginaries. There is now nothing to hinder us from forming another system of seven triplets, with *ahiklmn*, of the same type or not as the former. We may, *e. g.* have the fourteen triplets following:

$$\begin{array}{ll} +abc & +ahi \\ +ade - bdf + cdg & +akl - hkm + ikn \\ +afg + beg + cef; & +amn + hln + ilm; \end{array}$$

for, although we have proved that no one of the seven $ab \ldots g$ can form a tenth triplet with any two of *hikl*, such a triplet *can* be made with two of these and *any twelfth* imaginary n, or m. In the case of thirteen imaginaries, then, we have the product

$$Q_a Q_{a_{,}} = Q_{a_{,,}} + R_3,$$

where R_3 contains, besides the six $b_0 i_0 B_i$ . . $g_0 i_0 G_i$, and the six $b_0 l_0 B_l \dots g_0 l_0 G_l$, another six, $b_0 n_0 B_n \dots g_0 n_0 G_n$; but it contains not the pair $h_0 l_0 H_l$ and $i_0 l_0 I_l$, because the terms (H_l) are now part of the function $n_{,,}$, and the terms (I_l) of $m_{,,}$, by virtue of our new triplets. It is hardly necessary to observe, that the six duad equivalents of $b_0 n_0 \dots g_0 n_0$ are deduced from equating $m_0 n_0$ to the values of a_0 in the first system of seven, as in equations (B.). The reasoning (C.) applies here as before.

If there are fifteen $(=2\cdot 4+7)$ imaginaries $ab \dots nop$, R_4, in the product

$$Q_a Q_{a_{,}} = Q_{a_{,,}} + R_4,$$

will contain, supposing that we add the triplet aop to the preceding fourteen, the eighteen terms of R_3, and the two additional sixes, $b_0 p_0 B_p \dots g_0 p_0 G_p$, and $h_0 p_0 H_p$, $i_0 p_0 I_p$, ... $n_0 p_0 N_p$.

In the product of two pluquaternions of seventeen, R_5 has the thirty terms of R_6, and (aqr being the sixteenth triplet) the two sixes $b_0 r_0 B_r \dots g_0 r_0 G_r$, and $h_0 r_0 H_r \dots n_0 r_0 N_r$, besides the additional pair $o_0 r_0 O_r$ and $p_0 r_0 P_r$. And in the product

$$Q_a Q_{a_{,}} = Q_{a_{,,}} + R_6$$

of two pluquaternions of nineteen, the condition-function R_6 contains, besides the preceding seven sixes, the two additional sixes $b_0 t_0 B_t \dots g_0 t_0 G_t$, and $h_0 t_0 H_t \dots n_0 t_0 N_t$; but not the pair $o_0 r_0 O_r$ and $p_0 r_0 P_r$, as the terms (O_r) are now part of the function $t_{,,}$, and (P_r) of $s_{,,}$; the twenty quantities $w_{,,} a_{,,} \dots s_{,,} t_{,,}$ being formed by the twenty-one triplets,

$$\begin{array}{lll}
+abc & +ahi & +aop \\
+ade-bdf+cdg & +akl\ -hkm+ikn & +aqr-oqs+pqt \\
+afg+beg+cef; & +amn+hln+ilm; & +ast+ort+prs;
\end{array}$$

if we suppose that the three systems are identical in form. But this is not in anywise necessary, for any one of them, *e.g.* the third, may be any one of eight systems containing the three triplets $a_0 o_0 p_0$, $a_0 q_0 r_0$, $a_0 s_0 t_0$, all of one sign: that is, the function $a_{,,}$, indicated by the imaginary common to all the twenty-one triplets, may have any one of the eight values,

$$\begin{aligned}
a_{,,} = aw_{,} + wa_{,} &\pm (bc_{,} - cb_{,}) \pm (de_{,} - ed_{,}) \pm (fg_{,} - gf_{,}) \\
&\pm \{hi_{,} - ih_{,}\} \pm \{kl_{,} - lk_{,}\} \pm \{mn_{,} - nm_{,}\} \\
&\pm [op_{,} - po_{,}] \pm [qr_{,} - rq_{,}] \pm [st_{,} - ts_{,}];
\end{aligned}$$

where the similar brackets have all the same sign. Any other function, as $q_{,,}$, will have the form, if we take the *first* of our two types,

$$q_{,,}=qw_{,}+wq_{,}\pm[ra_{,}-ar_{,}]\pm\{\overline{so_{,}-os_{,}}-\overline{tp_{,}-pt_{,}}\};$$

but *which* of these *four* shall be $q_{,,}$, can only be determined when the system of seven triplets, to which $q_{\circ}$ belongs, shall have been exactly defined. We may thus have any number of complete systems of seven, having *one* common imaginary.

It is obvious that the reasoning (C.) applies to every duad coefficient in R_4, R_5, R_6; and it is easily shown to be true that the property

$$m_{\circ}n_{\circ}+n_{\circ}m_{\circ}=0$$

belongs to every pair $m_{\circ}\,n_{\circ}$ in the product of any two pluquaternions, whether that product be a pluquaternion or not, provided that that product be formed consistently with a system of congruous triplets. And all this is the inevitable consequence of our first definitions of the imaginaries, and of the supposition that Q_a and $Q_{a,}$ have, *if possible*, a product of the same form.

The preceding analysis shows that

$$\begin{aligned}
R_1&=6\cdot 1\\
R_2&=6\cdot 2+2\\
R_3&=6\cdot 3\\
R_4&=6\cdot 4+6\cdot 1\\
R_5&=6\cdot 5+6\cdot 2+2\\
R_6&=6\cdot 6+6\cdot 3\\
R_7&=6\cdot 7+6\cdot 4+6\cdot 1\\
R_8&=6\cdot 8+6\cdot 5+6\cdot 2+2,\\
&\&c. \qquad \&c.,
\end{aligned}$$

the law being obvious.

Generally, if we form the product

$$Q_a\,Q_{a,}=Q_{a,,}+R_m$$

of two pluquaternions of $(2m+7)$ imaginaries, the number of terms in the condition-function R_m, if $m=3k+h$ (k not <0; h not <0, not >2), is always $[(3k)^2+3k\cdot(2h+3)+h\cdot(h+5)]$.

Since each of the imaginary duads in R_m must of necessity appear, either by itself or by its equivalent, in the product of Q_a and $Q_{a,}$, it is impossible that this product should contain fewer than $[(3k)^2+3k\cdot(2h+5)+h(h+7)+8]$ terms; and if

R'_m be any function exhibiting a less number of imaginaries than R_m, the equation

$$Q_a Q_{a_\prime} = Q_{a_{\prime\prime}} + R'_m$$

is a contradiction, if $ww_\prime\, aa_\prime \ldots rr_\prime$ are subjected to no limitations.

We shall next inquire how many possible forms there are of the product of two pluquaternions Q_a and $Q_{a_\prime}$, or how many different sets, $w_{\prime\prime}\, a_{\prime\prime}\, b_{\prime\prime}$, &c., functions of the $4n$ given positive numbers $ww_\prime\, aa_\prime \ldots rr_\prime$, $Q_{a_{\prime\prime}}$ and the condition-function R can be made, consistently with our definitions, to exhibit. Here it may be useful to remark, that $w_{\prime\prime}$ is invariable, and that the two first terms, which contain w and $w_\prime$, of any one of $a_{\prime\prime}\, b_{\prime\prime} \ldots r_{\prime\prime}$, are also invariable, through all the changes of form of the pluquaternion $Q_{a_{\prime\prime}}$, so long as the values and signs of the given real numbers are unaltered.

We take first $Q_{a_{\prime\prime}}$, the product of two biquaternions Q_a and $Q_{a_\prime}$. Considering the set (bc, de, fg) as one with (bc, fg, de) and with (cb, ed, gf), but distinct from (cb, dc, fg) and from (bd, ce, fg), we can form sixty distinct sets of three duads with the six imaginaries $b_\circ c_\circ d_\circ e_\circ f_\circ g_\circ$. Prefixing $a_\circ$ to the three duads of any set $(c_\circ b_\circ\, d_\circ e_\circ\, f_\circ g_\circ)$, the resulting triplets, $a_\circ c_\circ b_\circ$, $a_\circ d_\circ e_\circ$, $a_\circ f_\circ g_\circ$, can be completed in eight different ways into a complete system of seven. For example, the three just written, considered as all of one sign, give for $a_{\prime\prime}$ the two following values, taking all the upper or all the lower signs together:

$$a_{\prime\prime} = aw_\prime + wa_\prime \pm (cb_\prime - bc_\prime) \pm (de_\prime - ed_\prime) \pm (fg_\prime - gf_\prime);$$

to either of which values can be appended four distinct sets, $b_{\prime\prime}\, c_{\prime\prime}\, d_{\prime\prime}\, e_{\prime\prime}\, f_{\prime\prime}\, g$, two of either type.

Therefore the biquaternion $Q_{a_{\prime\prime}}$, the product of two given ones, Q_a and $Q_{a_\prime}$, has 480 distinct forms, each exhibiting a different set of eight numbers, $w_{\prime\prime}\, a_{\prime\prime}\, b_{\prime\prime}\, c_{\prime\prime}\, d_{\prime\prime}\, e_{\prime\prime}\, f_{\prime\prime}\, g_{\prime\prime}$.

Let it now be supposed that two pluquaternions of nine, Q_a and $Q_{a_\prime}$, are to be multiplied together. The product $Q_{a_{\prime\prime}} + R_1$ is determined completely, as soon as an eighth triplet is added to a given complete system of seven.

Thus, if we add to any one of the eight systems which give either of the last written values of $a_{\prime\prime}$, the triplet $a_\circ h_\circ i_\circ$ of the same sign with $a_\circ c_\circ b_\circ$, we obtain for $a_{\prime\prime}\, h_{\prime\prime}\, i_{\prime\prime}$ the two sets of values,

$$\begin{aligned}
a_{\prime\prime} &= aw_\prime + wa_\prime \pm (cb_\prime - bc_\prime) \pm (de_\prime - ed_\prime) \pm (fg_\prime - gh_\prime) \pm (hi_\prime - ih_\prime),\\
h_{\prime\prime} &= hw_\prime + wh_\prime \pm (ia_\prime - ai_\prime),\\
i_{\prime\prime} &= iw_\prime + wi_\prime \pm (ah_\prime - ha_\prime);
\end{aligned}$$

and the function R_1, the same for either set, is deduced from the equality of the four values of a_0. If the eighth triplet is $a_0 i_0 h_0$, we obtain the two sets

$$a_{,,}=aw_{,}+wa_{,}\pm(cb_{,}-bc_{,})\pm(de_{,}-ed_{,})\pm(fg_{,}-gf_{,})\pm(ih_{,}-hi_{,}),$$
$$h_{,,}=hw_{,}+wh_{,}\pm(ai_{,}-ia_{,}),$$
$$i_{,,}=iw_{,}+wi_{,}\pm(ha_{,}-ah_{,});$$

and a different function, R_1, is given by the equal values of a_0, and is the same for either set.

To each of these four sets of $a_{,,}\, h_{,,}\, i_{,,}$ can be appended four distinct sets of $b_{,,}\, c_{,,}\, d_{,,}\, e_{,,} f_{,,}\, g_{,,}$, two from either of our two types.

But it is not necessary that either $a_0 h_0 i_0$ or $a_0 i_0 h_0$ should be the eighth triplet which is added to the given complete system of seven. It may be either $f_0 h_0 i_0$ or $f_0 i_0 h_0$, for example. Either of these will add to the value of $f_{,,}$, which is exhibited by the biquaternion $Q_{a_{,,}}$ (made by the given system of seven), a binomial function of $hi\, h_{,}i_{,}$, while no such addition will appear now in $a_{,,}$; and the values of $h_{,,}$ and $i_{,,}$ will now contain f and $f_{,}$, but not a or $a_{,}$. The function R_1 will in either of these two cases be formed by the comparison of the four equal values of f_0.

It is clear then, that, if $h_0 i_0$ be the two imaginaries which are excluded from the given complete system of seven, we can add to any one of the seven terms $(a_{,,} \ldots g_{,,})$ of any one of the 480 biquaternions that can be made with the given complete system of seven, either of the quantities $\pm(hi_{,}-ih_{,})$, and append to the eight quantities $w_{,,}\, a_{,,} \ldots g_{,,}$, thus increased, the corresponding values of $h_{,,}\, i_{,,}$ and R_1; that is, we can construct the sixteen functions $w_{,,} a_{,,}\, b_{,,}$, &c. in the product $Q_{a_{,,}}+R_1$, in $7\cdot2\cdot480$ ways. If $a_0\, b_0$ were the excluded duad, we should have another series of as many different sets of $w_{,,}\, a_{,,}\, b_{,,}$, &c.; and, in fact, the total number of ways of forming the product of two pluquaternions of nine is $36\cdot7\cdot2\cdot480=241{,}920$.

Since the case of fifteen or (2^4-1) imaginaries possesses some interest, appearing, as it does, to follow next in order after those of (2^2-1) and (2^3-1), it may not be considered mere learned trifling, to assign the number of ways in which $(Q_{a_{,,}}+R_4)$, the product of two pluquaternions of fifteen, can be constructed. This product is completely determined, when to two given complete systems of seven triplets is added a fifteenth triplet, containing an imaginary which is common to both the systems of seven. Let a_0 be that imaginary; and of the fourteen, $b_0\, c_0 \ldots m_0\, n_0$, take any six, $b_0\, c_0\, d_0\, e_0 f_0\, g_0$, and any other six, $h_0\, i_0\, k_0\, l_0\, m_0\, n_0$, $o_0 p_0$ being the duad excluded from both the sixes. With a_0 and the first six, form a complete system of seven triplets, of which the three containing a_0 shall be any three, $a_0 b_0 c_0$, $a_0 d_0 e_0$, $a_0 f_0 g_0$; and with a_0 and the other six, form also such a system, containing *e. g.* $a_0 h_0 i_0$, $a_0 k_0 l_0$, $a_0 m_0 n_0$. Then $a_{,,}$ will be one of the eight,

$$a_{,,} = aw_{,} + wa_{,} \pm (bc_{,} - cb_{,}) \pm (de_{,} - ed_{,}) \pm (fg_{,} - gf_{,})$$
$$\pm \{hi_{,} - ik_{,}\} \pm \{kl_{,} - lk_{,}\} \pm \{mn_{,} - nm_{,}\} \pm [op_{,} - po_{,}],$$

where the *like* brackets have all the *same* sign. If $(Q_{a_{,,}} + R_4)$ should reduce itself to a biquaternion, by the vanishing of all the real quantities in Q_a and $Q_{a_{,}}$ except the twice eight $(ww_{,} aa_{,} bb_{,} cc_{,} dd_{,} ee_{,} ff_{,} gg_{,})$, we should have before us in the product only the eight functions $(w_{,,} a_{,,} b_{,,} c_{,,} d_{,,} e_{,,} f_{,,} g_{,,})$ belonging to that one of the 480 forms of such biquaternion, which is determined by the system of seven triplets made with $a_0 b_0 c_0 d_0 e_0 f_0 g_0$; and if all the real quantities were to vanish from Q_a and $Q_{a_{,}}$ except the twice eight $(ww_{,} aa_{,} hh_{,} ii_{,} kk_{,} ll_{,} mm_{,} nn_{,})$, we should have before us one of the 480 forms of another biquaternion, determined by the triplets made with $a_0 h_0 i_0 k_0 l_0 m_0 n_0$. And, since any one of the latter 480 may be combined with any one of the former 480, and since in every case the resulting value of $a_{,,}$ may receive either of the increments $\pm [op_{,} - po_{,}]$, according as the triplet $a_0 o_0 p_0$ is supposed of one sign or the other; it is plain, that we can form our product $(Q_{a_{,,}} + R_4)$, so that $o_0 p_0$ shall be the duad excluded from both the systems of seven, and that the two sixes shall be any pair of sixes, from which a_0 o_0 and p_0 are excluded, and that a_0 shall be the imaginary common to all the fifteen triplets, in $2 \cdot (480)^2$ distinct ways; although, be it remarked, the resulting values of $a_{,,}$ abovementioned will not amount to a number so great as this. But *any one* of the thirteen, from which o_0 and p_0 are excluded, may be the imaginary common to all the triplets, and determine, in a different way, to which of $a_{,,}$ $b_{,,}$ $c_{,,}$, &c. the increment $\pm [op_{,} - po_{,}]$ shall be added; and we may combine any one of $15 \cdot 7$ excluded pairs $(o_0 p_0)$ with any one of $11 \cdot 42$ pairs of sixes which exclude $(o_0 p_0)$. Consequently, the total number of ways in which the forty-six functions $w_{,,} a_{,,} b_{,,} \ldots p_{,,}$ may be formed, in the product of two given pluquaternions of fifteen imaginaries, is $2 \cdot (480)^2 \cdot 13 \cdot 15 \cdot 7 \cdot 11 \cdot 42$, or 290,580,480,000.

In all these cases, the condition-function R_m is easily formed, being given by the comparison of the fifteen equal values of that imaginary which is common to all the fifteen triplets; and the values $o_{,,}$ and $p_{,,}$, indicated by the pair of imaginaries excluded from the two complete systems of seven, are given by the single triplet, which is added to those systems.

[To be continued.]

LXVII. *On the Duration of a Solar Spot.*
By W. Pringle, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It is stated by Dr. Dick and other writers that "no spot has been known to last longer than one that appeared in the year 1676, which continued upon the sun for seventy days." I am inclined to think that a large spot now going off the sun's disc (21st Nov.), has lasted still longer.

On the 19th of August last I observed a double spot, or two adjacent spots, near the sun's eastern verge, which as they advanced increased in size, and on the 23rd were visible to the naked eye as one spot. Their longest diameters were 27,000 and 31,000 miles respectively, being each about a third less in breadth, embracing the penumbræ. They were about 10,000 miles apart, though for the first two or three days they appeared to be linked together by a curved chain of minute spots or shallows at their northern extremities, but which became eventually absorbed into the penumbra of the largest of the two. They were both of an oval form, and were followed till the 28th or 29th, when the nucleus of one had divided into several parts. They probably disappeared on the 31st of August.

On September 16 I remarked a large spot (at 4 p.m.) so far advanced on the sun's eastern limb as distinctly to exhibit several black nuclei within the edges of the penumbræ, but no great nucleus in the centre; and on the 17th, when it had come more round, this peculiarity became more apparent, the interior sides being studded with small dark spots, while the centre showed nothing but one vast clouded space or shallow. As I had been looking for the reappearance of the two spots of August, it occurred to me that they might have become conjoined, the conjunction of the two penumbræ forming the central space of the spot now advancing. There being no other traces of them, I conceived I was justified in this conclusion. The smooth contour of the two oval spots had become since the contact a vast irregular polygon, as if by the concussion of two forces the penumbræ had been dashed or shaken into angular protrusions. The entire spot on the 17th measured about 50,000 miles in its longest dimensions. A very fair engraving of its outline appeared in the Illustrated London News of October 7th. Its size on the 21st was estimated at 60,000 miles: I made it somewhat more afterwards. It was quite perceptible to the naked eye for nearly a week after the 17th. On the 27th it was about half a digit from the sun's western margin, and probably disappeared the day after; but cloudy weather prevented it being followed.

About a fortnight afterwards, on the 13th of October, I descried a long lenticular-shaped spot about a quarter of a digit advanced on the sun's eastern limb. This I had little doubt was the spot on its return; and, as it came on, the general resemblance confirmed its identity. It was, however, much contracted in dimensions, measuring about 44,000 miles when near the sun's centre. The irregular pointed outline was still prominent, though the projections were less than on its previous appearance.

On the 26th of October I saw it as a very dim dot or line, scarcely distinguishable from the line of circumference of the sun's western limb.

On the 9th of November, at some distance north of the point of Mercury's entering the disc, a faint depression, having the appearance of a spot of some size, was visible. This being the very time when the spot of October might be expected to return, I felt confident it was the same; and its further progress confirmed the identity. It was again contracted much in size, being somewhat triangular in shape.

If the inferences above stated be well-founded, the spot will have lasted about ninety-five days. Whether it will yet make another revolution, or half a one, remains to be seen; but I shall not probably have it in my power to make observations at the period of its next appearance. It may be looked for on the 4th or 5th of December.

In many respects this spot was a remarkable one, and in all likelihood did not escape the observation of the illustrious author of the Cape Observations, whose chapter on solar spots is so interesting.

I should be happy if you think this worth noticing in your Journal as a report of some particulars to which my attention was directed, and which more profound observers may have overlooked.

The opinion of Dr. Dick, author of The Sidereal Heavens, &c., to whom I submitted the observations of August, September and October, that in all probability the spots were identical, according to the grounds stated, has the more emboldened me to address you.

I am, Gentlemen,
Your very obedient Servant,
Edinburgh, Nov. 21, 1848. W. Pringle.

LXVIII. *Further investigation of the Nature of Aërial Vibrations. By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

BEFORE I proceed with the inquiry carried on in several preceding Numbers of this Journal, I wish briefly to notice the views put forth by Mr. Stokes in the November Number, respecting a supposed remarkable difficulty in the Theory of Sound which he says that I have pointed out. What he alludes to I have not myself called a difficulty, nor do I so regard it. By an investigation contained in the Philosophical Magazine for last April, I found that the general character of aërial vibrations is non-divergence, and that the theoretical velocity of sound is different from that usually adopted. Mr. Airy urged against these conclusions, that my equations represent a particular case of the propagation of plane-waves: in answer to which I proved, by a *reductio ad absurdum*, that plane-waves are physically impossible. This proof, which forms the subject of Mr. Stokes's remarks, is given in the Philosophical Magazine, S. 3, vol. xxxii., from line 16 of page 496 to line 12 of page 497. The absurdity to which the hypothesis of plane-waves conducts is, that the points of maximum velocity and of no velocity in the same wave may be at the same point of space at the same time. Mr. Airy did not reply. Mr. Stokes, however, undertakes to maintain plane-waves by the following considerations. He first finds that a point of maximum velocity of a wave travels at a rate different from that of a point of no velocity, and consequently that there is at least great *danger* of one overtaking the other. When this absurdity is on the point of being consummated, the wave, as he *conceives* (for there is nothing in the analysis to indicate such a result), is converted into a *breaker*. What the subsequent motion is Mr. Stokes thinks it would not be worth while to inquire, but proceeds to support by considerations, which it is not necessary to particularize, the possibility of the physical existence of a surface of discontinuity at the position where the abrupt alteration of the character of the wave takes place. How then stands the question? According to my reasoning plane-waves are physically impossible; according to Mr. Stokes's, plane-waves are *wholly incompatible with the transmission of articulate and musical sounds*. The only conclusion from either result is, that the hypothesis of plane-waves is inadmissible.

It may, however, be urged that spherical waves are physi-

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[*] Communicated by the Author.

cally possible; and that as these become plane-waves at an infinite distance from the centre, the latter are also physically possible. I have already met this argument in the communication above referred to; but as the reasoning is given briefly, and may possibly have been overlooked, I will repeat it here. I take the results of the hypothesis of spherical waves as they are given in Poisson's *Traité de Mécanique* (vol. ii. p. 706, 2nd edit.), and as they are commonly admitted. The pressure p being $a^2(1+\sigma)$, the following expression is obtained for the condensation σ at the distance r from the centre at the time t,

$$\sigma=\frac{f(at-r)}{ar};$$

and it is stated that there is no condensation wherever r is greater than $at+\epsilon$, and less than $at-\epsilon$, 2ϵ being the breadth of the sonorous undulation. Hence, supposing 2ϵ very small compared to r, and putting for r outside the function, its value at corresponding to the middle of the wave, the quantity of matter existing at any time in the wave beyond what would occupy the same space in the quiescent state of the fluid, is very nearly

$$4\pi a^2t^2.\frac{\int f(at-r)\,dr}{a^2t},$$

the integral being taken from $r=at-\epsilon$ to $r=at+\epsilon$. Calling A the constant value of this integral, the expression for the quantity of matter becomes $4\pi At$. Hence the *matter increases in quantity with the time*! Now the very equations from which this result is derived are founded on the supposition that the quantity of matter is constant. There is consequently no difficulty here which any physical considerations can explain, but strictly a *reductio ad absurdum*, which necessitates the important conclusion that the hypothesis of spherical waves is inadmissible. The physical impossibility of plane-waves was proved by the same kind of reasoning; and any attempt to reconcile the contradiction in either case is simply *illogical.* As neither the hypothesis of plane-waves nor that of spherical waves is admissible, the theoretical value of the velocity of sound which rests on those hypotheses necessarily fails of support. I return now to the consideration of non-divergent waves, or, as they may also be called, *ray-vibrations.*

The general equation which gives the density ρ in any instance of fluid motion, the velocity V being known, is

$$a^2\,\text{Nap. log}\,.\,\rho+\int\frac{dV}{dt}\,ds+\frac{V^2}{2}=F(a_1b_1c_1t).$$

As the integration, for reasons I have elsewhere insisted upon, must generally be taken along a line of motion, the arbitrary function added contains the co-ordinates a_1, b_1, c_1 of a given point of this line. Supposing $udx+vdy+wdz$ to be an exact differential $(d\psi)$, we have $\mathrm{V}=\frac{d\psi}{ds}$. Hence

$$a^2.\,\mathrm{Nap.}\log\rho+\frac{d\psi}{dt}+\frac{\mathrm{V}^2}{2}=\mathrm{F}(a_1, b_1, c_1, t). \quad . \quad (1.)$$

In the instance of motion before us, the axis of z is a line of motion. Also

$$\psi=f\phi, \quad \frac{d\psi}{dt}=f\frac{d\phi}{dt},$$

and for a point on that line

$$\mathrm{V}=\frac{d\psi}{dz}=f\frac{d\phi}{dz}.$$

Consequently,

$$a^2\,\mathrm{Nap.}\log\rho+f.\frac{d\phi}{dt}+\frac{f^2}{2}\cdot\frac{d\phi^2}{dz^2}=\mathrm{F}(a_1, b_1, c_1, t). \quad . \quad (2.)$$

I shall now proceed to investigate a general equation which must be satisfied whenever a given state of density is propagated with a constant velocity, for the purpose of ascertaining whether the propagation along the axis of the ray-vibrations obeys this law. If $a_1 =$ the constant rate of propagation, and ds be the increment of a line s drawn in the direction of propagation, it is clear that the following equation must be satisfied,

$$\frac{d\rho}{dt}+a_1\frac{d\rho}{ds}=0,$$

because the integral of this equation is $\rho=\mathrm{F}(s-a_1t)$. Now

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

Hence

$$\frac{d.\rho u}{dx}+\frac{d.\rho v}{dy}+\frac{d.\rho w}{dz}-a_1\frac{d\rho}{ds}=0. \quad . \quad . \quad (3.)$$

This is the general equation sought. For a first application, let us suppose the motion to be that of plane-waves propagated in the direction of z. The equation for this case becomes

$$\frac{d.\rho w}{dz}-a_1\frac{d\rho}{dz}=0,$$

or

$$\frac{dw}{dz}=(a_1-w)\frac{d\rho}{\rho dz}.$$

Now, as is known, for plane-waves

$$\frac{dw}{dz}=a.\frac{d\rho}{\rho dz}.$$

Hence the condition of uniform propagation of a given state of density is *not* satisfied in this case of motion. Next let us take the case of ray-vibrations. We will suppose the axis of z to be the axis of the vibrations, so that $u=0$ and $v=0$. Then equation (3.) becomes

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=(a_1-w)\frac{d\rho}{\rho dz},$$

which, since

$$u=\varphi\frac{df}{dx},\quad v=\varphi\frac{df}{dy},\quad w=f\frac{d\varphi}{dz},\quad f=1,\text{ and }\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}=-\frac{b^2}{a^2},$$

gives

$$-\varphi\frac{b^2}{a^2}+\frac{d^2\varphi}{dz^2}=\left(a_1-\frac{d\varphi}{dz}\right)\frac{d\rho}{\rho dz}.$$

Differentiating now the equation (2.) along the line of motion, we have

$$\frac{a^2d\rho}{\rho dz}+\frac{d^2\varphi}{dt^2}+\frac{d\varphi}{dz}\cdot\frac{d^2\varphi}{dz^2}=0.$$

Hence, substituting in the above equation,

$$-b^2\varphi+a^2\frac{d^2\varphi}{dz^2}+\left(a_1-\frac{d\varphi}{dz}\right)\left(\frac{d^2\varphi}{dzdt}+\frac{d\varphi}{dz}\cdot\frac{d^2\varphi}{dz^2}\right)=0.$$

Consequently, by comparing with the equation which I have called (B.), viz.

$$-b^2\varphi+a^2\frac{d^2\varphi}{dz^2}-\frac{d^2\varphi}{dt^2}-2\frac{d\varphi}{dz}\cdot\frac{d^2\varphi}{dzdt}-\frac{a\varphi^2}{dz^2}\cdot\frac{d^2\varphi}{dz^2}=0,$$

the result is,

$$\frac{d^2\varphi}{dt^2}+a_1\frac{d^2\varphi}{dzdt}+\frac{d\varphi}{dz}\left(a_1\frac{d^2\varphi}{dz^2}+\frac{d^2\varphi}{dzdt}\right)=0.$$

Putting Φ for $\frac{d\varphi}{dt}+a_1\frac{d\varphi}{dz}$, and $\frac{dz}{dt}$ for $\frac{d\varphi}{dz}$, this equation becomes

$$\frac{d\Phi}{dt}+\frac{d\Phi}{dz}\cdot\frac{dz}{dt}=0,$$

or

$$\left(\frac{d\Phi}{dt}\right)=0,$$

and is plainly satisfied by the equation $\Phi=0$. Hence

$$\frac{d\phi}{dt}+a_1\frac{d\phi}{dz}=0,$$

and ϕ is a function of $z-a_1t$. Hence $\frac{d\phi}{dz}$ is also a function of $z-a_1t$, and a given state of the wave as to velocity is propagated uniformly. Now it may be assumed that at some point of the line of motion, viz. at the commencement of the series of vibrations, the velocity and density vanish together; so that $\rho=1$ where $\frac{d\phi}{dz}=0$, and consequently, from what has been shown above, where $\frac{d\phi}{dt}=0$. Hence it appears from equation (2.) that $F(a_1, b_1, c_1, t)=0$. Putting therefore $f=1$ in that equation, it follows, since $\frac{d\phi}{dt}$ and $\frac{d\phi}{dz}$ are each functions of $z-a_1t$, that ρ is a function of the same quantity. Thus a given state of the wave, both as to velocity and as to density, is propagated with the same uniform velocity a_1. This result is in perfect accordance with known facts respecting the transmission of articulate and musical sounds.

I will take this occasion to advert again to a difficulty regarding the supposed effect of the development of heat on aërial waves, which I have pointed out in the Philosophical Magazine, vol. xxxii. pp. 283 and 498. Let the relation between the pressure and density, *inclusive* of the effect of temperature, be expressed by the equation $p=a^2\rho^{1+k}$, as is allowable. Then putting $1+\sigma$ for ρ, and supposing σ small, we have

$$\frac{dp}{dz}=a^2(1+k)(1+\sigma)^k\frac{d\sigma}{dz}$$

$$=a^2(1+k)\frac{d\rho}{dz}(1+k\sigma+\&c.).$$

If now the terms $k\sigma+\&c.$ be neglected, the equation is of the same form as that derived from the relation $p=a^2\rho$, and is consistent with the uniform propagation of a given state of velocity and density. But those terms stand in the way of such a result; and though they are of small amount, yet their effect on the form of the wave is *accumulative*, and must in the end entirely alter its character. This is the nature of the difficulty I have alluded to, which, as we have seen, does not exist in the case of ray-vibrations.

Cambridge Observatory,
Nov. 21, 1848.

LXIX. *Notices respecting New Books.*

The Dodo and its Kindred; or the History, Affinities, and Osteology of the Dodo, Solitaire, and other extinct Birds of the islands Mauritius, Rodriguez and Bourbon. By H. E. STRICKLAND, *M.A., F.G.S., President of the Ashmolean Society, &c.; and* A. G. MELVILLE, *M.D., Edin., M.R.C.S.* London: Reeve, Benham and Reeve, 8 King William Street, Strand. 1848.

AMONG the multitude of sublime inquiries embraced by natural science, there is none more interesting than that of the duration of species of animated beings; none more attractive, through the variety of knowledge demanded for a reasonable theory of the introduction, continuance, and withdrawal of *species*, and of their functions as active agents or passive subjects in the existing œconomy of life; and none more impressively demonstrative of the infinite resources of creative intelligence.

Geology makes known to us a succession of epochs during which there flourished, not only particular species of animals and plants, but also certain genera and families, which, after performing the functions allotted to them during a space of time immeasurable by us, have finally disappeared, and have been succeeded by other and new races as perfectly adapted as their predecessors to the physical conditions under which they were placed. In these vast and mysterious operations we can read and, to a certain degree, comprehend the changes of physical condition, by means of our knowledge of the structure of these ancient extinct forms when compared with their recent analogues; thus deriving a reasonable inference regarding the external circumstances—the existence of land, of sea or of freshwater, of heat, light and air, and of the nature and probable variety of minor and subservient beings.

The work before us relates to beings which have lived almost in our own day, and have disappeared through causes and under circumstances altogether different to those just referred to. Their extinction has been brought about through the agency of a species whose power and predominance over all others is the effect of the combination of a moral and higher intellectual nature, with an amount of activity and physical strength, inferior to those of many contemporary species. The beings referred to, so conspicuous from their size, so defenceless, and so incapable of escaping the assaults of more powerful and predacious species, could only be adapted to the peaceful security of those sunny isles abounding in the richest profusion of vegetable life, where successive generations of these happy birds had continued in the undisturbed enjoyment of an indolent and luxurious life, for which their heavy and bloated forms were only adapted.

Notwithstanding the direct testimony of the first discoverers of these birds, and the evidence of numerous others who had either seen them in their native islands or had beheld specimens brought to Europe, and whose descriptions and figures were published to the world; and notwithstanding the preservation of portions of such specimens, and those the most important in determining the zoological characters of the bird to which they belonged, some naturalists of the past and present century have not scrupled to deny the reality of the Dodo itself, and have attributed the whole to the inventive imagination of voyagers and artists. Scepticism in matters far more important has frequently been productive of the greatest benefit, by exciting attention to the subject in dispute; and thus have we become indebted to the authors of the present work, on which they have exercised much labour and ingenuity, and have brought to their aid the most skilful delineation which modern art could yield.

Much contrariety of opinion has existed among naturalists, who, believing in the reality of the Dodo, have been unsuccessful in their attempts to reconcile the anomalies presented by its form and structure. To both of these subjects our authors have addressed themselves; and they have divided their researches into two distinct parts; the first containing the history and external characters of the birds, by Mr. Strickland; and the second part consisting of a minute osteological investigation, by Dr. Melville.

The first part comprises three chapters; the first being devoted to the consideration of the Dodo from the Mauritius, the second to the Solitaire of the island of Rodriguez, and the third chapter to the allied birds of the Isle of Bourbon. The evidence used in treating of the Dodo is of three kinds,—historical, pictorial and real, agreeably to the plan adopted by a preceding writer, Mr. Broderip. Although the discovery of the Mauritius was made during the first half of the sixteenth century, yet no published account of the island or of its natural history is known prior to the voyage of a Dutch fleet under Admiral Van Neck in 1598.

> "In the published narrative of this voyage it is stated that they found in the island a variety of pigeons, parroquets, and other birds, among which were some which they denominated Walckvögel, the size of swans, with a large head furnished with a kind of hood; no wings, but in place of them three or four small black quills; and the tail consisted of four or five curled plumes of a gray colour. The Dutch sailors called them Walckvögel, or disgusting birds, from the toughness of their flesh, as might be expected in the strongly developed crural muscles of a cursorial bird, though they found the pectoral muscles more palatable."—P. 9.

Accompanying this notice of Van Neck's discovery is a fac-simile of plate 2 of his work, in which the voyagers are represented in bustling activity in their several occupations; and the figure of a large bird resembling the above description forms a conspicuous object in the drawing. By diligent research among the works of the voyagers and writers in natural history of the seventeenth century, especially the earlier ones, a very considerable amount of evidence has been collected and carefully inserted in the work; and though the descriptions and drawings of that period are not so precise and faithful as those of the present day, yet they are sufficiently exact to leave no doubt in the mind of a candid reader both as to the reality and identity of the subject described under different names, as Walckvögel, Dodo, Dodar, Dodaersen, Dronte, besides various specific names under the genera *Cygnus*, *Gallus*, *Raphus*, *Struthio* and *Didus*. In about a century from its first discovery, the Dodo had become extirpated through the direct and indirect agency of man :—

> "In 1644 the Dutch first colonised the island Mauritius; and it is probable that these gigantic fowls, deprived of flight, slow of foot, and useful for food, were speedily diminished in number, and finally exterminated by the thoughtless rapacity of the early colonists. Their destruction would be further hastened, or might be mainly caused, by dogs, cats and swine, which accompany man in his migrations, and are speedily naturalized in the forests. .... That the destruction of the Dodos was completed by 1693, may be inferred from the narrative of Leguat, who in that year remained several months in Mauritius, and enumerates its animal productions at some length, but makes no mention whatever of Dodos."—P. 27.

In the second series of evidences, the pictorial, we find five oil paintings described as extant in different cities of Europe; two of which are in this country, one of them in the British Museum, the other in the Ashmolean Museum at Oxford. "Three of the five pictures bear the name of Roland Savery, an eminent Dutch animal painter of the beginning of the seventeenth century, and one is by John Savery, the nephew of Roland."—P. 28. As to the fifth painting, or the one in the British Museum, it is said, "Unfortunately there is neither name nor date upon the picture; but from the style of execution, and the identity of the design with the picture next to be noticed (that in the Royal collection at the Hague), it may be attributed to one of the two Saverys."—P. 29. These pictures are justly claimed in support of the historical evidence, which is itself partly pictorial from the numerous engravings introduced into the works of several of the earlier writers; and which, though for the most part rude, and in some degree discordant, are highly corroborative of the accompanying written evidence, and also of the independent pictures under consideration.

Of the third kind of evidence, the real or anatomical, there is but a very small number of specimens to appeal to,—a foot in the British Museum, a head and a foot in the Ashmolean Museum, and a mutilated head in the public Museum at Copenhagen, are all the known relics of this extraordinary bird.

Few and imperfect, however, as these latter sources of evidence unfortunately are, we shall see that they possess well-defined characters indelibly stamped upon them, by the deciphering of which the Dodo, with all its strange anomalies, has been assigned a fixed position in its class, after having been tossed about among various families, none of which were content to receive it with much cordiality.

It is not a little startling (so apt are we to be misled by size and general configuration) to find this uncouth and unwieldy bird, after having been deemed too inactive for the *Raptores*, too awkward for the *Gallinaceæ*, and too slow for the *Struthionidæ*, at last settling down among the graceful, gentle, and volatile *Columbidæ*. Yet such is the conclusion formed by a minute examination of the anatomical characters of the detached remains of the bird, in connexion with the other sources of evidence; a conclusion which it is freely conceded had been previously arrived at by Professor Reinhardt of Copenhagen, but which does not detract in the least from the merit of our author, who attained the same point by a different and independent path.

The alliance of the genus *Didus* with the *Columbidæ*, through the genera *Treron, Verrulia* and *Didunculus*, appears to be satisfactorily made out; the points of difference, which are candidly set forth in the work before us, being few and comparatively unimportant.

The second chapter is devoted to the Solitaire, the brevipennate bird of the island Rodriguez,—the *Didus solitarius* of Gmelin, and named by our author *Pezophaps solitaria.*

Historical information respecting this bird is almost entirely confined to the account published in 1708 by Francis Leguat, a French voyager, who furnishes many interesting particulars of its history and habits, from which it appears to have been closely allied to the bird of the Mauritius, though belonging to a distinct genus. The Solitaire has long since disappeared; and the only *real evidences* of its form and structure now existing, are some bones preserved in the Paris Museum, and others in the Museum at Glasgow. The former consist of—

> "a femur, a tarso-metatarsal, a humerus, the medial portion of a sternum, and a portion of the cranium. Unfortunately they are all incrusted uniformly over with stalagmite from $\frac{1}{16}$th to $\frac{1}{20}$th of an inch in thickness, which prevents all examination of the surface of the bones, or any minute description of their structure. They nevertheless supply us with several important elements to guide us in reconstructing the skeleton of this lost bird.
>
> "The Glasgow series of bones are all portions of the hinder extremity, and consist of three femora, a tibia, and two tarso-metatarsal bones. Their appearance as well as their history, proves them to have been obtained under different circumstances from those last mentioned. They still contain nearly the whole of their animal matter, present a glossy surface, considerable specific gravity, and are neither changed in colour nor incrusted with extraneous matter."—P. 53.

Figures of these bones are given in the plates, and a copy of Leguat's engraving of the bird accompanies the text.

The third chapter contains a notice of the brevipennate birds of the Isle of Bourbon, which "is proved by indisputable evidence to have been inhabited by two species of birds, whose inability to fly, and their consequent rapid extinction, brings them into the same category with the Dodo of Mauritius and the Solitaire of Rodriguez." —P. 57. No less than six writers between the years 1613 and 1735 are quoted in support of the existence of one or both species down to the latter date. No specimen or detached bones are known; nor does any figure of the birds accompany the descriptions of these writers. One of them, a Sieur B, in an unpublished manuscript journal belonging to the Zoological Society of London, thus describes the two birds:—

> "1. *Solitaires.* These birds are so called because they always go alone. They are the size of a large goose, and are white, with the tips of the wings

and tail black. The tail-feathers resemble those of an ostrich; the neck is long, and the beak is like that of a woodcock, but larger; the legs like those of turkeys. This bird has recourse to running, as it flies but very little.

"2. *Oiseaux bleus,* the size of Solitaires, have the plumage wholly blue, the beak and feet red, resembling the feet of a hen. They do not fly, but they run extremely fast, so that a dog can hardly overtake them; they are very good eating."—P. 59.

In reviewing the structure of the Dodo in connexion with other anomalies occurring in all classes of the animal kingdom, and glancing at the unphilosophical notions held by some naturalists, of the *imperfection* of certain species presenting such anomalies, the author thus expresses a far more sound and consistent interpretation of the laws of organization.

"It may appear at first sight difficult to account for the presence of organs which are practically useless. Why, it may be asked, does the Whale possess the germs of teeth which are never used for mastication? Why has the *Proteus* eyes when he is especially created to dwell in darkness? And why was the Dodo endowed with wings at all, when those wings were useless for locomotion? This question is too wide and too deep to plunge into at present; I will merely observe, that these apparently anomalous facts are really the indications of laws which the Creator has been pleased to follow in the construction of organized beings; they are inscriptions in an unknown hieroglyphic, which we are quite sure mean *something*, but of which we have scarcely begun to master the alphabet. There appear, however, reasonable grounds for believing that the Creator has assigned to each class of animals a definite type or structure from which he has never departed, even in the most exceptional or eccentric modifications of form. Thus, if we suppose, for instance, that the abstract idea of a Mammal implied the presence of teeth, the idea of a Vertebrate the presence of eyes, and the idea of a Bird the presence of wings, we may then comprehend why in the Whale, the Proteus and the Dodo, these organs are merely *suppressed*, and not wholly *annihilated*."—P. 34.

The second part of the work consists of an elaborate account, by Dr. Melville, of the osteology of the Dodo and Solitaire, so far as a most minute and skilful examination of the few bones extant could be rendered available. This important service to science has been greatly facilitated through the enlightened and liberal feeling of the trustees and curators of the museums of Paris, Glasgow and Oxford, who not only permitted free access to these anatomical treasures, but allowed, in the two former instances, the bones themselves to be conveyed to this country for examination.

The evidence of Dr. Melville derives additional weight from its being free from prejudice, and unshackled by previously declared opinions: to use his own words, "My testimony is that of an impartial observer with no hypothesis to defend, and who claims no share in the merit due to those who have succeeded in restoring the Dodo to its proper rank."—P. 70. The first chapter is occupied by a detailed statement of the structure of the bones of the Dodo, and a comparative view of that of birds belonging to the several orders and families to which this bird has been hitherto regarded as allied. It results that the characters indicate a close alliance with the

Columbidæ; and the examination of the osteology of the Solitaire in the second chapter tends to the same conclusion. The characteristic features of both birds are clearly set forth in a most complete and beautiful suite of lithographic plates executed by three artists (Ford, Dinkel and West).

A History of the Royal Society, with Memoirs of the Presidents. Compiled from Authentic Documents by CHARLES RICHARD WELD, *Esq., Barrister-at-Law, &c.* 2 vols. 8vo. London: J. W. Parker. 1848.

Although the names of Sprat, Birch and Thomson, together with that unkind cutter, Hutton, have long been classed as historians of the Royal Society, we cannot but at once say that Mr. Weld's production is the only one which is really deserving of the designation of a history of that well-known Body. Bishop Sprat's work was published in the early days of the Society, and can only be ranked as an apology for its conduct; Dr. Birch's is an abstract of its papers; Dr. Thomson's is a rapid sketch of the progress made by the several sciences since its establishment; and Dr. Hutton's Jeremiad, in the first edition of his Mathematical Dictionary, is statement rather than history, and moreover is strongly influenced by personal irritation. A clear unbiassed account of the rise, advance, and present state of the institution was therefore open to Mr. Weld; and his situation as Assistant Secretary and Librarian, together with his intercourse with the Fellows, eminently qualify him for the task.

It is usual to commence the foundation of the Royal Society with the troublous times of the Commonwealth; when weekly meetings took place at Dr. Goddard's lodgings, in order to discourse on subjects connected with mathematics and natural philosophy. But were its real origin strictly searched after, it might, we think, be justly dated half a century before that time. The real founder was Lord Bacon, who by his Instauration of the Sciences opened the gates of the Castle of Knowledge; and in his philosophical romance of the New Atlantis, distinctly planned and set forth such an institution; hence old Aubrey's marked recognition, *secundum mentem Domini Baconi*. Still to the aforesaid weekly meetings, and the strenuous exertions of Wilkins, Wallis, Ent, Goddard, Haak, Boyle and others, must be ascribed the stand which procured the charter of incorporation from Charles II. in 1662. Since that period the affairs and proceedings of the Society have been matter of public history; and its efforts are shown in that invaluable series of papers, the 138 volumes of Philosophical Transactions which it has published. This work is a truly important chronicle of the discoveries made by the Fellows, or their correspondents, together with a vast quantity of varied and profound information in every branch of science and art. By this grand series, the establishment of the Royal Society is proved to have been beneficial to the human race; and though often assailed from without, and torn by intestine broils, it is still the intellectual gem of Great Britain, and wields a mighty power over the scientific destiny of the country. For the whole view of the rise and advance of the Transactions, we must refer the reader to the detailed narrative by Mr. Weld, wherein they are fully and ably treated.

Nurtured in the Civil Wars, when party and sects had sickened people of divinity and politics, the constitution of the Royal Society was independent in its form and support, and liberal in its association. On the subject of religion they were more especially cautious, not only in the days of their infancy, but even when under the direction of Newton himself: in this spirit, says Sprat, "they have freely admitted men of different religions, countries, and professions of life; not to lay the foundation of an English, Scotch, Irish, popish, or protestant philosophy, but a PHILOSOPHY OF MANKIND." And when the Society for promoting Christian Knowledge were desirous of holding their meetings in the Royal Society's rooms, Newton, who was a bit of an autocrat in his way, drew up a number of sound arguments against their admission. By this sturdy stand the *Princeps Philosophorum* wished to preserve the body under his charge from the passions of the age. "It is a fundamental rule of the Society," said that illustrious President, "not to meddle with religion; and the reason is, that we may give no occasion to religious bodies to meddle with us:" and he refused to comply with the request, lest the compliance might "dissatisfy those of other religions."

And did this wise policy actuate the great men of a century and a half ago? Alas for the march of intellect so proudly boasted of! Compare this with the conduct of the present magnates in the formation of an Architectural and Archæological Society in the year 1848, of which the President is the Lord Bishop of Oxford. The constitution of this body is entirely exclusive, and the door is closed to all religions, persuasions, and sects but their own; this being the published form of admission:—

We, the undersigned, do hereby recommend _______________ BEING IN COMMUNION WITH THE CHURCH OF ENGLAND, *to be an* _____________ *Member of the Architectural and Archæological Society of Buckingham.*

Proposer, _______________
Seconder, _______________

There, Newton, is a lesson for you!

The earlier functions of the Royal Society appear to have been the scraping together of information, some of it *jejune* enough; but it soon created that keen spirit of experiment which has placed it on the highest plinth of utility, and for which it is justly entitled to the unfeigned gratitude of the world. At the period of its formation, the diffusion of new discoveries was an affair of difficulty and delay, and a first step therefore was to publish periodically all discoveries which came to their cognizance. A man of science could not then well appear before the world unless he had a book to publish: but Oldenburg, the Society's first secretary, soon taught how every new fact might be readily communicated. Nay more,—however useless, or even trifling, some essays may have appeared at first sight, experience has shown that the same investigations, pursued by abler men, have frequently led to discoveries of the greatest import. Hence the fitness and utility of furnishing inquisitive minds with easy opportunities of communicating the progress and effects of their respective researches, in what manner soever they may have been conducted, or to whatever purpose they may have been originally intended. Now the Society, which has largely, by these means, promoted the advancement of knowledge, surely merits to be recorded as a beneficial acquirement to the general public.

Although the Royal Society has subsisted on the voluntary contributions of its own members, it has not been without munificent gifts. Among the earliest of these was the present of the celebrated Arundelian Library, consisting of several thousand printed volumes and numerous manuscripts. On this occasion, it is whispered, that the Society were as much obliged to Evelyn as to Mr. Howard,—but we dislike looking a gift horse in the mouth. Other presents and considerable benefactions have followed; still Mr. Weld shows how often the proceedings have been interrupted, and even the fraternity endangered, by pecuniary difficulties. Indeed it is painful to notice the loss and inconvenience even now occurring to the establishment by the mean backwardness of some of its members in paying their moderate subscription. Much pseudo-sentimentality has been expended on this topic, and we believe idly expended; our own opinion being, that it is those who can that *won't* pay; and that in 98 cases out of 100, the defaulter is more deficient in principle than purse, and exercises sheer and *prepense* fraud. If a person is so pressed by his circumstances that he cannot pay, surely he ought to resign; but such is only the exception to the rule of defaulting, and we think that the energies of the laws have been allowed to slumber too long. As to all those who wilfully cheat the Societies to which they have signed obligations, we hold that they forfeit the character of gentlemen in the act, and should be proceeded against. In the present day the Councils have little excuse for allowing any losses of the kind, since there is the ready aid of the County Courts,—the which, we are happy to state, have just been appealed to with great success by the Zoological Society.

Constituted as the Royal Society is, a sort of democracy ruled by an oligarchy, it is no marvel that there should be frequent "flares-up;" and that they should so often have happened, the more 's the pity. Mr. Weld gives a very fair statement of the most serious of these, though he does not pronounce upon the animus of each; still his means of ascertaining and showing the interior management and œconomy of the Society, render him the best authority on the subject. One thing is clear, namely, that the medical men, who form a legion, and the scarcely less numerous naturalists, are ever mighty desirous of ruling the roast*. And a long run they had of it, when

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* A conclusion very different from that which is here adopted by the esteemed friend to whom the editors are indebted for this interesting Notice seems to be warranted by Mr. Weld's account of these disputes, in which a violent and domineering spirit seems not to have been evinced so much by the Naturalists as by the opposite party, headed by Horsley, whose conduct and motives appear in no favourable light.—R. T.

the small corps of mathematicians and astronomers were defeated under the dictatorship of Banks. On that occasion it was that Dr. Horsley uttered his indignant oration to the President; and which, though frequently interrupted, he delivered with great force. As Mr. Weld has not given this speech, an extract may serve to show the nature of the argument.

> "We see that great numbers may be occasionally brought down to ballot upon particular questions, who do not honour the Society with a very regular attendance. We are well aware, Sir, that oppressive statutes may be framed in the Council, and with *this* support in the Society at large, received. We understand that motions personally offensive and injurious may be brought forward, and perhaps may be carried; and by these means the remedies which the scientific part of the Society would wish to apply to the abuses which exist, may be prevented. But, Sir, I am united with a respectable and numerous band, embracing, I believe, a majority of the scientific part of this Society,—of those who do its scientific business. Sir, we shall have one remedy in our power when all others fail: if other remedies should fail, we can at least secede. Sir, when the hour of secession comes, the President will be left with his train of feeble amateurs; and that toy (*pointing to the mace*) upon the table, the ghost of that Society in which Philosophy once reigned, and Newton presided as her minister."

Apropos of this toy. It is well-known that in 1663 Charles II. presented the Royal Society with a mace; and that the same practice respecting this official symbol exists at the Royal Society as is observed in Parliament, namely, that it is put on the table when the chair is taken. Now this mace was confidently asserted to be the identical one referred to by Cromwell when he expelled the Commons from their house, by ordering his soldiers to "Take away that bauble;" and which was therefore an eyesore to the king. Such was the assurance; and the story was as popular as accredited. In this state of happy ignorance in comes Mr. Weld, and with his meddling among the papers destroys the long-cherished illusion; for he shows that it was ordered to be made for the Society, and produces an every-day kind of warrant for its being prepared and delivered. We know not how this dissolution of the bauble of the Long Parliament will be relished by the Fellows at large; but we think they must approve of the perseverance with which our author followed up his chase after truth, and of the candid manner in which he has related it.

An attentive perusal of the pages before us leaves the *quæstio vexata* as to the best composition of the Royal Society—whether it should consist of *e merito* members only, or the mixed leaven it now exhibits—pretty much where it was; there being, according to the sage observation of Sir Roger de Coverley, much to be said on both sides. Still it certainly does appear *outré* that the phalanx of science should contain such discordant elements, and admit of so many counterfeits; and it is unfortunate that these many are palmed on the country with F.R.S. to their name, for no other reason than the homely one of their money being required. To be sure, public opinion settles this in some degree, since the necessity of so strange amix ture is pretty well known; and though the πολλοὶ are noisy and rudent at elections and the like, they are of course excluded from the honours and consideration of the Society; that is, from their state and condition they are not likely to be selected by the Council to read the Croonian or Bakerian lectures, which are followed by a small pecuniary reward; nor are they likely ever to aspire to the Copley, the Rumford, or the Royal medals.

Mr. Weld gives a very elaborate insight of the composition of the body at large, and its statistics, together with a lucid retrospective review of its labours from the commencement to our present time. And however trite the matter may be to a large class of our readers, there may be many who will like to see the form and ceremony observed during its sessions.

> "The Session commences on the third Thursday in November, previously to which a card of the Meeting-days, and a list of the proposed new Council, are sent to every Fellow of the Society. On Saint Andrew's day, or on the day after should that day fall on Sunday, the Anniversary Meeting is held for the election of the President, Officers, and Council; after which, the Fellows and their friends dine together. Hitherto, the Medals have been distributed, and the President's Address delivered, at the Anniversary; but the day for the election of Fellows (in June) is now devoted to these purposes.
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> "The evening Meetings continue with the intermission of the Christmas, Easter, and Whitsun vacations, until the third Thursday in June. As soon as the President, or, in his absence, a Vice-President, takes the Chair, the Mace is placed on the table before him. He then reads the names of those individuals who desire, on the introduction of Fellows, to attend the Meeting, and who, on leave being granted, are admitted into the Meeting Room from the ante-chamber, where they are in waiting.
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> "The Senior Secretary, who sits on the right of the President, then reads the Minutes of the last Meeting, and an abstract of the Paper or Papers read on that occasion; after which, any newly-elected Fellow who is present, and has not been formally admitted, inscribes his name in the Charter-book, and is introduced by a Fellow to the President, who admits him according to the form prescribed in the Statutes.
>
> "The Junior Secretary, who sits on the left of the President, next announces the Presents made to the Society; he then reads the Certificates of Candidates for Election, and proceeds to read such Paper, or Papers, as may have been thought proper to be communicated to the Meeting. At the conclusion of the reading, the President calls on the Society to return their thanks to the Author or Authors of the Papers read, which are thenceforth the property of the Society. A discussion is then invited on the subject treated of in the Paper, which is more or less animated, according to the subject, and the number of Fellows present.
>
> "When the speakers have concluded, the President adjourns the Meeting, and the Fellows proceed with their friends to the Library, where they partake of tea. A *conversazione* ensues, which lasts until about eleven o'clock. These réunions are extremely agreeable, and are frequently honoured by the presence of distinguished foreigners who happen to be visiting London."

Such is a *colpo d' occhio* of the work before us; and on the whole we can assure every F.R.S., everyone who aspires to become an F.R.S., and all who are interested in the welfare of the Royal Society, that this book will amply repay the perusal. Besides giving the dry matter of rules and ordinances, it is brimful of interesting details and rich anecdotes; and there is much intelligence that every English gentleman ought to possess. In the course of so critical an undertaking, there are many topics which required great care and delicacy in their treatment; and the author has been obliged to shape a middle course between the genius and the temper of Newton, the honesty and the limited intellect of Pringle, the wealth and the leaden sway of Banks, the good and the evil bias of Davy, and the suaviter and the pliancy of Gilbert. But he has dashed through in good taste, and with a fair spirit; and his memoirs of the Presidents form a valuable addition to British biography. We hope ere long to announce a second edition.

LXX. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 243.]

June 9, 1848. DISCOVERY of a new Satellite of Saturn*. By W. Lassell, Esq.

"In communicating to you the particulars attending the discovery of an eighth satellite of Saturn, I shall adopt the proper names proposed by Sir John Herschel for the seven hitherto known satellites, namely, Mimas, Enceladus, Tethys, Dione, Rhea, Titan and Japetus, beginning with the closest, and proceeding in order of distance from their primary. The new satellite I have proposed, in conformity with this nomenclature, to call Hyperion.

"On the 18th of September, while surveying the planet in the twenty-foot equatoreal, and looking out for Japetus (which I expected to find following the planet and not far from the plane of his ring), I remarked *two* stars exactly in the line of the interior satellites. Not being certain at the time which of these was Japetus (although the nearer of the two certainly seemed too faint), I made a careful diagram of their positions with respect to Saturn, and also to some neighbouring fixed stars.

"The next night, the 19th, proved fine, and I was astonished to find that the *two* stars had both moved away from the fixed stars to which they had been referred, and were still accompanying Saturn; the more distant of the two had also gone northward, in conformity with the orbital motion of Japetus, while the nearer and fainter, remaining precisely in the line of the inferior satellites, appeared to have slightly approached the planet.

"A consideration of this appearance suggested the idea that the more distant was Japetus, and that the nearer and fainter must be a new satellite of Saturn. To verify the suspicion, I took differences of right ascension between each and a fixed star, and found that in

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* The new satellite was, it is said, discovered at the observatory of Cambridge, U.S., on Sept. 16, but no account has been received from Professor Bond.

2^{h} 36^{m} the suspected satellite had moved westward $2^{s}\cdot46$, and that in 1^{h} 24^{m} Japetus (the identification of which was now certain) had also moved westward $1^{s}\cdot27$. It is true that these differences are somewhat greater than is consistent with the orbital motion of Saturn, yet perhaps not greater than is consistent with reasonable errors of observation during so short a period. Moreover, as the suspected new satellite was situated precisely in the line of the satellites interior to itself, I took micrometrical measurements of its situation at two epochs, four hours apart, and was satisfied that during that interval no perceptible change whatever took place in its position in the line of the satellites. As the motion of Saturn southwards in the same period amounted to 18″, he must have left the suspected satellite obviously behind if it had been a fixed star. I could therefore now arrive at no other conclusion than that I had discovered a satellite hitherto undetected.

"I regret that since the 19th of September the weather has been remarkably unfavourable. I obtained a good set of measures of the elongation of the satellite on the 21st, and two very hasty measures on the 22nd, caught between clouds, which, with an *estimation* of its elongation on the 18th, are all the data I have hitherto obtained towards the determination of its period. Since the latter date the weather has been uniformly cloudy.

"If these observations should be thought worthy of being recorded until better can be obtained, they would stand thus:—

	′	″
Sept. 18, Elongation east of Saturn (estimated by comparison with Titan)	4	20
21, Elongation, measured	3	54
22, Ditto ditto	3	27

"These observations would, I believe, be best satisfied by a period of about twenty-four days, which may at least serve as a guide in looking for the satellite. Its magnitude is very small, perhaps not intrinsically so great as that of Mimas, though it was much easier to see, on account of its greater distance from the planet.

"During this autumn I have twice been gratified with a sight of the whole seven satellites of Saturn at one time, viz. on the 19th of August and on the 16th of September, at 11^{h} 15^{m}. The observations of the latter evening were very interesting. I first saw the planet a little before 10^{h}, when Tethys was behind the planet, and Enceladus and Dione absolutely in one. I immediately recognized Mimas, and in a very short time Enceladus emerged from conjunction with Dione, and then appeared with the latter as the most delicate double-star possible. At 11^{h} 30^{m} Titan had approached close up to the ball, and was apparently in contact with it. At 11^{h} 35^{m} it had become occulted. At 11^{h} 5^{m} I observed Tethys just emerging from behind the ball, and noticed that he emerged evidently *south* of the line joining the other satellites. As the present position of the ring requires that, if revolving in its plane, the satellite should appear rather to the *north* on its emergence, this fact seems to afford some evidence that the satellite does not revolve precisely in the plane of the ring. Moreover, at 11^{h} 50^{m}, when Tethys and Mimas formed an exquisitely delicate double-star, Tethys was still south of Mimas, although, as Tethys was receding from the planet and Mimas approaching it, the reverse ought to have been the case if the former really revolves in the plane of the ring.

"At $10^h\ 5^m$ this evening (September 16) Mimas was, as nearly as could be estimated, at his greatest elongation eastward; and on the 16th of October 1847, at $8^h\ 35^m$, I observed him at or very near his greatest elongation westward. The interval elapsing amounts to 336·045 days; and supposing him to have made in that time 356·5 revolutions, $22^h\ 37^m\ 22^s{\cdot}6$ will be the period of one revolution."

Extract of a Letter from Mr. Lassell.

"I am happy to tell you that I have at length brought my polishing machine to do all that I ever hoped or purposed it should do. I had previously obtained very good surfaces with it, but they were obtained with some anxiety and uncertainty. I wished to be able to repolish a known good surface without hurting it, as well as to turn a bad one into a good one with certainty and expedition. This, I am happy to say, I can now do; and by certain rules, varying with the proportion of the focal length to the aperture, I can produce a parabolic surface which shall have the same focus in every part of its surface to the hundredth of an inch. The improvement in regularity of curve is not less than in the truth of its general form. I am about to make some experiments on the further shortening of focus, viz. a 12-inch metal of 7-feet focus, more, however, as a curiosity than for utility*."

Professor Challis's method of correcting Equatoreal Observations for refraction.

The corrections for refraction in differential equatoreal observations made at the Cambridge Observatory, when the hour-angles for the star of comparison and object compared are the same, are calculated by the formulæ given in p. 188, vol. i. of Bessel's *Astronomische Untersuchungen*. In the triangle of which the angular points are at P the pole of the heavens, Z the zenith of the observer, and S the place of the star, draw from Z a perpendicular ZQ on PS. Let $d'-d$ be the apparent excess of the north polar distance of the object compared, above that of the star of comparison, and Δ the true north polar distance of the latter, or, more correctly, the mean of the north polar distance of the two objects; and let $\mu'-\mu$ be the excess of right ascension given by the difference of times of transit. Then

Correction applied to $d'-d = \varkappa(d'-d)\sec^2(\Delta-\mathrm{PQ})$

Correction applied to $\mu'-\mu = \varkappa(d'-d)\sec^2(\Delta-\mathrm{PQ}) . \dfrac{\tan \mathrm{QZ}}{15} . \dfrac{\cos(2\Delta-\mathrm{PQ})}{\sin^2\Delta}$

For calculating $\varkappa$, which depends both on the zenith distance and on the barometer and thermometer readings, the following empirical formula is sufficiently accurate and of ready computation:—

$$\log \varkappa = \log k + 0{\cdot}015\,\mathrm{B} + 0{\cdot}001\,(100^\circ - \mathrm{T}),$$

where $\log k$ is $\log \alpha''$ (in Bessel's table, pp. 198 and 199 of the work

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* Mr. Lassell is preparing an account of his polishing-machine and methods of grinding specula, &c. to be laid before the Society.

already cited) diminished by the constant 0·4957, B is the barometer reading in English inches, and T is the thermometer reading in degrees of Fahrenheit (Bessel's log α'' takes account approximately of the variation depending on zenith distance of the factor A in the expression, A tan zen. dist. for the total refraction). To select the proper value of log k, the argument zenith distance (z) is first obtained by the formula, $\sec z = \sec QZ \sec(\Delta - PQ)$. The calculations are much facilitated by the formation of two tables; one containing the values of PQ, log sec QZ, and log $\frac{\tan QZ}{15}$, for every ten minutes of hour angle from 0^h to 6^h, whence, by interpolation, and by proper attention to changes of sign, the values may be obtained for any given hour-angle; the other, a table of values of log k, in which the argument is z, or, which is preferable, log sec z, the difference between the consecutive logarithms being 0·01.

In an instance in which the apparent difference of north polar distance was 32′·1, the hour-angle $3^h\ 49^m{\cdot}7$, and the zenith distance 82°·3, the corrections calculated by the above approximate formulæ were 18″·12 and $0^s{\cdot}769$. By exact calculation they were found to be 18″·27 and $0^s{\cdot}762$.

When differences of right ascension and north polar distance are measured while the equatoreal is carried by clock-movement (which is sometimes done with the Northumberland equatoreal), it is necessary to compute exactly the total refractions in right ascension and north polar distance of each object, no other method appearing to possess sufficient accuracy.

Extract of a Letter from Mr. Alfred Weld, Director of the Observatory at Stonyhurst College.

"On September 20, I observed a large spot on the sun with our equatoreal, and found that it consisted of several dark nuclei enveloped in one large penumbra. On the 21st, I made several observations, from which I deduced the following values of the angles subtended by the spot and nucleus:—

	′	″
Greatest diameter of spot	2	41·1
... ... nucleus	1	7·2
Equatoreal diameter of spot	2	14·1
... ... nucleus ...	0	49·2
Meridian diameter of spot	2	14·1

"The spot was distinctly visible to the naked eye before sunset."

By comparisons with δ Ophiuchi, Spica and Antares, Mr. Weld found the declination of Mr. Hind's changing star to be −12° 37′ 15″.

LXXI. *Intelligence and Miscellaneous Articles.*

ON A SIMPLE AND READY WAY OF PRODUCING TOOLS FOR GRINDING LENSES. BY N. S. HEINEKEN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Sidmouth, Nov. 15, 1848.

IF you think the following simple and ready way of producing tools for grinding lenses likely to be of service to any of the readers of the Philosophical Magazine, you will oblige me by making it known. I was led to employ this method in consequence of an injury done to a lens, for the regrinding of which I had no corresponding brass tool. Rather than take the trouble of making a set of brass gauges and also a set of grinding-tools, I took a cast of the *lens itself* by pressure upon the cooling surface of some fusible metal contained in a frame of card. On this tool, thus readily formed, I reground and polished the lens; and where great accuracy of figure is not required, have found the method to answer very well. Hoping that this plan may be useful to others who may meet with a similar casualty, I communicate it. Perhaps I may also be allowed to mention, that I have found zinc exceedingly useful for the formation of ordinary grinding-tools, being readily cast and turned; and though not equal to brass, yet being in many respects superior to the soft metal tools sometimes employed.

I am, Gentlemen,
Respectfully yours,
N. S. Heineken.

DR. BIALLOBLOTZKY'S JOURNEY TO THE SOURCES OF THE NILE.

Letters have been received by Dr. Beke from Dr. Bialloblotzky, whose intended exploratory journey into Eastern Africa was noticed in the last Number of the Philosophical Magazine*, by which letters that traveller's proceedings are brought down to the 8th of November, when he was at Alexandria.

Before Dr. Bialloblotzky quitted Vienna on the 7th of October, the Imperial Academy of Sciences granted him the use of some valuable instruments made by M. Kapeller of Gumpendorf and selected by M. Schaubl of the Imperial Observatory, and the Government procured for him and his son a passage *gratis* from Vienna to Trieste by the railway, and from Trieste to Athens, Syra and Alexandria, by the steamers of the Austrian Lloyd's.

It was his intention to quit Suez for Aden on the 23rd of November by the East India Company's steam-packet, by which a free passage has been granted to him by the Court of Directors.

ON THE ARSENITES OF IRON.

M. Filhol made a few experiments on the arsenites of iron with the view of discovering a certain method of ascertaining the state in which arsenic exists in chalybeate waters and the deposits which they form; the author states that he did not find what he looked for, but still he observed some facts which he deemed not unimportant.

Arsenic does not always exist in chalybeate waters in the state either of arsenite or arseniate, and its presence in certain ferruginous deposits may not be recognized, on account of the state in which it exists.

In proceeding to analyse a deposit from a spring at a little distance

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* See page 399.

from the baths of Rennes, called *Eaux rouges d'Alet,* the author observed, that this deposit, which he at first thought completely free from arsenic, contained a small quantity of it, but could only be detected by washing the gas furnished by Marsh's apparatus, by passing it through a solution of gold or silver. For this purpose the deposit previously carbonized must be employed, for if the water employed to wash was used instead, not the smallest quantity of arsenic was obtained; and when the deposit was used in the apparatus, so small a quantity of arsenic was obtained in a given time, that no spots were procurable.

The arsenic exists therefore in these deposits in very minute quantity, and in the form of a compound insoluble in boiling water, even when rendered strongly acid by sulphuric acid: the author admits, that not having obtained any arsenic by his first analysis, he stated that these waters did not contain any; but he afterwards found that he had been deceived by the process which he employed.

It was demonstrated by experiments, that if in certain cases the arsenic existed in the chalybeate waters in the form of a compound soluble in acids, in other cases it is found in the state of an insoluble compound; it was also found that the insoluble arsenical compound was so slightly attacked by nascent hydrogen in Marsh's apparatus, that it did not contain enough to yield even very slight spots, and that it was requisite to condense it in solution of gold or silver to ascertain its existence.

It remained to determine the nature of the arsenical compound which acted in this manner, and the author suspected that it might be sulphuret of arsenic; the deposit had imbibed water containing sulphates, and itself contained a large proportion of them, besides a great quantity of organic matter; it will then be readily imagined that part of the sulphates might be converted into sulphurets under the influence of the organic matter, and these acting upon the arsenical compound which previously existed in another form, might convert it into sulphuret.

Another considerable difficulty however remained; sulphuret of arsenic is considered by chemists as perfectly unacted upon by nascent hydrogen, and if this be the case it must be admitted that the ferruginous precipitates examined did not contain sulphuret of arsenic.

The author endeavoured, but in vain, to isolate the suspected sulphuret of arsenic by ammonia; and it afterwards occurred to him to determine whether the natural sulphurets of arsenic were acted upon in Marsh's apparatus; for this purpose very pure orpiment and realgar were reduced to fine powder, and it was ascertained that the water with which they were washed contained no trace of arsenic; each of the sulphurets was then subjected to examination in Marsh's apparatus, the gas liberated having been passed before adding them, through solution of nitrate of silver for half an hour without producing any change in it. In a quarter of an hour after adding the sulphurets, the solution of silver began to be turbid; the turbidness at first increased very slowly, but at the expiration of six hours, a considerable quantity of black precipitate was formed; this solution was set aside, and the gas disengaged from the apparatus was passed through a fresh solution, which became immediately turbid, and gave in a few minutes a very evident precipitate; when the apparatus was uncorked, M. Filhol was surprised on finding that the gas disengaged had the odour of hydrosulphuric acid: the operation was discontinued till the next morning, and the current of gas was then passed into a third solution of nitrate of silver, which became immediately turbid; in a few minutes nearly the whole of the silver was precipitated, the gas emitting a strong smell of hydrosulphuric acid.

The author states he always employed very dilute sulphuric acid, in order to avoid the production of hydrosulphuric acid pointed out by MM. Fordos and Gelis.

The black precipitates obtained, evidently contained sulphuret of silver, but it was possible that arseniuretted hydrogen might also be disengaged, independently of the hydrosulphuric acid; to ascertain this hydrochloric acid was added to each solution, the chloride of silver was separated by filtration, and the filtered liquids were treated with hydrosulphuric acid; in all of them a yellow precipitate was formed, which was found to be sulphuret of arsenic.

M. Filhol is of opinion that from these experiments he may conclude that the natural sulphurets of arsenic are not absolutely unacted upon in Marsh's apparatus, as hitherto supposed; that they are very slowly decomposed, and that the quantity of arsenic brought over by the hydrogen is too small to produce arsenical spots; that it requires a long time to ascertain its presence in the metallic solutions which have retained it; that nascent hydrogen acts on both elements of these sulphurets, and always contains traces of hydrosulphuric acid and arseniuretted hydrogen.

The author observes that M. Chevallier's experiments on the natural sulphurets of arsenic are nowise in opposition to the preceding facts, since the washings of the sulphurets in the experiments of M. Chevallier contained traces of arsenious acid, which M. Filhol's did not; and he thinks the slowness of the action on the sulphurets of arsenic will account for their having been supposed to be not acted upon at all.

From the experiments detailed the author is of opinion, that though he has not succeeded in rigorously demonstrating the presence of arsenic in the cases alluded to, he has shown that it may exist in the state of sulphuret in the deposits of certain natural chalybeate waters, and that it ought to be searched for in those which contain sulphates; the same remark applies to the searches for arsenic in the earth of cemeteries.—*Journ. de Chim. Méd.*, Octobre 1848.

ANALYSES OF DIFFERENT VARIETIES OF EPIDOTE.

BY M. HERMANN.

The author has subjected to analysis a great number of the varieties of epidote, such as zoisites, pistazites, orthites, &c., found in part in the Ural.

Zoisites.—Grey zoisite from Falltigel in the Tyrol; density, 3·28. Imbedded in granite.

Silica	40·95
Alumina	30·24
Lime	21·56
Protoxide of iron	4·96
Carbonic acid	1·13
Water	0·56
	99·40

This analysis agrees with the previous results of M. Geffken from the zoisite of the same locality, and with those of other analysts from different places.

Green zoisite from Arendal; density, 3·17.

Silica	37·32
Alumina	22·85
Lime	22·03
Magnesia	0·77
Peroxide of iron	11·56
Protoxide of iron	1·86
Carbonic acid	2·64
Water	0·29
	99·32

Green zoisite from Achmatowsk; density, 3·33.

Silica	37·32
Alumina	22·85
Lime	22·03
Magnesia	0·77
Peroxide of iron	11·56
Protoxide of iron	1·86
Carbonic acid	2·64
Water	0·56
	99·59

Green zoisite from Schumnaja; density, 3·43.

Silica	36·45
Alumina	24·92
Lime	22·45
Peroxide of iron	9·54
Protoxide of iron	3·25
Carbonic acid	2·73
Water	0·77
	100·11

Pistazite from Arendal, in large blackish-green crystals; density, 3·49.

Silica	36·79
Alumina	21·24
Lime	21·27
Peroxide of iron	12·96
Protoxide of iron	5·20
Carbonic acid	2·31
Water	0·55
	100·32

The previous analyses of Rammelsberg, Geffken and Kühn, agree with the foregoing.

Pistazite from Burowa, south of Miask, imbedded in quartz; density, 3·35.

Silica	36·87
Alumina	18·13
Lime	21·45
Magnesia..............	0·40
Soda	0·08
Peroxide of iron	14·20
Protoxide of iron........	4·60
Carbonic acid	0·89
Boracic acid	a trace
	96·62

Pistazite from Werchneuvinsk, or puschkinite; density, 3·43. The crystals are red or green, according to their position against the light.

Silica	37·47
Alumina	18·64
Lime	22·06
Soda with traces of lithia	2·28
Peroxide of iron	14·15
Protoxide of iron........	2·56
Carbonic acid	0·79
Water	0·65
Boracic acid............	a trace
	98·60

Pistazite from Bourg d'Oisans in Dauphiny, of an olive-green colour; density, 3·38.

Silica	37·60
Alumina	18·57
Lime	21·19
Magnesia..............	1·40
Peroxide of iron	13·37
Protoxide of iron........	5·55
Carbonic acid	1·22
Water	0·46
	99·36

Pistazite from Achmatowsk, of a grass-green colour; density, 3·39.

Silica	37·38
Alumina	18·25
Lime	24·72
Magnesia..............	0·39
Soda	0·91
Peroxide of iron	12·31
Protoxide of iron........	2·20
Carbonic acid	1·61
Water	0·59
Boracic acid	a trace
	98·36

Bucklandite. This mineral, considered hitherto as black sphene, is met with at Achmatowsk, in isolated crystals in calcareous spar, accompanied with garnet and diopside; density, 3·51. M. Auerbach determined the measures, which coincide perfectly with the form of other epidotes.

It yielded by M. Hermann's analysis,—

Silica	36·97
Alumina	21·84
Lime	21·14
Peroxide of iron	10·19
Protoxide of iron	9·19
Carbonic acid	0·32
Water	0·68
	100·33

Orthite or ceriferous epidote. Tschewkinite is often confounded with the orthite of Miask, but the former is much the denser (4·50). Several trials gave M. Hermann numbers varying between 3·4 and 3·6 for the density of the orthite of Miask. By analysis orthite yielded—

Silica	34·47
Alumina	14·36
Lime	10·20
Magnesia	1·08
Peroxide of iron	7·66
Protoxide of iron	8·24
Oxide of cerium	14·79
Oxide of lantanium	7·66
Water	1·56
	100·02

Journ. de Ph. et de Ch., Sept. 1848.

ON THE PREPARATION OF BLACK SULPHURET OF MERCURY.

M. C. Vogler remarks, that the usual method of preparing Ethiops' mineral consists in triturating pure metallic mercury with washed flowers of sulphur in a porcelain or marble mortar. The mixture is sprinkled occasionally either with water or spirit of wine, and the trituration is continued until globules of mercury are no longer discoverable by the glass. Such a process ought not, however, to be recommended; for when the mixture ceases to be moistened sufficiently, part of it rises in dust, which not only occasions loss of material, but exposes the operator to the inconvenience of respiring mercurial vapour.

To remedy these inconveniences the author proposes the following plan:—Take a thick glass vessel, capable of holding twelve to sixteen ounces, put into it four ounces of mercury and one ounce of finely-powdered flowers of sulphur; shake them continually for about two hours, then add another ounce of sulphur, removing the cork of the vessel occasionally; shake again until globules are no longer visible with the naked eye; lastly, add two more ounces of sulphur and continue the agitation till particles of mercury are no longer visible with a glass.

According to M. Vogler, this method is particularly to be recommended, because it requires less time than trituration, is more convenient and less hurtful. At all events, the process which includes warming the mortar before trituration should be abandoned; for by these means a still larger portion is lost in dust, and the mercurial vapour becomes more hurtful to the operator.—*Journ. de Ph. et de Ch.*, Sept. 1848.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1848.

Chiswick.—October 1. Densely overcast: clear. 2. Heavy clouds: fine: overcast. 3. Very fine. 4. Rain: cloudy. 5. Cloudy and mild: clear. 6. Very fine. 7. Slight fog: very fine: heavy rain. 8. Very fine: heavy rain. 9. Rain. 10. Cloudy. 11, 12. Clear and fine. 13. Clear: cloudy. 14. Rain. 15, 16. Hazy: rain. 17, 18. Cold rain. 19. Cloudy. 20. Cold rain. 21. Rain. 22. Rain: clear. 23. Overcast: rain. 24. Very clear: overcast: rain. 25. Boisterous, with slight rain. 26. Slight fog: fine: clear. 27. Heavy rain. 28. Heavy showers, with intervals of sunshine. 29. Low clouds: overcast: foggy. 30. Fine: overcast: foggy. 31. Foggy: rain at night.

Mean temperature of the month	49°·27
Mean temperature of Oct. 1847	52 ·14
Mean temperature of Oct. for the last twenty years	50 ·44
Average amount of rain in Oct.	2·56 inches.

Boston.—Oct. 1. Cloudy: rain early A.M. 2. Fine. 3. Fine: rain early A.M. 4. Rain. 5. Cloudy. 6. Fine. 7. Cloudy: rain early A.M. 8, 9. Fine. 10. Cloudy: rain A.M. and P.M. 11, 12. Cloudy. 13. Rain. 14. Cloudy: rain A.M. and P.M. 15. Rain. 16. Rain: rain early A.M. 17. Rain. 18. Snow and sleet. 19. Rain. 20. Fine: rain P.M. 21. Fine. 22. Rain. 23. Fine: rain P.M. 24. Fine. 25. Rain: rain A.M. 26. Fine. 27. Rain: rain A.M. and P.M. 28. Fine: rain P.M. 29. Cloudy. 30. Fine: rain P.M. 31. Foggy.

Applegarth Manse, Dumfries-shire.—Oct. 1. Drizzling day. 2. Fair A.M.: slight drizzle P.M. 3. Fine: threatening P.M. 4, 5. Very wet. 6. Fair: strong wind. 7. Slight showers: cleared P.M. 8. Showers A.M.: cleared: shower P.M. 9. Showers. 10. Slight showers. 11. Fair, but dull. 12. Slight showers. 13, 14. Fair and clear. 15. A very slight shower: cleared P.M 16. A very slight shower. 17. Bright and fair: snow-shower seen at a distance. 18. Frost: aurora borealis. 19. Frost. 20. Frost, but mild. 21. Slight rain. 22. Heavy rain all day. 23. Rain morning and evening. 24. Heavy rain early, morning. 25. Rain early: cleared: fine. 26. Frost: fine: shower P.M. 27. Threatening rain: heavy P.M. 28. Rain: hail: thunder. 29. Frequent showers. 30. Raw frost A.M.: rain P.M. 31. Rain all day.

Mean temperature of the month	46°·5
Mean temperature of Oct. 1847	49 ·5
Mean temperature of Oct. for the last twenty five years	46 ·6
Average amount of rain in Oct. for twenty years	3·56 inches.
Rain in Oct. 1847	5·09 „

Sandwick Manse, Orkney.—Oct. 1. Drizzle. 2. Bright: clear: aurora. 3. Damp: clear: aurora. 4. Rain: aurora. 5, 6. Cloudy. 7. Bright: clear: aurora. 8. Fog: clear: aurora. 9. Rain: clear. 10. Showers: clear: showers. 11, 12. Cloudy. 13. Fine: hoar-frost: clear. 14. Cloudy. 15. Clear: cloudy. 16. Showers: hail-showers. 17. Snow-showers: hail-showers. 18. Snów: snow-showers: aurora australis. 19. Snow: clear: aurora. 20. Clear: aurora. 21. Damp: showers: aurora. 22. Drizzle. 23. Showers: clear: aurora. 24. Rain: clear: aurora. 25. Showers: clear: aurora: hoar-frost. 26 Showers: cloudy: hoar-frost. 27. Damp: showers. 28. Rain: clear. 29. Shower: thunder: shower: clear. 30. Shower: thunder: shower: clear. 31. Clear: hoar-frost: clear.

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.
1848. Oct.	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.	29·681	29·673	29·16	29·40	29·46	29·47	29·48	63	39	56	56½	52	54	53	sw.	s.	w.	se.	......	·30	......	·05
2.	29·688	29·604	29·29	29·49	29·43	29·60	29·60	67	46	54	59	47	53	52	s.	calm	e.	se.	·03	......	......	·09
3.	29·940	29·711	29·22	29·49	29·49	29·57	29·54	67	49	56	59½	47½	55	52½	sw.	calm	e.	se.	·04	·09	......	......
4.	29·988	29·804	29·23	29·37	29·61	29·28	29·54	65	58	60	59	50	54	52½	sw.	calm	sse.	ssw.	·01	·14	......	·50
☽ 5.	30·151	29·588	29·46	29·76	29·81	29·74	29·82	67	49	60·5	61	55	55½	52	sw.	calm	sw.	ese.	......	......	......	·24
6.	30·143	30·124	29·54	29·77	29·98	29·48	29·85	74	49	62	64	57	57½	54	sw.	sw.	sw.	w.	......	......	......	......
7.	30·128	30·030	29·59	29·92	29·90	30·00	29·94	73	50	62	62	48½	51½	49	sw.	calm	sw.	e.	......	·08	......	·05
8.	30·092	30·011	29·43	29·74	29·78	29·65	29·63	68	46	63	60	53½	54	50	w.	calm	wsw.	ssw.	......	......	1·55	......
9.	30·013	29·758	29·39	29·68	29·51	29·52	29·48	62	46	57	56	49	53	45	sw.	calm	sw.	calm	·06	......	......	·39
10.	29·803	29·652	29·11	29·54	29·80	29·75	29·94	59	41	55	58½	46	52	50	w.	calm	nw.	n.	......	·94	......	·07
11.	29·927	29·793	29·45	29·88	29·90	29·98	30·02	60	35	51	55½	42½	48½	48	nw.	calm	nw.	n.	......	·15	......	·07
○ 12.	29·975	29·946	29·52	29·93	30·02	30·04	30·21	56	38	47·5	54	35	48	47	n.	calm	nw.	n.	·01	......	......	·01
13.	30·041	29·997	29·60	30·18	30·20	30·34	30·42	58	44	51	55	42½	48	47	ne.	n.	ne.	calm	·02	·32	......	......
14.	30·046	30·005	29·70	30·22	30·17	30·42	30·36	54	40	53	54	40	47	47	ne.	n.	nne.	ne.	·14	·38	......	......
15.	29·815	29·755	29·35	30·04	29·88	30·27	30·18	52	41	50	56	42	49½	46½	nw.	nne.	nne.	ne.	·21	·10	......	......
16.	29·749	29·697	29·34	29·82	29·86	30·05	30·13	54	44	50	55½	45½	45	39	ne.	nne.	nne.	n.	·04	·25	0·15	......
17.	29·944	29·808	29·44	29·94	29·90	30·10	30·04	49	33	47	50½	40½	37	32½	ne.	n.	nnw.	n.	·02	·10	......	·15
18.	29·636	29·550	29·28	29·77	29·78	30·01	30·03	42	36	36	41	30½	33½	34	n.	nnw.	n.	nne.	·04	·12	......	·16
☾ 19.	29·738	29·596	29·34	29·80	29·90	30·04	29·98	50	41	45	45	32½	36	39	ne.	n.	n.	s.	·01	·03	......	·18
20.	29·789	29·747	29·52	29·97	29·94	30·04	30·05	50	41	48	46½	29	41½	39½	ne.	ne.	nne.	s.	·34	·03	......	·03
21.	29·887	29·759	29·42	29·81	29·70	29·97	29·90	53	29	47	51	29½	46	46	ne.	e.	ne.	ese.	·10	·24	......	·06
22.	29·720	29·594	29·33	29·49	29·30	29·79	29·66	58	36	45	54	40½	46	47	s.	e.	e.	e.	·12	·06	......	·16
23.	29·624	29·487	29·22	29·35	29·28	29·49	29·44	56	44	48	49	43	49	44	sw.	s.	se.	ne.	·56	......	0·53	·20
24.	29·536	29·493	29·04	29·08	29·30	29·19	29·30	60	47	50	50	41½	47	47	sw.	s.	sw.	n.	·15	·64	......	·28
25.	29·652	29·163	28·73	29·24	......	29·28	29·46	56	31	48	...	35	41	40	w.	nne.	nw.	n.	·02	·66	......	·11
26.	29·861	29·812	29·46	......	29·60	29·58	29·76	61	38	44	62½	...	39	36	s.	ssw.	sw.	e.	·08	·18	......	·10
● 27.	29·488	29·280	29·05	29·30	29·09	29·50	29·25	56	43	51	53	43½	50½	50	s.	se.	se.	e.	·39	·06	......	·03
28.	29·594	29·452	29·03	29·06	29·20	29·32	29·34	54	37	50	50	41½	46½	47	sw.	s.	se-sw.	e.	·34	·23	......	·45
29.	29·511	29·493	29·10	29·33	29·40	29·44	29·57	53	31	48	48	38	47½	45	sw.	s.	sw.	ne.	·01	·12	......	·19
30.	29·528	29·498	29·17	29·44	29·46	29·67	29·72	56	32	45	46½	35½	45½	48½	sw.	se.	se.	calm	·07	......	1·97	·25
31.	29·566	29·545	29·18	29·48	29·50	29·75	29·72	50	38	43	44½	41½	40	37	sw.	calm	se.	calm	·12	·11	......	·15
Mean.	29·814	29·658	29·31	29·643	29·671	29·752	29·785	58·16	40·38	51·0	50·5	42·5	47·46	45·69					2·93	5·33	4·20	3·97

THE
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AND
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XXXIII. THIRD SERIES.

LXXII. *On the Phænomenon of Luminous Rings in Calcareous Spar and Beryl, as produced by tubular cavities containing the two new Fluids. By* Sir DAVID BREWSTER, *K.H., LL.D., F.R.S., and V.P.R.S. Edin.*[*]

IN a paper read at the meeting of the British Association at York in 1844, I showed that the two rings seen to pass through a luminous body, when viewed through certain specimens of calcareous spar, were produced by a great number of minute tubes parallel to one of the edges of the primitive rhomb. When the light is incident on a natural face of the rhomb, two rings are generally seen, varying in magnitude as the angle of incidence varies. At one incidence in the plane of the principal section, one ring vanishes by contracting into a luminous point; and at a different incidence the other ring vanishes in a similar manner, the rings always passing through the luminous body, and vanishing in a point coincident with it. At another incidence the luminous rings are equal, the place of the luminous body being at their point of contact, and a ring on each side of it. If we now incline the rhomb in the plane of the principal section, one of the rings will vanish, as already described, while the other enlarges itself; and by continuing the inclination, the vanished ring will reappear within the other ring, and touching it at the place of the luminous image. As the inclination increases (the refracted ray approaching the axis of the rhomb) the rings increase in size, the lower ends disappear, and the upper segments, where they are in contact, become almost rectilineal.

Beside these two primary rings there are two secondary ones; one of them a complete ring, and the other only a large segment of one. The complete ring commences at the centre of the lesser ring in a *red* spot at an incidence of 57°; and as the angle of incidence increases, *yellow*, *green*, *blue*, and *violet* tints successively emerge, till we have a beautiful circular

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[*] Communicated by the Author.

spectrum with a *violet* disc in its centre. As the inclination increases the spectrum becomes a ring, which gradually expands and approaches to the luminous body or to the primary ring in which it is inclosed, the *red* rays being on the *outside* and the *violet* on the *inside.* The other secondary ring or segment lies beyond the primary rings. It has a curvature of contrary flexure at its extremities, and gradually approaches the luminous body, or the primary ring beyond which it lies. It is also highly dispersed, but less so than the other secondary ring, and its *violet* side is on the *outside* of the ring. These two secondary rings are very feeble in the intensity of their light; but their intensity increases with the angle of incidence, and at the greatest possible inclination their intensity becomes equal, and finally superior, to that of the two primary rings, which almost disappear.

The ring E, or that formed by the Extraordinary image of the spar, vanishes at an incidence of 19° from the perpendicular, or 109° from the surface of the rhomb between the point of incidence and the obtuse angle of the rhomb, and in the plane of the principal section. The ring O, or that formed by the Ordinary image, vanishes at an incidence of 33° or 123°, similarly reckoned. The two rings E and O are equal at an incidence of 26° or 116°. The inner secondary ring commences or disappears at an incidence of 57°. These results prove that the fibres or *tubes*, as I have shown them to be, are parallel to one of the edges of the primitive rhomb.

The two secondary rings are produced by the duplication of the ordinary and extraordinary pencils when they are reflected from an interrupting line or stratum, in the manner which I have described in the Philosophical Transactions for 1815*.

The light of the two primary rings is polarized like that of the two pencils from which it proceeds; the light of the complete secondary ring is polarized like that of the ring which incloses it; and the light of the secondary segment is polarized like that of the adjacent primary ring, in conformity with the results given in the paper now referred to.

If we incline the rhomb in a plane perpendicular to that of the principal section, commencing at the incidence where the two primary rings are equal, both of them expand equally, the rings intersecting each other, and the point of intersection keeping in the plane of inclination. The remote portions of both rings soon disappear, and the visible portions, with the

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* On the Multiplication of Images, and the Colours which accompany them in some specimens of Iceland Spar, p. 270. See also Malus, *Théorie de la Double Refraction,* p. 194.

luminous body at their intersection, become rectilineal. In this case the secondary rings are not visible.

The phænomena of colour accompanying the rings are very interesting. In the plane of the principal section, when the two rings are on different sides of the luminous body, the *inside* of each ring is *red* and the *outside violet*. When one of the rings passes to the other side of the luminous body, its *inside* becomes *violet* and its *outside red*. The degree of dispersion at any point of the rings increases with its distance from the luminous body. The colour of the *secondary* rings diminishes as they approach to the *primary* rings. When the rings intersect one another, by inclining the rhomb in the plane of the principal section they are perfectly white at their intersection, and the colour gradually increases towards the middle part of their different segments.

These various phænomena depend on the law of dispersion in Iceland spar. I have long ago shown that the dispersive power accompanying the extraordinary refraction is much greater than the ordinary dispersive power of the mirror, a result subsequently confirmed by Rudberg; and Malus* and others have described the general phænomena of dispersion when the ray is incident upon the natural face of the rhomb. At a perpendicular incidence the ordinarily refracted ray is neither refracted nor dispersed, while the extraordinary one is both refracted and dispersed, the violet rays being those which are least refracted. At a few degrees of incidence in the principal section both rays are refracted; but in the *ordinary* ray the *red* rays are *least* refracted, while in the *extraordinary* ray they are *most* refracted. At an incidence of 16° 45′, according to Malus, the extraordinary rays cease to be refracted, but continue to be dispersed, the violet rays being always the least refracted. At an incidence of about 40° both rays are refracted, but the extraordinary one is not perceptibly dispersed. At incidences above 40° both rays are refracted and dispersed in the same manner, the violet rays being most refracted. When these rings are produced by a well-polished specimen of Iceland spar, and in a dark room with the sun as the luminous body, they form one of the most beautiful optical phænomena which can be seen. If the luminous body has some considerable breadth, and is crossed with dark or differently-shaded or coloured spaces, the phænomenon becomes still more beautiful.

The phænomenon of luminous rings has been very recently observed in a specimen of Beryl, brought from India by the Marchioness of Tweeddale, who was so kind as to present it

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* Malus, *ut supra*, p. 201.

to me. In cutting the crystal Mr. Sanderson found that one end of it was foul, and produced a luminous ring round a candle. I have examined it with particular care, and have found that the rings are produced by tubular cavities, and that several of these cavities contain the two new fluids, while others have contained them, and have been emptied of their contents by being cut through by the lapidary at one or both ends. These tubes are accurately parallel to the axis of the six-sided prism; several of them have irregular shapes like some of those which I have figured in my papers On the two New Fluids in Minerals*. In the opened tubes their interior is covered with an indurated crust which the fluid has left behind, while in others the fluid is distinctly visible with high magnifying powers. In two or three parts of the specimen there are spaces or strata of fluid cavities perpendicular to the axis of the prism, and of a hexagonal form. Hundreds of these cavities are like mathematical points, which the highest magnifying powers are unable to resolve, but many of them are sufficiently large to exhibit clearly the two new and immiscible fluids which exist in the same cavity, and the great expansion by heat of the volatile fluid. In the many hundred specimens of topaz and other minerals in which I have had occasion to examine the physical character of these fluids, the denser of the two which occupies the angles and narrow portions of cavities is much smaller in quantity than the volatile fluid, which flies off in a gaseous form when the cavities are opened or burst by heat. It is quite otherwise, however, in the beryl cavities. The volatile fluid, with its apparent air-bubble or vacuity, floats in a small quantity of the denser fluid, and expands so as to fill that vacuity at a temperature of 60°. In several of these cavities there are minute crystals, but I have not ventured to apply such a degree of heat as to determine whether or not they will melt and recrystallize like those in the topaz cavities.

As the tubular cavities in this remarkable specimen of beryl are larger than those in calcareous spar, the luminous ring which they produce is not such a smooth and regular band of light as it is in the latter mineral. The ring is composed of a number of radial lines closely packed together, and in particular lights it derives from this structure a great degree of beauty. As there is no unbalanced dispersion, the ring is perfectly white; and at *three* points of its circumference 120° distant, there is an image of the luminous body. The ring of course does not consist of polarized light like the rings in calcareous spar, but is composed of two rings oppositely polarized. If the cavities

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* Edinburgh Transactions, 1823, vol. x. p. 1, 407.

had been considerably inclined to the axis of double refraction, the images might be sufficiently separated to produce phænomena analogous to those in calcareous spar. When the spar or the beryl is bounded by planes perpendicular to the axis of the crystal, the rings become straight lines.

In the uniaxal negative system of polarized rings which I discovered many years ago in Beryl, I noticed an imperfect development of the black cross near its centre. In the system of polarized rings shown by the present specimen, the black cross is still more imperfectly developed, the arms of the cross being slightly curved and separated, as if there was a very feeble axis of double refraction perpendicular to the real axis. This effect is produced by a very interesting cause, to which I have had occasion to refer at some length in another place*. When the beryl was in a soft state from heat, the fluids or gases contained in the tubular cavities had compressed the substance of the beryl to a certain distance around them, and thus modified the double refraction due to the forces of aggregation by which the crystal was formed. Different degrees of compression have been produced by different cavities; but the tint resulting from them is generally a full *white* of the first order, and in some cases a *yellow* of that order. When in the polarizing microscope the light is incident obliquely to the axis of the crystal, and nearly in the plane of primitive polarization, the regular tints are singularly modified and serrated by those produced by compression.

In the sapphire or asterial corundum, where we have three systems of cavities, each parallel to the sides of an equilateral triangle, the three systems of luminous lines give us by reflexion the beautiful hexagonal radiations which are so much admired. These systems of lines will be converted into rings when the sapphire is cut by planes that are not parallel to the direction of the cavities.

St. Leonard's College, St. Andrews,
December 20, 1848.

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* On the Modification of the Doubly Refracting and Physical Structure of Topaz by Elastic Forces emanating from minute cavities.—Edinburgh Transactions, 1845, vol. xvi. p. 7, and reprinted in this Journal, vol. xx i. p. 101, August 1847.

LXXIII. *On Pluquaternions, and Homoid Products of Sums of* n *Squares. By the* Rev. Thomas P. Kirkman, *A.B., Rector of Croft with Southworth, Lancashire**.

[Continued from p. 459.]

THE product $Q_a Q_{a_,}$, when simply written out, the substitutions of a single imaginary for certain duads according to the conditions implied in the triplets being not yet made, is in all cases of the form,

$$Q_a Q_{a_,} = (ww_, - \Sigma a a_,) + (\Sigma a_0 \cdot \overline{aw_, + wa_,}) + \Sigma a_0 b_0 \mathrm{H},$$

say

$$Q_a Q_{a_,} = (\mathrm{A} - \mathrm{B}) + \quad (\mathrm{C} + \mathrm{D}) + \quad \mathrm{E}.$$

Let now Q_{-a} differ from Q_a, $Q_{-a_,}$ from $Q_{a_,}$, $Q_{-a_{,,}}$ from $Q_{a_{,,}}$, only in the signs of all the imaginaries. Then, since a change in the order of the factors Q_a and $Q_{a_,}$ alters nothing except the sign of E, and a change of sign of all the imaginaries in both the factors alters nothing except the sign of (C+D), we have

$$\left.\begin{aligned} Q_a\,Q_{a_,} &= (\mathrm{A}-\mathrm{B})+(\mathrm{C}+\mathrm{D})+\mathrm{E},\\ Q_{a_,}\,Q_a &= (\mathrm{A}-\mathrm{B})+(\mathrm{C}+\mathrm{D})-\mathrm{E},\\ Q_{-a}Q_{-a_,} &= (\mathrm{A}-\mathrm{B})-(\mathrm{C}+\mathrm{D})+\mathrm{E},\\ Q_{-a_,}Q_{-a} &= (\mathrm{A}-\mathrm{B})-(\mathrm{C}+\mathrm{D})-\mathrm{E}. \end{aligned}\right\} \quad . \quad . \quad (\mathrm{E.})$$

Hence, if

$$\left.\begin{aligned} Q_a\,Q_{a_,} &= Q_{a_{,,}} + \mathrm{R},\\ Q_{-a_,}Q_{-a} &= Q_{-a_{,,}} - \mathrm{R}; \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{F.})$$

for in $Q_{-a_,} Q_{-a}$, both c_0 and its equivalent $(a_0 b_0)$ have a sign contrary to that which they have in $Q_a Q_{a_,}$; and every term in +R appears with a changed sign in the product $Q_{-a_,} Q_{-a}$.

The following are also evident:—

$$\left.\begin{aligned} Q_a\;\,Q_{-a_,} &= (\mathrm{A}+\mathrm{B})+(\mathrm{C}-\mathrm{D})-\mathrm{E},\\ Q_{-a_,}Q_a &= (\mathrm{A}+\mathrm{B})+(\mathrm{C}-\mathrm{D})+\mathrm{E},\\ Q_{-a}Q_{a_,} &= (\mathrm{A}+\mathrm{B})-(\mathrm{C}-\mathrm{D})-\mathrm{E},\\ Q_{a_,}\;Q_{-a} &= (\mathrm{A}+\mathrm{B})-(\mathrm{C}-\mathrm{D})+\mathrm{E}. \end{aligned}\right\} \quad . \quad . \quad (\mathrm{G.})$$

Further,

$$\begin{aligned} Q_a\,Q_{-a} &= w^2 + a^2 + b^2 + \dots + r^2 = \mu^2,\\ Q_{a_,}Q_{-a_,} &= w_,^2 + a_,^2 + b_,^2 + \dots + r_,^2 = \mu_,^2,\\ Q_{a_{,,}}Q_{-a_{,,}} &= w_{,,}^2 + a_{,,}^2 + b_{,,}^2 + \dots + r_{,,}^2 = \mu_{,,}^2; \end{aligned}$$

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* The indulgent reader is requested to correct the following errors in the preceding part of this paper.

Page 450, line 14 from top, *for* e·db *read* e·bd.
— 456, — 20 from top, *add* the terms in.
— 459, — 4, 14, 18 from bottom, *erase* all.
— 459, — 4, 5, 18 from bottom, *for* fifteen *read* seven.

for every term of the product $Q_a\,Q_{-a}$ which is not in μ^2, is cancelled by virtue of the property common to all the $2n+1$ imaginaries,

$$m_\circ\, n_\circ + n_\circ\, m_\circ = 0.$$

Wherefore

$$\mu^2\,\mu_{\prime}^{\,2} = Q_a\,Q_{-a}\,Q_{a_{\prime}}\,Q_{-a_{\prime}}.$$

Now since $Q_{a_{\prime}}Q_{-a_{\prime}}$ is real, $Q_{-a}\cdot Q_{a_{\prime}}Q_{-a_{\prime}}$ cannot differ from $Q_{a_{\prime}}Q_{-a_{\prime}}\cdot Q_{-a}$; for

$$A+\sqrt{-1}B = A+B\sqrt{-1};$$

and $A+\sqrt{-1}B$ is a case of $Q_{-a}\cdot Q_{a_{\prime}}Q_{-a_{\prime}}$, namely that case in which the $(2n+1)$ imaginaries are reduced to any one of them. We say boldly, then,

$$Q_{-a}\,Q_{a_{\prime}}\,Q_{-a_{\prime}} = Q_{a_{\prime}}\,Q_{-a_{\prime}}\,Q_{-a},$$

whence

$$\mu^2\mu_{\prime}^{\,2} = Q_aQ_{-a}\,Q_{a_{\prime}}\,Q_{-a_{\prime}} = Q_aQ_{a_{\prime}}\,Q_{-a_{\prime}}Q_{-a} = (Q_{a_{\prime\prime}}+R)(Q_{-a_{\prime\prime}}-R)$$
$$= Q_{a_{\prime\prime}}Q_{-a_{\prime\prime}} - Q_{a_{\prime\prime}}R + RQ_{-a_{\prime\prime}} - RR.$$

$-Q_{a_{\prime\prime}}R+RQ_{-a_{\prime\prime}}$ represents a sum of imaginary terms of the form $-(c_\circ\cdot b_\circ\, n_\circ + b_\circ\, n_\circ\cdot c_\circ)c_{\prime\prime}B_n$. That these are none of them real, is plain from the consideration that $b_\circ\, n_\circ$ has no equivalent single imaginary, whence that $b_\circ\, n_\circ = \pm c_\circ$ is impossible.

$-RR$ has two kinds of terms. One kind is a sum of imaginary quantities of the form $-(b_\circ n_\circ\cdot d_\circ l_\circ + d_\circ l_\circ\cdot b_\circ n_\circ)B_n\,D_l$, which cannot be real; for if $b_\circ\, n_\circ\cdot d_\circ\, l_\circ$ had any real value, $b_\circ\, n_\circ$ would have a value different from that equivalent duad which alone it can represent, and which does not appear in the condition-function R. The other kind are all of the form $-b_\circ\, i_\circ\cdot b_\circ\, i_\circ\, B_i^{\,2}$.

Now

$$b_\circ\, i_\circ\cdot b_\circ\, i_\circ = -i_\circ\, b_\circ\cdot b_\circ\, i_\circ = -i_\circ\cdot b_\circ\cdot b_\circ\cdot i_\circ,$$
$$= -i_\circ\cdot b_\circ^{\,2}\cdot i_\circ = -i_\circ\cdot i_\circ\, b_\circ^{\,2},$$

since $b_\circ^{\,2}$ is real;

$$= -i_\circ^{\,2} b_\circ^{\,2} = -1;$$
$$\therefore\ -b_\circ\, i_\circ\, b_\circ\, i_\circ B_i^{\,2} = B_i^{\,2}.$$

Let

$$\mathfrak{B}^2 = B_i^{\,2} + \ldots + B_l^{\,2} + \ldots\ldots + B_n^2 + \ldots\ldots \text{\&c.},$$

or = the sum of the squares of all the real quantities in the condition-function R: then, equating real quantities, we have the very interesting result,

$$\mu\mu_{\prime}^{\,2} = \mu_{\prime\prime}^{\,2} + \mathfrak{B}^2;$$

or the following

Theorem: *The product of two sums of* $[2\cdot(3k+h)+8]$ *squares is always a sum of*

$$[(3k)^2+3k\cdot(2h+5)+h\cdot(h+7)+8] \textit{ squares,}$$

where k *and* h *are positive or nothing, and* h *not* > 2.

Cor. *The product of two sums of* $[2\cdot(3k+h)+8]$ *squares is always a sum of* $[2\cdot(3k+h)+8]$ *squares, if* $[4\cdot(3k+h)+12]$ *of the roots in the two factors are such as will satisfy* $[(3k)^2+3k(2h+3)+h\cdot(h+5)]$ *assignable conditions, viz. the conditions* $\mathfrak{B}^2=0$.

In the conditions $\mathfrak{B}^2=0$, the four quantities $w\ w_{,}\ a\ a_{,}$ do not occur: these are therefore arbitrary; and if the conditions are satisfied by $(4n-4)$ of the given $4n$ numbers, we can make $w=w_{,}=0$, and $\frac{a}{a_{,}}$ can always be so taken as to cause one of the quantities $(w_{,,}\,a_{,,}\,b_{,,}\,c_{,,}$, &c.) to vanish; so that we then shall have the product of two sums of $(2n-1)$ squares equal to a sum of $(2n-1)$ squares.

A glance at the functions $B_{\iota}C_{\iota}\ldots B_{l}C_{l}\ldots$, &c. will suggest a simple relation between the numbers $(bc\ldots r)$ and $(b_{,}c_{,}\ldots r_{,})$, by which the conditions $\mathfrak{B}^2=0$ are all at once satisfied; and as a case of the preceding corollary, we have the theorem,

$$(w^2+a^2+b^2+b_{,}^2+b_2^2+\ldots+b_{2n-3}^2)(w_{,}^2+a_{,}^2+m^2.[b^2+b_{,}^2+b_2^2+\ldots+b_{2n-3}^2])$$

$$=\mathrm{a}^2+\mathrm{a}_{,}^2+\mathrm{a}_2^2+\ldots+\mathrm{a}_{2n-1}^2;$$

whatever be the numbers in the first member of this equation.

We have proved, that, whenever the real quantities in two pluquaternions of $(2n-1)$ imaginaries are so related among each other that the product of the two functions is also a pluquaternion, the product of the $2n$ squares $(w^2+a^2+\ldots+r^2)$ into the $2n$ squares $(w_{,}^2+a_{,}^2+b_{,}^2+\ldots+r_{,}^2)$ is always equal to the sum of the $2n$ squares $(w_{,,}^2+a_{,,}^2+b_{,,}^2+\ldots+r_{,,}^2)$; and that the product of any $2n$ squares into a sum of any other $2n$ squares can always be reduced to a given even number of squares. The conditions that a sum of ten squares into a sum of ten squares should give a product of ten squares, we find to be not more than six, among sixteen of the roots in the factors; while the number of conditions required that the like should hold for sums of twelve squares, we have ascertained to be at the most fourteen, among twenty of the twenty-four quantities.

In the equation

$$\mu^2\mu_{,}^2=\mu_{,,}^2-\mathrm{Q}_{a_{,,}}\mathrm{R}+\mathrm{R}\mathrm{Q}_{-a_{,,}}-\mathrm{R}\mathrm{R},$$

the imaginaries must of necessity destroy each other, and that in every term. From this it follows, that the duad imaginaries of the condition-function possess the properties

$$(b_\circ n_\circ)^2 = (d_\circ l_\circ)^2 = \,..\, = -1,$$
$$b_\circ n_\circ \cdot d_\circ l_\circ + d_\circ l_\circ \cdot b_\circ n = 0,$$
$$c_\circ \cdot b_\circ n_\circ + b_\circ n_\circ \cdot c_\circ = 0.$$

And if it can be proved that

$$c_\circ \cdot b_\circ n_\circ = b_\circ \cdot n_\circ c_\circ$$

in all cases, it will follow, that whenever $b_\circ \cdot n_\circ c_\circ$ *is imaginary*,

$$b_\circ \cdot n_\circ c_\circ + b_\circ n_\circ \cdot c_\circ = 0.$$

By equations (G.), for any two pluquaternions of $(2n-1)$ imaginaries,

$$\mathrm{Q}_{a_\prime}\mathrm{Q}_{-a} + \mathrm{Q}_a\mathrm{Q}_{-a_\prime} = 2(\mathrm{A}+\mathrm{B})$$
$$= 2(\varpi\varpi_\prime + aa_\prime + \,\ldots + rr_\prime);$$

wherefore, $\mathrm{Q}_a\mathrm{Q}_{a_\prime}$ being $(\mathrm{Q}_{a_{\prime\prime}} + \mathrm{R})$,

$$\mathrm{Q}_{a_\prime}\mathrm{Q}_{-a_{\prime\prime}} + \mathrm{Q}_{a_{\prime\prime}}\mathrm{Q}_{-a_\prime} = 2(\varpi_\prime\varpi_{\prime\prime} + a_\prime a_{\prime\prime} + b_\prime b_{\prime\prime} + \,\ldots\, + r_\prime r_{\prime\prime}).$$

But, by equations (F.),

$$\mathrm{Q}_{a_\prime}\mathrm{Q}_{-a_{\prime\prime}} + \mathrm{Q}_{a_{\prime\prime}}\mathrm{Q}_{-a_\prime} = \mathrm{Q}_{a_\prime}\mathrm{Q}_{-a_\prime}\mathrm{Q}_{-a} + \mathrm{Q}_a\mathrm{Q}_{a_\prime}\mathrm{Q}_{-a_\prime} + \mathrm{Q}_{a_\prime}\mathrm{R} - \mathrm{R}\mathrm{Q}_{-a_\prime}$$
$$= (\mathrm{Q}_{-a} + \mathrm{Q}_a)\mathrm{Q}_{a_\prime}\mathrm{Q}_{-a_\prime},$$

since $\mathrm{Q}_{a_\prime}\mathrm{Q}_{-a_\prime}$ is real;

$$= 2\varpi \cdot \mu_\prime^{\,2};$$

therefore

$$\varpi = (\varpi_\prime\varpi_{\prime\prime} + a_\prime a_{\prime\prime} + \,\ldots\, + r_\prime r_{\prime\prime}) \cdot (\varpi_\prime^{\,2} + a_\prime^{\,2} + \,\ldots\, + r_\prime^{\,2})^{-1};$$

and in the same way can be proved

$$\varpi_\prime = (\varpi\varpi_{\prime\prime} + aa_{\prime\prime} + \,\ldots\, + rr_{\prime\prime}) \cdot (\varpi^2 + a^2 + \,\ldots\, + r^2)^{-1}.$$

Let $a_\circ\,\mathrm{Q}_a = \mathrm{Q}'_a$, and $a_\circ\,\mathrm{Q}_{a_{\prime\prime}} = \mathrm{Q}'_{a_{\prime\prime}}$: Q'_a and $\mathrm{Q}'_{a_{\prime\prime}}$ are plainly pluquaternions, whose real terms are $-a$ and $-a_{\prime\prime}$ instead of ϖ and $\varpi_{\prime\prime}$. We can therefore deduce by the preceding reasoning, from the equation $\mathrm{Q}'_a\mathrm{Q}_{a_\prime} = \mathrm{Q}'_{a_{\prime\prime}} + a_\circ\,\mathrm{R}$, the value of a in terms of $\varpi_\prime\, a_\prime \,..\, r_\prime\, \varpi_{\prime\prime}\, a_{\prime\prime} \ldots r_{\prime\prime}$; for $(\mathrm{Q}_{a_\prime} \cdot a_\circ\,\mathrm{R} - a_\circ\,\mathrm{R}\mathrm{Q}_{-a_\prime})$, like $(\mathrm{Q}_{a_\prime}\mathrm{R} - \mathrm{R}\mathrm{Q}_{-a_\prime})$ above, must vanish, and by a like necessity. In the product $\mathrm{Q}_a\,\mathrm{Q}_{a_\prime}$, every function $m_{\prime\prime}$ of $\mathrm{Q}_{a_{\prime\prime}}$ affected by the imaginary $m_\circ$, is of the form $m\varpi_\prime + \varpi m_\prime + \mathrm{M}$. In the product $\mathrm{Q}_{a_\prime}\mathrm{Q}_a$, the same term takes the form $m\varpi_\prime + \varpi m_\prime - \mathrm{M}$: let this be $m'_{\prime\prime}$; and let $\mathrm{Q}_{a_{\prime\prime}}$ become $\mathrm{Q}''_{a_{\prime\prime}}$, after all the substitutions, $m'_{\prime\prime}$ for $m_{\prime\prime}$, &c. are made. Then, reasoning as before upon the equation $\mathrm{Q}'_{a_\prime}\mathrm{Q}_a = a_\circ Q''_{a_{\prime\prime}} - a_\circ\,\mathrm{R}$, where $a_\circ\,\mathrm{Q}_{a_\prime} = \mathrm{Q}'_{a_\prime}$, we can obtain $a_\prime$, expressed in terms of $\varpi a b \ldots r \varpi_{\prime\prime} a'_{\prime\prime} b'_{\prime\prime} \ldots r'_{\prime\prime}$.

These curious equations of the form $w=(w_{,}w_{,,}+a_{,}a_{,,}+ \,.\,.+ r_{,}r_{,,})\cdot\mu_{,}^{-2}$, as well as $\mu^2\mu^2_{,}=\mu^2_{,,}$, are familiar to those who are acquainted with the masterly Theory of Quaternions invented by Sir William R. Hamilton, by whom they have all been discovered several years ago, for the case of three imaginaries; and as the process of finding w, a, b, c in terms of $w_{,}\,w_{,,}\,a_{,}\,a_{,,}$, &c. is called by him Division of Quaternions, so may the extension of the process to cases of more than three imaginaries be called Division of Pluquaternions.

We have here arrived at these remarkable results by purely *à priori* reasoning; and we accept them at once, without asking for the application of any test, or troubling ourselves about the multitude of vanishing terms.

In our first definitions of the imaginaries, and in the supposition that, if possible, the product of two pluquaternions shall be a pluquaternion, irrespective of all limitations upon their real quantities, we have found it to be of necessity implied, that the form of that product should be in general, for $(2m+7)$ imaginaries,

$$Q_a\,Q_{a_{,}}=Q_{a_{,,}}+R_m;$$

and, from the condition that this product is congruously formed, (a condition imposed by our first supposition), no matter in which of the various practicable ways the multiplication is consistently effected,—before we have examined, before we have constructed, before we have even learned how to construct, the functions $w_{,,}\,a_{,,}\,b_{,,}$, &c.—we conclude inevitably, that

$$\mu^2\mu_{,}^2=\mu_{,,}^2+\mathfrak{B}^2,$$

and that

$$w=(w_{,}w_{,,}+a_{,}a_{,,}+\ \ldots\ +r_{,}r_{,,})\mu_{,}^{-2}.$$

After what has been delivered concerning the different forms of the product of two pluquaternions, whose constituents are all given *positive* numbers, it becomes an easy problem in combinations to find the number of ways in which the sum of squares, $\mu_{,,}^2+\mathfrak{B}^2$, can be assigned equal to the product $\mu^2\mu_{,}^2$. For instance, $\mu_{,,}^2$, the sum of the squares of the constituents of $Q_{a_{,,}}$, *any one* of the 480 forms of the product of two given biquaternions Q_a and $Q_{a_{,}}$, whose constituents are $(wa\,.\,.\,g)$, $(w_{,}a_{,}\,.\,.\,g_{,})$, *all of determined signs*, is equal to $(w^2+a^2+\,.\,.+g^2)$ $(w_{,}^2+a_{,}^2+\,.\,.+g_{,}^2)=\mu^2\mu_{,}^2$. If now in all the functions $w_{,,}a_{,,}\ldots g_{,,}$, the eight roots in $\mu_{,,}^2$, we change the sign of any one or more of the sixteen numbers $w\ldots g$ $w_{,}\ldots g_{,}$, we shall, unless we change the signs of all the sixteen together, obtain in every case a different set of roots $w_{,,}a_{,,}\ldots g_{,,}$; and, except in a small number of the cases, a different set of squares $w_{,,}^2+a_{,,}^2+\ldots+g_{,,}^2$;

of which sets no one can be identical with $\mu_{\prime\prime}'^2$, the sum of the squares of the constituents of $Q'_{a_{\prime\prime}}$, *any other* of the 480 abovementioned forms; because $w_{\prime\prime}$, and the terms of $a_{\prime\prime}b_{\prime\prime}\,..\,g_{\prime\prime}$ which contain w and $w_{\prime}$, will vary through all these sets, while they are invariable through all these forms. If, in the constituents of $Q_{a_{\prime\prime}}$ the first here mentioned, we change the signs either of all of $wa\,..\,g$, or of all of $w_{\prime}a_{\prime}\,...\,g_{\prime}$, we shall simply reverse the signs of all the roots in $\mu_{\prime\prime}^2$; if we change the signs of the eight $wabcw_{\prime}a_{\prime}b_{\prime}c_{\prime}$, indicated by *any* triplet $a_{\circ}\,b_{\circ}\,c_{\circ}$ in the system of seven, or if we change the signs of the eight $bcdeb_{\prime}c_{\prime}d_{\prime}e_{\prime}$, indicated by any two triplets $a_{\circ}\,b_{\circ}\,c_{\circ}$, $a_{\circ}\,d_{\circ}\,e_{\circ}$, having *any* common imaginary, we shall merely reverse the signs of certain of those roots. Finally, after making all these variations of sign of the constituents of Q_a and $Q_{a_{\prime}}$, we may essentially alter Q_a and $Q_{a_{\prime}}$ by exchanging in either or in both of them any other constituent, as c or $c_{\prime}$, for the real term w or $w_{\prime}$, and thus give rise to another series of variations of the eight squares $w_{\prime\prime}^2 a_{\prime\prime}^2\,...\,g_{\prime\prime}^2$.

Making now a slight change in our notation, for clearness sake, let

$$Q_i,\ Q_i',\ Q_i'',\ \ldots\ldots,\quad Q_{-i},\ Q'_{-i},\ Q''_{-i}\ \ldots\ldots,$$

represent any pluquaternions of i imaginaries, the second set in order differing from the first only in the signs of all the i. Let $\mathfrak{Q}_i$ represent a pluquaternion which arises in the product of two or more, of i imaginaries, or *of the order i*. And let

$$Q_i\,Q'_i = \mathfrak{Q}_i + \mathfrak{R}_i,$$

$\mathfrak{R}_i$ being the function R_m, of the second degree, already found. If

$$2m = \overline{a+1} + \overline{b+1} + \overline{c+1} + \ldots + \overline{n+1},$$

to n terms, when $a \mathrel{\bar{>}} b$, $b \mathrel{\bar{>}} c$, &c. . . , and if $\mu^2\,\mu_{\prime}^2$ be any two sums each of $2m$ squares; then

$$\mu^2 = Q_a\,Q_{-a} + Q_b\,Q_{-b} + Q_c\,Q_{-c} + \ldots + Q_n\,Q_{-n},$$

$$\mu_{\prime}^2 = Q'_aQ'_{-a} + Q'_bQ'_{-b} + Q'_cQ'_{-c} + \ldots + Q'_n\,Q'_{-n};$$

$$\begin{aligned}
\mu^2\mu_{\prime}^2 = [Q_aQ_{-a}Q'_aQ'_{-a} = Q_aQ'_aQ'_{-a}Q_{-a}] &= (\mathfrak{Q}_a + \mathfrak{R}_a)(\mathfrak{Q}_{-a} - \mathfrak{R}_a) + \\
[Q_aQ_{-a}Q'_bQ'_{-b} = Q_aQ'_bQ'_{-b}Q_{-a} =\,] &(\mathfrak{Q}'_a + \mathfrak{R}'_a)(\mathfrak{Q}'_{-a} - \mathfrak{R}'_a) + \\
.\qquad .\qquad .\qquad .\qquad .\qquad . &\qquad\qquad + \\
[Q_bQ_{-b}Q'_aQ'_{-a} = Q_bQ'_aQ'_{-a}Q_{-b} =\,] &(\mathfrak{Q}_a^{(n+1)} + \mathfrak{R}_a^{(n+1)})(\mathfrak{Q}_{-a}^{(n+1)} - \mathfrak{R}_a^{(n+1)}) + \\
[Q_bQ_{-b}Q'_bQ'_{-b} = Q_bQ'_bQ'_{-b}Q_{-b} =\,] &(\mathfrak{Q}_b + \mathfrak{R}_b)(\mathfrak{Q}_{-b} - \mathfrak{R}_b) + \\
[Q_bQ_{-b}Q'_cQ'_{-c} = Q_b\,Q'_cQ'_{-c}Q_{-b} =\,] &(\mathfrak{Q}'_b + \mathfrak{R}'_b)(\mathfrak{Q}'_{-b} - \mathfrak{R}'_b) + \\
\&c. \qquad\qquad \&c.;
\end{aligned}$$

for Q'_b in the second line, and Q_b in the third above written line, are forms of Q_a, which have $(a-b)$ zero constituents; and Q'_c in the last written line is a form of Q_b. Hence, if $\mathfrak{m}^2_a+\mathfrak{B}^2_a$ be the sum of the squares of the real quantities in $\mathfrak{Q}_a+\mathfrak{R}_a$,

$$\mu^2\mu_{\prime}{}^2=(\mathfrak{m}^2_a+\mathfrak{B}^2_a)+(\mathfrak{m}'^2_a+\mathfrak{B}'^2_a)+\ldots+(\mathfrak{m}^2_b+\mathfrak{B}^2_b)+(\mathfrak{m}'^2_b+\mathfrak{B}'^2_b)+\ldots$$

(to n^2 terms).

Let $a=7=b=c=\ .\ .\ =n$; then $\mathfrak{B}^2_a=0$, and we have the theorem,

**The product of any two sums, each of* $8n$ *squares, is always reducible to* $8n^2$ *squares.*

$(\mathfrak{m}^2_{15}+\mathfrak{B}^2_{15})$ is always forty-six squares, which is therefore not the least number to which the product of two sums of sixteen squares is reducible. It is thus proved also, that (k not < 0; h not < 0, not > 2).

The product of any two sums, each of $n\cdot(6k+2h+8)$ *squares, is reducible to a sum of* $n^2[(3k)^2+3k(2h+5)+h\cdot(h+7)+8]$ *squares.*

Let now

$$i=2m+7=6\cdot n+1-2e,$$

where e is either $=2$ or $=0$. We know that

$$Q'_iQ_i=\mathfrak{Q}_i+R_m,$$

R_m being of the second degree as to its imaginaries. Let

$$S_m,\ T_m,\ \ldots\ Y_m,\ Z_m,$$

represent functions of the third, fourth, ... $\overline{n-1}$th, nth degree as to their imaginaries, the functions having no real term; that is, let S_m be a sum of terms such as $b_{\circ}\,i_{\circ}\,p_{\circ}\,B_{ip}$, and T_m of terms such as $c_{\circ}\,l_{\circ}\,r_{\circ}\,v_{\circ}\,C_{lrv}$. Then, paying attention only to the *form* of the products in the *second* members of the equations which follow,

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* This theorem and its demonstration were suggested by the following proof, kindly communicated to me by its discoverer Prof. J. R. Young, that the product of two sums of sixteen squares is reducible to a sum of thirty-two squares; a proof certainly simpler and more elegant than that of the same property given in the text, and containing, in fact, the whole theorem.

Let $\Sigma_8 q$ denote a sum of eight squares:

$$\because\ \Sigma_8 q\Sigma_8 q'=\Sigma_8 q''\ \therefore$$

$$\begin{aligned}\Sigma_{16}q'\Sigma_{16}q''&=(\Sigma_8 q+\Sigma_8 q')(\Sigma_8 q''+\Sigma q''')\\&=\Sigma_8 q\Sigma_8 q''+\Sigma_8 q'\Sigma_8 q'''+\Sigma_8 q'\Sigma_8 q''+\Sigma_8 q\Sigma_8 q'''\\&=\Sigma_8 q_{\prime}+\Sigma_8 q_{\prime\prime}+\Sigma_8 q_{\prime\prime\prime}+\Sigma_8 q_{\prime\prime\prime\prime}\\&=\Sigma_{32}q'.\end{aligned}$$

$$Q_i''Q_i'Q_i=\mathfrak{Q}_i+R_m+S_m,$$

$$Q_i'''Q_i''Q_i'Q_i=\mathfrak{Q}_i+R_m+S_m+T_m,$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot$$

$$Q_i^{(n-2)}Q_i^{(n-3)}\ldots . Q'_iQ_i=\mathfrak{Q}_i+R_m+S_m+T_m+\ldots+Y_m,$$

$$Q_i^{(n-1)}Q_i^{(n-2)}\ldots . Q'_iQ_i=\mathfrak{Q}_i+R_m+S_m+T_m+\ldots .+Y_m+Z_m,$$

where $R_m\,S_m$ &c. are in every line the same, as to the form and number of their imaginaries; inasmuch as each of them, when first it appears, is supposed to contain all those of its own degree, or equivalents of all those, which cannot be reduced to simpler forms. This follows of necessity, although we know nothing as yet about the number of terms in S_m, T_m &c., from the property already proved, that no duad $b_0\,n_0$, or $r_0\,v_0$, made with any two imaginaries taken from any two complete systems of seven triplets, can be reduced to a *monad*, or to an imaginary of the first degree. In the product, therefore, of three or of four pluquaternions, if $b_0\,n_0\,p_0$ and $b_0\,i_0\,r_0\,v_0$ be a triplet and a quadruplet, of which neither contains two monads taken from the same system of seven triplets, $b_0\,n_0\,p_0$, or both $b_0\,n_0\,p_0$ and $b_0\,i_0\,r_0\,v_0$, or else their equivalents of the same degree, must be exhibited; since no substitution can be made that will reduce either of these to a multiplet of a lower degree. And since $i(=6n+1$, or $6n-3)$ imaginaries must be arranged either in n complete systems of seven, or in $(n-1)$, and one additional triplet, the product of n pluquaternions, Q_i &c., will exhibit Z_m, a function of multiplets of the nth degree.

If we now multiply the above expression for the product by $Q_i^{(n)}$, any $(n+1)$th pluquaternion of the same *order*, it is not at first sight impossible, or improbable, that, since we have only n complete systems of seven triplets at the most, all the $(n+1)$plets of this product may reduce themselves to multiplets of a lower degree; so that the same expression shall represent the product of $(n+1)$, and consequently of $(n+r)$, pluquaternions of the order i.

The importance of the question, how far such reduction can be made, will appear from the following reasoning.

It has been already proved, that

$$Q'_i\,Q_i=\mathfrak{Q}_i+R_m,$$

$$Q_{-i}Q'_{-i}=\mathfrak{Q}_{-i}-R_m,$$

and that

$$\mathfrak{Q}_iR_m-R_m\mathfrak{Q}_{-i}=0,$$

by the mutual destruction of the imaginaries, independently of the constituents; that is, whatever be Q_i'' or R_m, of these forms,

$$Q''_i R_m = R_m Q''_{-i},$$

the same triplets being employed. Further,

$$Q_i''\cdot Q_i' Q_i = Q_i''\mathfrak{Q}_i + Q_i'' R_m = \mathfrak{Q}_i + R_m + S_m\cdot$$

$$Q_{-i}Q'_{-i}\cdot Q''_{-i} = \mathfrak{Q}_{-i}Q''_{-i} - R_m Q''_{-i}$$

$$= \mathfrak{Q}_{-i}Q''_{-i} - Q_i'' R_m = \mathfrak{Q}_{-i} - R_m - S_m\cdot$$

$$Q_i''Q_i'Q_i\cdot Q_{-i}Q'_{-i}Q''_{-i} = Q_i''Q_i'\mu^2 Q'_{-i}Q''_{-i} = Q_i''Q_i'Q'_{-i}\mu^2 Q''_{-i}$$

$$= Q_i''\mu_{/}^2\mu^2 Q''_{-i} = Q_i''Q''_{-i}\mu_{/}^2\mu^2 = \mu_{//}^2\mu_{/}^2\mu^2;$$

$$\therefore \mu_{//}^2\mu_{/}^2\mu^2 = \mathfrak{Q}_i\mathfrak{Q}_{-i} - \mathfrak{Q}_i(R_m + S_m) + (R_m + S_m)\mathfrak{Q}_{-i} - (R_m + S_m)^2,$$

$$= \mathfrak{Q}_i\mathfrak{Q}_{-i} - \mathfrak{Q}_i S_m + S_m\mathfrak{Q}_{-i} - R_m R_m - R_m S_m - S_m R_m - S_m S_m,$$

if μ, $\mu_{/}$, $\mu_{//}$ be the moduli of Q_i, Q_i', Q_i''.

$\mathfrak{Q}_i\mathfrak{Q}_{-i}$ is real, and $=\mathfrak{m}^2$, if $\mathfrak{m}$ be the modulus of $\mathfrak{Q}_i$;

$-R_m R_m$ we know to be $=\mathfrak{B}^2 +$ certain imaginary terms which all vanish, where $\mathfrak{B}^2$ is the sum of the squares of the constituents of R; and it is evident, that all the imaginary products in the five terms containing S_m must destroy each other likewise.

For in $\mathfrak{Q}_i S_m$ every term is of the form $c_\circ\cdot b_\circ n_\circ p_\circ\cdot C$, which is not real; since, if

$$c_\circ\cdot b_\circ n_\circ p_\circ = \pm 1,$$

$$-b_\circ n_\circ p_\circ = \pm c_\circ,$$

and $b_\circ n_\circ p_\circ$ would be reducible to a simpler form, contrary to the supposition which we have a right to make concerning all the imaginary triplets in S_m.

In $R_m S_m$ also every term is imaginary; for if, *e. g.*,

$$c_\circ l_\circ\cdot b_\circ n_\circ p_\circ = \pm 1,$$

$$-l_\circ\cdot b_\circ n_\circ p_\circ = \pm c,$$

and

$$b_\circ n_\circ p_\circ = \pm l_\circ c_\circ,$$

a contradiction of a similar kind.

In $-S_m S_m$, the only real terms are of the form

$$-b_\circ n_\circ p_\circ\cdot b_\circ n_\circ p_\circ\, B^2_{np}.$$

Now

$$b_\circ\cdot n_\circ p_\circ = -n_\circ p_\circ\cdot b_\circ,$$

because

$$Q_i R_m = R_m \cdot Q_{-i};$$

wherefore

$$-(b_\circ \cdot n_\circ p_\circ)(b_\circ \cdot n_\circ p_\circ) = n_\circ p_\circ \cdot b_\circ \cdot b_\circ \cdot n_\circ p_\circ = -n_\circ p_\circ \cdot n_\circ p_\circ$$
$$= -(-1) = 1;$$

or

$$-S_m S_m = +B_{ip}^2 + B_{lp}^2 + B_{np}^2 + \ldots + C_{ip}^2 + \&c.$$

+ certain terms containing imaginaries of the sixth degree.

It is too evident to require proof that these latter terms in $-S_m S_m$, as well as the imaginary terms of the fifth degree in

$$-R_m S_m - S_m R_m,$$

and those of the fourth degree in

$$-\mathfrak{Q}_i S_m + S_m \mathfrak{Q}_{-i},$$

must respectively in pairs destroy each other; wherefore, whatever be Q_i''',

$$Q_i''' S_m = S_m Q'''_{-i};$$

or, if

$$\mathfrak{R}_m = R_m + S_m,$$

whose modulus is $\mathfrak{B}_l$,

$$Q_i''' \mathfrak{R}_m = \mathfrak{R}_m Q'''_{-i};$$

$$\mu_{ll}^2 \mu_l^2 \mu^2 = \mathfrak{m}^2 + \mathfrak{B}_l^2.$$

Further,

$$Q_i''' \cdot Q_i'' Q_i' Q_i = Q_i''' \mathfrak{Q}_i + Q_i''' \mathfrak{R}_m = \mathfrak{Q}_i + \mathfrak{R}_m + T_m$$

$$Q_{-i} Q'_{-1} Q''_{-i} \cdot Q'''_{-i} = \mathfrak{Q}_{-i} Q'''_{-i} - \mathfrak{R}_m Q'''_{-i}$$
$$= \mathfrak{Q}_{-i} Q'''_{-i} - Q'''_i \mathfrak{R}_m = \mathfrak{Q}_{-i} - \mathfrak{R}_m - T_m,$$

when T_m is supposed to be reduced to as few terms as possible, the number of which we do not yet know, any more than that of the terms in $\mathfrak{R}_m = R_m + S_m$.

$$Q_i''' Q_i'' Q_i' Q_i \cdot Q_{-i} Q'_{-i} Q''_{-i} Q'''_{-i} = Q_i''' Q_i'' Q_i' \mu^2 Q'_{-i} Q''_{-i} Q'''_{-i}$$
$$= Q_i'''' Q_i'' Q_i' Q'_{-i} \mu^2 Q''_{-i} Q'''_{-i} = Q_i''' Q_i'' \mu_l^2 \mu^2 Q''_{-i} Q'''_{-i}$$
$$= Q_i''' Q_i'' Q''_{-i} \mu_l^2 \mu^2 Q'''_{-i} = Q_i''' \mu^2_{ll} \mu^2_l \mu^2 Q'''_{-i}$$
$$= Q_i''' Q'''_{-i} \mu^2_{ll} \mu^2_l \mu^2 = \mu^2_{lll} \mu^2_{ll} \mu^2_l \mu^2;$$

$$\therefore \mu^2_{lll} \mu^2_{ll} \mu^2_l \mu^2 = \mathfrak{Q}_i \mathfrak{Q}_{-i} - \mathfrak{Q}_i \cdot (\mathfrak{R}_m + T_m) + (\mathfrak{R}_m + T_m) \mathfrak{Q}_{-i}$$
$$- (\mathfrak{R}_m + T_m)^2.$$

$$= \mathfrak{m}^2 - \mathfrak{Q}_i T_m + T_m \mathfrak{Q}_{-i} - \mathfrak{R}_m \mathfrak{R}_m - \mathfrak{R}_m T_m - T_m \mathfrak{R}_m - T_m T_m.$$

By reasoning as before it can be shown, that the only real terms in $-\mathfrak{R}_m\mathfrak{R}_m$ are the positive squares in $-R_m R_m$ and $-S_m S_m$, and that the imaginary terms in it, of the fourth, fifth, and sixth degrees, mutually destroy each other in pairs. It is shown also by reasons like those adduced in the discussion of $\mathfrak{Q}S_m$ and of $R_m S_m$, that the imaginaries of the sixth as well as those of the seventh degree in

$$-\mathfrak{R}_m T_m - T_m \mathfrak{R}_m,$$

every term of which is imaginary, destroy each other in pairs.

Of $-T_m T_m$, we can prove, as before in the treatment of $-S_m S_m$, that the only real terms are of the form

$$-(blpv \cdot blpv) B^2_{lpv}.$$

Now

$$b_\circ \cdot l_\circ p_\circ v_\circ = -l_\circ p_\circ v_\circ \cdot b_\circ,$$

because

$$Q_i''' S_m = S_m Q'''_{-i};$$

wherefore

$$-(b_\circ \cdot l_\circ p_\circ v_\circ \cdot)(b_\circ \cdot l_\circ p_\circ v_\circ) = l_\circ p_\circ v_\circ \cdot b_\circ^2 \cdot l_\circ p_\circ v_\circ = -l_\circ p_\circ v_\circ \cdot l_\circ p_\circ v_\circ$$
$$= -(-1) = 1.$$

$-T_m T_m$ is equal therefore to a certain number of positive squares ($+ B^2_{lpv}$ &c.) + certain imaginaries of the eighth degree, which must destroy each other in pairs.

Of $\mathfrak{Q}_i T_m$ we can prove, by considerations like those employed in discussing $\mathfrak{Q}_i S_m$, that every term is imaginary. Hence all the imaginaries of the fifth degree in

$$-\mathfrak{Q}_i T_m + T_m \mathfrak{Q}_{-i}$$

must vanish in pairs; and, whatever be Q_i'''', if

$$\mathfrak{R}_m + T_m = \mathfrak{R}'_m,$$

whose modulus is $\mathfrak{B}_{\prime\prime}$, we obtain

$$\mu^2_{\prime\prime\prime}\mu^2_{\prime\prime}\mu^2_{\prime}\mu^2 = \mathfrak{m}^2 + \mathfrak{B}^2_{\prime\prime},$$
$$Q_i'''' \mathfrak{R}'_m = \mathfrak{R}'_m Q''''_{-i},$$
$$Q_i'''' \cdot Q_i''' Q_i'' Q_i' Q_i = Q_i'''' \mathfrak{Q}_i + Q_i'''' \mathfrak{R}'_m,$$
$$Q_{-i} Q'_{-i} Q''_{-i} Q'''_{-i} \cdot Q''''_{-i} = \mathfrak{Q}_{-i} Q''''_{-i} - \mathfrak{R}'_m Q''''_{-i}.$$

By pursuing this analysis we shall at length obtain; if

$$R_m + S_m + T_m + \ldots + Y_m = \mathfrak{R}_m^{(n-4)};$$
$$Q_i^{(n-1)} \mathfrak{R}_m^{(n-4)} = \mathfrak{R}_m^{(n-4)} Q_{-i}^{(n-1)},$$
$$Q_i^{(n-1)} \cdot Q_i^{(n-2)} \ldots\ldots Q_i' Q_i = Q_i^{(n-1)} \mathfrak{Q}_i + Q_i^{(n-1)} \mathfrak{R}_m^{(n-4)}$$
$$= \mathfrak{Q}_i + \mathfrak{R}_m^{(n-4)} + Z_m,$$
$$Q_{-i} Q'_{-i} \ldots Q_{-i}^{(n-2)} \cdot Q_{-i}^{(n-1)} = \mathfrak{Q}_{-i} Q_{-i}^{(n-1)} - \mathfrak{R}_m^{(n-4)} Q_{-i}^{(n-1)}$$
$$= \mathfrak{Q}_{-i} - \mathfrak{R}_m^{(n-4)} - Z_m;$$

from which can be deduced, as before, if

$$\mathfrak{R}_m^{(n-4)} + \mathrm{Z}_m = \mathfrak{R}_{(i)},$$

whose modulus is $\mathfrak{B}_{(i)}$;

$$\mu_{n-1}^2 \mu_{n-2}^2 \ldots \mu_{\prime}{}^2 \mu^2 = \mathfrak{m}^2 + \mathfrak{B}_{(i)}^2,$$

$$\mathrm{Q}_i^{(n)} \mathfrak{R}_{(i)} = \mathfrak{R}_{(i)} \mathrm{Q}_{-i}^{(n)},$$

$$\mathrm{Q}_i^n \cdot \mathrm{Q}_i^{(n-1)} \ldots \mathrm{Q}_i' \mathrm{Q}_i = \mathrm{Q}_i^{(n)} \mathfrak{Q}_i + \mathrm{Q}_i^{(n)} \mathfrak{R}_{(i)} = \mathfrak{Q}_i + \mathfrak{R}_{(i)} + \Theta_i,$$

$$\mathrm{Q}_{-i} \mathrm{Q}'_{-i} \ldots \mathrm{Q}_{-i}^{(n-1)} \cdot \mathrm{Q}_{-i}^{(n)} = \mathfrak{Q}_{-i} \mathrm{Q}_{-1}^{(n)} - \mathfrak{R}_{(i)} \mathrm{Q}_{-i}^{(n)}$$
$$= \mathfrak{Q}_{-i} - \mathfrak{R}_{(i)} - \Theta_i ;$$

where Θ_i is a function of imaginaries of a degree higher than the nth. If we can now satisfy ourselves that all the $(n+1)$plets of these two products can be reduced to multiplets of lower degrees, or that $\Theta_i = 0$, we shall obtain by multiplication, as before,

$$\mu_n^2 \mu_{n-1}^2 \ldots \mu_{\prime}{}^2 \mu^2 = \mathfrak{m}^2 + \mathfrak{B}^2{}_{(i)},$$

or the product of $(n+1)$ sums, and consequently of $(n+r)$ sums, each of any $(2m+8)$ squares, is always reducible to a constant number of squares, which is known, as soon as we can assign the constant number of terms, if such a number exists, to which it is possible to reduce the product of $(n+r)$ pluquaternions of the order $i (= 6n+1$ or $6n-3)$.

We now proceed to inquire, not without interest in the result of our investigation, whether such a thing exists as *the complete pluquaternion product* of $(n+r)$ pluquaternions of i imaginaries, which is always of one form $(\mathfrak{Q}_i + \mathfrak{R}_{(i)})$, for every value of r. The subindex (i) may serve to characterize this product.

I flatter myself with the hope of establishing the propositions which follow:—[n not < 0, r not < 0].

The product of $(n+r)$ *pluquaternions of* $6n-3$ *imaginaries is always reducible to* 2^{2n} *terms.*

The product of $(n+r)$ *pluquaternions of* $(6n-1)$ *imaginaries is always reducible to* 2^{2n+1} *terms.*

The product of $(n+r)$ *pluquaternions of* $(6n+1)$ *imaginaries is always reducible to* 2^{2n+1} *terms.*

Of the truth of these propositions, once established, the inevitable consequences will be the following:—

The product of $(n+r)$ *sums, each of any* $(6n-2)$ *squares, is reducible to a sum of* 2^{2n} *squares.*

The product of $(n+r)$ *sums, each of any* $6n$ *squares, is reducible to a sum of* 2^{2n+1} *squares.*

The product of $(n+r)$ *sums, each of any* $(6n+2)$ *squares, is reducible to a sum of* 2^{2n+1} *squares.*

These results, formidable as they appear in extent, and in the labour which would be required to verify them, are capable of demonstration, if it be true that every multiplet imaginary is equivalent, either with or without a change of sign, to every permutation of itself; and if it be allowable to substitute, in any multiplet, for any two contiguous imaginaries which belong to the same system of seven triplets, that monad from the same system to which their product is equivalent.

I will not undertake to demonstrate that the first is true, and that the second is allowable. I will not even say that the argument is conclusive whereby I have endeavoured to prove that, in any imaginary triplet,

$$b\cdot np = n\cdot pb = p\cdot bn.$$

It is certain, however, that to deny this property of odd multiplets in general, implies a contradiction.

Let it be supposed, for the present, that it is possible to determine the signs of all the permutations of any irreducible multiplet, or that the signs of certain of them may be so assumed, as congruously to fix those of all the rest.

Let the i $(=6n+1=2m+7)$ imaginaries be a and the n sixes, $bcdefg$, $b_1c_1 \,..\, g_1$, $b_2c_2 \,..\, g_2$, $\ldots\, b_{n-1}\, c_{n-1} \ldots g_{n-1}$, with which n similar systems of seven triplets are constructed, a being the imaginary common to all the systems.

Let $\mathrm{A}=f_1c_3c_4g_6e_7$ be any quintuplet and irreducible imaginary arising in the product of five pluquaternions of the order i. The multiplication of these by any sixth pluquaternion will include the operation $\mathrm{Q}_i\mathrm{A}$, of which it will be here sufficient to attempt $(b+c+b_1+c_1+b_2+c_2+b_3+c_3)\mathrm{A}$: the triplets being

$$\begin{array}{ll} abc & ab_1c_1 \ldots\ldots c_1e_1f_1, \\ ade-bdf+cdg & ab_2e_2 \ldots\ldots c_2e_2f_2 \\ afg+beg+cef, & ab_3c_3 \ldots\ldots c_3e_3f_3, \text{ \&c.} \end{array}$$

The equal values of a give a series of conditions

$$\begin{array}{ll} b_\alpha c_\beta = c_\alpha b_\beta & b_\alpha e_\beta = c_\alpha d_\beta \\ b_\alpha b_\beta = -c_\alpha c_\beta & b_\alpha d_\beta = -c_\alpha e_\beta, \text{ \&c.} \end{array}$$

If A were f_1c_3, a duad in R_m, the operation $b\cdot f_1c_3$, which by definition of f_1c_3 is $b\cdot f_1\cdot c_3$, would be of doubtful sign; for there is no reason why $b\cdot f_1\cdot c_3$ should be considered either as $b\cdot f_1c_3$ or as $bf_1\cdot c_3$, the one rather than the other; and it has been shown, that $bf_1\cdot c_3 = -b\cdot f_1c_3$, is a consequence of

$$b\cdot f_1c_3 = f_1\cdot c_3b = c_3\cdot bf_1.$$

In all probability, the sign of $b\cdot\mathrm{A}$, or $b\cdot f_1c_3c_4g_6e_7$, when considered for convenience of reduction (when possible) as $bf_1 \cdot c_3c_4g_6e_7$, is arbitrary, whatever multiplet A may be, by an indeterminateness like that which we have met with in constructing a system of seven triplets. Let us leave it undetermined thus:

$$\pm b \cdot \mathrm{A} = (bf_1 \cdot c_3c_4g_6e_7 = -cg_1 \cdot c_3c_4g_6e_7 =) \pm cg_1c_3c_4g_6e_7,$$

because $b_\alpha f_\beta = -c_\alpha g_\beta$; and we do not stay here to ask whether the transformation of $cg_1 \cdot c_3c_4g_6e_7$ into $cg_1c_3c_4g_6e_7$ involves a change of sign or not. We suppose that the question is capable of being congruously decided, either in one way or in two ways.

$$\pm c \cdot \mathrm{A} = (cf_1 \cdot c_3c_4g_6e_7 = bg_1 \cdot c_3c_4g_6e_7 =) \pm bg_1c_3c_4g_6e_7,$$

because $b_\alpha g_\beta = c_\alpha f_\beta$. The object is here to show, that there is no necessity that b, d, or f with any subindex, should appear except in the first place of the resulting irreducible multiplet, when the subindices are arranged in ascending order.

$$\begin{aligned}
\pm b_1\mathrm{A} &= (b_1f_1 \cdot c_3c_4g_6e_7 = d_1 \cdot c_3c_4g_6e_7 =) \pm d_1c_3c_4g_6e_7,\\
\pm c_1\mathrm{A} &= (c_1f_1 \cdot c_3c_4g_6e_7 = -e_1 \cdot c_3c_4g_6e_7 =) \pm e_1c_3c_4g_6e_7,\\
\pm b_2\mathrm{A} &= (b_2f_1 \cdot c_3c_4g_6e_7 = g_1c_2 \cdot c_3c_4g_6e_7 =) \pm g_1c_2c_3c_4g_6e_7,\\
\pm c_2\mathrm{A} &= (c_2f_1 \cdot c_3c_4g_6e_7 = -f_1c_2 \cdot c_3c_4g_6e_7 =) \pm f_1c_2c_3c_4g_6e_7,\\
\pm b_3\mathrm{A} &= (b_3c_3 \cdot c_4g_6e_7f_1 = a \cdot c_4g_6e_7f_1 =) \pm b_4 \cdot g_6e_7f_1,\\
&= (\pm f_1b_4g_6e_7 = \mp g_1c_4g_6e_7 =) \pm g_1c_4g_6e_7,\\
\pm c_3\mathrm{A} &= (c_3c_3 \cdot c_4g_6e_7f_1 = -c_4g_6e_7f_1 =) \pm f_1c_4g_6e_7.
\end{aligned}$$

For $b_1f_1 = d_1$; $c_1f_1 = -e_1$; $b_2f_1 = g_1c_2$; $b_3c_3 = a$; $ac_4 = -b_4$; $f_1b_4 = -g_1c_4$. The equation $\pm b_3\mathrm{A} = \pm g_1c_4g_6e_7$ is meant to assert that $b_3\mathrm{A}_3$, with one sign or other, is equivalent to the quadruplet $g_1c_4g_6e_7$.

Enough is here done to show, that, on our suppositions, the operation $\mathrm{Q}_i\mathrm{A}$ will give rise to terms affected with sextuplet, quintuplet, or quadruplet imaginaries, each reducible, with one or the other sign, to an equivalent, in which the subindices shall ascend, and which shall exhibit b, d or f in no place except the first, and in the other places, no letters except repetitions of c, e, g. If A were a quintuplet having c, e, or g, in the first place, instead of f, it would be easy to show that the multiplication $\mathrm{Q}_i\mathrm{A}$ would yield results of the same forms. Generally, if V_m be the function of e-plet imaginaries, the operation $\mathrm{Q}_i\mathrm{V}_m$ will produce all the $(e+1)$plets of the system which have $(e+1)$ different subindices, together with a number of e-plets and of $(e-1)$plets, the whole of which can be reduced to equivalents which shall exhibit b, d or f only in the first place. If Z_m be the function of n-plets, n being the number of different subindices, $Q_i Z_m$ can yield only n-plets and $(n-1)$plets, since every imaginary in Q_i can be made to coalesce with some one having the same subindex in every term of Z_m, as in the products b_3A and c_3A, above considered.

The number of irreducible e-plets in the product of e pluquaternions of $6n+1$ imaginaries is obtained, by writing out all the 3^e permutations that can be made with repetitions of c, e, g, taken e together, and then under each permutation writing every possible combination of e different ascending indices that can be made with $0\;1\;2\ldots(n-1)$. The first letter, if c, may then be exchanged for b, if e for d, if g for f, in every permutation, which doubles the number of them.

The *no*-plets made with *ceg* are the two first terms in $\mathfrak{Q}_i$; whence

$$\Sigma_{e=0}^{e=n}\frac{2\cdot 3^e\cdot n\cdot\overline{n-1}\ldots n-\overline{e-1}}{1\cdot 2\ldots e}=2\cdot(1+3)^n=2^{2n+1},$$

is the number of terms in the product of n pluquaternions of $(6n+1)$ imaginaries. This, at the most (I apprehend that, in general, this exactly), is also the number of terms in the product of n pluquaternions of $(6n-1)$ imaginaries.

If we now suppose the addition of two imaginaries more, making $6n+3$, it is clear that the product of e pluquaternions of $6n+3$ will exhibit all the e-plets already found, besides those made by the combination of all our former $(e-1)$ plets with one of the two new monads. This, reckoning the two new *one*-plets, doubles our former result, giving the number of terms $=2^{2n+2}$; or the number of terms in the complete product of n pluquaternions of $6n-3$ imaginaries is 2^{2n}.

The reasons above given will shield me from any charge graver than that of a pardonable credulity, if I confess my belief in the existence of this *complete pluquaternion product*: and I hope that, instead of incurring the blame of presumption in touching upon questions which can be completely discussed only by far better analysts, I may even earn the thanks of the mathematical reader for pointing out the connexion between such a product, and the remarkable properties of products of sums of squares, to which it must conduct us. For whatever is obscure, unfinished, or even illogical, I trust to receive every indulgent allowance to which the confessed difficulty of these subjects may entitle those, who, while they are not forbidden to speak on them, are yet not expected to bring to their discussion the powers of mind possessed by such writers as the distinguished inventor of quaternions, or to exhibit his brilliant results in rich and varied applications.

Notwithstanding that my arguments thus far have been all *à priori*, and that no attempt at verification of results, either by arithmetic or common algebra, has at any point been made; so long as my right is not successfully disputed to the assumption, that *any irreducible multiplet imaginary, in a product of pluquaternions, is equivalent to any of the permutations of itself, or differs, at the most, from certain of them only in sign*; so long I shall venture to affirm, and hope to convince the reader, that the much-desired generalization of Euler's four-square theorem, and that of the more ancient two-square theorem, are thus correctly enunciated:

The product of $(n+r)$ *sums each of any* $(6n-2)$ *squares, is a sum of* 2^{2n} *squares.*

The product of $(n+r)$ *sums each of any* $(6n+2)$ *squares is a sum of* 2^{2n+1} *squares.*

Croft Rectory, near Warrington,
December 19, 1848.

LXXIV. *On the passive state of Iron.*
By Reuben Phillips, *Esq.*[*]

IN a small platinum crucible ·7 inch high I placed 35 grains of chromic acid and 62 grains of water, and having incorporated the acid and water, the crucible was connected with one end of the wire of a galvanometer and a piece of iron wire with the other end, and then part of the iron was immersed in the acid; the needle of the galvanometer was thrown about 45° from its place of rest, and settled at 0°. Having caused the needle to have a barely perceptible motion about 0°, the circuit was closed and opened through the galvanometric wire at such times as should increase the motion of the needle, supposing the iron to act as zinc to platinum; a vibration of 10° was easily obtained. A copper wire rather smaller than the iron wire was now substituted for it; on its first immersion the current was so strong as to cause the needles of the galvanometer to rotate, then a constant current was established which held the needle at 43°.

The acid was now diluted with about six times its bulk of water, and the crucible nearly filled with this dilute acid, everything else remaining as before. The iron wire caused the needle to move about 30° and then to stand at 0°; a swing of the needles could however be obtained by closing and opening the circuit as before, but the current was weaker. The copper wire caused a permanent deflection of 21°. In this and the following experiments the crucible was filled to

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[*] Communicated by the Author.

about the same height as before, and the metals were clean and similarly immersed.

The acid was again diluted with six times its bulk of water. The iron wire at the moment of its entering this acid moved the needle 20°, which then rested at 0°; a swing could only be communicated with still greater difficulty than in the foregoing experiment. The copper wire caused the needle to mark permanently an angle of 12°. In these experiments with chromic acid the iron remained perfectly bright.

With nitric acid, specific gravity 1·39, instead of the chromic acid, the needle of the galvanometer was permanently deflected 5°. I found it advisable to screen the galvanometer from the violent initial current of the arrangement, by opening another circuit besides the wire of the galvanometer for this first current.

40 grains of iodic acid and 65 grains of water were placed in the crucible; and when the acid was dissolved and mixed, the crucible was connected with the galvanometer as before. The iron wire at the first moment of its introduction acted very powerfully, and then the needle stood at 4°; the iron remained perfectly bright. A copper wire of the size before mentioned gave a permanent deflection of 45°, notwithstanding a dense film which formed on the surface of the copper.

The action of chloric acid on iron was best observed with a galvanometer far less sensitive than that before used, which consisted of a pocket-compass having a few turns of insulated copper wire round it. The metals and acid were employed as before; the chloric acid was a rather concentrated solution. When the iron was put in its place in the acid the needle remained quite stationary; but on touching the iron with a piece of active iron or a copper wire, the needle was strongly affected, and ultimately stood at 40°. This is an example of the production of passiveness without the powerful initial current which is generated by iron and nitric acid. With the first-mentioned sensitive galvanometer the needle stood at about 35° when the passive state existed, but was remarkably unsteady.

The foregoing experiments were performed without any considerable interruption, and with the same piece of iron wire, which was $\frac{1}{20}$th of an inch thick.

A dilute sulphuric acid consisting of one of acid and five of water, when cold was mixed by degrees with moist hydrated peroxide of barium of the consistence of cream, until the bulk of the acid had increased from one to three; no gas was evolved. The sulphate of baryta was isolated on a filter, and the fluid evaporated *in vacuo* over sulphuric acid until it had acquired an oily appearance. This acid was now placed in the platinum crucible which was connected with one end of the wire of the less sensitive galvanometer, an iron wire communicating with the other end; the needle showed the passive state was readily assumed by the iron and this mixture of peroxide of hydrogen and sulphuric acid. When the iron having no connexion with the platinum was plunged into this mixture, it produced gas; but after having brought the iron in contact with the crucible, the evolution of gas on the surface of the iron ceased; gas continued to be produced, as before contact, on the platinum, which arose from its action on the oxygenized acid. The acid was now increased to about four times its former bulk by the addition of water, and was found still capable of assuming the passive condition. The current from active iron, platinum and oxygenized sulphuric acid, produced a much larger deflection of the needle than sulphuric acid, either dilute or concentrated. The peroxide of barium was prepared by Wöhler and Liebig's process, and contained no chloride, nitrate or chlorate. I found it advisable, when I wished to observe the passive state with oxygenized sulphuric acid, to be careful not to remove any portion of the iron from the acid when once wetted by it; and to avoid using the iron when at all heated, as it is apt to become by being cleaned with sand-paper.

It was observed by Daniel, that a mixture of nitric and sulphuric acids does not act on iron; and I have found that when to a dilute sulphuric acid, one measure acid and six water, a little bichromate of potash is added, the acid and iron can easily be made mutually passive by contact with platinum; so that if a substance can exhibit the passive state, its mixture with sulphuric acid does not annihilate this property. Thenard found iron was without action on peroxide of hydrogen. That this is really owing to the formation of the passive state is, I think, shown by the action of oxygenized sulphuric acid, in which I look upon the peroxide of hydrogen as acting towards the iron as nitric or chromic acid would under similar circumstances.

There are now, then, the following bodies with which passiveness has been developed,—nitric acid, peroxide of hydrogen, chromic acid, iodic acid and chloric acid. Peroxide of hydrogen is an electrolyte, and was found so, I think, by Thenard; and solutions of nitric, chloric, iodic, and chromic acids are electrolytes; all these substances are also easily decomposed by giving up their oxygen, so that their molecules have a tendency at an anode (but not at a cathode) to combine with the iron entire, and also to decompose and so oxidate the iron. I think from this, that the passive state results from these two forces acting simultaneously and balancing each other. A parallel case is perhaps presented by hydrocyanic acid, which readily decomposes when pure with the formation of a brown substance and ammonia; and by concentrated mineral acids it is rapidly converted, together with the elements of water, into formic acid and ammonia, while with a certain small quantity of a strong mineral acid neither of these two modes of decomposition appear.

In these experiments with nitric, chloric, iodic and chromic acids, it is seen that the passive state is not absolute; the mixture of peroxide of hydrogen and sulphuric acid was not examined in this particular, but I cannot doubt it forms no exception. The current between passive iron and platinum was always the same in direction as between zinc and platinum in sulphuric acid. When two clean iron wires were immersed in a mixture of about equal volumes of nitric acid, specific gravity 1·39, and sulphuric acid, the galvanometer always showed that one of the wires could act to the other as zinc to platinum, the current being in different experiments various both in force and direction.

The following experiment shows, independently of the galvanometer, the existence of chemical action during the passive condition. I took a piece of clean iron wire $\frac{1}{20}$th of an inch thick, and about $1\frac{1}{4}$ inch long, and twisted some platinum wire about one end of it, leaving a portion of the platinum wire projecting; then laying hold of this projecting portion, the iron was placed in half a fluid ounce of nitric acid, specific gravity 1·39, and left there for about two hours; the iron was then removed by means of the platinum to another half-ounce of the same acid, in which it was well-stirred about to wash it, and then the metals were thrown into a vial containing a similar quantity of the same acid; the whole of the iron lay beneath the surface of the acid, and the vial was closed and put away in a dark place. Examined at the end of two days, the acid was found to be rather coloured; it was poured off the iron, evaporated nearly to dryness, and a solution of ammonia added, which caused an abundant precipitate of the sesquioxide of iron.

The comparative perfection of the passive state with chromic acid, even when very dilute, suggests that bichromate of potash would be a useful addition to the nitro-sulphuric acid employed in Schönbein's voltaic pile.

7 Prospect Place, Ball's Pond Road,
November 23, 1848.

LXXV. *Geometry and Geometers. Collected by* T. S. Davies, *Esq., F.R.S. and F.S.A.*[*]

No. III.

DR. SIMSON might, possibly, have alluded to his intention of publishing the Euclid in a letter which is, unfortunately, missing from this series, the date of which appears (from the following one) to have been Feb. 5, 1753. It is very probable that Dodson retained the letter, with Nourse's permission, as a memorial of the aged geometer; but I am altogether ignorant as to what became of Dodson's papers, and have hence no clue for tracing this letter, even if it be in existence. The chief curiosity of the letter would, however, in all probability turn out to be—the particular "problem" which it contained; and as the Doctor's correspondence is, I believe, preserved in a tolerably complete state, this may probably be learnt from Dodson's enclosure through Nourse, of May 29 that year. The following letter of this series is the first that contains a reference to that work. Though the whole of the letters are upon matters of business, there is so much that is characteristic in them, that I shall give a small number entire, and extracts from others. They relate, too, to that publication which forms an æra in our mathematical literature; and any circumstances, however slight, attendant upon the publication of the Euclid will be of interest in mathematical history.

"Glasgow March 1755.

"Dear Sir,—Your last letter to me is of the date May 29, 1753 in answer to mine of Febry. 5th that same year, and as you delayed writing near four months because, as you say, you had little to write, the like apology must be made for my delaying to write near two years; but I confess my neglect is a great deal more blameable than Your's, not only in respect of the far longer time, but that I ought to have returned you much Sooner thanks for your kind letter, and the Solution inclosed in it by the very ingenious Mr. Dodson of the Question[†] I had sent up, which is very well done. Pray give my

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[*] Communicated by the Author.

[†] Most likely the "question" was some one relating to "chances." Simson was a great devotee to *whist* (see Trail, p. 76); and Dodson's *forte* lay in this direction. The problem no doubt (if such were its character) originated in his "club," and involved an inquiry *why* he should so often be a loser (as Dr. Trail reports him to have been) when he *ought* to have been the winner. There are two or three orders in these letters for "Blanchard's cards," which he bought by the dozen. In one letter (the last of the series) he denominates them his "little books;" and in another he complains that those last sent "were vile trash, so soft that they would serve but for one or two nights, whereas those you used to send before these served at least four or five times."

kind service to that Gentleman, and tell him in answer to what he wrote at the foot of the operation, that tho I should put him to no further trouble with any Problems, I would render him any assistance in my power in difficulties that may occur to him, but that I think he is much or more capable to assist me than I am to do any service, in the way he proposes, to him, especially now when I am so old, and in a manner worn out*. I am glad to See a 3^{d} Volume of his repository advertized, and doubt not it will give me equal pleasure, at least, with the other two. If you are sending any books to this town, I desire you may put up a copy for me, and of any other new thing you may think will be agreeable.

" Now, Sir, I must desire you will forgive me for doing a thing I ought first to have asked your leave to do; it is the advertising in my proposals for printing the first Six, the 11th and 12th Books of Euclid's Elements, which proposals are ready to be sent to London, that Subscriptions are taken in at your Shop. I advertise them to be taken nowhere else in London. The proposals and receipts are to be sent to Mr. Ruat who will give you as many as you think proper to take; in the mean time I have enclosed one copy with this. After you have read them I pray you may write me any directions or hints with relation to my design which will be most gratefully acknowledged by

" Dear Sir, Your most humble and
" obliged servant,
" ROB: SIMSON.

" If the books were printed I design they should be had at your shop allowing the usual rate for your trouble and pains. I shall be glad to hear from you with your first conveniency.

" I have often wrote to Mr. Ruat to take a glass with you on my Account, and hope that he has done it."

This Mr. Ruat, who is often mentioned in Simson's letters, appears to have been engaged somewhat in the business that is now denominated a " commission-agency;" but at the same time he appears also to have been an intimate personal friend of Dr. Simson's. He is described in the letters as a considerable traveller. It was probably through him that Simson negotiated with Nourse the entire business, which is not detailed in the letters, relative to paper, &c. for the quarto edition of the Euclid. This, at least seems to be implied by the next letter.

" Glasgow, 23^{d} Septr. 1755.

" SIR,—I wrote to you when I sent a copy of the proposals for printing the 8 Books of Euclid, and believe that you thought it needless to write to me when you could advise me of anything that was proper by means of Mr. Ruat. I designed that you should have gotten as many of the receipts as you wanted as soon as they came

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* Dr. Simson was then in his sixty-eighth year, and the first edition of the Euclid was not then published or even printed.

to Mr. Ruat's hands, and wrote to him expressly for that purpose, but by what he wrote me after he had gotten them, I found he had not given you any, and I believe did not for a good time, at which I was much displeased, as I signified to him, and told him I designed to deal with you as my good friend, and not merely as a Bookseller.

* * * *

"I thought the book would take about 44 sheets but there will be some more than 50 in it, and I have caused make a new font of types for it, exactly like those in the proposals, both which will be no disadvantage to the Subscribers. They are but printing the 3d Book of the Elements and but 2 sheets in the week, by which means even the English copy which I print before the other cannot be ready against the time mentioned in the proposals; and I desire your advice if it will be proper to make an apology by way of an Advertisement in the newspapers, for the delay.

"I wish you could give me a guess of the number of Subscriptions for the two kinds of paper. Mr. Ruat sayes he knows of 28 for the best paper including Earl Stanhope's 10, whose generosity I have been much ashamed of, and these with 20 I know of, 8 of which are from Holland, and 9 for Stationers hall included, make 48, So that the number 50 which you advised me to print by Mr. Ruat will be too small. Mr. Foulis urged me to print 200 which I thought too many by far, but have yielded to print 96. because I expect some from Dublin and other places in Ireland and Britain.

"I shall be much obliged to you if you could inform me of the price I ought to pay for the wooden cuts per piece, for I am afraid that Mr. Foulis, one of whose lads cuts the figures may ask too great a price for the work; I furnish the boxwood my Self.

* * * *

"Pray let me hear from you as soon as you can, and Excuse my giving you So much trouble. I am, Dear Sir,

"Your most humble and obliged Servant

"ROB: SIMSON.

"Mr. Foulis sayes that 250 Copies of the Latin will be too few to answer the expence of printing; but I gave him no answer untill I should get your advice."

"Glasgow, 15th Octr. 1755.

"SIR,

* * * *

"As the Book cannot be printed, I mean the English copy, till a good time after the Parliament Sits down, I hope you may come by better Speed in getting Subscriptions than hitherto. I much incline that the Latin copy should be published along with the English, but am afraid it would be too long to defer the publishing untill the Latin can be printed; but in this I shall be wholly at your direction and advice, which I desire you may give in your next.

"I looked through Mr. Ruat's letters and find that in one of the 7th of June last he said you advised me to print 1000 copies, 750

English and only 250 Latin. I think 500 Latin may be ventured upon, and shall print so many if you advise me.

* * * *

I shall take your advice in making no apology. [In reference to the delay, as proposed in the preceding letter.] You did write to me formerly* what you now repeat about the price of the Cuts in Mr. Simpson's books; and I will gladly pay 15^{d} for mine and furnish the boxwood too; and even for some larger ones would pay 18^{d}, but I am afraid they will insist on a greater price, in which case I design to refer the matter to such as you who knows the affair, for certainly they ought to take no more here than at London.

"I am obliged to you for sending the Proposals to Mons^{r}. Clairault, I heard from Mr. Ruat that he, or Mons^{r}. Monier was to give an account of them in the Paris Journal, but have heard nothing since about it.

"I wish you could find some way of sending me Mr. Landen's Lucubrations as soon as may be. I am affectionately

"Dear Sir, Your obedient humble Servant,

"Rob: Simson."

Simson appears from the next letter of the series to have determined upon keeping back the English copy till the Latin one was printed. The English had been finished, except title-page and preface, three weeks before; but he did not expect the Latin to be printed off before November. Only three sheets of the Latin were printed at this date; viz.—

"Glasgow, 29th March 1756.

"Dear Sir,

* * * *

and I return you hearty thanks for your care in choosing it [paper for the work] and sending it off in time, and that you have been so good as to give me credit for the above Sum [£41 16*s*. 6*d*.] which I hereby engage to pay at the time it becomes due, and to relieve you from all hazard in giving me Credit.

"I know you have little time to spare in writing letters to such as I am, but intreat when you get a little leisure you will give me some mathematical news. Pray let me know something about a Book I see advertised last week, Geometry made easie &c. by John Lodge Cowley. If it be well done it will be four times a better pennyworth than the book I am printing, because it contains twice as much and is twice as cheap. * * *

"I know not whether either Mr. Ruat or you have gotten one Subscription during this Session of Parliament. * *

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* If Nourse gave the information in consequence of a request from Simson, it must have been made in the letter of Feb. 5, 1753, for it does not occur in any of this series before me; nor is there the least indication of any other letter being missing from the correspondence.

"With my best wishes for your prosperity every way, I am, Dear Sir,

"Your much obliged and most humble Servant,

"Rob: Simson.

"I do not dedicate the book to Earl Stanhope for a reason you will know afterwards."

Cowley's book is but little known; and though it is probable that Dr. Simson would be somewhat surprised at its appearance, and disappointed with its contents, it is a production of some value to the student of the Geometry of three dimensions. Its value is simply this: that it contains a series of well-designed models of planes and solids, such as occur in the earlier parts of the Geometry of Space. The work was constructed for the use of the cadets in the Royal Military Academy (in which Cowley was Professor of Mathematics); and I believe the practice of employing such illustrative models has more or less prevailed in this institution ever since. The idea itself was, however, by no means new, and had been used by a writer then so familiarly known as Ward. The method was probably first used in explaining the principles of perspective, and is still much employed by writers on that subject. I am not alone in wishing it were more freely used by modern writers on pure geometry, and especially in geometrical instruction, by our teachers.

It is stated by Dr. Trail (Life, p. 31), on the authority of some of Simson's letters to Earl Stanhope, that a private act of parliament was contemplated for preventing the importation of any copies (especially of the Latin edition) from abroad, after the example of Buckley's edition of Thuanus. No traces of such an intention exist in these letters, and hence it must have been completely abandoned before he entered upon the printing. There is, however, abundant proof of Simson's great care and circumspection as a man of business, and the most rigid attention to a compliance with the terms of the statute of Anne, by delivering and registering the *nine copies* required by that act to be placed in the hands of the Stationers' Company. Nay, so scrupulous was he, that he sent not only nine copies of the work, but nine copies of large paper and nine of the small—as though the size of the paper might constitute the larger *a different edition* from the other!

It is altogether unnecessary to give the further letters, or those parts at least which relate to mere business details,—such as the number of copies sent off, speculations as to the mode of disposing of them, and the prices to be charged. A few occasional passages, however, possess interest.

A great object of Simson's solicitude was the manner of binding and presenting certain copies of his work. The issue to the public was kept back for some time for this purpose; probably till some time in the following spring (1757). Three short passages are worth transcribing.

> "Also give me your advice about giving a copy to the Royal Socitie, one to the E. Macclesfield President, and one to Mr. De Val Secretary whether it be proper to give these or not." (Letter, 26th Nov. 1756.)
>
> "Notwithstanding of what you Say about books given to the R. Societie, which I know is true, I incline to send to the Societie and to the President and to Mr. Duval." (Letter, 10th Dec. 1756.)
>
> "You may cause bind up three copies, one, the large Latin, to be presented to the Royal Societie, a 2^d^, the large English to be given to the Earl of Macclesfield, and a 3^d^ English Large or Small paper as you incline, but I think it ought to be large to Mr. Du Val the Secretary. I leave the binding entirely to your discretion and judgment in every point, and I request you will be so good as to present them Yourself with my best compliments in the manner you think suitable to each of the three." (Letter, 31st Dec. 1756.)

Dr. Simson was deeply impressed, even for his time and country, with "the greatness of the great." His fastidiousness about binding and presenting copies sometimes approaches the ludicrous. He never suspected the possibility of the gracious reception given to his book by the then Prince of Wales being a cheap and courtly mode of repaying the loyalty of a Scotchman in those days,—a Scotchman, too, who possessed such extraordinary influence as Simson did amongst the intellectual classes of Glasgow.

Earl Stanhope was an object almost of veneration to Simson; and there is good proof that the Earl venerated the great geometer in an equal degree. For once it has happened that the Lord did *not* "lord it over" the man of science. The fine monument erected to Simson's memory in the publication (at his own expense) by the Earl, of the *Opera Reliqua*, is the best proof that Simson's estimate of that nobleman was not higher than his lordship deserved. The Earl is often mentioned in these letters; and, as I have said before, he was probably the originator of the intercourse between the author and the publisher.

In the first of these papers I alluded to the feud between Nourse and Andrew Millar. The following passages somewhat illustrate its extent.

> "I hear that Mr. And. Millar (but not from himself) wants to take some copies, [of the Euclid] about which, and any thing else proper to promote the sale of the book your friendly advice is very necessary and will be most obliging to me." (Letter, 20th Oct. 1756.)

"The character you give me of a certain Bookseller is the same which he has in this Country, and I would never have entered into any bargain with him." (Letter, 19th Nov. 1762.)

Most of my readers are more familiar, probably, with Simson's Euclid than with his other writings (perhaps, indeed, we all are); and no doubt many of them will have formed, from the somewhat dogmatical *tone* of his "notes" on that work, an opinion that Simson was an imperative and overbearing person. Nor does the character of him, as given by Dr. Trail, tend altogether to remove this impression (Life, p. 75–77). In his criticisms he speaks positively and emphatically, as was the stilted custom of didactic writers of the class familiar to him, then to speak; but a perusal of these letters has convinced me, that even his severities upon the hypothetical Theon himself, as the great perverter of Euclid's work, were honestly uttered, and formed upon preceding models of controversy. He, in several of these letters, invites "the correction of any errors he might have committed," and desires to know the "views of the learned" on his performance. He placed great confidence in Nourse's judgement, not only as a man of business but as a geometer; and some stray papers in Nourse's hand, in my possession, show that the publisher was himself a really superior geometer.

Dr. Simson, anxious as he was to see his work brought into general circulation, never adopted any means to gain for it a spurious reputation or a forced sale; but, on the contrary, over and over charges Nourse, though he may "go to the expense of 4 or 5£ for advertising in the newspapers" to avoid all "puffing." "Be sure," says he (1st Nov. 1756), "to put nothing like a puff into the advertisements which I believe you like as little as I do: I fancy the Title will serve, but if you shorten it, I shall not be against it, tho' I see not how it can be much shorter." In a subsequent letter, he thinks that some intimation of the "notes" should be added to the advertisement.

He was very solicitous to know what the "Reviews" said about his work; and he says in a letter 23rd Jan. 1761—"Montucla gives me a good deal of amusement; I am obliged to him for mentioning me 4 or 5 times very discreetly." Other passages show that though he was the very reverse of a vain man, he was still sensible to the approbation of competent judges, and alive to his own permanent reputation. Yet he claimed no infallibility, and never paraded or vaunted his own great powers.

These letters contain a few mathematical criticisms well-worthy of preservation. One passage occurs in the letter of

1st Feb. 1765, short but to the purpose: "Forget not to send me Lawson's Translation of Apollonius concerning Tangencies as he improperly names it." The work was in reality a translation of Vieta's attempted restoration of that treatise of Apollonius, and was most imperfectly executed. Many subcases were omitted altogether; and the determinations in general were very loosely given, even where given at all. Nor are all the lemmas given by Pappus, as necessary to the solution of the problem employed by Vieta or his translator,—the celebrated vii. 117 for instance. I may, however, refer to a paper printed in the Mathematician, vol. iii. pp. 76–78, for some further remarks on this subject.

Meanwhile it is proper to keep in view that the restoration of the solution given by Apollonius is not *necessarily* getting the best solution. Indeed as regards this problem, it is certainly *not* getting the best. By taking different routes, several foreign geometers have arrived at constructions of great simplicity and beauty: for instance, Cauchy, Gaultier, Durrande, Poncelet, Steiner and Gergonne. The last is very remarkable: but there is a total absence of all attempts to fix the determinations and discriminate the special sub-cases in all these solutions. In short we have neither a complete restoration nor a completed solution of this one of the Apollonian series of problems. Yet it is worthy of both.

The commentaries on Jack's edition of the Data, though slight, are certainly correct and interesting; though there are some geometers who still think that a perfect restoration of Euclid's Data has not been accomplished, even by Simson himself.

"I thank you for what you write about Mr. Jack's Edition of the Data which good or otherwise let me have a Copy by the first occasion; and in the mean time in your next, which pray let be as soon as you can, let me have his translation of the first paragraph in page 512 of Gregory, viz. a part of Prop. 73. Also let me know if he has put in the translation of the 2^d Demonstration of Prop. 24. which is a mere paralogism." (Letter*, 10th Dec. 1756.)

"I thank you for what you give me from Mr. Jack's book, though he has by himself or from others found some thing amiss in these two Propositions, yet he does not speak of them as if he knew wherein the faults lie. I am just obliged to go out so cannot write particularly about what he sayes; only there is not the least occasion of finding what he calls M, because that fourth proportional is already expressly named by ΓK in the exposition which immediately follows the Enunciation of the Proposition. * * *

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* In a postscript of the same letter he first expresses his intention to "make an Edition of the Euclid in small [word missing, probably 'size'] for the use of schools."

"You need not be afraid that Mr. Jack's Edition will have the least influence on me to delay giving another when I get leisure, but when I see the book I will be able to tell you more about it." (Letter, 31st Dec. 1756.)

Dr. Simson published the Data as an appendix to the 8vo edition of Euclid in 1762, and indeed began printing immediately. It appears that 1000 copies of this edition were published; and Nourse and Balfour bought them and the copyright together. The terms for the whole do not appear; but for the *copyright alone* 150*l.* appears to have been given (Letter, Nov. 19, 1762).

Nourse appears, after the publication of the Data, to have raised some objection to the character of the notes; to which the Doctor replies—

"I know not well what you mean by remarks on the Data, there are but very few. for the sake of Young Geometers I have given the compositions to all of them but the easie ones in the first part of the book, and have added some that were wanting." (Letter, 19th April 1762.)

In a subsequent letter, however, the apparently sharp tone of this is moderated.

"I believe the Data will fall a good deal below your expectation, to have added as many problems as would have been proper to show the use of them, would have made a good large book, but however you shall think of them, I will be glad to have your impartial opinion and censure of them." (Letter, 18th Oct. 1762.)

"I am very glad that you receive any Satisfaction by what you have read of the Short notes on the Data, and that the notes on the 29th Prop. Book 1st seem to be right. but I request you to give me your censures on any thing with the utmost freedom, for they will be very acceptable." (Letter, 19 Nov. 1762.)

Another slight reference to the Data occurs in the letter of 25th June 1763.

I have already given the principal part of the letters dated 11th April and 29th June 1767; and I shall close this series with extracts from that of the 30th March, 1764.

"I am pleased you have sent me Castiglione's Commentary. I had about 20 years ago a letter from Castiglione when he resided at Vevay on the lake of Geneva in which after commending the Conick Sections very much he asked my leave to translate into Latin a book of Mr. Thomas Simpsons by which I saw he took us for the same person, but I think it strange he should have again fallen into the same mistake. I shall give you some opinion about his book when I have read some of it, for my age will not allow me to peruse fully such large treatises on subjects that require a good deal of thought.

in the mean time I join with you in thinking that he will not be able to do any thing to the purpose on Euclid's Porisms*. . . .

"I have taken Emerson's 3 books rather to help Mr. Foulis Sale of those you sent him than for any other reason; I believe your opinion of them is much the Same with my own; what think of you of his saying at the foot of page v. of his Geometry 'For it is an Axiom in Logick, that *that Supposition must needs be true which destroys the contrary Supposition*†' for what if the contrary supposition be true?

* * * *

"I desire you to let me know in what respect you think that Dr. Stewart has deviated Something from the Antients, for he gives the Analysis as well as the Composition of the Theorems, for I do not observe that he gives any of his Propositions as Problems, and so has no occasion to make use of Euclid's Data. When I get your next, I shall more fully let you know my opinion of his great merit‡.

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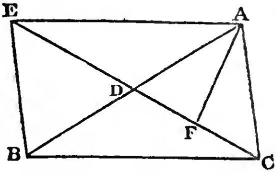
* I am warned by my own paging in the MS. to abstain from making some remarks here which I fully intended to give as a note on the *Porisms.* It may be as well this should be deferred; for though the general principle of the porismatic system has been, beyond all doubt, established by Dr. Simson, there remain difficulties sufficiently formidable about this class of inquiries to inspire caution in speaking dogmatically on the subject. Whenever I may offer an opinion on either the principle or the details of the porism, I shall at least have had the advantage of the criticism and discussion of the most eminent geometers (in strictness of language) in Europe, both as opposed to, and as coinciding with, my own views. The seventh book of Pappus is not entirely unravelled yet.

† The only instance I find in all these letters of "marking for italics." Even here it is not underscored but overscored—an instance, I think, not often found.

‡ This has reference, without doubt, to Stewart's *Propositiones Geometricæ,* published in 1763; a work which contains a most remarkable series of properties of circles and triangles, which are often referred to a later date, and many of which are even now but little known to geometers. A great number of these properties, slightly varied in their enunciations, hold good, too, in the conic sections; and I cannot point out a more useful exercise for the young geometer than to attempt the extensions here suggested. Very many years ago I myself found this exercise most beneficial; and I venture to predict, that whoever follows the same steps will find himself in a position to follow with little difficulty, one of the most important sections of the *géométrie supérieure* of the modern French School.

It is singular enough that whilst Dr. Stewart's writings are of a kind calculated to render them peculiarly attractive to the non-academic school of English geometers, they remain to this day less generally known than the writings of any geometer of these kingdoms. Even his "physical tracts," and the supplementary one on the "sun's distance," though admitted to be of little physical value, are yet as replete with specimens of geometrical skill, as even the Principia itself. Besides his separate works, however, there are some stray pieces of Dr. Stewart. One is a discussion and extension of *Pappus,* iv. 4, which was printed in the "Essays Physical and Literary, read before the Philosophical Society of Edinburgh,"—the parent of the present Royal Society of Edinburgh (Ed. 3 vols. 8vo, 1754-71). These essays are rarely to be met with now; and at my suggestion, a good many years ago, this paper was reprinted in Leybourn's Mathe-

"You can do nothing more agreeable to me than to point out any mistakes you think I have made, for certainly nobody is readier to commit them than me, as I know from long experience; but what you observe of the Corollary to Prop. 76 of the Data is no mistake as you will easily perceive by reading the Demonstration of Prop. 76 with the annexed figure, only after the words 'because the angle BAC is given' unto the words 'therefore, &c. read in place of what is in print, and the angles ADC, DCA are given because they are equal and their sum is given. And which I should have told you in the first place, after the words 'Isosceles triangle DBE' add, to the base ED produced, therefore, by the 2d case of the Lemma, the rectangle DE, CF is the space by which the square of BD, that is of the difference of BA and AC is less than the square of BC. Also at the end of the Demonstration, instead of what follows the words 'and the rectangle DC, CE is the' read Space by which the square of the difference of the sides AB, AC is less than the square of BC.



"I am, Dear Sir, much obliged to you for the warm expressions of your kindness to me, my sentiments with regard to you are reciprocal, and if my age allow me to travel so far as London, one of the chief inducements for such a journey would be the pleasure of being some time with you. Let me hear from you as soon as your leisure permits. I am, My Dear Friend, Yours very affectionately

"Rob: Simson."

matical Repository (vol. vi). The paper is, however, most remarkable for some properties of the conic sections (the enunciations only of which are given) that are commonly considered to be discoveries of more recent times. Besides these, the propositions in the second edition of Simson's Conic Sections (4to, 1750), which are marked (x), are due, as Simson states in his preface, to Dr. Stewart. It is the more important to notice this, from the great philosophical historian, M. Chasles, having not remarked this circumstance, and hence having considered them to be Simson's own; whilst their importance in the *géométrie supérieure* renders it but an act of common historical justice that Stewart's name should be attached to them. Several of Simson's *Porisms* were the discovery, too, of Stewart (and some of Trail), which were, with Simson's characteristic honesty, duly acknowledged in the MS. on the Porisms. Stewart was a pupil of Simson's; and there is good reason to believe that he had also divined the true character of this class of propositions at a very early period. In fact, as I have shown in "An Analytical Discussion of Dr. Stewart's General Theorems" (Edinburgh Transactions, vol. xvi.), a large portion of the propositions which he published in 1746 (he being then a candidate for the chair vacated by the death of Maclaurin) are, in the strictest sense of the word, Porisms—even the formal enunciation being only disguised to the extent of not interfering with Dr. Simson's restoration of Euclid's definition of them.

The remark quoted from Simson's letter is important, too, in respect to its bearing upon another question—which, however, the length of this note forbids my entering upon here,—as it would be impossible to do justice to the subject within the prescribed space.

This, and other suggested corrections were, however, never made in any subsequent edition of Simson's Data. There are, in fact, other errata and corrigenda mentioned in these letters which were not corrected in the subsequent editions; and Simson complains in strong language of the "stupid conduct of John Balfour" in getting the second 8vo edition printed in Edinburgh; and says that some of it is "so spoiled" as to be unintelligible. I have had no opportunity of collating the earlier editions since these letters were placed in my hands, and can hence offer no distinct opinion as to the corrections of the errors thus introduced. I have done, however, the next best thing,—communicated the errata noticed in these letters to the editor of the best edition of Euclid we possess, Mr. Potts of Cambridge; although from the evident care which that gentleman has bestown upon his work, I think it likely that few, if any of them, will have previously escaped his scrutiny.

I propose in the next sections to give three original letters from John Bernoulli to Cramer. Their length, however, will preclude the possibility of the slightest comment.

Royal Military Academy, Woolwich,
November 8, 1848.

LXXVI. *On the Atomic Weight of Molybdenum and some of its Compounds.* By L. Svanberg *and* H. Struve.

[Continued from p. 434.]

Part II.

Behaviour of Molybdic Acid towards Phosphoric Acid.

ALTHOUGH the behaviour of molybdic acid towards phosphoric acid was not intimately connected with the object of our investigation, yet we were so frequently led to it that we considered it necessary to make some experiments upon the subject. This behaviour is so peculiar and so complicated that it requires a far more minute examination than we were able to devote to it, but since we have noticed some phænomena which have not hitherto been described, we consider it important to communicate in this place all that we have observed. The curious behaviour of molybdic acid towards phosphoric acid has been briefly noticed by some chemists. Berzelius* says, "When moist molybdic acid is mixed with phosphoric acid, it instantly becomes of a lemon colour; heat then dissolves it. The filtered liquid is colourless, and on evaporation leaves a tenacious transparent mass, which exhibits not the least sign of crystallization, and possesses a

* *Lehrbuch der Chemie*, 1845, vol. iii. p. 1044.

very astringent taste. Both water and alcohol dissolve it readily, the latter with a yellow colour, which on evaporation changes to blue, and leaves a brown opake residue, which dissolves in water with a blue colour. When an excess of molybdic acid is digested with phosphoric acid, the latter is precipitated and forms with the molybdic acid a lemon-coloured basic salt which is insoluble in water."

These observations are quoted by Gmelin*, who has added to them the following:—" When a very little phosphoric acid is added to a concentrated solution of the molybdate of ammonia, and then large quantities of muriatic, sulphuric, or nitric acid, or first one of these acids and then a little phosphoric acid, a lemon-coloured crystalline powder falls, the more quickly the more concentrated the liquid; this powder contains besides molybdic acid some ammonia, but none, or extremely little, phosphoric acid; it forms with potash a colourless solution with evolution of ammonia, from which it is again precipitated by muriatic acid by long boiling; but if the solution in potash is evaporated to dryness, and the residue dissolved in water, muriatic acid then merely gives it a yellow colour, but muriatic acid and chloride of ammonium precipitate a yellow powder. The yellow powder gives with hot sulphuric acid a colourless solution which is not precipitated by water. It is but very sparingly soluble in cold water, dilute sulphuric, hydrochloric, or nitric acid, it dissolves slightly in boiling water without colour."

When molybdic acid, obtained by calcining a pure molybdate of ammonia, is mixed with an excess of phosphoric acid, scarcely a trace is dissolved at the ordinary temperature; but on heating the mixture, a perfect and colourless solution soon results, which forms, when quickly evaporated, a colourless vitreous non-crystalline mass. But if some water is added to this, or it is allowed to absorb moisture from the air, it acquires a beautiful blue colour, which however again disappears on evaporation. If, on the contrary, an excess of molybdic acid is employed, and gradually some drops of phosphoric and nitric acid added to it (we employ nitric acid to be sure of having all the molybdenum as molybdic acid), on the application of heat the colour of the molybdic acid first becomes lemon-yellow, then gradually the solution acquires also a yellow colour; a portion of the molybdic acid remains unaltered, whilst another portion is converted into a yellow powder. When nothing further is dissolved the solution is filtered; on evaporating it to dryness to expel the nitric acid, a yellowish-white amorphous mass is obtained, which dissolves in water, and also

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* *Handbuch der Chemie*, 1844, vol. ii. p. 501, 511.

in weak and in strong alcohol, with a yellow colonr. But if the dry mass is heated for some time, it gradually assumes a dark yellow colour, but still continues soluble in water. On treating molybdic acid with a little phosphoric acid, a portion separated as a yellow insoluble powder; more of this is formed every time the solution is evaporated, whether the evaporation be carried on in the water-bath or on the sand-bath. If the dry mass be heated somewhat more strongly in a platinum crucible, it turns green, and on increasing the temperature, fuses to a liquid which solidifies on cooling into a vitreous dark blue or dark green mass. If the temperature be raised so high that the crucible is incandescent, some molybdic acid is volatilized. This coloured mass dissolves with difficulty in cold water; with boiling water it forms a coloured solution, which, when evaporated with access of air, first becomes colourless and then reacquires the yellow colour. This colour may be instantly produced by the addition of a few drops of nitric acid.

This more or less yellow compound of phosphoric and molybdic acid dissolves readily in ammonia, forming a colourless liquid. But if to the concentrated solution either sulphuric, muriatic, or nitric acid is added (the last is best), a lemon-coloured precipitate is formed as soon as the liquid has an acid reaction. In this case, therefore, none of the phænomena previously described as occurring in the treatment of pure molybdic acid and ammonia take place; there acid salts, partly crystalline, partly amorphous, separated with a white colour whatever acid was used, whilst here, however great an excess of acid is employed, the yellow precipitate only is obtained. Moreover all the compounds of molybdic acid with the alkalies are very readily soluble in an excess of muriatic acid, whilst this yellow precipitate is insoluble.

When this yellow precipitate is collected upon a filter, the mother-liquor passes through with a yellow colour, and furnishes on evaporation more of the yellow salt, so that at last the liquid becomes perfectly colourless. On washing with pure water, the precipitate goes through the filter, which may however be prevented by the addition of a few drops of nitric acid. The colourless mother-liquor, which contains a large excess of nitric acid, furnishes on evaporation a colourless vitreous mass, from which ammonia and nitric acid precipitate no more yellow salt; but this residue nevertheless contains molybdic and phosphoric acid, and is acted upon by heat in the same manner as the yellow salt; however, the amount of the precipitate is far greater; but further experiments must show how this different behaviour is to be explained. Perhaps the state of the phosphoric acid in these compounds acts a very important part accordingly as we have to do with *a*-, *b*-, or *c*-phosphoric acid.

We adopted this method for the preparation of the insoluble yellow compound in order to be perfectly certain of obtaining a pure preparation; however, as the following analyses will show, the salt can be prepared more quickly in a much more simple manner. Molybdic acid is dissolved in an excess of ammonia and a few drops of a solution of phosphate of soda added, the liquid heated and mixed with an excess of one of the above-mentioned acids, when the yellow salt is instantly precipitated. To ascertain the composition of this salt we made the following analyses:—

I. 1·2258 grm. salt, prepared according to the first method, and dried at 212°, gave on calcination, with the addition of a few drops of nitric acid, 1·1095 molybdic and phosphoric acid. This residue was dissolved in ammonia, the molybdenum precipitated as sulphuret, and the phosphoric acid determined in the filtered solution, after it had been somewhat evaporated and mixed with chloride of ammonium, by magnesia. The weight of the ignited phosphate of magnesia amounted to 0·0699 = 0·0445 phosphoric acid. 100 parts of the salt consequently contained—

Oxide of ammonium + water . .	9·488
Phosphoric acid	3·631
Molybdic acid	86·881

II. 4·7247 grms. salt, prepared according to the second method, and dried at 212°, lost on ignition 0·478 grm. oxide of ammonium and water; the phosphoric acid was not determined. 100 parts contain—

Oxide of ammonium + water .	10·117
Molybdic and phosphoric acids .	89·833

In this analysis the loss on ignition is too high owing to the difficulty of oxidizing the residual mass, which consisted partly of oxide of molybdenum.

It seems to follow from analysis I. that the phosphoric acid in this salt is not an essential constituent according to the ordinary laws of chemical combination, and if it be neglected, the two analyses agree tolerably well with the theoretical composition of a pentamolybdate of ammonia, which contains—

	a.	Calc.	*b.*	Calc.
1 at. Oxide of ammonium	324·980	6·656	324·980	6·747
1 at. Water	112·479	2·304	112·479	2·335
5 at. Molybdic acid .	4444·830	91·040	4379·145	90·918
	4882·289	100·000	4816·604	100·000

This salt, which is insoluble in water and acids, appears amorphous even under a high magnifying power, and to consist of minute spheres. When treated with potash, ammonia is expelled and a colourless solution obtained. When this salt is heated for some time in a closed platinum crucible, at a temperature below red heat, the whole of the ammonia and water escape, and a confused mass remains, which is partly of a brown, partly of a green colour. On treating this residue with concentrated nitric acid, very little reaction occurs at the ordinary temperature, but on heating it, nitrous acid is given off; the reaction, however, lasts only for a few moments; the liquid has now a beautiful yellow colour, and the sediment, a grayish-blue powder, is not further acted upon by nitric acid. The insoluble residue behaves like molybdic acid, dissolves in potash and in ammonia, and upon the addition of an acid, again furnishes the yellow salt. But the yellow solution possesses totally different properties. If this solution is dilute it loses the yellow colour in the cold and becomes colourless, but when heated it again makes its appearance. If this solution, which contains a large excess of nitric acid, is evaporated, some crystals separate on cooling, which appear under the microscope to be minute rhombohedrons. But this compound is not very stable, the mass absorbs water, all the crystals dissolve, and now another compound separates in regular octahedrons. Both these salts are very readily soluble, owing to which we were unfortunately unable to obtain a sufficient quantity of a pure salt for analysis.

If this yellow liquid be further evaporated, the yellow crystalline salt gradually passes into the amorphous yellow compound above described. If the yellow ammonia salt is heated with an excess of potash until the liquid no longer smells of ammonia and is partly evaporated, a colourless solution is obtained. On adding nitric acid to it, a yellow crystalline precipitate falls, which is insoluble in an excess of acid; it is collected and washed in the same manner as the preceding ammonia salt. It crystallizes in minute four-sided prisms, is insoluble in water, parts with water at a red heat, melts and solidifies into a crystalline yellow mass on cooling.

Analysis.—1·472 grm. salt lost on ignition 0·072 water, and gave 0·306 sulphate of potash = 0·1654 potash. But the sulphate of potash contained traces of phosphate of potash. The per-centage composition of the salt dried at 212° is accordingly—

Potash . . .	11·234
Molybdic acid	83·875
Water . . .	4·891

The anhydrous salt contains—

Potash . . .	11·812
Molybdic acid	88·188

In this analysis the molybdic acid was estimated from the loss. These results correspond with the pentamolybdate of potash previously described, with this difference, that the salt dried at 212° contains 4·891 water of crystallization, corresponding to 2 atoms. The theoretical composition of the salt is—

	a.	Calc.	*b.*	Calc.
1 at. Potash . . .	588·856	11·198	588·856	11·339
5 at. Molybdic acid	4444·830	84·524	4379·145	84·329
2 at. Water . . .	224·958	4·278	224·958	4·332
	5258·644	100·000	5192·959	100·000

If the yellow salt of ammonia is dissolved in ammonia and chloride of barium added, a white precipitate is formed, which possesses all the properties of the neutral molybdate of baryta described in the first part of this paper, with this sole difference, that when an acid is added to it, it instantly assumes a yellow colour. We have made two analyses of this salt, prepared at different times.

I. 0·780 grm. salt lost 0·0143 grm. on ignition, and gave 0·5994 sulphate of baryta = 0·3905 baryta.

II. This salt was analysed according to the following method, as we wished to determine the amount of phosphoric acid. After the loss had been determined by heating to redness, the powder was mixed in a beaker with a solution of sulphate of ammonia and hydrosulphate of ammonia. In this manner the whole of the molybdenum and phosphoric acid were obtained in solution of a dark brown colour, and the sulphate of baryta could be collected on a filter and easily washed. The filtered solution was mixed with muriatic acid, the sulphuret of molybdenum collected on a filter, and the liquid evaporated: this contained the phosphoric acid; it was first mixed with an excess of ammonia and then with sulphate of magnesia, and the precipitated phosphate of magnesia collected upon a filter after having stood for 24 hours, washed with water containing ammonia, dried, ignited and weighed. 0·937 grm. salt lost on ignition 0·015, and gave 0·7279 sulphate of baryta = 0·4736 baryta and 0·016 phosphate of magnesia = 0·0102 phosphoric acid.

The salt dried at 212° consequently contains in 100 parts—

	I.	II.
Baryta	50·074	50·543
Oxide of ammonium . .	1·833	1·601
Molybdic acid } . .	48·093	46·769
Phosphoric acid }		1·087

and the anhydrous salt—

	I.	II.
Baryta	51·009	51·366
Molybdic acid	48·991	47·530
Phosphoric acid		1·104

These numbers almost coincide with those found with the neutral molybdate of baryta, described in the first part of this paper.

From the analyses of these salts and the phænomena described, it is quite evident that but a very small quantity of phosphoric acid is contained in combination with molybdic acid, and that its amount is apparently variable. Hence arises the question, What part does the phosphoric acid act in these compounds, and how are we to view the molybdic acid thus metamorphosed? Can we admit a different allotropic state in this molybdic acid? and should we distinguish it from the ordinary molybdic acid by the sign of *b*-molybdic acid?

We have made numerous experiments to convert molybdic acid into this different state by means of other bodies besides phosphoric acid. It is unnecessary to relate the experiments, as we came to the result, that, when perfectly pure molybdic acid was employed, in which the absence of phosphoric acid had been ascertained, no change could be effected. But the tendency of phosphoric acid to produce yellow precipitates or yellow solutions with molybdic acid is so great, that we look upon molybdic acid as the most sensitive test for phosphoric acid, since in cases in which we obtained mere indistinct signs of the presence of phosphoric acid by other reagents, the reaction with molybdic acid was quick and perfectly distinct.

Phosphoric acid, which acts so important a part both in the vegetable and animal organism, we have found, with the assistance of this reaction, to be most generally distributed in nature. All the rocks, all the felspars which we have examined, contain phosphoric acid, and to prove this by means of molybdic acid is exceedingly simple. The sulphuret of molybdenum which we used in our investigation was remarkably pure; only minute traces of gangue, consisting of fragments of quartz and felspar, were here and there perceptible, but the molybdate of ammonia prepared by roasting this mineral and exhaustion with ammonia contained a little phosphoric acid. To detect this impurity, it suffices to add to the concentrated solution of the salt an excess of muriatic or sulphuric acid, when instantly the solution assumes a yellow colour, and deposits in the course of a few minutes a yellow precipitate of *b*-molybdate (phosphomolybdate) of ammonia. Or if we take the alumina which has been obtained in the analysis of any felspar, dissolve it in an acid, then add some pure molybdate of ammonia, and afterwards an excess of an acid, a copious yellow precipitate instantly falls. If we attempt to demonstrate the presence of phosphoric acid in the same alumina by a solution of magnesia (preventing the precipitation of the alumina by ammonia by the addition of tartaric acid), the reaction only occurs after several hours, and is then always very imperfect.

Phosphoric acid has so great a tendency to accompany molybdic acid in all its combinations, that we are unable to direct any other plan for procuring pure molybdic acid than by preparing an artificial sulphuret in the dry or humid way. In the decomposition of the potash double salt (obtained by acting with nitric acid upon a solution of molybdic acid in potash) by water, into trimolybdate of potash, which separates, and a neutral salt which remains in solution, traces of phosphoric acid accompany the insoluble compound. The presence of the phosphoric acid in this salt is best proved by converting it into the silver salt. On decomposing this salt with muriatic acid, separating the chloride of silver and evaporating the filtered solution, with the addition of a few drops of ammonia, a yellow precipitate of the ammonia salt is always formed: its amount is very small, and it has no influence upon the analysis; but it is quite sufficient to show the presence of the phosphoric acid.

III. *Determination of the Atomic Weight of Molybdenum.*

We shall here describe a large number of experiments which we have made in order to determine accurately the atomic weight of this metal. Although most of these experiments have not led to the desired result, they will nevertheless be of scientific interest. If, from their not being so accurate as we had reason to expect, they did not assist in fixing the atomic weight,—and exhibited in several instances differences from the theories previously admitted,—they nevertheless remain quantitative results, which show in what manner various compounds of molybdenum behave towards different reagents.

The atomic weight of molybdenum was ascertained by Berzelius in 1818, by precipitating a certain quantity of nitrate of lead with neutral molybdate of ammonia, and calculating the atomic weight from the quantity of molybdate of lead thus obtained. Berzelius, however, says of this determination, "that it is far from being trustworthy, and the number found is far more erroneous than the atomic weight of chromium ascertained in the same manner. The atomic weight of molybdenum requires, therefore, further experiments. It is probable that molybdic acid may be reduced by hydrogen, and then the atomic weight might be obtained with tolerable accuracy, especially if the acid after being weighed was saturated with ammoniacal gas to prevent its volatility, the ammonia salt decomposed by heat, and the residual fixed oxide ignited in hydrogen."

It is seen, therefore, that Berzelius places no great reliance upon his determination, which, moreover, is based upon one single experiment; and it is remarkable that he should have deduced the number 596·101, which differs but very little from the atomic weight we have found. As Berzelius points to the reduction of molybdic acid by hydrogen, we began with this experiment; we shall now proceed to describe it.

Action of Hydrogen upon Molybdic Acid.

Experiment I.—Molybdic acid was heated in a reduction-tube in a current of dry air, to deprive it of all moisture, and to be certain that the whole of the molybdenum was in the state of molybdic acid. We may observe that molybdic acid is not a very hygrometric substance.

3·0388 grms. of this molybdic acid were heated in a current of hydrogen over an ordinary spirit-lamp. Reduction immediately ensued, a large quantity of water was given off, which, however, soon decreased, and in the course of an hour and a half had almost entirely ceased. When this point had been attained, the tube was allowed to cool, the hydrogen in it expelled by dry air, and the residue weighed; it amounted to 2·6846 grms., and was reddish-brown. Upon this the reduction was continued with an Argand lamp, when again considerable quantities of water were given off; and although the reduction was continued for eight hours under these conditions there was still a deposit of water. By this continued intense red heat the glass had gradually become so brittle and crystalline that the tube on cooling cracked. The contents of the bulb were partly brown, partly gray, and had a metallic lustre. The acid employed for the experiment had lost, according to the first weighing, 0·3548 oxygen, or 11·656 per cent.; but this result agrees tolerably well with the supposition that the molybdic acid, on reduction with hydrogen at a low temperature, loses 1 atom oxygen and is converted into the oxide which Berzelius calls oxide of molybdenum. But it may also be conceived that 2 atoms molybdic acid $= 2MoO^3$ are converted into molybdate of the protoxide of molybdenum = MoO, MoO^3. Admitting this, 100 parts of molybdic acid should furnish—

	a.	*b.*	Found.
Molybdic oxide .	88·751	88·582	88·344

Experiment II.—2·1927 grms. molybdic acid were exposed for nine hours in a current of hydrogen to the most intense red heat that a lamp with double draught is capable of furnishing. The loss in this case amounted to 0·3793 or 17·298 per cent., and the substance in the bulb had throughout a gray metallic colour, which exhibited merely in the centre of the mass some spots of brown. The loss supposes that 2 at. molybdic acid = $2MoO^3$ have been reduced to the state of MoO, MoO^2; this formula may also be written $Mo^2 O^3$, and it then corresponds to a sesquioxide, such as we find in many metals. It may also be assumed that 4 at. MoO^3 have been converted into $2MoO, MoO^3$, which agrees better with the preceding experiment and with what occurs in treating the acid salts of potash and soda with hydrogen. Whichsoever of these views is admitted, the calculated result always remains the same; and 100 parts of molybdic acid furnish—

a.	*b.*	Found.
83·126	82·873	82·702

If in this experiment 2 ats. molybdic acid have lost 3 ats. oxygen, and have become converted into the compound $Mo^2 O^3$, then the atomic weight of molybdenum is 567·137, which does not differ much from the atomic weight we have arrived at. We did not repeat these experiments, as for various reasons they cannot yield results sufficiently accurate to base thereupon the determination of the atomic weight. We are, however, perhaps justified in concluding from the experiments just described, that molybdic acid exposed to the reducing influence of hydrogen, at the highest temperature which glass is capable of bearing, cannot be reduced to the state of metal, but only to the lowest oxide.

Experiments with Sulphuretted Hydrogen.

Although the treatment of molybdic acid and of the neutral and trimolybdate salts of potash with sulphuretted hydrogen gas did not lead to the desired object, they nevertheless present various interesting phænomena.

2·739 grms. of dried and ignited molybdic acid were exposed in a reduction-tube to the reaction of dry sulphuretted hydrogen. As soon as the gas came into contact with the acid, heat was evolved and the colour of the acid changed to black, but this soon stopped. If the bulb is now heated, there is immediately a disengagement of water; and the action of the sulphuretted hydrogen is so energetic, that when the current of gas is not too rapid, all the sulphuretted hydrogen is absorbed. The experiment lasts very long; at first a small spirit-lamp suffices, but gradually the temperature must be increased, and, finally, the strongest heat which an Argand lamp can give must be used. It appears that at first an oxysulphuret is formed, which is very difficult to decompose. After the operation had been continued for twenty-one hours, no further formation of water could be observed; but nevertheless no two weighings agreed. The difference between two weighings amounted to as much as 0·0006 grm. At the time we were unable to explain the cause of these differences, but we subsequently discovered it and shall allude to it in the following experiments.

When the reduction-tube had become perfectly cold, the sulphuretted hydrogen in it was expelled by dry air, and it was then weighed as quickly as possible; two successive weighings gave 3·0545 and 3·0551 grms.; the mean of the two is 3·0548.

Assuming that in this experiment the whole of the molybdic acid has been converted by the action of the sulphuretted hydrogen into sulphuret of molybdenum MoS^2, we find the atomic weight of molybdenum by the following equation, in which the atomic weight of sulphur is admitted to be 200·75, and also 200:

$$2{\cdot}739 : 3{\cdot}0548 = x + 300 : x + 401{\cdot}5$$

$$x = 580{\cdot}336;$$

or

$$2{\cdot}739 : 3{\cdot}0548 = x + 300 : x + 400$$

$$x = 567{\cdot}231.$$

If this result is compared with the theoretical calculation, 100 parts of molybdic acid should yield—

	a.	*b.*	Found.
Sulphuret of molybdenum	111·416	111·416	111·529

We made a similar experiment with molybdic acid which had been prepared from native sulphuret from a different locality, when 2·855 grms. molybdic acid gave 3·1652 sulphuret of molybdenum, or 110·865 per cent. Even though this experiment is not perfectly in accord with the preceding one, it at all events proves the identity of different molybdic acids.

Experiment with Neutral Molybdate of Potash.

Calcined neutral molybdate of potash was heated in a current of dry air in a counterpoised reduction-tube, weighed after cooling, and then connected with a sulphuretted hydrogen apparatus. The dry sulphuretted hydrogen acted instantly, and with considerable evolution of heat, upon the potash salt, water was formed, and the salt assumed an orange-red and gradually a dark reddish-brown colour. After some time the reaction ceased, the bulb became cold, and it was now gently heated with a small lamp; a considerable disengagement of water immediately ensued, and the sulphuretted hydrogen was so rapidly absorbed that not a bubble passed through the apparatus. It is necessary to raise the temperature gradually towards the end, that the entire mass may be incandescent: a portion of the contents fuses. At the commencement the action of the sulphuretted hydrogen is very energetic, but it soon decreases and proceeds very slowly, so that much time elapses before no further formation of water is perceptible. On attaining this point the bulb was allowed gradually to cool, and here a peculiar phænomenon occurred,—as soon as the bulb had acquired a certain lower temperature, the whole of the sulphuretted hydrogen was absorbed by the mixed sulphurets: not a single bubble past through for some time. This condensation of the sulphuretted hydrogen we first observed in making this experiment; it is probable, however, that the differences in the weighings in the former experiment were owing to the same cause.

As this condensation must give rise to an error, we expelled the sulphuretted hydrogen by a current of dry carbonic acid, keeping the bulb at a gentle heat; but although we passed carbonic acid through the tube for an hour, sulphuretted hydrogen was constantly given off, but not a trace of sulphurous acid was perceptible. As this led to our object, we expelled the sulphuretted hydrogen, immediately after the cooling, by dry air, and then weighed it as quickly as possible. At the end of the experiment the contents of the bulb were fused partly to a reddish-brown mass, which had acted very much upon the glass, and a portion had formed a mass with a plombiferous hue. The contents were now treated with water, which gave an orange-red solution, while sulphuret of molybdenum, $Mo\,S^2$, separated; it was collected upon a filter, washed, dried at 212°, and its weight determined. On evaporating the filtered solution, and slow crystallization, we obtained the beautiful compound described by Berzelius under the name of sulphomolybdate of potassium; but besides these crystals the microscope detected in the mother-liquor a salt which separated in white, minute rhombohedrons.

Experiment I.—2·1188 grms. neutral molybdate of potash furnished on treatment with sulphuretted hydrogen 2·9167 grms., in which were contained 1·081 grm. sulphuret of molybdenum.

Experiment II.—1·1305 grm. neutral salt furnished 1·582 grm. sulphurets.

Consequently 100 parts of the neutral molybdate of potash furnish—

	I.	II.
Sulphurets	137·658	136·753

in which is contained, according to experiment I., 51·019 per cent. sulphuret of molybdenum, $Mo\,S^2$.

Experiment with Trimolybdate of Potash.

Sulphuretted hydrogen does not act upon the trimolybdate of potash in the cold, but if the apparatus is warmed, water is instantly disengaged and sulphur absorbed. The same phænomena occur as in the preceding experiment; after sulphuretted hydrogen had been passed for ten hours over the salt, and two successive weighings exhibited but a slight variation, we looked upon the experiment as terminated, especially as in this case the glass was very much acted upon by the partially fused mass. In this case we also treated the mass with water and estimated the sulphuret of molybdenum.

2·5117 grms. trimolybdate of potash furnished 3·030 sulphurets, which contained 2·1656 grms. sulphuret of molybdenum.

100 parts of the trimolybdate consequently furnish 120·635 sulphurets, in which are contained 86·221 parts MoS^2.

The quantitative results which we have obtained in these three experiments it is difficult to explain and to connect with any theory. This is not merely owing to the bulb of the reduction-tube being so acted upon by the partially fused mass that the values are thereby rendered uncertain, but rather to the phænomena of decomposition being more complicated, so that to obtain a complete explanation it would be requisite to have likewise a quantitative determination of the sulphosalts and metallic sulphurets extracted by the water. However, as this would lead us far from the special object of this investigation, we did not make this quantitative examination, especially as we must then have entered upon new questions, for instance, what phænomena take place, and what combinations are formed in the treatment of potash or carbonate of potash with sulphuretted hydrogen at a high temperature, and how the compounds so produced behave when heated in a current of carbonic acid.

The only result which we are at present able to deduce from these experiments is, that although sulphuret of potassium is present, the molybdenum cannot be obtained in the state of tersulphuret (MoS^3), but that a portion is converted into bisulphuret (MoS^2), whilst at the same time a portion of the potassium is converted into a higher sulphuret, and that a certain excess of a higher sulphuret of potassium is requisite to form a certain quantity of sulphomolybdate of potassium, and to retain it as such.

Experiments to determine the Atomic Weight of Molybdenum by fusing Molybdic Acid with Carbonated Alkalies.

If only a neutral salt were produced on fusing molybdic acid with an excess of carbonated alkali, the atomic weight of molybdic acid and of molybdenum could be deduced in a very simple manner from the amount of carbonic acid expelled. Starting from this assumption we made the following experiments:—

I. 2·180 grms. molybdic acid fused with 10·1007 grms. carbonate of potash, lost 0·6866 carbonic acid.

II. 1·7112 grm. molybdic acid fused with 10·9066 grms. carbonate of potash, lost 0·5369 carbonic acid.

III. 3·135 grms. molybdic acid fused with 9·3958 grms. carbonate of soda, lost 0·9866 carbonic acid: accordingly, 100 parts molybdic acid expel,—

	I.	II.	III.
Carbonic acid . .	31·4954	31·3749	31·4705

And if we calculate from this the atomic weight of molybdenum, assuming the atomic weight of carbon to be 75·12 (α), or 75·00 (β), we have, according to—

	α.	β.
Experiment I.	573·524	573·143
Experiment II.	576·858	576·476
Experiment III.	574·216	573·834
Mean . .	574·366	574·484

These experiments were made in the following manner: the alkaline carbonate was heated to redness in a platinum crucible, and after cooling over sulphuric acid, weighed. This was repeated until two successive weighings entirely agreed. In another counterpoised crucible the amount of molybdic acid was weighed off, and a large excess of the alkali shaken into the crucible containing the molybdic acid, the whole mixed with a small platinum wire which had been previously counterpoised with the crucible, and the wire left in the crucible. The crucible with the residual alkali was heated to redness and weighed to see how much had been taken for the experiment. The crucible with the mixture of molybdic acid and carbonated alkali was now exposed to a slowly-increased heat, and as soon as the mass was perfectly liquid, the temperature was raised as high as it was possible with an Argand lamp and chimney. This heat was continued for half an hour, when the crucible was allowed to cool over sulphuric acid; the difference in weight gave the quantity of carbonic acid expelled. This ignition, cooling and weighing, had to be repeated five to six times with every experiment, and we took the average of all these weighings as being nearest to truth. The reason why we were compelled to take the average of the weighings is, that no two successive weighings were found to agree, there being sometimes an increase, at other times a decrease in weight. We are not able to explain the phænomenon, but it cannot be ascribed to the uncertainty of the balance, for this does not amount to more than $\frac{1}{10}$th of a milligramme.

Although these experiments have led to an atomic weight for molybdenum which agrees very well with one arrived at by another method, we cannot, on account of the variations in the weighings, place perfect confidence in the experiments.

Experiments with Chlorine Compounds.

We hoped to arrive at good results by the preparation and examination of the chlorine compounds of molybdenum; in this we have not succeeded, it being very difficult to obtain any one of the chlorine compounds perfectly pure. Besides, phænomena occurred which had not been previously observed and investigated; and, finally, we found it to be perfectly impossible to determine the chlorine accurately in the form of chloride of silver.

Although for these reasons we have not followed up the subject further, we shall describe our observations, as they are not without interest. Oxide of molybdenum, prepared by calcining a mixture of molybdate of potash with an excess of chloride of ammonium and subsequent exhaustion with water, had sometimes a brown, sometimes a gray metallic colour, according to whether a larger or smaller quantity of sal-ammoniac had been used; in general it consisted of a mixture of the two. This oxide was employed to prepare the biacichloride of molybdenum. For this purpose we used a glass tube furnished with two bulbs, about an inch apart; into one of the bulbs 4 grammes of the oxide were conveyed, and it was then heated quite gently in a current of hydrogen, to expel all humidity, and the tube connected with a chlorine apparatus so that only dry chlorine gas could pass over the oxide. When the apparatus had become filled with chlorine gas at the ordinary temperature at which no action is exerted upon the oxide, we applied a very gentle heat to the bulb containing the oxide: chlorine was absorbed with great avidity; biacichloride of molybdenum was formed, which was driven by a very gentle heat into the other bulb. When in this manner the greater portion of the oxide had been converted into the volatile chloride, the bulb was allowed to cool, and a current of dry air then passed through the apparatus for some time, to expel all chlorine gas, and the bulb containing the biacichloride melted off. This portion of the tube and its contents was weighed, the one extremity of the fused tube cut off, the contents dissolved in water, and after the tube had been well washed with water, dried and weighed, the loss furnished us with the amount of biacichloride taken for analysis. This chloride is readily soluble in water, but gave a faintly opalescent solution. When the solution had been very much diluted with water, we added a small quantity of nitric acid and then some nitrate of silver. At first pure chloride of silver fell, readily recognisable from its colour and external appearances; but on adding some more of the silver solution, the precipitate became very bulky and assumed a yellowish colour. On heating the solution, to cause the chloride of silver to subside and to dissolve the molybdate of silver which had been precipitated with it in the nitric acid, the liquid became more and more turbid, the precipitate increased, and the liquid, after twenty-four hours standing, still remained turbid. We now filtered, but with the precaution of pouring only the liquid on the filter; and after the precipitate had been washed several times in the beaker with hot water containing nitric acid, the whole was dissolved in dilute ammonia, and then nitric acid again added. These operations of dissolving, precipitating, and reprecipitating were repeated several times, for this was the only way of separating the whole of the molybdate of silver; finally, the chloride of silver was poured upon the filter and determined in the usual manner.

3·0881 grms. of biacichloride of molybdenum furnished 4·3245 chloride of silver = 1·069 chlorine.

If we compare the per-centage composition according to analysis and theory, we have—

	a.	Calc.	*b.*	Calc.	Found.
2 at. Molybdic acid.........	1777·932	48·095	1751·658	47·895	
1 at. Molybdenum	588·966	15·932	575·829	15·744	
6 at. Chlorine	1329·840	35·973	1329·840	36·661	34·622
	3696·738	100·000	3657·327	100·000	

H. Rose, who first quantitatively examined this compound and determined its formula, found in two analyses 34·295 and 35·001 per cent. chlorine.

This biacichloride of molybdenum forms yellowish-white shining laminæ, which exhibit no crystalline form even with a compound microscope.

We have prepared the biacichloride according to this method several times, but have never noticed the simultaneous appearance of any other chlorine compound. Although it is difficult to determine accurately the chlorine, owing to the molybdate of silver accompanying the precipitate, yet we believe that the principal source of uncertainty is to be ascribed to the difficulty of preparing pure biacichloride. This compound is generally mixed with traces of molybdic acid, for when perfectly pure, in which state it is only accidentally and very rarely obtained, it dissolves entirely in water, while in general it furnishes an opalescent solution. If this is the correct cause, it is readily explained why H. Rose, as well as ourselves, have found so small an amount of chlorine in the compound. This explanation is further confirmed by the circumstance, that when dry hydrogen is passed over the biacichloride and the bulb gently heated, nearly the whole of the chlorine compound is volatilized; but a small residue is left, which most probably consists of the molybdic acid contained in the biacichloride.

As we could not obtain a satisfactory result from the analysis of the biacichloride, we made the following experiment to obtain some other chloride.

A glass tube about 2 feet long, curved somewhat anteriorly, was filled, first with a considerable stratum of well-calcined fragments of charcoal, and then with a mixture of molybdic oxide and charcoal powder which had been previously well calcined; the straight end of the tube was connected with a chlorine apparatus, and dry chlorine passed for a considerable time through it. That part of the tube with the charcoal was now heated until the charcoal was incandescent, when the other portion of the tube containing the mixture was gradually heated to redness. The current of chlorine was continued during the whole operation; it was rapidly absorbed and various chlorine compounds formed, of which we were able to distinguish the following:—

The most volatile compound which condensed in the most anterior curved part of the tube was the biacichloride of molybdenum; the quantity was small; immediately behind this was the compound described by Berzelius under the name of chloride of molybdenum; it formed dark brown, shining crystals resembling iodine. The fragments of charcoal were coated with a black crust; and on allowing the tube to cool, its whole length was covered with a yellowish crystalline mass which did not appear to be very volatile. Of these compounds, which we will designate by the letters A, B, C, and D,

A consists of biacichloride, gives a clear solution with water, and deliquesces in the air;

B appears in fine, dark brown, shining needles, gives with water a blue solution, from which ammonia precipitates a brownish red oxide;

C is not altered by exposure to the air, possesses a black colour, is very slightly and slowly acted upon by alkalies, and insoluble in water;

D is readily soluble in water; the solution gradually becomes blue, but nothing is precipitated by ammonia.

These statements, which are in the highest degree imperfect, are merely intended to show that we know very little of the chlorine compounds of molybdenum, and that the subject requires a thorough investigation.

Experiments with Sulphuret of Molybdenum.

Starting from the fact that sulphuret of molybdenum, when heated with sufficient access of air, is gradually oxidized and converted into molybdic acid, we imagined that if we were able to prepare artificially a pure sulphuret of molybdenum with exactly the same composition as the native mineral, and converted this by roasting completely into molybdic acid, which remains, and into sulphurous acid, which is volatilized, it was to be expected that an accurate atomic weight of molybdenum might be calculated from the difference in weight of the sulphuret of molybdenum employed. No objection can be urged to this method *à priori*; it is simple, and the simpler an analytical method the more certain the result in general. If we assume that the oxidation can be effected completely, the method consists in three weighings:—1, counterpoising the glass tube in which the roasting is to be effected; 2, weighing with the pure sulphuret of molybdenum; and 3, weighing the molybdic acid obtained. We shall now prove that our theory was correct, and that we have succeeded in determining with certainty the atomic weight of molybdenum based upon the atomic weight of sulphur.

The artificial sulphuret of molybdenum was prepared according to the method described in the first part of this investigation for obtaining pure molybdic acid, with this difference only, that the sulphuret was dried over sulphuric acid under the air-pump. If this precaution is omitted, by drying it in the air or at 212°, the sulphuret always exhibits an acid reaction, owing to the formation of some sulphate of the protoxide of molybdenum. It soon dried under the air-pump, where it was kept until no further loss in weight could be observed. We thus prepared two different quantities of sulphuret of molybdenum, using the following proportions:—

For sulphuret A we fused together 4 grms. molybdic acid, 8 grms. sulphur, and 6 grms. carbonate of potash; for the sulphuret B we used 11 grms. trimolybdate of potash, 20 grms. sulphur, and 6 grms. carbonate of potash.

After we had thus prepared a pure sulphuret, the experiments were made in the following manner:—a glass tube provided in the centre with a bulb was stopped at one end with a little asbestos, and the whole then heated to redness in a current of dry air, and when cold counterpoised and the sulphuret of molybdenum weighed in it. Upon this the bulb with its contents was heated in a current of dry hydrogen, when a small quantity of water was expelled, but we were never able to observe any disengagement of sulphurous acid. But if the sulphuret of molybdenum had not been dried under the air-pump, there was always some sulphurous acid liberated on heating it in hydrogen. When the sulphuret had been perfectly dried in this manner, the hydrogen was expelled after cooling by dry air and the weight of the sulphuret again determined. The difference between the first and second weighing never amounted to more than from 1 to 2 milligrammes at the furthest. That part of the glass tube in which the asbestos was, was connected with an aspirator, and the bulb heated gently with a small spirit-lamp, ordinary moist atmospheric air, and only at the end of the operation dry air being allowed to pass over the sulphuret. This operation does not require a very high temperature, but the contents of the bulb must be frequently shaken, so as constantly to present fresh surfaces to the reaction, in order that the mass may not cake into one lump. If these precautions are neglected, a portion of the molybdic acid encloses some undecomposed sulphuret and prevents the complete oxidation, as we shall show further on. If dry air is allowed to act upon the sulphuret from the very beginning of the operation, the roasting is just as complete, but it is slower. The operation, when from 1 to 2 grammes of sulphuret is employed, lasts from two to three hours. It is not advisable to employ larger quantities, as the oxidation is then effected with greater difficulty, and it is impossible to be certain that no portion has escaped decomposition. When the sulphuret was converted into the yellow molybdic acid we passed dry air through the tube, first with the application of heat, and afterwards when it had cooled; it was then weighed. This operation of roasting was repeated twice, each time for half an hour, to be certain that the oxidation was complete. The difference in the weight between the first and third weighing never amounted to more than half a milligramme, and the second and third weighings always coincided.

To convince ourselves finally that the whole of the sulphuret had been really converted into molybdic acid, we dissolved the acid in ammonia, and always obtained a perfectly clear solution. The following are the experiments.

Sulphuret of Molybdenum A.

			grm.		per cent.	
I.	1·3558	grm. MoS^2 gave	1·2174	molybdic acid =	89·7919	molybdic acid.
II.	1·329	... ...	1·1925	... ...	89·7291	...

Sulphuret B.

III.	1·487	grm. MoS^2 gave	1·333	molybdic acid =	89·6436	...
IV.	1·4225	... ...	1·2761	... ...	89·7082	...
V.	1·403	... ...	1·2637	... ...	90·0712	...
VI.	1·268	... ...	1·1416	... ...	90·0315	...
VII.	1·2947	... ...	1·1622	... ...	89·7660	...
VIII.	1·4957	... ...	1·3426	... ...	89·7640	...
IX.	1·299	... ...	1·1652	... ...	89·9272	...
X.	1·1138	... ...	1·0009	... ...	89·8635	...

We must exclude from this series of experiments as erroneous V., VI. and IX., the shaking having been omitted.

If we compare the other seven experiments and take the average, we find that 100 parts of sulphuret give 89·7523 molybdic acid. But before we proceed to draw further conclusions from these experiments, we must first show that the artificial sulphuret used in these experiments has the same constitution as the native mineral. For this purpose we have analysed specimens of native sulphuret from two different localities in Sweden; the one was from Lindås in Småland, and the other from the district of Bohus. The analyses were likewise made by roasting, and when this was finished the molybdic acid dissolved in ammonia, when the insoluble gangue was left behind and could thus be estimated: of the sulphuret from Lindås we made three determinations, viz.—

I.	1·4847	grm. native sulphuret gave	1·3382	molybdic acid &	0·0427	gangue.
II.	1·400	... ...	1·2599	... ...	0·0112	...
III.	2·247	... ...	2·0235	... ...	0·0252	...

IV. 1·371 native sulphuret from Bohus gave 1·2348 grm. molybdic acid and 0·0430 gangue.

We have moreover examined the artificial sulphuret obtained by treating the neutral molybdate of potash with sulphuretted hydrogen.

V. 0·3432 grm. sulphuret gave 0·3064 molybdic acid.

If we deduct the quantities of gangue found in the native sulphurets of molybdenum, 100 parts of native sulphuret yield, according to—

I.	89·841
II.	89·912
III.	89·943
IV.	89·744
V.	89·277

and these results prove beyond a doubt the identity of the native sulphuret, and the artificial sulphuret of molybdenum. If we calculate the per-centage composition of the sulphuret of molybdenum according to theory, it consists of—

	a.	Calc.	*b.*	Calc.
1 at. Molybdenum	588·966	59·464	575·829	59·009
2 at. Sulphur . .	401·500	40·536	400·000	40·991
	990·466	100·000	975·829	100·000

The analyses give, after deducting the gangue, for the native sulphuret the following results:—

	Lindas, I.		Lindas, II.	
	a.	*b.*	*a.*	*b.*
Molybdenum .	59·522	59·067	59·569	59·114
Sulphur . .	40·478	40·933	40·431	40·886

	Lindas, III.		Bohus, IV.	
	a.	*b.*	*a.*	*b.*
Molybdenum .	59·588	59·133	59·458	59·004
Sulphur . .	40·412	40·867	40·542	40·996

It is evident that the gangue in the sulphuret of molybdenum is a mere mechanical impurity, from its quantity varying in the mineral. If in calculating the analyses we take the gangue into consideration, we find—

	I.	II.	III.	IV.
Gangue . . .	2·876	0·800	1·1215	3·1364
Molybdenum .	57·368	58·627	58·4700	57·1534
Sulphur . .	39·756	40·573	40·4085	39·7102

These analyses differ from those made by Buchholz, Brandes, and Seybert; but these chemists never determined accurately the amount of gangue in the native sulphuret, and, moreover, the atomic weight employed by them was less certain.

Having shown the identity of the native and artificial sulphuret of molybdenum, we will now return to the first experiments of roasting the artificial sulphuret, where we found that 100 parts sulphuret (MoS^2) furnished 89·7523 parts molybdic acid. This result is founded solely upon experiments; but if, starting from this, we wish to calculate the atomic weight of molybdenum, it must be based upon the number representing the atomic weight of sulphur. Berzelius determined the atomic weight of sulphur to be 200·75, by converting a known weight of chloride of silver into sulphuret. Erdmann and Marchand have arrived at the number 200·000 by the analysis of the sulphuret of mercury. The starting-point for Erdmann and Marchand's number is not only dependent on the atomic weight of mercury, but the most important question is, Were these chemists perfectly certain that the sulphuret of mercury was free from any mechanical admixture of metallic mercury? As this was not ascertained, we do not think that the atomic weight of sulphur found by them, although highly probable, can be admitted unconditionally. With regard to Berzelius's atomic weight for sulphur, it is liable to the objection, that perhaps a minute quantity of chloride of silver surrounded by sulphate of silver was withdrawn from the action of the sulphuretted hydrogen. We therefore treated 5·5967 grms. chloride of silver with sulphuretted hydrogen and obtained 4·8395 sulphuret of silver. Whence, admitting Berzelius's atomic weights for chlorine and silver, that of the sulphur is found to be 200·71; but on dissolving the sulphuret of silver in pure nitric acid, a small quantity of undecomposed chloride of silver remained undissolved, and, consequently, the number 200·71 must be too high. We wished to decide this important question, but have been prevented by various circumstances, and, on that account, all the theoretical calculations we have made, and also that of the equivalent of molybdenum, have been calculated with the two different atomic weights for sulphur.

Now, if we calculate the equivalent of molybdenum from the quantity of molybdic acid which we obtained from 100 parts of sulphuret of molybdenum, we have, when the atomic weight of sulphur is assumed to be 200·75,—

$$100 : 89{\cdot}7523 = x + 401{\cdot}5 : x + 300$$
$$x = 588{\cdot}966 \quad . \; . \; . \; . \; . \; . \; . \quad (a.)$$

and when the atomic weight of sulphur is 200·000,—

$$100 : 89{\cdot}7523 = x + 400 : x + 300$$
$$x = 575{\cdot}829 \quad . \; . \; . \; . \; . \; . \; . \; . \quad (b.)$$

Although the difference between the two atomic weights here assumed for sulphur is not very great, its influence in calculating the atomic weight of molybdenum is very considerable, the difference between *a* and *b* amounting to more than 13 units, consequently to more than 1 entire equivalent of hydrogen. That the equivalent *a* must be too high is not merely evident from the objections which we have made respecting the atomic weight of sulphur, but is also confirmed by the following circumstances:—

I. Without laying too great stress upon the reduction of molybdic acid by hydrogen, we may nevertheless direct attention to experiment II., this agreeing much better with the numbers calculated according to the atomic weight *b*, than according to *a*.

II. The analyses of the trimolybdate of potash, the only salt which can be obtained perfectly pure, agree much better with the numbers calculated according to *b*, than with those according to *a*.

III. The most decided proof for the lower atomic weight of molybdenum is derived from the results of the experiments of fusing molybdic acid with carbonated alkalies. These results agree with each other, and likewise with the atomic weight *b* very well. The atomic weight deduced from these experiments is founded upon that of carbon, which it may be admitted is determined as accurately as possible in the present state of chemistry. Even should a small uncertainty exist with respect to this number, it would have very little influence upon the atomic weight of molybdenum. We therefore consider ourselves justified in drawing the conclusion, that the atomic weight of molybdenum is 575·829, and the per-centage composition of molybdic acid

Mo 65·714,
O 34·286.

LXXVII. *On a Property of the Hyperbola.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THOUGH I am but an equation, and as such, more accustomed to be solved than to solve, I cannot any longer endure the injustice which is done to me with respect to the curves I have the honour to control. Want of long suffering cannot be imputed to me, or to any of my species; we even carry the Christian maxim further than the Christian law requires: for in what manner soever any one smites us on one cheek, we not merely turn to him the other also, but insist upon his treating it in the same manner, and cannot be satisfied until it is done. Coupled as we are with the very notion of justice (for what do the scales represent but an equation between weights?) I can no longer bear it should be thought that I have made a wrong balance, or consented to an unequal division of property.

All who have made me their study are generally aware of the impartial manner in which I have managed that every property of the ellipse should be accompanied by another of precisely the same character, belonging to the hyperbola. Why then should they allow that cases of palpable non-symmetry do sometimes occur: and, which is even worse, why do they suppose that I have allowed the ellipse, in certain cases, to rob the hyperbola of its birthright without any compensation? For an example of the first: in the hyperbola, the diagonal of the parallelogram described on semiconjugate diameters has, for the locus of its fourth point, the asymptotes, an extreme case of the hyperbola itself; while, in the ellipse, the corresponding point has an ordinary ellipse for its locus, having the semiaxes of the former increased in the ratio of $\sqrt{2}$ to 1. How can any one imagine there is not a word to be said upon this? Again, the circle on the major axis is, in the ellipse, the locus of the intersection of the perpendicular from a focus upon the tangent; and *this same circle* is still the locus, when an hyperbola on the same major axis is substituted for the ellipse. Now symmetry would require that, in this second case, the equilateral hyperbola should take the place of the circle: why did not the mathematicians, when they discovered that such was not the case, bestow no pains on the clearing of my character? Did they suppose that I would allow the ellipse to take and hold the share of the hyperbola in any property, without restoring the equilibrium by giving to the hyperbola the share of the ellipse in some other? I do not go for damages: but I think I have a right to such reparation as can be made by inserting demonstration of the following properties in future works on conic sections:—

1. Every ellipse, and every hyperbola, has, the first two ellipses, the second two hyperbolas, related to it. One of them is an extreme case, having vanishing axes: it is the centre in the ellipse, the asymptotes in the hyperbola. The other is ordinary, having axes increased in the proportion of $\sqrt{2}$ to 1. Each of either pair has this property, that if a chord P Q of the original cut either of the related curves in R, the rectangle under R P and R Q is equal to the square on the semidiameter parallel to that chord. The extreme case in the ellipse corresponds to the ordinary case in the hyperbola, and *vice versâ*: and thus the fourth point of the parallelogram above mentioned, has the asymptotes for its locus in the hyperbola, and the other sort of ellipse for its locus in the ellipse.

2. A line being given, let the line which makes with an axis of coordinates the supplement of the angle made by the first be called the *supplemental line* of the first. Take an ellipse and hyperbola with the same major, and any minor, axis; draw a tangent to either, and from the focus *of the other* draw the supplemental line of the perpendicular to that tangent. The locus of the intersection of this supplemental line with the tangent is the *equilateral hyperbola* which has the same major axis as the given ellipse or hyperbola, to whichever of the two the tangent was drawn.

Thus you may see that I have not, in this instance at least, favoured the ellipse at the expense of the hyperbola, without a corresponding reparation to the hyperbola at the expense of the ellipse. Should any similar reflections be cast on me in future, I shall take some opportunity of answering them, before they have run for 2000 years.

I am, Gentlemen, yours to command,
THE GENERAL EQUATION OF THE SECOND DEGREE.

Dec. 21, 1848.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 158.]

Nov. 23, 1848. "ON the Chemical Nature of Wax."—Part III. "On Myricine." By B. C. Brodie, Esq. Communicated by Sir B. C. Brodie, Bart., F.R.S.

This paper is the last of three papers on the chemical nature of wax, and contains the investigation of that portion of bees-wax which is soluble only with difficulty in boiling alcohol. This body could never be rightly investigated before the discovery of the true nature of the other constituent of the wax, namely, the cerotic acid, for the absence of which no test was known, and the products of the decomposition of which would materially interfere with any experiments on the nature of the myricine. When the cerotic acid has been absolutely removed by repeated boiling of the wax with alcohol, a substance remains, which is saponifiable, but with difficulty. From the products of saponification the author isolated palmitic acid, $C_{32} H_{32} O_4$, and a new wax-alcohol, analogous to, but yet different from cerotine, described in a former paper. This alcohol, melissine, has the formula $C_{60} H_{62} O_4$. By oxidation of this substance by means of lime and potash, the acid $C_{60} H_{60} O$, melissic acid, was obtained; and by the action of chlorine, a body analogous to chloral, a substance, that is, of the aldehyde series, but with a substitution of between fourteen and fifteen equivalents of chlorine for hydrogen. In its conversion into this substance the alcohol loses two equivalents of hydrogen, without substitution. The author also investigated the products of the distillation of myricine. From these he procured likewise palmitic acid and a solid hydrocarbon, which, rectified over potassium, had a melting-point of 62°, and contained, as shown by analysis, carbon and hydrogen in *equal* equivalents. The analogy of the mode of formation of this substance to cerotine from Chinese wax shows that it is the hydrocarbon, melene, $C_{60} H_{60}$. By repeated crystallization from ether a substance was obtained from the impure myricine, of a crystalline character, melting at 72°; the analysis of which agrees with the formula $C_{92} H_{92} O_4$, which explains the reactions of the substance.

The general conclusion from this investigation is, that waxes are a class of bodies which, chemically speaking, stand in the same relation to fat as do fatty bodies to the alcohol and acetic acid of vinous fermentation; all which bodies are members of one chemical series, possess an analogous chemical character, and are susceptible of analogous transformations.

"An account of Astronomical Observations proposed to be made in South America." By S. M. Gilliss, in an extract of a letter to Lieut.-Col. Sabine, R.A., For. Sec. R.S. Communicated by Lieut.-Col. Sabine.

DEAR SIR, Washington, October 25, 1848.

During the month of April last I sent to you, through the Royal Society, a printed report from one of the naval committees in congress, recommending a grant of funds for the purpose of sending an astronomical expedition to the most available point in South America, to make observations which should have for their object the improvement or verification of the solar parallax. The basis of this report was a correspondence between Dr. Gerling of Marburg, other astronomers, and myself, that had been presented for consideration by the Secretary of the Navy, and, in accordance with the views of the Philosophical Society and of the Academy of Arts and Sciences, had been laid before congress for their decision.

Conformably with the recommendation of the committee, an appropriation was made, and the Honourable Secretary of the Navy directed to employ it in making the observations requested by the two societies just named. The command of the party has been assigned to me, and a plan of operations submitted to the societies having received their sanction, has been approved by the Secretary. This is briefly as follows:—

To embark the instruments and their assistants by the 1st of June next for Valparaiso, for which place I shall leave New York per steamer, *viâ* Chagres and Panama, at the same time. As the voyage by this route may be readily made in thirty-five days, I shall be able to select a suitable station and make all necessary arrangements prior to the arrival of the instruments; but it is very probable that Santiago or Talca will be chosen for our operations, and from the solicitude expressed by the resident Chilian Chargé d'Affaires, the preponderance is on the side of Santiago. Moreover, it is thought that if Santiago is chosen and we remain nearly four years, as contemplated, the Chilian government will be induced to establish a permanent observatory there.

The observations I propose to make are,—Mars on the meridian and extra-meridian, during the oppositions of 1849 and 1852; and Venus under analogous conditions at the inferior conjunctions and stationary terms of 1850 and 1852. These observations will be differential, and (as the grant of congress implies) are the paramount objects of the expedition; but as they will occupy only a portion of our time, the following series have also been decided on:—

1st. With a view to improvement in the constant of lunar parallax, Burckhardt's semidiameter, and the local longitude, the moon and culminating stars on the meridian; both limbs at opposition and near conjunction.

2nd. The smaller planets on the meridian.

3rd. A catalogue of stars to the 8th magnitude inclusive, within 60° of the south pole.

Three hours of every suitable night will be given to this work, and the arrangement is such that the whole 60° may be examined and three observations made of each star within the proposed term of residence.

4th. Lunar occultations. Longitudes resulting from *culminations* as compared with *occultations*, have exhibited an *extraordinary* difference, to be accounted for perhaps by an error in the assumed semidiameter; this series of observations is considered necessary.

5th. It has been thought that useful information to terrestrial refractions will be obtained from observations in the two hemispheres on circum-zenith stars at each place of observation, and six stars have been selected which are near the zeniths of Talca and Washington to be observed on the meridian.

6th. Comets when discovered in the course of other observations, and at their periodic returns.

7th. Magnetical observations. The term-days of such observatories as continue to keep them has been set apart to obtain the three elements of the earth's magnetic condition, and on occasional term-days the observations would be made uninterruptedly during twenty-four hours with the declinometer.

8th. Meteorological observations at three stated hours (9 A.M., 3 P.M. and 9 P.M) of each day; to embrace records of barometer; dry, wet, radiating and registering thermometers; rain-gauge; wind-vane and clouds. Hourly observation on equinoctial and solstitial term-days.

9th. Earthquakes, the registrations of a seismometer.

A meridian circle of 3 feet diameter, with a telescope of 52 lines aperture, has been ordered from Pistor and Martins; the Smithsonian Institution has just authorized me to obtain an equatorial of 6 (French) inches aperture, with clock motion, which they will lend me; in addition to which I have a 48 lines equatorial by Fraunhofer; these, with a clock and one or two portable instruments, comprise my astronomical equipment. In magnetism I am most especially desirous of your advice. Pray give me all possible information, and recommend such instruments, times and methods of observation as our limited grant from congress and the number of assistants will enable me to obtain and use as designated.

Such of the meteorological instruments as we do not possess will be ordered in a few days; but for a proper seismometer I know not where to go, the only account seen being in one of the earlier volumes of the British Association. The instrument is to be obtained at the expense of the Smithsonian Institution, and if you will do us the favour to confer with some of the members of your Association or of the Royal Society who have taken interest in the investigation of earthquakes, you will especially oblige Professor Henry and myself. The opportunity which my residence in Chili will offer to make accurate notes of these occurrences may perhaps be productive of useful results. I do not doubt that the instrument will be ordered the moment I hear from you.

Whilst I have asked your views especially on magnetism, I shall be grateful for counsel respecting any of the observations which are contemplated, or for suggestions of new series which the locality offers peculiar advantages for making. I have planned this expedition and embark in it with a desire to render the greatest possible amount of useful data to science; and you may be assured of hearty zeal in whatever yourself or any of the Fellows of the Royal Society may propose.

Believe me, dear Sir, very respectfully,
Your friend and servant,
S. M. GILLISS.

Lieut.-Colonel Edward Sabine, R.A., F.R.S. &c.

LXXIX. *Intelligence and Miscellaneous Articles.*

CALCULATING CUBES. BY J. E. RYFFEL.

A SIMPLE, novel, and efficacious method of instructing children in the elements of arithmetic has just been published by Mr. Ryffel (Ackermann, Strand), in the form of a box, containing one hundred coloured cubes. Their intention is to realize to a child's mind the idea of numbers by visible and tangible objects, illustrating by their juxtaposition the first rules of arithmetic. But "besides teaching younger children how to count," says Mr. Ryffel, "these cubes may be used in various other ways, all tending to prepare and exercise the child for more serious studies in arithmetic as well as the higher branches of mathematics." The pamphlet which accompanies these cubes contains instructions for their use, and lessons so fully worded, that elder children may with a little application easily master the system, and apply it in giving instruction.

ANALYSES OF THE PHOSPHATES OF MANGANESE.

M. W. Heintz has analysed the different protophosphates of manganese:—

Phosphate of Manganese.—This salt is obtained by precipitating a salt of manganese with common phosphate of soda, and dissolving the precipitate in phosphoric acid. By evaporation the salt is obtained in small prismatic crystals, which are very soluble in water, but insoluble in alcohol. This salt consists of—

Phosphoric acid . .	48·98	to	49·17
Protoxide of manganese	25·42	...	24·60
Water of hydratation .	13·24	...	12·77
Water of crystallization	13·19	...	13·24
	100·83		99·78

The water of crystallization is dissipated between 230° and 248° F.

Trisphosphate of Manganese.—This salt is obtained by precipitating sulphate of manganese with common phosphate of soda. It is a white light powder, very slightly soluble in water and perfectly amorphous.

M. Heintz found the trisphosphate of manganese to consist of—

Phosphoric acid	29·32	to	29·82
Oxide of manganese..................	43·91	...	43·66
Water expelled between 230° and 248° F.	15·95	...	15·85
Water expelled by calcination..........	11·32	..	11·12
	100·50		100·45

Ammoniaco-diphosphate of Manganese.—When a solution of sulphate of manganese containing ammonia and its hydrochlorate is gradually dropped into an ammoniacal solution of phosphate of soda, a viscid precipitate is produced, which after some time is converted into crystalline scales. This is Otto's salt. Analysis gave—

	Otto.	Heintz.
Phosphoric acid	37·86	38·37
Oxide of manganese	37·84	38·35
Ammonia	9·16	23·51
Water	15·14	
	100·00	100·23

Diphosphate of Manganese.—A solution of sulphate of manganese is to be rendered acid by acetic, hydrochloric or phosphoric acid, and to this is to be added common phosphate of soda until the precipitate, which at first disappears, becomes permanent, some manganese remaining in solution. By standing, this precipitate is converted into a reddish crystalline mass, which is but slightly soluble in water, but readily so in acids.

This salt contains—

Phosphoric acid.....................	34·61	to	34·86
Oxide of manganese................	34·58	...	34·43
Water expelled between 230° and 248° F.	23·30	...	22·05
Water expelled at 392°	4·23	...	4·23
Water expelled by calcination........	4·38	...	4·38
	101·10		99·95

Journ. de Ph. et de Ch., Novembre 1848.

DISCOVERY OF COLUMBITE IN THE ENVIRONS OF LIMOGES.

BY M. DAMOUR.

The specimen of this substance was found in a quarry near Chanteloube, belonging to M. Alluaud, Sen., collected by M. Mathieu, a mineral dealer of Paris, and confounded with specimens of wolfram, triplite, heterosite and dufrenite, brought from the same place.

Its colour, hardness and great density, induced M. Damour to think that it did not belong to any of the preceding species; and a qualitative examination proved that it was almost entirely composed of columbic acid and oxide of iron, with a small quantity of oxide of tin.

This specimen had the form of amorphous nucleus, of the size of a hazel-nut, imbedded in yellowish-white felspar. Superficially it was of a tarnished bluish-black colour, with a shining, smooth fresh fracture. The powder was of a grayish-black colour. It scratches glass readily; its density was found to vary from 7·640 to 7·651. It is infusible by the blowpipe. When reduced to powder and mixed with tartrate of potash and carbonate of soda, it fuses on charcoal, and yields some globules of tin. When fused with borax and phosphoric salt, it reacts like iron. Acids do not act upon it.

To analyse this mineral, it was reduced to powder and fused with eight times its weight of bisulphate of potash, and the fused mass when cold was treated with a large quantity of hot water. The oxides of iron and tin were almost entirely dissolved in the liquor: the columbic acid was deposited in the form of a white powder, but still retaining a little oxide of tin and iron. It was digested in hydrosulphate of ammonia, by which the oxide of iron was converted into an insoluble sulphuret, and the sulphuret of tin remained dissolved in the sulphurous ammoniacal liquor, which was filtered and saturated with acetic acid, and the sulphuret of tin precipitated was collected. The columbic acid, blackened by the sulphuret of iron, was treated with hydrochloric acid; by this the iron was dissolved, and the columbic acid after washing was perfectly white; it was dried, ignited and weighed.

The liquor separated from the sulphuret of tin was treated with ammonia and its hydrosulphate; by this all the iron was obtained in the state of sulphuret; the liquor separated from it contained no earthy base whatever.

The sulphuret of iron was dissolved in aqua regia; by evaporating the solution and treating the residue with water, a minute quantity of silica was left. The ferruginous liquor was saturated with ammonia, and the quantity of protoxide of iron was inferred from that of the peroxide obtained.

100 parts of the mineral yielded—

Columbic acid	82·98
Oxide of tin	1·21
Protoxide of iron with a trace of oxide of manganese	14·62
Silica	0·42
	99·23

M. Damour observes, that although the columbic acid is reckoned as pure, it may possibly contain a small quantity of the niobic and pelopic acids recently discovered by M. Rose in the columbiferous minerals of Bavaria and America. The columbite of Limoges is denser than these, of which the former has a density of 6·390, 6·078, 5·976, and the latter of 5·708, 5·495. That of Tamela in Finland, which M. Rose considers as pure columbic acid, is of density, 7·197, 7·476, 7·510.—*Annales des Mines*, tome xiii.

ON ALLUAUDITE,—A NEW PHOSPHATE OF IRON, MANGANESE AND SODA. BY M. A. DAMOUR.

This substance was recently found imbedded in pigmatite near Limoges.

Its colour when viewed in mass is clove-brown; its fracture is laminated and shining, and in some parts, which seem to have undergone some alteration, it is chatoyant like hyperstene. Fracture indicates a right rectangular prism as its crystalline form. It scratches fluor spar, and is scratched by a steel point; the powder is brownish-yellow. Its density is 3·468.

Moderate-sized specimens appear occasionally to contain accidental admixtures of peroxide of iron and black oxide of manganese: they are sometimes associated with green phosphate (dufrenite) and blue phosphate of iron.

By the blowpipe, in platina forceps, this mineral fuses very readily with intumescence, into a black globule, which is not magnetic; in the oxidizing flame, it dissolves entirely in phosphoric salt and indicates manganese; heated in a tube, it decrepitates and disengages a small quantity of neutral water.

When in fine powder it dissolves readily in cold hydrochloric acid; the solution at first is of a blackish colour and evolves chlorine; if the temperature be raised to 140° F., the liquor becomes bright, and of a brownish-yellow colour. Sulphuric acid diluted with four times its bulk of water does not act upon it cold. At 212° F. it dissolves slowly, and the solution is of a reddish-violet colour; this colour indicates manganese of a higher state of oxidizement than manganous oxide, but it occurs only with the chatoyant portions of the specimen.

Nitric acid acts upon it with difficulty, but oxalic acid when heated dissolves it completely; when the solution is suffered to evaporate spontaneously, granular crystals of oxalate of manganese and peroxalate of iron are deposited.

By analysis this mineral yielded—

Phosphoric acid	41·25
Peroxide of iron	25·62
Protoxide of manganese ..	23·08
Peroxide of manganese ..	1·06
Soda	5·47
Silica	0·60
Water	2·65
	99·73

M. Damour proposes the name of *alluaudite* for this mineral, in honour of M. Alluaud, well-known for attachment to the sciences. —*Ibid.*

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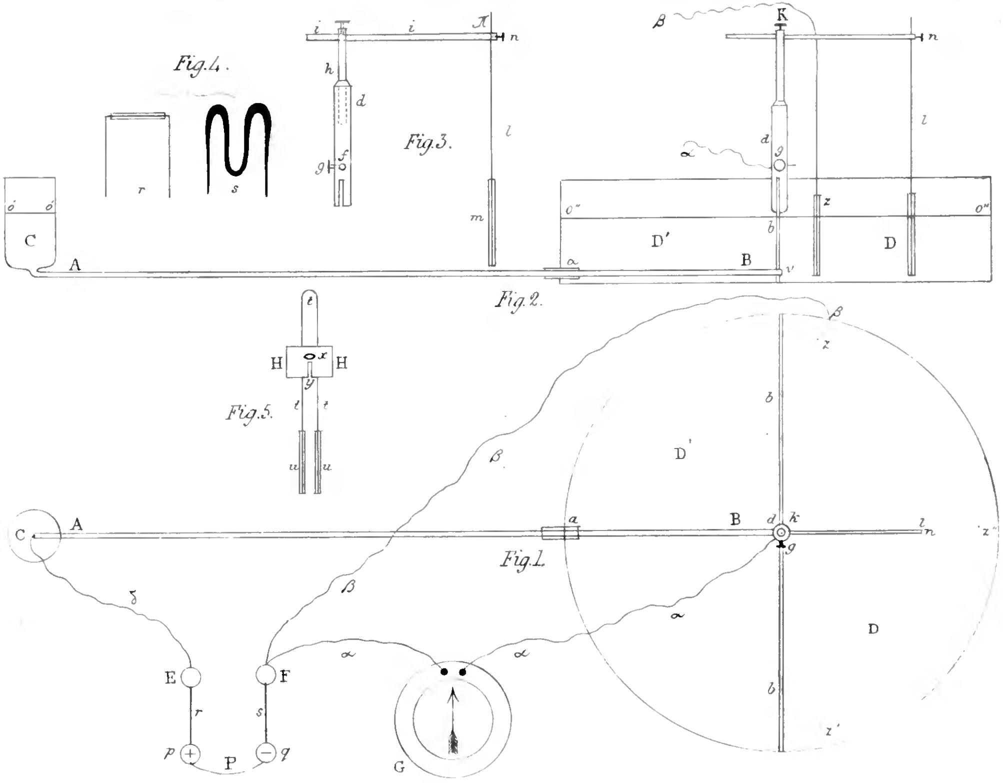

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

E. Wartmann, del^t.

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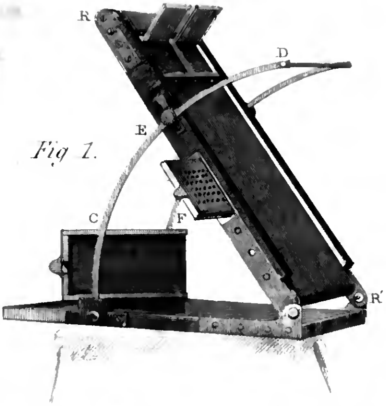

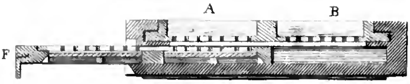

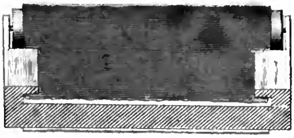

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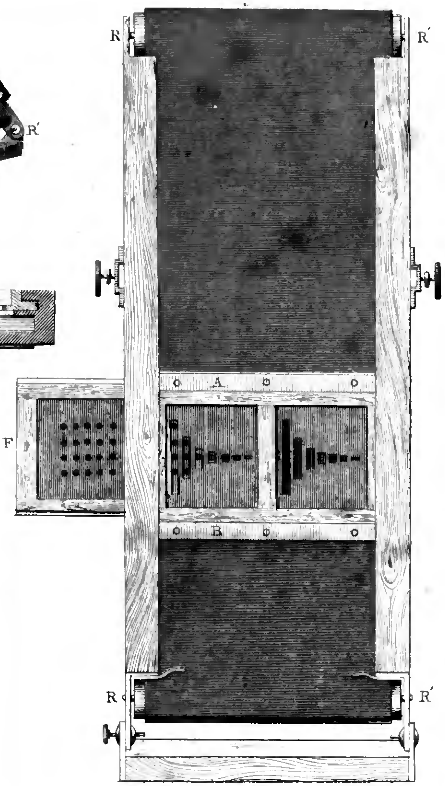

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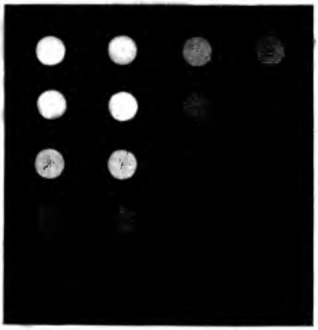

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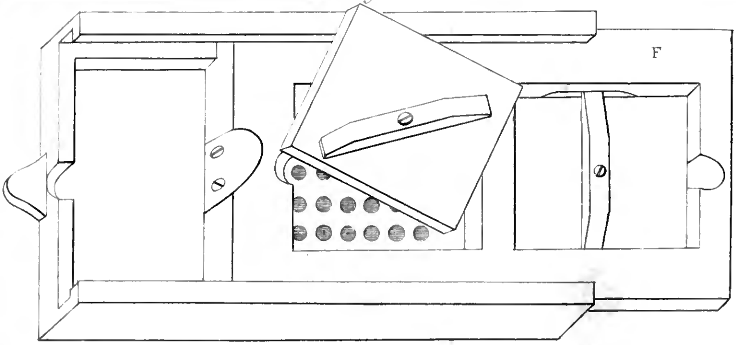

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